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**NEWSLETTER OF THE ASSOCIATION  
FOR THE STUDY OF LANGUAGE  
IN PREHISTORY**

**Issue 31**

**(MT - 31)**

**Fall 1998**



## **Mother Tongue : Newsletter of the Association for the Study of Language in Prehistory. Issue 31. Fall 1998**

The Association for the Study of Language in Prehistory (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 a journal to report these activities.

**Membership:** Annual dues for ASLIP membership and subscription to *Mother Tongue* are U.S. \$25 in all countries, except those with currency problems.

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(continued on inside back cover)



## Introduction to *Mother Tongue* 31 (MT 31) The Newsletter

Edited by John D. Bengtson and Roger W. Wescott

Assistant Editors: Alvah Hicks, Randy Foote

This is the first *Mother Tongue* Newsletter not edited and/or written by founding Father Harold C. ("Hal") Fleming. Hal was elected to the Board of Directors at the last ASLIP meeting, but has otherwise retired from active ASLIP work in order to pursue other activities, such as African linguistic anthropology and Indo-European studies. Alvah ("Pardner") Hicks and G.R. ("Randy") Foote have stepped forward to edit the archaeological, language evolution, and biogenetic news that Hal has sifted and distilled for the past dozen years.

We begin with a sad note: the passing of senior Long Ranger and ASLIP Council Fellow Carleton T. Hodge, and continue with Archaeological and Biogenetic News, Language Evolution News, and Paleolinguistic News.

### Obituary: Carleton T. Hodge

[An eminent presence has left us: ASLIP Council Fellow **Carleton T. Hodge**. This tribute was written by his friend **Saul Levin** for the LACUS (Linguistic Association of Canada and the United States) Newsletter, Fall 1998. It is reproduced here by permission of Professor Levin and LACUS. Ed.]

The privilege of composing a few words of tribute to Carleton Hodge, our lamented colleague and past [LACUS] president, has been entrusted to me. For many years he and I have been friends, and my admiration for him has grown as I learn more and more from his vast knowledge of languages, stretching over much of Europe, Africa, and Asia. Early in his career he wrote two excellent books, on *Spoken Serbo-Croatian* and *An Outline of Hausa Grammar*. He went on to publish many, many books and articles of permanent value; and he is recognized all over the world as an outstanding authority on the Afro-Asiatic family of languages and - beyond that - its connections with other phyla which he subsumes under the term *Lislakh* (roughly equivalent to Nostratic). Probably his greatest contribution, since he retired from a professorship at Indiana University, is the pioneering research into consonantal *Ablaut*; for he shows how this is hardly less patterned and significant for comparative grammar than the vowel *Ablaut* that the Indo-Europeanists and the Semitists of the nineteenth century worked out in precise detail. Carleton's accomplishments, as well as his personality, will be warmly remembered by all who have known him and drawn upon his unique mastery of a major field of linguistic science.

### ASLIP Business (1): E-Mail Survey

In order to communicate more quickly and efficiently with ASLIP members, we are requesting that ASLIP members who have an e-mail address inform us of that address. This may be done either of two ways: (a) with address public (with your permission to publish it in the Newsletter), or (b) with address restricted (used only for ASLIP business, and not published). So please e-mail us right away, designating category (a) or (b), and help us save time, money, and trees. Send the message to:

ASLIP Secretary, Alice Faber <faber@haskins.yale.edu>, and/or

ASLIP President, John D. Bengtson <john.bengtson@co.hennepin.mn.us>

In the future we plan to send the Newsletter via e-mail as well.

# Mother Tongue Newsletter No. 31

## Fall 1998

### Archaeological and Biogenetic News:

SELECTED QUOTES AND REFERENCES compiled by Alvah Hicks (1997-1998)

[NOTE: Treatment of these quotations should be made with caution. A full appreciation of these "selections" should be drawn from the original sources. MT editorial comments are preceded by "CC - AMH (and/or) JDB." Ed.]

### CROSSROADS TO AND FROM THE AMERICAS

Crowell, Aron L., and Mann, Daniel H. **Sea Level Dynamics, Glaciers, and Archaeology Along the Central Gulf of Alaska Coast.** *Arctic Anthropology* Vol. 33, No. 2, pp. 16-37, 1996.

The Gulf of Alaska coastline, located at the contact between the Pacific and North American tectonic plates, is a geologically unstable zone affected by frequent earthquakes, crustal movements (tectonism), and volcanic eruptions (Fig. 1). Glacial advances and retreats from ice fields in the coastal mountain ranges contribute to the physical dynamism of the region. The Gulf also is biologically and culturally rich, with resources of fish, sea mammals, shellfish, and birds that have supported maritime-adapted human populations since the beginning of the Holocene. p. 16

A rise in relative sea level causes the submergence or erosion of shoreline habitation locales, whereas a drop in relative sea level leaves sites on protected backshore berms and terraces. Significant changes of relative sea level can occur instantaneously during major earthquakes, when the destructive effects of shoreline subsidence may be compounded by accompanying tsunami waves. p. 17

CC- AMH: *Migration either into or out of the Americas would be much easier during deglaciation when the Bering Sea exists. Were the Americas peopled before the onset of the Wisconsin (~35,000 bp) or during glaciation when the coastal passage would have been the only route available? If the coastal passage was used as a migration route during glaciation it can be affirmed that the coastal shoreline was very narrow. Permanent settlement of the corridor - during glaciation - seems unlikely.*

Pucciarelli, Hector. **The Zhoukoudian Upper Cave skull 101 as seen from the Americas.** *Journal of Human Evolution* (1998) 34, 219-222.

Since 1988 we have been systematically investigating the morphological affinities of the first Americans (Neves & Pucciarelli, 1989, 1991; Neves et al., 1993; Munford et al., 1995; Neves et al., 1996a,b) and the results obtained so far can be, in our opinion, of significance for this debate. We have detected in these analyses that the first South Americans do not show any special resemblance to modern Mongoloids of northern Asia, but instead they show a marked biological affinity with South Pacific modern populations. In the majority of the bivariate and multivariate analyses we have carried out, the first South Americans showed a remarkable similarity to Australians, and to a lesser degree to Africans.

Consequently, we have suggested elsewhere that the first Americans cannot be described as mongoloids. Independent results using different methodologies and other paleoindian skull samples, this time from North America, amply confirmed the results we obtained with samples from South America (Steele & Powell, 1992, 1993, 1994; Powell & Steele, 1993).

Summarizing, we have found no morphological affinity between the first Americans and Mongoloids, or between Zhoukoudian Upper Cave hominids (including UC 101) and Mongoloids. We have, however, found some affinity between the Upper Cave hominids (UC 101 and UC 103) and the first Americans, and also between the Upper Cave hominids and South Pacific and African populations. The most economic way of interpreting these results is in our opinion to assume that people very similar to the native populations that presently occupy most of South Asia and Australia once dominated all of Eastern Asia, and departed to the Americas before the differentiation of Mongoloids in the Old World, a conclusion also recently reached by Cornell & Jantz (1997). In this scenario, Zhoukoudian Upper Cave hominids (including UC 101) should be seen as part of this non-Mongoloid population. pp. 219-221



*CC-AMH: The "Out of Asia" hypothesis (Johnson et al. 1983) can find warranting evidence in this re-appraisal of the upper cave Zhoukoudian hominids, Homo Sapiens, that overlay Homo Erectus populations. The direction, as suggested in dating the onset of mankind's migration throughout the Old World suggest that anatomically modern Homo Sapiens (AMHS) arrived at Klaisies River in southern Africa 36,350 bp. The dates for the Zhoukoudian H. Sapiens nears the limit of C-14 while the modern from Niah Cave near Java is similarly dated (~39,000). This paper further suggests that the first Americans have been in the Americas longer then there have been "Mongoloid" affinities since, as is suggested, they maintain the original morphological affinity associated with the radiation of fully modern humans (AMHS) throughout the Old World, perhaps even into Africa.*

Grahovac, Blazenka, Sukernik, Rem I., O'hUigin, Colm, Zaleska-Rutczynska, Zofia, Blagitko, Nadezhda, Raldugina, Olga, Kosutic, Tanja, Satta, Yoko, Figueroa, Felipe, Takahata, Naoyuki, and Klein, Jan. **Polymorphism of the HLA class II loci in Siberian populations.** *Hum Genet* (1998) 102:27-43.

Based on the HLA-DRB1 frequency data, the seven Siberian samples can be divided into two groups which correlate with the geographical distribution of the populations but not with their linguistic affiliations. The two populations in the interior of Siberia, the Evenks and the Kets, are on a different branch of the phylogenetic tree (Fig. 4) than the five eastern Siberian populations. The Evenks speak a language belonging to the northern Tungusic branch of the Altaic language family (Ruhlen 1988). The Ket language is of undetermined origin, a language isolate. The Udegeys speak a language of the southern Tungusic branch, the Koryaks and Chukchi speak Paleo-Asiatic languages, the Siberian Eskimos speak Yupik, and the language of the Nivkhs is another isolate of unknown origin. The clustering of the eastern Siberian populations with Alaskan Eskimo and Amerind populations supports the hypothesis that the colonizers of the New World originated in the Asian part of the North Pacific Rim. These results are in agreement with other genetic studies (Greenberg et al. 1986; Torroni et al. 1992, 1993; Horai et al. 1993; Shields et al. 1993; Sukernik et al. 1996). The HLA-DRB1 data also suggest that most of interior Siberia may have been populated long after the colonization of the New World. The significance of the clustering of the central Siberian populations with the Polynesians (Fig. 4) is not clear. It may be an artifact (other HLA class II genes, as well as the three-locus class II haplotypes tend to cluster all six or seven Siberian populations together) or it may reflect an ancient connection between the two population groups. More extensive typing of the Siberian populations and sampling of non-Siberian populations will be necessary to resolve this unexpected affinity. p. 41

*CC-AMH: The close relationship between eastern Siberians and Amerinds was seen by Boas as to indicate that they descended from Native Americans who had migrated into Siberia. Boas believed Tungusic Languages were unrelated to the Eastern Siberian groups. Is the probable relationship of Na-Dene, Ket, and Sino-Tibetan perhaps representative of an western (out of the Americas) expansion into Siberia of Dene speakers? Also, the Polynesian marker found in one central Siberian could have come from Nepal where it is found in 10% of the Tharu who trace their own origins to the Indian coast where the 9bp deletion is found in association with Polynesian/Melanesian settlement.*

*CC-JDB: The authors' assumptions about linguistic relationships are very conservative, with no reference to the Nostratic or Dene-Caucasian hypotheses. See the editorial article "What is Nostratic?," elsewhere in this Newsletter, where I describe the consensus of several paleolinguists that the Evenks, Udegeys, Chukchis, Koryaks, Nivkhs (= Gilyaks), and Eskimos (among others) are all speakers of Nostratic (or, to Greenberg, Eurasiatic) languages. Of the seven populations studied here, only the Kets are not Nostratic, but Dene-Caucasian. AMH asks if Dene-Caucasian could represent a reverse migration, i.e., back out of America, with only the Na-Dene remaining there. Linguistically, there is little or no reason to rule that out, though with five of the six families (or three of four) in Eurasia, the law of least moves would point to a Eurasian homeland (see McCall's article on Afroasiatic, discussed below under the caption AFROASIATIC, and also under Paleolinguistic News).*

Letters to the Editor. **Further Comments on the Characterization of Founder Amerindian Mitochondrial Haplotypes.** *Am. J. Hum. Genet.* 61:244-246, 1997, from Nestor O. Bianchi and Graciela Baillet.

Easton et al. (1996) confirmed our assumption, changed the letter "E" to "X," and reported X6 and X7 as two forms of founder haplotypes corresponding to the haplogroups that we formerly had designated



descendant traditions by sudden and clear-cut splitting, and (c) that these daughter traditions subsequently change in their own separate and isolated ways. As Bloomfield wrote, these are useful suppositions; they are not meant to be factual claims about historical reality, linguistic or otherwise. In spite of what common sense may tell us, the cautions raised by Bloomfield and others about these convenient simplifications are as applicable to Pacific populations as they are to any other on earth. p. 185 Perhaps we do need to stress that we are not trying to say that free-wheeling interaction was the rule in the Pacific regardless of location, geography, voyaging skills, motivations, and the like. Of course some people in the Pacific have lived (and still live) more isolated lives than others. They tell us that if we had paid more attention to Micronesia we would not have considered it significant that geneticists have now discovered that Polynesians, biologically speaking, cannot be derived directly from Southeast Asia. p. 186 We wish that Kirch were right that the myth of the primitive isolate is a straw man. Unfortunately, as our review of the literature documents, the history of science shows that there is no such thing as a truly "dead horse" and "straw men" walk the earth (and intellectually reproduce). How else is one to understand the current heated debate, for example, about human genetic history (Terrell and Stewart 1996)? p. 186

*CC- AMH: The important work contained in Terrell et al. are based on multi-disciplinary observations that initiate a change in the "given" dynamics of Polynesian history; that their migrations were not chance encounters with little if any subsequent contact following new discoveries of previously unknown atolls. Several points should be highlighted, 1) that there is little, if any, biological support for a southeast Asian source for the Polynesians, 2) that there was substantial knowledge of and verifiable inter-island contact during and following colonization, and 3) that the relationship between coastal Melanesians and Polynesians -- conforming with an advanced maritime cultural proficiency -- suggests that the original Pleistocene inhabitants of Oceania were pushed back into the interior. If the original Pleistocene inhabitants of Oceania were pushed back into the interior -- and Polynesians did not have a southeast Asian origin -- genetic affinities could link them with South American Indians. Further, genetic, linguistic, and cultural links between coastal cultures of Borneo and the original inhabitants of Madagascar indicate that the exploration's of the Polynesians was not limited to the Pacific and may have included the Arabian, Red, and Mediterranean Seas (Phoenicians ?). There are numerous bio-genetic traits shared by South American Indians and coastal Melanesians. Heyerdahl's book; *Native Americans in the Pacific* 1952 identifies many archaeological links to the New World that should now transcend -- given the overwhelming genetic relationship binding these "first People" -- an Amerindian source for the first Polynesians as NOT a migration out of Asia. That the same genetically identical people would migrate into separate unpeopled areas tens of thousands of years apart is as unfathomable to me as is the likelihood that Polynesians "first peopled" the remote Islands of the Pacific from the Americas, is for others.*

Lum, J. Koji, and Cann, Rebecca L. **mtDNA and Language Support a Common Origin of Micronesians and Polynesians in Island Southeast Asia.** *American Journal of Physical Anthropology* 105:109-119 (1998).

Ever since European explorers ventured into the Pacific and found virtually every island inhabited, scholars have debated the origins and relationships among Micronesians, Polynesians, and Melanesians. Archaeological and linguistic evidence supports two distinct stages of Pacific Island settlement. The first phase resulted in the colonization of the Melanesian regions of New Guinea at least 40,000 years before present (ybp) (Groube et al., 1986), the Bismarck Archipelago 33,000 ybp (Allen et al., 1988), and the Northern Solomons 29,000 ybp (Wickler and Spriggs, 1988). The distribution of these Pleistocene sites coincides with the distribution of Papuan-speaking people in the Pacific. p. 109

*CC- AMH: The proposed source for the Austronesian-speaking populations from Island Southeast Asia does not exclude the possibility that Polynesians came from the eastern Pacific and dispersed into Island southeast Asia since common genetic links (including HLAs and mtDNAs) to a "founding population" were already present in South Americans while southeast Asians and the original Papuan people preserve few, if any, of the genetic markers associated with the spread of the first Polynesians.*

van Holst Pellekaan, Sheila M., Frommer, Marianne, Sved, John A., and Boettcher, Barry. **Mitochondrial Control-Region Sequence Variation in Aboriginal Australians.** *Am. J. Hum. Genet.* 62:435-449, 1998.

