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Dene-Yeniseian: a critical assessment

The paper gives a detailed critical assessment of the so-called "Dene-Yeniseian" hypothesis of genetic relationship between the Na-Dene language family of North America and the Yeniseian family in Siberia (represented today by the Ket language as its sole survivor). The hypothesis, recently promoted by Edward Vajda and supported by several prestigious scholars, has drawn much attention from the linguistic community, but, as the current paper indicates, still lacks a thorough critical evaluation that would focus exclusively around the quality of the comparative data. The paper attempts to present such an evaluation for at least some of the data, such as comparative verbal morphology, certain phonetic correspondences, and basic lexicon involved in Vajda's comparison. It is concluded that only a part of these comparisons stands proper historical criticism, and that this part, by itself, is insufficient to prove a specifically "Dene-Yeniseian" link beyond reasonable doubt. However, it may be quite useful for the ongoing research on Na-Dene and Yeniseian languages as parts of a larger taxonomic unit (the "Dene-Caucasian" macrofamily), within which these two taxa may be related on a more distant basis than originally proposed.

Keywords: Dene-Yeniseian hypothesis, Dene-Caucasian hypothesis, Na-Dene languages, Yeniseian languages, linguistic macrofamilies, deep level language relationship, verbal morphology, typology of phonetic correspondences.

Introduction

On February 26–28, 2008, the University of Alaska Fairbanks held a special Dene-Yeniseian Symposium, intended to spread information on and initiate a productive discussion around research recently carried out by Edward J. Vajda — research that has allegedly resulted (as has been claimed by a number of specialists) in establishing a strong, methodologically sound claim to a genetic relationship between the Na-Dene family in North America and the Yeniseian family in Siberia (today, exclusively represented by its sole survivor, Ket). Two years later, the results of the Symposium were officially published as a special volume in the *Anthropological Papers of the University of Alaska* periodical series, entitled *The Dene-Yeniseian Connection* (University of Alaska Fairbanks, 2010).

Since Vajda's hypothesis has attracted significant press attention and has been endorsed by several experts in historical linguistics and linguistic typology, *The Dene-Yeniseian Connection* volume is not to be taken lightly; it is clearly a book that deserves a more detailed and thorough assessment than it has received in the few brief professional reviews of it that I have encountered so far (such as [Campbell 2011] and [Rice 2011]). I myself have already published a brief note on Vajda's theory [G. Starostin 2010a], following up on a presentation made at the Athabaskan Conference (University of Berkeley, 2009); the published paper, however, only voiced the principal concerns without backing them with sufficient argumentation, and its chief focus was on the idea that it is substantially incorrect to explore the possible genetic connection between Yeniseian and Na-Dene without an equally thorough look at other potential members of the same deep-level language family.

It is now high time to look in more detail at Vajda's evidence on its own merits, and attempt to answer the two most pressing questions: (a) is the presented evidence sufficient to establish a genetic link between Yeniseian and Na-Dene "beyond reasonable doubt"?; (b) are the methods and argumentation paths employed in presenting the evidence generally valid for establishing *any* kinds of intuitively non-obvious genetic links between language families?

First and foremost, one would think that detailed answers to these two questions, coming from a variety of experts specially assembled for the occasion, should be found in the pages of *The Dene-Yeniseian Connection* itself. While the centerpiece of the volume is undeniably Vajda's extensive, 60-page long paper ("A Siberian Link with Na-Dene Languages") that lays out the typological, grammatical, and lexical evidence for Dene-Yeniseian, the remaining 300 pages could certainly have incorporated at least several papers of comparable length — papers that would demonstrate that their authors have thoroughly studied the presented evidence and given it an objective evaluation based on a well-defined set of criteria.

However, the papers that may be qualified as actual assessments of Vajda's results comprise a surprisingly humble amount compared to works that only bear an indirect relation to the main subject at hand. In particular, nearly two hundred pages of the volume are allocated for a section called *The Interdisciplinary Context for Dene-Yeniseian*. This section contains at least one linguistic paper that is of significant importance to the issue: Jeff Leer's "The Palatal Series in Athabascan-Eyak-Tlingit, with an Overview of the Basic Sound Correspondences" (pp. 168–193), which presents some of the author's important recent advances in the reconstruction of Proto-Na-Dene and upon which, consequently, Vajda's own research on Dene-Yeniseian depends significantly. But the rest are, indeed, interdisciplinary papers, carefully distributed between geneticists (G. Richard Scott and Denis O'Rourke), archaeologists (Ben A. Potter), specialists in comparative mythology and ethnography (Yuri Berezkin), etc., most of which basically follow the same scheme in answering the question: "*Supposing the Dene-Yeniseian hypothesis is correct*, is there any direct or indirect evidence from branches of science other than linguistics to confirm it?"

The papers in question contain all sorts of useful data and valuable insights, but, no matter how strong the temptation to put "Dene-Yeniseian" into an interdisciplinary context here and now may be, all of these insights are completely irrelevant when it comes to resolving the main issue. The fact that there *are*, or that there *aren't* any conjectural correlations between comparative linguistic and genetic / archaeological /cultural, etc. data has no direct bearing on this main issue: whether or not Na-Dene and Yeniseian languages share a lowest common linguistic ancestor. Predictably, most of these papers neither rule out the possibility of a prehistorical "Dene-Yeniseian" ethnos, nor confirm it; but even if a convincing set of genetic or archaeological isomorphisms were to be found, the linguistic data would still have to stand on their own, since extralinguistic evidence is well known to be inadmissible in demonstrations of genetic relationship.

Out of the seven papers included in the section entitled *Commentaries on the Dene-Yeniseian Hypothesis*, four (by Michael Fortescue, Willem J. de Reuse, John W. Ives, and Don Dumond) do not deal with Vajda's evidence at all, presenting instead a series of stimulating speculations on the prehistory of the hypothetical Dene-Yeniseian taxon, and only three contain opinions or analyses that actually quote the comparative data and present concrete assessments.

Of these three, Eric Hamp's "On the First Substantial Trans-Bering Language Comparison" (pp. 285–298) produces a strange impression. Although its first sentences are phrased with remarkable boldness ("Yeniseian-Dene of Edward Vajda is correct. His demonstration, the truly important aspect of his scientific achievement, ranks among the great discoveries of this type of productive inferential reasoning, i. e. linguistic modern cladistics..."), the overall structuring of the paper, where offside excourses into Indo-European analogies are more frequent than remarks on Vajda's hypothesis itself, make it rather hard to understand exactly *why* "Edward Vajda is correct". As difficult as it is for me (although I fully acknowledge that this may be just a personal problem) to follow the author's somewhat convoluted train of thought, it may at least be understood that he expresses sincere admiration for the elegant homologies between Yeniseian and Na-Dene prosodic features and verbal patterns established by Vajda. No attempt, however, is made to test any of these homologies; they seem to be accepted on sheer trust, which, unfortunately, reduces the overall usefulness of the paper.

Johanna Nichols ("Proving Dene-Yeniseian Genealogical Relatedness", pp. 299–309) presents a far more robust argument in support of Vajda's evidence. She has devised a somewhat crude, but reasonable and well-explained statistical test that is supposed to show whether the amount of similarities in form and meaning observed between binary pairs of compared languages exceeds what should be naturally expected by chance or does not pass the threshold. This test, it is asserted, works reasonably well on Vajda's grammatical and lexical comparanda for Dene-Yeniseian, while at the same time failing to uncover statistically valid results for M. Ruhlen's earlier set of Dene-Yeniseian comparanda, established through "mass comparison" [Ruhlen 1998].

Nichols' statistical test is undoubtedly an interesting and thought-provoking idea, although I have doubts as to whether it incorporates a sufficiently well-detailed number of parameters to be able to serve as a universally applicable tool. However, regardless of whether the test itself is sufficiently robust or not, it goes without saying that *any* results of *any* formalized test may, at best, only be as good as the input data. In this particular case, the tested evidence rests on two assumptions that, as I will try to show below, are highly questionable: (a) the phonological and semantic correctness of Vajda's Proto-Yeniseian reconstructions of a set of verbal grammatical morphemes; (b) the historical correctness of the system of phonetic correspondences established by Vajda between Proto-Yeniseian and Proto-Na-Dene. If these assumptions turn out to be wrong — even if they turn out to be *partially* wrong — the results of Nichols' tests are essentially meaningless, and all the calculations will have to be redone, possibly on diminished evidence. Consequently, the paper suffers from the same flaw as Hamp's: the critical assessment of Vajda's evidence begins by missing the crucial first step — assessing the correctness of "first level" reconstructions and the credibility of the "second level" correspondences.

The third evaluative paper, by Andrej Kibrik ("Transitivity Indicators, Historical Scenarios, and Sundry Dene-Yeniseian Notes", pp. 316–319), is very short and does not venture far beyond typological argumentation. It does make one extremely important critical point, to which I shall return below, but overall, the briefness of the paper and the author's own admission ("...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...") clearly prevent it from playing a decisive role in the argument.

So why are the evaluative papers so short? And how has it become possible for a "mini-consensus" around Dene-Yeniseian to have formed so soon, when the majority of similar deep-level genetic relationship hypotheses, sometimes backed up with far more bulky collections of comparative data, still fail to gain approval from specialists in respective and adjacent fields? In my opinion, the reason behind this lies in a certain, intentionally chosen, *strategy of presentation*, which is as important for Vajda's principal paper as the comparative data themselves, and to some readers, perhaps even more important. There should be nothing surprising about the fact itself: comparative-historical linguistics is still a long way from becoming a fully integrated branch of mechanistically rigorous "science", and, as in any other branch of linguistics, its results often find acceptance or rejection based on a complex mesh of objective and subjective criteria. The strategy chosen by Vajda is undeniably much more *persuasive* than strategies usually chosen by "long-rangers" (below, I shall try to explain why), and this persuasiveness, from a certain point of view, is admirable. But in the long term, persuasiveness only works when it has been coupled with thorough objectivity; and I believe that it is every researcher's duty to be able to look beyond such concepts as "elegance", "originality", and "expectation-matching" when we are dealing with such a complicated object as language — which, as we all know, may just as well be "inelegant", "unoriginal", or "defying expectations" when it comes to specific situations.

I do not necessarily share Andrej Kibrik's humble opinion that only a practicing comparative linguist may be thoroughly qualified to assess a historical hypothesis of Vajda's caliber. In fact, one does not even have to be a professional "Yeniseianist" or "Athabaskanist" to make such an assessment, *as long as* the argumentation in favor of the hypothesis has not been based on specially selected data. On the contrary: I believe that a careful, line-by-line analysis of Vajda's paper will reveal quite a few weak spots even to those readers who have never had to deal with a single Yeniseian or Na-Dene language before, but are well aware of such things as historical phonetic typology, regularity of correspondences, and lexicostatistics. Unfortunately, it is quite likely that the majority of these readers will not want to perform such an analysis, concentrating on the conclusions more than on the gist of the argument.

My own position on Vajda's "Dene-Yeniseian", already voiced in the aforementioned short paper [G. Starostin 2010a], is clear enough: I am convinced that there exists significant evidence showing that both families may well be genetically related within the framework of a much larger macrofamily, provisionally called "Dene-Caucasian" (DC), and that this evidence may to some extent overlap with the comparanda amassed by Vajda for "Dene-Yeniseian" (DY). However, the same evidence does not, by any means, confirm that there is a specific "Dene-Yeniseian" node on the DC genealogical tree, i. e. that Na-Dene and Yeniseian languages share a "lowest common ancestor". If *all* of Vajda's comparanda were acceptable, "Dene-Yeniseian" could be perceived as a historical reality; the ratio of those that actually *are* acceptable strongly suggests that it cannot.

Within the scope of one paper it would be difficult to focus on both the "constructive" side of the argument (positive evidence for Dene-Caucasian) and the "critical" side (negative evidence for Dene-Yeniseian). Since the "constructive" side is currently being clarified in a joint paper by myself and John Bengtson, dealing with the current state and issues of the DC hypothesis [Bengtson & Starostin 2012][1], this paper will have to concentrate on the criticism. Namely, I will try to show that a large portion of Vajda's evidence for DY rests on (a) internal Proto-Yeniseian reconstructions that are themselves based on improbable assumptions rather than factual evidence; (b) phonetic correspondences that are not only questionable from a typological perspective, but also not sufficiently recurrent to be fully credible. What remains of the evidence is hardly enough to serve as convincing demonstration of DY as a realistic taxon.

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[1] The current taxonomy of Dene-Caucasian, advocated for in this paper on the basis of lexicostatistical calculations and shared innovations, is as follows: A. "Western Dene-Caucasian", consisting of two equidistant subbranches: **A.1.** North Caucasian + Basque; **A.2.** Yeniseian + Burushaski; **B.** "Eastern Dene-Caucasian": Sino-Tibetan + Na-Dene.

Not being an expert on issues of Athabaskan and Na-Dene comparative phonology, I will be evaluating the evidence primarily from the Yeniseian side; that said, thanks to the aforementioned detailed paper by Jeff Leer in the same volume, it is now much easier to distinguish between "stronger" and "weaker" Na-Dene reconstructions of phonemes, grammatical and lexical morphemes, and these issues will occasionally be addressed as well.

Before we proceed, however, I would like to specifically emphasize the fact that Vajda's research consistently rests on a professional foundation and takes into account most, if not all, of the results of previous studies on the subject — in this I completely concur with all the "admirers" of his work, who point out that diligence and methodological accuracy of this level are rarely met in the field of long-range comparison. If this accuracy remains insufficient to achieve the stated goal, it is only, I believe, due to the fact that the methodological foundations for historical comparison of language families on a deep level still remain on a "preliminary" level. Few people engage in long-range comparison, and even fewer can bring themselves to agree on the right way to do it. This implies that the specific "data-based" critical remarks, offered below, will sometimes inevitably plunge into methodological discussion. Personally, I believe that this is a good thing.

Verbal morphology evidence for "Dene-Yeniseian"

Typology.

Vajda notices significant typological isoglosses between the basic structures of complicated verbal templates in Yeniseian and Na-Dene, claiming that the homologies between the two are generally more striking than between Yeniseian and other prefixing languages of Eurasia, such as Burushaski, Sumerian, and Abkhaz (pp. 36–40). Shared "slots" include spatial prefixes, tense/aspect/mood (TAM) prefixes, subject agreement prefixes, and — possibly — semantically vague "classifiers", which are partially fossilized (fused with the root) and partially shifted to express other functions in Yeniseian, but still retain morphological "vitality" in Na-Dene.

It must be emphasized, however, that there is no concrete attempt on Vajda's part to reconstruct the basic structure of the DY verbal template: comparative tables that present such templates for attested and reconstructed languages alike only go as deep as "generalized Athabaskan" (table 8) and Vajda's own reconstruction of the Proto-Yeniseian template (table 11). An expected question is — why not, if these templates are so similar? Andrej Kibrik, in his aforementioned reply, may have the answer. He reminds (p. 317) that the Yeniseian template contains nothing that could be transparently analyzed as "transitivity indicators" or "classifiers", a crucial component in the typical Na-Dene form and, quite likely, one of the oldest sets of morphological markers in the paradigm, since it occupies the slot that is immediately adjacent to the root morpheme itself, and morphology is known to "grow in concentric circles".

Although Vajda does make attempts to discover some traces of Na-Dene "classifiers", they are universally weak (see below), which leads Kibrik to a logical assumption: "as long as the status of the immediately pre-root TIs is not clarified, morphological argument for the relationship largely fails" (p. 318). More precisely, it is not the "morphological argument" that fails, but the "morpho-typological argument": this particular incongruence does not, per se, invalidate the specific grammatical morphemes that Vajda is comparing — it invalidates the idea of an elegant common origin of the *templates*.

