

The “Nostratic” roots of Indo-European: from Illich-Svitych to Dolgopolsky to future horizons

ABSTRACT: The paper presents a brief assessment of “Nostratic” – the controversial, but promising hypothesis on deeper linguistic connections of the Indo-European family, as envisaged by Vladislav Illich-Svitych and his contemporaries (particularly Aharon Dolgopolsky). We discuss some of the most important developments of the theory since the 1960s, and explain how emphasis on “quantity over quality” of data in the new huge corpora of “Nostratic” comparanda is less useful for advancing the hypothesis than a narrowly targeted emphasis on identifying the “core” evidence for the macrofamily. Identifying this “core” evidence, consisting of a small, but generally stable layer of the basic lexicon, is necessary to lend a more historically realistic flavor to the hypothesis, and its statistical evaluation will also help better understand the place of Indo-European among the other potential constituents of “Nostratic”. We argue that, in weighing the evidence, typological plausibility of semantic shifts and absence of topological conflicts in the tree are no less important than regularity of sound changes. We also show how the credibility level of various theories on the external connections of Indo-European can be arranged along a gradient – from “Indo-Uralic” to a general “Nostratic”, and indicate implications that such an arrangement may hold for future studies.

Key words: Nostratic, Indo-European, long-range comparison, Illich-Svitych, lexicostatistics

The term “Nostratic linguistics” or simply “Nostratics”, particularly in the Russian scholarly community where it first came into active usage, may be seen employed in two different ways. Whereas most serious scholarly studies usually choose the original and “proper” application (i.e., the comparative-historical study of those particular linguistic families of the Old World that were incorporated into the Nostratic macrofamily by its original proponents or their followers), informally “Nostratic linguistics” has also come to be associated with macro-comparative linguistics in general, where any scholar, professional or amateur, interested in deep-level comparison of linguistic families, may be known as a “Nostraticist”.

Despite the obviously erroneous nature of such usage, which essentially turns “Nostratics” into some sort of ideological denomination rather than a specific branch of study (in the same way, “comparative-historical linguistics” used to be sometimes confused with “Indo-European linguistics” in the past), the reason behind it seems obvious. Just as the Indo-European (IE) family used to play the part of the basic training camp where linguists would elaborate and refine the comparative method, so did research in Nostratic linguistics and the various attempts to provide a scientific justification for the Nostratic hypothesis provide the necessary foundation for conducting similar studies in long-range comparison of other linguistic families. It is only natural that this should impose certain obligations on modern research in Nostratic linguistics: if Nostratic is to go on serving as the default model for “grand-scale” macro-comparative theories, then most of the general critical observations on such theories have to be addressed and, if possible, refuted first and foremost for Nostratic.

The “classic age” of Nostratic linguistics should probably be counted not from the publication of Holger Pedersen’s famous paper (Pedersen 1903), in which the author first came up with the very term “Nostratic” and proposed the approximate borders of this hypothetical macrofamily, but rather from the emergence, in the 1960s, of the first truly serious studies in this subdiscipline, associated primarily with the names of Aharon Dolgopolsky (1964a, 1964b) and Vladislav Illich-Svitych (1964, 1968), who were the first to apply rigorous, Neogrammarian-style methodology to reconstructed proto-languages (Proto-Indo-European, Proto-Uralic, Proto-Altaic, etc.) and attempt to produce systematically justified morphemic reconstructions for Proto-Nostratic. Half a century later, the Nostratic hypothesis, despite its ongoing lack of general acceptance, may still be said to retain its original positions – it is controversial, but respectable, with not a few authoritative and talented scholars continuing to voice strong support for all or at least some of its basic claims. But what about “progress”? With fifty years behind its back, one should certainly feel justified to ask whether this period has managed to bring about any substantial advancements in Nostratic linguistics, promoting it to a higher level of development compared to the pioneering works of Illich-Svitych and Dolgopolsky.

One of the first attempts to analyze this situation was carried out approximately twenty years ago in a series of publications by Alexis Manaster Ramer (1993, 1994, 1995) – one of the few American linguists whose attitude towards “Nostraticism” mixed general sympathy with elements of constructive criticism. Nevertheless, on the whole his conclusions were rather pessimistic. According to Manaster Ramer’s observations, the Moscow school of comparative linguistics, instead of continuing to develop the ideas of Illich-Svitych, had largely become mired in less “risky” issues of perfecting the reconstructions for the daughter branches of Nostratic (such as Uralic or Altaic), without doing much in the way of elaborating the methodology of Nostratic reconstruction as such. Some optimistic hope for the future did remain – after all, it is all but impossible to reconstruct anything to absolute perfection, so sooner or later the balance would be restored – but was largely theoretical.

Unfortunately, one must admit that in those further twenty years which lie between the critical papers of Manaster Ramer and the present time, this important breakthrough that the optimists were hoping for did not come to pass. Like Manaster Ramer, the authors of the present paper believe that, although occasional interesting and significant works in Nostratic linguistics, including several huge etymological dictionaries to which we shall return later, still continue to appear, in general, ever since the publication of the original comparative materials of Illich-Svitych (the brief comparative lexicon in Illich-Svitych 1967 and three subsequent detailed volumes of his Nostratic dictionary, 1971–1984, none of which have been translated into English), Nostratic linguistics has remained in a state of permanent crisis. Roughly speaking, those scholars who adopt and strictly adhere to Illich-Svitych’s methodology have little hope of achieving truly substantial new results in this sphere; and yet, at the same time, no Nostraticist so far can be said to have truly understood how to do Nostratics *better* than Illich-Svitych.

Said crisis has its external front as well as internal reasons. On the external side, fifty years of the existence of Nostratic linguistics still have not resulted in its incorporation in “mainstream” linguistics, i.e., it has failed to procure the status of a commonly accepted, authoritative discipline, upheld and developed by an international community of Nostratic scholars. The situation remains more benevolent in Russian comparative linguistics, where such notable scholars as Vladimir Dybo, Anna Dybo, Sergei Nikolaev, Ilia Peiros, and the late Sergei Starostin and Eugene Helimski have all lent their support to the theory; Western linguists, with rare exceptions, continue to remain skeptical and sometimes almost militantly antagonistic.

A good example of this attitude is a paper by Lyle Campbell (1998), more recently reproduced with minor edits as part of a more general monograph (Campbell & Poser 2008), where the author critically analyzes the Indo-European and Uralic data in Illich-Svitych’s dictionary (actually, not in the dictionary itself, but rather in the English language indexes to the dictionary, published as Kaiser & Shevoroshkin 1988 and omitting most of the explanatory parts of the etymologies) and concludes that most of the comparisons are unsatisfactory, one way or another.

Despite an impressive number of factual errors in Campbell’s analysis (many of which have been themselves thoroughly criticized in Starostin 2009) which, in our opinion, uncovers a seriously biased attitude towards the subject, even their complete elimination probably could not have forced the author to renounce his conclusions, particularly since the main one (general weakness of argumentation in favor of Nostratic) today seems to be shared by the majority of those Western linguists who have shown an active interest in the subject (e.g., Donald Ringe, Eric Hamp, Brent Vine, Bernard Comrie, the late Calvert Watkins and others).

This circumstance by itself is hardly tragic. The negative attitude towards Nostratic is partly explained by the general state of affairs in comparative-historical linguistics (which is hardly among the most popular branches of linguistic science today), and partly by certain formal flaws that could be easily rectified, given enough strong will and enough resources (such as the lack of a detailed monographic treatment of the Nostratic issue from a positive perspective).¹ And yet, unless the status and general impression of Nostratic among specialists worldwide remains the way it is now, one fears that this will imminently lead to the extinction of the hypothesis even in the Moscow School of comparative linguistics, where it continues to retain some respect largely due to the undeniable authority of its “founding fathers” and unambiguous support from a large number of specialists (partially listed above).

