

Circular two.

12/29/86

Herewith I give some sort of label to the pan-directional missives (Hodge) or club-open personal letters (Dolgopolsky) which I intend to keep sending out to friends and/or colleagues. One new networker (link?) has maintained that "Newsletters" have to be responsible and cite sources properly and so forth. Well, this is not yet a "Newsletter" and there is no organization of any sort involved and there are no formal rules and I will probably quit if some anal compulsive tries to make us get it all "organized properly" and bound up in academic formalities. At least two of us, Bender et moi, resisted any formal organization for the Ethiopianists but we lost -- and look what happened to them! But I would appreciate it if those who truly lack an interest in this enterprise would send me a post card, telling me to send them nothing more.

Due to reactive letters from Hudson, Dolgopolsky and Shevoroshkin we have a number of scholars, not all Russian, to add to the list. Some of them are making truly weighty contributions, some in adding important new genetic connections and some in demolishing bad etymologies and/or grammatical analogies or just bad data. There appears to be a group at Michigan who are much farther down the road than anyone in this present network. We might consider joining them altogether, although they are already linked network-wise because Shevoroshkin wrote to me and he is in contact with Greenberg.

The winnowing process or getting rid of chaff is always going to be a very serious problem. (But on the other hand we have some new kernels of wheat which are rich and exciting!). I am already obsessing about the problem of chaff in relation to hurt feelings. That chaff was someone's hypothesis! I think the only answer is politeness and good will but combined with frankness. We are all probably familiar with the reputedly famous tendency in central/eastern Europe for formal academic discussion in print to be accompanied by the most violent personal abuse. Who needs that? BUT, there is a difference, of course, between "abusive" and "aggressive". In several European conferences I have noticed that it was, in fact, Americans who pushed and shoved and demanded and bullied more than any other group. But they didn't use abusive language. I guess we will just have to be ourselves!

The plan of this letter is to present a long letter which Dolgopolsky wrote me recently. Not only does it give his views, which are much advanced over mine, but also reflects accurately and in detail one of the Russian views. Like American scholars, Russian scholars have a variety of often conflicting views on almost everything. Then I want to make a few comments on some points raised in his letter because they will surely concern us soon. Finally, I take the liberty of not including his evaluations of proposed new networkers, even though he thinks well of them, because some of the information is undoubtedly private.

(My address)

Frankfurt am Main, 26/11/1986

Dear Harold,

My congratulations to your brilliant invention: a club-open (1) personal letter. This invention really makes wonders. The first one is that I AM READING YOUR LETTER SEVERAL WEEKS BEFORE THE POST BROUGHT IT TO ME. Now your very kind letter is somewhere on the way to Israel or most probably in my post

(1) CLUB-OPEN, since it is open not to everybody, but only to the members of our Long Range Comparison Club (or, if you prefer some other name, Afroeurasian, or Super-Nostratic, or Macrophyllum, club.)

box at the University of Haifa. In a week or two my wife will go to the University to collect my post and to re-send it to Germany (I am now on a sabbatical semester which I spend in Europe collecting Chadic etc. lexical material for my etymological research). It means that I shall get your letter in a fortnight or so (if not later, by the end of December). But -- I have read it already! How? Prof. Jungraithmayr has been very kind of giving it to me immediately on getting it. A time machine, isn't it?

Now to the serious content of your letter. It is really exciting to see you excited by what the Moscow School (or Circle) of Long-Range Comparative Linguistics has done (I dare say WE HAVE DONE, since the late Vladislav Illič-Svitič, Vladimir Di"bo and myself created it in the 60-ies, and then our students-disciples joined it) and by what my students and colleagues are going on doing. Serge Starostin and other boys and girls are quick-minded talented linguists with good "Schulung" and what they are doing is really important and will have far-reaching results -- especially if all of us will exchange our results (you are absolutely right!). I regret very much not having been allowed to assist the Ethiopian conference in Moscow (I was invited, but the Soviet authorities did not give me an entrance visa!) and to see all of my colleagues and pupils again.

About their hypothesis on Hamito-Semitic as a coordinate branch of Nostratic and your idea of an Afroeurasian super-phylum:

1) I do not see any decisive arguments for supposing that the subclassification of Nostratic is like that:

Nostratic	
Hamito-Semitic (AA)	"Eastern Nostratic"
	(a) Indo-European
	(b) Kartvelian
	(c) Uralic & Altaic
	(d) Elamito-Dravidian

(I have transformed Aharon's diagram, preserving its logic. I hope. HF)

To prove it we should have found SHARED INNOVATIONS in IE, Kartvelian, Uralic, the Altaic languages and (Elamito-)Dravidian which are not shared by HS. I do not see such shared innovations (1). As to the personal pronouns, (a) they are not so different from other Nostratic pronouns as it seems: *kV "thee, thy" is present not only in HS, but in Kartvelian and Turkic as well (see my "Nostratic pronouns", p.89 and Table A). Ham-Sem *7- / *7y / *yV "I, we, my" (< Nostratic *HoyV) has etymological cognates in IE (-H in -oo of 1st sg., -He in Hittite -xi and WIE perfect -Ha "I"), Kartvelian (*hw- "I") and Elamito-Dravidian (see pp. 85-87), (b) EVEN IF THE PRONOUNS LOOK DIFFERENT, it is a result of AA (HS or HAM-SEM) INNOVATIONS, hence it does not say anything about the place of HS

(1) By the way, I have similar objections to your Omotic hypothesis (namely that Omotic does not belong to Cushitic). You have convinced me only 50% :

I do agree that we do not know that Omotic belongs to Cushitic, but I do not agree that we do know that Omotic does not belong to Cushitic. To my opinion, the *ta/ne- pronouns argument is not decisive: the Omotic pronouns (which in the light of Ari-Hamar-Hanna forms should be reconstructed as *ita "I" and *hana "thou") may well be of AA (or even Cushitic ?) origin: *ita "I" < *ya-tV "me" (-tV is AA accusative marker) (> Akkadian, Saho, Afar) and *hana "thou" is either from *7antV "thou" or from *ka- "thee". In order to prove that Omotic DOES belong to Cushitic, we must find enough exclusive Omotic-Cushitic lexical isoglosses. I hope it is possible. (Ari-Hamar-Hanna forms are from M.Lamberti)

within Nostratic. Subclassification of any language family (phylum, etc.) is to be based on reconstructing its history rather than on apparent similarities and differences -- otherwise we should have considered Sinhalese, Maldivian and Assamese (which have lost almost everything which looks IE) as a coordinate branch within IE !

As to the Afroeurasian macro-phylum [including Nostratic, Nigero-Kordofanian, (Dene?) Sino-Caucasian or maybe something else, like Nilo-Saharan and Austrasian], I find it quite possible. When we shall have reconstructed proto-Nostratic, and somebody else will have reconstructed proto-Niger-Kordofanian, proto-Sino-Caucasic, etc., it is worth comparing it in order to descend to a deeper chronological level. In the early 70-ies (1973?) I tried to find common roots in Nostratic and Proto-Sino-Tibetan and really found some striking Anklange (agreements - HF), e.g., *lapV and *laptV for "leaf, flat", etc. But it was not enough for supposing genetic relationship (there were too few items of core vocabulary). In 1974-76, while discussing Nostratic roots at our Moscow Linguistic Circle (Dybo, myself, Starostin and other young people) we found striking Nostratic-East Caucasian lexical parallels (*xant.V "forehead, front", *s^a9rV "hair", etc., but it could be explained not necessarily by genetic relationship, but by contacts as well. (Note: his [t.] is dotted underneath and = my [t'] & his [s^] has wedge superscript and = my [hl]. I'll have to "upgrade my printer". HF). The trouble was that the pronouns (personal, interrogative) are so different that I didn't see any possibility of connecting them (and such pronouns, as you know, are the most stable items of vocabulary/grammar!). Once in Hamburg I was asked by Prof. Kay Williamson (1) why I do not connect Niger-Kordofanian (with its *mi "I", etc.) with Nostratic. I answered her that I had never seen a reconstructed proto-Niger-Kordofanian. As soon as they will reconstruct it, the question may be considered seriously. Both Starostin and V. Di"bo (a brilliant linguist,Indo-Europeanist..., Slavicist and Nostraticist) are inclined to think that a Nostrato-Afro-Sino-Caucasic genetic connection is likely to exist.

The main practical question (I agree with you completely) is that for those things team work is needed. Therefore I accept your proposal as to exchanging materials, reconstruction, etc. and making a sort of "Newsletters of Long-Range Comparative Linguistics" (or "Afroeurasian Linguistic Newsletters"?) I have been making (for MANY years) a "Nostratic Vocabulary (Register of Common Roots of IE, Hamito-Semitic, Kartvelian, the Uralo-Altaic languages and Dravidian) (2) and now there are more than a 1000 roots (proto-stems or words). In some years (Inshallah!) I shall publish it. About 500-600 of these roots have been studied and partially reconstructed by Illic-Svitic in the late 60-ies (see in1965, ..1967 and the 3 volumes of his ...1971-1984) (3) and partially revised (with addition of new material, esp. Cushitic, Chadic, Samoyed & Dravidian) by me. The rest has been done by me. I have not "computerized" it, since I prefer handwriting and an IBM typewriter with all possible (and nearly impossible) signs & scripts on gulf-balls, but of course I may send some of my etymologies to our Newsletters (in camera-ready form) to be discussed by colleagues and possibly to be compared with other languages. And vice versa: if our colleagues send their reconstructions (including Niger-Kor-

(1) By the way, why is she not on your mailing list?

