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Soup de jour. ARCHEOLOGICAL HEURISMS. Sub-title = Focus on India.

Herein one is invited to take a break from technical linguistics and its difficult jargon ("metalanguage") and join in some strategic thinking more akin to that of archeology. Then we get some specific ideas from archeology.

We have invited a number of colleagues interested in Dravidian and Austroasiatic and Nehali (Nihali) to join the Club. The questions which Vitalij raised about Austro-Thai (an alleged larger phylum consisting of Austroasiatic, Thai-Kadai, and Austronesian [Malayo-Polynesian]) are some of the most important ones which we face. Austroasiatic is crucial to everything. Thus, it can be seen, from the perspective which you will know in a moment, that Sino-Tibetan bridges northern Asia and southern Asia -- but this is not such a major consideration. Thai-Kadai didn't go much of anywhere except to be pushed down into south China and further Southeast Asia. (Benedict's reconstructed old Thai borrowings in old Chinese argue most powerfully for an old Thai location in CENTRAL CHINA.) Austronesian spread all the way across the Pacific, and west to Madagascar but seems to have a west Pacific homeland somewhere between Shanghai and Melanesia. (I wager it is northern Sunda-land & the Philippines.)

So what? So in a sense Austronesian, Thai-Kadai and Sino-Tibetan are products of China and the South China Sea. Sino-Tibetan is the northerner of the three and thus the most likely on purely geographical grounds to be related to alleged Nostratic or other phyla of northern Eurasia (e.g., Yeneseian). But by almost everyone's reckoning Austrasiatic is ABORIGINAL IN S.E.ASIA; include part of south China in that. Therefore, by a geographical and prehistorical reckoning, Austroasiatic would be farther from northern Eurasian phyla than any save Indo-Pacific and Australian. Then it ought to be (= may be expected to be) the phylum closest to Indo-Pacific, except for Australian. That seems to be exactly how Swadesh saw it in his KHMER-TASMANIAN phylum (see Circular.2). But he included Austronesian and Thai-Kadai in that too, apparently. It is also interesting that Morris joined Tasmanian to his Southeast Asian bunch, not to Australian, just as Greenberg joined it more recently to Indo-Pacific. (I wish we had the internal details of Swadesh's eleven great phyla of the world!)

HOWEVER, Austroasiatic is also well grounded in INDIA, where everyone agrees that it is ABORIGINAL. Probably native to eastern and southern India would be the dominant reaction but some see the long hand of Austroasiatic as far north and west as Himachal Pradesh and even the Indus valley (southern). That basically has this stretched-out phylum on the borders of the Middle East, where it is most likely on purely geographical grounds to be the next of kin to Nostratic via its alleged Elamito-Dravidian sub-phylum. Dravidian of course came in from Baluchistan and drove the Aborigines eastwards or absorbed them into Dravidian populations. Or so they say. Austroasiatic is also widely reputed to be associated with the so-called Australoid physical type of mankind which is also thought to be ABORIGINAL IN INDIA. The concensus on this must be nearly 100%.

Who is related to Austroasiatic? (Nowadays we should say "more closely related" because the notion of all being related is being bruited about!) For me it would be quite enough to know IF Austroasiatic has any linguistic kin because my interest in India is strong enough by itself. Perhaps because of the great influence of the late Pater Schmidt everyone seems to look eastward -- to Thai-Kadai or Austronesian. We also know, incidentally, that there will be strong resistance from at least the American experts on Austronesian. The logic of geography has already argued above that an inquiry into Austroasiatic vis-a-vis Indo-Pacific might turn up roses. Since Indo-Pacific is vast and so

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deep in time, one might in fact detach a piece of it and give it to Austroasiatic, e.g., Andamanese or Timorese. (One doubts that, since Greenberg's phyla seem to hold together like glued; but in Africa Thilo Schadeberg seems to have successfully torn away part of N-K and given it to N-S. The only known case, although Mukarovsky strongly and Bennett weakly think that some of western N-C should go with Basque instead of N-C. Sorry about the digression!)

Why not look to the WEST? To Nihali, Kusunda and above all Dravidian? Shades of old Nostratic: Austroasiatic has "checked" consonants which I would translate as glottalic ingressive (implosive) were I given permission by Norman Zide, David Stampe, Gerard Diffloth or H-J. Pinnow. My mistake. Shades of central Africa: We have glottalic ingressive from western Somalia to Dakar and we always have the same two that Austroasiatic has, to wit, [B] and [D] (or [b'] and [d']). In fact some Munda languages have the whole implosive set [B,D,J,G] which is just like Konso of southern Ethiopia. South of that belt, in Tanzania, begins the north-south belt of velaric ingressive (clicks), split in half by Bantu; the belt always lacks glottalic ingressive but does have glottalic egressive. (Do you agree, Khoisanologists?) But what do "typological" resemblances prove anyway? Prove? Nothing! We might all agree. But suggest heuristically? Maybe a lot!

By now the archeologists must be in tears! Can't we ever stop talking about bloody guttural consonants and get on with those "heurisms"? Okay, let's do that. What is a heurism? The word was just invented as a back formation from "heuristic" on the model : capitalism :: capitalistic as heurism :: heuristic. A heurism is something that helps us to invent or discover, like a heuristic device only shorter and more clearly factual. It is NOT a proof but something which MAY STIMULATE or encourage hypotheses.

A linguistic heurism is that Austroasiatic shares a peculiar kind of consonant with central Africa; so we might look to central Africa or just to Dahalo (South Cushitic), Boni or Konso (East Cushitic), or even Masai of N-S, as the closest thing, just across the well-traveled Indian Ocean from India. A physical or biological heurism would be the so-called Australoid physique which supposedly invites us to look towards New Guinea (Indo-Pacific) or Australia itself. Another physical heurism would be the so-called "Veddoid" or "Australoid" despised caste groups in Aden (e.g., the Zabidi) and other purported evidences of a former Australoid presence in southern Arabia. Some people have much emphasized these alleged facts.

The archeological heurism I have in mind is the famous "Movius's Line" which separates the western Old World from the eastern Old World, separates the axe tradition from the adze tradition, and runs north to south through India. More precisely, Movius's Line runs from the Bay of Bengal north thru Tripura & Assam to east Bhutan, thence west along the wall formed by the Himalayas, Pamirs and Hindu Kush until the wall turns north and northeast through Tien Shan to the Altai Mts. and half way to Lake Baikal. North of the Tien Shan and Altai portion of the Line it is presumed that no human habitation existed during the Pleistocene. So a giant human fault line of sorts runs through India as an old Paleolithic divider of humanity into fundamentally different groups. That's pretty strong! The inventor, Hallam Movius, is still alive in Cambridge, Mass. but long retired and out of touch with current opinion. The late Carleton Coon, a famous physical anthropologist also of Harvard, thought Movius's Line was dandy and used it in his "The Living Races of Man". In his scheme the Line divided the ancestral turfs of the Australoids and Mongoloids of the east from the Caucasoids, Capoids (Khoë & San) and Congoids (Negroes) of the west. Of course, although northern Tibet, Sinkiang and Siberia north of Movius's Line

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were frozen over during most of the great moments in human physical evolution, still during warmer phases of the Pleistocene, and after it, human intercourse and travel and migration could traverse the corridor from Soviet Central Asia, especially Khazakstan, to inner Mongolia and north China. Or go the other way. One might even call it the Sino-Caucasic or Vasco-Dene Corridor, if one wished to. Then again, one might NOT wish to! And Coon himself does not lack critics!

One WOULD like to hear from archeologists about the Line as viewed currently. Since so much of Movius's Line depends on the obvious and eternal (humanwise) barrier of our world's greatest block of mountains, then it is hard to see the Line as seriously mistaken. But it may have leaked more than Movius thought and in more important ways than he thought. Like the suspicions of "Neanderthal"s traveling from central Asia to China to Alaska and beyond. Yep, we definitely need to hear from archeologists and/or physical anthropologists on this.

Coon and others find much evidence of Australoids so-called in India. But by Movius Line reasoning they should have moved into India from EAST OF BANGLADESH, i.e., from the Naga hills and Burma or beyond. That movement COULD be the first movement of Austroasiatic from its homeland in Southeast Asia into India. BUT, BUT, and here the problem gets hot, Movius's Line runs almost through the center, just to the west of center, of Austroasiatic's PRIMARY DISTRIBUTION. According to Gerard Diffloth in his Brittanica article on Austroasiatic, the family (phylum) has three primary sub-families, viz., MUNDA, NICOBARESE, and MON-KHMER. Munda is found only in central and eastern India proper, west of Bangladesh and Movius's Line. Nicobarese is found only in the Nicobar Islands which lie just south of the Andamans and probably could be reached most easily during low sea-levels by land from Chittagong or Rangoon, south of but basically west of mainland Southeast Asia. Mon-Khmer has twelve branches spread from Viet Nam to Assam, with its center of gravity in Thailand. Despite its diversity it does not "weigh" more than Munda when we come to postulate the balance point or center of gravity of the three sub-families. That point or center would seem to lie more along the eastern shores of the Bay of Bengal than anywhere else, perhaps near Rangoon. The weight of Nicobarese counts here. If something else were to be thrown onto the scales, the balance would shift decisively either east or west. As things stand now, there is only a modest tilt towards the east.

Well, there stand India's THREE SMALL PHYLA, to wit, Nihali, Kusunda and Burushaski. (The rest of you guys call them "Isolates".) Naturally, Paul Benedict may be right in seeing Austroasiatic as connected to Austronesian and Thai-Kadai and I greatly respect his opinion. Then the balance would shift most certainly to the east and Munda would be an old migration or expansion from Burma. However, if Yaxontov does not convince Dyen and other Austronesianists, then the India cards ought to be played. Nihali especially, but also the other two, may have much to show us.

Nihali (Nehali, Nehari, Nihari) or Kalt.o is located smack in the middle of India proper, near Indore. As others have said in print, I too found that this "bandit's jargon" was loaded with borrowings from Indic (IE), Dravidian, and Austroasiatic, i.e., everyone in central India or nearby lent Nihali words. The Albanian of India but more so. Since Nihali has lost so much of its own native lexicon, it might be the first human language to be UNCLASSIFIABLE IN PRINCIPLE because it has so little of its original self left. To those who teach the primacy of morphological evidence in genetic taxonomy Mbugu (South Cushitic) of Tanzania has been unclassifiable because it borrowed its whole grammar (almost) from adjacent Bantu languages, whilst showing a basically non-Bantu lexicon. (Or else they want to call it a Bantu language).

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But all has not turned out to be hopeless re Nihali. Pinnow has made a proposal that Nihali is coordinate to all of Munda as a distinct western sub-family of Austroasiatic, as opposed to Mon-Khmer and Nicobarese which make up eastern Austroasiatic. In effect it all balances now right on Movius's Line. But Zide does not agree with Pinnow about Nihali. Why? Because Pinnow used pronouns and verbs for his argument while Zide found that ordinary lexicon was not related to Austroasiatic. Also Zide quotes Pinnow as suggesting that Nihali may have a non-Austroasiatic substratum. Und so weiter!

It IS possible that Nihali is coordinate, not to Munda only, but to all of Austroasiatic. Yes, but, there are those who say that Vietnamese is also coordinate to all the rest, thus throwing weight on our balance towards the east! Such a linguistic balance is not supposed to exist along Movius's Line so maybe the Line itself is wrong. Or maybe we need to relate Austroasiatic clearly to another phylum to settle the matter. Yet in the settling it must be remembered that the basic movements of Austroasiatic-speaking peoples into India or Southeast Asia, one or the other, must be pretty old. Is anyone working on Austroasiatic "external relations"? Tell someone what you think!

It will be useful to quote a section from a recent letter from Vitalij Shevoroshkin. Quote: "About Austro-Tai, Miao-Yao, etc. If one is attentive enough not to include borrowings into inherited basic lexics one operates with when reconstructing proto-languages, determining genetic relationships, etc., one gets the following picture: Austro-Tai (Austronesian and Para-Tai [Tai, Kam-Sui, Ong-Be, Li, Lakia, and one or two more group(s) which include Lao, etc.] is remotely related to Austro-Asiatic (Mundu, Mon-Khmer, Viet-Muong, Khasi, Palaung-Wa, Khmu7 and Miao-Yao). This is, more or less, the classification which is proposed by I. Pejros in the materials of the '84 conference (Pejros wrote a paper on Proto-Miao-Yao, but I don't know if it has been published) and which, in principle, corresponds to S. Yaxontov's (Jaxontov or Yakhontov) data -- see S. Yaxontov in the materials of the '77 Nostratic conference ("Defending the Austro-Tai Hypothesis") and in "Genetic, Areal and Typological Connections of Asian Languages", Moscow '83 (pp. 19-33). Yaxontov gives solid stuff: roots (words) which almost certainly are no borrowings common in Austronesian and Tai; stable words of this kind common in Miao-Yao, Mon and Khmu, etc. The problem of Dyen's, Benedict's and others is the lack of understanding what is inherited and what belongs to ancient borrowings (B's AUSTRO-TAI was a pioneering work and, in principle, correct, -- but the amount of mistakes is large and reconstructions are inaccurate; his last book is wrong in principle, exactly because of the above lack of understanding). " Unquote.

As one will see from Ruhlen's letter (later) Greenberg admits to being mistaken about Miao-Yao and would seem to agree with Pejros and Yaxontov. Since I was defending Greenberg's error as an alternative hypothesis, I feel foolish now. However, since I keep mentioning Dyen's old position on Austronesian, it would help if we heard from him directly.

A final note about typological and morphological criteria in genetic classification. These are constant preoccupations of historical linguistics, for sure. Gerard Diffloth tells us in the Britannica that Munda and Vietnamese have come so much under the influence of Indian languages and Chinese in their respective cases that their value in reconstruction is limited. Typologically they are very dissimilar. Vietnamese is monosyllabic and tonal; Munda is polysyllabic and non-tonal. Munda has complex morphology with affixes of all types, while Vietnamese has essentially no morphology. Like Chinese vis-a-vis Tibetan! India is also the place where Gumperz has shown adjacent Dravidian and Indic languages swapping grammars with each other. Burushaski is, of course, a very typical Indian language from the viewpoint of typological phonemics.

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India has SEVEN of what I call phyla represented therein. Indic entered from the outside, as did the bit of Thai-Kadai in Assam. Dravidian is autochthonous but suspected of being from the outside. Austroasiatic has one foot in Southeast Asia too. But Nihali, Burushaski and Kusunda have no relatives inside or outside of India. It is an old tropical area, famous for its unity and diversity, but with archeological knowledge well behind our knowledge of its languages, cultures and bodies. In this case the archeologists might consider using linguistic heurisms to help themselves.

