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

Issue 29 (MT-29)

Fall 1997

MOTHER TONGUE : NEWSLETTER of the Association for the Study of

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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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INTRODUCTION TO MT-29: The Newsletter (Editor this issue: H. Fleming)

THE HOTTEST AND THE LATEST NEWS, AS OF MID-OCTOBER, 1997.

The hottest, latest news is not necessarily the most important news -- in the wisdom of hindsight it may even be irrelevant to our common enterprise. But, since the items are new, they have within them the potential of establishing something or dis-establishing something else.

This time around, the hot news is very brief, yet highly important. As everyone knows, evidence that confirms an hypothesis is not decisive, at least according to logicians and philosophers of science, although working scientists are very fond of confirmations. What is more decisive, again as everyone knows, is contradiction and falsification. It remains the case, however, that the interpretation of evidence as confirming or falsifying is not so easy -- not always.

It would seem that Neanderthal has lost his paternity suit. The claims to Neanderthaloid ancestry for modern humans evidently have been falsified. Perhaps decisively. Moreover the African distribution of Homo sapiens sapiens in the time period 125,000 to 100,000 BP seems decisively confirmed by human footprints in South Africa. Oh, yeah? We'll see about that!

NEWS OF MEMBERS' ACTIVITIES, INCLUDING LETTERS OF COMMENT

This rich lode of material was promised for February. So our promises are still none too good! But some of it appears herein. Since the wave of headline-grabbing discoveries and announcements has crested, and probably broken for a while, we predict that members' activities and comments will become a more prominent part of this Newsletter in the foreseeable future.

ANNOUNCEMENTS & ADVERTISEMENTS: THE MEMBERSHIP (PERMITTED) LIST.

The list of members who permit their names to be made public is held in abeyance for this issue. Some few people added themselves but it is better to wait until the next issue for an update of reasonable size. Parsimony!

A few advertisements of things are added on to the end, principally to let our members benefit from book review choices or to support some endeavour which one of our members is engaged in.

OBITUARIES: JOHN KERNS, Søren Egerod, Jan Winter, Mary Haas, R. Stopa.

We note with personal sorrow that good ole Aimo Murtonen joined this group. Southeast Asia took another hit, as Henri Haudricourt has died too. Then ASLIP's officers, as well as Southeast Asia, took an even larger hit this summer, when Paul Benedict was killed. Now Afrasian studies has taken its turn, quite recently; the gifted Robert Hetzron has died.

Let's stop the presses! Never mind the hotter or cooler news for now! Let us see to our fallen colleagues. Let this be an issue primarily devoted to the Obituaries. Five of these colleagues were long rangers in the full sense of those words, while four were wide-ranging and venturesome within more limited realms. Our bonny battalion of frontiersmen has been depleted.

ASLIP BUSINESS

There is much:: Our Web Site which was in a state of flux has now gone cryogenic (deep freeze), until we can pay a professional to handle it. Mary Ellen Lepionka couldn't rescue it :: The "Good Guys" list is enclosed.

Now is time for many ASLIPers to finally pay their 1997 dues. See the attached colored sheet. :: And are there any volunteers out there? You must nominate some officers and directors. Get involved in ASLIP governance.

SOME HOT NEWS, BRIEFLY DISCUSSED

Considerable public attention was paid to two items of prehistory this summer. The dominant one was the successful determination of mitochondrial DNA (or mtDNA) from a very old fossil *Homo*, truly a record-breaking achievement as the DNA came from a 30,000-100,000 ya Neanderthal. As we had predicted, such an event would be viewed as extraordinary. The biogeneticists -- Svante Pääbo and his team at U/Munich -- were painstaking, almost neurotically punctilious, checking everything like a compulsive-obsessive checks his whatever. But the results obtained from upper arm bone were worthwhile scientifically -- this Neanderthal's mtDNA was so distinct from modern human mtDNA that modern humans could not be derived from it. The point of coalescence, if one could be determined accurately, was estimated to be circa 800,000 ya, i.e., the female ancestral to both Neanderthal and us must have lived during what has been generally regarded as later phases of *Homo erectus*.

Basically, this is what Rebecca Cann has been saying for the past 10 years. In this she appears to have won big because a most crucial and startling postulate of her theory -- that no evidence of special inputs from Neanderthal existed -- has been confirmed where it counts, on Neanderthal's own mtDNA. In current American usage we might say: "I knew Eve -- and Lady Neanderthal is no Eve." (That's becoming a cliché remark).

Please recall what we discussed in MT-29 (p.2 et seq.)

". . .The gist of it is that around 780,000 years ago (reckoned by a 'new technique' of geomagnetic dating) a different kind of hominid lived in Iberia. It seems to be ancestral to Neanderthal but not itself the expected *Homo erectus*. . ."

- (4) H.er ---> H.ant ---> H.n
H.er ---> H.er2 ---> H.ss
- (5) H.er --> H.h --> H.n--> H.ss

No doubt (2) is the dominant model nowadays. But either (3) or (4) should replace it, if H.ant holds up well as a taxon."

". . .the Spanish team has declared that their fossil men represent a new species of Hominid younger than *erectus* generally but older than *neanderthal* or modern man. They and some others claim that this new species, to be called *Homo antecessor*, is ancestral for sure to *neanderthal* and very likely to *Homo sapiens sapiens*. . . ."

(Then Rosalind Harding et al's report) --> "Summary. A 3-kb region encompassing the β -globin gene has been analyzed for allelic sequence polymorphism in nine populations from Africa, Asia, and Europe. A unique gene tree was constructed from 326 sequences of 349 in the total sample. New maximum-likelihood methods for analyzing gene trees on the basis of coalescence theory have been used. The most recent common ancestor of the β -globin gene tree is a sequence found only in Africa and estimated to have arisen ~800,000 years ago. There is no evidence for an exponential expansion out of a bottlenecked founding population, . . ."

(Then later -->) "Let us sketch briefly the competing schemes proposed til now in our phylogeny. Let H. = *Homo*, n = *neanderthal*, s = *sapiens*, ant = *antecessor*, er = *erectus*, H.h. = Heidelberg man.

- (1) H.er ---> H.n ---> H.ss
- (2) H.er ---> H.n (Europe)
" ---> H.ss (Africa)
- (3) H.er ---> H.ant ---> H.n
" " ---> H.ss

It is hard to ignore the persistent date of 800,000 more or

less which is associated with these three things, to wit, the Iberian fossils, the new β -globin gene analysis, and the new 'coalescence' for Neanderthal and us (H.s.s.). Possibly this is a coincidence. Maybe it supports new schemes, involving *Homo antecessor*. ¿Quién sabe? (/wák'ni béeka/ 'God knows'. Oromo)

What is not supported, however, and therefore ostensibly falsified, is "multiregionalism" or the "rising tide lifts all boats" theory of modern human evolution; or at least the postulated descent of modern Europeans and the rest of the Caucasoid Realm from Neanderthals. Ah, but was it tested? (See below) Maryellen Ruvolo said (in SCIENCE) "You can't prove [H.n.] were a separate species from just this sequence, but it's very unlikely they contributed to the modern gene pool."

The lesser item of prehistory was the stunning discovery of Eve's very foot prints on a beach in extreme southern Africa, near the Cape of Good Hope. And dated to 117,000 ya, just about the right time. Wow! Although we did have evidence of modern humans in South Africa (e.g., Klassies Mouth), the dates were somewhat disputed and the classification a bit wobbly. But now we had clear evidence that Eve and her tribe extended from the bottom of Africa to the top (Qafzeh in Israel) in the millennia before 100 kya. That is a 6000 mile spread and very hard to blow away.

The National Geographic Society (USA) which had funded much of the archeological work was content to say that the foot prints probably belonged to Eve and that their troops had located the human homeland. There was very great publicity given to these foot prints and the already very rich NGS milked the discovery for everything they

could get out of it. It was in fact so hyped up, so boastful, so modern American, that reaction set in almost from the beginning of the TV broadcasts. (In a later era the NGS may want to be ashamed of this whole episode! The blatant commercial exploitation of an inherently shaky archeological site is surely reprehensible; for a respected 'scientific' society to do it = non buono.)

Ordinary folk wondered how you could tell who the foot prints belonged to. Walking along the beach in Ipswich, I asked many people what they could tell from the foot prints in the sand (firm, wet areas). Mostly they wondered politely how such an odd creature (me) ever got on the beach in the first place. Some excellent non-verbal communication! My wife was much more explicit when I told her about the NGS article. "Pooh!" and then "Baloney!" was about the sum of her comments. Finally, I asked stalwart & reliable David Pilbeam, a fine paleoanthropologist, if we laymen were just too ignorant to tell an old human foot print from one made by a *H. erectus* or Neanderthal. Essentially he agreed with my wife, explaining that he had been consulted on the matter and had advised NGS not to publish the stuff. There is also doubt about the dating because there was almost no serious scientific content amid the hype. How was the dating done, etc.?

Nevertheless, the NGS's hype and hypothesis may very well be true! Could be!

Between the hypothesis and the empirical test (data) stands the Instrument.

As is very well-known in physics and astronomy, one has to consider the instruments when judging if a working hypothesis is true or not. The notion of the

instruments was carried farther by philosophers of science, so that roughly the notion now means all those things which can prevent the actual (logical) test of an hypothesis from occurring. These things include the mathematics or logic of deriving consequences from a theory such that the consequences (or predictions) can be confronted empirically.

Example: "Our theory, based on computer simulations, predicts that this year many hurricanes will hit the Caribbean and eastern USA, causing great damage." Sorry, it was a quiet season. Theory was falsified but probably due to errors in calculation in the computer simulations.

In the case of Neanderthal's mtDNA a host of nasty little factors, such as contamination by the Munich team itself, the test tubes, the measuring equipment, etcetera etcetera, and then the genetic calculations themselves, all could conceal the true mtDNA and the analysis of it. Small wonder that team Pääbo was in a virtual neurosis about their techniques.

And just because they have addressed the instrument problem so heroically their colleagues are tending to accept the findings. No doubt someone eventually will extract more DNA from another Neanderthal and that will be a test of the instrumental phase of the work, as well as the genetic. As both Ruvolo and Harding might predict, we must not expect that other **autosomal** or **Y-chromosome** tests will give the same results, if we manage to get them from Neanderthals or whoever. Yet the basic hypothesis must somehow survive both autosomal and Y-chromosomal tests, if we are to believe it, if we are to see it as confirmed (so far). If mtDNA shows that Neanderthals and modern humans have a common ancestor much earlier than our calculated Eve, but

autosomal DNA fails to support this, then what are we to believe? (Probably the mtDNA results are better, as Ruvolo argues.)

