

Book Review

Reconstructing Proto-Nostratic, Comparative Phonology, Morphology, and Vocabulary,

Bomhard, Allan R. 2008. Volumes 1 (xxi, 875pp) & 2 (976pp). Leiden, Boston: Brill.

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1. Description

The book is the second opus magnum by Allan R. Bomhard on Nostratic. The two thick volumes represent an enormous amount of work and a real achievement. It bears testimony to the issues and hardships that must be addressed in such an endeavor. The review focuses on Volume 1, especially on methods and principles. Discussing the immense data listed in Volume 2 is not possible here.

The book is fairly linear in spite of its size. Volume 1 begins with historical and methodological considerations, defines the perimeter of Nostratic languages, discusses the theory of PIE taken into account, provides synthetic descriptions of each sub-family, describes the theory of Proto-Nostratic hypothesized by the author, proposes a homeland and a framework for the breakup of the super-family, lists potential grammatical morphemes and outlines a grammar of Proto-Nostratic. It provides a huge list of references and a compilation of roots ascribed to Proto-Nostratic. Volume 2 lists the proto-words and their supposed reflexes in the sub-branches. The last part of Volume 2 is an appendix listing Caucasian items which the author considers loanwords resulting from contacts rather than cognates (*cf.* I:447-8 about Colarusso 1992). It is emphasized at the beginning of Volume 1 that a novel addition is “a comprehensive treatment of Nostratic morphology” (I:XX), a point which has been insufficiently dealt with before: “This addresses one of the main criticisms often directed at the Nostratic Hypothesis, namely, the relative dearth of morphological evidence presented by its proponents. For the first time, all aspects of the putative proto-language are discussed in detail: phonology, morphology, vocabulary, syntax, and homelands”. The term ‘morphology’ is nevertheless misleading as Part II, titled Morphology, includes primarily lexical items like pronouns, deictics and indeclinables.

2. Critical assessment

This theory of Proto-Nostratic is centered on PIE: “This book will focus on Indo-European. The purpose is to show that Indo-European is not genetically isolated” (I:1). In this approach Proto-Nostratic is a kind of Super-Pre-PIE. This explains that large sections are devoted to discussing PIE issues. The validity of Proto-Nostratic as theorized in the book depends on the validity of the approach(es) chosen for PIE. This is a distinctive feature of Proto-Nostratic as investigated by the author: a strong reliance on PIE. Other approaches were/are not centered on PIE to that extent. What methodological artefacts this preeminence of PIE may generate is an issue in this approach of Nostratic. In all cases the book cannot provide any feedback on PIE as these points are a given, a reference, to which the non-IE languages are compared. The methodology applied to the putative members of the Nostratic family is to sieve them with PIE and to sort out all that can go through that riddle according to a number of semantic and formal criteria. What can be observed from Volume 2 is

that the processes applied by the author do have an output. The non-IE languages do show a number of PIE-compatible items, which are assumed to be inherited. Now the core issue is: What does the output of the processes applied in the book and claimed to represent Proto-Nostratic really mean? How reliable is it?

Chapter 1 begins with a historiography of Nostratic, starting with Holger Pedersen who coined the word in 1903, and reviewing the works of Illič-Svityč, Dolgopolsky and Greenberg among others. The chapter delimits the perimeter of Nostratic taken into account. Are included: Indo-European, Uralo-Yukaghir, Altaic, Chukchi-Kamchatkan, Gilyak, Eskimo-Aleut, Etruscan (forming a Eurasiatic subgroup), Kartvelian, Afrasian, and Elamo-Dravidian (considered distant relatives of PIE). Sumerian is considered related to but not included in Nostratic (I:6-7). Caucasian is discussed several times but assumed to be outside Nostratic. The author emphasizes that he applies the standard comparative method (I:8). The basic principles of this method are described (I:10). The prejudice or dogma that comparative work cannot be done after a period of time is refuted (I:11) on the basis of Australian languages. The next pages (I:22-26) explain some reasons why the author favors the Glottalic Theory of PIE: traditional *t, *d, *dh > glottalic *t', *t', d. The riddle applied to non-IE languages is hence Glottalic PIE. The term 'reconstruction' of Proto-Nostratic is somewhat misleading as what the book does is more a generalization of Glottalic PIE to a larger perimeter of languages than the IE languages. This generalization relies on a modelization of Proto-Nostratic primarily based on Glottalic PIE with a number of additional proto-phonemes. This is the scientific object that the reader is invited to discover in the book: Proto-Nostratic is assumed to coincide with the output filtered through the compatibility with an enriched Glottalic PIE. As will appear below the compatibility applies more to consonants than to vowels. We have the input: the available theories and reconstructions of PIE's alleged relatives; the filtering riddle: Glottalic PIE; the output: a list of words and morphemes; the criteria for formal and semantic compatibility: the comparative method and Buck's *A Dictionary of Selected Synonyms in the Principal Indo-European Languages*.

Chapter 2 is a synthetic introduction to the Nostratic languages, including Sumerian. Specific chapters dealing with the phonologies of each sub-family are included later on in the book. It could have been interesting to list the main sources of data used for each sub-family [this is provided II:4] and to discuss their qualities and drawbacks. The internal classification of IE languages is based on Ringe et al. (1998), even though it is not clear what impact this point has on the rest of the theory. Concerning Afrasian it is noteworthy that "the study of Afrasian as a whole is still not far advanced" (I:37). This typically raises the issue of determining whether Afrasian, or any other group, is considered a distant relative of PIE because it is actually so, because it is too poorly known or because linguists have not been able to make a better use of existing data. The same question applies to Sumerian and Etruscan. Yukaghir is nearly dealt with as a component of Uralic. Elamite is considered to be close to Dravidian, although I doubt this point is firmly settled and accepted. Altaic refers (with ambiguity) to a minimalist perimeter: Mongolian, (Manchu)-Tungus, and (Chuvash)-Turkic. The other (short) sections are Chukchi-Kamchatkan, Gilyak, Eskimo-Aleut, Etruscan and Sumerian.

