

## II. BESPRECHUNGSAUFSATZ

### The Recent Development of Nostratism

It may be useful to discuss some (of the many) weak points of Nostratism. Some time ago I enjoyed dealing with this pseudolinguistic method (1973a). In the meantime Nostratism has developed, but not improved. This is what I wish to show, with reference to two works by Helimsky (one of the most outstanding representatives of Nostratism) as a point of departure (1986, 1992).

1. In Illič-Svityč's original conception (1971) "Nostratic" comprised six language families: Indo-European (IE.), Semitic, Kartvelian, Uralic, Dravida, and Altaic. In the meantime, the number of its alleged members has soared to infinite heights. From the outset, Nostratism had a surfeit of deficiencies. One of them is the lack of nuclear basic words (NBW.) in Nostratic and particularly in "Altaic", which comprised four language families (Tk. = Turkic, Mo. = Mongolian, Tg. = Tungus, Korean), whose relationship is still disputed. In the following explanations I will leave aside Korean, since so many adherents of the "Altaic" relationship do not acknowledge this language as belonging to "Altaic" (Cf. Janhunen). In Poppe's index Mongolian words cover 32 columns, Tg. 22, Tk. 21, but Korean has only 2, comprising only 82 words - too limited a material.

According to Helimsky (1932: 326) "it is possible to trace the Proto-Altaic origin of 10 (out of 11) KGW. [= NBW.] in this list" (namely Doerfer 1988). Let us consider this.

1.1. "'eye' Alt[aic] \**niā* > Mong. *ni-dün* ..., Tung. \**niā-sa* ... Turk. *jāš* 'tear'". - The Tungus root must not be \**njā(sa)*, but \**jā(sa)*. This is confirmed by the forms of Evenki, Solon, Negidal, Arman, Lamut, Oroch, Udehe, Ulcha, Orok, Kur-Urmi (*esal, isal, isala*), Manchu, and Jurchen (*yaši*). These are twelve of the thirteen Tg. languages. An exception is to be found in Nanai *nasal* (literary language and some dialects). Helimsky (following Starostin) combines \**jā(sa)* with Nanai *na(sal)*, thus reconstructing *niā* (= *njā*). This is equivalent to the method of a Romanist who combines Italian *cant-are* 'to sing' [kantare] with French *chant-er* [šäte] > Proto-Roman = Latin \**kšant-are*. Nanai has influenced adjacent Kur-Urmi (*nasal*, cf. Doerfer 1984) and Ulcha subdialects (*nasar*). On the other hand, such distant Nanai dialects as Bikin (*isala ~ nisala*) and Gorin (*isala*, Grube) ~ *yāsa* (Maak) evidently go back to \**jāsa*. Furthermore, the Samar subdialect of Nanai has also *nasar* (cf. also Manegir *nise*). It would, then, also be possible to reconstruct, using Helimsky's method, Altaic \**nja-sa*. But when we investigate the problem from the aspect of linguistic geography (a method almost completely neglected by Nostratists) it is not difficult to see a) that the original Tg. form can only be \**jā-sa*; b) a disturbance emanated from Central Nanai dialects (not touching other, more preservative ones, spoken at the northern and southern peripheries of Nanai). Furthermore, Central Nanai *nasal* cannot be traced back to \**njā*, since, e.g., Tg. \**njā*- 'to putrify' has become Nanai *ná(i)-, nja-*. This is the regular development, namely Tg. \**njV-* > Nanai *njV-*. Sound laws are a norm, not a rule; but it is not possible to detect

the norm or deviations from it without the help of solid linguistic geography. (Nostratists terminate their investigations where the problems begin.)

We should also note that some Tg. dialects (beside the norm \**jā-sa*) have forms with *-l* (generally a plural suffix), *-li*, *-la*, *-r*. We find *-l* in Solon, Lamut, Kur-Urmi, Ulcha, Orok, Nanai, *-li* in Ulcha, *-la* in Nanai (Gorin, Bikin), Kur-Urmi (Hezhen), Arman, *-r* in Ulcha, Nanai; Jurchen *-i* in *yaši* is another deviating form. This is a plenitude of forms; and it seems meaningful that just such older editors as Grube and Maak present Nanai forms satisfying the norm \**jāsa*. The root \**jā* has been preserved in Nanai (and in all Tungus dialects) in the word *jarsi-* 'to look' < \**iā-ro-si-*.

To conclude, it is evident that Mo. \**ni-dün* (or \**nidü-n*) cannot be compared with Tg. \**jā-sa*.

In other words: in a small territory of Nanai \**jāsa* has been transformed, replacing *j-* either by *n-* or *ŋ-*. This is an irregular development, caused, presumably, by taboo (to avoid the similarity to *yāyan* 'shaman's ritual song').

We must distinguish between three "realms":

(1) The "first realm" is that of sound laws. They are the norm of the development. All scholars quite naturally try to find clear norms. However, a "norm" is not equivalent to a "rule". The norm of human teeth is 32. The rule (the average, when we count all human teeth and divide their number by the number of the persons themselves) may be 24.3712 ... But the norm is always valid, not depending on its numerical realization. At any rate, exceptions to the norm must be explained. The norm is, say, predictable (that does not mean that this prediction is realized in every single case).

(2) The "second realm" is that of explicable exceptions, e.g., dissimilations (as Latin *arbor* > Spanish *árbol*, Italian *albero*), metatheses, many assimilations, all explicable by allegro speech; sound imitations, synaesthetics, baby words, popular etymology, mixtures of dialects, but also by analogy (e.g., in Tk. of Turkey *oyan-* 'to awake' has become *uyan-*, in analogy to *uyu-* 'to sleep': awakening is the termination of sleeping). The second realm is not predictable, but it is understandable for reasons that later become apparent.

(3) The "third realm" contains, above all, taboo phenomena. Since these are not external, phonetic, but internal, psychic, they are, like psychoanalysis, neither predictable nor understandable. Although very powerful, they can only be ascertained or divined. (Many items in dialect dictionaries belong here.)

This "third realm" has also to be considered - not for its help making sound laws or for finding long range connections, but as a warning for the researcher: that a form may be inexplicable. The normal method of a linguist should be:

- (1) state what is clear,
- (2) investigate that which on thorough examination can be clarified,
- (3) leave aside that which cannot be clarified (as, e.g., the Nanai forms with *n-*, *ŋ-* in the terms for 'eye').

These three rules hold for all scholarly studies. And I believe that the more one concentrates on realms (1) and (2), the third realm becomes smaller. But when one does not distinguish between these three realms, one cannot adequately explain anything, and one arrives at the land of fantasy. The confusion of the three realms is proof of an unsatisfactory linguistic method.

