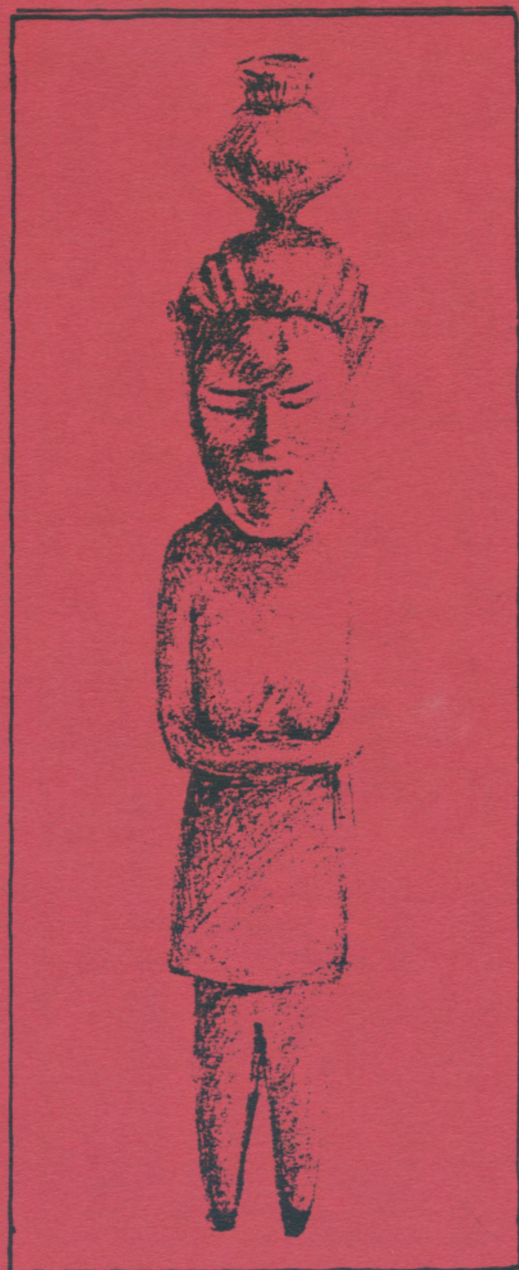
Bomhard

MOTHER TONGUE



NEWSLETTER OF THE LONG RANGE COMPARISON CLUB

MARCH 1988

MOTHER TONGUE
Circular 5
March 1988

TABLE OF CONTENTS

NOTE: Let us agree on some MT publishing rules. To get the contents of your letter put into Mother Tongue just start out your letter something like this:

"Dear Hal, (or) Dear Mother, (courtesy of Grover Hudson)

How are you? I am fine. Hope your dean gave you a raise and your wife has kept that good job. Now PLEASE PUT THIS IN NEXT MOTHER TONGUE...

Although you tried hard to explicate Starostin's root dating, you failed to notice that...."

(OR) "Dear Hal,

How are you? I'm in bad shape. DON'T PUBLISH THIS! It all started when a border patrol jumped me at the LSA meetings and trashed my..."

(New Americanese: "To trash" = to beat up, destroy, turn something into trash

ANOTHER NOTE. A few people have repeated their generosity, making money contributions again to LRC Club. I want to thank them very much because they have made this issue possible, especially Dr. Susan Park and ABD Linda Arvanites. And what can I say about the six wonderful people who are mailers? Right! You are six wonderful guys (guy = person, in youth American English) and I appreciate your help very much. But many of you in western Europe and North America have sent NOTHING -- ever. (Recently seen on a bumper sticker: "LOVE MEANS NOTHING TO A TENNIS PLAYER.") Well, mes amis, we are rapidly reaching the point where a small number of voluntary contributions won't sustain us at the new levels we are reaching. The stuff to be reproduced is piling up and the membership is nearly 160. More issues and more members = mor money needed or more mailers. Come on, you guys! Throw some money in the pot!

THIRD NOTE. We are also late in getting out this fifth circular. The reasons are primarily four: responses to computer questionaire come back ever so slowly; the money ran out with the dispatch of MT4 so I've been waiting for contributions; some time is spent negotiating with people over publishing things they write; and being a college professor does unfortunately take up quite a bit of time. SORRY! (Ok, I did sneak some time to work on Omotic.)

G O O D   N E W S

KAREN EBERT (U/Marburg, BRD) is going to a considerable amount of trouble and inconvenience (but SHE didn't say that) for the sake of our common goals. On her forthcoming trip to Nepal she will stop off and try to find some Kusunda people -- there are said to be very few left -- and to check/confirm the published material on the Kusunda language and add to the corpus. Almost as important as the gathering of new data is the fact that Karen is a Siniticist or Sino-Tibetanist, as well as a Chadicist! No one who has listened to Kusunda so far or evaluated the written material -- so far as I know -- has been a trained Sino-Tibetanist. Her opinion will then be most important. One of my deepest fears about Kusunda has always been that, when good data are obtained, some expert will find that it really is Sino-Tibetan, albeit a distinct branch, and thus take all the fun out of it. If anyone can help Karen in some way or other, we will appreciate it very much. When Karen comes back, we will present her with the first ever IN SEARCH OF MOTHER award. With careful investments and judicious manipulation of the Japanese and BRD stock markets we should increase the award's funds which currently stand at 6 Fr. francs, 50 It. lire, 2 Eth. bir, 3 DM, and 1 Austrian shilling.

REBECCA CANN, DOUGLAS WALLACE and their colleagues have begun to attract significant public interest in their hypotheses. Most importantly, from a North American point of view, was the headline treatment given the search for "Eve" in the popular magazine, NEWSWEEK. I thought the discussion both quite competent and interesting; I recommend it to you-all Long Rangers, if you have not already seen it. In case everyone has not been focused on the differences between Rebecca's and Douglas's conclusions, or perhaps 'working hypotheses' is a better way to say it, Douglas favors an ASIAN location for "Eve" rather than the African homeland preferred by Rebecca. Both work with mitochondrial DNA (hereinafter called mDNA) and very similar technologies and agree that the other is not necessarily wrong. Is that a fair way to put it, good colleagues?

It is quite a hassle (= c'est difficile) to get permission to copy the Newsweek article for those who don't have access to it. Perhaps some kind soul in BRD, for example, would send a copy to Anna Belova, for example, so she could read it and pass it around among the Moscovite Long Rangers. If either Rebecca or Douglas have reprints of the article, they might be inclined to send one to Thomas Gamkrelidze, for example. Just a suggestion.

It is not to be denied that the conclusions reached by the mDNA researchers are intensely controversial. Those paleoanthropologists whose work was in fact criticized by Stephen Jay Gould (cf MT4) are attacking those conclusions strongly (Cf Newsweek). Myself, for example, does not know who is right or who will turn out to have the most fruitful hypothesis but the mDNA research has excited me and my hunch is that Rebecca and Douglas are HOT! I should also apologize in absentia to Wilson and Stoneking, Rebecca's co-authors, for neglecting their part in the mDNA discoveries. It was Alan Wilson, of course, along with Vincent Sarich, who started much or most of the amino acid research which first proposed a BIOLOGICAL CLOCK for measuring separation times among the "higher" Primates and greatly shortened the time between the common ancestor and us.

Finally, it also seems important to mention one thing that Cann, Wilson and Stoneking actually said. While the common beginning date of "Eve", arrived at through mDNA calculations, was 200,000 years ago (plus or minus 100K), still the estimated date of dispersal from Africa was around 100,000 years. Thus, I have colleagues who think that the mDNA dates cancel the fossil "modern man" dates and colleagues who think that the fossil dates falsify the mDNA dates. Yet I find them to be compatible and thus exciting! (See below)

Mother Tongue and the Long Range Comparison Club have received some very modest attention in public, mostly as a consequence of Shevoroshkin's popularizing efforts in the New York Times and the Toronto Globe (this courtesy of Derek Nurse). There are also Dell Hymes' remarks in CURRENT ANTHROPOLOGY (hereinafter called CA). See TIDBITS. However, Roger Lewin of SCIENCE NEWSLETTER has had long interviews with proponents (myself included) and opponents of the Mother Tongue endeavour and will be publishing a long article on the whole subject one of these weeks.

PLUS IMPORTANTE. MAS GRANDE. One Americanist, ever so slightly amused at finding himself described as an "Amerind Border Patrol", has joined the Club. Terrence Kaufman has been asked to present some of his views in Mother Tongue and may choose to do so. Lyle Campbell has offered to smoke the pipe of peace, so to speak, and has a letter to Long Rangers which you can read below. Also Dr. Victor Golla who edits a newsletter for the Society for the Study of the Indigenous Languages of America (SSILA) has joined LRC Club and explicitly desires swapping of views and newsletters. (To get his newsletter, write him at Dep't. of Anthropology, George Washington University, Washington, DC. 20052 (USA)). These are most encouraging events, of course, because we may be able to actually discuss Amerind linguistic taxonomy in the rational manner that naive philosophers of science often imagine we do. Let us hope!

