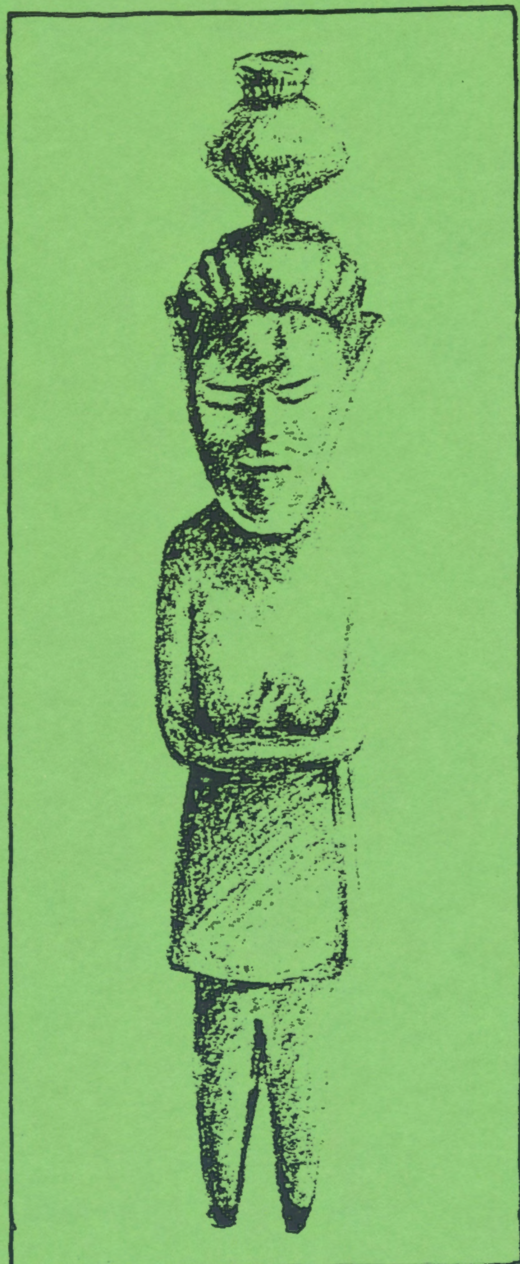
MOTHER TONGUE



NEWSLETTER OF THE ASSOCIATION FOR THE STUDY OF LANGUAGE IN PREHISTORY

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MOTHER TONGUE 13 April 1991

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Editor (April): Harold C. Fleming

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1) mtDNA and THE AMERICAS. Focus on DOUGLAS WALLACE.

The Pima, Papago, and Hualapai of Arizona are often said to be descendents of the archeological Hohokam (Amerinds) who migrated north from Mexico into the arid Southwest of the USA. Their kinfolk among Amerinds include the famous Aztecs, the Hopi of the Pueblos, the Shoshone of the southern Plains, and the Southern Paiute of the Great Basin. Their archeological ancestors and their linguistic kin appear to be a desert-adapted group of northern Mexico and adjacent USA. They have long been called the Uto-Aztecan group or family of languages. To Edward Sapir Uto-Aztecan belonged to a larger family which he called Azteco-Tanoan, one of his 6 phyla in North America of which one was Na-Dene and one was Eskimo-Aleut. In the Greenberg classification Uto-Aztecan joins Kiowa-Tanoan and Oto-Mangue as one of three branches of his major Amerind sub-phylum, most aptly called Central Amerind. With later improvements on Amerind taxonomy Central Amerind became one of only three sub-phyla of Amerind. That is what Pima, Papago, and Hualapai represent in the study we are looking at now.

The Maya of the lowlands, in this case the Yucatec of the Yucatan peninsula, Mexico, not only are descendents of the even more famous Mayan civilization but also are close to the central area of the Amerind distribution in the New World. The Chibchan group which includes the politically famous Miskito of Nicaragua is at the very center. Nevertheless the Maya are not at all close to the Chibchan group linguistically or culturally. Sapir put Mayan into the Mexican branch of his large and widespread Penutian phylum. In the Greenberg scheme the Maya remain where Sapir put them except that the whole Penutian taxon has become a major branch (one of three) of the Northern Amerind sub-phylum. Most of the Penutian kin are spread from Oregon down through California (many languages) and down into Mexico and over to Alabama in the southeastern USA. The Maya represent the extreme southerners of the large Penutian stock. And even though they are in some ways quintessential Mexicans and are treated in Wallace's study as Central Americans, they in fact represent the Northern Amerind part of Greenberg's classification. Neither the Maya nor their kin are associated with deserts to any strong degree. The Maya are usually thought to derive from the lovely green highlands of southern Mexico. A great majority of their Penutian kin live in highland or forested areas.

The Ticuna of northwestern Brazil live in the Amazonian rain forest. In the amazing old linguistic taxonomy of South America which listed over 110 'independent' families (phyla, I guess, if they are independent) the Ticuna are said to be absent, unlisted. Greenberg therefore is the beginning for them. He gives them and their close kin the name of Ticuna-Yuri, joins them to the Tucuna group and 17 others in the medium-sixed sub-phylum which he calls Macro-Tucanoan, a coordinate half of Equatorial-Tucanoan, one of the six major Amerind sub-phya. All the Ticuna kinfolk are concentrated in the north Amazon and adjacent Caribbean, their most famous (distant) relatives being the Arawakan group who are supposed to have greeted Columbus and lived to regret that --but not for too long. Apparently they also gave the world the words 'tobacco' and 'canoe', among others. The Ticuna and their cousins are very little known to the outside world, even to anthropologists who may recognize the name Nambikwara, one of the 17 other groups. Geographically and culturally isolated, the Ticuna show almost no gene flow from the outside (around 02%). In the improved Amerind taxonomy they represent Southern Amerind.

These three groups, Pima, Maya, and Ticuna, represent a major part of the cultural and ecological diversity of the native Americans. Linguistically, they represent Greenberg's Amerind range from Northern to Southern, although one would like more. Contrariwise, to Greenberg's and Sapir's opponents, the Pima, Maya, and Ticuna represent three independent phyla whose origins may indeed have nothing in common, who may have gotten to the Americas at very different times, following very different routes. Almost everyone, pro-Sapir or pro-Goddard, seems to agree with Thomas Jefferson that the Amerinds came from Asia at some time(s) in the past.

Douglas Wallace and his colleagues have made a pointed and robust test of the two linguistic theories of Amerind origins. The theories have genetic and prehistorical implications, as the splitters are sometimes reluctant to admit. Wallace et al proposed to measure -- biogenetically -- the implications of each theory.

Greenberg's scheme clearly implies a common genetic origin for all non-Na-Dene and all non-Eskimo-Aleut, i.e., one population or cluster of closely related populations. Given the common belief in Asian origins, he implies or states that one grand and very early migration brought proto-Amerind to the Americas there to differentiate into many hundreds of cultures and languages. He has also clearly stated that there is even a date for the migration, circa 11,000 BC, based on archeology and a glottochronologically-informed estimate.

His opponents, the splitters, are an intellectually amorphous lot, characterized primarily by scepticism and scientific inertia. But their doubts do add up to null hypotheses --whatever group they study has no necessary genetic connection with somebody else's. Yes, of course, they might say, everybody really knows that the native Americans are either a race or a group of old Asian emigrants and they probably came over during the past 13,000 years like our friends in archeology say. But there is no evidence that there was one grand migration or that different groups did not have their own migrations. Thus, for example, the Iriquois may have left Siberia in 9000 BC, while the Ticuna possibly left the Kuriles as early as 10,500 BC to get to the Amazon. The Pima could have left, say Mongolia, as late as 6000 BC. And so forth. But, except for being ostensibly committed to the null hypothesis, the splitters do not like to take stands of this sort. As Descartes didn't say: "I think, therefore I don't know."

In anthropological tradition there is an option open to the sceptics -- the Boasian option. As most American anthropologists have been trained to believe that there is no one-to-one correlation between race and culture, race and language, or language and culture, one could maintain (and still remain quite popular) that there is simply no correlation between native American genes and languages. Hoorah for the biogeneticists and archeologists, but their conclusions have nothing to do with language. It's an entirely different thing. (Please note: Boas doubtless never took such an extremely negative stance.) This is a strong, logical and defensible position to take. But, unlike scepticism and inertia, the Boasian option is testable. Are there no correlations? We'll see!

What Douglas Wallace and his colleagues found is very clear and awfully exciting. Since it has been reported in a number of places by now, I choose to give you herein lots of references to pursue, few of the technicalities, and an overview of their main conclusions. If one were a Popperian, one could say that the amorphous null hypothesis had been falsified. If one were more relaxed and more like ordinary scientists, one could say that the Greenberg hypothesis had been confirmed in all particulars, except for the date. They are still working on that.

An overview of their main conclusions first:

Most important is that at a minimum the Pima-Papago, the Maya, and the Ticuna do have a common genetic origin, as opposed to Asians, Europeans, and Africans. Extending the logic to what the three ethnic groups represent, we can say that those three groups show that all/most/many speakers of the three languages --or the three populations associated with the three languages --are derived from the same original population of Asians who migrated to North America sometime between 15,000 and 30,000 BC, speaking a language ancestral to Pima-Papago, Mayan, and Ticuna.
And by extended logic the populations associated with Greenberg taxon -- Amerind -- are genetically related in the same way.

However, there are other North Americans not covered by the exclusion of Asians, Africans, and Europeans. Neither the several Na-Dene populations nor the Eskimo nor the Aleut have been segregated by this study. Perhaps the Apache or Navaho, the source of the Dene in Na-Dene, are not genetically distinct from the Pima or Papago who live not so far from them in Arizona? Or the more distantly related Hopi who live right inside Navaho territory? Although Douglas and his colleagues did not get to the Navaho or the northern Athapascans, other genetic studies have separated the Na-Dene, Eskimaux, and Aleut quite smartly from the great mass of 'American Indians'. (Please recall reports on Christy Turner's and Cavalli-Sforza's work in earlier issues!)

As to the origin of Amerind, the more precise conclusion is that the Amerinds are descended from an Asian population of 15-30K BC, or more precisely some Asian females, and that there is evidence of four matri-lineages within the mtDNA data. So one is not forced to believe that nobody ever adopted a stranger's

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language, like wives from another tribe. One can still suspect that the Maya way down there in Guatemala may have married a few local non-Maya and even non-Penutian Mexicans in the past.

Another piece of evidence for diversity within overall kinship comes from the 7000-8000 year old brain of an 'Archaic Indian' who lived in Florida. That is about 3200 km from the Pima and 5600 km from the Maya; yet still the Amerind mtDNA pattern shows. There are also some different developments from the 'founding haplotype'.

Douglas and his colleagues are rushing to plug up holes left over from their study. I say holes in the sense of gaps in our knowledge. One is the Na-Dene et al data base and analysis. Another is an exciting investigation into the smaller ethnic groups of the Soviet far east, to be done in cooperation with USSR scientists; these may help to nail down Asian links with genetic Amerind. By default one has to use the data from other studies and in Asia they have tended to be from Japan, China (a few), or southeast Asia, although strangely enough we know the mtDNA of the Tharu (Sino-Tibetan) of the Himalayas. Then Douglas's team will extend their work over to India and eventually farther west.

An overview of the references to and in the study:

Part of the reason for taking a more general approach to this study is that it has been reviewed or summarized in a number of other places. There are also two key original articles themselves to refer to. Let's do them first:

In the AMERICAN JOURNAL OF PHYSICAL ANTHROPOLOGY 68:149-155 (1985) the authors were Douglas C. Wallace, Katherine Garrison, and William C. Knowler, the first two from the Depts. of Biochemistry and Pediatrics, Emory University School of Medicine, Atlanta, Georgia 30322 and the third from National Institute of Arthritis, Diabetes and Digestive and Kidney Diseases, Phoenix, Arizona, 85014, USA. The title was "Dramatic Founder Effects in Amerindian Mitochondrial DNAs". Data from Pima, Papago, and one Hualapai were compared with Asians, Africans, and Europeans. The abstract reads, as follows:

"Southwestern American Indian (Amerindian) mitochondrial DNAs (mtDNAs) were analyzed with restriction endonucleases and found to contain Asian restriction fragment length polymorphisms (RFLPs) but at frequencies different from those found in Asia. One rare Asian HincII RFLP was found in 40% of the Amerindians. Several mtDNAs were discovered which have not yet been observed on other continents and different tribes were found to have distinctive mtDNAs. Since the mtDNA is inherited exclusively through the maternal lineage, these results suggest that Amerindian tribes were founded by small numbers of female lineages and that new mutations have been fixed in these lineages since their separation from Asia."

Their more recent article is in AMERICAN JOURNAL OF HUMAN GENETICS 46:613-623, 1990. The authors are Theodore G. Schurr, Scott W. Ballinger, Yik-Yuen Gan, Judith A. Hodge, D. Andrew Merriwether, Dale N. Lawrence, William C. Knowler, Kenneth M. Weiss, and Douglas C. Wallace. All are at Emory University, except Y-YG who is at Dept. of Biotechnology, University of Agriculture, Serdang, Selangor, Malaysia; and DAM & KMW are at Pennsylavia State University, University Park, PA, USA in the depts. of Biology, Anthropology, and the Graduate Program in Genetics; DNL who is now at AIDS Program, NIAID, National Institutes of Health, Bethesda, MD; and WCK at the address noted in the first article. The title is "Amerindian Mitochondrial DNAs Have Rare Asian Mutations at High Frequencies, Suggesting They Derived from Four Primary Maternal Lineages". Their Summary of the article is, as follows:

"The mitochondrial DNA (mtDNA) sequence variation of the South American Ticuna, the Central American Maya, and the North American Pima was analyzed by restriction-endonuclease digestion and oligonucleotide hybridization. The analysis revealed that Amerindian populations have high frequencies of mtDNAs containing the rare Asian RFLP HincII morph 6, a rare HaeIII site gain, and a unique AluI site gain. In addition, the Asian-specific deletion between the cytochrome c oxidase subunit II (COII) and $tRNA^{Lys}$ genes were also prevalent in both the Pima and the Maya. These data suggest that Amerindian mtDNAs derived from at least four primary maternal lineages, that new tribal-specific variants accumulated as these mtDNAs became distributed throughout the Americas, and that some genetic variation may have been lost when the progenitors of the Ticuna separated from the North and Central American populations."

An article about Douglas Wallace and his team's work appeared in THE ATLANTA CONSTITUTION, in the Science/Medicine section, on August 7, 1990. For our purposes here it involved a sharpening up or focusing of what the authors have said about their work. Responding to the reporter's questions about migrations, Douglas was quoted as saying: "The mitochondria suggest these Indians were founded by a single migration and the tribes radiated out from this group.... This can be extrapolated to linguistics too. There was one language and all the current dialects were derived from it." And later on he is quoted as saying that his findings support the theories of ... Greenberg... As is customary with newspaper and magazine accounts there is a long discussion of the controversy about Amerind origins. One interesting new datum about the date of proto-Amerind is a quote from Rebecca Cann who claims that her analysis of mitochondrial genes indicates that there could have been a wave of immigration from Asia as early as 40,000 years ago.

In the Science/Anthropology section of the WASHINGTON POST, Oct. 22, 1990, an article by Sally Squires, entitled "Tracking Telltale Genes in America's Ancient Mystery", again quotes Douglas Wallace. In fact the article is primarily about his work. Another attribution to him is that " The fact that these unusual genetic markers show up in widely separated modern tribes supports the idea of common original ancestors who probably moved from Asia to America during a single, major migration, said Wallace. If there had been hundreds of migrations by separate, genetically unrelated groups you would not expect these rare markers to appear at high frequency in the Americas."

Finally, in the Los Angeles Times on July 28, 1990, a Times science writer, Thomas H. Maugh, II, clarified things in a slightly different way. He says "More than 95% of all North and South American Indians are descended from a small band of hardy pioneers that included perhaps as few as four women, who crossed the Bering Strait from Asia between 15,000 and 30,000 years ago, according to new genetic studies. to become tribes as disparate as the Algonquins of the U.S. Northeast, the Maya of Central America and the Ticuna of South America, geneticist Douglas Wallace of Emory University said in an interview Friday... By charting similarities and differences among mitochondria genes in cells from widely separated groups of Indians, Wallace was able to show that the groups had common ancestors that must have migrated to the Americas together."

Q. E. D. (HF).

I doubt that I am misquoting Douglas, since I obtained these clippings from him.

Two last notes about the fruitful work on mtDNA come to mind. On the one hand Douglas Wallace in a telephone call agreed with me about the dating problem. (see Editorial below) To obtain biogenetic dates, or rates of change from which dates of splittings or mutations can be calculated, would free the mtDNA and other biogenetic research from the uncertainties of the archeological dates apropos the New World and Australia. Douglas said that his team is working on the problem and that solutions are not so far off. Now that is REALLY EXCITING! I'll bet Joe Greenberg 50 French francs that paleo-Indians got to North America closer to 30,000 years ago than to 13,000. Want to bet?

On the other hand we can help expedite the process of getting results from mtDNA. The Wallace team can use biological data of a wide variety, especially from isolated tribes or those not now known. Frozen blood is an excellent source. If someone knows of such, s/he can inform Douglas of it. I know there is blood from the Hadza (Bushmen) of Tanzania, frozen in liquid nitrogen in London or vicinity. The problem is that a colleague refuses to let anyone use it. Perhaps someone can persuade him that this small population from the likely Khoisan homeland is crucial for biogenetic and prehistorical studies?

2) GENETIC EVIDENCE ON HUMAN ORIGINS: VERNE SCHUMAKER, ET AL, POINT TO AFRICA

Through the kindness of Dr. Verne Schumaker (Dep't. of Chemistry and Biochemistry, U/California, Los Angeles) we were given a reprint of a remarkably valuable recent article by him and six colleagues. Thanks to Sheila Embleton for putting me on the trail of this work. Entitled "Identification of the ancestral haplotype for apolipoprotein B suggests an African origin of Homo sapiens sapiens and traces their subsequent migration to Europe and the Pacific", it was published in Volume 88, pp.1403-1406, February 1991 of the PROCEEDINGS OF THE NATIONAL ACADEMY OF SCIENCE USA.

The authors are listed as Jan Rapacz, Linda Chen, Esther Butler-Brunner, Ming-Juan Wu, Judith O. Hasler-Rapacz, Rene Butler, and Verne N. Schumaker. Rapacz and JOH-R are at the U/Wisconsin, Madison. All the others are at UCLA, exccept for RB and EB-B who are with the Swiss Red Cross Blood Transfusion Service, CH-3012 Bern, Switzerland. The abstract of the article is, as follows:

"The probable ancestral haplotype for human apolipoprotein B (apoB) has been identified through immunological analysis of chimpanzee and gorilla serum and sequence analysis of their DNA. Moreover, the frequency of this ancestral apoB haplotype among different human populations provides strong support for the African origin of Homo sapiens sapiens and their subsequent migration from Africa to Europe and to the Pacific. The approach used here for the identification of the ancestral human apoB haplotype is likely to be applicable to many other genes." End.