Furthermore, even if we somehow prefer to close our eyes on the “classifier” issue, the origins of the template still remain confusing. Over and over again, the reader encounters reference to the idea that at least some of the compared morphemes may be derived from ancient “auxiliary verbs”, in particular, the reconstructed “telic/atelic” markers *x^yi* and *ɢa* (see below). This idea, probably inherited from some of J. Leer’s work on internal reconstruction in Na-Dene, is never explored in sufficient depth, but adds an unpleasant element of vagueness to the discourse. If the original structure of DY veered more towards the analytic side, with auxiliary verbs bearing a large part of the grammatical information, does that imply that similar paths of grammaticalization took place independently in Na-Dene and Yeniseian already after the split? This would seem unlikely, not to mention that it seriously reduces the importance of morphological evidence as such. If, on the other hand, the system of cognates between morphological markers is projected by Vajda onto the original DY stage, why is it necessary in the first place to speculate on the possible origins of these markers, provided that such speculations are based not on comparative evidence, but on purely internal reconstruction within a highly hypothetical “macrofamily”?

That said, the typological argument on its own hardly means anything from the genetic point of view if the actual morphemes that occupy the morphological slots cannot be shown to share a common etymological origin in sound and meaning. Let us now take a brief look at some of that “fleshy” evidence, particularly the morphemes that play the most important part in J. Nichols’ statistical evaluation: TAM markers and spatial prefixes.

TAM markers: the telic/atelic opposition.

For the earliest stage of DY, Vajda reconstructs a binary set of markers, supposedly originating from even earlier “auxiliary verbs” (?):

	DY	Yeniseian	Ket	Navajo	Eyak	Tlingit
“Telic” marker	**x^yi*	**si-*	*s, i, ʌ, a*	*si-*	*s-*	*ɯu-*
“Atelic” marker	**ɢa*	**ɢa-*	*qo, o*	*yi-*	*ɢə-*	*ɢa-*

Without questioning the Na-Dene side of the reconstruction (which, at least from the phonetic side, is not completely obvious), I have to say that the proposed Yeniseian reconstruction, explained on pp. 43–45 of Vajda’s paper, is completely untenable. In order to arrive at “visually elegant” matches between Na-Dene and Yeniseian, Vajda has to (a) find phonetically similar external Yeniseian correlations to the Na-Dene “sibilant marker” and “uvular marker” and (b) be able to explain away everything else in the same slot of the Yeniseian paradigm as secondary transformations of these two markers — otherwise, the Yeniseian system will not be a proper “two-member paradigm” (as it is defined by J. Nichols on p. 305), and the likeness of chance similarities between Na-Dene and Yeniseian in this particular slot will increase.

The only part of these conditions that is satisfied concerns the match between Na-Dene **x^yi* → Eyak-Athabaskan **si* and Ket *s*. These morphemes are evidently similar and their consonantal constituent may be integrated into a regular system of correspondences. But even if we agree with Vajda’s treatment of Ket *s* as a former auxiliary, rather than a morpheme of pronominal origin (as it is argued in [Reshetnikov & Starostin 1995], and I am not ready to abandon that argument), nothing else checks out.

First, Vajda's attempts to derive nearly *all* of the so-called "conjugation markers" in modern Ket from a single original morpheme **si* are extremely forced. They were absent from the first draft version of his paper and represent an entirely new conception, which will probably be viewed as revolutionary by everyone with a background in Ket / Yeniseian verbal morphology studies. A detailed analysis of this conception will take a lot of space, so I will present just one brief point.

According to the analysis in [Reshetnikov & Starostin 1995], most of the verbal paradigms in Ket may be classified into two "conjugations", one of which contains the basic conjugational marker *-i-* both in the present and past tenses, while the other one has *-a-* in the present and *-o-* in the past. The morphophonological properties of these markers differ depending on the context, especially for the marker *-i-* which frequently falls victim due to vowel reduction and is deleted from the form, but the basic opposition is undeniable, as well as the correlation between "present *-a-* : past *-o-*", as in *d-a-j-śuk* 'I wade across' : *d-o-ń-śuk* 'I waded across', etc.

Now Vajda yields a complicated reanalysis of this situation, merging *-a-*, *-i-* (together with the morpheme *-s-*, which, according to most previous treatments, actually even occupies a different *slot* in the verbal form) as historical variants of one morpheme, and past tense marker *o* as a variant of *another* morpheme. In other words:

	Present tense	Past tense
"Verbal conjugation I"	*i* < **si*	*i* < **si*
"Verbal conjugation II"	*a* < **si* (*!*)	*o* < **ɢa*

The incongruence is not only obvious, but is also utterly unnecessary. It requires setting up complex, phonetically improbable transitions ("after a fricative, affricate, or aspirated stop, **x^yi* yielded allomorph *a-*, regardless of what prefix followed...") with lots of subsequent changes by analogy that still leave a lot of internal Yeniseian questions unanswered. Why has this been done? The only possible answer is — to make the system look more like the one established by J. Leer for Na-Dene.

Furthermore, even the past tense morpheme *o* does not look very much like Na-Dene **ɢa*, since it does not contain any traces of a back (let alone uvular) consonant. According to Vajda's correspondences, Na-Dene **ɢ* should yield Yeniseian **q*, not zero. A possible solution comes through the discovery of an irregular Ket verb, 'to kill', which forms its past tense in a unique way, by adding the morpheme *qo* instead of the more productive affixes *l* or *n*: *t-**qo**-k-ej* 'he killed you', etc. This morpheme is presented as the most transparent and segmentally compatible correlation with Na-Dene **ɢa*; however, since one irregular grammatical marker in one irregular paradigm is fairly thin evidence when we are aiming for a definitive paradigmatic reconstruction, an ingenious solution is presented — *qo* is etymologically equated with the much more frequent marker *o*, in which, according to Vajda, the original consonant was deleted because of its frequently occupying a word-internal position.

In other words: **qo-ku-ej* (where *qo-* = original TAM marker, *ku* = 2nd person obj. marker, *ej* = root morpheme) '(he) killed you' → **qo-k-ej* (the subject marker *t-* 'he' is a later morphological addition that did not influence the articulation of *qo-*), but, for instance, **d-ɯs-qo-l-bed* 'I rowed' (literally 'I-rowing-made') → *d-ɯs-o-l-bed*, with regular deletion of **q* after the final consonant of the first root morpheme.

This is a highly improbable, if not impossible, explanation. How could it apply to, for instance, numerous cases of paradigms such as *d-a-v-a* 'I am braiding it' vs. past tense *d-o-m-n-a* (← **d-o-v-n-a*), where *d-* 'I' is also a recently added subject prefix, so that the original paradigm must have been **a-v-a* vs. **o-v-n-a*? Why did the uvular consonant disappear in *this* case? Through analogy with complex paradigms like the one for the verb ‘to row’? But if we bring analogy into the discussion, why have all the paradigms suffered the same analogical fate *except* for the verb ‘to kill’?

Furthermore, Vajda does not mention the *structural* difference between **qo* and **o*. In the verb ‘kill’, the marker **qo* occupies the same “floating” slot as the regular past tense markers *l* and *n*, which are regularly placed before the 1st and 2nd p. pronominal object markers, but *after* the 3rd, cf. (past tense markers are in bold, object markers are underlined):

*di-**l**-gu-s* ‘I dressed you’	*t-**qo**-k-ej* ‘I killed you’
*d-o-**l**-s* ‘I dressed him’	*d-a-**q**-ej* ‘I killed him’

This shift of position never affects the “conjugational marker” *o*.

To sum up, Vajda’s internal reconstruction of the Yeniseian opposition **si* : **ɢa* is beset with problems: it does not offer an economic solution, it leaves plenty of unanswered individual questions, it raises doubts of a phonetic-typological nature, and the overall impression is that it was heavily influenced by the corresponding reconstruction of the Na-Dene opposition. Before this reconstruction can be made use of in any DY comparison, it has to be presented in much more detail, and with far more convincing force, within a purely Yeniseian context. And even then, there can hardly be any question of using it as a serious argument in *establishing* a DY link. At best, the scenario extolled by Vajda can be presented as an answer to the question: “How could the TAM markers of Na-Dene and Yeniseian be brought together under a possible historical scenario, provided we have already demonstrated that the families are related?” Consequently, the very fact that these “reconstructions” occupy a prominent position in J. Nichols’ statistical argument in favor of DY weakens said argument quite significantly.

TAM markers: past tense markers.

The second piece of evidence — the actual tense/aspect markers — is much stronger in general and may actually count as real, “non-forced” argumentation. Progressive tense marker **-ł* in Eyak-Athabascan is phonetically and semantically compatible with Yeniseian **l* (or ***r_1, according to S. Starostin’s reconstruction[2]), whereas Athabaskan perfective **ñ* is a possible correspondence for Yeniseian **n*. Vajda’s analysis of the semantic peculiarities of the Yeniseian markers concurs with the conclusions independently arrived at by other Yeniseianists, and is compatible with Na-Dene semantics.

The problem concerning the different slots which these markers occupy in Yeniseian and Na-Dene is explained by Vajda as due to different strategies of grammaticalization: in Na-Dene, the strategy involved joining them as suffixes to the main lexical root, in Yeniseian — to the “auxiliary verbs” reconstructed as **si-* and **ɢa-*. Unfortunately, once again this reverts us to the issue of analytic vs. synthetic nature of DY. It is one thing to propose cognation between two pairs of cognate morphs within a homologous paradigm, and quite another one to propose independent grammaticalization, since this transforms our supposedly “paradigmatic” evidence into one that is decidedly *not* paradigmatic.

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[2] Most of the phonetic and lexical reconstructions for Proto-Yeniseian are quoted according to the comparative phonology of Yeniseian [Starostin 1982] and the etymological dictionary of Yeniseian languages [Starostin 1995].

Nevertheless, the parallels between this binary contrast in Na-Dene and Yeniseian are undeniable and may be accepted as evidence for genetic relationship.

Shape prefixes.

The bulk of this argument (pp. 53–55) revolves around the issue of cognation between the so-called "shape prefixes" *n-*, *d-*, and *h-* in Ket (which, following an alternate tradition, I will be calling "preverbs" for short), and their supposed equivalents in Proto-Athabaskan, reconstructible as **n-*, **d-*, and **qv-*. On the surface, the argument may look convincing: a quasi-paradigmatic homology is found between three prefixes that share comparable phonetics, similar semantics, and the same slot in the verbal paradigm. Thorough analysis, however, shows that on the Yeniseian side at least, the argument runs into the same problem as usual: a selective approach to evidence, allowing to draw generalized conclusions that are not supported by the total weight of the data.

Of the three morphemes discussed, Ket *n-* is the most unusual one. First, it is very rare; in his seminal monograph on the Ket verb [Krejnovich 1968], Ye. Krejnovich, at best, lists a tiny handful of verbs in which it is attested, and that number has not increased significantly since then. Second, it is never found in Kott, let alone met in a paradigm that can be historically associated with a Ket correspondence. Third, the consonant **n*, easily reconstructed for Proto-Yeniseian in the word-medial position, is *never* reconstructed word-initially.

These considerations alone would make any comparison with Na-Dene material highly dubious. But the main problem is centered around semantics: to reconstruct the meaning 'round' for this prefix is to beg the issue. The two examples quoted by Vajda, *n-a-b-hil* 'cuts it around the edges' and *n-a-b-do* 'hews, chisels it (a round object)' may convey the impression that such a reconstruction is obvious, but it is not. The form *n-a-b-hil*, where the root is **kil-*, is not part of a minimal verbal pair, so there is no certain way of knowing whether the meaning 'round' is really conveyed by *n-* or is contained in the root itself.

For the form *nabdo*, minimal pairs do exist, but the form itself is dubious: I have not encountered it in either Krejnovich's, Dulzon's, or my own materials, nor could I locate it in any of H. Werner's three quite extensive vocabularies; neither is it found in [Vajda 2004], a grammatical description of Ket, where such a perfect form should have been adduced as evidence. According to Vajda (p.c.), the form *nabdo* comes from his own field records, and I have no reason to distrust this, but still, a proper reference would be in order here, considering that Ket has been rather extensively studied, with vast corpora of textual evidence, for the past fifty years.

Finally, there are other examples with the preverb *n-*, most of them not mentioned by Vajda, for which the suggested semantics is completely inapplicable. One particularly unsettling example is in the verb 'to give', cf.: *d-a-n-b-o* 'I give it to him'. Vajda mentions this case in a footnote (№ 27, p. 54), but brushes it away, noting that "round-shape *n-* never follows the object marker and is probably a different morpheme". However, *-a-* '(to) him' in this particular case is not a direct object marker; it is an indirect object marker, belonging to a different series, as is clearly proven by such forms as *d-ba-n-b-o* 'he gives it to me', etc. The regular slot occupied by these markers is always before the preverb, not after, so the counterargument does not work, and there is no easy way to prove that *n* in *d-a-n-b-o* is not the same *n* as in *n-a-b-hil*. A handful of other examples may be found both in Krejnovich's monograph and Werner's dictionary that also do not suit the semantics of "roundness" at all. With such flimsy positive evidence, the reconstruction seems to me semantically untenable on internal Yeniseian grounds.

Ket *d-*, on the other hand, is a rather frequent prefix; however, again, there is about as much evidence to suggest the original meaning 'long' as there could be to suggest an original meaning 'wide' or 'high' or 'narrow' or 'low'. The form *d-a-b-do* 'hews, chisels it (a long object, such as a log)', adduced by Vajda, generally means 'cuts it out (as a boat)', if dictionaries are to be believed. This is not a problem: a boat is a long object. But many other verbs with Ket *d-* have nothing to do with long objects: for instance, *d-a-v-tiĺ* 'he warms it' (said of a shaman's tambourine, hardly "long" in shape).

Likewise, for the corresponding Kott marker *d́-* Vajda only quotes the form *d́ati* 'subject hits with long object, such as a whip', but what about such paradigms as *d́-ājaŋ* 'to expel', past tense *d́-ōnajaŋ*, or *d́-ašiaŋ* 'to dress up', past tense *d́alašiaŋ*, etc.? How is it possible to boldly draw the proto-semantics of a clearly desemanticized morpheme, when the counterexamples for our hypothesis outnumber the examples?

The situation with Ket *h-* = Yugh, Kott *f-* is equally unsatisfactory. The equation of this prefix with the idea of 'flat surface' is highly subjective, and I cannot refrain from pointing out that in [Vajda 2004: 62], this exact morpheme was defined as follows: "probably derives from a classifier of straight or long objects" — whereas "superficial contact with a surface" was actually a meaning associated with an entirely different preverb *t-*! Clearly, this is a situation in which multiple interpretations are possible, but not a single one will be highly convincing.

Consequently, I insist that the "spatial prefixes" comparison should be abandoned in its entirety. The semantic treatment of Yeniseian preverbs is forced and seems to have been heavily influenced by the corresponding meanings of the compared prefixes in Na-Dene. This does not necessarily invalidate the homologies (as long as we are unable to precisely define the functions of Yeniseian preverbs, Vajda's treatment of their semantics is as good as anybody's), but it makes them irrelevant as first-order evidence for demonstrating the common origins of DY morphology.

One final point is necessary. The "spatial prefixes" *n-*, **ǯ-* (→ Ket *d-*), **p-* (→ Ket *h-*) = Athabaskan **n-*, **d-*, **qv-* play a significant part in J. Nichols' statistical test, where, among other things, the following is mentioned: "I gather these exhaust their paradigm, i. e. there is no search among a larger set of forms" (p. 305). This is an incorrect assumption: not only are these three Yeniseian preverbs only a part of a much larger subset, which also involves such morphemes as *k-*, *t-*, and *q-* occupying the same slot, but at least two of them, *n-* and **p-*, happen to be very rare, compared to the ultra-frequent *k-* and *t-*, for which no cognates have been discovered in Na-Dene. Clearly, even if we accept Vajda's highly dubious semantic reconstruction, this circumstance has to be reflected in the application of the statistic algorithm.

Pronouns.

The pronominal evidence for Dene-Yeniseian, contrasted with pronominal evidence on a much larger, "Dene-Caucasian" scale, has already been discussed in brief in my previous publication on the subject [G. Starostin 2010a], where it was shown that the paradigmatic connections of Yeniseian 1st and 2nd p. pronouns and pronominal markers are much easier to establish with the "Western" area of this macrofamily (Burushaski and North Caucasian) than with its "Eastern" part (Sino-Tibetan and Na-Dene).