Whether or not there is a speck of dirt on our telescope does make a difference.

In the South African case it is clearly reasonable to suppose that foot prints that look just like a modern woman's beach prints did in fact belong to a modern human female. It is also reasonable to suppose that the dating is correct since responsible South African archeologists were named. But since none of the seriously scientific reports have come out, one is also being quite reasonable to suspect that there might have been an instrumental error. We will simply have to wait!

Sources: The extraction of DNA from a fossil Neanderthal by Svante Pääbo and his Munich team was reported multiply, including on American TV. For a good written summary source see "Research News: DNA From an Extinct Human" in SCIENCE vol. 277, 11 July 1997: 176-77. The original detailed report came out in CELL on the same date, a journal chosen because of its exacting standards, as a way of showing the precision of the team's work. Svante was joined by colleague **Mathias Krings** at U/Munich and two from Penn State, **Anne Stone** and **Mark Stoneking**. However, deserving much credit for initiating the process and providing the fossil were **Hans-Ekhard Joachim** (Rheinisches Landesmuseum) and **Ralf Schmitz** (Rhine State Dep't. of Archeology).

Eve's foot prints in South Africa were reported in a full article with color pictures in the NATIONAL GEOGRAPHIC MAGAZINE this summer (exact reference misplaced) and the world news services of many sorts.

Stone Tools and the Evolution of Modern Humans

Gracias, Marta Mirazon Lahr! A very important paper by Robert Foley and Marta appeared in the CAMBRIDGE ARCHAEOLOGICAL JOURNAL this year. We nearly missed it because we were not scanning journals for this issue, not with any particular system. Theirs is far more significant than most papers because of its scope, its authority, and its true synthesis of two or three fields. Its title is "Mode 3 Technologies and the Evolution of Modern Humans"; it's in CAJ 7:1 (1997), 3-36. Let's go directly to the ABSTRACT:

"The origins and evolution of modern humans has been the dominant interest of palaeoanthropology for the last decade, and much archaeological interpretation has been structured around the various issues associated with whether humans have a recent African origin or a more ancient one. While the archaeological record has been used to support or refute various aspects of the theories, and to provide a behavioural framework for different biological models, there has been little attempt to employ the evidence of stone tool technology to unravel phylogenetic relationships. Here we examine the evidence that the evolution of modern humans is integrally related to the development of the Upper Palaeolithic and similar technologies, and conclude that there is only a weak relationship. In contrast there is a strong association between the evolution and spread of modern humans and Grahame Clark's Mode 3 technologies (the Middle Stone Age/Palaeolithic). The implications of this for the evolution of Neanderthals, the multiple pattern of human dispersals, and the nature of cognitive evolution, are considered." [End of ABSTRACT]

Page 4 of their paper has a

marvelous summary of "The modern human origins debate -- the story so far". Small print and an amazing amount of information on one page sums up the past decade very well indeed; and that is only background for the rest of their article! Now to briefly summarize their conclusions, immediately insisting however that one should consume the whole article. It is that rich. Briefly, the core of their conclusions:

"From the point of view of the origins of modern humans debate, the key conclusion we would draw is that the development of Middle Stone Age technologies in Africa around 250 Kyr is of greater universal significance than the origins of the Upper Palaeolithic. The former may mark a major cognitive development associated with the biological changes leading to the evolution of modern humans; the latter is merely a regional shift in behavioural patterns. Contrasts between the Middle and Upper Palaeolithic should not be underestimated; they represent a significant discontinuity in the archaeological record. But at a global scale continuities of Mode 3 industries also occur. Rather than undermining the "Out of Africa" model of modern human origins, these continuities in fact provide further support by solving various anomalies." Enough said for now. (See below: Marta Mirazon Lahr)

Once Again: Those Stunning Early Dates from Australia

As we reported in MT-27:2-3, the site of Jinmium in northern Australia is crucial to most hypotheses concerning the evolution of modern humans. To rehearse the gist of it is to rehearse the unprecedented dates of more than 115 kya, possibly as much as 175 kya. Such dates would necessarily shift attention from Africa to Australia and by implication to Southeast Asia. But we said: "Key questions

which arise are three: (a) were they really moderns?, (b) was it really art?, and (c) above all, are the dates really true? . . . Since the dating is by **thermoluminescence**, a sometimes unsure procedure, then perhaps the 'wild' dates of 116-176 kya are bogus? It has happened before that such dates were off by a lot, always too too old. Yes, but they have also been right! And we do not know which it is this time - right or wrong!"

So, one year later, after intensive study by several teams, it is now quite clear that -- we still do not know! The problem, or the exquisite technicalities of thermoluminescence (TL) dating, has indeed spread like some purple fungus to other well-known archeological sites, especially to some of our most valuable like Qafzeh in Israel and Katanda in Congo. The latter two are associated with Ofer Bar-Yosef and Alison Brooks respectively who have buttressed their TL dates with electron spin resonance (ESR) dates and others. As everyone knows, these can be correlated with radio-carbon (C^{14}) only up to 40 kya after which the C^{14} dates are unreliable.

The fact that TL dates of 50-60 kya in similar rock shelters in northern Australia have been obtained by Richard Roberts (La Trobe U.) suggests that very painstaking use of TL dating can produce more cogent results. Nigel Spooner (Australian National U.) has even suggested that the Jinmium dates may be as low as 10 kya, although the C^{14} dates appear to vitiate that conclusion since 10,000 years is a reliable date in radio-carbon terms.

So you see that the TL dating in Australia is no trivial matter!

Sources: For a long careful discussion see Ann Gibbons "Doubts over Spectacular Dates" in *SCIENCE*, vol. 278, 10 October, 1997:220-222.

Still Arguing Over Dogs, Genes, and Dates

An exchange of letters in *SCIENCE* recently showed that sharp disagreements still exist among students of the canine domestication of humanoids (or is it vice versa?) Primarily the issues come down to the relationship with wolves, also trouble telling the difference between wolf bones and dog bones in the fossil record. We still see dates as low as 14 kya and as high as 135 kya for proto-dog (domesticated). One clear argument is that there is a dog 'clade' distinct from that of wolves and coyotes, such that there was basically only one domestication, possibly of a Middle Eastern variety of wolf, rather than multiple domestications as some have proposed. Another argument which strikes me as singularly uninformed was that human societies around 5000 to 12,000 years ago "would not then have been capable of keeping dogs separate from wolves..." Yet it would have been quite easy in many tropical areas because dogs would not have found any wolves to keep away from! Like most of Africa and Australasia.

Source: Letters in *SCIENCE*, vol.278, 10 October, 1997. Until the recent proposals for proto-dog of 135 kya, there was a gross lack of fit between biological cum archeological estimates of canine domestication and the conclusions of linguistic research showing ancestral names for dogs in large taxa like Nostratic, Amerind, and probably Borean. The conundrum is that linguists cannot prove the existence of old dogs merely because they have words for such, while archeologists cannot deny the existence of old dogs merely because they've not found the bones yet. A lovely puzzle! . . . But no problem, since the language evidence is usually ignored in the materialist bent of modern biologists & archeologists.

MEMBERS' ACTIVITIES & COMMENTS:
LIMITED AND PRELIMINARY NEWS

Gyula Decsy (U/Indiana) went to Paris this summer to a grand assembly of linguists. At that meeting he delivered a petition from both ASLIP and LOS (Language Origins Society) to the French association and read papers about their activities to a specific session of the meetings. His report is that the petitions failed to receive enough votes to pass. He does not wish to discuss the matter further in public. We cannot escape the conclusion that organized French linguistics, or more precisely one international panel of linguists meeting in France, refused to lift the ban on discussions of language origins, established circa 130 years ago. Mon dieu, c'est fromage!

Jan Vansina (U/Wisconsin). I must report that Jan, one of our greatest historians, strongly disagrees with the picture of the Lemba presented in MT-28. Not only does he believe that the Lemba can be explained quite adequately in ordinary historical terms, qua Bantus. The Lemba are not a caste nor are they 'Venda', being "ordinary people from the Zimbabwe plateau c.1400/1450+ who became Muslim during the E.Coast trade (at Sena most likely) and as traders spread all over the plateau but post c.1560/1600 were shunned + retreated towards the Limpopo. So I am not too gullible re Y-Chromosome and wait for more. As to Jews in East African coast pre-1500, S.D.Goitein's Geniza which traces Jewish trade to India and Somalia has no trace of them at least for c.960-1250+. Not impossible (absence of source is not proof of absence) but not likely, given the wealth of the Geniza. Moreover there is no sign at all that any of this has ANYTHING to do with the rise of old Zimbabwe (c.1250) or its ancestor Mapungubwe." There is no greater Bantuist

than Jan Vansina, although I suspect that he has underestimated what we can learn from the Y-Chromosome. Nevertheless the Lemba story is now in limbo. Or purgatory?

On the matter of the Pygmies of the rain forest: "Blench recovers Hiernaux, but given paleoclimatic history of the forests (quit complex + seerefuges) [sic] ± dispute of whether these people could live in the forests (poor biomasses) or at its fringes only, one has (a) there are people there well before say 2000/3000 BC, (b) perhaps in forests, perhaps on fringes and (c) they most likely did NOT form one single population (demog.) or a single language group but different ones although (d) there were contacts apparently ranging from Ubangi to Uele-Ituri? FINALLY, modern hunter gatherers called 'pygmies' are NOT all descendants of those oldies. A good number of them are farmers who abandoned their fields or perhaps fishing people, etc. This stuff requires someone to really sit down and work the alternatives out." [Apologies to Jan for not giving him a chance to tidy up the punctuation.]

Apropos our discussion of borrowing in MT-28: "This discussion misses the tertius aliquid: internal innovation, the most important bit for historians! 'Borrowing' of course is a kind of innovation, but it is probably no more common than that other kind of innovation: internal invention of either new forms derived from older ones + new meanings or old forms with new meanings. This really does not need to be stressed much more. ... It is the innovations rather than the borrowing which are most precious for culture history at least in the shorter range (last few millennia)."

Finally: "As to family trees see Bantu today (Journal of African History 1995) where in fact a wave model does much better than a tree

because it is such a dialect continuum. That does not mean that trees are useless but that they should be tempered by wave modeling, which is not done much by long rangers. Yet wave models account for obvious cases where one language has more than one ancestor (Horror! Mischsprache!!!) which occurs in situations where the ancestors are both close to each other in a dialect continuum (Good case in Bantu . . . and modern English)."