Even with the great differences among the metalanguages of modern sciences the main meaning comes through to all of us, I believe. (One is reminded why we have tried to keep writing this newsletter in common English) But, since I phoned Verne Schumaker to be sure I translated their metalanguage properly, and so learned some more things, I'll try to spell out the logic and import of this substantial discovery. The abstract is not enough to grasp what they have packed into 4 pages of very small print!

First, the term haplotype is mostly equal to gene cluster or a piece of a chomosome. Second, the DNA used herein is nuclear DNA, not mitochondrial DNA (mtDNA), so it is different from the DNA used by Rebecca Cann and Douglas Wallace, mainly in being inherited from both males and females and in a different shape. Third, the immunological analyses in question ultimately derive from the compatibility studies of transplanted human organs and blood transfusions. Different systems of immunity against foreign elements, based on varying systems of genes, are found in all populations. Everyone has her own array of biochemical defenses, highly similar to her own family's array, and differing in detail more and more as her genetic distance from others increases. Common inheritance, natural selection, gene flow, and genetic drift are the names of four processes which tend to account for the genes of various populations. Fourth, the apoB haplotype should be well known to all middle-aged or over-weight people in modern industrialized societies. It is the protein found on low density lipoproteins (LDL), the principal carriers of cholesterol, i.e., the bad ones! Fifth, there are variants of apoB, alleles, found in different populations. Their name shares the same logic as the term 'allophone', a variant of something, the realization of something in a specific environment. Or its AVATAR (Hinduism). It's the world distribution of the alleles for apoB in humans and other apes which are the gist of this tale.

Leaping and stumbling over the specific technical details of the DNA and alleles, we find that humanity has 14 haplotypes of apoB. All 11 populations studied have at least half (7) of these;

the Swiss have all but two of them (12/14)). Also crucial are the 'epitopes' which make up the haplotypes; I'm not prepared to explain what they are. (Hang on! Kindly Drs. Nwokoro and Hoffman, Molecular Biology, U/Pittsburgh helped out, after I spent two days poking around bio-books. An epitope can be defined roughly as an antigenic site or any site which will bind a single antibody, especially a monoclonal antibody. They are something like the 'supergenes' found in HLA studies. Now do you grasp it?)

The authors say: "...it is now known that all five Ag (Antigen group - HF) sites represent pairs of single amino acid substitutions at five different locations along the apoB polypeptide. Because different individuals possess different mixtures of these genetically determined epitopes, the Ag polymorphism provides a convenient system for assessing relationships." End of quote. It seems that there are 10 letters -- c g a d x y h i t z -- which represent whatever goes into the make-up of a haplotype. Thus, no.13, a haplotype of crucial significance, is called 'ydgti'. Two of these letters, e.g., 'cd', are called epitopes, I think. Or one letter.

Testing 20 unrelated chimpanzees and 8 unrelated gorillas and comparing their epitopes and haplotypes with those of 11 human populations, they find that haplotype 13 is the one linking men and apes, hence the original. They say: "These sequencing data together with the immunological data of Table 2 strongly suggest that the Ag haplotype of the ancestor of man, chimp, and gorilla was haplotype 13 -- that is, Ag (g,d,y,i,t) -- which is the only haplotype found among the unrelated 20 chimpanzees and 8 gorillas examined. Thus, for the apoB gene, the outgroup approach provides strong evidence that the evolution of the human Ag polymorphism is anchored to haplotype 13. Haplotype 13 also represents the most common human haplotype among the Bantu, the Senegalese, and the Swiss. All of the 10 epitopes are found among the Bantu... and eight subsequent crossing-over events can account for the remainder of the observed haplotypes without necessitating fixation of back-mutations at any of the Ag sites." End of quote.

There are four haplotypes found in all 11 human populations. Two of them (no.8 and no.11) both have low percentages and tell us little. But two (no.13 and no.2) are universal and show clinal features, called 'genocline' by the authors. No. 13 (ydgti) starts big in Africa -- about 70% for Bantus and 58% for Senegalese -- and runs down steadily towards the Pacific where it reaches about 02% among the Native Australians and 01% among the Chinese. No. 2 (xagti) starts big in Australia and China --about 59% and 68% respectively -- and decreases towards central Africa (by way of Indonesia, hence most likely southeast Asia) where it reaches 02% among the Bantu. No. 13 declined as we left Africa, while no.2 increased to the east probably because it "confers some genetic advantage over haplotype 13 in a non-African environment." Besides being anchored in the other apes in Africa, no.13's path towards the east, towards the Pacific, is "supported by an abundance of archeological, linguistic, and genetic evidence

(8,9)..." The 8 and 9 refer to the by now familiar writings of Cavalli-Sforza, Allan Wilson, Rebecca Cann, and colleagues. I would be happier were Rapacz et al's argument to stand by itself, without Cavalli-Sforza's blood & serum groups and Wilson's mtDNA evidence and their correlations with linguistic and archeological data. Let us try to get their logic clearer and see if we can make the apoB hypothesis stand on its own two feet.

To begin with, the 'outgroup approach' mentioned above is precisely the same as the concept of RETENTION used in historical linguistics. If ten Chadic languages have each their own word for X, none of them can be said to represent the proto-Chadic word for X. But let us say that Hausa's word, for example, is the same as Berber's and Hebrew's, the chimps and gorillas so to speak. In that case Hausa's word would be seen as a retention from proto-Afrasian and thus represents in Chadic the archaic form. The other nine Chadic languages have innovations, we would say.

Since we don't do clines in linguistics, the analogy would break down in detail later on. In any case the real problem is not whether haplotype 13 is the archaism (retention). That is easy to accept. Why can't East Asia or Australia be the dispersal point, instead of central Africa? After all they do have the archaism too, even if only 02% (of haplotype 13). Maybe their strength in haplotype no.2 shows that it is the beginning of a cline which is associated with Homo sapiens sapiens only?

Another interpretation might be that haplotype 13, linking us to the apes as it does, is an indication of our common human past, e.g., the Homo erectus stage of development. Homo erectus did have an Old World-wide distribution and many paleoanthropo-logists do maintain that Homo sapiens developed out of the local varieties of Homo erectus, explicitly so in the case of China. Thus human populations in different areas would retain different percentages of haplotype 13 for purely local reasons. And haplotype 2, being peculiar to all Homo sapiens populations, would show that Australia or China are the start of our dispersal around the world because their natives have the highest percentage of no.2. Up to here it seems to me that we DO NOT HAVE TO accept that the two genoclines mean that we-all came out of Africa and moved to Australia, leaving kinsmen off in Europe and Asia along the way. The key question that will not easily go away is this: why does having more of an archaism mean that one area is a starting point for a migration, while having least of it means that another area is the terminus? Let's think about it some more!

Still the authors are sure that the genoclines have an important bearing on the logic. They say that: "The genocline observed in Table 1 begins with the 0.695 frequency observed for haplotype 13 among the Bantu and progressively declines in the order of the proposed human migration from Central Africa through Europe and Asia to the Pacific and Australia. The abundance of haplotype 2 increases in an ALMOST IDENTICAL PROGRESSION (my emphasis - HF) from Africa to Australia. While change in haplotype frequency might be explained by accelerated genetic drift among migrating groups because of their small sizes, with genetic stabilization occurring as the population of each newly colonized region increased, it is hard to account for the progressive rather than random nature of the changes observed for haplotypes 2 and 13 by this mechanism. The probability that the genocline in haplotype 13, which decreases in order of geographical contiguity, is due to random chance is estimated to be less than 1 in 5000." End of quote. I'd love to know how they calculated that!

My motive here is not to torment the authors of this article. They are probably right about the African origins of Homo sapiens; after all that theory has been 'ahead on points', as they say in boxing, for some time now. When I was a graduate student, however, people favored an Asian homeland for MODERN man. Two of our colleague experts, Christy Turner and Douglas Wallace, still do. Rapacz et al have delivered a good hard blow to the Asian theory but I don't think it was a knock-out punch.

In the article the eleven populations were the Bantu of central Africa, the Senegalese of northwestern (sic) Africa, the Turks, the Swiss, Indians (north?), Tamil of Ceylon, the Tibetans, Chinese (north or south?), Indonesians (Balinese and East Indonesians), and Australians (aborigines). The terms Bantu, Indians, and Chinese are somewhat unfortunate because each term covers quite a bit of variation. But in a global inquiry the variability will be lost in the total contrasts of continents. In order to show some of the primary data we will display the frequencies of two haplotypes from the fourteen. We'll call it Table 1 as they did. Then we will try to show 'genetic distances' among these 'ethnic groups', as they call them, and label these as Table 3, as they do. (Tables 1 and 3 are shown at end.)

These data confirm another point made previously and reported in earlier issues -- Central Africa is farthest away from everybody else. But the Australians are next farthest away, not being much closer to Indonesians (next door) than to distant Turks or Swiss. Senegalese are much closer to the outside than the Bantu are, 10-12% on average. Here the source of the data is important. Some Senegalese populations have borrowed genes from Middle Easterners (Berbers primarily, also Arabs), while others have not.

Some things dear to old-fashioned ideas of race really struck out absolutely. Skin color, for example. Not only are the black Africans farthest of all from the black Australians, genetically and geographically, but the light north Indians (presumably) and the dark Tamils have the second closest distance on the whole chart. The Australians had been supposed to be 'archaic Caucasoids' and India was once populated by them, under the rubric of 'Australoids'. Yet the non-hairy un-Caucasoid looking Indonesians, Tibetans, and Chinese are closer to the wooly Turks, Swiss, Indians and Tamil than the Aborigines are.

Cavalli-Sforza and colleagues are strongly supported by this article. Naturally, there are too few groups for linguistic correlations to be attempted. However, the data on the Indonesians strongly suggest that Australia is NOT the direction to look for the linguistic relatives of Austro-Thai. Rather to the west (Austro-Asiatic or Nostratic) or north (Sino-Tibetan) would seem to be more promising. And, finally, if these links between biogenetics and glottogenetics have as much merit as they seem to, then a binary comparison of Niger-Congo and Australian languages should be the most difficult one ever, because that is probably the remotest relationship on earth! If they are related at all! (This a HEURISM, of course. Remember?)

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TABLE 1: Percentages of Ag haplotypes: Rounded off.

Haplotype no.2 (xagti). Bantu = 02, Senegalese = 04, Turks = 37, Swiss = 19, Indians = 39.5, Tamils = 56, Tibetans = 51, Chinese = 68, Indonesians = 57, Australians = 59

Haplotype no.13 (ydgti). Bantu = 69.5, Senegalese = 58, Turks = 27, Swiss = 21, Indians = 24, Tamils = 17, Tibetans = 04.5, Chinese = 01, Indonesians = 09.5, Australians = 02

The extreme eastern end of the clines seems actually to be China.

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TABLE 3: Genetic distances: Figured from apoB frequencies.

	Ban	Sen	Tur	Swi	Ind	Tam	Tib	Chi	Bali	Aus
Bantu	0	19.5	40	44	42	48	60	64	52	67
Senegal . . .		0	30	32	32	39	49	57	46	58
Turks			0	16	06	14	28	31	23.5	39
Swiss				0	19	26	33	39.5	32	45
Indians					0	10	23	28	21	38.5
Tamil						0	18	23	18	35
Tibetans							0	19	18	35
Chinese								0	14	34
Indonesians (Bali +)									0	38
Australians (aborigenes)										0

Lowest numbers = closest groups; highest numbers = farthest.

3) ARCHEOLOGY: INDO-EUROPEAN HOMELANDS. DAVID ANTHONY, POOR FARMERS, and HORSES.

Dr. David W. Anthony has argued for an Indo-European homeland in south Russia but on primarily archeological grounds rather than linguistic. This is an updated version of an archeological report which previously came out in CURRENT ANTHROPOLOGY (cf Vol.27:291-313, 1986, entitled "The 'Kurgan' Culture', Indo-European Origins, and the Domestication of the Horse: A Reconsideration". Also another with Bernard Wailes in Vol.29:441-445, 1988 which was a CA Review of Colin Renfrew's "Archeology and Language: The Puzzle of Ino-European Origins). The present article switches the attention to migrations as a class of phenomena and has a jazzier title, to wit: "Migration in Archeology: The Baby and the Bathwater", AMERICAN ANTHROPOLOGIST 92:895-914, 1990.

It is very satisfying to me because it puts PIE, i.e., proto-Indo-European just where it should go, using Dyen's dispersal theory + common sense. It is close to Maria Gimbutas's homeland for PIE but very far from Renfrew's PIE homeland or that of some of our colleagues. I'm not trying to proselytize for our homeland, just reporting Anthony's views, but our conception of PIE-land has not been presented in MOTHER TONGUE for a long time, if ever.

Not to be facetious but theoretical, I have recently thought that PIE-land ought to be put squarely in the middle of the Black Sea! That would be a wonderful compromise between the south Russian and the Anatolian hypotheses. Yes, and the Balkan too. However, Bomhard has reported to me that there is no reason to believe that the old Indo-Europeans could walk on water. Only one person, a Semite, has ever been able to do that. So be it.

Nevertheless, Anthony's statements on migration are also useful. His abstract says:

"Migration has been largely ignored by archeologists for the last two decades. Yet prehistoric demography and population studies are accepted as central concerns, and neither of these can be studied profitably without an understanding of migration. Recent books by Rouse and Renfrew have resurrected migrations as a subject of serious analysis. It is proposed here that systems-oriented archeologists, in rejecting migration, have thrown out the baby with the bathwater. Traditional archeological approaches to migration fall short because a methodology for examining prehistoric migration must be dependent upon an understanding of the general structure of migration as a patterned human behavior. Aspects of such a structure are suggested and an application to a particular case in Eastern Europe is described."

Some day one of us will have to study Social Science fads; they come and they go. Often they throw babies out of bathwater and put carts before horses. Linguistics & anthropology have each more or less freaked out on their current dominant interests, giving up most of what excited them a few years back. There seems to be some sort of cultural phenomenon that rules these fields. It reminds me of A.L. Kroeber's famous study of fashions in womens' dress. Yet there is also steady improvement, steady increase in reliable information, mostly because not everyone joins the fads and people keep loyalty to old fads. And so forth. So now we can do migrations and systems theory + eco-evolutionism -- both. Wonderful! But, alas, Marxism is in eclipse or dead.

Sooner or later we must all confront Anthony's ideas about migrations. But for now I'm after PIE-land. As Alexander Militariev would say, the homelands and movements and contacts of the deep prehistoric phyla and super-phyla should be a high priority item in our collective research. David Anthony locates PIE-land in south Russia, associating it with some particular archeological sites, the domestication of the horse, the development of a pastoral culture, and the spread of that into other areas (including Hungary). He draws a convincing picture, although he hedges a bit about IE and PIE. Quoting parts of his main "Case Study" now:

"...The case to be presented is a much-abused chestnut in European prehistory; the expansion of Copper Age horse-using societies (possibly Indo-European speakers) from the grassland steppes north of the Black Sea (the North Pontic region, possibly the proto-Indo-European homeland;...)"

"(paragraph omitted)... In outline, the North Pontic region supported two markedly different cultural traditions throughout the Copper Age (ca. 4500-3000 B.C., recalibrated). West of the Dnieper River in the rolling hills of the forest-steppe were the large (up to 300 ha) agricultural towns of the Cucuteni-Tripolye culture, characterized by rows of substantial two-story houses (thousands in the largest sites), copper metallurgy used for both tools and ornaments, highly decorated polychrome ceramics, numerous ceramic female figurines (some heavily tempered with grain), a developed economy based on mixed agriculture and stockbreeding, incised signs that might represent a notation system, mortuary rituals of an undocumented nature that did not ordinarily involve inhumation, and regular contact or exchange with similar societies in the Balkans and Carpathians to the south and west. East of the Dnieper River, within view of some Cucuteni-Tripolye sites, were the small scattered hamlets of incipient farming or stockbreeding societies that had evolved directly from local Mesolithic hunter-gatherers, under the influence of the farmers to the west. They lacked copper metallurgy, female figurines, substantial architecture, or sophisticated ceramics, and they ordinarily buried their dead in formal cemeteries. The Dnieper River, separating those two traditions, was perhaps the sharpest cultural boundary in all of Copper Age Europe."

Interrupting here just to locate the Cucuteni-Tripolye farmers in the 4th millennium BC roughly between Kiev on the east and Transylvania on the west. The Sredni-Stog or undeveloped folk began ca 100 km southeast of Kiev along the Dnieper and extended across the Donets to the great bend of the Don just west of Stalingrad -- Cossack country -- and south to the Sea of Azov.

(Continuing) "During the period 4000-3500 B.C., there were major changes east of the Dnieper. Unlike the Cucuteni-Tripolye farmers west of the river, whose settlements remained confined to the forest-steppe ecological zone, the settlements of the eastern Sredni-Stog societies followed the river valleys south into the treeless steppe, where rainfall agriculture was impossible outside the confines of the wooded riverine environment. Within these circumscribed riverine environments, population pressures apparently led to an important extension of the resource base: the domestication of the steppe horse as a food animal. Numerous socioeconomic changes accompanied the domestication of the horse east of the Dnieper, including changes in settlement locations, increased use of weapons, great increases in imported Cucuteni-Tripolye wealth, and the appearance of artifacts interpreted as cheekpieces for bits. (For reins on horses, not computers - HF) The exploitation of horses reoriented the economy of the Sredni-Stog culture toward the underexploited steppe environment, which was essentially an open niche. At about 3000 B.C., when wheeled vehicles were first adopted in the region, the critical triad of sheepherding, long-distance horse transport (riding), and bulk ox-drawn wagon transport came together for the first time. With this new adaptive package, there was an explosive expansion of the Yamna culture (closely linked to the earlier Sredni-Stog) eastward across the entire Pontic-Caspian steppes, and westward into the Hungarian plain. During the Yamna period the Cucuteni-Tripolye culture and its Balkan sister cultures collapsed, and European cultural development was reoriented in a fundamental manner."