Chapters 3 and 4 deal with the phonology of PIE. These chapters are of paramount importance as they provide the riddle used to sort out the data of the non-IE languages. Chapter 3 is a historiography of the successive PIE proto-systems from Schleicher, the Neogrammarians, etc. to the Glottalic Theory. The chapter discusses classical issues: the near absence of (trad.) *b, the typological implausibility of the system embedded in the traditional graphemes, the laryngeals, the issue of velars and palatals, etc. The author fervently supports the Glottalic Theory and the pattern of sound correspondences is chosen accordingly. Nothing proves that a different pattern of sound correspondences would not yield more lexical output. Maybe a better or different match is possible

among the same languages or a subset of them. What is most troublesome is that the “results” are to a large extent the (methodo)logical consequences of a number of premises: they are profiled by the sound correspondences entailed by Glottalic PIE applied as a riddle. I remain somewhat unconvinced that this is the only choice possible, or the best one. In all cases it is troublesome that a theory about Nostratic presupposes that a theory of PIE should be first adopted irreversibly. The author should address the issue of how much of the “results” is contained in his method or in the data. The book is fairly linear, which is comfortable for the reader, but at the same time this feature blurs the fact that a number of choices and premises with a potential bearing on the final results are implicit. The proof that PIE is indeed not isolated would be stronger if Nostratic were reconstructed without PIE and then compared to PIE.

It can be further observed that none of the languages included in the Eurasiatic subset has glottalized phonemes. A theory relevant for the Eurasiatic subset does not need that feature. From a comparative point of view it stands as a contradiction that this feature is introduced where it is not evidenced by internal data. This feature is there because it is implicitly hypothesized that Afrasian emphatics are inherited from that kind of phonemes. Were that feature not introduced in Proto-Eurasiatic then the whole process of comparison would stall. It is not difficult to see what is at stake with the Glottalic Theory of PIE but it is paradoxical that the glottalized feature is introduced in PIE to account for the languages which are supposedly its most *distant* relatives. It would be safe to work on the intermediary level of Proto-Eurasiatic without that constraint. The reconstruction of Proto-Nostratic cannot be a “take-it-all” or “leave-it-all” alternative. Different ways must be found to assess the validity of intermediary stages in the integration of already established families into increasingly larger groups.

Apart from these theoretical and methodological considerations the heuristic potential of Glottalic PIE may not be as high as the author wants it to be. This theory is supposed to explain the gaps in the PIE root structures (I:55-56). But Kartvelian itself accepts roots with two (or more) glottalized phonemes, as is amply exemplified in Klimov's dictionary: **c'k'ep'-* ‘to cut into small pieces’, **č'eč'k'-* ‘to cut in little pieces’, **k'reč'- / *k'rič'-* ‘to shear, cut off’, **k'ep'-* ‘to cut in little pieces’, **c'k'ent' / *c'k'int'il* ‘(bird) faeces’, **c'ump'-* ‘to get wet’, **c'k'wert'-* ‘to roll up’, **k'uč'-* ‘to crush; small’, **p'rt'q'el-* ‘flat’, **k'ak'* ‘to bend, hook’, etc. Kartvelian, which somehow acts as an intellectual substrate for the Glottalic Theory (I:57), supports neither the impossibility or rarity of **p'* nor the impossibility of two (or more!) glottalized phonemes occurring in a row. That situation is troublesome as one cardinal premise of the theory lacks solid foundations.

What is more a look at Pokorny's dictionary shows that this work is strewn with variant forms where (trad.) **t/*d* and **k/*g* freely alternate. For example: Pokorny 357 **gel* = 544 **kel* ‘hill’ but no **ghel* with that meaning, Pokorny 369 **gēmā* ‘to marry’ = 612 **kom* ‘with’ but no **ghem*, Pokorny 369 **gēm-bh* ‘tooth’ = 902 **kem* ‘bit, mouthful’ but no **ghem*, Pokorny 380 **gēnu* ‘knee’ = 566 **kenk* ‘heel, bend of the knee’ but no **ghen*, Pokorny 370 **gen* = 558 **ken* ‘to compress into a ball’ but no **ghen*, Pokorny 373 **gēnā* ‘to give birth, beget’ = 563 **ken* ‘young, new’, but no **ghen*, Pokorny 383 **ger* = 567 **ker* ‘to cry hoarsely (bird)’ but no **gher*, Pokorny 382 **ger* = 938 **(s)kerp* ‘to gather’ but no **gher*, Pokorny 392 **gerebh* = 945 **(s)kerb(h)* ‘to scratch, scrape’ but no **gherb(h)*, Pokorny 385 **ger* = 948 **(s)ker(bh)* ‘curved’ but no **gher*, etc. All these items suggest that the gaps in the root structures of PIE may not originate in some phonological constraints as the Glottalic Theory has it, but may originate in the fact that the underlying opposition, dating back to some Pre-PIE phonological horizon, is between two series (trad.) **gh* and (trad.) **k/*g*. In that case the theory proposed in the book is flawed as it tries to establish sound correspondences for a (trad. voiced) series that may not exist. As the opposition between (trad.) voiced and (trad.) voiceless is erratic throughout

Indo-European lexical corpora this situation cannot be excluded. Another issue is that the current PIE theories are biased as they rely heavily on Germanic, Greek, Balto-Slavic and Indo-Iranian and less on peripheral branches like Anatolian, Tokharian or Italo-Celtic, which have retained many archaic features. See for example Mallory-Adams (2006:15) for feedback on this bias.