Zowie! Jan said a lot which is worth discussing at length. But later!

But I must point out that we did cover, indeed stress, his internal innovation but under a different label. In MT-28:16 read: "Strangely enough, ethnology's real counterpart to diffusion was INVENTION which has been the characteristic stance of so much of the 'new archeology' and the many 'eco-freaks' who see everything deriving from systems and contexts. Another logical alternative was HERITAGE or all the genetic traits of body and language. And culture (e.g., religion, song style, games, common law, etc.)." Does invention not equal internal innovation?

Wilfried Schuhmacher. (Dated January 1996, one of the older messages. The argument is important to Southeast Asia and Benedict's work. We will let Southeast Asianist 'pros' respond or not, since Paul cannot answer now.)

"Proto-Austro-Tai *p+l, L, r :
Fact or Fantasy?

Paul K. Benedict, on the basis of evidence from Southeast-Asian mainland languages (especially Kadai, also including Tai), has postulated for his <proto-Austro-Tai>, or PAT, the existence of various consonant clusters with *l/L/r as second element (where the two liquids differ in place of articulation, viz. 'front' versus 'back'), which have been simplified (and unified) in

proto-Austronesian (PAN) (and other languages).

The following remarks are centered around the 'unusual sound change' of PAT *pl, pr > PAN *t. E.g., PAT *(m)lalaq 'earth' > PAN *tana?; PAT *mapra 'eye' > PAN *mata. (As for PAT *pL > PAN *t, a development pL > pl > t may be assumed.) Regarding the Sapir discipline's (PKB) reconstruction of p+sonorant > t, keeping the stop from the p and coronality from the sonorant, from a purely phonetic (articulatory) point of view -- recalling all what my esteemed teacheress (Professor Eli Fischer-Joergensen) taught me about 30 years ago -- such a sound change sounds suspicious."

(Schuhmacher, continued : Ed.)
"Adding Austroasiatic (AA), i.e., tackling the issue with Austric -- the superstock first proposed by Wilhelm Schmidt in 1906 and recently dug up again by I.T. Pejros and La Vaughn H. Hayes -- does not give us, it seems, new insight as reconstructing Proto-AA clusters is not a straightforward and easy matter: In many cases, also AA has t, or what we see as clusters in the modern languages were probably CV+l, L.r sequences at the proto-AA level.

Therefore, though not as ultima ratiō [sic], I want to point to the possibility of a combinatory sound change:

	PAT(Ben)	PAN	PAT(Sch)
'die'	*(ma-)play	*matay	*matlay
'eye'	*mapra	*mata	*matra
'live'	*qubrip	*u[d]ip	*qudrip
'earth'	*(m)plalaq	*tana?	*mtlalaq

(?)

I.e., in the case of the presence of a PAT (Schuhmacher) initial / final labial consonant, progressive/regressive assimilation would explain the occurrence of another labial stop in Kadai (and maybe even the different AA reflexes) whereas the second (sonorant) element would have been lost in PAN. Quod demonstrandum est in Macro-

Australic: Cp., e.g. PAT (Schuhmacher *matra 'eye' and Australian: proto-Paman *maari; Indo-Pacific: Tasmanian (ME) *mongte(na) 'eye' -- with no labial involved. The 'unusual sound change' therefore would turn out to be realitter [sic] a combinatory sound change." [End of Schuhmacher quote. ED.]

Dell Hymes (U/Virginia wrote a long and very helpful letter about the problems raised in MT-28 about the fossil man of 9000 ya and Umatilla tribe of Oregon that wished to protect the fossil as one of their ancestors. Dell is a world authority on Oregon and its neighborhood. To the question of what larger linguistic group the Umatilla belonged to, Dell replied that Sahaptin was the answer. This makes the Umatilla a member of the great -- and naturally controversial -- Penutian branch of the North American subphylum of Amerind. As Sahaptin the Umatilla could generate a decent claim of long long residence in Oregon or the areas around it. Eschewing that controversy, Dell gave a great deal more information about the Umatilla and their relations with whites, etc. Too much to reproduce here but his letter can be copied for interested members. (I'm sure he has no objection to that; if he does, he'll tell me!)

Marta Mirazon Lahr has long since settled in to Brazil, the giant country bordering her native land of Argentina. Since Marta has given us such valuable material before, and hence members may wish to write to her, we give her address here. MML / Departamento de Biologia, Instituto de Biociências / Universidade de São Paulo / Rua do Matão, Travessa 15, N°321 / 05508-900, Cidade Universitária / São Paulo / Brazil.

We just barely got her article before printing this Newsletter, so we had no time to get permissions to reproduce from Cambridge. We

hope to do so in the near future. Subject to the blessing of our President and V.P.s, we would wish to have a MT*Treatment of this paper as soon as possible.

It needs also to be said that (a) much of their hypothesis was anticipated in Ofer Bar-Yosef's remarks made in MT-23, (b) their focus is on 'human origins', not on 'language origins' and (c) it is fair to say that language evidence plays virtually no part in their evidentiary base. This is basically how the past decade of human origins research has proceeded; language is subsumed under a general cognitive/behavioural trait along with culture, but the conclusions of linguists tend to be ignored, despite Renfrew & LL C-S. Clearly, the belief of most linguists that they have little to say about deep prehistory is fundamentally responsible for this cognitive cavity. Our friends in archeology and paleoanthropology do not know what to do with us -- and many are friendly and interested -- because of our self-imposed taboos. Oui, encore, c'est fromage!

| OBITUARIES |

It is with a substantial sadness that I write these brief notices. In most of the cases full bibliographies and biographies have been published elsewhere. Or the information was simply not available to us to its fullest extent. Consequently, these notices are written casually, with less attention to the more formal things usually put into obituaries, such as good birth/death dates, marriages, education and such, but more attention to the significant from our standpoint. The major exception will be John Kerns whose obituary was written by Allan Bomhard.

We do invite ASLIPers to send comments small or large to augment what we say here about our good colleagues.

Paul K. Benedict. A prince of a man. To say -- gentleman and scholar -- is not to be trite; he truly was both. PK or PeeKay as his family called him made major contributions to Southeast Asian prehistory in setting up Austro-Thai, a crucial taxon for the region. His unraveling of Siamese (Thai) from Chinese in particular and Sino-Tibetan in general was one of the great enabling hypotheses in historical linguistics, freeing a mistaken classification from the bonds of deep old borrowings and influence. The bio-genetics of the Chinese north-south clines which made little sense earlier became much more intelligible when it was realized that the south Chinese patterns were not based on mysterious alien aborigines but on a large and vigorous absorbed people -- both former and current Daic speakers -- who had made many valuable contributions to early Chinese itself.

His contributions to Sino-Tibetan studies were serious. His *Conspectus* or reconstruction of proto-S-T has remained a standard reference book. His ideas on the internal classification of S-T have also been influential. It was on this subject that I most seriously misread his message last year when I thought he was favoring George van Driem's complete reorganization of S-T. After I presented the van Driem revision to ASLIPers with enthusiasm -- because Paul had told me about it -- some months passed before Paul mentioned that he was rather miffed that I had replaced his taxonomy with George's. It seems he still thought his own was better, but being the big-hearted fellow he was, he had also heralded George's work.

That a happy man, full of joie de vivre, respected by colleagues at the many conferences he attended, completely recovered from earlier heart surgery, loved by family and friends, that such a man should be killed in a stupid automobile

accident (not his fault) seems a damned bloody shame!

He had just turned 85 and was on his way to the vast wide beach of Daytona to have a good walk and a swim. No doubt capricious Fate pointed its fickle finger at him that day, July 24, 1997. F---ed by the fickle finger of Fate.

ANYWAY, let us move on. Such would be Paul's attitude. Anyway, what else to say? Like a minority of linguists and long rangers, Paul thought of himself as an anthropologist first and foremost. He was trained in that field by Americanists (e.g., Edward Sapir) who included both linguistics and history in their purviews. But that was the 1930s when 4-field approaches still existed and were actually followed. It was the anthropological viewpoint that made Paul tolerant of long ranging when he first encountered us. That orientation plus Paul's own experiences in SEAsian historical linguistics where mighty concepts like Austric and Malayo-Polynesian were standard fare.

Yet in many ways Paul was a fairly conservative long ranger, more like the Muscovites in his predilection for reconstructed forms and sound laws. His best buddy in all this was the conservative Matisoff of Berkeley, to whom Paul left his large language data base. In a sense Paul was also torn in recent years between Matisoff's inclinations and those of the ASLIP long rangers. Paul firmly rejected the Austric hypothesis because he believed that the few lexical look-alikes incorporated in the evidence for Austric could be accounted for by borrowings. In this respect he became more of an obstacle to further progress than anything else; his basic stance was more like that of Campbell or Trask. In due course his younger colleagues, especially Blust, Hayes, and Diffloth, had to work around him to get to Austric. But we must remember that it was the attention to borrowings that

had led to his first major breakthrough, the separation of Thai from S-T.

Still the boldness, again akin to the Muscovites, propelled him towards the 'Japanese is Austro-Thai' hypothesis. Like grabbing honey from an alert hive, Paul aroused plenty of opposition from Altaicists who had just settled down to incorporating Japanese and Korean in their larger conception of Altaic. They went after him from all directions, stinging mildly however. What is good to notice is that Paul went about Japanese and Austro-Thai in his typical manner, depending primarily on proto-forms and careful reasoning about the history of Japanese. Most recently he again confronted the borrowing problems of which Japanese is a supreme example, having decided that the Altaicists had a point -- there was a lot of Altaic in Japanese -- but deciding further that those Altaicisms were **borrowings** or sub-stratum, while the Austro-Thai-isms were proper old cognates.

No doubt Japanese will enter the 3rd millennium as a Mischsprache, much like Mbugu (Ki-Ma7a) of Tanzania, with roughly equal parts from each of two genetically divergent super-phyla. Contrary to Paul, however, some of us scholars reckon that Japanese entered the islands as an Eurasiatic language with sister Korean near by, both akin to Altaic, where Japanese-Ryukyuan met a strong population of Ainu and southerners of Austro-Thai persuasion who were absorbed eventually. Of course, Ainu itself is another problem but we'll skip that for now. This interpretation bases itself more on archeology than anything else; many Japanese scholars agree.