(2) Proto-Chukchee-Kamchadal has not yet been reconstructed hence serious comparison is very difficult. But with a query it may be considered as well.

(3) I write it in Cyrillic characters on purpose: why shouldn't you and other colleagues learn them? Some of my German and Italic colleagues know them and I hope so do you.

(Note: I can read Cyrillic characters when printed but not as writ by Aharon!)

dofanian, Sino-Caucasian, etc., or of smaller families & sub-families within Nostratic), I shall be happy to say my opinion and to compare with what is at my disposal. The best way of cooperation will be if everyone (members of the Club or Circle) who has reconstructed some proto-language (e.g., proto-OmotiC in your case) will send an abstract of his reconstruction (list of words, sound correspondences, list of grammatical morphemes) to the Newsletter. I can do with Nostratic (as soon as my Nostratic vocabulary will be nearly completed). On reading all this, other Club members send their comments. Do you agree?

Let us not forget one sad detail: the Soviet colleagues are not allowed to send their papers abroad for publication (and for the Soviet authorities & the Soviet post anything typed and having a form of an article IS an attempt to publish) without special permission & previous censorship. Usually they allow to publish abroad only what has been published already in the USSR. More than that: Western publishers cannot publish papers (by Soviet authors) smuggled to the West without infringing upon laws, since (c) of the Soviet citizen belongs to the Soviet state (i.e., authorities). Therefore the best way of getting scientific information from the USSR and publishing it in our Newsletter is as follows: our Soviet colleagues write PERSONAL LETTERS (handwriting!) to you or to some other Western colleague (not to me: Israel is a nomen odiosum there, & contacts with Zionism & with the Zionist state are severely punished), and we publish it in the following form:

"Professor.... has been recently informed about the new reconstruction (or results, theories, etc.) made in the USSR (with the full names of the authors, with their permission, otherwise with initials only): X Y Z (roots, sound correspondences, or whatever." (X Y Z = content of Soviet contribution. HF) I had this sort of cooperation with the late Morris Swadesh. Once (ca1966) I sent him a letter on my Nostratic research. It was written in Spanish (then Swadesh lived in Mexico and our correspondence was in Spanish), and M. Swadesh published my letter (the whole of it, including Muy sen~or...) in a Mexican journal, with his comments.

I am sending you a table of my Unified Transcription which is a compromise between different systems of transcriptions used in comparative linguistics (Fenno-Ugric Transcription, IPhA, Africanist transcription, Brugmann's system, etc., etc.) which I use in my Nostratic papers. (The transcription MUST be unified, otherwise it will not be understood: if you use "c" for [ts] in Slavic, Caucasian languages, etc. and for [tʃ] in African and Indian languages, or "y" for a back (or middle) high unrounded vowel in the Altaic & Slavic languages, and the same "y" for a palatal glide in other languages, it will be difficult to understand what you mean.).

It works well with the most complicated phonological systems (as the North Caucasian, the Chadic languages, etc.). After getting your letter from H. Jungraitmayr (this morning) I thought how can we put lexical material into a computer without losing phonetic features. I have some ideas about it, and I shall write about it later (here in Germany they have two weeks of Weihnachtsferien, so that I shall have free time to think about it). Maybe the first thing to be published (by us) in the Newsletters will be a proposal for a Unified Transcription and its computerized version (just as the Finno-Ugrist E.N. Seta"la" offered the first volume of "Finnisch-Ugrische Forschungen" in 1901 by a proposal on a Finno-Ugric Transcription which is used up (til) today by all Uralists).

I propose to include into your mailing list for Long-Range Comparison some very important linguists who can do much in this field (as either participants or critiques):

1) Dr. Vladimir A. Di"bo (or Dybo). Institute of Slavic Studies, Moscow, Leningradskiy prospekt 7. Or maybe better to his home address near Moscow. The address must be written in Russian (Cyrillic - HF), since outside Moscow postmen do not read the Latin alphabet. The best thing to do will be to send two copies of each letter: one to the home address, and another to the Institute. Then at least one of the copies will not be confiscated by the KGB. Please take such things serious (I know it too well from my personal experiences). Since his office (which he visits not more than once a week) is in another (old) building in Trubnikovskiy Prospekt, which is far from Leningradskiy Prospekt, so that I have my doubts as to the delivery of your letters. (Note: I have his home address, written in Cyrillic, but this computer cannot produce the Cyrillic letters. I will try to figure some way of putting those characters on a gummed label mailing list. HF)

2) Dr. Evgeni Khelimski. Institute of Slavic Studies, Leningradskiy Prospekt 7 Moscow. He is my former student (like Starostin, Porkhomovsky, Stolbova, etc.) and is ... Uralist (esp. in Samoyed and Ugrian languages) ...comparativist. He knows very well Nostratic comparative phonology, etc.

3) Dr. Ilya Peiros. He is the author of a Comparative Sino-Tibetan Vocabulary (part of his unpublished PhD thesis) which is much better (in many respects) than... I do not know his addresses (Institute of Chinese Studies, Moscow ?), but Starostin will give you his address.

4) Serge Nikolaev. A brilliant comparatist in many fields (incl. Nostratic). I do not know where he lives & works (maybe outside of Moscow). Starostin will give his address, or maybe the correspondence will be through Starostin (his close friend and co-author.)

5) Vladimir Terent'ev. Uralist, esp. Samoyedist & Nostraticist. Moscow 109390, Lyublinskaya ulica, 35, KORPUS 2 (? -HF), kv. 6

6) Prof. Dr. Karl-H. Menges. Do"blinger Hauptstrasse 64, A-1190, Wien. He is an Indo-Europeanist, Altaist (Turkologist, Tungusologist), knows much about Dravidian, etc. He has studied morphological question of proto-Nostratic reconstruction. He may not wish to work actively with us but he may be a very good critique.

7) Prof. Kay Williamson, University of Port Harcourt, School of Humanities, PMB 5323, Port Harcourt, Nigeria. She is... authority in proto-Niger-Cordofanian.

8) Dr. Werner Vycichl. 2, rue de Penater, 1203 Geneve, Switzerland. Hamito-Semitic, esp. Berber, Egyptian, Coptic, Semitic, Beja. He has written a Dictionnaire etymologique de la langue copte. He will be good as critique.

9) Prof. Dr. Vladimir Skalic"ka, Praha 2, Dr"evna 6, Czechoslovakia. IE, Uralic, IE-Uralic comparison, typology.

10) Prof. Dr. Bojan C"op, Martina Krpana 4f, YU-61000, Ljubljana, Yugoslavia. He has written some 20 papers on IE-Uralic comparison. They are interesting, although I do not agree with his sound laws & with his methodology. We have not corresponded with each other.

11) Prof. C. Street, Dep't. of Linguistics, University of Wisconsin, Madison, Wisconsin 53706. Japanese as an Altaic language, Mongolian, Altaic studies.

12) Prof. Nicholar Poppe, Far Eastern Dept., University of Washington, Seattle 5, Washington. He has already retired and I do not know his home address, but I hope the letters will be brought to him from the university. He is ... authority in Altaic studies (author of a Comparative Altaic Grammar, etc.) and in Mongolic comparative studies. He will be very good as critique.....

13) Prof. Th. Gamkrelidze, (or better still - HF) Akademik T. V. Gamkrelidze, Institute of Oriental Studies, Georgian Academy of Sciences, Tbilisi 380062, GSSR (Georgia), USSR. Kartvelian and Indo-European, author of the "glottal theory" in Indo-European (along with V. Ivanov & P. Hoffe [?-HF]). We do not

always agree about that theory.... (As a tidbit of incidental information, Aharon mentions that Akademik is higher than Professor as a title in USSR. HF)

14) Prof. Vjaceslav Vs. Ivanov. Institute of Slavic Studies, Moscow, Leningradskiy Prospekt 7. An ...Indo-Europeanist, Hittologist, general linguist ... advocate of "glottal theory" of IE.

15) What do you think about your compatriot and neighbor Allan Bomhard, 186 Waltham St., Boston, Tel. 5429454 ? He has written a book on IE-AA "Towards Proto-Nostratic" which I have criticized in the Bulletin de la Societe de Linguistique in Paris (about to appear or already appeared). He did his research without having read Illic-Svitic and Dolgopolsky, and his methodology laisse a desirer. But I want to hope that he will improve his methods. I hope you know him better than I do. Let us hope that he will be helpful.

(Note: When I sent out the first letter, I had only recently heard of Bomhard's work. Sadly my memory is so poor that I forgot to add him to my list, for which I hope he will forgive me. And, alas, scholars can live in the Boston area for years and not know each other. HF)

16) Prof. Vitali V. Shevoroshkin, 133 Michigan Avenue, Ann Arbor, MI 48104. IE, esp. Anatolian, interested Nostratic.

17) Dr. Mark Kaiser, somewhere in the USA. Shevoroshkin can give you his new address. Talented Indo-Europeanist, interested in Nostratic. An American with perfect command of Russian. They write together (Shevoroshkin & Kaiser) on Nostratic >>> I-E.