As a sequel also see Anthony and Dorcas Brown, "The Origins of Horseback Riding" in ANTIQUITY, in press 1990. One might want to pursue about 100 pages of the original thesis in: Maria Gimbutas, 1970. "Proto-Indo-European Culture: The Kurgan Culture". In INDO-EUROPEAN AND THE INDO-EUROPEANS. G. Cardona, H.M. Hoenigswald, and A. Senn, eds. Pp.155-197. Also her 1977 article, "The First Wave of Eurasian Steppe Pastoralists into Copper Age Europe." JOURNAL OF INDO-EUROPEAN STUDIES 5:277-337. Isn't thinking about PIE-land a pleasure?

***** FLASH *** Late-breaking news!**

On the front page of the New York Times, May 9, 1991, a report by Wm. K. Stevens on a new article in "today's" NATURE, saying that Drs. Robert Sokal, Neal Oden, and Chester Wilson (S.U.N.Y., Stony Brook) had a new genetic analysis of the spread of agriculture from Turkey to the corners of Europe, supporting Renfrew. Another genocline affair!

4) ON DEAF CHILDREN and BIRD SONGS. ARE GESTURES OR SONGS INNATE TOO?

Announced in the New York Times (March 22,1991), Dr. Laura Anne Petitto discovered that infants BABBLE with their hands, if they are deaf children. A more recent letter (April 7, 1991), following up on the first, Dr. Susan Goldin-Meadow added that the deaf children of deaf parents learn sign language with the same speed that hearing children learn language. Furthermore deaf children of hearing parents not only babble but also develop their own ideosyncratic sign language, providing that no one tries to teach them sign language. These ideosyncratic systems are structurally similar to the oral language of children their own ages, including gestures that are strung together to make sentences, "rudimentary rules of syntax (ordering regularities and deletion patterns)". And the ability to combine phrases in recurrent fashion to make "complex sentences". They "even demonstrate evidence of regularity within individual gestures reminiscent of single-word morphologic structure". These gestural systems are "...far more complex than the spontaneous gestures of their hearing caretakers and more complex than the gestures produced by their hearing playmates of the same age." She adds that David McNeill has found that hearing people produce gestures with HOLISTIC meanings to go along with their speech. That is qualitatively distinct from the linear and segmented gestures produced by (her) deaf children. Well, well, hmmm! Still McNeill's point is probably not news to most of us.

What does all this mean? One could write to Dr. Susan Goldin-Meadow, Dep't. of Psychology, University of Chicago for more information. Also colleague long ranger Phillip Lieberman (Brown University, Providence, Rhode Island) has incorporated data on deaf children in his theories about language origins; one might write to him. Or Eric de Grolier (UNESCO, 1, Rue Miollis, Paris 75015, France), the founding father of LOS. Or Gordon Hewes (Dep't. of Anthropology, University of Colorado, Boulder, CO 80309, USA) who, among others, has theorized about the relationship between gestures and the origin of human language. At a minimum the new information on deaf children strongly suggest that human children have some sort of innate 'program' for developing a full system of gestural communication which may or may not be innately linked to proper (natural) human language. This really is fascinating material!

And what about <u>bird songs</u>? Another recent and quite large article in (again) the New York Times by Jane Brody focuses on the degree to which bird songs are species innate; she makes comparisons to human language acquisition. While marred by a tendency to see individual human languages as analogous to bird species, thus confusing the innateness question, Brody's article presents an extraordinary range of studies on the singing capacities of various species of birds, most of them common varieties like sparrows, starlings and warblers. Brody also has some surprising quotes: For example, "And students of man may have overrated the uniqueness of human language and the ability to communicate through sound." Also a biologist, Dr. Peter Marler(U/California at Davis), said that " For too long those studying language development in people have overemphasized cultural and social influences at the expense of the biological side." (Really? HF) He said his research underscores the importance of biology in the development of speech and language. Earlier on, drawing upon Marler primarily, Brody had said: "Contrary to popular belief, bird song is not entirely instinctive, although most birds show an innate propensity to learn the song of their species. Song birds raised in isolation in the laboratory without having heard their species song develop an incomplete and abnormal version of the song.... But they produce a song that is nearly correct if allowed to hear a tape of it during the so-called sensitive period of song learning, which varies from species to species."

"Deaf birds, on the other hand, never come close to singing the right song. Even if they heard it before becoming deaf, they sing with serious distortions, apparently because they cannot hear and correct their own performances."

"As for the sequence of song learning, the findings show that birds are very much like human children. As fledglings they babble a so-called subsong of nonsense syllables. As pre-adolescents they sing often-mispronounced fragments called plastic song. As young adults they are able to articulate properly the song characteristic of their species, crystallized song."

"Like children learning to talk, birds learn songs from their elders. But unlike children, who can learn any language they are exposed to, the musical language of most birds is somewhat constrained by their genetic heritage. Given a choice of two songs -- their own and that of another, even a closely related species -- they will learn their own. But, if exposed only to the song of another species, they will learn a version of it." End of quoting.

Of some interest to phoneticians especially may be the quotes from Dr. Jeffrey Cynx (Rockefeller University, New York). Quoting from Brody: " While poets may rhapsodize about the beauty of avian song, the birds themselves seem to have a limited appreciation of their musicality. Dr. Cynx's studies suggest to him that 'birds don't really hear melodies -- rising and falling pitch -- the way we do.' If a song the bird knows is transposed up or down an octave, the bird fails to recognize it, his studies showed. In other words, birds respond to absolute pitch (as do only about 5 percent of people) rather than to relative pitch."

"On the other hand, he...showed that zebra finches could distinguish subtle differences in timbre or quality of a sound, which is determined by its harmonics...'This is not something you would expect such a small-brained animal to do. It shows an exquisite ability -- comparable to that of a skilled musician --to detect, learn, remember and produce the most subtle changes in a complex sound.'"

"When tempos are distorted, Dr. Stewart H. Hulse found, a bird continues to recognize sound patterns even if they are considerably slowed down or speeded up. Dr. Hulse, a psychologist with a musical background at Johns Hopkins University, suggested that 'the ability to hear rhythm seems to be present early in animal evolution, but the ability to hear pitch relationships may be unique to humans.'" End of quoting. Brody's article was very rich in other aspects of bird song, including the vast numbers of sounds that can be imitated (mimicry) by some species and the great differences among species in the numbers of songs that they produce. We should close this with the note that bird song/talk is produced in the SYRINX (located in the branching of the trachea just before the lungs), so there are two SYRINGES per bird. They do not use a LARYNX for sound production, unlike humans. Alas, I do not know which long rangers are for the birds (heh, heh) but they may identify themselves after reading this.

(For reasons of space an article by Merritt Ruhlen on "Evolution of Language" cannot be put in this issue.)

5) A GREAT & FRIENDLY DEBATE: ACT 1: BLAZHEK and BENGTSON on BASQUE.

Vaclav Blazhek and John Bengtson have agreed to have a debate among friends and inside of MOTHER TONGUE. The focus of the debate is BASQUE, the perennially hard to classify language unique to western Europe. However, as we will see, Etruscan now lies over the horizon as another potential debate, not to mention Japanese or Sumerian. The issue is NOT whether they are related at all. I think Vaclav and John would agree that Basque is related to both alternatives -- probably. The issue is which alternative is BETTER and which alternative is MORE REMOTE or perhaps to be explained by borrowing and/or sub-strata. What is sought is TAXONOMIC CLOSENESS, not sheer relatedness.

The debate is preceeded by a one page summary of alternatives in transcription which John gives us as a valuable gift. We need these little Rosetta Stones when confronted with each other's symbols for various sounds. If you will recall our discussions of IPA, APA and DPA in an early issue, you can appreciate our problem better. Most of the Americans and Russians have kept to their own phonetic alphabets quite stubbornly, myself included. Some of us discussed these problems before and agreed that there was no hope of international agreement on a new IPA, despite the fact that there is some sort of official one which Peter Ladefoged thinks we should use. A small group of people made up the new IPA and now ask the rest of us to use it. But we probably won't, so it is best to follow the other alternative -- translate from one alphabet to another to another. That's what John does for us. I've added some equations, under the rubric of HF.

Unfortunately, the Blazhek part of the argument will be so poorly represented, or so unfairly, by the one page summary of his view which I have, that the next issue will present a fuller account of his argument than this. John can join in next time too. We also invite Mukarovsky, Čirikba, Militariev, Diakonoff, et al, plus Afrasianists at large, to join in the discussions.

All who participate, however, will be bound by one rule. It is not a whim of mine but dictated by the logic of our debate. One cannot appeal to a reconstructed form from some phylum to compare with another form, unless that reconstruction is SOLID and TESTED and internationally agreed upon to a reasonable degree. Otherwise one has to use attested forms from some known languages. Let us not make up reconstructions as we go along! The first round of the debate cannot be bound by this rule. Still I am, for example, very sceptical of the proto-Caucasic forms used to relate Basque to Macro-Caucasian. There is no backing for them in citation forms. The same thing applies to the purported proto-Afrasian forms used. As an Afrasianist, I cannot relate to or recognize most of them. Again the problem is failure to cite supporting forms. This is not to blame the two worthy authors!

John Bengtson's **Guide to Transcriptions:** *+ HF equivalents if different*

Basque (most vowels and consonants roughly as in Spanish):

ph, th, kh, lh = aspirated consonants (in some dialects)
h = glottal fricative (in some dialects)
s, ts = [ṣ], [ç̣] (retroflex)
z, tz = [s], [c] (dental)
x, tx = [š], [č] (palatal)
ñ = [ɲ] = Spanish **ñ**

HF symbols **Caucasian:**

p', k', c', etc. (glottalized consonants)
ts, ts', dz — c, c', ʒ, etc. (dental affricates)
c, c', ȷ — č, č', ǯ, etc. (palatal affricates)
sʸ, zʸ, tsʸ — ś, ź, ć, etc. = [sʲ], etc.
l, ł (lateral sonants; phonetic qualities uncertain)
hl — λ = ł = x̌ = ŝ (unvoiced lateral fricative)
tl — ƛ = tł = ǩ = ĉ (unvoiced lateral affricate)
tl' — ƛ' = tł' = ǩ' = ĉ̣ (unvoiced glottalized lateral affricate)
dl — λ̌ = dl = ǧ = ǯ̂ (voiced lateral affricate)
x (unvoiced velar fricative)
q, q' (unvoiced uvular stops)
G (= g) (voiced uvular stop)
R — X (= ḫ = x̣) (unvoiced uvular fricative)
ɣ (voiced uvular fricative)
ʔ (glottal stop)
ʡ (pharyngeal stop)
H or ħ or ḥ ħ (= ḥ) (unvoiced pharyngeal fricative)
ʕ or ʕ — ʕ (voiced pharyngeal fricative)
hI, ħI, ʡI (pharyngealized fricatives)
H ("laryngeal" [h, ħ, ʕ, ʡ] of undetermined phonetic quality)
ɨ (= y) (high mid vowel)
aI, etc. (pharyngealized vowel)
a̠, etc. (special prosodic condition [tense voice?], conditions geminated consonants in Caucasian languages)
V (vowel of undetermined quality)

Burushaski:

ph, th, ṭh, kh, qh, ch, čh, c̣h (aspirated consonants)
ṭ, ṭh, ḍ, c̣, c̣h, ʒ̣, ṣ̌ (retroflex consonants)
x, ɣ (velar fricatives)
ə or ă — A (= ă) (short low mid vowel)

Macro-Caucasian Again

John D. Bengtson

Readers of **Mother Tongue** may recall my article of the past year (Bengtson 1990a), in which I proposed that we regard Basque, (North) Caucasian, and Burushaski as the three extant branches (families) of a linguistic phylum provisionally named Macro-Caucasian. I suggested that the relationship of these languages is not remote, but roughly comparable to the time-depth of Indo-European.

Václav Blažek has recently written an article (Blažek, forthcoming) relevant to this issue. The main material of the paper consists of 30 lexical parallels among Basque, Caucasian, and Afroasiatic, and it is claimed that there is as much evidence for the Afroasiatic affinity of Basque as for the Caucasian. He concludes that the question of the genetic classification of Basque remains open, and that any definite resolution is "very far" away.

I could grant that Blažek's conclusion were possible if we had to rely <u>only</u> on those 30 lexical parallels. However, my classification of Basque as Macro - Caucasian is based on (a) lexical isoglosses in the most basic semantic fields, (b) traces of underlying grammatical paradigms, and (c) phonological correspondences and tendencies.

Category (a) has been documented in my earlier articles (Bengtson 1990a, 1990b, 1991a, 1991b), where about 250 etymologies are given as evidence for Macro-Caucasian (and the deeper taxon Dene-Caucasian = Sino-Caucasian). Other etymologies may be found in the works of, e.g., Bouda, Trombetti, Čirikba, Ruhlen.

Category (b) was briefly discussed in my earlier article (Bengtson 1990a), where I presented evidence for seven case endings in Macro-Caucasian. This degree of close grammatical correspondence is another indicator that the relationship is not remote.

Here I would like to discuss another set of evidence that points to a class (gender) system in Proto-Macro-Caucasian. First, the evidence from each family:

Basque now lacks any grammatical gender or class system, but lexical evidence indicates that the language once had a system of noun classes distinguished by prefixes:

A- prefix: Persons and animals:

> **a-hiz-pa** 'sister' (cf. Cauc ***?i̲ćʷV** 'brother, sister', Bur **-Ač̣o** 'brother, sister');
> **a-gure** 'old man' (cf. Bur **gūro** 'grayish');
> **a-tso** 'old woman' (DC: cf ND ***ču** 'mother-in law, grandmother');

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a-huña 'kid' (cf. Cauc ***HʷV̄nxV** 'sheep, lamb');
a-kaiñ 'tick' (cf. Cauc ***q'ī̱n?V** 'louse, nit', Bur **khīn** 'flea');
a-xuri 'lamb' (cf. Cauc: Hatti **wa-zar-** 'ewe').

Body parts:
a-dar 'horn, branch' (cf. Cauc ***ƛ̆V̱rV** 'horn', Bur **-ltur** id.);
a-ho 'mouth' (DC: ST ***Khō(w)H**, Yen ***Xowe** id.)
a-tal 'limb' (cf. Bur **-ltAlt-Ar** 'limb');
a-hur 'hollow of the hand' (cf. Cauc: Batsbi **kor** 'hand', etc.);

Intangible nouns:
a-din 'age' (cf. Bur **den** 'year');
a-mets 'dream' (cf. Cauc ***Hnićʷ V** 'night, dream').

E-/I- prefix: Natural phenomena:
e-lhur 'snow' (cf. Cauc ***ƛʷiu̯V** id.);
e-uri 'rain' (cf. Cauc ***ɣ̂ʷer-**tV id., Bur **hAr-ālt** id.);
e-gun 'day' (cf. Cauc ***Gʷem-**tV and/or ***?ʷɨ̄Gɨ̄nV** id., Bur **gun-c** id., **gōn** 'dawn');
e-sne 'milk' (cf. Cauc ***šinʷV** id.);
i-thoi 'a drop' (cf. Bur **thī-š** id.);
i-tsaso 'sea' (cf. Cauc (W) ***čə** / ***ǯə** 'salt', Bur **ṣ̌au** 'oversalted');
i-bar 'valley' (cf. Bur **bAr** 'nullah, ravine, valley');
i-guzki 'sun';
i-zar 'star' (cf. Cauc ***ʒʷa̱rʕi** id.);

BE-/BI- prefix: Body parts, fluids, and attributes:
be-larri 'ear' (cf. Cauc ***lerħIV** id.);
be-hatz 'thumb, toe': cf. **hatz** 'finger, claw, paw' (cf. Cauc ***kʷač'e** 'paw', Bur **qAṣ̌** 'cubit');
be-koki 'forehead, crown' (cf. Bur (W) **-kAk** 'crown');
be-ha-zun 'bile' (cf. Cauc ***cʷäm?i** id.);
bi-hotz 'heart' (cf. Bur **-As** id.);
bi-rika 'lung' (cf. Cauc ***jerk'ʷi** 'heart', comparison suggested by V. Blažek);
bi-zka-r 'back' (cf. Cauc (Abkhaz) **a-zkwa** id., Bur **-sqa** 'on one's back');
bi-zi 'life, alive' (cf. Cauc ***siʔʷV** 'soul, breath').

O-/U- prefix: Body parts and fluids:
o-dol 'blood' (cf. Bur **del** 'contents of an egg'; DC: Na-Dene ***deł** 'blood');
u-zki 'anus' (cf. **bi-zka-r** 'back', above);

u-kondo 'elbow' (cf. Cauc: Lezgi **q'ünt** id.);
u-khabil 'fist' (cf. Cauc ***q^w^a̱IpV** 'paw');
u-khai 'forearm' (DC: Sino-Tibetan ***kaj** 'arm', Na-Dene ***qai-** id.).

Other prefixal remnants:

L- in dialectal **la-kain** 'tick' (cf. **a-kaiñ**, above), **li-s-tu** 'saliva', **l-urrin** 'odor';

N- in **in-khatz** 'coal', dialectal **in-gel**, **ne-gel**, **ne-gal** 'frog', **in-har** 'spark', **in-hurri** 'ant'.

In sum, the Basque evidence points to the prior existence of a noun class system, whose phonetic remnants are still sporadically present, and whose semantic significance is at least partially evident. (Other scholars, e.g., Trombetti, Uhlenbeck, Bouda, and Čirikba, have noted aspects of this underlying prefixal system and related it to the Caucasian class system.)

In Caucasian, on the other hand, the noun class system is fully alive (except in some Lezghian languages and the extinct Hurro-Urartian), and is reconstructed as follows:

I.	"rational-masculine"	(prefix:)	***u̯-**	pl. ***w-**
II.	"rational-feminine"		***j-**	***w-**
III.	"irrational-non-collective"		***w-**	***r-**
IV.	"irrational-collective"		***r-**	***r-**

In Proto-East-Caucasian a few nouns could have prefixed class markers, mainly parts of the body and kinship terms. In some languages the prefix marks the class of the noun stem, as was apparently the case in pre-Basque:

Avar **w-as** 'son': **j-as** 'daughter'.

While in others the prefix denotes the class of the possessor, as in Burushaski:

Dargwa **w-aIħ** 'face (of a man)': **r-aIħ** 'face (of a woman): **b-aIħ** 'face (of an animal).
(Diakonoff & Starostin 1986: 10, 71)

Burushaski has a class system very similar to the Caucasian system:

I.a.	"human-masculine"	**i-**	(3p poss.)	pl. **u-**
I.b.	"human-feminine"	**mu-**		**u-**

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II.	"non-human-animate"	**i-**	**u-**
III.	"non-human-inanimate"	**i-**	**i-**

As in Caucasian, "prefix-bearing nouns" are mainly terms for body parts and kinship terms, and also a few words pertaining to social practices. The class markers cited above are third person possessive. When non-possession is expressed, one uses **i-** or **mi-**:

> **i-čhAr** 'a voice' = 'his voice';
> **mi-me** 'a tooth' = 'our tooth'. (Lorimer 1935: I: 14ff, 127ff, 134-39)

When we compare the three families, there can be little doubt that the fossilized class prefixes of Basque must be related to the living class systems of Caucasian and Burushaski. Significant differences have indeed developed during the (five to seven?) millennia the languages have been dispersed, just as grammatical systems have diverged in Indo-European and other language families. The Macro-Caucasian class system was probably very similar to the Caucasian one, with four or more categories based on distinctions such as human-non-human, animate-inanimate, collective-non-collective.