1.2. "'hand': Alt. \**ḡālā* > Turk. \**äl(ig)* ..., Tung. \**ḡāla*". - Illič-Svityč would not yet have dared such a comparison; in his system (1971: 150) \**ḡ-* has not been considered. Actually, only a very few, thoroughly doubtful Tk.-Tg. equations exist, with the exclusion of Mo. (for the reason cf. Doerfer 1985: §8.1). In the Tk. word *-ig* may indeed be a suffix (as in *aziḡ* 'eye tooth'). But comparisons of the type Tk. vocalic Anlaut = Tg. *ḡ-* have not been documented by Helimsky, nor by anybody else. (I suspect that they don't exist). In comparing Tk. *ä* = Tg. *ā* Helimsky follows Starostin. Illič-Svityč, more cautiously, compares Tk. *ä* = Tg. *ā*, Tk. *ā* = Tg. *ā* (1971: 171). Not so Starostin. For him a) vowel quantity does not matter, b) Tk. *a* may be = Tg. *a* or *ā*, Tk. *ä* may be = Tg. *ä* and *a*, depending on a divined (not documented) Nostratic \*vowel of the second syllable. But when we scrutinize the whole of Starostin's material and assess all the examples he gives in his book, we see that he actually compares Tk. *ä* with numerous Tg. sounds, namely *ī, ā; o, i, u, ū, e*, and Tk. *a* is compared with Tg. *a, e; i/ī, o, (ō), u, ü/ū*. Here is a complete list, as an abstract of Starostin's main rules, followed, after a semicolon, by all other comparisons (e.g., on pp. 274-297).

Tk.	Mo.	Tg.
<i>i</i>	<i>i; a, ū, ö, e, u</i>	<i>i; e, ö (ū), au, jā, o, ū, a</i>
<i>e</i>	<i>e; i, ä, ü, a</i>	<i>e; o, i, ā (jā), u</i>
<i>ä</i>	<i>a, e; ö, i, ü</i>	<i>ī, ā (jā); o, i, u, ū, e</i>
<i>ü</i>	<i>u, ū; e, ö, i</i>	<i>u; e, o, i</i>
<i>ö</i>	<i>o, ö</i>	<i>o; ī, e, u, ü</i>
<i>ī</i>	<i>i; ū, u, a, o</i>	<i>i, u (o); jā</i>
<i>u</i>	<i>u, ū, o, abu/au/ayu, i, ö, u, ü; e, o (ō)</i>	<i>u, ū; e, o (ō)</i>
<i>o</i>	<i>o, ö; u</i>	<i>ū (~ ā), ö; ū, u (ū), i, a, o (ō)</i>
<i>a</i>	<i>a, e; i, o, ö, u</i>	<i>a, e; i/y, o (ō), u, ü/ū</i>

This means that almost every vowel may be compared with any other. Starostin's method (if one may call this a method) is Make a sound law, but don't abide by it. The same "method" is followed in comparing Mo. with Tk., etc., e.g., Tk. *bar-* 'to go' = Mo. *mör* 'way' (*a = ö*).

This recalls Voltaire's words "L'étymologie est une science où les voyelles ne font rien et les consonnes fort peu de chose" (etymology is a science in which the vowels don't matter at all and the consonants are of little account. It is obvious that when the vowels are *de facto* disregarded the number of available comparisons augments enormously. This, however, is no method, but a trick.

Incidentally it is very likely that Tg. \**ḡāla* must be explained as \**ḡā-la*, with the root \**ḡā* and the well-known Tg. suffix of body parts *-la* (cf. 1.1, also 1.6: \**hāb-ti-lä*, furthermore Doerfer 1984: 241 \**hūi-ktä-lä* 'tooth', 244 \**bāḡdi-lä* 'leg', \**sapo-la* 'a kind of hairs'). The root \**ḡā* survives also in Tg. \**ḡā-dī/-ri/-si-* 'to motion to somebody (with one's hands)' > 'to call somebody in'. It is obvious that a comparison with Tk. *äl(ig)* is hardly possible.

1.3. "'head': Alt. \**mal'V* > Turk. *baş* (< \**bal'(č)*) ..." Helimsky's only compari-

son is with a Korean word, and this he himself judges to be "somewhat problematic". Furthermore, a sound law Tk. *b-* = Korean *m-* has not yet been proven.

1.4. "'foot': Alt. \**pal'kV* > Tung. \**palgan*", and also "Alt. \**p'agdV* > Tk. *adaḡ*, Mong. \**fadaḡ*, Tung. \**pagdi(-kī)*". - Tg. *palgan* normally means, not 'foot', but 'sole (of the foot or of the hand = palm)'; 'foot' is \**bāḡdi* in Tg. With Tg. *palgan* one may better compare Mo. *halayan* 'sole, palm'. This is no NBW, but a PBW. = peripheral basic word, and PBW.s are easily loaned, as in the English muscle, stomach, palate, also 'palm' itself, all these words going back in the last instance to Latin or Greek. Mo. \**fadaḡ* is not provable, since the word is documented only in recent texts (where *h-* < \**f-* < \**p-* has vanished) and is lacking in those dialects which have preserved ancient *h-*. Since the meaning in Mo. is, not 'foot', but 'extremity, estuary, worst' (all of them PBW.s) the thesis that this is a recent loanword must be preferred.

1.5. "'ear': Alt. *k'ūjhu* (~ \**k'ūlju*) > Turk. \**kul-kak*/*\*kul-gak* ... preserved also in Mong. *gulki* 'earwax'". - The usual Mo. word for 'ear' is *čiqin* < \**čiqin* or \**tiqin*. Perhaps 'earwax' is the earlier meaning in Tk.: 'earwax' is a PBW. and it is a well-documented fact that PBW.s rather frequently become NBW.s, whereas the development NBW. > PBW. hardly occurs. The original Tk. term for 'ear' may have been \**äl'i*, preserved in *äsi-d-* 'to hear' (with a well-known suffix). The nuclear terms are different in the three "Altaic" languages: Tk. \**äl'i*, Mo. \**t/čiqin*, Tg. \**siān*; moreover, the roots of these three NBW.s are not to be found in any other of the respective languages, not even in a different meaning. There is a contrast to this in IE.: IE. \**ped* 'foot' has been lost in Slavic - as far as the original meaning is concerned - but it still exists in *pod* 'beneath'. But there is no correspondence to, say, Tg. \**siān*, either in Tk. or in Mo.)