Paul's working methods were not stupid, incompetent, misguided or peculiar, as some linguists are wont to say. Paul was supremely **intuitive** even if he could crank

out the reconstructed forms and sound laws to satisfy the purest Aryanology. His methods are much like mine and, in less explicit ways, very much like Greenberg's mass comparisons. Paul carried in his head a mass of data. When working on a problem, he inspected more masses of data, formulated hypotheses, tested them internally, then spat out the conclusions nicely formulated in the proper Indo-European manner.

He was very quick at analyses, even on data new to him. Once on a visit I showed him some comparative Nomotic data which he promptly reduced to a prefix and a following base/root. For example, a series for 'bone' in the forms /megats, muk'ats, mik'ic, etc./ he immediately found a prefix **m-** and a base probably in ***k'ets**. Before he got far in comparing this to Austro-Thai, I had to tell him that we already knew that ***mak'-ets** was closer to the truth, based on comparative evidence for 'bone' and the existence of an old glued-on suffix in /-ts/. Some others have made the same mistake using this Omotic data, even some Afrasianists. So Paul's **quickness** is the point of this, not his ability to make mistakes.

The above also illustrates Paul's marvelous **confidence**. Once he had examined the data carefully, made up his mind, and formulated the theory he was very hard to dissuade. Politely, with humor, never offensively, he stood his ground calmly. On the 'bone' discussion above Paul and I argued for an hour, with me finally losing my temper and shouting, before he conceded that I probably knew more about Omotic than he did. He never even raised his voice. A prince of a fellow -- but with iron on the inside!

Paul could argue very capably. He once reminisced about his childhood family, basically of 1920s western New England, saying that

his father heartily encouraged the family members to debate things at supper. The ability to debate was cherished by the family, along with the ability to stay reasonable and even-tempered during the debate. What a rational background! This reminds me that Paul was what the Germans call 'Landsman' to me. We came from the same area (west Connecticut), same ethnicity (Yankee) even probably related by marriage, same dialect, but not the same religion as Paul's family were free-thinkers while mine were devout Protestants. We were not encouraged to debate or sing or enjoy ourselves during supper.

Paul's wife: Marilyn Benedict, 104 River Lane, Ormond Beach, Florida 32176, USA (tel. 904-441-2694) would love to hear your comments, memories.

Robert Hetzron. Recent and unexpected. Robert was born in Budapest on New Year's Eve, 1937. His wife to be, Gabriella, was born on New Year's Day. He died this summer as he approached age 60. He had been ill for several years and retired from U/California @ Santa Barbara, but his illness had no particular morbidity expectation attached to it. So it was not exactly a surprise but the unexpectedly expected as in many men in their 50s.

Think of a mild-mannered man with a soft Hungarian accent, talking to you politely in a friendly way. Imagine that he can hold that same conversation with speakers of many other languages. I once got a count of his language skills and was very impressed. Forgot how many there were.

Robert had studied in Paris among other places and had lived in Israel among other places, ending up as a professor of Germanic at the University of California @ Santa Barbara. For those who have not visited that city on the Pacific suffice it to say that Robert was not unlucky to live in that

beautiful spot.

Unlike Paul Benedict and his robust good cheer, Robert exuded a sadness which was sometimes palpable. Not necessarily because he was a Hungarian, although Hungary is a leading country for suicide rates, since two of our Hungarian colleagues are hearty and cheery. But it had not always been so; memories of a much happier Hetzron persist from the 1970s.

Fundamentally, Robert was a Semiticist and grounded in the traditional Semiticist erudition; Hebrew and Arabic, of course, but many others. However, he was ultimately seen as a Cushiticist and an emerging expert on the whole of Afrasian. His Semitic work included much serious change in traditional Semitic sub-classification, most pointedly perhaps his moving Arabic from its association with Ethiopic and Sabeian in South Semitic to a new membership in the 'central' group, which includes Canaanite and Hebrew. In the case of the faulty yet established 'Gurage' group of Ethiopic, Hetzron presented a new scheme for South Ethiopic which restricted Gurage to a small group of languages around Chaha and recognized that so-called Gurage (the original erroneous one) contained at least three distinct South Ethiopic lines (clades) to be distinguished from each other. Fleming had come to similar conclusions in an earlier lex-stat study of South Semitic.

In Cushitic Robert did important work on Somali and Agau, producing the first ample data on Awiya (Awngi). Shifting to a more comparative mode he examined the southern regions of Cushitic, concluding finally that Eastern and Southern Cushitic were much closer to each other than had been proposed and indeed together constituted a distinct branch (clade) of Cushitic. Finally, he extracted Beja from Cushitic, setting it up as a distinct branch of Afrasian as a whole.

These were all fairly bold moves!

While his work on Agau and Somali is still appreciated, and his interpretation of Gurage being now widely accepted, his other ventures are respected but too bold for many scholars. The closeness of East and South Cushitic is debated but is still controversial. Some like myself do not believe the two to be at all close, even if similar in many respects, others like Ehret are inclined to go along with Hetzron. On the subject of Beja the opposition is stronger, not vociferous, just quiet but strong. Fleming is in agreement with Hetzron on Beja because he himself proposed the same thing at a conference prior to the one where Hetzron made his proposal. Ehret, however, does not go along with Hetzron (or Fleming) on Beja, preferring to see Beja as Cushitic. Unfortunately, as far as I know, Robert did not follow up on his Beja venture. This would have made a difference either way because he was a very careful scholar, even if bold about standing by his conclusions.

Perhaps more than most historical linguists, certainly more than most Afrasianists, but more like other Semiticists, Hetzron primarily emphasized morphology and the criterion of shared innovations but only grammatical (morphological) ones. Such an approach does require that a strong notion -- of what the ancestral morphology was like -- be present to begin with. Then one can detect which changes have been made in that structure and which languages (which grammars) share in the specific changes. Robert was not always successful at persuading colleagues that he knew the ancestral grammar well enough to detect the changes in the structure over time. Occasionally he trumpeted the deficiencies of merely lexical approaches, and insisted a trifle dogmatically that morphology was the only real vehicle which would carry us towards taxonomic truth.

But he was far too gentle a man to persist in the fruitless lexicon-versus-morphology wars for too long. His predominant view was that he was happy that lexicalists got the same results he got morphologically. In the absence of good grammars for many key languages many of us found the lexicon more available and more practically helpful in classifying languages. Although some of us got labeled as lexicalists by the grammarians, all of us really supposed that both morphology and lexicon were valuable. To underscore Hetzron's gentleness one need only remember how harsh and overbearing morphologists could be, armed by their training with direct access to God's truth. Or so they believed.

Robert was seared by personal problems which affected him and his work tremendously. Although, for the rest of us, it is not our business what the problems were, there was no escaping the inference that he was suffering. You could feel his pain as he described his troubles. No doubt this pain partially blocked or aborted his full development, for he did not come to full fruition. With his solid background in Uralic, Semitic and Germanic, he would have made sizable contributions to the still developing field of Nostratics. As he was a long ranger, a member from the beginning of what became ASLIP, he was examining the possibilities of that Nostratic field when his illness slowed him down drastically. Very sad! For all of us!

Robert had a reputation among historical linguists as brilliant. It was richly deserved, in my opinion. I surely would like to have him back and working as he did in the 1970s. I know many others would join in that sentiment. Robert also had many colleagues, given his work as co-editor with Bender of a book on Ethiopian linguistics and given the time he served as editor of the journal AFROASIATIC LINGUISTICS.

A good man has died. A fine scholar has stopped his work prematurely. Amín. Amen.

We do not know what the final disposition of his books and papers will be. For those seeking information, or to help, contact:

Gabriella Barber, 698 Zink Avenue, Santa Barbara, CA 93111 USA, tel. 805-964-5575 or E-Mail

< magyar@west.net > If you write, do mention where your expertise lies. She could use your help.

Aimo Murtonen. Since Professor Murtonen followed the British habit of reducing his first name to an initial, I persisted in calling him 'Adam' in hopes of provoking him to tell me what his true first name was. He never told me. Only on getting notice of his death did I learn that he was 'Aimo' not 'Adam'. While it is possible that Aimo is Finnish for Adam, I have not pursued this matter. It seemed of so little importance to him.

Here is the difficulty in writing obituaries. Aimo is another good man who I knew personally and liked a great deal. He was also quite distinct from Paul and Robert. I would call him very Finnish -- intrepid and self-reliant, good-hearted and sturdy, sceptical but not intolerant, needing to be shown why something ought to be believed but willing to believe if he could be shown why. A fine mind and an open one. (There are many Finns in New England. My village, Lanesville, in Gloucester was populated primarily by Finns who worked in the great stone quarries of Cape Ann. There is an operating sauna less than a kilometer from my house.)

Aimo was also very tough. Perhaps hardy is a better word. We first met when he was already elderly but had nevertheless set off on a trip around the United States by bus and all by himself. We met in a student pub on campus and soon my students loved him for

his stories and hardiness. The next time I met him he was traveling alone in Ethiopia, already having troubles peculiar to the aged (e.g., palsy), nevertheless still working with informants. Gutsy!

Aimo was primarily a Semiticist, although he did spend some time working on Australian languages. He did not fall into the grammar-first bag but rather sought information from both grammar and lexicon. He was a pioneer in arguing that the Modern South Arabian languages (Soqotri, Mehri, Shhauri, Botahari) were a distinct branch of Semitic, not simply derived from Sabeian, not such close sisters of Ethiopic. While he did not overturn traditional Semitic classification, his ideas were promoted by both Muscovites and Fleming. Robert Hetzron, however, strongly opposed Aimo's hypothesis and was a most crucial person in its defeat.

Aimo was so much like Juha Janhunen, and reminding me somewhat of Raimo Anttila, that I wondered if there were a Finnish school or style in historical linguistics. Unfortunately I cannot find the source of a quote to the effect that a earlier Finnish guru had stressed caution as the most important thing for his students to consider. For it has been characteristic of our Finnish colleagues that they have been very well-informed about particular topics, certainly highly intelligent, yet tending towards such scepticism that they supported few long range hypotheses, even though they listened carefully to other long rangers and were tolerant of their viewpoints. Indeed it is a long way from a sceptic like Aimo to a howling counter-revolutionary like Poser or Goddard.

Aimo was a thoughtful and reasonable man. I shall miss him.

Roman Stopa: (Written by Eric de Grolier, First Secretary, Language Origins Society, to whom thanks!)