T I D B I T S

1. CHRISTY TURNER has suggested strongly (recently) that a Southeast Asian homeland for Homo sapiens sapiens is the best bet. At least for Eve's teeth. So our mother(s) got her/their mDNA in Africa or Asia, her/their teeth in Southeast Asia, and her/their tongue(s) in ....XYZ.... But the fossil Homo sapiens sapiens seem — at this moment in the publication of results -- to favor eastern Africa with the Levant a very close second. (115,000 to 110,000 as against 92,000 years ago.) Is it not true that Niah Cave in Borneo contains the oldest fossil Homo sap. sap. known for eastern or southern Asia? And the date does not exceed 41,000 BC? Will someone stop me if I utter falsehoods? (Either Michael Day's GUIDE TO FOSSIL MAN or Gail Kennedy's PALEOANTHROPOLOGY could be recommended to Long Rangers who want to look up some fossil facts.)

2. EARLY MODERN MAN OF LEVANT? Everything is controversial, at least for a while, and one controvery is now developing about the DATES of the archaic Homo sapiens, non-Neanderthal, found at Qafzeh in Israel. This is because the New York Times and Boston Globe announced that the dates had been changed from around 40,000 ?? to 92,000. David Pilbeam was quoted in the Times and Globe, saying that the new dates made a difference in the way Neanderthal was related

to modern people. However, the news items were based on a recent article in NATURE, which I have not seen yet, and one critic said that the NATURE article was misquoted. Some archeologists here are saying that the association between the dated materials (burned flint) and the human remains (indubitably "archaic" modern) has NOT been demonstrated. (No doubt any thing said to be both archaic and modern seems oxymoronic!). Archeology naturally and necessarily obsesses about dates and associations.

On a hopeful final note to the above, Paul Zimansky suggested that Prof. O. Bar Yosef of Israel would be an authoritative opinion on the merits of the new hypothesis from a veteran Israeli archeologist's viewpoint. After I failed to find Bar Yosef at Harvard where he has been a visiting professor (he has returned to Israel), I was expressing my dismay to some students, when one of them pointed out that Bar Yosef himself was one of the group of authors who wrote the original article in NATURE! Well, well.... I think that David Pilbeam and Bar Yosef (et al) are going to win THIS debate.

3. EARLY MODERN MAN IN CHINA? Lest the Africanists and Near Easterners run away with the prize for finding Mama sapiens' home, a new and strong claim for equally old dates has been made for east Asia. Ruth Gruhn, as part of Tidbit #10 (below) has reported that "in China the local transition from archaic Homo sapiens to anatomically modern man was under way ... by 100,000 years ago." One would have to read the primary articles to get a clearer picture than the one presented here. They seem to be either Wolpoff, A., X. Wu, and A.G.Thorne, writing in F.H. Smith and F. Spencer, eds., THE ORIGINS OF MODERN HUMANS: A WORLD SURVEY OF THE FOSSIL EVIDENCE, 1984, pp.411-483, or X. Wu and M. Wu, writing in Wu Rukang and J.W. Olson, eds., PALEOANTHROPOLOGY AND PALEOLITHIC ARCHAEOLOGY IN THE PEOPLE'S REPUBLIC OF CHINA, 1985, pp. 91-106. There is absolutely no reason in general principle that precludes eastern or southeastern Asia as THE homeland of modern man. Moreover, since it is also very clear that our penultimate ancestors, the Homo erectus (pl.), were found in east Asia, Sundaland, southwest Asia, Europe and Africa, then it is still possible that Homo sapiens developed independently in each of those places or in many of them or that Homo sapiens developed in two of them and spread out into the other regions, including INTO Africa.

It is probably important to point out -- and here for the linguists primarily -- that Africa is the likely homeland of humans as distinct from the other apes because three of the four apes taxonomically or genetically closest to us live in Africa and because of all that stunning fossil evidence of the Australopithecines from eastern Africa. Gorillas (both Mt. and Lowland), pigmy chimpanzees, chimpanzees, and orangutans are the four closest relatives. Jeffrey Schwartz argues (and rather well) that orangutan is just as close to us, or even closer, than the others. But, but, that general presupposition of African origins does not apply to the SUBSEQUENT DEVELOPMENT of Homo sapiens. Why? Because Homo sapiens could have arisen from any number of Homo erectus populations found throughout most of the Old World. It is a whole new ball game! Homo erectus lived for hundreds of millennia in Sundaland, for example.

And where, pray tell, are the fossils from INDIA? Or where is the evidence or arguments for human antiquity in INDIA? Pretty sparse stuff, yet it figures that India would have participated in developments which seem to span the old tropical world. It is after all the MIDDLE of the zone of permanent and long term human residence!!! Nothing there but abdominal snowmen? Non credo!

Nevertheless, my intuition urges me to bet on Rebecca Cann's theory.

4. Two Long Rangers, MARCELLO LAMBERTI and DAVID APPLEYARD, have expressed big reservations to me about my tendency to consider linguistic conclusions in relation to archeological and bio-genetic data and/or conclusions. Marcello wrote me a long thoughtful letter, ages ago, for which effort naturally he got no reply. I'm asking for his permission to reproduce most or all of it in a future issue. His view is the authentic voice of autonomous linguistics -- let us do linguistics by itself and not get mixed up with archeological and

- 4 -

bio-genetic stuff. David has been impressed with the recent book by Colin Renfrew which shows among other things how difficult it is to relate IE movements to archeological cultures of Europe. Seeing the LRC Club as originally a group of linguists, David would like to see us stay that way. It is important to point out that neither Marcello nor David are saying that linguists can't "do the job"; rather that our work gets confused if it is mixed with that of other disciplines. (I would rather let their letters speak for themselves - later.)

While the issues they have raised are vitally important in the long run, for the short run let me say two brief and non-vital things. First, as said before, I call myself a "four-field" type. For credibility in technical matters I am restricted to linguistics and ethnology. If I tried to publish an analysis of Lucy's anatomy or the burned flints at Qafzeh, everyone would laugh heartily. But I am allowed to DISCUSS the historical and global import of various archeological and fossil conclusions, as well as as proto-IE. WE ALL ARE. Old fashioned anthropology is a helluva lot of fun! Try it some time!

Second, if one looks at the original mailing list in the letter to Aharon Dolgopolsky, one can detect the presence of 9 archeologists and 4 biologicals, not to mention 10 social/cultural anthropologists & historians. The LRC Club is no ancient collectivity but it has always been catholic.

5. APROPOS OF INDO-EUROPEAN. I formally solicit a brief commentary or review of Colin Renfrew's book, mentioned above. He is not the only one addressing these problems. DAVID ANTHONY had a superb article on IE and south Russian fossil cultures recently in CA, vol.27:291-304. In the *CA treatment many important people, especially MARIJA GIMBUTAS (U/Cal) and Soviet expert D. YA. TELEGIN (Kiev), expressed their views. It would seem that the question of IE origins, far from being a murky matter, is almost "down to a gnat's eyelash".

6. CA recently ran a special book review with *CA Treatment of Greenberg's LANGUAGE IN THE AMERICAS. The reviewers included Dell Hymes, Wallace Chafe, and Ives Goddard. Greenberg had a long presentation at the beginning of the review and a final rebuttal at the end. That is standard procedure at CA, except that in this case the original presentation was a review by the author rather than an original article. You all are urged to read it (CA, Dec. 1987, vol. 28: 647-668), particularly those who do not feel inclined to read the book. Long Rangers are invited to submit (to MT) their own reviews of Greenberg's book, graciously restricted to a page or two. But, please, do not forget to mention whether you want your comments published or to be kept private!

7. SIDNEY LAMB had a comment on LANGUAGE IN THE AMERICAS, along with ERIC HAMP, a well-known IE scholar. The pair of them took diametrically opposed views on the subject of Greenberg's taxonomy. (His book had not actually come out yet but pre-publication circulations did occur.) Read it in CA, vol. 28:101-2.

8. ALEXANDER MILITARIEV has written thrice, twice recently. They are delightful letters and ought to be published, but he has not given me permission. He also corrects his name -- I am NOT to call him Yuri. My apologies -- I seem to be addicted to nominalistic disinformation. I have distorted or misspelled practically every Russian's name and some of the Germans' too, as other people have pointed out. (Now is my chance to apologize to Werner Vycichl.).But it's simply that I don't know Russian codes of address and reference; when do you call him "Sir Robert" and when do you call him "Bob"?

The Long Rangers of Muscovy want some help, some serious help. They have had their appetites whetted by an Olivetti computer and Starostin has started churning out new and exciting things on the one they get to share. They want an IBM PC XT because it has the hard disk and megabytes of memory. (I'd like to have one too, for that matter.) Can we help them? That involves a number of pieces of information, which I hope are not MISinformation, about the computer business of today in the USA. First, I do not know if my government will permit us American Long Rangers to send an IBM PC XT to Moscow or even Prague. That involves legal questions for which I have no answer yet. Second, while it might be possible that IBM itself, that lovable old megacorporation, would give the Muscovites a computer FREE of charge, it is equally possible that the US government would not permit them to do that. Third, if it were permitted but not free, we could try to raise the money. Or someone could. Fourth, it might be easier or preferrable to try to get an IBM clone in Europe; also clones tend to be cheaper. Are there not Olivetti clones of the XT?