1.6. "'nose': Alt. \**k'/u/aḡa* > Mong. \**qaḡ* in \**qaḡ-bar* (> *qabar ~ qamar* 'nose') ... *qoḡ-sijar* 'muzzle, beak, snout, nose', Tung. \**xoḡa* 'the bows', \**xoḡa-cta* 'nose'". - The earliest Mo. form is *qabar*. It is not comparable with \**hoḡa*, allegedly 'the bows', whose actual meaning is 'spike, extremity, protrusion' (originally of the boat, the horn, the boot, only metaphorically of the face: a PBW.). Mo. *a* (in *qabar*) ought not to be compared with Tg. *o* (in \**hoḡa*), to avoid Voltaire's irony. Mo. *qoḡsijar* is a derivation of the root *qoḡ* (Lessing 962-3) 'hollow, hollow sound', it has nothing to do with *qabar*. Furthermore, Mo. *q-, k-* cannot be compared with Tg. \**h-*. (The original Tg. sound is, not \**x-*, but \**h-*, cf. the arguments of mine in 1973b. Prof. Ikegami, Sapporo, was so kind as to send me tape recordings of Ulcha; they showed that the actual pronunciation of Ulcha "x-" is unlike Russian, German, Scotch *x-*, but similar to the rough, heavily aspirated Chinese *h-*. That is to say: this consonant may be due to Chinese influence; we may transcribe it *H-*.) Helimsky's comparison Mo. *q-* = Tg. *h-* is apparently due to two facts: a) Illič-Svityč's wish to find the tripartition *t' : t : d, p' : p : b, k' : k : g* in all Nostratic languages (1971: 147); b) to Cincius' comparison of some Tg. words with *H-* in Nanai (Tg. \**h-*) with Mo. words beginning with *q-/k-*. This attempt has failed. Cincius gives, apart from the special case Mo. *qorin* 'twenty' = Nanai *Horin*, cf. Doerfer 1985: 151-3, only very few (namely 13) comparisons of this type, such as Nanai *Hado* = Mo. *kedü(i)* 'how much' - unacceptable for phonological reasons. The same holds true for Manchu *üren* 'corpse' = ? Mo. *keḡür*; Nanai *kun, ukun* 'breast' = ? Mo. *köken*; Lamut *ömge* 'skin' = ? Mo. *köm*, Nanai *uḡe* 'musk-deer' = ? Mo. *küderi*; Nanai *Hoḡia-* 'to lean' = Mo. *qajai-*; Nanai *Hesikte* 'squama' = ? Mo. *qair-un*; Nanai *Hai* 'who' = ? Mo. *qamiya* 'where'. These are examples with ?- in Evenki (according to the rule Tg. \**h-*

> Nanai *H-*, Evenki *ʔ-*). We find only four examples of this type which show some similarity and which at first sight might be compared: Mo. *qabud-* 'to swell, to puff' ~ Nanai *Haolo-* < proto-Tg. *\*habolu-*; *qali-sun* 'the outer layer of something' (e.g., husk, rind, bark) ~ *Halo-hta* 'flesh side (= insideside) of a fur' (semantically not so clear); *qoryol* 'dung of sheep, goats, or camels' ~ *Hori-hta* 'dung of rein-deer'; *qusi* 'cedar' ~ *Hosa-hta* 'acorn' (Evenki *usi-hta* 'oak'). In contrast to these few and unclear examples we find six sure comparisons Nanai *H-* = Mo. *ʔ-* (cf. Doerfer 1985: 150-4, e.g., Mo. *ebčë-gün* 'sternum' ~ Nanai *Heuč-ile*). But the normal correspondences are: Mo. *q-/k-* ~ Manchu *x-* (older loan) or *k-* (younger loan), Evenki *k-*, Nanai *k-* (e.g., Mo. *qada* 'rock' ~ Evenki *kadar*, Manchu *xada*, Nanai *kadar*). There are altogether 41 examples, namely in Doerfer 1985 numbers 7, 8, 46, 100, 108, 118, 119, 128, 135, 152, 177, 178, 199, 223-5, 233, 236, 237, 258, 262, 371, 372, 377, 380, 381, 398, 400, 424, 435, 436, 438, 448, 549, 631, 655, 656, 658, 659, 669, 670). Furthermore, we find Manchu *x-* = Nanai *H-* (21 examples, namely 9, 87, 198, 200, 232, 234, 235, 239-241, 257, 423, 434, 635, 636, 657, 667, 668, 671-3). These 62 clear examples constitute the norm; Mo. *q-* = Nanai *H-* is an unclear random exception. To summarize, a comparison Mo. *q-/k-* ~ Nanai *H-* (Tg. *\*h-*) cannot be proven, and this is one of the reasons why the whole comparison Mo. *q-/k-* ~ Nanai *H-* (Tg. *\*h-*), which has also other weaknesses, ought to be cancelled.

1.7. "'mouth': Alt. *\*am/a/* > Mong. *aman*, Tung. *\*am-ŋa* id., (?) Turk. *\*am* 'vulva'" - Helimsky himself admits several weak points of these comparisons. We must consider: Tk. *am* (recte *ām*) goes back to *\*hām* (so in Khalaj) < *\*pāma*; in Mo. *\*haman* ought to correspond, but it is *aman*. It is unclear whether the Proto-Tg. form is *\*am-ŋa* (< *\*am-ga*) or *\*aŋ-ma*. Both *-gA* and *-mA* are well-known Tg. suffixes for body parts (cf. Doerfer 1984): *aŋ-ma*, from a synaesthetic root *\*aŋ* 'hole, opening' is likelier than *\*am-ŋa*. On the whole, the comparison *ama(n) ~ aŋ(ma) ~ pāma* is weak, it is apparently due to an "elementary relationship".

1.8. "'heart': Alt. *\*miāñV-m* > Tung. *\*miāwan ~ niāman*". - Tg. is compared only with Korean, the correct Tg. form ought to be *\*mjāban*; "*~ niāman*" is superfluous, since *niyaman* (Manchu) follows a well-known sound law *\*mjā- > njā*, also found in Manchu *niyakûra-* 'to kneel' = Nanai *mixorān-*, Jurchen *mjakuru-*, cf. Benzing (40). Helimsky's other comparison leads to Mo. *\*dirük-kän* or *\*jirük-gän* ~ Tk. *\*dür-äk* or *\*jür-äk*. These forms are similar, but the comparison is not obligatory.

