MOTHER TONGUE

NEWSLETTER OF THE ASSOCIATION FOR THE STUDY OF LANGUAGE IN PREHISTORY

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Editor (August): Harold C. Fleming

ASLIP is a non-profit organization, incorporated under the laws of the Commonwealth of Massachusetts. Its purpose is to encourage and support the study of language in prehistory in all fields and by all means, including research on the early evolution of human language, supporting conferences, setting up a data bank, and publishing a newsletter and/or journal to report these activities.

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(1) REPORTS FROM RUSSIA by MARK KAISER and the NEW YORK TIMES.

OUR RUSSIAN COLLEAGUES

I recently returned from a one month stay in Vladimir, Russia. Due to some unfortunate circumstances, I was unable to meet directly with any of our Russian colleagues. Nevertheless, I was able to learn much about the plight of scholars and educators in Russia today.

I have visited Russia frequently during the past twenty years, but during this trip I was struck by two new developments: the increase in crime and the further deterioration in the functioning of society.

Over the course of one month I was involved in one pick-pocketing, one extortion, and the disappearance and rifling of one suitcase. Foreigners are frequently the targets of crime: the contents of one suitcase could easily bring the equivalent of six months salary.

Even more discouraging is the impression of general deterioration: new buildings that appear 50 years old, public telephones forever out of order, airline flights cancelled due to a shortage of fuel. The public sector is crumbling, and a private sector has not yet been created to take up the slack. The good news is that there are more consumer items, including food, on the shelves than one year ago. But, ...

Since price controls were removed in January, Russia has experienced hyperinflation. Prices for most goods and services have increased approximately 1000%. Some examples: a plane ticket from Siberia to Moscow was 64 roubles, now is 1000 r.; a pack of Marlboros was 20 r., now is 175 r; a loaf of bread was .40 r., now is 5 r.; a kilo of tomatos was 15 r., now is 130; etc. At the same time salaries have risen only 400% (average monthly salary is approximately 2,500 r.). Those professions that rely on direct government funding, i.e., doctors, teachers, scholars, have seen their salaries increase at a much slower rate than the national average.

To understand the burden placed on Russian families, imagine two pounds of tomatos costing $104, two pounds of sausage for $140, a transatlantic plane ticket for $72,000., or a pair of shoes for $2,400. In terms of percentage of monthly salary, that is what Russians are paying.

Fortunately, we can help. The current exchange rate is 100 r. = $1.00. Thus, a small number of dollars can go a very long way. In many academic fields scholars are assisting their Russian colleagues. This spring ASLIP was able to send a small amount of humanitarian aid, and it was greatly appreciated. Please consider sending Hal Fleming a donation earmarked for Russia.

-Mark Kaiser

(Editor's Note: The city of Vladimir lies about 150 km East Northeast of Moscow. Mark intended, after visiting Vladimir, to distribute some Christmas cards in Moscow for ASLIP. The first move in the distribution was to hand the cards to Serge(i) Starostin or Nikolaev who would then play Father Christmas to about ten Muscovites. Mark had troubles and so was able only to give our cards to a third person to hand over to the Sergei(s). We've heard nothing since! Mark says: "Don't worry!")

From the New York Times:

A very recent (August 2, 1992) front page article by Serge Schemann, entitled "Yeltsin's Team Seems in Retreat As Its Economic Reform Falters", gave a bleak appraisal of the situation which our colleagues and their countrymen face. Quoting now: "There is a growing sense that the administration of ... Gaidar, who designed Russia's economic reform plan, miscalculated its plans and has lost its authority and bearings. Indications are that the radical reformers are rapidly losing ground to tough directors and managers of state-owned industries who demand a far slower pace of change. The shift, however, goes beyond politics or competing power groups, to a spreading sense that the promise of a new era after the fall of the Communist state has been lost, that Russia has lost direction, that authority and even statehood are rapidly eroding."

"Democratic Idea Devalued. 'The problem is not only the high prices or a shortage of goods,' said Pavel Voshchanov, who quit in February as Mr. Yeltsin's press spokesman. 'The problem is a sense that there is no political or social course. There seemed to be a start toward a civilized market, but now it's faltered -- taxes, corruption, instability, wavering authority, a new decree every day. The democratic idea has been devalued, wars rage along the periphery, the army is disorganized, statehood is collapsing.' (An official recently warned Yeltsin, saying that) > -> 'No doubt remains that the economic-political course of the current Government is at a dead end. The fall of production in conditions of inflation, combined with various political factors, is leading to the uncontrolled regionalization of Russia, to her disintegration." End of quote.

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Victor Shnirelman, writing in February, added this information: " . . And professor's salary is about 1000-2000 rubles per month. It means that one should be engaged in two-three different jobs simultaneously to make money. And there is no place for academic studies in this schedule. The employment possibilities for academics are shrinking. Only an example --> The department I worked in until recently is non-existent now; it was closed. I still have a job, but I had to shift to quite another field. You are quite correct that it is not a good time for prehistory and comparative linguistics here now. Academic Institutes lost a great deal of their former support. My Institute has not any money for field researches this year. It is impossible now to go abroad for conferences and the like because of crazy prices that should be paid partly in hard currency (and this part is enormous!). So, this only in brief what are living conditions for intellectuals here. Probably, it is boring but I had to respond to your challenge and give you some information."

(Editor's note: Winfred Lehmann suggests a good way -- contact a local colleague likely to travel to Moscow on business. Ask her to be your Pony Express. Best bets are: Slavicists and returning Russians; or merchants and journalists or Finns. ASLIP can furnish you with a list of names and addresses. Mark Kaiser, Vitalij Shevoroshkin, and Ekkehard Wolff can do the same.)

(2) Hypotheses in Conflict: A review of the battlefield.

At this point in the evolution of the "emerging synthesis" it behooves us to sum up the points at issue -- in the main, not in detail. For long rangers collectively it is useful to take stock in a summary fashion. We must thank Colin Renfrew for the apt name for the interdisciplinary effort, truly an emerging confluence of separate rivers of inquiry. He himself has produced some general hypotheses about Nostratic and/or archaic human plus biological factors plus Old World archeology. We have not had space enough to re-publish his views but they are powerful and exciting. It is not unpleasant to see the strong resemblances to the "Borean hypothesis" offered you in MT-14 -- naturlich!

At almost every point where the probing fingers on the frontier have threatened to dis-establish previous theories or models there are now wee battles raging. For some people there is a great deal at stake and everyone can understand their passion in resistance. Some of the conflicts resemble paradigm revolts where very serious changes in belief and methods might have to occur in order to accomodate the new models. Everyone has my sympathy because many have been bruised already. Yet it seems worth it because the outlines of a whole new discipline, or a major realignment of sub-fields in anthropology, are emerging.

A. The battle over an African origin for Homo sapiens. There are several conflicts here. First, there is an old theory of pan-human common or joint evolution, which I call the rising tide lifts all boats theory, which relies primarily on postulated gene flow around the world to explain why such distant and dissimilar populations as Nigerians, Papuans and Swedes are all in the same species, can interbreed, yet have been separated geographically for hundreds of thousands of years -- since the time of emigrant Homo erectus out of Africa. Developed by Franz Weidenreich and Aleš Hrdlička in the early 20th century, it barely survived its advocacy by Carleton Coon in the 1960s. By the 1980s it was just hanging on, until revived by Milford Wolpoff (U/Michigan). The opposing model was called the Garden of Eden by Wolpoff and others -- quite an apt label. Its roots can be traced back to the Judeo-Christian Old Testament -- at least. That does not invalidate it, of course (heh, heh). Modern theories of this ilk fundamentally reject the proposal that humanity remained one species, even after a million years of dispersal around the Old World where thousands of generations lived in vastly different habitats with all their selective pressures. Finally, the Garden of Eden was not necessarily in Africa; advocates of an Asian homeland (especially China) and a South American, as well as a European one, are to be found. (Those who argue that Neanderthal was the ancestral European usually also favor 'rising tide' theory but some believe that Neanderthals begat Homo sapiens)

B. The struggle between biogeneticists and the fossil hunters. Our colleagues in paleoanthropology and archeology have long had deep prehistory all to themselves. Careful measurements taken on bits of old bone -- with luck most of a skeleton -- combined with careful geological and survey work, lots of high tech analyses of debris, experienced field crews and plenty of luck have been the basis for reconstructions of the bodies and general attributes of our remote ancestors. Scientific conceptions of human evolution, as well as popular ones, rest solidly on the fossil base and the analyses of the fossil hunters. But now the geneticists have invaded their turf, proposing theories based on comparisons of bits of flesh & hair and drops of blood. Drawing upon the considerable power and prestige of modern genetics and molecular biology, they relate modern populations to each other and reconstruct taxonomies, indeed phylogenies, with dated ancestors and homelands, yet never work in the dirt under the sun out in the bush. Precise lab work and powerful mathematics, combined with genetic theory and computers, are their forte. Neither group of theorists needs to disagree with the other. If a model is true in biogenetics, it ought to be confirmed eventually by fossil evidence. And vice versa.

In fact sometimes we can see arbitrariness or subjectivity that explains why there is not more agreement. The careful measurements and analyses of the fossil hunters are often more subjective than is generally realized. Disagreement about the role of Neanderthal in human evolution is commonplace, for one example. There is a lot of 'art' in paleoanthropology; witness the bitter quarrels between the Leakeys and Johannson. Granting their brilliant forensic skills, still Hercules Poirot will sometimes disagree with Sherlock Holmes.

It is not in the forensic arts that the biogeneticists find their vulnerability; it is in their calculations and deductions from theory. One can see right through their work to where they draw their conclusions; there is no hiding behind a skull which only three other colleagues have ever examined. As we will see below, their calculations are easy to attack, but not necessarily to refute. A final note would be that, despite the vigor of Wolpoff's attack on the African origins theory, a number of competent, verily world class, paleoanthropologists disagree with him and support the biogenetic conclusions. Reports in the American press have been plainly biased lately on this matter.

C. <u>The nasty fight over Greenberg's Amerind hypothesis.</u> Since this is very well known to long rangers, suffice it to say that there seems to be a stalemate. The evidence presented by Greenberg has not been confirmed or refuted, as far as I know, although some of his etymologies have been rendered doubtful by criticisms. Supporters of the amorphous opposing theory, i.e. that the Americas have far more linguistic phyla than all of the Old World, seem to argue by insisting and raising the criteria for acceptability. This is a problem well known to pole vaulters.

Other elements in this conflict include persistent attempts by Shevoroshkin and other Muscovites to extract some Amerind branches from Greenberg's overall Amerind and relate them to either Na-Dene or some Asian phyla instead of Amerind. Almosan or some other branch with representatives on the northwest coast of North America is usually involved. So far as one can tell, none of these Muscovite proposals have been adopted or attacked by Americanist border patrols; nor has Greenberg said much publicly about them. Some of the Muscovite effort here seems more like a willy nilly patching together of similarities, replete with dubious reconstructions, rather than serious attempts to test the overall Amerind hypothesis. Other than resembling loose cannon balls on the deck, it is hard to say what their role really is in the great debate over Amerind. Maybe 'doing their own thing'.

D. The puzzling and silent struggle over Indo-European. Beyond this lies the acceptability of Nostratic and/or Eurasiatic. The puzzle in this is the complete lack of Greenbergismus and the total committment to traditional Indo-European methods on the part of the advocates of Nostratic, viz. the Muscovites, Bomhard, Hodge, Levin and others. One cannot criticize them for not revering sound correspondences or not reconstructing or not using good data or any of the other baloney some throw at Greenberg. So what is the problem? Indo-European still cannot be related to Uralic or Altaic or Kartvelian despite the mountain of starred forms offered up by Bomhard, Dolgopolsky and the Muscovites? Why not? Nobody knows! The traditional Indo-Europeanists seem to be handling this problem by silence and silent disapproval. That is surely easier than trying to give reasons for their opposition!

E. When did ancestral Amerinds invade the Americas? There is also the opinion that the 'Garden of Eden' was in South America. However, the main & crucial issue falls between 40,000 and 13,000 years ago. The cautious Americanist archeologists adhere by and large to the Clovis horizon model (13,000 BP) which by now has become well established. Yet some have drifted into neutral scepticism. Though a quiet and steady erosion of belief in the Clovis dates goes on, still few Americanists seem willing to support MacNeish, Dillehay, or Guidon openly. While some have involved their passions in the dispute, it seems rational and polite on the whole to my ears. But MacNeish and his crew report that the pole vaulter syndrome has been applied to them too.

F. Who came to Japan and when and whence? Japan is turning out to be the mystery of the eastern Old World. Where once the Ainu with their supposed Caucasoid hairiness were mysterious 'archaic' residues of the distant past, now it is the Japanese themselves. There is good evidence to connect the Japanese with the Koreans and other northeast Asians -- linguistically and physically. Yet there is good evidence to connect the Japanese with Southeast Asians, especially Austro-Tai, linguistically and physically. The archeology of Japan seems to accomodate either conclusion. The old Ainu problem is reviving, there being good reasons to relate their bodies to both northeast Asia and to southeast Asia. At some points linguistic hypotheses flatly contradict biological ones while elsewhere archeological conclusions undermine linguistic ones. Yet there are lots of good data on most aspects of these problems and numerous smart scholars at work. Japan at least is one very good place to remember Franz Boas's famous dictum -- there are no necessary relationships among race, culture and language. We remind the reader of Ben Rouse's discussion of Japan in his 'migrations' book. In MT-1 & 2.

The linguistic classification of Japanese-Ryukyuan is crucial. Controversy about that is getting hotter. Paul Benedict has, malheureusement, irritated the Altaicists who were already frustrated and angry about the attacks on Altaic in Germany and elsewhere. One has recently told me of his outrage over Benedict's hypothesis; I have chosen not to share it with the rest of you because it displays only his passion, not linguistic arguments. Much of the problem in settling this dispute is that the Altaicists seem to know little about Austro-Tai and (I also suspect) Benedict is not too well versed in Altaic.

VOLUNTEERS! That's what we need. This is Paul Benedict's suggestion. How about some long ranger(s) who have acquaintance with Altaic and Daic or Austronesian trying their luck at classifying Japanese? You can read Roy Andrew Miller's proposals and Benedict's or go straight to the Japanese data. Your opinion will be honored. Bert Seto is perhaps best equipped of all to make such an attempt, carrying all that Japanese data in his head as he does. At the moment the Altaicists who claim Japanese as an Altaic language are ahead on points because both Greenberg and Starostin have also classified Japanese as near or in Altaic. Forsooth, were it not for Paul Benedict's strong argument plus his eminence as a long ranger, one might say that the Altaicists had won the match on points.

However, Allan Bomhard and I separately reached the conclusion that both parties to the dispute may be correct. Japanese might relate to both Austro-Tai and Altaic but not in the same ways. Its primary or most recent affiliation may be with Altaic, hence Eurasiatic, hence Nostratic, but Benedict may have found evidence of a deeper or older affiliation with Austro-Tai, one that must also be shared by Altaic -- or it is a Mischsprache, so suffused with old borrowings from Austric or Austro-Tai that its taxonomic proximity to Altaic is concealed? More on Japan below in The News, and in the review of Sydney Lamb's new book below.

We should mention Karl Krippes' admirable review of the problem in his article, "The Genetic Relationship between Japanese and Austronesian Revisited" in UAJb 64 (1992), 117-130. As a Koreanist par excellence, yet one obviously very familiar with Japanese linguistic research, Karl is critical of Paul Benedict's command of the literature on Korean, Altaic, Japanese plus Japanese efforts (e.g., Murayama's) to relate Japanese to Austronesian. Like Sam Martin (see below in book review), he singles out the 1985 Harvard linguistics doctoral dissertation of John B. Whitman, "The Phonological Basis for the Comparison of Japanese and Korean", as vital to such comparisons. Karl makes no pretense of being a neutral commentator -- being a recent grad of U/Indiana's famous Altaic program -- but his treatment of Paul Benedict is sober and non-polemical. Apparently conclusions similar to Bomhard's and mine -- essentially outsider opinions -- have been in the literature on Japanese for many years.

(G) Could Neanderthals talk? Or did they just grunt and sign?

Philip Lieberman and his champion, Jeffrey Laitman, proposed that the Neanderthals were NOT human language users because of deficiency in their anatomy. Their critics in France & USA match them in strong debate. Battle rages, issue undecided. Formidable!

SPRUNG from SOME COMMON SOURCE:
Investigations into the Prehistory of Languages. 1991. Sydney M. Lamb and E.Douglas Mitchell, eds. Stanford University Press, Stanford, CA. Pp. 411, maps, figures, tables, index.
Reviewed by Harold C. Fleming

Long in gestation this impressive book sums up the Symposium on Genetic Classification of Languages, sponsored by Rice University and held at Houston, Texas in March, 1986. Not by chance was that year the 200th anniversary of Sir William Jones' famous speech to the Asiatic Society in Calcutta. Roughly half of the papers concern Indo-European in itself or its possible relations with other language families. But the other half venture far beyond that topic, both in the study of particular phyla and the very interesting papers on mathematics and historical linguistics. Since the impact of this conference was considerable, one can label this The First American Long Ranger Conference. Moscow had another famous one in 1984.

Sydney Lamb and Douglas Mitchell should be praised for their imagination and inspiration in conceiving such a conference and thanked for seeing all the papers through to completion. Most of the papers are up to date in information, despite the five year delay in publication, and some of them make very telling points. For me perhaps the most informative paper was Roy Andrew Miller's on "Genetic Relations Among the Altaic Languages" which makes one grasp finally the peculiar history of work on that phylum. How an incompetent scholar of high status can tear apart a valuable enterprise, generating negativity and obfuscation lasting a whole generation. The authority who didn't really know what he was talking about -- we have had them in Africa too.

There are eighteen papers, gathered together in five major parts, plus a valuable introduction by the editors. The first part concerns Sir William Jones, his ideas and context, with reviews of Indo-European history and development. Winfred Lehmann and Garland Cannon hold forth admirably. Lehmann represents mainstream Indo-European linguistics, as his fellow ASLIPers know. Cannon is a historian of linguistics, particularly an expert on Jones. He reveals an interesting split in assessments of I-E's beginnings. Those most enamored of the strict comparative method, especially Henry Hoenigswald, tend to see 'sound' historical linguistics and I-E starting with the Germans Bopp, Herder, Rask and Grimm. Those more inclined to a less judgmental view see the inspiration and example of Jones as the real beginning of I-E. No one doubts that German scholars of the 19th century fundamentally built I-E and no one claims that Jones had much influence on them.

Part Two is supposed to belong to I-E and so it does, but more in terms of religion and (other) culture than technical linguistics. Jaan Puhvel provides a rational or commonsensical approach to Hittite, I-E taxonomy, and problems of 'fringe' dialects like Hittite versus 'central' dialects like Greek or Sanskrit. He sets up tenets which boil down to this roughly: lexical or grammatical items shared by Hittite and Tocharian or some western I-E fringe dialect like Keltic or Germanic are most likely to derive from early (=proto-) I-E; other sharings between I-E dialects may be explained in a number of ways. So, if Greek and Armenian share many things (which they do), those things are not necessarily from proto-I-E. Edgar Polomé is concerned with the retrieval of I-E culture, in this case religion, through the reconstructed vocabulary of I-E. His paper is one of a genre, a rich genre I might add, of attempts to know the Indo-Europeans via language. Paul Friedrich's valuable efforts on I-E kinship terms come to mind. Polomé focuses on the work of Gamkrelidze and Ivanov and finds it stimulating. "despite its shortcomings, it reflects fairly well the present state of our linguistic knowledge.." and more progress will be made when the authors look outside of linguistics at cultural anthropology and archeology. Maria Gimbutas has a longer paper on "Deities and Symbols of Old Europe and Their Survivals in the Indo-European Era: A Synopsis". Her Old Europe (including Anatolia) was the Neolithic which lasted from 9000 years ago until the Indo-Europeans overwhelmed its local cultures between 5000 and 3500 years ago. Her picture of the old folks confronting the I-E pastoralists has been until recently the dominant view. It goes like this: "The world view of the agricultural and matricentric Old Europeans was diametrically opposed to that of the patriarchal Indo-European ideology that more or less successfully destroyed it, transforming social structure and religion from matrilinear to patrilinear and from matrifocal to patrifocal, from matristic and gynandric (female / male balanced) to androcratic (male dominated)." One can suppose that the early Aryans were probably a very patri-lineal/-archic / -focal group of people; that seems given. The other question is whether the proliferation of female figurines, especially the famous 'Venus of Willendorf' fat lady, necessarily mean 'matri' type qualities. Both contemporary Denmark and Holland, and the USA and Japan, abound with displays of naked females, albeit not swollen by pregnancy, yet strong social movements strive to throw off the accursed male dominance. I am only raising this question, not objecting to Marija's thesis which I suspect is true.

Part Three, "The Search for Relatives of Indo-Europeans", is named properly but too Eurocentrically because this section of the book bursts free of the bonds of the I-E mind set on taxonomy and explores larger things like Nostratic, Dene-Caucasic and the outlines of Borean (without mentioning it). A paper by Merritt Ruhlen in Part Four logically belongs here too. This section could be called the Greenberg and Muscovite united team effort. Papers by Greenberg, Carleton Hodge, and Saul Levin concentrate on the evidences which link I-E languages to external families, such as Eurasiatic or eastern Nostratic (Greenberg), Afroasiatic (Hodge), or Semitic (Levin). A fourth paper by Vitalij Shevoroshkin and Alexis Manaster Ramer, entitled "Some Recent Work on the Remote Relations of Languages", goes the farthest, creates the most excitement, and presents a substantial amount of hard-to-get Russian data and reconstructions. In Part Four below some serious questions are raised about the quality of those reconstructions, an inquiry which is long overdue.

Greenberg's paper reviews a small part of the morphological evidence supporting his Eurasiatic hypothesis -- 2 grammemes out of the 70 which he has amassed --, discusses his reasons for not including such perennial Nostratic stalwarts as Kartvelian, Dravidian, or Afroasiatic, and presents a few startling comments on the future of what was west Nostratic. First, a word about the membership of his Eurasiatic. Being anchored in I-E as it is, his classification inevitably confronts the question of I-E's closest relative. While noting that some key people have suggested Uralic, he remains silent on that question. It may shock some people to find that Semitic, hence Afroasiatic, not only fails to be genetically close to I-E but also has its alleged previous closeness attributed to 'nonlinguistic' factors (like the expectations of Europeans and Jews to find common kinship with each other, due to sharing Biblical myths). One is reminded of Sir William Jones's remark that he could not relate Arabic (Semitic) to Indic (I-E) any more than he could relate Tatar (Altaic) to either of them.

Yet another surprise is the rejection of Uralic + Altaic as a special sub-class of Eurasiatic, while another is the excision of Japanese-Korean-Ainu from Altaic and the separation of Gilyak from both of them. His seven Eurasiatic groups from west to east are:

Indo-European, including Anatolian
Uralic + Yukaghir (remote eastern Siberia)
Altaic: Turkic, Mongolian, Tungusic
Ainu-Korean-Japanese
Gilyak
Chuckchi-Kamchadal or Chukotian
Eskimo-Aleut or Eskimoan

What Greenberg finally does with Afroasiatic is so deft and understated that the implications do not dawn on one quickly. In my second or third reading I finally realized that he had set up most tentatively a large southern Nostratic which incorporated Nilo-Saharan (one of Africa's super-phyla) and Elamo-Dravidian. What is not included in this new quasi-phylum, somewhat vaguer than Borean but clearly at odds with it? Kartvelian and Afrasian, the remnants of old west Nostratic! Yet the pair are not grouped together in a specific taxon, even though they are said to be related to each other and the others at some deeper level. Greenberg, it seems, has tried to do the sub-classification of the giant and over-extended Nostratic super-phylum which many had urged be done; he has partly succeeded.

It is not trivial to note -- once more -- that the vigor of research in the past six years has basically upset the Nostratic apple cart, whilst preserving, even extending, the idea of genetic relationships among the individual families. The original Nostratic of Illič-Svityč, Pedersen, Dolgopolsky, and the earlier Bomhard is outmoded now. Old west Nostratic may now be an invalid taxon, as noted above. Moreover, the carefully constructed phonological rules created by Hodge for the parent of I-E and Afroasiatic (his Lislakh) will have to be re-evaluated. It is not that they are wrong but rather their target is mistaken; while ignoring closer relatives, they focus on a remote ancestor.

Bomhard and Dolgopolsky, for examples, try to recreate the same ancestor but they pay strict attention to Kartvelian, Dravidian, and the Eurasiatic group in the process. For is not the ancestor that Hodge creates one and the same as Bomhard's proto-Nostratic? Yet one with a smaller data base, hence accuracy. Nevertheless, some of Hodge's sound correspondences are not only different from those of his peers, as the editors mention, but probably more accurate. In other words my criticism would be of Hodge's strategy, not necessarily his tactics. He is a fine craftsman!

So, whatever the Nostratic variety that one believes in, has everyone not seen Kartvelian as an important presence between Afroasiatic and I-E? Just as Uralic has been repeatedly mentioned as I-E's closest neighbor and likely sister phylum? What then is the logic or sense in pursuing the exclusive ties between I-E and Afroasiatic? Why does everyone love binarism so much? Because it's easy!

Nor can Levin's dogged pursuit of I-E and Semitic (which Anttila calls 'Indo-Semitic') slip by much longer without serious debate; it is one he has started. Levin's rationale is this, his preface (p.166):

"It devolves on me to point out the necessity of comparing Indo-European phenomena directly with Semitic, instead of treating Semitic as just a subdivision of Afro-Asiatic. The preoccupation with Afro-Asiatic, no matter how intense, cannot really give to this loose constellation of language families a coherence on the order of Indo-European (IE), but it can and does keep many linguists from noticing the phenomena within Semitic and within IE that are clearly linked across family lines."

(His footnote #1: "This is not a criticism of Prof. Carleton Hodge, who at the symposium in Houston represented Afro-Asiatic studies. On the contrary, he is noteworthy for his deep interest in Semitic as well as IE")

What is the sense in pouring all his vast erudition into an invalid taxon? Is the study of Semitic to remain forever an oddity -- a family which is usually compared with distant strangers instead of with its own true kin? Why don't Italicists devote themselves to studying the links with Semitic, instead of Keltic or Albanian ? One regrets to add that Levin's attitude does serious damage to Afroasiatic and other long range comparison because it denigrates the solid genetic unity of an ancient super-phylum -- even Ringe of Pennsylvania thinks Afroasiatic is one of the verities like I-E and Uralic (cf the BBC tape). Malheureusement Levin's argument has specifically been used in the attempt to shut off long range comparisons. His Binghamton colleague, Paul Hopper, used almost identical words in dismissing anything older that 6000 years or so -- which really means anything older than I-E -- in his attack on long range comparison, reported in MT-12, p.15. If Semitic is not Afrasian, then what is it? The closest relative of I-E? No way, José!

