Cognate maximization versus cognate minimization: in search of a "golden middle" for Altaic etymology

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It is extremely gratifying for me to be able to dedicate this paper to the memory of Eugene A. Helimski. Although his principal scientific interests always resided in the field of Uralic linguistics, as one of the members of the Moscow School of comparative linguistics, Dr. Helimski always took a keen interest in both specific and general issues of long-range comparison, and had made important contributions to Altaistics (e.g. Helimski 1986a, 1986b) as well as the general methodology of macro-comparative linguistics (particularly important in that respect is his critical evaluation of overpermissive semantic standards in these studies, which has significantly influenced my own views on the subject, see Helimski 1987). His extremely sober, yet generally sympathetic attitude to such hypotheses as Altaic and Nostratic was almost unique among his colleagues, most of whom tended to sway too far in either of the two opposite directions, and this is why I have a feeling that, out of all my former professors and senior colleagues with whom I have had the honor to study and work over the years, he might have been particularly close to sharing the concerns and ideas expressed in this paper, had he still been with us today.

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While the hypothesis of (linguistic) genetic affinity between several major stocks of Central and Far Eastern Eurasia commonly known as "Altaic", contrary to rumors, is still well alive and kicking today (Robbeets 2017), it has also to be acknowledged that in general Altaic (or "Transeurasian", to use the more recent term which is actively promoted by Martine Robbeets and some of her colleagues) linguistics currently remains at an impasse. Despite obviously widespread skepticism about the idea that observed correlations between Turkic, Mongolic, Tungusic, Koreanic, and Japonic languages should be explained in terms of a common linguistic ancestor rather than prolonged areal contacts, active re-

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search on the hypothesis still goes on in at least a few scientific hubs around the world (such as Moscow's Institute of Linguistics, or the Max Planck Institute at Iena). Yet it may be argued that few, if any, significant methodological breakthroughs in Altaistics have been achieved over the past 15 years — breakthroughs, that is, which could decisively turn the wheel in one or the other direction and result in either abandoning the hypothesis for good or, conversely, producing a unified working model of the Altaic theory which could serve as a base reference point for generations of future scholars to come.

The present paper is a humble attempt at explaining the reasons underlying the lack of such breakthroughs and offering, at least speculatively, a possible direction of study which present day and future Altaicists might want to consider if they truly care about making the field more robust and the theory itself more credible to skepticists — or, at least, more falsifiable from a Popperian point of view and, therefore, easier to confirm or rebuke on a permanent basis. Considering that the Altaic hypothesis is one of the best known and most promising cases of so-called "long-range comparison" — theories of genealogical relationship between such distant linguistic taxa that application of the standard Neogrammarian comparative method becomes controversial — any progress or lack thereof in this field should have significant theoretical and methodological implications for all similar hypotheses.

One of the first things that comes to mind when briefly surveying the history of the Altaic debate over the past decade and a half — much of which was inevitably focused on the release of the massive *Etymological Dictionary of Altaic Languages* (EDAL) by Sergei Starostin, Anna Dybo, and Oleg Mudrak in 2003 — is that the methodological positions occupied by opposing sides tend to veer towards what might be dubbed "linguistic extremism", i.e., a hyper-strict, almost dogmatic adherence to strategies which are more likely to help gain (or lose) subjective supporters of the hypothesis than to construct an approximation of genuine linguistic prehistory of the families in question.

In the case of the so-called Moscow school of comparative linguistics as specifically represented by the authors of EDAL, this position is perhaps best illustrated by a telling turn of phrase in the middle of the "Introduction" to the dictionary: "The very fact that it is possible to compile a dictionary of common Altaic heritage appears to be a proof of the validity of the Altaic theory" (EDAL: 9). Justifiedly picked upon by some of the critics of EDAL such as Alexander Vovin (2005: 73) and Stefan Georg (2004: 449), this statement in itself, especially when taken out of context, suffers from rather unfortunate wording, given the huge amount of "dictionaries" published in support of all sorts of bogus hypotheses of linguistic relationship over the past two centuries. Naturally, what the authors really meant was that the dictionary in question represented an etymological corpus, tightly bound together and validated by regularly observed sound laws — but another veiled implication may have been the sheer size of the dictionary: 2800 etymologies, at least several hundred more than, for instance, the total number of etymologies included in

Julius Pokorny's classic dictionary of Proto-Indo-European (1959), and at least twice as many as may be found in the most extensive previously published Altaic comparative corpora by Gustaf Ramstedt, Nicholas Poppe, etc.