If we restrict ourselves to a narrow investigation of the Dene-Yeniseian connection and nothing else, the only plausible isomorphism between the pronominal systems that emerges "on its own" is the parallel between Yeniseian **ˀaw* 'thou' and Tlingit *wa-* in *waˀé* 'thou', but, remarkably, it is dismissed by Vajda as a "chance resemblance" (p. 50). What remains is a long, complex, and not highly probable scenario based on a series of internal assumptions which I will not analyze in any details, since even Vajda himself is ultimately forced to admit that "Dene-Yeniseian differs from established families... in the relative inscrutability of its pronominal morphology... In fact, understanding Yeniseian pronoun morphology from a historical perspective may require perspectives gained from an already well-demonstrated external genetic connection, rather than pronominal forms helping to demonstrate the connection beforehand" (p. 53).

To which I would add that this is one of the more transparent areas where it really helps to view Yeniseian languages in a broader "Dene-Caucasian" context; in particular, some of the homologies that can be easily and without too much speculation be established between Yeniseian, Burushaski, and North Caucasian pronominal systems go directly against some of the hypotheses suggested by Vajda in the "pronominal" section of his paper (see [G. Starostin 2010a] for more details).

Conclusion.

For space reasons, I omit specific comments on two other subsections of Vajda's paper that deal with verbal morphology ("Classifiers" and "Action nominal derivation"). The parallels discussed on those pages are not dealt with by J. Nichols in her statistical tests, have no "paradigmatic" value on their own, and suffer from the same problem: inconclusiveness of the evidence, which usually has to go through the filter of internal reconstruction, based on subjective assumptions.

All said, I find it impossible to believe that the basic structure of the verbal form in Yeniseian and Na-Dene could have been inherited from a common ancestor. Two of the most important counterarguments are (a) the fate of Na-Dene "transitivity indicators", brought up by A. Kibrik and (b) the puzzling difference in the relative position of the perfective/progressive markers — essentially the *only* piece of verbal evidence that can boast immediate credibility, but only on a "segmental" level, never on a morphosyntactic one. This "migration" of the compared morphemes within the form is never explained by Vajda, and I do not think it can be explained through any reasonable historical scenario.

If a "Dene-Yeniseian" ever existed, there is no need to insist that it must have been morphologically simple. Complex morphological patterns do not generally tend to be stable over periods of several millennia, and it is possible that either the Yeniseian system, or the Na-Dene system, or both, could have undergone the process of erosion of the original patterns and rebuilding of new ones in the meantime. (Even such closely related languages as Ket and Kott show significantly different patterns of affixation that turn the reconstruction of the original verbal morphology into a serious chore). This could, in particular, explain the typological similarities between the families.

However, attempts to use the evidence from verbal morphology as "first-order" evidence, i. e. the principal argument in favor of Dene-Yeniseian as a historic reality, cannot be called successful. Since evidence provided by morphological paradigms is frequently (but not universally) regarded as "definitive proof" of genetic relationship, I can understand Vajda's thoroughness in presenting his argument. But let us not forget that, whatever be the case, we are at best dealing with a "macro-level" relationship here, with Dene-Yeniseian going much deeper than Indo-European (I will return to the dating issue later). Common verbal morphology of such tremendous complexity at such a deep level goes beyond "amazing": it is, as far as my entire experience suggests, impossible. Those few isomorphisms that can be salvaged from

Vajda's verbal morphology evidence, such as the tense markers, should rather be regarded as relics of old auxiliary verbs or adverbs that have undergone independent grammaticalization in both families. The rest should be shelved until further progress is made in other areas.

Lexical and phonological evidence for "Dene-Yeniseian"

The entire second half of Vajda's paper is dedicated to the issue of regular phonetic correspondences between Proto-Yeniseian and Proto-Na-Dene, which are established on the basis of around one hundred common etymologies — a number that J. Nichols considers sufficient for exceeding chance expectations when the compared lexical corpora on both sides do not exceed 1000 units.

A detailed analysis of each of these etymologies would take up an enormous amount of space, and would probably be superfluous for our current purposes. Vajda strongly emphasizes the fact that, in order to be convincing, lexical parallels between the compared families must fit inside the patterns of regular phonetic correspondences, rather than simply display different degrees of phonetic similarity, as well as share semantically identical or close meanings. The first of these "filters", in particular, makes his work more methodologically sound than the parallels assembled in [Ruhlen 1998].

That said, although I find the lexical part of his argument far more efficient for the purposes of demonstrating a genetic link, there are some serious problems with it as well — problems that, at worst, could make Dene-Yeniseian lose credibility *in toto*, or, at best, shatter the idea of a "lowest common ancestor" for these language families (i. e. force us to turn our attention away from "Dene-Yeniseian" and look for much closer relatives to Yeniseian within Eurasia). These are as follows:

1. A suspiciously low count of reliable direct lexical matches in the basic lexicon. Extensive testing has clearly shown that no hypothesis of genetic relationship between two languages, historically attested or reconstructed, can pretend to historic reality without a statistically significant proportion of direct matches on the Swadesh list, and there are serious doubts as to whether Vajda's lexical evidence, especially when it is subjected to careful scrutiny, satisfies that demand.
2. Some of the presented correspondences strongly disagree with the usual typology of phonetic change, ranging from typologically rare to typologically unique, and it is not clear that the supporting evidence is robust enough to justify setting up such "odd" phonetic developments from "Dene-Yeniseian" to the daughter languages.

For our critical analysis, it will be sufficient to concentrate on these two issues, because the scarcity of lexicostatistical matches by itself suggests that at least a certain share of semantically and distributionally weaker etymologies may really be chance resemblances; and the unusual oddness of certain correspondences strongly indicates that some of them could have been set up only to justify one "impressive" look-alike. These statements will be further clarified below.

Evaluating Dene-Yeniseian lexicostatistical matches.

The question of how many direct lexicostatistical matches (i. e. words with the exact same "Swadesh meaning" going back to a common ancestral word and linked through regular phonetic correspondences) one should discover between two languages or reconstructed proto-languages in order to confirm their genetic relationship beyond reasonable doubt, remains open. If we tentatively set the age of the hypothesized "Dene-Yeniseian" at around the same time period as Indo-European, a reasonable number would be something like 25–30% exact matches (cf. a comparable number between Hittite and Old Indian, whose relative dates of attestation are not far removed from the glottochronological and "intuitive" datings of the reconstructed Proto-Yeniseian and Proto-Athabaskan, although Proto-Na-Dene itself seems to be much older). This number is unattainable in Vajda's lexical evidence even if all of it is accepted unequivocally: not a problem if the real age of "Dene-Yeniseian" is much older than, say, six thousand years, but one must also keep in mind that, the older the age of the genetic connection, the more difficult it becomes to distinguish between data that are statistically relevant and those that can hardly be distinguished from chance. Something like a figure of "5% matches" would be useless.

Below, one by one, I shall consider all of Vajda's etymologies that satisfy the following conditions: (a) follow the proposed system of correspondences on both the Yeniseian and the Na-Dene sides; (b) are reconstructible with the semantics of a "Swadesh meaning" on the Proto-Yeniseian level; (c) are reconstructible with the semantics of a "Swadesh meaning" on the Proto-Na-Dene level, *or* on the Proto-Athabaskan level, *or* at least have this meaning in either Eyak or Tlingit (keeping in mind that the overall number of reliable Tlingit-Eyak-Athabaskan cognates is not very large, and that meticulous semantic reconstruction on the Proto-Na-Dene level is only possible in exceptional cases).

If the word is only reconstructible in a "Swadesh meaning" on one side of the equation (Yeniseian or Tlingit/Eyak/Athabaskan), the comparison does not constitute a proper lexicostatistical match, being weakened by semantic inexactness. However, if the difference in semantics is "trivial", that is, follows a typologically common, well-attested semantic development (e. g. 'see' → 'eye', 'black' → 'night', etc.), I will include the comparison in a separate group.

Finally, although the evaluation focuses first and foremost on the validity of "Dene-Yeniseian", I find it useful to occasionally list potential external cognates to Yeniseian or Na-Dene etyma in other branches of the larger "Dene-Caucasian" macrofamily (most importantly, Burushaski and Sino-Tibetan), particularly in those cases where they seem less "forced" than Vajda's DY etymologies.

1. '**liver**': PY **seŋ* (Ket *sēŋ*) — PEA **=sənt'* (p. 66).

Acceptable. This example illustrates Vajda's rule of nasal coda simplification in Yeniseian and also agrees with his main prosodic correlation: glottalic coda in ND = high level tone in Yeniseian. It should, however, be noted that the particular correspondence "PY **-ŋ* : PEA **-nt'*" is unique, and Vajda avoids setting up a DY reconstruction — should it be **sVŋt'*, with assimilative fronting of the nasal in EA, or something else?

This does not invalidate the comparison, but it is still a question waiting to be answered. In the meantime, both words are also comparable to Sino-Tibetan **sin* 'liver'[3], where the quality of the nasal is closer to the EA equivalent — a hint that, if all three families are ultimately related, it is perhaps the ST and ND forms that share a "lowest common ancestor", not the ND and Yeniseian ones. (Vajda mentions the comparison on p. 114, but does not draw attention to the nasal consonants).

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[3] All Sino-Tibetan reconstructions are drawn from [Peiros & Starostin 1996].

2. ‘**head**’: PY **ciʔɢ-* (S. Starostin) ~ **čiʔɢ-* (Vajda) — ND **kʸeŋʼ ~ *kʸiŋʼ* (PA **=tsiʼ*) (pp. 66, 83).

Dubious. This is a complicated case that raises several problems at once. First, I must voice a general concern about the phonetic reconstruction on the Na-Dene side of things. Vajda devotedly sticks to J. Leer's reconstruction of a set of *five* back consonantal series for Proto-ND: palatal **kʸ*, velar **k*, uvular **q*, labialized velar **kʷ*, labialized uvular **qʷ* (see the table of correspondences on p. 170 in Leer's paper). The principal "novelty" in this system is the palatal series, based on such correspondences as:

Na-Dene	PAE	PA	Eyak	Tlingit
**kʸ*	**ts*	**ts*	*ts*	*k* (or *sh*)
**gʸ*	**dz*	**dz*	*dz*	*g*

i. e. the phonemes in question shift to plain velar articulation in Tlingit, but become front affricates in PAE through palatalization.

The reconstructed system does not seem too realistic from a typological point of view: languages that show a strict phonological opposition between *kʸ*, *k*, and *q* are extremely scarce. It may be more productive, after all, to regard this special "palatal" series as having more in common with the affricate / sibilant series than the "back" series, i. e. reconstruct **tsʸ*, **tsʸʼ*, **dzʸ*, **sʸ* with subsequent velarization in Tlingit than **kʸ*, *kʸʼ*, **gʸ*, **xʸ* with subsequent affricativization in PAE.

Although this is essentially just a question of phonetic interpretation and it need not have any direct bearing on the proposed system of correspondences, in this particular situation, reinterpretation of ND **kʸeŋʼ ~ *kʸiŋʼ* as **tsʸeŋʼ ~ *tsʸiŋʼ* would actually help the comparison, bringing the Yeniseian and ND forms phonetically closer to each other without the non-economic necessity of postulating independent affricativization on both sides of the Bering Strait.

There are, however, additional, more serious problems with the comparison. Reconstruction of the final nasal in ND is far from certain, since it is extracted only from certain morphophonological variants (Eyak *tsĩʼ-de* 'neck', etc.); but evidence for a former nasal in the Yeniseian form is utterly lacking. Vajda's attempt, following H. Werner, to postulate a common etymological background for **ciʔɢ-* (**čiʔɢ-*) 'head' and **cəŋe* 'hair', deriving the latter from **cəŋ-* + 'fur' (a morpheme that is actually reconstructible as **χäʔʒ* [Starostin 1995: 300] and does not mean 'fur' as much as it means 'overcoat', which, admittedly, is mostly made of fur in a Siberian background), runs into too many problems at once to be qualified as anything other than a folk etymology.

If the ND word is truly to be reconstructed as **tsʸeŋʼ* 'head', I would rather be inclined to compare it with such a ST parallel as **tsʰaːŋ* 'high' → Jingpo *n=saŋ* 'great, noble, exalted', Lushai *saːŋ* 'high, lofty', Garo *tsaːŋ* 'high', Rawang *tsʸaːŋ* 'up' [Starostin & Peiros 1996: IV, 19–20], and the same word may have independently shifted to the meaning 'head' in Konyak: *saŋ ~ šaŋ*.

Still, for objectivity's sake, we should tentatively keep the comparison for now, as there is a remote possibility that both codas could eventually stem from another cluster, i. e. the DY reconstruction could look something like **tsʸeɴɢ* with cluster simplification in both branches.

3. '**earth**': PY **baʔŋ* — PA **ñə̂nʼ* (p. 71).

Implausible. This etymology is mentioned only "in passing", with the following note: "plausibly cognate if from earlier **m-yə̂n*". The nature of the hyphen is unclear (is *m-* a prefix?), and in any case, the initial correspondence is not corroborated by additional examples (in Vajda's version, Yeniseian **b-* is supposed to correspond to ND *ɢ-).

4. '**stone**': PY **čɨʔ-s* — ND **kʸay* (PA **tse:*) (p. 72).

Acceptable. This is probably the strongest etymology in the whole bunch, since it does not violate any consonantal correspondences, is fairly well reconstructible on deep levels in both families, and belongs to a generally stable lexical layer. The ND reconstruction should, perhaps, be amended to **tsʸay* (see notes on 'head' above); *-*s* in PY is most likely a fossilized singulative marker (cf. the plural form **čəʔ-ŋ*).

It should be noted that a very close semantic match can also be found in Burushaski: Yasin *tsʸiṣ*, Hunza, Nagar *tsʸʰiṣ* 'mountain' (with the same fossilized marker as in Yeniseian?).

5. '**foot**': PY **giʔs* — PAE **qe'* (p. 72).

Rejected first and foremost because this is not a proper lexicostatistical match: the proper PY word for 'foot' must have been **bul*, which has this meaning in Ket, Yugh, Kott, and Arin, whereas **giʔs* is primarily a Ket/Yugh isogloss with the meaning 'leg'. This does not exclude an etymological connection, but there is an additional phonetic problem: this time, PY *-s* is clearly part of the root rather than a fossilized suffix — cf. the paradigm in Ket: sg. *kiʔs*, pl. *kis-eŋ* — yet there are no traces of a sibilant in PAE, which does not correlate with any recurrent pattern in Vajda's system.

6. '**stand**': PY **ʔipɨn* (Ket *i:n*, Yugh *ifɨn*) — PA **he:n* (p. 76).

Implausible, because the correspondence "PY **p* : PA **h*" is clearly irregular: on p. 89, it is stated that "comparison with Yeniseian strongly suggests that **b* and **p* merged with labialized velars and uvulars in Pre-Proto-Na-Dene", and **h* is clearly not a labialized velar or uvular. On p. 74, it seems to be suggested that **p* in the Yeniseian verb is a fossilized "thematic prefix", but such segmentation is quite arbitrary (one could just as well claim, instead, that *-n* is a fossilized suffix, and reconstruct the original root as **ʔip-*).

7. '**belly**': PY **pəj* (Ket *hɨ:j*) — PA **wə̂t'* (p. 76).

Dubious, because the consonantal codas do not constitute a regular match and are quite distant phonetically. Coronal consonants in Yeniseian do not "lenite" that easy, so, unless it can be demonstrated that **-t'* in PA is a fossilized suffixal extension, the entire comparison rests upon the word-initial consonants. In that case, one might just as well drag Sino-Tibetan **puk* 'belly' into the comparison.

8. '**many**': PY **ʔo:n* (Ket *ɔʔn*) — PA **=la:ñ* (p. 76).

Acceptable, although the note that "*l* is fused classifier" in PA needs clarification: it is not evident on what basis the segmentation is performed (all of the Athabaskan reflexes feature an initial lateral consonant in this root). S. Starostin's comparison of the Yeniseian form with Burushaski *=jó:n* 'all', although it does not constitute a lexicostatistical match (but features a trivial semantic shift), seems more plausible on phonetic grounds.

9. '**blood**': Ket *del* — PAE **dəɬ* (p. 80).