Additionally, it is hard not to admit that the “external front” is in many ways dictated by the internal reasons. It is fairly simple (and, to some extent, justified) to say that Nostratic linguistics has been so poorly received by the Western linguistic mainstream primarily because fifty years ago, few mainstream linguists took the trouble to read and analyze in detail the works of Illich-Svitych, most of which were physically out

¹ With the exception of numerous works by Allan Bomhard, whose shortcomings will be briefly touched upon below.

of their reach anyway; or that nowadays, when data accessibility has largely ceased to be a problem, mainstream linguists prefer to focus on the works of Allan Bomhard as the principal “newsmaker” for Nostratic, rather than the allegedly obsolete publications by Illich-Svitych, and that this makes the whole theory more vulnerable for criticism. But none of this has anything to do with Nostratic studies as a specific sub-discipline of comparative linguistics: clearly, problems with Nostratic are not at all limited to lack of popularization among the scientific community. As far as pure science is concerned, our main worries should lie elsewhere – and there are reasons to believe that even if every single word on Nostratic ever written by Illich-Svitych were translated into English and published with an even more extensive commentary by his followers, this would hardly lead towards a general acceptance of Nostratic as a “proven” theory.

The one major methodological breakthrough achieved by Illich-Svitych (even regardless of whether one accepts or rejects his practical results) was his ability to show how reconstructed protolanguages, usually believed to be the “end” of the reconstruction, such as Proto-Indo-European, Proto-Uralic, or Proto-Dravidian, may actually be substituted for historically attested languages and compared between themselves, as part of a recurrent process, in strict formal accordance with the classic comparative method. This has elevated macro-comparative linguistics from mere speculation to the status of a discipline that is at least capable of generating falsifiable hypotheses, and thus deserving of scholarly attention. Illich-Svitych’s research transcended the preliminary stage of accumulation of lexical and grammatical *comparanda* between the various language families of Eurasia on the basis of phonetic and semantic similarity (an activity that was widespread among all sorts of linguists, from Robert Caldwell to Alfredo Trombetti) and progressed towards a more rigorous systematization of this material, including the establishment of regular correspondences between the phonological systems of reconstructed proto-languages. These correspondences, such as the systematic triple opposition of voiced, voiceless, and (provisionally) ejective consonants, as well as a small group of the most widespread and convincing grammatical and lexical *comparanda* that obey these correspondences, still provides the cementing foundation upon which the entire Nostratic edifice, shaky as it may seem to critical opinion, continues to reside.

It is, however, not a matter of debate that, since the original publications of Illich-Svitych, not a single piece of research on Nostratic has appeared that the “international community of Nostraticists” would unanimously value as equal to, let alone surpassing the value of Illich-Svitych’s pioneering enterprise, adding it to the figurative “golden fund” of important discoveries in the field. Not a single correspondence has been made more precise, not a single new grammatical morpheme has been reconstructed, not a single new lexical etymology has been suggested in such a way that would convince *every* single supporter of the Nostratic hypothesis of its truthfulness, or would make it look undeniably more probable than any possible alternative. Of course, new ideas are constantly suggested by those few scholars who continue to publish their research in Nostratic, but they are either rejected off the cuff, or quietly ignored, or, just as quietly,

“accepted” (usually due to respect for authority, especially of the likes of Aharon Dolgopolsky or Sergei Starostin), only to be forgotten and neglected in further studies in a couple of years. It would not be much of an exaggeration to state that the *only* base reference point that ties together all new research on Nostratic is still the original Illich-Svitych system.

This does not mean that new ideas do not work because they just happen to be glaringly erroneous or insufficiently confirmed by data – the major problem that is being faced here is the lack of transparent and easily understandable criteria for their wholesome evaluation, *especially* such criteria that would be shared by all or most of the researchers dealing with Nostratic.

One could suggest that, perhaps, the tremendously efficient activity of Illich-Svitych has led to Nostratic linguistics running the same course in about ten years that Indo-European linguistics had run in over one hundred – namely, all the major, unambiguous phonetic correspondences have been established, all the undeniable etymologies have been discovered, and now one is left in “fringe” territory, where most of the evidence for further decisions will be circumstantial and open to endless debate, and research will largely focus on issues of “internal reconstruction”, for which no objective and universally applicable method has ever been set in stone. But if this is indeed the case, and the duties of modern day Nostraticists should be restricted to “scraping the bottom” of Illich-Svitych’s barrel, the perspectives of Nostratic linguistics as a whole are rather grim.

The undeniable core of uncontroversial Indo-European etymologies consists of hundreds, rather than dozens, of units, and the phonological system of Proto-Indo-European may be said to have been reliably recovered almost in its entirety (ongoing debates usually concern the phonetic interpretation of recovered phonological units, or certain “advanced level” reconstructions, such as the number and the status of laryngeal units). Compared to this situation, a large number of Illich-Svitych’s phonetic correspondences between the daughter branches of Proto-Nostratic remain questionable (cf., for instance, attempts at radical revisions of correspondences in the affricate and fricative series in Dybo 2004, etc.), and the fundamental corpus of Nostratic etymologies that would be accepted among all Nostraticists hardly exceeds a hundred units (we are talking about such etymologies the reflexations of which would be attested in at least three of the large families that constitute Nostratic and would not raise significant phonetic or semantic objections). In many cases, there are also problems with “exclusive” Nostratic etymologies, i.e., roots that would be well reflected in Nostratic but not observed outside of Nostratic (e.g., in such Eurasian families as North Caucasian or Sino-Tibetan): “non-exclusive” Nostratic etymologies cannot be taken as strong evidence for the taxonomic reality of Nostratic. Is it then actually true that this rather humble collection of significant evidence is *everything* that we can come up with as reliable proof of the Nostratic hypothesis?

The good news is that even if we answer this question in the positive, such an answer should not by itself signify that Nostratic should be regarded as a less reliable historical proposition than Indo-European. Scarceness of strong evidence for Nostratic should

Table 1: Etymological matches between Proto-Germanic and Proto-Iranian within the 50-item wordlist

#	Meaning	Proto-Germanic	Proto-Iranian
1	ashes	*askō(n)	*āh-
2	finger nail	*naga-la-z	*nāx-
3	dog	*xunda-z	*śvan-
4	egg	*ajjā-n	*āya-
5	foot	*fōtu-z	*pāda
6	heart	*xirt-an	*źrd-
7	horn	*xur-na-n	*śru-
8	I	*ika	*aža-m
9	moon	*mēnēn	*māh-
10	name	*naman	*nāman-
11	new	*niwja-	*nava-
12	nose	*nasi-z	*nāh-
13	not	*ni	*na
14	one	*ai-n-a-	*ai-va-
15	star	*stirna-z	*stār-
16	sun	*sōwila-n / *sunnēn	*x ^w ar-
17	thou	*θū	*tuv-am
18	tongue	*tungōn	*źvā-
19	tooth	*tánθu-z	*dantan-
20	two	*twai	*duva-
21	we	*wīz	*va-
22	what	*xwa-	*či-
23	who	*xwa-	*ka-

only be regarded with suspicion if we somehow expected that the reconstructed proto-languages for those families that allegedly descend from Proto-Nostratic should yield at least as many reliable *comparanda* as, for instance, the protolanguages for the various groups within these families – in other words, that Proto-Nostratic should just as effortlessly “flow out” from Proto-Indo-European, Proto-Uralic, Proto-Kartvelian, etc., as Proto-Indo-European itself “flows out” from Proto-Germanic, Proto-Celtic, Proto-Balto-Slavic, and attested ancient languages like Greek, Sanskrit, and Latin. In reality, however, we have no reasons whatsoever for nurturing such elevated expectations.