Chapter 4 develops the issues relative to PIE phonology. It begins with the laryngeals. On account of Hittite four laryngeals are posited. Their phonetic identity and the numerous laryngealist theories are discussed. A table (I:68) summarizes the identifications proposed. In addition to the “orthodox” vocalic coloring into *a, *e and *o, the author accepts the idea that laryngeals may have changed *i / *u to *e / *o respectively. This idea is “controversial” (I:66). It can be interpreted negatively as a trick to introduce more leeway. On the whole the way the consonantal system of PIE is dealt with is fairly conventional and acceptable, with the two reservations precedently made that the Glottalic Theory is not secure and that the opposition between (trad.) voiced and (trad.) voiceless may be a comparative illusion within PIE, which may not exist in Pre-PIE stages.

In contrast the way the vocalic system is handled is more debatable. On account of laryngeal coloring and lengthening (Cf. Saussure) and of the accent system (Cf. Hirt) it is possible to reduce the three “graphemes” *a, *e and *o to only one reconstructed phonological entity (I:84-5): the so-called “apophonic vowel”. When pitched this vowel appears as *e (high formants) and otherwise *o (low formants), which in my opinion means it is /a/. PIE is coherent with a limited set: *a (= trad. *e/o), *i, *u. Without providing explanations the author opts for a rich system, plus variants: (*i ~ e), *e, *(a ~ ə), *o, (*u ~ o). It is a kind of postulation pasted several times without clear justification: (I:87 & 101) for PIE, (I:143) for Kartvelian, (I:172) for Afrasian. This postulation eliminates the task of explaining how the families may have developed richer systems, although this is alluded at for Kartvelian (I:144). In addition this generates more freedom to “adjust” comparanda. Vocalism is the least acceptable part of the way PIE is pre-processed. The author transforms what are allophones of a single unit into three full-fledged phonemes, seemingly inherited. This ontological and descriptive trick amounts to an unacceptable multiplication of entities. Besides it is not possible to have the rich laryngeal inventory and the rich vocalic inventory at the same time. In other words the PIE theory chosen as riddle to filter the data of the alleged relatives of PIE is insecure as regards consonants and unacceptable as regards vowels. In addition IE peripheral languages are under-represented in “orthodox” PIE. All these points undermine the groundwork of Proto-Nostratic as modeled in the book.

The last part of Chapter 4 deals with accent and root structure. It is fairly conventional, even though the switches between strong stress and pitch seem ad-hoc and undocumented. The root structure is mostly based on the thoughts of Benveniste and Kuryłowicz. It would take too much space to discuss that root theory. It can be observed that the reconstruction of PIE is conspicuous for having a large number of similar roots with similar meanings and for accepting any phoneme as “erweiterung”, which does not seem realistic. The end of Chapter 4 is an appendix dealing with a description of the changes from Proto-Nostratic to (late) PIE through four hypothetical stages.

Chapter 5 explains how the phonological system of PIE developed in the different IE branches. This chapter might not exist as the developments from PIE to the daughter-languages are little controversial. In addition what happens after PIE started to split is irrelevant when it comes to external issues like Proto-Nostratic. Some parts are repetitive with Chapters 3 and 4. The interest is that the developments are explained in the Glottalic format. This chapter may be too strongly abridged to be useful as a reference.

The following chapters deal with each family or language of Nostratic. It could be interesting to have the main reference books and quantitative information on the number of roots and words reconstructed for each sub-family on an internal basis and available for external comparison. It could likewise be interesting to know what percentage of the reconstructed items of a given family can be accounted for by this theory of Proto-Nostratic. All these statistics would help assess the levels of representativity of the theory and of integration of each subgroup. At the present time it is not possible to make such an assessment for any of the subfamilies.

Chapter 6 describes the phonology of Proto-Kartvelian. Several key-features of Kartvelian are strikingly similar to those hypothesized for PIE, especially the root structure and the way vowels are inserted in the root structure. It is therefore puzzling and disturbing that Kartvelian is not considered to be a close relative of PIE. There is no clear explanation for that situation. The page (I:144) reads more like postulations than descriptions or explanations. In all cases Proto-Kartvelian seems to be a reliable reconstruction and its vocalism would support a three-phoneme vocalic system for Proto-Nostratic, as retrievable factors and processes seem to account for the emergence of *e and *o in Kartvelian.