With regard to the founding populations of Australia, several points emerge. The genetic-distance calculations place Papua New Guinea highland sequences nearest to both Australian groups, lending



as "E." Forster et al. (1996) also named as "X" one additional founder haplotype within what we formerly had called haplogroup "E." However, since the X haplotype of Forster does not correspond to X6 or X7, we propose to name it "X8," and we recommend using the letter "X" instead of the letter "E," to avoid confusion with the haplogroups "E," reported by Torroni et al. (1994) in Tibetans. It is worth mentioning here that we have found the X8 haplotype in 6 of 41 Sioux individuals studied. p. 245

*CC-AMH: The identification of Type E or, as suggested, Type X in northern Amerindian Populations could represent a Holocene migration from northeast Asia of Upper Paleolithic-like Cultures and technologies that demically assimilated into pre-Clovis Amerindian populations. The migration from northeast Asia of people carrying mtDNA types X-6, X-7 and X-8 lineages at the end of the last Ice Age could represent admixture into pre-existing Amerindian populations living in the northernmost regions of un-glaciated North America. Similar findings in Ojibwa populations suggest admixture while the virtual absence of X-type markers in other North American and South American populations suggests that the X type was not a "founding lineage" or, for that matter, even present before its (here-in proposed) post Ice Age arrival from northeast Asia (when Kennewick Man arrived?). Since pre-existing pre-Clovis Cultures were in the Americas during Glacial times the removal of the formidable Glacier Barrier would have preempted contact between, once isolated, New and Old World people. If the populations of the Americas were in the Americas long enough to develop their own regional set of mtDNAs (i.e. A, B C, and D haplotypes) then a migration "out the backdoor of the Americas" could have followed contact and assimilation with peoples from the Old World carrying Type X haplotypes. Thus, Haplogroup X in northern Amerindians could be evidence of admixture resulting from a northeast Asian based migration into pre-Clovis populations following deglaciation.*

*Alternatively, the presence of A, C, and D lineages in northeast Asia could represent a migration, by once isolated "pre-Clovis" Amerindians, into Siberia following the invention of fluted Paleoindian styles. Archaeologically based chronologies confirm a northern migration of Amerindians carrying Paleoindian tools north into Alaska (and Siberia?). Could the Intuit/Eskimo/Aleut and Athapaskan (Na-Dene) have originated from Amerindian pre-Clovis Populations? Compatible appraisals of mtDNA data must include the implications that afford an earlier "pre-Clovis" occupation of the Americas, and, more specifically, the possibility of Native American contributions to Siberian populations.*

## POLYNESIA

Terrell, John E., Hunt, Terry L., and Gosden, Chris. **The Dimensions of Social Life in the Pacific.** *Current Anthropology*, Volume 38, Number 2, April 1997, pp. 155-195

One of the remarkable things about human beings is the complexity of human thought. Today it may be more obvious than it used to be that time-honored ways of thinking about people and the past did not look closely enough at how people construct and maintain social fields reaching beyond the limits of their own face-to-face community, their own "society." Yet even this statement must be qualified. As the work of Alkire (1965), Harding (1967), and Sillitoe (1978) documents, this is not something that no one has said before. We do not need to throw away what has already been learned. It is time for more of us to look farther afield. We now need to favor historical realism over research convenience and deductive simplicity. We need to avoid sorting people into arbitrary types. We need to be wary about thinking that history and diversity in the Pacific can be reduced to a few grand moments of genesis and immigration. p. 175

**CA Comments From Ben Finney:** This is a most important work. The authors have done a magnificent job in analyzing a major shift in thinking about Pacific island prehistory from treating island cultures as isolates to realizing just how peripatetic the islanders actually were. After recalling the "1950s research agenda" for studying island societies as isolated "natural laboratories" of cultural differentiation, they concentrate on how archaeologists who had embraced this agenda in whole or in part are now abandoning it as evidence mounts for intentional settlement and for extensive post-settlement, two-way voyaging. p. 176

**CA Reply Terrell et al.** In light of what Goodenough, Bellwood, and Kirch say in their comments, we are beginning to think that it can never be said too often that the tree diagrams of historical linguistics are convenient analytical fictions. Some too readily accept the assumptions imbedded in such diagrams—for example, (a) that parent languages are uniform, (b) that such primal speech communities give birth to



support to the model of more-recent shared ancestry. However, the specific nucleotide variants in the sequences are different, so acceptance of common ancestry also implies that the ancestral mitochondrial gene pool from which both populations arose was diverse at the time of entry into Sahul and/or that considerable diversification has occurred since that time. p. 443

The model for separate migrations into New Guinea, leading to the consolidation of earlier immigrants in the highlands, is supported by the observation of highland people being closer, in genetic distance, to the Australians than to New Guinean coastal and Pacific people. This model is also supported by the confirmed absence of the 9-bp deletion in Australian samples, in the light of Redd et al.'s (1995) confirmation of its presence in Pacific populations, including coastal New Guineans. However, the Australian and New Guinea highland mitochondrial sequences are different, so further work is required in order to address suggestions of descent from the same biological group. p. 446-447

*CC- AMH: The Australian and New Guinea Highland Populations are genetically closer to each other than they are to coastal populations indicating that coastal populations represent a much more recent peopling event from an as yet determined source that was not present in southeast Asia when the original Australian and New Guinea highland populations crossed the Timor Strait. That the 9bp deletion is not a founding lineage in interior populations or found in high frequencies outside of coastal populations, suggests that it can be traced to Polynesian explorations, all the way to Madagascar. As evidenced by its predominance in coastal populations and near fixation in Remote Polynesia the only place where it occurs in nearly as many people is the Americas. Question, why do Native Americans seem to be confined to the Americas? Given the belief that they Peopled the vastness of the Americas why would they not return to northeast Asia as Boas believed or accidentally discover Tahiti when the currents affording even an accidental discovery are favorable only to them?*

## ASIA

Letters to the Editor; Tasha K. Altheide and Michael F. Hammer. **Evidence for a Possible Asian Origin of YAP+ Y Chromosomes.** *Am. J. Hum. Genet.* 61:462-466, 1997.

Similar patterns of variation at non-Y chromosome loci have been interpreted to support a recent African origin of contemporary human genetic lineages (Cann et al. 1987; Armour et al. 1996; Tishkoff et al. 1996); however, Hammer et al. (1997) have raised the possibility that YAP haplotype 3 originated in Asia and migrated to Africa. This hypothesis is supported by the finding of high frequencies of haplotype 3 in some Asian populations (i.e., 50% in Tibet) and by the observation of higher levels of diversity (based on the number and frequency of alleles at the DYS19 microsatellite locus) associated with Asian versus African haplotype 3 chromosomes. Because YAP haplotypes 4 and 5 evolved from haplotype 3 and account for the majority of Y chromosomes in Africa (table 1), this hypothesis implies a substantial Asian contribution to the African paternal gene pool (Hammer et al. 1997).

We now report additional evidence in support of the hypothesis of an Asian origin of YAP+ chromosomes, based on the distribution of a G→A transition in the SRY region. p. 463.

In sum, the ancestral states associated with polymorphisms that originated just before and after the YAP insertion into the Y-haplotype tree are currently found in Asian-and not in African-populations. The obvious implication, if this pattern continues as new systems are discovered, is that a major component of African Y-chromosome diversity had its roots in Asia. Similar patterns of variation at other loci are needed to support the hypothesis of an ancient migration of human populations from Asia to Africa. In this regard, it is interesting that recent studies of  $\beta$ -globin sequence variation indicate that modern human populations, including those from Africa, carry ancient globin haplotypes that also appear to have originated in Asia (Harding et al. 1997). p. 465

*CC-AMH: More genetic data supporting an Asian origin for AMHS dispersal(!)?*



## AFROASIATIC

McCall, Daniel F. **The Afroasiatic Language Phylum: African in Origin, or Asian?** *Current Anthropology*, Volume 39, Number 1, February 1998, pp. 139-143.

Three factors are involved in creating genetic distance: migration of peoples, mutation within populations, and natural selection. Migration creates separate populations. From the time of separation, each generation allows the process of acquiring and losing genes to proceed in both of the now separate populations. Each change in either population increases the genetic distance between the two, and, although there are a few factors that may affect these changes, the overall process is essentially regular. This is a simple matter when the movement is into an unoccupied territory but becomes more complicated when the move is into the habitat of another population. Calculation of the time elapsed since the separation of populations is possible but may require hypotheses about gene mixture or language replacement. Mutation rates are critical to this process, and their regularity or lack of it is still debated. At the moment the "molecular clock" is not totally reliable. p. 140

Paolo Francolacchi (1995;395) puts the elements of the problem succinctly:

"the mechanism at the base of the differentiation of languages (diffusion and subsequent isolation) is the same as that which is at work in the evolution of living beings. However, the linguistic transmission is not only vertical (from parents to offspring) as in the case of transmission of genes, but also horizontal (learning from neighbors). A single individual or an entire people can replace a language in a relatively short time, while obviously this cannot be done for genes. This can explain the incongruities when comparing the linguistic affiliation of a population with its genetic pattern . . . Nevertheless, in most cases, the correlation between the tree drawn from the genetic distances and that based on linguistic families is strong."

According to Cavalli-Sforza, Menozzi, and Piazza (1994:99) "The one to one correspondence between genetic clusters and linguistic families is remarkably high, but it is not perfect." p. 140

David Phillipson (1985) points to the importance of the Dabba culture site at Haua Fteah, Cyrenaica, because of its relation to contemporary industries in western Asia and Europe: "the closest connections of this phase of the Haua Fteah sequence are with the Levant." The tools in question have been radiocarbon-dated to 32,000-38,000 B.P. The movement of an Asian population into North Africa 20,000 years or more before the beginning of food production could, perhaps, account for the genetic closeness found by Cavalli-Sforza et al. but would be prior to the Neolithic, which they propose as the driving force for the expansion of Afroasiatic.

Butzer (1964:295) found that there was a movement of Palearctic (i.e., Eurasian) fauna and flora into northern Africa during the Wurm glacial regression (i.e., the late Pleistocene); it is plausible that some hunters in the Levant followed them. p. 142

Afroasiatic could have originated in Africa in accordance with the linguistic principle and still have a genetic affiliation with Southwest Asian populations. This does not dispute the calculations of genetic distances from existing samples, it merely questions the adequacy of the samples and the interpretation drawn from them. Bringing Aurignacian into the picture may point to a possible resolution of the problem. Archaeology may help to provide an answer; more discoveries of Aurignacian tools, especially associated with skeletal remains in Northeast Africa, would be desirable, as would genetic studies designed specifically to test the question. In other contexts Cavalli-Sforza et al. specify other factors besides food production that contributed to sustained population growth. Why not then see the rapid spread of Aurignacian tools as the moment of arrival of foragers with a Southwest Asian genetic makeup? p. 143

*CC-AMH: Important points found in this paper include; 1) discussions of modern human population movements from southeast Asia into Africa during the Wurm glacial epoch, 2) a generally accepted overview of the movement from Asia into Europe that led to Aurignacian or Upper Paleolithic technologies and 3) the identification of language and genetic correlates. Greenberg's "successful identification" of 4 distinct language groups in Africa could suggest that language diversification accrued during the past 40,000 years. Why is it that there remains, for many, a problem in accepting "Amerind" as one language family? Could HMHS have been in the*

*Americas longer or has Africa not been the cradle of humankind as the "Out of Asia" alternative dictates?*  
CC - JDB: See also the discussion under *Paleolinguistic News*.

Passarino, Guiseppe, Semino, Ornella, Quintana-Murci, Lluís, Excoffier, Laurent, Hammer, Michael, and Santachiara-Benerecetti, A. Silvana. **Different Genetic Components in the Ethiopian Population, Identified by mtDNA and Y-Chromosome Polymorphisms.** *Am. J. Hum. Genet.* 62:420-434, 1998.

Seventy-seven Ethiopians were investigated for mtDNA and Y chromosome-specific variations, in order to (1) define the different maternal and paternal components of the Ethiopian gene pool, (2) infer the origins of these maternal and paternal lineages and estimate their relative contributions, and (3) obtain information about ancient populations living in Ethiopia. p. 420

Considering both paternal and maternal lineages, only 5.4% of the mtDNAs can be classified as Caucasoid (table 3), whereas 25.4% of the Ethiopian Y chromosomes have a clear Caucasoid origin (12f2-8 kb; table 6). If one also includes as Caucasoid mtDNA types the ambiguous haplogroup U and the 10 DdeI10394AluI10397 (- -) haplotypes that did not show any tested non-Caucasoid feature, there could be a maximum of 27.0% of "Caucasoid-like" mtDNAs in the Ethiopian population. p. 431-432

The frequency of the DdeI10394AluI10397 (+ +) haplotype is of interest in this regard. As shown in table 4, this haplotype is virtually absent in Caucasoid populations (Indians excepted) and other sub-Saharan Africans. It has been found in India (Passarino et al. 1996c), in eastern Asia, and in peoples who migrated very early from eastern Asia (i.e., Australians, Papua New Guineans, and Amerindians: Ballinger et al. 1992; Torroni et al. 1992, 1993a, 1993b, 1994a, 1994c, 1994d). On the basis of its distribution and antiquity (estimated at 40,250-80,500 years ago [Chen et al. 1995] and 30,250-60,500 years ago [Passarino et al. 1996a]), we have suggested elsewhere that it preceded the split between proto-Indians and proto-eastern Asians (Passarino et al. 1996a, 1996c). p. 432

This haplotype reaches a frequency of ~20% in Ethiopia and has never been observed in mtDNA molecules of the other African or Caucasoid lineages (Torroni et al. 1994b, 1996; Chen et al. 1995; Passarino et al. 1996c; present study). Thus, it is likely that the Ethiopian and Asian DdeI10394-AluI10397 (+ +) haplotypes have a common origin. If so, then this marker either (1) has been acquired by Ethiopians through interchanges with Asians (indicating an Asiatic component in the Ethiopian genetic structure) or (2) was present in the ancient Ethiopian population and was carried by groups who migrated out of Africa. p. 432

*CC-AMH: The DdeI10394AluI10397 (+ +) haplotype is found in Amerindian Haplogroup C and D. This haplotype could have originated in Asia as a regional variation and later admixed with Ethiopians ~3,000 years ago since 20% have this combination. Hypothesizing a Pleistocene migration of Homo Sapiens -- Out of Asia into Sub-Sahara Africa -- could change the way we look at the original northeast Africans and later secondary migrations resulting in Neolithic admixture. Population expansion into Africa could be seen, under this scenario, as a "first peopling" with new markers found only in sub-Saharan Africans and the original Ethiopians the result of regional variation in Africa.*

*The "bootstrap model", used in defining the Eve-Out of Africa" hypothesis, identifies the abundance of additional 'African specific' mtDNAs as evidence of "greater sequence diversity" in African populations. The "Eve hypothesis" suggests that time depth is greater for mtDNA diversity in Africans. In order to support this, cladistic trees must use the "bootstrap model" to define, a presently unknown common ancestor in order to link presumably ancient mtDNAs not found outside of Africa with one recent exodus. If the Out of Asia hypothesis is considered then the DdeI10394AluI10397 (+ +) haplotype would not have been carried into Africa during the first modern peopling of Africa (~40,000 ybp) while all subsequent mtDNA diversity, found to be African specific, would have arisen following Africa's colonization. The size of the initial population(s) and drift (as well as other factors) have been identified as alternative reasons for finding "greater sequence diversity" in African populations (Harpending 1994; Long 1993). The presence of archaeological and/or anatomical evidence for earlier "archaic Homo Sapiens" habitation of Africa remains problematic since many, if not most, researchers specializing in this question have, in my mind, successfully challenged claims of fully anatomically modern Homo Sapiens in Africa earlier than 40,000 ybp and certainly, all claims greater than 50,000 ybp (Binford 1984; Parkington 1990; Mellars 1991; or Australia, see Bowdler 1990). For example, Binford (1984) identified in Faunal remains at Klaisies River Mouth, that Homo Erectus was still scavenging food remains from animal kills as recently as 35,000 ybp here in southern Africa while all the C-14 dates for modern human activities associated with the Howison Poort Industry fall well within the limits of C-14 (36,300-26,800). In fact, there is a relative*



dearth of interior "Later Stone Age" African sites to support modern Human behaviors or habitation elsewhere in Africa before 37,000 ybp (Klein 1983). With all the press given the possibilities of earlier AMHS the question still remains, is this really so?

## EUROPE

Letters to the Editor. **Reply to Cavalli-Sforza and Minch.** *Am. J. Hum. Genet.* 61:251-254, 1997, Martin Richards, Vincent Macaulay, Bryan Sykes, Paul Pettitt, Robert Hedges, Peter Forster, and Hans-Jurgen Bandelt.

In a recent paper (Richards et al. 1996), we used a phylogeographic approach to infer that most (>85%) of the mtDNA control region (D-loop) variation in present-day Europeans has an ancient ancestry within Europe, coalescing during the Upper Paleolithic. This seems to be in contrast with earlier principal-component analyses of nuclear-gene frequencies in Europe, widely interpreted as evidence for a substantial Neolithic settlement from southwest Asia, which overwhelmed the Mesolithic hunter-gatherers. p. 251

Contrary to some of the most detailed considerations of the archaeological evidence in recent years (e.g., see Whittle 1996), the mtDNA data suggest that new colonization of Europe from southwest Asia did indeed occur during the Neolithic, as Cavalli-Sforza and his colleagues proposed (Menozzi et al. 1978; Ammerman and Cavalli-Sforza 1984). Nevertheless, it seems likely that their interpretation underestimates the Mesolithic contribution. p. 252

*CC-AMH: The argument can made that Europe was colonized twice from southwest Asia, by both early Cro-Magnon ancestors and Neolithic people. The assimilation by the latter of the first Upper Paleolithic Peoples of Europe is a question of percentages of assimilation for total replacement seems beyond credibility, unless there were holdovers from the days of the European Neandertals (yuck yuck). Forty thousand years earlier total replacement was very likely as separate species do not mate, but they could imitate.*

## AFRICA

Churchill, S.E., Pearson, O.M. & Grine, F.E., Trinkaus, E., and Holliday, T.W. **Morphological affinities of the proximal ulna from Klasies River main site: archaic or modern?** *Journal of Human Evolution* (1996) 31, 213-237.