Finally, we realize the theoretical importance of Afrasian. For those now pushing the '6K' (6,000) theory that strangles the inquiry, Afrasian is a threat. It is obviously much older than

6000 years. If it is not more than a 'loose constellation', then apparently linguistic thugees are not afraid of it. But as a valid taxon, a real, knowable historical and genetic Entwicklung, it refutes their theory. As Hodge argues (p.141), "Archeological evidence indicates that the time depth of the proto-language involved (Lislakh - HF) is over 16,000 years, possibly 20,000 (Munson 1977, Hodge 1978). The proportion of items attested as having survived over 4,000 years within Egyptian (my underlining - HF) (Hodge 1975) gives us confidence in the relatability of languages at the greater time depth (Swadesh 1959:27)." Hodge 1975 refers to his "Egyptian and Survival", in Bynon, J., and T. Bynon, eds. HAMITO-SEMITICA, pp.171-191. Mouton, The Hague.

(Reviewer's note: Written Egyptian offers us a bit more than 4000 years of its history. Yet evidence of shared words between Egyptian and old Semitic, or their descendents, must show more like 10,000 years. None of this to denigrate the remarkable morphological evidence, especially verb paradigms, which connect Afrasian branches much more anciently separated than that!)

Shevoroshkin and Manaster Ramer's paper is a gold mine! A large part of the Muscovite research is summed up therein and it is impressive for its boldness and sense of responsibility. Much of this research has been broadcast to long rangers during the last six years and/or published in books edited by Shevoroshkin in recent years. Here I will just mention the topics covered and mention that the supporting data are fairly clear and convincing --it's a good paper!

The main topics are: (a) Nostratic, especially morphological evidence; (b) Sino-Caucasian, incorporating a lot of Čirikba's evidence on Basque and showing part of Ivanov's evidence for Etruscan being a Sino-Caucasian language; (c) Dene-Caucasian, showing some of Nikolaev's proto-Eyak-Athapaskan, or roughly Na-Dene, and an unusual tentativeness. The omission of Haida remains a serious flaw in Nikolaev's hypothesis; (d) some methods and consequences, including their noting that Dolgopolsky's list of the "stablest" meanings ought to be used, that "multiple comparison is better than binary" and just why that is so, and that successful hypotheses should be fruitful, i.e., they shed new light on sound correspondences and aid reconstructions; (e) wider connections: Nostratic and Sino-Caucasian, citing some etymologies proposed by Starostin and G.K. Verner; (f) Nostratic Connections with Amerind,, drawn up by the authors collaborating with Merritt Ruhlen, culminating in nine etymologies showing Sino-Caucasian links with Amerind.

A few extra comments can be made. Noteworthy is a qualified acceptance of the Amerind hypothesis, "but before any reconstruction of proto-Amerind can be possible, it will be necessary to have reconstructions of the individual subfamilies" (p.180). I also have difficulty with their inability to see that giant groups like Indo-Pacific and Australian are not "smaller families" plus their continuing inability to perceive Niger-Congo and Nilo-Saharan as existing, not to mention important. But Moscow is in the vast Eurasian north; this is the view one would get from there. I look at everything from Ethiopia. They also define Austric as Austronesian and Austro-Tai, really meaning Austroasiatic and Austro-Tai. See Shevoroshkin's letter in MT-3, p.IV., for the correction. In effect the authors have laid out some of the evidence for the Borean hypothesis, showing the links among Afrasian, narrow Nostratic, Dene-Caucasic and Amerind. We thank them. To repeat: theirs was a good paper and the supporting data rather good, certainly better than some offered in the past; there is still plenty of room for quibbling. For example, in his proofs that Etruscan is a Sino-Caucasic language, Ivanov compares Etruscan [s/śa] = 'four' with Northwest Caucasic [-s] (with a hook on it) and [-ĺâ] = 'four'. In fact all Northwest Caucasic forms for 'four', including those cited by Catford, contain an initial [p], yielding forms like [phlâ] cited in MT-1. Either Ivanov knows something the rest of us don't, namely that the [p] is some kind of numeral classifier, or he is cheating. Or he is mistaken.

Merritt Ruhlen's paper will be discussed here, not in Part Four, because it deals with Amerind. It is preoccupied with internal taxonomy (sub-grouping), rather than etymologies or other proofs, and with the consequences of that taxonomy for the unfolding of Amerind prehistory in the New World. Using the method of shared innovations and borrowing some statistical methods from Cavalli-Sforza, Ruhlen concludes that Central Amerind is coordinate to all the other branches (or sub-phyla), i.e., the Central Amerind bunch got separated from the others before they separated from each other. Who are the Central Amerinds now? Basically, they live in northern Mexico and southwestern USA, speaking Tanoan, Uto-Aztecan, and Oto-Manguean languages. One of their clusters conquered central Mexico where their enclaves can be found. Such pueblos as Tewa and Taos; Kiowa; the Mixtec and Zapotec; the Nahuatl and Yaqui but also Shoshoni, Pima and Hopi; these are a good sample of the Central Amerinds.

The hundreds of languages outside of Central fall into two gross groupings: Northern Amerind and Southern Amerind. Northern occupies most of North America, including much of Mexico (e.g., Mayan), while Southern is partly in central America but occupies most of South America as South American Amerind. The latter divides into Andean and all the rest, suggesting very strongly an original occupancy of the western mountains before a great expansion into the lowlands and the Amazon. As Ruhlen reconstructs the presumptive homelands of the various sub-phyla and their later movements -- a daunting task one can see from a map -- a clear basic picture emerges of a first home near Montana followed by movements south and then movements eastward on both continents. In any event some groups later moved westward and some northward. Then to complicate things more the Na-Dene piled in on top of the native North Americans so that the Navaho-Apache cluster of Na-Dene separates the Central Amerind Taos and Hopi from each other and the North Amerind Zuni, another pueblo folk. Then came the Mexicans, some Basques, the Mormons, the Texans, and the rest of us as tourists. I cannot evaluate Ruhlen's thesis, even though it is very clear, because the work involved would be formidable. But, if he is right, thinking about Amerind prehistory will now be easier and the Greenberg hypothesis easier to savor and test.

Part Four. This is now a potpourri with its focus on Asia. The article by Søren Egerod on "Far Eastern Languages" is immensely erudite but confusing to this reviewer. Some places he produces fairly standard family trees and areal maps of phyletic distributions; however his brief excursion into Africa is way out of date and seriously flawed. In east and southeast Asia where his expertise is obviously based he agrees for the most part with Benedict, Peyros and others, even including Japanese within his Austro-Tai, but also accepting Austric which Benedict rejects. Then he attacks the same languages with typology and leaves me scratching my head.

Ian Catford, colleague of Shevoroshkin at Michigan, has produced a gem of a paper on "The Classification of Caucasian Languages". (I do wish he and his peers would call them Caucasic languages, like German, Italian, and French do, because Caucasian has been preempted in English to mean 'white person'. You can hear it in police reports all the time: "Male Caucasian arrested for uvular trilling.") Catford's paper by itself is worth the price of the book but it is too rich to discuss adequately here. Since Caucasic languages traditionally include Kartvelian, one finds that discussed; he follows the Nostraticists in separating it from North Caucasic but discusses both groups carefully and comparatively in all respects. Since North Caucasic is famous for noun classes, few or no vowels, and scores of unpronounceable consonants, one finds out about these things too. It may surprise some to know that, while Catford concludes that Northwest has two or three primary vowels [â, a] or [â, e, a], depending on the whether it is Abkhaz-Ubykh or Circassian, some Northeast Caucasic languages exhibit "a certain exuberance" in vowels, maybe 30 vocalic nuclei, 15 vowels and 15 diphthongs but nine basic vowels.

The discussion of grammatical features is fascinating and will delight any true grammarian, like many long rangers. One cannot help being struck by the noun classes of Basque, North Caucasic, proto-Sino-Tibetan (at least) plus Tibetan, and Na-Dene. There are very rich grounds for comparison therein and for testing the Vasco-Dene thesis. The non-linguist should know that noun classes are not restricted to these languages. In phonology, while living Caucasic languages get up to 80 consonant phonemes (Ubykh), reconstructions often get more than that. Proto-Lezghian has 101 consonants, while proto-North Caucasic has 180. Wow! Both are based on the work of Sergei Nikolaev and Sergei Starostin. If they are true, they represent a tremendous accomplishment. Catford is very respectful. But they strain our credulity, just as the 130 consonants of one Khoisan language did before. Unfortunately, the basis for creating these 101 or 180 consonants is not quite clear -- thus inevitably causing doubts about them -- and Catford awaits the publication of their ETYMOLOGICAL DICTIONARY OF NORTH CAUCASIAN LANGUAGES. We must wait too. With luck it really will be in English! With even more luck we may get Catford to review it for us in MOTHER TONGUE!

Samuel E. Martin, or usually Sam Martin, has a tough assignment, coming after the Catford article. Sam's paper on "Recent Research on the Relations of Korean and Japanese", however, quickly shows its merit. It is very informative and convincing. Whether the genetic classification of Japanese is settled or not is another question, of course. However, Sam Martin's testimony is special because for many of us his Japanese-Korean hypothesis of 1966 has been 'the word', truth as we knew it. Yet in 1987 at the Stanford conference he seemed to disavow that hypothesis. 'Seemed to'? He told me directly, standing at the standard American 18 inches distance, that he "used to believe in Japanese and Korean but I don't anymore". That was reported to long rangers. Thus his reversion to his own hypothesis is a surprise, but not entirely a delight, since in the interim Paul Benedict's appeared.

It is probably quite important to distinguish once again between two separate issues. The one is: does Korean have a special relationship with Japanese? The other is: should Japanese or Korean for that matter be classified as Altaic languages? If the answer to the first is 'yes', then the Austro-Tai hypothesis will have to incorporate Korean too or admit defeat. The first issue is now joined with very specific phonological reconstructions and sound laws proposed by both Sam Martin and Paul Benedict -- and they are at variance with each other. There is very little an outsider can do to settle the issue because it is extremely technical. Perhaps they can convince each other or persuade others expert in Japanese or Korean of their correctness. In any case the issue is not at all an exercise in the 'taxonomy first' tradition. The court, the judges and the lawyers in this case all play by Indo-European rules. The issue is more analogous to someone arguing that Hittite is related to Hurrartian and Khatti, instead of Armenian or the rest of I-E. Most of the data are already known and analysed; details of morphology and theories of sound change will determine everything.

For the non-linguist we should point out that Japanese and Korean are not very close languages, their potential relationship is not obvious lexically, the common vocabulary was once estimated by Martin as around 20% (something like German and Bengali), both are like French in having much erosion of old words, and both show great influence from Chinese civilization and language. Yet some linguists have argued that the morphology and syntax of the two are much alike. One colleague, Owen Loveless, was arguing in the late 1950s what has become a classic: "Japanese and Korean are related structurally without a doubt and they are unrelated lexically without a doubt."

<u>Roy Andrew Miller's</u> "Genetic Connections Among the Altaic Languages" is an extraordinarily informative short history of linguistic 'science' as realized in one area (north Asia) and one time (mid 20th century). There is no doubt at all that Miller is a vigorous, passionate advocate of one position -- that Altaic is a valid taxon and it includes Japanese and Korean -- but that does not necessarily mean that his account of events is untrue. One may understand one's opponents very well. The real question is whether one lies about them. That appraisal will be made by Altaicists and anti-Altaicists but I suspect that they will agree with each other.

Miller's basic thesis is that such scholars as Ramsey and Poppe and Menges had established a good solid Altaic, or were shortly to accomplish that, when one high status person, Sir Gerard Clauson, took it upon himself to do the opposite of Sir William Jones, viz., to unravel a valid taxon, to undo the work of others and to leave little or nothing in its place. Miller points out that Altaic had been started as early as 1730 by Philip J.T. von Stralenberg, a Swede, and by 1850 was fairly well established, having also been named Altaic instead of "Scythian" or "Tartar" or the like. Clauson attacked Altaic in 1956, citing a lack of basic vocabulary shared among Turkic, Mongolian, and Tungusic languages. His argument was picked up and expanded by Gerhard Doerfer in "four thick volumes" between 1963 and 1975. Among other things Doerfer's key argument was that borrowing of words, especially between Turkic and Mongolian, could account for the few lexical similarities. I can report that Doerfer's argument was reverberating among the Altaicists at the Stanford conference, or rather the anti-Altaicists. (See MT-4 for more on that conference) Doerfer's position became the classic anti-Altaicist position which has also -- curiously enough -- become the dominant attitude among "Altaicists". It seems to be like anti-matter!

Miller's argument, or perhaps 'diatribe" will please some, should be read in itself. He has retired from U/Washington and now lives in Honolulu in or around U/Hawaii. Contact him for more information. But one key counter-argument of his resonates with Owen Loveless's old argument, to wit, the Altaic languages share a tremendous amount of morphology (structure) and Altaic as a taxon could be proposed on structural grounds alone. 'Damn the bloody lexicon! Full speed ahead by grammar!' Another key point of his is that the alleged borrowings become true cognates when Japanese and Korean comparisons are brought to bear on the issues, as in how could Japanese (for example) have borrowed an old Turkic word and in a form more archaic than the Turkic? Geography and sound laws both rule out the borrowing.

Part Five in general is called "Methods in Genetic Classification of Languages". Well, this section is misnamed because the whole book is about methods in part and from time to time. Not the least effort is the editors' own introduction. Aside from a paen of doubt and confusion from the eloquent and gifted Robert Austerlitz, enthralled with the mystery of it all, the section is about mathematical aids. Sheila Embleton contributed "Mathematical Methods of Genetic Classification", while Robert L. Oswalt proposes "A Method for Assessing Distant Linguistic Relationships." Clearly these matters are too complex for even partial discussion here. Suffice it to say what the editors said in their introduction to Part Five (p.351): "Sheila Embleton presents a survey of mathematical techniques for genetic classification, using the family tree model as a primary basis but with great attention to incorporating the consideration of borrowing among related languages, an area in which much of the mathematical work she describes is her own. She is thus developing an enrichment of the family tree model."

"Robert Oswalt concludes the volume by describing a computerized technique for developing and testing hypotheses of distant genetic relationships among languages. One of its welcome virtues is a simple but effective means of determining whether observed similarities among any given languages can reasonably be accounted for on the basis of chance. His technique is not intended as an alternative to the established comparative method; rather, it is a device to be used before detailed comparisons are made, that is, a method for identifying those cases in which closer comparison is most likely to yield results. Oswalt's testing procedure, like lexicostatistical studies in general, is limited by its restriction of comparisons to items with identical semantic value. It thus invites an opportunity for future development: a systematic way of dealing with semantic differences, analogous to Oswalt's method of allowing for phonological differences."

Someone will have to say this eventually, so this reviewer might as well. As perceived by this mathematical innumerate, Oswalt's method does not test long range relationships even as deep as the parameters of Afroasiatic. It cannot reach too far back in time because it seems limited to binaristic comparisons, to Swadesh's short or 100-word list, and to semantic identity in cases to be judged cognate. What it will tell us is what we already know because such relationships are essentially obvious. When it gets beyond Indo-European or Uralic, it stumbles and runs out of gas. How can you test an hypothesis about 500+ languages like Amerind which has probable dates of 20,000 (Ruhlen, this volume) or 40,000 years ago (Fleming, MT-15) by using a method that only compares two languages at a time, cannot cope with the probabilities of thirty languages sharing the same cognate, and in any case can only test relationships of 10-15,000 years age? As Carl Hempel might say, testing hypotheses often involve assumptions about instruments. If Joe Lulu's theory is correct, then you should be able to see the red dwarf star on Tuesday night by pointing a powerful telescope at angle Q in the northern sky. Hah, but if you don't have a powerful telescope or any way of measuring off angle Q, you cannot test Joe Lulu's theory! Robert Oswalt's telescope is too weak to test most of our current theories, like Eurasiatic or Sino-Caucasic; indeed it probably would reject most of our tropical super-phyla like Niger-Congo. More ominously, it can be predicted to reject or falsify most theories of relationships beyond the obvious. It does not know that its own limitations are involved, not the failures of the hypotheses it cannot test.

As a final comment on the book as a whole, Sydney Lamb and Douglas Mitchell should be proud of themselves. And we should thank the much maligned Stanford University Press for publishing their book and presenting it in such a beautiful package.

HAS THE AFRICAN 'GARDEN OF EDEN' BEEN TOTALLY DISCREDITED?

It reminds me of the Olympics. Hooray for our side! Africans won 25 medals; Ethiopia and Kenya between them won 11 medals. Two of Ethiopia's were garnered by Oromo, one of them a woman. Most heart-warming in terms of old Olympic ideals of individual prowess was the gold medal of an Algerian woman. Getting to run at all was her first triumph.

Anyway there seemed to be a victory for the 'rising tide' team against the 'Garden of Eden' team this spring. I do not wish to impose a false sporting model on these events but it was very striking how one bunch of people was chortling with glee and another got gloomy. A note ('Technical Comment') by Alan Templeton (Washington U., St. Louis) was published in SCIENCE (Vol. 255, 7 Feb. 1992, p. 737). It was entitled "Human Origins and Analysis of Mitochondrial DNA Sequences" and said, in essence, that the family tree of woman, established by Cann et al in 1987 and confirmed by Vigilant et al in 1991, was invalid. Both of those papers were reported by MOTHER TONGUE with praise and excitement. (Wait! there is more.)

Templeton's key argument was that both studies had used the PAUP program in calculating their family trees, in finding the MP (maximum parsimony) analysis which resulted in their cladograms, and hence in concluding that the tree should be rooted in Africa with non-African branches being less ancient. BUT they had not used the PAUP program properly. Since the PAUP is a computer program, it demands a lot of computer time to work properly. What Cann and her colleagues had done was to make "a single heuristic run of the computer program PAUP with simple addition. . (which) is inadequate for a phylogenetic analysis of large data sets.", said Templeton. He also got trees with non-African roots many times and has an article on all this (in press) in the AMERICAN ANTHROPOLOGIST.

\>>>> The PAUP program can be examined if one obtains a copy of D.L. Swofford, PAUP, Version 3.0, Computer Program (Illinois Natural History Survey, Champaign, 1990.) or an updated 'User's Manual' of 2/9/91. (Different scholars used different variants of PAUP. Could this make any difference?) PAUP means Phylogenetic Analysis Using Parsimony and "it strives to find the 'most parsimonious tree' -- a tree that traces everyone's lineage back to a common ancestor with a minimum number of mutations along the way. The 'shortest path' is considered most likely to reflect what happened during evolution. Unfortunately, PAUP doesn't often offer just one most parsimonious tree for each sample. Indeed there may be millions of equally good trees. After each computer run a number of possible trees pop out -- and it's up to the researchers to decide how many computer runs to do, and how many hundreds or thousands of trees to ask the computer to save after each run for later analysis." (This
<<<< quote from SCIENCE, vol. 255, p.868).

Simultaneously, another blow was struck by David Maddison and team (Harvard). Their report will be published in SYSTEMATIC BIOLOGY or has been already, but tain't as harsh as Templeton's. Crew Maddison is reported on in SCIENCE (same volume) in a 'Research News' squib, entitled " "African Eve" Backers Beat a Retreat"; it also discusses another 'Technical Comment' by a Penn State team (see below).

>>>> It is a characteristic sign of intense activity or 'hot science' when papers submitted to journals are abstracted for faster publication in the big scientific weeklies. As our readers have seen, much of the mtDNA research falls in
<<<< this category. They can't get it out fast enough!

The Cann & Vigilant group, or the Wilson group as they are called most frequently, "drew its conclusions after looking at 100 trees from only a single run". Swofford joined the Maddison crew in writing the note; he told SCIENCE that "They shouldn't have stopped at 100. That's a very nonrandom sample of the trees that are considered equally good by the program. The group should have looked at trees from many different runs because all the trees produced on any one run are related." Swofford added that he didn't want to condemn the Wilson group for not using his complicated program to its best advantage, but he nevertheless felt compelled to point out the weakness in their analysis. Maryellen Ruvolo of team Maddison added that the lesson is a general one, saying "I think a lot of molecular workers haven't fully appreciated that . . . you have to search through [the tree] in a particular way, so you can actually give yourself the chance to reveal the trees that are most different." It would seem that the heat has been turned up enough for all to learn this basic lesson. The Wilson team has acknowledged it.

What happens when more runs or an 'adequate' number of runs are done? Something like chaos -- apparently! Templeton said his first run disproved the African rooted conclusion, supporting instead a non-African one. Team Maddison found support for both African and non-African rooted solutions and Maddison concluded that the non-African hypothesis is "basically just as good" as the African. However, his colleague, Maryellen Ruvolo, added that "there is one piece of mitochondrial DNA evidence from the Wilson group that remains unchallenged. That is the fact that Africans have greater diversity in their mitochondrial DNA than the inhabitants of any other continent. And that diversity is the strongest piece of evidence for an African origin -- because it suggests that, to accumulate the largest number of mutations, humans must have lived longer in Africa than anywhere else."

>>>> That last point was anticipated several years ago by a geneticist who maintained that the African rate of mutation, especially that of Khoisan-speakers who could not otherwise be explained, was greater/faster than that of the rest of us. Can someone help us remember this source and its
<<<< presumed rebuttal? Somewhere in that vast literature!

On the very page of Templeton's note came a combined acceptance of his main point and an attempted rebuttal to some of his conclusions. A team from Penn State, associated with Masatoshi Nei's well-known laboratory, averaged results from 50,000 trees. S.Blair Hedges, Sudhir Kumar, Koichiro Tamura, and Mark Stoneking had written their rebuttal 11 days after SCIENCE received Templeton's. Fast! One will recall that Mark Stoneking was a co-author in both articles of team Wilson. They pointed out that:

(BEGIN QUOTING) ". . .To determine the groups supported in all MP trees, we obtained a strict consensus tree (Fn.6) of the 5 X 10^4 MP trees (Fig.1A). Although this number of trees represents only a small fraction of the total set of MP trees, the poor resolution of relationships (Fig. 1A) indicates that parsimony analysis is unable to resolve the deep branches of the tree. Additional MP trees would not alter that conclusion."

"Our neighbor-joining reanalysis (Fn.7) resulted in a single tree showing some geographic cohesiveness among the Africans (Fig.1B). Most notably, all 16 !Kung form a group, in contrast with the original tree (Fn.1) where they were placed as 13 independent deep branches. This difference is important because it was the deep branching of the !Kung that provided statistical support for an African origin. Although the two deepest branches of our neighbor-joining tree lead exclusively to Africans (!Kung and Pygmies), those bifurcations are not statistically supported (bootstrap, P=0.13 and P=0.07, respectively). Only six nodes in the tree, all defining small clusters (two to six individuals), are statistically significant (bootstrap, P≥ 0.95).

"The reason that this reanalysis differs so greatly from the original study (Fn.1) is that the tree on which the first conclusions were drawn was not representative of the total set of MP trees. Thus, the two statistical tests made in the original analysis are not valid. Those tests cannot be performed on the trees presented in Fig.1 because their branching order is not statistically resolved. Although an African origin for humans is supported by other kinds of data and other molecular data (Fn.8), and is suggested by the mtDNA sequence data (Fig.1B), the available sequence data are insufficient to statistically resolve the geographic origin of human mitochondrial DNA."

"Templeton concludes that the original phylogenetic analysis (Fn.1) was inadequate for the same reasons described here. However, we note that the 100 trees he found are four steps longer (Editor's note: this means less parsimonious) than the 50,000 trees we have analyzed (Fn.6); hence, the tree he presents (his figure 1) is not an MP tree. Furthermore, the African origin hypothesis was not solely derived from the phylogenetic analysis; patterns of mtDNA variation within different human populations also have been used to support an African origin (Fns.1,9)."

"What data are needed to resolve the evolutionary history of our species if this data set, perhaps the largest available, is insufficient? The absence of a strong association between mtDNA sequence and geography, especially among the non-Africans (Fig.1B), suggests that the same multiple mtDNA types have been maintained in widely separated populations since those populations diverged, thus confounding an evolutionary interpretation of the data. DNA sequence data from multiple nuclear genes, in combination with the mtDNA sequence data, likely will be needed to overcome the effect of individual gene phylogenies. We then may be able to gain a better perspective of human origins and evolution." (END OF QUOTING)

>>>> Fn.1. L.Vigilant, M.Stoneking, H.Harpending, K.Hawkes, A.C. Wilson, Science 253, 1503 (1991).

>>>> Fn.6. (Editor's note: This concerns the strict consensus tree. For our few statistically sophisticated members we take the trouble to offer it here. Others may skip away to FN.7) "In order to obtain representative samples of MP trees from the total (unknown) number, we used the ramdom addition sequence of PAUP with maxtrees = 10^4 and obtained strict, semistrict, and majority-rule consensus trees of those 10^4 trees, each of length 522 (one step shorter because of the increase in 'maxtrees'). This was repeated five times with different random numbers (for the additional sequence), and a strict consensus tree was made of the five separate strict consensus trees. The total of different MP trees in this sampling probably is fewer than 5 X 10^4 because of possible overlap between the five subsets, although the differences in the majority-rule consensus trees suggest that there is little, if any, overlap. A strict consensus tree is used because there is no a priori reason to favor one MP tree over another (the length of this strict tree, 545 steps, is much longer than the length of each individual tree). A semistrict consensus tree showing only uncontested groupings
<<<< was nearly identical to the strict tree."

>>>> Fn.7. "The neighbor-joining method [N.Saitou and M.Nei, Mol.Biol. Evol. 4, 406 (1987)] was used with the proportion distance (p); a very similar tree was obtained with the Jukes-Cantor distance. Statistical significance of the groups on the tree was determined by the bootstrap method [J.Felsenstein, Evolution 39, 783 (1985) with 2000
<<<< replications (S.B.Hedges, Mol. Biol. Evol., in press).

>>>> Fn.8. C.B.Stringer and P.Andrews, Science 239, 1263 (1988); M.Nei and G.Livshits, Hum.Heredity 39, 276 (1989); in Population Biology of Genes and Molecules, N.Takahata and J.F.Crow, Eds. (Biafukan, Tokyo, 1990), pp.251-265; S.Hora
<<<< K.Hayasaka, Am.J.Hum.Genet. 46, 828 (1990).

>>>> Fn.9. R.L.Cann, M.Stoneking, A.C.Wilson, Nature 325, 31 (1987); M.Stoneking and R.L.Cann, in The Human Revolution, P.Melbars and C.Stringer, Eds. (Edinburgh Univ. Press,
<<<< Edinburgh, Scotland, 1989), pp. 17-30.

So what do we make of all this?

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First, it is clear that mtDNA nucleotide sequence data used in conjunction with the PAUP and used properly (statistically) cannot produce a clear decision as between a more likely African home and a more likely non-African home -- at least at this time. An obvious and neglected implication of this would be that neither Europe nor Asia nor South America are indicated either.

Secondly, the mathematical and/or statistical and/or computer portion of the Wilson's team 'Garden of Eden' has been falsified; it doesn't work. If that does not follow, what does?