Such a result may be seen as counterintuitive: given the inevitable accumulation of loss of cognates over millennia, an etymological dictionary for a family that is, according to a general Altaicist consensus, *older* than Indo-European, should hardly be expected to yield more comparanda than its much younger counterpart. This becomes even less comprehensible if one considers that Altaic consists of but five roughly equidistant branches, whereas Indo-European comprises at least a dozen or so (meaning that out of its 2000+ etymologies, quite a large number may rather represent post-Proto-Indo-European areal innovations). To use a particularly instructive piece of statistics, EDAL contains 1115 isoglosses between the two most remote branches of the family (Turkic and Japonic); the number of isoglosses between two geographically and phylogenetically comparable members of Indo-European, Old Indian and Latin, found in Pokorny's dictionary, fluctuates somewhere in between 600–650, depending on the strictness of etymological criteria.

How has it been possible to achieve such an impressive result? Analysis of the actual material shows that it is largely due to the authors adopting the strategy of *cognate maximization*, which is never stated explicitly but, based on a general overview of the corpus, may be roughly summarized as follows: "If lexical root R_1, found in languages $L_{1.1}$...$L_{1.n}$ of language family F_1, phonetically corresponds to and shares a common semantic component with lexical root R_2, found in languages $L_{2.1}$...$L_{2.n}$ of language family F_2, then lexical roots R_1 and R_2 will be judged as cognates and united in a single etymological nest, regardless of any other potential solutions".

On the surface, this chosen strategy seems to match classic Neogrammarian prescriptions and could, in fact, be said to underlie any well-respected "traditional" etymological dictionary — supporting the claims of many of the Moscow School's long-rangers, beginning with Vladislav Illich-Svitych and Aharon Dolgopolsky, that there is no significant methodological difference between "long-range" and "short-range" comparison other than "long-range" comparison being dependent on the results of "short-range" comparison in that the objects of comparison are protolanguage reconstructions rather than data from living or historically attested languages. In reality, things are much more complicated.

In his frequently unacademic, but often justified criticism of EDAL, Alexander Vovin (2005) gave quite a few examples of Altaic comparanda which seem to play fast and loose with at least two parts of the above definition. The first is the requirement of a common semantic component: Vovin lists many examples of semantic connections which seem typologically improbable or at least require extensive justification. Naturally, one might argue that semantic shifts should be expected to be both more frequent and less predictable across distantly related lineages; still, whenever one sees an etymology which attempts to relate, e.g., Mongolic **(h)alag-* 'jerboa', Tungusic **(x)algi-n* 'male otter', and Japonic **irúká*

'dolphin' (EDAL: 289), the question immediately springs to mind of how many semantically far-flung connections like this may be accidentally generated by comparing dictionaries, and whether it would be at all possible to find a proper, convincing way to separate true etymologies based on such extraordinary semantic connections from fake ones. Similar concerns have been voiced in other reviews of EDAL as well, both those written from a generally anti-Altaic standpoint (e.g. Georg 2004) and a pro-Altaic one (e.g. Miller 2004, Stachowski 2005).

The second and arguably even more important part is that of the "lexical root R_1, found in languages $L_{1.1}$...$L_{1.n}$". It should be immediately clear to any practicing comparative linguist that such a definition, despite being followed to a tee in EDAL, is nowhere near the same as the definition of *reconstructibility* of lexical root R_1 to the topmost level of language family F_1 (Starostin 2016). Thus, if we take the Tungusic material in EDAL, we shall find that, out of 2435 etymologies, about 200 only have reflexes in one single language; 81 are Even-Evenki isoglosses; around 300 are only found in the Northern branch of the family, etc. Again, in each single case it is perfectly possible that the original root simply happened to be retained in only one branch or, sometimes, even in only one language of the family. But without a proper supportive criterion to help us assess the chances of this being true from *within* the Tungusic family rather than through the exclusive application of the criterion of external comparison, how can we protect ourselves from the danger of a drastic increase in chance similarities when, instead of limiting ourselves to the relatively small corpus of reliably reconstructible items, we expand the basis of comparison to isolated languages as well? Unfortunately, no safeguards at all are provided in EDAL.

There are also other, more subtle ways, in which the cognate maximization strategy may be consistently exploited. One of these is the idea of *contamination*, when an annoying phonetic irregularity in the reflexes of a certain root is explained away by purely hypothetical influence on the part of another, phonetically and semantically close, root; and the idea of *merger*, when a seemingly single root is split into several different ones merely because it has several potential external correlates. A good example of the latter is the separation of Proto-Turkic **jāĺ(-ıl)* 'green' and **jāĺ* 'fresh, raw' into two different roots — something which looks very strange from the point of view of standard Turcology as well as semantic typology, given the extremely frequent connections between these meanings across the world's languages; in EDAL, however, the first form is deemed cognate with Mongolic **ǯöl(ü)ge* 'green meadow' and Tungusic **ńoli-* 'greenish, bluish; green moss' and traced back to Proto-Altaic **ńiōĺe* (EDAL: 1015), whereas **jāĺ* 'fresh, raw' is compared with Mongolic **nilaɣu* 'raw', Tungusic **ń(i)ali-* 'raw' and Koreanic **năr* id., yielding a different Proto-Altaic root: **niāĺi* (EDAL: 985). In my opinion, strict analysis of the semantics and distribution of cognates shows that the second etymology, inherited by EDAL from much earlier comparisons by Ramstedt and Poppe, is quite robust (all the items are relia-