A recent study, carried out by members of the Nostratic seminar of the Moscow School of comparative linguistics (including the authors of this paper), has shown that, if the canonical 100-item Swadesh wordlist is reduced to 50 most historically stable items (in accordance with the “average stability index” calculated by Sergei Starostin 2007; see G. Starostin 2010 for more details on the methodology), one finds, on the average, no less than 20 lexicostatistical matches between any two group level reconstructions within Indo-European, e.g., 23 out of 50 matches between Proto-Germanic and Proto-Iranian (see Table 1), etc.; at the same time, we have no more than 7 obvious (plus a couple more phonetically dubious) comparisons between the Proto-Indo-European and Proto-Uralic 50-item wordlists (see Table 2).

Even if we refrain from interpreting this discrepancy in absolute chronological terms (i.e., stay away from the controversies of glottochronology), it would be useless, based on this and similar pieces of evidence for other taxa, to deny that Proto-Nostratic must

Table 2: Etymological matches between Proto-IE and Proto-Uralic within the 50-item wordlist

#	Meaning	Proto-IE	Proto-Uralic
1	drink	*e:ghw-	*iyi-
2	hear	*k ^h lew-	*kuwli
3	I	*me-	*mi-n
4	name	*nomn	*nimi
5	thou	*ti	*ti-n
6	water	*wed-	*weti
7	who	*k ^w i-	*ku-
8	? hand	*g ^h yes-ŋ	*käti
9	? mouth	*o(:)y(-)es	*s ^u wi

Notes on the table:

1. For etymological details and comments on individual forms, see Kassian, Zhivlov & Starostin (2015).
2. Our selection of the “old school” transcriptions of Proto-IE etyma without the three laryngeals reflects a deep skepticism, shared by two out of three authors of the present paper, towards the absolute necessity of laryngealistic reconstruction in all those cases where the presence of a laryngeal phoneme is not explicitly confirmed by Anatolian data – a position widely (though not universally) shared among specialists that belong to the Moscow School and explained in detail in such works as, e.g., Lehrman (1998). Being very much a matter of internal reconstruction, the laryngeal theory may be expected some day to be ultimately confirmed or disproved through external comparison of IE with its closest relatives; as of today, we believe that presenting IE reconstructions in their “classic” form should by no means be considered a sign of unprofessionalism or intentional controversy. In any case, it must be strongly emphasized that the algorithms of automated comparison touched upon in this paper are robust enough to allow for either the “old” or the “laryngealistic” interpretation of IE reconstructions without any discrepancies between the results, making the issue irrelevant in this particular context.
3. The reliable etymological matches #1–7 demonstrate the identity of CC-structures (two first consonants) in terms of consonant classes, e.g., both *wed- and *weti = WT. See Kassian, Zhivlov & Starostin (2015) for a detailed description of the whole procedure.

have been far more “distant” from its daughter branches than those, in turn, would differ from the “granddaughter branches” of Nostratic. Consequently, a reconstruction of Proto-Nostratic on the basis of Proto-Indo-European, Proto-Uralic, Proto-Altaic, etc., would be somewhat analogous to an attempt to reconstruct Proto-Indo-European on the basis of such modern languages as German, Greek, Russian, and Spanish (and this is taking into consideration that these are far from the most phonetically innovative Indo-European languages). Claims that such a reconstruction would, at best, be completely different from the conventional Indo-European reconstruction as we know it (Beckwith 2004: 217) or, at worst, be downright impossible are in total agreement with the general skepticism of historical linguists towards the Nostratic hypothesis.

How well-grounded is this analogy in fact? A formal look at the data in Tables 3–4 (German/Greek, Russian/Spanish) in comparison with Table 2 (Proto-IE/Proto-Uralic) suggests that, if we concentrate on those proven (for Indo-European) or potential (for Indo-Uralic) cognates that retain a strong degree of phonetic similarity, the reconstructed Proto-IE and Proto-Uralic languages suggest the same degree of chronological proximity as exists between the modern Indo-European languages.

Table 3: Etymological matches between Modern Literary German and Demotic Greek within the 50-item wordlist. Pairs with transparent phonetic similarity are highlighted²

#	Meaning	Modern German	Modern Demotic Greek
1	finger nail	n'a:gəl	n'íy-i
2	ear	o:ɣ	aft-'i
3	egg	aɪ	avɣ-'o
4	eye	'aʊgə	m'at-i
5	foot	fu:s	p'oð-i
6	heart	hɛɾc	karð-y-'a
7	horn	hɔɾn	k'erato
8	I, me	ɪ̯, mɪ̯	eɣ'o, em'ena
9	leaf	blat	f'il-o
10	name	n'a:mə	'onom-a
11	new	nɔɣ	n'e-o-
12	night	naxt	n'ixt-a
13	star	ʃtɛɾn	ast'er-i
14	sun	z'ɔnə	'iɫ-o-s
15	thou	du:	es'i
16	tooth	ca:n	ð'od-i
17	two	cvaɪ	ð'io
18	we	ɔns [obl. stem]	em'i- [dir. & obl. stem]
19	what	vas	ti

Notes on the table:

1. Data in Tables 3–4 are encoded in the unified transcription system of the Global Lexicostatistical Database project, which is generally based on the IPA alphabet with just a few specific discrepancies.³
2. There is also a fifth German-Greek pair with a strong degree of phonetic similarity: German *aɪn* / Greek 'en-a-s 'one', although these numerals are etymologically unrelated.

It is easy to see that we still have about 19–21 etymological matches within the 50-item wordlist between such modern Indo-European languages as German, Greek, Russian and Spanish. However, much less than a half of these etymological pairs show phonetic similarity of the same degree as the Proto-IE–Proto-Uralic pairs listed above (Table 2). The 7 Russian-Spanish pairs (highlighted in Table 4) are comparable with the 7 Proto-IE–Proto-Uralic pairs (Table 2), but the situation with German and Demotic Greek is somewhat worse: there are only 4 or 5 phonetically transparent pairs (highlighted in Table 3). One should keep in mind that the real amount of Proto-IE–Proto-

² “Transparent phonetic similarity” is understood here in a formalistic manner: only those pairs of cognates whose consonantal structures (limited to the first two consonants of the root) can be reduced to the same common invariant are understood to be phonetically similar. Thus, German *n'a:mə* ‘name’ and Greek *'onom-a* ‘name’ both have the consonantal structure NM; German *hɛɾc* ‘heart’ and Greek *karð-y-'a* ‘heart’, however, differ, since German has the structure HR and Greek has the structure KR. A more refined variant of the procedure, allowing for specified shifts between consonantal classes or attempting to establish regular correspondences, could permit to regard these items as cognates, but what matters here is not the degree of refinery, but the *uniformity* of the method as applied to any pairs of languages or proto-language reconstructions (i.e. the exact same standards should apply to the Proto-Indo-European/Proto-Uralic comparison and the German/Greek comparison). See Kassian, Zhivlov & Starostin (2015) for more details on the principles according to which consonants are grouped into consonantal classes.

³ See <<http://starling.rinet.ru/new100/UTS.htm>>.

Table 4: Etymological matches between Modern Literary Russian and Castilian Spanish within the 50-item wordlist. Pairs with transparent phonetic similarity are highlighted

#	Meaning	Modern Russian	Modern Castilian Spanish
1	finger nail	n'ogacʹ	'un-a
2	to die	u=mir-	mor-'i-r
3	to drink	pi-, py-	beβ-'e-r
4	ear	'ux-ə	or'ex-a
5	egg	iyc-'o	'weβ-o
6	heart	s'erc-ə	kor-aθ'on
7	I, me	ya, minʹ'a	zo, mi
8	moon	lun-'a	l'un-a
9	name	'imʹ-ə	n'ombr-e
10	new	n'ov-	n'weβ-o
11	night	n'oč	n'oč-e
12	nose	nos	nar'iθ
13	no	ne	no
14	smoke	dīm	'um-o
15	sun	s'on-c-ə	sol
16	thou	tī	tu
17	tongue	iz'ik	l'engw-a
18	two	dva	dos
19	we	nas [obl.]	nos-'otr-os
20	what	š-to	ke
21	who	k-to	kyen

Uralic pairs with phonetic similarity can actually be higher than 7, since it is possible that a certain proto-lexeme with the required Swadesh meaning has simply been replaced in all daughter languages and therefore becomes technically non-reconstructible for the proto-language.