Quoting now:

"Professor Dr. Roman Stopa
(August 8, 1895 - April 15, 1995)

Roman Stopa had been elected honorary chairman of the Language Origins Society at its first meeting in Cracow, 1985. This was indeed a justified recognition of his life-long research in the field of language in prehistory, begun with the preparation of his book on clicks Die Schnalze, published on recommendation by the famous Africanist Carl Meinhof in 1935 (reprinted in 1986 with an introduction in English by Gyula Décsy) and ending with a typescript comparing Khoisan forms with those listed by Dolgopolsky in his 1964 paper on the supposed 15 'stablest' words in human languages, which he sent to me just three weeks before his death.

My own interest in 'long range' comparisons was very much stimulated by reading Stopa's book on Structure of Bushman and its Traces in Indo-European (1972), which was a follow-up of what I consider his 'magnum opus': The evolution of click sounds in some African languages (1960), unfortunately almost ignored, the whole edition of 750 copies having been left in storage and not distributed, due to the stupidity of a Polish bureaucrat, who labelled the book as 'racist'.

Stopa was invited in 1971 to participate at the colloquium organized in Roma by the Italiana Accademia nazionale dei Lincei, for the centenary of Darwin's The Descent of Man; his paper 'The Origin of language' (pp.295-315 in the Atti, 1973) is probably the best synthesis of the views he developed during the following 24 years of his life. Stopa's comparisons between Khoisan and chimpanzee's vocalizations, which he found in Miss Learned's booklet (1925) were apparently first published in this 1971 paper, and then reproduced in

The Structure of Bushman . . . cited above (pp.29-35 and 50-57), in his 1979 Clicks: their form, function, and their transformation (1979:44-51 and 100-102) and in his presentation at the Symposium I organized in 1981 (edited in the volume of proceedings: Glossogenetics, 1983:491-512); they were scorned by Traill (1978:139), as well as Stopa's comparisons Khoisan-Indo-European (idem:145) which the best German specialist of Khoisan, Köhler, had more mildly qualified as 'without demonstration strength (Beweiskraft)' (1975:337). Stopa's writings were deserved; in his critics negative appreciations, by his somewhat old-fashioned vocabulary, which included obsolete terms like 'Apeman', 'Grimaldi race', 'primitive traits', etc. After all, these were the words usual when he studied anthropology!

I am convinced that, when unprejudiced scholars will seriously study Stopa's works, they will find there ample stuff we would qualify in French as 'matière à réflexion' (data worth thinking over) and will consider him as a precursor in this (still largely to be developed) discipline I proposed to name 'glossogenetics'.

As for me, and many colleagues who knew Roman personally, we will certainly keep in our memory his fascinating charisma." [End of quoting].

[Editor's note: 'Beweiskraft' is strong in German but lacks an apt mate in English. One dictionary suggests 'probative force' as a translation. Not bad but probably better would be 'power of evidence, evidentiary force'. Best of all would be to say that Stopa did not 'make a strong case'.]

[Personal note: Eric de Grolier has touched on a topic of some concern to us, not great concern but noteworthy. He mentions the ideas of an older anthropology and here we

might invoke the notion of 'cultural lag', wherein one part or sector of a culture lags, has not picked up changes in other parts or zones of a general culture. No doubt we can say that much of anthropological and biological reasoning after Spencer and Darwin became infused with what is now called social Darwinism but also there arose racial hierarchies with 'the Nordic man' at the top and 'primitive' Africans and Australians (and others) at the bottom. That is, a century ago it seemed totally obvious to many scholars that the dark peoples of tropical lands, many of whom couldn't even grow crops or write their names, must represent the archaic human condition, the primitive state of nature, what we all evolved out of, up from. It seemed totally natural to say that their intelligence was less, their awareness less evolved, their cultures backward, and their languages primordial, archaic, aboriginal. Their bodies, their cultures and their languages were ipso facto also inferior, while Nordic bodies, European cultures and languages were also superior. We really do know, if we merely glance at our past history, that Hitler and his party did not invent the 'master race'. Similar notions suffused the upper classes of western Europe and North America. At one point the English upper classes even got so enamored of themselves that they began to support theories about the fundamental inferiority -- racially, culturally, and linguistically -- of the English working class! So this was the status quo ante. Should we add the powerful eugenics movement of the 1920s too? Sure!

Anthropology began to change first, although the acid comments of Marx on European ruling classes no doubt contributed too. A very substantial force in that change was Franz Boas and his students

from say 1900-1940. Another was the alliance of secular Jews and lapsed Protestants against Christianity and the established ruling culture in the United States. (See a most interesting recent book: SCIENCE, JEWS, AND SECULAR CULTURE by David A. Hollinger. 1996. Princeton University Press.) But in the rest of the anglo-phone world the Social Darwinist paradigm was undermined by neglect; the functionalist and structuralist paradigms forcefully threw out most interests in evolution or history. Parallel developments in franco-phone anthropology had similar effects. From Durkheim to Levi-Strauss dominant attention turned away from 19th century pursuits. So it came to be that most of the Atlantic Rim countries gave up the old paradigm, first in the social sciences, more slowly in biology.

One may resist a culture change, indeed one may defeat one. Either way from the standpoint of this concept the unchanged or resistant variant is an example of culture lag. Compared to my children, for example, I am the perfect example of culture lag with respect to music and dance. This does not necessarily mean that my taste is inferior or silly; it just means that I reject their music/dance culture. Well, I like some of their music.

So it has been in central and eastern Europe and the former USSR. Some elements of the old paradigm were dropped. In official Communist theory which sat on the area for 50-70 years 'racism' was a bad thing, but in that same theory a strongly marked cultural evolution, unilinear type, was de rigeur. What seems to be true, and it always surprises me, is that the notion of primitive continued in linguistics and it was attached to the same dark tropical people as always. The notion of primitive in Atlantic Rim linguistics is dead, kaput, as far as I know. Unless we are talking

about modern concerns about language origins, one doesn't hear about primitive languages or what they are supposed to be like. There are no criteria that I know of, other than recent theoretical ones.

So I take the assumption that Khoisan is an archaic, primitive or aboriginal language to be a case of culture lag in international linguistic culture. Since we are dedicated to getting back to the primitive state of human language (in our time machine), and to reconstructing proto-human, then questions of archaic or primitive contemporary languages is hugely important to us. If Khoisan or Nihali or Asmat Papuan are truly archaic, then we should be focusing all our attention on them, n'est-ce pas? Why? Because they are supposed to exemplify what proto-human was like.

But for Stopa and his colleagues the key questions are five. First, do you already know what proto-human was like? Second, if you do not know what proto-human was like, why assume that Khoisan is most like it? Third, was proto-human probably simple or complex, grammatically or phonetically? Fourth, why not choose Polish or English or Chinese as examples of simple proto-human language types, since Khoisan is far more complex by the above criteria? Fifth, why not compare Yoruba or Thai with chimpanzee talk, since both are highly tonal and have short words & morphemes? The sounds emitted by any Khoisan speaker vastly exceed the vocal output of any chimpanzee. Myself thinks that Parisian French with its avalanche of nasal and friction sounds would come closer to chimpanzee. Or Portuguese. But not very close! (Je vous demand pardon, mes amis!) (Perdoeme)

Mary Haas. (Formal obituaries are presumed to be available in a number of places in American linguistic and/or anthropological

journals. We present here very briefly a short appreciation of her contribution to Americanist linguistics. For those unfamiliar with her biography it is pertinent to mention that she was wife to Morris Swadesh at one point in their lives.)

Joseph H. Greenberg, writing in ANTHROPOLOGICAL LINGUISTICS, has a forthcoming article entitled, "Mary Haas, Algic and the Scientific Consensus", which we may quote from briefly. Some observations made by Greenberg include: "In 1958 Mary Haas published a landmark article which for all practical purposes ended the widespread doubts and long period of controversy concerning the validity of what is now usually called the 'Algic' stock, consisting of Algonquian, Wiyot, and Yurok, first proposed by Sapir in 1915. It is a tribute to the enormous influence of Mary Haas in American Indian linguistic studies that this intervention proved decisive and essentially ended debate on the basic point.

The importance of this intervention, whose implication extends beyond the specific point at issue and has implications for historical linguistics in general, is highlighted by the fact that although the main point was settled, there remained a penumbra of problems as shown in continued discussion virtually up to the present day."

[End of quoting.]

Basically Greenberg is observing that Sapir's Algic hypothesis was proposed in 1915 but opposed so vigorously by Truman Michelson, the leading Algonquianist of that time, that until 1958 Algic had not yet been accepted by Americanists, including Algonquianists. Mary Haas had proposed another grouping, Muskhogean and Natchez, which had been accepted without fuss. She remarked that, had she been opposed by some sort of Michelson of Muskhogean studies, her hypothesis probably

would not have been accepted either. In any case Mary Haas announced in 1958 that she had reviewed Sapir's argument and data and had found that Algic was quite acceptable to her. That carried the day for Algic. Greenberg also pointed out that, contrary to some interpretations of the event, Mary Haas had no new data to bring to bear on the topic. Her approval was based on Sapir's original article of 43 years past. . . . Why does all this remind me of Sr. Trask?

[Editor's Note: Something else of importance included in the above article about Mary Haas has to do with a rare analogy. Greenberg in commenting on the difficulty of the Algic problem had this to say: "I can only report my reactions at that time [1953± Ed.] to the Algic controversy. At this point in my work on my African classification I was concentrating on what at that time were called the Sudanic, or Eastern Sudanic languages. These are now generally accepted, with the exception of Kordofanian which belongs with Niger-Congo, as forming a valid grouping Nilo-Saharan, a family of great historic depths. At that time, however, I still posited 12 families in Africa. I had heard about how difficult and controversial Algic was. When I looked at the evidence, therefore, I was astonished. In the African context, I would have considered Algonquian, Wiyot, and Yurok as probably belonging to the same branch of Niger-Congo. Experience in looking at language on a wide scale does, I believe, give a background for evaluating the significance of resemblances. I had expected a marginal and difficult case and instead I found a commonplace instance of a reasonably close relationship and my comments reflected this."

[Editor's Note - The meaning here is by analogy. Had these Algic

languages been Niger-Congo they would have fit together in a branch, i.e., they are no farther apart than are the members of a single branch of Niger-Congo.]

This provokes an aphorism from me. Given a group of 3 languages, a troop of Americanists will immediately perceive how positively unique each one is. Given those same languages, a troop of Africanists will immediately perceive how much the three have in common.