I am wont to think that Austroasiatic is the "soft underbelly of the Asian problem", as Sir Winston might put it. Suppose that Austroasiatic turns out to be fought over because it occupies, as Morris Swadesh would put it, a key central position in the NET of human languages. On the one hand, just suppose that it is remotely related to Austronesian (pace Isidore!) and Tai-Kadai; on the other hand just suppose it is remotely related to Nihali and Kusunda. Look what it has linked together -- central India and central China! Not to mention Hawaii and New Zealand. It reminds me so much of Indo-European which seems also to have this central role between Mauritania and the Bering Straits. AA, Kartvel, Dravid to the south; Uralic, Altaic, Chukchee nor' east.

New Wine. HEURISMS FROM PHYSICAL ANTHROPOLOGY.

Serogenetic research (blood groups & serum proteins) has added greatly to the study of human variation and has given us something better than the old gross race classifications. As all American anthropologists are taught not to forget that "there ain't no correlation between race, language and culture", we cannot use biological evidence to settle matters in linguistic disputes. But, used as heurisms, those same data can give us useful or potential hypotheses about language or fossil cultures. In some cases, African bodies and African languages for example, where theories relating bodies to languages are so controversial and long fought over, heurisms rapidly become sources of pain. Yet there are levels of inquiry where heurisms need not be so disturbing. In the Austroasiatic case, for example, serogenetic heurisms would encourage one to look towards Austronesian and Thai-Kadai but away from Burushaski. There seems to be no serology of the Nihali or Kusunda. Not every genetic system tells us much about human variation but some genes or gene clusters (haplotypes, allotypes) like Rhesus, Duffy and Gammaglobulin (above all) can be very helpful.

The Rhesus and Duffy data on the Munda-speaking populations are significantly more like the data from Southeast Asia and the western Pacific than they are like that from Indic and Dravidian folks. The few instances of GM (Gammaglobulin) add zest to that conclusion. GM data on Austroasiatic speakers suggest that "Australoid" does NOT mean Papuan or Australian aborigine. (I once was awed by a photograph of some Stieng men from Cambodia because they looked exactly like a friend of mine from Bihar!). In Rhesus, Duffy, and GM the speakers of Thai-Kadai, Austronesian and Austroasiatic are so similar that they could almost be considered to be one population. Even the Polynesians mostly fit into this pattern. Some of the Melanesians are exceptional in that they seem transitional to New Guinea's two basic (native or non-Austronesian) patterns but in a multitude of different ways! Outside of the northern coasts most of New Guinea is strongly opposed in GM, although very similar in Rhesus and Duffy. Australia is opposed in Rhesus and GM for the most part.

At an even higher level of abstraction, however, all three genetic systems shout at us that all Africans are not the same but that some Africans are more like the outside non-African world than other Africans are. Pigmies

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and most central Africans are less like the rest of the world than anybody else on earth. The Khoi and the San are appreciably more like outsiders. Say the comparisons are with Amerinds or native Australians or Papuans or the English; whoever is compared achieves the greatest genetic distance or the smallest similarity with the Pigmies of central Africa. (Cavalli-Sforza and other professionals use mathematically-sophisticated measures of genetic distance; I just add up the similarities. I have no idea why I frequently get the same taxonomic results that they do, but it must be an accident!). When one uses more genes and/or haplotypes than these three, one no longer gets the results that I get with just Gm, Rhesus, and Duffy.

Sometimes the "dendrograms" (trees) produced by serogeneticists lead to absurdities. I saw one which carefully separated two groups of Sardinians from each other and assigned each to groups roughly as dissimilar as the Irish and the Arabs. The early results in some systems, e.g., histo-compatibility testing, have given the most outlandish results of all, like lumping the equivalent of Eskimos, Pigmies and Papuans together, while thoughtfully creating new races of other Eskimos, Pigmies and Papuans -- and throwing the Scots in for good measure with the Papuans.

Hey, nobody's perfect! Surely all Africanist language classifiers can point to the equally absurd things that we have done. (We have declared, once, that mutually intelligible dialects in Kordofan belonged to totally unrelated phyla because of typological dissimilarities!) But for a lot of heurisms that are useful, in my opinion, and at least interesting, try Arthur Steinberg's DISTRIBUTION OF THE HUMAN IMMUNOGLOBULIN ALLOTYPES. Of course, thanks to Herb Lewis, I heard about the new super duper mitochondria stuff. It was later featured in the science section of the Boston Globe and then still later it appeared on educational television in eastern New England. Purports to DATE the divergence time between human populations by means of differences (or is it similarities?) in mitochondrial DNA between samples of females. This could be useful for Long Range Comparisons, n'est-ce pas? Does anyone know anything more about it? Obviously, I do not "command the subject" too well but I'm trying simply to forestall a week's cram reading in the Biology Library! Adrian Hill of Oxford, Rebecca Cann of Berkeley and Douglas Wallace of Emory have been invited to share their expertise on the subject with us and maybe learn something from our members too. (For Cann's response, wait a bit!)

Concluding heurisms. I expect that Niger-Congo will prove to be the hardest of all to relate to other phyla, if one counts only the (physical) evidence of Rhesus, Duffy and GM. BUT Cavalli-Sforza in one publication, in his dendrogram, puts Europeans and Africans together, as opposed to the rest of humanity, WHILE Christy Turner in his very interesting study of TEETH also places Africans with Europeans (See the magazine "Natural History", Jan. 1987). ("Europeans" here includes North Africans, Arabs and many west Asians). One would then expect Niger-Congo (N-C), Nilo-Saharan (N-S), Khoisan, Basque, Afroasiatic (AA), and Indo-European (IE) to form a genetic unity, as opposed to the rest of the phyla. The underlying force of Turner's and Cavalli-Sforza's working hypotheses could be geography, i.e., Europeans and Africans represent the western side of the Old World with common selective pressures and shared gene flow. Shades of Movius! Is this axe versus adze?

Of course, it does not make any difference at all what the biological evidence might predict. The linguistic conclusions could be flatly opposed to them and still be perfectly valid. Besides that, however, I doubt that our biologically oriented colleagues have any consensus on Africans, Europeans and others and their taxonomic proximities. But possibly, just possibly, the mitochondrial studies may tip the scales towards one of the working hypotheses,

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rather than the other. BUT THIS STUFF IS CERTAINLY FASCINATING !!

One of my reasons for mentioning the biological heurisms in the African case is that we seem to have in Africa an even larger counterpart to the Movius's Line problem. Everyone who is interested in AA and its connections seems to look towards Europe or western Asia -- on the assumption that that is the direction old p-AA came from. Like the "look eastwards for Austroasiatic" case. Even though I remember saying in Circular.2 that I did not expect either N-S or N-C to be related to AA, still I HAD looked at them. Also Greenberg in 1963 suggested that one or both of them might yet turn out to be related to AA. So it might be a good thing if everyone who is now trying so hard to relate AA to IE would just turn around and look the other way for a while. Maybe you will find more evidence than I did!

Another view naturally is that AA is an African phylum IN ORIGIN. Grover Hudson has held that view and me too. (See how careful I am getting to be about what others have said in the past!) If AA is related to IE or Nostratic, it means that they (IE and Nostratic) came from Africa originally. And I am positive that is what Carl Hodge and Karl Petracek think.

Alas, Nilo-Saharan (N-S) derives virtually no help from serogenetic heurisms, including no help at all in the case of GM and Duffy. In Rhesus what data there are, mostly on Nilotes, emphatically link them with Tuareg serfs, central Africans, Pigmies, the San hunters, Hadza and others, less so on average with Bantu and some coastal West Africans, and even less so with Ethiopians and most peoples of the Sahara. But mostly the N-S picture is blank. Would you believe that the famous supposed "Nilo-Hamites" of Kenya (e.g., Masai, Nandi, Turkana, etc.) are less known serologically than most Papuans? And the southern tiers of AA too, e.g., 100-150 Chadic-speaking peoples are practically unbled? All of Omotic is represented by one small GM sample of Wallamos; no Rhesus, Duffy, MNS, P, Hp, not even useless ole ABO data are known for any Omotic-speaking peoples. Or that the area where more sophisticated GM data would make a great difference -- India -- there is little. Ditto Polynesia. Biological researchers just have their own strategies for dealing with the questions that THEY are interested in. Our problem is to get more of them to see how much they can help heuristically in the solution of prehistoric problems.

Christy Turner's dental data and conclusions which were aimed at the New World and its problems, not at Africa and Europe, constitute significant heurisms for Amerindists. He segregates Na-Dene people, the Dene of Vasco-Dene, from the main mass of Amerinds of North and South America. Not to mention the Eskimaux and Aleuts who are also different. Both the Na-Dene and Eskimo (and Aleut) have dental kin in Siberia, according to Turner. Does that not resemble what the venturesome linguists like Swadesh, Greenberg and Nikolaev are saying? (In that same "Natural History" article it mentioned that William Laughlin had concluded that the Aleuts must represent the most ancient immigrants from Asia to the New World! Well, reporters often misquote their scientists. I doubt that Laughlin said that because it seems to be the opposite of linguistic conclusions and violates what I would call "common sense".)

There surely exist sources of misleading information, which I will call DIS-HEURISMS on the model of the famous political "disinformation". One of the most famous must be Thor Heyerdahl's discovery that a raft could float from Peru to Polynesia. And therefore the Polynesians were migrants from Peru or at least South America? Africa has had the almost equally famous case of the Fulani of West Africa whose physiques suggested that ancient tribes of Israel or Egyptians were their ancestors and that therefore Fula (Peul) was a Hamitic language. Potentially, in my opinion, the greatest case of Dis-heurism is that

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of the Bantu of South Africa, even though most scholars have not been fooled by it -- strangely enough! On the one hand, most Bantu languages of the southern tier, like Xhosa, Zulu or Tswana, are loaded with clicks (velaric ingressives) and glottalic egressives -- just like the Khoisan languages. On the other hand southern Bantu bodies bear much resemblance to those of Bushmen and Hottentots (old terminology), especially in GM where Khoi and San populations have very special haplotypes. In some Bantu populations, according to Trefor Jenkins and Arthur Steinberg, gene flow from Khoi and San reaches the 50% mark. One could say that the Xhosa are serogenetically just as close to the !Kung as they are to some northern Bantu population like the Kongo or Luba. One could make a substantial argument that Xhosa's proper genetic classification was Khoisan. Yet no one seems to have been fooled! Perhaps colleague Westphal knows of such a case. I would guess that the answer is that everyone has seen the truth -- a large amount of BORROWING (clicks and genes) can produce a situation ripe with possible dis-heurism. This business of BORROWING will always be one of our pre-occupations.

Stale Beer? WHO INVENTED MOTHER TONGUE ??

We of course seem to be serving up stale beer. The question of the origins of human language hangs over the Long Range Comparisons Club like the odors of an old beer hall. It is supposedly unanswerable if put in the form of a question like: "When did humanity acquire speech?" or "What are the origins of human language?". Notoriously, the French are said to have outlawed the whole discussion long ago. And everyone followed their lead, as usually happens when the French take a stand of some sort or other. So this topic is dead?

Apparently not! Lenneberg and Chomsky's ideas, Hockett's careful review of design features, Lieberman's laryngeal hypothesis, Westcott's discussions (not read, unfortunately, by myself), and many others indicate that the topic is truly alive. It has possibly not been noticed by linguists but most introductory textbooks in Physical Anthropology have something to say on the question of the evolution of human language, by which they mean origins. Some, like Bernard Campbell, have quite a bit to say. Not too long ago a book on the topic came out. It was entitled GLOSSOGENETICS: THE ORIGIN AND EVOLUTION OF LANGUAGE, edited by Eric de Grolier (ISSC, Paris) and published by G+B/Harwood (Harwood Books, London). Perhaps it escaped most people's notice.

Frank Livingstone and Philip Lieberman had articles in it, among other people -- some of them obviously deep probers on Mother Tongue.

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Eric de Grolier: In Search of Semantic Universals (Summary).

Appendix I: Eric de Grolier: Proposal for a Transdisciplinary Symposium on Glossogenetics.

This came out in 1983. Its # is ISBN: 3-7186-0158-3. Pp. 546. Price \$38.50. It should prove to be very interesting and valuable; I have not read it yet. However, so as to inhibit our ever-expanding jargon, I hope that the term "glossogenetics" does not catch on! (More on these colleagues of ours later.)

Unlike other protolanguages, our common human Mother Tongue has special problems associated with it. I think that we all presume that an early language like proto-Australian or proto-Macro-Maya is itself derived from a pre-existing language about which we know nothing concrete but which we assume has the "usual design features" of human language. Or to put it another way, when we propose proto-Ket-Kot (Yeneseian), we do not have to solve the origin of human language question. We presume that p-Ket-Kot had some sort of ancestor -- another language much like p-Ket-Kot in general attributes. I presume that we presume. You may not agree and, as usual, a spirited disagreement helps our inquiry along its way. But a key question would be something like this: let us take that paragon of expert reconstruction -- proto-IE (p-IE) -- the model of all our methods. We say that p-IE was spoken by a population of cowboys, wielding tomahawks, in south Russia around 4000 BC. But, of course, we do not say: our ancestors who lived in south Russia around 4000 BC INVENTED LANGUAGE and passed it down to us. That would be so silly! But why would it be so silly?

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Because we know that there are thousands of other human languages which are not descended from p-IE and because we presume that these other languages, or at least some of them, are in principle RELATABLE to our IE languages. And I do believe that in this context "relatable" means to have one or more COMMON ANCESTORS.

But Mother Tongue has this special problem, as well as being at root a great ambiguity. (Again, let me say that "Mother Tongue" is Vitalij's term, not my invention. Also one should try to obtain from him a copy of that small Michigan paper on Mother Tongue. It is nearly poetic in its feeling for the subject). We do NOT presume that Mother Tongue had an ancestor, at least not like the one which preceded proto-Ket or p-IE. Mother Tongue was the FIRST and therefore has eternally attached to herself the HOW COME? question. Let us presume for the moment that Mother Tongue was a language basically like her daughters, and their daughters, and so weiter. She had phones and phonological rules, morphemes and/or words, and grammatical rules at a higher level (phrases and sentences). If she had not those attributes, then she would not be like her daughters basically. Now let us set up a name, time and place, as we did for p-IE, and borrow them from Rebecca Cann (except for the name). Then we MIGHT say: Mother sapiens or Mama sapiens who was the common ancestor of all modern human females invented language in 200,000 BC in Africa and passed it down to us. Then, given the way our minds work, we are likely to ask: HOW DID SHE DO IT? It would seem that Eric de Grolier's book and much theorizing on the subject have addressed that question.

But we could also ask the standard p-IE question in a different form: who was Mother Tongue's ancestor and did she have any cousins or did there exist others similar to her? Let us suppose that Mother Tongue had an ancestor, just as Mama sapiens had one too. Mama sapiens was descended from Homo erectus and perhaps so-called archaic Homo sapiens just before her. This seems to be well known. But Mother Tongue has only some vague and questionable entity preceding her, like "earlier forms of human communication", and there are those who think Mother Tongue sprang into being as a mutation or (it strikes me) as a Deus ex Machina, a miracle. That's a handy way to escape the How Come? problem but it entirely escapes the CONTENT questions too, i.e., what was the phonetic, lexical, and grammatical CONTENT of Mother Tongue.

