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 evi-

dence 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

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

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 **-ŋʸi* is found only in resultatives, though the suffix **-ŋʸi* 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 **ɢa-* 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* ~ **Ga* 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·l^{j}*	*ura*	*kur*	*uŕga*
**piLaŋ* 'sweet'	*hilaŋ*	*fɔraŋ*	*falaŋ ~ p^{h}alaŋ*	*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 *l^{j}* corresponds to Kott, Arin and Pumpokol *r* have a morphophonemic explanation: the Yugh liquid in *ū·l^{j}* '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·l^{j}* '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 **ŋ^{j}* 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 **n^{j}a-*. 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 **pʰāj* '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 **pʰāj* '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 **pʰāj* were it not for the unexplained first syllable *yu-*. Also conceivable is an etymological connection between PY **pʰāj* '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 + ~*η^{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 ~*η^{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^{y}e/i(·)η’ ‘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^{h}ad* ‘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

[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 **ɬu·q’-e·* ‘fish, salmon’ > Modern Ahtna unpossessed *ɬu·q’e·* but possessed *-lu·ge·*. Leer further suggests that the unsuffixed root PA **ɬu·q*’ gave rise to the form PA **ɬû·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 *tun^{j}il ~ tun^{j}ɔl* ‘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 *ban^{j}r* ‘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 **-ʦ'inG*, Tlingit *-tɬ'iG*) 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 *čɨ: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 *tès*, Yugh *čɛ: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 cul-

tural 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.