Third, as both Maryellen Ruvolo and Becky Cann (personal communication) have pointed out, it does not follow that mtDNA analyses are premature or valueless. There is quite a bit of less global mtDNA work that has not been falsified. Mark Stoneking's work in the southwest Pacific and team Wallace's in the Americas and Pacific still stand. (More about these, below)

Fourth, mtDNA studies have shown that Africa has more diversity than other places; that has always been an argument in itself for places of origin.

Fifth, a number of other biogenetic studies most of which have been reported in MOTHER TONGUE support the African origin hypothesis. Cavalli-Sforza's massive study of virtually all known marker genes will shortly land with a great thud in bookstores. We already know that an African origin is supported therein.

Sixth, and very important, is that one aspect of team Wilson's results has now to be re-evaluated. It is not clear to me whether it was falsified because it was involved in PAUP or not. That is the argument that African Eve's descendants replaced all the remnant Homo erectus types in the Old World, especially Neanderthal in the Middle East and Europe + evolved versions of Peking man in China, source of the so-called 'Mongoloid' traits which link late Homo erectus of China with modern 'Mongoloid' populations of Asia and the Americas. Milford Wolpoff could be right about this crucial matter, although I would personally hate to admit it. It might be that the muddle in the MP trees (above) is due to gene flow from remnant Homo erectus types in a few places like the Levant and China. But Becky Cann says that no part of her analysis would support this attenuated version of 'rising tide' theory. It's also called 'polyphyletic theory'.

One long ranger, author Jean Auel, wrote recently that: (QUOTING) "I think he (Wolpoff --HF) has brought up some issues that needed to be brought up. In fact, when I first heard him speak about archeology, it brought home to me one of the questions I have long had about the whole mtDNA hypothesis. There was an interesting article in a magazine . . . discussing the concept of species in general. I don't have any problem with the idea of a pre-Sapiens "Eve" type people coming from Africa or even a H. sapiens sapiens. My problem is the part of the theory that claims that they replaced <u>all</u> of the populations. Of course that is the point Milford Wolpoff also resists. I think there is some pretty strong evidence to indicate that they may have intermixed with existing populations. That, to me, is a far more logical theory. The idea of wiping out people that had been in place for hundreds of thousands of years, since Homo erectus, just simply doesn't hold water." (END OF QUOTING)

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<u>A Simple Sketch of 'Rising Tide' or Polyphyletic Theory</u>

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Homo erectus evolves in Africa. Arrows <--> indicate gene flow between major regions, incorporating evolutionary grade changes. Regions to the right = Europe/Middle East; North Asia; South Asia; and Sundaland. Regions are merely illustrative, not precise

. , . ,

<u>A Simple Sketch, Showing What African Homo sapiens did to Cousins</u>

```
|   |  1500 AD  \ \ / /    \\/////   \\\\////    \/ \//
| A |               \   /       \   /      \   /      \   /
|   |  < ---- >     |   |       |   |      |   |      |   |
| F |               |   | ➡ ⬅  |   |      |   | ⇇  ⇉ |   |
|   |               |   |       |   | <--> |   |      |   |
| R | ______________|   |_______|   |______|   |______|   |__________
|   Homo sapiens spreads around Old World. 135,000 ± 3k           >>
| I                                                               >>
|   ______________________________________________________________
|   |  < ----> \ \\| /// \\ //////\\\\|//////\\//\\///
| C |              \ \ / /    \\/////   \\\\////    \/ \//
|   |               \   /       \   /      \   /      \   /
| A |               |   |       |   |      |   |      |   |
|   |  < ---- >     |   |       |   |      |   |      |   |
|   |               |   | ➡ ⬅  |   |      |   | ⇇  ⇉ |   |
|   |               |   |       |   | <--> |   |      |   |
|   |               |   |       |   |      |   |      |   |
|   |               |   |       |   |      |   |      |   |
|   | ______________|   |_______|   |______|   |______|   |__________
|      Homo erectus spreads around Old World. 1,750,000 ±          \\
|   ______________________________________________________________//
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'RISING TIDE' VOLLEY REPELLED: NEW DATES FOR mtDNA LUCKY WOMAN.

In March a Penn State team, led by Mark Stoneking, submitted an article for PHILOSOPHICAL TRANSACTIONS OF THE ROYAL SOCIETY B. Proceedings of the Discussion Meeting, "The Origin of Modern Humans and the Impact of Science-Based Dating". February 26-27, 1992. We have received a pre-publication version of the paper, which may by now be published, through the generosity of Mark Stoneking. (Royal Society of London, Series B, vol.336 is likely)

His associates were Stephen T.Sherry, Alan J. Redd, and Linda Vigilant, all of the Dep't. of Anthropology, Pennsylvania State University, University Park, Pennsylvania 16802, USA. Their paper is entitled:

New Approaches to Dating Suggest a Recent Age for the Human MtDNA Ancestor.

I propose hereinafter to call that lucky woman, that Eve, by a Hebrew name. In his interesting THE BOOK OF J, scholar and writer Harold Bloom, serving as interpreter of David Rosenberg's original Hebrew text, refers to her as Hava. I don't know or care whether Bloom's book is a spoof or not. Hava seems apt for Eve and may be the original. Let's use it!

Only the summary and a few other points are presented here because to say more would detract from the spirit of copyright laws. We do not want to steal from the final publication but rather to call attention to it. This is a crucial paper.
(BEGIN QUOTING)
"SUMMARY

The most critical and controversial feature of the African origin hypothesis of human mitochondrial DNA (mtDNA) evolution is the relatively recent age of about 200,000 years inferred for the human mtDNA ancestor. If this age is wrong, and the actual age instead approaches 1 million years ago, then the controversy abates. Reliable estimates of the age of the human mtDNA ancestor and the associated standard error are therefore crucial. However, most recent estimates of the age of the human ancestor rely on comparisons between human and chimpanzee mtDNAs that may not be reliable and for which standard errors are difficult to calculate. We present here two approaches for deriving an intraspecific calibration of the rate of human mtDNA sequence evolution that allows standard errors to be readily calculated. The estimates resulting from these two approaches for the age of the human mtDNA ancestor (and approximate 95% confidence intervals) are 133,000 (63,000-356,000) and 137,000 (63,000-416,000) years ago. These results provide the strongest evidence yet for a relatively recent origin of the human mtDNA ancestor."
(END OF QUOTING)

After noting that Vigilant et al of 1991 was intended to correct weaknesses perceived in the original study by Cann et al in 1987, Stoneking et al note that "The statistical tests used by Vigilant et al (1991) to buttress the support for an African origin of the human mtDNA ancestor have since been shown to be invalid, due to the inadequacy of parsimony analysis for these data." What then can be done about all this? Well, first, they maintain that the date of the ancestor is the crucial problem. Then, secondly, they find two parameters which are critical to estimating Hava's age. These are: "(1) the amount of sequence evolution that has occurred since the ancestor lived; and (2) the rate of human mtDNA sequence evolution."

In brief the Penn State team managed the two parameters by not rooting the trees in chimpanzee mtDNA and by taking a probable archeological date as a base for calculations. They assumed that there was some evidence that humans and chimpanzees had differences in mutation rates. In order to avoid settling the question definitively they just quit using chimpanzee rooting. Instead they measured it within the parameters of several known human populations in New Guinea. Then they based their dating calculations on an extension of known dates in New Guinea to 60,000 years ago. If other archeological finds changed the probable dates for human entering New Guinea, then the rates could be re-calculated. For example, if the present top date of 40,000 BP remained solid, then 40,000 could be substituted for 60,000 in the calculations. One remembers that the presence of a 55,000 year old site in northern Australia influenced their decision to pick 60,000 years.

Just to rehash now. Stoneking et al (1992) chose the oldest known virgin (initial) settlement of an area by Homo sapiens and measured the amount of internal (intraspecific) diversity among the natives, the proper autochthones, of the region. This procedure can be repeated in Australia, of course, which has older known dates than New Guinea. It might also be done with Amerinds, if we ever get the dates of settlement stabilized.

A number of different statistical measures were used by the Penn State team, explicitly designed to get around the problems encountered by team Wilson earlier. Since dating was defined as the crucial problem, there was only a brief discussion of maximum parsimony and family trees. They seemed not to use PAUP, turning instead to NJ (neighbor-joining) analyses alluded to earlier and to UPGMA tree analysis. I've not yet been able to find out what UPGMA is or even to get the words in the acronym.

Assuming for the moment that their formulae worked and that other biogeneticists do not torpedo their analyses, we are left with the need to comment on their conclusions. First, the most clear and obvious is the date itself; it fits remarkably well with the east African fossils thought to represent earliest anatomically modern people. Secondly, it seems to preclude or discourage the assumption of absorption of late Homo erectus types. Thirdly, they sharply distinguished coastal Melanesians (Austronesians) from highland Papuans and confirmed the Polynesian connections with Indonesians. Finally, they showed the great diversity among Papuans. No wonder Indo-Pacific is tough!

BIOGENETICS AND DENTAL GENETICS IN THE PACIFIC: AN INTERFACE

A rich trove of genetic studies, focused on the Pacific Rim west, has appeared recently. Two were 'just coming out' when shared with us. Although the studies are not fully comparable -- due to population sampling and research strategy differences --, still the contrasts are powerful. One study shows the march of mtDNA research around the world, while two studies present the results of dental genetic research to confirm or deny the mtDNA conclusions. A fourth genetic study acts as a referee.

How to handle such complex articles in our limited spaces? We summarize and compare gross results but avoid technical details. For those who want to attack mitochondrial research we give references to the originals but little technical detail. For those who are fascinated by the dental studies -- we cannot help you. Go to the originals and/or pursue the many works of long ranger Christy Turner II who is the master of this field. Here are the formal studies and their own abstracts:

1) Appearing in GENETICS 130:139-152 (January, 1992), Douglas Wallace's team at Emory University has given us another fine mtDNA study. First author is S.W. Ballinger and co-authors, Theodore G. Schurr, Antonio Torroni, Y.Y.Gan, J.A.Hodge, K.Hassan, K.-H. Chen, and Douglas C. Wallace. All are in the Dep't. of Biochemistry at Emory University School of Medicine, Atlanta, GA 30322, USA, except Gan (Department of Biotechnology, Universiti Pertanian Malaysia, Serdang, 434000 Selangor, Malaysia), Hassan (Institute of Medical Research, Kuala Lumpor, Selangor, Malaysia), and Chen (currently: Institute of Public Health, National Taiwan University College of Medicine, Taipei, Taiwan). Correspondence to Douglas Wallace. Article is entitled:

Southeast Asian Mitochondrial DNA Analysis Reveals Genetic Continuity of Ancient Mongoloid Migrations.

The ABSTRACT says:

"Human mitochondrial DNAs (mtDNAs) from 153 independent samples encompassing seven Asian populations were surveyed for sequence variation using the polymerase chain reaction (PCR), restriction endonuclease analysis and oligonucleotide hybridization. All Asian populations were found to share two ancient AluI/DdeI polymorphisms at nps 10394 and 10397 and to be genetically similar indicating that they share a common ancestry. The greatest mtDNA diversity and the highest frequency of mtDNAs with HpaI/HincII morph 1 were observed in the Vietnamese suggesting a Southern Mongoloid origin of Asians. Remnants of the founding populations of Papua New Guinea (PNG) were found in Malaysia, and a marked frequency cline for the COII/tRNA^{Lys} intergenic deletion was observed along coastal Asia. Phylogenetic analysis indicates that both insertion and deletion mutations in the COII/tRNA^{Lys} region have occurred more than once."

From the following Asian populations .."blood samples were collected from 153 independent maternal pedigrees (unrelated through at least one generation)" :

(a) Koreans (13): From South Korea < Seoul, Taejon, Tamyang.
(b) Central Chinese (20) or Han < Taiwan < Hunan originally.
(c) South Chinese (14): In Malaya. From Fujian/Guangdong.
(d) Vietnamese (28).
(e) Malays (14)
(f) Malay Aborigines (32) or 'Orang Asli', divided into Temiar (7), Semai (5), Jakun (1), Jeni (2) and unidentified tribals (17)
(g) Aborigines (30) and Bisaya (2), both of Sabah, Borneo divided into Kadazan(Dusun)(24), Berungei (2), Rungus (3), and Murut (1)

On page 140 the Malay and Malay Aborigines are lumped together but not elsewhere, fortunately; the Malays being rather closer to the Sabah Aborigines in 'genetic divergence', while the Malay Aborigines are somewhat closer to the Vietnamese. This reflects linguistic classes because most of the Malay Aborigines belong to the Aslian sub-branch of Mon-Khmer of Austroasiatic -- being thus cousins to the Vietnamese. Malay plus the Borneo languages are Austronesian; very distantly related to Austroasiatic if you accept the Austric hypothesis.

One would wish that this aspect of team Wallace's work be repeated with much better linguistic taxonomy. Barely mentioning the important distinction between Mon-Khmer and Austronesian, they do say that most of the Malay Aborigines are Senoi and thus as Austroasiatic speakers related to Vietnamese. However, their whole discussion of Papua New Guinea -- coastal or highland -- is flawed because they do not distinguish between two great phyla which are found along the Papuan coasts and adjacent islands of Melanesia. Most physical anthropologists have for some time now recognized 'AN' and 'NAN' linguistic affiliations in Melanesia. 'AN' = Austronesian and Non-AN = Papuan. In some genes the Austronesians and Papuans appear as archtypes or endpoints of very marked clines. The great Malayo-Polynesian interface with Indo-Pacific produces tremendous variability in Melanesia, extending to islands as far east as Fiji, making all the more remarkable the Polynesian resemblance to the Indonesians (in mtDNA, Gamma Globuline, etc.). They dispersed from Fiji, it is widely believed, but remained different physically from Fijians.

For example, high M in southeast Asia gives way to rising N in Melanesia and very high N in highland New Guinea; high 'fanb' Gamma Globulin in southeast Asia gives way to variable 'fanb' in Melanesia to very low 'fanb' in the highlands but high 'fanb' again in Micronesia and Polynesia; fairly high Rhesus 'R1' in southeast Asia increases to extremely high 'R1' in highland New Guinea. The deletion haplotypes within haplotype group D* do not occur in the highlands, says Stoneking. The correlations with linguistic AN and NAN are very respectable. And NAN is Greenberg's Indo-Pacific!

It is hard to understand some of the data or the cladograms; one misses the kind of cladistics one gets in other genetic studies or indeed in historical linguistics. There are trees with Koreans and Malays hanging from the same branch, while Koreans, Chinese, and Aborigines hang together on some other branch. One cannot tell where a branch that is Korean as opposed to a branch that is Malay as opposed to . . .is supposed to be. How can one make taxonomic or historical sense out of the intricate results? Then how can one check their conclusions? Malheureusement, the mtDNA people are so intent on their excellent and high tech research that they neglect to communicate with the rest of us! And that is a pity precisely because their use of the concepts of mutation and common ancestor are much like historical linguistic concepts of shared innovations and protolanguages.

Here are their conclusions. After all the splendid research they pour their hot molten results into a leaky old pot -- the concept of Mongoloid. And worsen things by vague remarks about "Asians", as if it were true that Asian = Mongoloid. Neither is a very useful concept. One can count over 1,200,000,000 Asians who nobody thinks are Mongoloids. Biogeneticists must take naming and labelling seriously, as well as the cultural and linguistic parameters of their work! The excellent Wallace team did not invent this custom. Physical anthropologists have habitually ignored these things for ages. Their summary follows:

"In summary, all Southeast Asian populations analyzed in this study appear to have common origins, consistent with a hypothesized southern Mongoloid origin of the peoples in this region (Bellwood 1985, and references therein; Turner 1987). These mtDNAs are divided into two major branches by the AluI/DdeI nps 10397/10394 polymorphisms. The populations from the Malay peninsula and Borneo (Sabah) appear to have genetic ties to those of coastal PNG (Papua New Guinea --HF). The high sequence diversity of the Vietnamese and the high frequency of the HincII/HpaI morph haplotypes suggest that Southern China is the center of Asian mtDNA radiation (Blanc et al 1983) and, it appears that the deletion and insertion mutations have occurred multiple times in Asian mtDNA lineages. The high frequencies of the deletion haplotype group D* mtDNAs in Southeast Asia, the Pacific islands, and the New World implies that the migrants carrying this marker were descendant from a single founder population."

The last sentence more precisely means, as they said earlier on p.146, that one migration went from China south along the Asian coastline, eastward into Indonesia, and out into the Pacific islands. A second migration went north into Siberia and eventually crossed the Bering land bridge into the New World, yielding the Amerindians. A key assertion on the same page is quite instructive --> "All the deletion haplotypes seen in aborigines from PNG (Stonekind, et al 1990) and Amerindians (Schurr et al 1990) fall within haplotype group D*. Thus, it would appear that the recent migrants from Asia that carried the

COII/$tRNA^{Lys}$ deletion belonged to haplotype group D*." <--
If only we knew what kind of Papuans they were!

On page 142 a remarkable sentence occurs --> "Since Vietnam was colonized by a southeast China migration, this would imply a southern Chinese origin of Mongoloid people about 59,000 to 118,000 YBP (assuming that mtDNA divergence is 2-4% per million years, . .)" <- (Underlining by editor. Four references omitted). This date of 88,500 BP ± 28,500 years is very interesting. But it is quite different from the dates of 40,000 BP by Masatoshi Nei and 53,000 BP by Cavalli-Sforza for the separation of the Caucasoids and Mongoloids. (See MT-14, "Borean: A Taxonomic Hypothesis"). Without wishing to rub salt into anyone's wounds, one must mention that this date is based on Wilson team dates and rates of change. Those dates for African Hava now have been falsified in mtDNA terms, even though some of them are just right archeologically. Thus we must be sceptical of team Wallace's proto-'Mongoloid' dates. Also don't forget that Cavalli-Sforza found a group called northern 'Mongoloids' -- native Americans included -- who were closer to Caucasoids than to southern 'Mongoloids'. So these issues are not settled.

What are the linguistic consequences or correlates of team Wallace's venture into Sundaland. Well, if they are correct, they have blown away the correlations between Amerind, Nostratic and/or Eurasiatic, Afrasian, and Dene-Caucasic. The new linguistic picture focused on the Pacific Rim would have to look something like this in order for correlations to show up again:

```
                  proto-Indo-Amerind
                   /       |       \
                  /        |        \
      Amerind-Austric      |      Indo-Australian
        /        \         |        /        \
  Amerind     Austric    Sino-    I-P      Australian
                         Tibetan
```

Of course the Indo-Australian sub-phylum may not join this group at all. It depends on who the PNP people are and how much the Australians are included. It could be argued that the article's lack of clarity on these points means that only Austronesian was being considered in the great migration into the Pacific.
If we considered an anomaly -- the case of Gamma Globulin --, then we would have other genetic support for the above picture, except that Australian would join Amerind and the eastern highland parts of I-P (Indo-Pacific). New Guinea as a whole has some huge clinal differences in Gamma Globulin within it. Roughly coastal PNG, eastern highlands and southwestern highlands, especially around Asmat, have differences greater than any found in southeast Asia. Stoneking et al (above) support such deep cleavages in New Guinea. Just fascinating, in't it?

Let the carping criticisms be seen as "emerging synthesis" type comments; things wished for. Basically the study was superb!

2) In AJPA, the AMERICAN JOURNAL OF PHYSICAL ANTHROPOLOGY 88:163-182 (1992) and 88:183-196 (1992) appeared two articles by Tsunehiko Hanihara of the Department of Anatomy, Sapporo Medical College, South 1, West 17, Chuo-ku, Sapporo, 060, Japan. They will be presented in sequence so as to preserve their logic.

Dental and Cranial Affinities Among Populations of East Asia and the Pacific: The Basic Populations in East Asia, IV

>>>> "ABSTRACT The origins of the four major geographical groups recognized as Australomelanesians, Micronesians, Polynesians, and East and Southeast Asians are still far from obvious.The earliest arrivals in Sahulland may have migrated from Sundaland about 40,000-50,000 years B.P. and begun the Australomelanesian lineage. The aboriginal populations in Southeast Asia have originated in the tropical rain forest of Sundaland, and their direct descendants may be the modern Dayaks of Borneo and Negritos of Luzon. These populations, the so-called 'Proto-Malays', are possible representatives of the lineage leading to not only modern Southeast Asians, but also the Neolithic Jomon populations of Japan. The present study suggests, moreover, that the Polynesians and western Micronesians have closer affinities with modern Southeast Asians than with
<<<< Melanesians or Jomonese."

We should establish three prehistoric geographical terms which pervade discussions of east/southeast Asia and the southwest Pacific. You may already know them but a rehearsal is a good idea. Fetch a map and sit down with it!

Sundaland = an area nearly as large as Australia about half of which is now under water, extending from just west of the Celebes (Sulawesi) and just east of Bali to Vietnam on the west and Rangoon on the west, not including the Andamans & Nicobars. Or roughly from Wallace's Line on the east to the Rangoon-Canton line on the west, including almost all sea areas in between. This area at low water during the Pleistocene was much akin to Amazonia -- a vast lowland tropical forest. By extension many also refer to the continuous strip of now-submerged lowlands from Hanoi via Taiwan to Korea and Japan and up through the Japanese main islands via the Kuriles to Kamchatka or via Sakhalin to Okhotsk. The Yellow Sea and half of the East China Sea were dry land, while the Seas of Japan and Okhotsk were either lakes or bays of the ocean. Neither the Ryukyus, Philippines, or eastern Indonesia were connected to Sundaland or its extensions north.

Sahulland = Australia and New Guinea plus the seas between, Tasmania and the Bass Straits, and the islands just east of New Guinea as far as Rossel Island. All of this was one land during low water phases of the Pleistocene. The main mass of Melanesian islands were interconnected and almost touched New Guinea.

Wallacea = the deep water area between Sunda and Sahul lands, including three major clumps of connected land. One was most of the Philippines; another the Celebes; and the third the narrow strip of eastern Indonesian islands from Lombok to Timor.

One key feature of Sundaland and its extension to the north is its size -- the largest submerged area on earth. Its underwater archeological potential is vast, roughly equal to two Japans plus New Guinea. A second is the old connections among places now islands. Most noteworthy are Japan, Formosa and the bulk of Indonesia. All were connected to the Asian mainland for long periods of time during the low water or glacial phases of the Pleistocene -- the main evolutionary period of humanity.

Hanihara stresses these geographical regions. Indeed his primary theory is that Sundalanders crossed Wallacea to settle Sahulland some 40,000-50,000 years ago. His second theory is that the northern extension of Sundalanders included the Jomonese who were occupying Japan when the Yayoi arrived there. Jomon of 8000 years ago is closely related to modern Ainu. Also the Sundaland extensions (outward from the Chinese coast) includes the homeland of Austro-Tai or at least Austronesian by consensus nowadays.

Here we present several of Hanihara's diagrams to show the relationships postulated among a series of populations based on dental or cranial characteristics. I should point out that the reliability or prestige of cranial measurements is far less than it used to be, since many believe these to be phenotypical. The dental factors appear to be more direct expressions of genotypes and enjoy more respect.

TABLE 4. Distance matrix based on B-square distance

	Jpa	Chi	Doi	Kan	Tok	Ain	Jom
Japanese	--						
N. Chinese	0.686	--					
Doigahama	0.375	0.357	--				
Kanenokuma	0.189	0.559	0.321	--			
Tokunoshima	0.412	1.227	0.623	0.448	--		
Ainu	1.638	2.997	2.193	1.879	0.608	--	
Jomon	1.127	2.652	1.607	1.331	0.538	0.647	--
Negrito	1.696	2.582	2.444	2.025	0.997	0.714	1.083
Guam	1.115	1.999	1.225	0.795	0.391	0.991	0.616
Hawaii	1.245	2.443	1.985	1.033	0.614	0.985	0.851
Marquesas	0.753	2.072	1.462	0.705	0.510	1.310	0.788

Also add: Negrito vs Guam = 1.362, Neg. vs Hawaii = 0.994 & 1.429 vs Marquesas; Guam vs Hawaii = 0.540, vs Marquesas = 0.509; Hawaii vs Marquesas = 0.387 .

Notes: The Chinese are from Manchuria (Liaoning), a rare sample of North Chinese who generally contrast fairly sharply with South Chinese. Almost all comparative studies use South Chinese who are much more like Southeast Asians. The Doigahama are Yayoi of west Honshu of 2000 BP, while the Konenokuma are Yayoi of north Kyushu of the same time. These Yayoi may be the first or 'real' Japanese entering from the west. Tokuno-shima on the other hand is modern; from the Amami islands, halfway between Okinawa and Kyushu. The Ainu are 19th century Ainu from Hokkaido, less mixed with Japanese than modern Ainu. The Jomon sample is from Honshu from

5300-2300 years BP. The Negrito sample is from Bataan, Luzon, the Philippines. Guam = Chamorro or Micronesians but from the period before the Spanish arrived. The Hawaii sample is from the Mokapu site on Oahu, dated from 500 to 600 years ago, i.e., before the modern admixtures with East Asians, Filipinos, and Europeans. From these data Hanihara plotted a cladogram; it is below.

" Fig. 3 Clustering patterns derived from a B-square distance matrix based on nine discrete characters of dental crowns in human populations."

```
Japanese     -------,
                    |--,
Kanenokuma Yayoi --'   | --,
                       |   |
Doigahama Yayoi  -----'    |-----------------------,
                           |                       |
North Chinese ------------'                        |
                                                   |
Jomonese -------------------,                      |
                            |------,               |
Ainu -----------------------'      |               |----
                                   |               |
Tokunoshima ---------,             |-----,         |
                     |---,         |     |         |
Guam ----------------'   |         |     |         |
                         |---------'     |         |
Hawaii  --------------,  |               |--------'
                      |---'              |
Marquesas ------------'                  |
                                         |
Negrito ---------------------------------'
```

, . , . , . , . , . , . , . , . , . , . , . , . , . , . , . , . ,

TABLE 6. Distance matrix transformed from Q-mode correlation coefficients between each pair of populations. (Crown diameters)

Population	Day	Neg	Ain	Aog	Tok	Jom	Gua
Dayak	--						
Negrito	0.987	--					
Ainu	1.221	1.423	--				
Aoga-shima	1.044	0.585	0.595	--			
Tokuno-shima	1.322	0.690	0.575	0.343	--		
Jomon	1.223	0.873	0.747	0.722	1.108	--	
Guam	1.320	1.155	0.922	1.274	1.075	1.366	--
Hawaii	1.229	1.159	1.221	1.442	1.063	1.028	0.792
Marquesas	1.470	1.038	1.480	1.596	1.320	1.153	0.531
Fiji	1.069	1.401	1.341	*0.158	1.452	0.323	1.110

* This number is surely an error, at odds with his dendrogram. See Fig.5 below for his clustering or dendrogram.
Also add: Hawaii vs Marquesa & Fiji --> 0.291 & 1.210; Marquesas vs Fiji = 0.824 .
Notes: Dayak, modern, from Pontianak, west Borneo. The last four

(Guam, et al) are Austronesian, as are Dayak and Negrito. The Aogashima and Tokunoshima speak Japanese now, whatever their earlier speech. The Fiji are listed as Melanesian by Hanihara. Their sample (8) is very small, as is the Dayak (12), considering all the prehistoric weight placed on them by Hanihara.

Fig.5 Clustering by group average method. Distance matrix transformed from Q-mode correlation coefficients based on 13 M-D crown diameters.

