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Joseph C. Salmons and Brian D. Joseph (eds)

Nostratic: Sifting the Evidence

NOSTRATIC
SIFTING
THE EVIDENCE

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INTRODUCTION

In the summer of 1996, with the announcement of possible signs of life found in a Martian rock from Antarctica and the public controversy that ensued, Daniel Goldin, the head of NASA, described his reaction as one of ‘skeptical fascination’. So also with Nostratic, of late much in the public eye:¹ Some scholars pursue Nostratic vigorously, while many others — the editors among them — approach the topic with a reaction more akin to Goldin’s. Our goal, therefore, in this book is to bring together historical linguists representing proponents of Nostratic and those not yet convinced by what they have seen, so as to let readers sift the evidence and decide for themselves.

1. *What is Nostratic?*

‘Nostratic’ is the name given to a language hypothesized to be the common ancestor of a number of families of languages, including Indo-European, Uralic, and Afroasiatic. For a century now, various forms of this hypothesis² have produced one of the most enduring and sometimes intense controversies in linguistics. Largely, though, supporters of the hypothesis and those who reject it have seldom dealt directly with one another’s arguments.

For at least several thousand years, people have speculated about the origins of human language and the historical relationships among languages.³ The proposals that arose from these discussions were often theologically or ethno-centrally driven; even more enlightened attempts did not reach much beyond

¹ Nostratic has been immensely popular: the *New York Times* and other publications which seldom note work in our field have recently shown interest in Nostratic and, following an article from the 27th of June, 1995 in the *New York Times*, we have received numerous letters from interested lay people. For the most recent contribution to the popular press, see Joseph (1997).

² Nostratic has been variously characterized as a ‘theory’ or ‘hypothesis’, and no doubt in other ways as well. In a technical sense, it may be neither, but we follow conventional looseness in allowing these multiple labels, as do numerous contributions to this volume.

³ The Tower of Babel story is a familiar example. See Pedersen (1931) for a brief treatment of ancient Greek and Roman views on language origins, history and relationships. For a survey of current work on language origins, see Wind (1992) and other volumes in that series, as well as Aitchison (1996) and Eco (1995).

taxonomies based on the word for 'god' in various languages.⁴ Then, two centuries ago, comparative linguistics succeeded in establishing solid and uncontroversial genetic relationships among such superficially diverse languages as English, Sanskrit, Greek and Irish.⁵ Amid the vast differences among these languages, early comparativists were able to tease out a core of shared items in vocabulary and grammar on which elements of the common ancestor language, Indo-European, were proposed and have been refined ever since. Similar success with Finno-Ugric, Semitic and other families has repeatedly confirmed that the method is reliable and broadly applicable.⁶ Still, efforts to move to a higher-level grouping, ones uniting the just-mentioned families, have been controversial.

Against this familiar background, it becomes a scientific obligation to press onward with the application of the techniques and principles to ever greater time depths, investigating deeper possible relationships. Historical linguists thus are driven to investigate Nostratic, and it seems that the time is ripe for a reevaluation of the central controversies that constitute the debate over Nostratic, including:

1) *Is there any there there?* The basic questions of whether Nostratic is a valid construct and how seriously the hypothesis should be taken continue to bring a wide range of answers. In the present volume, Vovin, who seems generally sympathetic to Nostratic, pleads that the hypothesis cannot be dismissed out of hand. Going a step further, Manaster Ramer, Michalove, Baertsch & Adams present cogent arguments that the assumption of Nostratic can yield solutions to language-particular problems in subgroups of Nostratic. On the other hand, Comrie baldly states, in answer to his own question of the relatedness of Altaic, Uralic and Indo-European pronominal systems, "I do not know". Other agnostics represented in this volume, such as Ringe, Vine, Campbell, and even Hamp, demonstrate that the hypothesis is being taken seriously indeed by skeptics specializing in Indo-European and Uralic, at least. While these scholars seek to test the hypothesis, Nostratic has been around long enough and has been discussed widely enough that some regard the genetic affiliations as established. As Bomhard puts it, "it is no longer reasonable to hold ... that Indo-European is

⁴ These efforts continue to this day, with just as little merit as their predecessors. For example, both Oak (1984) and Poonai (1994) claim Sanskrit as the source of numerous, if not all, human tongues.

⁵ For accessible discussion of the contributions of Bopp, Rask and other great early comparativists, see Pedersen (1931).

⁶ It has sometimes, however, been claimed that the comparative method is not applicable to all language families of the world. See Hoenigswald (1991) for discussion:

a language isolate". Hodge, like Bomhard, is sufficiently convinced of the grouping to use Nostratic to shed light on the prehistory of lower-level groups. Nostratic has emerged then as a (perhaps even the) key seam in linguistic comparison between a universally accepted construct, like Indo-European, and one overwhelmingly rejected by historical linguists, like Proto-World.⁷

2) *If so, what is it?* Let us now return, though, to the difficult question of defining Nostratic, specifically the matter of which families belong under this rubric. Nostratic has overwhelmingly been portrayed as a monolithic proposal, most often including exactly the six groups considered in Illich-Svitych's dictionary, published beginning in 1971: Indo-European, Uralic, Altaic, Kartvelian, Afroasiatic and Dravidian. In fact, Nostratic is, as Campbell puts it, a 'shape-shifter'. At its narrowest, proposals uniting only two of these groups have been called 'Nostratic'. At its broadest — and, as Greenberg argues, this view is more widespread among orthodox Nostraticists than usually noted — Nostratic has been regarded as including various sets of languages stretching as far east as Eskimo-Aleut. For instance, both Greenberg's 'Eurasian' and Bomhard's view of Nostratic include links well beyond the traditional notion of 'Nostratic'.⁸ It is thus perhaps small wonder that much work focuses on the relative strength or weakness of particular links within this complex web.