One hesitates to make sweeping comparisons of the markers attested in the three families, since there are important differences. Basque **be-/bi-** (with body parts) is most likely cognate with Caucasian III ***w-** (**b-** in some languages); Basque **e-/i-** may be compared with Caucasian II ***j-** and Burushaski **i-**; Basque **o-/u-** with Caucasian I ***u̯-** and Burushaski **u-** (plural); and Basque **le-/li-** with Caucasian IV ***r-**.

We then come to category (c), phonological correspondences and tendencies. Let me emphasize immediately that sound correspondences, in and of themselves, do not prove the genetic relationship of languages. In fact, diagnostic isoglosses in categories (a) and (b) have already "proved" the relationship before one begins tabulating sound correspondences. However, the latter are useful in providing a scientific control on the body of evidence being developed. To be able to show that the forms in proposed etymologies and paradigms are phonologically related removes the possibility of chance resemblance. (Cf. Greenberg 1987, chapter 1; Fleming 1987: 206.)

The phonological correspondences of Macro-Caucasian are briefly outlined in my earlier article (Bengtson 1990a), and more extensively in a manuscript in preparation (Bengtson 1990b). Despite a few vexing cases, the correspondences are recurrent and regular, just as in Indo-European and other language families.

Beyond this, I will mention some phonological tendencies, which, though they appear to be quite irregular and sporadic, are distinctively Macro-Caucasian. These are <u>metathesis</u> and <u>"labialization of n"</u>:

<u>Metathesis</u>, while infrequent and sporadic in most language families, seems unusually prolific in Macro-Caucasian languages. This is explained, in the case of Proto-East-Caucasian, as due to the large number of distinct phonemes in these languages, so that "the order of phonemes was comparatively less relevant" than in languages having fewer distinct phonemes (Diakonoff & Starostin 1986: 9). Thus, Nikolaev and Starostin assume many metathesized variants in their reconstruction of Proto-Caucasian:

> ***cV̱̄nV /*nV̱̄cV** 'aunt, cousin' (cf. Bur **-nco** 'aunt');
> ***ćamV /*maćV** 'kinsman' (cf. Bsq **seme** 'son', etc.);
> ***sēmhIV /*hIēmsV** 'muscle, sinew, intestine'
>   (cf. Bsq **zaiñ** 'vein, nerve, root');
> ***qʷā̱pV /*pā̱qʷV** 'root';
> ***t'ihʷV /*hʷit'V** 'little'; etc. (NCR)

In the modern languages, forms such as Tindi **t'uḱa ~ ḱ'uta** 'he goat' are in free variation. Likewise in Basque we find variation in the word for liver, generally **gibel**, but **bigel** in Haut Navarre. The cognate word in Caucasian is reconstructed to at least three variants:

> ***ƛ̆'ʷäHälV** (Hatti **taḫala-in**, Urartian **zel-də**);
> ***Häläƛ̆'ʷV** (Lezgi **leq'**);
> ***läHäƛ̆'ʷV** (Chechen **doʕaħ**) 'liver'.

The Basque forms can be accounted for by metathesis or assimilation of the labial element:

> ***ƛ̆'⁽ʷ⁾äHʷälV > gibel;**
> ***Hʷäƛ̆'⁽ʷ⁾älV > bigel.**

(**b** is the regular Basque correspondence to Cauc ***w** or ***Hʷ**, as is Basque **g** to Caucasian ***ƛ̆'⁽ʷ⁾**.) The remote evidence of Na-Dene (Haida) **ƛ̆'ak'ul** 'liver' would point to Basque **gibel**, Hatti **taḫala-** as retaining the older order. In several other cases, the order in Caucasian differs from that in Basque:

> Bsq **hosin** 'well, pool': Cauc ***ʔʷɨ̱̄n(ɨ̱)cV** 'spring, well';
> Bsq **negu** 'winter': Cauc ***Hƛ̆'ʷi̱nī** id.;
> Bsq **hortz** 'tooth': Cauc ***cɨlħV** id. (Bur **-hAše** 'molar');
> Bsq **zahar** 'old': Cauc ***sʷɨrHo /*rɨhɨsʷo** 'old, year';
> Bsq **txahal** 'calf, heifer': Cauc ***ʔIɨć'ʷɨlV** id.

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It is seen that an awareness of the tendency to metathesis in Macro-Caucasian is essential to the recognition of these cognates. Likewise, the other tendency, labialization of n, allows us to recover the following cognate sets:

> Bsq **eme** 'sweet': Cauc *HVnēʔ^{w}V id.;
> Bsq **gune** 'place': Cauc *G^{w}ī̱InhV /*G^{w}ī̱ImhV 'village, house';
> Bsq a-**mets** 'dream': Cauc *Hnić^{w}V /*Hć^{w}inV 'night, dream';
> Bsq **niga**-r 'tear': Cauc *newq'u (Lak **maq'**) 'tear, pus': Bur **nAgei ~ mAgei** 'boil' (pus);
> Bsq ema-**khume** 'woman': Cauc *q^{w}änV id. (Archi **Xom**, Lak **qami** 'women'): Bur **quma** 'concubine';
> Bsq e-**gun** 'day': Cauc *G^{w}em-tV 'day' (24 hours): Bur **gōn** 'dawn', **gun-c** 'day'
> Cauc *Hinis^{w}u 'cheese': Bur **hAmenǯ** id.;
> Cauc *näwšE 'sheep about 2 years old': Bur **mamuši** 'lamb'.

In every case of labialization of **n** we note that there existed a labial element (**u, w**) in the root, which, when transferred to the neighboring **n**, transforms it to **m**. Compare the Caucasian variants:

> *d^{w}ānhV /*dāmhV 'drum'.

Ultimately, the labialization of **n** turns out to be another manifestation of metathesis, the metathesis of a feature (labialization) from one consonant to another in the same root. (As we saw above in the words for 'liver', the same phenomenon can account for Basque **b.**) So both of these phonological tendencies can be viewed as one and the same: the tendency to metathesis in a proto-language abundantly endowed with distinct consonant phonemes. As far as I know, this strong tendency to metathesis is confined to Macro-Caucasian languages, and is relatively unknown in Afroasiatic and other Eurasian language groups.

So while there are undoubtedly lexical parallels between Basque and Afroasiatic, few of them involve the most basic, non-cultural vocabulary, and these can be explained as residue of a long-range relationship between Dene-Caucasian and Afroasiatic. When Basque, Caucasian, and Burushaski are compared, basic isoglosses become much more numerous. Furthermore, the Basque noun case endings and fossilized class prefixes have counterparts only in Caucasian and Burushaski, and the phonological systems of these three language families are interrelated to a degree that clearly distinguishes them from Afroasiatic.

To return again to the Indo-European experience, we have seen that Albanian, after the peeling away of layers of loanwords, was diagnosed as a distinct branch of Indo-European. In the same way we can base the classification of

Basque as Macro-Caucasian on the presence of diagnostic vocabulary and grammar. Reconstruction is not necessary for classification, as Blažek seems to imply in his article. To the contrary, proper classification must precede reconstruction.

Apart from the archaic residue (from the common ancestor of Dene-Caucasian and Afroasiatic), mentioned above, many of the parallels between Basque and Afroasiatic may be ascribed to contact of early Basque with a known or unknown Berber dialect, possibly in southern Iberia. This is indicated by the fact that some of the most exact parallels are "too similar" and therefore probably cultural loans, e.g.:

> Basque **izten, ezten** 'awl': Berber t-**isten**-t id. (Mukarovsky 1969: 33, 36).

On the other hand, the probable remote cognates with basic meanings have diverged much more phonetically and semantically, e.g.:

> Basque **zahar** 'old': Berber **usser** 'to be old';
> Basque **hil** 'moon': Berber ta-**lli**-t 'new moon';
> Basque i-**zar** 'star': Berber ta-**ziri** 'moon' (Mukarovsky 1969: 37, 39; Blažek, to appear, §23).

Facts like these indicate that the primary genetic link between Basque and Berber lies farther back in the past than the more recent contact which generated loanwords. Based on the total multilateral picture, this genetic link dates, as indicated above, to the common linguistic ancestor of Dene-Caucasian and Afroasiatic.

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(a fragment of the paper) BASQUE AND NORTH CAUCASIAN OR AFROASIATIC?
forthcoming in the Festschrift für Hans G. MUKAROVSKY

	Basque	North Caucasian	Afroasiatic
1.	AGORR, IGARR, IHAR "dry"	*ĭGGwVR- '(to) dry'	*K[AW]R- '(to) dry'
2.	AHUNNA 'kid'	*HʷV̄NXV 'sheep, lamb'	*Hʷ-AN- 'sheep'
	A(H)UNTZ 'goat'	*HĪʷƷ̄NXV 'ram'	
3.	ARR 'male'	*HĭRKʷV 'man'	*ʔAR[Y]- 'man, kinsman'
4.	ASTO 'ass'	(W) *ČʷVDə 'ass'	*ʒAT- 'ass'
5.	AZA(GA)RI, AXARI 'fox'	*CʷƷ̄HƷ̄LĔ 'fox'	*CUḤAR- 'wild feline, hyena'
6.	BEL(H)ARR 'forehead'	plwg *BEL 'forehead'	*BAL-/*BAR- 'forehead'
7.	BIGAR, BIHARR 'tomorrow'	*P̄əĔV 'dawn'	*BAKʷ-(AR-) 'morning'
8.	EGUN 'day'	*ʔwɨGGɨNV 'day'	*(Hi)GAN- 'sky; morning'
9.	EGURR 'firewood'	*KARV 'stick, log'	*KAR-/*ḲUR- 'wood, beam'
10.	EZNE 'milk, juice'	*ǮINWV 'milk, udder'	*SIN- 'butter, oil'
11.	GARRATTHOIN 'rat, mouse'	*Qʷ+IRDV 'hedgehog'	*ḲVRD-/*GʷVR(D)- mouse, rat
12.	GOIZ, GOTX, GOX 'morning'	*Q̄QVS̆wV/*Q̄QVĆʷV 'morning, evening'?	*KVS- 'morning, sun'
13.	GOSE 'hungry'	*ḲAŠĪ/GGAŠĪ 'hunger'	*GA(WA)C-/*ḲA(WA)C- hunger
14.	GUTI 'few'	*KOTV 'short'	*K[U]T-/*Ḳ[U]T- 'small, short, few'
15.	HARR(I) 'stone'	*Q̄ƷRQ̄V 'stone'	*ḲAR- 'rock'
16.	HAUR 'child'	*KʷĂRNV 'young of animals'	*Ḳ[U]R- 'child'
17.	HERRO 'root'	*Q̄URĪ 'stalk, root'	*ḴḤWAR- 'root'
18.	HIR(R)I 'city'	*XʷƷ̄IRV 'village, farm'	*ʕYAR- 'city, house'
19.	HOR 'dog'	*XWAR 'dog'	*[HW]AR- 'dog'
20.	HORTZ 'tooth'	*GWARǮI/*[Ḳ]WARǮI 'tooth'	*KʷAR-/*ḲARW- 'tooth'
21.	HUR 'water'	*ħWIRə 'river, lake'	*HVR- 'lake, river, well'
22.	ITZAL 'shadow'	*[ʕVN]ČČʷĪLV 'green, gray, blue'	*ČIL- 'shadow, dark, green'
23.	IZARR 'star'	*ʒWHARʕI 'star'	*ʒUHAR-/*ʒIHAR- 'star, moon (=light)'
24.	KERETZ, GERIZA 'shadow'	*ḲəRV 'black; coal'	*KAR-/*ḲAR- 'black; night'
25.	MARRO 'ram'	*MAIRʒʒV 'sheep'	*MAR- 'sheep, ram'
26.	SAGU, SABU 'mouse'	*CWARĞGWV 'weasel, marten'	*CIG[A]R- 'mouse, rat'
	SAGARROI 'hedgehog'		
27.	SAMIN 'sour'	*ÇWENHV 'salt'	*ÇAM- 'sour'
28.	TXIKI 'little'	(W) *Čəkʷə- 'little, young, boy'	*CIKʷ- 'little'
29.	ZA(HA)R, ZAGAR 'old'	*SWɨRHO 'old'	*ŚWAR- 'old'
30.	ZORROTA 'creek, brook'	*ŠORV 'lake, river'	*SVRY- 'river; to flow'

6) ¡ LA LUTA CONTINUA ! THE NEWS

Juha Janhunen discovers a new Altaic species in Manchuria! The hottest news in town is from Finland. Writing from Helsinki on Feb. 15th, Juha says: "I have been continuing my field work in Manchuria, where I found the previously unknown KHAMNIGAN (my emphasis - HF) ethnic group. They are interesting in that they have kept on speaking two native languages -- mother tongues --for several generations. One of their languages is Tungusic (Evenki, with two dialectal varieties), the other is Khamnigan Mongol. The latter is especially remarkable since it is without doubt the most archaic Mongolic language that survives today. I published a brief description of it under the title "Material on Manchurian Khamnigan Mongol" (Castrenianumin toimitteita 37, Helsinki, 1990), 110 pp. Unfortunately, I have no author's copies left, so I cannot send it to you. Does MT Society (ASLIP - HF) have a library? Anyway, I am going to the Khamnigan again next summer. Maybe in a few years I can get enough material for a bigger dictionary of both of their languages. . . . I have been in contact with colleagues in Moscow and St. Petersburg, and soon I will attend a symposium on the languages of the so-called peoples of the Far North, in SPb." Goodness! Juha, watch it with the place names! Do you think the monarchy is coming back? And our congratulations on your field work. Can you send us a Swadesh list (100 word) for publication? In Khamnigan Mongol?

The second hottest item of news is the conclusions reached by Adrados, concerning ETRUSCAN. As we have mentioned before in earlier issues, Orel and Starostin (in "Etruscan as an Eastern Caucasian Language", in press) have classified Etruscan as Dene-Caucasian (D-C) which is also Bengtson's tentative conclusion. Meantime myself and Dolgopolsky were on record, but most tentatively, saying that Etruscan looked most of all like a relative of Indo-European (IE). Now, according to Bomhard in a new book he and John Kerns are writing, Etruscan has been classified as a part of IE, or at least a part of Nostratic close to IE, not D-C. So saith Adrados 1989, pp.363-383, whose conclusions seem "sober and persuasive" to Bomhard. The key part of what was said is: "Adrados draws the conclusion that Etruscan is an archaic Indo-European language and that it is particularly close to the languages of the Anatolian branch." The Adrados is presumably Francisco R. Adrados of Madrid but the rest of the reference is not at hand. Je vous demande pardon! The news is so hot it hasn't finished cooking!

Volume 264, No.4, April 1991 issue of the SCIENTIFIC AMERICAN mentioned 'us'. Under the more general rubric of "Trends in Linguistics", Philip E. Ross, a staff writer, wrote an article entitled "Hard Words". Its lead or blurb said: "What's in a word? If it's ~~TIK~~, the answer is controversy. Linguists are at each other's throats over attempts to trace language to ancient roots. Some radicals believe that they can discern echoes of words not spoken for millennia and that it is possible to relate all languages to a single tongue spoken by the first humans. Conservatives think the radicals bark up the wrong tree." It is 10 pages long. The discussion is quite sophisticated and includes an up-to-date statement by Philip Lieberman on his views and a mention of his new book, UNIQUELY HUMAN: THE EVOLUTION OF SPEECH, THOUGHT AND SELFLESS BEHAVIOR. 1991. Harvard University Press. There are errors too, e.g., Rebecca Cann is senior author of the key paper on mtDNA, not Allan C. Wilson whose name follows hers in their joint article. Wilson is the scientific leader of their group, of course, and is senior author of a more recent book which they have done together.

On the first two pages there is a massive great chart of human language "families" and "superfamilies". Although it is impossible to reproduce the chart here, impressive though it may be, it is legal to discuss the taxonomy involved. The author is not given but is probably Ross, drawing on Ruhlen and Muscovites. Here we go: (from left of his chart to the right)

Nostratic : Afroasiatic: Egyptian & Chadic, Berber; Semitic
Elamo-Dravidian: (Elamitic, Dravidian languages)
Kartvelian: (Georgian, Mingrelian, Laz; Svan)
E Indo-European: Anatolian; Greek & Tocharian & Iranian + Indic; Armenian & Celtic,
U Albanian, Italic & Germanic & Baltic & Slavic
R Uralic-Yukaghir:
A Altaic:
S Korean:
- - - - - - - - - - - - - - - - - (Nostratic limit) - -
I Japanese:
A Ainu:
T Gilyak :
I Chukchi-Kamchatkan:
C Eskimo-Aleut:

Dene-Caucasian: Na-Dene:
Sino-Caucasian: Sino-Tibetan; Yeniseian; North Caucasian; Hurrian; Urartian; Hattic; Etruscan; Basque

Amerind: North Amerind:
South Amerind:

Indo-Pacific: Andamanese:
Papuan:
Tasmanian:

Australian:

Austric: Daic: (Thai-Kadai)
Austro-Thai: Austronesian:
Tai:
Austro-Asiatic:
Miao-Yao:

Nilo-Saharan: Songhai; Nubian, Nilotic, Kanuri

Niger-Kordofanian: Niger-Congo:
Kordofanian:

Khoisan:

 We've listed the primary superfamilies (super-phyla to me) and the first two levels of sub-classes = families (roughly phyla to me). There are sins of omission to be mentioned as well as internal taxonomies which I believe would be voted down by specialists in the particular fields. I'll mention a few of them too. -30

evaluation

One can see that Nostratic and Eurasiatic are in fact quite different, the latter having no fewer than 5 phyla on the east that Nostratic lacks (in this version but not in Aaron Dolgopolsky's). Afroasiatic is missing two of its six families or sub-phyla, Cushitic and Omotic, which have the greatest internal diversity. Chadic has greater numbers but more closely related languages. Few Afrasianists would agree that Egyptian is closer to Chadic than to Berber or Semitic. I doubt that the Indo-European picture correctly reflects a majority view and some special relationships like Armenian and Celtic, or Greek as a 'satem' language, would be rejected. Neither Sumerian, nor Nahali nor Burushaski show up anywhere. The Sino-Caucasian interior is seriously flawed: Hurrian and Urartian are fairly close and both go in Northeast Caucasian, while Hattic is part of Northwest Caucasian, all this according to Diakonoff and Starostin -- first reported to you in MT-1. Na-Dene as a coordinate half of the great Dene-Caucasian super-phylum is new and interesting. The two old and deep super-phyla, Austric and Nilo-Saharan, are badly reported. Daic, for example, is the same as Thai + kin and its branch is part of Austro-Thai, not a separate entity. Paul Benedict would not agree that Austro-Asiatic is as close as Miao-Yao is to Austro-Thai. The Nilo-Saharan picture ought simply to be rejected; it is awful. The Niger-Kordofanian scheme is out of date, newer and improved internal taxonomy has been reported in earlier issues of MOTHER TONGUE. And finally poor old Khoisan is stuck on the end of the diagram like a pig's tail. Its internal diversity into Hadza, Sandawe, and SAK (South African Khoisan) is very great and ought to be taken more seriously. Its link with the outside is much more likely to be Afrasian than Niger-Congo.

more publicity

An even longer article on 'us' appeared in THE ATLANTIC, Volume 267, no. 4, April 1991, pp. 39-68 or 21 pages of text other than the advertising. Written by Robert Wright, a senior editor of the NEW REPUBLIC, it was featured on the front cover of this magazine of 465,000 circulation (more or less). Thus it was the longest article so far in the 'popular media', yet fewer people were exposed to our great debate than was the case with the US NEWS AND WORLD REPORT (2,210,000) or DISCOVER (1,053,000) or NEWSWEEK (3,180,000) where Rebecca Cann's "EVE" first burst on the national scene. One can hardly say that probably being read by close to 4 million upper middle class Americans (+ unknown numbers of people in the offices of doctors [M.D.] and dentists + students in libraries) means that we are not getting our message across! Now if those few thousand linguists and anthropologists could also be reached! They must go to a doctor or dentist?