18) Prof. Georgiy Klimov, Institute of Linguistics: Moscow K-9, ul. Semashko 1/2. He is ... Kartvelist... Author of a Kartvelian Etymological Dictionary. He will not participate in any Nostratic (or the like) research, but his remarks on Kartvelian words & reconstruction will be helpful.

19) Prof. Lee Ki-Moon (or: Lee Gi-Moon), Professor of Korean, Seoul National University. He is the only person in the world to know well Middle Korean (he is writing a Dict. of Middle Korean). Without him Korean cannot be compared to anything in the world. His critique & remarks concerning Korean and its (Altaic) prehistory are crucially important. ...

20) Dr. Marcello Lamberti, Bru"sseler strasse 47a, D-5000, Koln 1, BDR. As you know, his is a Cushitologist & Omotologist. Recently he has written a paper trying to connect the Kuliak languages (Ik, Teuso, etc.) to Cushitic.

21) Dr. Songmoo Kho, Department of Asian and African Studies, University of Helsinki, Fabianinkatu 24 A, 00170 Helsinki, Finland. Koreanist, history of Korea. Some 20 years ago he tried to compare Korean to Uralic. Today he is an Alti-Altaist.... (sic - HF). He will be helpful in critique.

(Note: Probably "anti-Altaist" >> "Alti-Altaist because they have developed consonant harmony to go with their vowel harmony! HF)

22) Dr. Juho Janhunen, Helsingin Yliopisto, Suomalais-ugrilainen laitos, Fabianinkatu 33, 00170 Helsinki 17, Finland. A brilliant Uralist and Samoyedist (author of Samojedischer Wortschatz). He is a benevolent scepticist as to the Altaic & Nostratic hypothesis. He will be ... helpful in critique....

Now I want to ask you something. You write that you are digesting the Moscow "meal". May I taste that "meal" together with you? What I mean is : if you have something from the Russian materials (by Starostin, Dyakonov, Stolbova, Militarev, Porkhomovsky, etc.), may I take the liberty of asking you to copy it and to send me a copy? I do not mean books, but papers for conferences, booklets, abstracts or the like. Unfortunately, I cannot maintain contacts with my Soviet colleagues (even with my former students) without putting them into great trouble ("connections with Zionism" is a mortal sin in the USSR!), and therefore I have almost nothing of new papers and research materials: I have only the 5 booklets of abstracts of the 1984 conference

"Linguistic Reconstruction and ancient history of the East" (...) and the 3rd fasc. of the COMPARATIVE AFRASIAN DICTIONARY by Dyakonov et al, as well as Majzel's & Khelimsky's book. If you have anything else, will you be able to make a copy for me?

Now I shall try to answer the question you put to M. Swadesh and J. Greenberg (how can one compare languages with totally different phonologic systems, e.g., glottalizing, clicking, etc., with non-glottalizing, tonal, etc., etc. ?) In my opinion typological differences (including phonological differences even if they are enormous) are not an obstacle at all. It is enough to find regular sound correspondences and then to reconstruct historical phonology, and you will understand how glottalized consonants (or cliques, or tones, or "hlaterals", or whatever you want) appeared or disappeared. In the history of the Semitic languages: ejectives were transformed to Arabic uvularized consonants ("emphatic" X, Y, Z, Z.), as you know, affricates disappeared (in the very stage of history of Semitic) and then appeared again (Arabic C. z^ < g). In the history of the Ham-Semitic languages we see how lateral obstruents ("hlaterals") disappear (proto-Semitic *s^ = *hl changes to \$ in Arabic and s in Aramaic) or appear (proto-Chadic & proto-Hamito-Semitic *s > Semitic \$, Egyptian s', West Chadic s, East Chadic s, then become s^ or hl in Central Chadic: *sim "name" (> Semitic *\$im, West Chadic *s3m (1) > Karekar s3m, Angas sIm, Daffo sum, etc., East Chadic *sVm > Mubi same, Mokilko suma, etc.) changes to Central Chadic *s^3m (or hl3m) > Logone s^3m, Gisiga s^im-ed., Bura, Kilba s^im, Tera z^im, etc.) The same is true of tones, injectives, etc., etc. Everything may appear and disappear in the history of languages. The key question is to reconstruct their history and to see what happened (and how it happened). In my opinion, TYPOLOGY (including phonetic typologies) PLAYS NO DECISIVE ROLE (either positive or negative) in ESTABLISHING LINGUISTIC RELATIONSHIP and in the genetic classification of languages (including sub-classification of languages within a family).

To your question about the Berber emphatics, i.e., uvularized consonants. I do not think that Punic (2) or Arabic influence could have been the reason of a change glottalization >> uvularization. The change in Arabic and Berber is parallel. Such parallelism is typologically possible (do you remember that paper on experimental study of the Tigray emphatics which both of us heard in Addis?): in some Neo-Aramaic dialects there is a similar (though not completed) direction of change, and in some Cushitic languages glottalized k. (or [k'] - HF) changed into an uvular q .

Do you see how much have we to tell each other? If one takes your "club-open" letter and this letter of mine, they will be enough for an issue of Newsletters. Isn't it so?

Maybe our Newsletter will have an epistolar form ("Afroeurasian Letters: Letter Exchange among Comparatist"). If so, we have made the first volume. In any case, this genre (as French say) will be not new: let us remember the famous letters written by Champollion (his decipherment of Egyptian script appeared in epistolar form) or by d'Abbadie.

(1) Written [3] = the central middle vowel or schwa, which the Africanists transcribe as @ . (Note: my computer writes it as [A], right now anyway. HF)

(2) We have no reason to believe that Punic k, t, and s were uvularized. To judge from the Greek and Latin transcriptions, Phoenician and Punic "emphatics" were not-aspirated (& therefore rendered by 2, 4 unlike aspirated k, t, p which were rendered by X, 5, 6). (Note: He lost me here. HF. Methinks 2 = Kappa, 4 = Tau, X = X, 5 maybe = Theta, and 6 maybe = Phi). I guess they were glottalized in Ancient Hebrew (see my paper "Emphatic Consonants in Semitic")

May I ask for another favor? Can you send me (to Germany, Professur fu"r afrikanische Sprachwissenschaften, Johann-Wolfgang Goethe-Universitaet, Feldbergstrasse 22, D-6000 Frankfurt am Main 1, GERMANY) the addresses of your other addressees (at least , the linguists among them)? The matter is that after your "club-open" letter I feel it my duty to send to some of them my paper on the Nostratic personal pronouns mentioned by you. The text (as it is published in LINGUISTICA ET PHILOLOGICA: GEDENKSCHRIFT FU"R B. COLLINDER) is full of terrible misprints (they did not send me any proofs!) and practically unintelligible. I have sent you a checked copy, and so shall I do to some of the linguists if I have their addresses. If they are in your computer, it will not be difficult, I hope. (Note: What goes in, does it come out again? HF)

Merry Christmas to you. [\$alaama}, which, as you know, means in proto-Semitic "health and peace, & well-being", which I wish to you. (Note: and then he writes it in four other alphabetic systems! HF)

Aron Dolgopolsky

PS On the history of the Nostratic theory. The first scholar to discover the Nostratic relationship (in modern scientific terms) was Holger Pedersen at the beginning of this century (1). His list of the Nostratic daughter-languages was almost identical to what we know today (but he included Basque, did not include Dravidian, and he did not distinguish between Kartvelian and North Caucasian). He saw the relationship of the personal pronouns and of some roots. (See his papers "Tu"rkische Lautgesetze", ZDMG 57, 1903; "Die indogermanisch-semitisch Hypothese und die indogermanische Lautlehre", Indogermanische Forschungen, 22, 1907-08; "Urverwandschaft des Indoeuropaischen und Ugrofinnischen", Memoires de la Societe Finno-Ougrienne [= Suomalais-ugrilaisen seuran toituksia (?-HF)] 67, 1933 and the last paper of his book SPROGVIDENSKABEN I DET NITTENDE ARHUNDREDE, Kobenhavn, 1924 [translated to English as Discovery of Language: HISTORY OF LINGUISTIC SCIENCE OF THE 19TH CENTURY]. But in his days serious research (with exact sound correspondences and a reconstruction of the Nostratic historical phonology) was still impossible, since Finno-Ugric (or Uralic), Kartvelian, Dravidian, Tungusian, Hamito-Semitic comparative linguistics made their very first steps, so that no reconstructed proto-languages (except Indo-European and Semitic) were known. For half a century many good scholars worked on binary comparisons: IE-Uralic (Collinder, etc.), Uralic-Altaic (Ra"sa"hen, Collinder, Sauvaget), IE-Semitic (terrible studies made by H. Mo"ller and A. Cuny, which were more harmful than useful), Uralic- Dravidian (Th. Burrow), etc.