One of the earliest and most intensely pursued links has been that between Indo-European and Semitic, from Möller (1906, 1911), down to Bomhard (1984) and now Levin (1995). That tradition is represented here by Hodge, who calls his own version 'Lislakh'.⁹ Within Nostratic circles and beyond, though, the status of the connection between Indo-European and Afroasiatic is now more often accorded the status of a deeper, more distant connection. Greenberg shows

⁷ The notion that we can uncover the ancestor of all human languages is hardly a new one; see Trombetti 1905. For recent proposals sympathetic to Proto-World, see Ruhlen (1994) and references given there; for counterarguments, see Hock (1994), Salmons (1992, 1997). Of course some languages cannot possibly be brought in under the umbrella of Proto-World, such as signed languages (e.g., American Sign Language) or artificial languages (e.g., Esperanto); cf. Hock & Joseph (1996:485–506).

We hasten to add that adherents of Nostratic are not necessarily committed to Proto-World investigations, or, indeed, to other distant genetic hypotheses. In fact, one strong claim made by at least some proponents of Nostratic is that there are clearly non-Nostratic languages, just as Indo-Europeanists talk about non-Indo-European languages.

⁸ Another dimension to pinning down 'Nostratic' is the question of *whose* Nostratic, since — as is already apparent — different researchers see themselves as part of distinct traditions of Nostratic scholarship, with Illich-Svitych's 'Muscovite School' the usual central point of reference.

⁹ See Hodge's contribution for an explanation of this name.

that this view of less immediate connections between Indo-European and Afroasiatic has a long history in Nostratic circles, where Indo-European has long been seen as more closely connected to Uralic. Indeed, most researchers, including those represented in this book, find the strongest case in a connection between Uralic and Indo-European. Indeed, Ringe, like a number of other Indo-Europeanists over the years have, accepts these as demonstrably, though weakly, related. Vovin accepts the Nostratic hypothesis but finds serious problems with the inclusion of Altaic, even while suggesting that Indo-European, Uralic and Altaic are related in some way. Yet more open to dispute is the internal structure of the Nostratic family tree. Vovin distinguishes 'northern' and 'southern' branches, the former including Indo-European, Uralic, Altaic, and the latter Kartvelian, Afroasiatic and Dravidian, while Bomhard and Greenberg now prefer to distinguish a 'Eurasianic' group within Nostratic.

3) *(How) has this hypothesis evolved?* Most of the authors provide their own perspectives on the history of the Nostratic hypothesis, inevitably beginning with Pedersen's first 1903 discussion and going down to Illich-Svitych. Campbell's discussion of the Slava cult underscores the tremendous reverence for Illich-Svitych and points up how this potentially hinders sober scientific evaluation of the hypothesis. Nonetheless, as this volume shows, Nostratic remains an evolving construct.

4) *How has this hypothesis been evaluated?* Disagreements reach even into the current state of evaluation of Nostratic and work on Nostratic. Vovin, for instance, regards the Moscow School as almost alone in its support for the hypothesis — Manaster Ramer providing the notable exception — while Bomhard sees broad and international support for Nostratic. More crucial yet is the question of how this hypothesis *can* be evaluated, and thus we turn to methodology.

2. *Method as the central issue*

All of the above disagreements pale beside the fundamental matter of appropriate methodology. As elsewhere in science, methodological flaws can undermine reliability and replicability of results. Just as acoustic phonetic investigations can be fatally compromised by background noise or sociolinguistic studies can be felled by problems in representativeness of sampling, so too can comparative linguistic houses be built on methodological sand.¹⁰ Specialists in individual

¹⁰ Pinker (1994:255–256) makes a similar point:

As an experimental psycholinguist who deals with the noisy data of reaction times and speech errors, I have no problem with Greenberg's use of many loose correspondences, or

families work primarily on problems within those languages, tending to shy away, some would say, from confronting questions about the methodological underpinnings of the overall enterprise.¹¹ Under these circumstances, Nostratic provides us with an excellent opportunity to raise these important questions explicitly. Indeed, much of the present volume is dedicated to these questions and their import to the field. We will let the authors' work speak for them, but signal here some groupings of core issues that arise repeatedly in papers in this volume. To a certain extent, therefore, the method is the message.

Comparison

- Does Nostratic reflect application of the comparative method and comparative reconstruction in the same way that more accepted groupings do?
- Does time depth constrain the utility and validity of the comparative method?¹²

Other sources of similarity

- Could chance account for much or all of the corpus of proposed Nostratic comparanda?
- What role does borrowing play?

Standards of evidence

- What breadth of attestation across branches is required for inclusion in a proto-language?
- What kinds of data constitute evidence for proto-languages? What counts as a 'grammatical form'? Are some forms too small to be reliably compared?

even with the fact that some of his data contain random errors. What bothers me more is his reliance on gut feelings of similarity rather than on actual statistics that control for the number of correspondences that might be expected by chance.

¹¹ Koerner (1989:1) has written:

Historical linguists have generally tended to be reticent about making theoretical statements regarding the practice of their field. Most of them have been avoiding laying bare the methodological principles which underlie their research or facing up to epistemological questions about what they are really doing. This observation is particularly true about one of the most essential aspects of their craft, the practice of reconstruction of unattested forms.

This has helped fuel the debate over whether long-distance relationships are posited on evidence and methods similar to those used by the earliest scholars working on Indo-European.