bly reconstructible for Proto-Turkic, Proto-Mongolic, and Proto-Tungusic in the exact same meaning 'raw'), whereas the first etymology — an EDAL innovation — is far more questionable semantically, and its components are nowhere near as easily reconstructible for the Proto-Mongolic and Proto-Tungusic parts of the equation; EDAL, however, treats them as essentially equal, and the contradicting stance that it takes here against typology of semantic change may only be explained by the persistent application of the cognate maximization strategy.

Precisely the opposite methodological approach has been selected by some of the most ardent present day critics of the Altaic hypothesis such as, e.g., Stefan Georg (2000, 2004) and Alexander Vovin; we may respectively call it the *cognate minimization* strategy, though, in all fairness, it eventually results in a *cognate elimination* strategy. Based on their criticism of EDAL as well as other studies in Altaic or Altaic-related (e.g. Koreo-Japonic) etymology, this strategy may be briefly defined as follows: "If and only if lexical root R_1, reconstructible to the topmost level PL_1 of language family F_1, phonetically corresponds to and is completely or at least near-completely semantically identical with lexical root R_2, reconstructible to the topmost level PL_2 of language family F_2, *and* if it has been successfully demonstrated that the proximity of lexical roots R_1 and R_2 cannot be the result of linguistic contact, then lexical roots R_1 and R_2 may be judged as cognates". In other words, the cognate minimization strategy requires a tremendous burden of proof on the part of the etymologist — not only do the phonetic and semantic correlations between comparanda have to be virtually impeccable right down to the tiniest details, but one must also know how to decisively eliminate borrowing as a potential explanation before daring to make an actual cognacy judgement.

These methodological principles are richly and accurately illustrated in Vovin's lengthy critical review of EDAL (2005) and, perhaps, even more so, in his monograph-size critical examination of John Whitman's binary comparison between Koreanic and Japonic (Vovin 2010). Many specific counter-criticisms to these works have already been published (e.g. Dybo, Starostin 2008; Starostin 2013), so there is no need to repeat them here; suffice it to say that, in this author's opinion, consistent application of the cognate minimization strategy will always be enough to invalidate not only the Altaic theory, but just about any "long-range" hypothesis ever produced, since the evidence on such deep levels will by definition always be less robust than evidence for phylogenetic connections on levels such as Indo-European.

It would be far-reaching to call either of the two strategies completely useless. Cognate maximization may and will occasionally result in fresh, innovative ideas that sometimes rightfully challenge accepted conventions on reconstruction, whereas cognate minimization, taking scientific skepticism to the extreme, may and will provide reasons why certain etymologies should be unquestionably abandoned, as well as point out possible ways of improving certain others (for instance, justifying dubious semantic connections

with data from the field of semantic typology, or looking more closely into the issue of reconstructibility of certain items on intermediate levels, e.g. explaining why a certain isolated Evenki word may or may not be traced back to Proto-Tungusic). However, one thing which the two strategies have in common is *oversimplification*. Cognate maximization tends to place too much trust in phonetic correspondences, virtually ignoring, among other things, the semantic side of the comparison; cognate minimization, on the other hand, tends to ignore the qualitative differences between various etymologies, rejecting them indiscriminately regardless of the respective degree of seriousness of the observed problems (e.g. it seems rather clear that the semantic distance from 'jerboa' to 'dolphin' is much larger than, say, the distance from 'jump' to 'run', but consistent application of the cognate minimization strategy may simply reject both comparisons as "semantically problematic").

Among other problems, the cognate minimization strategy simply refuses to acknowledge the possibility that, due to the chronological factor, producing a completely "waterproof" Neogrammarian model of phonetic correspondences for a macrofamily older than Indo-European may be technically impossible — leaving the researcher before the choice of either abandoning any macro-comparative research for good or trying to find alternatives, perhaps by way of introducing "relaxed" models with emphasis on regularities between some parts of the system and clearly defined "fuzzy zones" where multiple equiprobable, but essentially unprovable, historical scenarios of development could have taken place.

In any case, it seems clear enough that some sort of middle ground is necessary in order to move Altaic — or any other macro-comparative — linguistics away from this stalemate and put it back on a progressive path which could preserve all the benefits of both strategies without succumbing to their excesses. Unfortunately, while in theory many scholars would probably subscribe to such an appeal, in practice there is no clear and easily acceptable recipe for such a "golden middle" strategy.