Results suggest an archaic total morphological pattern for the Klasies ulna. Analysis of diaphyseal cross-sectional geometry reveals an ulnar shaft with relatively thick cortical bone, but the specimen cannot be readily distinguished from Neandertals or early anatomically modern humans on the basis of shaft cross-sectional properties. If the isolated ulna from Klasies is indicative of the general postcranial morphology of these hominids, then the MSA [Middle Stone Age] -associated humans from KRM may not be as modern as has been claimed from the craniofacial material. p. 213

Arguments about the modernity of the Klasies MSA sample are paralleled by questions of just how "modern" the Near Eastern early modern humans from Skhul and Qafzeh Caves really are (e.g., Corruccini, 1992; Kidder et al., 1992). In the CVA, the multivariate centroid of the Skhul/Qafzeh sample falls close to, but on the archaic side of, the pooled modern sample mean. p. 232

*CC-AMH: Note that Churchill, Trinkaus et al. here, purposely (and properly) omit the 'h' in the spelling of 'Neandertal'. Also, the general overview of modern human behavior as different to archaic behavior of Homo Erectus, shows that it is logical to ascertain that species identification can distinguished sapient and non sapient levels when interpreting archaeological data, (see Pilbeam 1986).*

Lam, Y.M., Pearson, O.M. and Smith, Cameron M. **Chin Morphology and Sexual Dimorphism in the Fossil Hominid Mandible Sample From Klasies River Mouth.** *American Journal of Physical Anthropology* 100:545-557 (1996).

**ABSTRACT:** The site of Klasies River Mouth (KRM) in South Africa has produced a small sample of early Upper Pleistocene hominid remains that have been a focus for discussions of the origins of modern

humans. Despite certain primitive characteristics exhibited by these fossils, proponents of a single recent origin have attributed them to early modern humans. Critics of this hypothesis have emphasized the significance of the archaic features evident in this sample; including the absence of pronounced chins among the mandibular specimens. p. 545

The chin has been recognized as a synapomorphy of anatomically modern humans (Stringer et al., 1984; Stringer and Andrews, 1988), while its evolutionary significance has been the subject of continuing debate. Its development corresponds with the reduction in mandibular length and/or the size of the anterior dentition that occur first and uniquely among hominids in anatomically modern humans (Daegling, 1993; Weidenreich, 1936). p. 553-554

Tiller (1989, 1990) has observed a similar situation among the early Upper Pleistocene hominids from Qafzeh, noting that the chin development of these early anatomically modern humans seemed retarded in comparison to modern Europeans. Each KRM mandible that cannot be aged on the basis of dentition appears too large to represent a juvenile, so Frayer et al.'s (1993) argument concerning the frontal fragment cannot be adopted to explain the poorly developed chins observed here. p. 554

*CC-AMH: More evidence of the problems physical anthropologists have identified in deriving Homo Sapiens from Homo Erectus. If we directly evolved from the first African hominids to colonize the Old World it seems to have been very sudden. Moreover, the idea of "missing links" confounds the procession to modernity that suddenly and uniformly occurred over the entire globe, suggested by Multiregional proponents, as a - rising tide lifting all ships. Could this process have, hypothetically, if you will, affected American Indians if they were to have descended from Homo Erectus or had they migrated into the New World while they were Neandertals? Isolation and subsequent gene flow (often used to explain the multiregional model), becomes even more unexplainable were we to hypothesize H. Erectus in the Americas. This may be reason enough for most anthropologists to concede that any site older than 50,000 years is simply, unfathomable. If the behaviors of people living at Monte Verde at 13,800 bp is typical of other pre-Clovis sites then MV may help in distinguishing the problems archaeologists have had in identifying other mid-Pleistocene sites that are also missing Old World diagnostic components. What has long been proposed (Krieger; 1958), is that pre-Clovis represents "a pre-projectile stage" while this distant "horizon" would suggest Pleistocene isolation from developing technologies born and introduced from the Old World explorations of the first AMHS who may have come from the Americas?*

## THE AMERICAS

Heymann, Eckhard W. **Giant fossil New World primates: arboreal or terrestrial?** *Journal of Human Evolution* (1998) 34, 99-101.

In three recent papers Hartwig & Cartelle (Hartwig, 1995; Cartelle & Hartwig, 1996; Hartwig & Cartelle, 1996) reported on two fossil New World primates (Platyrrhini), *Protopithecus brasiliensis* and *Caipora bambuorum*, from eastern Brazil. Based on measurements of the skeletons, they calculate a body mass of about 20-25 kg, and an intermembral index of 04 for *Protopithecus* and 106 for *Caipora*. These two species thus represent the largest platyrrhines, living or extinct. p. 99

This argument includes some circularity, and a different conclusion should be taken into consideration; if the habitat was in fact very similar to current conditions, it is possible that *Protopithecus* and *Caipora* led a way of life that included a high degree of terrestriality. Although terrestriality has evolved in all paleotropical primate radiations, the question regarding why this way of life is lacking in New World primates has remained enigmatic. *Protopithecus* and *Caipora* might fill this gap. p. 100

*CC-AMH: The fossil record of New World Higher Primates is the weakest of the lot. That we are beginning to fill the gaps left by primarily looking to the Old World for human progenitors is exciting. That the New World does not have an "Olduvai Gorge" of its own should not reflect the negative difficulties in analyzing the potential of evidence for hominid evolution to have occurred from within the confines of the Americas. The evidence from Africa supports the evolution of hominids from earlier ape-like forms but does this evidence forestall the search outside of Africa, especially since we are still unsure that Homo Erectus is the only available ancestor for AMHS.*



Thompson, E.A., and Neel, J.V. **Private Polymorphisms: How Many? How Old? How Useful for Genetic Taxonomies?** *Molecular Phylogenetics and Evolution*, Vol. 5, No. 1, February, pp. 220-231, 1996.

We have previously pointed out that failure to recognize this aspect of most modern populations can lead to false inferences concerning such diverse subjects as the past occurrence of population bottlenecks or the frequency of slightly deleterious mutations (Chakraborty et al., 1988; Thompson et al., 1992). The present demonstration, that most rare variants have arisen relatively recently, as human population numbers dramatically expanded, and were quite localized, underscores that earlier conclusion. p. 231

*CC-AMH: The rare presence in northeast Asia of common mtDNAs to the Americas could indicate that instead of them being "founding lineages" for Amerindians that they are evidence of migrations into Asia at the end of the last Ice Age. Boas (1905; 1910) warned of this from data collected during the Jesup North Pacific Expedition. He based his preliminary conclusions on cultural, linguistic, anthropometric, and oral histories and myths that detail observations that now have genetic correlations to support them. The E or X-6-7-8 mtDNAs found in only northern Amerindians including the Ojibwa and Sioux may indicate immediate post Ice Age radiations of common Asian mtDNAs into pre-existing pre-Clovis populations, a migration that preceded later Holocene migrations of Amerindians "out the backdoor of the Americas" (Waters 1963). "Archaeologically speaking, the Nenana culture, with ties to northeast Asian Paleolithic cultures (and E or X type mtDNAs), may have introduced hunting technologies into isolated Amerindian populations before they (Paleoindians) migrated into northern North America. "Fluted stone projectile points" were first invented in eastern North America and carried into Alaska in what must be seen as evidence supporting an Amerindian contribution to the formation of Athabascan and later Paleoarctic cultures. Seen this way A, C, and D, mtDNAs found in northeast Asian people could represent a reverse migration of "rare variants have[ing] arisen relatively recently" of once isolated Amerindian haplotypes that arose in the Americas while glacially confined in pre-Clovis times.*

*Final Comment: Population movements and issues of first peopling, isolation, secondary migration must combine relevant inferences from related, yet diverse, fields of anthropology. Simple analyses that fail to look for resolutions by incorporating multidimensional relationships in order to better interpret the complex history of mankind's exploration of the Earth will fail to see the big picture. Absolute truth anthropologists attempt to unmask are best revealed by finding compatible solutions that encompass interdisciplinary research. Paradigm growth through theory building is often initiated by incorporating bias and building new observations from it. Bias is the fuel that drives both the accepted given and untested alternatives. New ideas or theories, if you will, that offer viable truths should ably resolve, by indemnification, presumed problems held by the accepted given. These problems must have viable explanations, embraced by the un-tested alternative.*

## **SELECTED QUOTES AND REFERENCES -- Settling the New World: Compiled by Randy Foote**

Over the past year it has become increasingly clear that the "Clovis Paradigm" for the settling of the Americas is no longer tenable. The Clovis theory, which has been the accepted idea for the past fifty years, derives from the discoveries in the 1930's of a highly successful hunting culture - featuring the famous Clovis fluted spear points -- that began to flourish in North America about 11,500 years ago. The assumption was that these people were based in Beringia during the last glacial period, and began to move south when the Cordilleran Gap, east of the Canadian Rockies, became deglaciated 12,000 years ago.

A corollary to the Clovis theory -- the "Clovis Overkill" -- was developed by Paul Martin to explain how Paleo-Indians were able to spread from Canada to the tip of South America in the space of 1000 years. Martin showed how highly successful hunters in a game-rich ecology could easily advance at such speed. These reputed hunters had refined their skills on the cold plains of Northeast Asia and Beringia, and when they arrived into the American Plains they found large fauna that had never encountered human hunters. The hunters flourished and advanced, while almost all the large fauna -- from mammoths to horses to saber-toothed tigers -- apparently died off in the span of one or two thousand years.

Clovis had developed into a very tidy theory. It also seemed to accord well with the "three migration" ideas, brilliantly developed on the linguistic side by Greenberg and supported by Cavalli-Sforza's genetic studies. The Clovis invasion would correspond to Paleo-Amerind, which includes the languages of most of North America and all of Central and South America. The relatively shallow time depth of only 11,000 years helped to explain why Greenberg could discern the relationships between the many Amerind families -- that the linguistic establishment still resolutely denies.

But it does not seem so simple now. (Henceforth *italicized* notes by Randy Foote = GRF.)

### **Monte Verde**

*Tom Dillehay of the University of Kentucky has been leading an excavation at Monte Verde in Chile for twenty years. He found one strata of undeniably human occupation ( a non-Clovis culture) that he carefully dated at 12,500 years ago. This dating -- and similar dating of other pre-Clovis sites -- has been resisted by the Clovis traditionalists for years. However, last January, a dozen of the most respected Paleo-American archeologists, including several of the most ardent doubters of anything pre-Clovis -- visited Monte Verde, and came away convinced that Dillehay had indeed accurately dated the human habitation there.*

.David J. Meltzer, **Monte Verde and the Pleistocene Peopling of the Americas**, *Science* 276: 754-755

"Since finishing the excavations, Dillehay has directed a painstaking analysis of the site materials and spatial patterning, reported in a volume of over a thousand pages. The effort was analytical overkill. Yet, overkill was necessary, given the great skepticism facing this (or any) potentially early site and the doubts about Monte Verde's antiquity that have been expressed since the site's discovery was announced over a decade ago. The first volume on the site resolved some of those initial concerns; the second volume puts the remainder to rest. These volumes, and an examination of the site and its collections in January, convinced a group of Paleoindian specialists--staunch skeptics among them--that the Monte Verde site is indeed archaeological and ~12,500 years old."

"As such, its implications are profound. Although only slightly more than a thousand years older than Clovis, the site's great distance from the Bering Land Bridge (the entry route from Siberia) indicates initial arrival in the Americas must have occurred much earlier than 12,500 years ago. How much earlier depends partly on obstacles encountered along the way: Interior and coastal routes south from Alaska, for example, were impassable for long periods (~20,000 to after ~13,000 years before present on current



evidence), as continental glaciers formed a physical and, for several millennia after their retreat, an ecological barrier to migration. It also depends on how quickly these groups adapted to the diverse and (as they moved south) increasingly exotic and unfamiliar New World, how easily they coped with novel pathogens and diseases, and how they maintained their population size and reproductive viability, contended with the potential genetic costs of inbreeding, all while living in relatively small numbers spread thinly over large and apparently unpopulated continents. On the basis of what is currently known of these variables, Monte Verde would imply an arrival in the New World before 20,000 years ago."

*The excavations at Monte Verde continue. And as much as the site described above revises old understandings of New World settlement, there may yet be even more revision needed. As Meltzer concludes:*

"Some 70 m away from the 12,500-year-old deposits, Dillehay's team recovered traces of a separate occupation that appears to date to >33,000 years before present. Dillehay remains noncommittal about those materials. He feels further excavations are required to confirm this occupation. If confirmed, its implications will be even more profound. Until then, however, those interested in the peopling of the Americas have plenty to occupy themselves, in the effort to fully explore the ramifications of the 12,500-year-old occupation at the site."

*Of course, not everyone agrees that the Clovis paradigm is disproved.*

Ann Gibbons, **Monte Verde: Blessed But Not Confirmed**, *Science* Volume 275, pp. 1256-1257

"In a discipline as contentious as this, one field trip is unlikely to unite the warring factions. A few skeptics remain unconvinced: 'Total consensus will only come when the final report is out and the pattern repeats itself at other sites,' says archaeologist Tom Lynch, director of the Brazos Valley Museum in Bryan, Texas, who doubts that humans were at Monte Verde so long ago. And even though opinion has been gradually moving away from the Clovis-first model for years, many bristle at the implication that the discipline can be regulated by one or two key people. The Monte Verde trip, they point out, came down to the conversion of just two leading researchers--hardly a paradigm shift."

*Alvah Hicks notes: "There are some, including John Driver, who feel that the dozen or so participants who went to Monte Verde should not, nor do not, validate the site for others who were not there and remain skeptical. Also, Dina Dincause expressed some reservations at the 1997 ASA when, as a discussant, she stated that it may take another 10 years to say it proves that there were Pre-Clovis people. This rhetoric continues while the development of a new paradigm awaits!"*

*In truth, this extension of Man's time of occupation of the Americas is not a sudden scientific revelation. And Monte Verde is not the only site in the Americas that has been dated earlier than the Clovis culture, but it has become the most intensely examined and - ultimately - accepted. But -- in spite of those still holding to the tidy ideas that have developed over the past fifty years -- most archaeologists now agree that it is no longer at all tenable to hold to the simple Clovis theory. As the "Clovis barrier" has been broken through, there are several other discoveries that might help shed some light on the early occupations. It is as if the floodgates are opening for new discovery and understanding as the Clovis barrier has crumbled.*

*As reported in Science, recent excavations in Peru, at Quebrada Jaguey (Sandweiss et al) and Quebrada Tacahuay (Keefer et al), seem to indicate the presence of maritime peoples on the Pacific Coast about 11,000 years ago.*

Sandweiss et al, Keefer et al; Heather Pringle, **Traces of Ancient Mariners Found in Peru**, *Science*, 281, 1775 - 1776 and 1830 - 1834:

"Two independent research teams report finding the first hard evidence, albeit indirect, for the maritime settlement theory [as an alternative to the Clovis model]. The discoveries, which reveal an ancient maritime culture in South America about 11,000 years ago, are 'about the best kind of evidence that you're

going to find that people familiar with the ocean were migrating down through the Americas,' says geologist David Keefer of the U.S. Geological Survey in Menlo Park, California, lead author of one study.

"As long ago as the mid-1970s, archaeologist Knut Fladmark of Simon Fraser University in Vancouver proposed that coastal peoples from Asia settled the Americas by paddling southward down the Pacific Coast with simple watercraft and a hefty dose of maritime savvy. Fladmark also noted that the theory would be hard to verify, because most of the clues left along the coast by these putative coastal explorers would now be underwater, drowned some 10,000 years ago by sea levels rising after the last ice age.

"Along the southern coast of Peru, however, the sea floor slopes steeply away from the coast. As a result, 'very little land horizontally was lost to rising sea level,' says archaeologist Daniel Sandweiss of the University of Maine, Orono. 'This is one of the reasons I was looking for sites in this region.' There, one U.S.-Peruvian team led by Sandweiss and another led by Keefer found two ancient campsites of a maritime culture. Radiocarbon tests on charcoal indicate that Quebrada Jaguay, Sandweiss's site, is 11,100 years old, while Keefer's, Quebrada Tacahuay, dates to 10,700 years, making these cultures among the most ancient in South America.

"Both the early dates and the maritime lifestyle make it unlikely that these people were the descendants of land-lubbing Clovis people, says Anna Roosevelt, an archaeologist at the University of Illinois, Chicago. After they reached South America, the Clovis were thought to have headed first for the Andean highlands, where the temperate, open habitat supported big game. 'They weren't supposed to reach the coast ... until later,' says Roosevelt.

"What's more, she and others have found equally old Paleoindian sites in South American rainforests, where they adopted a plant-collecting, foraging, and fishing lifestyle, again very different from that of the Clovis people (*Science*, 19 April 1996, pp. 346 and 373). Thus the ancient maritime sites 'suggest that Clovis is just one of several regional early Paleoindian occupations. There's no apparent ancestral relationship between Clovis and these people in South America,' says Roosevelt.

"But if Clovis isn't the mother of these maritime cultures, who is--and how did the ancestral stock get there? The obvious answer is by sea, says Keefer, although such a claim is far from proven yet. In Keefer's view, the net fishery and reliance on ocean food sources indicate a sophisticated and ancient knowledge of the ocean. That means that 'the most logical scenario would be for them to migrate down the coast', he says. The extreme aridity of the Peruvian coast--one of the driest places on Earth, both then and now--argues for water travel, says Sandweiss. 'If you had watercraft, then you could carry water and you could move more quickly' than traveling overland, he says."