```
Dayak  ----------------------------------------------,
                                                     |
Negrito ---------------------------------,           |-----,
                                         |           |     |
Jomonese -----------------------------,  |-----------'     |
                                      |-'                  |
Ainu ------------------------,        |                    |
                             |---------'                   |
Aoga-shima --------,         |                             |---
                   |---------'                             |
Tokuno-shima ------'                                       |
                                                           |
Guam  -------------------------,                           |
                               |-------------,             |
Hawaii --------,               |             |             |
               |---------------'             |             |
Marquesas -----'                             |-----------'
                                             |
Melanesia  ----------------------------------'

, , , , , , , , , , , , , , , , , , , , , , , , , , , , , , , ,
```

Hanihara sums up his data and hypotheses in Fig.10 (see overleaf) "A hypothetical schema showing racial diversification in East Asia and West Oceania during late Pleistocene times (modified from Omoto, 1984). . ."

Minatogawa represents one of the oldest sites in Japan, no doubt the oldest with human remains (19-16,000 BP). Another which is not shown is Liukiang in southeast China; some think it an archaic Homo sapiens. Its cranial data link it closely to Jomon.

Perhaps Hanihara meant his diagram in Fig.10 to merely symbolize the split between native Australians and Papuans, rather than to reflect properly their diversity. If not so, then his diagram is grossly inadequate because the split should go down towards the bottom of the diagram.

In Fig.5 the clustering patterns show two branches or moieties, one Oceanic from Guam to Melanesia and the other a Sundaland plus northern extension. The Oceanic is absolutely correlated with Malayo-Polynesian. The Sundaland branch invites correlation with either Daic (Thai-Kadai), Miao-Yao, Mon-Khmer, or old Austro-Tai (containing pre-proto-Japanese?)

