MOTHER TONGUE

NEWSLETTER OF THE ASSOCIATION FOR THE STUDY OF LANGUAGE IN PREHISTORY

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Editor (December): Mark Kaiser

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

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Professor Dr. Ekkehard Wolff
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Mittelweg 177,
D - 2000 Hamburg 13,
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It is a pleasure to become part of the production of Mother Tongue, although I am but the nominal editor for this issue. Please send your letters and mss. to Mark Kaiser, 1002 South Fell, Normal, IL 61761 for inclusion in future issues. I join the Board in wishing you a prosperous and productive New Year. -MK.

OBITUARY

Samuel Noah Kramer was born שמחה נוח בן בנימין on September 18, 1897, and departed for the "Land of No Returning" on November 26, 1990. He was one of those very rare scholars who had the fortune to have a great, lasting impact both on his field of research and on the larger public's knowledge of it.

His autobiography is written in the same first person which was familiar from his popular work on Sumerian literature. Sam's warmth and commitment to his work imbued his writing with a special charm. Part of that charm was due to his happy marriage to Milly (née Tokarsky), and was inherited by their children, Daniel and Judith.

His scholarly studies and publications began with linguistic topics: a dissertation on "The [Akkadian] Verb in the Kirkuk Tablets" and an article in support of Arno Poebel's work on the forms of verbs in Sumerian texts from ancient Lagaš, followed by a monograph which developed and expanded on Poebel's insights. Then, in 1933, Edward Chiera died before he could publish his sketches of Sumerian literary tablets, and the thirty-six-year-old Kramer was given the responsibility of preparing them for publication. Kramer published the two volumes of Chiera's "hand copies" within a year, arranging the introductory notes, and seeing the books into print. From that day on, he dedicated his scholarly life to putting those tablets into the context of all the excavated Sumerian literary texts, continuing to devote his efforts to *Sumerian Epics and Myths* and *Sumerian Texts of Varied Contents.* (The cuneiform tablets were at the University Museum of the University of Pennsylvania, which Kramer eventually joined after seeking a position for some time.)

Since the cuneiform tablets inscribed with ancient Sumerian literary texts are commonly broken and damaged, it is only thanks to our multiple (students') copies of many of them that we can piece together the stories, hymns, dialogues, songs, and the like that they record. Reuniting the broken parts of a single tablet, identifying different copies of same text and recalling parallel passages in other compositions are important preliminaries to understanding such texts. Kramer's efforts at putting together these obdurate jig-saw puzzles were very fruitful, despite the fact that so many pieces of them are now lost to the ravages of time. He gave nearly sixty years to the tablets, and in the course of his work, he restored Sumerian literature to twentieth-century readers.

His mission led him to all the important collections of cuneiform tablets in the world. He started his search where the tablets from ancient Nippur had been sent by their nineteenth-century excavators: Istanbul, Philadelphia and Jena, but he continued it at Moscow's Pushkin Museum, the Louvre, the Ashmolean, and the British Museum.

Kramer's enthusiasm drew inexperienced volunteers to help sketch cuneiform tablets, and he co-authored works with non-cuneiformists. He enlisted artists, poets, and folklorists to help him bring the wider world to Sumerian literature. His commitment eventually brought him to see *his* texts in the wider picture of Sumerian cultural achievement and history, and to present his own synthetic sketch of their civilization.

Sam's generosity to younger scholars was legendary. Realizing early that one person couldn't even publish all of the texts, let alone penetrate their meaning, he welcomed others to the task, and he shared his time with them and gave them his help.

Thorkild Jacobsen (of The University of Chicago, and later of Harvard) can be described as Kramer's only American rival before Kramer retired from the University Museum; they were cordial adversaries. Jacobsen came from Denmark, and is a cool, analytic, sophisticated, Scandinavian perfectionist, a bit aloof. Kramer was more relaxed and "homier" (if such a term may be applied to one who travelled the world spending his time on the study of Sumerian). They both had done post-graduate work with Poebel in Chicago, but Kramer was a Jew who believed in "positivism" and science, while Jacobsen was a Lutheran, committed theologically, if not practically. The humanist Jacobsen emphasized our differences from Sumerian culture, while Kramer saw what we have in common with the Sumerians. Despite the differences in their personalities and approaches, there was a deep mutual respect, just as there were long detailed reviews of one another's publications showing their different understandings, and collaboration. Kramer did not shy from sending his reconstructions of Sumerian literary works to other cuneiformists before they were published, hoping to learn from their insight. He commonly published their contributions as appendices to his own work. He did this with Jacobsen many times, and anyone who reads his scholarly articles will find such appendices listing the names of Adam Falkenstein and Benno Landsberger, giants of the last generation, and of Kramer's student Jacob Klein, among current Sumerian litterateurs.

Even though I had written my dissertation under Jacobsen, Kramer welcomed me when I came to Philadelphia to work on the Sumerian Dictionary Project under Åke W. Sjöberg, his successor as Clark Research Professor at the University of Pennsylvania. He benefited me not only through his avuncular nature, but by opening his private files, and even by helping me to get support for my work, although it differed from his.

Kramer's scholarly integrity went far beyond the call of duty. One must admire what he did in the *Bulletin of the American Schools of Oriental Research* in 1966. Kramer had maintained, against all others in the field, that the god Dumuzi was not a dying and rising god. That he went to the netherworld, but didn't return. Adam Falkenstein reviewed a volume of cuneiform texts and read a line in "Inana's Descent to the Netherworld" which proved Kramer mistaken. Kramer didn't wait for a footnote to concede or let his contention die unnoticed. He published a short article, and wrote that Falkenstein's interpretation "helps to correct a serious misinterpretation, on my part, of the denouement of the myth." This integrity and modesty, that of a scholar wise enough to know that perfect understanding is not possible, that all anyone can really do is advance knowledge, not complete it, pervaded Sam's work.

A publication series at the University of Pennsylvania, and the Institute of Assyriology at Bar-Ilan University in Israel have been named after him. Sam left us, however, an even greater monument to his singular devotion to Sumerian compositions. I don't mean his books and articles, but a continuing, growing field, a field of interest not only to scholars, but to students and creators of literature itself.

Stephen J. Lieberman
Philadelphia, Pennsylvania

F L A S H ! F L A S H !

Just as we go to press -- some late-breaking news arrives.

Dr. Antonio Torroni, Ass't. Professor of Anthropology at Emory University, Atlanta, Georgia (USA), speaking for Douglas Wallace and his colleagues, gave us (by phone) valuable new information. The reader will please view this against the background of the inquiries made by A. Hicks and H. Fleming to obtain Douglas Wallace's reactions to Ward et al (see above).

The Wallace group was able to react to the Ward group's research because their own research was comparable and had reached fruition. They were not working simply to test Ward et al's hypothesis. The two groups have serious interest in each other's research; relations are friendly. Moreover there is not great difference between the two groups in the assumptions they make or how they work. The differences lie primarily in the unfortunate choice of the Nootka to represent Amerindians plus probable errors in calculations. Technical details are forthcoming. See below.

What we have to report here is:

(a) The conclusions of Ward et al are not now tenable, says Torroni.

(b) The Wallace group will report their full research in GENETICS, an important journal for biogenetic reports, soon. It's In Press now. Since there are 12 authors, we will call the article Torroni et al.

(c) Basically, Torroni et al confirm the basic hypothesis that American Indians display mtDNA patterns which presuppose four (4) founding lineages and conform to the expectations of bottleneck theory, i.e. a population under constraint experiences genetic drift or to put it another way only a portion of a larger populations goes through or makes it through a bottleneck. The resultant new small population is a selected group, a sample if you will, of the larger older population. In this sense American Indians were those Asians who once passed through a bottleneck (the Bering Straits and/or a canoe trip) and became the ancestors of the modern native Americans. We could call those ancestors the proto-Amerind speakers.