In the 60-ies (ca1964) V. Illic-Svitic and myself came to practically identical conclusions about the genetic relationship of those major language families, although for a year or so we did not know of each other's research (we worked at different institutions in Moscow, I was then a Latinist and a Romanist, and Illic-Svitic a Slavist, and we were not acquainted). But we had a common friend named Vladimir Dybo (Di"bo, ...). He heard the whole story from me and smiled without discovering the secret. Then he heard the same story from Illic-Svitic, smiled and kept silence (and compared our conclusions). Then once the three of us met at Lenin's Library in Moscow. That period of independent research is reflected in my article published in Voprosi" jazi"hoznanija (?-HF) 1967, No.2 and partially in my paper "Long-Range Relationship of the Languages of Northern Eurasia" read at the 7th Intl. Congress of Anthropological & Ethnographic Sciences, 1964. As soon as I return to Haifa, I shall send you a

(1) By the way, it was H. Pedersen who invented the name "Nostratic".

copy of it. The real break-through was when Illic-Svitic discovered the sound correspondences between the daughter-languages, e.g., the Indo-European Consonant Shift:

Nostratic voiced >>> IE voiced aspirate

Nostratic voiceless >>> IE voiced

Nostratic ejective >>> IE voiceless

He discovered that the IE labialized velars (*gw, *ghw, *kw) go back to velars before labial vowels, and IE palatalized velars (*g^h, *gh^h, *k^h) go back to velars before front vowels, and the same is true of the proto-Hamito-Semitic (>> Cushitic) and proto-Kartvelian labialized velars. (1)

Here I shall stop (since otherwise my letter will get too voluminous). Now I understand that all this historical phonetics must be published in English. I shall do it in my Nostratic vocabulary and maybe even earlier.

Once again,
Merry Christmas

Aron

(1) What was impossible in the Pedersen days -- a comparative etymological phonology of the Nostratic languages --, has become possible now, since there are fairly good reconstructions of proto-Uralic, proto-Tungusic, Ancient Mongolian, proto-Kartvelian, proto-Dravidian, etc. to compare with proto-IE and the Hamito-Semitic languages. What we still miss, is a good proto-AA (proto-Hamito-Semitic) reconstruction based not only on Semitic and Egyptian, but on proto-Chadic, Cushitic (and Omotic) and proto-Berber.

- - - - - (that's all, folks) - - - - -

If Aron has discovered that English is a Must, I have given in to Russian. Even though this old dog does not like at all to learn new tricks, still I'll have to learn Russian or miss too much good stuff. But I was also going to start Arabic this year ! Unlike Aaron, if I may adopt the more familiar American spelling, I do not enjoy learning new languages. Even less all those other alphabets and syllabaries.

Well, on to business. Although Aaron has stimulated me a great deal in this letter, I'll have to refrain from saying very much because there is a great deal more to report on, not the least of which is Shevoroshkin's and Ruhlen's work and Bengtson's report on the Rice conference on Genetic Classification. None of these things did I know anything about before I wrote Dolgopolsky the first time. In addition there have been significant responses from Murtonen, Trigger, Bender, Hodge, Hudson, Newman but not a word in THREE months from EIGHT Soviet scholars. I suspect foul play in Moscow and wonder about Aaron's advice about letter writing.

Actually it will be better if I end this circular after the following personal comment because there is too much to cram into this already long epistle. (Isn't that what disciples write?). Please stay tuned for Circular.3

Personal comment on AA (Afroasiatic or Afrasian) in relation to the alleged phylum Nostratic and all of these in relation to Niger-Kordofanian or Nilo-Saharan. I do not have much doubt that AA will turn out to be related to IE and probably Kartvelian too. Maybe Ural-Altaic too but only by way of IE and Kartvelian, i.e., if AA cannot be related to IE, it will not be related to

Uralic or Altaic. (I was taught in the 50s that Ural-Altaic was believable.) But for reasons very different from either Aaron or his students in Moscow I think the latter are more likely to be correct about AA being half of Nostratic. Afrasian is a great big phylum! Anyone who thinks he can reconstruct proto-AA by concentrating on Semitic and Egyptian, as many comparativists seem to do, is like a man trying to describe an elephant's ears by measuring its tail. When you only compare Semitic, however, you reduce AA to the size of one of those little Eurasian phyla like IE, Kartvelian, Uralic or Altaic.

Forgive me if I sound offensive; I do not mean to be. But it is important for us to see that internal diversity within a phylum and the age of the common ancestor are surely related to each other. Most Indo-Europeanists seem to favor the period of 3000-4000 BC for P-IE; Watkins would make it older. Anttila cites a probable date of ca4000 BC for proto-Uralic (p.301 of his excellent text), while Klimov put proto-Kartvelian at ca1900 BC, i.e., the split between Svan and the Tzanic-Georgian branch dates to 1900 BC more or less. Rouse, quoting Miller or starting from Miller's estimates, gives rough dates of 4228 BC for proto-Tungusic, 2665 BC for both proto-Mongol and proto-Korean-Japanese. Proto-Altaic is not dated, except as "many thousands of years ago" but the scale of Rouse's diagram suggests that perhaps 8000 BC for the great split between Eastern Altaic (all the rest) and Western Altaic (Turkic) and 6000 BC for the splitting up of Mongolic, Tungusic and Japanese-Korean. (All this from Rouse's MIGRATIONS IN PREHISTORY, 1986, p.77-79. Sorry I gave it the wrong title in my first letter). Miller's own opinion (p.29-30) exudes great contempt for glottochronology and his remarks suggest that his 1967 Altaic dates were set up in jest.

Proto-AA is obviously much older than any of those.

Glottochronologic estimates literally go off the charts since the extremes in AA hover around 0% to 2% common retention. Another kind of evidence is that of estimates made by a group of Chadicists and Semiticists at Hamburg (1982), none of whom used glottochronology, but ALL of whom agreed that 15,000 BC was quite a reasonable figure for proto-AA. Also ancient Egyptian was first written circa 3100 BC; her Semitic cousins in the Levant start writing circa 2500 BC. At that date Akkadian and Egyptian were quite distinct from each other morphologically, not to mention their dissimilar cultures, and lexical retention was only around 10% in Swadesh (short) list terms. If we compare Middle Egyptian with Akkadian or Ugaritic, roughly contemporaries in the late 3rd millennium BC, we find in a Swadesh short list comparison that :

Middle Egyptian vs Akkadian = 10.5%

Middle Egyptian vs Ugaritic = 10.1%

whereas Akkadian vs Ugaritic = 66.7%

Experts in Semitic and/or Egyptian will probably come up with different figures but I would bet serious money that the top two figures will not rise above 20%, nor will the disproportion between those numbers and the third change very much. Just to show what kind of difference the passage of time can make, I offer a comparison involving Coptic whose data are of uncertain post-Christian date, but probably around 3000 years more recent than Middle Egyptian.

Neo-Aramaic of Kurdistan is the closest I can get to a living descendent of Akkadian or Ugaritic, albeit one probably 1000 years more recent than Coptic:

Coptic vs Neo-Aramaic = 05.5%

In order to avoid as much as possible the distortion obtained by using literary or liturgical languages I chose Coptic data from Bohairic, supposedly a regional dialect. I have no idea how close my Bohairic comes to spoken Coptic of, say, 1000 AD. (Baer, Brovarski and Hodge know these things far more than I will ever know them!). But it is interesting to compare this insecure Coptic data with modern AA languages other than northern Semitic. For example,

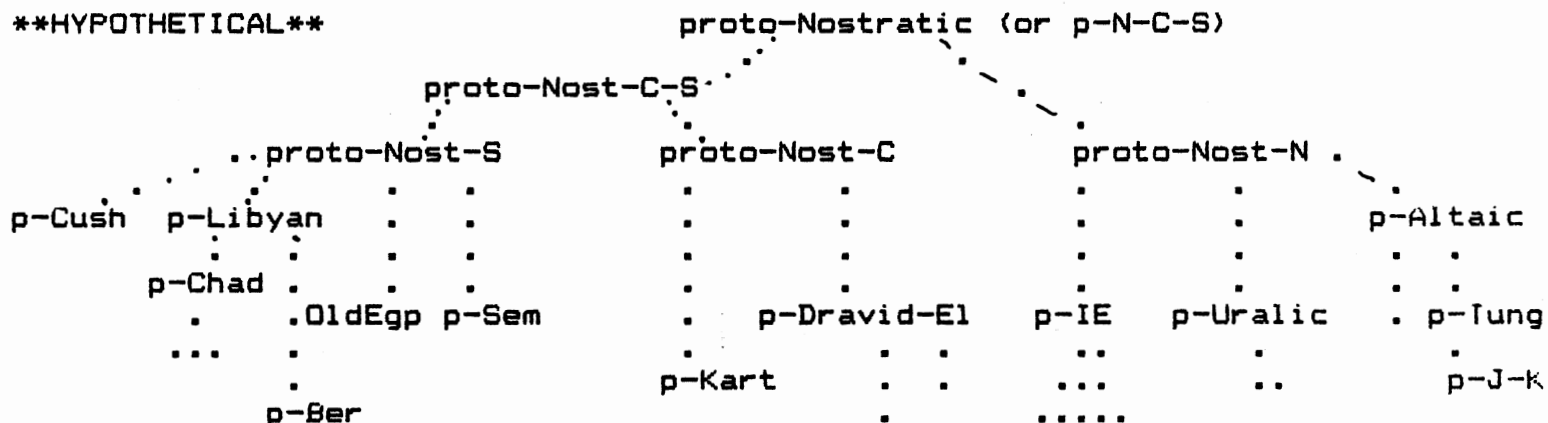
southern Semitic and Berber:

whereas Coptic vs Modern South Arabian (Soqotri) = 02% (!)
but Coptic vs Ethiopic (Gumer) = 04.5%
and Neo-Aramaic vs Arabic (Iraqi) = 27% (borrowings)
Neo-Aramaic vs Soqotri = 18.5%
Coptic vs Berber (Tuareg) = 08.5%
Coptic vs Berber (Ait Izdeg) = 07.5%

Time does seem to be related to closeness, at least in basic vocabulary. But, despite all the critiques of Morris Swadesh and glottochronology, did anyone ever really doubt this simple truth?