Chapter 7 deals with Proto-Afrasian and its “six separate branches: Semitic, Berber, Egyptian (now extinct), Cushitic, Omotic, and Chadic.” (I:149). This chapter is long as this family is huge and not well-known at the same time. The different branches differ significantly as regards the number of vowels. There is much obscurity on the vocalism of Proto-Afrasian: “it must be cautioned that much work still needs to be done here” (I:172) and “the Proto-Afrasian vowels were subject to ablaut alternations that cannot be precisely defined at the present time.” (I:173) On the whole it would appear that Proto-Afrasian is not clear enough to be harnessed securely in the process of deep comparison. Besides the theory of the root structure adopted for Proto-Semitic is dubious: “It is thus now certain beyond any reasonable doubt that the third consonantal element of the Proto-Semitic root, be it infix or suffix, was simply not a part of the root, in the overwhelming majority of cases, at the Proto-Afrasian level and that the underlying basic root structure patterning was biconsonantal.” (I:176) There are hundreds of cases in Arabic which show that this (traditional) approach of the third consonant as being extra-radical is flawed. For example: ‘to cut’ can be said *batta*, *batara*, *barata*, *batala*, *balata*, *sabata* in Classical Arabic. The root *b_t accepts affixes in all positions. This verb also raises the issue of voice in Afrasian: Cf. in the book “Proto-Semitic *bad-ad- ‘to split, to divide, to separate’” (I:8) and “*pVt- ‘to break, to split, to cut’ > ‘to crush, to crumble’ (cf. Orël—Stolbova 1995:433, no. 2030, *pVtok- ‘to split, to cut’, 1995:178, no. 784, *fatVq- ‘to pierce, to split’, and 1995:180, no. 795, *fet- ‘to break, to cut’)” (II:105). In other words *bad, *bat, *pat, *fat all mean ‘to cut’. Is there not the same kind of problem as with PIE? Considering that the vowels are obscure, the consonants uncertain and the root structure dubious, Voltaire would probably have recommended not to deal with Proto-Afrasian. This raises the issue of the possible integration of unclear proto-languages into larger groups. This is mentioned in the book's concluding remarks: “the lack of adequate reconstructions for the non-Indo-European Nostratic proto-languages as a crucial problem that needs to be addressed” (I:520), originally Lehmann's assessment.

Chapter 8 deals with Uralic. Yukaghir is assumed to be related but is left undiscussed. The author states that vowel harmony is a “notable phonological characteristic of the Uralic languages” (I:177). More exactly vowel harmony is attested rather than characteristic. A large majority of Uralic languages do *not* have any kind of vowel harmony. The only subgroup where vowel harmony cannot be suspected of being a Turkic influence is Finnish and its closest relatives. The author observes that “there are still many uncertainties regarding the reconstruction of the Proto-Uralic vowels” (I:179) and “due to the uncertainties surrounding the reconstruction of vowels in Uralic, only consonants are presented in the following tables.” (I:181) The author does not address the issue of

geminated versus simple, which amounts de facto to positing two different series of consonants, and not just one. Another issue is Permic words with voiced initials in apparently inherited Uralic words. This feature, which casts doubts on most theories of PU, is not mentioned. The vowel system postulated for Proto-Nostratic cannot explain how the PU vowel system developed. The consonant system does not account for the phenomena that can be observed in Uralic. The reconstruction of only one series provides maximal leeway for comparison with PIE's three series. Whatever external comparanda are chosen, PU is statistically likely to yield a large output of compatible words.

Chapter 9 deals with (Elamo-)Dravidian. The author adopts the approach of Zvelebil (1970) and Krishnamurti (2003) for consonants. This approach has it that Proto-Dravidian did not have a voiced / voiceless contrast word-initially, nor word-internally. This approach cannot account for internal data, as reflected in Burrow-Emeneau, which indicates that Dravidian languages display that contrast: for example **daṭ*- 'big' versus **taṭ*- 'to knock, slap'. The idea that Dravidian languages could have developed an immense body of data displaying that contrast, each of them independently of the others, is just unbelievable. This example and Uralic typically raise the issue of how to integrate existing reconstructions into a wider construction, when they are dubious, obscure or most probably defective. The book does not explain how Proto-Dravidian vowel system could have developed out of a five-unit system. It is not clear whether Dravidian is considered a distant relative because it is really so or because the reconstruction chosen for Dravidian is inadequate. As with Uralic this one-series reconstruction offers maximal leeway for comparison with PIE's three series. In that respect it is worth pondering why the comparison of PIE and Dravidian does not yield more material.

Chapter 10 deals with the so-called Altaic languages. It begins with a historiography of the field and the difficulty of disentangling areal diffusion and cognacy among this "family", the perimeter of which has been warringly debated for decades. Actually one of the sentences of the chapter could apply to all the book: "diffusion, especially lexical diffusion [within Altaic], has tended to complicate the picture and has made it difficult to differentiate between that which is borrowed and that which is inherited" (I:201). An overarching issue is indeed to decide between chance coincidence, borrowing or inheritance as main causal principle. The author never explains in the book why the output of the processes he applies should be considered inherited cognates rather than chance coincidence or borrowings. The book discusses the different theories which have been proposed for Altaic consonants. The author relies on Starostin-Dybo-Mudrak (2003), after mentioning that "this dictionary must be used with caution" (I:203). There is a contradiction in indicating the sound correspondences listed in that work, including for Korean and Japanese, without accepting the conclusion that these two languages are part of Altaic or Nostratic. The book even mentions that "the quality of Japanese vowels in the first syllable is normally a good indicator of the original quality of the second vowel, which itself may have been assimilated or have disappeared altogether." (I:204) Proto-Altaic is thus integrated into a larger group: Nostratic, on the basis of features belonging to languages which are *not* accepted as Altaic by the author. It is hard to accept the idea that Altaic, whatever it really is or means, can be considered a close relative of PIE when so many key-issues are unsettled and insecure.

Chapter 11 deals with Eskimo-Alteut, Chukchi-Kamchatkan and Gilyak. In comparison with the preceding chapters, this one is rudimentary.

At this point of the review, three major features are emerging: (1) the theory proposed is only in a position to deal with consonants. The issue of Proto-Nostratic vowels is more or less beyond the scope of current knowledge and vocalism is the most elusive and uncertain part of the Proto-X's that the author strives to integrate into a common framework; (2) PIE is supposed to be the closest relative of Uralic, a dubious reconstruction, of Altaic, an insecure group, and of Eskimo-Alteut, Chukchi-

Kamchatkan and Gilyak, which are hardly dealt with; (3) Proto-Afrasian and Proto-Kartvelian which have the highest number of series and consonants and therefore offer the lowest probability for matches by chance coincidence are considered to be the most “distant” relatives of PIE. It is unclear if “distance” measures objective genetic reality or subjective difficulties in finding comparative output.