Rejected as a statistical match, since Ket *del* is by no means the basic Yeniseian word for 'blood' (which is reconstructed as **sur* and attested in all major languages); the analysis of the compound expression *del-es* as 'blood-sky', the "malevolent God of the West" is not be based on any explicit philological argumentation and seems arbitrary. Theoretically, an etymological connection could be possible, but the parallel may not be used as "first-order" evidence.

10. ‘**water**’: Ket *tu ~ to* ‘water, moisture’ (in cpds.) — PA **tu:* (p. 81).

Rejected as a statistical match, since the main Yeniseian word for water is **xur* (Ket *u:l*); Vajda actually lists three different Yeniseian morphemes as potential cognates, with no clear preference. Etymologically, the comparison is possible, but it should be noted that PA **tu:* finds a near-perfect correlate in Sino-Tibetan **tuj*(*H*) ‘water’ (as a direct lexicostatistical match, since distribution in daughter languages shows that **tuj*(*H*) is the best candidate for the basic meaning ‘water’ in PST).

11. ‘**lie**’: PY **te-*(*n*) — PAE **te:* (p. 81).

Acceptable; a straightforward match without any phonetic, semantic, or distributional problems.

12. ‘**dry**’: PY **qV*[ɢ]*i-* — PND **kʸo*()*x* (p. 84).
13. ‘**ashes**’: Ket *qolan* — PND **kʸi*()*tɬ’* (p. 84).

These two examples are tackled together because they share a common problem: namely, I hold the gravest doubts about the correctness of the word-initial correspondence. If the Na-Dene reconstructions are reinterpreted as containing palatal affricates (i. e. **tsʸo*()*x*, **tsʸi*()*tɬ’*), the discrepancy becomes all the more obvious: but even if they *were* actual “palatals”, a shift to uvular rather than simple velar articulation in Proto-Yeniseian, even “before back vowels”, as Vajda’s rule stipulates, is highly unlikely.

Furthermore, the relative antiquity of uvular articulation at least in the Yeniseian form for ‘dry’ is confirmed by an impressive match in Burushaski: *qaq-* ‘dry’, also ‘hungry’ (note that in Yeniseian, the word for ‘hunger’ is derived from the same root as well: it is reconstructed by S. Starostin as **qɔq-ante*).

Out of three other examples supposed to confirm this correspondence, only PY **tiχ* ‘snow’ vs. PND **t’ikʸ’* (**t’itsʸ’* ?) ‘ice’ is sufficiently impressive. Perhaps this etymology can somehow be salvaged by suggesting a dissimilative or assimilative process in the coda either in PY or in PND; in any case, the phonetic context here is entirely different from the one in examples 12 and 13 (word-final after a front vowel).

Considering also that Ket *qolan* ‘ashes’ is not easily reconstructible for PY, both of these examples are, at best, highly dubious, and, at worst, implausible.

14. ‘**sun**’: PY **xiɢa* — PND **xʷa:* (PA **ša:*) (p. 87).

Implausible. There are simply too many phonetic problems with this comparison. First, within Na-Dene the word is attested only in Athabaskan, so there is no knowing if **ša:* really goes back to an earlier **xʷa:* or not (diagnostic Eyak and Tlingit parallels that should have retained velar articulation are not attested). Second, the special rule “DY **xʷi-* → PY **xi-*” is set up on the sole basis of this example (in other cases, DY **xʷ-* → PY **s-*). Third, the word-medial back consonant, well preserved in Kott *e:ga* and Ket. pl. *iɣa-n*, is left unaccounted for in PND (Vajda transcribes the Kott form as *e-ga*, possibly implying morphological segmentation into **xi-ɢa*, but this is impossible — *-ɢa* is not a productive suffix, and, furthermore, the word **xiɢa* is clearly a morphological extension of PY **xiˀɢ* ‘day’, so the segmentation should really be **xiɢ-a*). The comparison is clearly forced.

15. ‘**name**’: PY **ʔiɢ* (Ket *i:*, Kott *ix*) — PND **=u:=sxe*(*w*) (PA **=u=že:*) (p. 88).

Implausible. Even if the word-initial correspondence were correct (which is impossible to prove, since it seems to be unique), Vajda once again leaves open the issue of the root-final back consonant that has no parallels in ND. At the same time, the Yeniseian word has a fantastic parallel in Burushaski, with even the grammatical paradigms matching: cf. Kott *ix*, pl. *i:k-ŋ* vs. Hunza, Nagar *=ík*, pl. *=itś-iŋ* (palatalized from earlier **=ik-iŋ*). In the light of this connection, the ND comparison looks even weaker than it already is.

16. '**dog**': PY **čip* — PA **ɬən̄ ~ *ɬi:n̄-kʸ'e:* (p. 92).

Implausible. Even if the word-initial consonant correspondence is correct (which is dubious, since the discussed set concerns PND **tɬ* rather than **ɬ*), the word-final consonants clearly do not match. Vajda notes that "cognate status of this set hinges on the possibility that final PY **b* is homologous with the unstable guttural in Athabaskan", but even if the unstable guttural was originally part of the root (which is not obvious), there is no explicit evidence for it ever having been a *labialized* guttural. Overall, there are too many phonetic problems with the "consonantal skeleton" of this word for the etymology to be credible.

Additionally, Vajda implies that the unclear "extra" syllable *al- ~ il-,* found in this word in certain southern Yeniseian languages (Kott *al-šip*, Arin *il-čap*, etc.), also reflects the presence of a former lateral affricate, i. e. that word-initial sequences *ilt- ~ alt- ~ ilč- ~ als-* have all developed from original **tɬ-*. The idea is elegant but, unfortunately, quite untenable, since the "presyllable" *al- ~ il-* may just as well be found before back consonants in these languages — the most famous example being the word for 'star': Ket *qɔˀ ~ qɔʁ*, Yugh *xɔ:x*, but Kott *al-aga*, Arin *il-koj* ← PY **qɔ:qa*. This and similar cases clearly show that *al- / il-* is a specifically Kott/Arin morphological element, a prefix of unclear origin, and any attempts to trace it back to an original lateral affricate are futile.

17. '**fish**': PY **ci:k* 'fish / snake' — PA **ɬuq'e:* 'fish / salmon' (p. 93).

Dubious. First of all, Vajda's equation of Ket *tuɣ* 'tugun, a species of fish' with Kott *te:g* 'fish' is not as convincing as S. Starostin's earlier comparison of Kott *te:g* with Ket *tiɣ* 'snake' because the vocalic correspondence "Ket *u* : Kott *e*" does not exist ("Ket *i* : Kott *e*" is also not entirely regular, but there are at least a couple other examples, and in any case, the phonetic distance between *i* and *e* is much shorter). The need to demolish the earlier etymology is triggered by Vajda's desire to compare Ket *tiɣ* instead with Tlingit **tɬ'ik'ʷx* 'worm' and get two Dene-Yeniseian cognate pairs instead of one.

Of course, this can still be done by comparing PA **ɬuq'e:* 'fish' with Ket *tuɣ* 'tugun', and Tlingit **tɬ'ik'ʷx* 'worm' with Proto-Yeniseian **ci:k* 'fish, snake, worm'. From a phonetic point of view, this is probably the best solution; however, it destroys the lexicostatistical matching.

NB: concerning Vajda's hypothesis that "both Yeniseian and Na-Dene words for 'snake' and 'fish'" may be "ultimately related to a root **tVl* referring generically to animals that crawl, slither, or move from side to side" (ibid.), I think that the situation requires a more thorough investigation in order to formulate a precise scenario, but, to add to the general picture, I cannot help but mention the Burushaski word for 'snake' — Yasin *tul*, Hunza/Nagar *tol*, which also fits perfectly in this paradigm (in Yeniseian, the most phonetically direct parallel would be Ket *tuln* 'lizard').

Altogether, there are between 7 and 9 matches on the Swadesh list that are not definitively rejected for various reasons (i. e. in the "acceptable" and "dubious" categories). To these we may, perhaps, add two more matches for the 1st and 2nd p. pronouns ('I', 'thou'), but *only* on the conditions that: [a] the ND 1st p. pronoun is to be reconstructed with a sibilant (phonetically close to PA **ši:*, which would bring it closer to PY **ˀaʒ*), [b] PY **ˀaw* 'thou' is actually cognate with Tlingit *waˀé*, although this is viewed by Vajda himself as a chance resemblance. As I already mentioned above, the pronominal links between Yeniseian, Burushaski and North Caucasian seem much more robust than between this family and Na-Dene.

At best, this gives us 11 matches, 4 of them viewed as acceptable ('liver', 'stone', 'many', 'lie') and 7 — as dubious to various degrees ('belly', 'head', 'dry', 'ashes', 'fish', 'I', 'thou'). Furthermore, it must be stressed that the criteria of "acceptance" were relatively lenient: for instance, I agree that detailed vowel correspondences, at this level of research, are an unaffordable "luxury", and that certain assumptions on internal segmentation of the morphemes may be made without direct proof (e. g. 'belly'). It is also not yet obviously evident that all the 11 comparanda on the ND side are optimal candidates for the "Swadesh meaning" on the Proto-ND level.

A figure of 11% matches on the Swadesh list between two reconstructed proto-lists is, to put it mildly, not very encouraging. Put together with what I regard as a general failure to demonstrate the common origin of the basic verbal morphology of these languages, it should lay to rest any idea of a "Dene-Yeniseian" family comparable in time depth to Indo-European, because not even any two *modern* Indo-European languages, let alone ancient ones or the reconstructed Proto-Germanic, Proto-Celtic, etc., fall as low as 11% common matches.

I am not able to say if this number, in this particular context, exceeds or does not go beyond what should be expected by random chance. Normally, random phonetic similarities on Swadesh lists that have been transcriptionally unified and automatically analyzed within the framework of the "Global Lexicostatistical Project" database cluster around a figure of 5–8%, so that 11% may be statistically relevant, after all. Granted, these particular matches at least claim to be rooted in regular phonetic correspondences, some evidence for the regularity of which is presented in Vajda's paper.

One thing, however, that presents an additional serious bother is that, of the 4 fully acceptable matches, only 'stone' belongs to the most stable 50-item half of the Swadesh list, and the total ratio is "6 items from the first half ('stone', 'head', 'dry', 'ashes', 'I', 'thou') : 5 items from the second half ('liver', 'many', 'lie', 'belly', 'fish')" — not a very credible proportion, since non-stable elements are usually expected to drop out at higher rates than stable ones (see [G. Starostin 2010b] for details of this gradation). For comparison, the number of phonetically convincing and semantically exact matches on the 50-item "stable" half of the Swadesh wordlist alone between Proto-Yeniseian and Burushaski amounts to 9 units ('I', 'thou', 'dry', 'eat', 'egg', 'eye', 'hand', 'leaf', 'name'; thorough calculations for the second half of the list have not been performed as of yet).

At best, the presented figures may be interpreted as reflecting an impressive chronological gap between DY and its immediate daughters (Proto-Na-Dene and Proto-Yeniseian, not modern languages) — a gap that could easily exceed 6–8 millennia, which could throw DY as far back as the tenth or twelfth millennium B.C. To a hardened skepticist, this would be the end of the hypothesis; to those who are more benevolently inclined towards research on "macrofamilies", this would simply confirm that "Dene-Yeniseian", in all likelihood, is a historical non-reality, and that one needs to focus on finding *closer* relatives to both families (such as Sino-Tibetan for Na-Dene and Burushaski for Yeniseian) in order to confirm the fact of their ultimately being related on a higher level.

Some notes on the typology of proposed phonetic changes.

One last concern needs to be voiced in conjunction with the regular phonetic correspondences that Vajda claims to have been able to establish between ND and Yeniseian. It is regrettable that the paper, despite its overall length, allocates no space to a concise, summarizing table, despite the fact that almost all of the well-represented and reliably reconstructible consonantal segments of Proto-Na-Dene and Proto-Yeniseian are ultimately aligned with each other, one way or another.

Some of the elegant solutions presented along the way — such as the suggested explanation of Yeniseian tonogenesis from the influence of formerly glottalic or simple consonantal codas — are indeed worthy of attention, and have justifiedly impressed specialists. However, they have also taken attention *away* from certain areas where the correspondences are far more speculative, and sometimes violate typological standards without sufficient justification.

One major problem, in particular, concerns the Yeniseian correspondences for the ND "palatal" series (*g^y, *k^y, *k^{y}', *x^y, which, as I already stated earlier, could perhaps better be regarded as an affricate/sibilant series *dz^y, *ts^y, *ts^{y}', *s^y). These are presented as follows (pp. 83–84):

Na-Dene	Proto-Yeniseian
*g^y	*ǯ
*k^y, *k^{y}'	*č (before original front vowels) *q (before original back vowels)
*x^y	*s

This series contains a significant violation of the principle of *systematicity*: standard typology of phonetic change dictates that the most common type of change is "feature-change", not "phoneme-change", and the expected change for an obstruent consonantal series sharing a single feature usually consists of a mutation of that feature (e. g. "voiced stops" → "voiceless stops", "aspirated stops" → "fricatives", "velarized stops / fricatives" → "palatal stops / fricatives", etc.). Three out of four proposed correspondences follow a single, typologically plausible pattern, namely, a process of fronting and affricativization of the palatal series, in which two of the developments are almost predictable if we know a third one (if *g^y → *ǯ, it is highly likely that *k^y → *č and *x^y → *š; since Proto-Yeniseian lacks a separate *š, an additional merger of *š with *s is not out of the question). As a matter of fact, they also indirectly support the reinterpretation of Na-Dene *g^y, *k^y, *k^{y}', *x^y as *dz^y, *ts^y, *ts^{y}', *s^y (such a solution would be more economic).

However, the fourth correspondence — a completely unforeseen and hard to explain split of *k^y depending on vowel quality — is utterly confusing. If, *before* losing its "back" qualities, original *k^y actually underwent back vowel influence and switched to the uvular series (apparently, "skipping" simple velar articulation), why did not its voiced stop and fricative counterparts, *g^y and *x^y, undergo the same procedure, and develop into *ɢ and *χ accordingly in the same contexts?

I find no explanation for this mystery whatsoever, other than the desire to accommodate a few comparanda that look impressive on paper (see above for notes on 'dry', 'ashes', and 'snow'), but are hardly robust and numerous enough to warrant such a jarring typological inconsistency. Of course, exceptions from "typologically common" situations do happen, and sometimes the abundance of comparative data easily forces us to admit them. But the data presented to support the *k^y → *q shift could hardly be called "abundant", and I would think twice before admitting this correspondence as positive evidence.

Even worse is the situation with Yeniseian counterparts to the Na-Dene labialized velar series:

Na-Dene	Proto-Yeniseian
*gʷ	*ǯ (before front vowels) *g (before back vowels)
*kʷʼ	*t (before original voiceless fricative auslaut) *d (elsewhere)
*xʷ	*x (before *-i-) *s (elsewhere)

This looks seriously messy. Again, we see conditioned split of reflexation, which is good; what is *not* good is that the conditions are different for each of the three members of the same series, and, furthermore, the outcomes of the splitting are even *more* different — the voiced phoneme either retains its velar or quality or undergoes affricativization, while the voiceless phoneme, for no clear reason, becomes dental. How did this happen?

The most probable answer is that it happened in order to accommodate two strikingly delicious look-alikes: Tlingit **tšʼáːɬʼ* ‘willow’ (= PA **tšʳʼə̂tɬʼ* ‘shrub, plant’, a correspondence that points to original **kʷʼ*) = Ket *dɤ̄l* ‘willow’ (← PY **dəĺi*), and PA **tšʳʼiːxʸ* ‘canoe’ = Ket *tiː*, Pumpokol *tɨg* id. The second comparison in particular produces a “grand” impression on people (cf. article titles such as “*Words for ‘Canoe’ point to long-lost family ties*”, reprinted by several Canadian media sources in 2010). Since J. Leer traces the word-initial phonemes in both words to **kʷʼ*, it becomes necessary to explain why they “correspond” to different segments in Yeniseian, and the current explanation is assimilative influence of a voiceless fricative.