Fifth, IBM PC XT is no longer "state of the art" or so I hear. That is because IBM is now marketing the IBM PS/2 (in various models). Americans interested in IBM PCs are now thinking about buying the PS/2 instead of the XT model. However, just because of its obsolesence, the XTs probably will get cheaper. It is still an excellent machine and many many pieces of software are designed to be compatible with it. ("Not compatible" means that the software, a specific floppy disk, will not work at all on the machine.) Sixth, if Alexander were permitted to buy an American computer by my government and his, then I would also like to call his attention to the Macintosh II because it has graphics and a hyper-card and some compatible software which will permit any alphabet including Cyrillic on "fonts" (sets of characters). The Mac II would probably cause a bright fellow like Starostin to go bananas (= become excited, or disturbed, to the point of madness).

Finally, next issue -- MT6 -- will be the computer issue. Therein Stanley Cushingham, Joe Pia, Keating Willcox, and others will give some information and opinions about options that people have. Stanley will also be giving a paper and holding demonstrations on some new software and fonts -- al this at the African Languages conference at Boston University in April. In MT6 we will try to give the results of the Computer Questionnaire. That is, if mor of you BOTHER to send back your questionnaires. Heaven's sake! it is not very much work to help out that much! For that issue I hereby SOLICIT suggestions for helping our friends in Moscow.

I want to stress that the LRC Club is not involved in international politics, nor ideological struggles; nothing of that kind. We are not even interested in Armenian nationalism or the Irish Republican Army. The LRC Club does not live in this century; its attention is fixed firmly on our COMMON ANCESTORS. I suppose they were lucky not to live in this bloody century.

This is not to say, however, that none of us have political or social passions. Indeed we do have them! Some of us have powerful and emotiona views on all the topics mentioned above. We could provide a debating society with debaters for many years of vigorous activity. That is precisely the reason why the LRC Club doesn't do politics in MOTHER TONGUE ; it would destroy us!

9. More on FOSSIL AMERIND DATES. J.M. ADOVASIO and R.C.CARLISLE, both of the University of Pittsburgh, have a strong letter in SCIENCE (vol. 239, February 12, 1988, p.713-714). They argue the case for their Meadowcroft Rockshelter dates being accurate and dispute points made by their critics -- all this in full archeological technicalities which nowadays read more like physics than anthropology. Their penultimate conclusion is worth citing. "If the six deepes dates unequivocally associated with cultural material are averaged, then human were definitely present at this site (and, by implication, throughout much and perhaps all of the Americas) sometime between approximately 13,955 and 14,555 years ago." Well, I read that as 12,250 BC and take it as basically confirming the "standard" date of 11,000 BC more or less, despite the authors' clear suggestion that they have broken that date. The reason is NOT that I think 125 years are trivial but rather that some versions of the standard date have always ranged back to 12,000 BC. The 250 years are not enough to falsify such robust hypothesis as the standard date.

The persistent yet "unacceptable" early dates in South America, reported in MT3 and MT4, continue in their limbo. They are always attacked on technical grounds -- and again this is vitally important to archeologists -- yet I wonder whether the technical standards don't get higher whenever the standard date is threatened. Granted that that is a nasty thought, still the history of science suggests that it could happen. Another possibility is that (a) there are indeed human remains (cultural, not skeletal) found around 30,00 BC in South America but (b) they belong to late descendants of Homo erectus, shortly to be displaced by Homo sapiens sapiens (Amerindensis). Such a possibility flies in the face of everything we know about the peopling of the Americas, so I mention it only as a logical possibility.

10. HOWEVER, a vigorous COUNTERATTACK by anthropologist and Americanist (?), RUTH GRUHN (U/Alberta), was mounted recently (CA, vol. 28:363-4) against Greenberg, Turner, and Zegura for saying (CA, vol. 27:477-97) that Amerinds ha only been in the New World 12,000 years. Sounding definitely offended, Gruhn wanted to know why either the Brazilian studies published by Guidon (cf MT-4) or Dillehay's Chilean discoveries (cf MT-3) had been declared to be incompetent. When one brings in French paleolithic specialists to look at one's excavations, and these experts do enjoy great prestige, then how dare Greenber et al declare the 32,000 dates to be unacceptable? Christy G. Turner II replie for the trio very briefly and politely. A key point was that he and Greenberg and Zegura had decided to wait "on the judgment of the archeological community for a decision about the antiquity of these sites." They were not in the business of deciding whether the new South American dates were tenable or not.

11. On SEMITIC MATTERS. Introducing the new INSTITUTE OF SEMITIC STUDIES at Princeton University. The director is Dr. EPHRAIM ISAAC whose incredible energy has brought it to fruition. The Institute fundamentally aims at establishing a major library, a digital and microfilm database for Semitic languages and civilizations. It will be a resource for Long Rangers who want to correspond with, or talk to, some expert Semiticists other than the ones they already know. The Institute is the only one of its kind in the USA. It also supports a new journal, of which more below. The address is: Dr. Ephraim Isaac, Institute of Semitic Studies, P.O.Box 1374, Princeton, New Jersey 08542, USA.

11. JOURNAL OF AFROASIATIC LANGUAGES (JAAL) has been established by the Institute of Semitic Studies; vol.I, number I, has just come out. JAAL's Editorial Board is full of Long Rangers. Robert Hetzron (U/California, Santa Barbara) is Editor. "JAAL brings forward contributions in linguistics of all types -- historical, comparative, theoretical, and other.." It also "welcomes notices of interest to our readers, book announcements, reactions to articles in JAAL or to relevant issues raised anywhere, and addenda to articles. JAAL intends to provide, from time to time, a forum for debates on specific issues, and invites suggestions." It costs US$20. Write Hetzron, U/C, SB, CA 93106.

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J O H N B E N G T S O N on SHEVOROSHKIN's LETTER

617 Madison St. NE #1
Minneapolis, MN 55413 U.S.A.
16 December 1987

Dear Mother Tongue:

I was very pleased to find the latest issue (Circular 4) in the mail recently. I can only commend editor Hal Fleming, and everyone else who has contributed, for what is becoming quite a substantial and engaging little journal.

First, I have some comments on the discussion by my friend and colleague, Vitaly Shevoroshkin. (We agree on so much; but it would be surprising if we did not have some differences, if only in style and emphasis, between the 'Soviet' and U.S. schools.) For example, I think we all agree with the importance of the principle of 'regular phonological change', as Vitaly stresses in his letter (Circular 4: pp. 19-20). I am all for 'sound correspondences' and reconstruction, but they must be placed in the proper sequence of operations. My position is probably somewhere between that of Vitaly and that of Joseph Greenberg, so that while I think sound correspondences are of value, and may sometimes provide the 'precision' Vitaly speaks of, they are difficult to apply to remote comparisons, and even in 'lower level' groups they may be totally violated (see Greenberg 1987: chapter 1, and Ruhlen 1987: 120-124, 224-227). For example, the Indo-European etymology 'spleen' shows the 'correspondences' of Indic <u>pl</u>- : Lithuanian <u>bl</u>- : Slavic <u>sl</u>- : Latin <u>l</u>-, yet we do not reject the etymology. (There are similar anomalies in the words for 'nail', 'navel' and 'tongue': Meillet 1937: 172, 406-407.)

So we must recognize the importance of regular phonological change, tempered with the cautions of Greenberg and Ruhlen. For example, when I was assembling the global etymology "ARM"(1) (in Bengtson 1987), I was cognizant of the general correspondence of:

Niger-Congo *<u>b</u>-= Nostratic *<u>b</u>- (in Bomhard's as well as Illic-Svityc' and Dolgopolsky's versions of Nostratic: subsumes the traditional IE *<u>bh</u>-) = Burushaski <u>b</u>- = Sino-Caucasian *<u>b</u>- (attested in Tibeto-Burman and Yeniseian, in this case) = Austronesian *<u>b</u>- = Amerind *<u>b</u>- (preserved, e.g., in Macro-Panoan; in some other groups *<u>b</u>- has apparently merged with *<u>p</u>- and/or *<u>p'</u>)

This provisional correspondence has been observed in other etymologies as well. But if we sometimes find the 'wrong' correspondence, we do not throw out the candidate if it looks likely in other respects: we can make a note or query the entry. We may later find that assimilation, dissimilation, accentuation, or some other factor can explain the apparent anomaly.

Also, some of us (myself included) feel more comfortable with assembling the lexical material, and letting those who are better qualified in phonology (such as Vitaly) take care of the details of reconstruction and sound laws.