(d) A Na-Dene mtDNA pattern is now known. Based on the Tlingit of wet temperate maritime Alaska, the Dogrib (Athapaskans of the Slave & Hare group) of frigid northwest Canada, and the well-known Navaho of the arid southwestern USA, it is distinct from the Amerindian pattern and it lacks the marker genes now known to be associated with Amerindian populations' mtDNA. The Na-Dene were founded by an immigration different from Amerinds.

(e) The rates of mutation which Douglas Wallace had promised to calculate (in MT-13) have now been realized and it is possible to date the Amerindian versus the Na-Dene founding populations. The Amerindian dates range from 20,000 to 40,000 BP, while the Na-Dene range from 5200 to 10,000. That is very exciting for a number of reasons. For one it suggests that MacNeish, Dillehay and others are correct; the dates for settling the New World probably are around 30,000 BP. For a second it suggests that proto-Amerind is a datable entity -- around 30,000 years old ± 10,000. Wow!

(f) There is a fascinating indication of another immigration around 15,000 BP which has a striking correlation with the Clovis point horizon on which the 'traditional" archeological dates were based. This is an Amerindian phenomenon, not necessarily associated with migration from Asia and, for now, not associated with any sub-phylum of Amerind. Wow again! Like the Plains Indian culture area, Clovis might be a technical, economic and cultural conjoining of different groups, based on big game hunting. Hmm?

MITOCHONDRIAL DNA: STILL IN THE NEWS

Two important new studies involving mitochondrial DNA (mtDNA) and human prehistory are reported here. One is a substantial confirmation of Rebecca Cann's earlier work (with Mark Stoneking and Allan Wilson). The other is a substantial disconfirmation of Douglas Wallace's earlier work on American Indian prehistory. However, we understand that Douglas will contest the conclusions.

The first study was reported in SCIENCE, Vol. 253, 27 September, 1991, pp.1503-1507; that makes it a major article in that publication. Entitled "African Populations and the Evolution of Human Mitochondrial DNA", it was authored by Linda Vigilant, Mark Stoneking, Henry Harpending, Kristen Hawkes, and Allan C. Wilson. Except for the late Professor Wilson and Professor Hawkes who is in the Dept. of Anthropology, University of Utah, Salt Lake City, Utah 84112, the rest are in the expanding and rather exciting Dept. of Anthropology, Pennsylvania State University, University Park, Pennsylvania 16802, having moved there from Berkeley, California. The formal abstract of the article follows:

> "The proposal that all mitochondrial DNA (mtDNA) types in contemporary humans stem from a common ancestor present in an African population some 200,000 years ago has attracted much attention. To study this proposal further, two hypervariable segments of mtDNA were sequenced from 189 people of diverse geographic origin, including 121 native Africans. Geographic specificity was observed in that identical mtDNA types are shared within but not between populations. A tree relating these mtDNA sequences to one another and to a chimpanzee sequence has many deep branches leading exclusively to African mtDNAs. An African origin for human mtDNA is supported by two statistical tests. With the use of chimpanzee and human sequences to calibrate the rate of mtDNA evolution, the age of the common human mtDNA ancestor is placed between 166,000 and 249,000 years, These results thus support and extend the African origin hypothesis of human mtDNA evolution."

The original Cann, et al study had been criticized extensively and/or rejected by some biogeneticists and paleoanthropologists. Vigilant, et al seek to answer those criticisms, in part to resolve or clarify "conceptual issues" and in part to change the design of the study so as to address the criticisms. One conceptual issue was that "some biologists were uncomfortable with the idea that all of the maternal lineages of a species must trace back to one female member of some ancestral population." Drawing upon the theoretical work of M. Kimura (THE NEUTRAL THEORY OF MOLECULAR EVOLUTION, NY, 1983), the authors argued that "the variation present within a population at any mtDNA or nuclear DNA site must ultimately trace back to a single common ancestral nucleotide at that site. . . This ancestral nucleotide was necessarily present in a single individual in that ancestral population. . . Although this single individual would of course have had ancestors that were identical at that DNA site, our focus is on the most recent of these ancestors, referred to as the last common ancestor or more simply the common ancestor. For DNAs that recombine (such as nuclear DNA), the geneological histories for different sites do not have to be identical, so that the variation at different DNA sites most likely traces back to different ancestral individuals."

A second conceptual issue was that "the last common mtDNA ancestor identified by genealogical analysis (as described above) might represent an early population in which there were not just one female but rather many females bearing the ancestral mtDNA type. Although this appears to be correct, the time required for the ancestral mtDNA type to rise in frequency from one individual to many is likely to have been brief (a few hundred to a few thousand years) compared to the time elapsed since the common ancestral mtDNA type rose (about 200,000). Hence the time of common ancestry estimated by genealogical analysis closely approaches the true time."

The design changes were meant to answer criticisms that (a) Cann, et al "used an indirect method of comparing mtDNAs, namely restriction analysis", (b) that they "used a small sample made up largely of African Americans to represent native African mtDNAs", (c) that they used an "inferior method (that is, the midpoint method) for placing the common mtDNA ancestor on the tree of human mtDNA types", (d) that they gave "no statistical justification for inferring an African origin of human mtDNA variation, and (e) that their "calibration of the rate of human mtDNA evolution" was inadequate. Such scientists as Milford Wolpoff, L. Excoffier, A. Langaney, P. Darlu, P. Tassy, Jim Spuhler, N. Saitou, K. Omoto, J. Krüger, and F. Vogel had published such criticisms. Vigilant, et al believe that all those criticisms have been adequately answered.

Perhaps the clearest answer was provided by the African samples which reinforced the conclusions based on black Americans, while also extending them. The reason has always been rooted in the history of American 'miscegenation' where gene flow has overwhelming been from north European males to African women and not the other way around. The reason was black slavery, of course. It does not mean that African American women lack European genes; they have just as many as their male relatives. Rather the case is that European males could not pass European mtDNA to anyone, even their pink spouses.

In a footnote the authors stress that they are NOT saying that "all genes in contemporary humans are derived from a single female ancestor who lived in an African population some 200,000 years ago". It is true that all mtDNA genes can be derived from that African lady but few of the other "genes or biological traits that define modern humans" can be derived from her. One would like very much to have the logic of this kind of conclusion spelled out a great deal more than it has been.

The Africans included Eastern Pygmies (possibly Mbuti), Western Pygmies (probably Babinga), !Kung (San of Khoisan, Bushmen), Yorubas (Nigeria), Herero (Bantu of Botswana), Naron (Khoi of Khoisan, Hottentots), and Hadza (north Tanzanian Khoisan, northern Bushmen). It was a feat to get data on the Hadza, since much of that has been a closely guarded secret; it was obtained from <u>hair</u> collected by Hawkes! There were also Papuans, native Australians, Asians (east), and Europeans. Of the 189 people sampled, 121 were Africans. The diversity among the Africans eclipsed that among or between non-Africans. Neither the Hadza nor the Naron appeared to be close to the !kung as none of them were to either group of Pygmies who were also not at all close to each other. These are all very ancient chips off the common human block. Nor are they close to West Africans, although the history of the slave trade was reflected in the closeness of some Yoruba and black Americans. We should also remember, as I keep repeating, that some areas of considerable diversity in Africa are <u>still</u> untouched, especially the southern Sudan and western Ethiopia.

Vigilant, et al were able to determine the most likely place of origin of modern humanity by using the two methods. First, the famous <u>outgroup rooting</u> method located our female ancestor in Africa. The reader will recall that such a method was at issue in the study by Verne Shoemaker and colleagues, reported in MT-13. Let us hear more on the rationale for this:

"TREE ANALYSIS. A genealogical tree . . relating the 135 mtDNA types was built using the parsimony method . ., in which a branching network is constructed . . to minimize the number of mutations required to relate the types. To convert the resulting network into a tree, the ancestor or root must be placed, which requires additional information or assumptions." (Because Cann et al used a method which assumed the same rate of mutations for African as for others then the tree might be false -- suppose Africans have a faster rate. Etc.)