This is already uncomfortable, but the ultimate irony of the situation is in that both Yeniseian forms actually have rather transparent internal etymologies. PY **dəĺi* ‘willow’, as Vajda mentions himself, may be explained as a borrowing from Turkic (cf. Proto-Turkic **dal* ‘willow’); Vajda himself (p.c.) considers the borrowing hypothesis inconclusive, but there definitely *are* examples of Turkic borrowings into Proto-Yeniseian, and the forms bear a far more striking resemblance to each other than to the ND counterpart — at the very least, this is not an item that could serve as “first-order evidence” for the correspondence in question (a side issue is whether the semantics of Tlingit ‘willow’ and PA ‘shrub’ are close enough to merit being joined in a single etymology, but this is ultimately irrelevant to the DY connection).

As for the (in)famous ‘canoe’, there are multiple signs in Yeniseian suggesting that the meaning ‘boat’ for this word is secondary. In Pumpokol, the form *tɨg* is glossed as both ‘boat’ and ‘vessel’. In Kott and Arin we also see the compound formation *ul-tej* (Kott), *kul-tej* (Arin) ‘vessel’, reflecting an original **xur-tVχV* ‘water + vessel’. It is hard to imagine the word ‘water’ added to the original word for ‘boat’ and modifying it to ‘vessel’ (!). Much more probable is the scenario, according to which the original meaning of the word was simply ‘vessel’, later broadened to include ‘boat (= delved vessel)’, after which the word ‘water’ was added in some dialects to the original word for ‘vessel’ in order to distinguish between the two meanings. Finally, there is little reason to doubt that the same root is found in Ket/Yugh *tīj* ‘to scoop water’, which, again, fits in much better with ‘vessel’ than ‘boat’.

Thus, even if the Yeniseian and ND roots do belong together from an etymological perspective, it can hardly be supposed that their modern semantics reflects a common DY idea of a ‘boat’. At best, the word could have meant ‘vessel’ (‘birchbark vessel’?) in the proto-language, with the semantics of ‘boat, canoe’ a later independent development in both daughter branches. But the non-systemic character of the observed “correspondence” makes me suspect that the word is really just a look-alike (and not even a particularly striking look-alike at that).

Conclusion

The examples presented in the previous section are meant to illustrate a major, if perhaps somewhat controversial, point: Vajda's "regular correspondences" are not, or, at least, not *yet* properly "regular" in the classic comparative-historical sense of the word. Most of them seem to be based around one "psychologically impressive" example, which is then backed up by 1–2 supporting comparanda that are usually weaker from either the semantic, or the phonetic, or the distributional points of view, but still manage to produce an aura of "regularity". The same could be extended to his treatment of the verbal morphology, where a tiny handful of intriguing isomorphisms are surrounded by an impenetrable sea of assumptions and highly controversial internal reconstructions that create an illusion of systemic reconstruction where there really is none.

Yet none of this should be blamed exclusively on Vajda, whose sincere dedication to the issue of clarifying the historical relations of Yeniseian languages cannot be doubted. To a large extent, he is simply attempting to strictly follow the "rules of the game" that have been set out for proving "long range relationship" by mainstream specialists in comparative linguists who hold everything and everyone to the "Indo-European standard". According to these criteria, no theory of genetic relationship will ever gain acceptance unless it is supported by paradigmatic morphological evidence (hence the verve applied to the reconstruction of the "Dene-Yeniseian verbal template") and a corpus of lexical parallels where all the segments are mechanistically correlated with each other in full accordance with the ideal Neogrammarian model (cf. the idea of *total accountability*, raised in Eric Hamp's paper).

In its ardent attempt to satisfy everyone and everything, "A Siberian Link with Na-Dene Languages" may have "officially" succeeded in the short run, but, I am afraid, will eventually prove to be a disappointment in many respects for those who have prematurely embraced all of its conclusions. The most troublesome aspect of it is that the prehistorical picture that it paints is not realistic. It presents "Dene-Yeniseian" as a language whose descendants on both sides of the Pacific have, for several millennia, carefully preserved its complex morphological features, with Proto-Yeniseian at least losing or reshuffling most of them only recently, on the verge of disintegrating into further descendants; as a language whose descendants have undergone typologically rare, sometimes even unique, phonetic shifts; as a language where technical, cultural terms like 'canoes', 'belts', and 'sled-runners' were carefully nurtured and preserved, whereas basic terms like personal pronouns were consistently either dropped or at least "mutilated" beyond easy recognition. None of this readily agrees with what we have learned so far about language change all over Eurasia, and even beyond. And much of the blame lies on the Procrustean "requirements" traditionally imposed on the "long-ranger", who is often held to a more rigorous standard than the "short-ranger", and made to concentrate his attention on finding isomorphisms among the less stable layers of language than among the more stable ones (e. g. paradigmatic verbal morphology instead of basic lexicon).

That said, I am a firm believer in the art of separating wheat from chaff. There is nothing in Vajda's paper on its own that would make me join Prof. Hamp in a chorus of "Yeniseian-Dene of Edward Vajda is correct"; and, because of all the flaws described above, I definitely cannot view it as a giant leap in quality over all the previous work performed on the issue, e. g. by H. Werner and M. Ruhlen. But it *is*, by all means, a step forward. The few acceptable comparisons between grammatical markers may eventually point the way towards research on grammaticalization paths in Yeniseian and ND. The prosodic hypothesis offers a scheme of tonogenesis in Yeniseian that is worth exploring, even though it may not necessarily turn out to be true[4]. The small handful of etymologies that puts together Yeniseian labial consonants and ND labiovelars looks promising. If this is not yet "proof", by any means, of a "Dene-Yeniseian" relationship (much as I dislike the use of the word "proof" in demonstrations of such relationships), it does offer some clues as to how we could eventually obtain one — clues that, I hope very much, will be used in conjunction with those offered by other potential members of the same macrofamily.

It is also pleasant to notice that Edward Vajda is not rigidly conservative in his research, and is always willing to abandon or modify certain hypotheses when they turn out to contradict facts or more realistic solutions. For instance, the first draft of his paper that was available on-line for some time after the Symposium, almost completely ignored Proto-Yeniseian reconstructions (the comparison was essentially between ND and Ket/Yugh) and contained a much higher percentage of unacceptable etymologies and typological inconsistencies. The final draft has corrected many of these problems; although the verbal morphology section, I am afraid to say, has remained as unconvincing as it used to be, the phonetic / lexical section has become far more robust and difficult to criticize. I can only hope that this new round of constructive criticism will benefit the theory some more.

Finally, the "negative" aspects of Vajda's work are, in and out of themselves, "positive" in that consistent poking at its soft spots ends up pointing the ways in which we should proceed from here and those that should probably be abandoned. "Typologically suspicious" correspondences turn out to have been established for etymologies that fall apart for other reasons as well, whereas typologically healthier correspondences work on lexical comparisons that hold up much better. Verbal morphology is a dead end unless we stop talking in terms of synthetic paradigms and begin talking in terms of grammaticalization (being very careful in the process and trying not to use such talk as "first-order evidence"). And the very fact that "something" remains of the hypothesis even after the harshest critique — "something" that does not seem right to abandon, but is not enough on its own to constitute a complete historic scenario — shows that "Dene-Yeniseian" should, by all means, be put back from where it was taken: the much larger context of "Dene-Caucasian", which might produce quite a few answers where "Dene-Yeniseian" cannot.

[4] It is interesting to note that a somewhat similar, yet substantially different scheme of correspondences was spotted by S. Starostin between Yeniseian tones and the feature of "tense / lax articulation", reconstructed for Proto-North Caucasian, where NC lexical items with "tense" phonation of the first root obstruent seem to regularly correspond to words with a glottal stop in Yeniseian, and vice versa [Starostin 2005].

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The Dene-Yeniseian connection: a reply to G. Starostin

This reply elaborates on the many useful observations in George Starostin's critique. A traditional "rebuttal" is unwarranted for three reasons. First, his Yeniseian data are, in my estimation, completely accurate. This is no trifle, since these are languages few linguists have studied in depth and fewer have worked with in the field. Second, his judgments regarding Yeniseian are authoritative and articulated in a way that makes it easy to expand on them where needed, agree with them outright where not, and argue for my earlier interpretation where our conclusions remain at variance. Finally, I do not believe the results of my binary Dene-Yeniseian (DY) linguistic comparison contradict G. Starostin's current position on Dene-Caucasian (DC), which would otherwise be a source of major disagreement.

At the outset it might be useful to clarify my view on the external classification of Yeniseian. G. Starostin concludes that even the "harshest" (I would substitute "most informed") critique of the DY hypothesis leaves "something that does not seem right to abandon". This has essentially been my position for over twenty years — that there is *some* detectable historical connection between these families that is fruitful to investigate. I haven't yet formulated a firm opinion on the extent to which the broader DC hypothesis is correct. I have certainly offered nothing to disprove that Yeniseian and Na-Dene (ND) somehow fit into a larger family. In the past I have been highly skeptical of parts of DC and optimistic about other parts, though without ever having thoroughly studied all of the assembled evidence. In light of what I have found (or not found) in my own comparison of ND and Yeniseian, and in particular thanks to my correspondence with G. Starostin during the past few years, I increasingly view many aspects of DC as promising for the same sort of reasons that led me to the DY comparison in the first place. Awareness that my study was not properly taxonomic without a principled assessment of the available DC evidence has led me to refer to a "DY link" or "DY connection" rather than a "DY family" (see in particular Vajda 2011b: 113–115), leaving open the possibility that either Yeniseian or ND (or both) might have a closer relative elsewhere in Eurasia. DY as it currently stands is a hypothesis of language relatedness, but not yet a proper hypothesis of language taxonomy. The articles in *The DY Connection* investigated only one specific relationship, and their results cannot answer questions requiring analysis of additional families. I see nothing in my DY linguistic findings so far to rule out the possibility of my adopting some (or all) of G. Starostin's current views on DC. Below I will point out a few areas where a broader DC context does appear potentially more fruitful than binary DY, touching on specific observations made by G. Starostin in his critique. I would be eager for the opportunity to write a review of *The Dene-Sino-Caucasian Hypothesis: state of the art and perspectives* (Bengtson & Starostin 2012) when it appears, with the aim of providing a long overdue assessment from an "outsider's" vantage.

The key difference between my and G. Starostin's work on Yeniseian derives, in my view, from our differing individual interests and objectives. I have devoted much of my career to studying a single microfamily (Yeniseian), attempting to make contributions to the synchronic description of Ket before it disappears and also to elucidate the historical processes that created the remarkable structures found in Ket and its extinct sister languages. My forays into comparative linguistics have been motivated by a desire to trace the specific historical development of Yeniseian and discover facts about North Asian prehistory. Demonstrating how Ket-Yugh phonemic prosody arose or how the verb's complex template and idiosyncratic agreement system developed seems at least as important as helping demonstrate external genealogical connections with other families. This "inside-to-outside" focus is what led me to compare Yeniseian specifically with ND. My motivation was not taxonomy but rather to investigate the origins of particular Yeniseian linguistic systems through the use of promising external comparanda.

By contrast, G. Starostin's work has centered more widely on historical-comparative linguistics and language taxonomy. While his publications specifically devoted to Ket and Kott (most notably Reshetnikov & G. Starostin 1995 and G. Starostin 1995) represent seminal contributions to Yeniseian-internal linguistics (and are unanimously recognized as such within the small community of Ketologists), his real passion and focus is broader, encompassing much of Eurasia as well as Africa. His impressive command of linguistic data from diverse families strengthens his ability to formulate and test hypotheses regarding how Yeniseian fits into the overall world classification of languages in ways that work on one family would not.

Now to the "meat" — the morphological and phonological comparanda. There are three areas to discuss. The first two are properly linguistic: parallels in templatic verb morphology and lexical cognates. G. Starostin treats them in this order, following their presentation in Vajda (2011a). The third is the broader extra-linguistic context of archaeology, human genetics, and anthropology that formed a large part of the original 2008 DY Symposium as well as the published volume. None of the critiques of the DY volume published so far, including G. Starostin's, have given these articles more than a passing comment. I view them as extremely important. Although only linguistic evidence can demonstrate a language relationship, knowledge from reconciling multiple ways of studying prehistory, of which linguistic comparison is only one, can provide valuable insights into when and where a language community might possibly have existed. My subsequent analysis of the non-linguistic evidence in *The DY Connection*, given as a two-hour lecture available online (Vajda 2012), concludes that the time depth for a common ancestor to modern Yeniseian and Na-Dene populations must have been at least 12,000 years. This is not at variance with G. Starostin's linguistics-based calculations for the timing of a DY language link.

DY evidence from morphology centers upon a comparison of the finite verb templates. There are several issues. How similar are the templates being compared? What are the possible reasons for the similarities? How far back in time can such structures reasonably be expected to persist? And finally: how acceptable are the reconstructions of Yeniseian verb morphology used in Vajda (2011a)?

Complex templatic verb morphology has not generally been regarded at a typical object for historical-comparative study, and the comparisons in the DY volume were pioneering in several ways. No reconstruction of the Proto-Athabaskan (PA) template had been published before, and the generalized model in Vajda (2011a: 38) was developed in collaboration with Jeff Leer, Michael Krauss and Jim Kari. It is reproduced below in Fig. 1, followed by the Eyak (Fig. 2) and Tlingit (Fig. 3) templates from Vajda (2011a: 39):

Fig. 1 Generalized Athabaskan model showing oldest prefix zones

	oldest prefix positions						
derivational or thematic prefixes of various sorts	objects and deictic pronominal prefixes	lexical “qualifier” prefixes, including **n* –round **d* –long **qʊ* –area	tense/mood/ aspect marker **s(ə), *ɢə, *nə*	speech-act-participant subject agreement	perfective-stative prefix *ñi*	classifier *∅, d* *ɬ, l*	verb stem (root + TAM suffix) **-ɬ, *-ŋʸi*

Fig 2. Eyak verb (based on Krauss 1965)

	oldest prefix positions						
derivational or thematic prefixes of various sorts	objects and deictic pronominal prefixes	shape or anatomical prefixes (from incorporated body part nouns), and other elements	tense, mood, aspect *ɢə* (prefix *s(ə)* has moved to the right of the subject prefixes)	1sg, 2sg, 2pl subject agreement	tense mood prefix *s(ə)*	classifier *ɬ ~ ∅* *ɬə ~ ɬi* *də ~ di* (*i* < stative prefix)	verb stem (root + TAM suffix) **-ɬ, *-ɬ*

Fig. 3. Tlingit verb (based on Leer 1991)

	oldest prefix positions						
derivational or thematic prefixes of various sorts	objects and deictic pronominal prefixes	incorporated nouns	tense/mood/ aspect *ɢa, ÿu* *ÿu* is cognate with Athabaskan-Eyak *s(ə)*	distributive	subj. agr.	classifier (*i* < stative prefix) *ɬa ~ ɬi* *da ~ di* *sa ~ si*, etc.	verb stem (root + TAM suffix) **-n, *-ÿ*

The models are reproduced here to illustrate the key point that causal inspection can detect that these structures derive from a common prototype. Cognate morpheme subsystems occupy homologous concatenations of prefix positions. Because lexicostatistic estimates of vocabulary retention date Proto-Na-Dene at 5000–6000 years old, this degree of preservation of complex syntagmatic morphology would seem remarkable, if not “impossible”. Still, despite the now uncontroversial acceptance of Athabaskan-Eyak-Tlingit (Na-Dene) as a valid family (Campbell 2011), a common “proto-template” has yet to be reconstructed.

Obstacles to reconstructing a clear-cut Proto-ND template include unexplained gaps (the lack of "qualifier" prefixes in Tlingit), unexplained insertions (the Tlingit distributive), metathesis of morpheme positions (most notably the migration rightward of the Eyak tense-mood prefix ahead of the subject prefixes and the stative prefix ahead of the "classifier" consonants). The rigid template also gave rise to frequent reanalysis of morpheme functions (Leer 2009). Such changes, though found in concatenative morphology, may characterize the evolution of templatic morphology more fundamentally. Yet in the case of ND, none of the incongruities succeed in obscuring the common origin of these complex structures, even at a time depth of several thousand years. My opinion is that templatic morphology is typically much more persistent than commonly thought and thus potentially valuable for historical-comparative study. Difficulty in reconstructing a PND verb template despite the overwhelming evidence that one must have existed suggests that methods for tracing the evolution of templatic morphology have not been worked out. Until this general problem is solved, it seems prudent to be cautious in equating homologies in templatic morphology with paradigmatic evidence from concatenative morphology. But ignoring their obvious value to historical-comparative linguistics, especially their potential for tracing shared innovations needed to establish subgrouping in a language family, is also unwarranted.