Arguably the most focused and successful work in establishing such a "golden middle" in the post-*EDAL* era has been conducted by Martine Robbeets, a firm adherent of the Altaic hypothesis who has, nevertheless, been openly and highly critical of the cognate maximization strategy. In her important monograph on the subject (Robbeets 2005), taking Japanese as her basic point of departure, she establishes a large number of specific parameters based on which she analyzes all of *EDAL*'s etymologies with Japanese reflexes word for word, rejecting more than two-thirds of the evidence as conclusive but still singling out a limited corpus of 359 etymologies as definitive proof for a genetic affinity between Japanese and the other Altaic languages. According to Robbeets herself, "the sifting process rules out entries for which internal evidence contradicts the proposed etymology, similarities due to general properties of languages, contact induced similarities and etymologies with problematic semantics" (p. 422).

Upon first glance, this approach might seem to be precisely the kind of "golden middle" that is being advocated in the present paper. However, while on the whole this advo-

cation of the "quality over quantity" principle is intuitively quite welcome, there are two major problems with Robbeets' solution. One of them, easily exploitable by anti-Altaicists, has to do with the lack of a strict, clearly defined threshold which separates the "good/acceptable" etymologies from the "bad/unconvincing" ones. While nobody would probably argue that some of the Japanese-Altaic parallels in EDAL may be put in doubt by the existence of alternate internal etymologies, or that others suffer from "semantic overpermissiveness", neither Robbeets nor, in fact, any other pro- or anti-Altaic scholar, has ever given a clear definition of what precisely consitutes "semantic overpermissiveness" or of when precisely internal etymologization should take precedence over potential external connections. Two examples on each parameter will suffice here:

1) Robbeets disqualifies the comparison of Proto-Japonic **pànà* 'flower' with other potential Altaic comparanda based on the suggestion that the word in question *may* be segmented historically as **pa-* 'red' + **na* 'plant'(Robbeets 2005: 84). But even if we accept the historical reality of **pa-* 'red', a root that is never attested individually and is only extracted hypothetically from other potential compounds such as **pa(-)ni* 'red clay', etc., it is never explained why 'red' should have been established as the default color for the generic term 'flower' — flowers, both in Japan and anywhere else, typically come in all colors, and to the best of my knowledge, there is no serious typological evidence, at least for Eurasia, which could confirm such an odd semantic development; much more common is derivation from verbs such as 'to grow', 'to bloom' (e.g. in Indo-European; see Mallory, Adams 1997: 207), as well as expression of this meaning by a primary nominal root.

On the other hand, for instance, the Japanese word for 'shadow' (*kage* ⟵ Proto-Japanese **kanka-*, p. 404) is considered perfectly acceptable, despite the fact that a potential internal etymology for this word had also been suggested, e.g. by S. Martin, who tentatively derives it from **ka-n/i/-ka*, lit. 'place of day', where both morphemes with the shape **ka* go back to two equally hypothetical monosyllabic roots (Martin 1987: 432). While I find such a segmentation just as suspicious as the one for 'flower', largely based on typological grounds, it is hard to understand what it is exactly which makes the first internal etymologization acceptable to Robbeets and the second one not even worth mentioning.

2) In the sphere of "semantic overpermissiveness", Japanese *te* 'hand' is denied cognacy with the Mongolian word for 'wing, shoulderblade' (p. 278), even if technically both terms could be derived with a single semantic shift from the original meaning 'arm'. On the other hand, Japanese *kobusi* 'fist' is listed as a potential cognate for Proto-Tungusic **kombo* 'wrist, hand, spoke-bone' (p. 401; more precisely, 'back of hand' or 'metatarsus' rather than 'hand' proper) without any semantic problems — despite the fact that a metonymic transition from 'wrist' to 'fist' is nowhere near as self-evident or confirmed by typological evidence as it might seem.

These two examples are in no way dismissive of Robbeets' thorough and extremely useful critical analysis of Altaic etymologies on the whole, but simply illustrate the casual difficulties which always accompany "good intentions" when said intentions are not accompanied with uniform, clearly stated rules of the game. Those subsequent reviews of her work which were negative rather than positive did not fail to capitalize on this circumstance, and efficiently pointed out that many of the 359 "waterproof" etymologies which remained after sifting the evidence are not at all immune to further and further sifting — until practically nothing remains (Vovin 2009).