On the cover the title was "QUEST FOR THE MOTHER TONGUE". A sub-title was "Is the search for an ancestor of all modern languages sober science or simple romanticism?" On the menu page, underneath a rather good drawing of a benign looking Greenberg, a lead or blurb said: (Start of quote). "Vitaly Shevoroshkin does not look like a guerrilla leader. 'A more plausible guess as to his occupation,' the author writes, 'would be: leader of a philosophical school whose upshot is unmitigated despair.' But Shevoroshkin is largely responsible for an insurrection in the world of comparative linguistics. Unlike most of his colleagues, he believes that all the great language families are descended from a common tongue. And he believes that parts of this tongue can be reconstructed." End of quote.

One more blurb came on page 39, just before the title and the main text. It said: "The story behind the search for 'proto-World,' a primeval language that most linguists believe will never be found, that many believe never existed, but that some say they're already piecing together" End of quoting.

The article was quite rich and extremely provocative and on the whole laid out the issues with reasonable clarity. Eric Hamp played the role of principal opponent and defender of the faith. Whether our colleagues will now become household names in America remains to be seen. The only thing which might exceed the magazine publicity could be the special on BBC TV which reporter Bettina Lerner was working on a few months ago. It may have come out already for all I know or it may have been blown away by the Persian Gulf war. Some of us have written letters to the Editor of THE ATLANTIC to vent our biles at the author for reasons which are obvious once you read the thing.

A little reality testing is useful from time to time. When you hear statements in the press, or from colleagues, that 'most linguists' disapprove of Greenberg or Shevoroshkin, or that 'almost all' linguists stand squarely behind the comparative method as understood by Ives Goddard and Eric Hamp, please try to remember that it ain't so! 'Most linguists' don't do historical studies. They are not interested. This is the era of description and theory, remember? The TG people, for example, could not care less about the great war in a coffee cup between the lumpers and the splitters. My informants say the numbers of us combatants are down around 4% or 5% of an average linguistics department.

- - - - - - - - - - - - N i l o - S a h a r a n - - -

M. Lionel Bender (Carbondale) and Franz Rottland (Bayreuth) have edited a new book on Nilo-Saharan. The particulars can be found in Bender's first letter to me (under LETTERS). Even with the publication of this valuable book, Nilo-Saharan will still be what some call "a literature poor phylum". But things are improving slowly. Rottland has been co-editing a newsletter for N-S for several years now. This is the third book Bender has co-edited on N-S. A good proto-Nilotic, with all the Cushitic borrowings wrung out, is approaching. Norbert Cyffer (Mainz) is working hard on Saharan along with Thilo Schadeberg (part time). Koman is getting more field work; Bender has done reconstruction on it. Chris Ehret believes he has reconstructed proto-N-S but his colleagues remain quite sceptical. And so forth. N-S is a 'family' that still needs a great deal of field work. If you have heard about the present dreadful conditions in the Republic of the Sudan, you can understand why more data are not forthcoming.

+ + + + + o l d A r y a n s + + + + + +

J.P. Mallory wrote an book in 1989 which several long rangers have mentioned to me. I have not seen it yet but it comes highly recommended. First thanks to James Egan, followed by many others. The title is: IN SEARCH OF THE INDO-EUROPEANS: LANGUAGE, ARCHEOLOGY AND MYTH. Thames and Hudson. Some see it as an acceptable alternative to Colin Renfrew's work; others see it as a great improvement. I'll compare it to Anthony's work.

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Ekkehard Wolff agrees to be European distributor! We have been much helped by Ekkehard Wolff's agreement to become the European distributor for MOTHER TONGUE. He has volunteered to copy and mail out each issue; to collect dues in European currencies from European colleagues; and to remind recalcitrant members that they are supposed to pay their dues. He will make it possible for those members who hate paying $16 for dues and another $20 in banking charges to stay with us at 1/3 the cost! I fervently hope that Ekkehard's kindness has given the European side of the old Long Range Comparison Club a new lease on life! I like honesty and candor. Europeans by and large prefer politeness and so don't say why they drift away from us. Let us hope that their reasons have not been intellectual rejection, but only $$.

. . . . C U S H I T I C . . . . .

Linda Arvanites has finished a noteworthy dissertation on proto-East Cushitic (a sub-branch of Cushitic of Afrasian), entitled THE GLOTTALIC PHONEMES OF PROTO EAST CUSHITIC. At the University of California at Los Angeles, 1991. It is 244 pages long, including 7 pages of reconstructed forms in Appendix Three, and should be a definitive work for some time to come. Our congratulations to her. For those who may wish more information her address is: Dr. Linda Arvanites, 2328 3rd Street,no.15, Santa Monica, CA 90405, USA. The East Cushites include such warlike pastoral peoples as the Somali, Afar (Danakil), and Oromo (Galla). As is my custom, I estimate that proto-East Cushitic is as old as PIE and p-Semitic, if not older. Linda's undertaking was therefore no trivial pursuit.

Her work can be added to the list of major contributions to the reconstruction of old Cushitic, from Appleyard to Zaborski. Also Tilahun Gamta's Oromo dictionary has finally been published (U/Addis Ababa Press, Addis Ababa, Ethiopia), Harry Stroomer's thesis (on Oromo dialects) may be too, and Gene Gragg quietly builds his mountain of comparative Cushitic morphemes. Now comparativists should dig deeply into the wealth of Cushitic reconstructions, as they should begin to take seriously the growth of proto-Niger-Congo.

Mary Ritchie Key has received an award from The Rolex Awards for Enterprise - 1990 for her project: "Computerizing the languages of the world" and has embarked on her labors to produce dictionaries a la Carl Darling Buck on all South American, Asian, and African languages.

Two sample pages from a draft of one dictionary will appear in MT-14. It is a list of some Amerind languages of Latin America and their words from 'hand'. She would dearly love to have some help from colleagues in finding experts on various groups of languages and in finding native speakers. Her project is going to cover most of the world; those inclined to work on global etymologies ought to get in touch with her. Any non-Americanists who feel inclined to check out the Amerind hypothesis on their own ought also to contact her at: Program in Linguistics, U/California, Irvine, CA 92717, USA.

Congratulations on your award, Mary! We hope that you stimulate many people to help with the work. If there is one thing that Americanists and Africanists can agree on, it is the the value of recording any human language or culture before it dies out. The news in the press about Amazonia suggests that soon many more of our cousins in Brazil and Venezuela will become endangered species too. Like Ongota or (Semitic) Mesmes which just died out in 1990.

. . . . fox among the chickens . . . .

One of the brightest of the younger Muscovites, Alexandra Yu. Aihenvald, has moved to Brazil. While this is old news to some of you, we all should be pleased that our skimpy coverage of South America will be improved. Having a friendly Muscovite present in South America will be akin to getting a friend into Fort Knox --all that gold and someone who will share with us! And to think she almost chose New York! Now Mary Key will not have to stand alone as long ranger extraordinaire in South America. Great! We wish Alexandra and her husband, J.P. Angenot, well. She/they have become very long rangers. Their proposal of a Noscau (NOstratic, Sino-Caucasian, AUstric) and Amerind relation is in my possession but, temporarily, I don't know where. Perhaps Allan or Mark will be able to incorporate it into their editions. Sooner or later we will get it out. Unhappily, I suspect it will go 'too far' for most of us. But they offer a lot of evidence! Alexandra Aihenvald, C.P. 5009, Campus da Trinidade, 88049 Florianópolis (SC), Brazil. Write to her!

> > > > > > > > E u r a s i a t i c and A m e r i n d < < < < < <

Joseph Greenberg in a telephone call, when asked the "how are we doing?" question, replied that "I am winning -- because of the 'hard science' data and analyses which are supporting my Amerind hypothesis". On the subject of Eurasiatic, his forthcoming hypothesis which basically = eastern Nostratic, his opinion at the moment is that "it contains Amerind as a branch or Amerind is coordinate to it". Since he had not read MT-12 yet, he had no opinion on the Dene-Caucasic and Vasco-Dene hypotheses. His views on purported Indo-European traditions and the real history of that crucial phylum have come out before but also are given in detail in the REVIEW OF ARCHEOLOGY. We will summarize much of that in MT-14.

(non-News, HF comment) It strikes me that much of the presumed tradition in historical linguistics is due to how and where and by whom it is taught. The emphasis seems to be such that detailed fussing gains browny points and honest scientific ventures (hypotheses) are shunned as speculation. With all the emphasis on IE examples and verities in introductory textbooks and in all the recent magazine articles on 'Mother Tongue' -- maybe everybody has forgotten that IE represents circa five percent of the world's languages? Perhaps the other 4750 languages in the world have something to teach us too? Historical linguistics stands on the verge of being the most parochial of all the sciences, unless it can lose its extreme Eurocentrism.

\- - - - - - now wait a minute! - - - - - - -

And yet I have the opinions of two editors that my remarks are out of date. They think that "half of all IE-ists" or "most IE-ists" would agree that IE is related, repeat IS related, to some outside = non-IE language. One of them would agree with me, and Hegedüs Irén's bibliography, that Uralic is the most likely next of kin to IE -- not Semitic, not Kartvelian.

. . . . . . a might- have- been meeting . . . .

Allan Bomhard was a featured speaker at the annual meetings of the ILA (International Linguistic Association) in New York in April. Some of the points he made in his speech will be reported in MT-14. Also heard to have been at that meeting were Winfred Lehmann, Roger Wescott, Sheila Embleton, Saul Levin, Maria Gimbutas, John Costello, Robert Austerlitz, Ernst Pulgram, et al; the last two strenuously opposed to Nostratic, the penultimate pair neutral.

, , , , , , , , not good news , , , , , , , ,

Few European members ever write; Israelis rarely write and Soviets almost never nowadays. Maybe Ekkehard Wolff has solved one problem and the rest are hung up on the severe troubles of their societies. But this is a NEWSLETTER and we need news!

7) L E T T E R S.

Some comments on the letters. Private stuff has been edited out, but not all personal stuff, e.g., not when relevent or apt. One long exchange was not published because one party wanted privacy but some points of it can be found in the Editorial.

. . . . guerrilla tactics . . . .

Peter Unseth writes from Ethiopia. He is a good fellow to write to (Box 6779, Addis Ababa, Ethiopia) if you want rich new field data on Surma (N-S) languages. Wishing us encouragement, he says: "... so I'm forced to sit back and watch the great debate as a spectator who knows the rules... I hope you pursue the matter to the limit. I suspect you will ultimately be successful (I may not get there, but my children will; I have a dream!), but even if you're not, you will have forced a total reconsideration of the entire current paradigm, ALWAYS a healthy stretch of the brain. You be the Long Ranger, I'll be a less helpful Tonto and cheer you on. .. (paragraph skipped).. You long rangers, if feeling squeezed out of the mainstream publications need to blitz conferences with small bits of the overall picture. That is, in a 20 minute paper you can only communicate a small integrated bit. Try the back door, also, submitting reviews of books on the subject. Slowly raise the LSA's consciousness."

. . . . ping pong . . . . ,

Shevoroshkin and Bomhard: The Finale.

The debate between Vitalij Shevoroshkin and Allan Bomhard re-appears herein but in its final act, as far as this editor is concerned.

Complaints of 'factionalism' sullying the bright face of our newsletter or simple fatigue with this particular debate have appeared. There are real issues involved in the debate, aside from the personal antipathies, but I would prefer that others take up the argument lest we all be distracted endlessly by the personal.

ON B/OMHARD/'s REVIEW of TYPOLOGY ... (MT, issue 10)

In his review, B is rejecting some obvious (or, for that matter, very plausible) data without giving any reason for this rejection. He sais, for instance, that "one runs into roadblocks at every turn" if one tries to arrive at "Shevoroshkin's revised PIE system". But, in reality, this system (PIE Th - T -D, instead of the traditional T - D - Dh), adopted, independently of me, by Dolgopolsky, Starostin, Kaiser, Griffen and many others, represents the most natural, the simplest, interpretation of known data in a way which does not lead to "roadblocks" to which both the traditional (T - D - Dh) and the "glottalic" (T/Th - T' - D/Dh) designs inevitably lead. The said system is a "concrete manifestation" of a sytem $\underline{T}$ - T - D ($\underline{T}$ = fortes stops) proposed years ago by Rasmussen. This system is identical to that of Proto-Armenian, and to the early stage of Proto-Germanic (cf. Griffen "Nostr. and Germano-Europ." in General Linguistics 29, 3, 1989, 139 sqq.), and, it seems, to Proto-Anatolian (Hittite-Luwian) as well. It fully fits the Proto-Altaic system of stops (first identified by Illich-Svitych /= IS/ and recently confirmed by Starostin in his brilliant book on Altaic and Japanese). Starostin even thinks now that the Nostratic system was not T' - T - D but Th - T - D (see his paper "Nostr. and Sino-Caucasian" in Explorations in Language Macro-Families, Bochum, Brockmeyer, 1989, p.42 sqq.) which I doubt. (Starostin thinks that Kartv. T' originated, under the influence of North-Caucas., from Nostr. Th /= traditional Nostr. T'/). - Note that the early IE system Th - T - D (or $\underline{T}$ - T - D, for that matter) was unstable and tended to shift (hence most systems in IE daughter languages); in any case, such system (and certainly not Th - T' - Dh or the like) has a strong confirmation in the shape of IE words borrowed from Proto-Semitic (see IS's paper in Problemy indoevropejskogo jazykoznanija, Moscow 1964) and from North-Caucasian (see Starostin's paper in Drevnij Vostok, Moscow 1988).

Now a few words about Laryngeals. B cites (note 2 on p..2 of his review) as an "excellent servey of the Laryngeal Theory" a hopelessly obsolete work by Lindeman. One may look, for the present-day state of research, at H.Eichner's excellent studies: they are fully supported by many comparatists, among them M.Mayrhofer (see his part of the Indogermanische Grammatik). Going "from Anatolian" Eichner arrived to the same conclusion to which Kaiser and myself (and, independently, Dolgopolsky) had arrived somewhat earlier, - namely, to a simple system of IE "laryngeals" *X (Eichner's $*h_2$) and *H: the former stays in Hitt., Luw. ḫ, ḫḫ; the latter disappears in Hittite and Luw. In both cases (*X; *H) we may deal not with just one laryngeal (stable versus unstable) but with a group. (For instance, *x and *ɣ might represent the "group *X": see below).

As for Nostr. sources of IE laryngeals, we operate with several consonants some of which were identified by Dolgopolsky in 1972. Nostr. uvular stops *q and *ɢ (= voiced counterpart of *q), fricatives *x, *ɣ (rather uvular than velar) and laryngeal *h yield, in IE, the stable *X. As for Nostr. pharyngeals *ḥ, *ʕ and the glottal stop *ʔ, they turn, in IE, to the unstable *H. If we accept the thesis about Armanian h as originating from IE *X, we get the following distribution: IE *X from Nostr. voiceless *q, *x, indeed, becomes h- in Armenian, but IE *X from Nostr. voiced *ɢ, *ɣ disappears in Armenian. This leads to the only possible conclusion: there were IE *x (from Nostr. voiceless consonants), and IE *ɣ, from voiced consonants. See, for details: M.Kaiser and V.S. in the Journal of IE Studies (JIES), 1985 (see also "Nostratic" in Annual Rev. of Anthr. 17, p.309 sqq.; V.S. "On Laryngeals" in the recent volume on the Laryngaltheorie edited by Bammesberger).

Now let us look through B's ADDENDUM to his above review (p. 10 sqq.). Despite a devastating criticism (see some data in my note in MT 9: Dolgopolsky's, Helimsky's, Starostin's critical remarks are quoted) B considers it appropriate to send out lists of comparisons which contain grave mistakes. Let me cite just one of B's sets, namely, F (on p.16). B dismembers here Kartv. root *m-k'erd- 'chest' (sic) in comparing it with (quite a different !) Kartv. root *k'ar-/*k'r- 'to bind together' - can anybody accept this ?! But this "enables" B, using his aprioristic approach, to compare the above words with IE "*k'er- (etc.) 'to turn, to bend' ⟨etc⟩": even if this comparison *were* phonetically correct, nobody else would dare to accept it because of semantic incongruency. - In mixing together several different roots, B deliberately drops IS's and Dolg.'s excellent comparison: Kartv. *m-k'erd- 'chest' : IE *ḱerd- (from *ḱerdh-) 'heart'. This comparison, as Bl/ažek/ correctly states it in a letter to me, is now confirmed by Aftro-Asiatic data: E.Cush.: Gid. kard 'belly', N.Omot.: k'irt'a 'brest', W.Chad.: Hausa k'irǯi 'bosom' (etc.). - B does not understand that IE *ǵ, *g (in trad. transcription; his "*k'") originates from Nostr. *k (and not *k') and corresponds Kartv. and AfAs. *k (and not *k'). On the other hand, Nostr. (and Kartv., and AfAs.) *k' corresponds IE *k.