Here are some simple facts or observations. We have no idea if Mama sapiens invented Mother Tongue or not (pace Lieberman). We really do NOT know whether it was a case of MONOGENESIS or POLYGENESIS, i.e., did Mother Tongue have cousins or not. Suppose that we never can show that Australian and N-C, at the two ends of the tropical Old World, are related to the rest of our languages. Then Mother Tongue is plural, i.e., polygenesis is correct (in one sense). Indeed at this very moment, if we take phyla that linguists generally accept, we have polygenesis on a grand scale. In one defeated grant proposal I counted 27 Old World phyla and between 3 and 15 New World phyla. If linguistics remains as cautious diachronically as it is now, then that number of 30 to 45 human language phyla will remain and the conclusion will have to be that our Tongues had at least 30 Mothers. (The rest of you may perhaps grant me this boon; that for the purposes of looking into the remote past any UNRELATED language should be called a PHYLUM. Just like the hundreds of languages in N-C, Kusunda represents a linguistic portion of mankind without kin and therefore potentially with its own Mother Tongue. As in the case of Basque the Kusunda branch may have lost many clans between then and now. To call these things "Isolates" is to look at them synchronically, from a warehouseman's viewpoint. Kusunda and Nihali are tiny and socially unimportant nowadays but like the duck-billed Platypus their importance lies in evolutionary taxonomy.).

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Besides that, Mother Tongue is quite ambiguous in a different sense. There is Mother Tongue-1, the basic pre-conditions in brain and anatomy for emergent human language. There is Mother Tongue-2, that language which in its CONTENT underlies modern languages. THE TWO COULD BE DIFFERENT! For example, suppose that Phil Lieberman is correct in saying that the anatomy of our speech apparatus was not ready for modern language until fairly late Homo sapiens times, that even Frau Neanderthal of 70,000-35,000 BC was not inventing Mother Tongue. Of course, if Neanderthal was a European offshoot which later became extinct or absorbed into Homo sapiens sapiens (Cro-Magnon with Aurignacian tools), then the archaic Homo sapiens of Kibish (Ethiopia), for example, might have been "anatomically ready" by 130,000 BC. According to G.E. Kennedy (PALEOANTHROPOLOGY, 1980), anatomically modern people (Homo sapiens sapiens) are now known to be distributed from Africa to Australia, and indeed even into California, by 40,000 BC. (Maybe 50,000 BC in California. As many of you know, of course, anything human before 12,000 BC in the New World is extremely controversial.)

So the speech apparatus is ready to work with the big modern brain to produce modern human language. That is what I call the brain + anatomy PRE-CONDITIONS. Let us say the date is before 40,000 BC and the location is halfway between Dakar and Timor, say Iran or Oman. That human population MIGHT begin to select sounds, arbitrarily assign meanings to sequences of sounds, and agree on grammatical rules or something like that. Because as one can immediately realize: THEY DID NOT HAVE TO DO IT! Although I have no idea at all, this day in my study, how they did it, let us just suppose that one can find out how they began "phonemically-based human language" by reading Gordon Hewes in Eric de Grolier's book. His title seems to be focused on the key problem from the standpoint of CONTENT. Maybe after our ancestor(s) were "ready" to start up a language they were still so lazy and slow-moving that it took them another 10,000 years to get one actually started.

Even though many of us hate or fear computers and their jargon, let me ever so briefly use a bit (not a byte!) of computese. By analogy, what I call the pre-conditions of human speech may be called the HARDWARE. The actual content of the language may be called the SOFTWARE. Then my question becomes one of : how long did it take, after the "hardware" was ready, for someone to write the first program, i.e., get the "software" ? Or, did we actually invent the "software" first and then get "hardware" to suit it? And, given that the "hardware" was in place, were different "softwares" invented at different times in different places, i.e., when people were ready to speak, they invented one new language (Mother Tongue) in Africa and another different one in Australia? Many colleagues will naturally think these questions totally ridiculous.

WHEN was Mother Tongue invented?

Swadesh thought 25,000 BC and the Moscow Circle seems to agree. Greenberg favors a date of 12,000 BC for proto-Amerind to arrive in North America, with Na-Dene and Eskimoan following later. He has also mentioned (publicly) that he thought proto-Afroasiatic was "Neolithic" in origin; that means 8000 BC more or less. Militarev and Diakonof (according to Militarev) would agree with Greenberg's Neolithic date for Afroasiatic. It is likely that Greenberg would agree with Swadesh on the dates of Mother Tongue, but he is free to speak for himself obviously. Shevoroshkin favors 25,000 BC also but supposes that it could be much earlier. Westcott favors an earlier date of "Upper Paleolithic" for Mother Tongue which by standard European archeological dates means between 35,000 BC for the first Perigordian in France or 32,000 BC

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for the appearance of Cro-Magnon and the Aurignacian in France. Lieberman would agree with Westcott or have it earlier because he wants to exclude Neanderthal but not necessarily archaic Homo sapiens. It is certain that many other people have other dates; right now I cannot remember them. Explicitly, I can remember Carl Hodge and Karl Patracek giving estimates for AA by itself and AA-cum-IE but I cannot remember them precisely. Probably much more than 8000 BC however. (Why does 20,000 stick in my mind?) And finally some physical anthropologists, including famous ones like Washburn(?), have put the dates back in early evolutionary times, more than a million years ago or whenever tool use occurred.

What is the logic, or what are the logics, of these estimated dates? Well, Swadesh extrapolated backwards from glottochronological estimates of specific phyla. Twenty years later we can see that by Morris's own way of figuring dates he would be obliged nowadays to give AA alone nearly as much time depth as he gave all of human language. The Moscow Circle? I don't know but they can tell us later on. Greenberg's dates are based on archeological correlations with Paul Thieme-type cultural reconstructions. (Thieme: since p-IE had words for trees and animals found in Germany, but not for trees and animals found elsewhere, ergo p-IE lived in Germany). There are agricultural terms in proto-AA, as Greenberg sees it, and therefore p-AA is Neolithic. Proto-Amerind entered North America with the first humans there, ergo proto-Amerind dates from the first fossil evidence of human culture in North America. Archeologists told him such evidence dated to 12,000 BC, und so weiter. It is also clear that Greenbergian dates are definitely NOT based on glottochronology which he dislikes. Westcott and Lieberman follow the logic of the transition from speechless early Homo to speechful later Homo. Some dates are based on intuition + hunch + guess, as in the case of the many Chadicists saying 15,000 BC for p-AA. (cf Circular.2). Chris Ehret has a partly formalized version of by-guess-and-by-golly, which I call pseudo-glottochronology, but his dates seem to stand up well archeologically in N-S and South Cushitic. Finally, the logic of Washburn and other physical anthropologists seems to be that language is just part of culture and that tool use or tool-making is good evidence of culture and so the first evidence of tool-making is evidence of the origin of language. It is, I think, likely that a substantial number of social and cultural anthropologists would also agree with the tool-making logic.

Since we are basically concerned with Mother Tongue here, not dating methods per se, we are not forced to evaluate each method. Let us wait to see what Starostin has come up with in the case of dating methods. But there are very general indications about the age of Mother Tongue which can be used to FALSIFY some of the specific hypotheses (not the methods) mentioned above. It is the process of getting rid of chaff. Some of the principles or logics mentioned above may, for example, be correct or prove correct but the case where we find it applied may contain some poor information or have mistakes in detail. By analogy many people use the Comparative Method but some use it poorly. The intuitive or by-guess-and-by-golly hypotheses depend on personal expertise and so cannot be evaluated easily, although the expert's specific hypotheses can be evaluated on a case by case basis. Finally, some of the intuitive dates are based in fact on archeological heuristics; the linguist makes a shrewd guess about the archeological segment which fits his language group and borrows the archeological date for his language group or leans towards it.

I believe that what we know about Australia, New Guinea and the New World require us to abandon all Mother Tongue dates later (= more recent) than 40,000 BC. Or else to abandon the notion that the languages of those places can be genetically related to other Old World languages.

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The New World case is strongly debated, as mentioned before, but Greenberg's date for proto-Amerind is 13,000 years too recent in the opinions of some archeologists. Scotty MacNeish for one and Fred Gorman for another believe that Man entered the New World as early as 25,000 BC or earlier, maybe as early as 50,000 BC in Gorman's opinion. Brian Fagan, a moderate centrist perhaps, in his PEOPLE OF THE EARTH (1983), an introductory text in world archeology, lists the 14-16,000 BC finds near Pittsburgh (Meadowcroft), 20-22,000 BC finds near Mexico City, 18,250 BC in Peru, and another widespread South American industry of 10,000 BC which was found way down in Patagonia. Fagan's general conclusion is that: Quote: "Any evidence for human occupation south of Alaska prior to 25,000 BC is still unproved. People were living in Mexico, and probably farther south, by 20,000 BC." Unquote.

Fagan also reports evidence for fossil man in Alaska possibly as early as 38,000 BC (Old Crow Flats) but agrees with the finder (Morlan) that the date is "highly uncertain." The 38,000 date was obtained by relative dating of mammal bones, but the same material yielded up radiocarbon dates of 23-27,000 BC which I read Fagan as saying "at least 23-27,000 BC". Moreover, had proto-Amerind come roaring over the Bering Straits in 12,000 BC they would have found much of Alaska already occupied by the "Paleoarctic" tradition. This was in place by 13,000 BC, had some connections with Siberian cultures of that time, and was ancestral to the tradition which settled the Aleutian islands and which had reached Anangula, a "third of the way along the chain" by 6700 BC. If this is not Eskimoan (Eskimo-Aleut), then I will eat raw blubber!

Gail E. Kennedy lists 4 sites in California containing fossil men and dated between 17,000 and 48,000. There is also a 30,000 BC human from Canada (Taber, Alberta). Technical problems have kept most of the sites in a questionable status but her dates are: Quote (p.405): "...obtained from the relatively new technique of amino acid racemization. Although the newness of this technique has not limited the acceptance of amino acid dates for the Old World, such dates for the New World are inexplicably controversial." Unquote.

So the dates proposed by Greenberg represent a conservative choice among opposing archeological views. Nevertheless, all we others can conclude is that proto-Amerind may be dated from 12,000 to 48,000 BC getting into North America. Again our expert colleagues in archeology and paleoanthropology will have to advise us about the merits of the controversy.

Since I wrote those words above, I have gotten two more and strongly opposed opinions. First, a new archeology/physical anthropology textbook (R.Jurmain, H.Nelson and W.Turnbaugh, 1987) categorically denies the validity of any of the dates cited above, saying that every single one of them earlier than 12,000 BC have been INVALIDATED by more recent excavations or recent attempts to verify them or, in the case of amino acid racemization dates, demolished by new improved radiocarbon dates. Some of the 30-48,000 year dates have been reduced to as little as 1-2000 years! Secondly, my colleague Ed Wilmsen tells me three interesting things, to wit, (a) there is a WILL to believe very early Amerind dates among many Americanists, (b) much of the evidence for early dates was based on "junk", and (c) the "Paleoarctic" tradition is not Eskimo-Aleut but rather Amerind and that the date of 13,000 BC is SOLID. For back-up he cited the opinions of four other archeologists who are expert on the Arctic. This is a strong argument!

Before I try to find a place to buy some raw blubber I'll have to put up a small struggle. Small because the Americas is not my area of expertise. But because I am profoundly sceptical of 12,000 BC for linguistic reasons I'll look around for some other kinds of reasons. First, once upon a time Americanists were also famous for the Hrdlicka tradition wherein anything older

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than 1000 BC was bitterly resisted. Secondly, if the Paleoarctic tradition is, as Fagan says, clearly continued by the Aleuts, i.e., if the Aleuts derive from that tradition and they have gone one third of the way down the Aleutian chain (Anangula) by 6700 BC, then the Aleuts must be part of Amerind or else the Paleoarctic tradition is not Amerind. As far as I know, NOBODY thinks that Aleut (-Eskimo) is part of Amerind linguistically or culturally. But even I can think up a rebuttal to my argument, viz. -- those who went down the Aleutian chain first were Amerinds; they were later replaced by Aleuts or became Aleut in speech. Alas, that is the best I can do but I remain unconvinced that 14,000 years is enough for all the linguistic diversity in the Americas, south of Na-Dene. But I WILL start looking for a source of raw blubber!

Hold it! Don't eat that blubber yet! Our good ole journal, NATURAL HISTORY, has struck again! This latest issue has an article by Tom Dillehay, an archeologist at U/Kentucky, entitled "By the Banks of the Chinchihuapi" in the series on THE FIRST AMERICANS which Natural History has been running. Christy Turner's article on American teeth was in this series, as well as Merritt Ruhlen's very informative piece on languages of the Americas. Dillehay's site is found in Mapuche (Araucanian) territory, way the hell down in southern South America (southern Chile) where the tropics recede and the climate resembles the Orkneys far more than the Amazon. In that WET place some extraordinary preserving of artifacts has gone on -- this is all quite unusual. For our purposes here there are but four things to say about Chinchihuapi. First, the stuff was well preserved and stratified and 18 radio-carbon dates were obtained. Second, the clear remains of stone tools, WOODEN ARTIFACTS, houses, human footprints and a "hunk of mastodon flesh" leave no doubt that it is a human settlement. Third, that settlement is about 13,000 years old or 11,000 BC, which can be seen as giving these folks a comfortable 2000 years to make it down from Alaska of 13,000 BC (the Paleoarctic tradition viewed as Amerind). But, fourth, that "Five feet deeper we found features and modified stones that belong to an even earlier culture." That earlier culture he dates to 32,000 BC. Dillehay had already argued that the 11,000 BC culture was more a food gathering than a big game hunting one and likely to be earlier than the Clovis point type of culture associated with the conservative first Amerind date of 12,000 BC. He goes on to say: Quote: "In the deeper, sandy levels, where the preservation of organic remains is poor, we found three apparent hearths containing charcoal. Radiocarbon dating of the charcoal showed it to be about 34,000 years old. Associated with the hearths were twenty-four fractured pebbles, seven of which have clear percussion scars. Four of these stones exhibit polish and striations on their sharp edges, resulting from scraping and cutting plants, hide and meat. There is no reason to believe these artifacts filtered down from above, since the strata between them and the higher occupation level of the site are culturally 'sterile', and there is no evidence of geological disturbance." But, he goes on to say, his results have been greeted with scepticism. He is in Scotland now, so I couldn't discuss his most recent thoughts on the matter with him.

I take Dillehay's work as an omen that Fagan, Kennedey, MacNeish and Gorman are going to win this argument. If I find some good tasty Yupik blubber, it will go into the freezer for the nonce.