There is yet another problem with Robbeets' solution, this one more relevant, perhaps, for proponents than opponents of the Altaic theory. While I do agree that, in general, it is quite important to be able to reduce available comparanda to a core group of etymologies which demonstrate beyond reasonable doubt that the Altaic macrofamily is real, the final product of the field of comparative Altaic studies is still expected to be a representative collection of etymologies that would, ideally, cover a large number of semantic fields, including basic as well as cultural lexicon which would allow scholars to more firmly assign "Altaic" to a specific time and place in prehistory. This means that while reductionism may be a solid strategy when it comes to "proving" Altaic, it is essentially unusable when it comes to making head or tails of "Altaic" as a meaningful historical phenomenon. And while an etymological dictionary of 2800 entries, generated under the cognate maximization strategy, is most likely overkill, the opposite strategy of reducing Altaic to 350 etymologies or so makes it look as if we might be intentionally refusing to make the most of the available means of comparative-historical linguistics.

In the remaining part of the paper, I would like to briefly talk about one such available means, a proper understanding and utilization of which, I believe, could vastly improve both our handling of the comparative data and the clarity of its presentation — namely, what goes today in certain scientific circles under the name of *onomasiological reconstruction* (Jäger, List 2018), i.e. the idea of matching reconstructed phonological shapes of lexical (or, for that matter, grammatical) morphemes with precise, discrete semantic concepts rather than vague meaning clouds. Unlike the usual practice, which generally sees no formal limit to the number of potentially reconstructed morphemes and pays much more importance to the regularity of phonetic correspondences than to accurate marking of semantics and explanations of meaning shifts, onomasiological reconstruction takes semantic concepts as a starting point — essentially, it regards the proto-language inventory as a more or less fixed set of pre-determined "semantic slots", each of which should ideally be occupied by one and only one reconstruction.

When it comes to well established families such as Indo-European, the onomasiological approach may be seen as not really necessary to either "prove" Indo-European or assemble an impressive corpus of Indo-European etymologies — largely because the phonetic and semantic distance between ancient Indo-European languages is relatively small — al-

though applying it to Indo-European could still be beneficial in that it could help distinguish between the various quasi-synonyms in Indo-European reconstruction, as well as draw a much clearer line between those roots which are truly reconstructible for Proto-Indo-European and those which represent later areal innovations. However, I believe that the true potential of onomasiological reconstruction could really shine when it comes to more far-flung macro-comparative hypotheses such as Altaic. In other words, reframing such questions as "is the phonetic and semantic connection between such-and-such morphemes in Turkic, Mongolic, and Japonic acceptable?" into something like "which specific concept could have been denoted by these phonetically-and-semantically similar morphemes in Turkic, Mongolic, and Japonic?" probably could bring about a whole new development in Altaic linguistics.

Once again, a small illustration will suffice. Taking Indo-European as a starting point and focusing, for instance, on the basic semantic slots within the general semantic field HEAD, we may with almost absolute certainty reconstruct the principal elements of that field as **ḱerH-* 'head', **okʷ-* 'eye', **Hows-* 'ear', **nas-* 'nose', **HoHes-* 'mouth', etc. Although many of the individual languages have replaced the reflexes of these roots with innovations, and although said reflexes are not always completely regular in the Neogrammarian sense, and certain phonetic components of the reconstruction are questionable, there is plenty of evidence to suggest that these particular phonetic shapes and no others served as the principal (stylistically unmarked) "carriers" of the corresponding meanings in Proto-Indo-European (see Mallory, Adams 1997 for confirmation). The very fact that we can successfully suggest and defend historical scenarios which corroborate this idea is a strong argument in favor of the historic reality of Proto-Indo-European itself.

When trying to do the same onomasiological reconstruction for Proto-Altaic, we instantly run into problems, since not a single one of these concepts is represented by cognate sets that would be sufficiently well distributed across all of its five branches. This glaring difference from Indo-European may mean one of two things — either Proto-Altaic is a phantom construct, or it is significantly older than Indo-European, which would explain a higher degree of cognate loss. But how could the methodology of onomasiological reconstruction help us understand which is the better solution? To gain such an understanding, it might be helpful to adopt a systematic approach which takes a look at potential cognates as well as non-cognates.

Let us illustrate this on the example of the actual word 'head'. The respective onomasiological reconstructions for the five branches of Altaic are Proto-Turkic **baĺč*, Proto-Mongolic **heki* or **tolu-gai* (here it is difficult to make a single choice), Proto-Tungusic **dili*, Middle Korean **məri*, Old Japanese *kasira* (all forms cited after EDAL). Of these five (or six, considering the ambiguous situation in Mongolic) etyma, two are etymological cognates according to the EDAL model — Proto-Turkic **baĺč* and Korean **məri*: they share the exact same semantics and can be regularly traced back to an Altaic prototype

like **meĺ-* (in EDAL: 910, the proposed reconstruction is **mélĺžu*, based on the addition of Mongolic **malža-* 'bald' whose inclusion is highly questionable on semantic grounds)[1].