¹² Note, as food for thought, Bender's challenging comparison of longer-distance reconstruction with an attempt to recapture the original sound waves of Lincoln's Gettysburg Address in which one "would be defeated by the fact that the energy has long since been swamped by random variations in movements of air molecules. It is this same sort of effect on a smaller scale that operates in the case of language change" (1973:8).

Proof

- Is it necessary to reconstruct in order to prove relatedness?
- Is it indeed possible to prove languages unrelated? Is the appropriate null hypothesis here that languages are related or that they are unrelated?¹³

Building on this last point, we note that the nature of proof of genetic relationship is fundamental to all the questions above, to all the papers in this volume, and to Nostratic in general. The depth of this problem is readily apparent from the fact that many scholars see connections between Indo-European and other families, for example, as having been ‘successfully demonstrated’ as far back as Illich-Svitych (Bomhard, with similar sentiments found in other papers in this volume and elsewhere), while many others find the evidence they have examined weak (Campbell, for instance). For the larger community of historical linguists, the responses to this issue, implicit and explicit, form one of the central, unifying themes of this volume. Maddeningly, of course, even if these or other sets of languages are related, their relations may not be demonstrable given the limits of our data and our methodological tools; sometimes, perhaps, we have to be agnostic, if nonetheless skeptically fascinated.

3. *The conference and this volume*

This volume grows ultimately out of the Second Workshop on Comparative Linguistics, held at Eastern Michigan University in the fall of 1993. Here, for the record, is the schedule of that Workshop:¹⁴

Thursday, Oct. 21

9:10 The Nostratic Enterprise. Moderator: Joe Salmons, University of Wisconsin/Purdue University

9:20 “The Insider’s View of Nostratic”, Mark Kaiser, Illinois State University

10:00 “The Outsider’s View of Nostratic”, Alexis Manaster Ramer, Wayne State University

11:10 Discussant: Brent Vine, Princeton University

11:30 Open discussion

1:30 Methodological & Historical issues. Moderator: Brian Joseph, The Ohio State University

¹³ See footnote 7 above, on clearly unrelated languages.

¹⁴ For a highly personal account of the conference, and one sympathetic to the Nostratic enterprise, see Hegedűs’ conference report posted to the LINGUIST list, Vol-4-984, from the 23rd November 1993, available on their World Wide Web archive site (<http://engserve.tamu.edu/files/linguistics/linguist/archives.html>).

- 1:40 “The History of Nostratic Scholarship”, Vitaly Shevoroshkin, University of Michigan
 2:20 “The Convergence of Nostratic and Eurasiatic”, Joseph Greenberg, Stanford University (read by Keith Denning)
 3:30 Discussant: Mark Hale, Harvard University
 3:50 Open discussion

Friday, Oct. 22

- 9:00 The role of chance. Moderator: Martha Ratliff, Wayne State University
 9:10 “A Probabilistic Evaluation of Similarities among Very Dissimilar Languages”, Robert Oswalt, California Indian Language Center
 9:50 “A Probabilistic Evaluation of Indo-Uralic”, Donald Ringe, University of Pennsylvania
 11:00 Discussant: William Baxter, University of Michigan
 11:20 Open discussion
 1:00 Family-specific connections. Moderator: Anthony Aristar, Texas A&M University
 1:10 “Implications of Lislakh for Nostratic”, Carleton Hodge, Indiana University
 1:50 “Nostratic and Altaic: The Level of Relationship”, Alexander Vovin, University of Michigan
 3:00 Discussant: William Rozycki, Indiana University
 3:20 Discussant (“Some IE models for Nostratic”): Eric Hamp, University of Chicago
 3:40 Open discussion
 4:30 Panel Discussion: Final assessment of the Workshop

As part of the organizing committee for the Workshop and as neutral participants in it, it fell to us to see about editing a volume on this topic. We decided, after discussion of the idea of a Nostratic volume with CILT series editor Konrad Koerner, to solicit papers from all participants in the Workshop, but not all accepted the invitation. We also invited a number of other colleagues with an interest in Nostratic, as well as one who had been invited but unable to attend the Workshop, Lyle Campbell. Of those, Allan Bomhard and Bernard Comrie accepted, while Sergej Starostin chose not to contribute.

We attempted to follow the paper-with-response format of the Workshop to the extent possible and for that reason invited some responses by individuals not present at the Workshop. Thus the papers contained in this volume are substantially refined and further developed from the original conference. Throughout, we have tried to exercise as light an editorial hand as possible, allowing authors to present their views freely.

Finally, we want to thank our Editorial Assistant, Dave Holsinger, for his steady, hard and smart work over the long process of producing this volume. Dave has carried a heavy load throughout, including preparation of indices and we are grateful to him.

Let the sifting begin!