1.9. "'hair' I: Alt. *\*kʰIV* ~ Turk. *qil* 'hair', Tung. *\*xiñna* (> /misprint, he means < /*\*xil-ŋa*)". - Tk. *qil* cannot be compared with Tg. *\*xiñga*, because Tk. *q-* ≠ Tg. *\*h-*, cf. 1.6. Furthermore, the Tg. forms (CSS.I.317: Evenki *inŋakta*, Nanai *siyalta*, etc.) evidently go back to *\*hin-ga-hta* or *\*hiñ-ga-hta*. There is not the slightest proof of an original *-l-*, comparable to Tk. *qil*, but Helimsky reconstructs a Tg. form with *\*-l-* (although contrary to all Tg. examples) in order that it be comparable with Tk. *qil*. This is a method quite typical of Nostratism. It resembles "socialist realism": what matters is not how things are, but how they ought to be.

"'hair' II: Alt. *\*pʰñV* > Mong. *\*fūsün* (> [recte: <] *pʰün-sün*". - This comparison is not so obviously false as 1.9.I, but the Tg. form is to be found only in Manchu, where it is *fuñexe*, Jurchen *funirxei*. This form might go back to a root Tg. *\*põñä*, as well, but is then incomparable with the Mo. word - which, in its turn, may also go back to a root *\*pü*. Another solution is offered by Poppe 1960: 70: that the Manchu word is connected with Mo. *hüne-gen* 'fox', and that the original meaning of the

Manchu word is 'fox fur'. The whole comparison is thus unclear, but Poppe's proposal seems to be the most readily acceptable for an "Altaist".

1.10. "'tongue': Alt. *k'älä* > Mong. *kele(n)* ... Tung. *xil-ŋü* ...". - Tg. word consists of three sounds, none of which is comparable to the three sounds of Mo. As to Mo. *k-* Tg. *\*h-* cf. 1.6; Mo. *-e-* is not comparable to Tg. *-i-* (cf. the quotation of Voltaire in 1.2); and the Tg. forms evidently go back to *\*hin-gü* or *\*hiñ-gü* (cf. Cincius I.316-7). The few Tg. forms with *-l-*, in some Evenki dialects, Kur-Urmi, Manchu, and Jurchen are influenced by *ile-* 'to lick', just as Latin *lingua* (Engl. *tongue*, etc.) has become *lingua* under the influence of *lingere* 'to lick', cf. Pokorny I.223. This is a case of analogy, the "second empire". In this respect Miller 40 is right in criticizing Benzing's reconstruction *\*xilŋü* and reconstructing in our word 1.9 *\*xin-ga-hta*. It is always worthwhile to consider parallels in other language families, which arose from the same conditions, the same life of languages. Nostratists do not compare languages, but accidental similarities.

To summarize: Helimsky's opinion (p.328) that "the majority of the above-cited etymologies fit the highest standards of comparative linguistics" cannot be confirmed. Most of the ten examples contradict sound laws and/or are refutable for other reasons. Only 1.5, 1.7, 1.8, 1.9 show some vague traces or hints of plausibility (plausibility is not a priori equivalent to correct), and 1.10 looks fairly sure, but may be synaesthetic, just as Swahili *kelele*, IE. *\*kel-* 'chatter', etc. An ideal and fully convincing result would have been that in the ten numbers quoted above Tk. would be comparable in most cases to Mo. and Tg. (maximum: 30 plausible comparisons). Let us compare the situation in the ten above-cited comparisons with that of *actually* genetically related languages:

	Dubious	"Altaic" Plausible
'eye'	-	-
'hand'	-	-
'head'	-	-
'foot'	-	-
'ear'	Tk.-Mo.	-
'nose'	-	-
'mouth'	Mo.-Tg.	-
'heart'	Tk.-Mo.	-
'hair'	Mo.-Tg.	-
'tongue'	-	Tk.-Mo.

Let us give one point to "dubious" and two points to "plausible" (or "correct"). This would mean 6 points (of 60 possible ones) = 10% for "Altaic". Cf. Doerfer 1988: 268-281. For Semitic one may find 60 points = 100%, for Karthvelian 44 = 73.3%, Dravidian 46 = 76.7%, Uralic 48 = 80%, IE. 49 = 81.7%, Bantu 30 = 50%, Austronesian 39 = 65%. This enormous difference constitutes a "qualitative jump" and is not explicable by "time depth". Furthermore: In "Altaic" the "general equations" (= identities of all compared members) are lacking (in contrast to, e.g., IE.). It would be

fair enough to weight these much higher, say 10 points (and also to weight "plausible" higher: not 2, but 5 points). In this case, "Altaic" would get 9 points out of 500 = 1.8%, Bantu (to quote the lowest scoring of the items given above, and a group of languages not sufficiently investigated before the 19th century) 225 = 45%, i.e., 25 times as much as "Altaic".

2. According to Illič-Svityč 1971 (pp.147, 68-9) the following sound correspondences may be established:

Nostr. Sem.	Kartv. IE.	Ur.	Drav.	Alt.	Tk.	Mo.	Tg.	
<i>p'</i>	<i>p</i>	<i>p, p'</i>	<i>p</i>	<i>p</i>	<i>p'</i>	<i>h &gt; O</i>	<i>f &gt; ... O</i>	<i>p</i>
<i>p-</i>	<i>p</i>	<i>p ~ b</i>	<i>p ~ b</i>	<i>p</i>	<i>p(v)</i>	<i>p</i>	<i>p &gt; b</i>	<i>f &gt; ... O</i>
<i>b-</i>	<i>b</i>	<i>b</i>	<i>bh</i>	<i>p</i>	<i>p</i>	<i>b</i>	<i>b</i>	<i>b</i>
<i>t-</i>	<i>t ~ t'</i>	<i>t</i>	<i>t</i>	<i>t</i>	<i>t'</i>	<i>t</i>	<i>t</i>	<i>t</i>
<i>t-</i>	<i>t</i>	<i>t</i>	<i>d</i>	<i>t</i>	<i>t</i>	<i>t</i>	<i>d</i>	<i>d</i>
<i>d-</i>	<i>d</i>	<i>d</i>	<i>dh</i>	<i>t</i>	<i>t</i>	<i>d</i>	<i>y</i>	<i>d</i>
<i>k-</i>	<i>q ~ k</i>	<i>k</i>	<i>k̄, k, k<sup>u</sup></i>	<i>k</i>	<i>k</i>	<i>k'</i>	<i>k'</i>	<i>x</i>
<i>k-</i>	<i>k</i>	<i>k</i>	<i>ḡ, g, g<sup>u</sup></i>	<i>k</i>	<i>k</i>	<i>k</i>	<i>k</i>	<i>k</i>
<i>g-</i>	<i>g</i>	<i>g</i>	<i>gh, gh, g<sup>u</sup>h</i>	<i>k</i>	<i>k</i>	<i>g</i>	<i>k'</i>	<i>g</i>

We may state two facts:

(1) Korean has not been represented, although it is mentioned as "Altaic" (pp.67-72);

(2) this scheme is arranged according to Illič-Svityč's general rule: the consonants of Nostratic are the same as those of Kartvelian (and the vowels are the same as those of Uralic); since Kartvelian has an opposition of three members respectively (e.g. *t, t, d*) Nostratic had to show the same structure.

