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

**Issue 30 (MT-30) Winter 1997**

MOTHER TONGUE : NEWSLETTER of the Association for the Study of  
Language In Prehistory. Issue 30. Winter 1997

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 US \$25 in all countries, except those with currency problems. For membership information, contact:

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ISSN 1087-0326 for **Mother Tongue: The Journal** (an Annual, not Newsletter)

INTRODUCTION TO MT-30: The Newsletter (Editor this issue: H. Fleming)

ASLIP BUSINESS

The business of managing our joint venture is most serious this time around. Very few nominations have come in, yet it is time to vote. There is an urgent need to replace the Secretary, the Treasurer, and the person who writes these Newsletters. If you do not find someone to fulfill this role, John Bengtson will have to do all the work. Predictably, he won't be able to do that for long and ASLIP will become 'history', as the Americans say.

Now's the time for ASLIPers finally to pay their 1997 dues. So we must invoke the laundryman's rule -- no tickee no washee. No pay dues, no get Journal. :: See the attached colored sheet.

Are there any volunteers out there? You must nominate and vote for some officers and directors. See another attached sheet, to fill out and send in to Hal Fleming.

THE HOTTEST AND THE LATEST NEWS, AS OF MID-FEBRUARY, 1998.

This time around, the hot news is very brief, yet very important. At the top of the list is a remarkable study by Stephen Zegura and Mike Hammer about the original Great Diaspora which neatly confirms the basic hypothesis first advanced by Becky Cann but also allows for some interbreeding with the Untermenschen in situ. As in other areas it still remains the case, however, that the interpretation of evidence as confirming, falsifying, or both (!) is not so easy -- not easy at all this time :: In addition Chomskyite theory of language origins seems to have taken a hit from research on chimpanzee brains -- maybe :: Yet another archeological site in the New World, 'too old' for the Maginot Line to withstand, is unearthed :: Extensive biogenetics of New World falsifies 3-migration theory -- maybe -- and dates first migrations to 30-40 kya :: Is a 0.56 correlation very good?

OBITUARIES

Professor Gordon Hewes has died. His many contributions to language origins research and anthropology are noted in a colleague's eulogy .

NEWS OF OFFICERS' ACTIVITIES, INCLUDING A CONFERENCE

Seven long rangers were invited to a workshop at the Santa Fé Institute in New Mexico in December. They were not adjuncts to some larger conference; their work was the central focus. Quite a set of energetic minds bumping into each other! John Bengtson reports on things as he saw them. Hal Fleming perceived some systematic differences in strategies, even tactics; he reports on them. Dolgopolsky's passionate performance is noted.

THE MEMBERSHIP (PERMITTED) LIST.

Add Giorgio Banti / U/Roma / Italia. Inter alios.

More names are added to the "Good Guys" list which is enclosed.

OUR JOURNAL, MT-III, COMES OUT SOON. (Despite several serious setbacks):

Your patience in awaiting the third issue of our Journal is appreciated. We hope to be aware of the Ides of March at out-put time. Key features of this issue will be (a) Paul Whitehouse's dauntless efforts on Nihali & Kusunda, (b) several people, led by our expert on antiquity, Igor Diakonoff, have a go at Sumerian, (c) a fine inter-scholar mutual review of several books on language origins, (d) Ken Hale reviews a major new book on native American languages by Campbell, and (e) Starostin's long review of Chirikba's new book on West Caucasian. Plus -- assorted goodies.

ASLIP BUSINESS

The Annual Meeting of ASLIP will be April 18, 1998, Saturday, from noon until 3 pm, at the African Studies Center, Boston University, 270 Bay State Road, Boston, Massachusetts, USA. Our meeting will be on the 4th floor.

The reason for moving ASLIP business to the top of the roster is simple: we are facing a management crisis. But a crisis of a different sort from the usual, i.e., top management is fine but the lower levels are departing. The Presidential support system will soon be lacking. We must replace two officers or find some individual to take two jobs. And we must find someone to write the biological and/or archeological portions of the Newsletters, or MOTHER TONGUE will become, simply dit, all historical linguistics.

This is also a crisis for ye, the members. For the past twelve years you have been models of passivity and nonchalance, letting one or two people do everything. Now you will have to decide whether you want this organization to continue or not. Sup 2 Yew.

But, as Paul Benedict used to say, where are the other enterprises that do what ASLIP and MOTHER TONGUE do? Hmmm? Perhaps you should bestir yourselves to think about it. Do it! It may be your last chance -- ever.

Requested Forms. Enclosed in this envelope are two forms; they are meant for your action. One is a yellow form which begs for money. Only some of you receive those because many have paid dues and some have given more. The second is a blue/purple form which asks you to vote for officers of ASLIP and for Board members. Those who have already been nominated are listed as alternatives in voting. However, in order to compensate for the very low number of responses to the original request for nominations, you are given the option of 'write-in' votes, i.e., you nominate and vote for someone at the same time. Thus, if you decide you want to be a member of the Board, you write your name in the space provided, instead of circling the name of a proper nominee.

We do understand that many of you do not make nominations because you are quite satisfied with the present officers and Board members. It is for those dissatisfied and those previously inactive that we direct these forms. As everyone knows, the Annual Meeting is able, legally and by precedent, to choose all the officers and Board members -- all by itself. The primary target of our effort here is to make the Annual Meeting process more democratic or give the appearance of being democratic. That is not a cynical remark; it just reflects the reality that 92% ± of the entitled voters do not vote. Legally, an election can be said to be democratic if some quorum or minimal number vote, but morally/socially one has trouble seeing 92% lack of interest as democratic! This means -- in social fact -- that the opinions and desires of 9/10ths of the body politic are not reflected or expressed in the selection of its governors.

TIDBIT

According to our best informant in Arizona, Joseph Greenberg will speak at the University of Arizona, Tucson (Arizona) on April 16, 1998. The topic is Eurasiatic. After the talk, drop by in Boston for the Annual Meeting.

THE HOTTEST AND THE LATEST NEWS, AS OF MID-FEBRUARY, 1998.

The hottest biogenetic news is exciting and even a little strange. It appears that strong confirmation for Rebecca Cann's theory of human origins has appeared, but along with some support for the opposing view, i.e., the 'rising tide lifts all boats' theory or multiregionalism. Extraordinary! Is it possible that both sides of a major scientific dispute are correct, at least in part, and incorrect, at least in part? Mon dieu! let us not take this question to philosophy; let us bring it to earth! What's happening?