The PY template reproduced in Fig. 4 was published in Vajda (2011a: 40):

Fig. 4. Proto-Yeniseian verb morphology

<table>
<tr><td></td><td colspan="6">prefix positions</td><td colspan="3">verb base</td><td></td></tr>
<tr><td rowspan="2">obj. agr. (proclitic or separate word)</td><td rowspan="2">incorporated body-part nouns, spatial and shape prefixes, including *n — round *ǯ — long *p^{h} — flat</td><td rowspan="2">3p inan.*w- anim.? *d^{j}- (anim. preceded by gender/ number agr.)</td><td colspan="2">tense, mood, aspect combination AUX + suffix</td><td rowspan="2">1p, 2p subj. agr.</td><td rowspan="2">imperative prefix *ǯ or perfective-stative prefix *jə</td><td rowspan="2">verb-deriving prefix *ǯ, also possibly *ɬ</td><td rowspan="2">verb root</td><td rowspan="2">perf.-stative suffix (-ej, -ŋ)</td><td rowspan="2">anim.-pl. subj. agr.</td></tr>
<tr><td>*s > s, i, a vs. *qa > qo, o</td><td>*l vs. *n</td></tr>
</table>

Ket and Kott, though separated by at least 2,000 years, have retained most of this overall structure, except for the addition of a new subject position: suffixed in Kott on the verb's rightmost edge, prefixed at the leftmost edge in Ket. The striking contrast of subject agreement at opposite ends of the verb complex tends to overshadow the even more striking fact that most of the rest of the template remains homologous, even down to vestigial features such as an imperative prefix before zero-anlaut verb roots, despite significant difficulty in reconstructing cognate morphemes in certain positional subsystems (about which more below).

Vajda (2011a) was a first attempt to describe homologies between the verb templates in Yeniseian and ND languages. The basic argument was that these structures all descend from a common prototype, though one that cannot be properly reconstructed as yet. Parallels between PY and the three ND templates include the general order of morpheme positions, as well as a system of tense-mood-aspect expressed through the interaction of three subsystems, one of them being a circumfix labeled "stative" or "perfective-stative in the models above.[4]

G. Starostin makes a number of astute observations about my verb morphology comparisons with which I

[4] G. Starostin does not critique this feature of my template, and I mention it to call attention to the overall similarity of morpheme positions and tense-mood morphology between the families. Still, I suppose it appropriate to supply some criticism of my own. A better name for these morphemes in Yeniseian would have been "intransitive affix", since in Yeniseian they appear not only in stative verbs denoting the result of a completed action such as *il-u-k-s-**ajə**-bed-**ej*** 'it is broken', but also vestigially in parts of the paradigm of action intransitives such as Kott "lie down" (see the full paradigm and discussion in Vajda 2011a: 48–49). The probable Na-Dene cognate prefix *-*ŋ*^{y}*i* is found only in resultatives, though the suffix *-*ŋ*^{y}*i* is found in both resultatives and perfective verb forms, so my name choice for Yeniseian unduly equated the function of this morpheme across the two families. There is also the problem of explaining the Ket suffixal allomorphs -*ej*, -*ŋ*, and the counterargument that the prefix -*jə* could be another morpheme. It is also not clear that the Na-Dene prefix and suffix are the same morpheme (Jeff Leer, p.c.), though their shape in Proto-Athabaskan is identical.

can immediately agree. First, it is not helpful to call the tense-mood prefixes "auxiliaries" (AUX), since their earlier origin is conjectured and is not in any case relevant to the comparison; it is better simply to refer to them as "conjugation markers" or "tense-mood prefixes". Second, all of his reservations about the "shape prefixes" (second slot on the left in Fig. 4) are well spoken. The same problems were already acknowledged in Vajda (2011a: 55, third paragraph), where I wrote that "the shape markers represent only a minority of the prefixes found in this zone in both families" and "are not the best evidence of genetic relatedness". These single-consonant morphemes are located between agreement markers and conjugation (tense-mood) markers in both families, a parallel that is probably relevant in tracing verb structure in both families to a common origin. Unfortunately, attempts by me (and others) to elucidate their origins and semantics have so far made only marginal progress. Ket shows only a few instances where these prefixes, traditionally called "determiners" after Krejnovich (1968), alternate in ways that clearly support the semantics I assigned to them (e.g., *d-n-a-b-do* 'I carve a round object', *d-d-a-b-do* 'I carve a long object'). Intensive fieldwork on Ket since the Feb. 2008 DY Conference produced little additional evidence. On the Na-Dene side, much work is still needed to compare Athabaskan "qualifier" prefixes with possible cognate prefixes in Eyak and Tlingit. I continue to suspect that some of the Yeniseian "determiners" and Athabaskan "qualifiers" are cognate, but without a better account of their origins in each respective family, this is one aspect of the comparison that probably should be "shelved" for the time being.

Closer to the verb root in the template, and presumably older, are various layers of tense-mood-aspect morphology. In Vajda (2011a) I argued that TAM marking in both families is achieved through an interaction of three subsystems: the conjugation markers (infelicitously labeled in Fig. 4 above as AUX), the aspect markers (imperfective *-l*, perfective *-n*), and the so-called "perfective-stative" circumfix (discussed above in footnote 1). The different location of the aspect markers in both families remains unexplained and this presents an obstacle to template reconstruction, though there are no problems with equating their phonetic form or semantics. With the conjugation markers, the opposite is true: their position in the two families is homologous, but establishing cognacy in their forms raises all of the problems described at length in G. Starostin's critique. I do not believe that my identification of Ket *s-* and *qo-* as tense/aspect markers is controversial or "forced". At least, it was already proposed earlier and not in connection with the DY comparison. Krejnovich (1968: 14) interpreted *s-* as a tense-mood marker. Reshetnikov and G. Starostin (1995: 87) concluded that *q* ~ *qo-* in the Ket paradigm 'S kills O' most likely represents an archaic tense marker, though one that is exceedingly rare. G. Starostin (1995: 165–166) further concluded that a *š-*conjugation existed in Kott, where he cited the following partially cognate verb forms: Kott *tha-č-a-pil-aŋ* 'I catch up' and Ket *d-ba-t-s-i-bil* 'he catches up to me' to illustrate an uncommon parallel between Kott *š-*conjugation and Ket *i*-conjugation. I would claim the sequence of Kott *tha-ča-pil-* and Ket *-t-si-bil* as evidence for a PY **si*-conjugation, though I agree that tracing the internal development of Yeniseian conjugation markers remains problematic for precisely the reasons discussed in G. Starostin (1995). G. Starostin's interpretation of *s-* in Ket as connected with the agreement system (Reshetnikov & G. Starostin 1995: 45–52) is harder for me to support because it occurs in numerous transitive as well as intransitive verbs and only in the present tense (e.g., Central Ket *d-sin-u-k-si-bäd* 'I get it dirty', *sin-u-k-si-bäj-aj* 'it is in a state of having been made dirty'). The alternation between Ket *si-* and *i-* is conditioned morphonologically (Vajda 2001: 411–415): *si-* occurs after certain determiners or single-syllable incorporates when followed directly by the base morpheme with no intervening prefixes; in verbs of the same positional configuration with (historically) polysyllabic incorporates, *-i-* replaces *si-*, since such verbs are composed of two phonological words: e.g., *d-don-si-bed* 'I make a knife", *d-donaŋ#i-bed* 'I make knives', where # marks a phonological word boundary. The fact that Ket conjugation marker *s-* obeys different phonological rules than Ket *s* elsewhere would seem to support the comparison with Na-Dene palatal *x^y. Yeniseian **s* that corresponds to Na-Dene **s* is stable word-initially, as evidenced by PY **sēŋ* and PAE *-*sənt*' 'liver'.[5]

My claim of cognacy between Ket *qo-* and the widespread ND *ɢ*a-* is weakened by the rarity of the former marker in Yeniseian, a point already made in Campbell (2011). While the *s-* marker in Yeniseian is widespread (however it may be interpreted), only a

[5] The three Yeniseian *s*-initial cognates to Na-Dene words with initial *x^y listed in (Vajda 2011a: 84) have irregularities within Na-Dene that were left unexplained in Leer (2011). It might be possible to explain this if the initial sound in pre-PND was not *x^y but velar **x*, which merged with **s* before front vowels in Yeniseian but in ND became *š* before front vowels and remained velar *x* elsewhere. If this is the case, Yeniseian cognates to genuine Na-Dene word-initial *x^y have yet to be found and would be expected to be zero-initial.

few irregular Ket verbs appear to show a clear parallel to the sibilant vs. uvular opposition that is fundamental to ND conjugation marking: cf. Ket *d-us-s-ej* 'I do a bit of hunting' ~ 'I kill (an animal) on a hunt', *d-us-q-ej* 'I did a bit of hunting' ~ 'I killed (an animal) on a hunt'[6]; *d-i-k-ej* 'I kill you' (where *-s-* is lost phonological-word initially, *d-* being a clitic), *d-qo-k-ej* 'I killed you'. What *is* new in *The DY Connection* is my attempt to explain the *entire* Ket conjugational opposition as (*s*)*i-* ~ *a-* vs. *o-* from original PY **s*(*i*)- vs. **q*(*o*)-. G. Starostin's critique clearly demonstrates that this explanation, at the very least, must be re-argued more convincingly and in greater detail. Obviously, evidence from internal reconstruction used to support an external genetic relationship must first pass muster among specialists in each language family before it can be established as non-controversial. To ignore the experts in either family would quickly lead the hypothesis to a dead end. I would maintain that my comparison of Yeniseian **s*(*i*)- ~ **q*(*o*)- with PND *x^y*i* ~ **ɢa* was predicated on earlier work by other Ketologists and should be retained as promising in light of the positional as well as phonological parallels, even if we reject my present attempt to trace the entire Yeniseian conjugation system from these two markers. The problem of understanding the synchronic opposition between Ket *i-* and *a*-conjugations seems partly connected with the nature of the preceding determiner consonants (e.g., Ket determiners *d-* and *h-* are always followed by *a*-conjugation). If this is the case, then progress in explaining the distribution of Ket conjugation markers will first require a better understanding of the origin of the determiner consonants that precede them, and this, as explained above, remains a challenge.

To round out the discussion of component systems in the Yeniseian and Na-Dene verb templates, I concur with G. Starostin (and Andrei Kibrik) that my comparisons of pronouns and valency-changing consonants are inconclusive. G. Starostin's DC pronoun comparisons do appear more promising. My comparison of infinitive/gerund formation (Vajda 2011a: 60–63) is one of the stronger pieces of morphological evidence for DY, and it too should be compared with similar structures in other putative DC languages. I would not agree that these infinitive forms have no bearing on discussions of verb structure, since they share a homologous derivational relationship to the finite verb template in both families.

What else can be concluded so far from my DY comparisons in verb morphology? I do not believe that early optimism about evidence from verb morphology is misplaced. The parallels in overall template structure far exceed change resemblance, though how precisely to quantify them remains problematic. I also continue to support the three interacting systems of TAM morphemes as homologous, while emphasizing the need to account for unexplained incongruities. Studying features of the templatic comparison that do not yet fit should lead to a better understanding of template evolution in both families. In the meantime, because more historical work with templates is needed before even uncontroversially related structures such as those inherited from PND into Tlingit, Eyak and Athabaskan can be fully reconstructed, it might be useful to develop a standard for assessing potentially inherited similarities in templatic morphology that represent "something that doesn't seem right to abandon", yet continue to defy clear-cut reconstruction. G. Starostin's suggestion to consider processes of grammaticalization seems very much worth pursuing. Because we already know that language families exist but don't yet understand how templates develop through time, discovering general historical patterns in template evolution may ultimately prove more important than the DY language link itself. Ancestral Na-Dene speakers need not have crossed into the Americas brandishing a stainless-steel template for the parallels between modern Yeniseian and Na-Dene verb structures to represent evidence of descent from a common prototype.

A few more comments in favor of the value of morphology to historical-comparative studies may be useful before moving on to the lexical and phonological correspondences. I would claim that reconstructing a proto-language's phonemic inventory requires morphological analysis, in addition to straightforward phonological comparison of basic vocabulary. S. Starostin's (1982) pioneering reconstruction of PY still stands as a benchmark for use in comparing Yeniseian with other language families. However, some details may eventually be amended based on evidence from Yeniseian-internal morphological reconstruction. S. Starostin (1982: 148) reconstructs five liquid phonemes for PY — *r*, r_1, *ŕ* [= r^j], *l*, and *l´* [= l^j] — based on sound correspondences in basic vocabulary between the daughter languages. This is typologically unusual, and the number may be reduced through further study of PY morphology.

[6] The incorporate *us-* in this verb is found in a number of other syntactically transitive verbs, where it has a partitive meaning with respect to an object not marked by verb-internal agreement: *d-us-a-dop* 'I drink a bit (of it)' vs. *d-a-b-dop* 'I drink it', *d-us-l-a* 'I ate a bit (of it)' vs. *d-b-il-a* 'I ate it', *d-us-si-bed* 'I make a bit (of it)' vs. *di-b-bed* 'I make it'. (Examples from my August 2008 fieldwork.)

To illustrate how "hidden" morphology can mimic phonemic contrast, the cognate sets shown in Fig. 5 seem to support four PY liquid phonemes. The generalized symbols *L* and *R* do not follow S. Starostin's actual system of reconstructions (S. Starostin 182: 152–156), which my discussion here does not challenge:

Fig. 5. Yeniseian cognate sets with four contrasting liquid correspondences

PY	Ket	Yugh	Kott	Arin	Pumpokol
**xuR* 'rain'	*ū·l*	*ūr*	*ur*	*kur*	*ur*
**xuRa* 'wet'	*ū·l*	*ū·lʲ*	*ura*	*kur*	*uŕga*
**piLaŋ* 'sweet'	*hilaŋ*	*fɔraŋ*	*falaŋ ~ pʰalaŋ*	*pala*	—
**buL* 'leg'	*bū·l*	*būl*	*pul*	*pil*	—

That all laterals and rhotics have merged in Ket as *l* is obvious, as is the presence of at least two liquid phonemes in Kott and Arin. The apparent need to posit additional liquids in PY arises from how Yugh pairs up with the southern languages. At least some (if not all) instances where auslaut Yugh *lʲ* corresponds to Kott, Arin and Pumpokol *r* have a morphophonemic explanation: the Yugh liquid in *ū·lʲ* 'wet' (inherited as the same sound as in *ūr* 'rain') absorbed a velar segment, still attested in Pumpokol *uŕga*, the second syllable of which represents an adjective derivational morpheme. Absorption of this suffix also accounts for the half-length in Yugh *ū·lʲ* 'wet', since Yugh half-length in high-tone syllables normally derives from an elided second syllable. Future work of this sort might decrease the inventory of PY liquid phonemes.

Another reason to pay attention to internal reconstruction at the outset, and not only after phonological analysis is completed, can be illustrated by examining instances where Yeniseian morphology shows vestiges of what may once have been additional phonemic contrasts in pre-PY nasals. Ket/Yugh *ŋ* normally corresponds to Kott *ŋ*. However, there are instances in morphological paradigms where an expected Kott *ŋ* is lacking except where it (probably) historically followed another nasal. I suspect such cases are traces left by an additional PY nasal phoneme **ŋʲ* that merged with **ŋ* in environments that preserved it from disappearing. In Ket/Yugh noun paradigms, case forms made from the possessive form regularly contain a "mystery" *ŋ*, though the bare possessive (genitive) form does not: Ket *ō·b* 'father', *ob-da* 'father's', *ob-da-ŋ-a* 'to father', *ovaŋ-na-ŋ-a* 'to the fathers', *ob-da-ŋ-ten* 'at father's place', *ovaŋ-na-ŋ-ten* 'at the fathers' place', *ob-da-ŋ-al* 'from father', *ovaŋ-na-ŋ-al* 'from the fathers'. The morphemes *-a*, *-ten*, *-al* are dative, adessive, and ablative suffixes, *da-* the masculine possessive clitic, and *na-* the animate plural possessive clitic. The nasal inclusion *-ŋ* appears to be the vestige of a generic possessive marker (probably cognate to the Na-Dene possessive nasal prefix to be discussed below). Kott inexplicably lacks the initial consonants *d-* and *n-* of the third-person possessive markers: *op* 'father', *op-â* 'father's', *op-â-'a* 'to father'. The nasal, however, *does* show up in Kott animate-plural forms, including the genitive: *opan-a-ŋ* 'father's', *opan-a-ŋ-a* 'from fathers', where it may have originally followed a PY 3p animate plural possessive marker **nʲa-*. The fact that this *ŋ* appears in the Kott animate plural forms but not in the singular or inanimate plural, suggests that its preservation was somehow conditioned by the preceding animate-plural marker. The possessive consonant elements — Ket 3sg. *d-*, Ket 3animate pl. *n-*, and generic possessive *ŋ ~ ∅*, with its partly overlapping distribution in Ket and Kott — each may represent a trace of a phoneme originally distinct from PY **d*, **n*, and **ŋ*.