*It should come as no surprise that men could have using boats for migration and sustenance some 15,000 years ago. After all, the settlement of Australia dates back to at least 40,000 years ago, and -- even during full glaciation -- Australia/New Guinea were always separated from Southeast Asia by open water. (Also note the item below -- Asian DNA Enters Human Origins Fray -- in which Cavalli-Sforza's work indicates an early coastal movement of man out of Africa all the way to East Asia.) The movement into the Americas could simply have been a continuation of that movement all around the Pacific Rim. This may well correspond to the lower level at Monte Verde, at 33,000 years ago.*

#### **Linguistic Implications:**

*So what does this all mean for the language families of the New World, and what light can linguistics provide toward an understanding of the peopling of the Americas?*

*One proposal was presented by Johanna Nichols, who has never agreed with Greenberg's Amerind studies.*

Anne Gibbons, **AAAS MEETING: Mother Tongues Trace Steps of Earliest Americans**, *Science* 279, 1306 - 1307:

"Using known rates of the spread of languages and people, Johanna Nichols, a linguist at the University of California, Berkeley, estimates that it would have taken about 7000 years for a population to travel from Alaska to Chile. Because that would put the first Americans' arrival squarely in the middle of the last major glacial advance, Nichols proposes that 'the first settlers began to enter the New World well before the height of glaciation'--earlier than 22,000 years ago.



"That date is early but is in accord with recent genetic studies suggesting that the diversity of DNA across American Indian populations must have taken at least 30,000 years to develop (Science, 4 October 1996, p. 31). In addition, Nichols's extensive analysis of Northern Hemisphere languages also suggests that several groups of Asians entered the New World, where they adapted rapidly to a range of habitats and adopted diverse ways of hunting and gathering.

"Nichols calculated that even if early Americans made a beeline, taking the shortest routes over the 16,000 kilometers of varied terrain from Alaska to southern Chile, the trek would have taken at least 7000 years. This would have put the Monte Verdeans' ancestors in Alaska when glaciers made it 'probably impossible' to enter the continent, she says. Instead, Nichols argues, the evidence 'strongly suggests' a migration before a major glacial advance began 22,000 years ago.

*{GRF NOTE: Notice that this disregards the likelihood of a coastal settlement, which would not have been made "impossible" by the state of glaciation, nor would have necessarily required "7000 years"}*

"Nichols checked her result against those obtained by other methods. For example, the New World has 140 language families--almost half of the world's total--and she estimated how long it would have taken this rich diversity of tongues to develop. Nichols began by surveying nearly all the language families of the Northern Hemisphere, from Basque to Indo-European, to see how often new language families have split off from an ancestral stock. She found that, on average, 15 new language families arose in each ancestral stock over the last 6000 years. Plugging that rate into computer models--which included an allowance for new migrations that carried in new languages after the glaciers retreated--yielded 40,000 years as the minimum time required to produce so many language families.

"...Nichols proposed the following scenario: The first immigrants from Asia crossed the Bering land bridge 'well before' 22,000 years ago and made it to South America. After the glaciers retreated, some people spread north, where they gave rise to the Southwest's Clovis culture, perhaps, and to other peoples. Meanwhile, human beings were again on the move along the Pacific Coast in Asia, with some language families heading south to Papua New Guinea and others north over the land bridge into Alaska--where they could have crossed once the ice sheets melted 12,000 years ago. Yet another group arrived at least 5000 years ago, she argues, giving rise to the Eskimo-Aleut family of languages."

*Well, that is one thought -- especially if one believes that there are indeed "140 language families" in the New World, rather than the three great families that Greenberg has exhaustively detailed. One might observe that Nichols' unspoken (at the AAAS meeting) corollary to her dating would be that, in light of this earlier settlement, no "reputable" linguist could possibly find relationships between long-divergent languages. And, by assuming that migration was impossible during periods of glaciation -- rather than allowing for coastal migration -- the time-depth becomes sufficiently extreme to demand that the Amerind languages be unrelated.*

*Another linguistic analysis of the situation -- making far more accurate use of climatic data -- was presented at the 63rd Annual Meeting of the Society for American Archaeology in Seattle in late March.*

*JDB: See also the discussion in this issue, under Paleolinguistic News, "Nichols on Settlement of the Americas."*

{Note: The entire issue of *Mammoth Trumpet*, Vol. 13, No. 3 is devoted to this meeting. It is easily available on the Internet at: <http://www.peak.org/csfa/mt13-3.html>  
For anyone interested in the peopling of the New World, it is well worth reading}

Donald Alan Hall, *The Americas After Monte Verde*, *Mammoth Trumpet*, Vol. 13, No. 3

"The new consensus that there were Americans who pre-dated the Clovis culture provides scientists such as Richard A. Rogers and Larry D. Martin the impetus to refine and renew arguments that people lived south of glacial ice that covered much of North America toward the end of the Wisconsin period. Archaeologist Rogers, formerly at the University of Kansas and now with a branch of the U.S. Department of Agriculture, has been analyzing the distribution of American languages; paleontologist Martin of the University of Kansas analyzes Pleistocene fauna. In separate papers, they explained to the

symposium chaired by Alvah Hicks and Rogers why they believe people must have been in North America at the time of the last glacial maximum, more than 18,000 radiocarbon years ago.

"To get a picture of North American languages through time, Rogers considered the distribution of more than 200 languages relative to four separate stages of deglaciation. The majority of these, 135, were distributed in the area unglaciated during the Wisconsin glacial maximum. Fifteen were distributed in areas unglaciated or deglaciated by 12,000 years ago, 13 in areas deglaciated after 12,000 years ago, 12 in the area deglaciated between glacial maximum and 12,000 years ago, 12 in the area deglaciated between glacial maximum and 10,000 years ago, and 11 in the area deglaciated after the maximum but before the present. None was found in areas deglaciated after 10,000 years ago. 'All languages isolated in that area,' Rogers said of the most-recently deglaciated region, 'can be shown to extend back into the areas that were deglaciated earlier.'

" 'That suggests to me--clearly--a movement into progressively younger areas where [languages] do not have enough time to diversify.' He displayed a graph charting through time, beginning with the glacial maximum, both the number of exclusive languages per million square miles and the percentage of exclusive languages in an area. The lines for number and percent were nearly identical.

" 'What I'm suggesting is that Wisconsin glacial ice left a readily discernible imprint on Native American languages and not just in the glacial maximum spread, but in the retreat. This is fascinating because it has lots of implications,' Rogers told the symposium. 'The obvious one suggests that human beings were already present 18,000 years ago.'

"Comparing evidence from linguistic studies with late-Wisconsin glaciation is known as barrier dating: 'Barrier dating,' Rogers explained, 'says that if you had a barrier in the past, say, a glacier, a changed seacoast, a giant lake, something like that, which disappears and yet which still seems to control boundaries of some linguistic, cultural, or biological phenomena like gene frequency, then it is very probable that those differences extend back to the time the barrier existed.'

" 'One of the big questions now in North American archaeology is how did people come into the New World,' said Rogers, mentioning the hypothetical ice-free corridor and the coastal routes. Then he returned to his illustration of North America on which language homelands were plotted as dots. 'Look at that map,' he said, noting the evident concentration of dots along the Pacific Coast from British Columbia into Northern California. 'Where do you think people were the longest? Which way do you think they came? I think it is pretty obvious.'

"Turning to human groups believed to have arrived in North America later, Martin traced Arctic tundra habitat, which is the home of people speaking Eskimo-Aleut languages. 'We can see the Eskimo-Aleuts will sweep across the north in the tundra environment, an environment similar to where they were in the Ice Age.' Then Martin showed a map of Na-Dene speakers moving south from coastal Alaska with the retreat of glacial ice and advancing spruce forests.

"With the exception of its Southwestern population, Martin said, Na-Dene people occupied an area that closely coincided with the range of a group of animals that included the snowshoe hare and pine martin.

" 'It's interesting,' he said, 'because these animals are in a distribution they could not have had during the Pleistocene because that area was occupied by continental ice. So since the Pleistocene--during the Holocene--these animals have expanded their ranges ... and have formed a distribution almost identical to that of the Na-Dene.' He called the correlations 'ideas of great power,' because they carry the important implication that languages maintain some sense of identity for a very long period of time.

"Martin suggested that languages could maintain certain broad categories of identity for 15,000 years. 'It's not an absolutely insane idea, because you have to remember that pharaonic Egyptian maintained a recognizable identity for almost halfway back to the end of the Pleistocene. 'Make that simply a language within the Semitic group and 'who knows how far we could recognize Semitic languages as a group.'"

*The occupation of the Americas is an excellent example of how linguistics, genetics, archaeology and paleo-climatology can -- and must -- work together in order to develop an understanding of the movements of modern humans across the earth. It seems to be a more coherent field of study than is Eurasia, because of the far shallower time-depth of human settlement, as well as having only one point of entry from Eurasia. And it is a wide-open field now, as old paradigms fall away in the light of new findings.*

*There has also been a variety of intriguing genetic research that might make the picture even more interesting, as Alvah presents, highlighting the mixture of what may be "Clovis" and "pre-Clovis" peoples.*

*Did the Clovis culture develop in the Americas as the maritime settlers moved inland, or was Clovis indeed one migration from Asia after the opening of the Cordilleran Gap? This is very much an open question. And, whether or not the Clovis hunting culture was home-grown or an Asian import, Paul Martin's "overkill" idea may still apply, in modified form.*

*As Alvah Hicks notes: "A presenter at the last ASA reminded us that Pleistocene epochs have been recurring every 30-40 thousand years for the past 3 million years. Is it coincidence that hunting technologies arrived in the Americas just prior to extinctions (people like Kennewick Man with mtDNA type X-6-7-8)? Could hunting leading to a reduction from a critical mass or necessary carrying capacity have contributed to said extinctions?"*

*The Clovis culture could still well be the dominant genetic and linguistic element in Amerind, particularly if it was indeed such a successful hunting culture. Or the picture could be even more complex than we can now see.*

*Consider the following as a starting point:*

- *Man entered the Americas earlier than 15,000 years ago -- maybe much earlier -- likely via the coast, as a maritime culture. This seems undeniable.*
- *Man could also have entered the Americas through the Cordilleran Gap prior to 20,000 years ago.*
- *The Clovis culture at 11,500 ya was certainly significant, and did expand throughout the Americas. The Clovis peoples may have come from Beringia, as successfully adapted hunters, or may have been "home-grown", an offshoot of the coastwise culture that settled the Americas earlier.*
- *Greenberg's Amerind theory implies that most of the Americas were settled by one linguistic group -- was this the coastal peoples or did the Clovis peoples linguistically swamp any earlier settlers whose languages may still have lingered in isolated pockets that do not quite relate to Amerind? Perhaps some of these pockets still retain their linguistic viability -- like Basque in the Pyrenees refugia -- and do not really fit into Amerind.*
- *Is the picture more complex than we have imagined -- both genetically and linguistically, and where do we go from here?? Mother Tongue would be an excellent place to collect some of these pieces in one place.*

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## NOTES AND REFERENCES --Early Man

B. Bower, **Asian DNA Enters Human Origins Fray**, *Science News*, October 3, 1998, p. 212.

A large genetic analysis of Chinese citizens and others indicates that modern humans, probably originating in Africa, migrated across Asia in a southeasterly direction before heading north into what is now China. This challenges the long-standing view of Chinese paleontologists, based on fossil evidence, that an East Asian branch of *Homo erectus* independently evolved into *H. sapiens*.

"It is now probably safe to conclude that modern humans originating in Africa constitute the majority of the current gene pool in East Asia," holds a research team headed by geneticist Li Jin of the University of Texas in Houston.

The scientists, who come from several institutions participating in the Chinese Human Genome Diversity Project, examined genetic relationships among 28 of China's 56 official ethnic groups, including the majority Han population. Analysis focused on short DNA segments, called microsatellites, that over time accumulate varying numbers of repeated sequences of nucleotides, the basic DNA subunits.

The researchers first used a computer program to construct an evolutionary tree of genetic relationships - based on 30 microsatellites per person - among 14 East Asian populations, 3 populations from Africa, and 8 from elsewhere in the world. This mathematical approach assumes that relatively isolated populations branch off from evolutionary precursors in a treelike progression. The researchers constructed a second



such genetic tree, based on 15 of the microsatellites, for 32 East Asian populations (including the 28 Chinese groups) and the 11 non-Asian populations.

Both reconstructions indicate that East Asian populations derived from a single lineage in Southeast Asia, Jin and his coworkers report in the Sept. 29 Proceedings of the National Academy of Sciences. This result is consistent with the existence of a prior genetic source in Africa, but not with a separate emergence of modern humans in East Asia, the scientists contend. The new DNA data also show that genetic differences exist between southern and northern Chinese groups and that greater genetic variation occurs in the southern populations.

The East Asian findings fit with the theory that humans migrated out of East Africa around 100,000 years ago and, perhaps by crossing short stretches of sea, traveled along Asia's southern coast before heading into East Asia, says geneticist L. Luca Cavalli-Sforza of Stanford University School of Medicine in an accompanying commentary. Further studies need to include DNA from mid-Asian populations to test for other possible expansion routes, he says.

However, Alan R. Templeton, a geneticist at Washington University in St. Louis, challenges the use of evolutionary trees. Jin's group did not employ methods for establishing whether far-flung populations maintained genetic ties through interbreeding, which would have undermined the assumption of a branching of separate ethnic groups, he says. In an upcoming *American Anthropologist*, Templeton reports that regional evolution in areas outside China, which he analyzed using others' data sets, occurred within a network of DNA links rather than a tree.

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Cavalli-Sforza, L.L. 1998. The Chinese human genome diversity project. Proceedings of the National Academy of Sciences 95(Sept. 29):11501.

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Ann Gibbons, **Ancient Island Tools Suggest Homo Erectus Was a Seafarer**, *Science* 279: 1635-1637.

"Most researchers have believed that Homo Erectus lacked the social and linguistic skills to pilot the deep, fast-moving waters that separate most Asian and Australian faunas, but in this week's issue of *Nature*, an international team presents new dates for stone tools from the Indonesian island of Flores that confirm H. Erectus's presence there 800,000 years ago. Although most researchers accept the new dates for the artifacts, questions linger about whether they are really tools, and researchers are sharply divided over the team's proposition that H. Erectus used rafts and may have had language. But if H. Erectus did indeed arrive on Flores by boat, it would mean that their cognitive abilities would be up for reappraisal and that the species was more adaptable than is commonly believed.

"The new findings also fit well with other work showing that Asian H. Erectus has been underrated. Controversial new dates from sites in Java suggest that H. Erectus persisted there from as early as 1.8 million years ago until as recently as 30,000 years ago, implying that they were able to adapt to varied terrain and climate. Other new studies suggest that H. Erectus left behind sophisticated hand axes in southern China. For those who have worked on Flores and long believed in H. Erectus's presence there, the new results are vindication. Says Sondaar: 'I am happy that the finds of Verhoeven are finally recognized.'"

Ann Gibbons, **In China, a Handier Homo Erectus**, *Science* 279: 1636.

"An invisible technological barrier has long been believed to separate the makers of sophisticated, two-sided stone hand axes in Africa, the Middle East, and Europe from their less adept cousins in Asia. Now new international excavations in China have revealed that at least a few early Asians were also making bifacial stone tools as much as 730,000 years ago."

Bernice Wuethrich, **Geological Analysis Damps Ancient Chinese Fires**, *Science* 281: 165-166.

"Studies of sediments at Zhoukoudian, China--long considered the site of the first use of fire--suggest that any flames there were not kindled by human hands. That means there's no strong evidence of fire use until about 300,000 years ago and none definitively associated with Homo Erectus, the hominid that began to spread through Asia and into cold northern latitudes starting about 1.8 million years ago. Researchers must now consider that this colonization may have happened without fire.

"In a sense, we spoil the story', says the lead author, structural biologist Steve Weiner of the Weizmann Institute for Science in Rehovot, Israel. Applying a battery of techniques, Weiner and his colleagues did confirm that there are some burnt bones at the site about 30 miles southwest of Beijing, but those might have been burned naturally. And they found no evidence of controlled use of fire: no hearths, no ashes, and none of the unique chemical signatures expected from fires."

**B. Bower, Language origins may reside in skull canals, *Science News*, vol. 153, p. 276, May 2, 1998:**

"A new analysis of ancient and modern skulls, the first to focus on the size of a bony channel that funnels a major nerve to the tongue, indicates that human ancestors living 400,000 or more years ago ... may have been able to talk much as humans do today.

"Fossil skulls attributed to *Australopithecus africanus* ... two million years ago, possess hypoglossal canals in the same relative size range as those of chimps, [Richard F.] Kay and his colleagues report in the April 28 PROCEEDINGS OF THE NATIONAL ACADEMY OF SCIENCES.

"In contrast, hypoglossal canals comparable in size to those of modern humans appear in two Neandertals ... 60,000 to 70,000 years old, a 90,000 year-old 'early' *H. sapiens*, and two *Homo* specimens ... 300,000 to 400,000 years old. ... Spoken language would have helped ancient hominids share critical information about local food sources and develop timely survival plans, [Erik Trinkaus, Washington University, St. Louis] says."

**Bone shift shaped modern human skull, scientist says, (Reuters), *The Boston Globe*, May 14, 1998:**

"LONDON - A US anthropologist may have solved a puzzle that has perplexed scientists for generations - why the skull of modern humans differs so greatly from that of its prehistoric ancestors. Daniel Liberman of Rutgers University in New Jersey said in a recent report in the journal *Nature* that a change in the sphenoid, a small bone at the base of the skull, altered the shape and dimensions of the modern human head ... 125,000 years ago.