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June 1998

REFERENCES

- Aitchison, Jean. 1996. *The Seeds of Speech: Language origin and evolution*. Cambridge: Cambridge University Press.
- Bender, M. Lionel. 1973. "Linguistic Indeterminacy: Why you cannot reconstruct 'Proto-Human'". *Language Sciences* 26.7-12.
- Bomhard, Allan R. 1984. *Toward Proto-Nostratic: A new approach*. Amsterdam & Philadelphia: John Benjamins.
- Dolgopolskij, Aaron B. 1986. "A Probabilistic Hypothesis Concerning the Oldest Relationships among the Language Families in Northern Eurasia". *Typology, Relationship & Time* ed. by Vitalij Shevoroshkin & Thomas L. Markey, 27-50. Ann Arbor, Mich.: Karoma. (Partial translation, with a new introductory note, of a 1964 article published in *Voprosy Jazykoznanija*.)
- Eco, Umberto. 1995. *The Search for the Perfect Language*. Transl. by James Fantress. Oxford & Cambridge, Mass.: Blackwell.
- Hock, Hans Henrich. 1994. "Swallow Tales: Chance and the "World etymology" MALIQ'A 'swallow, throat'". *Papers from the 29th Regional Meeting of the Chicago Linguistic Society*, Chicago. Vol. I, 215-238.
- Hock, Hans Henrich & Brian D. Joseph. 1996. *Language History, Language Change, and Language Relationship: An introduction to historical and comparative linguistics*. Berlin: Mouton de Gruyter.
- Hoeningswald, Henry M. 1991. "Is the 'Comparative' Method General or Family Specific?". *Patterns of Change/Change of Patterns* ed. by Philip Baldi, 183-191. Berlin: Mouton de Gruyter.
- Illich-Svitych, Vladislav M. 1971, 1976, 1984. *Opyt sravnenija nostraticheskikh jazykov (semitoxamitskij, kartvel'skij, indoevropejskij, ural'skij, dravidijskij, altajskij)*. 3 vols. Moscow: Izd. "Nauka".
- Joseph, Brian D. 1997. "On Nostratic: The search for linguistic roots". *Science Spectra* 8.74-77.
- Koerner, Konrad. 1989. "Comments on Reconstruction in Historical Linguistics". *The New Sound of Indo-European: Essays in phonological reconstruction* ed. by Theo Vennemann, 1-20. Berlin: Mouton de Gruyter.

- Levin, Saul. 1995. *Semitic and Indo-European: The principal etymologies*. Amsterdam & Philadelphia: John Benjamins.
- Möller, Hermann. 1906. *Semitisch und Indogermanisch*. Copenhagen: Hagerup. (Repr., Hildesheim: Georg Olm, 1978.)
- _____. 1911. *Vergleichendes indogermanisches-semitisches Wörterbuch*. Göttingen: Vandenhoeck & Ruprecht. (Repr., 1970.)
- Oak, Purushottam Nagesh. 1984. *World Vedic Heritage: A history of histories, presenting a unique unified field theory of history that from the beginning of time the world practised Vedic culture and spoke Sanskrit*. New Delhi: Institute for Rewriting Indian History.
- Pedersen, Holger. 1903. "Türkische Lautgesetze". *Zeitschrift der Deutschen Morgenländischen Gesellschaft* 57.535–61.
- _____. 1931. *Linguistic science in the 19th century*. Cambridge, Mass.: Harvard University Press. (Repr., Bloomington, Ind.: Midland Books, 1962.)
- Pinker, Steven. 1994. *The Language Instinct: How the mind creates language*. New York: Morrow.
- Poonai, Preamsukh. 1994. *Origin of Civilization and Language*. Daytona Beach: Pearce Publishers.
- Ruhlen, Merritt. 1994. *The Origin of Language: Tracing the evolution of the mother tongue*. New York: Wiley & Sons.
- Salmons, Joseph C. 1992. "A Look at the Data for a Global Etymology: *tik 'finger'". *Explanation in Historical Linguistics* ed. by Garry W. Davis & Gregory K. Iverson, 207–228. Amsterdam & Philadelphia: John Benjamins.
- _____. 1997. "Global Etymology as Pre-Copernican Linguistics." *California Linguistic Notes* 25.1–6, 15.
- Trombetti, Alfredo. 1905. *L'unità d'origine del linguaggio*. Bologna: Treves/Beltrami. (Repr., Bologna: Civitas Dei, 1968.)
- Wind, Jan, et al., eds. 1992. *Language Origin: A multidisciplinary approach*. Dordrecht: Kluwer.

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SOME DRAFT PRINCIPLES FOR CLASSIFICATION

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We believe at least the following principles, assumptions, and findings to have become assured and obligatory in an acceptable and persuasive pursuit of our field of comparative linguistics when the goal is the tracing of genetic descent and familial classification:¹

1. All raw data used must be checked and acceptable to expert philologists and fieldworking linguists in the respective languages. All such data must be copied accurately from source publications respected by such experts. If there is disagreement or distrust surrounding any source materials, these reservations must be discussed and clarified in specialized *Fachliteratur* where the attention of serious and informed scholarship can be attracted and summoned. Thus, Carian data and claims concerning this language must be debated and agreed upon in writings and journals on Anatolian whereas Phrygian is examined in broader Indo-European literature. To the extent this is incompletely agreed, the resulting argument is weakened; if this requirement is ignored or not observed, the argument is vitiated.

2. All such data must be segmented into elements that are considered to be reasonable and relevant by experts and native speakers of the respective languages. This means that the English phrase *my leg* may not be broken down into *myl + eg* or *my + le + g* nor *my letter (which I wrote)* into *myle + tter* or *my + let + ter* or *my + lett + er*. Only acceptable elements may be used in the first order of argument. Where experts already disagree, the argument is thereby weakened to that extent (see (1) above). All such dissonance must be made clear in any presentation of the argument.

3. In further orders of argument, only elements similarly established in prior explicit stages of comparison may be used. An important inequality always intrudes here in any interesting problem. Absolute similarity of analysis is rarely encountered. Thus the basis of claimed similarity must always be made explicit. Most instances of such necessary clarification arise from combined divergence in chronology and grammatical rule change; this calls for exposition of the

¹ We apologize for the dense prose of these lines and beg you to read them slowly and patiently. They were first written in the late 1980s in the unfulfilled hope that the public press would prefer reasoned discourse and foundational criteria to quixotic and episodic speculation.

chronological spans and the intruding rule mechanisms. Such exposition constitutes the heart and most of the bulk of technical scholarly argument relevant to our whole question. Absence of such argument renders proof of an interesting problem suspect.