If we assume that Turkic and Korean forms are indeed retentions from Proto-Altaic, then all the other forms must be innovations, and thus, require explanations. For the Tungusic form **dili*, such an explanation is implicitly provided in EDAL: 476, where it is compared to Mongolic **žilua* 'brain' and Turkic **yulɨŋ* 'marrow', indicating a possible semantic shift 'brain' → 'head' (as well as 'brain' → 'marrow' in Turkic); the probability of such a development is faintly supported by the fact that such a semantic shift would later once again take place within the Tungusic family, namely, in Manchu, where *užu* 'head' ← Proto-Tungusic **irgü* 'brain'.

Mongolic **heki* 'head' is compared in EDAL: 1130 to the somewhat vague Proto-Tungusic reconstruction **pe:yKe* 'brain / forehead / top of the head', which is itself assembled from Narrow Tungusic (i.e. Northern and Southern Tungusic without Manchu) **pe:ye* 'forehead' and Manchu *fexi* 'brain'. Since this consonantal cluster is fairly unique, it makes more sense to regard the form as original **pe:ye* 'forehead', with a suffixal derivative in Manchu — **pe:ye-ki* → *fexi* 'brain'[2]. Regardless of whether the complex Manchu form is a recent innovation or an archaism, the Mongolic form **heki* itself may be tentatively analyzed as **he-ki* (perhaps containing the well-known nominal 'converter' suffix **-kI*), in which case the first syllable **he-* is a perfect match for Proto-Tungusic **pe:ye*. This, in turn, gives us a solid candidate for Proto-Altaic 'forehead', with derivational semantic shifts to 'brain' or 'head' across different lineages. (Addition of Middle Korean *pákì* 'top of the head' to the same etymology is dubious: this form, encountered exclusively in compounds with meanings such as 'crown of the head' and 'top of a hill', seems to have the general meaning 'top' rather than specifically 'forehead', see Lee 1958: 109).

Finally, if EDAL's comparison between Japonic **kàsìrà* 'head' and Korean **kór* 'brain' (EDAL: 660) is phonetically justified (according to EDAL's general model, the two words may go back to **kVĺi*, with additional suffixation *-ra* in Japonic), this may hint at yet another 'brain' → 'head' (derivational) shift, although in this case Proto-Koreo-Japonic **kVĺi* 'brain' would be opposed to "Narrow Altaic" **dilu* 'brain', requiring its own explanation — but not at all contradicting the usual models of Altaic phylogeny, in which Koreo-Japonic is typically opposed to "Narrow Altaic" as, arguably, the first branch to split off.

This single example, in my opinion, illustrates fairly well the benefits of onomasiological reconstruction. In a cohesive, relatively well-rounded scenario we have not only

[1] Vovin (2000: 143) reconstructs the original Korean form of the word as **matay* or **matæ*, based on the transcription of this item as Middle Chinese *ma-tjej* in the *Kyeylim Yusa*,but this isolated case cannot be regarded as definitive proof that the original consonant was a stop rather than a sonorant.

[2] This reconstruction is further supported by the existence of Proto-Samoyed **päyå* 'forehead', quite possibly an old borrowing from a Tungusic source (Anikin, Helimski 2007: 126).

found a suitable candidate for Proto-Altaic 'head' (the isogloss between Turkic and Korean), but also defined several potential scenarios for the evolution of this concept in some of the daughter branches, found suggestive evidence for a widespread semantic shift 'brain' ⟶ 'head', and built up a case for not one, but three precise onomasiological reconstructions (**meĺ-* 'head', **dilu* 'brain', **peye* 'forehead'). Surprisingly, this relatively simple procedure, which tidies up the comparative evidence and sets up precise scenarios of historic evolution, is missing in the majority of etymological studies of Altaic.

It goes without saying that cases will differ depending on the amount and quality of preserved historical evidence, but as long as probable scenarios without internal contradictions may be generated, this issue is not fatal. To illustrate, let us consider a slightly more problematic case — that of the meaning 'ear'.

Within EDAL, there are no fewer than 4 items whose semantic definition on the Proto-Altaic level involves the component 'ear', namely, (1) **čʻi̯[i̯a]kʻi* 'temple; ear' (p. 437), (2) **kʻŭ̈jlu* 'ear; to hear' (p. 847), (3) **màjŋì* 'temple, forehead, ear' (p. 895), (4) **zi̯ə̀ni* 'ear, temple' (p. 1517). Again, this is by no means the result of semantic, let alone onomasiological, reconstruction, but merely a concatenation of the most commonly attested hypothetical semantic reflexes of these roots in daughter branches — understandable, since each of these four roots functions as the default equivalent for 'ear' in at least one of these branches, namely, (1) Mongolic **čiki*, (2) Turkic **Kul-kak*[3], Korean **kúi*, (3) Japonic **mìmì*, (4) Tungusic **sian*.