The second point is prompted by the last sentence in Vitaly's letter: ". . . without trying to establish sound correspondences . . will force us to stay on Trombetti's level." Something similar was stated in a letter to me from Claude Boisson (Dec 3, 1987): "I started reading your paper (Bengtson 1987) with a sceptical turn of mind, all the more so since you mentioned Trombetti and Swadesh." Now, I do not mind healthy skepticism, the kind that demands sufficient evidence for a hypothesis, yet is open to accepting the hypothesis if that evidence is convincing. I am only suggesting that we give due honor and credit to our predecessors and pioneers in long-range comparative linguistics, and pre-eminently Trombetti and Swadesh. This is far from saying that they were right about everything, but they did go ahead and attempt what most others feared to, and blazed trails, some of which are yet to be fully explored.

Some have been content to dismiss Trombetti and Swadesh, simply because they espoused, or were open to, theories of remote relationship and monogenesis. However, we must acknowledge that there is nothing inherently 'scientific' about espousing the idea of plural origins of language, or the 'splitter' mentality (as discussed by Hal Fleming on p. 24 of Circular 4). To paraphrase Sydney Lamb, it is just as bad to keep too many languages apart, as to put too many languages together!

Also, we may disagree with certain details in the work of our pioneer 'Long Rangers' (such as Swadesh's glottochronology), without throwing away the rest of their contributions.

We should be thankful for the reports on the Stanford Conference by Hal Fleming and Allan Bomhard (pp. 21-24). One was lively and provocative, while the other was factual and restrained. My own feeling about the fanatic Amerind 'splitters' is a kind of pity, since they had an opportunity to welcome a breakthrough in their field, but instead they are ultimately going to be remembered as the Simon Newcombs of Amerindian linguistics. (Newcomb was the astronomer who declared airplane flight 'impossible'.)

I am pleased to report that I have received two fine packages from fellow Long Rangers: from Vitaly Shevoroshkin, his new paper for Ur.-Alt. Jahrbücher entitled "On Macrofamilies" (which is his survey of the seven great macro-families or -phyla: Nostratic, Dene-Caucasian, Amerind, Austric, Indo-Pacific, Australian, and Khoisan; and of possible relationships among the phyla); and from Merritt Ruhlen, some of his "Materials for a Global Etymological Dictionary"; and a print of an article, "The First Americans Are Getting Younger" (Science vol. 238: 1230-1232), in which Greenberg and Ruhlen's Amerind work is cited. These papers are on the cutting edge of long-range comparative linguistics, so I assume Mother Tongue has received copies and will publish parts in a future issue.

best wishes, John D. Bengtson

REF:

Bengtson, J.D. 1987. "Paleolexicology: a tool toward language origins". forthcoming in Glossogenetics II (Language Origins So

Greenberg, J.H. 1987. Language in the Americas. Stanford.

Meillet, A. 1937. Introduction à la étude des langues indoeuropéenes. Paris. 8th ed.

Ruhlen, M. 1987. A Guide to the World's Languages. I. Stanford.

Swadesh, M. 1971. The Origin and Diversification of Language. N.Y.

Trombetti, A. 1905. L'unità d'origine del linguaggio. Bologna.

Vitaly Shevoroshkin

DISCUSSION

"The Ancient Near East and the Problem of Indo-Europeans" The discussion of this topic was presented in several articles published in "Vestnik drevneY istorii" (Moscow) in 1980-1984, namely, a long article by T. Gamkrelidze and V. Ivanov in VDI 3, 1980 and VDI 2, 1981; an even longer article giving the reaction of I. D'Yakonov in VDI 3 and 4, 1982 (see also L. Lelekov's paper in VDI 3, 1982) and Gamkrelidze and Ivanov's answe (in VDI 2, 1984). Gamkrelidze and Ivanov's main idea was that the territory of formation of the Indo-European proto-language was located in the 4th m. B.C. ir the extreme southeastern part of Asia Minor, and the Northern part of Mesopotar D'Yakonov prefers to locate the proto-language in the Balkan-Karpathian region.

In principle, Gamkrelidze and Ivanov's arguments (based on both linguisti and archaelogical data) seem to be rather strong; we assume that their book on the problem, written several years ago but apparently still not published,

will provide much additional confirmatory evidence. Nevertheless, several details in D'Yakonov's discussion seem to be well-founded. Indeed, it is unclear why IE *t', and *k' (traditionally: *d, *g) should become, in words borrowed by Kartvelian, Kartv. *d and *g, and not *t' and *k'. It seems that the whole reconstruction of *IE *p' *t' *k' by Gamkrelidze and Ivanov, inste of the traditional IE *b *d *g, is illusory (though (*p *t *k might be possit the respective Nostratic proto-phonemes had exactly this shape; accordingly we would have IE *ph *th *kh [traditionally *p *t *k] from Nostr. *p' *t' *q'/*k', cf. Altaic; and IE *b *d *g [traditional *bh *dh *gh] from Nostr. *b *d *g; note that the vast majority of foreign words with b, d, g, borrowe into IE showed exactly IE traditional *bh *dh *gh, i.e. *b[H], *d[H], *g[H], after Gamkrelidze and Ivanov). - Among other details we strongly oppose Gamkrelidze and Ivanov's comparison of Hitt. (istama-)hura- [istama- = 'ear' 'ear-ring' with Georg. q'ura 'ear': Hitt. -hura-, as well as the verb hura-, seems to mean 'pierce' (IE *Xwer- < Nostr. **qurV, cf. Kart. *qwr, 'make hol HS *xwr '[make] hole' as in Arab. xurr 'hole'; Drav. *ur 'pierce, make hole' Mong. *ur 'hole' etc).

V.S.

RECENT PUBLICATIONS

V.M. Illič-Svityč. Opyt sravneniYa nostratičeskix Yazykov. Sravnitel'nyY slovar' (p - q). [A Comparison of Nostratic Languages. Comparative Dictionary (*p - *q)]. - Moscow (Nauka) 1984, 136 pp.

This is the first issue of the third volume of the Nostratic Dictionary by the late Illič-Svityč. The first volume was published in 1971 [Introductory articles, comparative tables, dictionary *b - *K'], the second - in 1976 [dictionary *l - *ʒ]. Illič-Svityč compares Hamito-Semitic (=Afro-Asiatic), Indo-European, Kartvelian (all three - West-Nostratic), Uralic, Dravidian, Altaic (the latter three - East Nostratic) languages and reconstructs Nostratic proto-forms. Editor V. Dybo wrote in the foreward to the third volume that Illič-Svityč's work has received so far very high appraisal from the world's linguists including those who themselves worked in the field of comparisons of different linguistic families (B. Collinder, N. Poppe, K. Menges). [V. Ivanov's reviews of vol. 1 and 2 ~~will~~ appeared in ~~February~~ 1986 in English translation in the collection Typology, Relationship and Time, Ann Arbor, Karoma]. Dybo does not mention H. Birnbaum's very positive characterizat of Nostratic theory in his work on reconstruction published as JIES Mongraph 2 (1977).

Dybo also mentions two critical works on Nostratics: part of B. Serebrennikov's article on Uralic [translated in the above collection] an a few remarks by the Dravidologist M. Andronov. Dybo shows that this criticism is based either on misconceptions con't bottom p.11

UNPUBLISHED MATERIALS

<u>Proto-North-Caucasian Roots</u> [over 2,000], by S. Nikolaev and S. Starostin. This is the result of recent reconstruction of the North-Causian proto-language by S. Starostin and S. Nikolaev. The list was compiled by Nikolaev who provided an English translation for each meaning. A few years ago, students of the Linguistics Department at the University o Michigan, under the guidance of J.C. Catford, compiled, on the basis of thi list, a reverse list (i.e., "English to North-Caucasian") on cards in alphabetic order of English. We await the full evidence upon which this Proto-North-Caucasian reconstruction is based before we can properly evaluate it.

* * *

<u>Proto-Hokan Roots</u> [over 350], by D. Leščiner [with assistance of S. Nikolaev]. This is a preliminary list: though the reconstruction of consonants has been completed by Leščiner, the reconstruction of vowels will require some more work. Though there are only a few proto-Hokan (=PH) roots which are identical to their proto-Penutian (PP) counterparts (see below), the system of PH consonants is almost identical with that of PP (*p' *ph *p *b; *t' *th *t *d; *q' *qh *q; *k' *kh *k [and corresponding labiovelars]; *č' *čh *č; *c' *ch *c;ƛ' *ƛh *ƛ, etc.). The stablest words (such as the first and the second pronoun) of PH are identical with those of PP which confirms the thesis of remote genetic relationship of both Hokan and Penutian (they both belong to "Amerind").

<u>From Tubatulabal to Uto-Aztecan: Final Consonants and Consonant Clusters</u>

Internal reconstruction of Tubatulabal morphophonemics and compariso with Luiseño, Serrano, Hopi, Southern Paiute, and Nahuatl yield abundant new evidence of consonant clusters and final consonants in the protolangu (as had been proposed by Sapir and Whorf). Some of the resulting protofo follow. I give here proto-Tubatulabal forms, but for the most part they should also work for proto-Uto-Aztecan.