```
Fig.10
       | East Asia  |   Japan           | Sundaland             |Sahulland      |
  B.P. |            |                   |                       |               |
       | Northeast  | Main-    Ainu,    |SE Asian Negrito       | Australian    |
      0| Asian      | Island  Okinawa   |           Dayak       | & Papuan      |
       |            |   ↑       ↑       |   ↑         ↑         |  ↖      ↗     |
  2,000|            |   ↑       ↑       |                       |               |
       |   ==========> Yayoi    ↑       |                       |               |
  4,000|            |                   |       ↖  ↗            |      ↑        |
       |            |      ↖            |                       |               |
  6,000|            |                   |                       |               |
       |            |           ↑       |          ↑            |      ↑        |
  8,000|            |                   |                       |               |
       |            |      Jomonese     |                       |               |
 10,000|            |           ↑       |          ↑            |      ↑        |
       |            |    Minatogawa     |                       |               |
 20,000|            |                   ↖                       |               |
       |            |                   |   Proto-Malays        |      ↑        |
 30,000|            |                   |                       |               |
       |            |                   |                       |               |
 40,000|            |                   |          ↑            | Australoids   |
 50,000|            |                   |                       ↗               |
 60,000|            |               Ancestral population                       |
-------------------------------------------------------------------------------
```

3) Hanihara's second article is entitled:
<u>Negritos, Australian Aborigines, and the 'Proto-Sundadont' Dental Pattern: The Basic Populations in East Asia, V</u>. The Abstract says

>>>> "Five evolutionarily significant dental traits were identified from a B-square distance analysis of nine crown characters recorded for several populations of East Asia and Oceania. Intergroup variation in these traits distinguishes three major divisions of the Mongoloid dental complex: sundadonty, sinodonty, and the dental pattern of Australian Aborigines. The Australian crown features may be characterized as having high frequencies of evolutionarily conservative characters. Negritos, one of the probable representatives of indigenous inhabitants of Southeast Asia who may have shared a common ancestor with the Australians, possess the more derived sundadont dental pattern. As far as the five crown traits treated here are concerned, Australian dental features may be described as conforming to a 'proto-sundadont' dental pattern, applying Turner's terminology. This pattern may represent a microevolutionary step prior to <<<< the emergence of the sundadont and sinodont patterns."

Some quaint things about human teeth have long been discussed among physical anthropologists in ways remarkably similar to biogeneticists discussing DNA traits. There are traits found in some human populations -- e.g., shovel-shaped incisors -- which are also found in humanity's earlier stages. These are thus rooted or primitive -- in the parlances of the two fields. Other traits not so rooted are seen as mutations or innovations which help to define populations with a common evolutionary past. The so-called Carabelli's Cusp is one which Carleton Coon (at least) thought defined the Caucasoids. In Gamma Globulin there are several of these which help define several evolutionary clusters, examples include 'zabst' for northern 'Mongoloids', 'zab' for central African, 'zanb' for southwest New Guinea, 'fab' for Caucasoids, etc. Both the Ainu and the Khoisan-speakers have their own. So what Hanihara is doing, among other things, is using Christy Turner II's general theory to show such things around the west Pacific Rim.

Since the shovel-shaped incisor has long been associated with the concept of Mongoloid, we need to look at it and the two concepts which Turner has pioneered in relating everyone's 'classic Mongoloid' to other populations. The classics are, as you might suspect, Mongols and like peoples of northeast Asia, including Koreans and Eskimaux. They have the highest incidence of shovel-shaped incisors in the world and they are archtypical sinodonts in Turner's terms. Native Americans are sinodonts too. A key question would be -- are shovel-shaped incisors like the vermiform appendix, just an old retention? Or an innovation shared by sinodontists?

Both K. and T. Hanihara believe that sinodontry arose out of an older sundadontry; so does Turner. This would roughly be an intensification of a pattern already present in low incidence among sundadonts. Who are they? Southeast Asians for the most part but including Polynesians, Micronesians, Melanesians and the Jomon people and the Ainu. The two dontries form what K. Hanihara calls the 'Mongoloid complex'. How about the Australians & the Papuans? The complex is different but sufficiently related to be interesting. Turner and the Haniharas believe that the Australian pattern represents an old sundaland pattern which went out into the Pacific 40-50,000 years ago; it was replaced in its homeland by the sundadont pattern which probably also generated the sinodont pattern. Probably? Yes, because it may have arisen directly out of the Australian pattern.

Before showing his data matrix table and diagram, we should point out that Hanihara used many old Australian archeological teeth but precious few Papuan. Yet the basic hypothesis is strikingly similar to team Wallace's basic finding in mtDNA. While there are levels at which biological and linguistic correlations can be found, yet at a deeper level -- e.g., the Borean level -- there is sharp conflict. Neither Nostratic nor Eurasiatic nor Dene-Caucasic can be comfortable with a pattern of local evolution within, and movement out of, sundaland. These movements produced the modern (1500 AD) populations of East Asia, Southeast Asia, the Pacific and the New World. Or did they? Not a single one of these studies compared anyone with a western neighbor, either in India or west Siberia. It reminds me of the strikingly inward-looking mind set of the 'Atlantic community' during the cold war. Still the Pacific Rimmers could be right!

Austro-Tai and probably Austric as a linguistic super-phylum has clear biological correlates. Early Japan participated in that same biology. Ainu seems very likely to be related to the same in its Jomon aspect. Yet Ainu has cultural and linguistic features which smack of northeast Asia. Both Australian as a phylum of languages and Indo-Pacific as the same have strong biological correlates. And Amerind the same, as Turner has shown.

Sino-Tibetan, however, is a mess. From ordinary or regular genetic data (serum proteins, blood groups) the Tibetans are surely northeast Asians, much like the Mongols. When one does <u>finally</u> get data on the north Chinese, they most resemble the Koreans and Tibetans. But south Chinese are 'typical' southeast Asians. And we have little on the Himalayan and Burmese tribals of Sino-Tibetan. What does Sino-Tibetan correlate with?

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"TABLE 3. B-square distance matrix based on 9 non-metric crown characters."

Population	1.	2	3.	4.	5.	6.	7.
1.Japanese	--						
2.N.Chinese	0.614	--					
3.Yayoi # 1	0.379	0.275	--				
4.Yayoi # 2	0.342	0.376	0.337	--			
5.Jomonese	1.126	2.590	1.652	1.712	--		
6.Ainu # 1	2.021	3.388	2.614	3.007	0.789	--	
7.Ainu # 2	1.963	3.608	3.009	2.667	0.790	0.665	--
8.Hirota	1.951	3.868	3.047	2.971	0.730	0.997	0.735
9.Nansei Is.	0.493	1.315	0.906	0.993	0.597	0.666	0.819
10.Old Thai	2.196	3.476	3.017	2.463	1.229	1.195	1.846
11.Negrito	1.765	2.779	2.497	2.545	1.138	0.893	1.108
12.Fiji +	1.160	1.955	1.265	1.099	0.657	1.646	1.416
13.Hawaii	1.279	2.438	2.003	1.310	0.861	1.654	0.892
14.Australia	*0.295	2.366	1.814	1.363	0.921	2.491	1.645

Table 3, right hand extension continued,

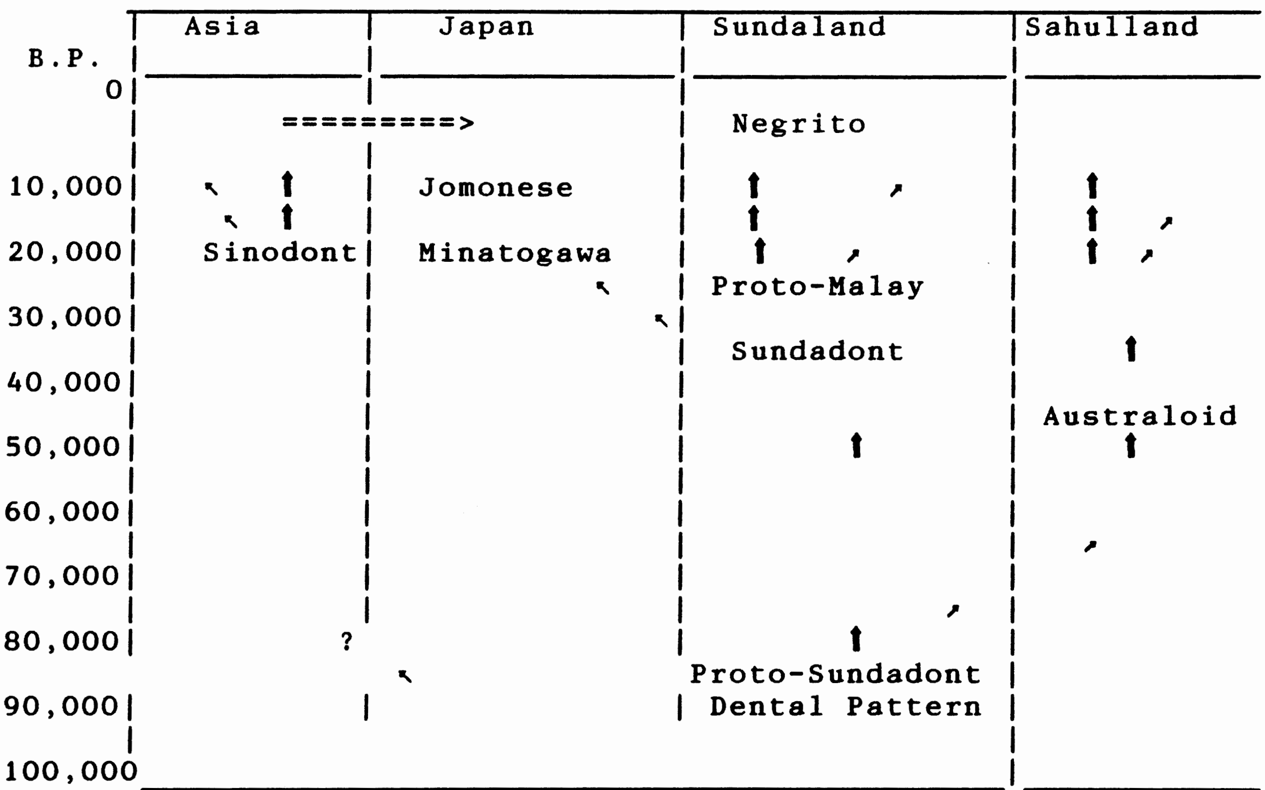
Population	8	9	10	11	12	13	14
8.Hirota	--						
9.Nansei Is.	0.969	--					
10.Old Thai	0.995	0.947	--				
11.Negrito	0.726	0.722	1.030	--			
12.Fiji +	1.251	0.639	0.917	1.405	--		
13.Hawaii	1.033	0.741	0.516	1.042	0.541	--	
14.Australia	1.092	1.119	1.207	1.480	*0.427	*0.560	--

*This number is inherently incredible or it is at odds with his other numbers or his general conclusion. An error is declared.

<u>Notes:</u> Hirota is from the island of Tanega-shima, off the south coast of Kyushu. Skeletal remains of Jomonese, he says, of 2000 BP and representative of northern Ryukyu islands. Nansei Is. stands for Nansei islands or Ryukyu islands. These data are

modern, from all over the 1200 kilometer chain of islands. Old Thai = Early Thailand in his terms; from Ban Chiang site, early metal age (6,000-3,000 BP) in northeast Thailand. The Negrito is now more closely identified as Aeta from Luzon -- basically Sunda pygmies. They are often said to be left over 'Australoids'; but Hanihara denies their special relationship to Australians. Fiji + is basically a small sample of Fijians, plus some others from here and there around Melanesia. Risky business this! Because the linguistic label 'Melanesian' means you can file them together does this compel the very heterogeneous Melanesians to congeal? The native Australians are also quite assorted lot [that's a pun] with specimens coming from an area as large as Brazil and from skeletal remains of many millennia time depth. Yes, it is true that all Aborigines look alike but so do all Japanese. Hanihara notes that the former have a low shovel-shaped incisor incidence but many lingual tubercles from which the latter evolved.

'Proto-Sundadont' dental Pattern'



In older textbooks in physical anthropology we sometimes found each of the Pacific populations defined as a 'race'. Long ranger Frank Livingstone was one who sought liberation from the fairly simplistic racial schemes that came down to us in the 1950s and 1960s. Perhaps we can persuade him to write something for us on this subject -- because there seems to be a need for it, especially since most linguists have no training in this area. Many older racial schemes were impressionistic, overly based on head measurements (instead of genetics), and tended to be a sort of synchronic warehousing. More recent and more sophisticated views have still tended to see the Micronesians, Polynesians, and Melanesians -- often not well distinguished from Papuans -- as three separate 'races'.

Hanihara has enlightened us on these Pacific local races. While they may or may not be separable on anthropometric grounds, they seem NOT to be genetically. Micronesians and Polynesians are quite close to each other and to Austronesians of the west. But the immense variation among the Melanesians defies easy summing up. For this I criticized Hanihara. Arthur Steinberg has vividly shown the genetic diversity in island clusters like the Solomons where clines often run up and down a single island. The whole lot of them can probably be called a local race based on a massive collision of gene pools owned by two dissimilar earlier populations, the immigrant Austronesians from west of Wallacea and the older resident Papuans (or real Australoids, if you like) who had lived separately east of Wallacea for tens of millennia.

4) Our last genetic study appeared in AJPA 88:27-36 (1992), writ by N. Saha and J.S.H. Tay, both of the Dep't. of Paediatrics, Division of Human Genetics, National University of Singapore, Singapore 0511. Its title was: (followed by its Abstract)

<u>Origin of the Koreans: A Population Genetic Study</u>

>>>> "ABSTRACT A population genetic study was undertaken to investigate the origin of Koreans. Thirteen polymorphic and 7 monomorphic blood genetic markers (serum proteins and red cell enzymes) were studied in a group of 437 Koreans. Genetic distance analyses by both cluster and principal components models were performed between Koreans and eight other populations (Koreans in China, Japanese, Han Chinese, Mongolians, Zhuangs, Malays, Javanese, and Soviet Asians) on the basis of 47 alleles controlled by 15 polymorphic loci. A more detailed analysis using 65 alleles at 19 polymorphic loci was performed on six populations. Both analyses demonstrated genetic evidence of the origin of Koreans from the central Asian Mongolians. Further, the Koreans are more closely related to the Japanese and quite distant from the Chinese. The above evidence of the origin of Koreans fits well with the ethnohistoric account of the origin of Koreans and the Korean language. The minority Koreans in China also
<<<< maintained their genetic identity."

The first question which pops up is -- whence came the 'Han Chinese'? Alas, the answer is not given. Since all Chinese look alike, just like native Australians and Japanese, then there is no need to specify. A few overseas south Chinese in Singapore can represent one thousand one hundred million others spread over an area bigger than Canada? Come on, you guys, when are we going to take China seriously? If you did this to the Europeans -- who some Chinese think really do look alike --, the cries of outraged statisticians would be audible on Mars!

Despite its initial flaw, the study has some important things to show. Above all, and this was its own main goal, it nails down the Korean gene pool as a northeast Asian one. Nobody really doubted this finding in the first place but with the Chinese appearing to lack such strong northern ties someone had to check out the Koreans. After all only the Manchu and Mongols are as close geographically to the heartland of Chinese civilization -- the Yellow River basin. Saha and Tay find the 'Mongolians' and Japanese to be closest to the Koreans. The three of them form a cluster with the somewhat distant 'Soviet Asians'. Together they oppose the Chinese & Malays, the Javanese a bit removed, and the Zhuang farther removed -- as southeast Asians.

The Zhuangs are Austro-Tai, more specifically the Northern twig of the Tai bunch of the Be-Kam-Tai branch of the Li-Kam-Tai major branch of the Daic part of Austro-Tai. The Soviet Asians are heterogeneous (tut tut!) and apparently from central and eastern Siberia, mostly Altaic-speakers but also Koryaks of the Chukotian branch of Eurasiatic.

(3) OBSERVATIONS on MacNEISH's PENDEJO CAVE.

Apropos of the Amerind invasion of North America and its dates, not to mention people questioning the reality of any site earlier than the Clovis horizon of 13,000 BP, a more intense examination of such a site is called for. As discussed briefly before, Scotty MacNeish's Pendejo Cave in New Mexico is deeply stratified and shows 'things' which are purportedly up to 27,000 years older than the Clovis horizon. So for MacNeish, for his critics, and for the rest of us -- there is a lot at stake in Pendejo Cave. Many archeologists have answered MacNeish's call to check the site out and render their opinions. Since I did not trust the experts to transcend their prejudices, I took my own biases down to New Mexico and had a good long look at the cave. After this report we will consider briefly some surprising new things said casually about the settling of the New World recently in the MAMMOTH TRUMPET and also under a separate heading an astonishing new site in Siberia which threatens everyone's general understanding of human prehistory -- no less!

The name of the cave itself is odd, meaning 'pubic hair' in New Mexican Spanish. It is located on a military reservation (Fort Bliss), more precisely the White Sands Missile Range, about 30 miles north of El Paso, Texas, in Oro Grande, New Mexico. To make a visit one contacts the A.F.A.R. (or Andover Foundation for Archeological Research), P.O. Box 83, Andover, MA 01810, USA. (Or AFAR, P.O.Box 77, Orogrande, NM 88342, USA) The welcome was warm and sincere. The crew were friendly, informative, and inclined to discuss the shortcomings of Pendejo's critics in much detail. Sue DiCara (El Paso) and I were much impressed by them; bright, hard-working, aware of what was going on, and painstaking in minute matters. A high tech dig. Damn good archeologists!

Summaries of exactly what was found at what levels and how they were dated can be found in the "1991 ANNUAL REPORT and 1992 BRIEFING BOOKLETT From February 3 through May 11"; it can probably be obtained for free (or maybe via a small contribution to AFAR) by writing to the Andover address. More recent reports may also be available. The discussion of the animal remains and numerous cultural artifacts remains the province of the experts. But here I want to mention a few things which struck me as convincing, as too hard to explain away. My only goal is to point to a few tokens of more archaic humanity at dates well beyond the range of the Clovis horizon.

(I) Human hair found at the 19,000 years ago level, thoroughly embedded in some charcoal. The young woman, Lee Swanson of Alabama, who found the hair said it was different from her own hair which she wore wrapped in a cap and there was no doubt that it was embedded in the piece of clay or charcoal which she saw at the time. The date is that of the charcoal. It will be possible to get some DNA readings from the hair; this should be exciting because it will be the oldest DNA found anywhere in the New World or the Old World too for that matter. How it relates to the DNA and mtDNA taken from modern Amerind and Na-Dene peoples will be most interesting to hear! Incidentally, the famous mummies of Peru will provide another source of DNA and mtDNA soon, from work being done at the University of Pittsburgh (S/ Public Health).

(II) A large bison bone found in a hearth of 'Zone M' or 35,000 to 37,000 years ago. I was standing near Dr. Roger Willis (Maine) when he uncovered the bone. There was no doubt that it was associated with a stratum across the cave which included the hearth. The bone had been broken off but not open and bone marrow extracted. Marks on the bone were being examined in March to tell conclusively if they were tooth marks or tool marks. A small animal could have removed internal marrow without breaking the bone in two. But I wondered how or why it would bring a large heavy bone to the hearth and I wondered if some of the bone was burnt by the fire in the hearth.

(III) The fact of the hearth itself which was deep inside a cave in a stratum that clearly contained it but was not itself a fireplace. Perhaps a small fire can have ignited spontaneously or burned by itself on the ground deep inside the cave but not burned the whole surface of the ground of its time (of what is now a stratum, not the surface of the ground). Perhaps it was started by lightning or represented a deep fire hole dug by inhabitants of a much later era (= much higher strata). But to my eye and those of the archeologists at the site the hearth was not derived from a later hole and it looked like a good substantial hearth which was locus to many fires, not just some accidental one. Many photographs of the hearth and its bison bone were taken; they bear out visually for anyone who wants to see the possibilities listed above. (I can lend some to those who wish to see some of this evidence. Courtesy of Sue DiCara & her camera.)

(IV) From one of the lower levels there was excavated an animal bone (deer?) in which is embedded an arrowhead made from the collar bone of a fox. Such was revealed by X-ray photography. Not only is it hard to believe that a little fox could impale a larger animal on its collarbone, presumably by running at it just as fast as it could, but also that the impact would turn the collarbone into the shape of an arrow. Come on sceptics! rack your brains and give us an alternative explanation! Make sure that your answer supports the Clovis hypothesis!

One is reminded that after one of the great New England hurricanes a paper straw was found embedded in the side of a telephone pole. Probably happens all the time during tornados in Kansas. Thus a prehistoric cyclone hurled the fox at the deer!

Incidentally, the use of bone points for arrowheads helps to explain Pendejo Cave's relative poverty of stone points at the lower levels. One of MacNeish's friendly critics (John Shea of Harvard) says that Pendejo Cave is a great site for paleontology but that the tools found at lower levels are too crude to be clearly human artifacts.

For the non-archeologist the tools at lowest levels are pebble and/or chopper types. Such objects are among the most primitive known to archeology, resembling nothing so much as rude stones from rivers or beaches which have been shattered or broken by some means and thus have edges. The crux of identifying pebbles as tools lies in showing evidence of human activity to produce the edges (rubbing, banging, chipping, etc., i.e. 'working' the stone. Having some resemblance to an adze makes it a 'chopper'.

The same criticism can be made of the lower levels at Tom Dillehay's 'Monte Verde' site in Chile and Niede Guidon's 'Piedra da Furado'in Brazil. Or the inhabitants used a few crude stone tools to supplement their primary dependence on other substances. Geoffrey Pope (U/Illinois) believes, as have others before him, that the secret missing ingredient in the archeology of greater Southeast Asia, for example, is tools made of bamboo . MacNeish believes that he has genuine stone tools in his lower levels -- above the lowest levels. It exceeds my capacity to judge what is a 'real' tool and what is a banged up piece of rock. But in Brazil Niede Guidon hired one of Europe's great experts on tool criteria -- Hans-Jürgen Müller-Beck (Tübingen) -- to examine the 'stuff' from their lower levels. He thought there were many 'real' stone tools and he thought them to be more than 30,000 years old.

(V) The fact of the palm & finger prints at 28,000 years ago has been challenged. While there is no argument denying that the prints are imprinted in the clay, critics argue that the clay may have been soft from time to time (through the ages) and a person at a much later age might have pressed her hand onto the soft clay. Ergo, while the clay was associated with (e.g. level M), the print embedded on it came from someone who lived later on (e.g., level C). This strikes me as the ultimate in sceptical thinking -- doubt to the point of idiocy. It may not be a true finger print on 28,000 year old clay because one can imagine how it could be different. What kind of reasoning is this scepticism? It is really an alternative hypothesis which ought to be examined in its own right. And let us think about probabilities as between these two theories. If the second or sceptic's theory is true, then we should have to explain how someone from a significant later level (= a level within the safe Clovis dates of 10-12,000 years ago) would have been able to make a print on clay buried several feet below her own ground surface but without leaving evidence of digging or other disturbance of the strata in between (i.e., intrusion). Methinks that has a natural improbability when compared with the alternative, namely that the fingerprint and its human maker come from the same level, the same age.

In fact there are three aspects of the palm prints that discourage doubt. First, the prints are very similar to a set from Kom Ombo, an old Egyptian site of 12,000 BC, embedded in hardened mud. Second, the Pendejo palm prints are adult palm prints, embedded or embossed in burnt clay, very hard stuff to press one's hand into/onto. Third, one of Scotty's critics, Dr. Dina Dencauze (U/Massachusetts), can testify that the palm print is not intrusive from above because she herself "stripped off zone G above it".

But the remorseless and relentless sniping at the data from Pendejo Cave moves me to another thought. Does every detail in a carefully excavated site have to be denied because scepticism can think up ad hoc alternatives? No! especially when the excavators were already considering these alternatives and looking for evidence of them. Can we not be relieved of this pervasive and fruitless doubting! No! It would be foolish to try to escape SCEPTICISM, one of the most characteristic and healthy scientific responses to new ideas, especially those which threaten established theories. . . Still, do you suppose that Hrdlička has come back to haunt us? Do linguistic bacteria cross disciplines?

(For linguists it is sufficient to know that a scholar named Aleš Hrdlička, a physical anthropologist, is famous most of all for his merciless criticism of other people's data and analyses to the point where his scepticism is sometimes credited with blocking progress in his field for nearly a generation. But our colleague, Bernd Lambert (Cornell), also notes that Hrdlička raised the standards of inquiry in his field and eliminated much 'garbage' from our common pool of knowledge.)

(2b) Some surprises about the settling of the New World.

In the most recent issue of MAMMOTH TRUMPET (June, 1992) there occurs a discussion of the "Diuktai culture". It is a Siberian Paleolithic, featuring "microblade tradition" and "microblade cores", which existed throughout much of northeast Asia somewhere around 25,000 to 35,000 BP. Ben Rouse mentioned it getting to Japan around 11,000 BP and it has been widely supposed to have reached North America. Being more specific, Alan Bryan (U/Alberta) was quoted saying that:

"The distribution of the microblade cores is the only typological tradition that can be traced from northeast Asia 25,000 to 35,000 years ago into North America. The microblade tradition persists through northern Canada and Alaska until quite recent times, but does not come south except in post-glacial times. You never have microblades farther south at an earlier time. I think this is a very significant thing. What Diuktai is is the peopling of northwest North America." Here endeth quote.

My word! Is it not the northwest part of North America? Is not its 'peopling' pretty close to the invasion of North America? Since post-glacial normally means Holocene and that generally means 10,000 years, is Bryan not telling us that northwest North American people moved south into the USA (presumably) not long after the Clovis type peopling of North America was taking place? In other words -- double leapfrogging! One peopling of North America right over the top of an earlier one -- which was over the top of a still earlier one. When the early Clovis types arrived across the Bering Straits land bridge, they must have walked through the Diuktai people who were already present in North America. A few millennia later the Alaskan variant of the Diuktai marched through the northern Clovis marches?

Theoretically it is possible that the heavy weapons (projectile points) Clovis culture could have evolved out of a Diuktai variant; just re-adapted to big game hunting -- maybe. But if that is so, then the Clovis did not come across the Bering bridge 13,000 years ago. They had already been in Alaska for perhaps 20,000 years! Lordy, lordy!

This business causes headaches! Ben Rouse had the opinion in 1985 ('migrations' book) that the late Diuktai or Paleo-Arctic culture moved south into British Columbia around 9000 years ago. Their "multiply hafted side-blades ... typically Asiatic and contrast strongly with the much larger, singly hafted end blades that were being used at the time by Folsomoid peoples south of the ice barrier . ." Folsomoid means Clovis type here and it poses a problem for us. Where did the big-game hunters (Clovis) come from? How did they avoid or intrude through the Diuktoid cultures of northeast Asia and Alaska at a time when pathways to the south were limited by continental glaciation? Why has no one found the archeological origins of the Clovis people in northeast Asia yet? Why, when we can trace the Eskimoan traditions all the way back to the Diuktoid cultures of northeast Asia and the Bering Straits, do the Asian prototypes of the Clovis folk evade us? Perhaps it is because the sources of the Clovis folk do not lie in Asia of 11,000 BC but rather can be found south of the glaciers in North America and rooted in the dimly perceived early Americans found in Pendejo Cave?

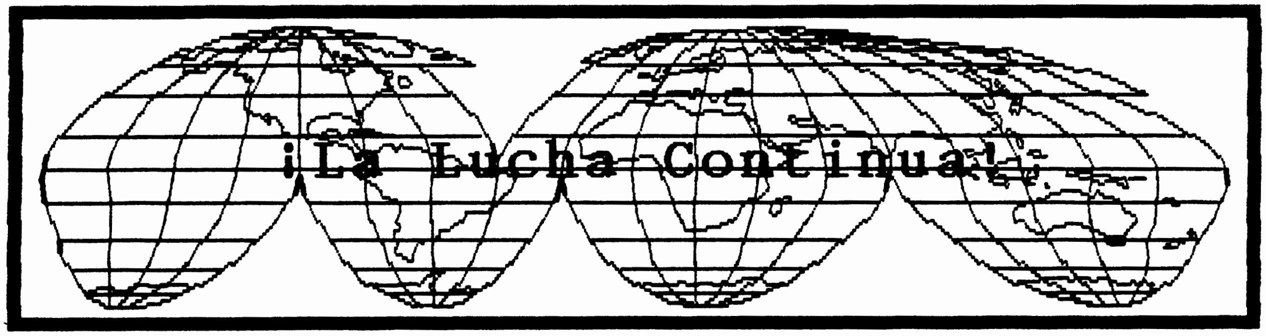
A Possible Analog in Japan: From Crude-lithic to Microblades. Ben Rouse summarizes early Japanese prehistory in a chart like this:

10,000-8,000	Projectile Point (Honshu & Hokkaido)
11,000-10,000	Microblade = Diuktai (from mainland)
18,000-11,000	Flake-and-Blade (local innovation)
30,000-18,000	Pebble-and-Flake (common East Asian)
130,000-30,000	Crude-lithic (pebble/choppers, barely tools)

Not too unlike the lowest strata at Pendejo Cave, n'est-ce pas? Might not early Americans be derived from those lower levels?

THE SIBERIAN ALTERNATIVE TO AFRICA: YURI MOCHANOV PROPOSES IT.

The man who gave us Diuktai Cave near the Algan river in Yakutska, Siberia, has expanded on his alarming earlier notions of really old 'human' habitation in the coldest place on earth, save Antarctica. Displayed prominently in the recent MAMMOTH TRUMPET, Yuri Mochanov's hypotheses are the boldest ever. Both Greenberg and Rebecca Cann are timid compared to him. Three million years of human physical evolution and the sources of modern man do not necessarily lie in Africa or Southeast Asia; they are found in Siberia, northeastern and coldest part of it. Human beings are the product of a long and profound exposure to cold climate, not the tropics. Yuri says. From his great gaping field site at Diring on the Lena river, more like a quarry than a typical dig and excavated with the help of bulldozers, Mochanov has come up with dates and artifacts aplenty. MAMMOTH TRUMPET reports that many Russian archeologists are critical or just don't believe. This is a case where one simply must be sceptical but open-minded and tolerant. It is partly a question for high tech archeology to cope with but, for the rest of us, our belief in man as a tropical animal goes back to DARWIN! It'll be tough!



BBC's 'HORIZON' PROGRAM: "BEFORE BABEL".

British Broadcasting Company did a program exploring issues centered around long range comparison in linguistics, and particularly global etymologies which propose a common human language ancestor, as well as biogenetic internal taxonomy of species Homo sapiens sapiens. In short they did human language origins from a number of standpoints. There was also an interesting section on Wm. Labov's studies of language changes as they happen in Philadelphia. Long rangers Ruhlen, Cavalli-Sforza, Greenberg, Shevoroshkin, Dolgopolsky, and Renfrew were featured speakers. The rest of us were left out as usual, even though the Stanfordites tried to have us included. Chris Stringer of the British Museum also added some valuable words about fossil East Africans. The opposition was barely represented, since their Professor Ringe of Penn was so offputting. His face work (kinesics) showed much arrogance. Bill Labov was absolutely neutral, as usual.

Some four copies of the original video tape are now circulating in North America. Long rangers are welcome to write ASLIP (Pgh, PA) to borrow a copy. We cannot sell it or rent it for money, since it is COPYRIGHTED, but we can lend it to friends and colleagues. For those who would like to see a typescript of the program, we can also lend them one -- courtesy of the ever-helpful Sheila Embleton. Indeed you might get faster results by writing to her, presuming you are in her network. The North American version is courtesy of Merritt Ruhlen who went to considerable trouble to have an electronic translation of the original made. In other parts of the world, whether you can get the BBC tape or not depends on the local system of VCRs. If the term VCR is unfamiliar to you, it probably means that you use a British system (which this tape was originally) or some other European or Japanese system.

NECROLOGY WITHOUT OBITUARY.

Sad news is always difficult to report. However, we regret that three worthy colleagues have joined their ancestors. Hans-Jürgen Pinnow of North Germany was a major figure in Indology, Southeast Asia studies, and the Northwest Coast of North America, especially the study of the Na-Dene. His theories and descriptions confront us all around the 'Pacific Rim'. Two colleagues, Norman Zide and Gérard Diffloth are preparing sections of a joint assessment of Pinnow's work. We hope to get Uwe Johannson to do a more formal obituary. There were rumors, whose veracity I do NOT know, that Pinnow had become embittered by the treatment he had received in Academia and had retired to a little island in the Baltic Sea. One of his great hypotheses was the linkage of Na-Dene and Sino-Tibetan. In this he was confirming the earlier work of Edward Sapir and Robert Shaefer, just as Starostin and Nikolaev have confirmed his own. It may yet come to pass that his relating Nahali to Austroasiatic will be accepted. IF that happens, it may also be the case that this bandit's language from central India will be the postulated link between Vasco-Dene and Austric. But I said 'IF'!

