MOTHER TONGUE

NEWSLETTER OF THE ASSOCIATION FOR THE STUDY OF LANGUAGE IN PREHISTORY

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ASLIP is a nonprofit organization, incorporated under the laws of the Commonwealth of Massachusetts. Its purpose is to encourage and support the study of language in prehistory in all fields and by all means, including research on the early evolution of human language, supporting conferences, setting up a data bank, and publishing a newsletter and/or journal to report these activities. Annual dues for ASLIP membership and MOTHER TONGUE subscription are US 15 in all countries except those with currency problems. In those countries the dues are ZERO. European Distribution: All members living in Europe and the former USSR will pay their annual dues to, and receive MOTHER TONGUE from Professor Dr. Ekkehard Wolff, Universität Hamburg, Seminar fur Afrikanische, Sprachen und Kulturen, Rothenbaumchaussee 67/69, D-2000 Hamburg 13, DEUTSCHLAND (Germany).

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OBITUARIES

Well, good colleagues, we have lost three more outstanding scholars. Two of them were long rangers and also friends. The area of the world sometimes called 'Circum-Mediterranean' has been deprived of their work, most especially that part of it usually called the Near East.

Stephen J. Lieberman was first a Sumerologist, secondly a Semiticist, and thirdly an Afrasianist. Many of you probably read his obituary on Samuel Noah Kramer in MOTHER TONGUE and so could see for yourselves how clear his writing was and how formidable his scholarship. It is ironic that Stephen, who mentioned Jacobsen's work in the Kramer obituary and was himself Jacobsen's student, should have his own obituary written by his teacher!

Hans Mukarovsky excelled in those virtues for which Vienna is justly famous, warmth, kindness, and hospitality. Like many of you I will never forget his 'Reinisch' conference in Vienna where an international group of scholars worked hard but left Wien muttering how much they enjoyed themselves and wondering if somehow they could get a position in Vienna! His great city is also famous for its scholarship; indeed only a few generations ago flocks of Americans studied medicine there (or in Germany). How many other cities spawned the likes of Freud and Carl Hempel? Hans studied Basque long and hard, finally rejecting (but reluctantly) the oft proposed connection with Caucasic. His very solid effort to show a trans-Saharan family tie between Basque and the Mande sub-phylum of Niger-Congo was unsuccessful in its time. We Africanists were unable to cope with this new idea what with the Greenberg classification just emerging showing Niger-Congo as the most likely source of the Mande group. But the Mukarovsky hypothesis was let down gently and with respect by most of us. His idea that Berber was also related to Basque has been supported by Vaclav Blažek. Not remarkably he may ultimately be proven correct in all these matters because the blood groups (Rh, MN) indicate old ties of some sort from Iberia to Liberia.

Thorkild Jacobsen was America's other great Sumerologist. He was not a long ranger or ASLIPer. Nobody ever asked him to join us. (Also true of Kramer. ASLIP is not famous for its systematic recruiting.) Jacobsen was sometimes spoken of with awe, as a scholar with such high standards that few graduate students at Harvard achieved the doctorate under him. In fact there were only three, Stephen J. Lieberman being one of them.

We lack space enough to do adequate obituaries with full bibliographies on these three sterling fellows. We do no obituary of Thorkild Jacobsen but we have borrowed heavily from his own obituary of Stephen J. Lieberman. The University of Pennsylvania in the person of the chairman of the Department of Asian and Middle Eastern Studies will furnish bibliographies of Stephen's work to interested scholars. His widow, Ms. Joelle Wallach, wishes to sell his great library. If interested in buying the whole library (20-30,000 books + many journals + some rare books), contact her. Ms. Joelle Wallach, 761 Raymond Avenue, St. Paul, Minnesota 55114, USA. If you wish to phone her, and your inquiry is serious, call Hal @ 412-683-5558 to get her phone #.

STEPHEN J. LIEBERMAN

Stephen was a Minnesotan, another of those gifted historical linguists from that state. Born in March 1943, he died from a heart attack shortly before his fiftieth birthday in March 1993. Entering the University of Minnesota a bit too early to experience the raucous 1960s as an undergraduate, he studied linguistics and Greek. As the high tide of Chomskyite theory was sweeping linguistics in those days, Stephen apparently was not swept along. After graduating cum laude, he studied for a year at Hebrew University in Jerusalem before entering Harvard for his graduate studies. He put down his roots in the ancient Near East and received his PhD in Near Eastern Languages and Literatures from Harvard in the early '70s.

Jacobsen believed that Stephen was inspired to take up Assyriology by the lectures of Tom Jones at U/Minnesota. Jacobsen said that Stephen was "one of the most learned and original scholars of his generation." It was a widely shared opinion. Stephen won many honors, being elected a Fellow in Mesopotamian Civilization of the Baghdad Center Committee of the American Schools of Oriental Research (1970-71); being a Fellow of the National Endowment for the Humanities (1975-76); being Visiting Fellow at Princeton University (1979-81) & John Simon Guggenheim Memorial Foundation Fellow (1979-80); then Inaugural Fellow, the Foundation for Mesopotamian Studies (1980-82). He received grants for travel or research from the American Council of Learned Societies, the American Philosophical Society, the University Museum of the University of Pennsylvania, New York University's Art and Science Research Fund, and an Institutional Grant from the National Science Foundation (USA).

He held positions of Ass't. Professor and Assoc. Professor of Hebrew at New York University before joining the Sumerian Dictionary Project in Philadelphia in 1976. Assoc. Prof, then Professor at Dropsie College (1982-86). He held visiting professorships at the Jewish Theological Seminary of America (1983-84) and at the University of California at Los Angeles (1989-90). From 1981 until his death he served once again on the Sumerian Dictionary Project as a Research Associate.

Of Stephen's Sumerian work Jacobsen singled out his "meticulous study THE SUMERIAN LOANWORDS IN OLD-BABYLONIAN AKKADIAN (Harvard Semitic Series no.22, Missoula, 1977) which constitutes a veritable mine of materials for the study of Sumerian phonetics, materials that are still to be worked up and made use of. The book also has a long, highly original analysis of the nature of cuneiform writing." Jacobsen was also very impressed with Stephen's 1986 study of the stem-afformative of the Semitic and Afro-Asiatic verb. (In BIBLIOTHECA ORIENTALIS 43, pp. 577-628). (I thought that was one of the best papers on comparative Afroasiatic morphology that I have ever seen. We wanted to re-publish it in MOTHER TONGUE but couldn't. HF)

"He will be sorely missed by all who knew him." That was Jacobsen's last comment. Indeed, he will be missed! He was a very fine, warm man -- a real Mensch -- and I liked him very much. I also recommend his library which has enough Judaica and classical Near Eastern material to support a new department somewhere.

Der Dekan der Geisteswissenschaftlichen Fakultät der Universität Wien hat die traurige Pflicht, vom Tode des

O. Univ.-Prof. Dr. phil. Hans Günther Mukarovsky,

Ordentlicher Universitätsprofessor für Afrikanistik, Wirklicher Hofrat,

GrOffK d. mex. OvAA, KmdK d. belg. LO II., poln. VO III. Kl.,

KmdK d. sen. NOd. L, ägypt. VO II. Kl.

am 29. November 1992 Mitteilung zu machen.

Das Begräbnis findet am Donnerstag, 10. Dezember 1992 um 13 Uhr auf dem Hietzinger Friedhof, Aufbarungskapelle 2, Küniglberg statt.
Eine Seelenmesse wird am Samstag, 12. Dezember 1992 um 8 Uhr in der Gersthofer Kirche St. Leopold, 1180 Wien, Bischof Faber-Platz 8 gelesen.

Die Fakultät verliert in Professor Mukarovsky einen hervorragenden Wissenschaftler. Sie wird ihm stets ein ehrendes Andenken bewahren.

Der Dekan:
O. Univ.-Prof. Dr. Herwig Friesinger

IN MEMORIAM HANS G. MUKAROVSKY, 1922-1992

The African Studies enterprise has known Professor **Hans Günther Mukarovsky** as a fine, creative scholar and a warm, understanding teacher. His untimely demise on November 29, 1992, shortly after his seventieth birthday, removed from the European Africanist scene one of the very extraordinary students, scholars and upholders of our field.

Hans Mukarovsky was born in Vienna on October 2, 1922, and in 1940 enrolled in African Languages, Cultural Anthropology and Arabic at the University of his home town from which he received his doctorate with a dissertation on Kissi grammar in 1949 *(Die Sprache der Kisi in Liberia: Abriß einer Grammatik mit Texten und Vokabular, bearbeitet nach Aufzeichnungen von Dora Earthy)*.

Five years later, Mukarovsky accepted an assignment to teach African languages, initially Swahili, Hausa and Fulfulde, at the same university and established himself with a "doctorat d'Etat" thesis, *Die Grundlagen des Ful und das Mauretanische*, in 1963. At this period of time already, he had written extensively on subjects of debate as receding, in terms of historical insight and theoretical orientation, as suffixal conjugation in West African languages and the sociopolitical trajectory of Katanga. By nature a peace-maker, Professor Mukarovsky stood for intradisciplinary, transdisciplinary and intercultural cooperation and mutual appreciation. He was, furthermore, among the very foremost to draw open the doors of the Austrian post-bellum general public to Africa in ascending cultural and political emancipation.

The year 1977 witnessed the publication of Mukarovsky's two-volume *A study in Western Nigritic*, the work by which he was perhaps best known in the English-speaking moiety of African language research, as well as the establishment of the "Institut für Afrikanistik der Universität Wien" the directorship of which he exercised until 1991/92. *Mande-Chadic common stock. A study of phonological and lexical evidence*, much more than a topically refined follow up, appeared in 1987. (The editors of the Professor's Festschrift are fortunate to have received his conscious and kind acknowledgement of that publication in November last: cf. *Komparative Afrikanistik: sprach-, geschichts- und literaturwissenschaftliche Aufsätze zu Ehren von Hans G. Mukarovsky anläßlich seines 70. Geburtstages; Herausgegeben von E. Ebermann & al.* Beiträge zur Afrikanistik 44; Vienna, AfroPub 1992).

The founding professor of the academic study of Africa in Vienna gave shape to a toddling, yet academically independent discipline by ways of lingually vivid and intellectually absorbing series of lectures and by virtue of his truly egalitarian principles of leadership. The magnamity of devotion to language and man alike was typical of Hans Mukarovsky, as generations of students and colleagues will testify.

Professor Mukarovsky's friendship and his scholarly stimulation will be missed by all who were privileged to know him, whether personally or through his contributions to the Central European brand of African Studies, Africanistics. His memory is best honored by a constant awareness of all methodological and substantive facets of our common body of knowledge, whether derived from the sciences of language and literature or culture and society, a candid openness that more than anything else characterized the heart, the mind, and the colorful personality of Hans Mukarovsky.

[by <u>Karl E. Thomanek</u>, University of Vienna]

SYMPOSIUM ON THE PACIFIC RIM, WEST

We asked fifteen prominent scholars of the languages of southeastern Asia and the southwest Pacific to give us their opinions on the taxonomic questions confronting us in those areas. As is the nature of these things the replies did not flow in rapidly. Nevertheless five scholars did give opinions of substance; three of them (Geoff O'Grady, Paul Benedict and Robert Blust) gave lengthy opinions of much value. Two (George Grace and H-Jurgen Pinnow) had highly important opinions contained in relatively brief communiqués. Above all, probably, Geoff O'Grady did the most work and produced the most startling conclusion. We also gained gratis two other opinions; one from Paul Sidwell down Melbourne way and the other from an outraged e-mailer. Finally, we opted not to cull opinions from the earlier publications of Norman Zide and Gérard Diffloth. Both abstained from the Symposium for reasons known to themselves but not to me.

Paul Benedict leads off because he certainly ranks as the pioneer in this part of the world. We've reproduced Paul's letter (the appropriate part) in its original typescript. One has to encounter one of his letters to appreciate his vitality. It has not been possible, nor will it probably ever be, to reproduce the debate between Roy Andrew Miller and Paul over the classification of Japanese. References to them are appropriate here.

In ACTA ORIENTALA 52 (1991): 148-68 Miller wrote a review article, entitled "Japanese and Austronesian". To give us a more vivid look at the style involved let us quote Miller's first sentence. Then we can quote another sentence in Paul's rebuttal. Miller began: "A glance through this astonishing little book immediately reveals that it deserves neither serious attention nor scholarly review; but given the present deplorable state of Japanese comparative studies, it is certain to receive the former, and so regrettably it becomes necessary to subject it to the latter." Professor Miller plays very rough! As Paul has said, "Roy is accusing me of undermining Western civilization!"

Paul's rebuttal was in LTBA (I have no volume # or year); it was entitled "Miller: all about Japanese. A review of a review" After commenting on Miller's devastating review of Benedict's SINO-TIBETAN: A CONSPECTUS which increased the book's readership greatly, Benedict said: "We come, now, to my third book, JAPANESE/AUSTRO-TAI (JAT), which RAM as a Japanologist was sure to review. Again his review takes up a lot of words and he finds absolutely nothing good about the book. He clearly has read parts of the book, as he must also have read sections of the CONSPECTUS but he apparently skipped some of the author's favorite passages . . ." The Benedictine response is remarkably gentle which might be attributed to modesty or terror. But many scholars, including Miller himself, would attribute the mildness to Paul's confidence and élan vital. 'Why should the oak tree concern itself with the sow scratching her back on its bark?" (Courtesy of Hermann Jungraithmayr. German translation courtesy of Fritz Ringer.)

Heinz-Jürgen Pinnow's letter follows, partly to show you that his obituary was premature. The German we leave to you. He strongly doubts the Muskovite reconstructions of Na-Dene and fails to support Dene-Caucasic. No other opinions (e.g., Nahali or Austric) because he hasn't looked at the data in years.

12/20/92
PAUL BENEDICT

Hal Fleming request of 9/21/92:

Austro-Tai: now substantially strengthened in many respects, with the discovery of incorporated morph. elements in KD and a solid YOU root to go with the previous I, along with a large number of newly uncovered cognate sets of various kinds.

MY is still the 'odd man out', an early split from PAT, but the relative positions of KD and JR with respect to AN remain arguable even though in JAT I place Jp. closer to AN (Austro-Japanese).

To use Matisoff's terminology, ST and AN are mega-groups while AT is an expanded mega-group whereas Dene- (or Sino-) Caucasic and others of that sort dealt with by long rangers are megalo-groups.

ATLC (1975) now badly out of date and should be used only with great caution (like write me about any roots you want to use); I do hope to get out updated version before very long and in the interim you should check with updated things I included in JAT; see also my long essay on Comparative Kadai: The Rules of Engagement, in Edmondson, J. A. and D. Solnit (eds.). Comparative Kadai (Dallas: SIL/UTA Series in Linguistics, 1988) and the numerous articles by me on various KD problems/roots in the KADAI journal (ed. by Edmondson), Vols. 1-4, many citing PAT-level roots. Three major KD groups have now been reconstructed: Tai, Kam-Sui, Hlai (≃ Li: Hainan), with a fourth (Gelao) now under way, hence substantial PKD-level recon's now available and will continue to improve rapidly, with several scholars in field. Significantly, the changed recon's (from those in ATLC) have consistently improved the cf's with AN, e.g. the newly reconstructed P-Hlai *l̥ is found in two cognate sets to date: *lou^A < *lu^A (reg. shift) 'eight', PAN *walu; *l̥iu^C 'sell', PAN *tsal̥iu 'exchange/buy/sell', both showing KD regular Canonical Reduction-on-the-Left (CRL), contrasting with the CRR that is typical both of MY and of JR; see below. This consistent improvement as our recon's get better is a hallmark of (genetic) cognates as opposed to 'look-alikes' or 'comps' (abbr. of Matisoff's 'comparabilia'); the basic rule here must be emphasized: 'LOOK-ALIKE'S LOOK LESS AND LESS ALIKE AS WE LEARN MORE AND MORE. We all - and esp. long rangers - are dealing with lang's and language families for which the recon's are grad. getting better and better; review your own proposals: for given roots, as the recon's have improved have these roots looked better and better - worse and worse? To cite one key example from SEA: for a proposed Austric, PMP *mata 'eye', PMK *mat look good yet even here the syllabic reduction remains unexplained since, unlike the monosyllabic KD, PMK is more disyllabic than monosyllabic and thus a simple CRR won't do; the later Formosan evidence has yielded PAN *maCa, with *C a cover symbol (Dyen) that ANists prefer not to talk about but that KD evidence makes clear, at least at the PAT level; PKD *(m)pra^A < PAT *mapra, whence PMY may^C < *maya, with -y for -pr-, and Jp. (another CRR lang.) me, Old Jp. më < *mai (reg. shift), paralleling MY; all this very good for AT but bad, very bad for Austric; what's even worse, the Munda evidence (Pinnow) points to an earlier PAA-level *mɔt, closer to PST *myak! In the case of Austric, clearly, the anser is: worse and worse.

Date for PAT? Think in terms of 5-6,000 BC, give or take a millenium or two. All the evidence, incl. that from prehistoric sites, points to an AT homeland along the coast of the South China Sea, with movements offshore as well as inland, mainly to the west and south. The Formosan lang's retain phonol. distinctions not present in MP, e.g. *C < *cluster (see EYE, above) vs. *t, and Blust, I and others have regarded the three Formosan groups (Atayalic, Tsouic and the larger Paiwanic) as representing early split(s) from PAN but Dyen and others are now emphasizing evidence for a Form/Phil. grouping, on morphological as well as lexical basis, while the KD evidence supports both *C vs. *t, etc. distinctions as well as the Form./Phil. lexical tie; I've called this the 'Form./Phil. dilemma', which promises to be debated at great length - with much intensity - at the coming symposium on Formosan lang's in Taipei. I still have the most likely historical scenario as having the Phil. and other MP (incl. Oceanic) groups bypassing Taiwan, so to speak, in offshore movements, prob. a series of them, rather than having all the AN peoples originating from the mainland via Taiwan (contra Peter Bellwood in Sci. Am. 7/91). But don't bet on it - in time prehistory may provide some clues.

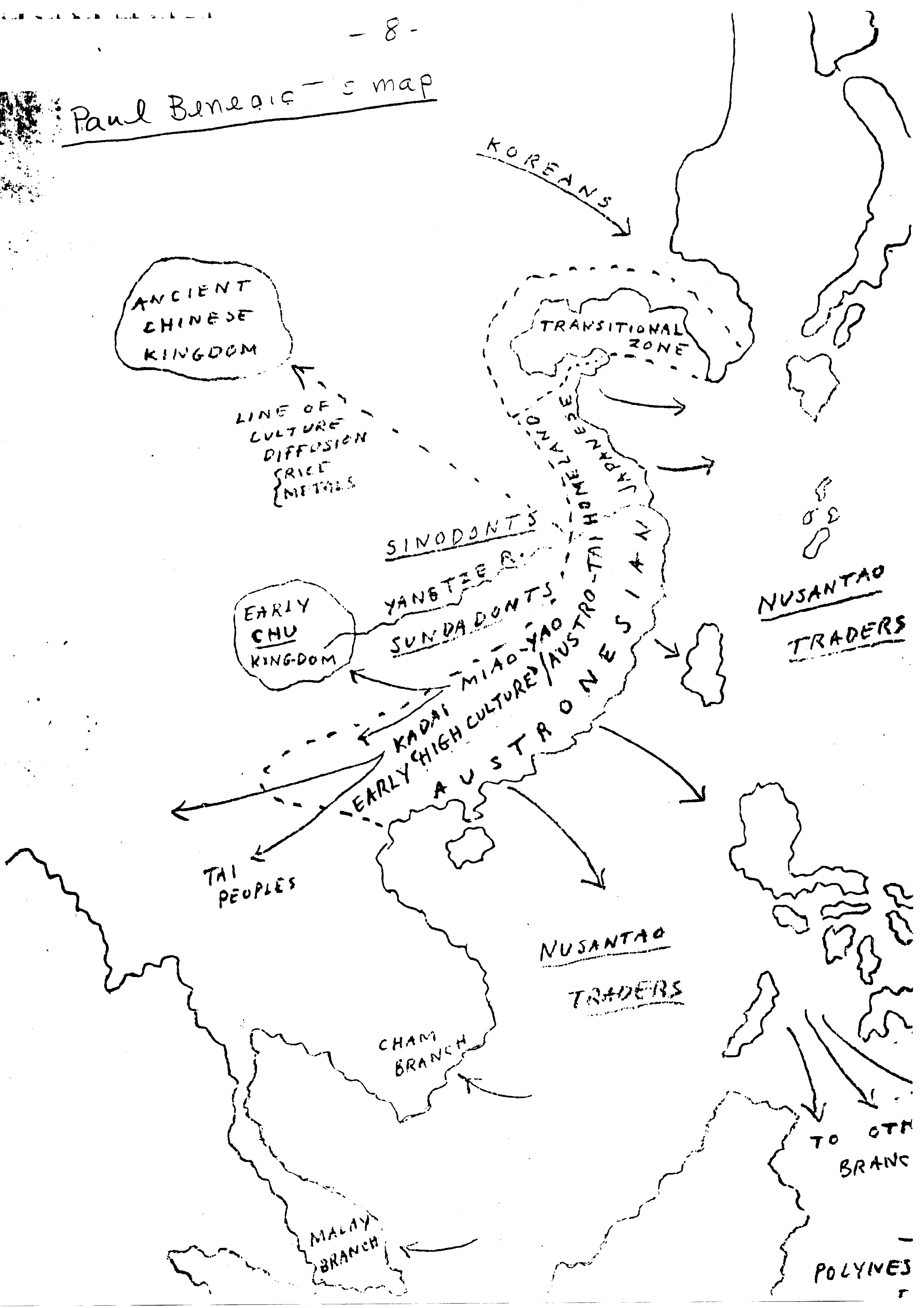
Austric: Diffloth and I have both published our pro- vs. contra-arguments recently; basically, we largely agree on the facts, that no core (Swadesh-type) roots to speak of are shared by MK/AA and AN/AT whereas, in contrast, there is some resemblance in morphology. D argues that it's okay if the Swadesh-type roots don't show up - it's really no big deal (he writes better than this!) - they got lost for one reason or another (he mentions a 'taboo' factor) while a variety of 'specialized' roots, such as SCRUFFY and SMEGMA, were retained. I argue that the morph. resemblance can be explained on an areal basis, in a region (SEA) that has become famous for this sort of thing (see the many papers on various aspects, incl. tonal diffusion, by Matisoff et al.) and that one cannot reconstruct a PA(ustric) on the basic of SCRUFFY/SMEGMA roots; no respectable IEist would accept it for PIE and we shouldn't go along with it for PA. I keep complaining about comparativists who think in Gertrude Stein terms: a word is a word is a word. They tend to count cognate sets/roots - often ask me 'how many you got'? I never know - all I know is whether what I've got looks like a proto-language. AT does, PA doesn't. No counting.

ST + AT (please not Sinitic = one side of ST, made up of Chinese and Bai): an old Conrady-Wulff idea (see ATLC: 450-51); as G. S. would say, 'There's no there there': no roots, no morphology. I've written on early AT loans to Chinese, from a Donor-to-Arch. Ch. lang. (DAC), and recently Sagart has collected items of this kind, from AN, as evidence for a genetic relationship. Long rangers should look elsewhere.

Sino-Caucasic = Dene-Caucasic (Bengtson): I've not been impressed by early attempts to link ST with Yenisei-Ostyak (Ket) or Dene - see Conspectus: fn's 8, 13 nor what what I've seen of more recent attempts for a Caucasian hook-up. Bengtson has sent me material for a review of the ST aspects and I'll proceed with that. I'll also compare the strength of such cf's with others that are available for ST and AT, to determine whether there is basis for PSC or PDC vs. P-Sino-AT.

Nahali, Australian, Indo-Pacific: I pass - don't know enough.

Paul Benedict's map



Robert Blust

Dear Hal,

Herewith my response to your call for opinions regarding distant genetic relationship.

I am one of the few Austronesianists who has worked actively with data representing the entire language family (around 930 members). For this reason I am familiar not only with "well-behaved" languages such as Tagalog, Malay or Samoan, but also with some of the more lexically divergent Austronesian languages of Melanesia and other areas. At present I have a three-year grant from the National Science Foundation to produce a new and greatly expanded Austronesian comparative dictionary to replace Dempwolff (1938). Let me hasten to add that — unlike a certain book recently published by Mouton with the misleading title *Comparative Austronesian Dictionary* — mine is a real comparative dictionary, not a modern-day *Mithridates* masquerading as something it is not.

To date I have produced about 800 printed pages of publishable material. Extrapolating from what this represents in Dempwoff (1938) I estimate that I am about 16-20% of the way through. In other words, the completed work will be some 3,500-4,000 pages, and will fill several volumes. It will have the following overall structure: 1. Introduction (discussion of earlier contributions, methodological principles, conventions adopted, including list of language name abbreviations, major subgroup membership of each, published sources, etc.), 2. 27 sections (*a to *z) of reconstructions on any of nine different, explicitly marked chronological levels, together with supporting evidence. Each entry begins with a number code marking the chronological level, followed by the reconstructed form, its gloss and any doublet or disjunct (terms explained in the Introduction) which needs to be cross-referenced to it. Following a space the supporting evidence is given by citing language-name abbreviations in a fixed geographical order grouped under major subgroup headings. Many entries conclude with a Note which contains information on possibly related forms which are problematic in various ways, on problems of semantic reconstruction, and so on. Some of the notes are a page or two in length, 3. an appendix of monosyllable 'roots' (submorphemic recurrent sound-meaning associations), 4. an appendix of loanwords which are widely distributed and hence potential traps which could lead (and in some cases in the past have lead) to erroneous reconstructions, 5. an appendix of what I judge to be chance similarities or "noise" (to squelch possible complaints that they are valid etymologies which I simply overlooked), 6. an English-Austronesian finderlist.

My appendix of chance similarities already contains over 380 entries. This was generated in systematically searching about 120 sources and producing somewhat less than 1,000 accepted etymologies. The judgement that a comparison is a product of chance convergence rather than of shared history is a distillate of several different considerations which include: 1. the number of languages in which a phonetically and semantically similar form is attested, 2. how similar and distinctive the semantic agreement is, and 3. whether the assertion of recurrent sound correspondences requires any kind of ad hoc ancillary hypothesis.

As you know, a number of proposals of rather startling variety have been made about the external relationships of the Austronesian languages. Some of these are quite obscure (e.g. the claim that Beothuk and Austronesian are related), while others are reasonably well-known. I have studied both published and unpublished evidence for 1. Austric (Schmidt, Reid), 2. Austro-Thai (Benedict), 3. Austronesian-Japanese (Kawamoto), and Japanese-Austro-Thai (Benedict), 4. Chinese-Austronesian (Sagart), and 5. Indo-European-Austronesian (Bopp). In addition I have corresponded with Merritt Ruhlen regarding the Austronesian content of his 'Global etymologies', and with some other long-rangers. Laurie Reid and I are friends and colleagues in the same department, Paul Benedict and I are friends who have corresponded and shared information for years, Takao Kawamoto and I have never met, but have corresponded and shared information for years, and Laurent Sagart is visiting this year in Hawaii, and we have had some useful friendly conversations. I mention this because so much of the discussion of distant genetic relationship that I have seen recently has been polarized to the point that honest and interested disagreement is sometimes dismissed as nothing more than ideological posturing. When scientific inquiry begins to resemble political squabbling to this extent I would rather go out into the fresh air and leave the room to others to battle it out over their cocktails.

This is my well-considered, and unbiased position: I have yet to see a body of evidence supporting any proposal concerning the external relationships of Austronesian which, if confined to Austronesian languages, I would not unreservedly consign, etymology by etymology, to my Appendix of "Noise". *All* of the proposals concerning the external relationships of Austronesian that I have seen to date suffer from serious methodological problems (much as I like and admire some of those who have proposed them). Contrary to the view of many long-rangers those of us who remain skeptical are not closed-minded or boorish plodders who cannot take the giddy excitement of great scientific breakthroughs. We simply insist on uniform canons of evidence for established language families and proposed super-families.