<u>Nouns</u>		<u>Verbs</u>	
*tɨpat - tɨ	'piñon nut',	*katɨC	'sit',
*pikkat - tɨ	'stone (knife)',	*tɨwaC	'go out',
*wip-tɨ	'grease',	*pitɨC	'arrive',
*tap-tɨ	'sinew',	*makaC	'give',
*kut-tɨ	'fire',		
*tɨn-tɨ	'rock',		
*kapsi-tɨ	'leg',		
*taCpun-ti [C=k?]	'rabbit',		

A. Manaster-Ramer

S. Starostin. PraeniseYskaYa rekonstrukciYa i vneŠnie sv'azi eniseYskix Yazykov [Reconstruction of Proto-Yeniseian and External Relations of Yeniseian Languages]. KetskiY Sbornik - Studia Ketica, Leningrad (Nauka Publishers), 1982, pp. 144-237.

In the late sixties, V. Toporov published several papers on the comparison of Yeniseian languages (living: Ket, Yug; dead: Arin, Asan, Kott, Pumpoko and reconstruction of proto-Yeniseian; other linguists joined him later. Starostin's paper represents a part of a collective work (with V. Toporov and G. Verner) on Yeniseian. The first part of the paper (pp. 144-196) represents reconstruction of proto-Yeniseian phonology; it is illustrated by a long list of cognate sets, each headed by a reconstructed proto-Yeniseian word.

In the second part of this paper, Starostin compares his proto-Yeniseian reconstructions with those of proto-North-Caucasian (as presented by S. Starostin and S. Nikolaev in 1976-78) and proto-Sino-Tibetan based on revised Tibeto-Burman reconstructions originally proposed by P. Benedict in 1982, and on Starostin's own reconstructions of Old Chinese presented in his Ph.D. thesis in 1978. As a result, the remote genetic relationship between Yenisei North-Caucasian and Sino-Tibetan is regarded as proven. The macro-family thus established is named Sino-Caucasian (after S. Nikolaev "added" Na-Dene to this macro-family, they began to use the term "Dene-Caucasian"). In many cases, Starostin compares Sino-Caucasian cognates with proto-Nostratic roots as reconstructed by V. Illič-Svityč in the sixties. Starostin does not specify where we may deal with borrowings, and where with ancient genetic relations between both macro-families (this kind of relation has been recentl discussed by V. Dybo, V. Ivanov, S. Nikolaev et al.)

V.S.

From p. 9

→ or on methodically incorrect points of departure (Andror insists that Illič-Svityč has ignored some 'linguistic facts'; under 'facts' Andronov understands here some very shaky hypotheses discarded by most Dravidologists as incorrect. [Dolgopolsky immediately found Andronov' weak points also when he read Andronov's remarks in Ann Arbor in September 1983; he stated his opinion in some notes which ~~will~~ appear in the above collection]).

The issue in question contains 25 entries compiled on the basis o material in the late author's archives (entries 354-378).

M.K., V.S.

V. Shevoroshkin —— Language & Prehistory
THE UNIVERSITY OF MICHIGAN • ANN ARBOR MI 48109
DEPARTMENT OF SLAVIC LANGUAGES AND LITERATURES

Febr. 3, '88

Dear Hal,

Please inform the readers of the Mother Tongue that our Fund Language and Prehistory has started the preparation of the 3rd collection of articles on most ancient languages: Ancient Homelands and Migrations. It will, mostly, consist of translations of the materials of 1984 Conference in Moscow (Ling. Reconstruction and the Prehistory of Orient).

Both, papers and money contributions are welcome. Our Fund has received some money from our well-wishers which made it possible to accomplish our 2nd collection of articles Genetic Classification of Languages: A New Approach (to appear in the U. of Texas Press). Not a single contribution came from the readers of the Mother Tongue.

* * *

The NEH is providing us with funds sufficient to organize a broad international Symposium Language and Prehistory, - apparently in Oct. or Nov. 1988 (or somewhat later). Our main goal — to assure the participation of a group of Moscovite linguists, the most qualified experts in the field of distant relationship of languages (S. Starostin, E. Helimsky, V. Ivanov, V. Dybo, S. Nikolaev, I. Pejros et al.). The pioneers of Nostratic studies — Prof. K. Menges (Vienna, Austria) and Prof. A. Dolgopolsky (Haifa, Israel) will participate as well. Symposium papers will be published.

* * *

We're working on a pop. book The Mother Tongue (to appear in Addison Wesley); any notes, remarks, suggestions, etc., are very welcome. We'll finish it in Oct. '88.

Good Luck! Yours, Vitaly

Dr. Patrick Bennett
2905 Burdick Road, RR6
Janesville, Wisconsin 53545

Jan. 8, 1988

Dear Pat,

Great God Almighty! so to speak. I have been completely at a loss for words -- the appropriate words -- with which to answer your letter. However, after prolonged discussions with my wise wife and smart kids, I .... still don't know what the right thing to do is. And so I have decided to let you make the decision. This affects your life much more than it does mine. Therefore, you are the most appropriate person to take charge of this matter.

But I am going to give you some warnings first. And this is a FORMAL letter.

In the last paragraph of your recent letter of Dec.23, 1987 (my birthday) you said "Hal, you will do with this what you see fit; even if you decide not to publicise it, I would be interested in any personal responses." Throughout much of the letter, as you made clear, you were nearly explicit in requesting that your letter be published in full in the next issue of Mother Tongue. But it was like a radio station fading in and out of one's hearing. Apparently, your message was to publish your letter but there was a strong undercurrent of misgiving, or hope that I might not actually publish it. So we must make it all more explicit.

Do you definitely and unambiguously want me to publish your letter of Dec. 23rd in Mother Tongue? Since I will simply zerox it, lacking any secretary as I do, then it will all appear. Do you explicitly wish me to zerox it all?

If I do publish it, this letter will precede yours. It will be followed by a presently unwritten letter from you, granting me the permission to publish your letter. This sounds terribly formal and legalistic -- and it is. But my concern is not primarily legalistic. It is MORAL, something to me much more serious that legalism. As they say in the bureaucracies of America, protect your rear, cover your ass. One way to do that morally is to try not to hurt your friends or let THEM do things that will hurt themselves. So you have to give me permission to help you do something which may hurt you. Does that sound like a rationalization or cop-out? Well, it is not because your request puts me into a real moral dilemma.

Why? Because I have to respect your right to speak, to say things that are deeply important to you. I clearly and without kidding understand the dimensions of your present cognitive map of the world and the meaning of it to you. I once shared that map and shared that passion. Also it is our common wish that Mother Tongue be a truly free and open vehicle of scholarly communication about human prehistory. Yes, it is true that I am loathe to publish "personal stuff" because of violations of individual privacy and possible hurt feelings. And, yes, I refuse to do anything political, despite my own passions. This international Club would be torn apart by politics and/or social ideology! But otherwise -- Freiheit! (It's like Aharon's joke which begins: "In England everything is permitted, except that which is forbidden.")

I believe that you will experience pain as a result of my publishing your hypothesis. But you are a highly intelligent and mature person and an internationally-respected historical linguist. Now you must tell me what to do.

Cordially yet anxiously,
Hal

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January 14, 1988

Dr. Harold Fleming
Mother Tongue
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Massachusetts 01966

Dear Hal:

It was a thrill to me to read your letter of January 8. I understand your position, and appreciate your concern, and am MOST honored that you did not - as one part of me feared you might - simply roll up my letter and discard it. I think in a very real way it will take more nerve to print that letter than it did to write it - for reasons you outline. And I thank you and honor you.

I am writing this as a short, formal, and, I trust, sane letter. The easy and informal style possible in such a newsletter lends itself to an appearance of incoherence, or of flippancy. I may occasionally be incoherent, even when trying to argue formally. I have been known to be flippant; I am working on quitting. But I do not want it thought that there is anything of whim, or of hobbyhorse, in what I am now saying.

I understand your feeling that this might hurt me. I am not sure that you are not right - at the same time that I am sure you are wrong. I worry about consequences of some of what I have been doing. But then, how will I be hurt? Personally? My experience says that friends will remain friends whatever they may think of their friends' ideas. There is the possibility that one or two may not be the friends I thought they were; that would hurt, but it is better to know. Professionally? As a comparativist in a world of generalist generativist theoreticians, it is hard to imagine being much less respected by the general linguistic world; and we have all seen that among comparativists there is a general agreement to allow, and even admire, our brothers' expression of what we feel are slightly crackpot ideas. But in any case, to quote I Esdras 4:41 (for those with a copy of the Apocrypha handy), whatever one may feel about the book's position in the canon, "Great is Truth, and mighty above all things." Even if there were hurt in this, fiat justitia, ruat coelum.