For the correspondences of Nostratic \**p-* (non-spirantic voiceless labial stop) in "Altaic", Illič-Svityč gives only 4 examples, 3 of which are clearly onomatopoeic (Tk. *pū-* 'to breathe', *pus* 'steam', *piš-* 'to cook'), whereas Tk. *pēliŋ-* 'to be afraid' cannot well be compared with Tg. \**pāl-*, since the Tg. root is actually (onomatopoeic) \**pā-* (-*l* is an inchoative suffix). This is to say that the correct scheme (describing sound correspondences of loanwords) should in my opinion be:

Tk.	Mo.	Tg. (Nanai)
* <i>p-</i> > <i>O</i>	* <i>p-</i> > <i>O</i>	<i>p-</i>
<i>b-</i>	<i>b-</i>	<i>b-</i>

Here, the Nostratic scheme of three items of opposition fails (cf. Doerfer 1973: 81).

What about the dentals and gutturals? According to Illič-Svityč 1963, 1964b the opposition \**t'* : \**t* : \**d-* is secured by modern Tk. dialects: Oghuz and Tuvinian, namely Altaic \**t'*- has become *t-*, \**t*- has become *d-* or *d- ~ t-*, and \**d-* has become *y-*, cf. our pattern given above. For the other Nostratists cf. Helimsky 1986b,

Starostin 1991: 6-10. Objections have already been raised by Doerfer 1973 a: 80 and Ščerbak 1984. Generally speaking, Ščerbak's objection that the Nostratists did not actually consider the Tk. dialects, which indeed show many variants such as *t- ~ d-*, *k- ~ g-* is correct. We may add: loanwords clearly prove that Oghuz *d-*, *g-* are of recent origin, e.g., Persian-Arabic *kāfir* 'incredul' > *kāfir* (high level loanword) ~ *gāvur* (loanword with the popular development *k- > g-*) or Greek *Kalliopolis* > Tk. *Gelibolu* (a peninsula conquered by the Ottoman Turks in 1357) or Greek *kopri* 'dung' > *gübre*, *timoni* 'rudder' > *dümen*, etc. (On the other hand, the later Slavic loanwords have preserved *t, k-*). In Ottoman Tk. *t, k- > d-, g-* is a development of the 14/15th centuries, whereas *t, k- > d-, g-* in Tuvinian is still more recent, it started in the 19th century, cf. Doerfer 1994. For the Nostratists these modernisms are very ancient developments, still earlier than the oldest "Altaic" documents. This is an anachronism of about 2000 years, just so as if one would confound Pope John Paul II with John the Baptist.

More interesting is Illič-Svityč's article 1964 a, which Collinder has called a "decisive breakthrough in IE.-Uralic-Altaic comparison". In this work Illič-Svityč tried to show that (A) IE. *K<sup>e</sup>* (*K = k, g, gh*) corresponds to Uralic/Altaic *Ku/o/ü/ö* (i.e., *K + labial vowels*), (B) IE. *KE = Uralic/Altaic Ka*, (C) IE. *K<sup>e</sup> = Uralic/Altaic Kä/e/i/i*. This would indeed have been an important result. However, a scrutiny shows that Illič-Svityč's thesis is not tenable. Since I am an "Altaist", I will only check the "Altaic" examples.

Illič-Svityč gives for IE. *K<sup>e</sup>* = Uralic/Altaic *K + labial vowel* 10 examples, 7 of which are only IE. Uralic. There remain 3 (only 3!) IE.-"Altaic" examples. The first of them is IE. *k<sup>e</sup>el-* 'to turn' = "Altaic" \**kol-* 'to turn', namely Mo. *qol-gida-* 'to turn, not to sit quietly', Tk. *koş-* 'to run'. Comment: the Mo. form has to be read *qolki-da-*. It is a derivation of the adjective *qolki* 'loose, not tight'; there is not the slightest proof that the root *qolki* (which fully corresponds to the usual structure of Mo. words) must be divided to *qol-ki*; furthermore, the meanings 'to turn' and 'loose' are not automatically comparable. Of course, everything *can* be compared with anything, i.e. as being semantically linked. As students we had a game in which one of us uttered two words with very different meanings, e.g., "wall" and "girl". The other had to "prove" that these terms were equivalents, responding "that's the same". "Why?" - "when a bomb arrives the wall falls down, and when a male sex bomb arrives the girl falls down". Now, such surprising semantic developments *do* occur; but for proving the genetic relationship of languages one has, first of all, to find *secure*, incontestable equations; only they can constitute a "decisive breakthrough". Furthermore, the original meaning of Tk. *goş-* is 'to unite, to add', so this comparison is extremely dubious. The correct formula is: IE. *k<sup>e</sup>el-* 'to turn' ≠ Mo. *qolki* 'loose' and ≠ Tk. *goş-* 'to unite'; Tg.: no phonologically comparable word.

IE. *k<sup>e</sup>er-* 'to make, to shape' is compared with Tk. *qur-* 'to put sth. in working order' (namely: to string a bow, to erect a building, to establish a society, etc.). These meanings are rather vague, accommodating almost any notion. This comparison is not sure (this can only be said about terms of languages whose relationship has been proven), but it is plausible. The formula is: IE. *k<sup>e</sup>er-* = ? Tk. *qur-*; Mo., Tg. no comparable word.