Australia and New Guinea are not so difficult. Both places were inaccessible, except by sea, throughout the Pleistocene with its rising and falling sea levels. Australia has just one phylum of languages on it. New Guinea has two, both with their other feet in Sunda-land. Amazingly, Tasmanian at the bitter end of the whole bloody world does not fall within the Australian phylum but within one to the north and west of Australia! (There are quite a

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few and quite diverse Tasmanian languages, or there were). Although New Guinea has archeological dates as ancient as 7000 BC for AGRICULTURE (!), those could be associated with Austronesian because of the crop connections to Southeast Asia. First settlement of New Guinea must be much earlier than that but, at the moment, I cannot find the relevant dates. (See below for a fossil person of 25,000 BC in Papua.) Since Indo-Pacific is most powerfully focused on New Guinea, we have to look elsewhere for some clues to its age. We find them far to the south.

Tasmania itself was settled for sure by 21,000 BC (a dated site) but possibly much earlier. Fagan thinks that date could be 28,000 BC. Presumably the Tasmanian date has something to do with Indo-Pacific, since it is part of Greenberg's Indo-Pacific phylum. What is most arresting is the archeological heuristic contained in Fagan's further remarks about the fossil cultures of Tasmania vis-a-vis Australia: Quote: "They had no hafted tools (that is tools composed of stone heads or points with wooden shafts or handles) and relied instead on scrapers and choppers somewhat like those used by early hunter-gatherers on the mainland; they lacked the boomerangs, spear-throwers, shields, axes, adzes, and lightweight stone tools the Australians of the mainland had when they first entered written history. Tasmania was settled when it was attached to the mainland during the Weichsel glaciation, but many of the earliest sites are probably buried under the sea.....The result of Tasmania's isolation was that its population, although forming part of the Australian cultural group, never received the later cultural innovations that spread through Australia after the sea levels rose." Unquote. (One Weichsel glaciation lasted from 28,000 BC to 8000 BC. The sea levels rose around 8000 BC, therefore, although an earlier rise in sea levels had occurred between 40,000 and 28,000 BC - HF)

The heuristic here is NOT about the sea levels because seacraft were ALWAYS necessary for the settling of Australia and New Guinea. Rather the conclusion that Tasmania was settled from Australia and that its fossil cultures resemble those of Australia is the arresting heuristic. One would have thought that obvious geographically. But when Tasmanian languages were classified as Indo-Pacific, not Australian, they suddenly became harder to derive from a whole continent packed wall to wall with aliens. The question of how they got to Tasmania FROM NEW GUINEA arises naturally in one's mind. Now we are virtually forced to think that Indo-Pacific PRECEDED AUSTRALIAN in the great dry continent or at least part of it. What is even more remarkable is that this conclusion was the one drawn by Swadesh in his "Linguistic Waves of 25,000 BC" map ! (Cf Circular.2 again). Was he prescient or are we both wrong? Another and simpler conclusion would be to suspect that both Swadesh and Greenberg are wrong about Tasmanian; maybe it IS related to Australian. Can we get another opinion on the matter? Does anybody else know anything about this problem?

But this "peopling of the Pacific" question is not finished. Since both Indo-Pacific and Australian are both involved now in the earliest dates, the first human settling of the Pacific -- beyond the deep water lines around eastern Indonesia -- has an implication for the date of Mother Tongue. Well, people got to Tasmania by at least 21,000 BC. Fagan reckons that Australia was settled pretty early: Quote: "The earliest traces of human settlement in Australia date to far earlier than 30,000 years ago; indeed they are near the outer limits of radiocarbon dating (earlier than 60,000 BC)." Unquote. Then he goes into a discussion of sea levels which I do not think is crucial for the peopling of the Pacific. Then there are "cut and charred animal bones" found in southwest Australia "more than 37,000 years ago". Then proper fossil cultural

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remains can be found again in the southwest around 29,000 BC. That site, Devil's Lair, has continuous occupancy until 3000 BC. Mammoth Cave nearby has occupancy from circa 33,000 BC until European contact! Cultural change seems to be extremely slow by comparison to other parts of the world. Another site (Koonalda), a long way to the east near Melbourne, started around 20,000 BC.

I'm using Fagan for convenient summary. There is other testimony. For example, our friend Kennedy discusses physically modern people (*Homo sap. sap.*) being present in various parts of Australia with dates from about 31,000 to 19,000 BC at twelve different sites. She includes a date of about 25,000 BC from Papua too. Her estimate is that man entered Australia "at least 40,000 years ago". There is also the interesting finds from Kow Swamp (Victoria) of a much more archaic population, similar to *Homo erectus*, yet dated to a mere 8000 BC! That strange anomaly may turn out to be very important. Both Kennedy and Fagan call attention to the presence of modern *Homo s.s.* in Borneo (Niah Cave), dated 39,500 BC, about whom Kennedy says: Quote: "This individual, an adult female, was very delicate in build and resembled some modern southeast Asians, particularly those on New Guinea..." Unquote. Finally, Bellwood (PEOPLING OF THE PACIFIC) puts the earliest peopling of Australia and New Guinea at 40,000 BC. Of course, I originally heard about these early dates from Mulvaney's work, which characteristically I cannot find at the moment.

Bernard Campbell thinks the Kow Swamp people of Australia, who have links to Solo man of Java, might show that the transition to modern people occurred in Sunda-land, far from the Neanderthal-Cro-Magnon problem of Europe. It is clear that these Sunda-land fossils and possibly the controversial ones of North America are older than Cro-Magnon of Europe. To put it more baldly -- modern man comes later in Europe than he does in Southeast Asia and Sunda-land, and probably Australia too. That is, if you don't consider Neanderthal as modern man. Heavens! Everybody's basic training in anthropology is being overturned! Cro-Magnons of Europe not the first modern men! Those primitives of Australia possibly modern longer than Europeans! This is all too too shocking for words!

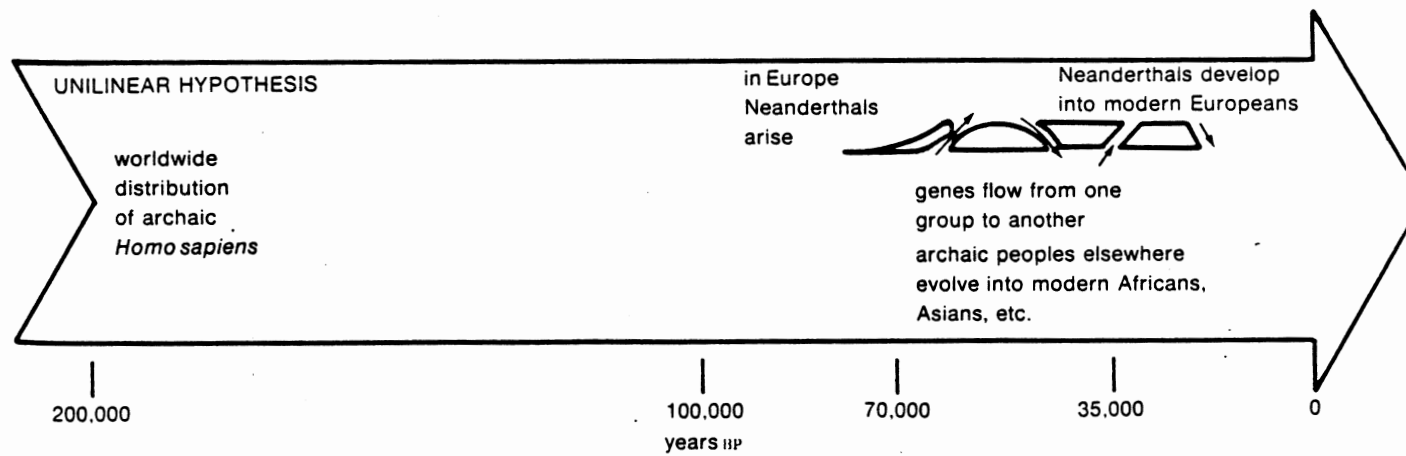
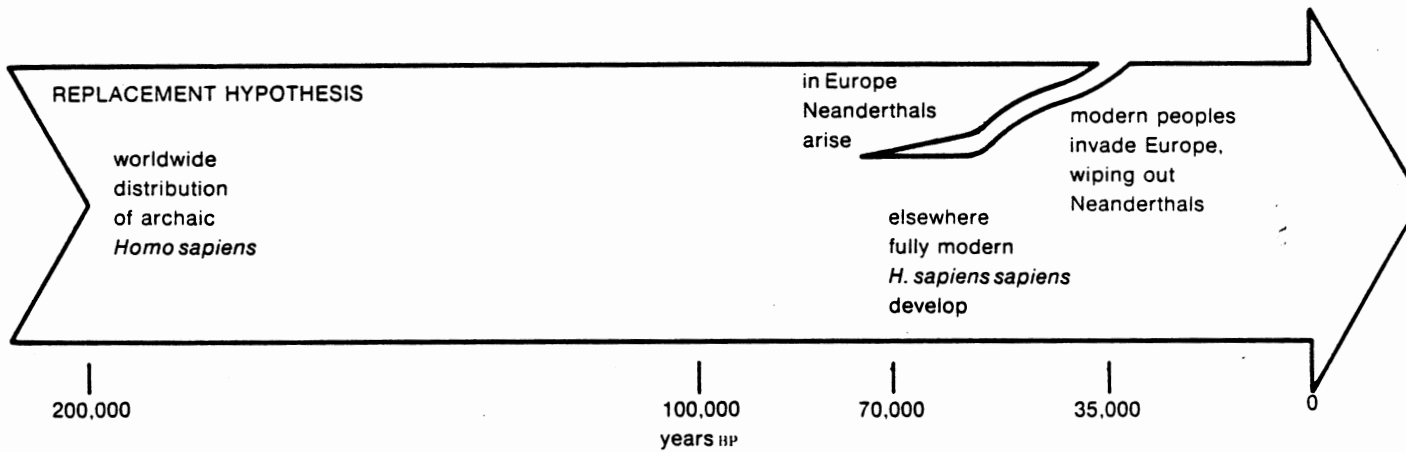
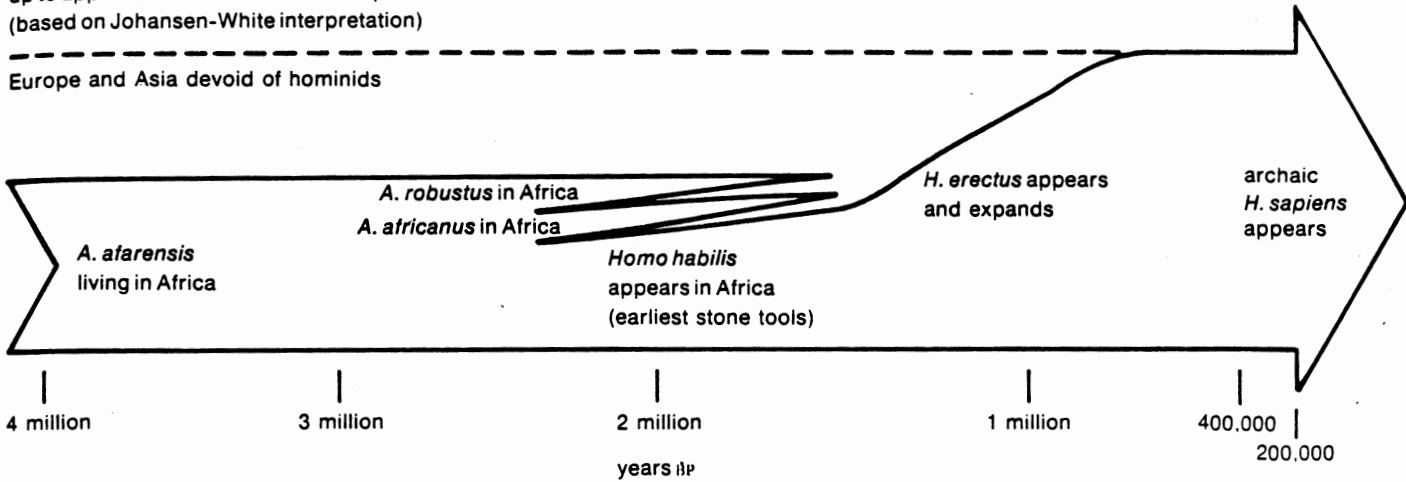
I conclude that fully modern human beings carried two linguistic phyla into the Pacific around 40,000 BC. Fully modern human beings carried between one and thirteen linguistic phyla into North America by 13,000 BC certainly and possibly as early as 20,000 BC. Na-Dene followed at a later date and Eskimoan followed still later.

One segment of Indo-Pacific made it to Tasmania by 21,000 BC or earlier. The chance that the Tasmanians might be derived from the Kow Swamp people of Victoria seems good but clearly undemonstrated. In any case we know that the Tasmanians could interbreed with modern man because they did. At least the same species as the British with whom they interbred. Either that makes the Tasmanians fully modern human beings or it shows that archaic perhaps even *Homo erectus* types could interbreed with modern men from the extreme opposite end of the Old World. If so, then why not Neanderthals too? One should also mention that British behavior toward the Tasmanians itself showed a lot of genuine primitiveness, i.e., basic savagery; they allegedly killed the Tasmanians off!

Almost as importantly, we see that New Guinea, which is very likely to have been settled initially by Indo-Pacific speakers, has human dates as early as 25,000 BC at least. Since the Tasmanians are derived from Indo-Pacific, which appears settled in New Guinea, then it seems that we have about 23,000 years of time depth in Indo-Pacific AT LEAST! Please follow this reasoning. The advent of Indo-Pacific in New Guinea is a date for the presence of language in the Pacific; it does not date proto-Indo-Pacific (p-I-P). That dates from the splitting up of the several branches. Tasmanian gives us that date because by

HOMINID EVOLUTION
up to appearance of archaic *Homo sapiens*
(based on Johansen-White interpretation)

Europe and Asia devoid of hominids



The place of Neanderthals. Time line represents hominid evolution and two theories of Neanderthals' place in the line to modern Homo: In the unilinear hypothesis, they are in the main stream; in the replacement hypothesis, they were bypassed.

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23,000 years ago it was several thousand miles south of its Papuan kinfolk, i.e., it had split off from p-I-P, or the Papuans had. (I had better pray that Greenberg's hypothesis stands up to test!)

Then there are branches of Indo-Pacific in Halmahera (not too far away), Timor (over 1000 miles west of New Guinea) and the Andaman Islands (not less than 3600 miles west of New Guinea and very far from Tasmania). Andamanese and Tasmanian are both major sub-phyla, along with the greater New Guinea crowd, by Greenberg's reckoning. So their times of splitting off tend to date the phylum at large. I am content to argue that Indo-Pacific has probably 25,000 years of time depth in it. It is a very large phylum with great internal diversity, indeed a model for thinking about Amerind.