Yes, Hal, I wish my views published; any misgivings sensed reflect doubt as to your reception as responsible editing party. And, that they may be maximally clear, let me state:

I am a scientist, with a long-demonstrated commitment to careful examination of all hypotheses

I believe, wholeheartedly, in an omnipotent and benign Creator (having reached that conclusion once I was challenged to examine the evidence fairly)

I hold that scientists - comparative linguists included despite the artistic aspects of our field - have a duty to examine carefully evidence for and against those hypotheses which follow from the twin premises that God is and that the Bible records valid data on His nature and doings.

Hal, thank you. I'll write again.

Gratefully yours,

Patrick R. Bennett
Professor

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December 23, 1987

but, Mary (my wife) likes Mother Tongue in English as given.

Dear Hal,

It must be close to exactly a year since I last took typewriter in hand to write you as glorious leader of what was not yet 'Mother Tongue' (Nyarurimi would be a nice, slightly Kikuyu-esque, Bantu appellation which I must say comes more naturally to me, and if you will use a Kamba-made figurine on the cover.... but given the emphasis of the newsletter in its present state, let me enter the announced contest by suggesting one of the stone 'Venus' figurines whose steatopygia used to get people aware of Bushmen all het up. Of course, they have no clearly defined head usually, hence no tongue, but you can't have everything. I am again reminded of the old English pub sign advertising 'The Silent Woman'.) Well, the latest issue coupled with events in my life stimulates me to write you again, on several issues. And, despite the light tone of the above and probably the light tone which will be seen below, licensed by the general tone of such newsletters and the specific tone of what I will call ours, please believe that on all substantive points to come I am (deadly) serious. If you put any or all of this in, feel free to edit, of course, but do not feel you have to. Before we begin, a great Christmas to you; the enclosed sheet, received a couple years back, states it better than we know how to say, and we have been sending them out in lieu of cards.

To work: Item, on 'Root Dating' : this sounds, as you have outlined it, much like modified lexicostatistics I have done on occasion; in weighted statistical counts I will often include semantic skewing as a minus point equal to a serious formal skewing (I hope by now even non-Bantuists are familiar with the Guthrian term 'skewed'). There are of course serious problems which remain, as you point out; see also the enclosed paper on Rössler's 'Nuclear Vocabulary' and my reinterpretation thereof, which seems to be related. THE CRITICAL PROBLEM, in all of this: linguists should not get involved in dating (except, perhaps, in their personal lives, but not as linguists). I complained of this to Chris Ehret once; he essentially responded that he understood my points, but as officially a historian he was forced to name numbers or lose credibility. I maintain that linguists should date innovations only relatively, and then with extreme caution.

Item, on the Stanford Conference; from one who was not there. It is very sad that African affairs were represented only by the Afroasiatic section (though we can guess why), and sadder that the majority of the Afroasiaticists were Chadicists and Cushiticists (I am not familiar with Faber and Lieberman and do not know their specialties). While some of these people are familiar with Berber/Egyptian/Semitic, I'm not sure how far I would trust their commentary. At such a conference, we need Semitists, and Niger-Congo specialists if not genuine Bantuists, and while I have problems with a lot of work on so-called Nilo-Saharan, I have friends in that camp who could contribute. Why so many Australians and no real Africanists (prejudice against Chadicists, I fear - at least in their capacity as comparativists, not personally)? Mind you, I am not sorry I was not there. I am - despite my hypothesis, which I am not about to work on at this point, but which I believe in, that Basque has to be linked to Niger-Kordofanian - not a true long ranger, though as I earlier said more than willing to help out and even be convinced if you can. I am no Comparative Bantu-thumper, but I BELIEVE in regular correspondence, I cringe at glottochronology and pseudoreconstructions, I firmly maintain that comparative work can reach back so far and no further. Altaicists and SOME Indo-E. etymologies bother me, I am happiest with Bantuists, Semitists, Algonquianists My sympathies are with the Americanists of your report; good for them. But let us face it. A conference of this sort is meaningless. Comparative linguists are now and always have been and while this world lasts always will be divided. There is Meinhof and there is Sir Harry. There is Guthrie and there is Greenberg. For every Fleming there is a Bennett, and probably vice versa. No conference will ever resolve what are basic personality differences and achieve consensus on the domain and techniques and standards of linguistic history. Over time, some Greenberg leaps of the linguistic eye will be vindicated by the nose-to-the-ground bloodhoundism of a Guthrie - and some will not. The next conference may be loaded the other way. Who knows? It might include Africanists. But as long as ALL linguistics is an art and not a science (try to convince me otherwise) squabbles like this will keep up. Hint - loud criticism is the first sign of a non-scientific approach.

Item, not directly responding to the last issue of Mother Tongue: How many out there believe that Neanderthal (and other anatomically not really modern men and women) did NOT speak human language(s)? Haw many care? The claim was, I believe, made that they did not/could not, I believe (please correct any misquote) on the grounds that reconstructe palates yielded non-human acoustics hence non-human phonology hence non-human language. Well, if I were to define human language (and I am NOT ready to do so) I would do it in terms of grammatical structures and presence in grammar and semantics of the non-concrete, not on phonology. I don't care if Neanderthal sounded like a gorilla and could not speak English and sound like a native; could they manipulate the basic code of English structure with alternative phonetics? And how can you test that? Personally, I think emphasis on looking at earliest dates for probably talking humans or hominids in MT is empty (pun intended) of meaning. Let us ALL please remember that languages are culture even if language is physiology, and that the genetics of Jamaican English speakers will be irrelevant to the roots of the language. EVEN IF (and see below) we assume all the worlds's myriad speechforms to have a sole origin, the nature of things (see also Gould's twiggy article in the last issue) prevents us from assuming that origin to be contemporaneous with either the dawn of humanity or with Gould's non-Eve ancestress.

Item, here comes the big one. I write this part with some trepidation, and ask to be understood as perfectly serious even if nerves force me to be jocular in places. As said, I am not by nature a long ranger. I also have a longstanding fear of being seen as crackpot, which is part of my not being a true long ranger. But if we are to try to be scientists (and while basically artists, we do try to be as scientific as possible), we need to examine all the hypotheses on the table, however crackpot at first sight. Some of them prove not to be so crackpot. Take the Omotic question - I was extremely dubious about the Omotic as Afroasiatic hypothesis - until certain data you published, Brother Hal, let me accept Omotic without reservation.

So, last year, one item you repressed as personal from my letter to you was that I 'got religion'. This is relevant here only in that it has forced me to reassess my stand on a lot of issues, one being the reliability of Genesis, including the Creation/Evolution fight which still goes on (ask my brother the paleontologist about it). Now, where I stand at present is not the ultimate 'believe it or else' position. I once heard someone say 'You cannot call yourself a Christian unless you believe every word of the Bible is literal truth.' Well, I'm afraid that falls, given for example the contradictory genealogies for Christ in Matthew 1 and Luke 3, the which contradiction may be why Paul tells Timothy 'Neither give heed to fables and endless genealogies, which minister questions, rather than godly edifying which is in faith". It certainly is not my earlier 'ignore it' position; on my personal experience, I cannot ignore that part of scripture that focusses on rebirth and remission of sin, and if I cannot ignore that I need to be very careful about deciding what parts to ignore. Neither am I one of those apologetic types who, for example, say that we do not need to take a day as equal to a 24-hour period, so that all the eons of time the geologists and big-bang astronomers want can be reconciled with the 6-'day' creation. I was not brought up to twist things that way - there is a conflict between 6-day creation and big-bang theory, and I see no point pretending the conflict is not there, anymore than I can see pretending the conflict does not matter because Genesis is obviously true and all else must be disregarded or Genesis is patently ridiculous and has to be ignored.

I do want to say that I believe the Bible to be a message to us from God, but that not every word of every translation of every version need be literally and simultaneously accurate record of fact; that I believe in the power of God to have done exactly what Genesis says, and in the fallibility of scientists EVEN WHEN AND IF ALL SCIENTISTS AGREE - and that will be the day, fellow scientists!

single inv.

Linguistically, I find it hard to accept multiple inventions of human language; ~~which~~ implies the once existence of MOTHER TONGUE. But I find it equally hard to find any evidence that there was such a single ancestral language, and do not believe that any creditable linguistic research can demonstrate its existence. So let me quote one text and lay on you some hypotheses, which may be accepted, tested, or rejected, as all long and short rangers reading these words see fit.

"Go to, let us go down, and there confound their language, that they may not understand one another's speech." So the Lord scattered them abroad from thence upon the face of all the earth: and they left off to build the city. - Genesis 11: 6-9

Item: it is pretty generally assumed the tower of Babel was a ziggurat - some specify one particular ziggurat in Babylon; I find that explanation a little weak, because there are loads of Ziggurats around Mesopotamia and no Canaanite visitor is really about to believe was left incomplete because of divine intervention.

Item: it is easy to forget that this is AFTER the flood and Noah (please note that Gould's mitochondrial ancestress does not have to worry about being Eve, she is more likely to be 'Imm Shim, as we may denote Noah's wife!); it is also easy to ignore the dispersal that is indicated as accompanying the linguistic diversification.