Zelig Harris of Pennsylvania, creative theorist and teacher of Chomsky, is the second man in our necrology. There is a fine obituary by Dell Hymes, a sometime colleague at Penn, in the latest issue of SSILA. I can remember as a student being more or less in awe of Zelig Harris, wondering how anyone could be so smart, and then later in life wondering why he had not gotten more credit for the 'revolution' in linguistics.

Also Steve Johnson, an Australian linguist and sometime long ranger, has died. There is a small obituary in SSILA. My ignorance of his work blocks further comment, no disrespect intended. We may get Geoff O'Grady, Ken Hale, or Adam Murtonen to comment on Steve Johnson's work. A number of us met him at the famous Stanford conference as part of that amiable Aussie delegation.

SETO DATA ON NORTHEAST CAUCASIC AND YENISEIAN.

Professor F. Seto (Tokyo), one of our founders and a stalwart long ranger has submitted large amounts of comparative material. This will be published in sections from time to time, since MOTHER TONGUE is not primarily a data journal. For this issue we offer a set of matchups or similarities between Northeast Caucasic and Yeniseian (Ket, Kot, et al). The final two pages show a small sample of matchups between Northeast Caucasic and Ostyak and/or other Samoyedic languages. Bert Seto has no axe to grind in this presentation, i.e., his basic motive is not taxonomic; nor is he testing specific taxonomic hypotheses. His interest is in sharing with us a large amount of data he has collected over the years. While he does draw some historical conclusions from the particular data for individual clusters of languages, that is not displayed here. His data are presented as given, the phonetic or phonemic symbols appropriate for Caucasic or peculiar to the sources of his data are still beyond the capacity of my printer. Seto's data are in a separate section following 'La Lucha Continúa'.

Given the constant discussion of similarities or matchups -- as opposed to proper cognates -- in historical linguistics, it is interesting to note that the quality of the matchups with Yeniseian are obviously better and more more numerous than those with Samoyedic. One ought to find it so, if Starostin is right. Yeniseian is part of Sino-Caucasic. Samoyedic is part of Uralic, thus Eurasiatic, thus a more distant relative. All this from a Caucasic standpoint. --> In any case thank you for the data, Dr. Seto!

A NEW NIHALI/NAHALI SOURCE.

There is a distinct insufficiency of published data on Nihali/Nahali or Nehari of central India. This famous language, spoken by bandits, has long been recognized as a critical problem for South Asia. As mentioned elsewhere, it may be a key historical link between phyla of the west -- such as Sino-Caucasic -- and phyla of the east -- such as Austroasiatic. Since it is stuffed full of borrowings from Dravidian, old Indic and young Indic, and Austroasiatic languages, its classification has been difficult. More data would help us greatly. Now, thanks to Norman Zide (U/Chicago), we may be able to aid in the publication of a much larger corpus of data, gathered by an Indian scholar.

Even though MOTHER TONGUE is not primarily a data journal, still some things are of over-riding importance. We will just publish more pages of MOTHER TONGUE to accomodate it, hoping constantly that someone will toss some extra money in our tin can to help fund it. Commercial copying is quite competitive in Pittsburgh -- ditto university --, so those are not the critical costs. It's postage that sometimes breaks a budget.

NEITHER INDO-EUROPEAN HOMELAND SUPPORTED BY GENE STUDY.

Hot off the press. August 18, 1992. New York Times, p. B9, reported that Robert Sokol and team from S.U.N.Y., Stony Brook, Long Island, announced that they were unable to support either Colin Renfrew's or Marija Gimbutas' theories about the spread of Indo-European languages, or the homelands thereof by implication. "Last year . . (his team) lent support to the agricultural theory by demonstrating that genetic patterns are correlated with the spread of agriculture. If no correlation had been found . . the agricultural theory would have collapsed because there would have been no mechanism for the spread of languages. But the existence of a mechanism does not prove the theory. ." So now Sokol and his colleagues have to find a correlation between genetics and the spread of language itself. They analysed proteins from modern people at more than 3,300 sites across Europe last year and added more this year. They tried to match the genetic gradient to language patterns in Europe. They did find a correlation between genetics and language, "but no statistically significant evidence that the matchup is explained by either the language-follows-agriculture theory or the conquerors-out-of-Ukraine theory. (Renfrew and Gimbutas respectively). Sokol's final comment is interesting. "We have not disproved them but our genetic evidence does not support them. This argument has to shift to other ground." The article was published in the 'current issue' of THE PROCEEDINGS OF THE NATIONAL ACADEMY OF SCIENCES.

NEW FOSSILS FOUND IN ETHIOPIA.

As reported in SCIENCE (Vol.256, p.309, 17 April, 1992), Berhane Asfaw, director of the National Museums of Ethiopia, has announced the discovery of several fossil hominids in the Middle Awash lowlands of Ethiopia. As Berhane said, "despite other problems in the country, we keep moving. We have discovered more hominids." At Maka Berhane, Desmond Clark and Tim White found a jawbone of 3-4 mya, probably an example of Australopithecus afarensis. That was Lucy's species. Then at Gameda they found a mandible of someone living there 2-3 mya, but dates are preliminary. Some other "beautiful hominids" were found north of Hadar where Lucy was found. But everything is preliminary, especially dates and classifications. Dinq nesh, we call her.

More importantly for us a humerus from a hominid of 500-400,000 years ago was found at Bodo. It is "probably an example of archaic Homo sapiens". Since the archaic line in Europe which finally begat Neanderthal was just about the same age, this suggests that the Ethiopian line -- if it does lead to Homo sapiens sapiens -- is independent of the European line. How much can you tell from an upper arm bone?

NEW HANDBOOK OF AMAZONIAN LANGUAGES, REVIEWED BY AUSTRALIANIST.

It is an unusual event for an expert in a remote group of languages to review a book about another remote group of languages. It is even more unusual for said scholar to drop what he was doing and rush over to start work on the other group. Yet that is what reviewing the HANDBOOK OF AMAZONIAN LANGUAGES did to R.M.W. Dixon of Australian National University when he read it. Dixon, one of the leading Australianists in the world, found the grammars in volume I of this series so exciting that he went to Amazonia to do his own field work. Now, as reported in DIACHRONICA IX:1.111-114 (1992), he reviews volume III, consisting of large monographs of Macushi and Paumarí (Carib and Arawá respectively) plus a 145 page comparative study of Maipuran (Arawakan) languages. The Handbook is edited by Desmond C. Derbyshire and Geoffrey K. Pullum. Published by Mouton de Gruyter, it came out in 1991.

We're discussing this here to alert long rangers to the three volume series on Amazonian languages, not to review Dixon's review. (He is not entirely happy with the way the editors structure the grammars) The exciting part of volume III for Dixon and the rest of us is more likely to be David L. Payne's "A Classification of Maipuran (Arawakan) Languages Based on Shared Lexical Retentions". While noting that South American comparative linguistics has not measured up to I-E standards, still Payne's Maipuran study is "of the highest quality, and is thus particularly welcome." He also mentions that the Arawak family was recognized 10 years before Sir William Jones's famous address and its father was an Italian missionary, Filippo Salvatore Gilij. (Odd spelling of an Italian name, what?) Payne stays within the safe boundaries of the Maipuran cluster, eschewing comparative work on the whole Arawak family which "cannot be shown to be related". Even Paumarí of the Arawá family cannot be included. Apparently previous work on reconstruction has been fairly poor. But this is classic timidity of the sort that has made Americanist linguistics so exciting. Let us hope that Alexandra Aihenvald can ventilate the Amazon with some cool Russian breezes!

IMPORTANT DISCOVERY HALF ANNOUNCED, THEN SILENCED.

Flash! Followed by -- Hold the Press! Can that Story! We regret that we had to cancel the announcement of a hot discovery. MOTHER TONGUE will report it in a future issue; with luck the December issue. The discoverers themselves asked us to hold off until they get some key facts checked. They would have been embarassed by premature publication and so we held the story back. Your mad editor, of course, cannot resist teasing you with such a bon bon. Stay tuned in. You will hear soon enough and at that time you will appreciate the scholars who carefully checked their facts. By golly!

WAS THE FIRST EGYPTIAN RULING CLASS WHITE OR BLACK?

In the AJPA 87:245-254 (1992) Professor S.O.Y. Keita, Dep't. of Anthropology, Bioanthropology Laboratory, University of Maryland, College Park, Maryland, had an article entitled "Further Studies of Crania From Ancient Northern Africa: An Analysis of Crania From First Dynasty Egyptian Tombs, Using Multiple Discriminant Functions. His ABSTRACT said:

>>>> "An analysis of First Dynasty crania from Abydos was undertaken using multiple discriminant functions. The results demonstrate greater affinity with Upper Nile Valley patterns, but also suggest change from earlier craniometric trends. Gene flow and movement of northern officials to the important southern city may explain
<<<< the findings."

Buried in this semi-opaque anthropometric jargon are some interesting and mind-set altering points that Keita makes. His discussion of the background reveals that two sets of conflicting hypotheses exist vis-a-vis the ancient Egyptians. First, was Egypt united for the first time in the First Dynasty? And did the rulers of that dynasty come from the north, possibly even as elite immigrants from Sumeria? The Dynastic period is usually dated to 3100 BC; he lists 3050 BC or 5042 BP. Secondly, were not the first Egyptian rulers and/or the northerners from Lower Egypt actually white people, more explicitly Mediterranean whites? And the southerners very mixed with black people, more explicitly central African Negros? He has two general answers.

First, citing Bruce Trigger and others, he points out that unification may well date back to the late 'pre-dynastic' period or Nakada III = 3300-3050 BC. Writing may begin around 3100 BC and there may be unification myths of the same date but the myths may be reports of later local or provincial victories. Moreover the ruling class of Abydos, the founding Paris of united Egypt, were more local southerners than northerners, yet showed the physical influence of the north. That came from gene flow from mobile northern officials, Keita thinks.

Secondly, he concludes that the basic southerners, Up River Egyptians, were accurately described as neither Mediterranean white nor African Negro. They belonged to an African local race which might be called the Elongated Africans or Hamites were that not such a dirty word. Because he has to fight his way through the dense thicket of racist and anti-racist polemics he is unable to move beyond the taxonomic limitations of either camp. It seems clear from his descriptions that the southerners would look much like, or fall into the range between, modern Nubians and the Beja of the eastern hills of the Sudan. These folks in their biogenetics anyway strongly resemble a belt across Africa of what Frank Livingstone calls Saharan type Africans or what I call Ethiopids -- simply because they greatly resemble Ethiopians. Some 'modern' Africanist taxonomists have failed to distinguish between Nilotes and Ethiopids, so great is their similarity in linearity and sometimes height. Yet biogenetically the Nilotes much adhere to shorter cousins in the Sudan but keep a substantial distance from the Beja, northern Sudanese, most Ethiopians, and Somalis. More than anything else, Keita stresses, Africans are a very heterogeneous lot -- even more than Melanesians -- and scientific descriptions or thinking about them have been greatly hampered by the tendency of Europeans and some Africans and African Americans to cram everybody into the great dichotomy -- BLACK OR WHITE. Or let us think in Becky Cann's terms: if Africans display the greatest diversity in mtDNA in the world, while still many African populations remain unexamined, what is the scientific value of lumping them all together as "blacks"? They don't even all look alike!

What about the northerners? They come to dominate the ruling class at least and they are less 'Africoid' than the southerners. Just simply Mediterranean whites? No, says Keita, they are different. They most resemble a series of crania from the Magreb. They are intermediate between equatorial Africa and northern Europe. Keita wants to label them coastal northern Africans. He sees them as the result of long term local developments and mergings of local groups of north Africa and the Nile Valley, NOT as the result of a migration of Near Eastern whites into Egypt. So be it!

WAS NEANDERTHAL OUR ANCESTOR? A SKIRMISH AMONG THREE STUDIES.

All were presented in AJPA this year. All contribute something for our edification on the Neanderthal question but I don't know what. Their abstracts and titles are presented along with minimal comments of interpretation.

Vol.87:255-262 (1992) has U.Zilberman, M.Skinner, and P.Smith of Hebrew University-Hadassah, Jerusalem and Simon Fraser University, British Columbia, reporting on "Tooth Components of Mandibular Deciduous Molars of Homo sapiens sapiens and Homo sapiens neanderthalensis: A Radiographic Study. Their Abstract says:

>>>> "Tooth components of deciduous molars were measured from standardized radiographs of Homo sapiens sapiens and Homo sapiens neanderthalensis. Enamel height and width were greater in deciduous teeth of Homo sapiens sapiens than in Homo sapiens neanderthalensis and the differences were statistically significant ($p<0.01$). Dentin height showed no significant differences between the two groups, but enamel to floor of pulp chamber and pulp height and width dimensions were significantly greater in Homo sapiens neanderthalensis. Discriminant analysis carried out between groups, using deciduous tooth components, showed an accuracy of 98-100% for identification of Homo sapiens sapiens and 83-92% for identification of Homo sapiens neanderthalensis. The results obtained in this study on dental dimensions support the hypothesis of a distinct evolutionary line <<<< for Neanderthals."

Citing their final conclusion, "The characteristic morphologic traits of the Neanderthals dentition . . ., together with the analysis of tooth components, suggest that there are major differences in early stages of development between (the two -HF) and support the hypothesis that the Neanderthals are an evolutionary side branch . . .

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Vol.88:1-21 (1992) had Robert G. Tague of Louisiana State University reporting on Sexual Dimorphism in the Human Bony Pelvis, With a Consideration of the Neanderthal Pelvis From Kebara Cave, Israel. His abstract is long and inconclusive:

>>>> "Sexual dimorphism of the human pelvis is inferentially related to obstetrics. Howver, researchers disagree in the identification and obstetric significance of pelvic dimorphisms. This study addresses three issues. First, common patterns in dimorphism are identified by analysis of pelvimetrics from six independent samples (Whites and Blacks of known sex and four Amerindian samples of unknown sex). Second, an hypothesis is tested that the index of pelvic dimorphism (female mean X 100/male mean) is inversely related to pelvic variability. Third, the pelvic

dimensions of the Neanderthal male from Kebara Cave, Israel are compared with those of the males in this study."

"The results show that the pelvic inlet is the plane of least dimorphism in humans. The reason that reports often differ in the identification of dimorphisms for this pelvic plane is that both the length of the pubis and the shape of the inlet are related to nutrition. The dimensions of the pelvis that are most dimorphic (that is, female larger than male) are the measures of posterior space, angulation of sacrum, biischial breadth, and subpubic angle. Interestingly, these dimensions are also the most variable. The hypothesis that variability and dimorphism are inversely related fails to be supported. The factors that influence pelvic variability are discussed."

"The Kebara 2 pelvis has a spacious inlet and a confined outlet relative to modern males, though the circumference of both planes in the Neanderthal are within the range of variation of modern males. The inference is that outlet circumference in Neanderthal females is also small in size, but within the range of variation of modern females. Arguments that Neanderthal newborns were larger in size than those of modern humans necessarily imply that birth was more difficult in

<<<< Neanderthals."

Methinks this proves nothing but we shall ask Dr. James Egan, our authority on this subject, what he makes of all this.

Vol.87:433-445 (1992) had Robert S. Corruccini of Southern Illinois University, Carbondale, IL 62901-4502, reporting on "Metrical Reconsideration of the Skhul IV and IX and Border Cave Crania in the Context of Modern Human Origins. His abstract says:

>>>> "The 'out-of-Africa' models for origins of modern *Homo sapiens* incorporate Skhul as one site documenting that early origination. However, only Skhul V is usually considered in the comparative craniology of the question, neglecting the other substantial crania, Skhul IV and IX. Craniometric comparison demonstrates that IV and IX amplify the picture of continuous gradations of Neanderthal-to-modern variations throughout the Levant; much variation is thus represented within this one site, raising serious questions about Neanderthals and moderns being discrete and long-separated species. Qafzeh 6 too is craniophenetically closer to Neanderthals than to true anatomically modern people of the European Upper Paleolithic. Proper distance analysis of Border Cave cranium shows it is actually far removed from modern African populations. References to Qafzeh, Skhul, and

<<<< Border Cave as 'fully anatomically modern' require reconsideration."

The abstract is a politer version of his vigorous, polemical argument that the 'Garden of Eden' theory is old hat and all wet. He argues well, logically, and creatively but the whole argument cannot be repeated here. Do go to the original in AJPA. But there are surprises. In also rejecting 'rising tide' theory, he gives a good cuff in the ear to Milford Wolpoff whose well-known argument about finding all those 'Mongoloid' traits in East Asian Homo erectus fossils is dismissed emphatically as dead wrong. W.W.Howells, a leading senior paleoanthropologist, is cited as having investigated most carefully and copiously the question of links between modern humans and earlier types.

>>>> "Howells shows that among Neanderthals 'no real indication appears of a closer link to one living population group (e.g., Europeans) than to others' and that 'modern populations taken all together are indeed limited in the geographical variation of both shape and size of the cranium . . Some early anatomically modern specimens may tend to fall a little out of this range. The impression, true or not, is that the modern unity is fairly recent.' Howells' results, based on unprecedently good samples, numbers of measures, and sound methodology, indicate no support for the multiregional continuity idea resurrected and adapted from Coon (1962), or the early African origination of current human varieties suggested by mtDNA. . . (Some other studies -HF) Using what

I regard as convincing methodology, they show that there is no 'mongoloid' quality whatsoever to those hominids (Zhoukoudian Upper Cave hominids - HF) (but some vague similarity to Ainu and Australian aboriginals), denying the great antiquity of Asian races." <<<<

Howells also "rightly denies Stringer and Andrews'(1988) proposition that greatest modern population differences will be between Africans and all others: 'there is no sign that, cranially, Africans are set off as most distant from other populations -- quite the reverse --; there is no support for a sub-Saharan first source for anatomical moderns.'This confirms that fossil regional skull shape differences resembling the modern distribution appear only near the Holocene boundary (Washburn, 1944)." (Editor's note: the Holocene boundary is circa 10,000 BC)

Finally, Corruccini's own creativity shows up. First quote: "The still too Eurocentric idea would be that Progressive Neanderthal populations are the only confidently known predecessors both of (moderns - HF) and of glacially isolated extreme Western European Neanderthals of the later Wurmian time, and that anatomical modernity evolved in hominids of the general tri-continental crossroads of North Africa, Western Asia, and Southeastern Europe prior to 30,000 yr BP -- again an old idea . . but one I believe deserves much more circulation among the other revivals currently provoking debate . . A revived Preneanderthal formulation would incorporate a gradualistic version of the single origin model but deny its earliness and exclusively South African Eden." Wow! He debates powerfully. And his second quote: "The old McCown/Keith idea of Mount Carmel variation signaling a species in the throes of evolution may lack neontological theoretical backing, but if we cannot expect evolution to follow uniformitarian principles then this idea fits the current data at least as well as the 'out-of-Africa model.' End of quoting.

Some final notes on this. 'Phenetically' is a new word, borrowed from numerical taxonomy and systematics in biology. It goes in a triad of phenetic, cladistic, and chronistic relationships. Phenetic is based on the borrowed Greek stem which now means 'appearance' or the like in scientific English. So it corresponds to the linguistic terms similarity, match-up, or look-alike. Cladistic means having a common ancestor, relating through common descent. It is exactly what cognate means in linguistics. It is based on Greek 'klados'= branch, branching. Chronistic means being a contemporary, sharing the same period of time. Everyone knows the chronos root.

Border Cave is not in Israel or the Levant. It's in South Africa. I wonder why he thinks so little of the east African fossils which date to circa 100,000-120,000 BP. And Howells may be a great paleoanthropologist but cranial criteria by themselves will not make that African genetic diversity go away. Phrenology was wrong too, right?.

LANGUAGE REALLY DOES SEEM TO BE IN THE LEFT HEMISPHERE OF THE BRAIN.

In SCIENCE, Vol.255: 1258-1260, 6 March 1992, David P.Corina, Jyotsna Vaid, and Ursula Bellugi published a very interesting hypothesis testing type article, entitled: The Linguistic Basis of Left Hemisphere Specialization. Their summary follows:

>>>> "In humans the two cerebral hemispheres of the brain are functionally specialized with the left hemisphere predominantly mediating language skills. The basis of this lateralization has been proposed to be differential localization of the linguistic, the motoric, or the symbolic properties of language. To distinguish among these possibilities, lateralization of spoken language, signed language, and nonlinguistic gesture have been compared in deaf and hearing individuals. This analysis, plus additional clinical findings, support a linguistic basis of left <<<< hemisphere specialization."

A wounded deaf signer with sign language aphasia could still gesture symbolically.

ON ANECDOTAL UNIVERSALS IN HISTORICAL LINGUISTICS.

Writing in DIACHRONICA IX:1.135-137 (1992), long ranger Alexis Manaster Ramer of Wayne State University, Detroit, finds exceptions to two statements which he takes as universals by "two distinguished comparative linguists" -- Eric Hamp and Sergei Starostin -- and then unleashes a prescriptive universal of his own. First, he criticizes Hamp for rejecting "the Nostratic etymon" for 'wolf, dog' because lumping these two concepts together (Hamp says) would be "playing fast and loose with the semantic content." Alexis shows that Modern South Arabian does interchange the two concepts. One might add that he could have argued the issue on common sense alone. Secondly, he criticizes Starostin for saying that: "You do not borrow hand; it does not happen." Then Alexis shows two cases where it was borrowed. One wonders why the Moscovites seem to go rigid in defending the absolute universality of what are obviously only statistical universals. They will have to stop; Alexis is right. Very basic vocabulary does sometimes get borrowed -- or displaced -- but not very often.

Alexis then issues his prescriptive universal: "As a result, it might be a good idea to resist any facile claims about language relatedness based on similar-looking words for 'hand', or indeed any other particular 'stable' words. This is not to say that there is no such thing as 'stable' words, only that this fascinating topic should be approached with all due caution." Why, for heaven's sake, do we need to come to a complete halt because Starostin overstated a good working statistical universal? This does NOT follow at all. Moreover Hamp's case was not a universal anyway; just a foolish semantic statement. Just look at the things that have been related semantically in I-E!

THE BOOK ON THE FAMOUS STANFORD CONFERENCE GETS REVIEWED, ETC.

Also in DIACHRONICA, IX.1.87-104 (1992) Philip Baldi's edited book on the Stanford Conference of 1987 was the subject of a review article by Carol Justus of San Jose State University (California). Instead of doing all the names here, I refer you to the long review in DIACHRONICA. I confess to the same general reaction that Victor Golla of SSILA seems to have had when mentioning it in SSILA's recent newsletter. Not much! Other than to say that Justus writes a cool, 'professional' review of Baldi's book there is not much else to say. Go read her review! She seems to be one person who took the discussion of the 'comparative method' seriously and she discusses it. The conference as an interaction of many groups with various agendas was something else.

Some other articles and reviews in DIACHRONICA this year include the following:

VIII.1 Kees Versteegh (Nijmegen): "The Substratum Debate in Creole Linguistics", a review article.

T.L.Markey & John A.C. Greppin (eds), WHEN WORLDS COLLIDE (Ann Arbor, Mich.,1990), reviewed by E.J.W. Barber (Los Angeles). About the conference on I-E and pre-I-E, held at Bellagio, Lake Como, Italy in 1988. Another famous jamboree.

Mieko Ogura, DYNAMIC DIALECTOLOGY (Tokyo, 1990), reviewed by Sheila Embleton (who doesn't know her address?)

VIII.2 Salikoko Mufwene (Athens, GA): "Language Genesis and Human Evolution", a review article aimed at Derek Bickerton's new book on the same thing. (The exact title has gotten lost somewhere near here!). See Vol.IX for more volleys on this.

Yoël L. Arbeitman (ed.), A LINGUISTIC HAPPENING IN MEMORY OF BEN SCHWARTZ (Louvain-la-Neuve, 1988), reviewed by Ruggero Stefanini (Berkeley, CA).

IX.1 Theo Vennemann (München): "Language Universals: Endowment or inheritance". An article.

Julius Pokorny, INDOGERMANISCHES ETYMOLOGISCHES WÖRTERBUCH, 2nd ed. (Bern & Stuttgart, 1989), reviewed by Winfred P. Lehmann (Austin, Texas). One of the bibles of long range comparison reviewed by one of the masters.

Edwin Pulleyblank, LEXICON OF RECONSTRUCTED PRONUNCIATION OF

EARLY MIDDLE CHINESE [...] (Vancouver, B.C.,1991), reviewed by Laurent Sagart (Paris). Pulleyblank has also written a long tres compliqué article on another aspect of this, entitled "The Ganzhi as Phonograms and their Application to the Calendar" in EARLY CHINA 16:39-80 (1991). He should now pass his exams for Mandarin 1st Class. Such erudition! Ted Pulleyblank is also a long ranger of standing. On page 76 in his conclusions he says briefly what I struggled to express in the EDITORIAL (below). I quote him.

>>>> "To what extent can one regard any of these theories as proved? Proof in historical questions of this kind is, of course, different from proof of a mathematical theorem, but historical sciences are no different in principle in this respect from the so-called 'hard sciences' in which it is by now well-recognized that all conclusions are only provisional, standing only until they are overthrown by irrefutable counterevidence. There is, of course, an additional obstacle in that it is not possible to conduct experiments to test theories of the past. One can only look for such evidence as survives from the past and, in the case of historical linguistics, also use the evidence of the advancing science of general linguistics to help in discriminating between probable and improbable solutions to one's problems. With these limitations, I am confident that the hypotheses are strongly supported by the available evidence and will not be easily shaken, whatever <<<< modifications may have to be made in detail, as studies advance".

In IX.1: 145-149 Derek Bickerton (Honolulu, Hawaii) replies to Mufwene's review of his (Derek's) book. Then on pages 149-152 the reviewer who is now Salikoko S. Mufwene, and now of Chicago, responds to the reviewee. Note: we have never taken the space to examine the Bickerton thesis carefully or in any detail; nor his critics' arguments. We ought to sometime. Does anyone wish to take the topic and run with it?

ONE LOST ANCESTOR FOUND IN GEORGIA, IN AN OLD HOUSE.

In the Research News section of SCIENCE, 24 January 1992, p.401 there were excited reports that another old jawbone has been found. This one was " a remarkably complete mandible", large and heavy, "with all 16 teeth still in place, was embedded in the foundation of a house in the long-deserted city of Dmanisi, along with archaic stone tools, the skulls of two saber-toothed tigers, and the rib of an elephant." The mandible is thought to be clearly either 900,000 years old or 1,600,000, not in between because the dating depends of magnetic orientations which reverse themselves sometimes. Thus Homo erectus Dmanisi-ensis will be the oldest hominid in or close to Europe and one of the oldest or the very oldest outside of Africa. The primary work was and is being done by a Georgian-German team, led by Vachtang Dzarparidce of the Georgia Academy of Sciences (Tbilisi presumably) and Gerhard Bosinski of the University of Cologne (Köln). This was not found by a carpenter but by deliberate excavation of a house!

YET ANOTHER LOST ANCESTOR FOUND IN A MUSEUM

The same science reporter, Ann Gibbons, in the same journal 5 weeks later told of another remarkable discovery. Professor Andrew Hill of Yale led a team into the National Museum of Kenya and there excavated this old fellow from a tray where he may have been sitting for two and half million years. The team was to help fight off the bureaucracy. All that was left of our ancestor was a fragment of her skull but they used 'state-of-the-art dating methods' to find that 2.6 million years had passed since her demise. Even though the fragment was only a 3 inch fragment Steven Ward (Northeastern Ohio University) determined that it was genus Homo, rather than one of the Australopithicine offshoots. Thus it is "the earliest member of the genus Homo by about half a million years". The super-high tech dating was done at Berkeley and is called single-crystal laser-fusion argon-argon dating. It was not herself who was dated but rather the volcanic deposits above and below her resting place.

HEARSAY: YOUNG ASLIPERS ATTACK GREENBERG.

From a reliable colleague there is talk of articles in IJAL and elsewhere where the relatively new custom of Greenberg bashing has appeared among ASLIPers, erstwhile or alleged long rangers. That is certainly no sin. Joe Greenberg himself would maintain that we are doing science and what else would you expect? In any case Alexis Manaster Ramer attacked on the grounds that some of the Amerind etymologies really could be seen as Na-Dene and therefore the language involved (Tonkawa of Texas) could be classified as Na-Dene instead of Amerind and didn't that show that the Amerind hypothesis must be wrong. Ha! does Greenberg use his methods the same way as Alexis? But that is what I saw in an earlier draft; it might have been changed. Hearsay.