Australian is associated with 40,000 BC but not with respect to its ancestor. Rather the 42,000 years ago represent the phylum's arrival in the new continent, i.e., evidence for the age of language in the Pacific, and a minimum date for its separation from whatever kin it left behind in Indonesia. Were it related to Indo-Pacific one might see 42,000 as the time depth of the split between the two great phyla. Intuitively we might all agree that a large part of those 42,000 years go towards calculating the date of proto-Australian. I would not be surprised if both Kenneth Hale and Paul Black were to tell us that proto-Australian is off the charts, too old for glottochronology. But because there is reason to believe that Tasmanian occupied part of Australia for some time, then we have to suspect that Australian was confined for part of its history to a smaller part of Australia from which it later expanded to occupy all of Australia. But it is not only Tasmanian which would make us cautious but also that bunch of "archaic" folk in Victoria in 10,000 BC. They suggest that there were two different populations in Australia as late as 12,000 years ago. But who would doubt it if we drew a mild conclusion that proto-Australian has probably got a minimum of 12,000 years time depth in it and quite likely upwards of 25,000 years?

Penultimately, we seem also to have a basis in North America for saying that Amerind is comparable with Indo-Pacific in diversity, but not necessarily in time depth. Although they are very strongly opposed by many archeologists, the moderate centrist dates of Fagan suggest that Amerind has better than 22,000 years in its differentiation into most of the languages of Canada and the USA, and all those of Meso-America and nearly all of South America. (Some of them were left unclassified because of lack of data.) Come to think of it, those unclassified South American languages have the same consequence that the Kow Swamp people had in Australia. Not all the time of human occupancy necessarily belongs to the one phylum. Oh, damn! This topic can get difficult!

Morris Swadesh did make allowances for "lost languages" in Australia and South America, and only in those two places. (See Circular.2 again) If one thinks hard about the logic of what we are doing, one cannot help cursing those hypothetical lost languages. Their presence effectively vitiates the premise of archeological correlation that Greenberg has held and that we have been pursuing. If all the languages of the two southern continents belong to their two respective phyla, then all the archeological time belongs to those phyla too, i.e., the time of the entry of first modern humanity into those vast domains is also the time of entry of the phylum associated with each continent. But with "lost languages" the first date of human entry is the time of entry of some lost language, NOT the well-known phylum that in our time is in total possession of the continent. Its date of entry is "some time later". Malheureusement, in Australia there is good reason to believe that the great Australian phylum did not deflower a virgin continent. But some happiness is

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returned to us by Tasmanian. In South America? Well, Morris, just to be ornery, I don't think I believe in any lost languages! (At least it's an easy way to solve a nasty little problem!)

Final conclusion. Language seems to go back at least 42,000 years in the Pacific basin alone. Since the other 24 phyla in the Old World are still unaccounted for, we seem to need more time to get back to Mother Tongue. How much more time? Lord almighty, who knows? Purely intuitively, my personal hunch would be -- 130,000 years -- to get back to the early Homo sapiens in Ethiopia. In any case Mother Tongue is arguably much much older than most of us seem to think. And I do believe that those 25,000 BC dates for Mother Tongue are simply false because they have been falsified in the Pacific Basin.

TEA/COFFEE

GENERAL THEORY, PHILOSOPHERS OF SCIENCE & TYPOLOGY

Someone undoubtedly was reminded of Sir Karl Popper in that last sentence above. The falsification bit derives from the philosophy of science, most particularly the thinking of Sir Karl. Science advances by discovering truth by exposing falsehood. The good stuff is what doesn't get falsified. So get rid of the chaff and the deadwood. It's a great simplification of the complexities of logical empiricism, the Vienna Circle, Reichenbach, Carl Hempel (my own teacher), and so weiter. Sir Karl was no plodding inductivist, carefully marshalling his evidence and advancing another quarter of a snitch towards a higher level theory. (Sorry, that is very idiomatic American English. A quarter of a snitch roughly equals a very tiny amount.)

A Popperian scientist is allowed to think big thoughts, be deductive and to remember that theories come from the heads of scientists rather than arising like vapors from the smoldering data. But he must then TRY HARD to disprove the theories he invents, otherwise he will never know whether they are true or merely fragments of his imagination. Few of us are able to do this well; it is too much like going to the dentist. So that is what colleagues are for; it hurts them far less to disprove my theory than it does me!

Lovely stuff, this philosophy. But the question comes up from time to time about the relevance of it. Philosophers of science often say that they merely report in general terms what working scientists actually do and then try to help by clarifying things. But most of them get their knowledge of how real science proceeds from physics. Few of them (except Hempel) convince me that they know or care very much about historical sciences, except maybe astronomy which is part of physics. But still even that synchronically-oriented physics model can help us with one of our persistent problems -- typology. What these philosophers are really good at, in my opinion, is clarification of concepts and the logic of hypotheses. (We also have serious social problems but there history and the social sciences can help us more than the physics model can.)

Typology exists in social anthropology (e.g., Weber), archeology and physical anthropology (e.g., some race classifications in Europe), but I am only going to cope with the case of linguistics where it is often defined as a problem. Why is it a problem? Because "typological classifications" have some times been confused with "genetic classifications". Or perhaps the problem is that people have used the label "typology" but have not understood what it is; the term itself is the source of most of the problem.

There is one sense of the term where "typology" simply means "taxonomy or classification". There is another sense of it where it takes on meanings like "essence" or "Platonic essence" or "archtype". There is a third sense where it only means "grammatical or phonological features". The first sense in fact includes genetic taxonomy as a subset, because genetic taxonomy is a

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member of the general set, taxonomy, along with all the other things that can be used as the basis of a classification. For example, many African linguistic classifications have used culture, physical appearance, and geography as their bases. The second sense perhaps is best exemplified by the Nordic Man, as ridiculed by Hilaire Beloc ("Behold, my son, the Nordic Man. And be just like him if you can..."). The third sense seems to be indistinguishable from the general operations of descriptive and/or theoretical linguistics, at least in its Bloomfieldian or surface structure phase. We use the non-historical comparative method, common to most scientific activity, and classify things into similar, dissimilar and something else; then see what else correlates with those classes. When do you get languages with short words, many tones but few grammemes? Under what conditions do languages with grammatical gender occur? How come Japanese, Amharic and Javanese have so many pronouns? Is the order SOV universal? Und so weiter. You can repeat the method with a TG orientation too, e.g., what are the properties of languages that lack a passive transformation or are all sentences of the type : S ---> NP VP. ?

It seems obvious that typology in the third sense, which is the sense most contemporary linguists use the term, does different things than a genetic classification and should therefore not be confused with one. Nor should typology in the third sense be substituted for genetic taxonomy -- the results can be disastrous (e.g., Africa). But, since genetically related languages often have much in common grammatically and phonologically, typological taxa will overlap with genetic taxa a lot. As linguistic heuristics, typological taxa are swell, as long as we remember that they can mislead us, can be disheuristics, and as long as we remember that the relationships which they deny may be the true ones (e.g., Munda versus Vietnamese, Mbugu versus Iraqw) or the one which they assert may be the false one (e.g., Kusunda versus Austroasiatic, Thai versus Chinese). We could even work out a typology of disheuristics. The conditions under which linguistic disheuristics occur usually (always? often?) involve areal phenomena and borrowing and/or bilingualism.

Enough on this. My hunch is that everybody already knows all this.

SURPRISE LIQUER. JUST WHAT ARE WE ALL ABOUT, ANYWAY ?

An item not previously on the menu. A fine bottle of Drambuie has been discovered and added to the meal.

There is a serious risk that we may not understand each other, as in what are we about, what are we after, why have a Long Range Comparisons Club? So long as the name MOTHER TONGUE is used people may think that we assume that all languages can be genetically related to each other, that the mysteries of the origin(s) of human language(s) can be solved, and that we will be able to reconstruct an entity called proto-human language (mother tongue). N'est-ce pas?

Let me speak for some of us. The answers to the above questions are: NO, NO, NO! Some of us do not assume any such things. Rather we want to TRY TO FIND ANSWERS, WANT TO SEE HOW FAR WE CAN GO, but make no assumption that we will have success, either in our lifetimes or in anybody's.

Some others of us DO make the assumptions first mentioned and it was their enthusiasm that inspired the long discussions in the first sections of this circular.

Still others of us do NOT wish to try to find answers THEMSELVES for the seemingly eternal questions we first started with. However, we don't mind if the rest of us make the efforts and we will be interested to learn the results of our efforts. Many of these us have plenty of other work to do or

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were not trained to do long range comparisons in linguistics.

Finally, still others of us DISAPPROVE of the efforts the rest of us are making and are CERTAIN THAT NOTHING WILL COME OF IT. The methodology of long range comparisons is, I suspect, offensive to some of us, contrary to our high-quality, indeed magisterial, command of our discipline and contrary to the teachings of our gurus. For reasons of friendship, politeness, or curiosity we do not terminate our association with the Club but that is all that keeps us.

Now that Ruhlen's book has come out, Greenberg's is either out or just about to come out, Baldi's "workshop" at Stanford is only three and a half months away, et al, it seems certain that discussions on this our topic will become more frequent, more public and more heated. The Rice conference last year produced vigorous disagreement, according to both Carl Hodge and John Bengtson. Ruhlen's article in Natural History produced several strong critiques of it and equally strong rebuttals. (I do not feel at liberty to tell you all the particulars. Merritt may decide to do so, however.)

Well, strong criticisms and rebuttals, disagreements, intensity and heat, these are all expected from scholars who are not of the same mind on any particular topic. From the standpoint of the science of human prehistory this is all to the good because some things will get falsified but some things will be strengthened and in the end we will be richer for it.

However, there is another consideration which I will call the BORDER PATROLS. Among the Americanists a mindset is said to exist which is strong on hostility to long range comparisons, to Swadesh, and ultimately to Sapir. It seems not to be interested in discussion or empirical investigation of areas of disagreement but rather in stamping out the impulse towards deeper prehistory. This does not sound like a scientific approach to me but smacks much more of IDEOLOGY, RELIGION, or METAPHYSICS. That is why I use "border patrols" instead of "gate keepers" because those guys are prepared to get tough with people who try to cross their borders, violate their turf or fail to get proper visas from them. This is all hearsay to me, of course, because I have had just about nothing to do with Amerind linguistics. Africanists and Orientalists (Middle East to India) have their own problems and, yes, some Semiticists act like Border Patrols, but none of this seems to reach the high level of ideological conformity that Americanists talk about or outsiders mention as being particularly characteristic of Amerind linguistics. Come to think of it, it is also said to be the case that Indoeuropean linguistics does a lot of border patrolling. Maybe it is really a universal phenomenon; people hate to see their paradigms threatened. Why does this remind me of old Puritan Boston?

I see two critical aspects to all these considerations. First, there is the danger that people will fall into ideological or stylistic camps and carp at each other endlessly, until the initial impetus of this latest surge towards Mother Tongue gets lost in scholarly bickering. Second, there is the chance that some of us may try to PREVENT others of us from EVEN TRYING because we don't think that success can possibly be attained. Or some of us may denigrate the KIND of evidence or data presented to support a particular hypothesis, not by examining the data or the hypothesis but just by denying that that KIND of data can prove anything. The last version of the DON'T EVEN TRY attitude is the "we need more data" syndrome. Some times we could use more data, as in some South American cases or the Himalayas, but that is not what the syndrome is all about. It is simply a stalling tactic. I have one colleague who has so much data he can scarcely read it all but still insists that he needs more. He just likes to gather data!

Then there is the "wait until each proto-language along our way has been reconstructed" kind of attitude. That is willing to try but wants to rely

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on what it thinks is stronger data. Dolgopolsky, Hodge and I have, or have had, this attitude. It might be confused with the "we need more data" syndrome but it is basically different. Perhaps it is sounder than the aptly named MASS COMPARISON METHOD of Greenberg (and Swadesh). This last could also be called the Gung-ho or "go for it!" approach. I say that with RESPECT because it was the method which set up Greenberg's African classification.

To the DON'T EVEN TRY group. Let me beg you not to act like Border Patrollers but to acknowledge that the final outcome is REALLY AN EMPIRICAL QUESTION. If you do not think Mother Tongue can be achieved and it is foolish even to try, then by all means let that inhibit YOUR work. But please do not insist that no one else can make any progress in the direction of Mother Tongue. For example, Mother Tongue seems unattainable to you but someone else might succeed in joining, say, Uralic + Yukaghir or Altaic + Japanese + Korean + Ainu. After all, there is a whale of a lot of difference between showing that Ainu, Korean, and Japanese are in Altaic (or link up with Altaic) and reconstructing proto-human or even linking N-K with I-P ! Do be cautious, dear colleagues, but do be flexible too!

To the several varieties of WILLING TO TRY. Let me beg you not to get lost in methodological disputes and the merits of various writing systems and so weiter. Simply let us not carp each other to death! I say "let us.." because obviously I am in this group. Let us not think that one person's failure to show a new connection CONVINCINGLY means that no one can show it. We already have examples of colleagues who have failed to convince their peers. Let us NOT YELL at those who do things incompetently. It does hurt to see a poor presentation of the case for a hypothesis which you believe in. But let us not try to blow the other guy away. Try to show her/him how to do it convincingly. This stuff is fairly hard to teach and fairly hard to understand and some of our colleagues learned faulty methods of doing it. Everyone you blow away is one LESS of an already small minority of scholars who are willing to try. Many of the reconstructions I see in Africa, for example, are not up to Anttila's standards but Africanists are quite a heterogeneous lot, especially in Afroasiatics, and simply have had different training one from the other. And some times they use methods "improperly" but come up with the right answers anyway! (How could that be?)

It is probably salutary to take a more Popperian view of methodology. Linguistics, particularly in its well-known Bloomfieldian or "operationism" phase, has had more "methodology freaks" than any field except sociology and experimental psych. We owe it to Noam Chomsky for reminding us that we had mistaken PROCEDURE for PROBLEM, and METHOD for THEORY. (Pace, oh tribe of Bloomfield! I am neither a Chomskyite nor a Bloomfieldian. I value each approach for some things.) No one in their right mind would prefer a bad method to a good one. My point is that one cannot refute a hypothesis by claiming that its proponent is a methodological nonconformist. And one cannot say, as once a colleague argued in Koln, that one's hypothesis is true because "I followed all the proper procedures and standard methods". Well, one can say it but that doesn't make it a good argument. (Maybe we can get a physicist's view. Ron?)

Finally, we who are willing to try ought not to condemn those who fail to be convinced by our efforts. Let us be more tolerant! Sometimes we really are NOT convincing. And more often what it takes to convince one woman is quite different from what it takes to convince another woman. Although he will always be kind and polite, Juho Janhunen will require more convincing than Hal Fleming -- always. (More on his letter later.)

For those Border Patrollers who try to stomp out our inquiries through "administrative" (gate-keeping) acts, well, we must fight them -- vigorously.

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DESSERT

COMMENTS FROM VARIOUS PEOPLE

Even though I enjoyed writing the last 21 pages, most of you-all will probably be more interested in the comments of the other members of the Club. Roger Westcott made an amusing suggestion and I'd like to adopt it. It's so apt and short!. Let us call us the "Long Rangers". For those not addicted to American movies, the Lone Ranger is a heroic cowboy or cavalier figure who rides the ranges of the West with his faithful Indian companion, Tonto, giving justice to bad guys and help to good folks. (My kids call him the Stoned Stranger.) A large chorus of BOOS for this suggestion will CANCEL it fast!