"The outgroup method . . is a preferable method of rooting a tree because it does not rely on the assumption that the rate of evolution is the same in all lineages. This method uses a sequence from another species (the "outgroup"), such as an African ape, to place the human mtDNA ancestor on the network. The outgroup attaches to the network relating the human mtDNA types at the position that minimizes the total number of mutations in the tree. The point of attachment is the position of the human mtDNA ancestor on the tree. Although Cann et al . . could not use the outgroup method because high-resolution restriction maps of African ape mtDNA were not available, a control region sequence from a common chimpanzee has now been published . . , and this chimpanzee sequence was used to root the tree in Fig. 3 . ." (their immensely complex family tree drawn in a great circle -- HF)

On the tree the 'outgroup rooting' produced two primary branches, "one consisting of six African mtDNA types, and the other consisting of all of the remaining mtDNA types, including many African types. This is exactly the pattern found before, based on restriction analysis, which led to the original hypothesis of an African origin .. Furthermore, the next 13 branches (consisting of 16 types) lead exclusively to African types. These 14 deep African branches provide the strongest support yet for the placement of the common human mtDNA ancestor in Africa . ." (By analogy this is similar to Na-Dene where all primary branches are located on the Northwest Coast and the rest or sub-branches are spread out over a wide area of western North America. -- HF)

Two other tests of statistical significance comprise the second method. One is called the "Winning Sites method" and the other the "Geographic states method". In short they find that the top heavy weighting towards Africa is most significant, far beyond chance.

Recently, Mark Stoneking discussed the Vigilant et al paper, at the University of Pittsburgh. The most noteworthy thing to report of his talk is his current work on New Guinea and Australia -- as yet unfinished. One recent result has been that mtDNA analyses suggest that the separation between Austronesians and Papuans (so-called 'NAN' or non-Austronesians) is as much as 115,000 years old, while that between Australians and Papuans is almost as old. This might mean that their original separations were in or near Africa and go back to the original dispersal of modern humanity out of Africa. Whether that is true or not, another indication is that the 'Australoids' had already become distinct from each other when they arrived in Australia and New Guinea. We must await Mark's formal written publication for secure confirmation of these dates. It is possible that this reporter misread Mark's diagram. (Oh, no!)

The second study was to be reported in the PROCEEDINGS OF THE NATIONAL ACADEMY OF SCIENCE. My copy was marked In Press but it is now out. Thanks to Alvah Hicks (Ojai, California) for sending it and other interesting papers. Unfortunately, the volume and page numbers are unknown to me. But the year of publication is 1991 and the month November or December. The study is entitled "Extensive Mitochondrial Diversity within a Single Amerindian Tribe". The authors are R.H. Ward, Barbara L. Frazier, Kerry Dew-Jager and Svante Pääbo; all are at the Department of Human Genetics, School of Medicine, University of Utah, Salt Lake City, Utah 84132, USA, except for Svante Pääbo who is at the Department of Zoology, University of Munich, Luisenstrasse 14, D-8000 Munich, GERMANY. (The last scholar has been associated with the determination of DNA types from the remains of ancient humans; he has had his moment of fame in the New York Times for that reason and may be someone European colleagues might want to contact. And by the by European experts in the same thing are supposed to have examined the 'mummy' of the 4000 year old (Bronze Age) man found last summer in the Alps. Does anyone know where their findings will be/have been published? Maybe they could wake him up and see if he spoke old Celtic or proto-Italic!)

Because we hope to get Douglas Wallace or someone else to peruse the technical aspects of this report -- so very relevant and crucial to Amerind prehistory -- we will confine this summary to their Abstract and conclusions. The Abstract reads, as follows:

> "Sequencing of a 360-nucleotide-segment of the mitochondrial control region for 63 individuals from an Amerindian tribe, the Nuu-Chah-Nulth of the Pacific Northwest, revealed the existence of 28 lineages defined by 26 variable positions. This represents a substantial level of mitochondrial diversity for a small local population. Furthermore, the sequence diversity among these Nuu-Chah-Nulth lingeages is over 60% of the mitochondrial sequence diversity observed in major ethnic groups such as Japanese or Sub-Saharan Africans. It was also observed that the majority of the mitochondrial lineages of the Nuu-Chah-Nulth fell into phylogenetic clusters. The magnitude of the sequence difference between the lineage clusters suggests that their origin predates the entry of humans into the Americas. Since a single Amerindian tribe can contain such extensive molecular diversity, it is unnecessary to presume that substantial genetic bottlenecks occurred during the formation of contemporary ethnic groups. In particular, this data does not support the concept of a dramatic founder effect during the peopling of the Americas."

The tribe is/are the Nootka, a famous Northwest Coast people; their language classified as Wakashan by everyone. Kwakiutl is the other famous Wakashan language and tribe. They and the Salish and a few others formed the Mosan group which some linguists from Sapir onward linked with Algonquian and Ritwan in a larger group called Almosan. Algonquian and Ritwan by themselves are a sub-group which is presided over by Ives Goddard who calls them Algic, a term coined by his teacher, Karl Teeter, and accepted by Greenberg. At deeper and more controversial levels the Nootka belong in the Almosan branch of the Almosan-Keresiouan larger branch of the Northern Amerind sub-phylum of Amerind -- in the Greenberg classification.

The reader may recall from MT-12 that Swadesh -- an authority on the Mosan languages -- classified Wakashan and Kutenai (another branch of Almosan) next to Na-Dene and the rest of his Vasco-Dene, i.e., making Nootka and Kwakiutl logically closer kin to Na-Dene than to Algic and the other Amerind languages (of Greenberg). This was remarkable at the time. Now, if the reader will contemplate culture area maps and geography, she will note that the Wakashan live at the primary interface of Na-Dene and Almosan in North America. The Haida, Tlingit, and Eyak live just up the coast from the Bella Bella and Kwakiutl; Athapascans are nearby in the Canadian interior. The Wakashan also participated in the Northwest Coast culture area with Haida and Tlingit especially, but others too. That culture area was world famous, of course, but also, according to Harold Driver (INDIANS OF NORTH AMERICA, Chicago 1964, p.15), "The culture of this area competes with that of the Arctic as the most distinctive or the most foreign of aboriginal North America. This is due partly to the geographical environment but also to its history, which shows much evidence of contact with Asia." Finally, most of the Northwest Coast peoples practiced matrilineal descent, including most of the Na-Dene and some of the Wakashan, while their Almosan kin farther south and east tended to be bilateral in descent. Please bear these little details in mind. They might explain what happened here.

Since their "Conclusions" basically repeat their Abstract, we will use their penultimate summary as their conclusions. This is called "Implications for the peopling of the Americas" and it says:

> "It seems reasonable to assume that to a first approximation, the tribal groups that inhabited Beringia during the last glaciation and which eventually migrated to the Americas were not too dissimilar from the contemporary (Nootka - HF) in overall population size and underlying band structure. Consequently, the substantial mitochondrial divergence found with the (Nootka -- HF) argues against the notion, based on low resolution restriction site analysis (Wallace and colleagues work -- HF) that the formation of the Amerindian ethnic group was accompanied by a substantial genetic bottleneck. Our conclusion that the founding tribal groups introduced considerable genetic variability is borne out by the demonstration that the (Nootka -- HF) contain lineage clusters that predate the colonization of the Americas. Given the amount of temporal divergence between the clusters, a substantial amount of this ancestral sequence diversity must have been present in the populations that colonized the Americas during the last glaciation. Preliminary analyses of sequence data for the same mitochondrial segment from other Amerindian tribal groups indicate that the majority of tribes are as diverse as the (Nootka --HF) and that only a small subset of the lineages found in one tribe is shared with other tribes. Further detailed analyses of mitochondrial sequence variation in additional Amerindian tribal populations will not only determine the magnitude of mitochondrial diversity introduced by the original founders, but will allow a test of the "three wave" hypothesis advanced to explain the colonization of the New World (footnote refers to Greenberg, Turner, and Zegura 1986)."