Chapter 12 deals with the phonological system and root theory proposed for Proto-Nostratic. It begins with the sentence: “Proto-Nostratic had a rich system of stops and affricates. Each stop and affricate series was characterized by the three-way contrast (1) voiceless (aspirated), (2) voiced, and (3) glottalized. The aspiration of series (1) was phonemically non-distinctive.” (I:213). This sentence is a bundle of issues. It means that the “original” system would have had 31 stops and affricates, 9 fricatives, 2 glides, 8 nasals and liquids: that is to say 50 consonants. In comparison PIE would have had 22(+1?) consonants (I:59), Kartvelian 36(+1?) (I:142), Afrasian 39(+2?) (I:150), Uralic 22 (I:178), Dravidian 16(+1?) (I:188), Altaic 23 (I:203), Eskimo 15 (I:209), Chukchi-Kamchatkan 16 (I:211), Gilyak 33 (I:212). If that system were accepted this would mean that Proto-Nostratic was about two or three times more differentiated phonologically than any of the daughter-languages and that a colossal loss - in fact a collapse - of phonological contrasts, that can be calculated to stand between 50 to 70%, happened without serious disruption in the ability of the daughter-languages to permit communication. That premise is clearly not acceptable. This suggests that a reasonable system for Proto-Nostratic should have about 25 phonemes, which is roughly the average number, or fewer, if possible, assuming that contrasts which do not appear to be solidly evidenced (cf. voice) can be eliminated as innovations. As regards vowels the book has it that “the following vowels may be reconstructed for Proto-Nostratic: *a, *e, *i, *o, and *u. At least some of these vowels must have been subject to considerable subphonemic variation in the Nostratic parent language.” (I:214) This “considerable subphonemic variation” is a fallacy: it means that there was no phonemic contrast between those “vowels” and that the inventory can and therefore must be reduced. In addition the theory tries to surreptitiously sell one more “phoneme / grapheme” <ə>, supposedly “higher variants [of the central low vowel *a]” (I:214). The following sentences raise considerable theoretical problems: “The high front and back vowels *i and *u, in particular, may be assumed to have had lowered variants (indicated in the Proto-Nostratic reconstructions as *e and *o respectively), [...] To complicate matters, *e and *o must also have existed as independent vocalic elements.” (I:214) A sound is either a (distinctive) phoneme or not. Phonology is not a kind of awale game applied to phonological features. The conclusion is that the proto-system proposed for Proto-Nostratic is not acceptable for either consonants or vowels, as it is much too rich in both cases. The ambiguity between graphemes used to note allophones or subphonemic entities and real distinctive phonemes is a kind of conjuring trick which undermines the claim that the book applies the comparative method.

Chapter 12 is very synthetic and little argued so that it sounds like a list of postulations, which in some cases conflict with preceding chapters and in some other cases cannot be easily plugged on the descriptions of local families. The descriptive and logical connections between what Proto-Nostratic is supposed to be and what each local family is supposed to be on its own is not apparent and traceable. A striking claim is: “The Proto-Nostratic vowels were, for the most part, preserved in initial syllables in Uralic, Dravidian, and Altaic. They appear to have been originally preserved in Proto-Afrasian as well.” (I:214) This can be compared with previous statements: (1) about Uralic: “there are still many uncertainties regarding the reconstruction of the Proto-Uralic vowels” (I:179), “due to the uncertainties surrounding the reconstruction of vowels in Uralic, only consonants are presented in the following tables.” (I:181); (2) about Altaic: “the quality of Japanese vowels in the first syllable is normally a good indicator of the original [Proto-Altaic] quality of the second vowel, which itself may have been assimilated or have disappeared altogether.” (I:204); “the vowel correspondences [of Altaic]

are extremely complicated — for details on the vowels, cf. Starostin-Dybo-Mudrak (2003: 90-134)” (I:207); (3) on Afrasian: “it must be cautioned that much work still needs to be done here” (I:172), “the Proto-Afrasian vowels were subject to ablaut alternations that cannot be precisely defined at the present time.” (I:173) In other words Proto-Nostratic “vowels” appear to be best preserved where the darkest obscurity about them prevails. This situation can be compared with the last sentence of this paragraph dealing with Proto-Nostratic vowels: “The inherited vowel system underwent a thorough restructuring in both Proto-Indo-European and Proto-Kartvelian as a result of a complicated series of changes initiated by the phonemicization of a strong stress accent in the early prehistory of these branches. These developments diminish the importance of Kartvelian and Indo-European for ascertaining the Proto-Nostratic vowel system.” (I:214) In other words the testimony of the most reliable reconstructions currently available is dismissed and this “complicated” series of changes is not explained nor described. The reconstructions of Kartvelian and Indo-European and the theory of Proto-Nostratic proposed for the vowels are concretely in a situation of systemic dislocation. There is doubtless a major problem in the theory here.

The next part of Chapter 12 deals with the root-structure and part-of-speech theory proposed for Proto-Nostratic: “On the basis of the evidence of Proto-Indo-European, Proto-Kartvelian, Proto-Afrasian, Proto-Dravidian, and Proto-Altaic, it may be assumed that there were three fundamental stem types: (A) verbal stems, (B) nominal and adjectival stems, and (C) pronominal and indeclinable stems. Some stems were exclusively nominal. In the majority of cases, however, both verbal stems and nominal stems could be built from the same root.” (I:215-216). The reasons why or how Uralic came to an undifferentiated situation for verbal or nominal stems is not explained.