"He found that the sphenoid was the cornerstone of the skull and that it interacted with 17 of the 22 bones to determine cranial shape ... The bone was reduced by 20 percent to 30 percent in humans compared with other hominids. ... It may have helped humans adapt to speech by improving the ability to produce distinct sounds. That finding is also further evidence that humans and Neanderthals are different lineages because you don't get this feature in Neanderthals,' Liberman said."

*The June issue of Current Anthropology, Vol. 39 (Supplement) June 1998, was devoted to the "Neandertal Question", with archaeological research indication that Neandertal tools and strategies were not necessarily qualitatively different from those of Middle Paleolithic HSS.*

**D'Errico et al, Neandertal Acculturation in Western Europe:**

"If our empirically based conclusions are accepted and the acculturation hypothesis is rejected, the implication is that archaeologists should now turn their attention to the problems posed by the cultural achievements of the late Neandertals. In our view, the renewed research should take two directions: 1) a reevaluation of Neandertal cognitive abilities and 2) a critique of biological determinism.... Most discussions of the [Neandertal] body ornaments in the Chatelperronian levels of the Grotte du Renne, for instance, have proceeded without considering the profound implications of their manufacture and use. Objects created for visual display on a human body necessarily imply the communication of some meaning. [These objects] suggest the elaboration of a code in which different categories of pendants carry complex messages by their presence, absence, association or position on the body"

**J. J. Shea, Neandertal and Early Modern Human Behavioral Variability:**

"If Middle Paleolithic human behavior was as variable and as responsive to ecological variation in other regions as it appears to be in the Levant, then the kind of behavioral strategic flexibility regarded by many prehistorians as an emergent feature of 'modern' human behavior during Upper Paleolithic times may actually have a long Middle Paleolithic pedigree.... While the details of Neandertal's and early modern *Homo Sapiens*' strategies for adapting to the Levantine environment differed from each other, both human populations were capable of significant behavioral variability... Further studies of Middle Paleolithic behavioral variability will no doubt shed much light on the complex co-evolutionary relationships between Neandertals and early modern humans"

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## NOTES ON THE INTERNET:

There is a very interesting - and often high-volume -- list on the Internet discussing the evolution of language, fittingly called the Evolution of Language List. It is moderated by Matt Fraser from the University of Pittsburgh, and brings together linguists of all stripes, paleo-anthropologists, geneticists and a wide variety of interested professionals. It is wide-ranging and highly non-dogmatic. It is well-worth looking at, and can be accessed at:

**Majordomo@list.pitt.edu**

with the following command in the body of your email message: subscribe evolutionlanguage

Here is a piece of a current thread:

**Sent by: v.sarich@auckland.ac.nz (Vince Sarich)**

Apropos of the historical linguistics discussion of a few days ago, I send along a lengthy excerpt from my Race and Language in Prehistory article, and I suggest that you look in particular at the Campbell/Kaufman bit and my take on it.

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..... At this point, however, frustration sets in. The problem is that linguists have in fact long recognized strong lexical and grammatical similarities among the languages in Greenberg's proposed Amerind. The data are not the issue. What is at issue is their insistence that this is may not be due to common ancestry, and the defense of this, to me, inherently bizarre position, has resulted in some of the strangest arguments present in the relevant literature.

First, Greenberg's critics claim that he has gone about it backwards; that is, instead of proceeding from lower to higher level groupings, using the comparative method as his guide, he has simply followed the approach briefly described above; that is, increasing the probability of finding cognate retentions, and, therefore, isolating long-range relationships, by sampling as many languages as possible. The response here has to be that Greenberg has been doing precisely what was done to achieve the recognition of every other major language family, including Indo-European itself. There is no family that was discovered through the application of the comparative method as such, though the recognition of differing reflexes as deriving from some ancestral form implies an ability to make choices as to which sound changes are likely, and which are not. Thus any suggestion that Greenberg, or anyone else, should, or even could, go about it in some other way has to strike one as truly bizarre, and I use the term advisedly. What else is one to call a policy of rejecting the only approach that has ever worked?

The situation is exactly the same in the world of organisms -- it has always been higher-level groupings that were recognized first, and there is no instance where, for example, an order of mammals was recognized by starting with species, grouping them into genera, genera into subfamilies, those into families, families into superfamilies, then to suborders, until finally we got to the order. It has never happened in that direction, and there is every reason to believe that it could not. Indeed, the very idea of even trying to go that way would strike biologists as -- well -- bizarre. They would see a large number of bodies covered with "feathers". Granted, these "feathers" wouldn't all have exactly the same structure, but they would seem pretty similar in the context of the large variety of other body coverings in sight. Replace "feathers" with "hair", and we bring another large number of bodies into a second grouping. We would then note that those with feathers all have bills, lay hard-shelled eggs, have forelimbs that take the form of "wings" which enable most of them to fly, and on and on and on, thus rapidly arriving at the category "birds". But now I have somebody come along and insist that I am going at this the "wrong way". I, in effect, have to ignore these more general features in favor of ones which define much smaller numbers of bodies, and then proceed stepwise until I find them coalescing into "birds". But, I say, I have already discovered that category, and it sure was easier than it's going to be doing it your way. He responds that ease is not a relevant criterion, that there is a wrong way, and a right way, and that I am going about it the wrong way, and the fact that it works



is irrelevant. At which point I decide that this is a badly confused fellow who really doesn't have anything to tell me, and go on my own way.

But this is the minor problem. Far more important is the already-mentioned fact that the marked similarities among Amerindian languages which led to Greenberg's proposal of the existence of a proto-Amerind have been recognized for a long time. It is thus interesting and instructive to see what some of Greenberg's critics have made of these. Two of the latter, Lyle Campbell and Terence Kaufman, have called them [for example, first person singular /n/, second person singular /m/] "pan-Americanisms" (1980)]. Now one would think that these were *prima facie* evidence for a proto-Amerind; that is, pan-American = proto-Amerind, and it becomes the null hypothesis to be tested. In this view, then, what could "pan-Americanisms" be other than cognates by any other name? Common ancestry is always the simplest (in Occam's sense, requiring fewer events) explanation, and is to be rejected only when the evidence requires it. But most American linguists simply do not look at it that way. Bright (1984: 25) is representative:

"I would not be opposed to a hypothesis that the majority of recognized genetic families of American Indian languages must have had relationships of multilingualism and intense linguistic diffusion during a remote period of time, perhaps in the age when they were crossing the Bering Strait from Siberia to Alaska. We can imagine that the so-called pan-Americanisms in American Indian languages, which have attracted so much attention from 'super-groupers' like Greenberg, may have originated in that period."

A similar point was made by Levine in his 1979 doctoral dissertation (pg. 11) on the position of Haida:

"There are thus signs of a developing consensus within Na-Dene studies that a proto-Athapaskan-Eyak-Tlingit did indeed exist, and that extremely prolonged contact between this language or its daughter languages and the ancestors of modern Haida is entirely adequate as an explanation of resemblances between Haida and the revised Na-Dene group."

Levine's removal of Haida from Na-Dene is then quoted approvingly by Greenberg's critics as a specific example of the failure of his approach.

But note that both Bright and Levine go out of their way to highlight the inherent flaw in their arguments. Bright writes of "multilingualism and INTENSE linguistic diffusion", and Levine of "EXTREMELY PROLONGED contact". Why the use of these adjectives? Because, presumably, it isn't a small number of similarities that link Haida with the other Na-dene languages, and the many Amerindian languages with one another; those similarities must be many and obvious, as Greenberg and Ruhlen, and their few supporters, keep emphasizing, seemingly to no avail.

Consider the situation with Haida and the other Na-Dene units (Tlingit, Eyak, and the many Athapaskan languages). Levine by his words grants what was obvious to Boas in 1894, and Sapir in 1915 -- that there are strong similarities between the two. That leaves us with two possible explanations: common ancestry or diffusion. Levine chooses the latter for Haida and Na-Dene; as does Bright for Amerind. Why? After all, the diffusion scenario requires three events/ processes to explain the similarities: differentiation into Haida (presumably not from a proto-Na-Dene), differentiation into proto-Tlingit-Eyak-Athapaskan, and subsequent diffusion between the two. The common ancestry explanation requires only one: the development of proto-Na-Dene. Bright's explanation for the apparent Amerind would of course require many, many more events; again against the one for a proto-Amerind. So I look at this and again wonder what is going on. Why the denial of the relevance of Occam's Razor here? One would think that question had been settled centuries ago. So, trying to act in good faith, I wonder if perhaps there is evidence that diffusion is anywhere near as likely an explanation for observed similarities as is common ancestry? But, no, that couldn't be; if it were, then there could be no historical linguistics as we have come to know it. It thus seems to me, as already noted, that there is an untenable position being defended here, a conclusion perhaps best illustrated by reference to an exchange in the pages of the *American Anthropologist* between Witkowski and Brown, on the one hand, and two of Greenberg's severest critics, Campbell and Kaufman, on the other, concerning the relationship, or lack thereof, between two language groups of southern Mexico and Central America: Mayan and Mixe-Zoquean. Witkowski and Brown, in their original article (1978), presented 62 putative cognate sets linking the two groups. Campbell and Kaufman, in their first rejoinder,

rejected all of these for various reasons, including 14 for being "so-called pan-Americanisms". Witkowski and Brown responded (1981), leading Campbell and Kaufman to the following, which has to be one of the most bizarre statements in the recent anthropology/linguistics literature (1983:365-6):

"We do not take at all kindly to WB's (1981:908) caricature of our reservations concerning widespread forms, called Pan-Americanisms by some, for such reservation is a standard criterion of distant genetic research in the Americas (Campbell 1973). We in no way appealed to or necessarily believe in the hypothesis attributed to us of "a gigantic Proto-Amerind phylum" (WB 1981:908), rather we made reference to the legitimate practice in the investigation of remote relationships in the Americas of avoiding widespread forms. It is generally recognized that certain forms recur with similar sound and meaning in very many American Indian languages (cf. Swadesh 1954). Acknowledgment of the widespread forms presupposes no particular explanation; while some may feel that these support some far-flung genetic connection (cf. Swadesh 1954; 1967; Greenberg, 1960; etc.), it is possible that some widely shared similarities may be due to onomatopoeia, sound symbolism, perhaps diffusion, accident, or other undetermined factors."

Campbell repeated the last part of this message in a letter published in *Scientific American* (1993):

"Greenberg's methods have been disproved. Similarities between languages can be the result of chance, borrowing, onomatopoeia, sound symbolism and other causes. For a proposal of remote family relationship to be plausible, one must eliminate the other possible explanations.

These words here make it clear that whatever it is that's going on here, it certainly isn't science as we have come to know it. Now I am painfully aware that bringing in Occam's Razor and claiming to have a better insight as to the nature of science is not a procedure likely to convince the reader of the validity of one's views; indeed, it is probably more likely to do just the opposite. Arguing data rather than theory is almost always the better way to go. But, as already noted, the data that are not at issue here. It is the question of how best to interpret the data; that is, theory. Thus I am forced into the position of assessing theory in terms of its practice, and struck by how much illogic Campbell and Kaufman manage in so few words. They write, apparently oblivious to the import of their words, of "the legitimate practice in the investigation of remote relationships in the Americas of avoiding widespread forms." How remarkably convenient -- if you don't like the conclusion, then just rid yourself of the only data which could possibly lead to it. What, one wonders, would they say about a zoologist who wrote of "the legitimate practice in the investigation of remote relationships among organisms of avoiding certain widespread forms such as the presence of feathers, hair, tetrapod limbs, or amniotic eggs"? Is there any conceivable way of recognizing "remote relationships" other than by documenting "widespread forms"? Could there be? I think not.

Then they tell us that these "widely shared similarities may be due to onomatopoeia, sound symbolism, perhaps diffusion, accident, or other undetermined factors." Well, yes, so they might -- indeed, we can be quite certain that all of these, including the "undetermined" ones, will have been involved, to some extent, in producing linguistic "similarities".

But the critical point here is that, in the absence of written records, there is no possible way of isolating and identifying those similarities resulting from "onomatopoeia, etc." until one has developed a phylogenetic tree linking the languages under study. What that tree cannot explain, and there will always be a good deal that it cannot, is then to be looked at for evidence of "onomatopoeia, etc." But it is obviously and inherently true that ANY similarity could be "explained" by appealing to these other factors. It is just as obviously true that this is not the case for phylogenetic explanations. The latter are falsifiable; the former are not -- or, more fairly, they are not until we have a tree of relationships. That this remains an issue for most linguists is perhaps the most frustrating aspect of the controversy surrounding Amerind, for there can be no resolution of the issues involved until there is agreement on the rules of the game -- and I have to say that the prospect of having to begin, in 1998, with an argument about the relevance of Occam's Razor, does not strike me as auguring well for the success of the enterprise.

#### **Vincent Sarich**

*Note: Vincent Sarich is Emeritus Professor of Anthropology, University of California at Berkeley. He is currently with the Anthropology Department, University of Auckland, (New Zealand).*

Sent by: **Larry Trask** <larryt@cogs.susx.ac.uk>

> On Thu, 22 Oct 1998, Vince Sarich wrote:<

I find much to ponder in Vincent Sarich's long posting, though perhaps not a great deal that I can agree with. I propose therefore to respond to a number of his points. To avoid burning out anyone's mail spool, though, I'll respond in several installments, more or less paragraph by paragraph.

I cannot agree that there is anything "bizarre" in the position that shared linguistic features need not be due to common ancestry.

The comparative linguists of the past operated with the view that the "genetic" (or "family-tree" or "Stammbaum") model was the \*only\* viable model of linguistic descent, and that linguistic diffusion was generally insignificant, at least in all but the most exceptional cases. Greenberg and company still adhere to this view today, but many linguists do not.

Lyle Campbell and other Americanists have been pointing out loudly for years that the diffusion of linguistic features across language boundaries is demonstrable and substantial in the Americas, and hence that the mere presence of identical or similar linguistic items in different languages is by no means proof of a genetic relationship, or perhaps even strong evidence for one.

Investigation in many parts of the world in recent years has demonstrated to general satisfaction that lexical items, grammatical markers and even entire grammatical systems can be transferred from one language to a neighboring but unrelated language. Consequently, many linguists have become pessimistic about the unvarying validity of our venerable genetic model.

At least half a dozen new models of linguistic descent have been put forward in recent years, many of them stressing the importance of convergence phenomena at the expense of the more traditional divergence, and all of them rooted solidly in the study of living languages which exhibit good evidence of complex histories not readily treatable within the genetic model.

There is a growing suspicion in my field that the large language families whose validity has been securely demonstrated may be special cases -- indeed, it may well be \*because\* they are special cases that we have enjoyed so much success with them. Our comparatively modest success in the Americas, then, may indicate only that there exist relatively few special cases there.

That is, it may well be the case that the Americas have been populated by tiny families for millennia, with only a few languages succeeding in spreading out in the traditional manner to give rise to extended families like Algonquian and Oto-Manguan. Most families may simply have remained tiny, and they may have been undergoing constant diffusion by contact for many thousands of years.

Of course, I can't \*prove\* that such is the case, but Greenberg can't prove that it isn't, either, and I can see no good \*a priori\* reason to dismiss such a possibility out of hand, as Sarich would apparently have us do.

Lest it be suspected that I am a crazed diffusionist who doesn't believe in the genetic model at all, let me point out something. At various times in the 20th century, Trubetzkoy, Uhlenbeck and Tovar (at least) have suggested that IE might not be a valid genetic family, and that PIE might never have existed -- that, instead, the IE languages result only from massive diffusion. When somebody put this idea forward on another list last year, I criticized it ferociously. Why? Because we can \*prove\* that PIE must have existed. In the IE case, we can identify so many systematic correspondences, and reconstruct so much grammar in elaborate detail, that the diffusionist idea must be flatly rejected as inadequate. In the Americas, we can neither brandish large numbers of correspondences nor reconstruct any significant amount of grammar -- and hence a diffusional explanation for the data remains very much on the table. A flat rejection of it would be foolhardy.

**Larry Trask**

*R. L. (Larry) Trask teaches Historical Linguistics at the School of Cognitive and Computing Sciences, University of Sussex (UK)*



Another place folks might be interested in -- if you haven't already been there - is the pages of the Language Origins Society at:

Address: <http://baserv.uci.kun.nl/%7Eelos/index.html>

Available there are a number of papers from the meetings of the Society.

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Derek Bickerton and William Calvin are in the process of writing a book on the origins of Language entitled *Deus Ex Lingua: Reconciling Darwin and Chomsky With the Human Brain*. They have sent the rough draft out onto the Internet

Address: <http://williamcalvin.com/bk-lingua/>

asking for comments or criticism or whatever. Bickerton is taking his study of language origins another step beyond his position in his book *Language and Species*, with its assumption of the Chomskyan genetically-based Language Acquisition Device, in an attempt to discover how grammar and syntax developed in Man. Calvin, a neuro-scientist, had earlier theorized that the origin of syntax developed out of the neural connections that came into being with the hand-brain co-ordination involved in throwing weapons (*The Throwing Madonna*).

Bickerton posits that there first existed a kind of Proto-Language, words without syntax, that was developed during the time of Homo Erectus, and that there was no true human language until modern humans integrated the simple words and expressions of Proto-language with a true syntax and grammar that made possible more complex communication. This is a position very different than that taken by Terrence Deacon and others (see *Mother Tongue* (Journal) III -- the "Hardware Symposium") that what defines human language is symbolic speech, which might have originated as early as Homo Erectus, and that language gradually evolved from that time.