The dates that 'predate' the colonization of the Americas range from 41,000 to 78,000 years ago "with an average age of 60,000 years". Assuming the spread between the traditional date for the settling of the New World which has now increased to 15,000 and the 'radical' date which is 33,000 years, their 60,000 years essentially falsifies Doug Wallace's hypothesis (see MT-13) but also knocks the nonlinguistic props out from under Greenberg's Amerind hypothesis. If Amerind types were differentiating 30,000 to 40,000 years before they crossed the Bering Straits, then there is no good reason to suppose they came over as proto-Amerind. Rather proto-Andean might have come first, proto-X second, and proto-Almosan most recently. There would be lots of immigrations -- at least theoretically. Whether this picture would wash archeologically would be a different story -- naturally. Then perhaps the linguistic evidence would have to stand by itself; and this time in opposition to the powerful mtDNA data.

Those linguists who cannot stand puzzling over biogenetic analyses must now realize how serious a matter those studies are! It is worth stretching one's brain a little to incorporate some of this new kind of truth.

For those biogeneticists who reckon they can continue to ignore simple linguistic and cultural matters let it be said that there is a distinct possibility that Nootka is a lousy population from which to get an Amerind mtDNA sample in the first place. If the reader will recall those little details we sketched out in the beginning, she will have already reached their obvious implications, if she is a linguist or a cultural anthropologist. Ward et al have in probabilistic terms most likely been measuring a part Amerind and part Na-Dene population with elements from Asia thrown in for good measure. There is good reason, historically and culturally, to be very sceptical about the diversity among the Nootka and what all that is supposed to imply.

Ward et al checked the Nootka for Caucasoid gene flow and found that "individuals born before 1940 had less than 5% Caucasian admixture". What they did not check on was gene flow from Asia or from Na-Dene peoples nearby. Part of their difficulty was that, as yet, there seems to be no clear set of marker genes for Na-Dene peoples. But they might have checked with Christy Turner II to see if Nootka teeth showed affinities to Na-Dene peoples. Most of all they seemed unaware that Na-Dene peoples were something to consider, to be controlled for. In northwestern North America that constitutes a fault in research design.

PAUL BENEDICT'S VIEWS

In several letters over the past year Paul Benedict has made a number of observations and suggestions and posed some questions all for the good of our common endeavour. A condensed version of those Benedictine cognitions is offered herein. For the benefit of those members newly joined or those whose work keeps them far removed from the Pacific Rim, as it is called these days, Paul Benedict has been father or uncle to some of the most important prehistoric hypotheses on the western slopes and shores of the Pacific. By splitting the Siamese (Thai) off from the Chinese, by showing old Southeast Asian contributions (linguistic, cultural, and economic) to old Chinese civilization, by retrieving the large and significant group related to the Thai -- the Daic or Thai-Kadai family -- which had been buried in our ignorance, by resisting efforts to sweep the Miao and Yao groups into the Sinitic and Daic bags, and by championing the root linkage between Daic and the vast Austronesian world, he and his sons and nephews have greatly changed our conceptions of prehistory thereabouts.

Negatively from a long ranger standpoint, but wonderfully from a prehistorian's view, Paul has been outstanding in two things: one in reconstructions and the other in showing borrowings and influences from one phylum to another and back again. His concern over borrowings has drawn him back from an earlier acceptance of the AUSTRIC hypothesis, yet borrowings were the key to disentangling Thai from Sinitic in the first place. His view of Southeast Asia as an area where tones and monosyllables reign is a commanding one. Here we find the longer and less tonal cognates of Austronesian and Austroasiatic languages cut down to size with affixed grammemes ground up and roots shrunken nearly beyond recognition. And finally his bold reconstructions produced his 'Japanese is an Austro-Thai language' thesis which thrust him firmly in the path of Nostraticists wanting Japanese to be either Altaic or at least Nostratic. Indeed he has contributed!

Quoting now from his letters: (Quotation marks not shown)

At the 6th Austronesian conference in Hawaii this May at the symposium on extra-Austronsian relationships Paul gave a paper entitled "Austro-Thai Revisited" which he summarized as

(a) a flood of recent Chinese publications on the Kadai languages, including the key Buyang, a transitional language in the family that had previously eluded discovery.

(b) the presently achieved reconstructions of Miao-Yao, building especially on the work of Wang Fushi. (my underlining)

(c) the Japanese findings, presented in detail in the author's JAPANESE/AUSTRO-TAI.

(d) All of these throw new light on the earliest stages of Austro-Tai, as illustrated in a detailed examination of the core roots for 1st and 2nd person pronouns as well as the numerals 'one' through 'ten'.

In April he wrote from his new home in Ormond Beach, Florida: (Referring to MT-12 and questions of newsletter format versus journal format) -- So let someone else initiate another MOTHER TONGUE journal. . . Even in your newsletter you have an opportunity to get in things like your Nomotic word lists so, as I see it, you do have some flexibility. Why not . . ask members to turn in what Jim Matisoff and I are calling 'Squibs', of about the length of your word lists or shorter -- often only one page. Like a new 'long ranger' kind of etymology, of the sort you often include, sometimes even in hand writing (also on p.21). Got a synonym for 'Squibs'? I'd think that this would be a prime source of new etymologies of this kind. Anyway, I want to emphasize that for me -- and doubtless for countless others -- your newsletters are productions to be treasured -- and if some don't want to cite, let them be idiots!

Your points on pp. 5-6 are well taken and surely we should get away from 'Greenberg methods' and the like as terminology. Your Taxonomy First and Reconstruction First impress me as 'neutral' terms but, as we discussed on the phone, we need a term like 'comp' (for Matisoff's 'Comparabilia') to contrast with 'cognate'. Do you want to push that, or another? Possibly 'looka' (for 'look-alike'), but here this often has tended to be applied to a pair that have come to resemble one another through convergence or the like and so are demonstrably not cognates. So I favor 'comp'. Perhaps if you plugged this or another term in your newsletters it might gain some circulation. Another thought: throw it out to your readership: what do you like here? It would be very helpful if general agreement could be reached. (Amen -HF)

In my reply to Jim's (Matisoff) paper on Megalocomparison . . I make the general point that Greenberg and I are playing different 'games'; actually he (Greenberg - HF) is very receptive of results of my 'game', e.g. my AT (Austro-Tai - HF) and even gave up on MY with ST (relating Miao-Yao to Sino-Tibetan - HF) after I had strenuously objected to it and -- as you reported --has called it the 'worst mistake' of his life. I should add, in all honesty, that he now wants to put it with AA (Austro-Asiatic - HF) rather than AT but perhaps in time will see the light on this. I on the other hand am not playing his 'game' really. The key point here is that, unlike Joe, when I see only comps in the picture, without much of any possibility of reconstructing roots -- that is, when I see comps but not cognates -- as in the case of Schmidt's 'Austric' (see my recent blast: Austric: An Extinct Proto-Language, in the Shorto Festschrift) -- I say to myself: probably not related -- or at least, not demonstrably so (as often said, by Haudricout (sic) and others, one can never disprove the possibility of a relationship). As I do in the case of 'Austric' -- see above paper, which gives details, all about comps that look less and less alike as one knows more and more about them (my definition of comps as opposed to cognates), e.g. PMP *mata < PAN *maCa < PAT *mapra 'eye' and PMK *mat < PAA (Pinnow) *mât, which ends up resembling rather PST *myâk (PAA lacks *-y-, hence *myâk > *mât a not unlikely development!). (No, I'm not advocating a new ST/AA stock!) (PMK = proto-Mon-Khmer, PMP = proto-Malayo-Polynesian, PAN = proto-Austronesian, PAA = proto-Austro-Asiatic, PST = proto-Sino-Tibetan -- HF)

As for Joe's approach ('game'), I see that in terms rather of mathematics, say the ability to show that only X number of comps can be expected in unrelated languages and that the number of comps in this or that group of languages is significantly higher than that. Has anything like that been done yet? Should it be? That's not my bag -- I'm a true primitive in my work -- but I'd like to see it done. Any prospects? Something for ASLIP to promote, even get funding for? I have had rumors of similar things being done or being discussed but have heard of nothing more concrete.