Of all the proposals which I have studied Benedict's Austro-Thai is to my mind the most sophisticated, the one most strongly motivated by a priori plausibility, and the one most likely to have some historical basis. Having said this let me emphasize that I have worked through many of the etymologies in ***Austro-Thai: language and culture*** in relation to Benedict's tables of sound correspondences both on my own, and together with students in classes which I have taught. The results are very clear: the proposed sound correspondences work only if one accepts an elaborate machinery of hypotheses designed to save each failed etymology each in a unique way. In other words, the "generalizations" about sound correspondences betweend Tai-Kadai and Austronesian are spurious, since the only statement that can be reduced to tabular form is one which includes a tortured appendix of excuses as to why the correspondences really don't work the way they are said to work. Reid has suggested that Benedict has buried a valid argument for Austro-Thai beneath an avalanche of dubious etymologizing and hyperbole. In Benedict's view the attested Tai-Kadai forms sometimes correspond to the first syllable, and sometimes to the second syllable of disyllabic Austronesian reconstructions. In Reid's view the only valid etymologies show Tai-Kadai forms corresponding to the ***last*** syllable of Austronesian reconstructions. I agree with Reid that the most promising etymologies fit the pattern he describes. Some of these are quite striking (e.g. PAN *sakit, Kam *kit* 'pain', PAN *qetut, Kam *tUt* 'fart', PAN *Sapuy, Kam *pui* 'fire'). The problem with these and other similarly striking comparisons is that reconstruction ***within*** Tai-Kadai leads to proto-Tai-Kadai forms which are either phonetically less similar to PAN reconstructions than the attested forms, or which involve additional segments which seem to have no place in the PAN form. In other words, the further back one reconstructs in both Austronesian and in Tai-Kadai the *less* similar the two proto-languages appear. This, of course, is just the opposite of ~~one~~ what would expect if two groups of languages derive from a common ancestor. It is, admittedly, possible that the PAN reconstructions themselves need to be modified, but any modification should be made on the basis of internal Austronesian evidence, not on the basis of external evidence from languages whose genetic relationship to Austronesian is still in question.

In a casual communication such as this it is hard to do more than scratch the surface. You asked for a statement of position and a few remarks in justification. I continue to be a skeptic not out of any fixed ideological stance, but out of continuing disappointment with the quality of evidence that has been offered in support of most claims of distant genetic relationship. Let me conclude with some concrete examples using real language data to make my point. Consider the following comparisons: (1) Ilokano (N. Philippines) ***bárat*** 'variety of thin-skinned, greenish banana', Bontok (N. Philippines) ***bálat*** 'banana plant, banana fruit', Tanjong (Borneo) ***balat*** 'banana', (2) Ilokano ***(a)wanan*** 'tiebeam', Ifugaw (N. Philippines) ***wánan*** 'the four beams of an Ifugaw house or granary ... they serve as supporters of all the rafters of the pyramidal roof', Yamdena (S. Moluccas) ***wanan*** 'bamboo lath on which thatch is placed in making roofing', (3) Atayal (N. Taiwan) ***paga*** 'bed', Balinese ***paga*** 'bier', Sasak (Lombok) ***paga*** 'palanquin for bridal pair', (4) Bikol (N-C. Philippines) ***bárang*** 'bewitch, cast a spell on someone', Aklanon (C. Philippines) ***bárang*** 'black magic, voodoo, witchcraft', Gedaged (New Guinea) ***baz*** 'incantation, spell, magic, charm', (5) Javanese ***wilis*** 'counted, calculated', Sa'a (S.E. Solomons) ***wili*** 'give tribute, contribute money to a chief at a feast', Fijian ***wili*** 'count, read', (6) Tagalog ***bálok*** 'membranous covering structure of plants, fruits or nuts', Rembong (Flores, Lesser Sundas) ***balok*** 'sheath (betel nuts), cover of bamboo, corn', (7) Tagalog (N-C. Philippines) ***wálat*** 'be destroyed', ***walát*** 'destroyed', Javanese ***walat*** 'heaven-sent retribution', (8) Motu ***bala*** 'tail fins of a fish', Sa'a ***pala*** 'dorsal membrane of a swordfish', Woleai (Micronesia) ***pash(a)*** 'tail of a fish'. Despite their superficial plausibility, all of the above comparisons are treated in my Austronesian Comparative Dictionary as "noise," and are accordingly assigned to an appendix of rejects rather than to the main body of the dictionary. In (1) the Ilokano and Bontok forms are judged to be cognate, but the Tanjong form is not, in

(2) the Ilokano and Ifugaw forms are judged to be cognate, but the Yamdena form is not, in (3) the Balinese and Sasak forms are judged to be cognate, but the Atayal form is not, in (4) the Bikol and Aklanon forms are judged to be cognate, but the Gedaged form is not, and in (5) and (8) none of the forms are judged to be cognate. Space does not permit me to justify these judgements here, but a comparison of the above material with that presented for many arguments in favor of distant genetic relationships will, I believe, show no substantial differences in quality. The difference between the two cases is that all of the languages from which the above material is drawn *are* genetically related, and this claim can be supported through reference to many perfectly good etymologies. As noted already, I have generated some 380 rejects (consisting both of loanwords and of chance resemblances) in documenting less than 1,000 etymologies to date. Since I am less than 20% of the way toward completing the dictionary many more rejects clearly will be generated in the work that lies ahead. The rejection of such comparisons is based on a careful and thoughtful consideration of the evidence. The rejection of all claims regarding the external relationships of the Austronesian languages which have been made thus far rests on exactly the same kind of careful and thoughtful consideration.

Sincerely,

Bob Blust

Robert A. Blust

P.S. The paragraph beginning "My appendix" on page 1 should read "My appendix of rejects already contains" rather than "My appendix of chance similarities already contains." I couldn't get our printer here to work after I caught the error.

University of Hawaii at Manoa
Department of Linguistics
1890 East-West Road
Honolulu HI 96822

5 November 1992

Harold C. Fleming
Association for the Study of Language in Prehistory
5240 Forbes Ave.
Pittsburgh PA 15217

Dear Hal:

I've received your communication of Sept. 21. I don't think I have much in the way of opinions of an expert nature to offer. In an effort to be cooperative, I'll say the following:

When Benedict's original proposal for a relationship between Austronesian and Tai-Kadai came out, I thought it looked promising. Some years later I went through the available data somewhat more carefully, and found some--but not much--more than appeared in the original publication. I still think it looks pretty likely. However, there are also some indications of the relationship of Austronesian to Austroasiatic as proposed by Schmidt. However, from what I've seen, the Tai-Kadai case looks better. They might all be related, of course, but I have no idea what else might also be included, or in what subgrouping.

(I also think, for what it's worth, that Joe Greenberg's Indo-Pacific looks very good).

You point out that I've, as you put it, "seen fit not to join ASLIP". I suppose I feel kind of discouraged about the performance of the profession in dealing with this kind of problem. I've seen so many claims about relationships that show no appreciation of how easy it is to find words in any two languages that are similar enough phonetically that one could imagine how they might derive from a common source (in fact, sometimes they even select out parts of words), and then, given two words so selected, to figure out a plausible way that their meanings might have been derived from a common original. In short, they give no reason at all to think the whole thing isn't a matter of chance similarities and seem quite unaware that there could be a problem.

And then on the other side I see a resistance to new proposals that strikes me as reflecting either a near-pharisaical attachment to niceties of procedure or else a response to what is perceived as a territorial threat.

I grant you that the question of more distant relationships is intrinsically of great interest. It's just that I find it less embarrassing not to see so much of what is being done on one side and the other of the question.

Sincerely,

George Grace

George W. Grace,
Emeritus Professor

Pama-Nyungan: an entirely viable Family-level construct within the Australian Phylum

Geoff O'Grady

University of Victoria

Victoria, B.C.

Canada

Is your theory crazy
enough to be true?

For non-specialists in Australian or Pama-Nyungan comparative linguistics, what follows will become clearer if large-scale maps of Australia are consulted. Ideally, such maps should detail topography, rainfall distribution, the locations of the 230 languages originally spoken throughout the continent and an indication of the classifications that have been put forward for them. In particular, the following regions should be noted:

1. Northeast Arnhem Land

2. The remainder of Arnhem Land as well as the Kimberley District of Western Australia

3. North West Cape and hinterland

4. The islands of Torres Strait, west and east.

5. Cape York Peninsula

6. The Arandic-speaking area of the Centre

7. Gippsland

8. The remainder of mainland Australia

9. Tasmania

What follows is an airing of my views concerning the genetic affinities -- internal and external -- of Australian languages. These views have been evolving since June 1949, when I had my first opportunity to hear and study an Australian language, Nyangumarta (beginning with the noun **/wika/** 'fire'), and to realize the similarities in basic design which it bore to other languages I had studied, such as Latin, German and Russian. Some of these similarities were especially striking, e.g. with respect to the presence in all four languages of rich systems of nominal case-markers. But there were also profound differences. For example, no item of the vocabulary that I learned in those first weeks, such as **/wika/**, bore any formal similarity whatsoever to its semantic equivalent in any of the non-Australian languages I had dabbled in at the Adelaide Public Library years earlier, such as Malay, Swahili and Hungarian. I felt indeed privileged to be able to make a start in the study of a language in the Outback -- when not searching for sheep or fixing fences -- whose speakers were the epitome of patience and kindness in the face of my many questions.

The history of ideas on the internal and external relationships of the Australian languages is replete with hair-raising oscillations from one extreme in thinking to the other. As Bob Dixon points out in his 1980 book, The languages of Australia, members of Captain Cook's first expedition to the Pacific in 1770 transcribed nearly 200 words of Guugu Yimidhirr, spoken on the northeast coast of the continent. This material was generally taken at the time to be representative of a *single Australian language* spoken throughout the continent.

In 1788, when a convict settlement was established at Port Jackson (the site of modern Sydney, 2400km to the south), the local language, Dharuk, turned out to differ considerably from Guugu Yimidhirr. And in 1791, in what Dixon (1980:9-11) rightly terms the **first great breakthrough** in Australian linguistic studies, it transpired that the language spoken just 65km to the northwest of Sydney was different again! What did this imply for the continent as a whole?

The **second great breakthrough** came in 1841 with the publication of Sir George Grey's work on his often perilous expeditions in Western Australia. Grey noted 'recurrent similarities amongst the multitude of languages' (Dixon 1980:11). Thus 'foot' was **TJENNA** at Perth in the west, **TIDNA** at Adelaide in the south, and **TINNA** at Sydney in the east (with forms in upper case to identify the 'prescientific' transcriptions of the era). Buttressed by modern transcriptions of terms for 'foot' in still other languages, such as the Geytenbeeks' **jinang** for Gidabal or **jina** for Nyangumarta (see map), these forms can be confidently taken back to Proto Pama-Nyungan (PPN) ***jinang**.

For 'tongue,' Grey brought into focus the similarities between **TDALLUNG** at Perth, **TADLANGA** (Grey)/**TADLANYA** (Teichelmann and Schürmann 1840) at Adelaide, and **TULLUN** (Grey)/**TALLING** and **TAL-LANG** (Hunter and Collins respectively, in Curr 1887) at Sydney. None of these representations makes clear that the initial stop in each case was almost certainly a lamino-dental - **/th/** in the practical orthography used here. So also for the

'Adelaide' and 'Sydney' terms for 'foot' above. Forms transcribed in modern times by professional linguists make this apparent - witness Bidyara-Gungabula **thalany** and Warlpiri **jalanypa** 'tongue'. Thus Australianists are pretty well agreed on the reconstruction ***jalany**, and ascribe this to PPN. (Note: Nick Evans shows conclusively that this root, together with several dozen others, can in turn be referred back to a genuinely Proto-Australian level. By contrast, however, over 2,000 roots and suffixes can be reconstructed for Proto Pama-Nyungan).

Grey's observations resulted in the pendulum's swinging to the opposite extreme, with scholars now being led to believe that all of the languages of Australia belonged to a single language family. It is ironic, incidentally, that among the handful of words which he documented (1841:II: 131) for the Nhanda language, spoken well to the north of Perth, was the inconspicuous-looking form **MALO** 'shade'. This is reconstructible to PPN ***malung** without difficulty, but PPN acquired it from -- and here comes the bombshell!! -- overseas, namely as an early (ca. 4,000-year-old) Austronesian loan! That is, if O'Grady and Tryon (1990) are correct - and they believe they are, since over half a dozen further arguably Austronesian loans into Proto-Pama-Nyungan were documented as well. Here's a plausible scenario, then:

1. The dingo is introduced into Australia about 4,000 years ago.

2. New tool technology starts to spread across Australia, also about 4,OOO years ago.

3. Austronesian loanwords are introduced into Australia about 4,000 years ago and become part and parcel of the subsequent spread of Pama-Nyungan across seven-eighths of the continent.

Common sense demands that at least allowance be made for the *possibility* that these three developments are simply different manifestations of one and the same prehistoric event, namely, the meeting and intermingling of Australian and Austronesian cultures on the far northeast Australian littoral. There has been far too much closing of minds in Australian linguistics to such possibilities as the presence of old Austronesian loan words at the sites of present-day Perth or Adelaide, for example -- thousands of kilometers from where Austronesians presumably would ever have landed on the continent. Now that we have over a thousand roots and suffixes of PPN age reconstructed -- albeit roots of varying degrees of plausibility -- it behoves some enterprising, energetic, imaginative and computer-oriented graduate student to embark on a thoroughgoing comparison of ancestral Pama-Nyungan and Austronesian forms. The results, taken together with the recent findings of scholars such as Barry Blake, Nick Evans, Rhys Jones and Patrick McConvell, could well lead to a final, incredibly rich and detailed vindication of Pama-Nyungan as a quintessentially viable language family with a fairly shallow time depth. The estimate of 4,000 years that I have been bandying about for untold years, to all who would lend an ear, may yet turn out to be not far off the mark. I urge upon the sceptic a 630-second period of total suspension of disbelief -- in other words ten minutes for clearing the mind of preconceived ideas and half a minute for considering at least the possibility of old Austronesian loans having been carried by speakers of Proto Pama-Nyungan to far extremities of the continent. If such loans could be shown to be systematically absent from non-Pama-Nyungan languages, the implications concerning the further vindication of Pama-Nyungan as a genetic construct would be obvious.

All this must be viewed, of course, in the context of a 50,000-year (+) presence of humans in New Guinea-Australia-Tasmania, which would have been a single continent for something like a half of that colossal span of time (from 37,000 to about 10,000 BP). It seems highly probable that of the roster of the many languages spoken in the Australian supercontinent 27,000 years ago, say, more than 90% would have become extinct eons ago. And this process of language loss would have been considerably hastened by the spread of Pama-Nyungan, together with Austronesian loanwords and a new technology, over seven-eighths of the continent about 23,000 years later. (The concomitant spread of the dingo would not have been limited to what was to become Pama-Nyungia, of course!)

But more of such heresy anon. We noted above that Grey (1841) initiated the idea that all Australian languages were related. The pendulum was to take another wild swing in 1919 with the publication of Wilhelm Schmidt's Die Gliederung der australischen Sprachen. Although Schmidt agreed with Grey to the extent of recognizing that at least the languages of the southern two-thirds of the continent (apart from the Centre) were 'related through a number of common features,' he nevertheless concluded that 'the whole of the north of Australia contains languages which do not show any lexical relationship and only very few grammatical relationships with the larger group and even with each other. Here in the north we find a wealth of languages comparable with the diversity found in New Guinea' (Schmidt, cited in Dixon 1980:221). In terms of the nine regions detailed at the beginning of this airing of my views, Schmidt was proposing that the languages of regions 3, 7 and 8 were related, while those of the rest of mainland Australia fell outside this grouping and themselves formed a number of genetic groupings separate from one another.

While Schmidt never enjoyed the possibility, indeed the privilege, of doing actual fieldwork in Australia, Arthur Capell was able to spend two years in the field in the north and northwest of the continent, under extremely rugged conditions, and to effect the **third great breakthrough** in Australian linguistics. Armed with a grammatical elicitation schedule and a 600-item wordlist questionnaire, he sytematically surveyed about sixty -- yes, sixty! --

languages extending from La Grange Bay on the Indian Ocean 1600km eastward to the Gulf of Carpentaria (see map). A tiny part of this enormous work appeared in the journal Oceania in 1940.

Capell proposed a dichotomy of Australian languages into (1) suffixing and (2) prefixing-cum-suffixing (the latter being located in our Region 2 -- the Kimberleys and most of Arnhem Land). Although this was a typological classification, it did anticipate, with a few exceptions, the later Pama-Nyungan/non Pama-Nyungan lexicostatistic classification proposed in 1962 by Hale.

Capell correctly recognized that the suffixing languages of northeast Arnhem Land (Region 1) are genetically far closer to suffixing languages spoken 400km to their southwest than to their prefixing-cum-suffixing (hereafter 'prefixing' for short) neighbours. Indeed, this could be said of the suffixing languages as far afield as you can get within Australia and still not drown in icy southern seas -- Cape Leeuwin in the far southwest, Cape Howe in the southeast, at 37º30" S., and Saibai Island, nestling close up to Papua New Guinea at 9º S.

Capell pursued this and other themes further in his major 1956 work, A New Approach to Australian Linguistics. Evidence cited by him which bore on questions of genetic relationship among Australian languages in general included that of pronouns, nominal case-marking suffixes, verbal inflectional suffixes and 48 word roots. As one of his students, I showed in my BA thesis in 1959 that lexicostatistic evidence corroborated, in broad measure, the outlines of the **genetic** grouping which Capell's work had brought into focus. Thus Gupapuyngu, as witness language of Region1, turned out to share more cognates -- 20% -- with the Kanyara languages of the North West Cape area in Western Australia, 2400km distant, than with any other languages in Australia, including especially its 'prefixing' neighbours. (This discovery provoked equally as much rapture and intellectual excitement, on my part at least, as the

Algonkian/Ritwan or Sino-Tibetan/North Caucasian breakthroughs by US and Russian scholars were to later).

The **fourth great breakthrough** in Australian linguistics consisted in the very arrival of Ken Hale on Australian soil early in 1959. This prodigiously gifted, hardworking and insightful scholar and truly great human being was to effect a profound upgrading in the quality and quantity of research done on Australian languages. What concerns us mainly here is his contribution to the genetic classification of these languages (1962,1964). He perceived with great clarity that almost all of Capell's 'suffixing' languages fell within a single family. He drew on the terms for '(aboriginal) person' in the northeasternmost and southwesternmost corners of the continent to coin the very felicitous name 'Pama-Nyungan' for what he (and others, including myself) had come to regard as 'the largest coherent genetic linguistic construct' (i.e., language family) in Australia. And this name has stuck.

Outside of Pama-Nyungia but still within Australia, i.e., within the remaining one-eighth of the continent, Hale recognized no fewer than *twenty-eight* families and language isolates coordinate with Pama-Nyungan -- an indication of the enormous linguistic diversity in that part of Australia. (Later work by Wurm and by Blake pointed the way to a reduction in the number of distinct families, however).

One of Hale's most important and spectacular accomplishments was to demonstrate conclusively that great phonological diversity in the languages spoken *east* of the Gulf of Carpentaria -- i.e., on Cape York Peninsula -- belied their genetic homogeneity, which became apparent as his reconstruction of their common ancestor, Proto-Pamic, proceeded.

The non-Pama-Nyungan languages spoken *west* of the Gulf, on the other hand, are phonologically homogenous but genetically extremely diverse, as is made abundantly clear in Jeffrey Heath's 1978, 1981 and 1984 works.

Let us conclude this first instalment outlining my views on the Australian genetic linguistic picture.

I would like to provoke readers of Mother Tongue with a small lexical database involving languages distributed throughout much of the area between Indonesia and Tasmania. Those interested might want to rise to the challenge of actually getting to grips with some data, and to use it in forming hypotheses concerning some peculiarly Australian facets of comparative work. How many cognate sets would a reasonable linguist want to extract from the 114 forms given, after all? Better to mess around with some data than hear me pontificating in the abstract. More next quarter!

____	1	UMPila	aja	shallow
____	2	WadJuK	BUDJOR	ground
____	3	GUPapuyngu	buthuru	ear
____	4	GUP	dhirr'thirryu-n	frighten
____	5	WJK	DJAM	water
____	6	WJK	GABBI	water
____	7	WJK	GORAD	short; stunted
____	8	GUP	gurriri	short
____	9	Warlpiri(WLB)	jalanypa	tongue
____	10	PINtupi	jarlinypa	tongue

____	11	YiDiNy	jili	eye
____	12	PIN	jina	foot
____	13	GIDabal	jinang	foot
____	14	WaRriYangka	jirril	afraid
____	15	NYAngumarta	jiti-rni	flush (bird from cover)
____	16	NYA	jungka	ground, dirt
____	17	PIN	kapi	water
____.	18	DIYari	kapi	egg
____	19	GAWurna	KARTO	wife
____	20	NgarluMA	kartu	man, male -- as of animal
____	21	MaRDuthunira	kartu	thou
____	22	TIWi	kukuni	water
____	23	WEMbawemba	kupa-	drink
____	24	NGarLa	kupapirri	stooped posture
____	25	Bidyara-GUngabula	kupu thana-	bend, stoop
____	26	YDN	kurran	long, tall
____	27	MiRNiny	kurrartu	short
____	28	PIN	kuru	eye
____	29	NYA	kuta	short
____	30	UMP	ku'un	eye
____	31	WLB	langa	ear

____	32	WaRNman	langa	ground, dirt
____	33	Proto Eastern Oceanic	*malu	shade, shadow
____	34	GID	malung	shadow, shade
____	35	NhANda	malu	shade
____	36	ADNyamathanha	mangu	face
____	37	YinGgarDa	mangu	good
____	38	Proto-KAnyara	*mangu	cheek
____	39	GUP	mangutji	eye, seed ... sweetheart
____	40	ARaBana, PIN	mara	hand
____	41	WOIwurrung	marram	body
____	42	WOI	marrambik	I
____	43	WOI	marrambinherr	thou
____	44	UMP	ma'a	hand
____	45	PaNKarla	MENA	eye
____	46	WJK	MIKI	moon
____	47	Kala Lagaw Ya	MINA mina mina geth	true, real, good, perfect good right hand (<u>geth</u> 'hand')
____	48	JIWarli	mina	right hand
____	49	TASmanian	MEENA /mina/	I (SE and Oyster Bay)
____	50	ADN	minaaka	eye
____	51	'King George Sound'	MINAM	truly

____	52	KGS	MINANG	(Name of language at KGS)
____	53	KGS	MINANG	the south
____	54	Proto-Pamic	*mini	good
____	55	WJK	MINOB	to be jealous
____	56	WJK	MINYT (= /minaj/?)	the countenance
____	57	GUP	munatha	ground
____	58	WRN	mu(r)narta	ear
____	59	WJK	MURDO	in vain
____	60	WJK	MY-A /maya/	a house
____	61	TAS	NEENA /nina/	thou (SE and Oyster Bay)
____	62	NYA, WLB	ngaju	I
____	63	PIN	ngampu	egg
____	64	NYA, DIY	ngapa	water
____	65	PITta Pitta	ngapu	water
____	66	PKA	*ngapuru	brains
____	67	TIW	ngia	I
	68	proto-Australian	*ngin	thou (Dixon)
____	69	BAAgandji	nguku	water
____	70	WLB	ngukunypa	brain
____	71	WOI	ngupa-	drink
____	72	GUP	nhu-na	thee

____	73	WJK	NURGO	egg
____	74	GUP	nurrku	brains
____	75	PPN	*nyun	thou (Capell)
____	76	PIT	pampu	brain, egg
____	77	YGD	papa	water
____	78	ARB	papu	egg
____	79	WRN	parra	I
____	80	WRN	parrangku	thou
____	81	KLY	PARU paaru	forehead, face; front face
____	82	BAYungu	paya	deep wooden baby tray
____	83	Bahasa INdonesia	payung	umbrella
____	84	GID	payuung	sling for carrying a child
____	85	GAW	PIKI	moon
____	86	Yir-Yoront	pin	1. ear 2. site, place... country...
____	87	PIN	pina	ear
____	88	GID	pinang	ear
____	89	'GIPpsland'	prra (sic)	man, person
____	90	PIN	purtu	in vain]
____	91	Gugu YAlanji	tajali	deep water
____	92	PIN	tari	inside ankle bone

____	93	BGU	thalany	tongue
____	94	YINdjibarndi	thama	fire
____	95	UMP	tha'u	foot
____	96	WRY	thina	foot
____	97	BAY	thungkara	ground, dirt
____	98	NYA, PIN	tili	flame
____	99	WJK	TONGA (arguably /thungka/)	ear
____	100	PIN	tungku	short
____	101	UMP	uungku	long
____	102	YIN	wirri	play
____	103	NYA	witi	play
____	104	UMP	yampa	ear
____	105	Wirri (WRI)	yampa	... (on the) ground, place ...
____	106	NYA	yamparra	single person
____	107	WLB	yampirri	single men's camp
____	108	YY	yap	1. leaf 2. bush, shrub
____	109	TIW	yimitarla	tongue

ADDENDA

____	110	WJK	DILBI	leaf
____	111	WEM	kurumpaya	to be jealous

____ 112	WEM	-min	(EMPHATIC particle)
____ 113	PIN	nyalpi	leaves
____ 114	DIY	thalpa	ear

Note: Northwest and West Tasmanian /mang(a)/ 'I' shows excellent phonological and semantic congruence with items 36-39 above. This fact eluded me until the last gasp.

For References, see Dixon (1980) and O'Grady and Tryon, eds (1990).

POSTSCRIPT

Editor's Note: Geoff authorized me to add on this appendage which he dictated to me over the phone but later confirmed in writing. It is a very bold step!

Consider #1 Tasmanian (SE) MEENA "I, 1st person singular" which is arguably [mina]. It sits well with my item #45, PaNKarla (PNK) MENA 'eye' and with item #56 WadJuK (WJK) MINYT 'the countenance' which is probably [minaj]. So, semantically, we have 'face' > my person > 'I, 1st person singular'. (Or the other way around? ED.)

In summary Geoff believes that Tasmanian is related to Pama-Nyungan at a 10,000 to 12,000 year time depth -- and , by implication, Australian is also. He adds the SE Tasmanian and NW Tasmanian forms for 'I' to this group. Add also possibly 'lip'.

Editor's 2nd Note: I explicitly warned Geoff that he might experience pain for this proposal. He said he did not mind that because he believed that the Tasmanian < Australian hypothesis would grow now that he and others could start looking for cognates. One of the peculiarities of Australian linguistic prehistory is that it is perhaps opposite to Indo-European in that its phonology remained fairly stable while its meanings wandered far from their beginnings. One can see the meandering meanings in the list of words Geoff has given us.

For those who did not know before now ---> Tasmanian has been classified by Greenberg nearly 20 years ago as a member of his Indo-Pacific phylum. So Tasmanian could become controversial but in a polite friendly way, given the personalities involved.

Jürgen Pinnow
Gorch-Fock-Straße 26
D-2280 Westerland/Sylt
Germany

23.10.1992

Prof. Harold C. Fleming
ASSOCIATION for the Study of LANGUAGE IN PREHISTORY
5240 Forbes Avenue, Pittsburgh, PA 15217 USA

Sehr geehrter Herr Professor Fleming!

Mit leider großer Verspätung habe ich Ihren Brief vom 21.9.92, für den ich Ihnen vielmals danke, erhalten. Er hat mich sehr in Erstaunen gesetzt. Da ich ganz auf die Na-Dene-Sprachen konzentriert war, habe ich leider das Gebiet Austroasiatistik fast total aus den Augen verloren. Wird nun das geplante OBITUARY völlig fallengelassen oder in eine Art Festschrift umgewandelt?

