

Comment on:

Fournet, A. 2010. *Review of:* Bomhard, Allan R. 2008. *Reconstructing Proto-Nostratic, Comparative Phonology, Morphology, and Vocabulary, Volumes 1 (xxi, 875pp) & 2 (976pp).* Leiden, Boston: Brill // *The Macro-Comparative Journal*. vol 1.1.

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First, I would like to thank Arnaud Fournet for his thoughtful review of my book.

There are multiple things going on in this book. Though the main purpose of the book is to demonstrate that Proto-Indo-European is not genetically isolated but, rather, that it is related to several other languages/language families, to wit, Tyrrhenian, Uralic, Altaic, Gilyak/Nivkh, Chukchi-Kamchatkan, Eskimo-Aleut, Kartvelian, Dravidian, and Afrasian, the reconstruction of the Nostratic proto-language is not based upon Proto-Indo-European. Instead, the phonological system of Proto-Nostratic is extremely close to that reconstructed for Proto-Afrasian, while the morphology is based, to a large extent, upon Afrasian and Dravidian. (It may be noted in passing that my reconstruction of the Proto-Nostratic consonant system is extremely close to what Ehret [1980:37] posits for Proto-Southern Cushitic, but without the retroflex and prenasalized consonants.) This is how it should be. Afrasian is as an extremely old stock, going back at least 12,000 years, and even earlier, according to some scholars. In my opinion, it was the first to become separated from the rest of the Nostratic speech community. Consequently, we would naturally expect it to preserve archaic features that were lost elsewhere. To say Nostratic is a kind of “super-PIE” is, thus, not how I envision the situation. Indeed, one of the criticisms I direct against the views of Illič-Svityč (vol. 1, p. 24) is that he bases his reconstruction of the Proto-Nostratic vowels on what is found in Proto-Uralic, which is several millennia younger than Proto-Afrasian, which must, due to its age and archaic nature, play a more prominent role in the reconstruction of the Proto-Nostratic vowels (and consonants) than the younger daughter languages. It would be hypocritical of me, to say the least, to turn around and base my view of Nostratic on Proto-Indo-European, which is also several millennia younger than Proto-Afrasian.

Thus, the statement that follows that “The validity of Proto-Nostratic as theorized in the book depends upon the validity of the approaches chosen for PIE” also runs contrary to my views. I have tried to show that Proto-Nostratic is a valid linguistic phylum regardless of how one chooses to reconstruct Proto-Indo-European. Of course, I favor the glottalic model of Proto-Indo-European consonantism, and that means that I posit a different set of sound correspondences than do Illič-Svityč and Dolgopolsky. In my paper entitled “The Current Status of Nostratic Studies”, I address this issue explicitly on p. 7: “assumption 3 is not dependent upon any particular reconstruction of the Proto-Indo-European consonant system, though, it goes without saying, if assumption 1 is valid, it reinforces the likelihood that the revised set of Nostratic sound correspondences that Bomhard has proposed is correct.” The assumptions I make are listed (p. 6) as follows:

1. The traditional reconstruction of the Proto-Indo-European consonant system is flawed and is to be reinterpreted along the lines proposed, on the one hand, by Thomas V. Gamkrelidze and Vjačeslav V. Ivanov and, on the other hand, by Paul J. Hopper, as follows (the reconstruction of the Proto-Indo-European stop system posited by Lehmann [1952:99] is given for comparison):

Lehmann				Gamkrelidze—Ivanov			
b	b ^h	p	=	p'	bh/b	ph/p	
d	d ^h	t	=	t'	dh/d	th/t	
g	g ^h	k	=	k'	gh/g	kh/k	
g ^w	g ^{wh}	k ^w	=	k' ^u	g ^{uh} /g ^u	k ^{uh} /k ^u	

2. The frequency distribution of Proto-Nostratic stops (and affricates) in the reconstruction proposed by Illič-Svityč and Dolgopolsky is in contradiction to typological predictions, and is, therefore, highly suspect (see below).
3. Taking into consideration (1) the radical reinterpretation of the Proto-Indo-European consonant system proposed by Gamkrelidze, Ivanov, and Hopper, as well as (2) the problems in the frequency distribution of stops (and affricates) in the reconstruction of the Proto-Nostratic phonological system proposed by Illič-Svityč and Dolgopolsky, a different set of Nostratic sound correspondences is warranted.

Moreover, in volume 2 of my book, not all of the proposed Nostratic cognate sets contain Indo-European data. Again, this is as it should be, inasmuch as Proto-Nostratic is more than just an “enriched Glottalic PIE”.

Thus, my views about how Proto-Nostratic is to be reconstructed and what role Proto-Indo-European plays in that reconstruction appear to differ from how Fournet understands my ideas. This is problematic, because if he gets this impression, others are sure to get this impression as well. No doubt, the problem arose from the huge amount of attention that I paid to Indo-European as opposed to the other daughter languages as well as my failure to articulate clearly my views about the basis for reconstructing Proto-Nostratic. This is something I definitely need to be more explicit about.