John C. Kerns. Our obituary for this gifted and amiable colleague is two years past due. It was not the fault of the writer because Allan Bomhard submitted the following obituary many moons ago. My apologies for the several postponements of the obituaries. While Allan's obituary is eloquent enough, and cast in splendid type, I do wish to say that John was another of our gifted amateurs who have matched the professional linguists in their long range works in the past decade. More are joining every day, you might say. (Eh bien, plus de fromage.)

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[Editor's Note: Following Bomhard's obituary of John Kerns (overleaf), we have five pages of an extraordinary letter written by PK Benedict to me on Sadie Hawkins Day 1996. That is the Leap Year Day he refers to. We decided to print this chaos of ideas and undecipherable abbreviations, and interesting remarks on particular persons, because it was so Paul as they would say nowadays. No reputation is seriously damaged, etc., or we would not publish this personal letter. It reminds one so much of the living PeeKay that we urge you to struggle through it! And he did want it published, as he said so at the end + other places.]

[We also regret that obituaries on Egerod, Haudricourt and Winter are not included hereinafter. Sorry!]

John C. Kerns

Allan R. Bomhard
Charleston, South Carolina

On 24 November 1995, John C. Kerns passed away, and I lost a colleague, a co-author, and a friend.

Kerns first became interested in Indo-European in the mid-1930's on his father's farm in Mississippi. In his spare time, he would amuse himself by looking up the derivation of words in the family dictionary. As it happened, this book was a treasure in that it provided exceptionally full etymological information. Kerns quickly became aware of the importance of discriminating between genuine cognates and borrowed words and the need for strict adherence to the laws of phonetic correspondence insofar as these are known. Also, from books saved by his father from his high school days, Kerns taught himself the grammatical elements of German, Greek, Latin, French, and Spanish.

In 1940, Kerns enlisted in the United States Army, specializing in radio communications. Whenever it was possible, he would visit libraries to read articles on various linguistic families in the *Encyclopaedia Britannica*. Toward the end of World War II, in the Philippines, he became briefly acquainted with a cultured Finnish-American soldier who helped Kerns with an elementary investigation of Finnish. Since Kerns was well aware that Finnish was not an Indo-European language, he was surprised to find that it had considerable similarity to the more conservative Indo-European languages in its pronominal forms and in certain fundamental aspects of its morphology. At first, Kerns assumed that these similarities were due to borrowing from one Indo-European language or another.

After the war, Kerns obtained a bachelor's degree in electronic communications and spent his working career as a civilian engineer at Wright-Patterson Air Force Base near Dayton, Ohio, where he remained after his retirement. In 1958, Kerns became aware of Collinder's *Fenno-Ugric Vocabulary*. Reading this book, Kerns realized that the Finnish pronominal forms could not possibly be due to borrowing from Indo-European languages since they are shared by all other Uralic languages, including Samoyed. Kerns was further impressed by the fact that most of

these forms are found in Altaic languages as well. At that time, he was beginning to suspect that Indo-European, Uralic, and Altaic may have had an early common origin.

In 1959, on a short visit to Uppsala, Sweden, Kerns met Prof. Tryggve Skoeld, whose generous help and encouragement were a source of inspiration to Kerns.

Unfortunately, the increasing pressure of engineering problems prevented Kerns from pursuing the matter further at that time. However, several years later, Kerns returned to the matter and performed a series of statistical tests which demonstrated that the numbers of pronominal agreements among Indo-European, Uralic, and Altaic were highly significant, implying the existence of a unique historical cause for these agreements. Further consideration eliminated borrowing as a significant factor, leaving only the hypothesis of common genetic descent as a viable suggestion.

This analysis prompted Kerns to write a pamphlet entitled "The Eurasiatic Pronouns and the Indo-Uralic Question", which he reprinted in his 1985 book *Indo-European Prehistory* in edited and shortened form. In the course of writing that pamphlet, Kerns also came to the conclusion that the common conception of the undivided Indo-Europeans as an egregiously warlike horse-riding people was not supported by evidence then available.

Kerns retired from engineering in 1974. This left him time to consult current publications with greater regularity and frequency. The result was the publication in 1985 of his book *Indo-European Prehistory*, a copy of which he sent to Allan Bomhard, among others.

Bomhard was impressed by the extent of knowledge Kerns displayed and by the similarities of views regarding the genetic affiliation of Indo-European, Uralic, and Altaic. At the time, Bomhard was gathering material for a book demonstrating the common origin of the Nostratic languages. In due course, Bomhard invited Kerns to contribute a chapter on Nostratic morphology, and Kerns gladly accepted the challenge. This collaboration led to the publication in 1994 of their joint monograph entitled *The Nostratic Macrofamily: A Study in Distant Linguistic Relationship*.

Just before his death, Kerns had been working on a revised edition of his book *Indo-European Prehistory*. Regretfully, he did not complete this task.

PKB's letter

Dear neveu,

2/29/96

A rare date and how better to use than in replying to your recent letter et al.

First, re ASLP business. I was delighted to learn that all, incl. MT, will survive but have some qualms about your change of office! My main concern here, of course, is the journal, with that great start, and the newsletters. You are MT, or at least the father - does that make you a grandparent?! My neveu a grP? I'm getting old! Actually, come to think of it, last fall a nephew did in fact become a grP. And not too long ago when I criticized Pulleyblank, asking "Where did you get that from", he replied that it was sthg. of mine that as a student he had been assigned as a text (I told him he should keep up to date better). In any event, I assume (hope?) you'll continue editing journal and newsletter. Right?

As to your (2), you can put me down for the Web. Altho I suffer from dystechnia, perhaps as a developmental consequence of my early-acquired hyperlexia (my coinage) - after all, neuronal networks, unlike Web, are not infinitely expandable.

Re (4): for recon, need to know whether the lang's with disyllabic forms regularly reduce tri- to di-syllabics (AN has this - Blust calls 'drive towards disyllabism') -if so, keep *u- as 1st syllable, yielding *u(n)du(N)ga as root - actually, this might well be a PAN/PAT root, with homorganic NASAL INCREMENTS, a PAN/PAT hallmark (see my JAT), often variable in a root. If not, and this really a PAN/PAT root, the *u- much at home here as the widespread *u- nominal-marker (*Ibid.*), found inter alia in body part roots! Implied *g > /k/, *a > /o/ ~ /i/ and *u > /i/ shifts have parallels? -in AN/AT, at any rate, shifts of *nd > /n/ type are commonplace (see JAT) and no need to recon. the *d as retroflex since *d > /r/ also okay, unless there's need elsewhere for two *d's. This root really looks as if it belongs in AN/AT - shows how much shapes count for! As for the /sina/ form, can hardly just rule out in view of the *u > /i/ and *d > /t/ found elsewhere, the /t/ a possible (unlikely!) antecedent of the /s/ - know of possible parallels?

As for your closing Hal vs. Larry, which rather reminded me of the Pat vs. Bob in Rep. primary I watched earlier, I also like that /35/ and I remind you that my AT all began with that 1942 *Tai, Kadai and IN* paper in *AA* (published there because the great A. L. Kroeber, no less, had advised it be offered to this anthrop. journal, with the remark - his very words, as I recall - "linguists don't understand things like this"!), with 30 key Tai/IN cognate sets. As I relate in this article, however, the breakthru had come when I realized that the Kadai numerals simply have to be related to the IN - I present them in a table and note that Maspero, a 'sound and generally conservative scholar' (only later did I discover that he had screwed up by putting Vn. in Tai family because of tones!), had concluded that the Li (now read: Hlai - on Hainan) numerals "certainly" belong to the IN family. Much later Iz Dyen made an offhand remark to me re the Hlai numerals to the effect that, of course, they were borrowed from MP. Cruelly (Iz is an inviting target!) I pointed out to him, with his grad students looking on, that this was "odd" since the Gelao numerals, up in the interior of China [a key branch of KD], are obviously related to the Hlai and, trying not to smile, asked him where those MP donors had landed and how in Hell had they got up to Gwizhou Province?! Iz suddenly got a call on his beeper, too faint for any of the rest of us to hear, and had to get back to his office, leaving his students looking mystified.

v - 24
in. 56 -
stick
printed
ATLC

Fictive Kinship
Uncle to
Nephew

441 /

You've prob. heard this Iz story before. It makes an interesting point here since it shows that, at diff. periods, two (at the time already) eminent comparativists, HM and ID, came to realize that a connection of some kind must be posited between the Hlai numerals and the MP family. MP concluded that they belong to that family but did nothing more about this discovery! Or so we must assume from his silence on that point. From time to time - like right now - over the years I've wondered about this since it seems to make no sense, even at a grad student level. Did HM really think that only the Hlai numerals belong in the MP family?! Weird! Did he even look over the rest of the language, to pick up the obvious relationship to Tai, a family well known to him? Did he even check for possible further relationships with other 'isolates' in the region, viz. Laqua, Lati and Gelao (= Kelao)? Apparently not - he had made a sensational discovery and then done nothing about it! I have a problem with this - a brain (let me use neurol. res. lang. here), equipped with a vast neuronal network involving SEA lang. items, telling its master (yes, this is what happens but just who/where the 'master' is still eludes us) "no need to go any further"! Tell me, am I missing sthg. here? Does it make sense to any of you?

Now how about Iz, a full generation later? Iz is easy to kid and I've done my share of it but he's turned out some great work over the years, really more on AN than anyone else, with the likely exception now of Blust. This time his n[eu]ronal n[et]work told its master that yes, the Hlai numerals are connected with the MP (even a moronic nn would have come up with this) and yes, PKB has shown in that 1942 paper that these are part of a numeral system involving Laqua and Lati and even, in Central China, Gelao, but - let's face it, Iz - if we go along with PKB on this it's a loser! I got a great idea (nn's are always saying that - and even smart masters keep falling for it): let's just call the Hlai numerals, out there on an island adjoining Malay and other MP waters, a loan from MP! We don't have to talk about Laqua, Lati and Gelao, do we? [PKB approaches and finally the 'bad question' pops up, sthg. like abortion at a Rep. primary debate] Oh, oh - what to do now - how about getting a call on your beeper? I know - I know - what do you think I am, a dummy? How can you get a call that the others can't hear? Just say you got one and take off, stupid! Really, Iz, haven't you ever heard a politician talk? Have a life!