4. Every language form adduced must give overt representation to every distinguishing feature or characteristic borne by that form: so, it is not sufficient to write English *bid* as if it were phonologically or phonetically *bit* or *pit* or *bead*. Likewise, we must (somehow, in notation) distinguish (*I*) *bid* (*two spades; you good night*) from (*a*) *bid* (*of two spades; to join the group*); also (*a*) *sheep* (*is grazing*) from (*some*) *sheep* (*are grazing*); and (*his*) *arms* (*are sunburnt*) from (*his*) *arms* (*are illegal and contraband*). This means that we must write down and carry forward more than just the shape of the headword in the dictionary or the caption of a paragraph in a grammatical description or grammar book (e.g. *accusative, object, temporal, passive participle, conditional, if, quam, [omni]bus, and, so*). We must also attach a coding of the dictionary entry or the grammatical paragraph (or chapter).

5. In using such features and characteristics for comparisons that establish genetic equivalences, and hence familial relation and descent, we must strive to account for and assign to some successful equation every one of these features in each form; that is, we must strive to account for all discrimina without remainder. It is not enough to give an accounting of but one portion, leaving the rest with no matching equivalence. We may call this principle that of *total accountability*. We strive to apply this to all stages and all elements analyzed for each such reconstructed stage.

6. By registering such equivalences quasi-exhaustively, we construct branching diagrams of the relation of whole languages. These resemble constructions commonly called family trees. The entities so related are quasi-complete grammars. The completeness of the history which we reach depends upon our ability to interpolate with certainty or acceptable probability.

7. All modern languages traced to a single node in a tree are of equal age in divergence. For a language, the only meaningful *age* is that of divergence. All languages (of a given human species) are of equal age. No language is older than another until we succeed in dating two strains of *homo loquens*.

8. When quasi-exhaustive equivalences can no longer be found, we have reached the end of the construction of a tree. Thus, a tree is bounded by our knowledge, i.e. our ignorance and/or ingenuity.²

² Examples of ingenuity are evident in etymological investigations, upon which brittle science the construction of these trees rests; a matching such as Armenian *khirtm* = English *sweat* = Albanian *dirsë* is not evident at first glance, but emerges from a consideration of regular sound

9. All languages or trees not so included in that tree fail to be shown as related within that family tree. Thus what we discover is our failure (= non-doing).

10. It is illicit to claim a genetic relation for a language or tree where such a tree relation has not been shown. We do not have a usual name for not-doing or not-finding.

11. For such failures in tree-relation, two situations are true:

- a. claims of a familial relatedness across these discontinuities are fruitless;
- b. the demonstration of further more inclusive tree relations among languages remains a task for the future.³

Non-relatedness has at present no known proof.⁴

12. The presentation of extensive illicit claims means a costly intrusion on the working time of careful scholars who must inspect the total data and detect flawed equivalence claims. The disassembly of illicit trees is a time-taking and potentially annoying business which displaces useful scholarly work.⁵

13. The dissemination of such extended claims gravely misleads the public, a public not equipped to test for such technical failures.

With these principles in mind, hypotheses concerning possible related languages and language families can be judged.

correspondences known to have affected these languages, and thus requires some ingenuity to be arrived at.

³ Thus, Indo-European and Afro-Asiatic, Uralic, and Kartvelian may yet be shown to constitute a “Nostratic” family (and Hurro-Urartean may go with Indo-European); my own feeling here is that no genetic relationship has been proved at all by rules which we understand — at most, lines of possible genetic inquiry may have been pointed out by an extremely imaginative comparativist (Illych-Svitych) whom we all admire and whose premature loss we deplore. Other non-Nostratic inquiries remain unsolved: it seems to me, for instance, to be strongly likely that Chimakuan can be related to Wakashan, but we still have no real proof of attachment for the large Salish family; clearly the first necessity here is to produce an adequate reconstruction for the Salish family as a whole, for which we do not yet really know with assurance the range of forms even of the basic personal pronouns.

⁴ Though as noted in the introduction to this volume, what are essentially accidents of history may lead to situations in which we can know that two languages are not possibly related, as with Esperanto and American Sign Language; such situations are rare, to be sure, but they represent what the outer limits are in the determination of relatedness.

⁵ As does the composition of these lines by us!

NOSTRATIC, EURASIATIC, AND INDO-EUROPEAN

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1. *Introduction*

As the twentieth century draws to a close, it is no longer reasonable to hold to the view that Indo-European is a language isolate — thirty years have already passed since a group of Russian scholars (most notably Vladislav M. Illič-Svityč and Aaron B. Dolgopolsky) successfully demonstrated that Indo-European is related to several other language families of northern and central Eurasia and the ancient Near East. Since then, not only has this work been continued by the Russians (regrettably, Illič-Svityč was killed in an automobile accident in 1966), it has also been taken up by a number of other scholars in other countries, who have verified the initial results arrived at by the Russians, who have refined the methodology, who have greatly expanded the number of cognate sets, who have clarified issues related to phonology, who have identified additional grammatical formants and have begun to piece together the early development of morphology in each of the daughter languages, and who have made great strides in problems of subgrouping.