General policy on letters will be that personal stuff is always excluded here and that written matter may otherwise be summarized or extrapolated or reproduced in full. But there has to be permission for the last. My editor-wife tells me that I should be more careful about copy-right laws because this newsletter qualifies legally as a kind of publication. I hope Aaron and Vitaliy don't sue me!

A few letters are reproduced in full, not because they are necessarily superior, but because explicit permission was given. Some material on the other hand was so copious (e.g., Pia, Bennett) that summaries are all that I can do. In other cases a lot of material was received in the form of reprints -- too much to reproduce here. Some reprints and data are sooner or later going to be reproduced herein but at the moment I'm saving the space for our Soviet colleagues. Some of the reprints are quite valuable, especially those from Hans Mukarovsky which illustrate his work on Basque and its external relations.

Future issues will be MOSTLY BASED on letters, new data and reprints which authors want reproduced. That is, unless I feel a need to talk to the paleoanthropologists and biologists and archeologists again. Since I am a 4-fields anthropologist, I will not willingly turn us into a linguistic club. And I wish the archeologists and others would understand that most of the linguistic Long Rangers want very much to talk to them. Even if you don't say much of anything for a long time, still some day you may want to put in your two cents worth -- and we will want to hear you!

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- LIONEL BENDER. Friendly, helpful. Wondered if the /sn/ forms for "nose, smell" might not be onomatopoeic. (2nd). Friendly, more helpful even. Suggests we have a conference. (3rd) Some \$\$ for postage (thanks!) plus editorial help.
- A. MURTONEN. Friendly, polite. Sure that /sn/ forms were onomatopoeic. Numbers, like "4", are of no value in comparative work. Needs help with publishing his book on West Semitic.
- CARL HODGE. Friendly, worried about quality of Illich-Svitich's etymologies, bit discouraged by opposition to Nostratic, has lots of IE and AA etymologies, will try to publish them, discussed different interpretations of "4" and "nose, smell", interesting ideas on IE/AA sound laws, & much else. Carl definitely is one of our founding fathers.
- GROVER HUDSON. Friendly, helpful. Has done a lot of work on proto-Highland East Cushitic. Can't do much long range stuff right now because hot on theoretical topic but has some notions on null hypotheses. Helped me get \$\$ from my own university for postage!
- PAUL NEWMAN. Busy, polite. Can't do anything right now or later. Still has to fight off attacks on Chadic by French Semiticist!.
- JOHN BENGTON. Friendly, enthusiasm. Lots of information on Rice conference, two moieties and energetic but friendly debates there. Mentioned Trombetti's work which included /su/ "generate" in Khoisan and elsewhere. Also suggests Indonesian /empat/ be added to the "4" list. Gave me several names of "team

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members". Now actively pursuing and finding global cognates, one of which "one, finger, hand" is very good. (Ruhlen, Shevoroshkin mention this same cognate and it is to be published in Greenberg's Amerind, as it already has just been in Ruhlen's book). John has 20 years worth of investment in long range comparisons and has amassed a lot of etymologies! Indeed, he should be considered one of our founding fathers. (What I call his "Ticktacktoe" cognate will be reproduced at the first convenient place. It was discovered by both Ruhlen and Greenberg independently of each other and John, also by Swadesh!

IGOR DIAKONOV. Friendly, interested. Has books to finish on: "Afrasian Languages" (due out 1987) and "Proto-Afrasian and Old Akkadian: A Study in Historical Phonetics", with sample comparative vocabulary (due out 1987 or 1988). (That he is the ranking senior Afrasianist in the world today probably would not be contested. Encyclopedia Britannica agrees). He has heard that I think Omotic is not Afrasian and hopes that is not true because he sees at least Yamma (Janjero) grammar as Cushitic. He mentions difficulties in finding my work because of library cutbacks on journals. (Let me use this opportunity to urge colleagues who have reprints to send them to Diakonov. My library has problems of this sort frequently, but of course I can swim across the Charles River to the Harvard Library. That is like having the Moscow State Library just 3 miles away.)

ALICE HARRIS. Hurriedly, from deep in the Caucasus, she can't reply yet.

ROGER WESTCOTT. Friendly, interested. Mentioned already-existing "team" focused on Stanford and enclosed interesting reprints of his reconstructive work.

PAT BENNETT. Long four-page letter, small type, single-spaced, full of rich material, full of experience in teaching historical linguistics, full of cautions and precautions, basically leery of long range comparisons but willing to let us try, willing to comment on things involving his areas of experience and/or training which greatly exceeds N-K (includes AA, N-S, IE). I couldn't photocopy his letter because the private stuff interthreaded with his many hypotheses. (I'm sure he wouldn't mind my repeating that his hair is standing on end because of what I'm trying to do.). Pat, my friend, you are exactly what Aaron wanted our networkers to be -- competent, intelligent, totally honest, critical but helpful, and tolerant of us Long Rangers. What I will do with the problem of presenting Pat's opinions, since he is far too occupied elsewhere to do it for us, is in the next (4th) issue to summarize his (de-privatized) letter.

HANS SASSE. Friendly, polite, willing to let us try, willing to swap data and make comments on proposed cognates which involve Cushitic connections. Also has a general view, Quote. "throughout my career I have been a reductionist, preaching the ideal of examining smaller genetically unproblematic groups as thoroughly as possible before touching on farther-reaching relationships. After approximately 20 years of comparative research, I am still within the borders of Eastern Cushitic and I still am confronted with lots of problems in that field. I can see some ways leading to Proto-Cushitic., but there is still a lot of work to do until we can reach there. Not to speak of Afroasiatic as a whole. Consequently, for my taste genetic hypotheses linking together entire language phyla are something entirely inconceivable given the present state of knowledge. Nevertheless, I am willing to participate in your swapping network in order to help dissemination of information, especially along the west-east (and vice versa) axis... (Lots of work coming out on Cushitic in a series, edited by Hans, called "CUSHITIC LANGUAGE STUDIES", Hamburg: Buske).. please feel free to consult me if you (or someone else in the network) want any kind of

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information on things I might possibly know..." Unquote. I can certify that one can get some high quality data in a swap with Hans. Since Hans stressed it, I should remind Soviet scholars that he clearly means that he will swap with THEM.

BEN ROUSE. Friendly, interested. Glad I liked his book, pleased I understood his basic aims. He follows Sapir's good advice in not spreading oneself too thin. Work on archeology and linguistics in ONE area, like the Caribbean in his case, but stick to one DISCIPLINE if you're trying to work globally.

SHEILA EMBLETON. Friendly, received two circulars via networkers, please add me to the network. (2nd) She will be a 2ndary mailer of 12 letters (thanks!)

HANS MUKAROVSKY. Viennese (= friendly, warm, generous), hoping we all can meet at the Congress of Hamito-Semitic Studies, meeting in Vienna Sept.27-Oct.2, 1987. At least 15 of us (Long Rangers) will be there. (Including a few who would spit up at being called a Long Ranger! - HF) Five of our Soviet colleagues will be giving lectures (= plenary session papers in US parlance). (Having been subjected to Mukarovsky's hospitality in 1982, I desperately WISH I could go this year but so far I haven't been able to manage it.). Hans was also concerned, because of the post-glacial phyla differentiation (Swadesh's), shown in Circular.2, that, Quote: "That clearly implies that you still think that Basque, contrary to the view some Russians you quote hold, might have something to do with the group they newly name Dene-Caucasian, while it is clearly far away from Hamito-Semitic, and even closer to IE. But quite the opposite is the case! Your view simply ignores a lot of writing -- perhaps a dozen publications or so -- of mine since 1963! Certainly I am, at least in part, responsible myself for your ignorance, as I never sent you one of these articles, but simply because I never anticipated that they might interest you!" Unquote. My sympathy is entirely with Hans. If he thought I said that, then he should protest; certainly I appeared to be embedded in Omotic. However, that post-ice diagram was all Swadesh, not me. I have no opinion on Basque, except that in 3 FRAIL efforts at classifying it, I couldn't get anywhere. It is obviously no close kin of anyone's. And, since it does represent Atlantic Europe before IE took over and maybe the Maghreb too (before AA moved in), it surely is a very serious matter. Its importance is underlined by another serogenetic heuristic -- the Basque are a world climax point for Rhesus negative, while the Mountain Berbers are another for N. Also Basques have more N than the rest of Europe. Rest of AA folks have more M, and Semites much much more M, than Berbers and less Rhesus negative than Mountain Berbers. (This stuff is REALLY interesting!) (Highland New Guinea is the other climax point for N, and Rhesus R1). Hans goes on to say that he was sending me his reprints (since received - HF) and that maybe Basque did have Asian connections but: quote: "not in the first place with Caucasian languages (as some people tend to assume). But this is another -- very exciting -- question ...". Unquote.

JOHN C. STREET. Quote: "While I appreciate your including my name on your Nostratic mailing list, I'm really not much interested in such speculations -- so you might as well take me off the list." Unquote. (D'accord -- HF)

BRUCE TRIGGER. Friendly, interested, sceptical. Sees a revival of historical linguistics, after doldrums, and then his misgivings: Quote: "At the same time I have grave suspicions that perhaps up to 4 percent of roots in language families could be similar in form and meaning as a result of chance and the sort of reinvention phenomenon Murdock demonstrated for mama and papa. I therefore have serious reservations about even a moderate number of resemblances among major families indicating historical affinity. But I am prepared to keep an open mind." End quote. Also, since Bruce is one of my

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authorities on Meroitic, his thoughts on it can be quoted. Thus, "As for Meroitic the classification is still open. Shinnie a few years ago suggested it might be a Kordofanian language, but used only a few cognates (maybe only one). A man ... published an article in AZANIA suggesting it was Sumerian but his only good cognate was the -te locative postpositive. Hintze, as a joke, produced a paper calling attention to a lot of Altaic cognates and some have taken that seriously. ...Bechhaus-Gerst and I think Inge Hoffman still think the Meroitic-Eastern Sudanic cognates are loan words but I find this most improbable. Also they fail to note that many Meroitic-Nubian cognates are also Meroitic-Eastern Sudanic ones. Priese and others seem supportive of an Eastern Sudanic or perhaps more general Nilo-Saharan relationship and I still regard that as most likely...". End quote. Of course, as Bruce points out, Meroitic continues to have a basic problem that: "There is still not a diverse enough corpus of material to do much." And finally he welcomes the contact with Soviet scholars because Amerind-istics has benefitted from their work "in the past (Konorosoov and the Mayan tras.)" (I think tras = translations - HF). (Maybe someone at Koln can ask Marianne or Inge if they want to send their comments on Meroitic in? -- HF)

GENE GRAGG. (Telephoned). Friendly, interested. Gene is running a computerized data bank on Cushitic and Omotic languages primarily but includes some others too. More importantly, the data bank has the potential for much growth and Gene solicits cooperative responses from Long Rangers or short ones. He may also be interested in computer modem relations, where A connects with B via a special attachment plus telephone, and the two computers "talk to each other". (Joe Pia also has strong interests in this.)

DEREK NURSE. Friendly, interested, problems with wrong address. Everyone should know that Derek is now at Memorial University of Newfoundland. (1st). Too busy to say much but it sounded good to him. (2nd). ditto. (3rd). Started up a N-K sub-group to see if the five (6 ?) of them might get proto-N-K going stronger. The N-K sub-group consists of Derek, Kay Williamson, Pat Bennett, Tom Hinnebush, Gerard Phillipson, and Thilo Schadeberg. I don't know how many responded to Derek's initiative but let's hope! There is hardly anything more important than a reasonably well-crafted proto-N-K, unless it is proto-N-S. Maybe you-all should know that Stanley Cushingam and John Hutchison are also on the list. (Stanley Cushingam's correct address is : 345 Elsworth Avenue, New Haven, Conn. 06511, USA)

NEIL SKINNER. Friendly, busy with comparative work on Hausa lexicon. Hausa has /ji/ for "smell, feel, hear", /hanci/ for "nose" and /sunsun/ or /sansan/ for "sniff". Interesting discussion of other Hausa words. His wife, Meg, has a paper on "Is Bedaue a Chadic Language?". He's inclined to think that Meg showed Beja (Bedaue, Bedawie) to be more Chadic than many of the languages included in Chadic by Newman and Ma. "She never got around to publishing it, and now she works fulltime...(advising students - HF)." (This is the 5th time, I think, that someone has seen Beja as either distinct from the rest of Cushitic or closer to Chadic or as close to Chadic. Naturally, it pleases me because I think Beja is a branch of AA intermediate between Libyan (Chadoberber) and Cushitic, and Egyptian.)

HERB LEWIS. Friendly, interested. Very busy, however. Still, keep it up!

PETER UNSETH. Friendly, interested, but too much preoccupied by what he is doing to be distracted by long range comparisons. So he wants to be OFF the mailing list. Peter should, however, still be regarded by Long Rangers as a friendly resource person. He knows about Shabo (Mekeyir), Majang and southwest Ethiopia generally. He confirms the hearing of a retroflex set of consonants in Gimira AND Shako (both North Omotic), joining a group of 3

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trained linguists who have now heard that set (Klaus Wedekind, Mary Breeze, Aklilu Yilma). Master Phonetician and Magistrate Hayward will be hearing (or not hearing) the set soon in Addis Ababa. His opinion will be decisive. Why? Because two of us did NOT hear the retroflex series (Bender et moi) and so are inclined to put the whole set into "allophonic variation". It looks like we are mistaken, however. The importance of the Gimira and Shako retroflex set is considerable because it APPEARS to dictate a substantial enlargement of the sibilant/affricate set in proto-Omotic. That vis-a-vis Semitic should be very interesting. Ditto with respect to AA lateral affricates and fricatives. Perhaps one will be able to hear more about this at the Vienna meetings of AA (Hamito-Semitic) in September, especially if Wedekind, Breeze, or Hayward happen to be attending. (That's called a hint, son.)

PAUL BLACK. Friendly, interested, sceptical, helpful. Paul wrote a long very thoughtful letter which I will reproduce parts of here because he is a rare bird amongst us, having worked seriously on IE, AA, Austronesian and Australian not to mention lexicostatistics and mathematical models and computers. Some of Paul's viewpoint resembles that of Isidore Dyen and the strong faculty in linguistics at Yale but Paul is very much his own man, as they say. His focus herein is on Nostratic: Quote: (Margins increased - HF)

"Yeah, keep me abreast of developments, I want to hear more. But I'm sure you'll appreciate my position as a sceptic. It's not that I do not think that the 'Nostratic' groups are not related - it seems likely that they would be. But suppose I start with the hypothesis that most languages in the world are ultimately descended from a very small number of distinct protolanguages, perhaps just one. It would not surprise me if I did not see evidence of the relationship, because of the great time depth, but if I should see such evidence I am not surprised. The problem is thus not so much to demonstrate that the Nostratic languages are related, but rather that they are more closely related to each other than to non-Nostratic languages - i.e. to demonstrate that Nostratic is a valid subgrouping."