Now your nn may not talk like this - mine just picked up the "Have a life" from my kids, who don't seem to know what it really means - but I'm sure that at times we've all fallen for that "I got a great idea" line! This all speculative but at least it does provide an answer of some sort to explain ID while I still lack even a speculative one to explain HM. When I ventured upon the scene, a young anthrop. with minimal ling. training (my one teacher, with anthrop. background, a pretty good one: Sapir) and scant knowledge of Tai or other non-ST lang's, all I had to do was the obvious sort of thing that might be expected of a grad. student; it hardly took a rocket scientist, as we now say, to see that Thai (Siamese) ta 'eye', tay 'die' and ku 'T' (all tone *A) fit nicely with PMP [no Formosan then to yield PAN] *mata, *ma-tay (ANists now recon. *m-atay) and *aku 'id'. Right? Even at that time, as I came upon all this [= AT], these two-three dozen cg. sets for REALLY core roots, along with the numerals fit with MP [replaced in Thai by early loans < Chinese - here my ST very helpful], I often wondered how come all this had been saved for me?! I've compared it all with walking over a field of diamonds, simply picking up the shining jewels on the surface. No digging! I've even felt sorry for linguists apre's moi!

One more point here. While I was picking up these diamonds, feeling rather guilty about it (!), the areal linguists almost unanimously kept repeating, as if in a mantra, "Those: aren't diamonds - you're just wasting your time, and ours"! I kept thinking: they sure look like diamonds to me - and kept asking my nn to come up with an explanation. I even made use of my newly acquired psychological sophistication to kid Bill Gedney, an old friend, about having an unconscious acceptance of AT. I got nowhere with ANists on the other side. Finally, Haudricourt offered his support as well as JHG, the latter leading to considerable approval among anthrop's. My nn [it often works overtime] has pointed out that this trio of PKB/AGH/JHG is atypical (one might well use more emphatic expressions here), with unusual anthrop/culture historical backgrounds. Coincidental? Hardly, I should think. Does it mean that this sort of background ideal for long-rangers? A better explanation, it would seem to me, than crediting them with nn of a special kind, as you would appear to be doing in your p. 4 remarks about 'noodles' (another /n/!). Other opinions here?

About methodology, while on your p. 4. You may be surprised that I go long with your 'nuggets' approach (p. 5). But you must remember, dear neveu, that you have that same 'trio' background - make it PKB/AGH/JHG/HF, pronounceable (abbr.) as BAHF, which unfortunately is a homonym of Am. dial. verb: boff v.t. 'to hit, cuff, slap, treat roughly'; v.i. 1 'vomit' (we say barf); 2 'to have sexual intercourse' (we have another 4-letter word). I must admit that this gets our BAHF off to a bad start - maybe we should make it PAHF - and, no, there's no dial. poff. Please give it your earnest attention.

Back to more serious matters, re methodology. I myself make use of precisely the same methods when reconstructing forms from the several dialects of Karen, which I've done, incl. one paper just on the loss of final -?, as when reconstructing PAT forms. Phonological rules apply uniformly in both cases, with all segments - and suprasegmental features! - needing explanation. Let me cite an example. In my *JAT* book (pp. 87-88) I show that, in the typical Jp. disyllabic, final -N (velar nasal) after initial t- does not > /zero/, as generally, but is assimilated to *-n, regularly yielding -i. In this connection I offer - and only as a counterexample - PMP *taruN 'cylindrical', Jp. taru 'barrel'. Am I being too hard on myself here? Not really. Note that I do indeed cite this pair, in the event that future evidence might cast some light upon it. Similar pairs can be cited in Karen dialectology. There is a big difference here, one that both JAM and I have pointed out, viz. the longer the linguistic range, the greater the need for a precise phonology. If the above pair were Karen forms, the linguist should list them as 'probable cognates' or the like, with 'unexplained' *-N > /zero/. At the great Jp/MP range, however, they become only 'possible cognates', to be listed, as I have done in *JAT*, simply as counterexample. Long-rangers all agree on this?

Re your pp. 6, 7 on the 'relevance of a universal or common trait': *pa/ba for 'F' ~ *ma for 'M' are the anticipated parental terms and I include them in my PST root inventory but surely, esp. as regards long-range studies, the quirkily reversed PAT *ma/ba ~ *pa (see enclosed keynoter: 5) are of far greater significance, contributing much support for this long-range relationship. Yes, by all means we must include 'universals' and the like in our root inventories even while discounting their significance at long range. Actually, for SEA at any rate, the KEY kinship item is that for 'child', with solid ST, AA and AT roots: PAT *(u-)(N)'alak; see above for both *u- and (N). Same elsewhere, long-rangers?

As for Basque, where this all started, I'm in Larry's camp in seeing no really compelling evidence for any of the proposed relationships. Yes, a nugget (as Hal and I put it) here and there, but I'm looking for a COLLECTION OF NUGGETS, as laid out in my AT work (above). I agree with Hal in faulting Larry for failing to recognize even a single nugget. For me, there must be a collection of nuggets and the nuggets must be of good CORE quality - see my AT. Don't we need better terminology here, neveu?

Finally, LV's letter to Hal very interesting - and welcome to the club, LV! I share your concerns re the *HGHG* matter, esp. having become acquainted with Piet. (here I put on my anthrop. hat) at the Taiwan symp. on AN (I enclose copy of my paper). Simply put, his dendrogram (your copy) makes absolutely no linguistic sense as regards the key problems in the early SEA ethnic movements. See my enclosed keynoter for my present ideas on all this. I did my best, as a fellow anthrop., in talking with him in effort to make some sense of his findings. App. his collection methods leave sthg. to be desired. But let's face it - as I noted in that early AA paper, for AT we need roots, not skulls - nor pots!

Re your inquiry, LV, about non-AN vocabulary in the Formosan lang's. I'm afraid you've got it backward (p. 2: 3 - 4): the great bulk of basic lexical items show good MP/Formosan (often all three groups here) correspondences, beginning with all the pronouns/deictics as well as the numerals; the problem for the ANists is to come up with Formosan-only roots, thereby supporting scheme of Blust et al. whereby a 'Formosan' group can be viewed as a primary split from PAN, leaving MP as the other branch - this has been attacked by Starosta, as you note. This 'game' has been doing on for some years now, with various 'Formosan-only' roots proposed, often to be negated upon the discovery of an MP cognate, most often in the Philippines. As I note in my keynoter (p. 5), the Starosta scheme also important in making prefixes out of supposed infixes! Bad for your Austric! And your Austric needs roots - I mean nuggets - a nice little collection. Got a few for us? You might check over the AT roots cited in my enclosed symp. paper, many new ones made possible by recent additions by Blust et al. to the AN corpus - and all updated. Incidentally, I've found, in reviewing root assemblages put together for *ATLC* over 20 years ago, that I'm now much better at doing it - and I mean even where no new material is available! This discovery agrees with recent research findings that if one keeps busy his nn continues to proliferate! So do keep your nn's humming, all you long-rangers!

One more point here, for all long-rangers. It behooves us to be on the lookout not only for vocalic transfer, with a PAT *aku T" yielding Tai /kaw/ as well as /ku/ forms, but even trickier nonc's, typically men's lang. forms that have 'snuck' into the standard language - see my symp. paper: 431-5; 417 and 466 (and p. 1 of my keynoter). Any nonc's elsewhere? Must be on guard vs. invoking one to 'get around' an irregularity! Note that in my THIGH and FLOWER examples both the < qi and the < i > nonc's (note the transcr!) are evidenced in the men's lang. of Atayal: Mayrinax. Another Dyen tale here, illustrating an important matter. For years I kept pestering Iz about the PMP *paqa ~ *paqi 'thigh' doublet, for a possible source for it. And Iz kept answering to the effect, "It's a doublet, Paul. What's your problem"? The problem lay with Iz: he saw no problem. Please, all long-rangers, note that any given doublet must be given a source! Further, it is this source that must be cited in making cf's! We now know the source of the above doublet, from *paqa, with the < qi nonc yielding the secondary *paqi form. Most will be simpler!

5

Re the bit on EYE in LV's letter. For P-Tai recon., FK Li's problem was that he had already recon'd a PT *pr- and didn't know how to handle the Saek pra (and Liuchou pia), cited (p. 119 of hi Handbook of Comp. Tai). Another thing I've always wondered about, since FK knew that Lakkia, that marvelous source of archaic forms, has pla here! And that Tai commonly has labial clusters > dental shifts. Can you solve Li's problem? I give the answer in my 'KD clusters/dyads...' paper in Kadai 1 (1990): a distinction must be set up at PT level between intial cluster, here *pr- (cf. Eng. prayed) and *p-r- (cf. Eng. parade), the latter disyllabic with schwa for V-1 (very few of these minimal pairs exist in Eng. - try finding some - my favorites are: "Some women [fortunately] want a man for sport, others [unfortunately] for support." and scum vs. succumb. This very Englishy distinction has turned out to supply key here in Tai as well as in Arch. Chinese (below)! For updating on all this, see Table 1. in enclosed *Extra-AN*...

For John's benefit, in particular, and to help anyone who might want to cf. Basque, Caucasian, Yenisseian or whatever with ST, I enclose copy (two sides) of Old Chinese 'menu' in coming ST conf. this fall in Leyden. The two phonetic series chosen, from K's *Grammata Serica Recensa* (GSR), have been recon'd by K with d-'s and such, as cited, by Baxter et al. with l-'s (this now the Sinology Establishment and affecting even solid ST citizens such as JAM) and by me, and my intelligent followers (both of them) [this a joke, neveu - in ny event, after Leyden I'll have a host!], with sg-' and the like, with sg- > d- vs. s-g- > s-! Reminds me of JAM's wonderful takeoff on Sagart's disastrous (believe me!) attempt to link this Archai Ch. with AN: "Close, but no Sagart"! Actually, not close - neither Blust nor I has taken it seriously and long-rangers shouldn't either, unless you know sthg. that has escaped the attention of us both! In any event, the important point here for long-raangers is that in view of the general agreement on PTB recon's., unlike the above, it is prudent to use PTB forms for your primary cf. work - Arch Ch. seldom makes any significant addition (occ. vocalic length) to what the PTB is. And good luck!

I'll try to wrap this up in Leyden but my problem is that I don't really understand why everyone doesn't see what I'm seeing - excuse me, nn, what you're seeing [you have to be polite to your nn, esp. when you keep overworking it, as I treat mine - as I've noted, it does keep it nice and healthy - and growing!]. If anyone wants a copy of my Leyden handouts, just let me know - I'm trying to keep it readable, incl. my Ch. characters!

Ce. John, 2V

+ JAM

Call the best
Paul

Keep your nn spinning!

PS Looks like some of this should be
included in your Newsletter!
for comments, etc.