Erwähnen möchte ich, daß es statt
Hans-Jürgen Pinnow (Nordsee)
Heinz-Jürgen Pinnow (Westerland/Sylt)
heißen muß.

Zu Ihren Fragen möchte ich nur in einem Fall Stellung nehmen: "Nahali is Dene-Caucasic surely".

Ein Phylum DENE-CAUCASIC ist - bislang zumindest - eine bloße Hypothese, für die das Material noch äußerst dürftig ist. Ich beziehe mich dabei allerdings nur auf das Buch
Vitaly Shevoroshkin (ed.): Dene-Sino-Caucasian Languages, Bochum 1991.
Die Angaben dort über die Na-Dene-Sprachen sind z.T. sehr verbesserungsbedürftig und nicht auf dem neuesten Stand. Besonders die Rekonstruktionen von S. Nikolaev sind weitgehend nicht haltbar. Wenn für den genannten Stamm "Dene Caucasic" nicht anderes Material vorliegt als das erwähnte Buch, muß ich sehr davor warnen, solch ein Phylum als existent anzusehen.

Ein Verzeichnis meiner jüngeren Arbeiten über Na-Dene füge ich bei.

In der Hoffnung, bald von Ihnen zu hören
mit freundlichen Grüßen auch an die anderen Herren

J. Pinnow.

21/1/93
P.O. Box 87,
Kinglake, 3763,
Australia.

The Editor,Mother Tongue,
c/- Harold C. Fleming,
President, ASLIP,
5240 Forbes Avenue,
Pittsburgh, PA 15217.

Dear Editor,

I would like to take this opportunity to discuss the question of the classification of Japanese, spurred on by the challenge issued in a recent edition of Mother Tongue for "some long rangers to have a go at it" or words to that effect.

Firstly I would like to say that it strikes me that this is not a matter for casual consideration, nor is it an endeavour likely to bring an outsider to the field any quickly satisfying results. The complexity and obscurity of much of the relevant data require special skills and a sound grounding in the body of scholarship that has preceded us.

I fear for the intrepid long ranger who might wade into the data, armed with a set of preconceptions about the stability of particular sets of words (semantic categories?) and unaware of the tortuous phonological and morphological paths that have been tread by the Japanese (and for that matter Altaic) lexicon over millennia.

Having a longstanding interest in this issue, I feel obliged to express my view that the Altaic family is a real thing, and that Japanese is placed firmly within it. Starostin's Altajskaja problema i proisxozdenie japonskogo jazyka (Moscow: Nauka, 1991.) is a thorough and scholarly treatment of the data, packed with extensive comparative lexicon of core vocabulary items backed up with etymologies. The old assertion that the Altaic languages share little common vocabulary suitable for comparison can and should be consigned to history.

Of course, Starostin's contribution has not been to prove the Altaic origin of Japanese, but rather to flesh out a more detailed and accurate description of her Altaic pedigree. Convincing demonstration of the place of Japanese in the Altaic family has been available on library shelves for some decades. The state of knowledge at the end of the 1960's is well summed up in Roy Andrew Miller's Japanese and the Other Altaic Languages (University of Chicago Press, 1971).

Worthy of examination is Miller's table of Altaic pronouns, including Old Japanese(OJ), which shows the classic Altaic pattern of different stems for the nominative and oblique forms of the singular, but one stem only in the plural. For the benefit of readers I reproduce below part of the columns for Proto-Altaic(PA) (Miller credits to Poppe 1965: 193-94) and OJ pronouns from Miller:-

Singular	**PA**	**OJ**
1-p nom./oblique	*bi/*man	mï/wan-u[1]
2-p	*si/*sän	si[2]/sön-e[3]
3-p	*i /*än	a^{n}/r-, o^{n}/r-, onör-e
1-p undifferentiated number		wa-, war-e
Plural		
1-p	*bir	mar-ö, war-ö[4]
2-p	*sir	na-, nar-e ta-, tar-e
3-p	*ir	-?-

[1]M 3476, M 4358
[2]M 904, *Nihon shoki* poem 80
[3]*Kojiki* poem 11
[4]M 4343
M = Man'yoshu poetic text VIII AD

The first thing to note about the OJ forms are the epenthetic final vowels in the disyllabic pronouns, a consequence of the development of Japanese syllable structure. The correspondence of labials is consistent over the 1-p singular and plural forms. Also important to note the /r/ phoneme of the Altaic plurals is present through the plural and undifferentiated number forms of OJ. In the 3-p singular the /n/ - /r/ correspondence is perfectly regular, as the Azuma dialect texts often show /n/ where standard OJ has /r/. The /n/ initials of 2-p plural forms are explained buy the tendency for sporadic change of /t/ > /n/ in OJ where the phoneme /r/ follows in the same morpheme.

However, while Miller's data and conclusions are quite excellent in this case , I don't particularly agree with everything that Miller has ever written. I would like to draw the attention of readers to an astounding gaffe on Miller's part in his Origins of the Japanese Language (1980, University of Washington Press). Page 85 begins the most outrageous and fallacious debunking of the validity and utility of "Basic Vocabulary", in a piece that recalls the irrational intolerance of the more recent 'he must be shouted down at every opportunity' school of anti-Greenberg hysteria.

Miller informs us that "basic vocabulary" (e.g. terms referring to body parts, human functions etc.) is not only no more likely to be subject to historical change than any other part of the vocabulary, but is of no special importance in proving genetic relationships. There follows a scathing attack upon glottochronology and lexico-statistics. Nowhere in this tirade does Miller refer to the phenomenon of loan words. Now please correct me if I am wrong, but I have always been under the impression that historical linguists have believed that all vocabulary is subject to the processes of phonological and semantic change, but that many hold the view that "basic vocabulary" is less likely to be replaced by loan words, so that regular phonetic similarities between "basic vocabulary" in one language and another is more likely to be the result of genetic affinity than contact borrowings.

Miller is not alone among professional linguists in apparently misunderstanding some of the basic concepts and logic of historical linguistics. Traditionally its best practitioners have not been part of the mainstream English speaking linguistic establishment. I fear for linguistics students and others, encouraged to proceed with study, but dependent upon only English language or translated sources.

Is it any wonder that in the countries where linguists are trained by familiarising them primarily with the body of scholarship that has been accumulated in the English language, that historical linguistics has remained a largely neglected and misunderstood discipline. Much magnificent work has been done by dedicated professionals in many countries, only to be ignored, or often if it is read at all, sadly misunderstood.

If some of today's linguists are still arguing about whether or not Japanese can be linked to Altaic, I suggest that any of them who are afflicted by the curse of English monolingualism take some positive steps to equip themselves with the ability to read and comprehend the most important literature available on this subject. In the words of a great Australian saying, put up or shut up.

Yours sincerely,

Paul Sidwell.

2)
Date: Fri, 24 Jan 92 14:09:56 CST
From: Eric Schiller <schiller@sapir.uchicago.edu>
Subject: Re: 3.70 Proto-World

I, too, was outraged by the Scientific American article. Inthe July issue Austro-Tai is simply taken for granted, when it is merely a theory that spread due to lack of opposition. At the 6th International Conference on Austronesian Linguistics a session was devoted to the external affiliation of Austronesian, and there we heard proposals ranging from a link with Chinese, to Austro-Tai, to Nostratic etc. The old Austric Hypothesis (the best contender, in my opinion) has been recently reinvestigated by Diffloth and Reid, among others, and I contributed a BLS paper back in 1987.

It is shameful that Austro-Tai is taken as default truth by so many authorities, and even finds its way into introductory texts. Solid etymological evidence ('wood', 'bone') has been presented for Austric, which combines Austronesian and Austroasiatic, while the Austro-Tai hypothesis rejects such a link. Toss in the fact that both families seem to have been VSO, and show a great deal of shared affixes, and one would think that Austric should have at least equal status. This is not to say that the Father Schmidt's Austric hypothesis has been proven, but rather that it should not be ignored, especially in speculation about great time depths and in combination with archeological evidence.

Eric Schiller
University of Chicago

28 Sept. 1992

Is Kordofanian the Omotic of Niger-Congo?

Roger Blench, Cambridge

Since Greenberg, the membership of Kordofanian in Niger-Congo has scarcely been questioned and an article on Kordofanian placed in the Bendor-Samuel volume appears to set a seal of approval on this assignment. Oddly, however, the case for Kordofanian was seriously weakened by Thilo Schadeberg in 1981, author of this same reference article, when he proposed that Kadugli-Krongo [now being referred to as Kado] be excised and assigned to Nilo-Saharan.

The persuasive morphological feature of Kordofanian that has led to the Niger-Congo assignment is the alternating CV prefixes so characteristic of Niger-Congo. Greenberg backed this with a series of sound-meaning correspondences. However, once Kado (which also has functioning CV prefixes) is cut loose then the argument becomes surprisingly weak. Either the Kado prefixes (which bear a striking resemblance to Talodi) are borrowings or they are coincidence. Similarly, once the Kado ('Tumtum') languages are taken out of Greenberg's comparative list then the actual number of convincing cognates is much reduced.

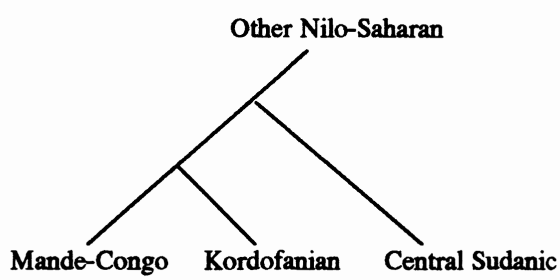
Greenberg proposes some 52 Niger-Kordofanian cognates. Nineteen of these include Kado -and so presumably would be equally good evidence of a Nilo-Saharan affiliation. Many others are certainly cognate with Niger-Congo -but also with Nilo-Saharan. Some, such as 'tortoise' or 'white' and 'and' also surface in Afro-Asiatic and are thus best regarded as 'pan-African' [at least!]. I have recently argued that Niger-Congo should be included in Nilo-Saharan to make a macro-phylum with the proposed name 'Niger-Saharan'. Whatever the fate of the hypothesis, the comparative series show that Niger-Congo and Nilo-Saharan share a substantial number of lexical items, thus casting doubt on their value in assigning languages to one phylum or the other.

Examples of items that are certainly cognate with Niger-Congo but can no longer be used as evidence for classifying Kordofanian because of external Nilo-Saharan cognates are 'blood', 'to buy', 'mouth', 'shoulder', 'thorn', 'three', 'throat', tongue', 'tooth'.

Some of Greenberg's resemblances, as Schadeberg notes, are so weak as to be almost unusable. See for example, 'hill', 'to take', 'to think', 'oil', 'spear' etc. Others depend on a single citation, but this is problematic, because of the significant lexical spreading between the branches of Kordofanian (see Schadeberg's example of 'large').

The sum of these exceptions makes the case for Kordofanian *no stronger* than the evidence linking Kado with Niger-Congo. The other side of the case is the CV prefix system. Williamson (1989) set out a table comparing the Kordofanian prefixes with other branches of Niger-Congo. The phonological correspondences are not close, nor does Kordofanian have the same rococo complexity as mainstream Niger-Congo. The main classes recognised in Kordofanian are human beings, trees, body parts and liquids -semantic classes that have parallels outside Africa.

The point of this note is not to throw out the case for Kordofanian altogether, but to suggest that published interpretations of the evidence have been strongly influenced by misleading factors. Schadeberg's case for excising Kado can be turned on its head to argue either that Kordofanian is Nilo-Saharan (keeping to the old dichotomies) or, more convincingly, is the bridge between the two phyla. In this case, the 'tree' would look something like this;



The presence of Central Sudanic is not an error - evidence is presented in the paper referred to above. Kado probably features higher up the tree. The historical consequence of this analysis is that the class-prefixes of Kado are ancient borrowings from Kordofanian that have co-evolved with the neighbouring languages.

Schadeberg T.C. (1981) 'The classification of the Kadugli language group' in Schadeberg and Bender (eds) pp. 291-306 in 'Nilo-Saharan' Foris Publications, Holland.

Williamson, K. (1989) 'Niger-Congo Overview' pp. 3-46 in Bendor-Samuel (ed.) 'The Niger-Congo languages' University Press of America, Lanham.

Later Version — May 1993

1. From an early period, scholars have noticed a series of resemblances, both lexical and phonological, between the African language phyla today called Niger-Congo and Nilo-Saharan. Westermann put the two together in the first version of 'Die Sudansprachen' in 1911. In 1972, Edgar Gregersen put forward a series of lexical isoglosses to support such a macrophylum and proposed the name 'Kongo-Saharan'. The debate over the classification of the Kadugli languages gives support to these similarities.

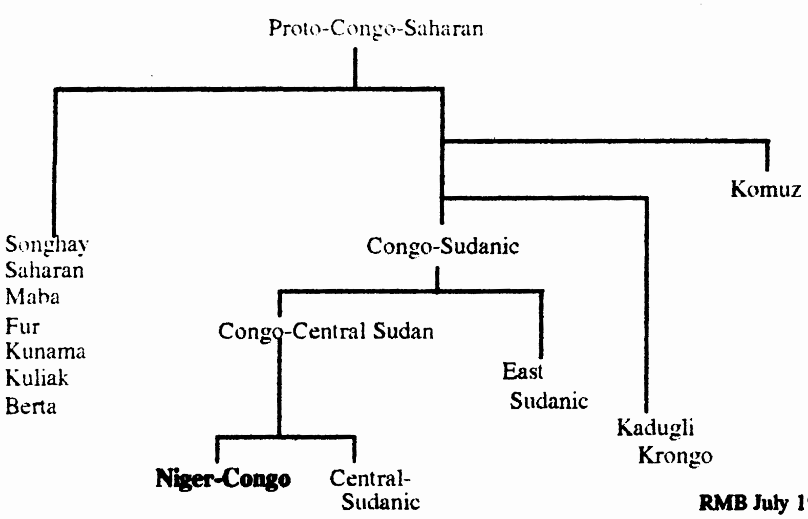
2. The massive increase in availability of data on both phyla since Gregersen's work suggests that the time has come to make this hypothesis more concrete. If Nilo-Saharan and Niger-Congo are to be put together then it should be possible both to list the common features that support the hypothesis. This in turn should provide a key to assigning one phylum in the 'tree' of another.

3. If an overall tree can be constructed then Niger-Congo will fall within the bounds of Nilo-Saharan. Niger-Congo is a far more coherent phylum with a useful number of lexical isoglosses and there is general agreement among researchers about its internal structure, as represented in Bendor-Samuel, 1989. Nilo-Saharan is far more diverse and major researchers have yet to clarify its exact membership, and are still far from agreeing on an internal subclassification.

4. The most striking phonological features the two phyla have in common are the presence of labial-velars and the existence of vowel harmony systems based on 5+5 +/- ATR. These are not recorded in this form elsewhere in the world and it would strain credibility to assume they arose independently. Their presence is confined to particular branches and it is likely that they are a useful indicator of genetic affiliation and subgrouping.

5. Bender proposed in 1989 a major division of Nilo-Saharan into two branches, uniting the Sudanic languages on one side with Gumuz and Kadugli. The phonological and lexical evidence puts Central Sudanic closest to Niger-Congo, followed by East Sudanic and Kadugli-Krongo. The 'tree' of Nilo-Saharan may then appear as follows;

Proposed Congo-Saharan 'tree'



RMB July 1992

This tree makes no hypothesis about the internal classification of the left-hand (Songhai to Berta) grouping.

6. Two historical conclusions can be drawn from these hypotheses; the significantly greater antiquity of Nilo-Saharan and a quite different location for the homeland of Niger-Congo. Previous writers, based on the concentration of families in West Africa, have tended to assume a location somewhere near the headwaters of the Niger and explained Kordofanian by the migration of a single group. If the present classification is accepted it becomes far more likely that the homeland was in the centre of present-day Sudan and that Kordofanian represents the Niger-Congo speakers who stayed at home.

Niger-Congo: The Deep Scattering Layer

David Dalby's attempts to classify African languages, both in his map published some years ago and his more recent Thesaurus [rudely but appositely reviewed by Paul Newman in the recent JALL] have not met with widespread assent from other scholars. In particular, the talk of a 'Fragmentation Belt' across the area where most researchers see Niger-Congo was ill received. The recent book on Niger-Congo edited by John Bendor-Samuel seems to put the phylum into fairly convincing order with a series of discrete language families arranged on a Christmas tree.

The problem is that this was achieved at the cost of ignoring any data that did not fit the schema. In reality, however, there seem to be a disturbing number of languages that are easy to assign to Niger-Congo and difficult to place -or at least the evidence for their classification is more geographical than deriving from a demonstrable common lexicon. The section below is a short list of some of these..

1. Pre. To be fair, this language has only recently (2/3/90) been reported by Denis Creissels from Cote d'Ivoire. Spoken in three villages on the Bouake-Seguela road. Although the language has clearly been influenced by the Mande speech that surrounds it, its classification is a mystery. Although there are plenty of Niger-Congo roots, none A preliminary guess might make it a single language co-ordinate branch with Gur-Adamawa.

2. Due to the origin of the Dogon on Sirius it is not surprising that their language is problematic. Rejected from Gur and Mande it is now treated as an isolate -but despite an abundance of data, no convincing argument for its place in Niger-Congo. Calame-Griaule (1978) says that there are some languages 'inside' Dogon -but '[ils] seraient a classer a part; la parente n'apparait pas comme evidente.' This comment is reprised in an endnote. However, it is clear that there are several speech forms in the Dogon area that remain unclassified.

3. Laal. Laal is a language spoken in on the Chari River in Chad documented by Pascale Boyeldieu. In one published article and in a long unpublished comparative wordlist he has questioned the classification of this language, which is either an Adamawa language under Chadic influence or vice-versa. In any case, there is a substantial amount of mysterious vocabulary. None of the large-scale classificatory articles published recently have been willing to take this language on board.

4. Fali. There are many languages called Fali, but ther most problematic is the large dialect cluster in northern Cameroun, which has been extensively studied by Gary Sweetman. Although this is usually treated as Adamawa, it is extremely remote from other Adamawa languages. Again it is easy to prove it is Niger-Congo, but difficult to place it precisely.

5. Daka [Chamba]. Daka is a language classified by Greenberg to be Adamawa, but now generally agreed, following Patrick Bennett's suggestion, to be Benue-Congo. Recent work by Raymond Boyd and Richard Fardon has increased the fund of lexical data on Daka substantially. However, it is extremely difficult to place due to an absence (erosion?) of affixes and a depressing lack of obvious shared glosses with neighbouring groups. Although I have been promoting Daka [Dakoid] as a co-ordinate branch of Bantoid with Mambiloid, honesty compels me to admit the relationship is not close.

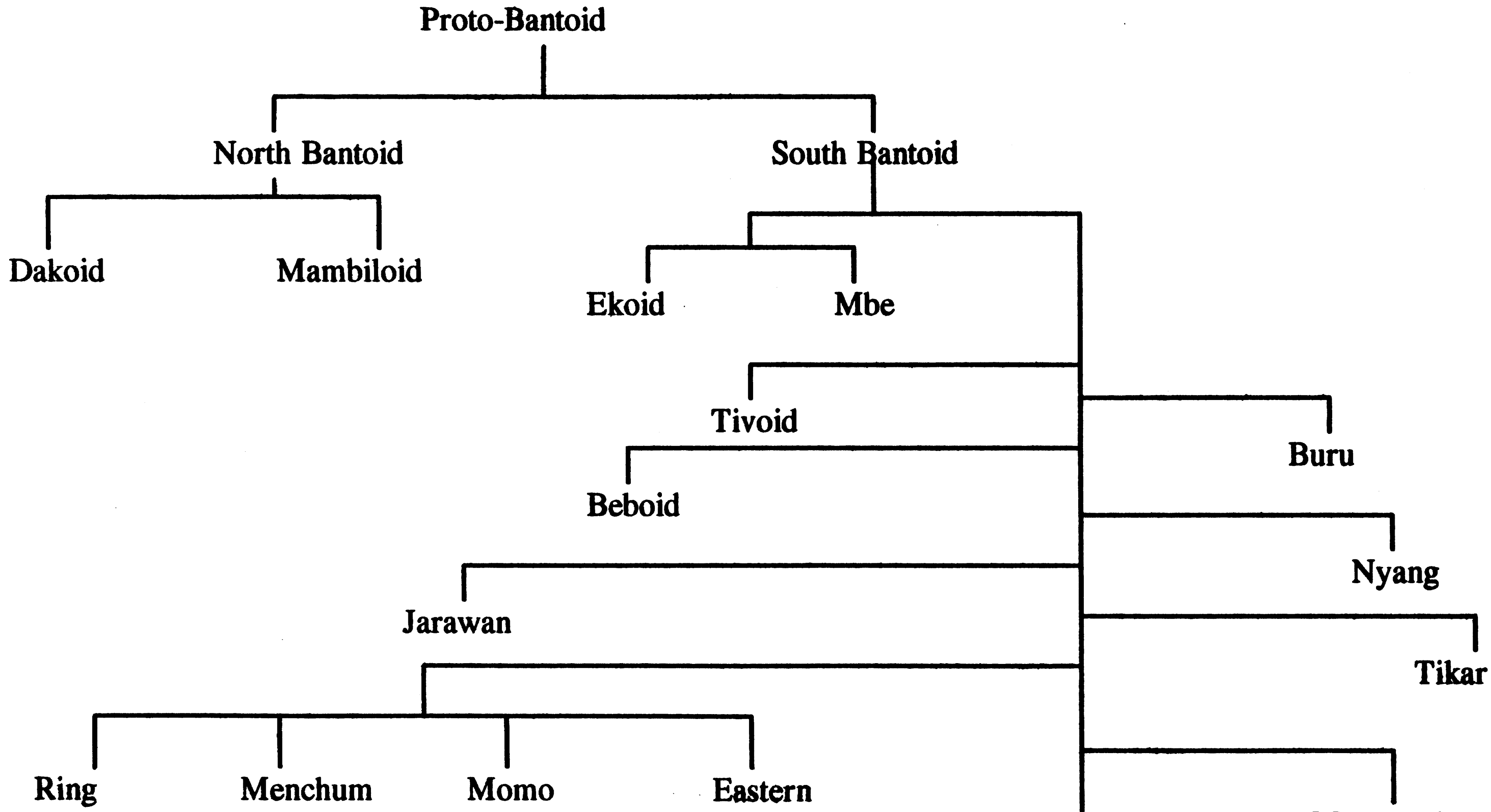
Examples could be multiplied, but I give these few to make a point -we are far from having a convincing tree of Niger-Congo and we should not discount the possibility that it is much more complex and elaborate with many more snaking branches.

From: Roger Blench
Cambridge

In; Inventaire de Etudes Linguistiques sur les pays d'Afrique Noire d'expresssion Francaise et sur Madagascar' ed. D. Barreteau. CILF. Paris

Roger Blench & Kay Williamson

Proposed Genealogical Tree of Bantoid



(or Bantu proper – KF)

The following languages are unplaced:

Ndemli, Bukwe, Mashi

RMB/KW July 1992

SWISS BIOGENETICIST DISTURBED AT OUR NEGLECT OF FRENCH WORK!

Dr. André Langaney of Geneva became upset when he received a copy of MT-17 and an invitation to join ASLIP. In his opinion:

"Once more I am puzzled that, as in any anglo publication, the 'garden of eden' discussion seems to come with Templeton 92 while, as you can check by the enclosed reprints, we have been opposing strongly to this stupidity since '87 in such minor journals as YPA, Am.J.Hum.Genet., J.Molec.Evol., Hum. Biol., Encyclopedia of Human Biology and so on, not to speak about french speaking literature.

Is the anglo science so closed that they cannot even quote foreign references published in their own journals in jolly colloquial English?"

It doesn't seem too likely that he will join ASLIP, does it? And I regret that because André is a fine fellow -- très aimable -- in addition to being an outstanding scholar. Actually his sample of MOTHER TONGUE was askew. Thanks to Eric de Grolier and several allusions in the popular press we knew about his work and had indeed mentioned it in earlier issues.

But even if the reason is that we sampled the wrong journals and so missed his articles, still his work has been underreported and we will correct that straight away. Since to report all of it would consume two MT issues, we will report the abstracts and make some few comments, not all of them complimentary. That is because the Africanist in me is not entirely happy with their analyses and conclusions about Africa.

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In C.R.Acad.Sci.Paris, t.307, Série III, p.541-546, 1988:

PHYLOGÉNIES DES TYPES D'ADN MITOCHONDRIAUX HUMAINES. PROBLEMES MÉTHODOLOGIQUES ET PRINCIPAUX RÉSULTATS.

Laurent Excoffier et André Langaney

Abstract. "Data on the polymorphism of human mitochondrial DNA (mtDNA) was reexamined and has been shown to contain numerous errors in the mtDNA type definitions as well as in their phylogeny. We were able to define ancestral mtDNA types and to build new phylogenies which show that all types found in different continents radiated from a few types. The present Caucasoid genetic stock could be the closest to a hypothetical ancestral population. A new data collection would permit to precise the genetic relations existing between the great continental groups."

Since this is a crucial article, we will also reproduce a fuller English summary. Forthwith:

"Abridged English Version. A recent study of the polymorphism of human mtDNA [1] has hypothesized the existence of an African Eve 200,000 years ago somewhere in Africa. This theory has engendered much debate about the origin of modern humans [2] to [6] and has led us to reexamine available data on the polymorphism of this small extranuclear genome.

Data from several studies in 10 populations [7] to [11] based on the same set of restriction endonucleases were used to construct a phylogeny of 61 mtDNA types (Fig.1). Two types are linked if they differ by only one mutation. The molecular nature of the restriction fragment length polymorphism leading to these types was carefully checked, and evident contradictions with some published results appeared. This led to different branchings for type 3, 11 and 58, 18, 23, 31, 33, 36, 43 and 44. Type 55 was also found to be identical to type 47. The root of the phylogeny was determined by finding a hypothetical ancestral type, which was postulated to present all polymorphic restriction sites in their more frequent state among the 61 types. This ancestral type is equivalent to type 1, which is also the most frequent type in the majority of the samples.

A partial phylogeny of 27 types (out of 133) defined by Cann et al [1] in 5 populations with a set of 12 endonucleases is presented in Figure 2. Type 69 is similar to the ancestral type. The 106 other types are not shown on Figure 2 as they cannot be unambiguously connected to any type by less than 2 mutations. Nevertheless, this partial phylogeny presents some fundamental differences with the published 'genealogical tree' based on a parsimony criterion: types differing by a single mutation are generally not grouped together (such as types 69 and 116, 69 and 80, 33 and 42 or 80 and 103) and the clustering level does not always correspond to a single mutation (see types 32 and 33, 69 and 70 or 63 and 64) on Figure 3 of Cann et al [1]. Moreover, type 114 is identical to type 116.

The detection of several errors concerning type molecular characterization and type connections in published studies led to new interpretations of the phylogenies. When considering the shared types between continental groups as ancestral, one may see on Figure 1 that nine ancestral types out of ten are found in Caucasoid populations, which may then be taken as the closest to a hypothetical original population. Both phylogenies agree in showing that very close types are found in different continental groups. This may be interpreted as the confirmation of a unique origin for all modern humans. In this view, types having accumulated many mutations would have appeared recently, after the first continental splits. Further studies would permit to precise our knowledge of continental group differentiations."

(We should adopt 'to precise' as a verb in English. HF) Basically, Excoffier and Langaney are arguing that (a) Cann and colleagues are mistaken, (b) that the Garden of Eden probably was located in Caucasoid areas, i.e., Europe, the Middle East or northern India. They are known to prefer the Middle East. Splendid, the Old Testament has had the same thought now for quite some time.