One final matter. Where do we go from here? I must warn you that I also am an anthropologist and probably share your own biases. (He means me -- HF) I think that Taxonomy First approach is very much an anthropological one and hence we can expect little sympathy from traditional linguists. (They complain about me even at times when using my stuff). I vote for an affiliation of some sort with LOS -- and I wouldn't worry about being swallowed up. I think an affiliation -- and I stress that word --would provide the flexibility needed, at least initially. You've thrown this out to the readership and let's see what they think but you have an enthusiastic vote YES from me.

In July, thinking about the crucial languages which had controversy attached to them, he wrote:

There are several languages that might be called 'critical', with Japanese an outstanding example, as you yourself have emphasized. How about trying to get some commitment from the others to 'do' one of these languages? I'd hope, of course, that Japanese would be selected inasmuch as I'd be able to make some contribution there, while leaving it to an impartial panel to come up with an opinion. As I wrote in my letter, I'll be happy to serve on a panel of this kind in connection with some other 'critical' language.

Please do try to see that some discussion is held along these lines. Maybe 'critical' is not le mot juste here -- got another? And family/language is better than language, e.g. Japanese -Ryukyuan is such a family -- small -- including Japanese and Ryukyuan. Really, if none of us -- no group of us -- can come up with any conclusions on a language like Japanese even after all the relevant data are in -- and here we can hardly claim that adequate primary data are lacking -- it'll indicate that we have basic problem(s) badly in need of clarification.

In a paper submitted elsewhere, entitled "Kadai incorporated *-um-infix", Paul carried the work of Dempwolff and Blust in Austronesian to a deeper level by showing "a key piece of evidence establishing the presence of an important infix at an early (Austro-Kadai) level". End of quoting of Benedict.

It is no small thing to find an ancestral grammeme via internal reconstruction; more so to reconstruct an infix in any language family outside of Austronesian; and quite significant to find such a grammeme (infix) joining two phyla, in this case Daic (Thai-Kadai) and Austronesian.

PAUL NEWMAN INTERVIEWS JOSEPH HAROLD GREENBERG

CURRENT ANTHROPOLOGY, Volume 32, Number 4, August-October, 1991, pp. 453-467, published a long interview on the views of Joseph Greenberg. Although some long rangers are patently followers of Greenberg, some others being opposed to him while others remain neutral, his own views have rarely been presented to long rangers in this newsletter. While we make no attempt to paraphrase the long interview, we will report on the two pages most relevant to our concerns -- his views on the Amerind battle. The interview was conducted and reported by Paul Newman of the University of Indiana, one of the two leading Chadicists in the world and one of the leading experts on Afrasian in the world. Paul is neither a student nor follower of Greenberg; nor is he a long ranger. However, he enjoys a warm personal relationship with Greenberg whom he respects.

Quoting from the interview begins here (p.453):

"PN: At a point in your academic life when other people might be sitting around collecting honors, you find yourself embroiled in a huge controversy regarding your American Indian classification.[2] I was wondering how you feel about this situation. (Footnote ignored - HF)

JHG: Well, I didn't start out to be polemical. Maybe in my earlier life, when I was younger, I might have been, but even then I don't think I ever made any ad hominem attacks on anyone. And what's more, the basic outlines of the classification have been known since the early 1960s, when I wrote about it [Greenberg 1960]. What I do feel is that there are two distinct things at stake. One has to do with methodology, and there I am absolutely sure that the American Indianists have been carrying out a method which, if it were applied to Indo-European languages or any acknowledged family, would obviously dismantle it. That is, they are setting up requirements which have nothing to do with what we know about actual processes of language change. I think I showed that pretty clearly in chapter 6 of LANGUAGE IN THE AMERICAS . . , where I demonstrated that in spite of even the most irrelevant criteria, and allowing for them, one still had far more etymologies within Na-Dene than would occur between three branches of Indo-European if one used the same method. Also, I'm quite clear -- this is hypothetical, of course, but I'm quite certain -- that if something like Indo-European had been spoken in native California, so that, let's say, Slavic corresponded to Maidu, Germanic to Wintun, and so on, and then somebody like Sapir[3] had come along and said that they formed one group, they would have spent a lifetime trying to prove he was wrong. So I'm quite sure that the methodology is right.

The second question is whether I applied my own method correctly. It's possible that I could use this method and make mistakes. Now, let's take the African classification . . It's clear that when dealing with thousands of languages, the only way that you can classify them is to look at them, so somebody has to make the effort. The important thing is not to fight about methodology but actually to apply it. By now, there is a large literature in print about mass comparison -- what I've called more recently "multilateral comparison -- but of all the people who talk about it, no one, with the exception, I believe, of me, has ever tried it. They simply don't want to face the fact that, as I showed in my book, if one just takes present-day Indo-European languages, taking into account a large number of languages at the same time, one immediately sees groupings of the kind I have suggested. Yet they are unwilling to do that. That is absurd. Instead, they go about with isolated hypotheses of resemblance and then try to prove that two languages or two groups of languages are related. Out

of that we'll never get a classification.
(Footnote[3]. Edward Sapir (1884-1939). Throughout his work, Greenberg has always been in the Sapirian tradition. This was particularly the case with his American Indian classification, for which Sapir (1929) was a direct precursor.)

PN: The initial response to your classification by specialists in American Indian linguistics was extremely negative [see, e.g., the review by Campbell 1988]. Do you think that there has been any change of direction in your favor, especially since the Boulder meeting?[2] (Footnote ignored - HF)

JHG: I think that I'm definitely gaining support, but the support has nothing to do with the linguistic merits of the case; rather it has come from outside of linguistics. There's been a lot written about the controversy recently, most of it by popular science writers. Now, if these science writers have a background in any field, it's likely to be in biology or anthropology or some such, so that what strikes them is that there is more and more support for my classification from studies of dentition, mitochondrial DNA, population genetics, and so on. Thus one finds that it is this external support which has turned the tide in the same way that at the beginning external things had much to do with the acceptance of the African classification. All in all, I think that these external factors have had a greater impact than the arguments about linguistic methodology.

PN: Even so, I suspect that it is hard for your linguistic opponents to be confident that they're right when so much nonlinguistic evidence points in the direction you've established.

JHG: Also, my detractors among the American Indianists don't agree with each other. If you look at Campbell and Mithun's book [1979], you see first of all that the people who wrote the different don't all share the view expressed in Campbell and Mithun's introduction, which consists in what they consider a safe thing and ends up with a lot of hedging about whether particular groups like Haida may or may not show a distant relationship. [Here I am reminded of a footnote to an article by [Edgar] Sturtevant about Siever's law . . to the effect that just because your opponents disagree with each other doesn't prove that you are right.] But the fact is that some people , such as Campbell and Kaufman [but not Ives Goddard, unless I'm mistaken], appeal to something they call pan-Americanisms in rejecting my Amerind family. Now, once they do that, they have almost given up their case. They say vaguely that someday we'll be able to account for these pan-Americanisms; but obviously if they exist, they should be very important for American Indian specialists, and they should have a reasonable explanation. Furthermore, in a recent paper on Amazonian languages, Kaufman [1990] has admitted that these do not occur in Eskimo-Aleut and Na-Dene, which clearly points to them as diagnostic characteristics of the Amerind family. For me the only question that remains -- and this is not a trivial one -- is that of subgroupings within Amerind.