Let us first consider etymology (2), since it is the only one which links together basic items from at least two branches (curiously enough, the exact same ones — Turkic and Korean — which, as we have just seen, may have also preserved the Proto-Altaic item 'head'). Common Turkic **Kul-kak* 'ear' is clearly a derived formation, with the old suffix **-kak* hinting at a probable verbal origin (as in, e.g., **bat-kak* 'swamp' ⟵ **bat-* 'to sink'), though no traces of an original verb **kul-* 'to hear' are found in Turkic. It is important to note, however, that Common Turkic **ẹĺit-* 'to hear' is compared in EDAL with such forms as Korean **ār-* 'to know', etc. (p. 293), and may be constructed as a semantic innovation, which strengthens the case for **kul-* as the original 'hear', preserved only within its nominal derivative.

That **kul-* may have been not only the pre-Proto-Turkic, but also the main Proto-Altaic verb meaning 'to hear' may be argued on the basis of the Tungusic and Japonic parts of the comparison: cf. Proto-Tungusic **xuːl-* 'to (re)sound' (easily traceable back to a passive 'to be heard'), Japonic **kí-k-* 'to hear' (loss of **-l-* in Japonic is regular). Whether the same root also expressed the meaning 'ear' is, however, debatable. We may reasona-

[3] Capital **K-* in the EDAL reconstruction implies that the original consonant could have been either **k-* or **g-* (the EDAL model reconstructs such a phonological opposition for Proto-Turkic, although it is not commonly accepted in Turkology).

bly hypothesize that Middle Korean *kúi* is historically segmentable as *kú-i* and ultimately goes back to **kul-i* (with the same regular loss of **-l-* as in Japonic), but if so, the deverbal suffix is clearly different from the one in Turkic **Kul-kak*, meaning that both nouns could have been formed in (pre)-Proto-Turkic and (pre)-Old and Middle Korean independently.

Let us now analyze the other candidates. Proto-Mongolic **čiki* 'ear' is compared in EDAL with Common Turkic **čęke* 'temple (of head)', an old Altaic etymology already found in works by Ramstedt and Poppe; other parallels (in Tungusic and Korean) are much weaker from the distributional and semantic viewpoints. Semantic correlation between these two meanings is well known in the region, with words meaning 'temple' typically derived from 'ear' (e.g. Tungusic **sian* 'ear' → Even *heː-čeːn* 'temple', etc.), which implies that, if the Turkic and Mongolic forms are genetically related, the meaning 'ear' should be primary, with a semantic shift in Turkic. However, if, according to most classifications, Turkic and Mongolic form a separate subclade within Altaic, this only means that Mongolic **čiki* may be as archaic in the meaning 'ear' as the chronological stage of Proto-Turco-Mongolic rather than Proto-Altaic.

The Japonic form **mìmì* 'ear' is compared in EDAL with Turkic **beyŋi* 'brain' and Mongolic **maŋlai* 'forehead'. While the latter two forms may very well be correlated (see above on the 'brain'/'forehead' correlation in other Altaic etyma), their further connection with the Japanese word is extremely dubious; at the very least, it features a "missing link" in the form of meanings like 'temple, side of head' and would require postulating not one, but two important semantic shifts with no evidence to back it up. This is likely to be an accidental resemblance, and the Japonic word itself, with its rather peculiar phonetic shape, may not be of Altaic origin altogether.

Finally, Tungusic **sian* 'ear' is compared in EDAL with Turkic **yąyŋak* 'cheek' and Mongolic **sinaɣa* 'temple; cheekbone'. The comparison, particularly in its Mongolic part, might have been acceptable if not for the fact that **-n* in the Tungusic form is almost certainly a fairly standard nominal suffix, while the original root is simply **sia-*, encountered without the nasal in quite a few other Common Tungusic derivates, e.g. **sia-kan* 'earring', etc. The ultra-short CV-type structure of this root might be an indirect hint at its relative archaicness, but it also makes the search for a reliable Altaic etymology particularly difficult, since accidental look-alikes await around every corner in such situations.

Interestingly enough, the Tungusic form **sia-kan* itself is present in EDAL, but only as part of a separate etymology, **siyu* 'ear-ring' (p. 1245), which also includes such reconstructions as Proto-Turkic **sɨrga* id. and Proto-Mongolic **süyi-ken* id. However, it is *a priori* extremely improbable that such a deeply cultural term as 'ear-ring' could have survived from Proto-Altaic in any of its daughter branches. On the other hand, provided the phonetic correspondences between Tungusic **sia-*, Mongolic **süyi-*, and Turkic **sɨ-* are indeed regular, and provided the historical separation of Turkic **-rga* and Mongolic **-ken* as suffixes is admissible (both of these conditions may be put under doubt, of course), we

could interpret these parallels as evidence for the survival of the original nominal root for ‘ear’ in Turkic and Mongolic within independently formed compounds.