So, hypotheses:

A. It is perfectly possible, assuming an omnipotent and benevolent God, that linguistic diversification and physical dispersal may have occurred at this time exactly as is described.

B. Language families exist which can be based on regular correspondence (e.g. Semitic, Bantu, Berber, Algonquian); other language groupings seem to be valid, but cannot be supported by regular correspondence (e.g. Niger-Congo, Afro-Asiatic); non-correspondent groupings often include as subgroups correspondent groups, and are linked generally by typology and by a relatively small number of often highly skewed shared morphemes.

C. *DANGEROUS SPECULATION - I will try no other dating : impressionistically and tremblingly I estimate time depth - maximum time depth - for regularly corresponding groupings as about 5-6 thousand years BP. This is based on Semitic/Indo-E primarily, and on estimates of relative differentiation in these and other language families where regular correspondence works. I would hate to have to defend it, it is NOT glottochonology based, anyone is free to shoot at it.

D. *INTERESTING COINCIDENCE: my readings of accepted prehistory baffle me with innovations like changes in flint industries and the Neolithic revolution which seem to appear over an unreasonably large portion of the globe in the twinkling of an eye. Look at how long it took to get the Industrial revolution spread, when that also revolutionized transport. Do we assume relays of Paleolithic couriers jogging along with the latest handax pattern? It looks like relatively little local innovation or steady development over time, just great leaps forward rapidly spread. And if I read it right, the first cities as opposed to villages (and the Babel story may be taken to reflect the start of urban life) date from about 5-6 thousand years B.P.

E. We know that linguistic change is an on-going process. We do not understand it. We can be sure it is not truly random. We can be sure that it is not predictable. It almost certainly is not uniform in rate. Can we explain it in terms of random sequencing of changes based on human-universal tendencies? I think not. It does not get explained logically nor statistically, it is more complex than economics. I put it to you that we should not rule out divine intervention (Creationism) as a factor along with randomicity and human universals (Evolutionism).

F. If Genesis 11 is worthy of belief, we must expect that an ancestral tongue will NOT be reconstructable. I will not - at this point - get into why then ANY language families are recognizable. Intuitively, I doubt the reconstructibility of MT; can anyone with an understanding of random processes like Brother Gould's demonstrate how long it would take random linguistic change to make MT unreconstructible? Can anyone defend the idea that linguistic change is truly random?

G. On dating: Every dating technique I have heard of, from glottochronology through mitochondriochronology, has to make the same UTTERLY UNWARRANTED ASSUMPTION: that the rate of loss/change/deposit/mutation has been constant throughout time. But I understand that where C-14 dating (assuming a constant ratio of atmospheric C/12/C-14 which I think is sunspot controlled or something?) has been checked against tree-ring counts it turns out to fluctuate noticeably. I am given to understand that language evolution (like, I am told, physical evolution) is NOT steady but comes in some cases in rapid bursts and in others in long stagnant periods - long live Lithuanian speaking coelacanths, as well as Anglo-Creole speaking Galapagos islands birds. And I have found in working with lexicostatistics that at the lower percentages of cognacy - for the glottochronologist implying the earliest datings - relationships are most unreliabl So I am not surprised that our early prehistory involves such shatteringly slow and wearying development - as currently dated. And I don't want to find ways to squirm out of that. Let the Bible say what it does; let the C-14 count be as it is. With God all things are possible.

Finally, I just want to say this for your consideration: why are we doing this? Not for fame or fortune - if there is any of either in linguistic prehistory I've missed it. For fun? Maybe. But ask ourselves, each of us: why are we talking about universals when there are languages still undescribed? Why are we more concerned about the pronunciation of Indo-E aspirates and Afro-As Emphatics than we are in the news from Central America and down-town New York? Why are we more concerned with changing our colleagues' minds on the origins of language and language families than we are with changing ourselves? Maybe no one else ever hid like an ostrich in linguistic prehistory - good, but I did, and I'm starting to wake up and look at the real world.

Hal, you will do with this what you see fit; even if you decide not to publicise it, I would be interested in any personal responses. Goodbye for now - God bless you.

Love,

[signature]

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December 29, 1987.

FACULTY OF HUMANITIES
DEPARTMENT OF LINGUISTICS & AFRICAN LANGUAGES

Professor Harold Fleming
Mother Tongue Newsletter
69 High Street
Rockport, Mass. 01966
U.S.A.

Dear Hal,

I was delighted to get MT (November 1987) today and I like the cover. Shouldn't you put the mailing address on the insider cover? (I used Ms. Reitz's letter this time.) Clear numbers for the issues and pages would help references.

I would like to get your readers' opinions on Worlds in Collision, by Immanuel Velikovsky. I read this in my teens and classified it mentally in a kind of intellectual limbo as 'fascinating but unproven'. I recently re-read it and felt there was a strong case that should be examined. He argues that within historical time, as well as before it, various world-wide catastrophes were caused by collisions or near-misses between Earth, other planets, and comets. The catastrophes include drowning of huge land areas by tidal waves, creation of mountains, hurricanes, fire, reversal of East and West, sudden climatic changes, changes in the length of the day and the year, etc. - all reflected in mythology in many parts of the world, in the Old Testament, etc.

I have the impression that catastrophic theories - about the extinction of the dinosaurs, mammoths, etc. - are now much more widely accepted than previously. Do our archaeologists and scientists think Velikovsky could be right to a considerable extent? If so, the extinction of Neandertal man, Java/Peking man, etc. (as argued by Gould) could be due to such catastrophes. Again, if the existence of Atlantis and its archipelago is correct, there could have been a route from Africa or Europe directly to South America for the inhabitants of the 'too ancient' sites you mention, independent of the Bering Straits.

Obviously, basically different land formations at earlier periods would much affect our view of likely migrations by Homo spp. How about it? Velikovsky also wrote Ages in Chaos, which I have not read.

Yours sincerely,

Kay Williamson.

Professor Lyle Campbell
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Jan. 31, 1988
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Dear Dr. Campbell,

Thank you for your letter of January 17th. It was good of you to write and I appreciate your response.

Although the agenda for our next newsletter, Mother Tongue 5, is actually excessive already, still I will try to squeeze your letter in. I am taking the liberty, mostly in my function as editor, of giving Joe Greenberg or Merritt Ruhlen a chance to respond to your letter. So their letter, if any, plus your letter plus this letter will appear in Mother Tongue 5 or 6. #6 will be the computer issue but it may be the first chance we get. I trust that I have your permission to reproduce your letter, given your sentence: "In fact, I really hope you might consider reproducing this in your next Newsletter."

These are very serious matters and I try to maximize our newsletter's stress on honesty, openness, freedom and tolerance -- in pursuit of a transcendental scientific and humanistic goal, to wit, the discovery of our ancestors. That goal has been so difficult to achieve or to strive for vigorously because there have been so many people telling us that it could not be attained or that we should not try because only idiots would "open up that question again." Yet one can wonder how such an interesting and important question got shut off in the first place! Who in hell are the scholars of the French Academie anyway to tell us we cannot look for our roots? It is a profoundly legitimate pursuit which has been stifled by several generations of sophomores and small-headed methodology freaks.

Your attitude of friendliness and cooperation is appreciated. Let us bury the hatchet, as you Americanists are wont to say, but not in each others heads -- Inshallah. It has been only the perceptions of hostility towards Greenberg and his work that have provoked us, many of us, to respond in kind. As you yourself may already know, Joe Greenberg is the original "soft-spoken and shy" person. There are an awful lot of linguists, Africanists and cultural anthropologists who like him very much. Try to think of them as a swarm of bees.

Oh, yes, by the way. Since you mention some 32 Americanist sources which Greenberg neglected to cite or (possibly) even look at in his LIA, could you be good enough to send me a list of them in rough or casual form (good enough to find in a library)? For publication in Mother Tongue? Then our members, most of whom are not Americanists, can peruse some of your-plural arguments and attitudes towards linguistic taxonomy and reconstruction.

I have to tell you that it will come as no surprise to Africanists or Oceanists to hear about Joe Greenberg's shortcomings as a bibliographer. He neglected to mention many more than 32 in both Africa and Oceania. Yet it made precious little difference in the truth value of his hypotheses. As they say in youth Americanese "Hey, he's just not into bibliographies!". But it is also true of MANY contemporary Africanists. They neglect each other's work, or older work, shamefully. What can we do? Standards are lower nowadays? Well, maybe it is really not so important? Remember Van Gennep's famous problem?

Sincerely,

Harold C. Fleming

Harold C. Fleming

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Jan. 17, 1988

Prof. Harold Fleming
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Dear Dr. Fleming,

I'm sorry that we have not met or talked, but I have heard only good reports about you from students and friends at Boston University, which makes me believe that it will be all right for me to write you. I guess ours is a small discipline, since I've been sent copies by four different colleagues/friends of your report in Mother Tongue 4, "THE STANFORD CONFERENCE: as seen by Hal Fleming" (with comments about it from several others). I hope you'll permit me to say something about your report. In fact, I really hope you might consider reproducing this in your next Newsletter.