In any case Alexis uses fussy logic. If 20 'Sinitic' languages had been misclassified and turned out to be Thai-Kadai, does that mean that Sino-Tibetan is overthrown? In a strict technical sense, yes. The initial hypothesis consisted of hundreds of Sinitic languages plus the 20 in question. With them extracted Sinitic was not quite its old self (X) but rather (X-20). But we get a leaner meaner Sino-Tibetan out of it plus a new strengthened phylum of Thai-Kadai plus the important knowledge that old Sinitic exchanged things with old Thai-Kadai. That case actually happened, as most of us know already. Only two years ago it happened again in Africa when Thilo Schadeberg showed that a few Kordofanian languages were Nilo-Saharan instead of Niger-Congo. Big deal! Did Thilo topple Niger-Congo? Of course not; he added important things to Nilo-Saharan. Did Alexis topple Amerind? He didn't even prove that Tonkawa was Na-Dene -- I hear.

William Poser of Stanford mounts violent attacks on Greenberg's Amerind from time to time and apparently one of them got into IJAL. Hearsay. (We didn't get to IJAL yet this year.) But it would be splendid if one of our courageous Americanists actually left the womb and counter attacked once in a while. Naturally people would pass you in the hallways without speaking and no one would invite you to dinner anymore and life would be hell. Courage, mes petites!

THE DIFFUSION OF AGRICULTURAL TERMS FROM MESOPOTAMIA.

This is the title of a recent paper published by ARCHIV ORIENTALNI 60:16-37 (1992). The authors were long rangers Vaclav Blazek of Pribam (Bohemia) and Claude Boisson (U/Lyon, France). The study is venturesome and valuable, especially since most of us have waited for decades for someone to do this. If, as everyone knows, the first agricultural revolution in the world started in the ancient Near East between the Zagros mountains and the Sinai, then which known historical peoples were involved? Which language(s) lent their neighbors new words for agricultural things and which peoples simply pushed their neighbors aside in seeking new lands. The passage of some time is necesary before the rest of us can check the etymologies and think about the meanings and decide how much to accept and how much to reject. But one thing already seems sure to me: most of their etymologies are carefully done and will be accepted.

Having said that, however, I regret to say that many of their presuppositions may be rejected. Sans doute their work is weakened by their taxonomic timidity vis-a-vis Sumerian versus Elamitic versus Dravidian. Most of their proposed etymologies involve those three. Whether to treat them as cognates or as loanwords is surely vital & crucial to their paper. They never quite decide. A vast literature of archeology is ignored because "we are linguists", but they then take the ideas of Renfrew and Shnirelman as somehow given. I can hardly blame them for doing that because both are fine scholars. But how does a linguist decide which archeologists have the best hypotheses? For another example, they accept uncritically Militariev's recent proposal that East Cushitic loanwords occur in Mesopotamia. But why, when it is an untested proposal? And why, when the subject is controversial, do they accept uncritically one person's idea of the p-AA homeland and not another person's? They presuppose but on a slim basis, i.e., loyalty to friends in Moscow. That's great socially but not scientifically.

NEW AUSTRO-TAI STUDIES INSTITUTE

Paul Benedict writes that ". . an Austro-Tai Studies Institute has now been set up (yes, you can cite it in MOTHER TONGUE), with Solheim, me, and the Japanese anthropologist Kazuro Hanihara (Kyoto) as founding members and trustees, centered in Guam, with offices planned for Honolulu as well as for Bangkok and Kyoto, with backing from Transpacific Associates (TPA), Guam, with long-range plans to study cultural and linguistic problems throughout the vast Austro-Tai area." Long rangers are implicitly invited to communicate but probably not just yet -- we need final addresses.

RAISING STANDARDS FOR CITATION FORMS, ITEMS USED IN ETYMOLOGIES

Another point in another letter by Paul Benedict concerns standards. In commenting on MT-16, he wrote: (note: we altered his text slightly because he abbreviates a lot.)

>>>> "The list of 11 Dene-Sino-Caucasian nicely illustrates the problems that critics bring up. The Sino-Tibetan citations would get a mark of about D. Bad enough if only the result of ignorance or carelessness but there are clear indications of deliberate 'doctoring' of some forms, e.g. in the Conspectus I make it quite clear that the Tibeto-Burman root for 'tongue' is /*b-lay/~/*s-lay/, with /*b-/ regularly yielding [m] in forms like Ao Naga /te-meli/, yet the root is cited as /*mlaj/! This is entirely unacceptable, of course, and can't you or others connected with MOTHER TONGUE try to put a stop to this indefensible practice? Why not institute some simple rules, e.g. when citing forms from a stock for which roots have been reconstructed, e.g., Tibeto-Burman or Sino-Tibetan, the writer must cite the reconstructed root, where available, and when citing any variation(s) must indicate the basis for the modification! Couldn't an editor try to see to it that a rule along these lines is followed? And why can't the editor, by arrangement, send the material to an expert in any of the stocks/phyla involved? Try to get a list of experts willing to look at material -- I volunteer for both Sino-Tibetan and Austro-Tai!

"I'm ordering the volume on Dene-Sino-Caucasian and will compare the North Caucasian roots with my Sino-Tibetan roots -- finally at least in this instance we'll be able to compare reconstructed roots for at least two of this trio -- I'll send you my results -- maybe also compare with what Matisoff and I have already got [illegible] comparing Sino-Tibetan with Austro-Tai (he has a paper on it and

<<<< I've added some [illegible]ings)."

(Editor's note: This is not a new conversation, as some of you will note. My response to the main suggestion is the same as before: I would if I could but I can't, or would I? So this question to Paul --> if reconstructions are meant to be the data retrieved from some ancestral period -- reversing the history of change as Anttila might put it --, then they ought to be fairly good in quality as data. This being the case, in order to trust the reconstructions as accurate reflections of the ancestor, then we ought to know how the ancestor (starred form) was reconstructed. In the case of I-E we believe we know how for the most part. But I think some non-I-E reconstructions are unreliable, often really bad, as retrieved ancestral data. For others I am from Missouri (=sceptical); someone has to show why something is trustworthy. Until they can do that, I prefer to compare good data, preferably analysed.

SHEVOROSHKIN's FIFTH BOCHUM BOOK COMES OUT IN SEPTEMBER.

As the editor of a series that carries great interest for long range linguistics, Vitalij Shevoroshkin announces the 5th volume is coming out in September. It title is something like 'Nostratic, Dene-Caucasian, Austric, and Amerind'. Many famous long rangers from Anttila to Wescott have articles in its 580 pages. Our problem with the series is that in 6 years no review copy has ever been sent to MOTHER TONGUE. So we cannot review it.

OUR MAN IN ALBANIA REPORTS ON THINGS AND HIS DIG.

Karl Petruso of U/Texas, Arlington, spent the summer in Albania, coping and trying to get lots of stuff from his excavations, like the good archeologist he is. However, it's a bit early for Karl to tell us all the things we would like to know. For the people of Albania it is a period of great political and economic change because the old regime is dead but the new one is not quite born yet. Karl will tell us something of conditions in Albania, not only for the people but also for archeologists & other savants.

His archeological work was primarily in the Saranda district in far southern Albania, really close to Greece. No definite conclusions of course as yet but some sites are yielding Neolithic, Bronze Age, Classical and also Paleolithic materials -- not necessarily all dug this summer. He will tell us more of this in a later issue. When asked if he knew anything new about the 'Illyrian as mother of Albanian question', he said maybe. So many people want the hypothesis to be true that it's hard to be objective. He will also consult with Eric Hamp, the master of Albanian as an I-E language.

FLASH! FLASH! HOLD THE PRESSES! MORE EXCITEMENT FROM PENDEJO CAVE!

Pretty much by accident we caught Scotty MacNeish and obtained his new results. Since our printing was delayed by end-of-summer office repairs at the copiers, we had a chance to try and find out what ever happened to the lab reports on the human hairs from 19,000 BP at Pendejo Cave. By pure luck Scotty answered the phone. He's about to go to China where he will look into early agriculture west of Shanghai. Our luck!

Why should anyone get so excited about this stuff? You will ask it yourself. And you will answer it yourself. The most ordinary of human exuviae when found in a properly conducted excavation in a well stratified site and confirmed by careful laboratory examination can have clear historical meaning. Which exuviae?

human hair — at 19,000 BP. Two of them are now reported, embedded in what is essentially concrete. The report from the Canadian Center for Forensic Research (Ontario), recommended by the FBI, say that the hair are definitely human and explicitly 'Mongoloid'. They noted 5 characteristics, including cross section, taper, etc. which led to the racial classification. The Center works for what we used to call "The Royal Canadian Mounted Police" or the 'mounties'.

root follicle of human hair — Found at 17,000 BP. This will not be given to the cops but to Dr. Savante Paavo who will probably be able to do mtDNA and other DNA analyses on the follicle. If you will remember the removal of hair follicles from Basque youth in the BBC program, you will see what this means. Dr. Paavo is a member of team Wallace (see above) or has been.

hair — Found at 36,000 and 40,000 BC. These may be animal hairs but since they fall within the strata of human habitation (proposed) they will be tested.

merde, Scheiss, govno — Or more familiarly coprolites. About 40 pieces of whatnot scattered about, i.e., found in different levels and areas. These have always been valuable archeologically -- but mostly for telling what people ate. However, Dr. Paavo will also probably be able to read DNAs in them because of bits of blood, tissue, and (dare I say it again) whatnot. If these coprolites fall into chronological sequences, they may tell us a great deal about things like (a) diet at various periods, (b) changes in diet associated with changes in tools or plant debris, (c) possible mutations in the people and/or population differences over time, and (d) diseases and their associations.

What a site! And my congratulations to the indefatigable Scotty MacNeish who has persevered in the face of underwhelming scepticism -- piling up the needed evidence!

SETO DATA

PARALLELS BETWEEN THE MIDDLE EASTERN NAKHO-DAGHESTANI AND THE NORTH ASIAN KET (= Yenisey River) AND KOTT (= Sayan Mountains) LANGUAGES :

1. NAKHO-DAGHESTANI LEZGI "kel̥e" (= skull) = KOTT "karakh" (= forehead).

2. NAKHO-DAGHESTANI LEZGI "ur̥" (= lake) = KET "ur" (= water).

3. NAKHO-DAGHESTANI CHEÇHEN "kog" (= leg) = KET "k'ok" (= calf of the leg).

4. NAKHO-DAGHESTANI DARGIN "tah̤›" (= foot) = KET "tok'" (= step).

5. NAKHO-DAGHESTANI LEZGI "tsur" (= copper) ; NAKHO--DAGHESTANI RUTUL "tsər" (= copper) = KET "tułä" (= copper).

6. NAKHO-DAGHESTANI GUNZIB "bəd", "bodu" (= he) = KET "buda" (= he).

7. NAKHO-DAGHESTANI AVAR "kuy" (= ram) = KOTT "koi" (= sheep).

8. NAKHO-DAGHESTANI GINUKH, NAKHO-DAGHESTANI DIDO "gulu" (= horse) = KOTT "kulún" (= foal).

9. NAKHO-DAGHESTANI BEZHITI "vara" (= cow) = KOTT "bal" (= cattle).

10. NAKHO-DAGHESTANI AKHVAKH "busha" (= cattle) = KOTT "bušôu" (= calf).

11. NAKHO-DAGHESTANI BEZHITI "hōs" (= 1) = KET "khusä" (= 1).

12. NAKHO-DAGHESTANI RUTUL "ti̥d" (= he) = KET "tût", "tûdi" (= he).

13. NAKHO-DAGHESTANI DARGIN "shara" (= lake) + INDIC VICHŌLĪ SINDHĪ (= lower Indus river valley) "jal^{u}" (= water) = KOTT "čâraŋ" (= tributary).

14. NAKHO-DAGHESTANI CHECHEN "kuġ" (= hand) ; NAKHO-
-DAGHESTANI TSAKHUR "kh~akh~" (= hand) = KOTT "kêgär"
(= hand).

15. NAKHO-DAGHESTANI TSAKHUR "ad" (= bone) = KET "at"
(= bone).

16. NAKHO-DAGHESTANI ANDI "chur" (= hair) = KET "kɑ̈ł"
(= beard).

17. NAKHO-DAGHESTANI AGUL "qaq" (= neck) = KET "kak'te"
(= neck, nape).

18. NAKHO-DAGHESTANI LAK "qami" (= womenfolk) = KET
"khâm" (= mother).

19. NAKHO-DAGHESTANI KHVARSHI "qine" (= woman) = KET
"khim", "k'îm" (= woman).

20. NAKHO-DAGHESTANI DIDO "kik" (= spring, source) =
KET "khuk" (= river).

21. NAKHO-DAGHESTANI LEZGI "khaba yiqich" (= palm) =
KET "khobdi" (= palm).

22. NAKHO-DAGHESTANI AVAR "qosh" (= hut) = KET "khus"
(= birch-bark tent).

23. CIRCASSIAN KABARDI "gu" (= heart) = KET "hɑ̈"
(= heart).

24. NAKHO-DAGHESTANI CHECHEN "ge" (= belly) = KET "hî"
(= stomach).

25. NAKHO-DAGHESTANI ANDI "heka" (= man) ; NAKHO-
-DAGHESTANI KHVARSHI "hiko" (= man) = KET "hîg", "hîgi"
(= man).

26. NAKHO-DAGHESTANI TABASSARAN "khul" (= fat) = KET
"huołi" (= fat).

27. NAKHO-DAGHESTANI TABASSARAN "quts-quts" (= tail) =
KET "hɑ̈'ut" (= tail).

28. NAKHO-DAGHESTANI DARGIN "kh~unul" (= woman) = KET "hun" (= daughter).

29. NAKHO-DAGHESTANI ANDI "tsʼeka" (= finger) = KET "tak'", "teak'" (= finger).

30. NAKHO-DAGHESTANI TABASSARAN "tsul" (= navel) = KET "tyl" (= navel).

31. NAKHO-DAGHESTANI CHAMALAL "tsʼā" (= salt) ; NAKHO--DAGHESTANI GUNZIB "tsā" (= salt) = KET "ƛa'a" (= salt).

32. NAKHO-DAGHESTANI CHECHEN "dig" (= hatchet) = KET "tuk" (= hatchet).

33. NAKHO-DAGHESTANI AKHVAKH "sʼeri" (= blood-vessel) = KET "sur" (= blood).

34. NAKHO-DAGHESTANI UDI "tsiq" (= squirrel) = KET "ƶak'" (= squirrel).

35. NAKHO-DAGHESTANI BOTLIKH, NAKHO-DAGHESTANI GODOBERI "basa" (= hair) = KET "bêsam" (= fur).

36. NAKHO-DAGHESTANI RUTUL "vəsh" (= night) = KET "bis" (= evening).

37. NAKHO-DAGHESTANI ANDI "bekʼu" (= hot) + INDIC VICHŌLĪ SINDHĪ (= lower Indus river valley) "bāh^{e}" (= fire) = KET "bok" (= fire).

38. NAKHO-DAGHESTANI KARATA, NAKHO-DAGHESTANI BAGULAL "-bi:se" (= not) ; NAKHO-DAGHESTANI CHAMALAL "-ba:sa" (= not) = KET "bèse" (= not).

39. NAKHO-DAGHESTANI KHVARSHI "-bo" (= not) ; NAKHO--DAGHESTANI AKHVAKH "-ba-" (= not) = KOTT "bô" (= not).

40. GEORGIAN LAZ "mo-n" (= not) = KOTT "mon" (= not).

41. NAKHO-DAGHESTANI TSAKHUR "vur" (= intestine) = KET "fŷɨ" (= intestine).

42. NAKHO-DAGHESTANI TSAKHUR "khav" (= sky) = KET "khip" (= moon).

43. NAKHO-DAGHESTANI KHINALUG "ēq̣" (= sun) = KET "ĕga" (= sun).

44. NAKHO-DAGHESTANI AGUL, NAKHO-DAGHESTANI KRƏZ, NAKHO-DAGHESTANI DARGIN, NAKHO-DAGHESTANI BUDUKH "dạr" (= tree) ; NAKHO-DAGHESTANI DIDO "tsar" (= fir) = KET "tile" (= fir-wood).

45. NAKHO-DAGHESTANI ARCHI "iq" (= day) = KOTT "ikh" (= day).

46. NAKHO-DAGHESTANI DARGIN "ka ghes" (= to kill) = KOTT "k'a" (= death).

47. NAKHO-DAGHESTANI TABASSARAN "eker" (= courtyard) = KOTT "agél" (= village).

48. NAKHO-DAGHESTANI DARGIN "i" (= thou) = KET "ū" (= thou).

49. NAKHO-DAGHESTANI TABASSARAN "c̣hemer" (= arrow) = KOTT "k'em" (= arrow).

50. NAKHO-DAGHESTANI ANDI "chor" (= stallion) = KOTT "tor" (= brown horse).

51. NAKHO-DAGHESTANI ANDI "c̣hor" (= bare-grained barley) ; NAKHO-DAGHESTANI AGUL "sul" (= rye) = KOTT "ŝuli" (= oats).

52. NAKHO-DAGHESTANI AVAR "sono" (= barberry) ; NAKHO-DAGHESTANI TSAKHUR "suna" (= barberry) ; NAKHO-DAGHESTANI AKHVAKH "shani" (= barberry) = KET "sam", "samma" (= berry).

53. NAKHO-DAGHESTANI LAK "q̣ini" (= day) ; NAKHO-DAGHESTANI TSAKHUR "qqina" (= today) = KET "kêne" (= dawn).

54. NAKHO-DAGHESTANI LAK "c̣hila" (= knife) = KET "sal" (= knife-edge).

55. NAKHO-DAGHESTANI TSAKHUR "qqilədzh" (= sword) = KOTT "kaleŝ", "kaliŝ" (= sword).

56. NAKHO-DAGHESTANI DARGIN "c̣haka" (= eagle) = KOTT "take" (= eagle).

57. NAKHO-DAGHESTANI ANDI "seyi" (= bear) ; NAKHO-DAGHESTANI AKHVAKH "shiy" (= bear) = KOTT "ŝayaŋ" (= bear).

58. NAKHO-DAGHESTANI TABASSARAN "fuy" (= doe) = KOTT "hui" (= deer).

59. NAKHO-DAGHESTANI DARGIN "kh~asha" (= badger) = KOTT "hâś" (= badger).

60. NAKHO-DAGHESTANI LAK "orvati" (= frog) ; NAKHO--DAGHESTANI ARCHI "orbiti" (= frog) = KET "öł" (= frog).

61. NAKHO-DAGHESTANI AGUL "kar" (= crow) = KET "kyl" (= raven).

62. NAKHO-DAGHESTANI DARGIN "qar" (= grass) ; NAKHO--DAGHESTANI LAK "khala" (= hay), "khulū" (= grass) = KOTT "keri", "keré" (= grass).

63. NAKHO-DAGHESTANI LAK "tsapi" (= leaf) = KOTT "dipi" (= leaf).

64. NAKHO-DAGHESTANI LEZGI "təvəl" (= root) = KOTT "ṫempul" (= root).

65. NAKHO-DAGHESTANI LEZGI "iki" (= flour) = KOTT "ukhei" (= flour).

66. NAKHO-DAGHESTANI DARGIN "var'a" (= honey) = KOTT "falaŋ" (= sweet).

67. NAKHO-DAGHESTANI KHVARSHI "kula" (= canyon) ; NAKHO--DAGHESTANI LAK "khali" (= rock) ; NAKHO-DAGHESTANI BEZHITI "k'olok'asl'i" (= slope) = KOTT "khêlêkh" (= ridge).

68. NAKHO-DAGHESTANI DARGIN "dah>i" (= snow) = KOTT "ṫîk" (= snow).

69. NAKHO-DAGHESTANI AGUL "tsal" (= wall) ; NAKHO--DAGHESTANI ARCHI "cher" (= wall) = KET "k'oał" (= wall).

70. NAKHO-DAGHESTANI TABASSARAN "chal" (= basket) = KOTT "teōl" (= basket).

71. NAKHO-DAGHESTANI LAK "q'ata" (= trap) = KOTT "khatn" (= trap).

72. NAKHO-DAGHESTANI DIDO "imu" (= needle) = KOTT "în"

73. NAKHO-DAGHESTANI TABASSARAN "tsur" (= hook) = KET "suoł" (= hook).

74. NAKHO-DAGHESTANI TSAKHUR "tsəra" (= rope) ; NAKHO-DAGHESTANI AVAR "cholo" (= rope) = KOTT "kûra" (= cord).

75. NAKHO-DAGHESTANI GUNZIB "chal" (= barrel) = KOTT "ŝu'ol" (= barrel).

76. NAKHO-DAGHESTANI TSAKHUR "kuk" (= spoon) = KET "k'ŷkti" (= spoon).

77. NAKHO-DAGHESTANI AVAR "kun" (= thread) = KET "kan" (= thread).

78. NAKHO-DAGHESTANI DARGIN "tsarka" (= blanket) = KOTT "đôrgan" (= blanket).

79. NAKHO-DAGHESTANI LAK "chon" (= bead) = KET "kun" (= bead).

80. NAKHO-DAGHESTANI TABASSARAN "urgam" (= sheepskin coat) = KOTT "olĉa" (= clothes).

81. NAKHO-DAGHESTANI DIDO "h˃och" (= boot) ; NAKHO-DAGHESTANI GINUKH "khots" (= boot) ; NAKHO-DAGHESTANI GUNZIB "hako" (= shoe) ; NAKHO-DAGHESTANI BEZHITI "haka" (= shoe) = KOTT "hêĉi" (= shoe).

82. NAKHO-DAGHESTANI TABASSARAN "tublan" (= ring) ; NAKHO-DAGHESTANI RUTUL "tibel" (= ring) ; NAKHO-DAGHESTANI AGUL "tavul" (= ring) ; NAKHO-DAGHESTANI LEZGI "tupal" (= ring) = KOTT "ṫâpura" (= ring).

83. NAKHO-DAGHESTANI LEZGI "behle" (= glove) ; NAKHO-DAGHESTANI TABASSARAN "behli" (= glove) = KET "bôk'" (= glove).

84. NAKHO-DAGHESTANI ANDI "kˤgom" (= trough) = KOTT "ham" (= vessel).

85. NAKHO-DAGHESTANI ARCHI "qˤutol" (= belt) = KET "kuot" (= belt).

86. NAKHO-DAGHESTANI LAK "k̰urt̰u" (= quilted jacket) ; NAKHO-DAGHESTANI DARGIN "k̰urt̰i" (= clothes) ; NAKHO-DAGHESTANI TABASSARAN, NAKHO-DAGHESTANI TSAKHUR "k̰urt" (= clothes) ; NAKHO-DAGHESTANI LEZGI "kuṛt" (= fur coat) ; NAKHO-DAGHESTANI DIDO "ḳurṭay" (= quilted jacket) = KET "hâltam" (= fur coat).

87. NAKHO-DAGHESTANI BEZHITI "ḳˤay" (= fur coat) = KOTT "hêi" (= fur ; upper garment).

88. NAKHO-DAGHESTANI AGUL "q̣areq̣" (= hide) ; NAKHO-DAGHESTANI TABASSARAN "q̣iriq̣" (= hide) ; NAKHO-DAGHESTANI DARGIN "k̰ule" (= hide) ; NAKHO-DAGHESTANI GUNZIB "qalu" (= goatskin) = KET "hèlät" (= hairless pelt).

89. NAKHO-DAGHESTANI KRƏZ "h̃ạc̣h" (= star) = KET "k'oaġ" (= star).

90. NAKHO-DAGHESTANI KRƏZ "q̣ud" (= winter) ; NAKHO-DAGHESTANI LEZGI "q̈ud" (= winter) ; NAKHO-DAGHESTANI RUTUL "q̈id" (= winter) ; NAKHO-DAGHESTANI TSAKHUR "q̈ədəm" (= winter) ; NAKHO-DAGHESTANI ARCHI "q̈o't̰ịq" (= winter) = KET "kə̂ti" (= winter).

91. NAKHO-DAGHESTANI ANDI "vots̰i" (= brother) ; NAKHO-DAGHESTANI BOTLIKH, NAKHO-DAGHESTANI GODOBERI, NAKHO-DAGHESTANI KARATA, NAKHO-DAGHESTANI AKHVAKH "vats̰i" (= brother) = KET "biseäp" (= brother).

92. NAKHO-DAGHESTANI KHVARSHI "obu" (= father) ; NAKHO-DAGHESTANI GUNZIB "abu" (= father) = KOTT "^ôp" (= father).

93. NAKHO-DAGHESTANI DARGIN "durh˃a" (= child) = KET "dyl" (= child) ; KOTT "ɗal" (= child).

94. NAKHO-DAGHESTANI KHINALUG "gada" (= child) = KOTT "kat" (= children).

95. NAKHO-DAGHESTANI ARCHI "ḳan" (= bottom) ; NAKHO-DAGHESTANI AGUL "ḳen" (= bottom) = KOTT "hânal" (= down).

96. NAKHO-DAGHESTANI LAK "tsu" (= fire) ; NAKHO--DAGHESTANI BEZHITI "tso" (= fire) = KOTT "tu" (= smoke).

97. NAKHO-DAGHESTANI DARGIN "u" (= name) = KET "ī" (= name).

98. NAKHO-DAGHESTANI LAK "tsan" (= darkness) ; NAKHO--DAGHESTANI TSAKHUR "kham" (= night) = KET "k'oʀ" (= dark).

99. NAKHO-DAGHESTANI GUNZIB "ĭcho" (= near) ; NAKHO--DAGHESTANI UDI "əsha" (= near) = KET "ɑc" (= near).

100. NAKHO-DAGHESTANI GODOBERI "bechukha" (= big) = KOTT "faĉâ" (= big).

101. NAKHO-DAGHESTANI GODOBERI "hitsi" (= high) ; NAKHO--DAGHESTANI AKHVAKH "hechedabe" (= high) = KET "hiĉal" (= high).

102. NAKHO-DAGHESTANI TABASSARAN "kar" (= deaf) = KOTT "kalkul" (= deaf).

103. NAKHO-DAGHESTANI KHINALUG "fara" (= warm) = KOTT "fal" (= warm).

104. NAKHO-DAGHESTANI ANDI "unsa" (= warm) = KET "ɑs" (= warm).

105. NAKHO-DAGHESTANI AGUL "taqanf" (= dirty) = KOTT "ṫagar" (= dirt).

106. NAKHO-DAGHESTANI ARCHI "chuh~" (= thick) = KET "suk'ŋ" (= thick /: person /).

107. NAKHO-DAGHESTANI GUNZIB "bosheru" (= fat /: adjective /) ; NAKHO-DAGHESTANI DARGIN "budzil" (= stout) = KET "bèsel" (= fat /: adjective /) ; KOTT "puĉar" (= fat /: adjective /).

108. NAKHO-DAGHESTANI LEZGI "kuru" (= short) = KET "hôli" (= short).

109. NAKHO-DAGHESTANI TSAKHUR "kinɣa" (= small) = KET "hènä" (= small).

110. NAKHO-DAGHESTANI LEZGI, NAKHO-DAGHESTANI TABASSARAN "tsiyi" (= new) = KET "ki'ä" (= new)

111. NAKHO-DAGHESTANI ARCHI "qatub", "qatut" (= flat) = KET "k'âdem" (= flat).

112. NAKHO-DAGHESTANI ANDI "tulu" (= bad) = KET "sel" (= bad).

113. NAKHO-DAGHESTANI ANDI "shur" (= cold) = KOTT "čal" (= cold), "šurgan" (= cold weather).

114. NAKHO-DAGHESTANI LEZGI "ichi" (= empty) ; NAKHO-DAGHESTANI BEZHITI "ocho" (= empty) ; NAKHO-DAGHESTANI ARCHI "acha" (= naked) = KOTT "êkh" (= empty).

115. NAKHO-DAGHESTANI RUTUL "qadə" (= old) = KET "kat" (= old).

116. NAKHO-DAGHESTANI LEZGI "qurey" (= dry) = KET "kôłeŋ" (= dry).

117. NAKHO-DAGHESTANI CHAMALAL "hōb" (= good) = KET "kôubat" (= good).

118. NAKHO-DAGHESTANI RUTUL "hikhdə" (= good) = KOTT "hagši" (= good).

119. NAKHO-DAGHESTANI DARGIN "akhsi" (= good) = KET "âk'ta" (= good).

120. NAKHO-DAGHESTANI GINUKH "kholu" (= broad) = KET "k'yl" (= broad).

121. NAKHO-DAGHESTANI TSAKHUR "kh~o" (= 5) ; NAKHO-DAGHESTANI UDI "qo" (= 5) ; NAKHO-DAGHESTANI LAK "kho" (= 5) = KET "khâ" (= 5).

122. NAKHO-DAGHESTANI LAK "qu" (= 20) ; NAKHO-DAGHESTANI DIDO "qu" (= 20) = KET "khû" (= 10).

123. NAKHO-DAGHESTANI ANDI, NAKHO-DAGHESTANI GODOBERI, NAKHO-DAGHESTANI AKHVAKH, NAKHO-DAGHESTANI TINDI "habu" (= coal) ; NAKHO-DAGHESTANI BAGULAL "hab" (= coal) ; NAKHO-DAGHESTANI BOTLIKH "haba" (= coal) = KOTT "hupôi" (= coal).

124. NAKHO-DAGHESTANI GUNZIB "baki" (= enclosure) = KET "baŋel" (= house).

125. NAKHO-DAGHESTANI GINUKH "ala" (= village) = KET "čäłc" (= street).

126. NAKHO-DAGHESTANI LEZGI "khapa" (= gruel) = KOTT "hâpan" (= porridge).

127. NAKHO-DAGHESTANI AVAR "sher" (= fox) ; NAKHO-DAGHESTANI AKHVAKH "shari" (= fox) ; NAKHO-DAGHESTANI UDI "shul" (= fox) ; NAKHO-DAGHESTANI BOTLIKH, NAKHO-DAGHESTANI TINDI, NAKHO-DAGHESTANI GODOBERI "sari" (= fox) ; NAKHO-DAGHESTANI KARATA "sare" (= fox) = KOTT "ṡeli", "ṡele" (= wild animal).

128. NAKHO-DAGHESTANI LAK "tunu" (= stall) ; NAKHO-DAGHESTANI TABASSARAN "tun" (= stall) = KET "tônos" (= earthen hut).

129. NAKHO-DAGHESTANI KRƏZ "chaqq" (= pus) ; NAKHO-DAGHESTANI DARGIN "shakha" (= pus) = KOTT "takŋ" (= pus).

130. NAKHO-DAGHESTANI GINUKH "shita" (= stocking) ; NAKHO-DAGHESTANI AVAR, NAKHO-DAGHESTANI GINUKH "shata" (= stocking) = KOTT "ṡèt" (= long stocking).

131. NAKHO-DAGHESTANI TSAKHUR, NAKHO-DAGHESTANI RUTUL "chalag" (= forest) ; NAKHO-DAGHESTANI UDI "chalag" (= forest) = KOTT "ṫalak" (= tree rot).

132. NAKHO-DAGHESTANI LEZGI "tsura" (= stall) ; NAKHO-DAGHESTANI TABASSARAN "duraϑ" (= stall) = KOTT "tura" (= room).

133. NAKHO-DAGHESTANI DIDO "hibo" (= stick) = KET "khûr" (= pole).

134. NAKHO-DAGHESTANI AVAR, NAKHO-DAGHESTANI ANDI "talgan" (= turnip) + GEORGIAN "talgami" (= turnip) = KOTT "ċâlgana" (= turnip).

135. NAKHO-DAGHESTANI KRƏZ "qqantarγa" (= bridle) ; NAKHO-DAGHESTANI TSAKHUR "qqantaraγa" (= bridle) = KOTT "kantêkh", "kantêg" (= halter).

136. NAKHO-DAGHESTANI CHECHEN "chish" (= urine) ; NAKHO-DAGHESTANI LEZGI, NAKHO-DAGHESTANI TABASSARAN "chukh"" (= urine) = KOTT "čaš" (= urine).

137. NAKHO-DAGHESTANI ANDI "chum" (= cloud) = KET "tumäs" (= storm cloud).

138. NAKHO-DAGHESTANI DIDO "gup" (= hill) = KET "k'up" (= peak).

139. NAKHO-DAGHESTANI TABASSARAN "khul" (= fat, suet) = KOTT "kîr" (= fat /: noun /).

140. NAKHO-DAGHESTANI TSAKHUR "kech e" (= thick felt) = KOTT "hatál" (= felt).

141. NAKHO-DAGHESTANI AVAR "kilish" (= ring) = KOTT "kolečka" (= finger ring).

142. NAKHO-DAGHESTANI LAK "qa" (= wing) = KOTT "kei" (= wing).

143. NAKHO-DAGHESTANI LAK "t alak" (= marten) ; NAKHO-DAGHESTANI AGUL "turk" (= marten) = KET "surak" (= marten).

144. NAKHO-DAGHESTANI ARCHI "ukh" (= field) = KET "ɑ" (= field).

145. NAKHO-DAGHESTANI GINUKH "ishe" (= snow) ; NAKHO-DAGHESTANI KHVARSHI "eso" (= snow) ; NAKHO-DAGHESTANI BAGULAL "ãzu" (= snow) = KOTT "ušo'u" (= ice).

146. NAKHO-DAGHESTANI GUNZIB "ale" (= belt) = KOTT "îre" (= string).

EXAMPLES FOR PARALLELS BETWEEN THE MIDDLE EASTERN NAKHO-DAGHESTANI LANGUAGES AND THE NORTH ASIAN OSTYAK AND SAMOYEDIC (= Ob-Irtysh river) LANGUAGES :

a. NAKHO-DAGHESTANI RUTUL "vad" (= thou) = SAMOYEDIC "paδᵊ" (= thou).

b. NA<u>KH</u>O-DAGHESTANI <u>KH</u>VAR<u>SH</u>I "ge<u>sh</u>a" (= hand) ; NA<u>KH</u>O--DAGHESTANI <u>TS</u>A<u>KH</u>UR "g̣uch" (= hand) = OSTYAK "kǒš" (= claw, clutch).

c. NA<u>KH</u>O-DAGHESTANI DARGIN "kani", "kuni" (= belly) = OSTYAK "k'ǒn'" (= belly).

d. NA<u>KH</u>O-DAGHESTANI AR<u>CH</u>I "s̱on" (= back) = OSTYAK "ṡonχ" (= back /: fish /).

e. NA<u>KH</u>O-DAGHESTANI GUNZIB "bil" (= lip) ; NA<u>KH</u>O--DAGHESTANI <u>CH</u>E<u>CH</u>EN "balda" (= lip) ; NA<u>KH</u>O-DAGHESTANI BE<u>ZH</u>ITI "poro" (= lip) = OSTYAK "pəlәm'" (= lip).

f. NA<u>KH</u>O-DAGHESTANI A<u>KH</u>VA<u>KH</u> "seme" (= lip) = OSTYAK "ṭv·ma`ṇ'" (= mouth /: bear /).

g. NA<u>KH</u>O-DAGHESTANI TINDI "miar" (= nose) ; NA<u>KH</u>O--DAGHESTANI KRƏZ "miel" (= nose) = SAMOYEDIC "m'ĭerşʌ̀ièş" (= to smell bad).

h. NA<u>KH</u>O-DAGHESTANI TABASSARAN "zuḳ" (= down) = OSTYAK "sǎχpǎn'" (= hair /: bear /).

i. NA<u>KH</u>O-DAGHESTANI ANDI "baḳol" (= stomach) = OSTYAK "pòχ°lị" (= paunch, belly).

j. NA<u>KH</u>O-DAGHESTANI <u>KH</u>INALUG "gada" (= child) = SAMOYEDIC "ŋaṭ'ṭşekkị" (= child).

k. NA<u>KH</u>O-DAGHESTANI ANDI "hir" (= copper) = SAMOYEDIC "xịʌ́n'i" (= gold).

l. NA<u>KH</u>O-DAGHESTANI ANDI "bis̱il" (= you) = SAMOYEDIC "piṭ'ş" (= you 2).

m. NA<u>KH</u>O-DAGHESTANI <u>CH</u>AMALAL "biti" (= you) = SAMOYEDIC "piṭṭvə̂" (= you).

n. GEORGIAN "me" (= I) = OSTYAK "mä`" (= I).

o. NA<u>KH</u>O-DAGHESTANI <u>KH</u>INALUG "da" (= he) ; NA<u>KH</u>O--DAGHESTANI LAK "ta" (= he) ; NA<u>KH</u>O-DAGHESTANI AR<u>CH</u>I "to" (= he) = OSTYAK "tǒu" (= he).

q. NAKHO-DAGHESTANI KHVARSHI "yu" (= he) = OSTYAK "iox" (= he).

r. NAKHO-DAGHESTANI AVAR "l'o" (= he) ; NAKHO-DAGHESTANI AGUL "le" (= he) = OSTYAK "lŏu" (= he).

s. NAKHO-DAGHESTANI GUNZIB "bəd", "bodu" (= he) = SAMOYEDIC "puδè" (= he).

t. NAKHO-DAGHESTANI GUNZIB "bedra" (= they) = SAMOYEDIC "paδàrà" (= you).

u. NAKHO-DAGHESTANI BEZHITI "qot" (= palm) = OSTYAK "k'ó`t'" (= hand).

v. NAKHO-DAGHESTANI TABASSARAN "kul" (= foot) = OSTYAK "kur" (= foot).

w. NAKHO-DAGHESTANI LEZGI "ner" (= nose) = OSTYAK "ńʌl" (= nose).

x. NAKHO-DAGHESTANI CHAMALAL "hacha" (= eye) = OSTYAK "k'o`s" (= eye /: bear /).

y. NAKHO-DAGHESTANI KHINALUG "tal" (= lip) = OSTYAK "tʌrôβ" (= lip).

z. NAKHO-DAGHESTANI AVAR "bekˤer" (= head) = OSTYAK "pătā·lə" (= head).

For Review in WORD. -69-

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 3 copies of your review, double-spaced with at least 2 cm margin on all sides. If your review will be less than one journal page or more than four journal pages, please check with the Review Editor before submitting your review.
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: June 5, 1992

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August 4-8, 1992. LACUS (Linguistic Association of Canada and the US), Université du Québec à Montréal. Write to Professor Ruth Brend, 3363 Burbank Drive, Ann Arbor, MI 48105, USA.
August 9-14, 1992. International Congress of Linguists, Université Laval, Québec, CANADA. Write to CIL 92, Département de langues et linguistique, Université Laval, Québec (Qué.), CANADA G1K 7P4. Telephone (418) 656-2625. FAX (418) 656-2019. Bitnet CIPL92@LAVALVM1.
October 16-18, 1992. NWAVE, University of Michigan, Ann Arbor, MI, USA. Abstract deadline June 15. Thomas E. Toon, UM Program in Linguistics, 1076 Frieze Building, Ann Arbor, MI 48109-1285.
November 6-7, 1992. Atlantic Provinces Linguistic Association. Theme: Sociolinguistic Studies and Language Planning. Includes a workshop with William Labov, "Assessment of Sociolinguistic Methods within the last 10 years". Abstract deadline: September 1, 1992. Catherine Philipponeau, Centre de recherche en linguistique appliquée, Université de Moncton, Moncton, New Brunswick E1A 3E9, CANADA.
January 7-10, 1993. Linguistic Society of America, Biltmore Hotel, Los Angeles, CA, USA.

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Possible job: Possibility of a tenure-track position in French linguistics, junior assistant professor with recent PhD or ABD (PhD by July 1993). Doctorate in French linguistics, as well as a record of publication in the field of specialization. Must be able to teach French language and linguistics at the BA level and in the proposed MA programme, which will focus on French-Canadian linguistics. Preference given to Canadian citizens or permanent residents, but others should apply too. For information, please contact Professor Moshé Starets, French Department, University of Windsor, Windsor, Ontario, CANADA N9B 3P4. (519) 253-4232 ext 2062.

EDITORIAL

"It is a terrible thing to lose your mind!" Such a malapropism was uttered this year by an American politician. He was trying to paraphrase a slogan --"A mind is a terrible thing to waste!" -- used in an advertisement soliciting money for an African-American college fund. Yet we ourselves can alter it again to: "It is terrible to have to change your mind and disturbing to search new horizons!"

Long rangers may become vexed because of the uncertainty involved in self-levitation or moving from the habitual thought ways of one cozy discipline (or specialty) to, or through, another discipline, until such time as they find themselves on a higher cognitive plane (level) with a more comprehensive viewpoint. As a dean once said to me: "Come see this university from my level, rather than from your tiny department's myopic view!" Seeing things from the standpoint of the 'emerging synthesis' (credits to Colin Renfrew) may be taxing to any linguist or biogeneticist or archeologist or paleoanthropologist. We have spoken of these things in the past but the problems still persist. It is in the nature of things. How shall a cooperative archeologist, for example, know whether the schemes of his friendly local historical linguist are good or not? Or fruitful or too limited?

One example. There was a recent serogenetic study of tribes in Amazonia which 'found' few correlations with linguistic groups. Fortunately they did not leap to the conclusion that Cavalli-Sforza was totally wrong in associating language taxa with biogenetic taxa! Their conclusions were essentially vitiated by their use of the hyper-conservative Loukotka classification which finds hundreds of phyla in South America. Remember the famous computer adage : 'Garbage in, garbage out.' Or we can paraphrase that to: 'Having flaws in primary presuppositions leads to flaws in final conclusions'.

There is also the danger, everpresent and immediate, of one discipline imposing its viewpoints on the thinking of the higher levels. The most obvious of course would be that of our majority population -- descriptive and historical linguistics. We have perhaps talked about that one too much already. Let us look critically at some of the tendencies in our cooperating disciplines. Immediately we see another strong one -- the famous 'stones and bones' viewpoint, shared by both archeologists and paleoanthropologists. It demands clear tangible fossil evidence -- "hard cold facts" we used to say -- and has trouble relating to inferential evidence from other disciplines.