Stephen Zegura and Michael Hammer (both U/Arizona) are publishing their formal article next month in JMBE. We are not allowed to 'steal JMBE's thunder' prior to publication, except only the informal general remarks about it made by Zegura in Santa Fé, New Mexico, in December. So our remarks are very general and serve to call attention to the forthcoming article in the JOURNAL OF MOLECULAR BIOLOGY AND EVOLUTION. The substance of the Arizonan efforts is highly technical, analytical and quite beyond an audience of non-specialists. They consulted with Templeton of U/Utah at several points, so that their models are fire-hardened as it were. Templeton is famous, as everyone knows, as the analyst who threw cold technical water on Becky Cann's hot new theory. His attentions were what is expected when we talk about testing and falsifying hypotheses. It is presumed that Templeton strengthened, nay improved, the Hammer-Zegura models. The rest of our remarks will be entirely free of technical details other than the statement that both mtDNA and Y-chromosome data were used, as well as  $\beta$ -globin. (Remember the Harding article in MT-28 ?)

Basically, Stephen and Mike found that a common modern human ancestor could be proposed for 150 kya and in Africa; that the Great Diaspora out of Africa could be dated to around 110 kya; that there had been some feedback in that some Asians had migrated to Africa circa 30 kya; and that the Diaspora was primarily a movement/migration of

males who interbred with the local females -- les autochthones -- found on their routes or in the new regions of Earth which they went to. At the end of this we also find another surprise -- social structure analysis or good old kinship study -- will now occupy one of the foci of our interest. This time for prehistory instead of synchronic theory. Since some of our long rangers are also experts on kinship, we bid the biogeneticists to seek their advice. I want especially to mention Bernd Lambert of Cornell and Stephen Tyler of Rice, two superb analysts of kinship systems in Oceania, India & elsewhere. We predict that you will need their help -- seriously.

Subject to the publishing restrictions mentioned above, we have pieced together an abstract or precis of the Zegura/Hammer paper from Stephen's oral presentation in Santa Fé and some remarks in a letter. This is not the formal presentation of their paper. This one, among other things, is writ in English. Heh, heh! (Hereinafter 'we' refers to Hammer and Zegura.)

"We surveyed nine di-allelic polymorphic sites on the Y chromosome of 1,544 individuals from Africa, Asia, Europe, Oceania, and the New World. Phylogenetic analyses of these nine sites resulted in a tree for 10 distinct Y haplotypes with a coalescence time of ~ 150,000 years. The 10 haplotypes were unevenly distributed among human populations: five were restricted to a particular continent, two were shared

between Africa and Europe, one was present only in the Old World, and two were found in all geographic regions surveyed. The ancestral haplotype was limited to African populations. Random permutation procedures revealed statistically significant patterns of geographical structuring of this paternal genetic variation. The results of a nested cladistic analysis indicated that these geographical associations arose through a combination of processes including restricted, recurrent gene flow (isolation by distance) and range expansions. We inferred that one of the oldest events in the nested cladistic analysis was a range expansion out of Africa which resulted in the complete replacement of Y chromosomes throughout the Old World, a finding consistent with many versions of the Out of Africa Replacement Model. A second and more recent range expansion brought Asian Y chromosomes back to Africa without replacing the indigenous African male gene pool. Thus, the previously observed high levels of Y chromosomal genetic diversity in Africa may be due in part to bi-directional population movements. Finally, a comparison of our results with those from nested cladistic analyses of human mtDNA and  $\beta$ -globin data revealed different patterns of inferences for males and females concerning the relative roles of population history (range expansions) and population structure (recurrent gene flow), thereby adding a new sex-specific component to models of human evolution."

Their analysis includes some dates of interest to us, remembering that biogenetic dating remains largely unverified but highly interesting. For the Adam in this scenario, he would be dated to  $147,000 \pm 35,000$ ; he would be an

African, and his tribe would remain in Africa for many millennia. Then around 110,000 they leave and eventually replace the males who had lived in the Old World outside of Africa, such as late *Homo erectus* types, Neanderthals, etc. Then around 55,000 begins a movement of East Asians who re-enter Africa around 31,000 years ago. There are other later 'events', including 20,000 ya in North Africa which might be the 'Iberians' entering, but these details will have to be left for the article in *JMBE*. Along with the whole technical and mathematical basis for their report.

On the subject of the male/female differences -- and this will become the hot topic -- we may borrow a bit from their original article, just to get the topic started. Now begin quoting:

"Both Templeton's (1993) original nested cladistic analysis of human mtDNA and his methodologically more rigorous re-analysis (Templeton 1997b) are highlighted by recurrent gene flow restricted by isolation by distance throughout the Old World for the entire time period encompassing the mtDNA TMRCA. This short-range gene flow is pervasive at all levels of analysis and underscores the paramount influence of population structure on the dynamics of human maternal genome evolution. No inter-continental range expansions similar to the three postulated on the basis of our Y chromosome data are detectable in global mtDNA data. Thus, the effects of population history seem to have left a much clearer inter-continental imprint on our paternal-specific genome than the regional signals left in our mtDNA. One possible explanation for this pattern is that males disperse more than females during long-range inter-continental population movements while females may



disperse more than males during short-range intra-continental migrations."

"If males and females do, indeed, exhibit major differences in their ancient population structure and demographic histories, then we might expect traces of these differences to be preserved in the autosomal DNA record. Templeton's (1998) re-analysis of Harding et al.'s (1997)  $\beta$ -globin data represents the only nested cladistic analysis of a human autosomal data set. The deepest clade in the  $\beta$ -globin cladogram showed an out of Africa expansion; however, the 800,000 year coalescence time for the  $\beta$ -globin gene tree makes it unlikely that this range expansion had anything to do with the out of Africa event detected by our Y chromosome data. On the other hand, this time frame is more concordant with the sudden appearance of the possibly African-derived *Homo antecessor* in Spain sometime before 780,000 years ago (...) [Note: cf MT-28 - HF]. Moving to less deep structures, all the mid-level (2-step) clades gave strong evidence for gene flow restricted via isolation by distance occurring more than 200,000 years ago throughout the Old World. Finally, two range expansions were detected at the 1-step clade level: (1) the aforementioned expansion from Southeast Asia [sic] back to Africa, and (2) an out of Africa expansion that involved the oldest haplotypes by outgroup rooting, making the temporal framework of this expansion unclear (i.e., it may be a recent expansion or the same one detected at higher levels in the cladogram)."