2. *Methodology*

At the present time, some of the work being done in distant linguistic comparison is of very high quality, adhering strictly to the methodological principles established by the founders of Indo-European comparative linguistics, while other work is quite speculative and less methodologically rigorous. Moreover, there are two main approaches being utilized: the first approach may be termed “taxonomy first”, which seeks first and foremost to classify languages into valid groupings, that is, into language families and/or macrofamilies, while the second approach may be termed “reconstruction first”, which, as the name implies, emphasizes reconstruction. The first approach is reminiscent of the beginnings of Indo-European comparative linguistics, where relationship was first established by the early pioneers such as Rasmus Rask, Franz Bopp, and Jacob Grimm, and it was only much later, beginning with August Schleicher, that actual reconstruction took place, though the need for reconstruction had been recognized as early as 1837 by Theodor Benfey. The two approaches are actually not mutually exclusive, but, rather, properly used, they can inform and

further one another. I, personally, would give the edge to “taxonomy first”. After all, one cannot successfully reconstruct until one has first established which languages might have a reasonable chance of being genetically related, that is to say that one must know which languages to compare. (See Ruhlen 1994: 195–196 for a discussion of the difference between classification and reconstruction.)

The early founders of Indo-European comparative linguistics placed great importance on the comparison of grammatical forms, and this bias continues to the present day in Indo-European studies and has even been carried over into the study of other language phyla. However, this overemphasis on the comparison of grammatical forms is far too restrictive and was the reason that the Celtic languages, which have developed many unique features, were not immediately recognized as Indo-European. Rather, as noted some sixty ago by Holger Pedersen (1931:245):

That agreement in the inflectional system is an especially clear and striking proof of kinship, no one denies. But it is only an anachronism in theory, which has no significance in actual practice, when such an agreement is still designated as the only valid proof. No one doubted, after the first communication about Tocharian..., that the language was Indo-European, though at that time virtually no similarities in inflection had been pointed out. Such similarities have since been shown, but even where they are almost obliterated, proof of kinship could be adduced from the vocabulary and from sound-laws. Hardly any one will assert that it would be impossible to recognize the relationship between, say, English and Italian, even without the help of other related languages or of older forms of these two languages themselves, although agreements between the inflectional systems are practically nonexistent.

From the modern point of view it must be said that proof for relationship between languages is adduced by a systematic comparison of languages in their entirety, vocabulary as well as grammar. The reason why earlier scholars felt they should disregard the vocabulary was that they knew of no method of systematic comparison in this field.

In Chapter III of his book *Essays in Linguistics*, Joseph Greenberg (1957:35–45) lays out a set of principles for establishing genetic relationship among languages, and these are worth repeating. Greenberg notes that the only way to establish hypotheses about genetic relationship is by comparing languages. However, the problem is in knowing which languages to compare and in knowing what to compare since not all aspects of language are equally relevant to comparison. To be meaningful, comparison must strive to eliminate chance resemblances and to separate borrowings from native elements. This is often easier said than done; however, Greenberg lays out two main techniques for detecting borrowed lexical items. First, he notes that borrowing is most

commonly confined to certain semantic spheres (for example, cultural items) and certain grammatical categories (nouns far more often than verbs). Second, borrowed words can be distinguished from native vocabulary by expanding the range of comparison to include additional languages.

The simplest way to establish genetic relationship is by identifying a large number of similar morphs (or allomorphs) —especially irregularities — in similar environments in the languages being considered. Another significant indicator of probable genetic relationship is the presence of similar rules of combinability. Unfortunately, historical processes over the passage of time bring about the gradual transformation and eventual elimination of such similarities. The longer the period of separation, the lesser the chances will be that similarities of morphological forms and rules of combinability will be found.

Fortunately, there remain other factors that can be helpful in determining possible genetic relationship. One significant factor is the semantic resemblance of lexical forms. Here, it is important to be able to establish recurrent sound-meaning correspondences for a reasonably large sample of lexical material. Lexical forms with identical or similar meanings have the greatest value. Next in value come forms that, though divergent in meaning, can convincingly be derived, through widely-attested semantic shifts, from earlier forms of identical or similar meaning. The chances that lexical resemblances indicate genetic relationship increase dramatically when additional languages are brought into the comparison and when these new languages also exhibit a very large number of recurrent sound-meaning correspondences. Greenberg has termed this method “mass comparison” (more recently, he has used the term “multilateral comparison”). He considers the comparison of basic vocabulary from a large number of languages from a specific, wide geographic area to be the quickest and most certain method to determine possible genetic relationship. To Greenberg, lexical data are of paramount importance in attempting to establish genetic relationship among languages, especially in the initial stages of comparison.

It is only after these preliminary steps have been undertaken that meaningful comparison can begin. That is to say, and to reiterate, we must first have a good sense of which languages are likely candidates for comparison.

Now let us look at the basic principles underlying the Comparative Method — they may be summarized as follows: The first step involves the arduous task of data gathering, placing special attention on gathering the oldest data available. Once a large amount of lexical material has been gathered, it must be carefully analyzed to try to separate what is ancient from what is an innovation and from what is a borrowing. After the native lexical elements have been reasonably identified in each phylum, the material can be compared across phyla to determine

potential cognates. Once a sufficient body of potential cognates has been identified, one can begin to work out the sound correspondences. Not only must the regular sound correspondences (that is, those that occur consistently and systematically) be defined, exceptions must also be explained. Here, widely-attested sound changes (palatalization, metathesis, syncope, assimilation, dissimilation, etc.) provide the key to understanding the origin of most exceptions. In other cases, the analysis of the influence that morphology has exerted provides an understanding of how particular exceptions came into being. Some exceptions, though clearly related, simply defy explanation. All of these must be noted. The final step involves the reconstruction of ancestral forms and the formulation of the sound laws leading to the forms in the descendant languages, identifying the laws that have produced the regular sound correspondences as well as the exceptions. The same principles apply to the reconstruction of grammatical forms and rules of combinability and to the identification of the historical transformations leading to the systems found in the daughter languages. Invariably, it takes the dedicated efforts of several generations of scholars to work out all of the details. Here, we may cite the case of Indo-European — as even the most casual reading of Lehmann's book (1993) on the *Theoretical Bases of Indo-European Linguistics* shows, after nearly two full centuries of investigation of what must surely be the most thoroughly-studied language family on the face of the earth, there still remain many uncertainties about the reconstruction of the Indo-European parent language.