An example. Once I had a Boston colleague who was unable to believe that evidence could be 'real', unless it was found in a properly excavated stratified site.

Hypotheses generated by biogeneticists or historical linguists may often seem frivilous to this viewpoint. A few of its practitioners hover around the edges of a know-nothing variety of hyper-empiricism which, oddly enough, they proudly view as 'really scientific'. They need to look more carefully at astronomy, even indeed at modern geology, to see how far their assumptions are from those of a mature diachronic science.

Many of these same practitioners are very 'high tech' as one says nowadays of those enamoured of technical aids to inference (or 'technico' in Brazil). I listened to bunches of them recently at a conference. They would cart their data off to several physics labs and be terribly precise about (e.g.) how many calories the average Yahoo ate in a year but they seemed to lack any idea of what their site might mean in comparative terms. The high tech dig seems to be the end in itself! Perhaps we should stop calling such people scientists; laboratory technicians are what they are! (This is not to scoff at those good people who lack the pretensions of being doctors and professors.)

It is hard to say which group of denizens of the modern industrialized world are the most in love with technology. One could nominate the Americans were the Japanese and Germans not even more so inclined. And why not? This is the century of radar, the jet fighter, the helicopter, the atom bomb, the computer, mtDNA analyses, men on the moon, triple by-pass surgery, and the movie "The Empire Strikes Back". Twentieth century technology is fantastic, magical, and awe-inspiring. If we could wake up Christopher Columbus, this must be what he would say. Surely no one would be surprised to find that the glorious technology of our century has influenced our sciences, especially rather small and insecure social sciences. But perhaps the bath water has become so contaminated that it now threatens to kill the baby? (Babies in bath water: everyone's favorite metaphor).

At least among the Americans in this metaphor it seems that many social scientists have forgotten their raison d'etre. They use buckets & loads & scads of technology but do not know why or what for. Some become so proficient at the game that they should rightfully be called engineers, instead of scientists. After all modern science is at least 400 years old and we know a lot about it; willy nilly data gathering is not basic to it. Hasn't international science been primarily concerned with <u>proposing</u> hypotheses (theories, models) which make sense of some portion of reality and then <u>questioning</u> / testing / proving those hypotheses and then building upon the newly acquired knowledge or understanding of "nature"?

An example of willy nilly data gathering. Many years ago the American government gave about $100,000 to a sociologist in New York State. That person did his research and reported the results in the leading newspapers. What results? He found that the best way to predict who would wear pajamas to bed at night was to see who ate soft boiled eggs for breakfast in the morning. The two were correlated. Wasn't that marvelous?

<u>Speaking of science, let us look at historical linguistics again.</u>

Thanks to Ron Christensen, physicist of Lincoln (Mass.), we can contrast the attitudes which physicists have towards hypotheses with that weird combination of hyper-empiricism, extreme operationism, and formal logic which appears among linguists as the criterion for 'really proving' linguistic classifications. Although the whole panoply of modern technology owes much of its existence to physics, and its growth in itself has surely affected the 'instrumental' aspects of physical theory, still physicists tend to see some things differently from linguists. For example, final proofs of hypotheses are elusive or 'just down the road a little farther' to physicists. Contrast that attitude with a traditional 'responsible' or 'careful' linguistic one, as for example the one seen recently on BBC discussing Ruhlen's hypothesis of a common human language ancestor. The linguist, Professor Ringe of the University of Pennsylvania, declared that he had no doubt that human languages had a common origin and that, incidentally, most linguists would be astonished to hear otherwise; but, he continued, that was a very different matter from 'proving' that all languages were related. Such proofs were not possible and it was a waste of time to try to obtain them. One might summarize, therefore, that most linguists believe that all languages are related but it is impossible to know that they are related. This was the attitude of 'scientific' linguistics, he continued with a slight upward tilt to this head, not idle speculation. Certo, a young fogy!

Operationism or operationalism in physics begat excessive methodology worship in psychology, linguistics and sociology.

Parts of anthropology and archeology were eventually affected too. A mystical view of mathematics as divine, as the ultimate language of science and the source of elegance in analysis was also borrowed from physics. Much of this cultural diffusion took place at Yale University, at the Institute of Human Relations therein, and within the rounded dome of one Leonard Bloomfield in the 1930s. Watson the behaviorist (psychologist) was similarly afflicted and begat more children at the same place. Other copycats existed at other universities too, of course. Clearly the Japanese have not been unique in their ability to imitate and prosper.

Yet, given the strangle hold methodology worship has on the many domes of Bloomfield's children, I am reminded of a tale told by Ron of Lincoln. While Carl Friedrich Gauss was establishing his theory of planetary movements in the 19th century, he made some mistakes in his calculations. Despite the mathematical errors, his basic hypothesis was so strong that it prevailed. Later, when his errors were discovered, testing had already shown his theory to be true -- anyway. Another tale told by another Leonard -- Doob of psychology -- concerns the rather common occurence in experimental psychology wherein the severe demands of methodology block many interesting hypotheses from being tested, while guaranteeing that uninteresting but ever so precise hypotheses can be tested which, however, shed no light and lack fruitfulness. Thus I give you an outrageous adage:

Methodology worship generates cognitive and conceptual poverty.

The question to Ringe and his colleagues then has to be the simple one: in the first place why do you believe that all languages are related? Then: why do you say that it is impossible to relate them? Is this a game that clever linguists can play -- but only clever linguists -- because only they know the rules? Or is it possible that Ringe and company are not as wacky as they seem because they harbor within themselves other ways of knowing things, different from their responsible and awesome methodology? Despite their handicaps (methods), they can still reason properly or they have not entirely lost their wits during indoctrination to linguistic methods. For surely some professor or program has taught them to throw aside their reason (raison, Vernunft, ragione) so s/he could brainwash them. Nicht wahr?

The shrewd reader will have noticed that we have a wobbly semantic pair in, 'related' and 'to relate'. Will Ringe seem to be less wacky if the wobbly pair turn out to have different meanings? And they do in ordinary scientific English. 'Related' carries the clear implication of real historical connection, probably genetic. "To relate' means to demonstrate, show or prove such a real historical connection. Ah, hah! Let us now restate the question more precisely. Why do Ringe and his colleagues believe that no one can demonstrate, show or prove real historical genetic connections among all the languages which they believe to have real historical genetic connections? Hmmm?

The answer is simple again. They are forbidden to try to show, demonstrate or prove that all human languages are related. Hence their belief is an unscientific belief -- being untestable and all -- which smacks more of ideology than anything else. Who forbids them? Their own belief plus their teachers. No graduate student or young professor is going to waste her valuable time trying to show, demonstrate or prove something that "everyone knows cannot be shown". You do not get browny points for being stupid or insensitive to the beliefs of others! You do not get ahead by contradicting your teachers. (Is there a country where this is not true?) So the original belief has become a social fact (in Durkheim's terms). Can nothing be done about this?

Professor Joseph Agassi (U/Toronto, philosopher, late of Boston) holds that testing hypotheses need not be restricted to science. Any metaphysical, religious, political, ideological or whatever belief can and should be tested or at least subjected to critical examination. Why should we believe things by faith or authority? He might say that the belief in unrelatability of human languages can be examined scientifically, not so delicately as we would examine the beliefs of Pope Paul. After all the question of language relatability is an empirical question, not a matter of taste, not something 'proven' by the authority of one's PhD committee, not something to be pre-judged by methodologists. Or else we for damn sure must stop calling linguistics a science!

The belief in the unrelatability of human languages rests on an extraordinarily fragile foundation. An array of empirically unsubstantiated assumptions, including some arbitrary dicta of the school called 'Neo-Grammarian', some casual ideas about time depths tossed off by thinkers like Charles Hockett, and some frivolous mathematical 'proofs' based entirely on the lexicostatistics of Swadesh which was supposed to be unreliable anyway. Most of this stuff does not have the status of solid theory established empirically and analytically by linguistic science. Really silly notions like "language keeps changing all the time so pretty soon you cannot tell what is related" are not empirical statements; they are hardly better than truisms and false ones at that. But most important, for those readers who disagree with me, is this question: where is the scientific support for these flimsy arguments for unrelatability? why not take one of them and examine it seriously as important assumptions in anyone's science ought to be? why do YOU believe this kind of thing anyway? Just because the rest of them believe the earth is flat, why do you have to believe it too?

Now the strange case of archeology's loathing for migrations.

Oddly enough, archeology which otherwise differs so much from linguistics has also generated some foolishness of its own, to wit, the attack on migration or the vigorous denial that migrations are important or worth studying. But in archeology one can see how thinkers have arrived at the apex of the folly, how the thought process was not so unreasonable as it might seem, and how archeology has begun to retreat from its own moment of wackiness, led or coaxed by gentle critics like David Anthony and Ben Rouse. The apex of folly came in the early 1980s when some archeologists began denying that migrations had ever occurred, when some said that "process" could account for just about everything, and when some tried to expunge migration from the list of causes of prehistoric events.

One example. Once I received an invitation to attend a meeting of prehistorians in Calgary (Canada) where one session was going to be devoted to getting rid of theories of migration and maybe the concept itself. Their flyer (brochure) reeked of hopeful young science, sure of itself and eager to get the dead wood cleared away. Zut, alor! You've gone mad, said I in reply. Would you like a list of all the migrations which can be documented in known history, never mind prehistory? Is there a better way to explain how millions of Europeans and Africans came to be located in the New World? Maybe your stress on 'process' will strain itself in coping with the Polynesians and Bantu!

More subtle and socially acceptable forms of anti-migration thinking generally pervade contemporary archeology, not so much in the form of a crude Calgary type statement as in the clear preference for being analytical and explaining things in terms of 'process' instead of migration. In classical anthropological terms this means a reversion to the concept of invention as opposed to diffusion. By hook or by crook we will find a way to show how local folks and 'determinants' (especially economic) did it all. (That is why we need to count how many calories the average Yahoo ate per year.) As Ben Rouse mentioned recently, most contemporary archeologists would rather not do culture history. I would be less polite and suggest that they get no reward from their colleagues and risk being considered old-fashioned, low tech, and unscientific. They have forgotten that culture history is one of their fortes. If archeology -- the very "study of the past" itself -- is ashamed to study the histories of cultures (Kulturgeschichte), then who will study it? And how, pray tell, can a diachronic science justify studying processes of change but not the changes themselves -- the narrative? Later in another issue we will examine how the New Archeology which relied so much on the philosophy of Carl Hempel grossly misread him.

These enthusiasms (fads) sweep the social sciences from time to time. A pig-headed insistence on doing things just one way and also, by the way, getting rid of dead wood consisting of older colleagues who have different views. Why for the love of sweet and everlasting Boas do we have to think of things in just one fashion? The reason old ethnological theory had concepts of invention and diffusion was that neither could explain everything and either could explain some things very well.

Two examples. There are probably no prehistorians anywhere who would attempt to explain the agricultural revolutions in the post-Holocene world in terms of migration. If ever there was a series of events which cried out for invention, adaptation, process, etc., the Neolithics in the Near East and Mexico at least were such events. There followed later expansions and migrations and re-adaptations to new habitats and so forth -- all events involving the peoples of the first process and their neighbors. First invention, second diffusion. A classic case.

Contrarily, even Lewis Binford, the priest of process, would be hard put to deny the probability of multiple moving bodies -- one or more migrations -- were he to confront the distributions of the English-speaking or Portuguese-speaking peoples of the modern world or of Arab Moslems of the last millennium. If our colleague, Alvah Hicks, is correct, then someone will have to explain how the South Americans took over the world. If Alvah is wrong, then someone will have to explain two continents full of native Americans where there is no process that could produce them out of South American monkeys. Since both Africa and Asia can produce or did produce modern humans out of earlier materials (primates), then how they got to, and all the way through, the Americas really requires some migration theories. N'est-ce pas?

We (the editorial we) have stepped on all possible toes. Many of these things needed to be said. Some have been repeated ad nauseam. Since you-plural, dear readers, are a mighty passive lot, we hope that enough of you have been offended to write some replies. Good strong ones. Just tell me if I may publish your letter, please.

ALSIP BUSINESS

This is a combined issue; the Summer and Fall together. We are sorry it is late but that really could not be helped. The editor was laid up with a bad back for a month. Then he had a long bout of examinations, probes, and so forth which led up to another diagnosis. But the worst of it was the medicine which essentially crippled him for another month. The upshot of it was that there will be heart surgery in early November to replace a congenital aortic valve flaw plus a double by-pass for the coronary artery.

Since the recovery period has to be allowed for, MT-17 had to come out as a double issue or there would have been only two for 1992. The name MT-18 is still reserved for the late December issue. . . . . Other ASLIP BUSINESS follows:

(a) Dues to increase next year. We held the line as long as we could but we had to increase the dues. The Board of Directors in April decided that it was not fair to ask the officers to make up the difference so much -- between income and outlay. We have been producing more than the agreed on 120 pages a year, although the frequency has dropped to three issues a year. We still have a large number of colleagues who cannot contribute because of currency problems. What a Muscovite would have to give up to pay his dues is a lot! 1000-1500 rubles or the better part of a month's salary. If the average American professor gave the equivalent, he'd turn over $2779 to us! So, beginning in January of 1993, ANNUAL DUES INCREASE TO $15 AMERICAN. For west Europeans that will be a mere bagatelle. By January one dollar will equal one deutsche mark, if it is lucky. For Europeans your actual dues might be figured in DM, depending on what Ekkehard Wolff's expenses are. For Israelis your dues are now negotiated with Orël who does all the distribution for Israel now.

(b) The Board of Directors of ASLIP has voted to protest to the Secretariat of the Linguistic Society of America on behalf of two long rangers, John Bengtson and Merritt Ruhlen, whose hypotheses the journal LANGUAGE refuses to publish. A letter was drafted, approved by the Board, and dispatched to the LSA two months ago. The suspicion of editorial bias is mentioned, but otherwise the letter was polite and formal. A Reply has been received: they have a committee looking into the matter. Should we be hopeful? Are you kidding?

In fact there is hearsay that the LSA decided the matter already but never bothered to tell us. Since they had promised to inform us, we will probably take offense and think about sterner measures. In October the Board will convene again in Boston for a war council. At that time they may decide that the matter is nothing to fight over. Or the opposite. In any case the Directors will read all the massive dossiers and make a collective decision. You can bet it will not be frivolous or predictable!

Apropos of these unpleasant matters we are publishing a three way exchange of letters among John Bengtson, Lionel Bender, and Sarah Thomason. Each has requested that their letters be published. They follow directly -- overleaf.

Bender -79-

April 27, 1992
401 Emerald Lane
Carbondale, Il. 62901

Harold Fleming, Ed.
Mother Tongue
5240 Forbes Avenue
Pittsburgh, Pa. 15217

Dear Hal,

I must respond to Bengtson's Language rejects "Global Etymologies" (un-numbered pp. 2-7, in MT 16, April 1992).

I agree that it is time for *Language* to give some space to the "long-rangers'" point of view. If it is any consolation, *Language* also rejected my article critical of "global etymologies". (As did *Anthropological Linguistics* after "losing" it for more than a year). The "establishment journals'" argument that the topic is still in the category of pseudo-science and that the scholarly world is not ready to take it seriously is unconvincing to say the least. They (the LSA, *Language)* are certainly doing nothing to refute the charges of operating like a closed corporation with a strong "old boys (and girls)" network.

But the errors in judgment and "proofs by assertion" are not entirely on the other side. Bengtson dismisses my test of "global etymologies" by saying that I have found "isolated words that happened to resemble the phonetic shapes of our etymologies". Of course: that is the point! This criticism ranks with an earlier one that my article is a "cheap shot". Recall that I am taking up the challenge which Bengtson and Ruhlen themselves threw out in their "Global Etymologies" paper (which was scheduled for the same non-appearing Michigan-meeting volume mine was to have been in). In fact, there is a prior literature on the kind of "shift test" which I applied. Bengtson does not mention the important paper by Robert Oswalt, "The detection of remote linguistic relationships" in *Computer Studies in the Humanities and Verbal Behavior* 3: 117-129.

To end on a conciliatory note, I could not help but notice that two of Starostin et al.'s proposed ten Dene-Sino-Caucasian etymologies (MT 16: 8) look much like Nilotic: 'tongue-2': Hatti: alup (Pr.-Nil. *lɪɛp), 'knee, elbow': Sino-Tibetan *kut (PN: kutuŋ). I did not do an exhaustive search -these just happened to pop up in my memory- but 20% *seems* to be impressive. Gentlemen, what does it mean? This is pecisely the question I tried to address in the "shift test" article and I don't think anyone has a definitive answer yet.

Copies: Sarah Thomason, Ed., *Language*
Douglas R. Parks, Ed., *Anthropological linguistics*
John Bengtson, Merritt Ruhlen

Thomason - 80 -

LANGUAGE
Journal of the Linguistic Society of America

Sarah Grey Thomason, Editor
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5 May 1992

Dr. Harold Fleming, Editor
MOTHER TONGUE
5240 Forbes Avenue
Pittsburgh, PA 15217

Dear Hal,

Lionel Bender has just sent me a copy of his April 27 letter to you, from which I infer that John Bengtson's article 'Language rejects "Global Etymologies"' has appeared in Mother Tongue. Bengtson sent me a prepublication copy of his article; I hope that, before it was published, at least some of the errors were corrected (e.g. his claim that the paper Johanna Nichols published in Language was the paper she gave at the 1990 Greenberg Conference in Boulder, and his bizarre assertion that a paper by Merritt Ruhlen was refereed for Language by eight different scholars).

I'm writing now on the assumption that Bender's April 27 letter will appear in Mother Tongue. If it doesn't, please just ignore this letter. But if it does, I would like to correct a serious error in it. Bender refers to 'the "establishment journals'" argument that the topic is still in the category of pseudo-science and that the scholarly world is not ready to take it seriously'; the implication, in the context, is that I offered this argument in rejecting an article of his that criticized Bengtson & Ruhlen's 'Global Etymologies'. This is simply false. In fact, Bender's article was not refereed or evaluated substantively, because it is standard policy -- standard not just for Language, but surely for every scholarly journal -- not to publish a response to, or criticism of, an unpublished article. I explained this to Bender. My letter to him said nothing at all about pseudoscience, and I expressed no opinion whatsoever about research on long-range genetic relationships.

Sincerely,

Sally

Sarah G. Thomason

Bengtson -81-

743 Madison Street NE
Minneapolis, MN 55413
23 May 1992

Harold C. Fleming, Ed.
Mother Tongue
5240 Forbes Avenue
Pittsburgh, PA 15217

Dear Hal,

This is in response to Lionel Bender's recent letter to MT (27 April, 1992). The main issue I would like to address is the allegation that Merritt Ruhlen and I have been intellectually dishonest in not acknowledging the implications of Bender's "shift test" for our "Global Etymologies". (In the words of one of the Language referees, we were "less than candid" because of this omission.)

Bender asserts that he is simply taking up the challenge that Ruhlen and I threw out in our paper. (The same Language referee claimed that Bender "presented results that were indeed comparable in quality to those presented in this paper.") It must be stated that no one, in fact, has taken up the challenge as we framed it. We invited the skeptic to "produce a set of 27 etymologies, comparable in quality to those presented below, where the meaning is shifted one number in each case, i.e. AJA 'knee,' BU(N)KA 'ashes,' BUR 'nose,'" etc. Bender's "shift test" did not produce any such etymologies: rather, he took a canonic shape, e.g. AJA (our 'mother'), and tried to find words meaning, e.g., 'knee' in one language family, 'ashes' in the next, 'nose' in the next, and so on. It is easily seen that one of our constraints is completely overlooked, i.e., our requirement that each putative root be attested in at least six language families. Bender, ignoring this important control, has not successfully carried out our challenge. We are well aware that isolated chance resemblances can and do occur, and that is the reason for the six-family constraint, which virtually eliminates the possibility of the chance resemblance explanation.

Bender's conclusion (as of 1988) was that global etymologies are an "illusion" and "artifact of the authors". It would seem puzzling, then, how we arrived at our canonic phonetic shapes and glosses in the first place! (This is reminiscent of a critic's claim that Joseph H. Greenberg arranged the data in his American Indian notebooks to fit a "predetermined classification"!)

Let me state that I do not consider Bender's efforts a "cheap shot". He has obviously worked very hard on his statistical test, and with no tangible reward in view. Ruhlen and I respect his expertise, and have found his advice valuable, both on general methodology, and on Nilo-Saharan data.

We continue to encourage Bender, or anyone else, to devise a test that would resolve all doubts one way or the other. So far, it has not been done, and Ruhlen and I believe the data in "Global Etymologies" speak for themselves.

Sincerely,

John D. Bengtson John

copies: Thomason, Bender, Ruhlen

NILO-SAHARAN AND DENE-CAUCASIC (a squib)

Lionel Bender (27 April 1992) notes Nilotic resemblances to two of eleven Dene-Caucasic etymologies (MT 16). Besides the resemblances in 20 of 27 of Bengtson-Ruhlen's "Global Etymologies," there are many others between Nilo-Saharan and Dene-Caucasic. The following are just a sample gleaned in a cursory survey:

1. HAIR: NS: Shabo **c'eeka ~ šek ~** ko-**jeka**, Aka **jɛ́kɛ̀** id. // DC: West Caucasic ***sq́a** 'head', Yeniseian ***cɨGV** (Ket **tɨʔ**) id., Na-Dene ***cìk'** (Navaho **-cìiʔ**) 'hair (on the head)'.
2. BLOOD$_1$: NS: Songhai **kuri**, Nandi **kòrò**-tì, Mvuba **húrú** id.; Songhai **kyirey**, Acoli **kwààr** 'red' // DC: Basque **gor** 'flesh', **gorri**-nko 'egg yolk', **gorri** 'red', Sumerian **gur**, **gurun** 'blood', ND (Haida) **Gay** 'blood; to be red' (***-r-**> **-y** is regular in ND).
3. BLOOD$_2$: NS: Maasai **-sárgɛ́** id. // DC: Cauc ***ć'äLwV** (Hurrian **zurgi** 'blood', Chechen **c'ij** 'blood', **c'jẽ** 'red'), Sumerian **sa** 'blood', **sa**$_5$, **si**$_4$, **su**$_4$, **su**$_9$ 'red', ND (Eyak) **č'eeʔ** 'to turn red'.
4. EGG: NS: Kara **kábi**, Bagirmi **kab-**, Burun **kɔ́m**-at id. // DC: Cauc ***k'ămbV** (Andi **k̄'unu**) 'kidney', ND ***k'ǘmʔ** 'roe, milt, kidney' (Hupa **q'oŋʔ** 'salmon eggs').
5. TREE: NS: Surma **kɛɛn** 'trees', Shabo **konna ~ k'on(n)a** // DC: Burushaski **kuna** 'rod, pole, stick', Sino-Tibetan ***kūŋ** (Lepcha **kuŋ**) 'tree, branch, stem', ND 'stick, tree' (Eyak **kɨh**, Hupa **k̆in**).
6. ASHES: NS: Meidob u-**pudi**, Kara **bítí** id. // DC: Burushaski **phɛt**-iŋ 'ashes', ST ***phut** 'dust'.
7. NIGHT: NS: Shabo **dippi ~ dippo** // DC: Burushaski **thap**.
8. TO BITE: NS: proto-NS ***k'ay** (Ehret): Acoli **kàày**ô, Ik **k'edz** // DC: proto-DC ***xGVjV** (Starostin): Cauc *ʔV̲-GV, ST ***k(h)aj**.
9. ALL: NS: Zagawa **soko**, Mursi **cɔk**, Bodi **c'ɔk** id. // DC: Basque **aski** 'enough', **asko** 'much', Cauc ***HVč'aqwV** 'big' (Lezgi **č'eXi**), Burushaski (W) **čhik** 'all', ST ***čok** 'enough', ND ***ćùhk'** 'big' (Beaver **-čiik'**).
10. SMALL: NS: Mursi **tìinì**, Majang **tɛm**, Jur **θiin** id., Karda **tànnà** 'thin' // DC: Cauc (Bezhta) i-**t'ino** 'small', ST (Old Chinese) ***tōnʔ** 'short', ND: Haida **t'Am-** 'thin and rounded', Chipewyan **-t'ànɛ̀** 'thin', Galice is-**t'an** 'small'.

I do not think these resemblances point to a special relationship between NS and DC. (Some are also shared by Nostratic, Amerind, and other macrophyla.) At least some of these resemblances might most parsimoniously be explained as common archaic residue from a proto-human language. This binary comparison is only a partial glimpse of the big picture we have been trying to show in "Global Etymologies."

John D. Bengtson
May 1992

BENDER GETS THE LAST WORD.

Lionel Bender asked in a letter on June 27, 1992 the following:

"Following on my letter which I asked to appear in the next MT and Bengtson's reply, I would like to add this postscript:

Bengtson states that my test of 'world etymologies' is not valid because I did not follow the Bengtson/Ruhlen formula to the letter. In fact, the way I did it is at least as effective and is based on the literature (which Bengtson/Ruhlen still do not mention) on background tests of this sort (see references in my article)."

Editor's note: Since the matter of the 'cheap shot' has come up again, it needs to be recorded that that remark was made in a private letter by me to Bender. The meaning was that an established scholar ought not pick on rising young scholars. It is a cheap shot; that means it is easy to shoot down young fellas and look good oneself. At that time I did not understand Bender's motives because I thought he was a real long ranger. Now I discern his motives more clearly. I must admit that I was wrong and I must apologize to Bender. Herewith: LIONEL, I WAS WRONG AND I APOLOGIZE. YOU ARE A GOOD SCHOLAR AND I REGRET MY HOT TEMPER.

SOME MATTERS PUT OFF TIL WE MEET AGAIN.

Roger Blench has some revisions to Niger-Congo plus a bold new venture at classifying Niger-Congo within Nilo-Saharan which will surely get a full hearing next time. Wilfried Schuhmacher, Bert Seto, Pat Ryan and others have things to say -- next time.

Also put off were the demands on some colleague to produce state-of-the-art estimates of the acceptability of such phyla as Austro-Asiatic, Austro-Tai, Austric, Australian, Indo-Pacific and particularly its alleged Andamanese branch. Norman Zide, Gérard Diffloth, Robert Blust, Geoff O'Grady, and Ken Hale among others have agreed to write or have been asked to write those opinions. After all, this was one of our initial ideas, one of the strong points about assembling experts into a tolerant organization, that Dolgopolsky and I first wrote about. So we hope that our colleagues will produce some opinions for us -- in the spirit of cooperation which lies in our foundation.

ATTENTION EUROPEANS! EKKEHARD WOLFF HAS A NEW/OLD ADDRESS.

Professor Dr. Ekkehard Wolff,
Universität Hamburg, Seminar für
Afrikanische Sprachen und Kulturen
Rothenbaumchaussee 67/69
2000 Hamburg 13
DEUTSCHLAND

(c) Solicitation of Nominations for the Council. As the number of Fellows on the Council of Fellows is limited by the total membership of ASLIP, it is therefore possible to have a few new Fellows. While the Annual Meeting of 1991 elected four new Fellows, representing small countries, the Board of Directors in 1992 decided that we should revert to the original intention of our By-Laws which was to have the Fellows elected by the membership at large.

Therefore the four Fellows of 1991 are considered to have been honored by the Board for a year but not to have been elected for life, unlike the original Fellows.

Therefore the Board has nominated Professors Ben Ohiomamhe Elugbe, Karl-Heinrich Menges, Hans Mukarovsky, and John Stewart for election to the Council of Fellows.

Also nominated by the Board in 1992 for reasons of inadvertently not being nominated before or representing small fields (from an ASLIP standpoint) are Professors Igor Diakonoff (St. Petersberg), Sydney Lamb (Rice), Dell Hymes (U/Virginia), Irving "Ben" Rouse (Yale), and L.L.Cavalli-Sforza (Stanford).

Since we may have 17 Fellows, and presently there are 7 permanent Fellows, then we may elect 10 new ones now. Presuming that our membership will continue to grow, we may elect others in the future.

We ask our members now to nominate persons they believe ought to be on the Council but who are not presently there. The nominees must be members of ASLIP, according to our By Laws. We ask that only 5 people be nominated per person, even though the final voting will be for ten, so that the field of candidates be small enough to manage and so that more decisive results can be obtained in the election. If you are contented with those scholars nominated by the Boards of 1991 and 1992, then it is not necessary to return the nomination forms. Then in MT-18 the ballots for final voting will be enclosed.

DETACH THIS PORTION OF THE PAGE. WRITE YOUR NOMINEES' NAMES ON IT AND RETURN THIS PORTION TO AN ASLIP OFFICER. If you do not wish to nominate anyone, don't do anything!

1)

2)

3)

4)

5)