The point I have been laboring hard over is that proto-AA is not very likely to be a contemporary of proto-IE, certainly not proto-Kartvelian, or proto-Uralic, or proto-Altaic. It is far more likely to be a contemporary of, and therefore possibly CLOSELY RELATED TO, the COMMON ANCESTOR of two or three of the small Eurasian phyla. We could call that ancestor, proto-Nostratic-N, for example, and specify its membership as : IE, Uralic, and Altaic. Then we could arbitrarily assign Kartvelian and Elamitodraavidian to another ancestor, proto-Nostratic-C, while saying that proto-AA = proto-Nostratic-S in fact. Thus, in this scheme proto-Nostratic produced three daughter dialects, -N, -C, and -S, which themselves are ancestors of different descent lines but in which the much greater antiquity of proto-AA is recognized more clearly. It might also turn out that, for example, -C was more closely related to -S than to -N (or, of course, vice versa). In that case we would have a binary split in the common ancestor, followed by an early additional binary split, thusly:

****HYPOTHETICAL****



(PLEASE NOTE that THIS is NOT a PROPOSED FAMILY TREE; It is IMAGINARY.)

Otmotic has been left off, so as to substantially DECREASE the diversity of AA. The differing heights are deliberate, e.g., p-Cush, p-Libyan, and p-Altaic seem more like contemporaries, while p-Sem, p-IE, and p-Tung share a later era. While the relationships at the highest levels are imaginary, the heights of the various better known proto-languages (e.g., p-Cush, p-Altaic, etc.) are supposed to be comparatively accurate. The exception is proto-Nost-S which is also proto-AA and is estimated at 15,000 BC (by guess and by golly, not by glottochronology. By the latter, p-AA is 26,000 BC plus or minus 8000 years by Kruskal, Dyen & Black's formulae or essentially incalculable but well over 8000 years by standard charts, e.g., Gleason's workbook. It is basically useless at near zero percentages, as is said of radio-carbon dating). P-Libyan is a compromise between Paul Newman's "Libyco-Chadic" and my "Chadoberber". Many Semiticists still do not accept Chadic as a member of AA. Baer reports Egyptologists who doubt AA as a whole and also Egyptian's relationship to

Semitic (even!).

Basically, I've tried to show graphically how much a difference phyletic internal diversity makes or why it is important not to mix up the phenomena of different levels. For example, the question arises: why has p-AA not been reconstructed yet? well, the diversity is very great, about 300 languages, big differences between branches and within branches, much of the data still coming in from Chadic, Cushitic, and Omotic -- some of it very important, like the fairly recent data on Yaaku, Dahalo, Dime, and (several) Mao; and very few scholars, outside of Semitic. (For example, there are more Russians working on Altaic in Moscow alone than there are students of Omotic in the entire scholarly world.) But most of all the diversity slows us down. And that diversity, I maintain, is not the equivalent of one Eurasian phylum but several of them.

It is not logical to discuss shared innovations as criteria of sub-classification within AA, or the case of AA pronouns being innovative within Nostratic, when we do not really know what the CONTENT of p-AA itself is. How can you tell what is innovative when you don't know what was inherited or traditional? Answer: you find out what was inherited first.

Niger-Kordofanian or Congo-Kordofanian as well as Nilo-Saharan may some day be related to either each other or some other phyla. But I mentioned them only in passing in my first letter because I was much more impressed by, and surprised by, the KHOISAN olfactory apparatus. It is that phylum that I reckon will be related to Afrasian before the others but only after serious reconstruction. My experience years ago in trying to find who Omotic was related to taught me not to expect Niger-Kordofanian to be related to Afrasian. After full reconstruction, well maybe. The pronoun *mi which Aaron mentioned for proto-Niger-Congo (p-N-C) is not so convincing because all the other singular pronouns begin with *m- too; the pattern is unlike AA, but it is confirmed but complicated by proto-Kordofanian's probable pattern with *N (velar nasal). I do not know what colleagues Kay Williamson, Pat Bennett or Thilo Schadeberg would reconstruct for proto-Niger-Kordofanian (p-N-K) but it would surely not relate to things like p-IE *me "I" but rather to the AA cases having a singular pronoun base prefix in /an-/ (/an-i/ "I" and /an-ti/ "thou").

But speak of p-N-C or p-N-K and you have diversity that clearly exceeds that of Afroasiatic and any other phylum in the world, save Greenberg's "Amerind" which may not stand up to criticism. Niger-Congo itself is huge, with hundreds of languages, great diversity within and between branches, and so forth. Yet great progress has been made towards reconstruction and Aaron may find himself confronted soon with a serious p-N-K to compare Nostratic with.

Nilo-Saharan has fewer languages but possibly even greater diversity. Heroic work is being done by some of our colleagues, e.g., Bender, Ehret, Heine, Rottland, et al, but the work is always haunted by the strange feeling that "these may not even be related to each other!". There are enticing tidbits linking AA to Nilo-Saharan but the two phyla have interacted intensely for millennia and the borrowing problem is enormous. It has bedeviled the literature. (1) Reconstruction is de rigueur in this case and we'll all have to wait a while! My hunch is that Nilo-Saharan may be connected to AA or Khoisan.

(1) By the way -- for Militarev's and Diakonov's information. Neither Trigger, nor myself, nor Bender, nor Ehret, nor other North Americans, nor our colleagues in Bundesrepublik Deutschland (I believe), none of us who are interested in Nilo-Saharan but especially East Sudanic and/or Meroitic, none of us think that Nubian is Afrasian. It does have numerous borrowings from AA in it. Trigger, who for me at least is authoritative, still thinks that Meroitic is probably at least Nilo-Saharan, if not East Sudanic.

(1) HF opinion to be noted. Austro-Tai is explicitly controversial. I did not know that it had survived Isidore Dyen's scathing review and rejection. I assume we are discussing the conjoining of Austronesian and Thai-Kadai. If that includes Austroasiatic, then it is Pater Schmidt's old hypothesis reincarnated; that one had a stormy history. Some of Paul Benedict's work has been first rate and important. Was he not the key man in getting Thai-Kadai excised from Sinitic? It would be interesting to see Yaxontov's arguments -- and evidence. My attitude derives from an old Greenberg article ("Historical Linguistics and Unwritten Languages", was that not it?). In that he (a) praised Benedict's work and agreed with it and (b) said that Miao-Yao was part of the Sinitic section of Sino-Tibetan, and coordinate to Chinese within it, i.e., one of two main descent lines. Benedict is probably wrong on Austro-Tai and Japanese but N. Poppe in Miller (p.xi) thinks that "it is quite possible that Japanese does have a Malayo-Polynesian stratum". Rouse does not find any clear archeological evidence of an old Austronesian or Thai-Kadai presence, although he thinks it reasonable to expect that there had been one at early times in the Nansei Islands (Ryukyus to me) and Kyushu or "western" Japan. Miller (1980) ("Origins of the Japanese Language: Lectures in Japan during the Academic Year 1977-78. Seattle. U/Washington Press) finds Austronesian loan words in Japanese.

More notes on specific things. Vitaly's AA (=AfAs) form for "brother" contains d_ which has the line under the d. I reckon that to be a voiced interdental fricative, as in English th in "wiTH". He has a Kart(velian) z1 which looks like a z with a stress mark on it, or accent aigu. Pronounced as [dz]? I don't know. (Between my lousy computer and their strange symbols, I was going to say characters, we may drive the rest of the network bananas!)

A note on the history of great performances in long-range comparisons, as Aaron would put it: While it may seem that Illic-Svitic has been well praised in both letters, I still think many many of us do not know his work in a language we are conversant with. When we all do, I suspect that the chorus of praise will grow softer. It is hard to be brilliant and always right and that venturesome! Hodge reports many problems with Illic-Svitic's etymologies. Nevertheless, we all should salute the great work because most of it is probably substantially correct and because of the impetus he gave to the work of others. Yet I should also point out, not because of my love for my two gurus but because they deserve it, that Joseph Greenberg and Morris Swadesh have helped rather a lot too. Greenberg got Africa down to 4 phyla and good solid ones too; he made a wonderfully helpful survey of southern Asia; he created Indo-Pacific which might be as vast as Niger-Kordofanian; and more recently has been steadily bringing order out of the American chaos. Swadesh just did the world! His "La red linguistica del mondo" could be laid over Shevoroshkin's system quite neatly. The most striking thing to me is the agreement between Swadesh's "Vasco-Dene" and the new Russian "Dene-Caucasian", except that Vasco-Dene also included Nostratic. Swadesh's network of world languages was, however, not accepted by the linguistic world. (If only I could find my copy of "La red..." 1964(?). Morris also invented glottochronology for which scientific contribution (called a hypothesis) he was reviled and scorned by a whole generation of scientific linguists. It wasn't just that people thought that Swadesh was wrong; they went out of their ways to condemn the hypothesis and so basically killed it in American linguistics. But why? What was so horrible about Swadesh's theory that unleashed such intolerant fury? Does anybody know?