IE. *pek<sup>e</sup>-* 'to cook' is compared with Tg. (Nanai) *päku* 'hot' and Mo. *he'üsüye-*, allegedly 'to suffer from heat'. \**päkö* is a well-known Tg. word (Evenki *häku*). Mo. *he'üsüye-*, however, does not belong here, not so much because of its actual meaning

being 'not able to endure the climate', but for reasons of sound law: Mo. *-ʹ* < *-g-* ≠ Tg. *-k-*. And the original IE. meaning seems to be 'to ripen (in various ways)'. Therefore the formula is: IE. *pekʰ-* 'to ripen' = ? Tg. *pākō* 'hot', ≠ Mo. *he'üsiye-*, for Tk. no comparable word. Thus we find this situation:

	plausible	dubious
Tk.	1	-
Mo.	-	-
Tg.	-	1
Tk./Mo.	-	-
Mo./Tg.	-	-
Tk./Tg.	-	-
Tk./Mo./Tg.	-	-
	1	1

This is a rather poor result. We may, furthermore make a verification: Do we find examples with IE. *Kʰe-*, where "Altaic" has other vowels, not labial ones? We find, e.g., IE. *gʰeid-* 'to overcome' = Tk. *qiy-*, *qīd-* 'to destroy', Mo. *kidu-*; IE. *gʰer-* 'mountain' = Tk. *qir* 'an isolated mountain or range of mountains', Mo. *kira* 'ridge of mountain'; IE. *ghyen-* 'to sound' = Mo. *ginggina-*, *gingsi-* id.; IE. *kʰei* 'who?' = Tk. *kām/kim*, Mo. *ken* id.; IE. *gʰhen-* 'light, shining' = Tg. *gīāban* (Evenki *gēvan*); IE. *gʰher-* 'hot, warm' = Tk. *qiz-* 'to be red hot'; etc. In other words, there is no unequivocal correspondence to be found for IE. *\*Kʰe-* = "Altaic" (whatever that may be) *k-*, *g-* + a labial vowel. The procedure so usual for Nostratists is evident here: they omit everything not suitable to their hypothesis. This means: a complete and thorough scrutiny shows that their thesis does not hold water.

Illič-Svityč's examples for IE. *\*Kʰ* (or *kʰ*) "Altaic" *k-*, *g-* + *ä/i* also show many faults, e.g. IE. *kes-* 'to cut' = "Altaic" *käs-*, where, according to Illič-Svityč 1971 No. 196 Nostratic *kāčā-* 'to cut' = Karthvelian *kac-*, Uralic *kāč-*, Tk. *kāč-* ought to correspond (cf. op. cit. 148) – but the Nostratists seldom care for their own sound laws. (By the way, it seems clear that *käs-*, *kāč-*, etc. is a synaesthetic without any relevance in the context of genetic relationship.) We may also cancel IE. *kes-* 'to comb' = "Altaic" (recte: Tk.) *qaš-* (Tk. *-š-* < *-lʹ-*). The following items in Illič-Svityč's list have no correspondence in "Altaic". B 1/11/12/15, C 5/6/9; and these examples are at least dubious: B 3 (Tk./Mo. *qirayu* is a derivation of *qir* 'grey'), 4 (IE. *k'er-* 'to bind' is hardly comparable with Mo. *kere-* 'to fight'), etc., so also B 6/8/10/13. Only B 5/7/9/14 are plausible, but all of them may be synaesthetics. As to C we find this situation: Only C 2/4 are plausible, C 1 is false, C 3/7/8/10 are dubious. In some cases false meanings are given, in C 7, e.g., the author compares IE. *"gəl-*" 'bald' with "Altaic" *kal'*, allegedly 'bald', but the actual meaning of Tk. *"qašqa"* (recte *qašya*) is '(animal) with a white head and darker body; with a white blaze on the forehead'. Let us give a full list of Illič-Svityč's comparisons. (Also in B and C counter-proofs are readily found, e.g., IE. *ger-* 'to gather' ~ Tg. *gārān* 'much' instead of *\*garan*, and Tg. *gārān* ought to be IE. *\*ger-*, etc.) But here is the list (pl. = plausible, du. = dubious):

	A. pl.	IE. <i>Kʰ-</i> du.	B. pl.	IE. <i>Kʰ-</i> du.	C. pl.	IE. <i>Ke-</i> du.	Total pl.	du.
Tk.	1	-	-	1	1	1	2	2
Mo.	-	-	1	1	-	-	1	1
Tg.	-	1	-	-	-	1	-	2
Tk./Mo.	-	-	3	2	1	2	4	5
Mo./Tg.	-	-	-	2	-	-	-	2
Tk./Tg.	-	-	-	1	-	-	-	1
Tk./Mo./Tg.	-	-	-	-	-	1	-	1
	1	1	4	7	2	5	7	14

This is to say: of Illič-Svityč's 10 + 15 + 10 = 35 examples only 5 show some plausibility (which does not mean that they are correct). Or in other words, Illič-Svityč's article 1964a obviously cannot be called a "decisive breakthrough". It merely proved that the author (in contrast to Starostin later) saw the gaps and weaknesses of his hypothesis, but nothing else.

3. By employing so lax a method one would be able to prove anything and everything (which means nothing). Give me two languages, and I'll prove that they are genetically related; they may also be spoken on galaxies X and Y. There is no difficulty in proving that, e.g., Malayan is "Altaic", cf. Mal. *abang* 'elder brother, elder sister' = Tk. *apa*; *angan* 'thought' = *aŋ*; *abai* 'unimportant' = *ap ... ap* 'neither ... nor'; *ajer* 'water' = *ayran* 'butter milk'; *alah* 'lost' = *al-* 'to take away'; *alangan* 'sand bank' = *alaŋ* 'level open ground', etc. (ad infinitum). (These comparisons are even better than those of the Nostratists.)

Illič-Svityč has indeed surmounted the level of the first half of the 19th century (when, e.g., Xylander compared Manchu with Greek, relying on mere similarities of words; but similarity proves nothing). He has understood that sound laws are necessary. What he has not understood is that *sound laws are a necessary, but not a sufficient condition* for proving the genetic relationship of languages. In 1944 I showed that Nostratism does not satisfy six conditions which are indispensable for an exact proof of relationship: it (1) neither gives a complete system of sound correspondences, nor (2) coherent semantic categories, nor (3) a coherent and sufficient investigation of linguistic geography, nor (4) investigations of the history of the languages compared, nor (5) sufficient word structure investigations, nor (6) comparisons free of the teleological method: words are not compared *because* they are comparable, but "reconstructions" are made in such way that what is actually not comparable may appear comparable.

I am positive that this article will not at once stop Nostratism, since the long-established facts it contains have not yet hindered Nostratists from putting their thesis forward; while the importance of the new facts presented will not be perceived because they contradict Nostratic one-way thinking: Nostratism contents itself with some loosely verified examples of the vocabulary and sound laws – which, however, are not observed too stringently.