Summing up this etymological investigation, we have: (a) established a solid and optimal candidate for the Proto-Altaic verbal meaning ‘to hear’ (**k'ü̯jlu* according to EDAL’s model of Altaic phonology), while at the same time placing under heavy doubt the idea that the same root could also function as the basic equivalent for the nominal meaning ‘ear’; (b) found a probable candidate for Proto-Turco-Mongolic ‘ear’ (**č'[i̯a]k'i*, according to the same model); (c) left open an investigative path into the origins of Tungusic **sia-* as a possible carrier of the nominal meaning ‘ear’ in Proto-Altaic. Clearly, these conclusions are not as encouraging as the semantic disentanglement presented above for ‘head’, but they still seem to introduce much more clarity into the overall situation than the way the evidence is presented in EDAL.

This is not to say, of course, that there may be no further objections raised against these types of scenarios. For one thing, the specific etymologies involved are not above criticism (see, e.g., the above footnote on Vovin’s criticism of the Korean entry for ‘head’). For another thing, the scenarios themselves are not set in stone and may be amended — or even completely rejected — if other types of evidence, e.g. superior candidates for ‘brain’ or ‘forehead’ appear on the horizon (so far, this has not been the case). But the very fact that onomasiological scenarios like these appear to be working for Altaic, and allow for the proposed etymologies to be presented not as random, disconnected bunches of phonetically and semantically similar comparanda, but as parts of a coherent network, within which we try to account not only for potential archaic retentions, but also for innovations which replace them, should be enough to significantly boost confidence in the hypothesis.

Consequently, in my opinion, a good way to achieve some much-needed substantial progress in the field of Altaic (and, by extension, any other macro-comparative) studies would require taking the following steps.

(1) One should preferably start out with an initial list of pre-defined discrete semantic concepts — one that may be subject to amendments over the course of actual etymological work, but preferably based on some general, rather than Altaic-specific, standard. This may initially be a Swadesh-type wordlist, though preferably expanded to a much larger size than 100 or 200 items (such as, for instance, Terrence Kaufman’s (1973) 700-item list), or the approximate 400-item list that is currently being worked on by linguists of the Moscow School (Starostin et al. 2017).

(2) Proper onomasiological reconstruction should be conducted for all the major branches of Altaic, selecting optimal candidates for each discrete meaning on the list[4].

[4] For a recent example of how this may be done see Kassian et al. 2021; a large supplement to the paper includes onomasiological reconstructions for most of the Turkic, Mongolic, Tungusic, Koreanic, and Japonic items on the Swadesh wordlist, with detailed justifications for complicated cases.

(3) The final step would be an attempt to perform similar onomasiological reconstruction for Proto-Altaic itself — which would combine the results of steps (1) and (2) with an analysis of all previously conducted etymological work from the point of view of how it fits into precise scenarios of historic development.

The two major requirements which would have to be fulfilled in order for the entire endeavor to be counted as validating the Altaic hypothesis are as follows.

1. No matter the size of the original list (as long as it is not specifically dominated by unstable cultural lexicon), many, if not most, of its elements have to be represented by a hypothetical Proto-Altaic reconstruction. The important thing to remember here is that even if the original meaning is only preserved in one branch (in extreme cases — even if it is not preserved at all in any of the branches), credible scenarios of semantic shifts may be recovered based on combining etymological reconstruction with data from the field of semantic typology (e.g. if a certain etymon means 'tree' in one proto-language and 'wood/k. of wooden object' in all other languages, semantic typology indicates that 'tree' must probably have been the original meaning).

2. Not a single element of the list must be represented by more than one hypothetical Proto-Altaic reconstruction, at least not without a substantial reason (though the opposite is certainly permissible, since merging similar meanings such as 'hand/arm', 'body hair/head hair', 'eat/drink' etc. in one lexical root is a very common practice). The more synonyms we find on the list (e.g. two or more equivalents for body parts such as 'head', 'eye', etc.), the less credible picture of Proto-Altaic will emerge from such an approach. At the very least, most of the emerging synonyms should be in accordance with the preferred phylogenetic structure (e.g. the distribution between Narrow Altaic **dilu* and Koreo-Japonic **kVĺi* 'brain', proposed above).

A presentation of comparative lexical evidence for Proto-Altaic which would follow these guidelines is, in my mind, the perfect "middle ground" between the strategies of overpermissive cognate maximization and hypercritical cognate minimization, both of which — as well as those attempts to navigate in between which also suffer from subjective bias — share the flaw of using phonology as pretty much the only systematic criterion for comparison. Whether such a presentation, covering all spheres of the basic lexicon, is at all possible remains yet to be seen; but unless the next *Etymological Dictionary of the Altaic Languages* strives to become the *Onomasiological Dictionary of the Altaic Languages*, I am afraid that comparative Altaic studies have little hope of getting unstuck from the relative rut in which they find themselves today.

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