A lot of the arguments at Boulder showed me that a meeting isn't necessarily the best place to get enlightenment about a question, because when people talk and argue, and so on, facts are often obscured. Take, for example, the question of the pronouns. It must have seemed to some of the people at the meeting who didn't know the languages that this thing was simply a statistical statement to the effect that nasals are more frequent in the New World than in the Old, but it isn't that way at all. I have five full pages in my book enumerating languages from Tierra del Fuego to British Columbia which have either /n/ in the 1st person or /m/ in the 2nd person or both. Such facts are not found anywhere else in the world.

PN: I thought that it was an interesting meeting in that it provided linguists and nonlinguists concerned with New World prehistory an opportunity to interact. The danger, of course, is that some people might not have had the background and training to follow the linguistic arguments.

JHG: As I said, things may sometimes get more obscured than cleared up. For example, take the question of what is meant by the comparative method. It's treated by people as if it were self-evident, but in fact it's a very complicated thing. The way I've tended to treat it it is historically, that is, to look at Indo-European and see what Indo-Europeanists really did and what were the things that really counted. The question is, having discovered that Indo-European was a family, how did they decide, when they encountered a new language, whether it was Indo-European or not? You find that it had nothing to do with sound correspondences; rather, their approach was to look at things closely and decide what was diagnostic. Hittite, for example, forced them to change the reconstructions. If they had acted according to the preachings of some the American Indianists, they would have decided that Hittite was all an illusion. In my Boulder paper I cited a passage from Hroznyˇ [1917], whose work led to the acceptance of Hittite as being Indo-European. He doesn't say anything about sound correspondences or things like that; rather, he says, "Take a look at words such as WATAR 'water', not to mention the genitive WETENAS, which shows that you have r/n stems, an archaic Indo-European feature that occurs in Sanskrit and Latin, although not in that particular word; or just cite a full paradigm representing regular verb conjugation; that's enough!"

(Skipping now to page 456 -- HF)

PN: If I may, let me turn back to the American Indian language question and pick up on what seems to be a methodological or theoretical inconsistency in what you have done. One of the strengths of your African classification -- one could say the key element, methodologically -- was getting away from culturally and physically based linguistic classifications; your book was a reaffirmation of the independence of race, language, and culture. [/One of the commandments of Boasian anthropology -- HF\] But very often now, in seeking support for your American Indian classification, you bring up what people are doing in the field of dentition, what the physical anthropologists have come up with, and so on. Isn't there an inconsistency in using the physical-anthropological/cultural evidence when it is useful and ignoring it when its's not?

JHG: No, I don't think so. I have never used the findings of other fields as an argument for the correctness of the classification as such: it was arrived at in virtual ignorance of what had been done in the other fields. One of the things I took for granted was the dictum that one classifies languages on the basis of linguistic evidence only, so in the first chapter of my book I think that I might not have even bothered to mention it. I do not believe, for example, that the concordant results of the studies of dentition . . and population genetics show that my classification is right. It's just a happy result -- which, in a sense, is not quite as irrational as rooting for somebody just because he's American and not British or French or because he seems to be antiracist because of what he said about Hamitic. If a physical anthropologist who doesn't know the linguistic data says, "Well, Greenberg must be right," because he in turn is looking for support for his physical classification, that's just the nature of science -- which is that one wants to see things fit together.

Having emphasized the point that language classifications such as I have done are based strictly on linguistic evidence, I should add that a high degree of correlation among physical, archaeological, and linguistic data might be expected in areas of new settlement. In the Americas, if you think of one group coming in and then some thousands of years later another group coming in, they are sure to have been very different culturally and to have come from different parts of Asia with different artifacts which would distinguish them archaeologically. A situation pretty much like this did exist in southern Africa, where the Bantu-speakers, who represented a new settlement, show a kind of archaeological homogeneity and thus archaeologists don't have any difficulty in identifying Bantu as opposed to Khoisan sites. So in the Americas, when you find a convergence of results from linguistics, archaeology, and physical anthropology, you can't say that it doesn't strengthen the case for my classification: I think it does strengthen the case." END OF QUOTING THE GREENBERG INTERVIEW

(Note: Bibliographic citations are available in the original.)

Editorial comment (HF): A certain amount of historical inexactitude crept into the interview, probably because JHG was thinking about his Americanist critics. To say that those contemporary linguists who talk about multilateral comparisons do not actually do them -- this may be true. To say that JHG is the only one who has ever done mass comparisons is false. French scientists -- Les Observateurs de l'Homme -- used the method in the Pacific in the early 19th century, certainly Sir Harry H. Johnston used it in Africa and George A. Grierson and colleagues on the LINGUISTIC SURVEY OF INDIA; both well before JHG's time. Kroeber and Dixon and probably Sapir and Powell, but certainly Swadesh, used it in North America. There are, I believe, lots of examples, even in the 19th century. I used it in India, thus discovering the saliency of Kusunda, and Africa, as did Archie Tucker, one of JHG's Africanist opponents. JHG may have been his inspiration!

Further comment: In responding to the question of support from nonlinguistic work JHG manifests a familiar tendency to underestimate his friends and allies. In the linguistic realm there have been more actors on the scene than himself and his opponents. There is above all Merritt Ruhlen (his Huxley); also Vitalij Shevoroshkin (who fired up the media), Allan Taylor (who set up the Boulder conference), Sydney Lamb (a major linguist and Americanist who set up an early symposium at Rice and supported JHG publicly), Dell Hymes (surely the top ranking American linguistic anthropologist and Americanist; he gave support to JHG in print, in public, even knowing how unpopular such support would be), plus the Muscovites, plus MOTHER TONGUE and scores of long rangers --it is fair to say that JHG has had lots of help!

From a global viewpoint it could easily be argued that the Nostraticists have had a much tougher time of it, starting with far fewer resources and little prestige (excepting Hodge) but taking on a much more formidable opponent -- the Indo-European establishment. Let us raise our steins in a toast to Bomhard and the younger Muscovites!

¡ LA LUTA CONTINUA ! THE NEWS.

H-J. PINNOW HAS DIED. The news came from Germany (thanks to Ekkehard Wolff). We regret that one of the great long rangers has left us. Pinnow was most famous for his contributions to South Asian and Southeast Asian historical linguistics and to the elaboration of the "Na-Dene is related to Sino-Tibetan" hypothesis. We also regret that he (allegedly) died not knowing the high regard in which he was held by long rangers. Had we been able to, we surely would have nominated him to be a Fellow of ASLIP. Obituaries are being solicited from those most familiar with his work, perhaps one for southern Asia and one from the Na-Dene perspective.

ALLAN BOMHARD has an article , entitled "Distant Linguistic Comparison and the Nostratic Hypothesis", in the October 1991 issue of FAIES NEWSLETTER. It is an up dating of his thinking on this matter.

NORBERT CYFFER AND JOHN HUTCHISON are the editors of a new work on Kanuri (Saharan of Nilo-Saharan), entitled DICTIONARY OF THE KANURI LANGUAGE. 1990. Dordrecht: Foris publications, 200 pages. As reviewed in MEGA-TCHAD 91/1 the dictionary is said to be "the result of more than five years of intense collaboration between Hutchison, Cyffer, El-Miskin, Abba and Modu. It is the first dictionary of the Kanuri language, containing apporoximately 10,000 entries and an elaborate system of crossreferencing which makes the morphology and grammar of the rather complex verbal system of Kanuri readily accessible to the user. Borrowings are indicated from Arabic and a variety of other languages, with some of the Arabic borrowings being almost unrecognizable due to their longevity in the Kanuri lexicon. The dictionary of the Kanuri language is a complete reference and research tool for those interested in the Nilo-Saharan family of languages in general." Norbert Cyffer is at Universität Mainz (Germany) and John Hutchison is at Boston University (USA).