That becomes clear when one reads, e.g., Helimsky 1986, with a "refutation" of my ideas, particularly on pp. 248–253 and 328. His article is rich in misunderstandings and/or odd imputations. Here is an example:

Helimsky asserts: Doerfer shows that the Junggrammatiker (of whom he is a convinced fan and whose opinions, methods and view-points he has never overcome) have not occupied themselves with more distant genetic relationships, linking the IE. languages known to them with other language groups. Following this consequence, Doerfer would have to forbid the investigation of the Anatolian and Tokharian languages, because they were not known to the Junggrammatiker. My answer: I have never asserted any such absurdity. It is clear that when the Junggrammatiker came to know these languages they would surely have integrated them into the circle of IE. languages and investigated them. Of course I do not forbid the investigation of languages hitherto unknown; on the contrary, it is a grand task to deal with them, and I myself have done that in respect to the Tk. language Khalaj. But this investigation must follow a *rigid scientific method*, and that is what is lacking in Nostratism. Not to know a language (group) and therefore not to be occupied with it, on the one hand, and to know several language groups, but *prudently* not linking them together into a supergroup, on the other hand – these are two different situations. What a hotch-potch!

Another objection of Helimsky is: Doerfer sets the conditions of IE. as an ideal to be followed by all long range comparisons which claim to prove genetic relationship. But his claims are so rigid that they are also not fulfilled by IE. And nevertheless these languages are genetically related. Thus it is admissible for the Nostratic languages, as well. The differences between IE. and Nostratic are only quantitative, not qualitative, because of the changed character of the material to be compared and *time depth*. That, for instance, many comparisons of Illič-Svityč do not follow strict sound laws does not matter, since the same holds true for IE. (Meillet: chaque mot a son histoire). Furthermore (1992, 328): when, e.g., Helimsky's Altaic comparisons quoted above combine very dissimilar words, "this is exactly what must be expected". In other words, since also IE. studies do not always present sure comparisons (uncontestedly!), Nostratism is entitled to be satisfied with unsure comparisons in general and no matter how great their percentage of the material is. And since time depth is still deeper than for IE., the lack of sureness may be still greater. "Time depth" is a plaster on all linguistic wounds. Does that need a refutation?

Before going on to answer Helimsky's objections in a more detailed way, I may say that his utterances are rich in misunderstandings. I do not want to imply that these are intentional, they may be due to his scant knowledge of German. Here are some examples, cf. Helimsky 1986:

(1) Helimsky misunderstood the word "Zirkelschluß" (vicious circle). When, e.g., "nine" is in Slavic, not \**nevet'*, but *devet'*, this is no vicious circle, but an analogy to *deset'* '10' (realm 2). Such things occur often, just in IE. numerals, and I would never deny the connections of the IE. numerals (or accept the loosest connections only out of prejudice). The IE. system, *as a whole*, is too sure. But that is just what is lacking in Nostratic.

(2) Helimsky misunderstands the idea of sound law. I never asserted that all words have to follow sound laws and *only* sound laws. Cf. my above-quoted explanation of the "three realms". Nostratism offers too many arbitrary explanations, apart from being too lax in sound laws.

(3) "Tote Periode" (dead period) means: a period before the first documentation

of a language (family). (The Baltic group, e.g., is not documented before 15th century.) But, generally speaking, the dead periods for a comparison of Semitic (in the narrower sense) and IE. are not so unfavourable, since both families are documented many centuries before Jesus Christ. Nevertheless the comparability of these families is regarded as extremely weak, doubtful and at any rate much smaller than, for instance, that of the Bantu languages, documented in modern times and with a long dead period. On the other hand, if the Nostratic languages were actually related, their common origin ought to be infinitely more remote than those of Bantu and IE. But this should cause caution.

(4) According to Helimsky Khalaj *h-* is secondary. Cf. my refutation of this outdated idea in 1981, 102–3 etc. and 1985, 148–150, 153–4, and 1993.

To Helimsky 1992:

(5) I think that Poppe's sound laws are a good basis for an "Altaic" comparison. (Ramstedt has in some cases been overcome by Poppe.) But I do not accept *all* sound laws of Poppe. For instance, no such sound law as Mo. *m-* = Tk. *b-* exists. Here we must affirm a gap – one of the many gaps of "Altaic" (which still augment in Nostratic).

(6) Tk. *yudruq* 'fist' = Mo. *nudurya* = Manchu *nujan* is one of the many examples given by me in 1985, to prove (pp. 226–231, 287, 1.2–1.5) that when we find comparable words in Tk., Mo., and Tg., the Tg. words always follow Mo. patterns, both in semantic and phonological respects, and this again proves that Tk. and Tg. had no direct contact. Helimsky disregards my explanation on p. 328 (perhaps it is too abstract for him).

(7) The last passages of Helimsky 1992: 328 run as follows: "The more remote a genetic relationship of languages is, the more difficult is the establishing and the investigation of it and the less consensus can be expected to be found among the scholars". As Doerfer states (p. 267), "als 'verwandt' bezeichne ich alle Sprachen, die durchgehend von allen (oder fast allen) Spezialisten als verwandt bezeichnet worden sind". This is a very democratic procedure for finding out whether the languages are related. But in science, as probably also elsewhere, even the most exact and democratic counting of votes cannot make up for the prejudices of the voters.

I am not sure whether Helimsky intentionally falsified my ideas or has not understood my explanations in 1988, 267–283. It may be that they were also too abstract for him, because Nostratists normally are satisfied with finding comparable words and affixes, leaving apart theoretical reflections and a good deal of scholarly scepticism. My train of thought was:

a) The question is: are the "Altaic" languages related? To answer this question we must *define* what the term "genetically related" means. Otherwise all languages of the world might be called "genetically related", provided they have one single word in common.

b) We now investigate a series of languages whose relationship has been generally acknowledged. What are the special features which have produced the idea of their relationship? We find that their subgroups (in IE., e.g., Greek, Germanic, Slavic, etc.) have NBW.s in common, as we have seen above. This belongs to the structure typical of "genetically related languages".

c) We should then establish: Do the "Altaic" languages show the same structure?

They do not. (As we have seen above.) Thus we cannot call them "genetically related" – otherwise this word would lose all meaning and significance. And the subterfuge "the Altaic languages are related, but in a more distant way, with a much smaller amount of quantity and quality" falls away. To quote Austerlitz (cf. Doerfer 1981: 40): "To be sure, we can say that 'we believe that X and Y were once related and that the indices which would normally be adduced to support such an originally systematic genetic relationship have disappeared in the course of time and have left only vague traces'. This statement of course disqualifies itself."