"This out of Africa expansion was not a replacement event because it was nested within a 2-step clade characterized by gene flow restricted via isolation by distance. In order to equate this out of Africa event with the one detected in our Y chromosome data, one would have

to argue that perhaps Eurasian males were replaced but females were not. This is consistent with the demographic picture from the nested cladistic analyses of mtDNA data (...) where females show no sign of replacement and where gene flow rather than range expansion is the oldest inference. Therefore, the  $\beta$ -globin locus integrates aspects of both the mtDNA and Y chromosome analyses, and provides support for the hypotheses of contrasting male and female population structure and demographic histories. Because there is evidence for restricted, recurrent gene flow throughout the Old World during the entire history of anatomically modern humans, as well for range expansions out of Africa >100,000 years ago, the nested cladistic analysis results from these three types of data conform with genetic predictions based on human origin(s) models characterized by interbreeding between migrating and resident populations. Thus, the combined data add a new sex-specific component to the conceptual framework of both Bräuer's (1989) African Hybridization and Replacement model and of Smith, Falsetti, and Donnelly's (1989) Assimilation model: the possibility that the Old World female genetic complement was preserved by hybridization, whereas the Eurasian male component was replaced by African Y chromosomes."

[Note: these two common terms are not actually clear here. Eurasian refers to pre-African *Homo* types, resident in Eurasia + Oceania. Old World seems to mean all that + Africa. But tis not certain. -- HF]

Since news of this paper came out, many people have wondered how Steve 'n Mike's theory could be worked out or understood. One natural observation would be that mtDNA ought to show evidence of inbreeding, gene flow from the *Unter-mensch*, but it doesn't seem to.

### First Americans: Has 3-Migration Theory been Falsified?

While Stephen Zegura was joining Mike Hammer in proposing their new theory, discussed above, Steve was involved indirectly in another journal, AJHG, by virtue of his participation in the original Amerind theory of three separate migrations to the New World, long known as the Greenberg-Zegura-Turner theory. As you know, it accounted for the Amerinds, Na-Dene, and Eskimaleuts by proposing three migrations bearing us across the Bering Straits around Clovis horizon time -- 12th millennium BP. Mind you, Steve was not under attack, nothing personal, but the import of the AJHG article is, in fact, a refutation of 3-migration theory. Well, maybe it is.

Writing in the American Journal of Human Genetics 61:1413-1423, 1997, Sandro L. Bonatto & Francisco M. Salzano of Universidade Federal do Rio Grande do Sul, Porto Alegre, Brazil published "Diversity and Age of the Four Major mtDNA Haplogroups, and Their Implications for the Peopling of the New World." Their Summary is given forthwith: "Despite considerable investigation, two main questions on the origin of Native Americans remain the topic of intense debate -- namely, the number and time of migration(s) into the Americas. Using the 720 available Amerindian mtDNA control-region sequences, we reanalyzed the nucleotide diversity found within each of the four major mtDNA haplogroups (A-D) thought to have been present in the colonization of the New World. We first verified whether the within-haplogroup sequence diversity could be used as a measure of the haplogroup's age. The pattern of shared polymorphism, the mismatch distribution, the phylogenetic tree, the value of

Tajima's D, and the computer simulations all suggested that the four haplogroups underwent a bottleneck followed by a large population expansion. The four haplogroup diversities were very similar to each other, offering a strong support for their single origin. They suggested that the beginning of the Native Americans' ancestral-population differentiation occurred ~ 30,000-40,000 years before the present (ybp). These values are in good agreement with the New World-settlement model that we have presented elsewhere, extending the results initially found for haplogroup A to the three other major groups of mtDNA sequences found in the Americas. These results put the peopling of the Americas clearly in an early pre-Clovis time frame."

Before going on, we should add to their conclusions that they find no support at all for the 100+ separate migrations required by the logic of the usual Americanist taxonomy -- with its 100+ phyla! I would suppose, albeit without proof, that the biogeneticists find the notion of 100+ linguistic phyla too ridiculous to comment upon.

Since more and more nowadays we ask whose data was sampled and how representative the sample was, we list here the tribes/ethnicities which their study used, either from their own field work or that of the others (like Ward, Torroni, and Merriwether). The donors were from: South America: Xavante, Zoro, Gavião, Wai Wai, Suruí, Mapuche, Yanomama, Wayampi, Kayapo, Arara, Katuena, Poturujara, Awa-Guaja, Tiriyo, Yanomami, Colombian mummies Central America: Huetar, Ngöbé, Kuna. North America: Bella Coola, Haida, Yakima, Athapaskan, @Nootka, Inupiaq & west Greenland Eskimo. :: Then we leave this problem for biogeneticists to solve. Interesting!



@Nootka. They wrote the silly name Nuu-Chah-Nulth which I refuse to publish. The people have been called the Nootka for ages!

In their Discussion a more nuanced, more prehistory-oriented argument is presented. Quoting: (p.1421) "In our previous study (Bonatto and Salzano 1997), using mainly haplogroup A sequences, we concluded that those mtDNA data strongly indicate that all Native Americans originated from a single colonization event that occurred in Beringia >22,000 ybp ago, possibly ~ 30,000-40,000 ybp. We suggested a scenario, based on Szathmary's works (e.g., Szathmary 1993), in which the Native American ancestral population settled in the Beringian landmass during sometime before expanding. Eventually they crossed the Alberta ice-free corridor and colonized the rest of the American continent. The collapse of that corridor, ~25,000-14,000 (Hoffecker et al.1993) or ~ 30,000-11,000 (Lemmen et al.1994) ybp, isolated the people still living in Beringia, from whom originated the Na-Dene and Eskimos (with their reduced overall mtDNA diversity); those south of the ice sheets gave rise to the Amerind-speaking peoples. The present results for the four major haplogroups' diversification ages agree very well with these estimates. When only the mean values are considered, these estimates suggest a very early date (~30,000-40,000 ybp) for the beginning of the diversification of the Native American ancestral population, with a lower bound of ~25,000 ybp."