"I would expect this to be a difficult task. The part people are picking up on is the finding of putative cognates. But recall that it is not phonetic resemblance that makes cognates, but regular phonological correspondence. Thus Persian BAD (or something like that) does in fact mean "bad" but apparently is not cognate with English "bad". We expect cognates to tend to become phonologically dissimilar at great time depth. If you can compare reconstructed protoforms, so much the better, but this probably tends to make reliance on the phonetics even more risky, since we are even less sure of the actual phonetics of the protoforms."

"O.K., so you know the problems and we all work within them. The second part is using what cognates we find to demonstrate that the e.g. Nostratic languages are indeed more closely related to each other than to outside languages. It's hard to make lexicostatistics work at such great time depths - percentages become too small, and if you increase the number of words compared you tend to get out of basic vocabulary. Good if you can reconstruct a Nostratic pronoun paradigm, but such data is also delicate - recall from my thesis how the great changes in the Dasenech pronominal system misled Tucker. Comparison of grammatic morphemes can become too easy - where suffixes are very short (e.g. -CV), resemblance can easily be due to chance. The problems with typological comparison have been known for most of the present century."

"I'll send you a concrete illustration of the problem as soon as our photocopier is again operational; it's a Dyen ms. entitled 'BACKGROUND "NOISE" OR "EVIDENCE" IN COMPARATIVE LINGUISTICS: THE CASE OF THE AUSTRONESIAN-INDOEUROPEAN HYPOTHESIS'. After listing 78 putative cognates between (often proto-)

XXVII.

Austronesian and Indoeuropean, Dyen concludes by saying, 'I must confess that I am impressed with the extent to which I have been successful in gathering matchings between the reconstructions of the two families. Granting that thus far I have not wavered in my steadfast belief in the likelihood that an Austronesian-Indoeuropean relationship can not be demonstrated, should I waver now?' The problem for you is somewhat different: can you make a better case for IE's membership for Nostratic than Dyen (tongue-in-cheekly) does for IE plus Austronesian? Or is Austronesian part of Nostratic too? If so, what about all the other groups that you haven't yet looked at? "

"Please use my cynacism constructively by running hypotheses over it; don't let it discourage you. But do note that much putative cognate gathering has already been done, if I'm not mistaken: Rader, I believe, of Yale (of all places) was busy in the '60s or so compiling volumes of putative cognates from 'round the world."

"On your \$uw-/*suu- 'bear, begat' set, note Japanese UMU 'bear', UMARERU 'be born'. (For 'four' I wanted to add Japanese HUTA-TSU (where H is a bilabial fricative before U, there is no F), but I guess I'd have to add it twice; it only means 'two'.)"

"Australia is my current area of interest, of course, but goodness knows how hard it is to find cognates among some groups of Australian languages, much less outside Australia. Some of the best evidence linking most Australian languages are monosyllabic verbs that should look something like NYAA- 'see', KAA- 'bring', carry, MU-/WU-/YU- 'give', PU- 'hit', TU-/RU-/LU- 'cry', YA- 'go', YA- 'say', PA- 'fall', MA- 'eat', TYA- 'eat' (and TYAAWA 'mouth'), and MA- 'take'. Cognates to those wouldn't help your Nostratic hypothesis, however." Unquote.

It's unclear to me what the last sentence meant. (Paul?). Let me correct a few details. "Your hypothesis" in my dialect and Paul's can mean either "one's hypothesis" or "thy hypothesis", as in "take your average Boston politician..". If you (Paul) mean "hypothesis is thine (Hal)", then I'll have to demurr. It belongs to Aaron, Carl, Allan, and the others. I think it is probably going to be a valid genetic group -- albeit with presently indeterminate parameters -- but I have not YET made up my mind about it. Greenberg's forthcoming EURASIAN actually cuts Nostratic in half by taking AA, Kartvelian, and Dravidian out of it, and by adding Ainu, Gilyak and Eskimoan to it. The forthcoming vigorous but amicable arguments among Long Rangers should have fruitful Popperian effects and illustrate dramatically Paul's point in his 1st paragraph, last sentence. Two other TRIVIA: the "bear, generate" root is SU7 or \$U7. Syllable or root final glottal stops are non-trivial in Omotic, matching up as they sometimes do with pharyngeals elsewhere in AA or coming from underlying glottalized consonants. (Similarly, the "nose, smell" cognate has a final T' in two phyla/sub-phyla (North Caucasian + Hurro-Urartean and Omotic); it's not just SN, but SUNT'. Also shows up as SU~7 in Khoisan and apparently with a final -G in some Siberian languages.) Finally, of Paul's 11 Australian verb roots 3 immediately match up with common or old AA forms ("say, go, eat") and 4 if MA- 'take' is allowed to match up with IM- 'give' in Omotic. North Caucasian, Elamitic, and Burushaski cognates for "see" also leap to mind. This may also prove Paul's point that "putative" cognates can be amassed. Did he say "easily"? Easily and in great numbers? (I know that this question is going to be DEBATED! There is so much to comment on here, but we must wait until next issue.)

ALEXANDRA AIHENVALD. Friendly, reassuring because one of two earliest responses from Soviet colleagues. Working on proto-Berber, including Guanche in it. Went to Bundesrepublik to a conference. (Hurray! Yippie! All right! - HF)

(sũ?)

XXVIII.

JOHN BENGTON's "Ticktacktoe" cognate. (Ruhlen has a nearly identical set, which you can read in his book.) John's << "Selected Global Cognates: Toward Sound Correspondences", presented at the Third Biennial Symposium on Linguistics and Semiotics, March 1986, Rice University, Houston, Texas :

TEK / TEK' "finger/toe, hand/foot" from which numerals: "one, ten, twenty" :

Nilo-Saharan: Maba /tek/, Dinka /tok/ "1", Merarit /tok/, Dendje /dok/ "10".

Indo-European: *dek- / deg-, etc. (Greek /déka, dak-tylos/, etc.).

Uralic: *tek- (Finnish /kah-dek-san/ "8", /yh-dek-san/ "9" ? from "two fingers held down", "one finger held down"; borrowing from Indo-European seems more fantastic than this.)

Altaic: *tek- (Ainu /tek/, Japanese /te/ "hand"; Kogurye /tek/ "10")

Yeniseian: *tok- "finger"

Eskimo-Chukoton: *tiki / a-tigo (Aleut /tik-laq/ "middle finger", /atgu/ *finger*)

Hokan: Karok /tiik/, Washo /it'sig/ "finger"

Chibchan: Chibcha /itigi-na/ "finger"

Arawakan: Paumari /diki/ "arm".

Ruhlen's set adds cognates from Sino-Tibetan, Miao-Yao, Austroasiatic, Austro-Tai, Indo-Pacific, Na-Dene, and culminates in (Greenberg's) Amerind /*tik/. The nice thing about the "Ticktacktoe" cognate is that it doesn't require one to follow a tortured semantic path from one form to another, nor to imagine incredible phonetic changes and arbitrary metatheses. It is quite simple!

John also reports a Trombetti global cognate for "fat, grease, oil" (Trombetti's base /sim-/) and one for "tongue" (roughly /tal/dal/til/dil/dol/). John has added much to Trombetti's original sets, including also Shevoroshkin's Nostratic reconstruction /*cim%E/ for "fat, etc.". The "tongue" set is easier for me to accept than the "fat" one. Finally, he has some interesting things to add to my earlier "Hannibal's cognate". (It was so named because Hannibal's family name was BARCA.) If John will forgive me for not reporting the first two because space is getting dear, I will add his "lightning, etc." forms, skipping his AA and IE ones as redundant. In the case of some more tenuous increments, which John excised because of METATHESIS and the existence of a different cognate, I have put them back in because metathesis in three consonant roots is more believable and in any case the resemblances are striking. (Let me remind you-all of a beautiful case of metathesis which Paul Black found in the Oromoid group of East Cushitic, to wit, Oromo /k'urt'ummi/, Busa /k'urmuc'a/, Gato /kurmuJa/, Mashile /kurmuJJa/, and Konso /murkuuJa/ = "fish".

Niger-Congo: (Bantu) Swahili /mulika/ "shine, gleam, make light" also

/ki-muli-muli/ "firefly/glowworm"

(Bantu) Rundi /murika/ "eclairer".

Sumerian: / p i r i g / "be bright"

Altaic: Korean /p e r a k/ "flash of lightning"

Yukaghiran: Chuvan /pon-paLag-enij/ "Nebel" (where /L/ = "dark l")

Eskaleutian: Yupik /kinix-piLag/ "lightning"

Arawakan: Kinikinao /p a r u k a -ti/ "lightning"

Jamadi/Jaruara /a-b a l i k u/ "moon". Cf also Culina /abadziko/, Paumari /massiku/ "moon"

Austronesian: Bugis /b i l a k/ "lightning"

Rotti /b u l a k/ "moon, etc."

(Malay /kilap, kilat/ "lightning" << /*pilak/ ?)

Nilo-Saharan: Kanuri /mollak/ "lightning, etc."

The other set: Ainu /pekere/ "light", /pekere-a\$/ "be light", Japanese /hikar/ "light" (OJ /Fikari/), Uralian: Magyar /feher/ "white", ? Yukaghir /pozer-xo/ "light, day, world, sun", Arawakan: Campa /im-pokir-o/ "star, etc." There's

XXIX.

some chaff in this, but many good grains too!

JUHA JANHUNEN. Friendly, polite, very sceptical. Juha's letter is so strong and clear and friendly, yet cognitively or stylistically similar to many other "splitters", that all should read it. I take the liberty of breaking the rules and zeroxing it entirely, in hopes that Juha will pardon me. (Three of his family terms may be unfamiliar to non-Asianists. YENISEIC [Ket, Kot], named after the Yenisei river in western Siberia, LUORAVETLANIC [Luorawetlan, or more commonly in the USA, Chukchi-Kamchadal] or Chukchee, Koryak, and Kamchadal of the eastern point of Siberia on the Bering Sea, and NIVKHIC [Gilyak] of northern Sakhalin Island and adjacent Soviet Pacific coast near the Amur river. Most "lumpers" put Luoravetlanic in Nostratic or its equivalent and some put Gilyak there too. These three families plus YUKAGIRIC [Yukaghir] are usually found discussed under the rubric of "Paleo-Siberian", a term which is widely regarded as unfortunate because it implies that they are all related, yet the term is "merely geographical". In fact, it seems that the Nostraticists and Greenberg view them all, except Yeniseic, as related to Uralic, Altaic [including or plus Japanese, Korean, and Ainu], and IE.) Juha's letter is the first overleaf, followed by Merritt Ruhlen's.

MERRITT RUHLEN. Friendly, interested, helpful, on a roll (as Americans say). In addition to his letter, which speaks for itself, it must be mentioned that his new book has finally come out. A GUIDE TO THE WORLD'S LANGUAGES. VOLUME 1 : CLASSIFICATION. Stanford University Press, 1987. Stanford, California. Pp.433. ISBN 0-8047-1250-6. I have read it cover to cover, almost without stopping to sleep or do anything else, and already use it as an authoritative reference book. It is a TREMENDOUS CONTRIBUTION to the "lumper" cause, yet it will delight "splitters" because it shows how taxa have grown and what alternative classifications of various language groups exist and indeed where the borders are that the patrols might want to build walls around. It has many useful points of methodology and theory. Perhaps most of all, a person can find out what the taxonomic "word" is on some language she might be interested in -- seen from a "splitter's" or a "lumper's" viewpoint. Thus, it should be valuable to archeologists, social anthropologists, biologists, paleoanthropologists, even journalists. One does not have to agree with Merritt's taxa or his placement of any given language because he makes a noble effort to show what your choices are.

It is not my job to review THE GUIDE in any formal way; I'm sure that many scholars have already been assigned that task by various journals. Anyone who writes on the languages of the world in such a comprehensive manner MUST expect some criticism -- some of it sharp, some of it heavy, some painful. But that is life in this Popperian business. I doubt, nevertheless, that any criticisms of this book will do serious damage to either the book's reputation or Merritt's. It is JUST TOO DAMN GOOD, as we say in Texas. I dare say too that shortly Merritt will come to be known as Greenberg's Huxley! Yet both seem to have forgotten Swadesh's life and work; this badly confuses me.

XXXIII.

There remain many letters to discuss. Too many for now because time is running on and the envelopes will soon be too full. TIDBITS needs a page, mostly announcements, so the rest of this will be simply a list of letters with minimum comments, maybe just one item of special interest or timeliness. Please assume that all of these have "Friendly, interested, busy." as prefaces. And the rest of the content will be reported in Mother Tongue.Four

KAY WILLIAMSON. Has some N-K additions to "nose, smell". Working with Derek, et al. Densely packed letter. Too much for now. Recommends Dr. John Stewart of

→ Pagination screw-up. Pp. XXX-XXXII are next →

~~XXX~~

Department of Asian and African Studies

University of Helsinki
SF-00100 Helsinki 10
Finland

To: Dr. Harold C. Fleming
Dept. of Anthropology
Boston University
232 Bay State Rd.
Boston, Mass.

Dear Dr. Fleming

Thank you for your most interesting circular. I am ready to join you as an occasional critic in the field of Siberian languages. As a matter of fact I am working on Siberia, in general, and Siberian languages are only one of my interests. On the other hand, I have no competence in Semitic, African etc. matters.

I found your discussion about transcription particularly important. I have also recently been thinking of a unified transcription for the Siberian languages, simple enough to be handled by small computers. I enclose a copy of a paper of mine on this topic. I have not used digits, but your way of using 3 and 7 appears rather sensible (I know that 7 has been applied for the glottal stop by many American linguists; I wonder who was the first to suggest this transcription).


As to the Siberian languages, they represent in my opinion the following 10 language families: Uralic, Yeniseic, Tungusic, Turkic, Mongolic, Yukagiric, Luoravetlanic, Nivkhic, Ainuic, and Eskaleutic. During certain periods, including the present period, Indo-European languages have also been spoken in Siberia and adjacent regions.

Of the Siberian language families, we have good proto-language-level reconstructions for Uralic, Tungusic, Turkic, Mongolic, and recently also for Yeniseic (Starostin) as well as for Eskaleutic (Bergsland). Nivkhic and Ainuic are each represented by a single comparatively homogeneous language, but there is still a lot to be done before we can reliably operate with proto-Nivkhic and proto-Ainuic data. Northern Luoravetlanic has been brilliantly reconstructed by a Soviet colleague (Murav'iova, if I remember correctly), but the work remains unpublished.

Suggestions of phylum-level relationships between Tungusic, Turkic and Mongolic ("Altaic"), or between these three and Uralic ("Ural-Altaic"), or between some other language families in Siberia are in my opinion without any scientific basis. The relationship between Tungusic and Mongolic has most recently been dealt with in an excellent monograph by Gerhard Doerfer.

I am not going to respond to each of your circulars in the future, if you send them to me, but if you have some special problems with the NE Eurasian languages, I will be glad to comment upon your suggestions. As you know, I do not consider Nostratic studies so urgent in today's situation, but I share your very positive opinion about the persons of many Nostratics, including Starostin, Dybo, and particularly Khelmskiy.