Vitaly was gently chiding me for doing once again what has already been achieved long ago. However, it is crucial that my Soviet colleagues and international colleagues who read Russian understand that in a most important

scientific sense NOSTRATIC HAS NOT BEEN ACHIEVED, nor have the other large new groupings of old phyla (e.g., Dene-Caucasic, Macro-Asiatic, etc.) been achieved yet -- NOT YET! I repeat -- THEY HAVE NOT BEEN ACHIEVED YET! Now, let us not think that there will be a shouting match. "Yes, we have achieved! No, you have not achieved!". Because of the exigencies of historical science we must think clearly of TWO meanings of "to achieve". The first is what might be called -- the Creation of New Hypotheses. Here wonderfully gifted, and hard-working, chaps like Pedersen, Greenberg, Swadesh, Dolgopolsky and Illyč-Svitič have insights, see connections, mass evidence, and generate new hypotheses which expand our knowledge of the past. Or speed us on our way towards what Vitaly so aptly calls MOTHER TONGUE. (I think of that antique and tiny woman of Ethiopia, called Lucy, as a kind of proto-mother. But, alas, I doubt that Lucy said more than an average Chimpanzee. The real mother of our MOTHER TONGUE is more likely to have been the fat lady of Wilmendorf. Or maybe Frau Neanderthal.) First step HAS BEEN PARTLY ACHIEVED, if the goal is MOTHER TONGUE.

The second achievement is Testing, Accepting or Rejecting, of Hypotheses. That is what the rest of us are for. We can't just be told that there are new hypotheses out there and they are wondrous and we must pursue them farther to find even newer things. We have to UNDERSTAND the HYPOTHESES and most fundamentally we have to SEE the EVIDENCE, we have to touch it, taste it, pull on it, sniff around it and smell it, look at it from different angles, be sceptical and feel free to reject it if we don't like it. Second step HAS NOT BEEN ACHIEVED YET. Indeed outside of the Soviet Union and a few places like Haifa and Bloomington and Ann Arbor the testing has not even started.

When we understand the hypotheses, but cannot find the arguments (evidence) to examine (test), then we will test the hypotheses directly IN OUR OWN WAYS. One way is to go directly to the languages involved and see what we can find. That is what I did, since after Moscow I had only hypotheses from the young scholars but no evidence (because I do not know Russian). I had a poor recollection of some of Carl Hodge's "Lislakh" (AA-IE) etymologies but couldn't find his formal written articles in my library when I started to work. From Carl I got the notion of starting with "nose" because I remembered the problem of the English /sn-/ nasal words and Latin /sent-/. In the case of "four" Carl and I had not agreed on what were etyma and what were not -- the problem of p-IE's /*kwetwer/ (a la Watkins) defeated us. But AA's "four" is one of my favorite cognates and I just followed it wherever I could find its trail. Carl thinks that trail leads to p-IE /*pd/ "foot" but I can't accept the semantics. But also Shevoroshkin reports a Nostratic "foot" in the form of /*p'atV/ from which >> p-IE's "foot". That delights me because Omotic and Amharic have /p'at-/ and /bat/ for "leg". Yet again Egyptian has /pd/ or /p3d/ for "knee" -- Coptic /phat/ -- and Berber /-fud/ = "knee", both extremely unlikely to come from any proto-AA "knee". While Egyptologists tend to have fits if laymen abuse their data, I hear that this Egyptian /p3d/ may have originally been /*p'-7-d/, rather than /*p'-r-d/, because of the Coptic. In any case Egyptian has a proper "four" in /fdw/ or /fd-w/ which seems to be distinct from the word for "knee", hence from "leg". QED.

So all that fussing and picking and searching is what we do when we want to examine genetic hypotheses. If I, in my searching, found four cognates that Illyč-Svitič found long ago, that is NOT a case of wasted effort; it is a case of INDEPENDENT CONFIRMATION. And the opposite for those cognates which Hodge rejects. As one can see from the above example, there are also cases where we cannot agree. Then other opinions or more data are needed. But this kind of work is crucial to final achievement. Let us not forget that Swadesh's efforts were basically rejected outright or passively rejected by those who simply refused to examine them or take them seriously.

Well, now I've changed my mind again. It won't be too much trouble to add Shevoroshkin's letter at this point. His news is really quite hot.

Here it is

Dec. 20, 1986

V. Shevoroshkin
1133 Michigan Avenue JI Dep't. of Slavic Languages & Literature
Ann Arbor, Michigan The University of Michigan
48104 USA JI Ann Arbor, MI 48104 USA

Dear Hal,

Merritt Ruhlen sent me a copy of your letter (Nov. '86) to A. Dolgopolsky (@) [who is in Europe on sabbatical], and I sent to you, on your univ. address, a copy of Michigan Today with our pop. publications on Nostr., Dene-Caucasian, etc. I'd like to comment on your letter as if were a Newsletter on distant relationships (we here prepared a list issue of such a thing 2 years ago but never sent it out -- no money for such thing as stamps at the Univ.)

First, the list of Russians who work in the field of distant rel-ships is very much incomplete: add V.A. Dybo who heads the Illic-Svitic's Seminar in Moscow and edits I-S's posthumous dictionary (Nostr.); V.V. Ivanov who wrote 2 very good reviews on this dict. [see translations in our book "Typol., Rel-ship and Time"] and broadly uses both Nostratic and Sino-Caucasian (= Dene-Cauc) data in his works; S.L. Nikolaev (Starostin's co-reconstructor of North Caucas.; he added Na-Dene to Sino-Cauc., hence "Dene-C."; his article on this matter will appear in our new book; see below); V. Terentjev who published a few excellent Nostr. etymologies in "Etimologija" and wrote a Forschungsbericht on Nostr. (with Dybo) for the important '84 conference, - and many others.

Secondly, the account on languages belonging to different macro-families is unsystematic, so that the reader is somewhat confused. The list of macro-families does not emerge in clear shape; it shall be :

Nostr.

Dene-Cauc (*) (usually called Sino-Cauc.)

"Macro-Asiatic" (Nikolaev's term)

Amerind (Greenberg's term) [Esk-Al (< Nostr.) and Na-Dene don't belong here, of course]

Khoisan

Indo-Pacific] maybe belonging

Australian] to one of the above

with possible dialectal grouping: Nostr. & Sino-C. (Starostin); "Macro-As." and Amerind (Nikolaev: *NV "I/me, *mV "thou/thee", etc.; hierarchy of stability, according to Dolg.; see translation of his article in "Typology") [Exact phonetic charts are needed, - as I.-S. and Dolg. provide]

(@) He will be part of our project (see next page); unfortunately, I couldn't manage to get him here for sabbatical ...

(*) Four main families (N.-Cauc., Sino-Tib., Yenisei-an, Na-Dene) + Hurr.-Ur., Hatti, Basque (see excellent work by V. Cirikba in the materials of the '84 conf.), maybe Etr. (see Ivanov's work in "Tekst, I think, Moscow '83) + maybe a few more "isolates". (Note: For those non-linguists, et al, methinks his short terms equal the following: Etr. = Etruscan, N.-Cauc. = North Caucasian or North Caucasian, Hurr.-Ur. = Hurrian-Urartean or HurroUrartean, Sino-Tib. = Sino-Tibetan, Nostr. = Nostratic, Esk-Al = Eskimo-Aleut or Eskimoan. HF)

Thirdly, the belonging of certain languages to certain families/macro-families (=phila) is not always given in accordance to new -- and very persuasive -- data. (Cf Yaxontov's remarks on Austro-Tai made over

years; so some languages you mention as being Sino-Tib. are, almost certainly, Austro-Tai). Of course, one may argue on this point, - but then, at least mention these recently proposed, important, connections (as for Benedicts's work, it IS rather poor; his last book on Austro-Tai character of Japanese seems to be one big mistake, - all data show that Jap. and Kor. are Altaic < Nostr., and Dolg. and Menges, et al, integrate Japanese as Nostr.)).(1)

Fourthly, if you provide broad connections, don't concentrate too much on WHAT HAS BEEN ALREADY ACHIEVED LONG AGO, e.g.: "nose/smell/sniff":(*) Nostr. *sun(g)V (Ur. *suNa, Kart. *s^un, AfAs [s]n- ; see I-S. in "Etimologija 1965", M. 1967, p.342 - this is his famous long list of Ns roots), etc. Or, "generate/bear/beget": cf. (same list, p.361) Nostr. *[e]wV "give birth" (IE *seu-/ *seuH-, Uralic [s]wV-, Kartvelian *sw-); or, "brother": cf I-S (2nd volume of the dict., M. '76, pp.111-112) Nostr. *z1[al]nyV "pregn., posterity", etc. (AfAs *d_n "get pregnant, posterity, bro, si", Kartv. *z^m-a "bro", Drav. can_-ai "fetus, pregnancy"), - one of the few roots to which Dolg. objects somewhat, reconstructing rather (Etimologija 1972, M.1974, pp.168-169) Nostr. *ziNmV "younger sibling" > "y. bro" (Kartv. *z1ma-, AfAs. *[z]VnmV, Yukagir *onmo/conmo, Drav. *cin_n_ "child, small"; AfAs. root means "y. sibl of same generation").

Since your letter was not a newsletter, my objections could be considered as invalid; but in a future newsletter they should be taken in consideration - I think.