It remains an open question how many non-highlighted pairs in Tables 3–4 could be suspected and etymologically proven in the absence of prior knowledge of ancient languages of the Germanic, Greek, Slavic and Italic groups. (It remains an even more open question, of course, how many “true” Indo-Uralic cognates have not been discovered in our lexicostatistical analysis, not to mention Illich-Svitych’s original research on Nostratic etymology, due to lack of transparent phonetic similarity.)

In any case, we are convinced that these comparative tables illustrate one of the chief problems, if not the biggest problem, of Nostratic, and that any essential improvement of this hypothesis – as well as any other far-flung macro-comparative hypothesis, for that matter – should begin with the acknowledgment of this rather obvious fact. Unfortunately, Illich-Svitych’s own research does not do this explicitly; although he must have been aware, one way or the other, of the uncomfortably large distance between his version of Proto-Nostratic and its immediate offspring, this was never specifically accentuated or reflected in his work methodology. But what about subsequent studies – was this factor eventually taken into account by any of the colleagues or followers of Illich-Svitych? To answer that question, one should first specify the various ways in which this could be done, and then briefly check the main directions in post-Illich-Svitych Nostratic studies to see whether any of them have been properly implemented.

First, if we agree that the “Nostratic legacy” of Illich-Svitych consists of evidence of highly varying quality, it seems reasonable to attempt a stratification of this legacy, with some preset ideas about what sort of linguistic levels in daughter languages should have preserved it best of all. In other words, instead of focusing on quantity, it is necessary to concentrate on quality – scrutinizing first the basic lexicon and certain types of grammatical elements, before turning to a potentially wider (but much less stable) circle of quasi-evidence.

Second, since the major purpose of etymology is to *explain* facts and *answer* questions, it may be argued that a really useful etymology is the one that offers more answers than poses new questions. Thus, the etymology of the English word *ear* states that it reflects the common Indo-European root **ous-*; that the majority of attested Indo-European languages still preserves this root in its original meaning; that in some of them it was replaced by words derived from the verbal root **kleu-* ‘to hear’ (Tocharian, some Celtic); that a few more have replaced it with words of obscure origin, e.g., Proto-Iranian **gauša-*, whose source is not known but which is clearly an innovation, according to distributional evidence (not found outside of Iranian) as well as the fact that the old dual form *ušī* ‘ears’ is still preserved as an archaism in Avestan. Ultimately, this etymology may be judged as valid on all counts (phonology, semantics, distribution).

On the other hand, the etymology that tries to link Proto-Indo-European **ok^w-* ‘eye’ with Proto-Altaic **uk^u* ‘to perceive, to understand’ (an etymology that goes all the way back to Illich-Svitych (1971–1984, vol. 1: 255–256) and has since then been reproduced, with or without minor modifications, in most subsequent Nostratic corpora) generates more questions than answers, even if, on the surface, it could be seen as flawless both phonetically and semantically; Illich-Svitych himself wrote that the semantic development ‘to see’ → ‘to know’ (more accurately, ‘to understand’, since the basic meaning ‘to know’ should rather be associated with other Altaic equivalents, Starostin, Dybo & Mudrak 2003: 1490) in Proto-Altaic poses no typological problems. However, an important unanswered question is – what was the *original* semantics of the reconstructed Nostratic root **Huk^a*: ‘eye’, ‘to see’, or something different? Since the semantic development ‘eye’ → ‘see’ is encountered very rarely, the verbal meaning is more probable to be primary, and one might declare that we have managed to reconstruct Proto-Nostratic **Huk^a* ‘to see’, preserved only in the form of a nominal derivative in Proto-Indo-European and shifted to such verbal meanings as ‘to understand’, ‘to submerge in thoughts’, ‘to look into’ in Altaic. But if so, where do all the new roots with the meaning ‘to see’ come from, both in Indo-European and Altaic? And, most importantly, is it really true that this Indo-Altaic isogloss is indeed the most probable and reliable “candidate” for the basic meaning ‘to see’ on the Nostratic level, the same way that **ous-* and **ok^w-* are the most probable candidates for Indo-European ‘ear’ and ‘eye’, respectively? Because existing Nostratic dictionaries have plenty of alternate “candidates” for this meaning (Dolgopolsky’s dictionary, 2012, alone has around a dozen).

It goes without saying that “synonymy” of this kind is frequently encountered in lower-level dictionaries as well, including Indo-European, but the mechanisms of its elimination on the Proto-Indo-European level are more transparent than on higher levels:

nobody will claim that Proto-Iranian **gauša-* is a more likely candidate for Proto-Indo-European ‘ear’ than **ous-*, whereas the situation with Nostratic is far more complicated.

In other words, out of all the different competing models of Nostratic the winner will be that one particular model that will present the most realistic scenario of historical development from Proto-Nostratic to its offspring, one that leaves the fewest number of unanswered questions and the fewest number of internal contradictions. From that point of view, even despite the obvious fact that gaps in our knowledge about Nostratic will always be larger than gaps in our knowledge about Indo-European, it is not likely that we will end up with any two or more equiprobable scenarios; most of the time, we simply do not bother trying to quantify, in a transparent and accessible manner, the competing lines of evidence.

Ultimately, the linguistic mainstream will be forced to acknowledge the merits of Nostratic only when/if we have clearly shown that the “Nostratic scenario” is historically inevitable – that the general Nostratic puzzle, correctly and objectively compiled out of several disjointed bits which, even today, we find hard to systematize, relying on little other than faint traces of regular correspondences, is actually coming together as we utilize the full strength of our knowledge of the typology of historical change. Phonetics, which has up to now served as the primary, if not only source of Nostratic reconstruction, has to be supplemented with detailed analysis of semantic change, as well as a careful analysis of the distribution of proposed cognates (*forms and meanings*) across the Nostratic tree. Only this triple approach (phonetics, semantics, distribution) will allow us to advance Nostratic studies to the next level – in fact, if successfully applied, it might allow for important advances in more traditional areas of comparative-historical linguistics as well.

Let us now take a quick look at how the modern varieties of Nostratic deal with this issue of making the hypothesis more realistic. If a particular “school of Nostratic thought” may be defined by a specific methodology and the availability of a large body of evidence, we should single out three such major branches: (a) the “Charleston School” (this and the following terms were first used in print by Václav Blažek 2011), mainly represented by Allan Bomhard and his Nostratic dictionary (Bomhard 2008); (b) the “Haifa School”, represented by the late Aharon Dolgopolsky and his Nostratic dictionary (Dolgopolsky 2012); (c) the “Moscow School”, represented by such scholars as the late Sergei Starostin, as well as the late Eugene Helimski, Vladimir Dybo, Anna Dybo, Sergei Nikolaev, the authors of the present paper, and some other scholars; their respective corpus is the large database of Nostratic etymologies, compiled mainly by Sergei Starostin and further replenished by his colleagues – this is essentially a compendium of etymologies drawn from Illich-Svitych’s works, early drafts of Dolgopolsky’s dictionary, and some other publications, but all of them heavily revised and converted into an open-ended “pool” of etymologies, subject to modifications whenever new evidence becomes available.⁴

⁴ The latest version of the database is publicly available at the “Tower of Babel” website: <<http://starling.rinet.ru/>>.