Bickerton's ideas imply that true language came as a relatively sudden leap, and that it is this leap that defines Modern Homo Sapiens. The most likely evidence of this moment of definition is in the Out-of-Africa theory, that we all have a common origin from one group of Africans less than 200,000 years ago. It was this step into true language -- however that might have occurred -- that enabled our ancestors to so rapidly spread across the earth. Such a single -- and relatively recent -- origin of language makes it far more likely that relationships could be discovered between currently existing languages.

*From Bickerton and Calvin's precis:*

"But it has been difficult to identify nonlanguage predecessors of structured language. Most books on language evolution seem to dissolve into a series of vague generalities when it comes to deal with specific issues, such as how language acquired its universal characteristics, or what novel processes in the brain made language possible. The linguists haven't known the relevant neurology or evolutionary theory, and the brain researchers haven't known enough linguistics to realize what features required a detailed explanation. And there is, in addition, the requirement of continuity: How do your proposed mechanisms create an easy series of improvements or conversions of function, stages that span the whole distance, starting with chimpanzeelike utterances, working up to words and protolanguage, and finally to the phrases and clauses that enable you to say "I think I saw him leave to go home" with its nested embedding?

"We do it here with about two-and-a-half preadaptations, yielding both argument structure and phrase structure or, if you prefer, the newer minimalist grammar. The coin in which these improvements were purchased was that of reciprocal altruism, the cognitive categories handy for detecting cheaters, and of ballistic movement planning, handy for toolmaking and hunting. "

# Mother Tongue Newsletter No. 31

## Fall 1998

### Paleolinguistic News:

#### Greenberg's Visit to Arizona

by Peter Norquest

University of Arizona

As announced in the last *Mother Tongue* Newsletter, we at the University of Arizona were very fortunate to be visited on the 29th of April by **Joseph H. Greenberg**. Dr. Greenberg, as most readers know, is both a pioneer and a veteran in the field of long-range language classification, having done extensive work on languages of Africa, the Pacific, and the Americas. It is Greenberg's classification of African language phyla, developed in the fifties, which has been used as the norm in textbooks and encyclopedias. His more recent, and much more controversial, Amerind phylum (introduced in his 1987 book, *Language in the Americas*) may still be considered to be in its infancy, and has thus far been contested by several Americanists, though it may in the end prove to be one of Greenberg's most important contributions to the field. (It should be noted that there has been subsequent work on Amerind, primarily by Merritt Ruhlen, in various articles since 1987: for his suggested sub-classification of Amerind, see his article "The Amerind Phylum and the Prehistory of the New World," in the book *Sprung From Some Common Source*, ed. by S.M. Lamb and E.D. Mitchell, Stanford, 1991.) Greenberg, it would be fair to say, is best known among students and younger linguists for his innovative work on language typology and universals, another area in which his contributions have been immense.

On the day of his lecture, Dr. Greenberg generously gave the better part of his morning and afternoon to half-hour appointments with individual professors and students. Upon meeting him, I was impressed by both his friendly demeanor and the attentive clarity with which he discussed the topics at hand. (He is, after all, a rough contemporary of other linguistic pioneers such as Morris Halle and the late Paul Benedict, and well on his way to his hundredth birthday.) Our discussion largely revolved around Eurasiatic (see below), particularly regarding its relationship with Nostratic, as well as the validity of Ainu as a member of Eurasiatic. Dr. Greenberg promised a forthcoming article entitled "The Convergence of Eurasiatic and Nostratic," in which he will treat the status of language families which were originally included in only one of the two overlapping super-stocks. He also mentioned, both to me and in the later lecture, that while he would continue to include Ainu in Eurasiatic for the present, time may eventually show that it is non-Eurasiatic after all. The most popular competing theory which has been suggested in one form or another is that Ainu is instead an Austric language. (For example, the recent work of Alexander Vovin, John D. Bengtson, and Václav Blažek [see *Mother Tongue* (Journal) II (Dec. 1996)].)

The topic of Greenberg's lecture, introduced by Jane Hill (president of the American Anthropological Association) was entitled "Is Indo-European Isolated?" Using this as a rhetorical question, an Old-World super-phylum (which includes Indo-European, Uralic, Altaic, Japanese-Korean-Ainu, Nivkh, Chukchee-Kamchatkan, and Eskimo-Aleut) was then introduced.

Greenberg devoted the first part of the hour to a discussion of the nuts and bolts of language classification, and I quote from his handout:

Reconstruction is not an end in itself. We may be very sure that a certain set of languages form a family without being able to reconstruct the ancestral language. Moreover, we can never verify a reconstruction. The purpose of historical linguistics is not to write asterisks. The goal, rather, is as an aid in recovering history, in the study of linguistic change and the discovery of universals of change.

Greenberg then proceeded to explain the importance of suppletive alternations in the Eurasiatic first- and second-person pronominal paradigm (the *mi-ti* forms which are found throughout all of the Eurasiatic super-phylum, with the exception of Ainu). This involved a comparison of Indo-European *\*eg(h)om* 'first person nominative singular' with other forms, such as Chukchi *gəm* 'first person singular' and *(i)gət* 'second person singular'; Western Kamchadal *kəmma* 'I' and *kəzza* 'thou'; Hungarian *en-gem* 'me' and *te-ged* 'thee' (where *en* is the independent pronoun 'I' and *te* is 'thou'); and the Eskimo suffix *-m-ket* 'I act on thee' (where *-m-* is a first-person marker, and *-ket* [*< \*gət*] is a second-person singular form).

Followers of Greenberg's work may have been disappointed that he did not lecture on a newer subject, since articles by him treating pronominal suppletion have already been published elsewhere (e.g., "Some Problems of Indo-European in Historical Perspective" in *Sprung From Some Common Source*; "The Indo-European First and Second Person Pronouns in the Perspective of Eurasiatic, Especially Chukotian" in *Anthropological Linguistics* 39, no. 2). The topic was nevertheless aptly chosen for an audience of linguists with a variety of backgrounds. Greenberg responded to questions after the lecture (e.g., "How does one know that pronominal similarity arises from a genetic relationship and not phonaesthesia?")

It was a great pleasure to have been able to meet Dr. Greenberg, and I am among the many who eagerly await the publication of his two-volume work on Eurasiatic. (Greenberg did give out five Eurasiatic roots during his lecture, however: *mel* 'good', *pol* 'leaf', *gel* 'freeze', *nək* 'night', and *mən* 'hand'.)

## McCall on Afroasiatic

Former ASLIP Vice-President (and now Council Fellow) Daniel F. McCall has contributed an article on "The Afroasiatic Language Phylum: African in Origin, or Asian?" to *Current Anthropology* 39(1): 139-144, 1998). His article provides a thorough analysis of the linguistic, archaeological, and genetic evidence offered by Cavalli-Sforza and others. McCall concludes that "in time the various types of data will join in sustaining a coherent interpretation of the whole, but that moment is not yet here. ... My prediction is that Africa will turn out to be the cradle of Afroasiatic." Among reasons given by McCall for this prediction is "the discovery, by Harold Fleming [1969], that Omotic is a family in itself," and "another language ... Ongotan, may also be a separate family ..." and also "Beja is suspected by some linguists ... to be ... separate from Cushitic." Thus the "five families of Afroasiatic specified by Greenberg now become at least six and possibly will eventually be recognized as eight, still with only one [Semitic] in Asia. The assertion of an African origin for Afroasiatic is therefore stronger than ever." (See also the report in Biogenetic news, above, under the heading AFROASIATIC.)



## Ruhlen published in *La Recherche*

*La Recherche* (No. 306, Feb. 1998, pp. 68-75) features an article by Long Ranger Merritt Ruhlen, "Toutes parentes, toutes différentes" (All related, all different), subtitled "Pourquoi l'idée de remonter à une langue ancestrale originelle n'est pas absurde" (Why the idea of demonstrating one original ancestral language is not absurd). Ruhlen argues that there is abundant evidence (e.g., global etymologies, and other recent breakthroughs in paleolinguistics) pointing to a fairly recent (within 50,000 years) original Proto-Human language, and that archaeological and genetic evidence also points to a similar recent origin of *behaviorally modern* humans. This Proto-Human may not have been the *only* language of early humans, but it was the one ancestral to all languages extant today. Contrary to the common belief among historical linguists, comparative-historical linguistic methods - says Ruhlen - *can* indeed be used to reach back to this type of recent origin of human language.

Ruhlen's article is followed by a rebuttal by Anne Szulmajster-Celnikier, arguing for the more orthodox view: that the complexity and relative rapidity of linguistic change make the hope of recovering very ancient linguistic relationships (before 10,000 or so B.P.) - not to mention the Proto-Human stage - highly unlikely. (See below, on Johanna Nichols.)

## Nichols on Settlement of the Americas

Johanna Nichols, Professor of Slavic Studies at Berkeley, has been in the news since her appearance at the "First Americans" Symposium in Philadelphia, February 16, 1998. Nichols is known to many *Mother Tongue* readers as a leading proponent of the orthodox historical linguistic position that "linguists cannot trace language descent much beyond 6,000 years or, at most, 10,000 years." [As quoted in the *Berkeleyan*, March 11-17, 1998. The 6,000 year figure happens to correspond to the commonly accepted age of the Indo-European family, and the 10,000 year figure presumably represents the age of the Afroasiatic (Hamito-Semitic) (macro-)family, possibly the oldest language family that is universally accepted. Thus, according to Nichols and like-minded colleagues, all the paleolinguists who accept older (macro-)families, such as Nostratic/Eurasiatic, Dene-Caucasian, Austric, Amerind, Nilo-Saharan, Indo-Pacific, etc., are simply wrong. Ed.]

In spite of these limitations, Nichols confidently draws far-reaching conclusions about the linguistic prehistory of the Americas. "The first wave of migrants from Asia arrived 30,000-40,000 years ago. They spread - perhaps thinly - through the Americas, and when the Wisconsin glaciation came they 'apparently huddled in South America to wait out the climate change.' After the ice sheets retreated, they spread north again, repopulating much of North America from the south, but eventually encountering new waves of immigrants 'different in both language and culture from the older natives,' who were moving down the Pacific Coast." [From the Associated Press story by Richard Cole.] According to the *Berkeleyan* article, one feature of the Pacific Coast linguistic group is a pronoun system with *n* in words for 'I, we' and *m* in the words for 'thou, you' - precisely the pronominal pattern posited by Greenberg for the Amerind family. According to Nichols, this pattern is common to "both the American Pacific Coast and the south Asian Coast [sic]." [As quoted in the *Berkeleyan* (see above). *n* / *m* pronouns on the "south Asian Coast" presumably refers to some Austroasiatic languages which have *en* 'I' / *mV* 'thou', or the like. Otherwise, the pattern is very unusual in the world. Ed.]

(See also the discussion in the Language Evolution News section, above.)

## Conferences at Cambridge (England)

Some conferences of major interest to Long-Rangers have been going on in Cambridge (England) this past summer, and others are planned for the future. A "Symposium on the Nostratic Macrofamily" was held at the McDonald Institute for Archaeological Research on July 17-18, 1998. The conference centered on Aharon Dolgopolsky's new book, *The Nostratic Macrofamily and Linguistic Palaeontology*, published by the Institute in 1998, and featured scholars on all sides of the debate, e.g., David Appleyard, Allan R. Bomhard, Lyle Campbell, Bernard Comrie, Gyula Décsy, Chris and Pat Ehret, Irén Hegedüs, Alan S. Kaye, Marisa Lohr, April and Robert McMahon, Peter A. Michalove, Vitaly Shevoroshkin, Denis Sinor, Sergei A. Starostin, R.L. Trask, Rainer Vogt, and Kamil V. Zvelebil. (Alexis Manaster Ramer and Alexander Vovin were not able to attend, due to illness, but their papers were made available at the conference. See Vitaly Shevoroshkin's report of the Symposium, below; and the editorial "What is Nostratic?" [in this issue] for a discussion of the importance of Nostratic and other macro-family proposals.)

Another conference was held at Cambridge on September 6, 1988, on "Languages and Genes in the Americas." Discussants included Victor Golla, Joseph Greenberg, Jean-Marie Hombert, Terence Kaufman, Roger Lass, April McMahon (conference organizer), Robert McMahon, Anna Morpurgo-Davies, Daniel Nettle, Donald Ringe, and Merritt Ruhlen. A reliable source tells us that the discussions were vigorous, yet mainly cordial.

A conference centering on a discussion of the Sino-/Dene-Caucasian hypothesis, organized by Colin Renfrew and Sergei Starostin, is planned for December 1999.

We understand that Lord Colin Renfrew has been the prime mover behind these conferences, and we (the paleolinguistic community) are thankful to him, and to the Alfred P. Sloan Foundation for sponsoring these vital activities.

## 1998 Symposium on Nostratic at Cambridge

by Vitaly Shevoroshkin  
University of Michigan

The Symposium on the Nostratic Macrofamily (Cambridge, July 17-18th, 1998) was organized by Professor Lord Renfrew, Director of the McDonald Institute for Archaeological Research in Cambridge, England. It was supported by the above Institute, as well as by the Alfred P. Sloan Foundation. The Symposium was based on A. Dolgopolsky's book *The Nostratic Macrofamily and Linguistic Palaeontology* (Cambridge 1988, xxii [= Introduction by C. Renfrew] + 116 pp.), published by the Institute. (The book was preceded by Dolgopolsky's seminar at Cambridge.) The book consists of a discussion of 124 Nostratic cognates from the new Nostratic Dictionary by Dolgopolsky. (To be published soon, the dictionary contains about 2300 Nostratic roots/words.) The 124 roots selected cover various aspects of the life of the Nostratic people: family, dwellings, man-made things, nature, verbs of the type 'wicker/wattle', 'cover', 'catch (with a net)', 'pierce', 'burn', 'tell (a story)'.

The participants received Dolgopolsky's book (called *NM* for reasons of simplicity) a few months before the Symposium, but they were free to present papers not necessarily confined to the topics of the book. They also received paper preprints. The Symposium was exceedingly well-organized.

Professor C. Renfrew underlined, in his introductory remarks, the importance of Nostratic studies for research in the prehistory of Europe and western Asia.

A. Dolgopolsky presented a paper titled "The Nostratic Macrofamily: A Short Introduction," covering only the linguistic aspects of Nostratic: phonology (including tables of sound correspondences between Semitic, Egyptian, Berber, Kartvelian, Indo-European (IE), Uralic, Turkic, Mongolic, Tungusic, Dravidian), grammar, grammatical typology, derivation, and the position of Semito-Hamitic [Afroasiatic = AA]. On the last point, Dolgopolsky argues with S. Starostin, who considers Semito-Hamitic a sister-language of Nostratic, not a daughter-language. It is clear from the phonological tables that IE had no "coloring laryngeals."

Dolgopolsky presents proto-Nostratic as based on a rigid word order, with auxiliary verbs and pronouns. The predicate appears at the end of a sentence; only a pronominal subject may follow the predicate; object precedes verb. Nominal attributives precede the noun, whereas pronominal attributives (e.g., 'my, this') may follow the noun; case markers follow the noun. (This order was lost in IE and AA.)

First and second person pronouns are: \**mi* 'I' (oblique case \**HoyV*), plural \**mi-Ha* 'we'; \**t'ü* / \**sü* 'thou' (obl. \**kV* / \**gV*), plural \**t'ü-Ha* 'ye'.

Some postpositional auxiliary words are: \**nu* 'of, from'; \**ta* or \**tä* 'from'; \**ma* (marked accusative); \**kV* or \**qV* 'to(wards)'. Proto-Nostratic had archaic postpositional adverbs \**nu* 'from' (cf. separation preverb in Baltic), \**ma* (formative of marked accusative, where Illich-Svitych reconstructed oblique case marker \**-n*, accusative marker \**-m*, etc.) Dolgopolsky analyzes the IE verbal plural ending \**-nt(i)* as a compound of pronouns traceable, say, in modern Uralic languages (Finnish *nuo* 'those' : *tu* 'that').

Dolgopolsky defended Illich-Svitych's (and his own) thesis of Semito-Hamitic (AA) as a daughter of Nostratic, indicating that among the 2300 Nostratic roots, about 1600 do appear in AA. Starostin objected by asserting that many roots are represented by only one language (and sometimes by just one word) in AA, and this is not acceptable methodologically. Besides, in many cases the given AA (frequently Arabic) root/word may simply be a borrowing. No solution to this problem was reached during the Symposium.

D. Appleyard in his paper "Afroasiatic and the Nostratic Hypothesis" maintains that "the lexical evidence in NM for Afroasiatic inclusion, at least, in 'Nostratic' is too insubstantial to prove a case." His material shows, though, that a genetic relationship between AA languages which belong to Nostratic is undeniable. Among grammatical words which he grudgingly accepts as "marginally more convincing" evidence for Nostratic are the AA pronouns of the first and second person: \**nV* and \**tV*, respectively; the interrogative \**mi*; causative \**sV*; negative-indefinite \**ma*; locative \**dV*; 'directive' \**kV*; demonstrative \**nV*.

As to lexical evidence, Appleyard's assertion that there are only a few roots in NM which show genetic links between AA and (other) Nostratic languages is no strong argument against the genetic relationship of these languages, since Dolgopolsky mainly discussed cultural words in NM (only 124 items). Cultural words are frequently replaced by other words, to say nothing about eventual borrowing. Still, Appleyard's objections should be examined, since some of them may support the hypothesis that AA is a sister (and not a daughter) of Nostratic.