Now, a few questions. Could you comment in some details on the new Starostin "glottochr." method? Could you think on some publishing house to publish our new -- introductory -- book on distant rel-ships (+ Dolg.'s article on Nostr. > IE vowels and Nikolaev's article on Na-Dene as part of DeneCauc., as supplement): our editor objects that I put the name of my younger co-author M.Kaiser as, indeed, co-author (which he is)**. Could you give me name of archeologists who work in our time-depth (25,000-12,000 years ago) and who would be interested to participate in a book on the prehistory of peoples and languages [I'm applying for a grant for a high-rise interdiscipl. project", as they say here: money is in the Univ., but no guarantee that we get it; we have linguists and a biologist (L. Cavalli-Sforza) and mathematicians already; if you would like to participate you're quite welcome -- again, provided we get this money (500,000 for 3 years starting with May '87): main aim is to summarize work which has been made so far and add new (EXACT) comparisons betw. macro-families, and absorb relevant info from arch./anthr./biol., etc.].

Best wishes for your important beginning, and for the network -- we're quite ready to participate, etc.

Yours,

Vitaly

(*) Seems nothing to do with Lat. sentire (< IE *sent- "go" etc.)

** The editor proposes that I just indicate his participation; the third co-author is I.C. Catford.

(Note: Vitaly also enclosed a 17 page "Recent Work on Remote Relationship of Languages" which he presented to the Houston conference (March 1986) on "Genetic Classification of Languages". That is too much for me to reproduce, even though it is rich and quietly but enormously exciting. I suggest that interested folks in the network get in touch with Shevoroshkin and I bet he would generate a copy. If that fails, I could make copies for individuals but at that point I would need Vitaly's permission! Just one example! Guess what the Greenberg Amerind "smell, nose" is? That's right -- *[i]n or [u]n! So my nose goes from Cape Town to Cape Horn -- by land! HF)

- - - - - that's all folks - - - - -

Tones are written over vowel letters (á, â, ã, ä), but after syllabic consonants: (ḿ̩, ḿ̩').

TRANSCRIPTION. It is amusing to find linguists discussing systems of phonetic transcription after two generations of the 20th century's great advances in phonetics, phonemics, and phonology. There has been one major international convention adopted -- IPA -- and yet scholars have been less than total in their obedience to it. But is there any other INTERNATIONAL STANDARD that has more adherents or that is as well known? Probably not but that is not the only question that can be asked. I am enclosing Aaron's proposed system of transcription with awe and praise. Awe at his knowledge of so many different ways of writing things. Awe that his typewriter can run circles around my computer (both IBMs). Praise at his efforts. So, dear reader, do look over Aaron's chart and make your own judgments. Let me get on with the questions:

1) Thinking internationally, is every tradition of writing going to be used and/or honored? Devanagari, Arabic, Hebrew, Cyrillic, Greek, Armenian, Georgian, Ethiopic, Chinese, et al? (Yes! Everyone would agree. Cultural relativity and all that.)

2) Ought every tradition of writing to be used for international communication among scholars concerned with phonology? Or -- is it even possible for every tradition of writing to be used in what Hymes called cosmopolitan communication?

3) Which is more important for a world-class cosmopolitan writing system -- simplicity and practicality on the one hand or maximum phonetic precision on the other? The first is likely to be perpetually insufficient in particular cases, while the second must keep generating new symbols. The first would be easy to learn relative to the difficulty of the second. Seeing Swadesh's proposed symbols for clicks reminds me that it is indeed Swadesh who represents the fullest expression of the first principle, while I nominate Dolgopolsky for the second. I cannot even get close to Aaron's symbols for clicks. My computer can do roughly half of the Africanist/Khoisanist practical symbols, but it can do ALL of Swadesh's clicks. E.g., let p! = the kissing click; let t! represent the "tsk, tsk" click (/ɬ/), and so forth. Swadesh's system has one potential advantage over Aaron's system and that of the Africanist, to wit, it can show much more cogently a possible cognate between a click and a non-click. For example, Elderkin's work shows changes or variations linking the clicks (/ɬ/) and (/ɬ/) with (/tɬ/), i.e., palatal-alveolar tongue-tip (apical) click and the lateral tongue-blade (dorsal) click with a glottalized lateral voiceless affricate. Swadesh could write those as: c! = tɬ and l! or tɬ! = tɬ. Release phenomena could be shown just as adjuncts, e.g. the famous :kung would become c!kung.

4) And finally, what is more important -- to be able to type the stuff on a typewriter and/or computer, thus being limited by the technology of monolingual engineers, or to be free to write anything we want BY HAND??

Sorry I got carried away by the fascinating questions raised by Aaron's system, which I shall now call DPA = Dolgopolsky's Phonetic Alphabet. Long ago I started using APA for the catch-as-catch-can system shared widely among Africanists, Amerindists and American linguists generally. It is close to IPA, modified mostly to suit typewriters.

PRACTICAL MATTERS. May I suggest that one way this network can work, and work much more effectively, is to break up the road block presented by one person writing a Newsletter. What is crucial is communication among all those who choose to transmit and those who choose to receive. As Aaron said -- we have a lot to tell each other. I have a suggested way around the problem of the road block. Even if I plan to keep sending out circular letters from time to time, when it is appropriate, there does not seem to be any good reason for the whole

network to depend on my limited funds and energy and knowledge. More is better: Normally, people write each other dyadically or they write formal articles for journals. The advantage I see to the network idea is that we can preserve the informality and freedom of the dyadic letter (well, not all the personal stuff) as well as the communicative scope of the journal article. Let's see.

Simple proposal = EVERYONE HAS THE MAILING LIST AND EACH TRANSMITTER WRITES TO THE WHOLE LIST WHENEVER HE HAS THE URGE. For example, suppose that Derek Elderkin is outraged by Swadesh's practical approach to clicks. That is very important business to Derek. So he writes his scorching review of Swadesh; takes the mailing list he received in Circular.3, gets it copied/zeroxed onto a sheet of gummed address labels or makes a poor graduate student do the equivalent, and sends his review off to the 115 members of the Long-Range Comparisons network. Or suppose that Rainer Vossen is so captivated by Elderkin's analysis that he feels a need to transmit some of his new field data. So he writes an article for SUGIA and sends a post card to members of the network, alerting them to the forthcoming article. And just to feel more comfortable he writes the post card in German. Or Dr. CanDo figures out a way to send letters to Soviet colleagues which do get through and don't put the colleagues in hot water with the KGB. He sends a one page letter to the network just as Elderkin did. And so weiter.

ON GATE KEEPERS. We all have to put up with gate keepers, not just other scholars but also governments and foundations. For example, you have a Circassian informant with a cleft palate who drinks too much cognac, resulting in much trouble pronouncing the middle portion of DPA. So you take very good notes, make a brilliant analysis, and send an article off to the International Journal of Pathological Glottalizations. A gate keeper, called the editor, tells you how much they appreciate your article -- but they must reject it. Hias, yes, but you can send your analysis off to the network and get fairly quick returns. Ah, yes, reply the Chadicists, do you know that in parts of our area the uvula undergoes a kind of circumcision? Hmmm! And so weiter.

ON SWADESH. I need to correct some of what I said about Swadesh a few pages back. The key article was "TRAS LA HUELLA LINGUISTICA DE LA PREHISTORIA", 1960. (Suplementos del Seminario de Problemas Cientificos y Filosoficas, 2a Serie, 26:97-145), Mexico, D.F., Universidad Nacional Autonoma de Mexico. I finally got to the library and found Joel Sherzer's THE ORIGIN AND DIVERSIFICATION OF LANGUAGE, by Morris Swadesh. 1971. Edited by Joel Sherzer (University of Texas). Foreword by Dell Hymes. Aldine, Chicago/New York. A posthumous collation, since Morris died in 1967. It was very nicely done. Both the bibliography and Hymes's very capable biographical section show that Morris was developing the hypotheses which went into TRAS LA HUELLA ... for many years before 1960 and that many of the hypotheses were grounded in earlier work and hypotheses of Edward Sapir and his circle in the 1920s and 1930s. Swadesh was one of Sapir's early students and thus grew up with the circle. Greenberg was a younger member of the circle and a sometime collaborator with Swadesh on roughly the same kind of search & survey the Moscow circle has been making. One example of the roots of Swadesh's long-range comparisons would be the fact that before World War II Sapir had reduced the 55 phyla of native America (Powell's classification) to just six. Swadesh later managed to get that 6 down to 4 (1960), while Greenberg has reduced it to THREE (possibly), this year (publication date of book).

Since Swadesh was the first trained linguist, that I know of (1), to have produced a total (?) and global scheme of genetic classification of human

(1) Two networkers mentioned Trombetti as a pioneer. I know little of his work.