Since I don't want to repeat known criticism, I am going to use Bl's data from the above letter:

B's example A (on p.15): Again, a "set" which is wrong both phonetically and semantically. There is a Nostr. root *t'VrpV 'get sated' etc.; hence IE *terp- 'rejoice'. B drops the IE root (because it has *t- and not *d-, i.e. "*t'-") but uses related roots of other daughter languages (dismembering all of them), adding a dismembered IE "t'ar-p⁽ʰ⁾- 'to tear, to rend, to pluck' (he bases on Gr. drépō 'pluck' but distorts the meaning and form of IE *derp-/*drep-). B mixes together the meanings 'tear'; 'pluck'; 'satisfaction'. All this to make work his "rule".Wrong reconstr.

B's set C (p.16): B dismembers a Kartv. root, getting Kartv. "*k'er-b-, *k̄'r-eb- 'to gather'", in order to be able to tie it to IE and Drav. roots with *-r- (but without a cluster). Bl correctly indicates that Kartv. *k'erb-/*k'reb-, *k'rep- has a good cognate in IE *kerp- 'gather fruit(s)' (see IS's Dict.,no. 206). As for IE *ger- 'gather' (used by B), it fits a different root, e.g., in Uralic: Fin.-Perm. *kerV 'gather'. B rejects the obvious connection and postulate an improbable link in order to make roots fit his aprioristic "rule" of sound correspondences (Kartv. *k' : IE *g).

(kavuḷ) B's set E (p. 16): IE "*k'ap⁽ʰ⁾-'jaw' (etc"; Georgian nik'api 'jaw'; Drav.: Tamil 'cheek' etc. Bl indicates that IE *geb- 'mouth, jaw' has a cognate in Tungus *kep-, whereas Drav. *kav-uḷ 'cheek, jaw' exactly fits Tungus *käwä 'jaw' (see IS's Dict. no. 160). As for B, he mixed different roots to fit his faulty "rule".

B's set F was discussed above.

B's set H (p. 16). Again a mixture of different roots: IE "*k'ew- (etc.)" 'to make a round hole in"; Kartv. "k'w-er-,*k'w-al- 'round'" (a dismambering, to make a comparison possible); AfAs. "*k'aw-/*k'ə w- 'make a round hole in'". This latter root (cf. also Burji k'aw-a 'a hole') originates from Nostr. *K'ajwV 'dig' (j=y). As for the Kartv. root, it originates from Nostr. *k'ol'V 'round' (see IS's Dict., nos. 209 and 202, respectively). Now for the IE. B simply invented this "root" in mixing two, genetically different, words: Greek gúpē 'hole' (from Nostr. *gop'a, see IS's Dict. no 87) and Greek gūrós 'round'. All this heavy mixture to make things "fit".

B's set J (p. 17): As usually, B makes no distinction between borrowings and inherited words. Bl correctly sais that IE *gʷrān, *gʷrn- (B's "*k'ʷerAn-" etc.) is a Semitic loan (see IS's paper in Problemy IE jazykoznanija M. 1964). - etc., etc., etc.

V.Shevoroshkin

REPLY TO SHEVOROSHKIN'S COMMENTS ON THE "ADDENDUM" TO BOMHARD'S REVIEW OF *TYPOLOGY, RELATIONSHIP, AND TIME*

Allan R. Bomhard

In the first paragraph of his comments on my "Addendum", Shevoroshkin totally misunderstands what I said in my review concerning his ideas on Proto-Indo-European consonantism. Therefore, I will give the complete citation:

> Shevoroshkin's ideas concerning Proto-Indo-European consonantism are not all that different from the proposals made my Joseph Emonds (1972). Where he runs into trouble is in trying to derive his revised system from Proto-Nostratic. One would like to know how the glottalized series became voiceless aspirates in Proto-Indo-European without merging with the plain voiceless stops somewhere along the way. When one tries to work through various scenarios to arrive at Shevoroshkin's revised Proto-Indo-European system from its alleged Proto-Nostratic antecedent, one runs into roadblocks at every turn. In other words, you cannot get there from here.

There is absolutely nothing wrong with the reconstruction of the Proto-Indo-European stop system as $Th \sim T \sim D$ instead of the traditional $T \sim D \sim Dh$, and I never said that there was. What I said was: **"Where he runs into trouble is in trying to derive his revised system from Proto-Nostratic"**, and, in a footnote, I proposed a more natural derivation by pointing to the developments in a Neo-Aramaic dialect. Since Shevoroshkin is obviously avoiding the real issue, I challenge him to address it.

In the second and third paragraphs, Shevoroshkin remarks on the laryngeals. Now, let us look again at what I said in my review:

> On the surface, Shevoroshkin's theories concerning "strong" laryngeals and "weak" laryngeals in Proto-Indo-European appear intriguing. The problem is that the data do not fit the theory.
>
> In order to be able to judge Shevoroshkin's theories concerning whether or not laryngeals changed the quality of contiguous vowels, one would have to know what phonetic properties he would assign to the laryngeals he posits. As long as he operates with cover symbols and employs ambiguous terminology, it is not possible to form an opinion one way or the other about the validity of his proposals.

My first critique concerning Shevoroshkin's ideas on the laryngeals was that "...the data do not fit the theory". Of the other Nostratic branches, only Afroasiatic has a full set of laryngeals. Though six laryngeals are traditionally reconstructed for Proto-Semitic, it seems that only four (namely, a glottal stop, a voiceless laryngeal fricative, and voiced and voiceless pharyngeal fricatives) are to be reconstructed for Proto-Afroasiatic, as noted by David Cohen, among others.

Extremely good correspondences, for a large corpus of lexical material, can be established between the laryngeals reconstructed for Proto-Indo-European on the one hand and for those reconstructed for Proto-Afroasiatic on the other. These data lend no support to Shevoroshkin's theories on the laryngeals. Characteristic of all of Shevoroshkin's work, including his papers cited in his comments, is his "appeal to authority". He seldom takes the trouble to back up his claims with concrete data of sufficient quantity to provide a means to verify whether or not there is any validity to what he is saying. In order for any theory to be convincing, the full data upon which the theory is based must be supplied. One must also be careful -- and this was my second point above -- to give clear, detailed explanations concerning the phonetics involved and the diachronic processes (that is, the rules) leading from the source phonemes to what is actually attested or assumed to have existed.

More serious, though merely a rehashing of the line of argumentation employed in his previous papers, is Shevoroshkin's critique of the individual etymologies. Now let us be perfectly honest -- when there are conflicting forms such as Proto-Indo-European **ker-* versus Proto-Indo-European **ger-*, for example, both meaning "to gather", how do we decide which goes better with Proto-Finno-Permian **kerV* "to gather", since Uralic does not have a voicing contrast in stops? It really is not easy. Even if we pile up etymology after etymology, as both Illič-Svityč and I have done, there are bound to be uncertainties and conflicts due to the nature of the material. Shevoroshkin gets around this dilemma, again, by appealing to authority and by being abusive to those who disagree with him. This approach does not win converts but only serves to alienate people. Rather, it is through rigorous adherence to proven methodologies and through supporting one's views with voluminous data, which display regular, consistent sound-meaning correspondences that one gets one's work accepted by one's peers.

The second point I would like to make is that we must be very careful about semantic plausibility. If there is not a one-to-one semantic correspondence, then we must be able to derive the proposed cognates from the postulated ancestor form by widely-attested semantic shifts and not by mere speculation. That is why I give a reference to Buck's *A Dictionary of Selected Synonyms in the Principal Indo-European Languages* at the end of each etymology in my forthcoming co-authored book. I had expected that people would look up the entry cited and would get an idea of how I arrived at my proposals. Unfortunately, I have seen that this is not happening, so I have started to give brief explanations within the etymologies. Now, when we apply this approach to Illič-Svityč's etymologies, a good number of them do not hold up. For example, looking at Buck (1949:4.40, 4.41), we find that "[t]he chief semantic source [of 'breast'] is the notion of 'curved shape, swelling'". To be sure, there are some other sources of "breast" as well, but there is never an overlap in the Indo-European material cited by Buck (1949:4.44) with the words for "heart", which "...may be used for the 'middle, center' and such various emotions as 'courage', 'love', 'anger', etc." For parallels, it many be noted that similar semantic development is found in Arabic *qalb* "heart; middle, center, core"; in East Cushitic: Burji *wodán-a* "heart" versus Konso *otan-ta* "center"; in Finnish *sydän* "heart, pith, kernel, core"; and in Chinese (Mandarin) *xīn* "heart; mind, feeling, intention; center, core". Given this, I think that an extremely good case can be made for deriving Proto-Kartvelian **m-k'erd-* "breast" from a meaning "curved shape, swelling". "Curved shape", in turn, can be derived from the notion "to twist, to tie, to bend" -- thus, for example, Lithuanian *krūtìs* "(woman's) breast" comes from the notion "curved, bent" < Proto-Indo-European **kreu-* "to curve" < **(s)ker-*, **(s)kereu-* "to turn, to twist" (cf. Pokorny 1959:624 and 935-38). For non-Indo-European semantic parallels, we may note Dravidian: Telugu *gubba* "knob, protuberance, woman's breast", related to, for instance, Kannada *gubāru* "swelling", *gubbi* "knob, protuberance". Consequently, Shevoroshkin's comment in the first paragraph on page 2 "can anybody accept this?!" has to be answered in the affirmative. Either that or we have to throw out nearly two centuries of painstaking work in Indo-European. I really believe that he does not have a good grasp of the types of semantic change that can actually occur in language. (By the way, Gidole *kard* "belly", cited by Shevoroshkin, cannot possibly belong here.)

The third and final point that I would like to make concerns his criticism about how I "dismember" the forms I deal with. There are some basic theoretical points that need to be made clear here. Comparison of the various Nostratic daughter languages, especially Proto-Indo-European, Proto-Kartvelian, and Proto-Afroasiatic, indicates that the rules governing the structural patterning of roots and stems in Proto-Nostratic were most likely as follows:

A. There were no initial vowels in Proto-Nostratic. Therefore, every root began with a consonant.
B. Originally, there were no initial consonant clusters either. Consequently, every root began with one and only one consonant. Medial clusters were permitted, however.
C. Two basic syllable types existed: (A) **CV* and (B) **CVC*, where *C* = any non-syllabic and *V* = any vowel. Permissible **root** forms coincided exactly with these two syllable types.
D. An inflectional **stem** could either be identical with a root or it could consist of a root plus a single derivational morpheme added as a suffix to the root: **CVC-VC-*. Any consonant could serve as a suffix.
E. A stem could thus assume any one of the following shapes: (A) **CV-*, (B) **CVC-*, (C) **CVC-VC-* (**CVC-C-* as well, before vowels), or (D) **CVC-CVC-*. As in Proto-Altaic, the undifferentiated stems were real forms in themselves and could be used without additional suffixes.

The original root structure patterning was preserved longer in Proto-Indo-European, Proto-Kartvelian, and Proto-Afroasiatic than in the other branches. The root structure constraints found in Proto-Indo-European were an innovation. Both the Proto-Dravidian and the Proto-Altaic root structure patterning can easily be derived from the above system. I assume that the Proto-Uralic rule that all words have to end in a vowel was an innovation. It should be mentioned that reduplication was a widespread phenomenon. On the basis of the evidence of Proto-Indo-European, Proto-Kartvelian, Proto-Afroasiatic, Proto-Dravidian, and Proto-Altaic, it may be assumed that there were three fundamental stem types: (A) verbal stems, (B) nominal and adjectival stems, and (C) pronominal and indeclinable stems. Uralic stands apart in showing no differentiation between verbal and nominal stems. In Sumerian, though nominal and verbal roots were identical in **form**, three separate word classes were distinguished: (A) nouns, (B) verbs, and (C) adjectives. In Proto-Nostratic, only pronominal and indeclinable stems could end in a vowel. Verbal and nominal stems, on the other hand, had to end in a consonant. This is all very, very important, because Illič-Svityč operates (and by implication, Shevoroshkin) according to quite different assumptions. I segment stems according to the above principles, while he does not. He assumes that all stems ended in a vowel, while I do not (though I do assume that vowels could serve as grammatical markers). My vision of Nostratic is based more upon Indo-European, Kartvelian, and Afroasiatic, while Illič-Svityč's is based heavily upon Uralic. Shevoroshkin never takes into consideration these fundamental differences in approach.

-40 -40

A. Murtonen
c/o Post Office
Tallarook, Vic. 3659
Australia

16 February 1991

Professor Harold C. Fleming
5240 Forbes Avenue
Pittsburgh, PA 15217
USA

Dear Hal,

Welcome back from Addis Ababa. I had to cut my planned round-the-world trip short and return here for family health reasons a year ago already.

Regarding your comments on my comments I just want to remark that I do not mean that Kulturwörter and other wandering words cannot be cognate - after all, there are loans even between different dialects of the same language. I only mean that their relevance to "family tree" comparisons is uncertain, and in the case of most Kulturwörter, prehistoric origin highly unlikely, unless shift in meaning be at least plausible, as cultural origins are not far removed from the threshold of history, apart from some elementary tools etc. The same goes for numerals - they presuppose material culture and commerce which make accurate distinction between different amounts necessary; in Australia, it is not just reconstructed proto-languages, but many still actually spoken languages which do not have numerals beyond 2 or 3 - the latter often means also "a few"; nowadays, when accuracy is required, "four" may be expressed "two-two", and "five" = "three-two" or "two-two-one", but usually they are subsumed under "many/all", or English numerals used, as for larger amounts. It is a matter of linguistic economy - new words are created only when they are needed, and the same goes for grammatical categories - plural is sometimes expressed by an element meaning "some" or the like, but usually not formally distinguished from singular - in the personal and demonstrative pronoun, what is called plural (and dual, in some persons) is really collective. I know I am at odds on some points with the Australian establishment and therefore cannot get my paper, "Pintupi etymology" published here, so I enclose a copy for further information and, if you think it suitable, publication as a supplement to MT. I do not think proto-Semito-Hamitic had a word for 4 either - irregularities in phonetic correspondences betray its nature as a Kulturwort borrowed through unusual channels - as plausible in connection with commerce - in all the non-Semitic attestations, etymological considerations even in most Semitic ones.

With best wishes,

All yours,

[signature]

 Winfred Lehmann, writing from Austin, Texas., suggests that: "As you know, my approach is to move back from the attested languages to their proto-forms. You may or may not be interested in that work for Indo-European. In Proto-Indo-European Phonology of 1952 I proposed earlier stages of the vowel system. This last year I tried to align that attempt with earlier syntactic systems, especially the active stage that Soviet linguistics have proposed. I am including a xerox of that article. If you do not have PIEP and would like a copy, I'd be happy to send you one."

"So much of the prehistoric work has dealt with the lexicon, and by individual items. I hope that more is done with the syntax. I'd also like to see efforts made with sets, such as kinship terms. I've written Georgij Klimov to that effect; as far as I know no one has done much with the kinship system terms in Kartvelian, or in Afrasian or the other languages that are assumed to belong to the Nostratic group. We've just begun to deal with some of the active residues in Proto-Indo-European, and further work would be of great benefit there."

"In your dealing with current publications I think you would want to deal with the essays in Philip Baldi's (edited) book: LINGUISTIC CHANGE AND RECONSTRUCTION METHODOLOGY (Berlin: Mouton de Gruyter, 1990. 752 pp.). Like other European books it is hideously expensive. Allan (Bomhard - HF) has the article on Nostratic in it, and might discuss the book in a coming issue of MOTHER TONGUE. I've written a review for GENERAL LINGUISTICS."

"In this connection I wonder whether the Association might not try for discounts for its members with Mouton de Gruyter, as the society for indigenous languages has done. The discounts are noteworthy and make the books more attractive."

"Last I might mention that for further fellows you might keep in mind Klimov and especially Ivan (Igor - HF) Diakonoff. ..."

Interrupting his series, I should mention that the Council of Fellows was elected by the members in a mail ballot (see ASLIP business, below). Diakonoff missed election by only one vote, as I recall. That I regret. Georgij Klimov is not a member of ASLIP, so he's ineligible. Having asked him repeatedly to join us and being spurned for my efforts, I cannot say I regret our By-Laws.

Winfred Lehmann continues (later): "... I want to include information on articles that you may not include in your scanning of the journals because of their location. The current issue of ANTIQUITY, 65 (1991) 39-48, has an article called "The archeology of language origins -- a review". It really deals with the beginnings, and I think you would like to scan it ... With the journal came a blurb on another article that apparently isn't yet printed. It's by the highly regarded archeologist Colin Renfrew, and has the title: "Before Babel: Speculations on the origins of linguistic diversity." It's to appear in the CAMBRIDGE ARCHAEOLOGICAL JOURNAL, which I don't get...". We thank you for this potentially valuable pair of references!

<<<<<<<< mighty nice of you >>>>>>>>

Ruth Bradley Holmes from Oklahoma gives a word of encouragement. As one of several Amerind long rangers, herself a Cherokee, she says:

"Of course it is NEVER my intention to quit - you're a window on the future of all of us!". Bless you, Ruth!

. . . . a Fellow from Indiana . . . .

Carleton Hodge sent best wishes and thanked everyone for electing him a Fellow. He also wrote that: "In '89 I gave a paper on AAs (Afrasian - HF) or LL, to be more precise, taking 30 of Bender's Omotic items and showing cognates. I've revised it and sent it to Prague for the Petráchek volume." I don't know the particulars of the book and my computer still cannot do Czech!

. . . . s e a w e e d w o u l d d o . . . .

W. Wilfried Schuhmacher, writing from Denmark, reports on Easter Island, noting that: "There have also been mythological references of Polynesian trips to Antarctic waters. So I hope I can send one of these months my paper on the Polynesian words for "ice". One thing that has surprised me to find on Easter Island (and in Hawai'i) is the existence of old sledding courses -- on grass, downhill. I think Man would only (be-HF) able to "invent" such a craft in a snow/ice environment. Maybe you have seen in Africa also such a sport without snow/ice." Well, I cannot recall. But it reminds me of the old Cape Cod sea chanty: "Cape Cod boys, they have no sleds, [Chorus], They slide down hill on cod fish heads." Wilfried enclosed an interesting small article he did on Easter Island toponyms for BEITRAGE ZUR NAMENFORSCHUNG, Band 25 (1990) Heft 2, pp149-152, entitled "Nomina montium Paschalis. The Name Shift". One interesting point he makes is that: "The Easter Island place names, apparently, all are of Polynesian origin, i.e., there is no indication of a (South American) substratum. Only names of Polynesian origin therefore have later been replaced by other (Polynesian) names." So there will be no joy in Norway this day. sorry, Thor!