Bomhard's "Charleston School" is probably better known to Western scholars than others, since its main representative is based in the USA, publishes everything in English, and has applied a lot of effort towards popularizing the Nostratic theory among a wider audience. Bomhard places a heavy emphasis on the principle of regularity of phonetic correspondences, to which he adds the importance of typological cross-checking of the postulated phonetic developments (see Bomhard 2008); however, much of this happens at the heavy expense of precision in semantic reconstruction, as shown, for instance, in the highly critical analysis of Bomhard's methodology by Helimski (2000a, 2000b), where it has been shown that the degree of "fuzziness" permitted in Bomhard's semantic comparisons would allow to establish any system of regular correspondences between any sounds, given a wide enough pool of comparative data to choose from. Unfortunately, these criticisms have not affected the overall quality of Bomhard's later research: methodologically, it has not progressed by much since his early publications (e.g., from Bomhard 1984 to Bomhard 2008), and we side with the late Helimski in considering these works as methodologically inferior to Illich-Svitych rather than methodologically advanced. The works of Bomhard do bring to light an important shortcoming of the Neogrammarian paradigm, especially as applied to long-range genetic relationships: they strictly observe the Neogrammarian principle of regularity of phonetic correspondences, but make use of the lack of equally strict etymological criteria pertaining to semantics, which allows to produce large numbers of phonetically regular, but semantically questionable (generally "possible", but potentially "improbable") etymologies that do not add up to a solid case in favor of the reality of Nostratic.

Much more problematic is the second direction of Nostratic, represented by the "Haifa School" of Aharon Dolgopolsky. The chief outcome of his activity is the fundamental etymological dictionary of Nostratic (Dolgopolsky 2012), encompassing around 2800 etymologies, elaborated over more than four decades of research. This is at least three times as much as the original corpus of Illich-Svitych, with an overwhelming mass of new data and new etymological solutions. One could view it as a logical continuation of Illich-Svitych's research – especially since Dolgopolsky has introduced minor, rather than major, revisions into Illich-Svitych's original system of correspondences, and has retained the majority of "classic" etymologies by his former colleague (and competitor). The etymological methodology has not undergone any changes whatsoever – not that this was at any time Dolgopolsky's intent.

The actual problems of this methodology, however, become more and more obvious as the corpus becomes enlarged, and the main formal problem could be defined as "rampant synonymy". Even a brief perusal of the semantic index to the dictionary Dolgopolsky (2012) shows us the following anatomic peculiarities of the speaker of Proto-Nostratic: 5 'heads' (note that this includes *only* the entries that have 'head' as their primary meaning; there are at least 5 more where 'head' is listed as a figurative meaning, alongside 'top', 'peak', etc.), 3 'brains', 7 'eyes' (2 "pure" 'eyes' and 5 with polysemy 'eye/see'), 4 'noses', 5 'tongues', 7 'teeth', 10 'hands' (not counting 'fingers' and 'palms'), also around 10 'feet' (not counting 'legs' and 'soles'), and at least 3 "pure" 'penises', plus 3 more with the polysemy 'penis/tail'.

Since this type of situation is completely alien to attested languages around the world, we cannot seriously consider that it could have been relevant for Proto-Nostratic, regardless of the time depth to which we assign it. One could resort to such *ad hoc* explanations as “stylistic quasi-synonyms” or the semantic imprecision of the reconstruction (e.g., the same term ‘head’ could really reflect such connected meanings as ‘top’, ‘occiput’, ‘forehead’, ‘face’, etc.), but neither of these would be substantiated by evidence unless we can draw explicit arguments for them from available data; besides, meanings like ‘top’, ‘forehead’, ‘face’, as a rule, have their *own* primary entries in the dictionary, and adopting this solution would only worsen the situation.

There is yet another uncomfortable aspect here. Any such etymology, provided the phonetic correspondences are satisfactory and no typologically unheard of semantic connections have been set up, could be correct in principle. But analysis of the “anatomical” field (as well as any other field) in general leaves us uncertain about whether we could, on the basis of this evidence, present a realistic, “optimal” historical scenario of the development from the Proto-Nostratic system of anatomical terms to the respective systems of its daughter branches. This means that Dolgopolsky’s new etymologies usually generate more questions than answers – simple, but unanswerable questions, such as “what was the Proto-Nostratic word for ‘nose’?” Trying to get away with an answer like “It was either **neqVrV*, or **ŋäqqaša*, or **pVnč’V*, or **q’uŋa*, or maybe it was all four at once” just will not do, and illustrates a fundamental problem of Dolgopolsky’s Nostratic as opposed to, for instance, Proto-Indo-European, where we may state, with a very high degree of probability, that the Indo-European word for “nose” was **nās-*.

A second unresolved problem, tightly connected with the first one is Dolgopolsky’s categorical (one might even say, aggressive) rejection of the *distributional* argument in Nostratic. Of particular note here are the objections that Dolgopolsky raises on p. 42 of the dictionary against the critical remarks of Sergei Starostin, who had earlier expressed doubts about the validity of bringing into comparison the reflexations of potential cognates that were preserved only in one of the many branches of a particular family – for instance, having the huge Afro-Asiatic branch represented in a given comparison only by Semitic, or, even worse, only by Arabic; or having the Altaic branch represented only by Turkic, or, even worse, only by Japanese, etc. In his answer, Dolgopolsky states that he sees no problem here, methodologically comparing this with, for instance, the situation of using only Gothic (but not Common Germanic) evidence for a particular Indo-European etymology. In addition, he justly notes that overrelying on such “isolates” could only be dangerous at the stage of *proving* language relationship, but not at the stage of exploring a relationship that has already been proven based on a smaller, unproblematic body of evidence – such as the initial evidence for Nostratic in Illich-Svitych (1967). If the etymological dictionary is not proving a hypothesis, but explores it further as an already proven hypothesis, there is nothing methodologically wrong about bringing all sorts of scattered isolated forms into comparison.

From a purely theoretical standpoint, this conflict of opinions seems to be irresolvable. If we treat Nostratic as a “working hypothesis” or even as a proven fact, it is clear that, for instance, a Permic-Svan or a Slavic-Manchu isogloss will be seen as a “weaker”

etymology than a comparison between Proto-Uralic and Proto-Kartvelian, or between Proto-Indo-European and Proto-Altaiic, but “weakness” is not the same as “impossibility”, and assigning probabilistic evaluations to these etymologies will do little, per se, other than expressing in mathematical terms what is already seen on the surface (poor distribution of cognates).

The only thing that can help resolve this problem for each particular case is a sort of “reality check” that would involve the suggestion of a specific historical scenario of development, one that does not merely take into consideration the semantic closeness of the compared words, but outlines particular directions of semantic developments in various branches. Let us illustrate this with a particular example, randomly drawn from Dolgopolsky’s dictionary.

Nostratic **náko* ‘soft parts of the animal’s body (liver, marrow, suet)’ (Dolgopolsky 2012: 1665) is reconstructed based on Indo-European **yek^w-r-t* ‘liver’ (Narrow Indo-European, excluding Anatolian and Tocharian), Finno-Ugric **nokwV* ~ **noγwV* ‘meat’ (actually only represented by Ob-Ugrian **neyeł*), Old Turkic *jaqr̥* ‘suet’, and Semitic **nqy* ‘marrow’ (actually only represented by Arabic *niqy*).

Putting aside the issue of the statistical probability of chance coincidences which is actually a serious problem under such an approach (see Ringe 1999 on the theoretical easiness to find isolated *comparanda* for any CVC-root when many languages are compared simultaneously), how does one go about verifying the aforementioned Dolgopolsky’s etymology?