"At least two types of evidence support the idea that haplogroups' sequence differentiation probably began during Beringia's settlement and not in Asia before the colonization process: (1) our estimates of  $\geq 100$  -fold ancient population expansion suggest that the diversification began during an intensive colonization process; and (2) if

the expansion had occurred somewhere else in Asia, then one should find there sequences, with all markers for each haplogroup, at a high number and frequency, similar to the ~90% frequency found in Native Americans; however, only the founding sequences for each haplogroup have been found in Asia so far -- and they have been found at a very low frequency (see Forster et al. 1996; Kolaman et al. 1996; Bonatto and Salzano 1997). The few additional founding sequences for haplogroup A that have been suggested -- in the Na-Dene and Eskimo (see Forster et al.1996) -- are probably derived ones and will be discussed elsewhere (authors' unpublished data) . . . As for Torroni et al's (1992,1994) hypothesis, our previous results do not support the idea of an independent Na-Dene migration (...), and our present analyses also do not support their suggestion of a more recent haplogroup B migration. Similarly, neither Horai et al's (1993) proposal of different migrations, ~14,000-21,000 ybp, for each haplogroup nor the hypothesis of a Polynesian contribution for haplogroup B sequences found in America (...) was supported. In any case, Torroni et al's (1994) estimated average arrival date, ~26,000-34,000 ybp, for the other three haplogroups is very close to our estimates (...). In general, Forster et al's (1996) scenario for the peopling of the Americas is similar to that which we proposed (...). They postulated a single and early entry (>20,000 ybp) and suggested that, although the Amerinds colonized all the continent and maintained their original diversity, Beringians (Eskimo + Na-Dene) reduced their diversity, because of the climate's deterioration until ~11,000 ybp, at which time they expanded to their present size. Forster et al also have presented coalescence ages for Native American haplogroups, using a data set very similar to our HSV-I -- but

very different methods -- to estimate the haplogroups' age. Although they did not calculate any CI [Note: Confidence Interval -HF] for their age estimates, they suggested ~20,000-25,000 ybp as the arrival time for the Amerinds, which is near our lower-bound estimates. Their haplogroup coalescence ages, however, are probably underestimates of the diversification times since these populations' entrance in the Americas, since they estimated the diversity values on the basis of each haplogroup with each tribe separately. Their results would receive a strong influence from the recent demographic history of each tribe, which would significantly change the ancient parameters that we are interested to estimate. A good example of this can be seen in their estimated age for the Central American Amerinds, which showed a coalescence age lower than that of the South Americans. Far from suggesting that Central American Amerinds originated more recently than South American Amerinds, this result only reflects the reduced mtDNA diversity found in the Chibcha groups, from which all Central American mtDNA sequences came. The Chibcha's reduced mtDNA diversity is thought to have occurred because of recent events (Kolman et al 1995)." End of quote.

It is really not terribly bad news for either Zegura, Greenberg or Turner. The guts of their proposal seems to remain. Again stating that biogenetic dating is not yet sooo reliable, still the Maginot Line archeologists can get small comfort from the Brazilians' paper!

#### Yet Another 'Too Early' Site

Well, just to add injury to insult, in the only terms the Maginot Line archeologists seem to credit, one more excavation shows greater time

depth than the Clovis hypothesis can allow.

As the headline in Mammoth Trumpet said in January 1998: "OHIO CAVE, SEALED SINCE ICE AGE, YIELDS DATA ON PALEO-AMERICANS." In Sheriden Cave which is part of a cave system in Wyandot County, about 40 miles south of Lake Erie the principal excavator, Kenneth B. Tankersley (Kent State U.), and a large team found very rich animal fossils, many of extinct species, plus human artifacts. "The area was repeatedly scoured by Pleistocene glaciation, and was deglaciated only about 14,100 years ago." The excavator had believed that large caves in Ohio, and indeed in the eastern United States, were empty as far as Pleistocene finds were concerned. Now much more can be done by looking in other caves.

The fossil animals included a giant long-kegged omnivore, the short-faced bear (*Arctodus simus*); stag moose with forked antlers (*Cervalces sotti*); giant beaver (*Castoroides ohioensis*) which got up to 9 feet long; flat-headed peccary (*Platygonus compressus*), wide-ranging American pig of the Pleistocene; and caribou (*Rangifer tarandus*). And many other kinds.

At the human artifact levels, which lay below 30 feet of sterile sediment, dates for the range of human artifacts ranged from ~10,000 to ~13,000 BP by radio-carbon. The range was not variability so much as it was top to bottom layers. The nearly 13,000 date was near bottom.

In that same January issue of the 'Strumpet Anne Roosevelt (Field Museum, Chicago) had a careful, precise summary of Clovis dates, along with corrections in them made by various scholars. Her article found that the Clovis horizon has been dated too early; it should be later: circa 11,000 BP.

### Chimps Have the Brain for Syntax?

Normally we leave the hardware questions about human language origins to the LOS folks. We do a bit here and a dab there but not much systematically. In the new Journal, Issue III, we will do much more with our Symposium/Seminar or Round Table discussion of various theories of, basically, brain/mind and language. For the nonce there is an interesting new development which has caused a mild stir in the press. It's based on an article which appeared in SCIENCE 279:220-2 January 9, 1998. The title grabs you right away: "Asymmetry of Chimpanzee Planum Temporale: Humanlike Brain Pattern of Wernicke's Language Area Homolog". An isomorphic link between our close relatives and ourselves in one of the key language (function) areas of the brain. Wernicke's Area is famous as the area most likely to be associated with the ability to decode utterances and generate them to send to Broca's Area where they will be pronounced. If syntax has a home, Wernicke's Area is where it is at. [Note: Pennsylvania usage for where it is located -HF]

Author P.J. Gannon (Mount Sinai S. of M., New York), Ralph L. Holloway (Columbia U.), Douglas C. Broadbent (C.U.N.Y.), and Allen R. Braun (N.I.D.O.C.D., Bethesda, MD) have this abstract:

"The anatomical pattern and left hemisphere size predominance of the planum temporale, a language area of the human brain, are also present in chimpanzees (*Pan troglodytes*). The left planum temporale was significantly larger in 94 percent (17 of 18) of chimpanzee brains examined. It is widely accepted that the planum temporale is a key component of Wernicke's receptive language area, which is also implicated in human communication-related disorders such as schizophrenia and in normal variations such as musical talent. However, anatomic hemi-

spheric asymmetry of this cerebro-cortical site is clearly not unique to humans, as is currently thought. The evolutionary origin of human language may have been founded on this basal anatomic substrate, which was already lateralized to the left hemisphere in the common ancestor of chimpanzees and humans 8 million years ago."

Our colleague, Terrence Deacon advised that this article not be published because the planum temporale is a red herring; it misleads us. Many more of Terry's ideas will appear in the Journal. For now, however, it is good to partake of the rich discussion in the article. Quoting now:

"... The most parsimonious assumption that may be made, however, is that the PT was already lateralized anatomically to the left hemisphere in the common ancestor of chimpanzees and humans about 8 million years ago. Within this evolutionary scenario, however, several distinct evolutionary hypotheses are embedded."