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In Am.J.Hum.Genet.44:73-85, 1989.

<u>ORIGIN AND DIFFERENTIATION OF HUMAN MITOCHONDRIAL DNA</u>

Laurent Excoffier and André Langaney

A summary is given which basically repeats the message found in the first article (above), except for these changes. ". . . A partial phylogeny of the types found in five other populations (not in the first study -- HF) also demonstrates that the myth of an African Eden was based on an incorrect 'genealogical tree' of mtDNA types. Two measures of molecular diversity have been computed on all samples on the basis of mtDNA type frequencies, on one hand, and on the basis of the number of polymorphic sites on the other. A large discrepancy is found between the two measures except in African populations; this suggests the existence of some differential selective mechanisms. The lapse of time necessary for creating the observed molecular diversity from an ancestral monomorphic population has been calculated and is found generally greater in Oriental and caucasoid populations. Implications concerning human mtDNA evolution are discussed."

The reader will please note that these two articles have appeared in reputable journals 2 or 3 years before Templeton's supposed break-through in falsifying the Wilson group's African Eve hypothesis. I suspect that this is what disturbed André most of all, not what MOTHER TONGUE said.

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The next article seems to find the Geneva group taking a more jaundiced view of phylogenetic reconstruction and analysis.

In HUMAN EVOLUTION, vol.7 - N.2 (47-61) - 1992
DO MOST HUMAN POPULATIONS DESCEND FROM PHYLOGENETIC TREES?
A. Langaney, D. Roessli, N. Hubert van Blyenburgh, P. Dard

The summary goes strongly to a viewpoint which oddly enough would probably be most compatible with an archeologist's perspective.

"Molecular biologists and some population geneticists have recently claimed to be able to reconstruct modern human populations remote history by means of phylogenetic trees. Many objections to this method are discussed in the present paper. The most important are
1) Inter-populations migrations are likely to have been important even in the remote past. So the 'treeness' of this evolution is disputable.
2) There is no reason to believe that actual molecular phylogenies would be convergent between different molecules and would therefore represent populations history.
The various kind of genetic data, their relations to other data and the limits of their possible use in the analysis of our past are then discussed, together with the ideological background of the most common theories and of their publication.
It is very likely that the history of different populations was heterogeneous. Small and isolated hunter-gathers frequently evolving close to a phylogenetic model, while dense and increasing populations, since the Neolithic, were closer to a dynamic network model, structured by isolation by distance.
In any case, our present knowledge is obviously insufficient to reconstruct our genetic past, especially on the long term, and we can only hope that the development of the HUGO Genome Diversity project is going to yield the significant information presently lacking."

And, indeed, André was present at the HUGO conference at Penn State last October and probably attended a subsequent one this February in Europe. (And HUM. EVOL. could use an editor!)

For a 'mere' 15 pages André's article is very powerful and thought provoking. Consider also this section of the article which dwells on the rivalry between 'polycentric' (Rising Tide) and 'monocentric' (Garden of Eden) theories. They needed a good editor, as already suggested. The section title is misleading.

"Common origin dating

For over a century now, the hypotheses in which different modern human populations would descend from different primate species, or even from different human species, have been abandoned. Nevertheless, the 'polycentric' hypothesis, stating that different races of the same Homo erectus ancestor would have generated, almost independently, different races of Homo sapiens sapiens, still has some rare supporters in the fields of anatomy and paleontology (see for example Wolpoff et al, 1981...). On the other hand, geneticists and other anthropologists (see for example Stringer & Andrews, 1988) support the alternative 'monocentric' hypothesis of a common origin of all modern humans, from a single Homo erectus population evolving into the first Homo sapiens sapiens".

"Under the polycentric model, races would have been significantly separated for 400,000 years, or more, and their common origin would be still older. Many studies, since Nei & Roychoudhury (1974), have shown that the observed gene frequency variation in human populations is not compatible with more than 200,000 years of separate evolution. So, the only way to sustain the polycentric hypothesis is to suppose that important inter-migrations prevented genetic differentiations of populations[1];" "but this balance between migration and isolation by time just leads to the idea of a worldwide monocentric network of populations. The unique argument that supports polycentrism is a claimed morphological similarity between Homo erectus and Homo sapiens sapiens from the same continent. Such an argument is extremely weak if one inspects the considerable differences between Homo erectus and Homo sapiens, and the very slight anatomical resemblances in face or dental morphology that are supposed to demonstrate continuity throughout hundreds of centuries, thousands of kilometers and analogous drastic changes in morphology. Moreover, if such similarities between Homo erectus and Homo sapiens from the same continent are genuine, they could easily be accounted for by the more parsimonious explanation of convergence between unrelated populations, either by similar environmental selection (very likely for morphology) or by chance. Nevertheless, we cannot exclude, with this type of arguments, a partial contribution of local Homo erectus to local modern Homo sapiens. But it is obviously a non-falsifiable

[1] Editor's note. The argument is too compressed but means that under polycentrism the populations of various continents are reproductively isolated from each other. Isolated races which swap no genes eventually become separate species or 'fissive' & inter-sterile as Langaney puts it. So polycentric theory must have a mechanism to keep the genes moving around from the Kalahari to the Orkneys, Tasmania and Patagonia. Thus Langaney argues that serious migrations are the only way to accomplish that and save polycentric theory. Pace process people!

hypothesis.[2] "

The arguments, favoring the monocentric hypothesis are of three types:

1) Until now, all the oldest and doubtless Homo sapiens sapiens have been found in Africa and the Middle East. Even if one cannot exclude future finding elsewhere, it is an argument for a geographical location of our origins in these areas.

2) Nothing, in modern human genetic pools, suggests an old partition of populations, or indicates a previous fusion of different genetic pools, when one considers large scale samples of representative data.

3) Genetic drift, founder effects, and migrations can perfectly explain the observed distributions of neutral gene frequencies within a history of about 100,000 years or less, while a longer isolation of separate races would have fixed a large number of racial specific genetic markers, which are not observed at all.[3] " (End of quoting and end of this section)

The last Langaney article that we have space for concerns Africa. It appeared in GENETICS AND HISTORY OF SUB-SAHARAN AFRICA 30:151-194 (1987), authored by Laurent Excoffier, Beatrice Pellegrini, Alicia Sanchez-Mazas, Christian Siman, and André Langaney.[4]

ABSTRACT "This paper aims to review the contribution of genetic data to the prehistory and history of sub-Saharan African peoples. The authors review briefly paleontologic data, which give limited information about modern Homo sapiens sapiens origins and isolation of present African gene pools. Most linguistic and archeological theories about African peoples' prehistory are then confronted with the most informative genetic

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[2] Easy for him to say but non-falsifiability, the lack of testability, is a deadly comment on the scientific merits of any hypothesis. If we cannot test a theory empirically to see if it is true or false, then such a theory is outside of science, like the beliefs of religion or myths of politics. However, what is obvious to one scholar is not necessarily obvious to another. Greenberg has suffered from this problem for 40 years. Why is polycentric theory 'obviously' untestable?

[3] We both tend towards hyperbole. There actually are some marker genes which correlate pretty closely to 'race'. Gmfab and Gmfanb adhere to so-called Caucasoids and southeast Asians or those ultimately derived from them, e.g., Somalis & Polynesians.

[4] For the benefit of our members. Most of the authors can be reached at the following address, except for Excoffier who is now at Rutgers University (New Brunswick, New Jersey 08903):

Laboratoire de Génétique et Biométrie et Laboratoire de Préhistoire et Paléoanthropologie, Université de Genève, 1227 Carouge Genève, Suisse (Switzerland).

data available. Rhesus, Gm, HLA, and DNA data are analyzed. Their frequent haplotypes are compared between populations by means of genetic distances and average linkage clustering. Despite heterogeneities between the quality and the quantity of data provided by different genetic systems, some clear conclusions can be drawn. Genetic differentiation clearly parallels the clustering of major linguistic families. These families of populations seem genetically homogeneous, suggesting either relatively recent origins or long-term important and continuous intragroup migrations. The well-known divergence between the historical theories suggested by immunological and DNA data about the relationship between Africa and other gene pools is discussed. Decisive conclusions about African origins of modern humans either from fossil or from DNA data seem very premature. An alternative hypothesis issued from overall genetic variation is proposed.[5]"

The alternative hypothesis is given in their Conclusions; it is interesting:

"Though Africans are genetically clearly differentiated from other populations of the world, they do not seem to constitute the latter's direct ancestors[6].If we accept a hypothetical

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[5] The parallels between genetic and linguistic clusters will please Cavalli-Sforza as it does many of us. The Excoffier team also mentions the saliency of the Tutsi and Hima genes, despite Bantu languages; the same for the Zulu, Xhosa and other South African Bantu. There is also a noteworthy dichotomy among some of the very poorly represented Nilo-Saharans: Kunama and Sara (very different branches) are much more like each other and West Africans than they are like the Nilotes. The data should put to rest the notion that tall East Africans are all alike. The Nilotes clearly are not the source of either the Tutsi or the Hima; nor are the pastoral Cushites the source of the Nilotes but they seem to be akin to the Tutsi and less so the Hima. The Swiss team's linguistic taxonomy is about 30 years out of date.

[6] Heavens! Who ever said they did? This is the second serious misinterpretation of collegial hypotheses found in team Excoffier's writing. To say that humanity derives from Africa of 100,000 to 200,000 years ago is quite different from saying modern Africans are the direct ancestors of non-African moderns. The team Wilson hypothesis says that Eve -- our hypothetical mother -- was the first woman in the lineage which led to modern peoples. Modern Africans, even ancient Egyptians, and Eskimaux are 'descended with modification' from those basic old humans.

Their second misrepresentation (elsewhere) is when they scold Rapacz and team Wilson for proposing that Africans are the 'missing link' between apes and modern humans (presumably not including modern Africans). They wax all indignant and scornful about this supposed hypothesis but it is a straw man. Rooting one's calculations in one area is quite different from putting the people of that area into the category [Primitive Man-Ape].

initial divergence between human groups around 200,000 B.P., it is very difficult to imagine that all the known genetic differentiations could have been developed from an African gene pool in such a short period of time on other continents.[7] That is why we favor the hypothesis of non-African populations colonizing Africa after having been subjected to a drastic founder effect and random genetic drift. The resulting loss of some alleles or haplotypes and a frequency increase of others are in agreement with our results on blood group data. In this view, populations still possessing numerous different haplotypes may be regarded as more or less representative of ancestral populations. This could be the case of some East Africans."

"Studies of DNA polymorphism, though showing that Africans have accumulated more DNA changes than others, cannot presume the direction of a primordial migration into or out of Africa. Distances based on mean number of codon differences do not take into account gene flow (Slatkin and Maruyama, 1975) or historical events. At any rate, as long as the questions of mutation rate and its mechanisms are not solved, any attempt in setting intraspecific divergence time seems premature." (End of quoting)

In MT-20 we hope to show some of their wonderful diagrams! These are quite distinct from family tree type diagrams, although they also use 'dendrograms' in most of their articles. The diagrams show a whole system of networks of populations which are also rooted in one place and have rooting type nodes which occur later on; these too being arranged in networks. The diagrams very much resemble an upside-down version of evolutionary diagrams used sometimes in physical anthropology. We once (long ago) reproduced a clear one from Michael Day's book on fossil hominids which was focused on the separate paths taken by Homo sapiens neanderthalensis and Homo sapiens sapiens.

Some of team Excoffier's conclusions, especially in their African work, are supported by a data base which appears to be inadequate. Either they use too few genes/haplotypes or they sample too few of the hundreds of African peoples. One study was based entirely on the Rhesus haplotypes, for example. Neither the valuable Duffy nor P nor MNS systems show up in their calculations. I am sure they would make a great difference.

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from 6 Anyway that category has been reserved for Tarzan, known to be an English lord manqué.

[7] But they mentioned in another article that 200,000 years is enough to produce species differences! How long does it take to produce a human race (not the human race)? The evidence of the Khoi and San suggest thousands of years but modern Hawaiians and 'black' Americans got distinctive in less than 300 years.

LET THE TAXONS (OR TAXA?) FALL WHERE THEY MAY

The Validity of Correspondences between Indo-European and Semitic

Saul Levin (State University of New York at Binghamton)

When Hal Fleming *(Mother Tongue,* 17, pp. 10-11) reviewed my chapter on *"Full* and Other Key Words Shared by Indo-European and Semitic" in the Lamb and Mitchell volume, *Sprung from Some Common Source: Investigations into the Prehistory of Languages,* he must have felt wounded to the quick, because I had begun by disparaging Afro-Asiatic as a "loose constellation of language families", not on a par with the coherence of Indo-European. "Afrasian", as he prefers to call it, is (I see) very close to his heart; by suppressing the *o-,* he symbolizes the perfect unity of those languages spoken partly in Africa, partly in Asia.[1] But it is the facts of language that are bound to prevail, in the long run, over any linguist's sentiment.

There are facts of vocabulary and morphology that led, reasonably if not conclusively, to the grouping of many languages under the name of Hamito-Semitic, later renamed Afro-Asiatic by Joseph Greenberg. There are impressive verb-paradigms indeed, as Fleming declares, although I do not share his certitude that these go back far more than 10,000 years. The most extensive and precise of them, to my knowledge, is best exemplified by Beḍauye (a Cushitic language) and Arabic (a Semitic):

	Beḍauye	Arabic[2]
'he has written'	*íktib*	يَكْتُبْ (yaktub)
'she has written'	*tíktib*	تَكْتُبْ (taktub)
'you (m. sing.) have written'	*tíktiba*	" "
' " (f. sing.) " " '	*tíktibi*	تَكْتُبِيْ (taktubi^{y})
'I have written'	*áktib*	أَكْتُبْ (ʔaktub)
'we have written'	*níktib*	نَكْتُبْ (naktub)
'they have written'	*ektíbna*	يَكْتُبْنَ (yaktubna) (fem. only)
'you (pl.) have written'	*tektíbna*	تَكْتُبْنَ (taktubna) " "

Lacking personal familiarity with the African languages (aside from a little Egyptian), I rely upon Leo Reinisch's chosen paradigm.[3] Doubtless he could have chosen a different verb-root, which would not on its face be – like *k(-)t-b* 'write' – a borrowing from Islamic civilization in the last thousand years or so. But that would not matter much; for, in his opinion, "Alle dreiradicaligen verba können fast allgemein als semitische lehnwörter bezeichnet werden" (p. 42).

What about the subsidiary morphemes? The four subject prefixes of Semitic – {y-} 'he', {t-} 'she' or 'you', {ʔ-} 'I', and {n-} 'we' – are reported to have clear cognates throughout Berber and Cushitic (though not in Egyptian). The sharing of them is probably as old as any other feature of comparative morphology, anywhere in the world – maybe older. Also {y-} and {t-} in the sense of 'she' (but not 'you') have cognates in Hausa and some other Chadic languages. The suffixes that express gender or number are, however, less widespread. I do not consider it proved that the entire paradigm which Beḍauye shares with Arabic must antedate the separation of Cushitic from Semitic (even if that was, in Fleming's words, one occurrence in "a real, knowable historical [!] and genetic Entwicklung"). An appreciable part, at least, of this impressive sharing of morphology may be due to more recent diffusion – say, from a superstrate or a substrate language (or languages) relatively late in the prehistoric period.[4] Many variables are involved, and most of them are only to a meager extent traceable.

So ALL comparative data ought to be welcome, as far as they go. While I, with my particular knowledge, happen to specialize in the links between Semitic and Indo-European, I want to see more research in the opposite geographical direction too, comparing Semitic with what Fleming calls "its own true kin". I am proud of my one article on "An Accentual Correspondence between Hebrew and Hausa," in *Forum Linguisticum,* 4 (1979-1980), 232-240. Only Biblical Hebrew, among the Semitic languages, affords evidence of a cognate to the opposition in the Hausa vowel between the jussive *ya̱* (low-pitched and short) 'let him' or 'he should' and the preterite *yá̄* (high-pitched and long) 'he did'. The facts about Hebrew that I ferreted out would have been accessible enough to any Hebraist who took an interest in such minutiae, if only it had occurred to somebody before me that something worthwhile was waiting to be disclosed.

Now Fleming, I dare say, is glad to learn from me about a accentual correspondence, not previously perceived, WITHIN Afro-Asiatic. However, when I publish other research along similar lines, but about correspondences of morphology between Semitic and Indo-European languages, he reacts with dismay. Why the difference? Because Afro-Asiatic (or Afrasian) is "a valid taxon", while the connection between Semitic and Indo-European constitutes "an <u>invalid taxon</u>" (his underline). A term from biology becomes the label for a linguistic fallacy.

We have only to reflect that a language is not transmitted through the chromosomes; it is LEARNED, from people who already know and speak it. Over the generations, changes develop in it, sometimes slowly, sometimes rapidly, depending upon various circumstances. Among the most powerful causes of change is the influence of persons who know and speak other languages. Many, if not all, of the morphemes shared by Semitic and Indo-European languages may well have spread in one direction or the other, together with important items of vocabulary, not very long before the Semitic peoples began to settle permanently in parts of southwestern Asia. From my perspective, that does not make them irrelevant to comparative linguistics. And I make no claim to have identified which morphemes, if any, bear the "Nostratic" stamp, however that term may be defined.

Nevertheless the model of language taxonomy to which Fleming adheres may be seriously affected, not to say weakened, by the success of my correspondences. In the article criticized by him, I have shown how much of the inflection of the noun {þawr} 'bull' in Arabic, ταυρ- in Greek, is shared (herewith I give a summary of the evidence):

Accusative singular, ثَوْرًا {þawran} : ταῦρον

Genitive sing., Latin *taurī* : Arabic {-ī} (at the end of a verse)

Nominative dual, Gr. ταύρω [-ɔ̄] : Arabic {-ā} (construct only)

Gen. dual, Arabic ثَوْرَيْن {þawrayn} (pausal) : Gr. -οιν

Nom. plural, Latin *taurī* (-EI) : Aramaic construct pl. תּוֹרֵי {toʷreʸ}

Gen. pl., Gr. ταύρων : Arabic collective ثِيرَانْ {þīrān} (pausal)

Derived nom. sing. fem., Gr. Ταυρώ (epithet of Artemis) : Old Aramaic שורה {S/šwrh} (= Biblical Aram. *{toʷrɔ̄ʰ} 'cow')

More of my article, however, concerns a broad system of stative forms with at least six inflectional morphemes, either internal or external; e.g.
Hebrew מְלוֹ {mɔlúʷ} 'full' : Gr. πολύ|καρπος 'fruitful'
Heb. fem. מְלֵאֲתִי מִשְׁפָּט {məleʾătīʸ mišpɔ́ᴛ} 'full of justice' :
Gr. πλησι|φαὴς σελήνη 'the moon full of light'
I also cite Hausa *fal;* I am not aware, however, of any inflectional correspondences to Semitic in this word.

But all these correspondences between Semitic and Indo-European cannot be wished away, or dismissed as illusory, and must therefore be fitted into any valid theory about the prehistory and classification of languages. The theory that Afro-Asiatic is one whole taxon, distinct from all the rest, like the zoological order *Carnivora* or *Primates,* will have to be revised. There may remain a tenable claim that in the languages called

Afro-Asiatic some common vocabulary, or morphology, or both, originated earlier than anything else in them, and that those shared features can be definitely identified. Only to that extent will Afro-Asiatic take precedence in time over correspondences of the sort that I have demonstrated, involving just the Asiatic part along with the Indo-European region beyond it.

The facts matter more than any theory. The same etymology that brackets Arabic {þawran} with Greek ταῦρον must take in the Finnish accusative *tarvaan* (referring to the bison or the aurochs). Here the nominative form *tarvas* (found in Estonian too) is likewise cognate to the Indo-European form exemplified by ταῦρος, Lithuanian *taũras*, and – above all – Gaulish TARVOS, which displays the same metathesis as Finnish. It won't do to infer that the inflections of this word uphold the genetic closeness of Indo-European to Uralic (= Finno-Ugrian), in which Fleming apparently believes along with many others, but nothing of the sort in regard to Semitic.

Not only in the book review but elsewhere in *Mother Tongue*, he manifests a consuming interest in genetic classifcation of ALL THE LANGUAGES OF THE WORLD, besides biogenetics (analysis of DNA, etc.), so as to chart the prehistoric development and movements of all branches of the human race. I consider this far-reaching quest admirable in itself, but liable to err by overlooking many particular facts, or even disregarding them methodically, if they appear incompatible with the one grand scheme envisaged by the researcher.

I personally am equipped for a different kind of research. How much the learned world will gain from it in the long run, is beyond my control. But those who disagree with me have to cope to something inherent in the data, which they may never be able to surmount: The ancient languages that I concentrate upon enjoy the advantage of having been accurately recorded, ages ago, in scripts sensitive to fine nuances of sound.[5] Accordingly, if we take the trouble – or rather, if we feel the urge – to study them, we can know them in much greater and more precise detail than thousands of modern languages that have been sketchily described in the nineteenth or the twentieth century. By comparing the few ancient Semitic languages with a somewhat larger base of Indo-European languages, we probe the prehistoric past along a limited but singularly clear trajectory. I do not attempt to estimate how many thousands of years it reaches back; others, no doubt, can do that better.

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[1] *Afrasian* is of course modeled upon *Eurasian*. However, *Eurasia* means '(all of) Europe and Asia'; and as a geographical term it makes good sense, now that we have progressed beyond the knowledge of the ancient Greek geographers. But hardly anyone would speak of **Afrasia*, in view of the narrow land-bridge at Suez.

What's in a hyphen, anyhow? We call a certain Slavic language Serbo-Croatian; but the scholars of the region, who spell it српскохрватски језик in the modified Cyrillic alphabet or *srpskohrvatski jezik* in Latin letters, have not allayed the hatred sundering the two communities.

2 These Arabic forms are jussive: 'let him write', etc. But the negative adverb لَمْ (lam) makes it 'he has not written' or 'he did not write'.

3 "Die Beḍauye-Sprache in Nordost-Afrika. III," *Sitzungsberichte der philosophisch-historischen Classe der Kaiserlichen Akademie der Wissenschaften*, 130. Band (Wien, 1894[1893]), Abhandlung VII, 56. I have, however, corrected an inaccuracy in his Arabic (probably a misprint).

4 In this connection, it seems pertinent that the Arabic dual and masculine plural forms are not represented in Beḍauye, but that all the Beḍauye forms are represented in Arabic, except for *tiktiba* 'you (masc. sing.) have written'. The identity of the 'she' form with the 'you (masc. sing.)' is the one most striking anomaly of the Semitic languages, NOT shared with any others.

5 The Phoenician(-Hebrew) consonantal alphabet, reformed by the Greeks to include vowels and thereafter reinforced with supplementary marks, is not only of unique import for cultural history in general – of more enduring consequence to mankind than any link between the Semitic nations and the Egyptians (let alone the other peoples speaking Afro-Asiatic languages). Also for linguistics in particular, alphabetic writing constitutes the inescapable framework within which we study all languages, including the ones not so written by the communities that employ them.

The alphabet of course spread easily throughout Semitic territory, in time extinguishing the Akkadian cuneiform syllabary; and beyond there the successful adjustment of it to the Greek language was what released the great genius of Occidental civilization. (Others too, such as the Berbers, picked up this Semitic invention, but it made no such difference in their culture.) So I make bold to say that the Greeks were, at any rate mentally, most AKIN to a Semitic people in their ability to analyze a language practically and intellectually. This shared skill was derived, perhaps, from some mutually profitable symbiosis in prehistoric times, which is evinced by the vocabulary and morphology found in the Greek and the Hebrew corpus, and which the Greeks of the classical period expressed mythically in their tales of Cadmus, the Phoenician founder of Thebes.

3291 S. Spring Branch Rd.
Bloomington, IN 47401
Sept. 9, 1992

Dear Hal,

You are doing a great job with MT. It is extremely useful and very informative. Thank you for your kind words about me in the review of the Rice conference volume. There is too much 'silence and silent disapproval' (5).

Herewith some reactions to your statement with regard to my 'rules' (never so labelled by me) that 'their target is mistaken; while ignoring closer relatives, they focus on a remote ancestor' (9). The next page implies that my reconstructions are at a level comparable to N. I am not sure what you mean by 'ignoring closer relatives'. I thought that I was dealing with just such.

Following is the last paragraph of my paper ('Hallelujah') for the 19th LACUS Forum 1992:

> It remains to say a few words about the implications of the approach followed here for the investigation of long range relationships in language. The consonant ablaut forms discussed here and elsewhere were unquestionably part of the proto language, but it is not yet clear to what stage of the prehistoric era they belong. They are certainly ancestral to all of Lislakh. One of the most striking features of this phylum is the manner in which consonant ablaut enables us to clarify the relationships of hundreds of roots. If a long range comparison includes Lislakh or any portion thereof and ignores this organization of the material, it loses a key tool in the setting up of regular sound correspondences.

This implies that I agree with you that the reconstructions are of broader application. There is, however, much more to be said about it. A distinction should be made between a base with its consonant ablaut variants (the simple base) and that same base plus other affixes (the affixed base). The affixes involved include both prefixes and suffixes, that fact itself being important when considering other possible relationships. It is in the distribution of affixed bases that we can see the narrower scope of LL itself, of a single branch thereof, or of a combination of branches. That is, these more restricted distributions are one of the criteria by which LL or any other proto grouping may be justified. I don't believe that my files to date allow me to form solid judgments along these lines, but one can observe some indications that such data are forthcoming. It follows that what are now being reconstructed as affixed bases may be a much stronger tool for uncovering long range relationships than are the simple bases alone.

An example of what I mean is **d-b 'margin(al)'. This yields Eg. d-b 'horn', IE *dwō 'two'. With affixes (in parentheses): **(ʔ-)d-b, Eg. ʔ-d-b 'bank (of river)'; **(ʔ-)Nd-b, Eg. ʔ-n-b 'wall'. An IE affixed form **d-b(-s) becomes Gk. dis (loss of b), Lat. bis (loss of d) 'twice'. Another affixed form, **(ð-)Nd-b 'pertaining to the margin' yields Eg. z-n-b-w 'battlements', Ar. ðanab 'tail' and, from **snwV-, IE *snē- (Watkins), *snā- (Pokorny) '(having to do with) the nose', with IE *s from **ð. So snout (which retains the w from **b) and tail are both regarded as marginal. Cushitic has *danb- 'hindquarters', apparently from the same affixed base. As more AAs related forms may show up, we are not justified in setting up an Eg.-Sem.-Cu.-IE subgroup at the present time. If a large number of affixed bases are found to occur in this set of branches of the phylum, we could consider such a subgroup. Likewise, if we can show that both simple bases, such as **d-b, and affix ones from the same bases are reflected in Uralic, Altaic or other preseumed relatives, we would have a much stronger case for claiming relationship. Reflexes of an affixed form only could point to borrowing.

Among the affixes which have been postulated are ?, h, ḥ and ᶜ (proto **?, **h, **?H and **hH respectively). ? and h have long been recognized as affixes. Leslau identified ḥ as such but did not include Egyptian in his examples. ᶜ was only a stepchild affix until recently, when it has come into its own. Work on LL has shown that all of these may serve as affixes. Møller included ?, ḥ and ᶜ in his 'Laryngale', and the IE Laryngeal Hypothesis has suffered from not following his lead. A suffix -x (presumably -ḥ) has been seen in Hitt. newax- 'to renew', but where there is no Anatolian evidence uncertainties abound. For example, Eg. ḥ-b-s 'clothe', 3-b-s (a type of headdress) and Sem. *l-b-s 'clothe' show that the base **b-s 'on' may take the prefixes ḥ and l (Eg. 3). IE *wes- 'clothe' shows no trace of a prefix and presumably reflects plain **b-s. It may, however, have had a laryngeal prefix now lost. The frequent use of affixes consisting of laryngeals must be recognized if we are to deal effectively with the Laryngeal Hypothesis in IE. The possible occurrence of more than one laryngeal affix on the same root greatly complicates the situation, but this complexity must be faced if we are to reach any useful conclusions. The considerable loss of laryngeals in IE is paralleled in other groups (e.g., Chadic) and may also occur in more distant relatives. To recognize this possibility is essential to broader based comparisons.