Another issue is the difference between stem and root. The reader has to wait until (I:418) to see a definition: “A root may be defined as the base form of a word. It carries the basic meaning, and it cannot be further analyzed without loss of identity [...]. A stem, on the other hand, may be defined as an inflectional base. A stem may or may not be coequal with a root.” This rather cryptic definition can be compared with other statements in the book. It would appear from a number of reasonings that a stem is a stable segmental morpheme comprising a skeleton of consonants, called *root*, plus a given set of vowel(s), referred to as *ablaut*: “different root vowels are sometimes posited by them for two (or more) stems, all of which are clearly variants of the same root.” (I:391) Besides §17.3 about Ablaut asserts that “an analysis of the Afrasian (and, to a lesser extent, Dravidian) data seems to indicate that there was an alternation between the vowels *a, *i, and *u in Proto-Nostratic roots” (I:391).

Chapter 13 deals with the presumed homeland and dispersal of Nostratic languages. It begins with the following words: “Here, we run into potentially serious problems, for we must turn to other disciplines such as archeology.” (I:221) Linguistic issues are indeed enough of a huge problem to deal with in the first place. This chapter is a digest of numerous books and theses dealing with the homeland theories of each individual family. It first discusses the two main theories about PIE: Anatolia or South-(Eastern) Russia as homeland. The author favors the latter location: “Johanna Nichols (1997) has argued that the earliest Indo-European speech community (“Pre-Indo-European”) was located in Central Asia. She proposes that Pre-Indo-European spread westward across the steppes, eventually arriving on the northeastern shores of the Black Sea (Nichols 1997:135). I support this scenario. I would place the Pre-Indo-Europeans in Central Asia at about 7,000 BCE, and I would date the initial arrival of the Pre-Indo-Europeans in the vicinity of the Black Sea at about 5,000 BCE — this is somewhat earlier than the date Nichols assigns.” (I:224) The author's scenario looks like accepting the location of one theory and the higher dating of the other theory, which remains unexplained. He then proceeds to discuss possible lexical traces of contact between Caucasian Circassian and (P)IE. The next paragraph is about Proto-Afrasian: “According to Militarëv, the

original Afrasian homeland was in the Middle East and the Arabian peninsula [...] In my opinion, Militarev's proposals have great merit." (I:226) The next family is Kartvelian: "Proto-Kartvelian must have been roughly contemporaneous with Proto-Indo-European, which would imply a slightly earlier date. Therefore, I very hesitatingly suggest a date of around 5,000 BCE for Proto-Kartvelian. It is certain, at the very least, that Kartvelians were in their current location by that date." (I:229) Next is Uralic: "There is general agreement about the homeland of Uralic — Décsy (1990:9), for example, places the Uralic proto-language "in the Forest-Zone-Steppe-Border (mainly north of it) between the Volga Bend in Eastern Russia and the Ob River in Western Siberia" (I:230) Then Elamo-Dravidian: "the Elamo-Dravidian homeland may be placed roughly in western and central modern-day Iran at about 8,000 BCE. Elamo-Dravidian gradually spread eastward covering all of the Iranian plateau and extending into modern-day Pakistan and northwestern India. There was then an east-west split." (I:232) The "homeland(s)" of the other families and subgroup seem to be even more elusive. However interesting and learned these issues may be, the central problem is that they remain undiscussed and unsupported assertions. Pages (I:243-252) are maps of the scenario of split and expansion proposed by the author. It remains to be explained what Caucasian, which is not supposed to be Nostratic, is doing right in the middle of Nostratic languages.

Chapter 14 deals with Etruscan, and its closest sister-languages: Raetic and Lemnian. The author's approach is stated (p. 260): "In my opinion, Adrados and Woudhuizen have indeed shown that Etruscan is related in some way to Indo-European, but not as a daughter language — this differs from my previous position on this matter." The chapter amounts to a rather superficial introduction. No clear distinction is made between potential loanwords and possibly inherited words. The comparisons do not add up to sound correspondences, which are not provided as a matter of fact. Even though some comparanda are suggestive and intriguing, much if not everything remains to be done. Etruscan had 4 vowels and 16 consonants (I:253), which can be compared against 5 vowels and 50 consonants in Proto-Nostratic as modeled by the author (I:213): -20% for vowels and -68% for consonants.

Chapter 15 deals with Sumerian, a language (?) or maybe several (?) that the author does not consider daughter-language(s) of Nostratic: "the evidence seems to indicate that Sumerian is not a Nostratic daughter language at all but that it is distantly related to Nostratic. However, there are also many problems that must still be solved regarding the exact nature of that relationship — we have only scratched the surface in this brief summary." (I:272) This conclusion might apply to more languages than just Sumerian.

The second part of the book is titled "Morphology" and addresses issues that can be broadly labeled "grammatical". It comprises four chapters: Chapter 16 is mainly a collection and classification of morphemes and forms taken for individual languages, Chapter 17 tries to show how these forms can be integrated into a unified grammatical or syntactical framework, Chapter 18 describes the trad. reconstructed grammar and morphology of PIE and Chapter 19 the author's hypotheses and theories for (deep) Pre-PIE stages. The introduction of Chapter 16 (I:273) states: "I shall not attempt a systematic reconstruction of Nostratic morphology here, but, rather, I shall merely present the evidence — a systematic reconstruction of Nostratic morphology will be attempted in the following chapter [17]." That section relies substantially on previous researches by Greenberg.