. . . . o v e r b o o k e d . . . .

Paul Benedict writes several times with good suggestions. Since there is too much substance to summarize briefly, we'll hold it for a later issue. James Egan with a new idea, Roger Blench with more to report, Wolfgang Schenkel whose letter's regained. Later in the year!

. . . . c a t a s t r o p h i s m . . . .

Josephine Silvestro writes from New Boston, New Hampshire. She has retired from her work in medicine. She says: "I have just read the article in April's ATLANTIC on MOTHER TONGUE. We are sure getting publicity. My research leads me to believe that language began in the Middle East 8,000 years ago when a comet hit the Rift Valley. It was a syllabic language. When people migrated, the language differentiated. The purest form remained in Egypt which had the most advanced and stable civilization --until 3,000 BC when a comet hit the Mediterranean area and the Egyptian diaspora began. The Egyptians went to Sicily, Spain, the British Isles, North America (Hopi), to southern India and to China." Hmm, Josephine, do you know Grafton Eliott Smith?

. . . . b y e b y e . . . .

Thilo Schadeberg quits. Writing from Leiden, Nederland, in March, Thilo said: "Mr. President, dear Hal, I did not become a member of ASLIP because, these days, my interests in historical linguistics are getting shallower and shallower. Nevertheless, thank you for remembering me. With my very best wishes, Thilo". Alas, we've lost a real good one. However, where Thilo works --on the N-S and N-C interface -- 'shallow interests' cover as much linguistic diversity as Nostratic does.

. . . . b y e b y e . . . .

Patrick Bennett quits. From Madison, Wisconsin, he writes that: "I have, true, not renewed with ASLIP - not in any way to offend, I find that I cannot read an issue through without aversive symptoms (some sort of psychic Antabuse, perhaps)... It is not, to be fair, your own free and enthusiastic contributions, and things like your lists at the end of #12 disturb me not one whit, of course -- we need more, not less, publication of the raw data without which all linguistic work is half-baked. But some of that stuff!" ... "I found myself momentarily tempted (pure pride and masochism) by your appeal for an editor. BUT that is not something I should be doing -- nor does MOTHER TONGUE need an editor who makes no secret of being disturbed by well over half of what long-rangers say. I can appreciate the problem - it is no easy task. Part of the problem, of course, is that Long Rangers, like NiloSaharanists, are strange bedfellows. Not so antagonistic as [some others - HF]. I am convinced that were I a convinced long range comparator I would be off on a very different tack and highly dubious (though courteous) about [some people - HF]." So sorry, we could use a first rate guy like Pat when we come to the great tropical super-phyla, like N-C & N-S.

. . . . W i e n e r F e l l o w . . . .

Karl-H Menges wrote last year that: "In the meantime, I have not been very active on the Nostratic line, although when establishing etymologies, I always add, if available, Nostratic data, so e.g. presently during work on an article about Chinese loans in Uighur, or in my grammatical & lexical part of Ryckov's Tungus shamanistic texts (to be published by the Rheinisch-Westfälische Academy of Sc. in Düsseldorf). Otherwise, I am in touch with Shevoroshkin." That letter took a year going to Ethiopia and back! More recently he wrote that:

"Altaic does certainly not play one of the first roles in MT, and this is only natural since there are in reality very few Altaicists around; of those who are around, there are a few who time and again imagine to be very doct (? -HF) when they emphasize the doubts about the very existence of an Altaic language family. They still are under the impression of ideas virulent in the 30'es down to the 70'es of our century, having their basis in a general refusal to recognize genetic relationship of languages. In so doing they act as Anti-Altaists, a species of scholars which I have not found among colleagues in the other linguistic fields, even not among Hamito-Semitists. Or am I too little acquainted with what is going on in those subjects?"

Interrupting again. His observations are interesting; I don't know any other sub-discipline quite like Altaic studies. Some of us have seen occasions where Austronesianists and Australianists -- who deal with far greater diversity than Altaic has -- have scorned opportunities for phylum-bashing, as I call it. Can groups hate the object of their study? Sono dubbio.

Karl-H. Menges continues: "In the last and in an earlier issue of MT I found an article by Karl Krippes, of whom I never saw anything before, and what I saw now in MT does in my opinion not appear as being a worthwhile contribution to MT. First, this gentleman has to make up his mind whether there is a genetically related group of languages called the Altaic one, comprising also, after Miller's work, Korean, Japanese and Ryu-Kyu. Concerning the latter three, he may not yet have studied the subject sufficiently. Then he has to learn how to quote other people's work -- not out of the blue sky, at random, not out of a not too good memory or selectively, what in some cases leads to misrepresentations. He ascribes to me a Maya-Altaic parallel, from my 'Altajische Studien, II: Japanisch und Altajisch' (1975), but such a parallel does not occur in any of my writings. On the other hand, he brings a number of Altaic-Nostratic parallels, among which there are some that had been put up by me years ago - but there is no quotation, and it looks as if that were first said by him. In his quotations, hardly more than 3, from my 'Jap. and Alt., II', he is either sloppy or his German is bad - in spite of a German name."

He continues: "Roy A. Miller is a man whom we ought to have in ASLIP, especially since he is well-inclined to Nostratic. .."

Four people, all major scholars, have asked why I don't get Miller to join ASLIP. Well, I do not know how to! I sent him MT for a long time plus our business letters. In four years he has never replied to me. So why don't one of you ask him to join?

He finishes: "I wrote a review (of about 13 pages typed) on Mark Kaiser's 'Lexical Archaisms in Slavic: From Nostratic to Common Slavic', but the ZEITSCHRIFT für SLAVISCHE PHILOLOGIE, to which I time and again contributed, did not take it ... Prof. Brang (Zurich U.) wrote me a friendly letter excusing himself that they could not take my review as it goes far beyond Slavic, and the Zeitschrift was presently confronted with the publication of too many Slavistic articles. Brang suggested the I F, but there, I think, there are too narrowly oriented Indo-Europeanists for liking Nostratics, and thus I may send it to 'ORBIS' in Louvain, where formerly I had published a number of articles, - but nothing after the mid-seventies...."

His I F is probably INDOGERMANISCHE FORSCHUNGEN. I presume that everyone knows that Indogermanisch = Indo-European = IE.

- - - - - - Gaam /pun/ = PIE /frd/ - - - -

John Rittershofer (Yonkers, New York) wrote best wishes and encouragement. He also said: "I like the data on Ongota (Birale). There is great complexity, but some eye-opening links. As for the future of ASLIP, I like the sound of linking up with LOS and letting them provide a financial umbrella. If the marriage doesn't hold up, MOTHER TONGUE can give LOS the slip (aslip?)."

Anna Belova writes from Moscow: "Now I read the new MOTHER TONGUE about the Nostratic Reconstruction and Classification. We also have here a new paper of Ruhlen about the origin of Human Language and about the last hypothesis for it (in: Voprosy jazykoznaniya, M., 1991, N1). And now I work on reconstruction of Semitic root vocalism. It is not so far in Language Prehistory, but may be one step there." Don't be coy, Anna! Your efforts in Semitic have been first rate and important.

Roger Wescott wrote several times, reporting on the ILA, on his own activities, on what mavericks can do in the world, and giving encouragement to myself at a time when it was needed. Some of his reports are: "Mavericks are poor advisors for breaking into established institutions like the LSA or NSF. The simplest advice is: conform! Can you?" In March he gave a talk on Nostratica at Swarthmore College. He also has a review article on Ben Elugbe's COMPARATIVE EDOID: PHONOLOGY AND LEXICON (University of Port Harcourt Press, Port Harcourt, Nigeria), 1989. The review will appear in "one of Shevoroshkin's Bochum books"; its title is "Apophony in Proto-Edoid" and it reaches out to compare PIE with proto-Edoid, an N-C language. 'Apophony' = Ablaut in IE terms.

Roger also chaired a colloquium at the Drew Graduate School, Drew University, Madison, New Jersey, which was concerned with EVOLUTION, AS A CROSS-DISCIPLINARY THEME. On April 19th and 20th,1991, it involved Anthropology, Biology, Ecology, Gender Studies, History, Literature, and Theology. Besides being a founding long ranger, Roger may best be called a Wide Ranger, a truly catholic intellectual. (Even at Methodist Drew?)

(Editor's note: I found some space that could not be avoided. So we'll use it to discuss unimportant things. Because no etymologies come to mind. In this issue I finally figured out how to do tricks with the computer interfacing (!) with the printer. Some of the results you may not like. Too much emphasis? Feel free to give me some reactions to the human versus high tech problem. Mas importante, tell us what you liked !

. . . . e x o t i c a o n e . . . .

Ping pong. Bender and Fleming in three parts.
It might also be called an irritating little scuffle, in which two people talked past each other, concentrating on different topics. Well, such is life. It begins Overleaf ---------->

401 Emerald Lane
Carbondale, Ill. 62901

Feb. 4, 1991

Dear Hal,

Thank you for the latest MT, always full of refreshing, if somewhat repetitious and sometimes idiosyncratic tidbits.

I have a suggestion for editor of *MT-The Journal* (or maybe *LINGUISTIC MONOGENESIS* - The Journal, a title which would make it clear what it is all about). To wit: Merrit Ruhlen; as an independent scholar, he would presumably have the time and he certainly possesses the boundless energy and he has established for himself the professional credibility and catholic outlook it would take.

I must say that a very brief look at the Oongota (Birale) lexicon reinforces my suspicion that all these "strange hybrids" (also Shabo, Kwegu, various Wattas, Manjos, Wandarobos, maybe some others) are "sub-strata phenomena" with different, perhaps unidentifiable substrata at this late date. Oongota certainly seems to have been heavily influenced by Tsamay and Shabo by Majang. I think they are of more ethnohistorical than linguistic interest since it is not clear that they will contribute to modifications of the genetic classifications now fairly well in place.

The book you so vaguely mention as having your Shabo material in it is Bender ed. 1991: *Proceedings of the 4th Nilo-Saharan Linguistics Colloquium,* Vol. 7 of the Nilo-Saharan Linguistic Analyses and Documentation Series Franz Rottland and I are editing.It is now with Rottland in final preparation to go to press and you can look for it at your friendly neighborhood bookseller before the end of the year.

Regarding your suggestion as to where to turn for a more sympathetic reception of "long-ranger" ideas, I have this advice to offer: put not your trust in archeologists and physical anthropologists, my boy!

Yes, this letter to here is for publication.

Sincerely,

Lionel

M. Lionel Bender (Prof.)

ASSOCIATION for the STUDY of LANGUAGE IN PREHISTORY
5240 Forbes Avenue
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Prof. M.Lionel Bender
401 Emerald Lane
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Dear Lionel,

Thank you fòr your letter of February 4, 1991. As you wish, it will be published in MT-13. Inevitably, it is vintage Bender, wishing us well as you criticize our work. Perhaps it is time for you to declare where you stand, lest you acquire a reputation for perpetual ambivalence. In any case I am exercising my right of reply. Since you comment on Ongota and Shabo, I respond as field worker and Africanist -- not as editor of this newsletter.

In your third paragraph you say some quite serious things about the small, isolated, remnant languages of northeast Africa which you want to call "strange hybrids". You think that "they are of more ethnohistorical than linguistic interest since it is not clear that they will contribute to modifications of the genetic classifications now fairly well in place." What a remarkable statement! What is the difference between an ethnohistorical interest and a linguistic one? What are 'sub-strata phenomena' anyway? Things like Shabo and Ongota, or Basque and Etruscan, or Nahali, Kusunda and Burushaski, are little windows onto an unknown past. They may reflect antique populations which did influence the now dominant peoples of their area -- in that sense sub-strata -- but that fact should not deprive them of genetic linguistic interest! They still have to be classified, even if their taxon is Mischsprache, just as the elements (languages) which went into their mixture have to be. (I suspect you've been reading Kaufman and Thomason). Furthermore these remnant languages DO affect taxonomy sometimes. Both Ongota and Shabo are going to force changes in their respective phyla --you think that is nothing?

What disturbs me basically about your argument is that it sounds like the old crap we had to fight our way through to get the good African taxonomies we have now. Do you remember how Huntingford used to dismiss those Okiek ('Dorobo') languages? Later we all, especially Chris Ehret, Franz Rottland, Rainer Vossen, Bernd Heine, Jürgen Winter and me, disentangled the pieces and built up a very interesting prehistory of interacting Nilotes and Cushites, not to mention fleshing out the skeletons of East Sudanic and Cushitic. Did you ever count how many little 'strange hybrids' there were lying around East Africa in, say, 1960? Probably none of these remnant languages have exceeded Albanian or Nahali in their ability to borrow from other languages. Nor is English too reluctant to borrow either!

Hal

-48

March 12, 1991

Dear Hal,

I hope you will print this letter down to END.

Your response to my letter of Feb. 4 puzzles me. What is bad about wishing MT well while criticizing some of its work? Declare where I stand? What is this: warfare? Is this a case of "you are with us or against us"? Surely there is no monolithic MT viewpoint. I had hoped that ASLIP would welcome various non-hostile viewpoints. I thought it was already clear that I am in favor of interphylum comparisons, but I have not decided on which- or which parts- of many competing classifications I accept. One thing I do not accept at this point is "world etymologies". Is my stand "perpetual ambivalence" or just remaining uncommitted in the face of insufficient evidence? Why is such a "ramparts mentality" rampant?

Regarding Ongota and Shabo. I don't think these force me to rethink any phyla because I don't know what these strange hybrids are yet. But I doubt that they will force such drastic changes in any case. None of my comments is based on Kaufman and Thomason, though I looked over the book and find much of value in it.

END

Lionel

8) THE SWAP SHOP. Formerly called the EXCHANGE

Two people wanted some reprints. Lack of interest seems to have shut down that aspect of this feature. So we have re-named it the SWAP SHOP. We try to do useful things for each other. So this is a good space to help people get jobs or hire the kinds they want or just inquire about something anonymously. For example, where can I find a cat that drinks vodka?

POSITION WANTED. (1) Very bright young Ethiopian linguist with a Master's degree and much field experience wants to find a PhD program in linguistics to apply to. His three most important criteria are (first) financial support to the PhD, (second) at least one potential guru in historical linguistics on the faculty, and (three) that English be the language of instruction. He can read French, German, and Italian too but does not feel competent enough to take graduate work in them.

POSITION WANTED. (2). Highly intelligent yet wise. Social scientist, generalist but focuses on psychology, cultural theory, linguistics and how society works --all in relation to specific problems. Independent and easily equivalent to a full professor in experience and knowledge. Lots of administrative experience. Wants a career change to academia or foundation position.

POSITION WANTED. (3). Red hot, brand new PhD in historical linguistics. One of the original long rangers. Student of two of our founding long rangers at A+ university. Can do I-E and AA, at least, if not more. Does high quality IE type reconstruction. A bargain as an Assistant Professor.

POSITION WANTED. (4). Computer freak, whiz kid of Ethiopian computer circles. Has BA in Physics and Math. Wants to study computer science at the graduate level. Most important need is to find a program with financial support. If someone knows of a program which gives help to 'foreign' (= non-USA) students in computer science, please tell us. If Ethiopia were in normal times, he would now be getting his doctorate at M.I.T.

POSITION WANTED. (5). Fairly old anthropologist, needs body work, performs sluggishly on hills. Knows lots when random access memory locates it. Bewildering lecturer. A bargain for a small college or someone who wants spare parts.

CHANCE TO DO BOOK REVIEW. Courtesy of Sheila Embleton, the following open advertisement from the prestigious linguistics journal -- WORD -- solicits book reviews. Choose a book from the list, write to Sheila, and hope that nobody took it already.

(See Advertisement Overleaf ------->)

The following books are available for review in *Word*. If you wish to review a book, please write to Sheila Embleton, Department of Languages, Literatures and Linguistics, South 561 Ross Building, York University, 4700 Keele Street, North York, Ontario, CANADA M3J 1P3. E-mail is embleton@yorkvm1.bitnet or embleton@vm1.yorku.ca.internet. Telephone numbers are (416) 736-5387 at York and (416) 851-2660 at home. Books are available on a "first come, first served" basis. Graduate students are welcome to participate under supervision of a faculty member. Reviews are due 6 months after you receive the book. Please send **2** copies of your review, double-spaced with at least 2 cm margin on all sides.
Books marked with * are appearing on this list for the last time. If you wish to write a review, this is your last opportunity. If there is somebody who would like to receive that book, but not for review, let me know — if nobody requests it, I might be able to send it to you (as a "gift").
Date of this list: April 20, 1991

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Please note: Nos. 1-23, 26, 28, 29, 36 and 38 are out of print and generally no longer available. A listing of these titles is available on request; of some of these titles we have still a small stock available. Information on availability on special request.

Issue of this listing: 20. Dezember 1990

24/ **Notizen zur Architektur der Bella Coola-Indianer.** Uwe Johannsen. 1978. 9 p. 3 DM/$1.50

25/ **Notes on the Early History of the Nicola Valley.** James A.Teit. 1979. 22 p. 5 DM/$2.80

27/ **Zur Entwicklung der Gesellschaftsordnung bei den Bella Coola an der Nordwestküste Nordamerikas.** Julija P. Averkieva (transl. by Volker Eisenblätter). 1980. 18 p. 5 DM/$2.80

30/ **Ethnohistoric Study of Haida Acculturation, Part 1: Introduction, Ethnography and Prehistory.** Ruth E. Smith. 1981. 37 p. 10 DM/$4.00.