First, it should be made more precise what we mean by the expression ‘soft parts of the animal’s body (liver, marrow, suet)’. If this pretends to be a case of semantic reconstruction, i.e., we claim to be reconstructing a *generic* term that includes (a) any internal organ, (b) any brain or marrow tissue, (c) any type of fat, it would make sense to present typological evidence that a word like this is actually attested in any natural language (let us emphasize that we are not talking about ‘innards’, but specifically about ‘everything that is found inside the body but is not meat or bones’). If no such evidence is found (and the burden of proof here is on the etymologist), then this expression is not a semantic reconstruction, since it is hardly permissible to reconstruct (without precise evidence) a meaning that finds no analogies in modern languages.

However, even Dolgopolsky himself would probably admit that this is not so much a formal semantic reconstruction as the extraction of a common semantic component that would justify the *comparanda* being placed in a single etymology. If so, our task is different: assuming the etymology is correct, we need to replace the question “do all these meanings share a common semantic component that justifies their comparison?” with the more historically relevant question of “what was the original meaning of the word and how did it acquire all these different meanings?”. With this question in mind, we provide a distributional evaluation of the *comparanda*:

- (a) Indo-European **yek^w-r-t* ‘liver’ may indeed be regarded as the optimal candidate for this meaning in Proto-Indo-European, but only if we agree to disregard Anatolian data (Hittite **les-*, Palaic **pVn-*);

- (b) it is theoretically possible to project Ob-Ugrian **meget* ‘meat’ onto the Proto-Uralic level with the meaning ‘meat’ (assuming a replacement in Finno-Permic → **siwVfV* and in Samoyed → **ǰǰǰ*), but, unlike Indo-European, this is hardly the optimal choice for this Proto-Uralic term;
- (c) Old Turkic *jaqr̥* ‘suet’ is frequently analyzed as derived from the verb *jaq-* ‘to smear’ (cf., for instance, Levitskaya 1989: 59), which is typologically realistic, and also sometimes suspected as somehow related to Common Turkic **jāy-* ‘fat’ (Dybo 2013: 243–245); regardless of these explanations, in Old Turkic this word performs essentially the same functions as Common Turkic **bañ* ‘fat, oil’ (Dybo 2013: 245–246), which, conversely, is not attested in Old Turkic monuments; this gives us an optimal scenario in which the old word **bañ* is replaced in Old Turkic with the derived neologism *jaq-r̥*;
- (d) in Proto-Semitic, the unquestionably optimal candidate for ‘marrow’ is the root **muh-*, which preserves this meaning in Arabic, Aramaic, and Old Hebrew (sometimes with a secondary development to ‘brain’, or, perhaps, the word had the polysemy ‘brain/marrow’ from the start; see Militarev & Kogan 2000: 169). Consequently, Arabic *niqy* either does not go back to Proto-Semitic at all, or it had some sort of different meaning in Proto-Semitic.

Already this first stage has left us without Turkic ‘fat’ and Semitic ‘marrow’; we are left with a strong Indo-European ‘liver’ and a much weaker Uralic (Ob-Ugrian) ‘meat’. The next stage is purely typological: if the original meaning was “meat”, is it legitimate to assume the semantic development ‘meat’ → ‘liver’ in Indo-European? And if the original meaning was ‘liver’, is it legitimate to assume ‘liver’ → ‘meat’ in Uralic? Both words form part of the Swadesh 100-item list, actively studied within the Global Lexicostatistical Database project, and so far, we have not observed a single case of such a synchronic polysemy or diachronic lexical change. The only possible solution is to assume a *third* meaning as a “buffer zone” between ‘meat’ and ‘liver’, from which both these meanings could be effortlessly derived. Whether this is possible is unclear; but until such a meaning has been found, we find it wise to reject Dolgopolsky’s **naḶo* as a credible Nostratic etymology, because we have failed to provide a realistic (let alone “optimal”) historical scenario that would derive the alleged descendants of this root from its reconstructed form.

Of course, this careless approach towards distribution and semantics permeates not only the research of Dolgopolsky; representatives of the Moscow School have often been guilty of it as well, and it remains infectious even in modern phylogenetic studies – for instance, cognate distribution and semantic precision are not being seriously taken into account in Pagel et al. (2013), one of the latest studies on the lexicostatistical phylogeny of Nostratic. In this paper, the authors may easily use any meaning associated with a given root on the shallowest level of taxonomy, and project it onto the deepest taxonomic level possible.

To quote an example, Pagel et al. (2013) consider the scantily attested IE root **pel-*, used in morphologically secondary stems for the meaning ‘ashes’ in the Balto-Slavic group (Baltic **pel-(V)n-* ‘ashes’ with an additional suffix, Slavic **pe-pel-* ‘ashes’ with

reduplication), to be directly cognate to Proto-Altaiic **p'olnie* 'ashes'. Simple analysis shows that, while **p'olnie* is indeed the main candidate for the status of Proto-Altaiic 'ashes' – it can be reconstructed for Proto-Mongolian and Proto-Tungus-Manchu with the meaning 'ashes' and for Proto-Turkic with the relatable color term meaning 'grey, dun, ash-colored' (Starostin, Dybo & Mudrak 2003: 1170) – it is unlikely that **pel-* was the basic Proto-Indo-European term for 'ashes', having only retained its original meaning in one internal group; the indisputably optimal candidate for Proto-Indo-European 'ashes' is the widely attested form **xās-*.

It is important, however, to understand that the weakness of this Balto-Slavic/Altaiic parallel, preventing the compared forms from the status of a "lexicostatistical parallel" with the meaning 'ashes', is not as problematic as the weakness of Dolgopolsky's **riaKo*, because in this case one might at least put forward a realistic historical scenario, such as an independent (homoplastic) semantic development "grey" → "ashes" in certain subgroups of Indo-European and Altaiic. And generally speaking, we are not advocating for a straightforwardly "reductionist" or "purist" approach to evaluating such hypotheses of long-distance relationship as Nostratic, where it is very easy to set up for oneself such a long line of filters (see, e.g., the list in Campbell & Poser 2008: 246–250) that every single etymology will end up being rejected for one reason or another.

What would instead be much more useful is a notion of *reconstructibility*, which could roughly be defined as follows: morpheme M_1 , possessing the meaning [S] in language L_1 , is deemed *reconstructible* in this meaning for protolanguage $P[L_1 \dots L_n]$ only if none of the languages $\{L_2, L_3 \dots L_n\}$, related to L_1 , show evidence for the presence of morpheme M_2 with the same meaning [S] that shows more *historical power* than morpheme M_1 .

"Historical power", in its turn, may be defined as the collective weight of plausible arguments in favor of reconstruction, such as:

- (a) the *topological* principle, i.e., a scenario which implies a minimum amount of parallel or backward semantic developments within the given phylogenetic tree for any given morpheme;
- (b) the *external etymology* principle, i.e., roots that have plausible external cognates (especially with the same meaning) have certain advantages;
- (c) the *internal etymology* principle, i.e., a morphologically simple term has some advantage over a transparently derived one;
- (d) the *semantic plausibility* principle, i.e., semantic shifts assumed in the scenario must be supported with typological evidence;
- (e) the *areal effect exclusion* principle, i.e., a meaning that is represented exclusively in neighboring lects and violates the topological principle is suspected to be contact-driven.

For a detailed and practical application of these principles to the reconstruction of Indo-European and Uralic basic lexicon see Kassian, Zhivlov & Starostin (2015).

In practice, this means that, when investigating problematic linguistic relationships like Nostratic, one should always pay attention to the "degree of reconstructibility"

of any given *comparanda*. For instance, in theory an item like Proto-Saami **tāktē*- ‘bone’ may be of Nostratic origin – but we should only include it in our etymologies if we trace it back to a hypothetical Proto-Uralic stem with a meaning *other* than ‘bone’, since in terms of “historical power” **tāktē*- clearly loses out to Proto-Uralic **luwi* ‘bone’.