"First, that the asymmetric PT in the common ancestor was unrelated to language or communicative functions but later became coopted to subserve the unique form of human language. Conversely, the PT did not evolve a functional role in communication-related tasks in the chimpanzee lineage and is currently involved with some other function."

"Second, that the ancestral, asymmetric PT was involved with communication-related functions, which then followed disparate evolutionary trajectories during the subsequent differentiation of the chimpanzee and human lineages. Because both of these discrete functional trajectories were founded on a communication-related basal neural framework, they gave rise to the unique and distinct forms of human and chimpanzee 'language' over the subsequent 8 million years. Within this hypothetical framework, chimpanzees would possess the neural

substrate for 'chimpanzee language', which may be mediated through use of a subtle 'gestural-visual' mode we have to understand better.. Many studies have supported this speculative notion based on the extraordinary and diverse cognitive abilities and purported prelinguistic capacities of chimpanzee.."

"Third, it may be that the PT was never, and currently is not, related directly to language or communicative functions in either humans.. or chimpanzees. Instead, the PT may be involved with yet to be understood or tangential functions that are also localized to the PT in the left hemisphere and that may even be common to both species. This latter interpretation would characterize the PT in humans, a brain region that current dogma mandates to be a key substrate for language and other related functions, as an epiphenomenon."

"It is less likely that the PT was symmetric in the common ancestor of humans and chimpanzees and then became lateralized to the left hemisphere in both lineages independently, because this would involve homoplasy, that is, separate evolutionary processes acting in parallel. Furthermore, evidence from SF [Note: Sylvian fissure -HF] length in another living hominoid species, the orangutan, which have shared a common ancestor with humans around 12 million years ago, also indicates that the PT was already asymmetric and lateralized to the left hemisphere at this much earlier time point.. For this reason, it would be instructive to further characterize this region in the closely related bonobo (*Pan paniscus*) as well as the other great apes and lesser apes."

"Regardless of its putative functional role in communication or language tasks, the anatomic substrate of the PT appears to have had a long evolutionary history

within the cerebral cortex of at least hominoid primates. Whether the PT represents the functional substrate of a species-specific communication-related behavior in chimpanzees is currently not known. It has been suggested, however, that cognitive and communicative abilities may have co-evolved during hominid evolution...Within this theoretical framework, it seems reasonable to hypothesize that the PT (which was already asymmetric and likely modally equipotential in the common ancestor) further evolved independently to subserve the species-specific repertoires that characterize human and chimpanzee communication and cognition."

#### Brandt Criticizes Luca's HGHG

In a paper read at the annual meetings of the American Anthropological Association this past Fall, Stephen Brandt (U/Florida) took L.L. Cavalli-Sforza to task over putative deficiencies in the giant book, called HGHG in our publications. Speaking as an archeologist and Africanist, and a specialist in the Horn of Africa, Steve listed these faults of Luca's giant opus:

- (1) HGHG used so-called populations which differed greatly by content, as tribes, pooled ethnic groups, or groups of languages.
- (2) HGHG chose 7 'populations' to represent the many different groups found in the Horn, but not a good sample. Too much lumping.
- (3) Cluster analysis produced some bloopers, like Somali & Khoisan!
- (4) HGHG tries to explain intermediate position of Ethiopians between Near Eastern and Negroid peoples by invoking Arab + Negroid migrants meeting and mixing + resident Khoisaners (Bushmen) in Horn early.
- (5) Khoisan substrate unacceptable; no archeology to back it up.
- (6) Bad business of racial labels.

### Is a 50/50 Correlation Good?

In the long quiet discussion of the correlation between genes and languages, or more properly biogenetic taxonomies versus genetic taxonomies in linguistics, most of the evidence has been supplied by Cavalli-Sforza or his students and/or colleagues. Make no doubt about it -- this discussion is truly worth our while.

But one key question has become salient: how much of an association is there between the two? Especially when we already know that the null hypothesis ( $r = 0$ ) is untenable because too many cases of high correlation exist. And we know that  $r \neq 1.00$  or 100%, proving false because there are too many cases of low correlation. You want examples? Okay, Eskimo languages & genes is almost a case of  $r = 1.0$ , yet English languages & genes is far from a case of  $r = 1.00$ , especially in North America. If we counted cases of pidgins & creoles, we could bring it down to  $r = 0.02$  or such.

We could turn the whole thing into nitpicker's delight and hire a bunch of linguists to worry at the problem for the next century. But the key question for prehistorians really is the one posed by Luca and his colleagues. Thinking in larger genetic units will tell us much more about prehistory than trying to find precision for individual languages. Modern English links up with a remarkable heterogeneity in bodies, but look at the taxon to which English belongs, as my father put it: "a mass of flaxen-haired barbarians", the Teutonic peoples.

The latest effort to find out how workable it all is and to find some correlation to home in on has been made by a group of Latins at Musée de l'Homme/Geneva, Pavia, and Calabria. Twas a major article, appearing in AJHG 61:1015-35, 1997 and preceded by an invited editorial by Barbujani. The title: "Human

Genetic Affinities for Y-Chromosome P49a,f/TaqI Haplotypes Show Strong Correspondence with Linguistics". The authors were E.S. Poloni, O. Semino, G. Passarino, A.S. Santachiara-Benerecetti, I. Dupanloup, A. Langaney, and L. Excoffier. A strong and experienced group of scientists!

#### Their Summary:

"Numerous population samples from around the world have been tested for Y chromosome-specific p49a,f/TaqI restriction polymorphisms. Here we review the literature as well as unpublished data on Y chromosome p49a,f/TaqI haplotypes and provide a new nomenclature unifying the notations used by different laboratories. We use this large data set to study worldwide genetic variability of human populations for this paternally transmitted chromosome segment. We observe, for the Y chromosome, an important level of population genetic structure among human populations ( $F_{ST} = .230$ ,  $P < .001$ ), mainly due to genetic differences among distinct linguistic groups of populations ( $F_{CT} = .246$ ,  $P < .001$ ). A multivariate analysis based on genetic distances between populations shows that human population structure inferred from the Y chromosome corresponds broadly to language families ( $r = .567$ ,  $P < .001$ ), in agreement with autosomal and mitochondrial data. Times of divergence of linguistic families, estimated from their internal level of genetic differentiation, are fairly concordant with current archeological and linguistic hypotheses. Variability of the p49a,f/TaqI polymorphic marker is also significantly correlated with the geographic location of the populations ( $r = .613$ ,  $P < .001$ ), reflecting the fact that distinct linguistic groups generally also occupy distinct geographic areas. Comparison of Y chromosome and mtDNA RFLPs in a restricted set of populations shows a globally high level of congruence, but it also allows identification of unequal



maternal and paternal contributions to the gene pool of several populations". [End quote] I have received permission from AJHG to write a letter, protesting some of their serious linguistic and prehistoric mistakes.