To conclude, the simple and affixed bases reconstructed by me may fairly be ascribed to LL as they are based upon LL data. It is probable that the simple bases will prove to be or to resemble the shapes of proto forms eventually to be reconstructed for a broader based relationship. I have not made comparisons beyond LL, nor will I have time to do so. The N reconstruction I have looked at ignore consonant ablaut and are, to that extent at least, flawed. If I were to broaden the comparison, I do not feel that I could rely upon what has been done but would have to make fresh analyses. This is the approach I have taken with LL. While many earlier comparisons have been validated, this has come about by re-assessing the data, not by uncritical acceptance of earlier results. I applaud my predecessors but have had to make my own way to the results awaiting discovery. Considering the large number of bases which have been observed to be in common between AAs and IE, I do not see how anyone can doubt the relationship of the two groups. (Something over 100 LL bases have been treated in some fashion or other in articles now in print. More to come.)

Changing the subject slightly, something should be said about Levin's approach (10). As I noted in my review of his 1971 book, one should not criticize him for not doing what he had no intention of doing, namely traditional comparative linguistics. He points out remarkable parallels between Semitic and IE, but he does not claim that these features are inherited - only that they exist and are not the result of chance. If I understand him correctly (we had discussions in Montreal in August), he thinks of them as areal phenomena, due to the geographic juxtaposition of the languages involved at some period prior to classical Sanskrit, Greek and Latin times. I am more inclined to see the same phenomena as inherited, but one should not put up a barrier between his work and that of others just because he has limited his field of investigation. His observations need to be included in our work.

I come now to a field in which I have no expertise, that of physical types (47-48). Years ago I asked our physical anthropologist Georg Neumann to what physical type the ancient Egyptians belonged. His answer was 'Mediterranean'. I have followed Munson in taking the AAs peoples from a homeland on the Upper Nile (11); IE must be added if LL is valid. Our evidence for their physical type is twofold, skeletal remains and ancient Egyptian representations. The latter clearly distinguish between black Africans and Egyptians. The former are painted black, the latter red for the men and yellow for the women. There is an (!) Old Kingdom reserve head which is clearly that of a black African. Others in the same collection are of a different physical type. Your observations about the variety of types in Africa is right to the point. My conclusion is that the LL peoples were a different physical type from the more southerly black Africans, who in turn were of various physical types. I would now like to hear from more physical anthropologists on the subject.

A minor note. The 4000 years of page 11 refers to the time presumably covered by the forms discussed in the 1975 article, not to the total length of time for which we have written records in Egypt.

This has become much longer than I intended. Even so, it is a poor substitute for a chat about it.

With best wishes,

Yours,

Carleton

Carleton T. Hodge

A Provisional Classification of Human Languages

by John D. Bengtson, 1993

MACROPHYLA:	Phyla:	Sample languages:
KHOISANIC	South Khoisan	!Kung, Nama
	Hadzan	Hadza
	Sandawean	Sandawe
KONGO-SAHELIC	Niger-Kongoan	Swahili, Zulu, Yoruba, Fula
	East Sudanian	Nubian, Maasai, Barabaig
	Mandean, etc.	Mandinka, Songhai, Kanuri
SOUTH NOSTRATIC	Afrasian	Arabic, Hausa, Somali, Riff
	Kartvelian	Georgian, Zan
	Dravidian	Tamil, Brahui, +Elamite
NORTH NOSTRATIC	Indo-Hittian	Welsh, Kurdish, +Hittite
	Uralian	Finnish, Magyar, Nenets
	Yukaghiran	Yukaghir, +Omok
	West Altaian	Turkish, Mongol, Manchu
	East Altaian	Korean, Japanese, Ainu*
	Chukotian	Chukchi, Kamchadal
	Eskaleutian	Inuit (Eskimo), Aleut
DENE-CAUCASIC	Macro-Caucasian	Basque, Avar, Burushaski
	+Sumerian	+Sumerian
	Sino-Tibetan	Chinese, Burmese, Garo
	Yeniseian	Ket, +Pumpokol
	Na-Denean	Haida, Tlingit, Hupa, Navajo
AUSTRIC	Austro-Miao	Khmer, Hmong, Vietnamese
	Austro-Tai	Malay, Fijian, Maori, Thai
INDO-PACIFIC	+Tasmanian	+Tasmanian
	Andamanese	Jarawa, Onge, +Aka-Bo
	Papuan	Siroi, Iatmul, Arapesh
AUSTRALIC	Pama-Nyungan, etc.	Dyirbal, Aranda, Tiwi
AMERINDIC	Northern Amerind	Cree, Zuni, Yuma, +Yahi
	Central Amerind	Hopi, Kiowa, Otomi, +Mangue
	Chibchan-Paezan	Tarascan, Timucua, +Chibcha
	Andean	Quechua, Yamana, +Puelche
	Equatucanoan	Jivaro, Guarani, +Taino
	Ge-Pano-Cariban	Bororo, Carib, +Mongoyo

+ Denotes extinct phylum or language.

* The classification of these languages is perhaps the most controversial of all. Most paleolinguists include Japanese and Korean in North Nostratic (J.H. Greenberg's Eurasiatic). Paul K. Benedict classifies Japanese as Austro-Tai. Karl Menges includes Japanese and Korean, but not Ainu, in Nostratic. Clearly, Japanese incorporates elements from both Nostratic and Austric. The problem is to determine which ancient type predominates in the genetic core of the language

"NEW" DENE-CAUCASICA

John D. Bengtson
August 1992

"SALIVA, PUS, SORE": Basque (Guipuzcoa) **lerde** "drivel, saliva" / Cauc. ***łwirdi** : Archi **łit** "manure", Avar **xwerd** "pus" // Na-Dene: Kutchin **łid** "scar", Chipewyan **łùr** "scab", Navaho **łóód**, **-lóód** "sore".

"SMALL MAMMAL": Bsq. **erbi** "rabbit" < ***e-rVgʷi** / Cauc. ***rigwV** : Ubykh **dəɣʷə** "mouse", Ingush **daXka** id., Akhwakh **reƛ̄'u** "squirrel, weasel", Tindi **reƛ'u** "marten" // Sino-Tibetan: ***ruak** : Maru **ruk** "rat", Burmese **k-rwak** // ND : ? Tlingit **dáá** "weasel", Navaho **ńš-dóí** "lynx, wildcat". (Starostin 1984 : Cauc + ST).

"SEED" : Cauc. ***k'ĕrk'ĕnV** : West Cauc. ***k'ak'anə** ~ ***k'anək'a** "egg, walnut", Hurrian **kirikiri-**(j)anna "seed of pinecone", Karata **k'ark'an** "egg", Avar **k'ork'onu** "grape, berry" / Burushaski (Nagir) **kakāyo** "(unbroken) walnut" // ST : Tibetan **khra** "a kind of seed", Old Chinese ***kra-s** "seed, to sow" // ND : Haida **k'áánk'aay** ~ **k'áánk'aan** "unripe berries", Navaho **-k'ǫ́ǫ́?** "seed, pit".

"INSECT WITH STING": Bsq. **liztor** (Guip.) "wasp", **lózer** (Souletin) "hornet", **loze-bĩã** (Soul.) "wasp" / Cauc. ***ƛamc'V** : Avar **ƛ'ož** "wasp" // ND : Chipewyan **ƛ'ízɛ̀** "bulldog fly", **ƛ'ízɛ̀-tθùwɛ́** "wasps", Navaho **ƛ'ěžìì** "horsefly, gadfly". (Bouda 1948 : Bsq + Cauc)

"FIREWOOD, FIREBRAND": Bsq ***i-łinti** > **ilhinti** (Souletin) ~ **illindi** ~ **ilindi**, etc. "firebrand, ember" / Cauc. ***łwindV** "firewood" > Andi **łudi**, Hunzib **hũdu**, Khwarshi **łido**, etc. // ND : Eyak **łid** "deadwood, firewood". (Nikolaev 1991 : Cauc + ND)

John D. Bengtson

Some Dene-Caucasic comparisons:

1. Sino-Tibetan: Magari **let** 'tongue', Kachin **siŋ-let**, Old Chinese ***lat**// Na-Dene: Tlingit **ɬ'ut'** 'tongue', Eyak **-laat'~ -n'at'**, Yakutat **kha-leth**.

2. Basque **anai(e)** 'brother' (male speaking), **ne-ba** 'brother' (female speaking)// Na-Dene: Tlingit **hunaX** 'man's older brother', Chipewyan **-unaɣa** 'older brother', Navajo **-ínaí**.

3. Basque **hotz** '(to be) cold'/ Caucasic: Avar **kʷač-** '(to be) cold'// Na-Dene: Athapaskan ***k'as** '(to be) cold'.

4. Caucasic: Bezhta **i-t'ino** 'small', Avar **hi-t'in-ab**// Sino-Tibetan: Old Chinese ***tōnʔ** 'short', Tibetan **thuŋ**, Kachin **gè-dùn**// Na-Dene: Haida **t'am-ǯu** 'thin', Chipewyan **t'ànè**, Navajo **t'ą̄hi**.

5. Basque **edan~eran** 'to drink'/ Caucasic ***HVƛwVnV** 'to drink' (Bezhta **Xuƛal**)// Na-Dene ***ƛaNH** 'to drink' (Yakutat **-lia**, Navajo **-ƛą́ = -dlą́**).

6. Basque **jan** 'to eat'// Na-Dene ***yan** 'to eat' (Tlingit **-yan**, Navajo **-yą́, -yą́ą́ʔ**).

FORTHCOMING ARTICLE BY BENGTSON & BLAŽEK

Some Kusunda - Dene-Caucasic Comparisons

1. Kus **gita-sē** 'child' (G, H)// Yen ***gəʔt** 'children' (Ket **kʌʔt**, Kott **kat** Yen 161)// ND: Haida **gIt∼giit** 'son', Tl **gIt yIt** 'son' (P §42) [cf. also Nahali **giṭa** 'younger brother'; Ruhlen 1989a Yen + ND + Nahali; Gurov 1989: 43 Kus + Yen].
2. Sum **na** 'person', **ni-ta**, **ni-taḫ** 'man' (B)// Kus **niyu** 'person' (R)// ND: Tl **na** 'tribe, people, Ath ***-ne∼*-n** 'person, people': Sar **dì-nə́**, Nav **di-né**, Mat **-nii** (P §19) [Bengtson NSC §129 Sum + ND].
3. Kus **duwai** 'husband' (R)// ST ***do** 'to be related by birth or marriage' (Kach **do**, Burm **tau** STC 59)
4. Bsq **e-ma-n** 'to give'// Sum **mu** id.// Kus **ma** 'to take' (H) [cf. Nahali **ma-** 'to give'].
5. Kus **bhoq** (R), **bhrok** (H) 'hot'// ST ***bok** 'white' (OCh ***bhăk**, Garo **gi-bok∼gi-pok** STC 181); cf. Kan **bokh** 'hot' (Bailey)// Yen ***boʔk** 'fire' (Ket **bɔʔk**, Pump **buč** Yen 161) [Semantics as in IE ***bhel-**>Slav **bělŭ** 'white' vs. OIc **bāl** 'flame' Pokorny 1959: 119].
6. Kus **qaaiwan** 'dry' (R)// Yen ***qʌj-** 'dry' (Kott **xuj-**, Arin **qoija** Yen 164) [An alternative, or complementary, etymology is given by Starostin Yen 212, and Bengtson NSC §115].
7. Cauc ***[ʔi]qV** or ***ʔirGwV** 'to get/be cold' (Lzg **req̄i-z**, Hur **eg-o**, **egi∼igi** HU 60)// Kus **yoʔau** 'cold' (R).
8. Bsq **zuzen** 'right (rectus, justus)'// Cauc ***c'inʔV** 'new' (Tsez **ec'no**, Ub **c'a**, Bats **c'in** 'nouveau', **c'ain∼c'ani** 'propre, saint' Yen 216; Sommerfelt 1938: 122)// Kus **jinda / jinaʔi** 'new' (R)// ST ***sĭn∼*sĭŋ** (Burm **sać** 'new', Tib **g-šin** 'good' Yen 216-7) [Starostin Yen 216 and Hyp 21 Cauc + ST + Yen ***tur-**; note semantic parallels in Bsq, Bats, Tib].
9. Cauc: Botl **biši** 'greasy, fat', Cham **beš-ab**, Hnz **boš-eru** (Gud 90)// Bur **bIs** (pl. **bIšo**)∼(W) **bes** 'fat' (n.)// Kus **biji** 'fat' (adj.) (H) [Bouda 1964: 604 Cauc + Bur].
10. Kus **manyi** 'many' (R)// ST ***maŋ** (Trung **də-maŋ** 'big; older', OCh ***măŋ** 'eldest; great, principal' STC 189).
11. Bsq **lanhű** (S)∼ **laino** (L)∼ **lanb(r)o** (L, BN)∼ **laño** (AN, G)∼ **laiño** (V, G, BN) 'mist, fog'// Cauc ***rVnħ'wV** 'cloud' (? God **hanlo**, Tnd **hinalu**, Cham **hãna** X 75)// ? Sum **dungu** 'cloud' (B)// ? Kus **duling** 'cloud' (RT) [cf. Alb **ren**>**rê** 'cloud', Illyr **rhinós** 'Nebel', possibly from DC substratum: Krahe 1955: 38].
12. Cauc ***gwɨmhV∼*m(h)ɨgwV** (Tsez **qema** 'rain', Bzht **qima-ro** 'clouds', Abkh **a-naq'ʷa** 'fog')// ST ***mŭk** 'fog' (Tib **r-mug-s-pa**, Lpch **muk** 'foggy', OCh ***mōk** 'drizzle' STC 77)// Kus **gaanigiling** 'fog' (RT; cf. preceding for element **-ling**)// Yen: Yug **xoaŋ** 'fog' [Starostin Yen 210 Cauc + ST + Yen].
13. Kus **khaangu** 'cold' (adj.) (RT)// ST: Tib **khyag-s** 'gefrieren, erfrieren', OCh ***Xjwan** 'Eiswasser'// Yen: Ket **qūŋ-el**, **qū**, **qou** 'ice' [Bouda 1957: 90 ST + Yen].
14. Bur **a-si** (pl. **a-sim-uc**) 'star', (W) **a-sum-un** id.// Kus **saaʔnaan** 'star' (RT)// ST: OCh ***sēŋ** 'star', Hruso **li-coŋ**, Yatshumi **cĭŋʰĭ**, Tengsa **lŭ-tiŋ tiŋ** (Hyp 22; IST 177; Shafer 1947: 194)// ND: Haida **sIŋ** 'sky, day'; Ath ***sUnʔ∼*cUnʔ** 'star': Minto **senʔ**, Chip **tθə̀n**, Nav **sǫ̀ʔ** (K 57) [Bengtson NSC §74 Bur + ST + ND].
15. Kus **duwu / du** 'earth' (R)// Yen ***tuʔw-** 'clay' (Ket **tuʔ**, Pump **tu-** Yen 147).
16. Kus **taang** 'water' (R)// ND: Haida **taŋ** 'sea water', Eyak **tạh** 'waves', Gal **ta-** 'water' (in comp.), Chip **tà-**, Nav **tá-** (Sapir 1915: 553; NDE).

Abstracted from Blažek & Bengtson "Lexica Dene-Caucasica"

AINU AND AUSTRIC

< John Bengtson

(AN = Austronesian)

	AINU:	AUSTRIC:
"head"	pa, pake	S. Bahnar *bo:ʔ; Munda *bɔk(bɔk); AN *buʔuk
"hair"	numa	Li *nom "head hair"
"ear"	kisara	Tai *xruɯ; Kam-Sui *qhya
"eye"	*ra(ra) in rara-numa "eyebrow"	Tai *traa; AN *maCa; Miao *maay
"mouth"	paro ~ čaro	S. Bahnar *bər "lip, bank"; AN *birbir "rim, edge, border"; Tai *bi ~ phi "lips"
"neck"	rekut	AN *likuḍ "neck, back"
"flesh"	kam	Li *xaam
"bone"	pone	AN (Atayal) *bani
"hand"	tek	Munda *tiʔ; Viet tay
"knee"	kokka	AN: Rukai *koko "leg"; Ong-Be kok "knee"; Tai *kok "foot" (of tree, hill)
"person"	guru ~ kuru	Munda *koro "man"; Mon kruaʔ "male"
"child"	tennep "very young child"	Munda: Parengi tonan "younger sister"; Miao-Yao *tɔn "son"
"year"	pa(ha)	Tai *pi; Li ʔbe
"earth"	tanina "a name for earth"	S. Bahnar *tne:h; AN *tanah "land"; Miao-Yao *ntaa(n); Tai *ʔdin
"water"	pe(he)	Bahnar bah "river's mouth"; Tai *ʔba "overflow, spill"; AN *baʔah "flood, water"
"fire"	abe ~ api	AN *Hapuy; Tai *vay

(abstracted from "Lexical Parallels Between Ainu and Austric," by Václav Blažek, 1993 ms.)

By Patrick Ryan

October 12, 1991

An Inquiry or Thought Paper. Comments solicited

Mr. Harold C. Fleming
5240 Forbes Avenue
Pittsburgh, PA 15217

Dear Hal:

Thanks very much for inquiring whether I have been getting Mother Tongue. It seems that we are now on track with my address, and I thank you for the trouble you took to insure delivery.

I am enclosing a check for 1991 dues.

I enjoyed your article in Mother Tongue 14 very much. It was an interesting oversight on current thinking regarding taxonomy and phylogeny.

I am also rather surprised when efforts like those of Aihenvald-Angenot result in strange labels like "Nos-cau". "Proto-Sapiens" as a designation for a language (!) seems particularly inappropriate semantically.

Yet your article made me realize that I have been barking up the wrong tree for some time. I have been able to identify the earliest syllables (*CV*) and their meanings that are the foundation for roots of the form *CVC*, so familiar from (P)IE and other early reconstructed languages.

I have designated these syllables "Nostratic" in my article and in correspondence with Alan when I really should have been calling them "Proto-Sapiens", or better *Proto-Language*.

Alan is so busy with his major project now that I am wondering if you would be available to offer a *critique* on these ideas, and suggest publications that might be willing to entertain the idea of publishing them.

Needless to say, reconstruction of these syllables and their meanings also entails my reconstruction of the earliest phonological system which differs in only mostly minor ways from Alan's reconstruction for Nostratic .

I have been thinking that it would be profitable to come into contact with others, perhaps ASLIPpers, who are working on and interested in African and Eurasian to see how these Proto-Language syllables and meanings (as well as phonological system) can be regularly related. Do you have any suggestions on whom I might contact?

I have been in correspondence with Professor Lehmann for over ten years; and though he has kindly sent me much interesting material, he has so far avoided making direct comments on my work (for understandable reasons). I am urgently in need of feedback from someone like yourself who is in a position to be familiar with everything that is developing in our rapidly burgeoning field of interest.

I would like to try another idea on you if I may.

Without getting into the question of whether the Aihenvald-Angenot "tree" is an accurate reflection of the development of relationships among the languages of the world, I would propose a taxonomy that might be a little more internally consistent.

Here is a list of those suggestions:

A-A	suggested...
Level 1 (from 130,000 B.C.)	
Proto-Sapiens	Proto-Language
Level 2 (from 92,000 B.C.)	
African	African
Proto-Noscau	Eurasian
Level 3 (from 40,000 B.C.)	
Eurasian ->	
Proto-Nostratic	North Eurasian
Proto-Scau	South Eurasian
Australian	Australian
Level 4 (35,000 B.C.)	
North Eurasian ->	

Caucasoid	Northwest Eurasian
Mongoloid	Northeast Eurasian
Amerind	American Eurasian

....

<u>South Eurasian</u> ->

Sino-Caucasian	Upper-South Eurasian
Austric	Lower-South Eurasian

Level 5 (from 20,000? B.C.)

<u>Northwest Eurasian</u> ->

Afro-Asiatic	Lower-NW Eurasian

....

<u>Northeast Eurasian</u> ->

Siberian	Siberian NE Eurasian
Arctic	Arctic NE Eurasian

....

<u>Upper-South Eurasian</u> ->

Na-Dene

Sino-Tibetan

Yenissean

North Caucasian

....

<u>Lower-South Eurasian</u> ->

Austro-Asiatic	Asiatic Lower-S. Eurasian
Austronesian	Pacific Lower-S. Eurasian

Level 6 (10,000? B.C.)

<u>Upper-NW</u> <u>Eurasian</u> ->

Kartvelian

Indo-European

Dravidian

Level 9 (from 6,000 B.C.)

<u>Siberian</u> <u>NE</u> <u>Eurasian</u> ->

Uralic

Altaic

• •

<u>Arctic</u> <u>NE</u> <u>Eurasian</u> ->

Chukchi-Kamchatka • Yukaghir • Gilyak

Eskimo-Aleut

I realize this involves some fairly lengthy nomenclature but the advantage that offsets this, in my opinion, is that it transparently reveals the postulated relationships (and is consistently <u>geographic</u>).

I hope that I have correctly interpreted the times of the arboreal sketch you provided.

Well, I hope to hear from you when it is possible.

Sincerely,

Pat Ryan
9115 West 34th Street
Little Rock, AR 72204
(501) 227-9947

Harold C. Fleming Ph. D.
5240 Forbes Ave.
Pittsburgh, Pa. 15217

February 28, 1993

Dear Hal,

In the Fall issue of "Mother Tongue" you were most kind to mention my work and the assessment I have promoted; that human/primate relationships may have untested parallels. The issue I have investigated started with a simple idea; that the American Indian has rarely been included in the search for human origins. Ultimately, my research led to the far ranging hypothesis; that should man have originated from within the American continents (A. R. Wallace 1887 1889; Sir A. Keith 1911; F. Ameghino 1915; and others) then a source for this origination must have been derived from within the higher primate family of which both modern humans and New World Platyrrhines are members. However, the general consensus has determined that New World anthropoids could not have achieved similar adaptations characteristic of the Old World apes.

The data I have accumulated so far leads me to counter that the arguments that would eliminate the evolutionary potential for the development of upright walking or any conclusions that would decree that terrestrial adaptation could not have been achieved by this isolated haplorhine (higher primate) group are arguments made from negative evidence. The fact that there are no known extant apes in the Americas and that fossil evidence to support their prehistoric existence has not been recognized (A.L. Bryan 1978)does not mean that paleontological evidence will never be confirmed. "The paleontologists studying the fossil history of primates have good reason to lament the fragmentary record that must be used to decipher the evolution of this important group. With the great strides that have been made in recent years in the Old World, it can truly be said that the paleontological record of the New World platyrrhines is indeed the weakest of the lot. There are many reasons for this, but these mostly stem from the fact that, with the push to find human ancestors, emphasis has been outside of South America" (Bruce MacFadden 1990 pg. 7).

My own research has led me to conclude that the discoveries of fossil hominoid forms in the Old World have been made through the efforts of financially supported research strategies that have grown from the need anthropological science has to verify the evolutionary history of our own kind. Carrying my own hypothesis to what I believe is its logical conclusion, from first the belief that our human ancestors may have been American Indians, then, the final analysis must leave open the question as to whether the evolutionary capacity of platyrrhines affords the same potential adaptations achieved by their haplorhini *sisters* of the old World.

I now believe that the sudden presence of modern humans in the Old World, marking - in European terms - the advent of the Upper Paleolithic, implies that modern humans - Homo *sapiens sapiens* -- were in the New World long before this event (> 35,000 ybp). A rationalization for shaping successful research strategies could be found in an archaeological consideration of the curious definitions of the New World paleoarchaic period. I welcome your skepticism and your open heartedness, however, I also recognize my own obligation to stand my (New World) ground in the belief that anthropology, as a science, has eliminated the American Indian from evolutionary concerns through the mis-evaluation of negative evidence. As you have suggested, my hypothesis can find wide ranging academic support from genetic (R. H. Ward 1991); linguistic (J. Nichols 1992) and historical (A . F. Chamberlain 1912; C. F. Lummis 1925) assessments that also promote the acceptance of a "pre-Clovis" archaeological definition and, at least, a mid-Pleistocene presence of mankind in the Americas.

Again, I thank you for the opportunity to respond to your counsel and remain,

Sincerely yours,

Alvah M. Hicks

Alvah M. Hicks
22050 Maricopa Hwy.
Ojai, Ca. 93023

LA LUTTE RAJEUNÉE: THE NEWS

NEW DICTIONARY OF ULWA. Kenneth Hale of M.I.T. said that he had been busy lately working on this, so he hadn't really had time to participate in the Pacific Rim Symposium. It is DICCIONARIO ELEMENTAL del ULWA (SUMU MERIDIONAL). 1989. CODIUL/UYUTMUBAL, Karawala, Región Autónoma Atlántico Sur; CIDCA, Centro de Investigaciones y Documentación de la Costa Atlántica; CCS-MIT, Centro de Ciencia Cognitiva, M.I.T.

Ulwa is a member of the Misumalpan cluster of languages, spoken mostly in eastern Nicaragua. It is maybe as far from Miskito as English is from German, or a bit farther.

The dictionary has about 1500 primary items, plus a very useful comparative list of Miskito. Translations into both English and Spanish are given. Short grammatical sketches are included. There is a valuable comparative section at the end which has some lexicostatistics and reconstructions.

We are not qualified to review this dictionary, so we happily invite some Americanist to review it in MOTHER TONGUE. I suspect it will get high grades.

GREENBERG RETORTS. While eager to finish his Eurasiatic book, Joe Greenberg finally decided he needed to take some time to respond to the critics of his LANGUAGE IN THE AMERICAS book or more precisely the scientific assumptions made by his critics. Here is a list of some of his current rebuttals or retorts:

"THE LINGUISTIC EVIDENCE REGARDING THE SETTLEMENT OF THE AMERICAS". This will appear in AMERICAN BEGINNINGS, edited by Frederick West.

"Observations Concerning Ringe's CALCULATING THE FACTOR OF CHANCE IN LANGUAGE COMPARISON.". This should have appeared in PROCEEDINGS OF THE AMERICAN PHILOSOPHICAL SOCIETY, vol.137, no.1, 1993, 79-90. Greenberg was motivated in part by his perception of Ringe's 'gratuitously derogatory remarks about my work'. He aims to show that 'his own approach [Ringe's] to that subject is fallacious.'

"On the Amerind Affiliations of Zuni and Tonkawa" which is a rejoinder to Manaster Ramer's article in CLN 24:1 "Languages in the Americas". We reported some of Alexis' remarks in MT-17. While Alexis was not perceived as a hostile critic, Greenberg was concerned to show that Zuni did belong to Penutian and Tonkawa to Hokan (both branches of Northern Amerind). It is possible that this article has not found a journal yet, since none is listed.

"The Convergence of Eurasiatic and Nostratic" which has been submitted to STUDIES IN LANGUAGE. This is aimed at clarifying the relationship between (in effect) two similar hypotheses, rather than answering critics.

"The Concept of Proof in Genetic Linguistics" which will appear in a Festschrift for Sydney Lamb. This is aimed at what I call 'high theory' in historical linguistics but also at critics.

A 10-page review article of Johanna Nichols LINGUISTIC DIVERSITY IN SPACE AND TIME (Chicago, 1989). Joe perceives Johanna as assuming that the comparative method can only reach back a short way and as attempting to give prehistory a viable alternative but still using language data. He doesn't think she is successful in creating a viable alternative to what she and her colleagues are destroying. (This also may lack a journal.)