Chapter 16 deals with a number of morphemes and forms which are divided into the following groups and sections: "pronominal, anaphoric and deictic stems" (I:274), "dual and plural markers" (I:302), "relational markers" (I:314), derivational suffixes" (I:334), verbal "non-finite forms" (I:354), verbal "finite forms" (I:359), "negative/prohibitive particles" (I:369), "interrogative, relative and

indefinite stems” (I:373), “indeclinables” (I:383). The extensive material collected in Chapter 16 and more specifically in Section I raises a number of issues:

1. A first issue is that the items are free or bound forms, in the latter case most often suffixes. It would be interesting to understand why the “same” morpheme can be a free form, a prefix or a suffix. There is a descriptive and theoretical issue in that disregard of temporal linearity and syntagmatic insertion.
2. Another issue is that forms have the ability to be affixed to about any kind of stems. This property undermines the claim that there were originally different classes of stems.
3. The next issue is that the forms are presented in an extremely atomistic way with very few examples in each individual language. This situation makes it hard to understand if the comparanda really belong to the same classes of linguistic entities. It is difficult to figure out what the paradigms may look like in the individual languages. Some forms are debatable: for example Mordvin *vana-n* ‘I see’ listed as evidence of Uralic *m. Nothing proves that *-n* is derivable from *-m(V). This theory, however received it may be in Uralistics, conflicts with Moksha Mordvin *vana-m(e)* ‘we see’. It could be taken as an example of *n. In addition the book states: “Proto-Uralic first person personal / possessive suffix *-m(V)” (I:276) but provides no example to support the second functional use. Incidentally Moksha Mordvin has *kud-oze* ‘my house’ and *kud-ne* ‘my houses’ (Nom.), again with no trace of that *-m(V) suffix. How does the theory account for the forms which do *not* enter its mold?
4. Another issue is that very often the real morphemes are longer than the hypothesized “proto-form”, which has to be extracted and clipped out of them. How does the theory account for the residual part? This pick-and-clip method tends to spiral naturally out of control: for example, why is “Semitic first person verb suffix: Akkadian *-ni*, Ugaritic *-n*, Hebrew *-nī*, Syriac *-n*, Arabic *-nī*, Geez *-nī*, etc.” (I:281) an example of *n but not of *iy(a)? Why is Semitic *naḥnu ‘we’ (I:282) an example of *n and not of *H?
5. The method has the potential to “prove” that any given phoneme can stand for any given meaning. And it precisely happens: 5 forms are proposed for P1 (Sg or Pl): *m, *k, *n, *H and *iya. One could hypothesize one more P1Sg “proto-form” on the basis of Moksha Mordvin *kud-oze* ‘my house’, Hurrian *išaš* (erg.) ‘I, me, P1Sg’, North Caucasian *zō, (erg.) *ʔez(V), (gen.) *ʔiz(V), (obl.) *zā-, Yenisseean *ʔaʒ, Burushaski *aʒ ‘P1Sg’. Even though this “omni-comparative” example is probably absurd, how can we tell the reliable examples from the absurd ones without circularity?
6. These forms are conspicuously homophonous with most deictic proto-forms and *n stands for P1 and P2, which suggests functional reinterpretations. This is alluded at in the book which cites Diakonoff 1988:70: “[t]he independent personal pronouns in the direct [absolute] case may be introduced by a special demonstrative element: Sem[itic] *’an-*, Eg[yp]tian] *jn-* and *nt-*, Berb[er] *n-*, *nt-*, Cush[itic] *an*, *a-*”. (I:284) Cf. §16.12. B (I:290) as well which deals with PIE *H₁e-ǵ(h)-o-m ‘P1Sg’. What looms out is that these “pronominal” proto-forms are fictitious and that only the deictic forms are correct.
7. The data are listed in the section to support the grammatical side of the theory but there are about never parallel sentences in the real languages which would substantiate the claim that the items look and function in the same grammatical way.

Section II deals with possible Plural markers. The morpheme *k^hi ‘dual’ is scantily attested and this meaning conflicts with several languages: Etruscan *ki* ‘three’ for example. The forms proposed for Plural raise the issue of being used both for nouns and verbs. It would seem that *t ‘Plural’ and *r ‘Collective’ offer a reasonable level of solidity.

Section III deals with ‘case-markers’. Several cases are distinguished but a minimal system opposing Absolute case *Ø ~ Oblique case *n seems reasonable, with subsequent refinements in each family. Local case-markers seem to originate in free forms specialized as case-markers: *m, *b, *r, *d. There are miscellaneous hitches: for example Erzya Mordvin *kudoń* (*kudo* ‘house’) is used to support both the Acc. *ma and Gen. *nu when the internal Mordvin reconstruction leans toward Gen. *ni > ŋ. In addition this form is cited as being “Mordvin” but is only Erzya.

Section IV deals with derivational devices. It begins with an ominous definition: “the cover term “nominalizer” is used for any suffix that is used to create nouns and adjectives [...] whether from verbs or nouns. Some of these forms are also listed under non-finite verb forms.” (I:334) There is clearly a descriptive problem with that definition, which incidentally undermines the difference between nouns and verbs. This section is messy and needs to be entirely reworked. The same problem exists in Section V about non-finite verbal forms. Some forms are aberrant: the Uralic “gerund” supposedly *nt (I:257). Mordvin, which is not cited, shows that these forms are local cases of the infinitive and therefore the comparison with PIE *nt- makes no sense. In addition Finnish may have been influenced by neighboring IE languages.