A clue to the absence of an expected dental consonant in the Kott masculine singular possessive may be found in instances where Ket/Yugh **d* corresponds to Kott *g*, as in Ket *do'p* 'to drink' vs. Kott *ši-gap* 'to drink', where *ši-* is an infinitive prefix mostly lost in Ket. Compare Ket *-dop* in verbs meaning 'swallow' with Kott *tôp-* in verbs meaning 'eat' — cognates that show the common correspondence of Ket/Yugh *d* to Kott *t*. The rare correspondence of Ket *d* to Kott *g* in *do'p* — *gap* is not part of S. Starostin's (1982, 1995) system and could be a morphonological relic of an earlier phonemic contrast. My typologically jarring mix of dental and velar reflexes for PND labialized velar phonemes might eventually find additional support within Yeniseian.

None of these observations entail actual changes needed in the reconstructed sound system in S. Starostin (1982, 1995). They are provided to illustrate how the "messy" and more labor-intensive work of morphological analysis can impinge upon external comparisons using reconstructed sound systems. Morphological analysis shouldn't be viewed entirely as secondary to phonological analysis — whether in work on a single family or in external comparison.

Turning at last to the evaluation of specific DY cognates, G. Starostin's judgments on the Yeniseian lexicon are, as always, very illuminating. I regret he did not have space to critique all of the lexical and phonological evidence. But the portion he discusses is sufficient to support his two main points: the DY link appears to be very old, and some of the cognates appear to be shared with other DC families. I agree with him that not all of the DY sound correspondences have been properly worked out, and that correspondences supported by a single example remain suspect. Correspondences that defy typological generality at best would seem to omit an intermediate stage, at worst may prove wrong. My goal in publishing Vajda (2011a) was to provide a tentative system sufficient for evaluating future evidence. I can confirm G. Starostin's suspicion that some of the correspondences were indeed conceived around what seemed to be particularly promising cognates. In some cases, this technique led to the discovery of a pattern, while in others it resulted only in a thin patch over what otherwise would have been a hole in the system. The latter cases are the ones most likely to be spurious. G. Starostin suggested I should have included a summary table of sound correspondences, but this I deliberately omitted so that readers would need to study my actual supporting evidence, seeing for themselves what is stronger and what is weaker. A polished table would have given the impression that all had been settled, and could not have helped the sort of informed critique G. Starostin has supplied here. The same applies to my omission of a reconstructed verb template, which I also think is premature, given that a PND template itself is not yet reconstructed, so that providing such a model would only serve to obscure important questions yet to be answered. A concise demonstration of Proto-DY phonology and morphology suitable as an encyclopedia entry is probably many years away. I noted in Vajda (2011a: 64) that what was offered in the sound correspondence sections was "merely a first attempt to apply the comparative method to a rather limited portion of basic vocabulary in the two families". I myself did not make a statistical analysis of the number of cognates, knowing that some of the proposed matches might be invalidated and new ones added as more data was compared.

I can now comment on the lexical comparanda specifically discussed by G. Starostin, bringing up additional points that might in future affect their acceptability. Any information that did not appear in Vajda (2011a) is not properly an answer to his critique. But since I am not defending DY as "proven" but rather describing it as a promising work in progress, giving new reasons to support (or reject) the cognates already proposed is not out of place. If the hypothesis were completely "proven", there would be no need to add new evidence.

The PAE reconstruction I gave in my article for 'liver' — **-sənt'* — would better have been cited as **-səNt'* (or preceded by the symbol ~ indicating approximation), since the place of articulation of the nasal is not actually attested in either Athabaskan or Eyak. There is no way to be sure if the PAE form contained the homorganic cluster of **-sənt'*, as I showed, or should rather be reconstructed as **-səŋt'* or **-səŋʷt'*. I agree that the main problem for DY here is not the quality of the nasal (which may be important in evaluating cognates elsewhere in DC), but rather in finding parallels to the final obstruent in the cluster.

I would rate PAE **-wət'* — PY **pʰə̄j* 'belly, stomach' (in the sense of 'surface of abdomen', not 'stomach as an internal organ') as more promising than G. Starostin concludes (and would not discount ST **puk* either), despite the obvious phonological problems. The Proto-Ket-Yugh **pʰɨčej* 'downward' (> Ket *hɨta*, Yugh *fɨčej*)[7], **pʰɨčər* 'below', **pʰɨčəkej* '(located) below', and many similar words in the semantic category 'below', 'lower' probably derive from PY **pʰə̄j* 'belly'. These derivates seem to show a closer coda correspondence with ND. A potential Tlingit cognate is problematic within ND: cf. Interior Tlingit *-yuwá* '(outer part of) abdomen', where the second syllable *-wá* would seem a logical candidate for cognacy with PAE **-wət'* and PY **pʰə̄j* were it not for the unexplained first syllable *yu-*. Also conceivable is an etymological connection between PY **pʰə̄j* 'belly' and the Ket-Yugh suffix **-pʰad*, which denotes a flat surface in compounds such as Ket *kassat* 'sole of the foot', *battat* 'face'.

The phonological problems with 'belly' might be part of a broader pattern that hinders a number of other basic words from being recognized as straightforward cognates. Several body part terms would appear to be cognate between the two primary branches of ND (and also with Yeniseian), except that in either PAE or Tlingit they show unexplained phonological irregularities. Putative cognates for 'head' are a good example: Ket *tɨ'*, Yugh *čɨ'* and PA **-tsi'*, Tlingit *-šá*, for which Leer reconstructs PND ~**kʸe/i(·)ŋ'* by including a nasal element attested in certain possessive compounds such as 'head hair', based on a nasal element found in PAE but absent in Tlingit (and Yeniseian). The irregular anlaut correspondence of PAE **ts* —

[7] The symbol [ɨ] in Ket and Yugh words transcribes a high back unrounded vowel, more properly IPA [ɯ].

Tlingit *š* (instead of expected *k*), as well as the unexplained nasal in compounds may stem from traces of possessive affixal morphology in inalienably possessed PAE nouns. Possessive constructions in ND may have consisted of: possessor noun or possessive pronominal prefix + ~*$ŋ^y$ (a generic possessive marker) + possessed noun + possessive suffix (not present at all in Yeniseian, but found regularly in alienably possessed nouns in the form of Tlingit *-i'* and PA *-*e'*)[8]. Generic possessive ~*$ŋ^y$ here is probably cognate with the Yeniseian possessive nasal element discussed above, and may even be homologous with the unexplained syllable *yu-* in Interior Tlingit *-yuwá* 'abdomen', though it is no longer found as a regular part of possessive formation in either family. In many Athabaskan languages it remains sporadically between personal possessive prefixes and inalienably possessed nouns: cf. Slave *si-ⁿ-lá* 'my hand', *ni-ⁿ-lá* 'your hand', etc., where *ⁿ* represents nasalization of the preceding vowel (see Rice 1989: 211–212 for a list of such nasal-prefixed inalienably possessed nouns in Slave). It is also the likely source of the nasal inclusion in PAE possessive compounds like 'head hair', where 'head' is the possessor; Leer's PND reconstruction of ~*k^ye/i(·)ŋ*' 'head' may represent a linguist's reanalysis of a formerly productive possessive marker as a part of the preceding root.[9] If incongruities in DY (and internal ND) sound correspondences in inalienably possessed nouns can be explained as vestiges of possessive morphology, the percentage of basic vocabulary in the DY cognate sets will increase.

Regarding the semantics of PY *ki's*, the Ket compound *kassat* 'sole of the foot' (< *ki's* + *p^had* 'flat surface') suggests it may have had the original meaning 'foot' as well as 'leg'. Non-canonical sound correspondences between Eyak *-k'ahš* 'foot, lower leg, paw', Tlingit *-q'os* 'foot, leg', and PA *-*qe'* 'foot' may likewise derive from ancient possessive morphology. Ruhlen (1998: 13,995) first proposed the Eyak and Tlingit forms as cognate to Ket *ki's* 'leg', but as noted in Vajda (2011a: 88), these words fail to obey regular ND-internal sound correspondences. If it becomes possible to identify the historical effects of ND possessive affixes on inalienably possessed nouns, the incongruities in anlaut and coda among these forms might find an explanation confirming their cognate status after all. In general, PA forms lacking obstruent codas, such as *-*qe'* 'foot', remain difficult to reconstruct with confidence. I suspect that an earlier possessive suffix similar to the PA alienable possession suffix *-*e'* interacted with the original root coda of PA 'foot', which must have been PND **x* or *x^y rather than **s*.

In connection with the discussion of 'head', G. Starostin's suggestion that the PND "palatal" series discussed in Leer (2011) might have actually been an affricate series in pre-PND seems logical to me, and I agree it fits with my earlier suggestion (Vajda 2011a: 84–86) that the PND affricate series might have arisen later through a split caused by palatalization of labialized velars (and plain velars) before front vowels.

G. Starostin's reservations about questionable morpheme breaks in such words as 'stand', 'earth', 'many' are all perfectly valid. I'm not ready to abandon these as possibilities, but I do agree they remain tentative until a convincing morphological analysis is presented.[10] As I argued above with reference to vestigial possessive morphology, problems with some proposed cognates may find resolution. Because I am more interested in solving problems in the historical development of these languages than insisting on quick "proof" that the families are related, I would prefer a skeptical approach to all my proposed DY homologies, yet one informed

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[8] For Tlingit see Leer (1991: 38), for PA see Leer (2005: 290–299). In Athabaskan, possessive suffixes are found on some inalienably possessed nouns (notably kinship terms) but not others. In alienably possessed nouns the possessive suffix sometimes changes the phonology of the root syllable coda to create non-canonical sound correspondences: e.g., PA **łu·q'-e·* 'fish, salmon' > Modern Ahtna unpossessed *łu·q'e·* but possessed *-lu·ɢe·*. Leer further suggests that the unsuffixed root PA **łu·q'* gave rise to the form PA **łû·χ* 'whitefish', showing another non-canonical sound correspondence. My hypothesis here is that inalienably possessed nouns such as body part terms in ND once contained possessive suffixes that were absorbed into the noun root rhyme, causing irregular correspondences within ND and also difficulty in establishing regular sound correspondences with the Yeniseian cognates. At present my hypothesis must be considered speculation, even "revolutionary" speculation with respect to traditional ND historical linguistics, and obviously requires a much more thorough treatment than can be given here.

[9] The nasal in Yeniseian words for 'head hair' could conceivably come from the same origin, but I agree with G. Starostin that my comparison raises too many other obstacles that would need to be overcome to support cognacy with the synonymous ND compound. Still, the fact that Yeniseian 'head' and '(head) hair' both begin with the same, rather uncommon Yeniseian sound, which S. Starostin reconstructs as **c*, may be noteworthy, and their could be some etymological connection.

[10] The same might be said about G. Starostin's Burushaski/Yeniseian comparison of Hunza *tul*, Nagar *tol* 'snake' and Ket *tuln* ~ *tulin* 'lizard', which is promising on both phonological and semantic grounds, yet leaves unexplained the final Ket *-n*. The Yugh cognate *tunʲil* ~ *tunʲɔl* 'lizard' further complicates the picture, since it is not yet clear which language — Ket or Yugh — underwent metathesis (cf. a similar pattern in Ket *baln*, Yugh *banɨr* 'bird cherry tree'). Again morphological analysis would seem to enter into the very first stage of historical-comparative investigation.

on facts so as to pose genuine questions for further research. G. Starostin's articulation of principled degrees of probability in accepting or rejecting cognates is extremely constructive. Every hypothesis of long-distance language relationship should be fortunate enough to attract this valuable sort of informed criticism.

I have always preferred a high bar of acceptability in evaluating proposed cognates and may have missed some through reluctance to admit semantic shifts. G. Starostin's observations on my lexical comparisons all seem logical and well founded to me. I only question the unavoidable rigidity of his (or any) lexicostatistic approach based on a universal set of basic vocabulary. Words for 'resin', 'conifer needles', 'grouse', 'wolverine' (all proposed as DY cognates) are surely basic in the context of northern forest life (and could also be ancient vocabulary shared with other languages). A body-part term such as 'finger' (more properly 'digit', 'finger or toe') — though admittedly not among the traditional Swadesh 100 — seems eligible on semantic grounds to be calculated as basic alongside 'liver' or 'neck'. Whether counted or not, the DY cognates for 'finger' (PY **tə'q*, PAE **-ts'inɢ*, Tlingit *-tɬ'ìɢ*) are a strong match (Vajda 2011a: 82), with the anlaut and coda obstruent, as well as the prosody each simultaneously obeying its expected systematic sound correspondence. The nasal inclusion in the PAE reconstruction again reflects a nasal found in possessive compounds and thus resembles the situation with 'head' discussed above; I suspect that it too is a vestige of an earlier possessive affix. I am not advocating changing lexicostatistic rules simply to accommodate DY, but merely wish to argue that the lexical comparisons in Vajda (2011a), notwithstanding all their warts and gaps, remain more promising overall than might seem from reading only G. Starostin's critique of a principled selection of them.

Yeniseian words for 'snake' and 'dog' both involve what I posit were anlaut lateral affricates. In the system proposed in Vajda (2011a), correspondences of Modern Ket *t* — Yugh *č* derive either from a lateral affricate **tɬ* (**tl* ?) or from original **č*. That Proto-Ket-Yugh **č* results from a merger of two formerly distinct sounds can be seen when comparanda are available from the southern Yeniseian languages. Proto-Ket-Yugh **č* from original PY affricate **č* corresponds to *š* in Kott and *k* ~ *q* in Arin and Pumpokol (Ket *tɨ's*, Yugh *čɨ's*, Kott *ši:š*, Arin *kes*, Pumpokol *kit* 'stone')[11]. ND cognates to precisely these words show reflexes of the so-called palatal **kʸ*. Ket-Yugh cognates with the same correspondence of Ket *t* — Yugh *č* that correspond to southern Yeniseian words with initial *als-*, *al-*, *il-*, *ils-*, on the other hand, seem to correspond to ND cognates beginning in the lateral affricate **tɬ'*. Neither the traditional Yeniseianist interpretation of *al-*, *il-* as a fossilized prefix of undetermined semantics[12], nor my reconstruction of PY lateral affricate **tɬ* is without problem, however, and neither can be fully accepted or rejected at present. As for the prefix solution, Modern Ket does have a similar prefix *il-* (always with the vowel /i/, probably from **i'r* 'breathing') that appears on a few words to add the meaning 'earthly' or 'mortal' (*ilbaŋ* 'earthly realm', *ilget* 'mortal person', *ildeŋ* 'mortal people') in contrast to 'supernatural' (cf. *esdeŋ* 'spirits' < *ēs* 'sky' + *de'ŋ* 'people'). But Ket words with this prefix are used only in folklore and not as basic vocabulary (cf. Ket *ba'ŋ* 'land', *ke't* 'person', *de'ŋ* 'people'), whereas Kott, Assan and Arin initial *al-* or *il-* appears to be integral to a few specific words; also, their choice of /a/ vs. /i/ usually follows the quality of the root vowel, as would be expected if this element were epenthetic. Particular vocabulary items on all known Kott or Arin word lists, though transcribed by different scholars at different times, either uniformly contain or uniformly lack this element in each language. So its origin as a prefix remains inconclusive. If on the other hand my interpretation is correct, then I don't think the evolution of this hypothesized PY **tɬ* has been satisfactorily worked out either. The phonological interpretation in Vajda (2011a: 92–93) cannot explain the anlauts of Ket *qòχ*, Kott *alaga*, Arin *ilqoj* ~ *il'xok* ~ *il'koj* 'star'[13]. Also, if Ket *tìɣ*, Yugh *či:ʰk* 'snake' are cognate with Kott *teg* 'fish' and Arin *ilta* ~ *ilti* 'fish', then according to my interpretation, the "expected" Kott form should be the unattested **ilšeg* rather than the attested *teg*.[14] I cannot explain this ei-

[11] Except where anticipatory dissimilation in Kott seems to have taken place: e.g., Ket *tὲs*, Yugh *čε:ʰs*, Kott *he:či* 'felt boot', and Arin *qesiŋ* 'felt boots' (with pl. suff. *-ŋ*)

[12] See Vajda (2001b: 273) for a description of earlier studies by L. Timonina advocating the prefix solution.