TWO SEVERE REVIEWS of Greenberg and Ruhlen have appeared. If what Victor Golla reports in SSILA is any indication, there are lots more where these came from! The first is a pitiless review of Greenberg's LANGUAGE IN THE AMERICAS by Robert L. Rankin (University of Kansas) in IJAL 58: no.3, July 1992, pp.324-353. It is a classical statement from the young priesthood. One sentence from his conclusions says it all. "We must remain open to new suggestions, being careful not to throw out any healthy methodological babies with this Greenbergian bathwater." Of more interest is Rankin's view earlier on that: "But classification is not a 'first step'; it is a final one. One may begin anywhere."

I once tried to point out to the popular press that, while Greenberg had had a fairly easy time of it in Africa, he was going to be savaged in the Americas. ATLANTIC's editors didn't think that was a very important point!"

Equally severe was William Poser's (Stanford University) review in LANGUAGE, vol.69, Number 1 (1993), 220-221, of Merritt Ruhlen's GUIDE TO THE WORLD'S LANGUAGES, VOL.1: CLASSIFICATION. 2ND EDN. Poser's final summary was that: "As a reference on classification this book is comprehensive and useful if one takes into account its strong Greenbergian bias. As a history of classification, it provides useful pointers to the literature, but its analysis and evaluation are unreliable. As a treatment of the methodology of genetic classification it is incomplete, inaccurate, and misleading."

As Paul Benedict might say -- Ruhlen seems to be undermining Western Civilization. Having written a long review article on the same book's 1st edition and having seen the modest changes in the 2nd edition, I cannot recognize the book Poser is talking about. Surely I am biased because I like Greenberg's work. However, Professor Poser might take a peek at his own biases! By the way -- what is with Stanford University? It resembles the Gun Fight at the OK Corral.

USEFUL WORK ON NILO-SAHARAN is being done by M.Lionel Bender. Granted that recognition is hard to gain when one works on poorly known African (or Amazonian or Papuan) languages far off the beaten track, still we should give him a round of applause. Not only should we be very concerned about disappearing languages -- a major point stressed by Kenneth Hale -- but we ought to remember that the final test of global genetic theories will come in great phyla like Nilo-Saharan, Niger-Congo, Indo-Pacific and Australian. The 'we' here is long rangers, of course.

Bender has done a mighty reconstruction of Central Sudanic, a critical sub-phylum of Nilo-Saharan. It looks pretty good. (Apologies! I've 'filed' it somewhere.Time pressure prevents.. .) More recently at the 22nd African Linguistics Conference, Nairobi, July 15-19, 1991 he gave a paper on "Comparative Komuz Grammar". That is a useful summary of morphology in perhaps the most distinctive sub-phylum of Nilo-Saharan. It also serves as a rebuttal to Fleming's argument, lexically based, that Shabo belongs to Komuz. (I actually said it was nearest to Koman, the usual name for what he calls Komuz.). Anyway he is right; Shabo grammar is not much like Koman, or any other for that matter.

Since people forget things, it should be repeated that Chris Ehret also has reconstructed all or parts of Nilo-Saharan. You'll have to write to him (UCLA) because his material is on tapes or has been published somewhere I'm unaware of.

THE ICEMAN OF THE ALPS which was reported to you previously has attracted much attention. There is a big article on this frozen fellow in the NATIONAL GEOGRAPHIC, vol.183,No6, June 1993, 36-67, complete with photos, maps and much archeology. It's great. But they still do not report on the physical findings in terms of DNA or other genetic traits. However, there is a sculpt of his head!

THE INDO-EUROPEAN HOMELAND, the never-ending but still interesting discussion, has a new facet. An expert, Igor Diakonoff, has reviewed an innovator's hypothesis. Colin Renfrew's book ARCHEOLOGY AND LANGUAGE: THE PUZZLE OF INDO-EUROPEAN ORIGINS, 1987, a well known book. But Igor's review is not well-known to most of us. It appeared in ANNUAL OF ARMENIAN LINGUISTICS, vol.9, 1988, pp.79-87; a fairly long review. Although 5 years old now, his conclusions are worth a lot in our discussions:

"Thus I would borrow Renfrew's processual approach to ancient populations (and language) movements, but I should stick to ca. 5-4000 BCE. for the date of PIE, and regard the farmers of Çatal-Hüyük as Pre-Proto-Indo-European speaking; i.e., speaking a language which could be the ancestor both of PIE and other languages. And I would certainly leave alone Eastern Anatolia as a candidate for the Indo-European 'homeland'. Eastern Anatolia -- or, as we in our country usually term it, the Armenian Highland [exclusive of the southern slopes of the Taurus, which belong to the primary agriculture zone], is a land of isolated valleys with, in antiquity, densely forested mountain slopes, unfavorable for very early agriculture, and, at all times, unfavorable for population movements [cf., P.Zimansky's recent book]. There are no signs of ancient IE speakers here, and no horses before the second millennium B.C. Thus, the country [just as neighbouring Iranian Azerbaijan] is unsuited for an Indo-Iranian homeland."

Please note that John Kerns has written a book on this subject and is preparing a new one. He postulates another area.

EDITOR'S NOTE: MT-19 is now too big, especially since we have crammed just about all the old MT-18 material into it. MT-20 will have to carry the burden of archeological and biogenetic reports which have by and large been left out of this issue. One last report for this bloated issue:

NEW FOSSILS IN SPAIN SHED LIGHT ON NEANDERTHALS. Widely reported in newspapers and based on an article in NATURE, April or May, 1993. Juan-Luis Arsuaga and colleagues at the University of Madrid found the jumbled skeletons of 24 human beings in a cave called the Pit of the Bones in Sierra de Atapuerca in northern Spain. The dates are 300,00 and more. Everyone agrees that the population is highly variable but ancestral to the Neanderthals, but what it all means beyond that is controversial. At least it surely implies that Neanderthals have their roots in western Europe, a point Eric de Grolier made some time ago.

THIS IS A LIST OF POSSIBLE SESSIONS. FURTHER IDEAS, OFFERS TO CO-ORDINATE, OF PAPERS ETC. ARE WELCOME FROM ANY QUARTER

WORLD ARCHAEOLOGICAL CONGRESS 3

New Delhi, India, 4-11 December 1994

MAJOR THEME 3: LANGUAGE, ANTHROPOLOGY AND ARCHAEOLOGY

Theme Organisers: S.P. Gupta (India), B.B. Lal (India), P. Bellwood (Australia), R.M. Blench (UK), J.P. Mallory (Northern Ireland), C. Renfrew (UK), and M. Spriggs (Australia)

[Version 3.2, May 1993]

For some sessions potential co-ordinators are listed who have not yet responded to our invitations **(names in bold)**. Better geographical and gender spread are needed--all suggestions welcome.

A.) ARCHAEOLOGICAL CORRELATES OF LINGUISTIC CHANGE: The processes of linguistic change and their archaeological implications. This consists of a series of primarily methodological papers relating to various sociolinguistic processes.
Sub-topics would include-

1. Archaeology/Biology and the Origins of Language. Suggested co-ordinator: **Iain Davidson** (UNE, Australia).

2. Language and the Spread of Agriculture. Examines the theory that the distribution of many of the world's larger language families can be explained by their association with the origins and spread of agriculture from key centres such as the Middle East, China etc. Co-ordinator: Peter Bellwood (ANU, Australia).
3. Language and Prehistoric and Historic Migrations. Examines the archaeological evidence adduced for migrations, along with the perhaps cautionary tales of the archaeological evidence (or lack of it) for historically known migrations which have had a linguistic impact. Suggested co-ordinator: **Kristian Kristiansen** (Copenhagen, Denmark).
4. Language and Society: Variation and Change. Includes topics such as language diversity, trade languages, pidgins and creoles, language levelling, language switch and obsolescence. All of these sociolinguistic processes can be expected to have archaeological implications but have been rarely considered by archaeologists. Co-ordinators: Tom Dutton and Darrell Tryon (ANU, Australia).
5. Dating Language Spread and Change. Examines the somewhat instinctive feel linguists have for how quickly languages change, hopefully to make more explicit their reasoning and the extent to which it is based on now-perhaps discredited methods such as glottochronology. Attempts to calibrate linguistic change to radiocarbon dates will be considered. Co-ordinators: Malcolm Ross and Matthew Spriggs (ANU, Australia).

B.) ARCHAEOLOGICAL, BIOLOGICAL AND LINGUISTIC ENTITIES COMPARED: After the more general methodological papers of Section A, this will get down to detailed case studies while not forgetting or ignoring methodologies involved.
Sub-topics would include-
1. The Question of Macro-Families and Possible Archaeological Correlates. How related are the World's languages and how might this have implications for the spread of modern humans? Co-ordinator: Colin Renfrew (Cambridge, UK).
2 The Genetics of Language Groups. Recent studies in various areas of the world at macro and micro-level are providing fascinating evidence of genetic boundaries in relation to language boundaries, and bringing out new

theories to explain the fit or lack of fit in particular cases. Suggested co-ordinators: **Susan Serjeantson** (ANU, Australia) and **Erika Hagelberg** (Cambridge, UK).

3. Oral Traditions, Myths and Archaeology. Considers traditions and myths of origin in relation to archaeology. Examples include French work in the Pacific attempting to relate voyaging traditions and historical migrations, and Australian research examining Aboriginal stories in relation to movements of groups and languages. Suggested co-ordinators: **Daniel Frimigacci** (CNRS, France) and **Bob Dixon** (ANU, Australia).

4. Proto-Lexicons and Proto-Cultures. How far can linguistics be used to reconstruct vocabularies relating to the "homeland" of particular language families, and to the cultural baggage of the speakers of reconstructed proto-languages? How do we cross from the proto-language to its presumed archaeological manifestation? Suggested co-ordinators: **Robert Blust** (Hawaii), Roger Blench (Cambridge).

5. Toponymy and Other Geographically-Informative Semantic Fields. Toponymy is perhaps an old-fashioned topic in Europe but has not been used enough elsewhere and is worthy of further consideration. Animal names, flora and meteorological terms are also useful in helping place the locations of particular language stages or in showing connections between areas. Suggested co-ordinators: **J-M. Hombert (Lyon II, France)**

C.) THE ARCHAEOLOGY OF LANGUAGE FAMILIES: A series of case studies bringing in the methodological concerns of earlier sessions. This can be seen as a summing up of the major theme. It will also give the opportunity to present more specialist papers relating to particular language groups.

Sub-topics would include-

1. Eurasia. Suggested co-ordinators: **Gina Barnes** (Cambridge, UK), **Aron Dolgopolsky** (Haifa, Israel) and **J.P. Mallory** (Belfast, Northern Ireland).
2. Indian Subcontinent. Suggested co-ordinators: **S.P Gupta and** B.B. Lal (New Delhi, India) and **K. Zvelebil**.
3. Southeast Asia and the Pacific. Co-ordinators: Roger Green (Auckland, New Zealand) and Andrew Pawley (ANU, Australia).
4. Africa. Suggested co-ordinators: Roger Blench and **David Phillipson** (Cambridge, UK) and Kay Williamson (Port Harcourt, Nigeria).
5. The Americas. Suggested co-ordinators: **John Rowe** (UC, Berkeley, USA) and **Joyce Marcus** (Michigan, USA).

[Best to contact Peter Ucko or Colin Renfrew @ Cambridge University (U.K.) in relaxed haste.]

ANNOUNCEMENT:

A World-Systems Electronic Conferencing Network

There is a new transnational and transdisciplinary e-mail network for scholars and researchers who are studying world-systems or other topics relevant to the world-systems perspective.
Its purpose is to facilitate the sharing of information about research, data, publications, announcements, meetings, syllabi, commentary, book reviews, scuttlebutt and etc.

The name of the world-system network is WSN and you can subscribe to it by sending the simple message "sub wsn" to mailserv@csf.colorado.edu

Messages to the network should be sent to wsn@csf.colorado.edu

Connected with WSN is an ftp archive (wsystems) which will be edited by Chris Chase-Dunn and Peter Grimes. This archive will be located at Boulder in csf. The archive is for sharing more permanent announcements, documents, syllabi, data sources, essays, book reviews, and etc.

If you are on internet you can retrieve materials from the wsystems archive by using ftp (file transfer program). ftp to csf.colorado.edu and login as "anonymous." Change directory (cd) into the wsystems subdirectory and then into the relevant subsubdirectory (e.g. "book reviews." Use the Directory (dir) command to list the files. You can then transfer individual files back to your home computer with the get command. The name of each file is on the far right of the directory list. (Unix is upper/lower case sensitive.) For those on bitnet it is also possible to retrieve files from the archive. For more information about using ftp in connection with csf send mail to csfserv@csf.colorado.edu and place the commands "help" and "help ftp" in the message proper.

Materials can be deposited in the wsystems archive by:

1. sending a diskette to Chris Chase-Dunn, Sociology Department, Johns Hopkins University, Baltimore, MD. 21218 USA (chriscd@jhuvm.hcf.jhu.edu) or

2. by ftp (anonymous login) for those who are on internet. ftp to csf.colorado.edu into the "input" subdirectory of the "wsystems" subdirectory. These materials should not be subject to copyright restrictions and should be provided in ascii format.

WSN is part of a larger "nested" mailserv network. The mother network is PSN, the Progressive Sociology Network founded and managed by Martha Gimenez at the University of Colorado, Boulder. PSN is in turn part of csf, a larger grandmother electronic conferencing and archiving operation at Boulder, which is organized by Don Roper.
WSN is a subnest of PSN in the sense that all messages sent to WSN go both to WSN subscribers and to PSN subscribers. This allows PSNers to listen in to world-systems conversations. World-systems researchers are encouraged to subscribe to PSN and participate in that more general conversation. When you sub PSN you get all the PSN messages. If you only want WSN messages, then sub WSN. In order to send to the WSN subnest, address your mail to WSN.

Another subnest of PSN is IPE, a network set up by Lev Gonick for the International Political Economy section of the International Studies Association. Many WSN subscribers will also want to participate in IPE. You may either subscribe to both IPE and WSN or just subscribe to PSN and get everything. As with WSN, in order to reach all IPEers you need to send your message to IPE.
For more information contact Chris Chase-Dunn -- chriscd@jhuvm.hcf.jhu.edu or Peter Grimes -- p34d3611@jhuvm.hcf.jhu.edu

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During about sixty years of nearly isolated existence, the African scholars in Russia have accumulated a considerable scientific potential, which is still virtually unknown to many academics in the West and elsewhere. Not many are those who are acquainted with the Dictionary of the Proto-Afroasiatic language composed by Igor Diakonov's group, or with works or Russian linguists on semantics of noun classes, or with discussions in Leningrad/St.-Petersburg concerning the social organism of kinship.

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The two first issues will be composed mainly of the most important works written by Russian scholars during the last thirty years which are still scientifically important. In the following issues we are going to publish both works written and edited in Russian from the 1950s onwards, and current papers by Russian and foreign scholars. In the section «REVIEWS OF BOOKS» we are planning to provide our readers with critical surveys of all the scholarly books on Africa currently published in Russia, and of some books published abroad. In the section «Dissertations» our readers will find short information on dissertations in African studies defended in Russia or other states of the former USSR.

INFORMATION FOR AUTHORS

The St. Petersburg Journal of African Studies publishes carefully selected papers dealing with various branches of humanities. Manuscripts in English, French or Russian (preferably English) should be sent to:

Valentin Vydrin, African Department, Museum of Anthropology and Ethnography, University Embankment, 3, St. Petersburg, 199 034 Russia (tel.: [812]-218-41-52, FAX: [812]-218-08-11),

or handed to any member of the Editorial Board. To ensure more rapid publication and to eliminate the possibility of typesetting errors, please include, besides a typewritten copy, an electronic copy (floppy disk) in ASCII-codes whenever possible. Footnotes should be kept to a minimum or given at the end of the paper.

Authors will be asked, upon acceptance of an article, to transfer copyright of the article to the publisher. This will ensure the widest possible dissemination of information under copyright laws.

Book reviews and books to be reviewed are welcome. Preference will be given to the reviews containing critical analysis.

CONTENTS OF # 2:

IV. Written source studies

V.Velgus. The medieval Chinese navigation towards Africa and Persian Gulf. Hypotheses and sources.

V. History of African studies in Russia

D.Olderogge. African languages' studies in Russia before 1917.

Reviews

Afrikanskij Etnograficheskij sbornik «Africana» XV. St. Petersburg, 1991.

E.Titov. Grammatika amkharskogo jazyka (The Amharic Grammar). Moscow, 1991.

N.Girenko. Sotsiologija plemeni (Sociology of Tribe). Moscow, 1991.

CONTENTS OF # 2:

I. Linguistics

A.Belova, I.Diakonov, A.Militariov, V.Porkhomovsky, O.Stolbova, A.Chetverukhin. Comparative historical dictionary of Afroasiatic languages: I. p, p (to be continued).

V.Dybo. The prosodic system of the Tubu language.

G.Melnikov, N.Okhotina. Classification of the Bantu morphemes through revealing of the determinant.

A.Koval. Behaviour of the class mark in a language with a bulky noun class system: some particular features.

II. Ethnology, ethnohistory, folklore studies

V.Misiugin, V.Vydrin. Some archaic elements in the Manding epics: the Sunjata case.

O.Tomanovskaja. The state genesis investigations on the African material.

III. Cultural studies

Yu.Poplinsky. Problems of interpretation of non-verbal sources on the history of North Africa.

IV. Museology, written source studies

Z.Pugach. On the destination of the Bari figurines.

V.Platonov. Notes on the Ethiopic manuscripts of the Saltykov-Schedrin State Public Library.

Reviews

S.Chernetsov. Efiopia v XVIII veke (Ethiopia in XVIII century). Moscow, 1991.

V.Iordansky. Zveri, bogi, liudi (Animals, Gods, Humans). Moscow, 1991.

V.Botcharov. Vlast'. Traditsii. Upravlenije: Popytka etnoistoricheskogo analiza politicheskikh kul'tur sovremennykh gosudarstv Tropicheskoj Afriki (Power. Traditions. Government:
An attempt of ethnohistorical analysis of political cultures of modern states in Tropical Africa). Moscow, 1992.

E.Contini-Morava. Discourse Pragmatics and Semantic Categorization. The Case of Negation and Tense-Aspect with Special Reference to Swahili. Berlin - New-York, 1989.

Please send orders to: Valentin Vydrin, Managing Editor

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SYMPOSIUM ON LINGUISTIC AND ETHNO-CULTURAL HISTORY OF SEMITIC PEOPLES

Dear colleague, Moscow, 25 November 1992

We are pleased to announce a Symposium on Linguistic and Ethno-Cultural History of Semitic Peoples from the 4th to 1st Mill. B.C., to be held in Moscow September 20-25, 1993.

The symposium will include a workshop on possibilities for an international project, The Semitic Comparative-Historical and Ethymological Dictionary (SCHED). A tentative table of subjects for papers and a list of topics for discussion at the workshop are enclosed. Papers on related subjects not mentioned in the table are also welcomed.

Scholars wishing to present one or more papers are invited to submit abstracts by June 1. Papers can be presented in English (preferably) and French. Additional material for circulation at the symposium should be sent by August 15.

If you wish to participate please send in reply forms before February 15. We regret to say that our accomodation facilities may be limited, and we may have to make a selection. The results of the selection will be announced before April 1.

Time allotted for each presentation is between 30 and 40 minutes, including discussion, depending on the number of participants.

The symposium will take place at a suburban Moscow hotel.

Terms of Participation

Registration fee for the symposium is US$90 (accompanying guests and students--US$50). The cost of accomodation is US$60 per diem. This amount covers the following services, provided by the organizing agency: visa support; meeting and transportation from and to the airport upon arrival and departure; single (or double) rooms at a suburban hotel; three meals daily, tea and coffee breaks, a banquet; and a cultural program including a sightseeing tour of Moscow by bus and a walking excursion in the Kremlin and the historical center.

Complementary cultural program may include tours to St. Petersburg, the Russian Golden Ring tour (two--three days), a trip to Zagorsk's St. Sergius Monastery. An additional cultural program during the symposium will be available for the accompanying guests upon request.

Directions for payment, a visa support letter and additional information about the symposium will be sent upon the reception of the Registration Form. Since the Russian mail is slow we would prefer to receive your replies by fax.

Feel free to share this information with your collegues!

Registration Form

Name + Title
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title of the paper:
will contribute to the workshop Yes/No
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Alexander Militarev (201 7276 h.)
Yuri Longinov (tel. 336 3709 h.)
The Institute of Oriental Studies of the Russian Academy of Sciences
12 Rozhdestvenka, Moscow, 103753, Russia
Fax: (095) 292 6511 box # 001608

Sincerely yours,
Alexander Militarev,
convener

WORKSHOP: Difficulties and Challenges in Doing a Semitic Comparative and Etymological Dictionary

Topics for discussion:

1. Why has it not been done yet and what are we all afraid of?

2. Consonant correspondences: are all of them established?

3. "Irregular" correspondences and variant roots (ʔibdālu-l-ḥurūf, "Maizelisms"): is it wise to continue ignoring them?

4. How to handle Semitic (and Afrasian) vocalism?

5. Is a step-by-step branch and group reconstruction of Semitic phonemes and lexical units a necessity?

6. Semitic historical morphology: what has been done and how to incorporate it in the dictionary?

7. What criteria are to be applied to telling inherited lexicon from inter-Semitic borrowings?

8. Is it reasonable to include old loan-words from non-Semitic languages (Sumerian, etc.)?

9. Are Afrasian cognates worth including and if so to what extent?

10. How elaborate should a system of references and comments be as regards previous studies on etymologies, the interpretation of meanings, etc.? How to react to other authors' incomplete data and unacceptable conclusions? How to deal in this context with the ethical problem of priority?

11. What are the best ways to computerize the "second labor after Hercules's" (Josephus Scaliger)?

12. Is a call for a real and effective international project doomed to remain the voice in the wilderness and if, hopefully, not--what to do?

-1d

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6th INTERNATIONAL HAMITO-SEMITIC CONGRESS * MOSCOW 1994

Dear Colleague, Moscow, 25 November 1992

We are pleased to announce that we are planning the 6th International Hamito-Semitic Congress in Moscow, Russia, April 24 to 30, 1994 (as suggested by Professor Hermann Jungraithmayr).

Scholars wishing to participate are invited to submit their proposals not later than May 1, 1993. Abstracts of papers should be handed in by November 1, 1993. Time allotted for each presentation is 20 minutes and an additional ten minutes for discussion. Papers may be presented in English (preferably), French and Russian.

The Congress will comprise eight divisions:
1. Hamito-Semitic/Semito-Hamitic/Afro-Asiatic/Afrasian; 2. Berber (including Guanche); 3. Chadic; 4. Cushitic; 5. Egyptian; 6. Omotic; 7. Semitic; 8. Internal and external (Sumerian, Indo-European, Nostratic, etc.) genetic and area connections of HS/AA.

Terms of Participation

The registration fee will be about US$ 100 (accompanying guests and students--US$ 60). The cost of accomodation will be between US$ 80-100 per diem depending on the economic and political situation in the country. This amount will cover the following services provided by the organizing agency: visa support; meeting and transportation from and to the airport upon arrival and departure; single (or double rooms) at a suburban Moscow hotel (where the Congress will take place); three daily meals, tea and coffee breaks, a banquet; and a cultural program including a sightseeing tour of Moscow by bus and a walking excursion in the Kremlin and the historical center.

Complementary cultural program may include tours to St. Petersburg, a three-day bus tour of the old Russian towns; and a trip to Zagorsk's St. Sergius Monastery. An additional cultural program during the congress will be available to the accompanying guests upon requests.

Directions for payment, visa support letters and additional information will be forwarded upon the reception of the Registration Form.

Registration Form

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Yuri Longinov (tel. 336 3709 h.)
The Institute of Oriental Studies of the Russian Academy of Sciences
12 Rozhdestvenka, Moscow, 103753, Russia
Fax: (095) 292 6511 box # 001608

Sincerely yours,
Alexander Militarev
convener

The Congress SH.A.A.L.O.M - Best wishes for the New Year!
Semito
Hamitic
Afro
Asiatic
Afrasian
Languages
Of
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SIMPOSIUM ON LINGUISTIC AND ETHNO-CULTURAL HISTORY OF SEMITIC PEOPLES

Tentative Subjects for Papers & Discussion (S--Semitic; AA--Afrasian/Afro-Asiatic/Semito-Hamitic; P--Proto)

1. Genetic relations & classification	2. Linguistic & historical chronologies	3. Area connections in lexicon	4. Ethno-cultural and language history
1.1. Criteria of genetic classification	2.1. New methods in glotto-chronology	3.1. Borrowings & cognates: problems of distinction	4.1. Principles of locating proto-languages
1.2. Distant relations: S&AA; AA-Nostratic-?	2.2. PAA split: when?	3.2. AA & its dialects/branches: area connections	4.2. PAA home: where?
1.3. Genetic classifications of Semitic	2.3. PS split: when?	3.3. Area connections of PS	4.3. PS home: where?
1.4. Dialects of Akkadian	2.4. Chronology of Sumero-Akk. sources & branching of Akk.	3.4. Early S & Sumerian. S & Elamite	4.4. East Semites & Sumerians: encounter of equipotential cultures?
1.5. Position of Eblaite		3.5. Eblaite & Sumerian: problems of interpretation	4.5. Ebla & Mesopotamia: cultural links
1.6. Amorite-Ugaritic-Canaanite: degree of relationship	2.6. Division of West S (or Central S, by Hetzron) & problems of Biblical chronology	3.6. S & Hittite; S & Hurrian	4.6. Hittites & Canaan
1.7. Subdivision of Canaanite. A problem of dialects in Biblical Hebrew	2.7. Hebrew & Canaanite and the problem of Jews' presence in Egypt	3.7. West (Central) S & Egyptian	4.7. West Semites & problems of identification of the Hyksos
1.8. Subdivision of Aramaic. Position of Modern Aramaic	2.8. Chronology of Aramaic branching	3.8. Sumero-Akkadian loans in Aramaic & other languages	4.8. Chaldeans: problems of identification
1.9. Arabic: South or West (Central) S? "Classical" Arabic and subdivision of Arabic "dialects"	2.9. Chronology of Arabic branching as compared with that of the Arabic expansion	3.9. Classical Arabic: problems of sub-stratum layers. Egyptian loan-words? Aramaic influence	4.9. Locating proto-Arabic speakers' home
1.10. ESA: subdivision and relations with Arabic	2.10. Problems of dating the oldest South Arabian inscriptions	3.10. ESA & Ethio-Semitic: close relationship or secondary influences?	4.10. Pre-historical Arabia: links with the Levant, Mesopotamia and Africa
1.11. Modern South Arabian and Soqotri: relationship with ESA and position within S	2.11. Problems of dating the peopling of Soqotra	3.11. Non-S & Arabic loan-words in modern Arabian	4.11. Ethno-linguistics of isolated South Arabian groups
1.12. Ethiopian Semitic: subbranching. What is South Semitic?	2.12. Semitic colonization of Ethiopia and chronology of Ethiopian branching	3.12. The Cushitic substratum vs. the inherited lexicon	4.12. South Arabian migration wave(s) to Ethiopia
1.13. The Himyaritic problem			
		3.14. Semitic and Libyo-Berber. The origin of the Libyan writing	
			4.15. The Antiquity & Semitic world (mythology, astronomy, scripts, etc.)

The following books are available for review in *Word*. If you wish to review a book, please write to Sheila Embleton, Dept of Languages, Literatures & Linguistics, South 561 Ross Building, York Univ, 4700 Keele Street, North York, Ontario, CANADA M3J 1P3. E-mail embleton@yorkvm1.bitnet or embleton@vm1.yorku.ca.internet. Telephone (416) 736-5387 at York and (416) 851-2660 at home. Books are available on a "first come, first served" basis. Graduate students are welcome to participate under supervision of a faculty member. Reviews are due 6 months after you receive the book. Please send **3** copies of your review, **double**-spaced with at least 2 cm margin on all sides. If possible, please **also** send your review on computer disk, specifying whether you used IBM or MAC, and which software programme you used. It may not be possible to return your disk to you. If your review will be less than one journal page or more than four journal pages, please check with the Review Editor before submitting your review. One journal page is roughly 1.5 double-spaced typed pages.