#### OBITUARY

A long ranger of considerable stature, a contributor to glossogonics, and a fine anthropologist has died. Our valued colleague, **Gordon W.Hewes**, has left a big hole in our ranks. Whether someone can fill the empty space remains to be seen, but we can at least mourn the loss and celebrate his life.

While Roger Wescott has written two obituaries, to be published in other journals, we decided that it was unseemly for us to copy what he wrote for them. Roger's eulogy of his good friend can be found in ISCSC and LOS publications.

The following obituary was written by Duane Quiatt, Professor of Anthropology, University of Colorado; (home address) 835, 7th Street, Boulder 80302, Colorado, USA :

" **Gordon W.Hewes**, an anthropologist whose interests embraced and extended well beyond anthropology's traditional four subdisciplines (archaeology, ethnology, linguistics, and physical anthropology), died at age 80 in Boulder, Colorado on 22 November 1997. Hewes, in his teaching as well as, more prominently, in his research and writing, provided something like a one-man justification of anthropology's claim as a 'holistic' approach to the scientific explanation of human behavior. As scholar and scientist he was, in this respect, if not the only show in town, certainly one of the main shows, and he will be sorely missed."

"Born in San Francisco on 29 October 1917, Hewes earned degrees in Anthropology (A.B.1938, Ph.D. 1947) at UC Berkeley. His graduate education was interrupted by World War II, during which he served in Washington, D.C. as geographer with the OSS and, for a year, the Department of the Interior. This duty enabled him to renew early interests in oriental studies and in Japanese and Chinese languages and cultures, which developed into a continuing concern for the comparative study of civilization (1959, 1961). In the last decades of his life Hewes focused this interest on the 7th century, a period which he saw as of particular importance for the rise and efflorescence of world religions, consequently for cross-fertilization of culture elements linked with Buddhism, Islam, and Christianity."

"Hewes's teachers at Berkeley included, notably, Robert Lowie and A.L.Kroeber. His doctoral dissertation, a study of pre- and post-contact fishing in native American populations of western North America (1942), was based on both ethnological and archaeological fieldwork, a combination less frequently observed in these days of specialty training. Meticulously documented, in a manner that Hewes's colleagues and students would come to recognize of his work, this early study continues to provide baseline data for research on fisheries in the west."

"After the War Hewes taught for short stints at the University of North Dakota (1946-49) and the University of Southern California (1949-1951) before settling in, following a year as Visiting Lecturer (1951-52), to a



career of teaching at the University of Colorado, Boulder, where he remained until retirement in 1988, with continuing appointment thereafter of Professor Emeritus. It can hardly be said, however, that Gordon Hewes 'remained' in any one place. He held two appointments abroad as Fulbright Visiting Lecturer, in Japan (1955-56, Keio University and Tokyo Kyoiku University) and in Peru (1960, University of San Marcos, Lima); he served as Visiting Lecturer for shorter periods at the University of Ibadan, Nigeria (1968) and at International Christian University, Mitaka, Tokyo (Summer, 1977); and he gave occasional lectures at other universities while traveling in Europe, Asia, Africa, Oceania and, of course, the Americas. Travel, always in company with his wife, Minna, was an important part of Gordon Hewes's life. He was an enthusiastic and a seasoned traveller. That noted, it must be emphasized that he was, first of all, a broadly accomplished anthropologist who pursued anthropology as an eclectic and a synthesizing discipline. The travel, then, was less for sight-seeing than for site visits, ethnographic observations, linguistic exercises, and comparative examination of, e.g., postural habits, gestural communicative practices, and behavioral customs around the world -- as well as, increasingly, scientific interchange with colleagues from other lands and other disciplines at international congresses and symposia. Anthropology, for many anthropologists, provides a fine excuse for travel. For Hewes, more than for most, it constituted the essential reason."

"Hewes had directed field excavations and archaeological surveys as a graduate student in California (1941) and, subsequently, in archaeological field schools that he ran during summers at the University of North Dakota (1949). In Colorado, he secured sponsorship from the Department of State and the National Science Foundation for a 3-year project of archaeological excavations at Wadi Halfa, Republic of Sudan, thus initiating the first of several salvage operations conducted there by University of Colorado archaeologists, physical anthropologists, and paleontologists. Hewes directed excavations in 1962-63 (1964) and was instrumental in extending the project over seasons to follow. However, around this time he began to concentrate his research and writing (thought by no means exclusively) on issues of human biocultural evolution, issues to which archaeological research was not as immediately important as was integration of knowledge from physical anthropology, ethnology, linguistics, neurobiology, primatology, and comparative studies of animal behavior more generally."

"Chief among these issues is, of course, the origin and evolution of language. His most recent publication, "A History of the study of language origins and the gestural primacy hypothesis" (1996), reviews a field of broadly interdisciplinary studies which Hewes himself was in no small part responsible for reopening and developing for serious scientific investigation, performing the necessary archival work (1975) and setting standards of research and theory formulation (consistently empirical and broadly synthetic) in a series of publications beginning in the 1970s. Readers of Mother Tongue will be familiar with Gordon's work in this area (e.g., 1973a,b, 1976, 1977, etc.), perhaps as well with earlier papers that bear only less directly on the subject (e.g., 1955, 1961)."

"In short, we have lost a general anthropologist whose 200 publications range widely and informatively across anthropology, linguistics, and other scientific disciplines. Many of them constitute major integrative contributions to knowledge, and not a few, particularly in the realm of

language origins theory, have proved foundational."

"Hewes is survived by his wife, Minna Winestine Hewes, whom he married in 1939 and who proved a steady companion and close intellectual confidante throughout his life. A celebration of that life is in planning for Spring, 1998, by the Department of Anthropology, University of Colorado."

"Contributions in Hewes's honor can be made to: The Gorilla Foundation, P.O.Box 620-530, Woodside, CA 94062 or to Friends of Washoe, Central Washington University, Ellensburg, WA 98926."

#### "References

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[End of Quiatt's obituary of Gordon W. Hewes]

We can publish some remarks Roger Wescott made about Hewes in a personal letter in November, 1997. Quoting now: "Dear Hal,

A week ago, the International Society for the Comparative Study of Civilizations, the Language Origins Society, and the Association for the Study of Language in Prehistory lost one of their most valued members -- Gordon Winant Hewes, Emeritus Professor of Anthropology at the University of Colorado in Boulder.