I do not like your idea about the global significance of the English language. We scholars in other countries have nothing against making part of our results available to the English-speaking public, but I see no reason for you not to use other languages, such as Russian, Japanese and Chinese, for your communications. I enclose a Japanese publication of mine in which I give a very general sketch of the ethnolinguistic situation in North Asia.


Juha Janhunen
(Reader in North Asian Studies)

XXXI

415-948-3248

4335 Cesano Court
Palo Alto, California 94306
February 8, 1987

Dear Hal,

Thanks for sending me a copy of the second circular. I had already read both circulars with much interest. Cavalli-Sforza sent me a copy of the first and I borrowed Greenberg's second to read some time ago. I do hope you will keep me on the "interested" list for any future circulars (which must take up considerable time and energy, not to mention the expense).

The Michigan group you referred to in circular 2 really only has Shevoroshkin at Michigan; the other members of the team (most of whom met at the Rice symposium last March) are myself, John Bengtson, Mark Kaiser, and Roger Wescott. Greenberg and Cavalli-Sforza serve as consultants, or rather will serve as consultants because we are still at the beginning of the project, not "far along" as you implied.

Concerning Miao-Yao: Greenberg has long considered Miao-Yao to be a member of Austro-Tai, or even Austric, and not Sino-Tibetan. You are correct of course that he assigned it to Sino-Tibetan in the early 50's article to which you referred, but he later came to regard that as his greatest taxonomic blunder, essentially accepting Benedict's evidence on the relationship. He has never published this revised opinion, so there was no way you could have known about it. In Benedict's recent Festschrift Benedict recounts the following amusing exchange with Frank LeBar at the Human Relations Area Files Press in the mid 60's:

"But how about Miao-Yao, Frank? You put it under Sino-Tibetan. You keep Kadai all by itself but then you go and put Miao-Yao under Sino-Tibetan! That's crazy! I may not know where it goes but I do know one thing: it's not Sino-Tibetan!"

Poor Frank could only come up with a very lame excuse.
"Greenberg put it there."

"Greenberg? He's a great Africanist but what does he know about SEA?"

"Greenberg thinks you're right about Tai-Kadai-Indonesian. In fact, Paul, he's been your key supporter in all this right along. You know, a lot of guys in the field will believe anything Greenberg tells them."

"I see what you mean. But I do wish that he hadn't got you to classify Miao-Yao with Sino-Tibetan. He'll have people believing that for years."

I am enclosing preprints of three articles. "Linguistic Evidence . . ." will appear in the March issue of Natural History,

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with the maps of Greenberg's new classification of New World languages "colorized," but I haven't seen the results yet. "The Amerind Phylum . . ." is a somewhat speculative paper I gave at the Rice Symposium, focussing on the subgrouping of Greenberg's vast Amerind phylum and the implications of such a subgrouping for prehistory. "A Taxonomic Analysis . . ." will be published by Blackwell in the Proceedings of a Symposium on "Linguistic Evolution" held at Stanford in 1984. It is a preliminary attempt to apply certain models used in biology to linguistic data and it indicates, I believe, that such approaches may be more promising than most linguists would suppose. With proper refinement we may be able to bring yet another tool to bear on uncovering the prehistory of our species.

By the way, I just received word yesterday that both my book on classification and Greenberg's on Amerind will be published at the beginning of next month, or so the Stanford Press swears. Since mine was scheduled for last December, I'm no longer holding my breath.

I have had two general reactions to the first two circulars. First, there seems to be a Nostratic-centric bias in that there is much talk of adding this or that family to Nostratic. I think at this level of classification it's really necessary to consider all the families/isolates simultaneously or otherwise specious conclusions are likely to be reached. If, as Greenberg states in his Amerind book, all languages belong to a single family, then the comparison of any set of subgroups will always find evidence that the subgroups are related--but this will not necessarily mean that they form a valid genetic grouping. Greenberg is currently working on a book on "Eurasianic," which is similar to Nostratic. It differs, however, in excluding Afro-Asiatic, Kartvelian and Dravidian, and in including Korean, Japanese, Ainu, Gilyak, Chukchi-Kamchatkan and Eskimo-Aleut. Greenberg has also put together a set of 62 etymologies connecting Dravidian and Nilo-Saharan. I think we are going to have to adopt a very broad perspective--the whole world--to discern truly valid genetic groupings.

My second reaction was to what I take to be an undue emphasis on reconstruction as proof of classification. Dolgopolsky said that he would compare Niger-Congo with Nostratic when someone reconstructed it, implying that without reconstruction classification cannot proceed. Yet none of Greenberg's classificatory work has ever depended on reconstruction in any essential way, though he has of course used them where they exist. Reconstruction follows classification rather than leading to it.

I have enjoyed the circulars and look forward to future issues. I'm glad that these topics are finally being raised again and I think the time is ripe.

Best regards,

Merritt

Edinburgh, Scotland; Dr. Remy Bole-Richard of Abidjan, Cote d'Ivoire; and Dr. John Bendor-Samuel of Dallas, Texas, all for membership in the Club. New book on p-N-K by Bendor-Samuel is coming out. Und so weiter und weiter!

DELL HYMES. Much on history of science aspects of Mother Tongue. Helped with \$. He has page proofs for review of Greenberg's new Amerind book in CURRENT ANTHROPOLOGY. I anticipate reading that review but it also means that THE book is about to come out, if it isn't already out. After July 1st, his new address will be : Department of Anthropology, University of Virginia, Charlottesville, Virginia 22903, USA.

SAUL LEVIN. Much to say about IE and Semitic and AA.

ALLAN BOMHARD. A great deal of help. Gave a ton of material to peruse. Has agreed to be co-responsible person for the Club for formal organizational (i.e., tax and grant) purposes. One of our founding fathers.

PHIL LIEBERMAN. Reprints, much on Mother Tongue-1 or "hardware".

JEAN LYDALL. Hamar informant with them at Mainz. Sent many cognate ideas, most pertinently two related to Circular.1, to wit, /tsuu/ is archaic term for "vagina, begetting" and a cognatic group of descendants. "Lightning" = /balkat/ and /plak/ = "flash, glint, shine", "pak/ = "bright". Too much on South Omotic pronouns to repeat here, except proto-SOM is /*inta/, not /ita/

KARL H. MENGES. A great deal to say about Altaic vis-a-vis Quechua, problems of Nostratic, Elamitic, etc. Also is working on Shamanism. Sounds interesting!

MARY RITCHIE KEY. A great deal about history of science aspects, South American languages connecting with North and Meso-American languages, and more.

JOE PIA. Flood of ideas and suggestions! Tops of waves include modems and software for computer intercommunication, ideas on Swadesh + history of science aspects, ideas on conferences & newsletters, how to survive outside of academia, systems of transcription, how to relate Khoisan phonology to Sinitic phonology, and much much more.

ANNA BELOVA. First Soviet response! Many thanks (from me). They-all are working on responses and other new things; it will be coming.

GERARD DIFFLOTH. Yes, let's resist creeping organizationalism. Sino-Tibetanists very successful sans formal organization. Much discussion of S.E. Asian matters and a reprint with permission to zerox (later -HF). "At any rate, Austro-Thai is NOT a reincarnation of Schmidt's Austric; in its 1975 version, it is almost the opposite." Forget about Nahali; there are better problems. And think re MacIntosh computer so you (Hal) can do IPA phonetics.

ALEXANDER MILITARIEV. "By New Years, 1987, Starostin and myself had completed a study on glottochronology based on new Starostin's method (about 700 pages) and before July this year I am to finish my Libyo-Guanche (Berber) comparative phonetics, and there is an awful lot of work... (we know! - HF)". He recommends a "very good archeologist, Dr. Victor Shnirelman in Moscow" for membership in the Club. (Everyone is salivating over the new dating method!)

BRIAN FAGAN. (Mind you, he didn't get this issue yet!) "Now that archaeology has filled in dramatic gaps in what we know about the late Ice Age, I think that Greenberg's important work on the languages in the Americas, also dental morphology and perhaps mitochondrial genetic studies, allow us to look at the events of the past 25,000 years on a much broader canvas than ever before."

YOEL ARBEITMAN. Correct his entry in the list of members. His Language, Topic, and Area entry should read "Semites, IEans & neighbors". Also "Sem., AA, IE contacts or IE loans". Several reprints and reviews. Co-authored BONO HOMINI DONUM: ESSAYS IN HISTORICAL LINGUISTICS IN MEMORY OF J. ALEXANDER KERNS, with Allan Bomhard, John Benjamins, Amsterdam. (Over 1000 pages & costs \$100 -HF)

VITALIY SHEVOROSHKIN. Two more letters with much information. Helped with \$\$ for postage (thanks!). I still have not described the basics of Starostin's

dating method; sorry Vitaliy. I was hoping that Serge would send a 5-10 page summary. Next issue I'll try to describe it from my uncertain memory.

SUSAN PARK. Has data on Paiute (Amerind language). Recommends Dr. Kay Fowler of University of Nevada for membership in the Club. Sent \$\$ for postage. Merci!

HERRMANN JUNGRAITHMAYR. Friendly, supportive, but I can't find his letter now.

REBECCA CANN. Sent reprints of her valuable work. She is now at U/HAWAII. Key article is in NATURE, vol.325, 1 January, 1987, pp. 31-36, entitled MITOCHONDRIAL DNA AND HUMAN EVOLUTION, written with Mark Stoneking and Allan C. Wilson. Go read it! Quite technical, not easy, but so is Hoenigswald's book.

GOOSH ANDRZEJEWSKI. Friendly, but has now retired and devotes himself to Somali matters, especially poetry and literature. Take me off list. Okay.

DOUGLAS WALLACE. Admin. ass't. wrote. Thank you for your interest in our work.

FRANZ ROTTLAND. Quick note. He will be a 2ndary mailer in Europe. DANKE!

DICK HAYWARD. Very very busy. Otopic book WILL be coming out. Bide a wee!

KARL PETRACEK. Heavens! since I mentioned him several times already, I thought I had remembered his letter. Many things to say. Lots of reprints.

WOLFGANG SCHENKEL. Only Egyptologist response so far. Willing to help, especially when questions within his expertise come up. Since comparativists often mangle Ancient Egyptian data, this may be of help to you. (Amen! -HF)

CHAIM RABIN. Now preoccupied with problems involved in revising what we might call the "received version" of how modern Israeli Hebrew was born.

ARTHUR STEINBERG. Having had no linguistics, he doubts that he can make much of a contribution. But we want to know about Gammaglobulin and Arthur is a world authority on that. Please stick with us!

WERNER VICICHL. (Misplaced his letter too.) Friendly, interested, and very helpful, especially in reporting on Prof. Seto's work, but I am most pleased by Werner's own interest -- as one of the senior authorities on AA, especially Egyptian and Berber.

NO NAME. This to represent the letter which was lost in the pile of papers on my desk. If it was yours, please forgive me! I'll find it eventually.

MORE WINE

TIDBITS

1) FLASH! FROM OUR SPIES ON THE WEST COAST IT IS RELIABLY REPORTED THAT GREENBERG HAS CROSSED THE BERING STRAITS, TAKEN MOSCOW, AND IS ADVANCING ON DELHI AND KHARTOUM ! FRESH FROM HIS CONQUEST OF THE AMERICAS..... MOPPING-UP OPERATIONS ARE PROCEEDING....FIERCE POCKETS OF RESISTANCE REMAIN IN....

The title of his new book is LANGUAGE IN THE AMERICAS, Stanford University Press, Stanford, California. If it is as good as Ruhlen says it is, then it should be a proper milestone on the trip towards Mother Tongue. Even if it is not that, as in that is as far as we go, still it will make a huge difference in New World prehistory. Since much of the next YEAR will probably be taken up with discussing this book, enough for now.

2) The Glossogenetic folks have had an organization since 1983. They call themselves LANGUAGE ORIGINS SOCIETY or "The International Society for the Study XXXV.

of the Origins and Evolution of Language" for short. They are interdisciplinary for sure, like us. Their President is Jan Wind, Dep't. of Human Genetics, Free University, P.O.Box 7161, 1007 MC, Amsterdam, the Netherlands; First Secretary is Eric de Grolier, International Social Science Council, Paris, France; et al (Et al = 7 others on the Executive Committee). Membership fees are "extremely reasonable" (US\$20, \$15 for students). They have annual meetings which usually go into a published book and they get a newsletter containing "pertinent bibliographies of recently published works and related book reviews".

They are having their 1987 meetings in Nashville, Tennessee, June 4-6

at Vanderbilt University. Conference registration costs \$10, for students \$7. For more conference information or whatever, write to the two co-hosts:

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I may be mistaken but I think these guys are primarily interested in the "hardware" option in reconstructing Mother Tongue. Despite their striking resemblance to a Roman legion -- all tight, hierarchical and pompous --, we ought to cooperate with, and support, them for our mutual scientific benefits.

3) Allan Bomhard once told me about an important workshop which will be held at Stanford University this summer. Lionel Bender said it has just been announced in the most recent LSA bulletin. It is co-sponsored by the Linguistic Society of America and the National Science Foundation. Organizer is Phillip Baldi of Penn State. Dates are late July-early August, 1987 and it should be quite a pow wow. Other details are in the LSA bulletin (q.v.). "Splitters" and "lumpers" will be discussing general questions of historical methodology, particularly questions about the reliability and applicability of orthodox Indo-European methods (for what?). I presume that spectators are allowed, but I don't yet know what it costs. Maybe I'll see some of you there, whether as gladiators or not. (It seems to be Roman image time.)

4) An old New York Times news clipping got found. Some time in the 1960s. It concerned the discovery that the Indus Valley script had been de-ciphered by the assumption that the language written in it was Dravidian. I never followed up on it, expecting to hear more later. I never did. Does anyone know the present whereabouts of the three Finns who made the discovery or just know about the discovery itself? Their names were Asko Parpola, Simo Parpola and Seppo Koskeniemi. They worked under the supervision of Prof. Pentti Aalto of Helsinki University.

5) The evidence and arguments for the often-mentioned Elamitic-Dravidian family or phylum. Where can they be found? Has it been published? S/he who thinks the two are related -- can you generate a short summary of the argument for us? Since I worked a little on Elamitic in the 1960s, but never got access to very much data, I suspected it might be related to Dravidian. But even more so to SUMERIAN ! Does anyone find any merit in that idea?

Please detach and mail this portion to Fleming
Besides English, I can read the following: My name _____

FRENCH___ GERMAN___ ITALIAN___ RUSSIAN___ SPANISH___ POLISH___
JAPANESE___ CHINESE___ ARABIC___ HINDI___

I recommend _____ for membership. Address is _____

Here is some \$ _____
for postage _____

Save Postage: _____
Take me off list _____

I will mail 12 _____
letters for you _____

I would like to _____
edit/take over
Mother Tongue for month of _____
198__