A colloquium to honor HERRMANN JUNGRAITHMAYR, called Studia Chadica et Hamito-Semitica, was held at Frankfurt-am-Main, Germany, from May 6 to 8, 1991. As described by Daniel Barreteau, "Ce colloque, discrètement mais soigneusement organisé par Dymitr Ibriszimow et Rudolf Leger, s'est tenu en hommage au Professor Herrmann Jungraithmayr. Environ soixante dix personnes y ont participé. Une quarantaine de communications ont traité des langues tchadiques et de leurs rapports avec les langues apparentées ou voisines (chamito-sémitique, saharien, mandé, voltaïque, fulfulde). Ce colloque a été marqué par une participation active de chercheurs africains, notamment du Nigeria, et par de nombreuses communications sur la langue tchadique "phare": le hausa (17 études). L'importance des recherches descriptives et comparatives, menées aussi bien en Europe qu'aux USA ou en Afrique, témoignent d'une vitalité et d'une certaine "maturité" dans le domaine de la linguistique tchadique." (Reported in MEGA/TCHAD 91/1, p. 28.) Long ranger Jungraithmayr is one of two top Chadicists in the world and a prominent Afrasianist. We raise out steins to him!

The demise of the Academy of Sciences of the USSR was announced in the New York Times in mid-December! Such an event which could be catastrophic for science in the (former) Soviet Union had been anticipated by many of us -- because state-funded scientific institutes were bound to suffer when the state itself evaporated. Only things were not quite so bad as anticipated because the Russian Republic stepped in, saying that it would support these institutions. However, many of us realize that the modern world increasingly supports things of utilitarian value, rather than fluffy pastimes like prehistory and linguistics. We fear for our 'Soviet' colleagues some of whom are going to become day laborers in the proximate future. In Ukraine this is already happening, says colleague Tambovtsev. One thing we are considering -- and a matter which will be put to our members for their votes -- is outright support for some colleagues, since there is such a huge disparity between the ruble and Western money nowadays. [When MOTHER TONGUE started in 1986, the ruble was worth US$1.50; now the ruble is worth one penny ($0.01)!! Next month it may be worth less than that!] US$10 or DM15 or 55FrFr or 8 quid (all roughly as in today's market) can probably support a colleague for a month, if she lives in Moscow! If and when housing (apartments) rents at 'free market' prices, then this whole analysis may be blown apart. A scarce commodity in a huge urban area (= high demand) means rents could go sky high, if they are allocated by capitalist rules. Oof! We need feedback from Russian colleagues on these points. Please give an answer.

A sub-committee of Directors and Fellows has been negotiating with the LACUS Forum for a permanent place for ASLIP in their annual meetings. The deed is done (mostly) and announcements will be made shortly. For those who want to give a paper at an ASLIP meeting start preparing your paper's title and abstract NOW in time for a Jan.15th deadline. Our thanks to Sydney Lamb, Carleton Hodge, and Saul Levin! Each is a past President of LACUS Forum.

CLAUDE BOISSON has published an article, entitled "Notes Méthodologiques sur les Racines Pré-Indo-Européennes en Toponymie", in the journal NOUVELLE REVUE D'ONOMASTIQUE, n° 15-16 (1990): 25-38. It is an important article for thinking about how to deal with the Nostratic and/or Dene-Caucasic elements which may underly place names in Indo-European-speaking areas. Since the print in rather small and the pages rather full, this represents much more than 14 ordinary pages. I found the French difficult at times, due to the subtlety of Claude's thinking. It is recommended that readers write directly to Claude for reprints or copies at: 6, place Jean Macé, 69007 Lyon, FRANCE.

BBC, more familiarly known as the BRITISH BROADCASTING CORPORATION, will produce a documentary to show on March 30, 1992 on its "Horizons" program. The title is temporarily forgotten but the topic is known to be: what we do! I.e., the coverage will focus on long ranger activity or "the emerging synthesis" (invented by Colin Renfrew). Proponents and opponents will be interviewed. Those in Stanford and Ann Arbor -- naturally -- have been filmed already. But on this occasion of THE MEDIA's activity the rest of us will finally be noticed. The director or reporter is Chris Hale and he has asked that all long rangers who wish write to him to express their views on whatever (relevant) topics they wish to. Conceivably, some of you/us may so titillate BBC's fancy that they will fly a camera and crew to your home to shoot (film) you. They are going to shoot Dolgopolsky in Israel, so other Israeli long rangers may get in on it too. The address to write to, to get yourself on the tube, is:

Chris Hale
Skyscraper Productions
St. Joan's Studios
Richmond, Surrey TW9 2QA
England, UK

One friendly admonition: Mr. Hale is quite sophisticated and rapidly becoming knowledgable about our common concerns. Treat with respect.

LETTERS

Due to space limitations, much less is reported from letters than usual. Moreover, for some reason, MT-14 produced a dearth of letters. One would hope that members would strain themselves just a little and write occasionally about their own work (at least) and their thoughts on varied topics or special problems they are facing.

KARL-H. MENGES writes from Austria among other things that: (a) the new European distribution system run by Ekkehard Wolff pleases him very much, (b) the Altaicists are still not properly seen by outsiders; their negative attitudes are a problem, (c) Paul Benedict would not propose his theory that Japanese is Austro-Thai, if he knew more about Japanese. It belongs in or near Altaic.

VICTOR SHNIRELMAN writes from Moscow that it is getting hard to make ends meet there -- financially. He has returned from a bunch of conferences and tours in North America and should have attended by now the meetings of the American Anthropological Association in Chicago in November.

The long-lost VLADIMIR OREL has surfaced in Israel. He has been very fortunate -- for an emigré intellectual these days -- to have acquired a university teaching job. His work on Afrasian is at an advanced stage and volumes of proto-Afrasian will soon be rolling off the presses. Vladimir has especially strong interests in ASLIP and will soon be asked to be our distributor there, since Israelis still cannot easily transfer money out of the country (to Ekkehard Wolff). For friends and colleagues who want to contact him his addresses are:

Dr. Vladimir Orel

3 Ha-Arnon Street	Department of Linguistics
Jerusalem 94517	Faculty of Humanities
(TEL.02-232869)	Hebrew University
	Mount Scopus - Jerusalem

ISRAEL

The long-lost ANBESSA TEFERRA has also surfaced in Israel. A surprise that, since we expected him in Washington (state). His wife is a Falasha, which explains many things. Anbessa is Orthodox Christian (Ethiopian) and has to spend much time explaining that he is not a Falasha, but rather a Sidamo (East Cushitic), and how a poor peasant (Falasha) came to speak such excellent English. Anbessa is the prime field worker on the Shabo language which we have mentioned several times in previous issues. He stands ready to share data and ideas with others interested in either Shabo or Nilo-Saharan.

YURI TAMBOVTSEV writes from Lvov, Ukraine. They are in dire straits there. All predictions about Soviet state employees are coming true. Yuri now must work as a dust bin collector in order to survive. He should be put on top of the urgent list. Informally, I might suggest that people cleverly pack away a ten dollar bill in a Christmas card and mail it to him. (This has worked already with one Muscovite.) Ask for reprints of articles or some data which he is willing to give. Better yet, offer him a job in your country. His address is: Professor Yuri Tambovtsev, Chairman, Dept. of Foreign Languages and Linguistics, P.O.Box 8834, 290044 Lvov-44, UKRAINE. We hope our 'Soviet' colleagues will not be too proud and will let us help them during these extraordinary tims. After all, the rest of us are indebted to you for your work of the past ten years.