Since Gordon and I have been friends, colleagues, and collaborators for nearly forty years, I will, with your permission, submit a remembrance

of his life and work to the editors of ISCSC, LOS, and ASLIP publications.  
... Yours sincerely, Roger "  
[Only a few technical details about format etc. were left out - HF].

Also, in a later letter, Roger reports: "Minna tells me that the Smithsonian funded a 1964 expedition to Tunisia."

This obituary may reappear in JRAI (U.K.).

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NEWS OF OFFICERS' ACTIVITIES, INCLUDING A CONFERENCE

A workshop or conference was held at the Santa Fé Institute in the city of said name in the state of New Mexico in December. Co-sponsored by Murray Gell-Mann and Merritt Ruhlen, the workshop was called "The Arrows of Time", an apt term for contemplating entities which persist through time.

We go directly to two perceptions of the workshop, John Bengtson's and Hal Fleming's. Since John's is more descriptive of the total scene, we start with that.

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ARROWS OF TIME

A Conference at Santa Fe, December 1997

by John D. Bengtson

There was snow on the ground, a nip in the air, and clear blue skies the three days we gathered at the Santa Fe Institute for the workshop "Arrows of Time and Founder Effects in Language Evolution." The Institute buildings lie on a slope of the Sangre de Cristo hills, overlooking the old city of Santa Fe in the valley. Northern New Mexico is of interest to linguists as a region of long-standing linguistic diversity. Four of Greenberg's major families are represented: the Amerind families of Almosan-Keresiouan (Keres), Penutian (Zuñi), and Kiowa-Tanoan (Tewa, Tiwa, etc.) and the more divergent Na-Dene (Navajo, Apache). Santa Fe itself is in Tewa territory, but the hotel we stayed in is owned by the Tiwa-speaking Picuris Pueblo.

Our host, Nobel Physicist Murray Gell-Mann, charged us with the following ideas as stimuli:

(a) Many features of known human languages, and the history of their evolution (as far as we can reconstruct it), may exhibit trends that go forward in time, and not backward.

(b) Furthermore, the origin of such languages may be sufficiently recent (say, less than 100,000 years ago) that significant characteristics of the original ancestral language (or languages) may be recoverable, through detectable influences (founder effects) that they still exert.

(c) One should take seriously the attempts to classify languages into "super-families" with great time depth, and to learn something about the corresponding proto-languages.

ASLIP/Mother Tongue was well represented at the workshop, with Founding Father Hal ("Father Tongue") Fleming, former ASLIP Vice-President Allan R. Bomhard, current President John D. Bengtson; ASLIP Council Fellows Luca Cavalli-Sforza (Stanford), Aharon Dolgopolsky (Haifa), and Sergei Starostin (Moscow); and others such as long-time ASLIP booster Merritt Ruhlen (Palo Alto), linguist William S-Y. Wang (Hong Kong), and geneticist Stephen Zegura (Tucson).

Discussions were frequently spirited and vigorous. Here are some of my impressions from the workshop, by way of my very personal reality filter:

Fleming: There is a major dichotomy in current approaches to language origins and evolution: (a) "The Deductive Way," dominated by neuro-anatomy, some linguistics (mostly synchronic, Transformational-Generative), evolutionary psychology, and the "hardware" of language; and (b) "The Inductive Way," associated with historical linguistics and paleo-linguistics, archaeology, biogenetics, and the "software" of language. The (a) group or school is mainly associated with the Language Origins Society (LOS), the (b) school with the Association for the Study of Language In Prehistory (ASLIP), though of course there is some overlap of the two memberships.

Ruhlen: "The Emerging Synthesis" (mainly group 'b' in Fleming's analysis) is leading to the insight that there was a very significant event or transition around 40,000 to 60,000 years ago, which is most clearly seen in stone tools. Something happened to dramatically alter the "style" of human activity, and that event was probably the development of modern language -- "Mother Tongue". Only because the event was so recent, we can still find traces of this Mother Tongue in modern and recorded languages.

Wang: Besides the dichotomy between "Apollonians" (logical, analytical, dispassionate) and the "Dionysians" (intuitive, synthetic, passionate), there can be a third way, the "Odysseans, combining the best from both ways in the quest for new ideas.

Dolgopolsky: Opposes the separation of Afro-Asiatic (Hamito-Semitic), Kartvelian and Dravidian from the rest of Nostratic. In response to Ruhlen & Greenberg's "Euroasiatic Cognates," Dolgopolsky provided a list of proposed cognates aiming to show that "Eurasianic" words are found in the other three families as well.

Bomhard: Outlined his current findings on the dispersal of Nostratic languages, the later dispersals of Afro-Asiatic and Indo-European, and their interrelationships with the spread of agriculture.

Bengtson: Offered results of recent research on the Dene-Caucasian macro-family; morphological and lexical.

Starostin: Presented his remarkable STARLING computer program for organizing lexical material. Starostin has also masses of his own lexical collections available on the Internet: Altaic, Caucasian, Sino-Tibetan, etc. For more information, check the web at [starling.rinet.ru] or e-mail at [starling@rinet.ru] .

In sum, I thought this was a very valuable workshop. It assembled a good representation of different views on approaches to linguistic prehistory. Like Rice University (1986) and Ann Arbor (1988), this was a meeting that will produce results for many years to come.

Fleming wrote a letter to many of the participants and some others to test their agreement with a scheme he had worked out. Nobody responded, save Merritt Ruhlen, and he disagreed with most of the ideas. So this scheme represents an unsupported but once opposed way of looking at the participants of ASLIP type at the conference plus some salient other types from the world of historical linguistics at large.

He presents it here in order to get some feedback, in the belief that this scheme stands close to the truth about the strategies and tactics of us 'software' types. In brief, it is a kind of epistemological analysis, aimed at basic assumptions, things stressed, things neglected or scorned.

Dramatis personae are: Joseph Greenberg (JHG), Merritt Ruhlen (MR), Aharon Dolgopolsky (AD), Sergei Starostin (SS), John D. Bengtson (JDB) and Allan R. Bomhard (ARB). These acronyms are to help identify ourselves on the chart below. In addition we throw in Hal Fleming (HF), Marvin Lionel Bender (MLB), Paul K. Benedict (PK), Morris Swadesh (MS), Edward Sapir (ES), your average Americanist (YAAM), your typical Indo-Europeanist (YTIE), and typical Afrasianists of the 1970s (TAA) who contrast sharply with the Muscovite school. Isidor Dyen & Paul Black are (Dyen). The severe enemies of JHG are Campbell, Kaufman, and Goddard (CKG).