It was necessary to discuss these issues in order to address concerns that have been raised about the applicability of traditional methods of comparison and internal reconstruction to long-range comparison. It must be made perfectly clear that the same principles are just as applicable to long-range comparison as they are to any other type of linguistic comparison. The fact is, these are the only tools we have. Moreover, they work — their efficacy has been proven over and over again. (The most thorough presentation of these methods is to be found in Anttila 1989:229–273 and Hock 1991:532–626.)

It has been claimed that these methodologies break down when one tries to apply them beyond a certain time limit, say, 5,000 to 10,000 years ago. However, these dates are really quite arbitrary. One can cite, for example, the case of the aboriginal languages of Australia. Archaeological evidence indicates that Australia has been inhabited by human beings for approximately 40,000 years. Though there remain many unsettled questions, such as exactly when Proto-Australian was spoken (probably at least 30,000 years ago), or about how the different languages should be subgrouped, and so on, all extant languages appear to belong to the same language family (cf. Ruhlen 1991:188), and

comparative work on these languages is continuing apace (cf. Dixon 1980). Another example that can be cited is the case of the Afroasiatic language family. Due to the extremely deep divisions among the six branches of Afroasiatic (Semitic, Egyptian, Berber, Omotic, Cushitic, and Chadic), which are far greater than those found, by way of comparison, among the earliest attested branches of Indo-European, the Afroasiatic parent language must be placed as far back as 10,000 BCE, or perhaps even earlier, according to some scholars. This extremely ancient date notwithstanding, the major sound correspondences have been determined with great accuracy (cf. Diakonoff 1992; Ehret 1995), excellent progress is being made in reconstructing the common lexicon (cf. Ehret 1995; Orel-Stolbova 1995), and scholars are beginning to piece together the original morphological patterning, though progress here lags behind other areas.

3. *Nostratic*

One large-scale grouping that has been proposed at various times and by various scholars is the so-called “Nostratic” macrofamily — the name “Nostratic” was first suggested by Holger Pedersen in 1903 (it is derived from Latin *nostrās* “our countryman”). Though the “Nostratic Hypothesis” has occupied the efforts of a handful of scholars from time to time, for the most part, it has been ignored by most scholars — the early work done was simply not of high quality and, therefore, was not convincing. However, beginning in the early 1960’s, interest in the Nostratic Hypothesis was revived by the work of two Russian scholars, namely, Vladislav M. Illič-Svityč and Aaron B. Dolgopolsky, who first started working independently and, at a later date, through the efforts of Vladimir Dybo, cooperatively. Their work, though not without its own shortcomings (see below, §4), was the first successful demonstration that certain language phyla of northern and central Eurasia, as well as the ancient Near East, might be genetically related. Following Pedersen, they employed the name “Nostratic” to designate this grouping of languages. In particular, Illič-Svityč, in the course of several publications, culminating in his posthumous comparative dictionary, which is still in the process of publication, included Indo-European, Kartvelian, Afroasiatic, Uralic, Dravidian, and Altaic in his version of the Nostratic macrofamily. From his very earliest writings, Dolgopolsky also included Chukchi-Kamchatkan and Eskimo-Aleut.

The most important question that should be addressed is: What is the basis for setting up a Nostratic macrofamily? First and foremost, the descendant languages can be shown to share a large common vocabulary. In an article published in 1965, Illič-Svityč listed 607 possible common Nostratic roots, but only 378 have been published to date in his posthumous comparative Nostratic dictionary. It should be noted that there are differences between the etymologies

proposed in 1965 and the items included in the later dictionary: first, some of the items listed in 1965 do not appear in the dictionary; next, minor changes have been made to several of the earlier etymologies. Dolgopolsky currently claims to have over 2,000 common Nostratic roots, but only a small portion of this material has been published to date. In a joint monograph by myself and John C. Kerns, entitled *The Nostratic Macrofamily*, a great deal of lexical material is supplied from the Nostratic daughter languages to support 601 common Nostratic roots — this has now been expanded to 651 roots in my most recent book (Bomhard 1996). It should be mentioned here as well that Greenberg is currently preparing a book entitled *Indo-European and Its Closest Relatives: The Eurasiatic Language Family*, in which a large amount of lexical material will be discussed, though Greenberg's Eurasiatic is not the same as Nostratic (see below, §5). As is to be expected, the various branches of Nostratic investigated to date exhibit regular sound correspondences (see Appendix 2 for details), though, it should be mentioned, there are differences in interpretation between Illič-Svityč and Dolgopolsky on the one hand and myself on the other. Finally, a moderate number of common grammatical formants have been recovered.

Notable among the lexical items uncovered by Illič-Svityč, Dolgopolsky, and myself is a solid core of common pronominal stems (these are listed below in Appendix 1, though only the stems represented in Indo-European are given — the Proto-Nostratic reconstructions are given according to my system; for information on other pronoun stems, cf. Dolgopolsky 1984). These pronominal stems have particular importance, since, as forcefully demonstrated by John C. Kerns (1985:9–50), pronouns, being among the most stable elements of a language, are a particularly strong indicator of genetic relationship (Ruhlen 1994:92–93 makes the same point). Kerns (1985:48) concludes (the emphasis is his):

The results are overwhelming. We are forced to conclude that the pronominal agreements between Indo-European and Uralic, between Uralic and Altaic, and between Indo-European and Altaic, did not develop independently, but instead were CAUSED by some UNIQUE historical circumstance. In short, it is extremely unlikely that the three pronominal systems could have evolved independently.