4. Indeed, the difference between the situation in truly related language families and Nostratic is not only quantitative, it is *qualitative*. There is a "qualitative jump" which does not allow the Nostratic language families to be called "genetically related". Science is not the study of things which *exist* but of those which can be *proven to exist*. And it is clear: the further we go back the greater the chance is that words vanish (are replaced, e. g., by taboo words) or change their meaning and/or shape so enormously in the different languages that their former connection is no longer provable. Time depth is like fog. One may drive at 150 mph with visibility of 100 yards; but when the fog thickens, so that one may see not further than 10 yards, no sensible person will drive at 150 mph (or the next stopping place will be a ditch). Nostratists are like car drivers going at 150 mph in a fog with visibility of 10 yards. In other words: the reliability of linguistic comparisons tends, with increasing time depth, towards zero. One may seem to preserve comparability by some artificial measures, namely: (1) by comparing *many* language families (law of probability: when we also range Palaeoasiatic languages, Japanese, Bantu, Austronesian, Eskimo as "Nostratic" the probability of finding comparable roots would be the greater), (2) by laxity of method (Starostin, e. g., compares p. 151 Tk. \**kān* and Tg. \**xuŋi-ka* 'blood': the incomparability of *-ā-* and *-u*, of *-n* and *-ŋ*, the "suffix" (?) - *i-* in Tg. are not explained (and Tk. *k-* = Tg. \**x-*, recte \**h-* is not proven, as we have seen above). Furthermore the "Altaic" reconstruction \**k'uāŋ* V, by combining *ā + u > uā* is odd (just as Latin \**kšantare*, cf. 1.1). (3) Gaps in sound correspondences or semantic series (such as numerals, NBS.s) are neglected. (4) Arbitrary, teleological assumptions are made (above all, by Starostin), e. g., in the comparison of Tk. *tūtün* 'smoke' = Mo. *utayan* < "Altaic" \**t'ut'* V- (with *u = ü* and an alleged dissimilative dropping of \**t'*): baby words are dealt with on the same level as normal words (e. g., Starostin's list of general Altaic etymologies, numbers *aka* 'elder brother', 429 *āk'ä* 'elder sister'), etc.

Characteristic is the small amount of "agglomerations" in Nostratism, i. e., cases where a common term is found in all six "Nostratic" groups. To give an example: when we compare (cf. Doerfer 1973 a: 93–108) the roots beginning with \**bh-* in IE., taking – in parallelism to the six Nostratic groups – six IE. groups (Indo-Iranian, Greek, Latin, Germanic, Baltic, Slavic) on the one hand with Nostratic words beginning with \**b-*, on the other hand, we obtain the following percentages:

This means that there is a "qualitative jump" between IE. and "Altaic". So small an amount of agglomerations is a proof not for, but against Nostratic relationship. It is just the result "to be expected" when we compare unrelated languages, whose (more or less plausible) parallels are only due to the laws of probability, "elementary relationship" and similar things and when we overmore employ a lax method.

I have also made a "test of sureness/plausibility", comparing the IE. examples

	IE. * <i>bh-</i>	Nostratic * <i>b-</i>
In 6 groups	32.1	-
5	17.9	-
4	21.4	21
3	15.5	29
2	13.1	50

with \**bh-* – with the Nostratic words with \**b-*. It would take too much space to discuss all examples relevant to this topic. I only wish to say that I distinguished six cases: sure (for IE.) or plausible (for Nostratic), likely – almost plausible, possible – less plausible, hardly possible – hardly plausible, rather unlikely – containing almost no plausibility, false – false. For IE. the first two categories contained 65% (the last three 28%), for Nostratic I found a relation of 6:72%. This means a "qualitative jump": the abyss between circumspection and fancy or linguistics and paralinguistics. (The figures result from Illič-Svityč – Starostin would give much worse results.)

I am afraid the assertion that "time depth" has to be considered for Nostratic simply means: Everything which a) seems somewhat similar or comparable and simultaneously b) does not satisfy the normal conditions of linguistic comparison is called "Nostratic", or more briefly: comparisons which do not work, are Nostratic.

An example for this statement is offered by Bomhard, who reconstructs p. 68–9 Nostratic \**t[h]i/\*t[h]e* 'you' [= thou], comparing Mo. \**ti > či* with Tk. *sen*, thus *t-* = *s-*, although according to his own sound laws on pp. 70–1, "Altaic" *t ≠ s*; here not even a single sound of the Mo. and Tk. pronouns does correspond from the viewpoint of Altaic sound laws. (By the way, coincidences in the pronouns – which are usually not normal words, but originate from exclamations – do not prove genetic relationship: they belong to the category of "elementary relationship". The statement "related languages have pronouns in common" is undeniably correct – but it is not reversible.)

5. The development of diachronic linguistics is, in a way, regrettable. It may be established that

a) Up to the eighteenth century comparisons were made on the basis of similarity alone. A typical example is Xylander. This state of affairs justified Voltaire's irony.

b) Enormous progress was made by the Junggrammatiker, who introduced rigid claims for proving the validity of comparisons and erected the ideal norm of sound laws.

c) In the following period many methodological refinements were made; the rôle of analogy, taboo, etc. was clearly defined and, first of all, linguistic geography and dialect studies were created as a second means of investigating languages and describing their connections. A certain culminating point is attained in IE. studies (Pokorny, Boisacq, Vasmer, Hofmann, Mayrhofer, Buck, etc.). This pattern b) + c) was also followed in such linguistic groups as Uralic, Austronesian, and others.

d) The human spirit always tends to go further, to leave ancient areas behind, to seek new fields. (Therefore, Illič-Svityč's activity was understandable.) But progress in scholarship can work only when complemented by a sensible scepticism.

It is possible that after the end of Nostratism one may find that this phenomenon

had its merits: it has collected ample material which may illustrate the major rôle of synaesthetics (including baby words) and of long range itinerant words which have migrated through the world just as, e.g., Greek *nomos* 'law' > ... Manchu *nomun*. This holds, above all, for Illič-Svityč, whose system still maintained remarkable remnants of sound common sense.

e) Departing from Illič-Svityč's semirational thesis two ways were open: the rational way, namely to investigate what was sure in all these comparisons, no matter whether proving genetic relationship or old loaning connections (this would presumably have meant cancelling about 90% or more of Illič-Svityč's examples) – or the irrational way, namely to produce new possibilities of comparing anything and everything, without any sufficient proof of reliability. This was the way Starostin has gone, returning to a pre-Junggrammatiker status.

For the time being, the relation of Nostratism to serious linguistics is the same as that of astrology to astronomy.

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