Appleyard is ready to accept a genetic relationship between IE and Kartvelian, as well as between IE and Uralic (which implies that Kartvelian and Uralic are mutually related, as well), but not between IE and AA, though his own material tells us otherwise.

A. Bomhard analyzes all 124 NM entries in his review, and, predictably, disagrees with many cognate sets. Still, Illich-Svitych and Dolgopolsky's studies seem to reflect Nostratic correspondences properly, hence many new sets which are much more plausible than Bomhard's. We may add that the reconstruction of the Nostratic stop series as *Th - T - D* (in the independent studies by Starostin, Griffen, Shevoroshkin, et al., mostly published in the eighties) is more realistic than Bomhard's *Th - T' - Dh*.

Correctly, Bomhard maintains that Dolgopolsky should have laid out, both at the beginning and at the end of his NM, some detailed conclusions concerning linguistic paleontology (namely, the possible age and location of the Nostratic homeland, social organization and material culture of the speakers of Nostratic, etc.).

L. Campbell discusses Dolgopolsky's evidence (as presented in NM) in his paper "Is it Believeable?: Nostratic and Linguistic Palaeontology in Methodological Perspective." Considering himself a "mainstream linguist" (but not defining the term) Campbell tries to show "why the mainstream historical linguists do not accept the Nostratic proposal." He asserts that "nearly all of Dolgopolsky's 124 sets exhibit serious problems from the point of view of methodology." His objections are mostly invalid, taken from the methodologically unfounded lists of other critics of Nostratic, showing a profound lack of understanding both of Nostratic data and of methods used in Nostratic research. His text contains a fair number of methodologically inadmissible mistakes. He was severely criticized during discussion.

G. Décasy's "Remarks on A. Dolgopolsky's Publication NM" represent several notes based on the assumption that "all languages of the world are related."

C. Ehret in his report "Nostratic - or Proto-Human?" wrongly maintains that "systematic historical reconstruction will never be possible for more than 6,000 (or perhaps 8,000 or 9,000) years ago." The Nostratic reconstruction by Illich-Svitych / Dolgopolsky / Starostin is a systematic historical reconstruction of a language spoken some 14,000 years ago, or more.

In contradiction of his own thesis, Ehret mentions some "systematic reconstructions of deep African families, whose reach may be as far back as 10,000 - 15,000 B.P." Ehret also provides a list of 24 cognate sets between Nostratic (Bomhard's version) and Nilo-Saharan roots. He calls these cognates "data of wider human occurrence," which he explains by massive borrowings in prehistoric times.

In her paper "Linguistic Palaeontology: For and Against," I. Hegedüs praises the pioneering collective research first presented at the First International Symposium on Language and Prehistory (Ann Arbor, 1988 [organized by V. Shevoroshkin and B. Stolz: Ed.]), mentioning a small but similar symposium on IE and Nostratic in Ypsilanti (near Ann Arbor) in 1993. The 1998 Cambridge Symposium is the third in this series, and may become "a catalyst in the development of Nostratic studies ... The process of proving this hypothesis ... has already provoked meaningful discussions, it attracts more and more attention and is gaining a more objective critical treatment than earlier."

Among considerable difficulties in the way of linguo-palaeontological research are: gaps which hamper identification of animal and plant names with palaeo-biological results (making homeland identification highly hypothetical); overestimation of evidence; weakness of semantic reconstructions. On the last point, Hegedüs argues, the Nostratic reconstruction of *\*moRE* 'body of water' does not justify Dolgopolsky's claim that the Nostratic people did not distinguish the sea from other bodies of water. Rather this distinction became redundant in daughter languages, and underwent semantic changes.



A. Keye's paper "The Current State of Nostratic Linguistics" is of an improper kind. As noted by several participants, it is frequently *ad hominem*, being only slightly camouflaged. Keye provides many quotes which totally distort the results of the generally very precise Nostratic research, such as "Ringe ... shows that the so-called Nostratic correspondences are not significantly different from the kinds that could arise purely by chance." Accordingly, for Keye "the systematic correspondences listed on pp. 102-5 [i.e., phonetic tables in NM] are far from convincing ..."

I can only say that the sound correspondences in the tables reflect not just 124 cognates as used in NM, but all 2300 entries of Dolgopolsky's Nostratic Dictionary.

Keye objects to my statement that pronouns (especially personal pronouns of the first and second person), being very stable, can be used as indicative of a genetic relationship. One can easily prove this by comparing Nostratic pronouns, which show *\*t'*- 'thou', to Amerind (Penutian, etc.) with *\*m*- 'thou'. Even more striking is the comparison of pronouns in Sino-Caucasian (= SC, to which the North Caucasian [NC] system is very similar) and, say, Amerind. We immediately see that the "Almosan-Keresiouan" languages of America are not Amerind, but Sino-Caucasian, or closely related to SC: cf. T: Sa[lish] *\*-ca*, *\*nV* : NC *\*zô*, *\*nV* : 'thou': Sa *\*ʔaxw*, *\*wV* : NC *\*ʔay/\*yu*, *\*wo*.

A. McMahon, M. Lohr, R. McMahon, in their paper "Family Trees and Favourite Daughters" try to show that IE reconstruction is influenced by better-known languages (like Latin). Accordingly, they consider Nostratic reconstruction to be influenced by IE reconstruction. They depend heavily on Ringe's purported falsifications of Nostratic data, and seem not to understand that Dolgopolsky's reconstruction is based not on 124, but on 2300 sets.

P. Michalove and A. Manaster-Ramer (the latter was absent due to health problems) discuss "The Use of Reconstructed Forms in Nostratic Studies." They show (with two words for 'fish') how distant reconstructions may depend on "low-level" reconstructions. For instance, Dolgopolsky has changed Illich-Svitych's Nostratic reconstruction *\*diga* 'fish' to an excessively complex *\*d[oT]giHU* by including Uralic *\*totke* in the set. But there is no *\*-oT-* in the other languages, and an implied reduplication inside the root is not typical of Uralic. Since Starostin reconstructs the Altaic root as *\*diog/kV* (formerly *\*dög/ki*), and IE shows *\*dghu*, the Nostratic form should be *\*digHU*.

R.L. Trask in his paper "Why Should a Language Have Any Relatives?" argues for the normalcy (vs. exceptionality) of language isolates and tiny families. He regards Nilo-Saharan and Khoisan not as families, but as "dustbin" groupings - "mere areal groupings sweeping up all the leftover languages that can't be fitted into the large language families adjoining them ... the overwhelming picture [in the Americas] is one of isolates and tiny families with two, three, or four members." What came relatively recently is massive areal diffusion and convergence. "When unrelated families converge strongly, the result may be an illusory 'family' ..."

Consequently, Trask objects to many sets in NM as too loose, etc. Trask does not reject Nostratic, though, but it seems too vast to him (see above). And he doubts the stability of unmarked consonants /*p*, *s*, *m*/, etc., though the lack of markedness may precisely be the cause of phonetic stability.

My own paper, "Nostratic Languages: 1) Inner Kinship vs. External Links; 2) Inheritance vs. Borrowing in Daughter Languages," lists some new results and theories in IE and Nostratic reconstructions (stop series; laryngeals; Nostratic labial stops, as per Starostin); obvious sets of archaic cognates both in Nostratic and Sino-Caucasian, as well as direct comparisons of Salishan

and SC/NC lexemes: e.g., Sa (\*)*t-q'aw?*- '2' : SC \**t'-?q'wE* '2'; Sa (\*)*c'ám*- 'bone' : NC \**c'wemV*; Sa (\*)*qUx*- 'horn' : SC \**qwVrHV* (unstable \**r*); Sa (\*)*cum*- 'eyebrow' : NC \**-c'wemV*. In the second part of the paper, some Nostratic and IE etymologies from NM were treated as doubtful or non-existent. (Usually, lexical borrowings were involved.)

D. Sinor criticizes Nostratic reconstructions in his paper "Provisional Remarks on the Nostratic Theory." There is nothing new or unexpected.

S. Starostin discusses Nostratic - Sino-Caucasian correspondences in his paper "Comments on A. Dolgopolsky's 'Nostratic macrofamily'." Starostin uses all 124 cognate sets from NM, rejecting 18 as based only on one family. He asserts that a low figure of AA cognates (only 47, compared with 65 for IE, 61 Altaic, 60 Uralic, 56 Dravidian) underlines that AA is a sister of Nostratic. He compares this figure with the 47 North Caucasian sets which are comparable with Nostratic: naturally, NC may be a sister of Nostratic, but not its daughter. (The low figure for Kartvelian roots - only 42 - is explained by the fact that there are too few Kartvelian roots known to us.)

R. Voigt ("Semitohamitic and Nostratic Comparisons") writes that "A. Dolgopolsky adheres to the method of comparative historical linguistics ... by relying on recurrent sound correspondences ... The Semitic material [in NM] is ... reliable and correct." Voigt makes some remarks on reconstructions by other scholars as well, adding that some convincing Nostratic comparisons "must be attributed to loan."

A. Vovin (who could not come because of illness) wrote on "Altaic Evidence for Nostratic," listing many Altaic reconstructions which he doubts. Both Dolgopolsky and Starostin accepted some of Vovin's criticisms, while rejecting others. (Starostin, with A. Dybo and O. Mudrak, is now completing work on his *Altaic Etymological Dictionary*.)

K. Zvelebil was not present, and his review of NM was read by colleagues. Zvelebil's objection to including Japanese and Korean in Altaic is contradicted by new evidence provided by Starostin in his book on Altaic (1992, in Russian), as well as in the forthcoming *Altaic Etymological Dictionary* (see the preceding paragraph). Zvelebil provides important analysis of many Dravidian lexemes. His approach to the Nostratic hypothesis seems to be that of considerable interest.

The Symposium book, with revised versions of the above-mentioned papers, may appear as early as January, 1999. C. Renfrew now plans a Sino-Caucasian Symposium for December 1999, based on Starostin's work on proto-Sino-Caucasian and its speakers. - V.

Shevoroshkin

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## ASLIP Business (1): E-Mail Survey

In order to communicate more quickly and efficiently with ASLIP members, we are requesting that ASLIP members who have an e-mail address inform us of that address. This may be done either of two ways: (a) with address public (with your permission to publish it in the Newsletter), or (b) with address restricted (used only for ASLIP business, and not published). So please e-mail us right away, designating category (a) or (b), and help us save time, money, and trees. Send the message to:

ASLIP Secretary, Alice Faber <faber@haskins.yale.edu>, and/or

ASLIP President, John D. Bengtson <john.bengtson@co.hennepin.mn.us>

In the future we plan to send the Newsletter via e-mail as well.

## ASLIP Business (2): a Reminder

Readers are reminded to pay their 1998 dues, if they have not done so already. Only those who have paid 1998 dues will receive the 1998 *Mother Tongue Journal*! If there is a question about your dues status, please contact ASLIP Secretary, Alice Faber. Please send payments to ASLIP Treasurer Peter Norquest. (See inside cover for addresses.)

## Editorial: What is Nostratic?

by John D. Bengtson

For readers of *Mother Tongue* who are historical linguists, or are otherwise already acquainted with paleolinguistics as reported in *Mother Tongue* newsletters and journals, the Nostratic concept is familiar. This introduction is aimed at newer *Mother Tongue* readers to whom the term Nostratic may mean little or nothing.

Basically, "Nostratic" is a concept or model of deep linguistic relationship, proposing that certain language families of Eurasia (and, in some versions, Africa) are related on a deeper level, and thus implicitly derive from a yet earlier language ("Proto-Nostratic"). All recent versions of Nostratic, as far as I know, include at least three families: Indo-European, Uralic, and Altaic. Thus "Nostratic," from Latin *nostras*, (gen.) *nostratis* 'of our country', an ethnocentric concept of the languages most closely related to "ours" (as members of Western European civilization).

The model of Nostratic which has been dominant in recent years, especially among Russians and other Eastern Europeans (but also influential in the West), was developed by two Muscovites: the late Vladislav Illich-Svitych, and ASLIP Council Fellow Aharon Dolgopolsky (now in Haifa, Israel). Illich-Svitych's classic Nostratic Dictionary (*Opyt Sravneniya Nostraticheskix Jazykov*) compares six families: Indo-European, Uralic, Altaic, Kartvelian, Dravidian, and Afro-Asiatic (= Hamito-Semitic). Dolgopolsky's recent Nostratic work, as reported at the recent conference in Cambridge, England, includes the same six families, with the addition of Yukaghir (a part of his Uralic), Korean, and Japanese. Sergei A. Starostin (another ASLIP Council Fellow, and a student of Dolgopolsky's) defines his Nostratic as "Dravidian (probably the earliest split off [the] family) and a core of more closely related families: Indo-European, Uralic, Altaic [including Korean and Japanese], Kartvelian. The inclusion of Eskimo-Aleut and Chukchee-Kamchatkan languages into Nostratic also seems quite probable ..." (For Starostin, Afro-Asiatic is itself a "macro-family" with a time-depth approximately equal to that of Nostratic, and as such is coordinate with Nostratic [and Sino-Caucasian], rather than a constituent of it.)

The Czech Long-Ranger and Nostraticist Václav Blažek divides the world's languages into eight macrophyla, of which one is Nostratic, with a membership as follows: 1.1 Afroasiatic (Semitic, Cushitic, Omotic, Egyptian, Berber, Chadic), 1.2 Elamite, 1.3. Dravidian, 1.4 Kartvelian, 1.5 Indo-European, 1.6 Uralic and Yukaghir, 1.7 Altaic (Turkic, Mongolic, Tungusic, Korean, Japanese), 1.8 Nivkh (= Gilyak), 1.9 Chukchi-Kamchatkan, and 1.10 Eskaleutan (= Eskimo-Aleut).

In the meantime, the American Allan R. Bomhard has been formulating his own model of Nostratic, which converges in large part with the Muscovite model, while differing from it in some details. In Bomhard's recent work, all six of Illich-Svitych's Nostratic families are included, as well as Chukchi-Kamchatkan, Eskimo-Aleut, Gilyak (= Nivkh), and Sumerian. Like Starostin, Bomhard sets Afro-Asiatic "apart as an extremely ancient, independent branch," and finds that "Indo-European, Uralic-Yukaghir, Altaic, Gilyak, Chukchi-Kamchatkan, and Eskimo-Aleut are more closely related as a group than any one of them is to Afroasiatic, Kartvelian, and Elamo-Dravidian."

Veteran American Long-Ranger Joseph H. Greenberg (see Peter Norquest's article in this issue) avoids the term "Nostratic," but his "Eurasianic" macro-family consists of Indo-European, Uralic, Altaic, Japanese-Korean-Ainu, Gilyak, Chukchi-Kamchatkan, and Eskimo-Aleut. For Greenberg, Afro-Asiatic, Dravidian, and Kartvelian are no doubt related to Eurasianic at some level, but he prefers not to include them in Eurasianic. Another significant difference between Greenberg's Eurasianic and the various versions of Nostratic enumerated above is Greenberg's inclusion of Ainu (see Norquest's article).

From the very beginning, the Nostratic hypothesis has played a vital role in ASLIP (and in *Mother Tongue*, before the founding of ASLIP). The embryo of *Mother Tongue* was Harold C. Fleming's circular letter dated November 3, 1986, in which he told of his keen intellectual enthusiasm for the Nostratic hypothesis, stimulated by his visit to Moscow in August of that year, at the Eighth International Conference of Ethiopian Studies. The title *Mother Tongue* for the newsletter (and subsequent journal) was invented by a transplanted Muscovite Nostraticist, Vitaly Shevoroshkin of the University of Michigan, who has played an active role in *Mother Tongue*/ASLIP from the beginning.

My own introduction to Nostratic was thanks to correspondence with Vitaly beginning in 1985. The next March I met Vitaly (along with several other prominent Long-Rangers and future ASLIP members: Raimo Anttila, Sheila Embleton, Carleton Hodge, Mark Kaiser, Mary Ritchie Key, Sydney Lamb, Saul Levin, Merritt Ruhlen, Stephen Tyler, Angela Della Volpe - sorry if my poor memory has left anyone out!) at the Rice University Symposium on the Genetic Classification of Languages. These two events in 1986: the Symposium, and the founding of *Mother Tongue* (later ASLIP), deserve much of the credit (at least in North America) for the growing interest in long-range linguistic research, and indeed for stimulating a vast amount of work in paleolinguistics since then.

What is the significance of Nostratic outside of the historical linguistic (paleolinguistic) field? In my opinion, it is significant that historical linguists - working independently of one another - have arrived at the same (or almost the same) model of deep linguistic relationship. At least in the beginning, Illich-Svitych and Dolgopolsky worked independently. Bomhard's Nostratic was also begun independent of the Russians, and Greenberg formulated his Eurasianic model according to his own methods, which differ in some ways from the Indo-Europeanist methods of most Nostraticists.

Note that the languages *not* included in Nostratic are just as significant as those that are included. No current version of Nostratic/Eurasianic includes certain other languages and families of Eurasia, namely Basque, North Caucasian, Burushaski, Yeniseian, or Sino-Tibetan, and indeed the same methods used in formulating the Nostratic model have found that these latter five families (+ Na-Dene) form a parallel macro-family, namely "Sino-Caucasian" or "Dene-Caucasian," familiar to readers of *Mother Tongue*.