Now, this leads to several other issues that Fournet correctly notes in his review. “The proof that PIE is indeed not isolated would be stronger if Nostratic were reconstructed without PIE and then compared to PIE.” Oh yes, these are very powerful words, and very true. Even though it is what I had envisioned, and it is the goal I had in mind, I found many obstacles to attaining this objective. Let us begin with Afrasian. While the phonological system of Proto-Afrasian has been reconstructed with a fair degree of accuracy (though not for all classes — the sibilants, affricates, and labiovelars are still fairly controversial), next to nothing has been accomplished regarding Proto-Afrasian morphology. The early attempts by Diakonov and Rössler are too dependent upon Semitic to be useful. There are still those who reject Chadic as a valid branch of Afrasian and who reject Omotic and Ongota as separate from Cushitic. These issues have a direct impact on Nostratic studies. Further understanding of Proto-Nostratic morphology must wait until more definitive work has been done on Proto-Afrasian morphology. Of course, I did use some tentative attempts to reconstruct Proto-Afrasian morphology, mainly from the work of Christopher Ehret (and others), but I have no delusions that the conclusions I reached are final. And, as I am sure everyone is aware, Ehret’s work has been criticized. Unfortunately, it is the best we have at present. Next, there is the problem of Altaic. And so on and so forth. Now it should be clear why I said at the end of volume 1 of my book (p. 520):

Comparison with other Nostratic daughter languages indicates quite clearly that a whole series of relational markers can be reconstructed for Proto-Nostratic, and at least some of these must have been inherited by Pre-Proto-Indo-European. As more work is done in reconstructing the proto-languages of the individual branches of Nostratic, future scholars will be able to arrive at a more accurate and more complete reconstruction of Proto-Nostratic. In so doing, the work done in one area will no doubt complement and further the work done in other areas so that we will be in a far better position to fill in the gaps that currently exist in our knowledge

concerning the early prehistory of the individual branches themselves. Lehmann (2002:250—251), in particular, identifies the lack of adequate reconstructions for the non-Indo-European Nostratic proto-languages as a crucial problem that needs to be addressed. I could not agree more.

It should now also be clear why I made the statement (vol. 1, p. 520):

In this and the preceding chapter, the Proto-Indo-European morphological system has been systematically analyzed in order to uncover the most ancient patterning. 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. This does not mean, however, that there is no validity to that picture. What it does mean is that we are not able to recover what has been lost on the basis of an examination and analysis of the Indo-European data alone, that is to say that the picture is simply incomplete.

Until the other branches of Nostratic have been reconstructed to the same level as Proto-Indo-European, we cannot accurately “fill in the gaps” that I alluded to by my statement that “[t]he picture that emerges is rather stark and, in my opinion, rather unrealistic.” With a better understanding of the prehistory of the closest relatives of Proto-Indo-European, we will be in a much better position to figure out what was most likely inherited by pre-Proto-Indo-European and to trace how these were replaced or remodeled as Proto-Indo-European developed its own individual characteristics.

Part of the problem with the traditional reconstruction of the Proto-Indo-European vowel system is that it is typologically bizarre. The low frequency of occurrence of **a* versus the great frequency of occurrence of the **e ~ *o* ablaut patterning has no parallels among attested languages. Universally, the vowel /a/ has the highest frequency distribution in attested languages. Fournet’s approach of seeing **á* > **e* and **à* > **o* gets around this problem quite nicely, and it is one that I had previously considered myself (in an article published in *General Linguistics* in 1981). It may be noted that just such an effect by pitch on vowels can be observed in the Saigon dialect of Vietnamese. But there is more, the vowels of non-initial syllables in Uralic are also difficult to reconstruct, and then, yet again, there is Altaic. More work needs to be done here, to be sure, and Fournet was right to point out these problem areas.

I do, in fact, consider Kartvelian to be a close relative of Proto-Indo-European, and I state (vol. 1, p. 229):

Nichols (1997:138) speculates that Pre-Kartvelian originated in Central Asia, near Pre-Indo-European, and that it spread westward along a southern route below the Caspian Sea, eventually reaching its present location, where it stayed.

And, further on (vol. 1, p. 240):

If Nichols is correct in seeing Pre-Proto-Kartvelian as having migrated from Central Asia westward below the Caspian Sea to the Caucasus, this would seem to imply that Pre-Proto-Kartvelian had first migrated northeastward from the Fertile Crescent along with or as part of Pre-Proto-Eurasiatic, that it stopped somewhere along the way, and that it then returned to the Middle East.

This means that Kartvelian could also have been considered as part of Eurasiatic rather than a separate node on the tree diagram on p. 28 of volume 1. I never intended to imply that Kartvelian and Indo-European were not close.