SERGEI NIKOLAEV wrote from Moscow. Believing that I had asked him for US$10 for ASLIP dues, he responded that things were far too tough nowadays for him to be able to send me any money. What I had asked for, as with all countries with currency problems, was that he send us a note or letter saying that he wanted to receive ASLIP. We have had 'Soviet' colleagues who never sent any reactions -- not in five years of receiving MOTHER TONGUE free -- so we cancelled their membership (which wasn't real anyway). That is why we ask for letters; it is a response indicating interest. That's all. Sergei is a stalwart fellow and I am sure he would be willing to sell some of his reprints. You can get them by writing to:

Sergei Nikolaev
Ulica Komarov 11
Apt. 84
Moscow 127276
RUSSIA

He probably would like Christmas cards too.

GIORGIO BANTI writes from Rome, saying that we should not make ASLIP into an elite outfit of those few who do the cognate hunting and etymology building. Rather we should realize that there are scholars who wish to hear what is going on across a wide range of language families and to hear about developments in related fields. Even if they are passive with respect to long ranger activities, still they do listen and react and they are a significant part of the scholarly public we want to reach.

SHEILA EMBLETON writes from Toronto, saying much the same things but also showing a level of participation other than passivity or elite activism. The expert on Something, the scholar who will give aid and advice, e.g., as Adam Murtonen did for us on Nostratic, that sort of member truly belongs in ASLIP. In fact this was one of the first things that Dolgopolsky recommended back in MT-2. It is an idea of great value. I plan to ask Sheila's advice on mathematica ASLIPica.

PATRICK RYAN writes from Little Rock, Arkansas. He is interested in Aihenvald-Angenot's NOSCAU and in Fleming's BOREAN. He suggested some nomenclatural changes in the various levels of proto-languages which are involved in those two large hypotheses. Much of his orientation is based on an attempt to see things from the origin, i.e., from the perspective of a likely proto-Human. We will hear more from him in the future.

ALVAH HICKS writes often from Ojai, California, keeping me informed on the results of his remarkable search for alternatives to the African origin hypothesis. Tentatively, he favors a South American origin. Hence he was not displeased by Ward et al's article reported above. Only Alvah suspects that the same evidence could be read as evidence for human antiquity in the New World -- as it certainly could; the only reason for postulating Amerindian differentiation in Asia is the belief that Amerindians have only been in the New World for 15,000-33,000 years. Right? Alvah is not ready to present his full hypothesis to MOTHER TONGUE, not quite yet. He may even change his mind, but he's working on the problem. One does not know what a Zulu would think of the notion that she came from Brazil. Or what an Andean (say Quechua) thinks of his derivation from the African forests. But we have an Asian colleague who is quite upset by the notion of an African homeland for us all.

The following books are available for review in *Word*. If you wish to review a book, please write to Sheila Embleton, Department of Languages, Literatures and Linguistics, South 561 Ross Building, York University, 4700 Keele Street, North York, Ontario, CANADA M3J 1P3. E-mail is embleton@yorkvm1.bitnet or embleton@vm1.yorku.ca.internet. Telephone numbers are (416) 736-5387 at York and (416) 851-2660 at home. Books are available on a "first come, first served" basis. Graduate students are welcome to participate under supervision of a faculty member. Reviews are due 6 months after you receive the book. Please send **3** copies of your review, double-spaced with at least 2 cm margin on all sides.
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Date of this list: November 11, 1991

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February 15-18, 1992. Berkeley Linguistics Society. University of California, Berkeley, California, USA.

April 20-23, 1992. Georgetown University Roundtable on Linguistics, Georgetown, Washington, D.C., USA.

April 24-26, 1992. International Linguistic Association Annual Meeting, Georgetown University, Washington, D.C., USA. Write to Professor Ruth Brend, 3363 Burbank Drive, Ann Arbor, MI 48105, USA.

May 21-23, 1992. Fourth Annual UCLA IE Studies Conference, University of California, Los Angeles. Papers on any aspect of IE studies are invited: linguistics, archaeology and culture. Papers on both interdisciplinary and specific topics (e.g., typology, methodology, reconstruction, relationship of IE to other language groups, interpretation of material culture, etc.) are welcome.
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May 24-26, 1992. Canadian Linguistic Association, University of Prince Edward Island, Charlottetown, Prince Edward Island, CANADA. Write to Professor Parth Bhatt, Experimental Phonetics Laboratory, Department of French, New College, University of Toronto, Toronto, Ontario, CANADA M5S 1A1. Internet exphonla@epas.utoronto.ca.

August 4-8, 1992. LACUS (Linguistic Association of Canada and the US), Université du Québec à Montréal. Write to Professor Ruth Brend, 3363 Burbank Drive, Ann Arbor, MI 48105, USA.

August 9-14, 1992. International Congress of Linguists, Université Laval, Québec, CANADA. Write to CIL 92, Département de langues et linguistique, Université Laval, Québec (Qué.), CANADA G1K 7P4. Telephone (418) 656-2625. FAX (418) 656-2019. Bitnet CIPL92@LAVALVM1.

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OVER!

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Wapapi Akonutomakonol

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The author of *The Wampum Records,* Lewis Mitchell — or Oluwisu — a Passamaquoddy of Pleasant Point, Maine, was born in 1847. He became the tribe's representative in the Maine Legislature, where, in 1887, he delivered an eloquent and impassioned speech in behalf of native land and subsistence rights. Mitchell was a self-taught man who was unusually well educated and well read for his time. He is remembered today as a militant advocate for his people.

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The Wampum Records is available in softcover at $10.00, postpaid, from the publisher, the Micmac-Maliseet Institute, at the University of New Brunswick.

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2 **Wabanaki Wampum Protocol**— *by Willard Walker*

Walker's article places the Wabanaki Confederacy in the broader context of the oral tradition and the ways in which wampum was used to symbolize relationships among peoples and convey messages at an international, diplomatic level, both among the Indian nations and between Indians and non-Indians.

3 **Wapapi Akonutomakonol: The Wampum Records**— *by Lewis Mitchell*

Mitchell's original Passamaquoddy-language text was first published by J. D. Prince in 1897. The 1990 edition sees the correction of many of Prince's transcription errors and updating to modern spelling, together with a new, accurate English translation. Mitchell relates the oral history of the Wabanaki Confederacy and the laws relating to marriages and to the making of a chief — a responsibility shared by all members of the alliance whenever any one chief died.

4 **The Wampum Records Annotated (Confederacy Section)**

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About the Editors

Since 1984, Robert M. Leavitt and David A. Francis have co-authored several books and articles, including *Neke naka Toke: Then and Now* and *Passamaquoddy-Maliseet Verb Paradigms.* They are also co-editors of *Kolusuwakonol,* the Passamaquoddy-Maliseet/English dictionary.

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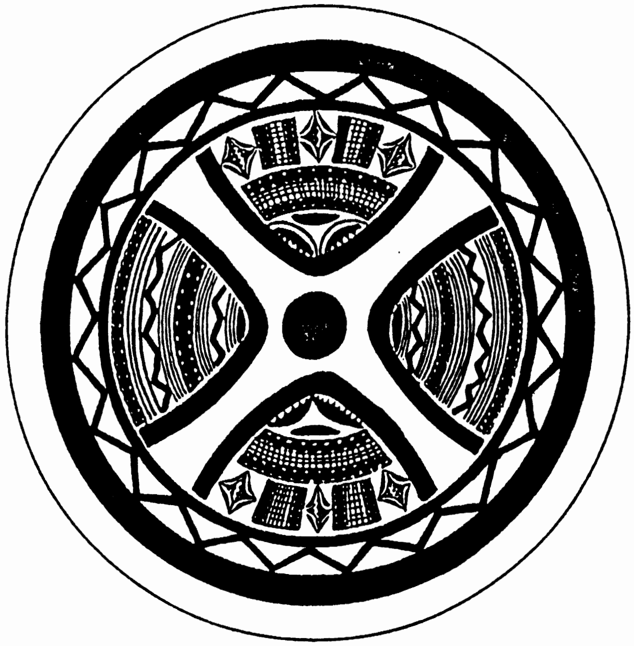

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