Books marked with * are appearing on this list for the last time. If you wish to write a review, this is your last opportunity. If there is somebody who would like to receive that book, but not for review, let me know — if nobody requests it, I might be able to send it to you (as a "gift").

Date of this list: January 5, 1993

Aarts, Bas. 1992. *Small Clauses in English: the Nonverbal Types*. Berlin & NY: Mouton de Gruyter. xi + 228 pages.

Abbi, Anvita. 1991. *Reduplication in South Asian languages. An areal, typological and historical study*. New Delhi, etc.: Allied Publishers. xxii + 193 pages.

Akamatsu, Tsutomu. 1992. *Essentials of Functional Phonology*. Foreword by André Martinet. (Série Pédagogique de l'Institut de Linguistique de Louvain, 16.) Louvain-La-Neuve: Peeters. xi + 193 pages.

Anderson, Stephen C. ed. 1991. *Tone in five languages of Cameroon*. Dallas: SIL & Univ of Texas at Arlington. x + 125 pages.

Andvik, Erik E. 1992. *A Pragmatic Analysis of Norwegian Modal Particles*. SIL & Univ of Texas at Arlington. ix + 136 pages.

Auer, Peter & Aldo di Luzio eds. date? *The Contextualization of Language*. Amsterdam & Philadelphia: John Benjamins. vi + 402 pages.

Barwise, Jon, Jean Mark Gawron, Gordon Plotkin & Syun Tutiya eds. 1992. *Situation Theory and its Applications, volume 2*. Stanford: Center for the Study of Language and Information. xiii + 637 pages.

Benzian, Abderrahim. 1992. *Kontrastive Phonetik Deutsch/Französisch/Modernes Hocharabisch/Tlemcen-Arabisch (Algerien)*. Frankfurt etc.: Peter Lang. 256 pages.

Bradley, C. Henry & Barbara E. Hollenbach eds. 1991. *Studies in the Syntax of Mixtecan Languages, volume 3*. Dallas: SIL & Univ of Texas at Arlington. ix + 506 pages. 1992..., *volume 4*. ix + 431 pages.

Brenzinger, Matthias ed. 1992. *Language Death: Factual and Theoretical Explorations with Special Reference to East Africa*. Berlin & NY: Mouton de Gruyter. viii + 445 pages.

Brogyanyi, Bela ed. 1992. *Prehistory, History and Historiography of Language, Speech, and Linguistic Theory, vol. 1*. Amsterdam & Philadelphia: John Benjamins. x + 414 pages.

Brogyanyi, Bela & Reiner Lipp eds. 1992. *Historical Philology: Greek, Latin, and Romance. Papers in honor of Oswald Szemerényi II*. Amsterdam & Philadelphia: John Benjamins. xii + 386 pages.

Bromberger, Sylvain. 1992. *On What We Know We Don't Know: Explanation, theory, linguistics, and how questions shape them*. Univ of Chicago Press: Chicago & Center for Study of Language and Information:Stanford. vii + 231 pages.

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April 16-18, 1993. International Linguistic Association. Theme: History of Linguistics. Marriott East Side Hotel, New York. Abstract deadline: January 15, 1993. Edward Fichtner, Germanic, Slavic & E European Languages, Queens College CUNY, Flushing NY 11367-0904.
May 27-29, 1993. Fifth Annual **UCLA Indo-European Conference**. Theoretical and applied papers on any aspect of IE Studies: linguistics, archaeology, comparative mythology, and culture; interdisciplinary and specific topics (typology, methodology, recontruction, relation of IE to other language groups, interpretation of material culture, etc.). Abstracts by Feb 15/93 to IE Conf Committee, c/o Germanic Languages Dept, 302 Royce Hall, UCLA, Los Angeles, CA 90024-1539. ibcwgkg@mvs.oac.ucla.edu. weekdays (310) 206-4396; evenings (310) 207-4834, (818) 919-3661, (310) 794-3446.
August 15-21, 1993. International Conference on Historical Linguistics. UCLA. Contact Henning Andersen, Slavic Dept, UCLA, Los Angeles 90024, USA.
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Possible job: Possibility of tenure-track position in French linguistics, junior asst professor with recent PhD or ABD (PhD by July 1993). Doctorate in French linguistics, record of publication in field of specialization. Must be able to teach French language and linguistics at BA level and in proposed MA programme focusing on French-Canadian linguistics. Preference given to Canadian citizens or permanent residents, but others should apply too. Contact Prof Moshé Starets, French Dept, Univ of Windsor, Windsor, Ontario, CANADA N9B 3P4. (519) 253-4232 ext 2062.

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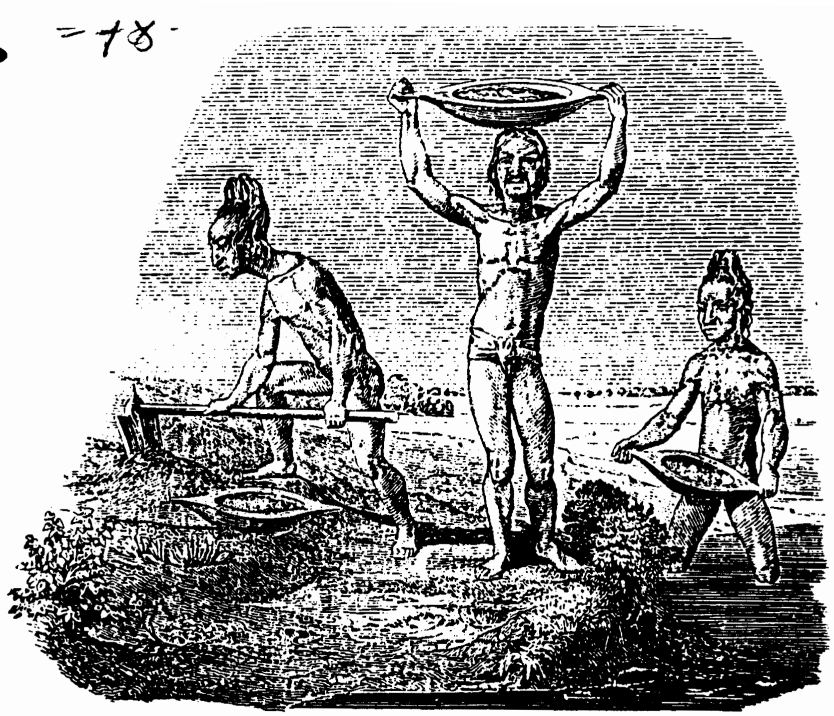
The Tainos

Rise and Decline of the People Who Greeted Columbus

Irving Rouse



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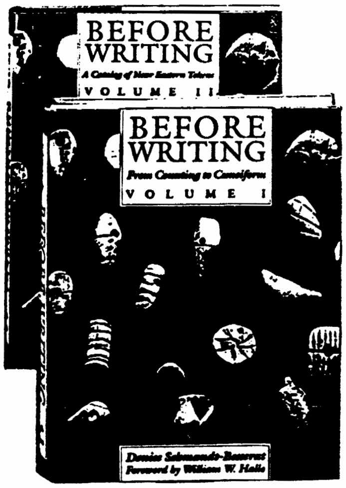

Before Writing

VOLUME I: FROM COUNTING TO CUNEIFORM
VOLUME II: A CATALOG OF NEAR EASTERN TOKENS

By DENISE SCHMANDT-BESSERAT
Foreword by WILLIAM W. HALLO

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Denise Schmandt-Besserat is professor of Middle Eastern Studies at the University of Texas at Austin.

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PRIÈRE D'AFFICHER

LE 25e CONGRÈS DES ALGONQUINISTES

PREMIER APPEL DE COMMUNICATIONS

Le département de linguistique de l'UQAM sera l'hôte du 25e congrès des Algonquinistes à Montréal les 29-31 octobre 1993.

Les organisateurs convient les chercheurs de toutes disciplines à soumettre un résumé **avant le 1er septembre 1993**. Les communications pourront être données en français ou en anglais. Le tarif d'inscription sera de $25 ($20 pour les étudiants).

Veuillez envoyer le titre et un résumé à:

Lynn Drapeau, Congrès des Algonquinistes, Dép. de linguistique, UQAM,
C.P. 8888, succ. A, Montréal, Canada H3C 3P8
Tél: (514) 987-3914; fax: (514) 987-4652; e-mail: R34534@UQAM.bitnet.ca

LETTERS FROM MEMBERS

Long letters which were intended for publication are simply copied. They are gathered together at the end of this section. Other letters have selections taken from them and entered into this section. In a very few cases a line or two of a copied letter is blanked out because I was pretty sure the writer would not want to say whatever publicly. There are some fascinating things going on.

< < < < < ------- > > > > >

KAY WILLIAMSON wrote from Nigeria, asking if we found no women worthy of nomination to the Council of Fellows. Should the journal now be called FATHER TONGUE? What can I say? As a collectivity ASLIP's members fouled up/screwed up. Kay herself should have been nominated. However, before I accept the 'guilt trip', please remember that ASLIP has many women in it. Not one of them nominated a woman, not even Kay herself. Next time I will personally put her name in nomination, not only because of her meritorious work on Niger-Congo but also because she is our first LIFE MEMBER. She will never get a dunning letter!

MYKOLAS PALMAITIS wrote from Kaunas, Lithuania. A most distressing letter! "Today I have received your letter and I am surprised that as if 1) I do not respond 2) that I must pay my dues. How to pay, my friends? To send in a letter? The letter will be robbed already in Kaunas. To send through the bank? I have no currency in our bank and Lithuanian currency is not convertible. And WHAT to send? I do not know how much I must send since I have not paid anything up to day (date -HF). Why? Because my 'wages' at the university equal $15 in a month and with prices nearing to those in the West my family can exist normally with this sum only one week. As a result my small daughters are constantly either without butter or without sugar. Maybe you can help me to come in America and to make money? I can lecture on the history of the Baltic languages, on modern Lithuanian and Latvian or Old Prussian, on the dialects of Lithuanian. I can clean W.C.es (toilets -HF) and wash dead corpses in policy(?-HF). I am ready to work as a personal helper of one of you. My aim is to earn $10,000 or $15,000 in one year and to return home."

"I have responded to all letters which demanded answers. But I do not want to discuss with you since all my ideas have been either ignored or used without any word by your colleagues. Now they are out of date but formerly I wrote tons of letters to Allan Bomhard showing his mistakes and he answered to me since it was useful for him. But he has never cited my ideas. He simply corrected his works. Even my criticism in the IF (?-HF) where I have shown such terrible and primitive his mistakes as comparing Greek χαί (I'm unsure of this -HF) with Lithuanian kaĩ has been left without quotation. And I have long ago written, I negate the schemes of Moscow Nostraticists with the accusative character of the Mother-Tongue and with rich vocalism (what's the meaning?-HF) For you and your colleagues I am only a statist (statistic -HF) in your performance. You may cross me off if you want. Sincerely"

Bomhard has written to Mykolas and sponsored his membership in ASLIP. We do not know how to get money to him, but we have never asked any from him. If our West European colleagues know anyone traveling to Lithuania, perhaps they can get some good solid DMs to Mykolas. Let's everyone give this some thought! Mykolas! Your colleagues around the world wish you well!

ROGER WESCOTT, enjoying his retirement, finds himself perhaps busier than ever. Going to five conferences this June. Also:

"I'm still pushing what I call consonantal apophony & asserting 'unphonemic' cognations, such as kid ~ goat, hound ~ cur ~ wolf, and have ~ give ~ keep ~ off. Also archaic infixation patterns, especially involving sonorants, as in PIE/PIH pe(y)k, fleece; g(y)eu, chew; bhe(w)g, flee; s(w)er, lift; ka(r)p, harvest; bh(r)eg, break; we(l)g, wet; k(l)eu, perceive; ghe(n)d, take; m(n)egh, great; ge(m)bh, teeth. (The lack of a C(m)VC pattern implies that CV(m)C is assimlated from CV(n)C. I had intended the article asserting this to go to the L.O.S.Forum; but it's already getting too long."

Roger is also president of the INTERNATIONAL SOCIETY FOR THE COMPARATIVE STUDY OF CIVILIZATIONS (ISCSC). They are having their meeting June 3-5 at the University of Scranton (Pennsylvania).

ANNA BELOVA wrote from Moscow last October. A short note filled with good will and seasons greetings (she didn't expect the letter to reach me until Christmas time). She adds that "our studies on Semitic and Arabic historical linguistics continue. Soon I hope to send you my critical review (in Russian) of Ehret's work in JAAL. (My review is published in the journal "Voprosy yazhykozhnaniya" 3, 1992). And my paper on Himyaritic."

Attention Chris Ehret. Look up her review. Should be good.

WILFRIED SCHUMACHER wrote two short notes. The one said: "Maybe you have seen my PULLUM review in WORD, December 1992. Do you by the way know of any black American linguist? Maybe the resistance to Amerind, Dene-Caucasian, etc. by U.S. linguists may be a kind of anti-Semitism, eh?" Well, Wilfried, let us redirect this question to Dell Hymes and Carleton Hodge who know a lot more about the history of American linguistics than I do. The only half-clear case that I know of was when the Harvard crowd opposed Boas back in the early part of this century. There were rumblings about Sapir suspecting some of his critics of being anti-Semites. That was after all the time when Hitler had gained power. But I never heard of anything like that with respect to either Swadesh or Greenberg. And about the Muscovites? Forget it!

Wilfried's other note said: "E.P.Hamp was in town (Denmark) speaking about 'Macrocomparison', which he elaborated as Macro-Sao-, Sopho-, Kindumo-, and Anoo-comparison' - meaning comparison of macrofamilies, be it sound, cautious, hazardous, or nonsense-like! . . . Brief - having in mind to attend the Easter Island Conference at the University of Wyoming, Laramie . . 3-6 August.

IGOR DIAKONOFF wrote from St. Petersburg on Columbus Day by our ritual calendar, saying: ". . vide your 'Reports from Russia' - and many thanks for the last Mother Tongue, which was as always, most exciting reading. It is a pity I can no longer actively work on long range comparison -- my team of collaborators working on an Afrasian Historical Comparative Vocabulary has fallen asunder for different private reasons, and my book Proto-Afrasian and Old Akkadian which being prepared for print by Robert Hetzron for the JAAL, is 'in the making' for the last two or three years. [Editor's Note: it once took Frank Livingstone five years to get a book published through my university. By which time it was obsolete in a rapidly developing field.]'

"I have been most touched by Mark Kaiser's report on p.1 and the editor's p.2. The report, however, is rather out of date; instead of 100 rubles a $ costs now 350 rubles. [Editor again. Now in May 1993 one can get 2000+ rubles for $1.]"

"If you allow me some criticism of MT, I would say that you are sometimes not discriminating enough in recruiting collaborators - they range from quite bright boys to graphomaniacs like a certain . . But be it as it may, it is exciting reading. I would like more stress on the fact that there need not be a direct correlation between genetic kinship [Editor reads that as biogenetic] and linguistic kinship -- except perhaps before the Ice Age or something like that."

"I wonder what Sergei Starostin has to say to Seto's list (p.56 sqq). A list of established phonetic correspondences would help; as it is, I am somewhat in doubt. Why does not he use Hurro-Urartian material?"

We could all probably agree that the correlations of genes and language groups are not, and never have been expected to be, of the famous 'one-to-one' type, i.e., 100%. For large phyletic groups that have been resident in fairly large areas for quite some time one gets some impressive correlations. But the details of some of the 'twigs' on the trees may be very uncorrelated. In Africa and Southeast Asia and Melanesia there are many areas where correlations are really lousy. Why? Gene flow especially.

I too wonder what Sergei thinks about a lot of things. But he can't be bothered to write to his 'mother'-- ever.

MERRITT RUHLEN reports from Palo Alto that "Lord Renfrew is here this week (April 1, 1993 -HF) delivering the Tanner lectures on Human Values and ethics. Joe and I met with him two days ago and Renfrew asked Joe his opinion of Dene-Caucasian and "Proto-World" both of which he endorsed. (Editor: which one of them?) Tanner lecture 1 was given last night on 'Archeology, Language, and Genetics: The Origins of Diversity'. In short, his view of the Emerging Synthesis. I sat with Joe, and he said after it was over 'we got in some good plugs tonight'. Bob Sokal and Luca Cavalli-Sforza were also in attendance. Joe left today for Utah to give two lectures at Brigham Young. I continue to work on my popular book for John Wiley, which should appear next Spring if all goes well."

Merritt also enclosed copies/reprints of two pieces which Greenberg recently wrote, essentially answering his critics. These and two others will be mentioned in The News.

M.LIONEL BENDER wrote in late March, partly to answer my letter to members and partly to wish me well. Key point was:

"One suggestion for MT. Let readers know about the brilliant article by Donald Ringe: 'On Calculating the Factor of Chance in language comparison.'Appeared in Transactions of the American

Philosophical Society 82.1: 1-110. It answers once and for all how to settle the 'long-range' problem. I am now applying it to Nilo-Saharan. I have already heard from Merritt who rejects it because it doesn't lead to the results he likes. Some scientific method."

"Yes, a lot of good work is being done in historical linguistics - as well as the 90% which is trash, as usual. But I am afraid I must include much long-ranger work in the latter ('world etymologies', notably)."

OFER BAR-YOSEF wrote from Cambridge, Mass. where he is George G. and Janet G.B. MacCurdy Professor of Prehistoric Archeology, Peabody Museum, Harvard University. This for those who may have thought he was in Israel. Perhaps because he wrote on St. Patrick's Day he said: "This time of year is hectic like hell and I am actually looking forward for the summer digging in Israel, Georgia and Turkey. Our papers on Kebara (last CA) and the one on Kebara and Qafzeh in the April issue of SCIENTIFIC AMERICAN will give you an idea where we are heading now. With better techniques and careful excavations we can learn more than by just looking at the stone artifacts and the fragmentary fossils. So, let's keep the fossils as fund raisers (as they cannot speak to us)."

UNNAMED COLLEAGUE wrote from an African place, not Ethiopia however. I have chosen to conceal his identity and the country so that we might hear his sentiments openly without fear for him.

"Also, I appreciate highly the concern on my safety: 'Are you safe and well', which the Editor of MOTHER TONGUE ISSUE 14; August 1991 inscribed on the envelope, at a time when University academics and faculty were going through the first stages of the struggle for Human Rights, Social Responsibility and Democracy in Whateverland. The evolution of state-intellectuals relationship and its impact on academic freedom: compliance, resistance, defiance, apathy and fatalistic acceptance of the state of repressiveness, overburdened the psyche. 'Are you safe and well', gave me a sense of belonging in the wider academic community and gave the academic environment in Africa, some lessons of behaviour. . . Yours sincerely, Professor Whomever."

VACLAV BLAŽEK wrote from Příbram in February, 1993, answering the critique of his and Claude Boisson's work in MT-17.

"Thanks for your comments informing long rangers about our paper (Claude had devised the name 'Urplough' [?-HF] for him.) But I cannot agree with some of your too categorically critical notes. Our knowledge of Elamitic[1] is too fragmentary to decide its position among Sumerian, Dravidian, I-E, etc. I have found the following remarkable isogloss: Sumerian GUD 'bull, Elamitic KU-TU 'Rinder' (W.Himz & H.Koch, p.159): Dravidian KOT.I 'young

kōṭi

[1] Editor's note. I presumed that they had, or were familiar with, McAlpin's book which presented evidence of Elamitic's ties to Dravidian. We have been discussing Elamo-Dravidian for years in MT -- which proves nothing about the validity of the proposed Elamo-Dravidian hypothesis.

kūt

bull', KUT.' 'cow' (DEDR 2199, 1886) (Unfortunately I had only short occasion to excerpt the newest ELAMISCHES WÖRTERBUCH of Himz and Koch. Berlin: Reimer 1987) Our attempts to explain some etymologies on the basis of certain scenario represent only one possibility, they are not meant definitively. We expect the alternative models the discussion with specialists, esp. with archeologists, historians, etc. Of course, new stronger arguments can cause the change of our opinions."

"I don't know that Alexander Militariev identifies the pre-Sumerian substratum with East Cushitic. I know only his hypothesis that there are some AA (non-Semitic) loanwords in Sumerian which are closest to Cushitic. I formulated a similar hypothesis quite independently yet before my first contact with Alexander in 1984 (p.c.), resp. 1985 (personally) on the basis of my proper comparisons. I think, this independent convergence of our views is a good test for this hypothesis. [D'accord -HF] Now I co-operate with John Bengtson on Dene-Caucasian comparative grammar and lexicon including Sumerian. Concerning Cushitic languages, I have found certain indications of their presence in Arabian peninsula before Semites and perhaps even traces of a direct contact with Indo-European (after the disintegration of AA but before departure of I-E from Near East).[2]

"You are true that Alexander is my friend but it is not a reason why I couldn't disagree with him. He can confirm you that our discussions have been very sharp, lastly in Frankfurt (May 1991) when I convinced him before others in his mistake in interpretation of some Chadic words as cognates to Berber (correctly late loans via Kanuri). Alexander admitted his misinterpretation in public and it is I think scientific."

"In following 4 months (March-June 1993) I will attend language course at Goethe Institute in Bonn. My expected address:

GOETHE INSTITUT BONN
Friedrich-Ebert Strasse 11,
D-5300 Bonn 2 (Bad Godesberg)
Tel. (0228) 35 80 21 - 22 " (End of quoting)

---

[2] If we grant all of Vaclav's unspoken assumptions, his analysis would be more cogent. However, we are required to believe that both I-E and Afrasian had their homelands in the Near East where they could be in contact. However, Diakonoff, Anthony, Gimbutas, Mallory and I think proto-I-E never was in the Near East, but rather in southern Russia. Moreover, a number of Afrasianists and I believe that the proto-AA homeland was in Africa; some say Ethiopia, some say Egypt or Nubia, and so on. Carleton Hodge solves the problem, at least for himself, by believing that p-I-E and p-AA were in contact in the Nile Valley which was homeland for both of them. Therefore my original objection to Vaclav's hypothesis was not because I knew he was wrong -- I don't know any such thing -- but rather because he ignored all those other beliefs. These homelands are usually controversial which most people tend to forget!

In his data we rendered his retroflex sound as [t.] because this computer cannot do his [t] with a dot under it. Sorry!

Prof. Dr. Marge E. Landsberg
1, Shikmona Street
Bat-Galim, Haifa 35014
Israel

January 12, 1992

SPECIAL APPEAL!

Dear Friends,

I wanted to ask you with all my heart whether you would be kindly willing to consider donating a very urgently needed small patient monitor to the Department of Surgery B at the Carmel Hospital here in Haifa. Their need is desperate since they don't even have a one to their name for critical patient care or emergencies...

For more information you are most heartily welcome to call me collect 04-537722 at any hour convenient to you, or fax 972-4-247532.

I am looking forward very much to hearing from you soon. Thank you very much in advance for your gracious consideration of my plea.

Respectfully yours,

Marge E. Landsberg

Marge E. Landsberg, NH,MCC,DLitt.

N.B.: The monitor factory's name is MENNEN MEDICAL LTD. Its address is Kiryat Weizmann, POB 102, Rehovot 76100, Israel. Phone 08-476751; TLX 381335 MMLTD IL; Fax 972-8-474519.

Any amount towards the monitor's purchase is most heartily welcome too, and can be deposited directly by telex - for the Department of Surgery B at the Carmel Hospital in Haifa - into the factory's bank account, number: 488870, Bank HaPoalim, Branch 615, Bank 12, 179 Herzl Street, Rehovot, Israel.

THANK YOU!!!

ORIENTALISCHES INSTITUT
DER UNIVERSITÄT WIEN
UNIVERSITÄTSSTRASSE 7/V, A-1010 WIEN, ÖSTERREICH

26 June, 1992

Dear Colleague Fleming:

This letter ought to have been written as soon as I read in MT of December 1991, pp. 10-12 "Paul Benedict's views". I read these elocutions of our great contemporary with utter disappoint= ment, and this particularly because of the fact that you appar= ently did not realize that all that has been stated, as a princ= iple of his, Benedict's conceptions, runs counter to all funda= mentals adhered to by ASLIP as well as MT. Benedict once more makes efforts to rejuvenate the nihilistic ideas of the great maj= ority of the structuralists of the 1940'es - 1960'es that gradu= ally are dying out with those who so staunchly propagated them, especially in the USA where they had enjoyed a strange but uncon= scious and unintended support by government offices, primordially via the "Intensive Language Courses" under the Departments of the Army, the Navy, and the Air Force. In their great majority, they were Americanists who, apart from a number of (entirely isolated) descriptions of American languages, usually beginning with the phonological structure and mostly also ending with that, did not, as a consequence of their principal training, produce any compar= ative grammar of one of their languages studied, and furthermore no basic work in the lexical composition and etymology of those languages was apparently even attempted. Thus, one easily arrives at "results" as Benedict elaborates them in this article in MT and, unfortunately but naturally, also in his books and other wri= tings. How can he, with his amorphous shoving back and forth lin= guistic entities or pieces of words and morphological elements, ever reach something like a "greatmother tongue"? he might, at the most, arrive at some 150 stepmother pidgin languages produced in the eternal melting pots of Great Maledict...! If you envisage the future of MT or even of all ASLIP processed through Maledit's poi= son foundry, you soon will get new maledicts who, then, will take matters into their hands - and a new era of linguistics will dawn on mankind. With some, the "pidgins" are already "in".....

Well, giving this part of my letter written non sine ira et studio, a more optimistic conclusion, I should like to call to

ORIENTALISCHES INSTITUT
DER UNIVERSITÄT WIEN
UNIVERSITÄTSSTRASSE 7/V, A-1010 WIEN, ÖSTERREICH

YOUR ATTENTION ~~TO~~ SOME publications (not written by Benedict):
Roy A. MILLER: "Japanese and the Other Altaic Languages" (1971).
Karl H. MENGES: "Altajische Studien II: Altajisch und Japanisch", Abhdlg. f. d. Kunde des Morgenlandes41,3: 1975:
Roy Andrew MILLER: "The Sino-Tibetan Hypothesis", in The Bulletin of the Institutue of History and Philology, Academia Sinica, Taipei, 1988:
Nelly NAUMANN and Roy Andrw MILLER in Oriens Extremus 33/2, 1990: Ursprünge der japanischen Kultur. Der Beitrag von Sprache und 'Vokkspoesie' zur Erforschung der Frühgeschichte Japans", pp. 21 - 55:
Roy Andfew MILLER: "Japanese and Austronesian", in: Acta Orientalia, 52, 1991, pp. ~~pr~~. 148 - 168, being a thorough review of P. Be=nedict's recent book "Japanese / Austro-Tai", 1990.e

This latter item brings me to your quotation of a letter of mine to you, also referring to Benedict. I did not say "It (i. e. Japanese. KHM)belongs in or near Altaic", but I said - and repeat - that Japanese is Altaic, in which a still unknown amount of Austro-Asiatic and Ainu elements survive.

Inasmuch as Altaic is concerned, it is with great regret that I time and again see that even in the ASLIP colleagues quote anti-Altaistic theses, passing, of course, as "Altaistics", without see=ing - or taking their time to look more closely at some work - the fallacy of their argumentation. Unfortunately, this fallacy is not rarely all too well camouflaged....

With kindest regards and the best wishes,

Sincerely,

Karl H. Menges

PROFESSOR
DR. KARL H. MENGES
DÖBLINGER HAUPTSTRASSE 64
A-1190 WIEN

 December 11,1992

Dear Hal,

Thank you very much for your kind letter and a small token as you called it.In fact it is really a shame to live under these poor conditions as we are experiencing now.It seems that my generation is the last being involved in academic studies in this country. Yesterday I had a talk with a young fellow who is really bright.But he said that he had to occupy himself with business since he cannot support his family while being a scholar.Of course,it is very sad.