Fournet's remarks about Arabic root structure are absolutely correct. But, as I have stated in several publications, notions of what Proto-Afrasian might have been like based upon the Semitic model are likely to be wrong. It is clear that Semitic has innovated in many areas, including the proliferation of triconsonantal roots. Fournet's statement that "[t]he root *b_t accepts affixes in all positions" agrees totally with my view. The root is, indeed, *b_t, that is, biconsonantal, to which affixes have been added. Comparison with the other Afroasiatic languages shows that this has, to a large extent (though not totally), happened within Semitic and is not to be projected back to Proto-Afrasian.

I do not understand the statement that "[t]he book does not explain how the Proto-Dravidian vowel could have developed out of a five-unit system." It is universally agreed that Proto-Dravidian had the following five vowels, long and short: *a, *e, *i, *o, *u. Some have also speculated that it may have had *ə as well. No criticism is intended here, I just do not understand what is meant.

Fournet is correct in his assessment that the vowels are problematic across the board, and this does not just concern my work.

In general, as noted on p. of volume 1, p. 206, I do not include Japanese data. However, throughout Chapter 10, I am citing the views of Starostin—Dybo—Mudrak, and, therefore, I repeat what they have said about Japanese (and Korean).

"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 series of changes assumed for Proto-Indo-European are explained in detail in Chapter 4, especially in the Appendix to that chapter. "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 doubles a major problem in the theory here." It is sad that Kartvelian and Indo-European have undergone such radical restructuring, but I cannot change the data to fit the theory, and, consequently, the other branches must be relied upon for the reconstruction of the Proto-Nostratic vowels.

"A stem, on the other hand, may be defined as an inflectional base. A stem may or may not be coequal with a root." That is to say that the root itself may serve as an inflectional stem, or the stem may consist of a root plus one or more determinatives.

As for Nostratic morphology, even though Chapter 16 relies substantially on previous research by Greenberg, I have independently verified, and have, thus, accepted or rejected or even modified, each of Greenberg's proposed grammatical elements on the basis of works available to me. I also propose several Proto-Nostratic morphemes not suggested by Greenberg.

Let us now turn to Fournet's points:

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." This is easy to address — it has to do with the order of meaningful elements and typological consistency. I assume that Proto-Nostratic was SOV and that it was exclusively suffixing, that is that it was head-final. A "head" is the member of the particular lexical category from which a phrase is named and which determines the syntactic properties of the phrase (for example, in a verb phrase, the head is a verb; in a noun phrase, the head is a noun). In general, a phrase of a certain type has a head of the same type; for example, a noun phrase (NP) has a head noun in it, and possibly other things as well. The same applies to a verb phrase (VP). Languages can be roughly classified as either head-initial or head-final. However, it is important to note

that a particular language does not necessarily display the order relations of one of these two types exclusively, even though the majority of the world's languages do exhibit consistent ordering relations across phrasal categories.

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." I have tried to identify each of the functions of the individual morphemes I am proposing, both in Chapter 16 and Chapter 17. To be sure, there are allomorphs.
- 4 & 5. I do not understand what is meant by these points, and, therefore, I cannot offer an opinion or a comment.
7. This is very, very true. Illič-Svityč was strongly ridiculed for attempting to rewrite Schleicher's fable in Proto-Nostratic, and I wanted to avoid getting caught in that trap.

Now for notes on Chapter 17:

1. This point is well taken, but I cannot change the way active-type languages work.
2. The statement that "adjectives did not exist as a separate class of words" is based upon what is found in the most archaic (Afrasian and Dravidian, but also later-stage Altaic) daughter languages. Of course, several later-stage daughter languages (Kartvelian and Indo-European, for example) did have adjectives.
3. The comparative data assembled in volume 2 clearly supports positing five vowels for Proto-Nostratic. Unfortunately, until Proto-Afrasian is more securely reconstructed, thoughts about possible ablaut variants remain purely speculative. I had to mention what seems to be indicated by the data, but there is hardly enough known about this to go further.

Fournet's comments on Chapter 19 raise concrete problems that cannot at present be resolved. Again, that is why I wrote on p. 520 of volume 1 (repeated from above):

Comparison with other Nostratic daughter languages indicates quite clearly that a whole series of relational markers can be reconstructed for Proto-Nostratic, and at least some of these must have been inherited by Pre-Proto-Indo-European. As more work is done in reconstructing the proto-languages of the individual branches of Nostratic, future scholars will be able to arrive at a more accurate and more complete reconstruction of Proto-Nostratic. In so doing, the work done in one area will no doubt complement and further the work done in other areas so that we will be in a far better position to fill in the gaps that currently exist in our knowledge concerning the early prehistory of the individual branches themselves. Lehmann (2002:250—251), in particular, identifies the lack of adequate reconstructions for the non-Indo-European Nostratic proto-languages as a crucial problem that needs to be addressed. I could not agree more.

To close, I like Fournet's concluding remarks. They point to real problems and areas for future research. I hope that I have explained herein some of the reasoning behind the proposals made in the book. Once more, I thank him for the time and effort he put into reviewing my book. I have learned much from his analysis and see that there is still room for improvement.