Section VI deals with finite verbal forms. It begins with a supposedly Imperative *-k which in my opinion is more adequately considered a form of P2Sg, as in Dolgopolsky 1984. The other items raise considerable problems.

Section VII deals with negative particles and Section VIII with interrogative, relative and indefinite pronouns, which are in fact lexical items. The material compiled is certainly worth considering and being actualized with examples and sentences. The last and very brief section IX deals with a handful of indeclinables. There is doubtless much more to say on such a topic, especially in relationship with deictics.

On the whole Chapter 16, misleadingly titled “Nostratic Morphology I: The Evidence”, does not fulfill the promise of “a comprehensive treatment of Nostratic morphology” (I:XX). The most secure items are lexical. The retrieval of the old layer of deictics seems secure, which casts doubts on the supposedly “pronominal” items.

Chapter 17 presents the theory of Proto-Nostratic grammar proposed by the author. It can be divided into four parts:

1. The main hypothesis is that Pre-PIE or Proto-Nostratic was an active-type language. The author lists some ideas of Klimov (1977) and Nichols (1992). That active-type system seems to dissolve grammar into semantics. The semantic features of nouns and verbs seem to predetermine their syntactical combinability. This leads to a kind of aporia and breakdown of grammar.
2. It is affirmed that Adjectives did not exist as a separate class of words. It is unclear what supports such a strong claim about the “original” syntax.

3. The next short paragraph about ablaut asserts that “an analysis of the Afrasian (and, to a lesser extent, Dravidian) data seems to indicate that there was an alternation between the vowels *a, *i, and *u in Proto-Nostratic roots and that that alternation had some sort of morphological or semantic significance.” (I:391) This seems to disprove that Proto-Nostratic had five (phonemic) vowels.
4. The next part is about root patterning. In spite of a number of excerpts and some rare examples it looks like a list of abstract postulates rather than features clearly rooted in and inferrable from the daughter-languages. The same kind of assessment can be made about the next part about syntax and parts of speech.
5. The following sections are about nominal and verbal morphology. They are significantly more concrete and descriptive. On the whole the theory is undermined by the fact that the morphemes are taken from Chapter 16 and are not solidly established. The complex issues addressed in these sections could probably be considerably developed so as to become a treatise of comparative grammar. Generally speaking it is not really clear how the daughter-languages and the purported mother-tongue interconnect. Besides some “explanations” are tautological: “Ultimately, these possessive suffixes had a pronominal origin.” (I:408)

Chapter 18 deals with the traditional approach of PIE morphology. To some extent it can be read as a digest and introduction to “orthodox” PIE, with interlinear comments by the author. On the whole the presentation is fairly conventional. The most debatable section is §18.7 about the issue of pitch or stress. Besides the author considers that the so-called *s mobile was not an affix, following Burrow or Lehmann rather than Benveniste on this point. This choice has a bearing on the root patterning as it eliminates a potential prefix.

Chapter 19 deals with what may have existed before PIE. A conspicuous feature of “orthodox” PIE is that it is a kind of commencement with no “before PIE”. It is therefore inevitable that a theory about the ancestor(s) of PIE would have to handle more than one stage. The author posits four stages: a very ancient stage called Pre-PIE, followed by three stages of PIE proper. For each stage the author lists a number of putative features that either appeared or were modified during that period. The last stage ‘Disintegrating-PIE’ about coincides with “orthodox” PIE as Anatolian languages are superficially taken into account as of now. The scenario relies heavily on the ideas of Lehmann (2002). In my opinion there are two major issues:

1. There is no clear traceability between the “original” Proto-Nostratic theory postulated in the book and the current reconstruction(s) of PIE.
2. Logically as PIE is supposed to be closer to “Eurasianic” languages one would expect that the sequence of stages would reflect the internal branching of Nostratic. For example, which stage do Uralic, a supposedly close relative, or Kartvelian, a supposedly more distant relative, originate in? This is actually not explained and it is not clear whether this would be possible.

In other words, even though the sequence of stages has a theoretical coherence, the problem is more that from a strictly comparative point of view it does not clearly interconnect PIE and Proto-Nostratic as hypothesized in the book and that from a macro-comparative point of view the internal structure of the family and the sequence of stages between PIE and its ancestor(s) as described and hypothesized in the book do not have a visible coherence. The concluding remarks of the author are indeed astounding: “This analysis has relied almost exclusively on Indo-European data with only passing reference to what is found in cognate Nostratic languages. The picture that emerges is rather

stark and, in my opinion, rather unrealistic [sic]. This does not mean, however, that there is no validity to that picture.” (I:520) The book begins with: “the purpose is to show that Indo-European is not genetically isolated” (I:1).

The following part is about potential links between PIE and Caucasian of either genetic or areal nature. This is dealt with as an Appendix. It could have been integrated earlier in the book as a chapter justifying the perimeter chosen for Nostratic.

The next and impressive section is References. There are a few typos and mistakes in the original edition which I have pointed out to the author.

The next and useful section is the *Index Verborum*, the roots and words. For each item it would be interesting to list which subfamilies have it, so as to provide an indication about the geolinguistic profile and relevance of the items.

3. *General conclusion*

I read the book as open-mindedly and favorably as possible without being complacent to what I perceived as inadequate. The main unsolved issues raised by the book are: the perimeter of Nostratic, the choice of Glottalic PIE and the subsequent risks of artefacts, the implausibly rich proto-system, the obvious difficulty of integrating inaccurate reconstructions, postulations without clear argumentation and sometimes visible incoherences. The book is nevertheless extremely stimulating on a very difficult topic and this is doubtless its most laudable virtue.

References

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