On the other hand, we often run into situations where two or even more morphemes show more or less the same “historical power”, so that even a careful weighting of the evidence gives us no clues as to which of these “competing” forms should be preferred. For instance, Proto-Uralic has at least three or four candidates for the basic meaning of ‘black’, such as Balto-Finnic **musta*, Saami **ćāppe-*, Mari-Permic **simV*, etc. Any one of these roots could also theoretically reflect Proto-Nostratic ‘black’, yet it is hardly likely that they could *all* reflect Proto-Nostratic (or, for that matter, Proto-Uralic) ‘black’, and assigning them to three different Nostratic etymologies with the meaning ‘black’ would be methodologically incorrect.

To take another example that goes all the way back to the original Illich-Svitych corpus, a comparison between Indo-European **ed-* ‘to eat’ and Mongolic **ide-* ‘id.’ agrees with the established system of phonetic correspondences and, on the surface, looks tempting; but it would be wrong to overlook the fact that the main Altaic root with the meaning ‘eat’, displaying far more “historical power”, is **ǰē* (not an isolated Mongolic root, but a straightforward parallel between Turkic, Tungusic, and Korean languages, cf. Starostin, Dybo & Mudrak 2003: 1530). More than that, **ǰē* has a phonetically and semantically satisfactory cognate in Proto-Uralic **sewi* ‘to eat’, and this means that we either have to reject the “Indo-Mongolic” comparison altogether (acknowledging that the “etymology” is really just a chance resemblance), or retain it only under the assumption of some original meaning that was different from the basic ‘to eat’. In the latter case, a thorough search for additional potential etymological parallels in other branches of Nostratic could perhaps be of use, or we might turn towards internal reconstruction: for instance, if the Indo-European equivalent for ‘tooth’, as is sometimes suggested, is indeed a former active participle from this root, this could indicate that the original meaning was ‘to bite’ rather than ‘to eat’, since the semantic development ‘bite’ → ‘tooth’ is typologically normal (e.g., in Eskimo languages), whereas ‘eat’ → ‘tooth’ seems unattested.

All of this means that the huge etymological corpus that we see in Dolgopolsky’s dictionary merely represents the second stage of etymological research, out of at least three necessary ones: (a) accumulation of “raw” material based on phonetic and semantic similarity; (b) filtering of the accumulated material through the established system of regular correspondences; (c) distributional-semantic organisation of the data into a genuine “informational database” on the lexical signs of Proto-Nostratic, with each component of each etymology tested for “reconstructibility”. In his reply to criticism, Dolgopolsky essentially negates the importance of stage (c) – but it is our firm conviction that only the acknowledgment of its critical significance can truly advance Nostratic linguistics onto the next level of development, as well as strengthen its positions on the international levels of the scientific community.

Let us now see how the third direction in Nostratic linguistics (the “Moscow School” in its modern formation) addresses this issue. If we are to judge by the officially published corpus – the “Nostratic etymology” database, available on-line at the “Tower of Babel” website,⁵ the results will not seem very impressive. Of the circa 2000 comparative sets included in the database, the most solid ones are usually those that go back directly to the original Illich-Svitych materials; the rest, just like Dolgopolsky’s etymologies, are not accompanied by explicit depictions of possible scenarios of semantic development. There is only one positive distinction: as a rule, the database collects *comparanda* that are relatively well represented in descendant branches of Nostratic, i.e., Sergei Starostin and his colleagues, in full accordance with their criticism of Dolgopolsky’s methodology, usually do not include forms that are found only in Germanic, only in Saami, only in Mongolian, etc. (although even here there are some slippery exceptions). However, as has already been stipulated above, “widespread distribution” is only one, and not even the most necessary, condition of “reconstructibility”.

What are we to do in this situation? Naturally, the open character of the Nostratic database presents a good incentive for constant revision and detalization of the etymologies. And as sacrilegious as the idea might sound for some Nostraticists or comparative linguists in general, it is arguably a flaw of the Nostratic hypothesis that, up until now, the major emphasis in all of its schools has been placed on regularity of correspondences, to the exclusion of other aspects – because in long-range comparison, where “perfectly solid” etymologies are few and far in between, it is often tempting to remain satisfied with the discovery of superficial semantic proximity, provided we have managed to convince ourselves of the reality of our system of phonetic correspondences. This leads to distorted, historically unrealistic perspectives, such as Bomhard’s system, or, for that matter, quite a few attempts to demonstrate “phonetically convincing” long-distance relationship at the expense of all the other aspects of reconstruction – cf., for instance, Christopher Ehret’s work on Nilo-Saharan (2001), as criticized in G. Starostin (2013).

A nice, if small, step forward is the recent publication by Kassian, Zhivlov & Starostin (2015), where we formulate explicit criteria for the reconstruction of 50 Swadesh concepts for Proto-Indo-European and Proto-Uralic. Such a formalization has allowed to reject a whole series of “parasitic” candidates for specific basic meanings in proto-languages – yet at the same time it has become possible to identify certain roots that are quite solid candidates, despite their relative “unpopularity” in Indo-European and/or Nostratic studies due to scarce (but still topologically strong) distribution (e.g., the Hittite/Tocharian isogloss for ‘to drink’, see Table 2, easily comparable with the basic Uralic root for ‘to drink’). The overall result for this type of analyses, therefore, may be predicted as a rational transition from quantity to quality of evidence, as we replace large bulks of questionable, historically unrealistic etymologies with a smaller body of cases, each of which is associated with its own historical scenario.

⁵ See fn. 4 above.

Before turning to final conclusions, we would like to briefly mention yet another serious problem facing Nostratic studies – that of the robustness of family-level reconstructions. If several proto-languages are to be compared, we expect that the reliability of phonological and lexical reconstruction will be approximately the same within Proto-Indo-European, Proto-Uralic etc. Unfortunately, this is not the case.

As an example, let us take the Uralic family, sometimes mentioned in general handbooks as one of the most studied among language families of the world. According to a definition by Ilia Peiros (2008: 193), a language family can be called “known” from the historical point of view if “(i) there is a phonological reconstruction of its proto-language describing also histories of individual daughter-languages; (ii) means to identify borrowings into its daughter-languages; (iii) there is an etymological dictionary of the proto-language”. Points (i) and (ii) are immediately connected, because loanwords cannot be successfully identified without a good historical phonology.

Let us see whether these conditions are fulfilled for the Uralic family. The reconstruction of Proto-Uralic consonantism, largely carried out in the first half of the 20th century and codified in such works as Collinder (1960), rather adequately describes the development of Proto-Uralic consonants in the daughter languages, although even here there is ample space for improvement. The same cannot be said of the reconstruction of Proto-Uralic vowels. Traditional reconstruction of Proto-Uralic vocalism, developed by such scholars as E. Itkonen, is in fact a projection of Proto-Finnic vocalism onto the Proto-Uralic level, much in the same way as Proto-Indo-European reconstruction before Neogrammarians was a projection of the Sanskrit system on the Proto-Indo-European level. W. Steinitz attempted to formulate an alternative theory, where the role of “key language” was assigned to East Khanty instead of Finnic. Numerous deviations from expected reflexions were explained by him as a result of levelling of a vowel alternation that supposedly went all the way to Proto-Uralic, but was preserved as such only in Khanty. This method of “explanation” made Steinitz’s theory practically unfalsifiable; the theory was not supported by the majority of Uralicists. Unfortunately, the main alternative to “alternation theory” was the theory of “sporadic developments” or “tendencies” that in this framework take the place of Neogrammarian sound laws. Under this theory, no explanation is needed if the reflexes are not regular: the “tendency” *may* work, but it *need not* work in every case.