The conclusion seems inescapable that the consistent, regular correspondences that can be shown to exist among the Nostratic descendant languages as well as the agreements in vocabulary and grammatical formants that have been uncovered to date cannot be explained as due to linguistic borrowing and can only be accounted for in terms of common origin, that is, genetic relationship — it would simply be unreasonable to assume any other possibility. This does not mean that all problems have been solved. On the contrary,

Nostratic studies are still in their infancy, and there remain many issues to be investigated and many details to be worked out, but the future looks extremely exciting and extremely promising.

4. *Critique of Muscovite views on Nostratic*

In this section, I would like to make several comments about recent Muscovite research on Nostratic. Specifically, I will deal with this research as it has been codified in Illič-Svityč's comparative Nostratic dictionary. Let me begin by stating unequivocally that I have the highest admiration for what scholars of the Moscow School have achieved. Their research has opened up new and exciting possibilities and has given Nostratic studies new respectability. However, this does not mean that I agree with everything they say. I regard their work as a pioneering effort and, as such, subject to modification in light of recent advances in linguistic theory, in light of new data from the Nostratic daughter languages, and in light of findings from typological studies that give us a better understanding of the kind of patterning that is found in natural languages as well as a better understanding of what is characteristic of language in general, including language change.

We can begin by looking at phonology. In 1972 and 1973, the Georgian scholar Thomas V. Gamkrelidze and the Russian scholar Vjačeslav V. Ivanov jointly proposed a radical reinterpretation of the Proto-Indo-European stop system. According to their reinterpretation, the Proto-Indo-European stop system was characterized by the three way contrast glottalized ~ voiceless (aspirated) ~ voiced (aspirated). In this revised interpretation, aspiration is viewed as a redundant feature, and the phonemes in question could also be realized as allophonic variants without aspiration. Paul J. Hopper independently proposed a similar reinterpretation at the same time (cf. Hopper 1973).

This reinterpretation opens new possibilities for comparing Proto-Indo-European with the other Nostratic daughter languages, especially Proto-Kartvelian and Proto-Afroasiatic, each of which had a similar three-way contrast. The most natural and straightforward assumption would be that the glottalized stops posited by Gamkrelidze, Ivanov, and Hopper for Proto-Indo-European would correspond to glottalized stops in Proto-Kartvelian and Proto-Afroasiatic, while the voiceless stops would correspond to voiceless stops and voiced stops to voiced stops. That is to say that this is where one should begin when looking for potential cognates. In so doing, one finds that consistent, systematic sound correspondences can indeed be established in which the glottalized stops posited by Gamkrelidze, Ivanov, and Hopper for Proto-Indo-European correspond to glottalized stops in Proto-Kartvelian and Proto-Afroasiatic, and in which the voiceless stops correspond to voiceless stops and voiced stops to voiced stops.

This, however, is quite different from the correspondences proposed by Illič-Svityč. He sees the glottalized stops of Proto-Kartvelian and Proto-Afroasiatic as corresponding to the traditional plain voiceless stops of Proto-Indo-European, while the voiceless stops in the former two branches are seen as corresponding to the traditional plain voiced stops of Proto-Indo-European, and, finally, the voiced stops to the traditional voiced aspirates of Proto-Indo-European. Illič-Svityč then reconstructs Proto-Nostratic on the model of Kartvelian and Afroasiatic with the three-way contrast glottalized ~ voiceless ~ voiced.

The mistake that Illič-Svityč made was in trying to equate the glottalized stops of Proto-Kartvelian and Proto-Afroasiatic with the traditional plain voiceless stops of Proto-Indo-European. His reconstruction would make the glottalized stops the least marked members of the Proto-Nostratic stop system. Illič-Svityč's reconstruction is thus in contradiction to typological evidence, according to which glottalized stops are uniformly the most highly marked members of a hierarchy (for details on phonological markedness in general and on the frequency distribution of glottalized stops in particular, cf. Gamkrelidze 1978). The reason that Illič-Svityč's reconstruction would make the glottalized stops the least marked members is as follows. Illič-Svityč posits glottalics for Proto-Nostratic on the basis of one or two seemingly solid examples in which glottalics in Proto-Afroasiatic and Proto-Kartvelian appear to correspond to the traditional plain voiceless stops in Proto-Indo-European. On the basis of these examples, he assumes that, whenever there is a voiceless stop in the Proto-Indo-European examples he cites, a glottalic is to be reconstructed for Proto-Nostratic, even when there are no glottalics in the corresponding Afroasiatic and Kartvelian forms! This means that the Proto-Nostratic glottalics have the same frequency distribution as the Proto-Indo-European traditional plain voiceless stops. Clearly, this cannot be correct. To bring the reconstruction of Proto-Nostratic into agreement with the typological evidence, the correspondences between Proto-Kartvelian and Proto-Afroasiatic on the one hand and Proto-Indo-European on the other should be modified so that the voiceless stops found in Proto-Kartvelian and Proto-Afroasiatic correspond to the traditional plain voiceless stops in Proto-Indo-European (which Gamkrelidze and Ivanov reinterpret as voiceless [aspirated] stops), so that the glottalics correspond to the traditional plain voiced stops in Proto-Indo-European (which Gamkrelidze and Ivanov reinterpret as glottalics), and so that the voiced stops correspond to the traditional voiced aspirates in Proto-Indo-European (which Gamkrelidze and Ivanov also interpret as voiced [aspirates]) (see below, §6.5, for additional remarks on the revisions proposed by Gamkrelidze and Ivanov).