31/ **Ethnohistoric Study of Haida Acculturation, Part 2: Haida History from 1774 to 1900.** Ruth E. Smith. 1983. 53 p. 12 DM/$5.00

32/ **Ethnohistoric Study of Haida Acculturation, Part 3: Haida History from 1900 to the Present, Retrospect and Prospect, Literature Cited.** Ruth E. Smith. 1984. 23 p. 5 DM/$2.80

33/ **The Origins of Nootka Whaling: A Definition of Northern and Central Nootkan Ecological Orientation for the Past Four Millenia.** John Dewhirst. 1987. 11 p. 3 DM/$1.50

34/ **The Potlatch in Anthropological Theory: A Re-Evaluation of Certain Ethnographic Data and Theoretical Approaches.** Marianne Boelscher. 1982. 12 p. 3 DM/$1.50

35/ **Potlatching, Warfare and Raiding Among the Southern Kwakiutl: Some Considerations.** Dick A. Papousek. 1982. 15 p. 3 DM/$1.50

37/ **A Historian of the Tlingit.** David E. Conrad. 1984. 11 p. 3 DM/$1.50

39/ **Sprachhistorische Untersuchung einiger Tiernamen im Haida (Fische, Stachelhäuter, Weichtiere, Gliederfüßer u. a.).** Heinz-Jürgen Pinnow. 1985. 40 p. 10 DM/$4.00

40/ **Der Potlatch als Spiel: Bemerkungen zu einer Interpretation bei Johan Huizinga.** Michael K. Heine. 1985. 27 p. 5 DM/$2.80

42/ **Potlatch and Stratification among the Tlingit.** Kenneth Tollefson. 1985. 15 p. 5 DM/$2.80

43/ **Das Haida als Na-Dene-Sprache: Materialien zu den Wortfeldern und zur Komparation des Verbs. I: Einleitung mit einem Überblick über die Na-Dene-Sprachen, Abkürzungs- und Literaturverzeichnis, Zur Orthographie, Phonetik und Phonemik.** Heinz-Jürgen Pinnow. 1985. 63 p. 12 DM/$6.00

44/ **Das Haida als Na-Dene-Sprache: Materialien zu den Wortfeldern und zur Komparation des Verbs. II: 1. Teil - Das Morphemmaterial I.** Heinz-Jürgen Pinnow. 1985. 90 p. 15 DM/$7.50

45/ **Das Haida als Na-Dene-Sprache: Materialien zu den Wortfeldern und zur Komparation des Verbs. III: 2. Teil - Das Morphemmaterial II.** Heinz-Jürgen Pinnow. 1985. 93 p. 15 DM/$7.50

46/ Das Haida als Na–Dene–Sprache: Materialien zu den Wortfeldern und zur Komparation des Verbs. IV: Der Grammatische Rahmen – Die Verbkategorien mit ihren Morphemen und ihre Stellungen im System. Heinz–Jürgen Pinnow. 1985. 73 p. 12 DM/$6.00

47/ Die Zahlwörter des Haida in sprachvergleichender Sicht. Heinz–Jürgen Pinnow. 1986. 33 p. 5 DM/$2.80

48/ Moieties or Phratries? That is The Question Among The Tlingit. Marjorie J. Pringle. 1986. 7 p. 2 DM/$1.00

49/ Die indianische Kunst als ethnisches Identitätsmotiv am Beispiel der Indianer der Nordwestküste Nordamerikas. Ralf Streum. 1986. 12 p. 3 DM/$1.50

50/ Säugetiernamen des Haida und Tlingit: Materialien zu ihrer historischen Erforschung. Heinz–Jürgen Pinnow. 1986. xi+61 p. 10 DM/$5.00

51/ Modernisierung der Karibujagd bei den Naskapi in Nordquebec, Kanada. Michael H. Weiler. 1986. 13 p. 3 DM/$1.50

52/ ONE PITCH OR TWO PITCH: Did Variance in Roof Style Depict Class Standing Among the Southern Puget Sound Salish Peoples? Marjorie J. Pringle. 1986. 8 p. 2 DM/$1.00

53/ Native Renewable Resource Harvesting in the Canadian North: Comments on the Significance and Methodology of Land Use and Harvest Studies. Michael H. Weiler. 1987. 17 p. 3 DM/$1.50

54/ Prehistoric Settlement and Subsistence Structure of Northern Plains Hunter/Gatherers. Jeffery R. Hanson. 1987. 20 p. 5 DM/$2.80

55/ The Forks of Delaware, Pennsylvania, During the First Half of the Eighteenth Century: The Migration of Some "Jerseys" into a Former Shared Resource Area North of Lenape Territory and Its Implication for Culture Boundaries and Identities. Marshall Joseph Becker. 1987. 163 p. 20 DM/$10.00

56/ The Nature and Function of Games in Tlingit Society: An Essay in Ethnographic Reconstruction. Michael K. Heine. 1987. 65 p. 12 DM/$6.00

57/ Achumawi und Pomo: Eine besondere Beziehung? Karl–Heinz Gursky. 1987. 12 p. 3 DM/$1.50

58/ Der Hoka–Sprachstamm: Nachtrag I. Karl–Heinz Gursky. 1988. 37 p. 5 DM/$2.80

59/ Bibliography of Canadian Indian Mythology. Kim Echlin. 1988. 48 p. 6 DM/$3.50

60/ Kognitive Anthropologie versus ethnologischer Historismus: Zur Situation der deutschen Ethnologie und ihrer Ausbrecher. Egon Renner. 1988. 70 p. 12 DM/$6.00

61/ Indianische Schwitzhäuser der Nordwestküste Nordamerikas. Helmut Krumbach und Reinhold Krüger. 1988. 55 p. 10 DM/$5.00

62/ Verwandtschafts– und andere Personenbezeichnungen im Tlingit und Haida: Versuch ihrer sprachhistorischen Deutung. Heinz–Jürgen Pinnow. 1988. 146 p. 18 DM/$9.00

63/ Der Hoka–Sprachstamm: Nachtrag II. Karl–Heinz Gursky. 1989. 31 p. 5 DM/$2.80

64/ Die Na–Dene–Sprachen im Lichte der Greenberg–Klassifikation. Heinz–Jürgen Pinnow. 1990. 38 p. 5 DM/$2.80

65/ Der Hoka–Sprachstamm: Nachtrag III. Karl–Heinz Gursky. 1990. 26 p. 5 DM/$2.80

66/ The Shawnee in Tecumseh's Time. John Sugden. 1990. 111 p. 18 DM/$10.00

67/ Vogelnamen des Tlingit und Haida: Materialien zu ihrer sprachhistorischen Erforschung sowie Auflistung der Vogel–arten von Alaska. Band I: Einleitung, Zusammenfassung u. a.; See–, Lappentaucher, Röhrennasen, Ruderfüßer, Stelz–, Gänse– und Greifvögel. Heinz–Jürgen Pinnow. 1990. 118 p. 16 DM/$9.00

68/ Vogelnamen des Tlingit und Haida: Materialien zu ihrer sprachhistorischen Erforschung sowie Auflistung der Vogelarten von Alaska. Band II: Hühner–, Kranich–, Wat– und Möwenvögel, Tauben, Kuckucke, Eulen, Nachtschwalben, Segler, Kolibris, Racken, Spechte, Sperlingsvögel. Heinz–Jürgen Pinnow. 1990. 112 p. 16 DM/$9.00.

10) EDITORIAL (In Three Parts).

Since the triple editorial in MT-12, I have received a great deal of communication from members. One thing is quite clear: ASLIP and MOTHER TONGUE are believed to be valuable. Many people also believe that we are winning our struggle. Which is what? To be accepted as a legitimate scientific endeavour, to be free of the silly 'prohibition' against seeking language origins, to be tolerated as unorthodox or mavericks, and to be even more than that -- to be thought to be INTERESTING! (Or even EXCITING!) Maybe now AT LAST we can settle down and do our real work which is discuss problems, share data, advance hypotheses, and so forth, as Dolgopolsky and I envisaged it at the outset.

Now is the time for good to high quality journal articles and books (see below). We should give more attention to such things as the proposed larger phyla and the super-phyla. We need to look harder at Indo-Pacific, Australian, all the African phyla, and Austro-Thai. We need to zoom in on the debates over Basque, Sumerian, Japanese and others which various writers have classified differently. We need to examine more coolly and more sceptically, or we need to test, the super-phyla proposed by our geniuses, Greenberg, Starostin, Benedict, and Dolgopolsky. We need to look much harder at Southeast Asia and India, chiefly the question of the giant Austric super-phylum which Paul Benedict does NOT accept. We need to focus powerfully on taxonomy, notably sub-grouping, surely including or developing criteria for relative proximity and distance in the old deep relationships, such as those outlined by Swadesh's Vascodene and/or Ruhlen's very interesting proposals (to be published elsewhere soon).

Some of us reckon that we ought to 'cool it' on global etymologies AS FAR AS THE EXTERNAL AUDIENCE IS CONCERNED. There is no good reason why people should ever stop actually doing global stuff or reporting etymologies to us in MOTHER TONGUE or in our networks. We will not, and cannot, get to our final goal without etymologies of global scope. How else could it be done? (Well, possibly by the transitivity principle) But from a strategic point of view I would argue that we have exceeded our colleagues' abilities and mind sets. They need time to expand their heads. And, verily, some of those heads ain't never goin to expand! But others are easing themselves into conceptions of older taxa (moi, par example). If one can stand back from what one is accustomed to hearing about these days, one might remember how startling it would have been ten years ago to hear that Basque, Circassian, Burushaski, Ket, Chinese, and Apache were related to each other! Many people still cannot accept Haida as a member of Na-Dene, for example; in Muscovite Na-Dene, Haida is not included.

A SPECIAL NOTE ON DATING.

As part of these editorial suggestions let me stress the serious problems presented by dating -- finding a time in years, or an era relative to some other era, for our ancient languages, proposed migrations or evolutionary events. It is apparent that we have now gone too deep into prehistory for glottochronology. As it is calculated nowadays, it is in water over its head. With due respect to Starostin's glottochronology, it is subject to the same limits as Swadesh's. When you get down below 10%, it is very wobbly. Down around 2% it becomes useless. But those percentages already exist in our large tropical phyla. Furthermore, unless Starostin takes our pleas for communication more seriously than he has in the past, his system of glottochronology will simply be ignored outside of the USSR. Bender's negative appraisal of Starostin's dating will not be challenged. But just think about the proto-Eurasiatic-Amerind that Greenberg seems to be proposing or Swadesh's Vascodene or Aihenvald's 'Noscau'. How old are they? How can we find out?

Malheureusement, one must also point out that there is NOT a solid basis for dating the biogenetic events proposed by Cavali-Sforza and the others. There is no rate of change, no formula, no molecular clock like the one so revolutionary in paleoanthropology, for saying that Europeans and Indonesians separated so many thousands of years ago. Why? Because most of the dating they do is based on controversial Amerind dates or the constantly changing dates for early man in Australia. This will not do! Let us regularly worry about dates, like archeologists!

About JOURNALS and how to get into them:

There are some new things to say. First, I must apologize to Sarah Grey Thomason, editor of LANGUAGE, and long after the fact to Bernard Bloch, late editor of LANGUAGE, for implying that their journal blocked Noam Chomsky in his early attempts to publish his views. They never did block him; au contraire it was Bernard Bloch who published Chomsky's famous long review article of B.F.Skinner's VERBAL BEHAVIOUR. Chomsky recently told me that "Bloch was very nice to me." No, it was other journals which blocked much of the early TG work, not LANGUAGE. Moreover, when Noam wanted to publish his book on 'Logical Structures' in the 1950s, he could not find a publisher anywhere in North America. So he excised a portion of that book and published it in Holland as SYNTACTIC STRUCTURES, the book closely identified with his revolution in grammatical analysis. Recently he has been able to get the whole LOGICAL STRUCTURES OF LANGUAGE published --three decades later.

Noam reckoned that I was wrong about the specific thing blocked but right in general about blockage. It got so difficult for him and his followers that they finally started their own journal -- LINGUISTIC INQUIRY. Yes, we know that they published some crucial books too. It may interest the many anti-TG people, including long rangers, that Noam saw quite clearly the bitterness and resentment which his school provoked, emotions which remain 34 years after his key book. Haven't I always said that scientists were cool, detached and objective?

Anyway, we have no editor for an MT, the journal. Furthermore, our most likely publisher turned us down when we asked, advising us to stay the way we are = half journal and half newsletter. One key reason is the recession going on in the USA. Another is that libraries do not want more small journals of our type. Finally, we would have to charge our members much more money in dues (an estimated $35 a year) to do our own journal.

Does that mean that we cannot publish good long articles, full of substance and analysis, if they have long range type topics? No, it does NOT mean that! I have traveled here and there, consulting with ourselves; it is clear that we who write linguistic type articles in English can try at least seven good journals where we face fair-minded, even-handed editors and procedures and where long range type topics have a decent chance --if we present the editors with good material and cogent analysis.

. . . .

If we write good stuff, presented in acceptable formats (e.g., typing, spacing, arrangement, etc.), then it might be accepted in the following journals:

WORD (New York, USA)
DIACHRONICA
GLOTTOMETRIKA (Trier, Germany)
GENERAL LINGUISTICS (Pennsylvania, USA)
CANADIAN JOURNAL OF LINGUISTICS
CURRENT ANTHROPOLOGY (Chicago, USA)
AMERICAN ANTHROPOLOGIST (with several sub-sections)

GLOTTOMETRIKA specializes in quantitative or mathematically oriented stuff, so articles about dating or lexicostatistics could go in there, for example.

Naturally, people can write archeology or physical anthropology, essentially without fear. European journals I am no longer sure about because of Karl-H. Menges's remarks (see above), except (?) ORBIS. Places where we might put an article on Mon-Khmer might not even consider a long ranger type article. We could use a list of journals in Europe or elsewhere where someone is sure tolerance exists. And others in the New World undoubtedly exist. My coverage was certainly not exhaustive. Please, good colleagues, tell us about them!

In one of the next two issues we will publish the work of Hegedüs Irén (Janus Pannonius University, Pécs, Hungary) who has written a Nostratic Bibliography. In it we will find the names of many European journals which have published long range type articles, i.e., on Nostratic.

What about LANGUAGE, the official journal of the Linguistic Society of America? I had a multi-page exchange of views with Sarah "Sally" Thomason, its current editor,a highly intelligent and likeable person. Nevertheless, all we could agree on was that LANGUAGE had not blocked Chomsky during the early days of his revolt. She insists that they have very high standards and that she/they are not averse to publishing long range type main articles, providing it has very high quality and can get past her referees. We did not discuss comments, book reviews, and other short stuff. I pass this on to you for your consideration.

My personal conclusion is based on reading a large dossier of correspondence about two rejected major articles submitted in 1990 and 1991 by two long rangers. I believe that the LSA editors do a linguistic version of what American politicians call the "good cop, bad cop routine". The referees are the bad cops. But maybe I'll have to apologize to Sally again. Quien sabe?

11) Report on ASLIP business.

Ekkehard Wolff saves the European side of ASLIP! See above under NEWS for general comments. More specifically, members in European countries, including the Warsaw Pact, NATO, USSR, and neutrals, may send their US $10 annual dues to Professor Wolff from whom they will receive their copies of MOTHER TONGUE. So too for members from Israel. But we need to hear from some Israeli to explain how the currency problem can be solved. Do the bank charges and government restrictions also apply to paying Ekkehard in Deutsche Mark, for example? Members in Africa, Asia, and Australia will continue to deal with us here in the USA. Just to remind everyone once again, his address is :

Professor Dr. Ekkehard Wolff,
Seminar für Afrikanische Sprachen und Kulturen,
Universität Hamburg,
Rothenbaumchaussee 5,
D-2000 Hamburg 13, Deutschland (Germany)

Board of Directors adds to the Council of Fellows. Given the purposes of the Council of Fellows written into our By-Laws, and given the geographical distribution of our members and their varied interests, the Board felt that distinguished colleagues from small countries could not normally get elected. Therefore the Board took it upon itself to elect four scholars to the Council of Fellows, to wit, John Stewart of Edinburgh, Hans Mukarovsky of Vienna, Karl-Heinrich Menges of Vienna, and Ben Ohiomamhe Elugbe of Nigeria. Professors Stewart and Elugbe are experts on Niger-Congo, an attribute which rarely gains one world renown. Hans Mukarovsky has undertaken the hardest job, linking African and European phyla, while Karl-H Menges has devoted most time to Eurasian phyla and is well-known as a Nostraticist or pioneer long ranger. There will be more elections.

New members elected to the Board at annual meeting. Officers are automatically on the Board of Directors. In addition to the re-elected officers the following were elected to the 1991 Board: M. Lionel Bender (Southern Illinois University, Carbondale, Illinois 62901), Sherwin J. Feinhandler (Social Systems Analysts, Cambridge, Massachusetts 02238), Frederick Gamst (University of Massachusetts, Boston, MA 02125), Mark Kaiser (Illinois State University, Normal, Illinois 61761), Saul Levin (S.U.N.Y., Binghamton, New York 13901), Daniel McCall (Professor Emeritus, Boston University. Home at 7 Wigglesworth Street, Boston, MA 02120). It may amuse colleagues to know that a majority of the Board does not do long range comparisons!

Annual dues regularized at US $10 for EVERYONE. Feeling that the needs of the members merit more consideration than the costs of postage and due to the huge saving of money that Ekkehard Wolff has generated for us, the Board decided to REDUCE the annual dues for all non-North Americans to US $10 per year, or ZERO for those who are prone to currency problems. Even though postal rates in the United States have INCREASED by about 18% on average and despite the fact that printing and postal costs in Europe are not yet precisely known (to the Board).

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Board approves three editors for MOTHER TONGUE. Let us welcome Mark Kaiser to the helm! Mark accepted the editorship of MOTHER TONGUE for the second half of 1991 or at least one issue in the Fall, as well as part of 1992. And the Board approved. The rest of the future has been left on a "let's wait and see how things work out for all of us" basis. The Board also decided that it would elect him to the Board of Directors. Mark can be expected to bring new energy and new viewpoints to MOTHER TONGUE during his tour of duty. We are very grateful to him for his timely and helpful decision!

Allan Bomhard agreed to edit the newsletter for the summer 1991 issue and one next year. He remains as Vice President for 1991, serving with Anne Beaman (Secretary) for another year. There was no candidate for Treasurer, hence no officer. Harold Fleming will continue to do the financial chores, no matter who is editor, and remains President.

Our main organizational problems remain the Annual Meeting and money. Again this year we couldn't agree on an alternative to Boston in April. Because our By-Laws require us to have a quorom of five Directors at Board meetings we cannot enlarge our Board beyond nine members. And because of money we cannot pay the travel expenses of Board members to attend meetings. So, what looks to the outside world as a small coterie of Bostonians running things is a correct perception. We cannot do it any other way. The officers, however, and through them the Board are profoundly influenced by communication from members. If you want to have an effect on any ASLIP activity, WRITE TO US!

If we can find money to generate a conference or big annual meeting, or if we can piggy back on some meeting that many of our members attend, we can solve these organizational problems. Or more to the point -- who would like to start a committee to look into some of ASLIP's problems? That ILA meeting in New York could have been our annual meeting too, for example.

ASLIP acquires a new legal address, not for mail. Our legal address has been changed to ASLIP, P.O.Box 2348, Cambridge, Massachusetts 02238, USA. That address exists purely to satisfy the rules and regulations of the Commonwealth of Massachusetts and the Internal Revenue Service of the United States government. Please do not send mail to it. Our old Rockport address was my personal residence. Since I have moved 1000 kilometers west of that village, it cannot be ASLIP's legal address. Don't use it!