[13] These Arin variants were recorded by different scholars and possibly represent different dialects (see Werner 2005: 157), but they illustrate the stable presence of the initial syllable in specific Arin words. Note that my hypothesis would expect prothetic *a-* not *i-* here, in keeping with the back vowel in the root.

[14] I also dislike the vowel mismatch in Ket *tugun* and Kott *teg*. My problem with S. Starostin's original comparison of Ket/Yugh 'snake' and Kott 'fish' is rather with the anlaut correspondence Ket *t* — Yugh *č* — Kott *t*, as each of the other seven proposed cognates with this correspondence (S. Starostin 1995: 214–215) seem to me to have morphological problems that call into question whether actual cognate forms are being compared. See Vajda (2011a: 83, final paragraph) for a note about this in relation to cognates for 'head').

ther. What makes my phonological approach worth investigating further are instances where Yeniseian (or other DC words) containing the syllable *tVl* correspond to ND words with initial **tɬ*' (again see Vajda 2011a: 92–93), or, albeit irregularly, to Yeniseian words containing the correspondences being discussed here. Donner (1955: 92) records Central Ket *toln* 'fishing worm, earthworm'. Ket *utix* 'earthworm' is plausibly derived from **ur* 'rain' modifying the root for 'worm, snake', with the coda of **ur* truncated by the anlaut affricate **tɬ-* of the following root. The same argument could be made about the first element of Ket *atix* 'freshwater lamprey', which may contain a truncated form of **an-* found also in Ket *anbok* 'wave', though admittedly the semantics of either syllable of *anbok* remain unclear. Finally, part of the difficulty in separating 'fish' from 'worm, snake' in both families suggests an earlier etymological connection between all of these words. Roots for 'fish, salmon' and 'snake, worm, eel' in ND both contain lateral anlauts and velar or uvular codas, though the two etyma cannot be linked by regular phonological rules.

Regarding 'water' I concur with G. Starostin in finding the ND + ST to be a clearer match, though I would continue to support the Yeniseian cognates, as well. The possible cognate status of basic ND and ST etyma for "water", "head", "belly", "liver" and others already identified by proponents of DC seems promising and intersects with what I have (sometimes independently) found between ND and Yeniseian. This I noted almost as a footnote in Vajda (2011a: 114); now, four years later, I see much more evidence of the need to unify my DY findings with the most current work on DC. Because this issue was a major thrust of G. Starostin's critique, I again emphasize that I agree with him.

To summarize this discussion of lexical comparisons, I agree with Campbell (2011) that a greater number of firmly supported cognates are needed. Only more cognates, if they exist, could solve many of the problems discussed above. My goal in Vajda (2011a) was to achieve a system that could be built upon — a fruitful framework for further research — and not to argue a jury verdict of "proof" to be offered up without right of appeal. That this has been successful is evidenced by the fact that G. Starostin can support certain comparisons, offer a principled rejection of others, and express specific degrees of acceptance or doubt about still others based on the system I presented. I would call this the "step forward" he referred to, if only a small step in the many that still remain to be made. Principled criticism of DY as a hypothesis is preferable to conclusive acceptance or rejection that indicates nothing new to investigate, and I hope to be the last mainsteam linguist who accepts the link as "proven". G. Starostin's informative and nuanced critique should be required reading for all who read *The DY Connection*, as it helps compensate for having only a single Ket specialist (myself) at the 2008 DY Symposium. My only genuine and uncompromising criticism of G. Starostin's critique is that he doesn't fully acknowledge the degree to which his earlier criticisms have already benefitted the DY hypothesis.

The DY volume offered no specific conclusions about time depth, and Nichols (2011: 299) rated DY relatedness as implausible on geographical grounds. G. Starostin is correct to summarize the volume's non-linguistic studies as predicated on the question, "*Supposing the Dene-Yeniseian hypothesis is correct*, is there any direct or indirect evidence from branches of science other than linguistics to confirm it?" These studies were innovative contributions in their own right, and provide crucial summations of what we currently know about North Asian/North American prehistory from a variety of additional fields. Potter's (2011) comprehensive synthesis of North Asian and North American archaeology identified the probable times of entry of new cultures into Alaska from Asia. Scott & O'Rourke (2011) showed that no markers in DNA link Modern Ket and Na-Dene populations specifically, and that shared ancestry between Yeniseians and Native Americans appears to be with all Native American populations (cf. the extremely high percentage of Y-NDA haplogroup Q1 among the Ket, which is related to the Q1a haplogroup found throughout the Western Hemisphere). Berezkin (2011) offered a pioneering survey of folklore motifs showing Ket parallels with North America, yet never exclusively with the Na-Dene. The DY linguistic hypothesis has gained a valuable broader context from this multidisciplinary approach. It has now become possible to take the assembled evidence (or seeming lack of evidence) from Mt-DNA, Y-DNA, archaeology, and folklore to argue that any direct ancestral population to contemporary Ket and ND peoples could only have existed at least 12,000 years ago as part of the late Pleistocene expansion of the Diuktai microblade hunting cultures (Vajda 2012). While this proves nothing about what language such a population might have spoken, it would be surprising if a DY language link did not coincide with this specific population link. Some Sino-Tibetan speakers also share the same defining combination of DNA markers with DY speakers (roughly speaking: Y-DNA haplogroup Q and Mt-DNA haplogroup A). And Northern China falls within the microblade cultural zone at the end of the Pleistocene, so at least this one DC family besides Yeniseian and ND can in theory be included in the same extra-linguistic scenario.

Using non-linguistic evidence to narrow down the possible time and place for a common ancestral population also has value in assessing potentially cognate vocabulary. While cognates stand or fall based on their sound correspondences, not on non-linguistic data from parallel investigations of prehistory, it is useful to pay attention to cognates with potential ecological or archaeological relevance. Certain DY cognates would seem to evoke northern forest life: wolverine, birch, conifer needles, conifer resin. But these realia are found widely in Eurasia and cannot pinpoint a DY homeland or exclude other DC families, some of which might share the same cognates. A few potential "ecological" cognates ('willow', 'birch') are problematic because Yeniseian shares them with other, genealogically unrelated Siberian families, so that some sort of borrowing almost certainly took place. The same word for 'willow' is clearly shared between Turkic and Yeniseian, probably through contact at the proto-level. It is not possible to conclude definitively that it came into Yeniseian from Turkic, however. Though there are clearly early Turkic loans in Yeniseian, there are also substrate Yeniseian river names across south Siberian Turkic territory, so that borrowing in the opposite direction, especially of words associated with forest ecology, cannot be entirely ruled out.

Archaeologically relevant cognates with a potential bearing on time depth would seem to include 'sled runner' and 'canoe'. Words for 'sled runner' plausibly derive in both families from a word meaning 'base' or 'underside', and likely have no connection with the time when snow sled technology developed. The "(in)famous" word for 'canoe' in Athabaskan resembles words in Yeniseian for 'vessel', 'boat' and was one of the look-alikes that early caught my attention. I agree with G. Starostin that the meaning of 'water craft' in Yeniseian must have developed secondarily from 'holding vessel', but since both meanings are represented across Yeniseian, the polysemy could have occurred before the breakup of Common Yeniseian. Athabaskan 'canoe' could in theory have arisen by polysemy from an earlier generic term for 'vessel'. However, there is no evidence of this, as the word is found only in Athabaskan and only in the meaning 'birch bark canoe'. Cognates in Eyak or Tlingit appear to be lacking. This in itself weakens the evidence for cognacy between Athabaskan 'canoe' and generic Yeniseian 'vessel'. But the biggest problem is that the sound correspondence linking these two words in DY is suspect and may turn out to be spurious. If so, I will be more than happy to let this vessel fill with water and sink. In any event, it increasingly looks probable that the DY language link is too old to include a specific word for 'canoe'. Genuine canoes appear on the archaeological scene long after the plausible time frame for a common DY population in North Asia had closed.

To summarize, nothing in my linguistic results so far contradicts what has been published so far by Sino-Caucasianists, though I know of no evidence from non-linguistic studies that might provide parallel support for the hypothesis that Yeniseian is closer linguistically to western DC branches (Burushaski, North Caucasian). My binary linguistic comparison of DY cannot demonstrate that Yeniseian and ND contain innovations unique to these two families when potential evidence of genealogical relationship with other families has yet to be fully calculated into the comparison. The homologies I have found might ultimately prove to be shared retentions across a larger family. I have no plans to remain attached to DY simply because I happen to have worked on it already. DY may yet turn out to be a valid taxon, or it may not (I remain non-committal on this point). If not, I suspect (for the time being on purely non-linguistic grounds, which cannot be conclusive) that Sino-Dine might instead be correct, and Yeniseian related to it as an outer branch, with any further DC relations being more distant still. But this is nothing more than speculation that follows human DNA patterns, and is *not* based on the necessary linguistic analysis. For the present, Dene-Yeniseian, Yeniseian-Burushaski and Sino-Dene are best each regarded as possible until such time as strong linguistic evidence is found to decide between them. I do not believe that lexicostatistic calculations alone can resolve such issues of language taxonomy. Because shared "quirky" morphological innovations can be of great value to subgrouping in a family, it is worth taking the trouble of looking for them — even among the thorniest templatic morphologies.

I would point out that it is not clear-cut historiographically to suggest that "Dene-Yeniseian should be put back from where it was taken: the much larger context of Dene-Caucasian", since the definition of what families fall inside "Dene-Caucasian" has evolved quite a bit in the past three decades, and even in the past four years since the time of the DY Conference. The original Sino-Caucasian hypothesis (S. Starostin 1982) linked only North Caucasian, Yeniseian and Sino-Tibetan. In later publications, subsequent to Ruhlen (1998), S. Starostin placed a question mark on the possibility of Na-Dene's inclusion in a broadened family (Burlak & S. Starostin 2001). Bengtson & G. Starostin (2012) could be called "revolutionary" for classifying Na-Dene with Sino-Tibetan and Burushaski with Yeniseian, since this reinterpretation leaves neither "classic" Sino-Caucasian nor any two of its three original members as a valid taxon. I think this merely reflects how developments in the comparison of these families have often been guided by the circumstance of uneven familiarity with the data, so that any future consensus around DC and its internal sub-branching will likely not mirror stages in how the hypothesis was investigated. It is interesting to speculate on the conclusions Edward Sapir might have drawn a century ago regarding Sino-Dene or Dene-Yeniseian had he possessed all of the Yeniseian data we currently have available, or what S. Starostin might have concluded thirty years ago from a detailed comparative description of Athabaskan-Eyak-Tlingit. The same might eventually be said about linguists working on these families today, since none of us possesses a thorough knowledge of all the languages at once, nor has anyone in history ever possessed this combined knowledge. Anything that facilitates collaboration across methodological or language family boundaries may turn out to be an important contribution in and of itself, even "technique of presentation". Ability to work collaboratively is more valuable than being "first" or "infallible" or any of the other auras that some comparativists seem to have cultivated in the past. I have often been accused of being easy to work with, but never of being infallible, and this probably bodes well in addressing the unsettled issues about DY brought up in G. Starostin's critique and elsewhere.

In evaluating Andrej Kibrik's (2011) critique of the DY hypothesis, G. Starostin argues that anyone presented with proper facts can evaluate a hypothesis of language relatedness. He is certainly correct, or else there would be no science of historical-comparative linguistics. But I wholeheartedly empathize with any reader who chooses instead to defer to more authoritative judgment when faced with a publication claiming new evidence of language relatedness. There are only twenty-four hours in a day, and usually far fewer than that. Why would anyone to devote the time needed to mastering new, complex, and arcane comparative data, let alone offering a principled judgment of it in print, when painstaking criticism by leading experts regarding new language relation claims has so often been ignored? Anyone who has slogged through my Siberian link article is probably heroic, and those who have taken the considerable time and effort to criticize it are truly admirable. Unfortunately, good work — perhaps much better work than mine — can languish uncommented in the general situation that has developed in comparative linguistics.

On another human note, I think that the idea of "discovering" or "proving" a language family has been greatly over-glamorized. Again and again I have had to stress that DY is built on the work of many linguists and represents a promising hypothesis worthy of the future collaboration required to advance it. My first book on Yeniseian (Vajda 2001b) was a historiographic treatment of over 1,500 publications that appeared before my own Ketological research. All of these studies informed my own in some way. I am neither "discoverer" nor "prover" of DY, but merely one of many linguists who have made a contribution. Native speakers, not linguists, establish language families. Anyone who would still insist on a linguist-centric approach to comparative linguistics should first examine the extensive bibliography in Vajda (2001b: 357–359).

Language relatedness is only one of many facts in the history of languages. No less important (and perhaps more important) are such things as detecting a layer of loanwords, identifying reanalyzed vestiges of possessive affixes, or solving the problem of how a conjugation system arose — though news headlines will probably never be written about any of this. Consensus that Athabaskan, Eyak and Tlingit is a valid family developed quietly, as the inevitable result of several decades of "unglamorous" work reconstructing classifier prefixes, tense-mood suffixes, labialized velars, and finally a set of Proto-ND palatals. Future acceptance of language families will accrue in the same way — from years of careful investigation into all facets of language history — work often done not in pursuit of language relationship as a primary goal. Only this sort of research can discover a family's system of shared characteristics or the shared innovations that uniquely define each of its branches. Maybe the best way to demonstrate a language family is not to try so hard.

Abbreviations

DC — Dene-Caucasian; DY — Dene-Yeniseian; (P)EA — (Proto-)Eyak-Athabaskan; (P)ND — (Proto-)Na-Dene; PA — Proto-Athabaskan; PY — Proto-Yeniseian; ST — Sino-Tibetan.

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В статье дается подробный критический разбор т. н. «дене-енисейской» гипотезы — предположения о генетическом родстве языковой семьи на-дене в Северной Америке и енисейской семьи в Сибири (сегодня представленной всего одним оставшимся в живых потомком — кетским). Эта гипотеза получила широкую огласку в результате исследований Эдварда Вайды и была поддержана рядом крупных специалистов, но, как подчеркивает предлагаемая вниманию читателя статья, до сих пор не была подвергнута детальному критическому разбору, в центре которого находились бы непосредственно сравнительные языковые данные Вайды. В статье предпринята попытка хотя бы частично исправить положение, дав такой разбор для сравнительной глагольной морфологии, отдельных фонетических соответствий и базисной лексики, задействованной в сравнении Вайды. Автор приходит к выводу, что критическую проверку выдерживает лишь часть сопоставлений, сама по себе недостаточная для того, чтобы доказать «дене-енисейское» родство как таковое. Тем не менее, соответствующие сопоставления могут быть полезными для дальнейшего сравнительного изучения на-дене и енисейских языков как возможных составных частей более крупной таксономической единицы — «дене-кавказской» макросемьи, в рамках которой эти две ветви, возможно, в конечном итоге окажутся родственными друг другу на более глубоком уровне.

Ключевые слова: дене-енисейская гипотеза, дене-кавказская гипотеза, на-дене языки, енисейские языки, лингвистическая макрокомпаративистика, дальнее языковое родство, глагольная морфология, типология фонетических переходов.