Meanwhile I would like to add some more references to Iren Hegedus's list (MT,april 1992)

Ilya Pejros and Victor Shnirelman. V poiskakh prarodiny dravidov (In search of Dravidian homeland).-Vestnik Drevnej Istorii, 1992,N 1: 135-148

Victor Shnirelman. Etnokul'turnyje zaimstvovanija i linguisticheskije protsessy: nekotoryje metodologicheskije aspekty (Ethnocultural borrowings and linguistic process: some methodological aspects). - Linguisticheskaja rekonstruktsija i drevneishaja istorija Vostoka.Moscow,1989,part 3: 132-134.

Ilya Pejros and Victor Shnirelman. Vozniknovenije risovodstva po dannym mezdistsiplinarnykh issledovanij (The emergence of rice cultivation according to interdisciplinary researches).- Linguisticheskaja rekonstruktsija i drevnejshaja istorija Vostoka. Moscow,1989,part 1: 179-195.

Ilya Pejros and Victor Shnirelman. Proiskhozhdenije risovodstva i problemy mezhdistsiplinarnykh linguoarkheologicheskikh issledovanij (The origins of rice cultivation and problems of the interdisciplinary linguoarchaeological researches).- Stanovlenije regiona: integratsionnyje protsessy v Yugo-Vostochnoj Azii. Moscow,1989: 27-28

Alexander Militarev,Ilya Pejros and Victor Shnirelman. Metodicheskije problemy linguoarkheologicheskikh rekonstruktsij ethogeneza (Methodological problems of the ethnogenetic linguoarchaeological reconstructions).- Sovietskaja Etnografija,1988,N 4: 24-38

Alexander Militarev and Victor Shnirelman. The problem of proto-Afrasian home and culture.Moscow: Nauka,1988.

Alexander Militarev and Victor Shnirelman. K probleme lokalizatsii drevnejshikh afrazijtsev (On the problem of the most ancient proto-Afrasian home).-Linguisticheskaja rekonstruktsija i drevnejshaja istorija Vostoka.Moscow,1984,part 2: 35-53

I look forward to hearing from you.All the best wishes for the Christmas and Happy New Year.

Victor Shnirelman

FAX: 938-06-00 E-mail: SHNIRV@IEA.MSK.SU

-90-

JANUS PANNONIUS TUDOMÁNYEGYETEM
Bölcsészettudományi Kar
Angol Tanszék
Pécs • Ifjúság útja 6. • H-7624
Telefon: (72) 27-622/140, 14-714 • Fax: (72) 15-738, (72) 14714

November 30, 1992

Dear Hal:

I am really grateful to you and ASLIP for sending me *Mother Tongue* despite my long silence. The fact that my life has been a mess for the past 2-3 years is not a good excuse for not writing to you and many other colleagues. But even in this period of turmoil receiving MT was a kind of umbilical cord that kept me within the general circulation of ideas.

Under separate cover I am mailing you a copy of Biblographia Nostratica (the final version that was published in Hungary, I received my copies ca. 2 weeks ago).

Some news:

1. A remarkable book by János MAKKAY (Az indoeurópai népek őstörténete = The Prehistory of Indo-European Peoples) was published in Budapest last year (so this is not hot news but it may not be known to those outside Hungary). This is the Hungarian contribution to the IE homeland controversy and I think this book may be the right approach to the problem. I would like to write a review of it in English, I will send it to you as soon as it is finished. A 50-80 pages summary of the book is forthcoming in JIES (personal communication from the author).

2. Another Hungarian publication in historical linguistics:
Sándor ROT: Old English. Budapest: Macmillan, 1991. (pp.608). Includes a survey of IE and Germanic languages and their diachronic development and the theoretical part is followed by control questions and tasks for analyzing Old English texts (supported by a glossary). It has all the features of a good handbook for experts and those of a good university textbook for students (both at the undergraduate and graduate level).

3. I got a big suprise from Australia! The first issue of a new linguistic periodical called ***Dhumbadji!*** launched by the newly founded Melbourne Association for the History of Language (MAHL) was sent to me accompanied by a letter from Paul Sidwell, the secretary of MAHL and editor of the journal, inviting to become a member and contribute to the publication. I think I owe the honour of being known in Australia to Vaclav Blazek, because he is mentioned in the editorial and is introduced in the journal + his review of the Old Church Slavonic Etymological Dictionary (Prague:

Academia, 1989-1990) is published on pp.15-30. Anyway, I think this is something ASLIPers should know about (if they do not know it already!?). If you have not heard about it yet, I could send you a detailed description of the first issue (or a xerox copy if you prefer). Or you can contact them the following ways:

Melbourne Association for the History of Language.
Dept. of Germanic Studies and Russian

Babel Building
University of Melbourne
Parville, AUSTRALIA, 3052

E-mail: John Bowden@muwayf.unimelb.edu.au

That reminds me of my own availability on E-mail, so please note my number: ihegedus@btk.jpte.hu

Will you, please, let me know if you are available in the international network, that would definitely ease communication. And you are free to publish my E-mail number, I would like to have my colleagues' codes as well.

I hope that when you are reading this letter you are again in excellent health. I was worried by the news of you undergoing an operation and I hope it has worked out all fine. Remember that silence from ASLIPers does not always mean indifference to you or to "cosa nostra"! I am actually amazed by the amount of work you are able to do for the publication of MT and we all owe you and the temporary editors a lot for doing that. I would definitely feel sort of cut-off and professionally lonesome without getting MT. So don't give up!

I will try to be more communicative in the future (and I hate breaking promises!). And as soon as my salary as assistant professor becomes higher than the minimum living standard in Hungary I will start paying my subscription (this is not sarcasm but another promise! - although, to tell the truth, my mood becomes more and more bitterly sarcastic when it comes to education and research financing, you know that is the easiest and most innocent prays for those ~~[illegible]~~ bad guys.
pray

Once again thank you for everything. I wish you the best of health and spirits for the future and send you my sincerest good wishes for Christmas and the New Year.

Best regards from,

Irén H.

LINGUISTICS RESEARCH CENTER
THE UNIVERSITY OF TEXAS AT AUSTIN

P. O. Box 7247 · Austin, Texas 78713-7247 · (512) 471-4566
email: "LRC @ utxvms.cc.utexas.edu"

19 May 1993

Dear Hal,

I was sorry to hear you had been ill. Please take good care of yourself; we need you.

I assume that your pessimism about the contributions of historical linguistics are the result of low morale caused by whatever hit you. In my view, they have been tremendous. When I was a graduate student at the end of the 30's, Ph.D. 1941, I hoped to be able to fill in something of the second millennium B.C. for Indo-European studies. As you may know, Kurylowicz demonstrated the laryngeals in 1927, but ideas like that take some time to be accepted. Then in 1935 he and Benveniste both published important monographs, giving strong support for their assumption. Still, many didn't accept them. I was lucky to be able to buy Benveniste's monograph from London; and when Sapir died I got his review copy--his library was sold, or much of it. (I also got his copy of Whitney's Sanskrit Grammar.) Since especially Kurylowicz's is very difficult, I spent one summer virtually on both. Also pertinent, Sturtevant put out his Hittite grammar, chrestomathy, and lexicon in the thirties. And Lane did something with Tocharian. As a possible last bit of pleasure before being swallowed up in the army--and possibly deeper--I attended the Linguistic Institute at Chapel Hill in 1942, where both Sturtevant and Lane taught, and possibly more important, Goetze, who taught me Sumerian. The rest need hardly be mentioned. By now virtually everyone accepts laryngeals. We not only have a fair bit of Hittite, but also other Anatolian languages. When I got out of the army in 46 I tried to relearn stuff; during the war I was in Japanese. And in 1952 I published Proto-Indo-European Phonology. That came to be the University of Texas Press book that was sold most widely behind the iron curtain.

To go on with important stuff, Ventris determined in 1957 that the Linear B materials were in Greek. So we had another Indo-European language with documentation from the 2nd millennium.

What with one thing and another, we can now reconstruct Proto-Indo-European of the 4th millennium and earlier. Moreover, the archeologists have developed adequate techniques that their findings can be correlated with our linguistic reconstructions. Cf. the stuff by Anthony on the horse. While there are various theories on the home of the Indo-Europeans, I think that Schrader of the 19th century was right, as was Leibniz before him, that it was in southern Russia.

There's a lot we don't know, but we can ask interesting questions. If you have a few minutes, you might pick up the copy of my Theoretical Bases of Indo-European Linguistics in your library; published this March by Routledge it's hideously expensive. There are also some things in the third edition of my Historical LInguistics, also Routledge.

To close this, we have good information, if restricted, on the three millennia before 1000 B.C., where we were pretty well stymied in the period around 1940.

With best wishes,

Sincerely yours,

W P Lehmann

SUOMALAIS-UGRILAINEN SEURA
SOCIÉTÉ FINNO-OUGRIENNE
[redacted]

Helsinki, July 29. 1992

Prof. Dr. Harold Fleming
Association for the Study of Language in Prehistory
5240 Forbes Ave., Pgh, PA 15217, USA

Prof. Dr. Ekkehard Wolff
Universität Hamburg, Seminar für Afrikanische Sprachen und Kulturen
Rothenbaumchaussé 67/69, D 2000 Hamburg 13, Germany

Dear Messrs.,

Having just received your letter, I hurry to send this statement of my intention not to quit you.

It just happens that last year, when the fees were due, I sent more money than was necessary at that time, and Hal wrote me that I would not have to pay the fee for 1992. I have a written document for that.

I am nevertheless enclosing 5 $ (to Hal), and will send more, if you ask. I find the Mother Tongue interesting reading, and I am not against paying for it, although it would be a good idea to make it even more like a real periodical - without dismissing the casual tone, which I like.

Since I am basically an anti-long-ranger, and many of the members are pro-long-rangers, it might also be worth while thinking, whether the profile of the ASLaP could be made more neutral with regard to long-range comparisons.

What is essential in your/our work, is, in my opinion, that all of us are operating with the linguistic diversity in this world. We all agree that this diversity is a clue to the (pre)history of mankind, so I do not really consider it so important where we draw the limit for genetic comparisons. I just hope that everybody will put facts before fantasy.

Now that at least one half of the world's languages are rapidly disappearing, I think it is also important to do something to save the diversity. This should be one of the topics and goals of ASLaP and Mothertongue.

As to my own activities, I have been further working on Khamnigan Mongol and Khamnigan Evenki. I just finished a paper on the genetic position of Khamnigan Mongol, coming to the conclusion that it is a separate (and exceptionally conservative) Mongolic language. (The paper will appear in October, and I will send a copy for the ASLaP library.)

I hope to find some time to prepare a report for Mothertongue about the basic lexicon of Khamnigan Mongol (and Khamnigan Evenki). The problem is that the Mongolic languages are all so closely related with each other, so that you cannot expect to find anything really amazing. The special position of Khamnigan Mongol is more transparent when you look at the phonological innovations (and their absence, as compared with other Mongolic languages).

Please note my new permanent home address:

Juha Janhunen
Lilla Robertsgatan 4-6 K 55
00130 Helsingfors 13
Finland

sincerely yours,

....

743 Madison Street NE
Minneapolis, MN 55413
April 1, 1993

Dear Hal,

This is specifically in response to your letter to ASLIP members (March 10, 1993).

It will be no surprise that I agree with you that much of today's historical linguistics is "dull and lifeless." You have used the metaphor of "little fiefdoms," where the various lords tend to their Indo-European, or Iroquoian, or Munda, weeding out all the irregular correspondences and loanwords, and making sure that their turf remains pure and untouched by other fiefdoms. If anyone attempts to show that there may be relationships between certain fiefdoms, they protest that some of their vassals and serfs (read 'words' and 'affixes') have been treated carelessly by the generalist in question. This is enough, for many of them, to dismiss the entire hypothesis!

For example, there is a prominent Indo-Europeanist who proposed, on the basis of the etyma of English **hound** and Russian **pës**, a proto-Indo-European ***peḱuon-** 'dog'. It is all very ingenious, and strictly from an internal IE standpoint, there seems to be no reason to doubt this solution. However, Nostraticists and other paleolinguists have proposed a different possibility. By looking outside of Indo-European, they find forms such as Uralic ***küjnä** 'wolf', or, farther afield, in Amerind: (Hokan) Tonkawa **?ekuan**, Yurimangui **kwan**, (Tanoan) Taos **kwiane-**, (Oto-Manguean) Popoloca **kuniya**, (Jivaroan) Esmeralda **kine**, etc. (all 'dog'). It would then appear that IE ***kwon-** is archaic residue from an old word that first referred to the wolf (Canis lupis) and later to the domesticated Canis familiaris. The presence of words of the form **/bitsu/** 'dog' around the world (Ruhlen, "Global Etymologies," 1987) leads one to suspect that Slavic ***pĭsŭ** may be residue of that etymon, and unconnected with IE ***kwon-**.

The Indo-Europeanist in question, of course, dismisses the Nostratic solution, as proposed by Illich-Svitych, because "they're playing fast and loose with the semantic content." To him, an exact correspondence of meaning is required, even if most people would readily accept the association of 'dog' and 'wolf'.

By trying to be 'safe', such linguists ignore a dictum that should become one of the basic theorems of prehistoric linguistics: "External comparison is the only way to tell which internal reconstruction is correct." (This was phrased by Aharon Dolgopolsky at the International Symposium on Language and Prehistory, 1988.)

As a corollary to this theorem, another of our founding fathers has this to say:

> . . . [S]ome linguists have wanted to work as if each level of relationship had to be fully reconstructed before a deeper level of relationship could be broached . . . I believe this approach to be demonstrably wrong. Certainly it was not the way of working of Sapir and Swadesh who moved back and forth between the immediate and remote levels of prehistory, finding the two mutually illuminating. (Dell Hymes, in Morris Swadesh's book The Origin and Diversification of Language, 1972, p. 265.)

This can be an antidote to the dull, lifeless, and stultifying historical linguistics which still has a chokehold on the full flowering of our science.

It is high time to re-start prehistoric linguistics (of which Swadesh was the first and only professor). He may not have been right in every detail, but that is not important. "There was just too much evidence that the paths he blazed did go somewhere, and that one would eventually have to follow them out." (Hymes, op. cit., 265-66) Let us go back to his works, and those of the other founding fathers and mothers. We can formulate more theorems, not as unalterable dogma, but as guides to "drawing conclusions from the totality of evidence," as Swadesh advocated.

It is good to have you back on the job!

Best wishes,

John D. Bengtson John

The College Of
WILLIAM&MARY

Department of Modern Languages and Literatures

Washington Hall
P.O. Box 8795
Williamsburg, Virginia 23187-8795

APR 3 1993

March 29, 1993

Hal Fleming
Editor - *Mother Tongue*
c/o A. W. Beaman
ASLIP Secretary
P.O. Box 583
Brookline, MA 02146

Dear Hal,

I am writing this letter to you directly because it was you (I believe) who posed the questions on the back of Letter to Members (No. 2; 3/10/93) and that you are still the editor referred to on the first page. I'm glad to hear of your successful bout with illness; since I've been hospitalized three times during the past six months (twice for very serious matters) I can sympathize more directly.

Please pass my check on to Ms Beaman.

With regard to your question on page 2: I would like to bring your attention to my recent book ***The Metaphorical Basis of Language: A Study in Cross-Cultural Linguistics*** (Edwin Mellen Press, Lewiston, NY). This book is, I believe, precisely of the type you are calling for:historical linguistic research which is not just of the standard (tiddlypush) variety. Indeed, I am attempting what you call "prehistoric linguistics" by seeking new consonant transformations "Beyond Grimm" (as my last chapter is entitled). I draw heavily on the work of Greenberg (he has read my book and called it "very stimulating") and from the past few years of M-T. I hope you get an opportunity to read it and will feel free to contact me if you have any comments.

Sincerely,

Morgan

(Prof.) E. Morgan Kelley

1106 6th St.
Las Vegas, New Mexico 87701
(505) 454-1902
February 6, 1992

Dr. Harold C. Fleming
5240 Forbes Ave.
Pittsburgh, PA 15217

Dear Hal:

Thanks for your letter of Jan. 31. I do have an adjunct appointment at NM Highlands U., though I don't teach much. Am directing one M.A. thesis right now. I also am a Senior Research Associate at the Laboratory of Anthropology, Museum of New Mexico and spend a fair amount of time in Santa Fe at the Lab Anth.

At the moment I'm working on a book on the upper (Pueblo) Rio Grande from Paleoindian times through the early Spanish period and so have been concerned with the interpretation of the early man data. I wouldn't call myself a total skeptic. For example, I am probably the first southwestern expert to push for the Greeenberg's new Amerind formulations (in a book entitled *The Frontier People,* U. New Mexico Press, 1987). But, to date, a <u>major</u> peopling of the Americas at the time of Clovis seems the best idea to me. Provisionally accepting the Greenberg point of view doesn't seem to me to necessitate an earlier occupation -- that is, I think 12,000 years might well be enough time for the linguistic diversity we find in the Americas.

However, I grant that the mtDNA evidence <u>seems</u> to point~~s~~ to an earlier occupation. If so it was likely a very scanty one and the Clovis population explosion[??] might be tied to a series of technological improvements (spear thrower with superior dart points, new hunting strategies -- whatever) that took place <u>in the Americas.</u> In this case Dillehay, MacNeish, etc. may be right (más o menos).

Hope you are able to drop by Las Vegas. It might be wise to give a call first, just to be sure I'm home. For example, this weekend I am in Las Cruces giving a lecture.

Cheers,

Cal

Carroll L. Riley

A VERY BRIEF EDITORIAL

As we have argued before several times American linguists as a tribe in the 2nd half of the 20th century may be characterized in cultural terms as having methodology worship. Admittedly much of that changed in the final quarter of the century as the new faith of mentalism spread among the young. But the special clan of historical linguists resisted transformation, clinging to their Bloomfieldian icons. Their worship of methodology was said to have been due to acculturation, too much contact with one kind of physicist, which encouraged them to borrow operationism or the primary philosophy behind methodology worship. So it was.

Later on the Americanist branch of this tribe sought to comprehend the maverick priest who had achieved great success in Africa and tried to succeed in the New World. They knew that this deviationist taxonomy-venerater might corrupt the young in the New World, so they mounted a great campaign to discredit him.

But they never were able to explain his African success. Nowhere in their methodological dogma was there a recipe for reflection, thinking, judgment or what we often call intuition. Any idiot could look at a bunch of similarities and find falsehoods. This deviant looked at the same things and in a disturbing number of cases he found taxonomic truths. Whose manual was he following anyway? Who taught him to do this?

The mother church for methodology worship in linguistics was or has been Yale. My Yalee friend Paul Black once told me that I did historical linguistics intuitively, not by proper methods. True enough. Still this caused me to wonder about Joseph Greenberg. True, he could explicate his methodology but somehow it made a difference whether he used his methods or someone else did. Why should that be? Who knows? But Greenberg has become famous or at least noted by his Africanist critics for his uncanny knack for selecting the good similarities and throwing out the bad. I calls dat intuition, savvy or just plain good luck.

But it is a factor. It makes a difference. And you cannot find it in the operating manuals of Americanist historical linguists. Can you?

Perhaps it is time for Greenberg's critics to begin looking at his taxonomies, at his classifications, to see if they are true or not, instead of wasting their time examining his rituals.

BRIEF EDITORIAL

The way things go in science it is a rare event for a discipline to admit that it has grown nearly pointless, having lost most of its élan vital, and has substituted mindless methodological ritual for serious investigation of hypotheses. Or simply to admit that it has been mistaken. Once as a graduate student I remember being thrilled at the courage and honesty of the school of anthropology called Kulturkreislehre, a Viennese and German Catholic discipline primarily, when it announced that it had been mistaken! My professor at the time (G.P.Murdock) told his classes about it, speaking with deep admiration for the plucky Austrians. We never heard of any other disciplines doing this, even the many continental European (really national) schools of anthropology whose scientific roots clearly were in German or Austrian versions of Kulturkreislehre.

I once asked an astronomer in Boston how his colleagues dared to invent the ultimate hypothesis of all prehistory -- the Big Bang theory -- when there was no good datable fossil evidence for an event supposed to have happened 15 to 20 billion years ago. After all such a theory would terrify linguists who were not used to proposing anything more than a few thousand years old and only then if the proofs were nearly mathematical in their certitude. So how do astronomers dare? "We have balls," answered my colleague. "One has to have balls to be an astronomer." "Oh, do you mean crystal balls?", I queried, playing the fool as usual. "Crystal balls might help us a little bit but you know perfectly well what kind of 'balls' I mean," said he. Even female star gazers have balls because the expression really means 'boldness' and 'courage'.

Well, said I to a friend in physics, is it true? But where do they get their arguments from? "Physics!", he replied, "they get their arguments from physics. Astronomy is really part of physics anyway." So astronomers can derive many useful things from the powerful and complicated theories of physics. This must give them courage! And then their data and discoveries bounce back and affect the theories of physics.

It seems that physics -- the very model of a mature and rich science -- has a substantial investment in diachrony which pays it dividends. Unlike so many social scientists who seem to have concluded that the 'physics model' demands synchrony or a-chrony physicists consort with astronomers. Indeed every strong and distinctive natural science, especially geology and biology, has an important diachronic aspect, where substantial research is carried on and, yes, scholars have balls. Try to imagine biology without Darwin or systematics. Try to imagine modern geology without Wegner's continental drift or the history of the earth. (Oops, I made a mistake. Methinks chemistry lacks diachrony.)

Scientific testosterone is rather limited in contemporary historical linguistics. So determined are his opponents to crush Greenberg that they seem to forget what it all means. (What did he do to deserve such treatment?) When they have finished 'proving' that historical linguistics cannot deal with prehistory seriously, that all taxa outside of obvious ones cannot be reliable, that only the most copious and meticulously gathered data can be used, and that only orthodox Indo-European procedures can be followed, whatever is left to be interested in? Theoretical synchronic linguistics! What else?

Do we need to invent a new discipline? After all, those people are killing historical linguistics or rather freezing it in its present pitiful state. So frightened is this young priesthood that they cannot allow a fair and open discussion of the issues. So they choke off long rangers in the journals. So dishonest have some of them become that they deny that they stifle debate.

Four members responded to my criticism of contemporary historical linguistics in the March letter. M.L.Bender thought that Donald Ringe of Pennsylvania had advanced the field greatly by his mathematics which show conclusively that Indo-European is the limit. Bender thought he would apply Ringe's wisdom to Nilo-Saharan. Oh, lord, there goes Nilo-Saharan too! Morgan Kelly (William & Mary) mentioned his own work to show that there is some action in historical linguistics (see THE NEWS). Winfred Lehmann pointed to the growth in Indo-European as a good sign. John Bengtson found much to criticize in the status quo.

Personally, I would love to see the present rigor mortis or rigor rationis (hardening of the attitudes) loosen up so we can regain some of the vigor (not rigor) of 19th century diachronic linguistics. No one wants to be bothered with setting up a new discipline. Besides some of us do not even have a university to base the discipline in. But there must be some alternative to nincompoop-istics!

If you do not believe that there is any cause for alarm, or any reason for one to bestir herself to resist, then consider the following falsehoods which are being used to throttle long range hypotheses. They abound in the Network (computer) where techno-poops are most numerous. (Thanks to Grover Hudson and Merritt Ruhlen for sharing this information.) I will state them just as fairly as I can. No authors are cited because this 'paradigm' has multiple origins and it is widely bruited about -- quite thoughtlessly -- among American linguists. Mind you, not a single one of the following statements is true. Not one! They can easily be shown to be falsehoods. You can do it yourself in the quiet of your own study. Forget your graduate training. Think! Regardez!

1) It is necessary to have a complete grammar and a lexicon of at least 2000 words of a language in order to classify it.

2) One cannot classify a language on the basis of short word lists or poor field data. (This is true if the language has no reasonably close relatives -- like Basque, Burushaski or Nahali.)

3) Two or more languages cannot be classified as related unless 'exact' sound correspondences can be established between or among them. This is surely Indo-baloney's prime principle.

4) 'Mere lexical similarities' cannot serve as a basis for classifying two or more languages as related.

5) If a proposed class of languages has sub-divisions, then the ancestor of each sub-division must be reconstructed before the common ancestor of the whole class can be reconstructed.

6) The same as (5) except that each sub-taxon must be reconstructed before the whole taxon can be accepted as such.

7) "You can throw mud at a barn and some of it will stick." (He probably meant cow dung.) This means that you can always find similarities between two or more languages just by accident due to spurious similarities. So seeking similarities is silly. (Contrast this oxymoronically with the next one.)

8) Two daughter languages of the same ancestor will lose more and more of their common features as time goes by. So they become less and less similar. Until finally the evidence of their common origin disappears. So long range comparison is fruitless.

9) Professors Bender, Oswalt, and Ringe have shown by exact and rigorous mathematics that the evidence of relationship of two languages becomes statistically meaningless after several millennia or roughly the same time depth as Indo-European. Some would make this somewhat older, perhaps even as old as 10 kya, but that is an unprincipled extension of this axiom.

10) Ergo, relationships older than Indo-European ones cannot be detected or at least cannot be demonstrated statistically. (Some say that Indo-Hittite is older than regular Indo-European)

11) Double ergo. Indo-European obviously cannot be related to any other family of languages. Nor can Semitic! Not ever!

12) A taxonomic hypothesis can be falsified simply by questioning its attendant methodology. If you say that Irish is related to Welsh but you did not inquire in a proper way, then Irish is not related to Welsh. You can undo history! It's fun!

12a) The above was akin to the famous fallacy of arguing ad hominem. Suppose that a drunken fool proposes that the earth is round. "If it was flat, I'd be able to stand up straight." Well, his hypothesis cannot be true because he is a drunken fool. Therefore the earth is flat.

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ASLIP BUSINESS

MT-19 is later than we expected it to be because the computer decided to break down at the crucial moment. The appropriate response is probably to shoot it. Ah, technology! An unrelenting batch of difficulties.

The editor wishes to thank the many friends and colleagues who wished him well! It is like having your limbic system stroked with velvet feathers. Thank you, thank you!

While the editorship will change its personnel for a spell, this has nothing to do with anyone's health. Rather it involves a need to publish on Omotic languages. Allan Bomhard will take primary responsibility for producing MOTHER TONGUE but the present editor will contribute variously to future issues, more on physical anthropology and archeology than other topics. Allan has started his own publishing business (SIGNUM, P.O.Box 6398, Boston, MA 02114, USA. Tel. 617-227-4923) and he promises to make many changes in our dull & feckless format. From what he has shown us so far you all will be very pleased.

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ELECTION OF FELLOWS: The Results:

Ms. Anne W. Beaman, Secretary of ASLIP, informs us that the final results* of the election of new permanent Fellows to the Council of Fellows are, as follows, subject to their accepting the honor and status:

Luca Luigi Cavalli-Sforza (Stanford University, biogenetics)

Igor Diakonoff (St. Petersburg, Oriental Studies)

Ben Ohiomamhe Elugbe (U. of Ibadan, linguistics)

Dell Hymes (U. of Virginia, anthropology & linguistics)

Sydney Lamb (Rice University, linguistics)

Karl-Heinz Menges (U. of Vienna, Columbia University; Central Asia Studies & Altaic languages)

Colin Renfrew (Cambridge University, archeology)

* Exact numbers and rules can be known. Write/phone Ms. Beaman.

The Annual Meeting of ASLIP and the Board of Directors of ASLIP was held in Boston, Massachusetts on the 21st of April at the African Studies Center of Boston University. Copies of the Agenda of the Annual Meeting may be obtained from Ms. Anne Beaman.