Most of the paleolinguists mentioned above - certainly Greenberg, Shevoroshkin, Starostin, and Blažek - agree that northern Eurasia was originally populated (ca. 10,000-15,000 b.p.) by two distinct ethnic and linguistic types stretching from West to East: across the northern tier was the Nostratic/Eurasianic type (later to become Indo-European, Uralic, Yukaghir, Kartvelian, Altaic, Chukchi-Kamchatkan, [Gilyak], Eskimo-Aleut), while just to the south of this band of peoples they trace the prevalence of the Sino-(Dene-)Caucasian type (Basque, Caucasian, Burushaski, Sino-Tibetan). The Yeniseians originally occupied a large territory of central Asia closer to their Burushaski and Sino-



Tibetan linguistic cousins, and their intrusion into northern Siberia - between Nostratic (Uralic and Altaic) speakers - appears to be more recent, as does the incursion of the Na-Dene into North America. Subsequently, expansion of some Nostratic/Eurasiatic peoples (notably Indo-European and Altaic) forced some Dene-Caucasian peoples (notably Basque, Caucasian, Burushaski, Yeniseian) into refuge areas, where they managed to persist linguistically.

\*\*\*\*\*

### "What is Nostratic?" (Table by Roger W. Wescott)

**Language Group: Illich-Svitych Dolgopolsky Starostin Blažek Bomhard Greenberg**

<b>Indo-European</b>	+	+	+	+	+	+
<b>Uralic</b>	+	+	+	+	+	+
(-Yukaghir)						
<b>Altaic</b>	+	+	+	+	+	+
(Trk-Mng-Tng)						
<b>Kartvelian</b>	+	+	+	+	+	
<b>Dravidian</b>	+	+	+	+	+	
(-Elamite)						
<b>Chukchi-</b>			+	+	+	+
<b>Kamchatkan</b>						
<b>Japanese-</b>		+	+	+	+	+
<b>Korean</b>						
<b>Gilyak</b>				+	+	+
<b>Eskimo-Aleut</b>			+	+	+	+
<b>Afroasiatic</b>	+	+		+	+	
<b>Sumerian</b>					+	
<b>Ainu</b>						+
<b>Totals:</b>	<b>6</b>	<b>7</b>	<b>8</b>	<b>10</b>	<b>11</b>	<b>8</b>
<b>Average:</b>			<b>8.2</b>			

[Overall, Starostin now seems to be the most 'representative' Nostraticist. R.W.W.]

\*\*\*\*\*

### The "Far East" of Nostratic by John D. Bengtson

Although the Nostratic membership of Chukchi-Kamchatkan and Eskimo-Aleut is widely accepted, very few published Nostratic etymologies include words from these languages. (Blažek and Bomhard, for example, have made such comparisons; and, of course, Greenberg, in his forthcoming book on Eurasiatic.) I have made some notes of my own on these languages, for example:

**PRONOUNS:** As Alfred Kroeber said about the early Algic hypothesis, "The pronouns turn the trick, alone ..." Note, for example, Proto-Chukchi-Kamchatkan (PCK) \**mur* 'we' / \**tur* 'you' (pl.), with the familiar \*\**mi* / \*\**ti* (= Eng. *me* / *thee*) stems of Nostratic, reflected also in Eskimo *wi* 'I' / *ci* 'thou' (cf. Mongolic \**bi* / \**či*, respectively); Gilyak *ni* (if < \**mi*) / *t'i* (č*i*) 'I/thou'; Yukaghir *met* / *tet* 'I/thou'; Proto-Indo-European (PIE) \**me*-/ \**te*- 'me/thee' = PUralic \**me*-/ \**te*- 'I/thou', etc.

Nineteenth-century recordings of Chukchi-Kamchatkan pronouns (as reported by Greenberg, 1997) include Kamchadal *tu* ~ *tue* 'thou' and Chukchi *tua* 'thou' = PIE \**tu* 'thou'. Though now obsolete in the singular, the element \**tu* was pluralized as PCK \**tu-r* 'you (pl.)', parallel with PCK \**mu-r* 'we', the latter formed in the same way as Turkish \**bi-r* (Turkish *biz*; cf. North Kamchadal *bu-že* < \**mu-r*) 'we', Korean \**ú-rí* 'we', Amur Gilyak *me-r* 'we'.

The interrogative 'who' is PCK \**mki*, \**mkin*-, Proto-Eskimo-Aleut (PEA) \**ken* 'who': cf. PIE \**k<sup>w</sup>i*- 'what', \**k<sup>w</sup>o*- 'who' (dialectal Norwegian *kven* ~ *kem* 'who'); Uralic [Finnish] *ken* 'who'; Yukaghir *kin* id.; Turkish *kim* id.; Mongolic \**ken* id., etc.. (See also Greenberg's examples, cited in Norquest's article in this issue.)

## LEXICAL PARALLELS:

'name' (1): PCK \**ʔəljə*: cf. PIE \**nōmn*, \*(*o*)*nōmn* 'name', Hittite *laman*; PUralic \**nime*-; Yukaghir *niu* ~ *nim*; Japanese *na(mae)* < PJap \**náN* 'name'; (Chukchi-Kamchatkan frequently has the velar nasal *ŋ*, corresponding to other nasals elsewhere: cf. 'many', below)

'name' (2): PEA \**atə*: cf. Turkic \**ât* (Turkish *ad*) 'name'

'many': PCK \**ɲinvə*; Aleut *amnaghuaq* 'many, much': cf. PIE \**men(e)gh-* 'many'; Uralic (PFenno-Ugric) \**mon-* id.; Old Japanese *mane-* 'much, many'; Turkic (Chuvash) *mōn* 'great', etc.

'old': Eskimo *utu-*, PCK \**ʔvitwə*: cf. PIE \**wet*-/ \**ut*- 'year, old' (Hittite *wet* 'year', Latin *vetus* 'old', Sanskrit *vatsa-* 'year', etc.); Uralic: Finnish *vuosi*, *vuote-* 'year', South Hanti *ot* 'year'

'tongue': PCK \**jilvə*, PEA \**ʔulvi* (Eskimo *ulu*): cf. Turkic \**dil*/\**dil* 'tongue'; Tungusic \**dilga-* 'voice'

'ear': PCK \**vilvV* (? < \**k<sup>w</sup>ilwV*): cf. PIE \**klew-* 'to hear, listen; ear' (Icelandic *hlust* 'ear', Gothic *hliuma* 'ear, hearing', English *listen*, *loud*, Slavic *slovo* 'word', etc.); Uralic \**kûle-* 'hear'; Turkic \**kul-kak* 'ear'; Mongolic \**kul-ku* 'earwax'; Korean \**kúi* 'ear'; Japanese *ki-k-u* 'to listen'; Kartvelian *q'ur* 'ear'

'nose': PCK \**χəŋ*; PEA \**qəŋa-*: cf. Mongolic \**kaŋ-bar* 'nose'; Tungusic \**xoŋo-* 'nose'; Korean \**kóh* 'nose'; etc.

'heart': Eskimo *qatə* 'heart, breast': cf. PIE \**kerd-* 'heart'; Kartvelian \**mk'erd-* 'chest, breast'

'breast': Eskimo *məlu* '(female) breast': cf. PIE \**melg-* 'milk, to milk'; Uralic \**mäl̥ye* 'breast'; Yukaghir *melu(t)* 'breast', *momolo* 'milk'

'leg': Eskimo *niyu*: cf. PIE \**nog<sup>w</sup>h-* 'foot, toenail, fingernail' (Russian *noga* 'foot, leg', etc.); Yukaghir (South) *nugon* 'hand', *nugen* 'finger'

**'brother-in-law':** Chukchi *cakiget* '(man's) brother-in-law'; Eskimo (Inupik) *sagqiaaq* 'brother-in-law'; cf. PIE *\*swek-* (Old High German *swâgur* 'brother-in-law', Latin *socer* 'father-in-law', *socrus* 'mother-in-law', etc.)

**'night':** PCK *\*nki* 'night', Kamchadal *nuku-lu* 'dark'; Eskimo *unuk* 'night, evening' : cf. PIE *\*nokt-* 'night', Greek *νύχτα* 'at night'; Uralic (PFU) *\*ñukkV* 'to sleep'; Tungusic: Evenki *nökuket-* 'to doze', etc.

**'moon':** Eskimo *tunqi* 'moon', PCK *\*tanke-* 'moon, to shine': cf. Japanese *tsuki* (PJap *\*tükúi-N*) 'moon'; IE (Icelandic) *tungl* 'moon'

**'sun':** Eskimo *mac(j)a* 'sun'; Kamchadal *mazamaz* 'vernal' ('sun-time') : cf. PIE *\*mên(ə)s-* 'moon' (Sanskrit *mâs-*, Latin *mêns-is*, English *moon*, etc.); Kartvelian *\*mZ-e* 'sun' (Georgian *mze*, Laz *bža*, etc.)

**'lightning':** Eskimo (Yupik) (kiniχ-) *pilaq* 'lightning' : cf. PIE *\*bhelg-/bhleg-* 'to shine, glitter; lightning' (Latin *fulg-us* 'lightning', Swedish *blixt* [*blik-st*] id., etc.); Altaic *\*balkV-* (Turkish *balk-iz* 'lightning', etc.)

**'fire':** PEA *\*ʔik(i)nə* : cf. PIE *\*Hng-n-i-* 'fire' (Sanskrit *agní-*, Latin *ignis*, Russian *ogon'*, etc.); Uralic *\*enkV* 'to burn': Eastern Mari *en* 'fire'

**'stone':** PEA *\*kew-* 'stone', PCK *\*xəvxə*, Kamchadal *kwa-l*, *kow* 'stone' : cf. Uralic (PFU) *\*kiwe-* (Finnish *kivi*) 'stone'; Kartvelian *\*kwa-* 'stone'

**'snow':** Eskimo *ciku* 'snow, ice': cf. PIE *\*sneigʷh-* 'snow'; Uralic (PFU) *\*šüje* 'wet snow' (Norwegian Saami *suovve* 'wet snow', Finnish *hyy* 'ice, melting snow', etc.); Tungusic *\*sünjü* 'hoarfrost, snow'; Japanese *šimo* '(hoar)frost'; etc.

**'mushroom':** PCK *\*pχon* : cf. PIE *\*s-bhongʷo-/s-gʷhombo-* 'mushroom' (Latin *fungus*, Greek *σπόγγος* ~ *σφόγγος* 'mushroom, sponge', Swedish *svamp* 'mushroom', etc.)

**'deer' (1):** PCK *\*ʔəlwa* 'caribou' : cf. PIE *\*Hel-n-* 'deer' (Lithuanian *elnis*, Russian *olen'*, English *elk*); Yukaghir *ile* ~ *ilel* ~ *ilbe* 'reindeer' (*il-be* = PIE *\*Hel-n-bho-* > Greek *ἐλαφος* 'deer'); Mongolic *\*ili* 'young deer, fawn', etc.

**'deer' (2):** PCK *\*qorə* 'deer' : cf. PIE *\*ker-w-* 'deer, horned animal' (Latin *cervos* 'deer', Eng. *hart*, Old Prussian *sirwis* 'chamois'; cf. PIE *\*ker-n-* 'horn', Hittite *kar(a)war* 'horns', etc.)

**'wolf':** PCK *\*lχəx-nə* 'wolf', *\*yəlχə* 'wolverine, wolf', *\*lχeymV* 'puppy' : cf. PIE *\*wlkʷo-* 'wolf', Greek *λύκος* 'wolf', etc.; PIE *\*lu(n)k-* 'lynx', Old Irish *lug*, etc.; Kartvelian *\*lek'w-* 'puppy, dog'; Uralic *\*lo/kk/a* 'fox'; Mongolic *\*nokaj* 'dog'; Tungusic *\*lukV* 'lynx' (To orthodox Indo-Europeanists, the words for 'wolf' and 'lynx' are distinct; this etymology may indicate a common origin of what later became distinct roots referring to predatory canids and felids.)

**'no(t)':** Chukchi *na-qam* 'no', PEA *\*na-* 'no(t)': cf. PIE *\*ne-/n-* 'not'; Uralic: Hungarian *nem* 'not', etc.; Yukaghir *ñe* 'no-' (in pronouns); Turkic (Chuvash) *an* 'not'; PKorean *\*an-* 'not'; Kartvelian *\*nu* 'not' (prohibitive); Japanese *nai*, *-na-* 'not'; etc.

**'to carry':** Eskimo *aγ-* : cf. PIE *\*wegh-* 'to carry' (Eng. *weigh*, etc.); Yukaghir *wogie-* 'to lead, carry, drag'; Uralic *\*wêγV-/wîγe-* 'to carry, transport'

**'to bring':** PEA *\*peru* 'to bring, to go': cf. PIE *\*bher-* 'to bear, carry, bring' (Latin *fer-*, Greek *φέρ-*, English *bear* (v), *bring*, etc.); Turkic *\*bêr-* 'to give'; PJap *\*píri-p-* 'to gather, collect' = Modern Japanese *híro-u-*, etc. (Illich-Svitych distinguished Nostratic *\*bari* 'to

bring' from \*be/rH/u 'to give', but both are probably ablaut variants of the same original root.)

This is just a sample. We look forward to Greenberg's book on Eurasiatic, and also for the Nostraticists to make more of this evidence explicit.

(I have made much use of the works of Oleg Mudrak and Morris Swadesh, as well as those of Greenberg and the Nostraticists cited above. Any errors or omissions are my fault.)

\*\*\*\*\*

Further reading:

Greenberg, Joseph H. 1997. "Some Grammatical Evidence for Eurasiatic, Especially Pronominal," in *The Twenty-Third LACUS Forum*, ed. by Alan K. Melby, pp. 59-65. Chapel Hill, NC: LACUS.

----- (forthcoming) "The Convergence of Eurasiatic and Nostratic."  
[see Peter Norquest's article in this issue]

Kaiser, Mark, and Vitaly Shevoroshkin. 1988. "Nostratic." *Annual Review of Anthropology* 17: 309-329.

Ruhlen, Merritt. 1994. "An Overview of Genetic Classification." In *On the Origin of Languages: Studies in Linguistic Taxonomy*, 9-38. Stanford University Press.

### Announcement by Vitaly Shevoroshkin

I have prepared three volumes on distant relationships of languages: 1) *Historical Linguistics and Lexicostatistics* (with a very long paper by I. Peiros on the classification of Australian languages, using Starostin's version of lexicostatistics; a translation of a long paper by Starostin [very important], etc.); 2) *Remote Relationships of Languages and Deep Reconstruction*; and 3) *Ancient Cultures as Seen through Language Reconstructions*. (Approximately 400 pages in each volume.)

The first book is fully prepared, and volumes 2 and 3 need some reprinting ([typesetting] which is being done by my students here). The Brockmeyer Verlag in Bochum [Germany, publisher of the BPX series, including *Reconstructing Languages and Cultures*, and several other volumes resulting from the Language and Prehistory Symposium in 1988: Ed.] has gone bankrupt, so I have to look for another publisher. Maybe some of our colleagues could advise something. Pity, I didn't have disks (except for a few papers), only printouts. (Bochum didn't use disks, just copies.)

[How about it, colleagues? If you have ideas, please write to Vitaly at the Department of Slavic Languages, 3040 Modern Languages Building, University of Michigan, Ann Arbor, MI 48109-1275. Ed.]



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## ASLIP Business (2): a Reminder

Readers are reminded to pay their 1998 dues, if they have not done so already. Only those who have paid 1998 dues will receive the 1998 *Mother Tongue* Journal! If there is a question about your dues status, please contact ASLIP Secretary, Alice Faber. Please send payments to ASLIP Treasurer Peter Norquest. (See inside cover for addresses.)



# Mother Tongue Newsletter No. 31

## Fall 1998

### TABLE OF CONTENTS

<b>INTRODUCTION</b>	p. 1
<b>OBITUARY: Carleton T. Hodge</b> by Saul Levin	1
<b>ASLIP BUSINESS</b>	1
<b>ARCHAEOLOGICAL AND BIOGENETIC NEWS</b> Ed. by Alvah M. Hicks and G.R. Foote	2 - 19
CROSSROADS TO AND FROM THE AMERICAS	2 - 5
POLYNESIA	5 - 6
ASIA	6
AFROASIATIC (see also p. 26)	7 - 9
EUROPE	9
AFRICA	9 - 10
THE AMERICAS	10 - 11
SETTLING THE NEW WORLD (see also p. 27)	12 - 17
EARLY MAN	17 - 19
<b>NOTES ON THE INTERNET</b>	20 - 24
<b>PALEOLINGUISTIC NEWS</b>	25 - 32
GREENBERG'S VISIT TO ARIZONA by Peter Norquest	25 - 26
MCCALL ON AFROASIATIC (see also p. 7)	26
RUHLEN IN <u>LA RECHERCHE</u>	27
NICHOLS ON SETTLEMENT OF THE AMERICAS (see also pp. 14-15)	27
CONFERENCES AT CAMBRIDGE	28
1998 SYMPOSIUM ON NOSTRATIC AT CAMBRIDGE by Vitaly Shevoroshkin	28 - 32
<b>ASLIP BUSINESS</b>	32
<b>EDITORIAL: What is Nostratic?</b>	33 - 38
<b>ANNOUNCEMENT: Vitaly Shevoroshkin</b>	38