The chart which follows is an effort to segregate and separate the basic epistemic approaches of the several kinds of long rangers and some other varieties of historical linguists. No scheme is championed here, least of all HF's which is atypical. Rather we seek to mark out more clearly the similarities and differences among us and others to help find the bases of our conflicts with each other and with the nincompoop linguists.

The left-right dimension or abscissa (symbol x) shows the regular use, or willingness to use, lexicostatistics and beyond that glottochronology, entirely lexicon-based cognate counts. The difference between the two, now partly established in American usage, is not accepted by the Muscovites -- apparently. Or at least that became evident in discussions with SS. So, on the extreme left there is no use of lexicostatistics, while on the extreme right there is full use of both techniques.

The up-down dimension or ordinate (symbol y) shows the preference for reconstruction and/or strict sound laws, as opposed to a preference for taxonomy, classification before reconstruction. Now there is another dimension which we cannot get on the chart which would show a marked preference for grammatical/morphological evidence as opposed to lexical. Most Semiticists and many Aryanologists would be affected by this additional factor. In order to use it at all we put it on the y-axis as a mid point between taxonomy and reconstruction<sup>1</sup>. That is not satisfactory, as you know, but it may be useful. There is a difference between most Semiticists and most Americanists. Let Robert Hetzron (HETZ) stand for the Semiticist dominant tendency. Ehret stands for Christopher Ehret of U.C.L.A.

The two dimensions which govern the chart do not cover one most important attribute of us 8 long rangers, to wit, **boldness & courage**. (Choose whichever you prefer). The dimensions on the chart do not, therefore, cover the thing which most separates long rangers from short rangers. Moreover, the absence of boldness as a dimension distorts Dolgopolsky's true position, as Ruhlen pointed out, which is much closer to JHG.

There are some surprises, if there is truth in the chart. (1) Aharon is quite distinct from other long rangers but much like JHG's enemies. (2) PK came out close to Sapir, his teacher; both scored high on the ordinate because they were taxonomic innovators but did want sound correspondences. (3) Allan and John are close yet this derives from different influences; the one from YTIE and the other from the Muscovites. (4) HF and MLB show the intersecting influences of JHG, MS, and Dyen. (5) TAA may surprise you. Few Afrasianists used Indo-European methods in the 1970s; AD came as a

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<sup>1</sup> Just to keep the record straight. Although some colleagues have implied that they thought of it first, HF invented the dichotomy of "taxonomy versus reconstruction" in 1987 after the Michigan conference. T was published in MOTHER TONGUE too, 1987.



surprise to most of them and was resisted by Semiticists! (6) JHG should be even closer to Sapir because of their strong mutual respect for grammatical analyses. (7) Ehret used ad hoc glottochronology, not Swadesh's.

## A ROUGH EPISTEMIC ARRAY OF HISTORICAL LINGUISTS

[illegible]

Everyone knows that this is not a picture of God's Truth, but let us see how much agreement we can get on the general outlines.

Some fascinating things can also be reported. Perhaps first to mention was the marked moiety division of the workshop into the Dionysian seeming historical linguists on a side of a great table and the Apollonian natural science types on tother side. AD exemplified the passion of the historian side, although HF and SS contributed significant amounts of that marvelous substance from time to time. But the cool high-tech folks showed an ability to generate passion, especially in Zegura's astute critique of the remarkable ways in which historical linguists approached problems. But most frequently the natural scientist cum mathematician sorts were confused, oft dumbfounded, by how the inductive, highly empirical heads on our side of the table worked. It was like C.P.Snow's Two Cultures, except that our side objected, and yes passionately, that we were scientists. At one point AD and SS angrily insisted that the high-tech folks quit trying to run 'our science'.

The whole experience could not have failed to impress an ethnographer or some 'neutral' observer that this was not a homogeneous group of



scientists -- at a minimum -- and that with patience and good will some would come to appreciate the others' results and the strange ways their heads worked. Some would not, thus departing scornful of the other side.

It should also be reported that the American long rangers, or at least HF, were most impressed with the high intelligence and remarkable memory for data, including fast retrieval, displayed by the two Russians, AD and SS. Other long rangers will appreciate that much mending of fences took place too. Not just good will. Both Muscovites are back in ASLIP again. Together we are stronger now. It has been a long decade waiting to regain what we started out with in 1986. Our ain wee Cold War did indeed wound us!

A good time was had by all. Thank you, Murray and Merritt!

#### THE MEMBERSHIP (PERMITTED) LIST

Professor Dr. Giorgio Banti wished to make sure that his name was added to the 'Permitted' list. Accidentally, he had not been listed before. His Roman address is: Giorgio Banti, Vile del Vignola 73, 00196 Roma, Italy.

An updated list of The GOOD GUYS of 1997 who are:

Miguel Aguirre-Martinez / ESA, The Netherlands  
Pietro Baglietto / Genova, Italia  
† Paul K. Benedict / Ormond Beach, Florida, USA  
Martin Bernal / Cornell University  
L.L. Cavalli-Sforza / Stanford University  
Gyula Decsy / Indiana University  
Ronald Christensen / Lincoln, Mass., USA  
Robert Eckert / Grosse Pointe Park, MI, USA  
Bruce Elliott / Sudbury, Ontario, Canada  
Frederick C. Gamst / University of Massachusetts  
Gene Gragg / University of Chicago  
Kenneth Hale / Massachusetts Institute of Technology  
Thomas J. Hinnebusch / U. of California @ Los Angeles  
Grover Hudson / Michigan State University  
Marta Mirazon Lahr / Universidade de São Paulo  
Sydney Lamb / Rice University  
Bernd Lambert / Cornell University  
Winfred P. Lehman / University of Texas  
Saul Levin / State University of New York, Binghamton  
Philip Lieberman / Brown University  
Frank B. Livingstone / University of Michigan  
Jean Lydall / Melle, Germany  
Victor H. Mair / University of Pennsylvania  
D. Andrew Merriwether / University of Michigan  
Kazutake Miyahara / Uji Kyoto, Japan  
Duncan Murray / Temple City, California  
Craig Oehme / Nevada City, California  
J. Joseph Pia / Rochester, New York  
Wolfgang Schenkel / Universität Tübingen  
Jan Vansina / University of Wisconsin  
Paul Whitehouse / London, England

It is a pleasure to be associated with such a group of good people. We can only thank you-all sincerely for saving our common enterprise!