You perceived it as an "ambush" on Prof. Greenberg, but the American Indian participants in the conference had the opposite intentions. We wanted very much to address the goals of the conference, what American Indian historical linguistic practice has to offer historical linguistics in general, particularly reconstruction. We did not want discussions of Greenberg's new book to take up all our time, preventing us from attending to the mission of the conference. This was appropriate, since Prof. Greenberg does not attempt reconstruction in his book, rather only classification. For this reason, we purposefully scheduled our topic of distant genetic relationship, of which discussion of Greenberg was only part, granted a large part, as the absolute last in or program, so that it would not prevent us from addressing the conference goals.

You took something I said as a "personal" and "vile" attack, reporting it as "something like ... 'Greenberg is lucky that he had Stanford University Press to publish his Amerind book because no one else would have touched it!'" I'm sorry you perceived it this way. The actual text of what I said might be helpful in clearing up misperceptions. I intended to present an argument and then to ask a telling question based on the argument. The text of my presentation, in context, was:

> Several American Indianists ... are characterized as being hostile and unwilling to entertain any proposal of distant genetic relationship. This is, however, not the case. Several proposals of remoter relationships were supported by authors in Campbell and Mithun 1979 (e.g. Campbell: Jicaque and Tequistlatecan, Paya and Chibchan; Crawford: Yuchi with Atakapa and Tunica; Davis: Keresan and Uto-Aztecan; Jacobsen: branches of 'Hokan'; Krauss: Eskimo-Aleut and Chukotan (Chukchi-Koryak-Kamchadal); Langdon: Pomoan and Yuman; and Thompson: Kutenai and Salishan). While not all of these are of equal strength, the attitude, contrary to

that attributed to us, has not been one of resistance to any "lumping" proposal in principle, but rather, is one of realism, a sober request for reasonable supporting evidence.

Greenberg's enthusiasm for his own technique at the expense of standard methods and to the tune of nearly total disregard for most recent work in the field is unfortunate. G's attitude seems to be that the work of American Indian specialists is without merit, since he disregards the work of the last twenty years or so. That is, a perusal of LIA's references reveals few from this period; the only citations from the 1980's are to G himself, or to non-linguistic works, save one review of LNA which mentions G favorably. Most cited from the 1970's are not about American Indian languages, i.e. on the philosphy of science, on African or other non-American languages, or on other anthropological themes. The articles of Campbell and Mithun (1979) are listed, but it is clear from LIA and the Greenberg notebooks, that these were not utilized. Of the few American Indian linguistic works cited from the 1970's (less than ten) most treat South America. How can G, with such an ambitious task, afford to ignore the work of the last twenty years? Why would he ignore works essentially supporting (actually predating) some of his conclusions? More importantly, how can he disregard proposals which are in conflict with his own? Just a few gaping absences, neglected by LIA, are: Berman 1983, Bright 1976, 1984, Campbell 1973, 1975, 1976, 1978a, 1978b, 1980, Campbell and Oltrogge 1980, Campbell and Kaufman 1981, 1983, Constenla 1981, Golla 1984, Justeson et al. 1985, Kaufman 1973, 1974a, 1974b, Klein and Stark 1985, Sherzer 1976, Shipley 1980, Sorensen 1973, Suárez 1974, 1975, 1979, 1983a, Whistler 1977. These absences are all the more shocking, since some criticize methods such as G's directly (cf. Callaghan and Miller 1962, Campbell 1973, Campbell and Kaufman 1981, 1983, Goddard 1975, etc.).

In light of this disregard for the work in the American field, it is indeed surprising that a publisher of the calibre of Stanford Press agreed to publish LIA; it is tempting to speculate that this would not have been possible if the book did not bear G's name. A scholar of lesser renown would not have been permitted to slight the canons of scholarship in this way.

As you see, I did not intend a personal attack; however, I hold my opinion as stated in the text that I am shocked by this neglect of the scholarship of an entire field for the last generation or so. I really do find it difficult to understand how such a book in its current state could have been accepted for publication. Others share my opinion.

Finally, I'm sorry you found me to be a "loud" and "aggressive" expert -- most know me to be soft-spoken and shy. For this very reason, I had none of the since of the home court advantage you describe, being on "the expert's own turf". Quite to the contrary, I was at Stanford, Greenberg's home court, and I had the sensation of walking into the lion's den. I really mean it when I say I wish Greenberg had come to participate with us -- he had been invited. I would have much preferred to say my piece in his hearing and then sat down to talk about it. In spite of appearances, I actually like him and his non-American Indian work very much.

About the other matters you mention in your report, I think it would be fun some day to talk about them with you, though I won't take up more time now. Meanwhile, since we are both interested in similar things, I hope we can stand on friendly terms; cooperation is less taxing that quibbling, anyway.

I wish you all the best.

Sincerely,

Lyle Campbell

Lyle Campbell,
Professor of Linguistics,
Anthropology, and Spanish

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February 12, 1987

Dear Hal,

I welcome your invitation to comment on Lyle Campbell's letter concerning his presentation at Stanford this past summer. I have already discussed Ives Goddard's performance at this meeting elsewhere (''Is Algonquian Amerind?,'' to appear in Genetic Classification of Languages, ed. by Vitaly Shevoroshkin, University of Texas Press).

Campbell goes to great length to excoriate Greenberg for his disdain of ''standard methods'' and ''nearly total disregard for most recent work,'' which taken together vitiate, for Campbell and Goddard at least, Greenberg's tri-partite classification of New World languages. Campbell's assertion that Greenberg uses his own special ''techniques'' at the expense of the standard methodology of comparative linguistics repeats an old allegation that is simply false. Greenberg's ''techniques'' are nothing more than the comparative method itself and certainly Greenberg has never claimed otherwise. What distinguishes Greenberg's work is the breadth of the application, not the techniques themselves. Until Campbell spells out more clearly what he takes to be the differences between Greenberg's methodology, as elucidated in Chapter 1 of Language in the Americas, and that of traditional comparative linguistics, it is impossible to say anything further on this topic.

The brunt of Campbell's criticism is clearly directed at Greenberg's ''shocking'' disregard for the Amerindian literature, which would, or at least should, have prevented Stanford University Press from publishing the book, had it not been for Greenberg's famous name. This is a rather serious accusation, impugning as it does both Greenberg and the Stanford University Press, and before making it Campbell should have read the first paragraph of the section entitled ''A Note on Methods of Citation and Notation'' (p. xv):

> In preparing this work, I used a very large number of sources, particularly for the comparative vocabularies. Listing all these sources in a general bibliography would have added greatly to the length and cost of the work. Hence only those sources actually referred to in the text--which are far fewer than those employed in the research--are contained in the References Cited at the back of the book.

In the Preface (p. ix) Greenberg estimates that the Comparative Amerindian Vocabularies upon which his book is based ''encompass well over 2,000 sources and contain perhaps a quarter million separate entries.'' If this constitutes ''disregard for the work in the American field'' the field has yet to be regarded. But even if Greenberg had completely overlooked the thirty-odd sources Campbell mentions, does anyone seriously believe that these few sources would have materially affected any aspect of the classification Greenberg proposed, much less the central point of the book, Amerind unity?

The most telling point of the Goddard-Campbell presentation was a question from Russell Schuh: How can it be that Greenberg's taxonomic work on African languages was so spectacularly successful, while that on American Indian languages is without any merit whatsover, since both were produced with the same methods? Goddard and Campbell were unable to come up with any satisfactory explanation for this dilemma, their suggestions (Greenberg is an Africanist, he's not an Amerindianist; the American literature is abysmal, the African must have been better; etc.) were rejected one by one by the audience until they came to their final response: ''well, maybe New World languages are just harder to classify than African languages.'' This response was met with silence, with good reason.

What struck me most in Campbell's presentation was his statement that he and his colleagues had been looking forward to Greenberg's book, had been hoping for the best, but had simply been disappointed by a poor piece of work. A year before Greenberg's book appeared, and without ever having seen the evidence it contained, Campbell had called for Greenberg's classification to be ''shouted down'' (Current Anthropology 27 (1986): 488). I do not believe such intemperate language is ever called for; we do not have to shout at those with whom we disagree. But to condemn someone's work without even bothering to examine his evidence violates the ''canons of scholarship,'' as I understand them, and undermines whatever credibility Campbell might otherwise have had in the discussion of the Amerind family.

Sincerely,

Merritt

Merritt Ruhlen

28 Jly '87

Illinois State University

College of Arts and Sciences
Department of Foreign Languages

Dear Prof. Fleming:

Thought you might be interested in the enclosed. This will form part of a book which Prof. Shevoroshkin and I are now working on.

This can only be a guide to Illich-Svitych's work — the omission of detail ~~to~~ to some extent damages the credibility — but anything is better than nothing for those w/ little access to the Russian.

Please feel free to distribute or have interested individuals refer to me.

Kindest regards,

Mark Kaiser

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Note: Main text not sent to colleagues who already know I-S's work.

Hal

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