(2) Swadesh's scheme was not actually total; e.g., Sumerian was not included.

languages, his scheme should be of great interest to us today. Especially interesting, as we will see, are what he does with the southern tier of the Old World (sub-Saharan Africa and Sundaland-Oceania). The two schemes which follow are, respectively, "The World Linguistic Net" which is from p. 218 of Snerzer and "The Theory of Linguistic Waves (Hypothetical scheme around 25,000 BC)" which is from p.225. It was the first, of course, that I remembered as "La red linguistica del mundo" and so confused with "Iras la huella..." These chapters do not give details of internal membership in the larger groupings, especially which phyla have been included. In some cases that is easy to infer (e.g., Africa) but in others it is not. In the case of Basque-Venuean (= Vasco-Vene) one would love to see the internal specifics. Here they are:

```

THE .....
NET .      EURASIA      . .      AMERICA      .
      .      .      .      .
      .      (Indo-European) (Basque-Venuean) (Macro-Mayan).
      .      .      .      [ Macro- ] [Macro-] .
      .      .      .      (Arawakan) (Carib ) .
      ..(Hamito-).....(Khmer-.....
.....(Semitic)..... (asmanian).....
      .      .      .      .
      . (Fula- ) (Macro- ) .      (Australian) .
      . (Bantuan) (Saharan) .      .
      .      .      .      OCEANIA      .
      .      (Macro-khoisan) .      .....
      .      .
      .      AFRICA      .
      .....

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G L A C I E R S		G L A C I E R S	
Indo- <<	>>>> Macro-Mayan >>> Macro-Arawakan		
Ham- << Euro-	<< Basque- >>	>>> Macro-Lario	
ito- << pean	<< Venuean >>	>> Lost Languages	
<< Sem- <<VVV	<< >>	>> uninnabited	
<< itic	VVVVVVVVVV		
VVVVVVVVVV	Macro-Australian >>	(South America)	
Fula- >> << Macro-	VVVVVVVVVV>>>>		
Bantuan > < Saharan	Khmer-Iasmanian >>		
<<VVVVVV>> <<VVVVVV>>	<<VVVVVVVVVVVV>>>>		
Khoisan	Lost Languages	THEORY OF LING.	
<<VVVVVVVV>>		WAVES: ca 25,000 BC	
Lost Languages	(Australia)		
(Africa)			

There are two more bits from Swadesh. One is his suspicion that Semitic might be closer to Indo-European than to Egyptian (on p.95). Scholars have viewed Hamito-Semitic (HA) as a given for so long that its traditional families (Semitic, Egyptian, Berber, Cushitic) are semi-sacred memiors. Adding new families to the roster, like Chadic, can arouse intense opposition.

Glottochronology. on p.284 his up-dated version of his dating method yields a date of 20,920 or 18,960 BC for 2% and 25,500 or 23,540 BC at 1%. He included the very low percentages only to "complete the table." Under 2% a correction factor was applied. ! Maopy New Year !

Hal

Hal/Harold C. Fleming

There are some pressing small things to pass on to you.

A. Paul Black could use a job. His contract is ending in Australia. He is very competent and a rare historical linguist in that he has been trained in or has done much field work in FOUR PHYLA: IE, AH, Austronesian, and Australian. Write to P.O. Batchelor, N.I. 5791 Australia, with your job offer -- dyadically.

B. I believe that Marcello Lamberti could use a job, although I may be out of date. Very bright Cush-Utoicist. Write to Bruesseleer strasse 47a, D-5000, KÖLN, Federal Republic of Germany. I've only just met Marcello but our colleagues in Cologne and Munich know him well. He has an important book on Somali dialects coming out in Hans Sasse's Cushitic series.

C. A. Murtonen wonders if anyone has any tips on how to get grants to support book publication. Or would someone like to support a book on West Semitic? His home address is: Prof. A. Murtonen, Wiltja, 35 Margaret Street, Moonee Ponds, Victoria 3039, Australia.

D. You cannot find Shevoroskin and Markey's new and important book "Typology, Relationship and Time" in standard references. Boston University librarians could find no mention of it in print or on their computer. Vitaili recommends that you write to KAROMA PUBLISHERS, 3400 Daleview, Ann Arbor, Michigan 48103 (USA). Costs US\$14.50 and "be sure to ask for the list of misprints!"

E. Can anyone settle the question of the genetic classification of Miao-yao? The problem is not lack of data, correct? It is what the data mean, right?

F. Can anyone get to Nepal before the last KUSUNDA dies? We only have a small corpus (S.I.L. has micro-fiche) but that stoutly refuses to relate to neighboring Sino-Tibetan or Indic or Burushaski or Nenari or Austroasiatic. Or so it seems from the data. I've tried twice to put it in some class but it won't go in anywhere. Most scholars do not even know it exists and I include Indoologists and Siniticists in that statement. Kusunda's region has been dominated by unproductive hypotheses about "pronominalizing" languages and the poor Kusunda are likely to be long dead before someone does there again. HELP! Since our Soviet colleagues have put me in the habit of thinking in Long Range terms, I would GUESS that Kusunda might be a lost branch of Sino-Caucasic, along with Burushaski, purely on geographical grounds. Maybe pronouns too. E.g. "I" is [ci] but its variant [ki] suggests [ci] < [ki]. Many candidates for that and [nu] = "thou". (My God, could it be Utoic? As in *ta/*ne. I'm joking!) But Nehali's /jo/ and /nee/ = "I" and "thou" do look encouraging. Dravidian often has /ni/ for "thou" but most of their pronouns begin with /n-/ anyway. And much of Himalayan Sinitic gets close too; e.g., Dhimal /kaa/ and /naa/. HELP!

G. Far southwestern Ethiopia has at least one, maybe two, new languages to classify. The first, SHABO, is spoken by hunters who live among the Majang (East Sudanic: Surma). Bender and Peter Unseth think Shabo is probably related to Majang. I don't think Shabo has ANY relatives. The second, or BIRELLE, is spoken by fishers along the wollo river. Sasse claims Birelle as a Dullay (East Cushitic) language but Hayward's limited data (gathered on a seize-this-chance basis) shows some of that, but also lots of Utoic Banna, but lots of something else. If it is a Dullay language, it is most unlike the rest in some important respects. It ought to be checked. Unseth has a lot of data on Shabo and the question is of Miao-yao or Burushaski type. What relates to it?

VERY HEAVY STUFF COMING

Proto-Cushitic and proto-Utoic. Several of us are working on them. The data from these languages will force changes in all reconstructions based on Semitic or northern AH but not on southern and will force revisions in Nostratic theory, and in Illic-Svetic's sound laws. E.g., proto-AH *b and *b' > *b in Semitic. Cf. Utoic Maa /p'at-/ and Semitic Hamaric /bat/ = leg. (over???)

hannibal's cognate: lightning, shine. bright: AA B :: IE bh

Internal Etymology. Hannibal's cognate. LIGHTNING (unmarked), to LIGHTEN, FLASH.

SOUTHERN CUSHITIC: DAHALOAN. Dahalo B i r i k' - iina
CUSHITIC: EASTERN: @ (Sasse) P-EC * b a r k'
* b i r k'

@ (Sasse's P-EC is clearly wrong. It should be *B...) SOUTHERN LOWLAND. Arbore b i l i 7 -ito
shine, catch fire b o l k'
set fire to, kindle b o l o k' -Is

@@ (These /q/ have allophones in [q']) DULLAY. @@ Harso B a q q - o
to lighten Gollango B a q
HIGHLAND. Sidamo b a n k'

AGAU: to lighten Bilen b i l i c'
<Semitic? o a r a q

? CHADIC: EASTERN: #i. Hausa w a l k' -iva
(Phonetic doubts re inclusion) Karekare w a l i 7 -vo

EGYPTIAN: (Greenberg) shine Ancien o - r - q

SEMITIC: EASTERN: Akkadian b i r o - u
CENTRAL: Ugaritic b r q
SOUTHERN: Amharic -o a r a k'
verb base b r k'

OMOTIC: NORTHERN: Gonda. Mocha b' a r i k'
SOUTHERN: bright, shine Dime B E l x - An
" " " D E l x - an

Note: Dime [x] < {k'} or /k'}

Proto-Afroasiatic initial /*b/ or /*b'/ plus final /*k'/ are easy to reconstruct because the /*k'/ is nearly universal with Bilen being the only serious problem and /*b/ for two important reasons. First, we already have evidence of proto-AA /*b/ "plain b". For example, Greenberg (1963)'s #20 "to come", which means "go" as often as "come", has initial /b/ in Chadid, Cushitic and Semitic. I would add to that South Omotic (Dime) /bi7/. Since Dime [i] often matches non-Omotic /a/, the match-up seems perfect, e.g., Arabic /baa7/. Beja /baay/. The Dime form is an archaism in Dime and not found in neighboring languages.

So Omotic and some Cushitic /b/ :: Semitic and other northern /b/, while Omotic and some Cushitic /B/ :: Semitic and other northern /b/. Ergo Semitic lost a *B.

These Afroasiatic forms might be cognate with Indo-European forms, such as English "bright" < P-IE */bhraq or */bhraq/, depending on Sanskrit /bhraj/ and Old Germanic */brixt/ (and Robby Burns too!).

Watkins (in American Heritage Dictionary) gives */bherEq/ with by-form */bherEk/ meaning "to shine, bright, white". But he also lists */bhel-/ meaning "to shine, to flash, burn; shining white and various bright colors: fire". He later in the discussion of */bhel-/ cites a zero-grade (form without vowel) form */bhlg/ which is the source of Latin */fulg-men/ > /ful-men/ = lightning, thunderbolt, as well as */fulg-ere/ = to flash, lighten.

Therefore, it seems that an equation or matching up can be made like this:

proto-Afroasiatic * B a r - k'
or * B i l - k'
:: P-IE * bh e r E o
or * bh - i - q