The consequences of this approach are much more serious than one might think. The problem is not so much in our inability to ascertain how precisely some reconstructed word sounded in Proto-Uralic, as in our inability to tell inherited words from borrowings and chance coincidences. Thus, unless the first condition (“a phonological reconstruction of its proto-language describing also histories of individual daughter-languages”) is fulfilled, the second condition (“means to identify borrowings into its daughter-languages”) cannot be fulfilled either. Consequently, the main source on Uralic etymology, *Uralisches Etymologisches Wörterbuch* (UEW, Rédei 1988–1991), abounds in “etymological rubbish” – a mix of chance coincidences and unrecognized loanwords. Uncritical use of this source in external Nostratic comparisons can lead to

grave errors. Unfortunately, neither Dolgopolsky nor Bomhard paid much attention to this problem.

In order to get out of the impasse, we need to have a theory of Uralic vocalism built not on projecting the data of one “key language” onto the Proto-Uralic level, but on a direct comparison of at least two daughter branches. Such a theory was proposed by Juha Janhunen (1981). Having reconstructed in sufficient detail Proto-Samoyed vocalism (Janhunen 1977), he compared this reconstruction with E. Itkonen’s Proto-Finno-Permic reconstruction (identical to Proto-Finnic in respect of vowels). This approach is in some way similar to an approach successfully used by Otto Dempwolff to reconstruct Proto-Austronesian, namely, comparison of selected daughter languages.

Janhunen also significantly reduced the corpus of Proto-Uralic etymologies, discarding those comparisons that exhibited irregular sound correspondences. As a result, there are only 140 Uralic etymologies in his corpus (including pronouns). For comparison, UEW lists 284 Uralic etymologies. Those who support a more “liberal” approach to etymology sometimes label Janhunen’s approach as “reductionist”. Nevertheless, subsequent studies by P. Sammallahti, E. Helimski, A. Aikio and others show that Janhunen’s model of sound correspondences can be confirmed by a serious number of newly found etymologies, while only some of the comparisons initially discarded by Janhunen were “rehabilitated” in the course of further research. Actually, Janhunen’s work became a turning point in the development of Uralic reconstruction.

Now it seemed obvious that the new theory of Uralic vocalism had to be checked and refined based on wider material, which would include not only Uralic etymologies as such (the etymologies represented both in Finno-Ugric and Samoyed), but also the etymologies on lower levels – Finno-Ugric, Finno-Permic and so on.

Eventually, such an attempt was undertaken by P. Sammallahti (1988), who adduced 533 etymologies on Uralic, Finno-Ugric and Finno-Permic level (compare UEW, where 900 lexemes are reconstructed for all those levels taken together). Sammallahti was the first to explicitly present the sound laws linking Proto-Uralic vocalism to that of Permic and Ugric languages. Nevertheless, he did not give any phonological rules linking Proto-Uralic to Finno-Volgaic languages (Mari, Mordvinic, Finnic and Saami); perhaps he believed them to be known all too well. In fact, in the history of those languages (especially Mari) there is still a great number of unresolved questions, and the absence of explicitly formulated rules may become a hindrance to solving and even to posing those questions. As regards Permic and Ugric languages, many of Sammallahti’s conclusions regarding development of vocalism in those languages were later put to doubt (cf., e.g., Reshetnikov & Zhivlov 2011; Zhivlov 2014; Aikio 2015). The major breakthrough, which Sammallahti’s work eventually achieved, was not the phonetic rules he formulated (they often do not work on examples from his own list of etymologies): the author carried out an effective “purge” of the whole etymological corpus on the Finno-Ugric and Finno-Permic levels, successfully following Janhunen’s Proto-Uralic work. For the first time a reliable body of Uralic, Finno-Ugric and Finno-Permic etymologies became available for further study (although the phonological side of reconstructions in Sammallahti 1988 is not as reliable as the comparisons themselves).

The consequence of all this for Nostratic studies is that uncritical use of Uralic etymologies taken from UEW or earlier sources in external comparison can lead to a significantly distorted view of Nostratic phonology and lexicon. Future attempts to revise Illich-Svitych's system of correspondences must take into account the recent progress in Proto-Uralic reconstruction, as well as in the reconstruction of other daughter branches of Nostratic.

Let us now turn to final conclusions. The most serious argument for the validity of a certain protolanguage reconstruction is its (typological) *realism* and *systematicity*, which, as we have shown, are not usually seen as the top priority for Nostratic reconstruction. Nostratic etymology will become truly convincing only when, figuratively speaking, the "Proto-Nostratic person" will be shown to consist of one 'head' with one 'brain', one 'nose', one 'tongue', one pair of 'hands', etc., with the reasons for these specific reconstructions (or, to be more accurate, the *optimality* of these reconstructions) clearly delineated in the dictionary. Ultimately, it is precisely with this aim that our team has set up the Global Lexicostatistical Database (GLD), a project bent on reconstructing the "optimal" basic lexicon sets for proto-languages (including Nostratic in the long run).

While the task of compiling accurate and comprehensive Swadesh wordlists for all Nostratic languages is rather time-consuming and will probably take a few more years to bring the completion, one of the intermediate "shortcut" tasks of the GLD was to compile a set of optimal reconstructions for the ultra-stable 50-item layer of the Swadesh wordlist on the levels of individual groups and families. A preliminary lexicostatistical and glottochronological evaluation of the evidence⁶ has yielded some interesting results, such as increasing the alleged time depth for Nostratic (clearly exceeding ten thousand years), putting Indo-European in close proximity with Uralic as its nearest relative (rather than Uralic with Altaic, as is sometimes conjectured by Nostraticists), placing Dravidian and Kartvelian on the far periphery of Nostratic (which, curiously enough, agrees with the scheme that was proposed by Joseph Greenberg on the basis of his "multilateral comparison"), and definitively excluding Afro-Asiatic from the macrofamily (agreeing with an earlier proposal by Sergei Starostin). Most importantly, restricting our comparanda to the 50-item set has allowed to clearly delineate between Nostratic and the "non-Nostratic" language families of the Old World, such as North Caucasian or Austronesian – families whose "optimal" 50-item sets show no links to Nostratic whatsoever.

We believe that this kind of scrupulous analysis of the "ultra-stable" elements of the basic lexicon – precisely those elements that may be expected to persist over many millennia, serving as the most reliable "genetic markers" for language relationship – should currently be the focal point of Nostratic research, rather than largely futile attempts to come up with even larger etymological corpora for this putative macrofamily. It is also of crucial importance to pay diligent attention not only to those words

⁶ All the lexical data, accompanied with cognation judgments, are openly available at: <<http://starling.rinet.ru/new100/trees.htm>>.

in any given branch of Nostratic that seem to *have* cognates in other branches, but also to those that do *not* have such cognates – in fact, it may be wise to shift the focus away from the reconstructed *root* (understood as “relatively precise phonetic shape, correlated with a relatively vague semantic field”) and turn it to pre-set *precise meanings*, organised into semantic fields and directly associated with phonological reconstructions. In other words, Nostratic etymology, instead of answering the question “are there any potential cognates for the Indo-European word for ‘nose?’”, should rather ask the question “what is the optimal reconstruction for the meaning ‘nose’ in Proto-Nostratic?”. This method, while it may be deemed too cumbersome and superfluous for Indo-European (not that applying it to Indo-European could not have resulted in quite a few important historical discoveries), seems to us the only reasonable solution for advancing etymological research on such levels as Proto-Nostratic.

Ultimately, it is our firm belief that Nostratic linguistics, while currently in a state of mild stagnation, may overcome this state by means of important methodological reforms – even if many of these reforms might not be for the liking of conservative supporters of the hypothesis who believe that the “classic” comparative-historical method, good enough for Indo-European, would be just as good for Nostratic without additional restrictions. We also believe that these reforms, in the long run, will be useful not only for all the other promising hypotheses of long-distance relationship as well, but also for further research on uncontroversial families of smaller time depth, including Indo-European itself.

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