ASLIP Business

ASLIP BUSINESS

OUR FUND-RAISING EFFORTS

Being perpetually broke does not benefit anyone, even the pure in heart. We have been struggling to improve our finances so that we can begin to realize some of the goals set forth in our charter. Thus far we have been preoccupied with simple survival which in our case means being able to produce and distribute both the Journal and the Newsletters. Last year we managed to purchase airfare to bring our new President from the frozen lakes of Minnesota to balmy Boston for the Annual Meeting and to share the expenses of his trip to Utah for the LACUS meetings the year before. We were also able to pay Allan Bomhard a portion of what his labors have been worth. And we splurged on the reproduction of a color map of pre-Clovis archeological sites in the Americas; they were in MT-28.

That's been all -- survival + a very few carefully selected 'treats'.

Older members will be familiar with our remarks about postal costs -- they keep rising. But we have also found that mailing by "Surface Mail" is entirely unreliable, very slow, and frequently not advised by the Postal Service. Being the recipient of an anthropological journal from Japan, I have found that trans-Pacific "Surface Mail" is extraordinarily slow -- slow boat to China and all that -- and there seems to be no sense to news being many months out of whack between two countries. Not in the Space Age with the Buck Rogers Century coming up soon! Since we believe that a newsletter ought to get to members in Europe and Asia as soon as possible (shortly after North America), we indulge in First Class Mail and World Priority Mail most of the time. We have found African mail, at least to Nigeria, to be essentially hopeless but we do not know why this is so. We suspect that the delivery system is corrupt but we cannot prove it. (Any good advice will be appreciated.)

In sum, then, our postal costs are quite heavy but we would not have it any other way.

As was the case last year, we have received a boon -- to be our last -- from Boston University in the form of a printing + binding grant for the Journal. Our plea to members for gifts is designed to produce enough extra revenue to settle the Web Site problem, i.e., we wish to hire expert consultants for long enough to re-establish our Web Site and E-mail address. Mary Ellen Lepionka has been unable to do that on her own. For the expert consultant who will replace the free services of Jennifer and Sara Fleming (who set up the original Web Site) we will need \$2000 to \$3000 per annum (professional estimate). Will your gift giving make that dream come true? It's up to you.

Ironically we note that those who clamored for E-mail and web sites have not been conspicuous among the gift-givers. Come on, computer freaks!

Concerning dues-paying, we must report that less than half of members have paid 1997 dues. If the other half pay their regular dues, we'll make our goal. If more gifts come in, our odds of success increase. By October 1st 20 people had sent gifts, ranging from \$25 to \$100 US. We promised we would mention the gift givers names -- on a "Good Guy" list. Here it is. (Two people who gave generously in 1996 are included. Some very generous gifts from earlier years were appreciated but not reported here.)

The GOOD GUYS of 1997 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
Ronald Christensen / Lincoln, Mass., USA
Robert Eckert / Grosse Pointe Park, MI, USA
Bruce Elliott / Sudbury, Ontario, Canada
Frederick C. Gamst / University of Massachusetts
Kenneth Hale / Massachusetts Institute of Technology
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
Frank B. Livingstone / University of Michigan
Jean Lydall / Melle, Germany
Victor H. Mair / University of Pennsylvania
Kazutake Miyahara / Uji Kyoto, Japan
Wolfgang Schenkel / Universität Tübingen
Jan Vansina / University of Wisconsin
Paul Whitehouse / London, England

NOMINATIONS FOR OFFICERS AND DIRECTORS OF ASLIP

The governing body of our Association has been limited in membership and location since our incorporation in 1989. Jokingly called the 'Boston Mafia', this body was created by the intersection of a need to meet and deliberate with a lack of money for travel. Our range was a good half day's travel by car, i.e., southern New England. Rather than appealing to a wide range of linguists with their multiplicity of opinions, the Boston Mafia has largely been a group of non-linguists, united by general interest + ties of friendship and dedicated primarily to the survival of ASLIP. So long as we kept our core group intact and our resources efficiently used we could survive against the unremitting hostility of 'mainstream' linguistics and nearly complete disinterest of anthropology. Had those two disciplines welcomed the creative energies of long rangers the history of ASLIP and **Mother Tongue** would have been entirely different. Had the Zeitgeist been more favorable, like the 19th century for example, our history would have been far more rewarding than it has been. But life was not to be cozy or snug, for we had been born at the wrong time, as Kroeber would have said.

So these considerations can account for a certain toughness and a certain disinclination to spread the governance around too much. But now it is clear that we have not only survived. We are also starting to thrive (wax, flower, burgeon). What is holding us back more than anything else is our finances -- and elder fatigue. We have too little money to hire simple things like secretaries, research assistants, Internet experts, new fast computers, or commercial mailing services. Our officers have grown tired, especially from doing so much **scutwork**. We need people with energy and fresh ideas about fund-raising. We need help with all that bloody scutwork. We need help in writing up the many grant proposals which we have dreamed of. . . . If we had the staff and resources available to one, repeat one, senior professor at a major university like Harvard or Stanford, we could

easily do all the ASLIP work and find time to write a book and go to six conferences a year.

Accordingly, it is time to spread the governance around. It is time for some rich professors to volunteer to do some of the work. It is time to use the electronic communication age to eliminate the geography which stands between us. For example, it is completely reasonable to reach a Board of Directors' decision by electronic networks. It is completely reasonable for one scholar to write up a newsletter in her city and send it to a colleague in another city for reproduction who can then give the issue to a mailing service for distribution. Some of our friends do this sort of thing regularly in the publishing business.

Consequently, let us regard each and every Officer position, and each and every Directorship on the Board, as subject to election by the membership at large. Following nominations which you are asked to make now or in the near future, we will have a paper ballot distributed and returned before the Annual Meeting on April 15th, 1998.

To rehearse this briefly -- you are asked to nominate someone for each position indicated below. Then you are asked to return your set of nominations to Secretary, ASLIP, 16 Butman Avenue, Gloucester, MA 01930-1006. At your own pace but, please, not later than the New Year. Then you will be sent an official voting ballot which you should return before April 15th.

By our By-Laws the Annual Meeting elects officers and directors. That means the Boston Mafia by default elects everyone. This is well-known. But by our own precedents we now can incorporate the results of mail ballots. Even if only a few people return their ballots they can swing the election. Thus, if a determined group of short rangers in Michigan or Illinois wished to seize control of ASLIP (in order to kill it off), they need only gather around 15 ballots to wrest control away from the Bostonians. All legal and proper, provided that they are all dues-paying members, of course. And first of all, of course again, they would have to get their people nominated. It would make it much easier if they all attended the Annual Meeting too because the Board elects the Officers at that meeting. But a Board elected entirely by mail could be polled by mail or e-mail to determine their slate of Officers. However they get to be there, the Board of Directors is the final power of ASLIP.

Each member is free to nominate herself/himself or anybody else. But only one per position. It makes better sense if the person nominated is also willing to perform the duties of the office. However, contrary to our instructions in previous years, you are not required to ascertain whether a nominee will serve or not serve. You may nominate a complete lout who has no interest in prehistory, even to be President, and that is simply legal.

YOUR PROPOSED NOMINATIONS (NOTE: THIS IS NOT A VOTING BALLOT)

Please print legibly or write legibly. Or we cannot record your choices.

OFFICERS OF ASLIP (Circle the incumbent or write in another nominee.)

President John D. Bengtson (or) _____

Vice President Roger Williams Wescott (or) _____

Vice President Daniel McCall (or) _____

Secretary (no incumbent, so I nominate) _____

Treasurer (no incumbent, so I nominate _____

[Note: The incumbent, Harold C. Fleming, is not running for re-election.]
[Some may want to combine the Secretary office with the Treasurer office.]

BOARD OF DIRECTORS (Incumbents are automatically re-nominated by the Secretary. Nominate as many as you wish up to ten -- or none at all.)

Ofer Bar-Yosef (Harvard U.) (or) _____

Anne W. Beaman (Brookline, MA) (or) _____

Allan R. Bomhard (Charleston, SC) (or) _____

Ronald Christensen (Lincoln, MA) (or) _____

Frederick Gamst (U / Massachusetts) (or) _____

Kenneth Hale (M.I.T.) (or) _____

Jerold Harmatz (Tufts U.) (or) _____

John Hutchison (Boston U.)(or) _____

Judith Leader (Lexington, MA) (or) _____

Philip Lieberman (Brown U.) (or) _____

Mary Ellen Lepionka (Cambridge, MA) (or) _____

(Please return this form with your nominations to Secretary, ASLIP)

Detach and mail

The following books are available for review in *Word*. If you wish to review a book, please write to Sheila Embleton, Dept of Langs, Lits & Linguistics, South 561 Ross Building, York Univ, 4700 Keele Street, North York, Ontario, CANADA M3J 1P3. E-mail embleton@yorku.ca. Telephone (416) 736-5260 at York and (905) 851-2660 at home. FAX (416) 736-5623. Books are available on a "first come, first served" basis. Graduate students are welcome to participate under supervision of a faculty member. Reviews are due 6 months after you receive the book. Please send 3 copies of your review, **double-spaced** with at least 2 cm margin on all sides. If possible, please also send your review on diskette, but only if you can provide it in IBM MS-DOS or Windows format. It is not possible to return your disk to you. If your review will be less than one journal page or more than four journal pages, please check with me before submitting your review. One journal page is roughly 1.5 double-spaced typed pages. Please remember to include your name and address on any correspondence with the journal. **YOUR REVIEW MUST BE DOUBLE-SPACED**; if it is not, the type-setter will not accept it, and we will have no alternative but to mail it back to you for correction, which will delay your review and also involve everybody in unnecessary correspondence.

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

Date of this list: September 22, 1997

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- August 9-13, 1999. International Conference on Historical Linguistics. University of British Columbia, Vancouver, British Columbia, CANADA. [Laurel Brinton, English, UBC, Vancouver, BC V6T 1Z1; ichlxiv@interchange.ubc.ca, fax (604) 822-6906]
- January 7-10, 1999. Linguistic Society of America. Westin Bonaventure, Los Angeles, CA, USA.
- August, 2001. International Conference on Historical Linguistics. Latrobe University, Melbourne, AUSTRALIA.

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From MT-4 to MT-24 : November 1987 thru March 1995

by Mary Ellen Lepionka, Cambridge, Mass.

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Secretary's Note

Some issues were not available to Ms. Lepionka and thus are not included. These are the first three in 1986 and 1987, often called Circular One (and so forth). Lepionka's compilation stopped in March 1995 with Issue 24, although issues 25-27 may be indexed at a later date. There are no plans for indexing the Journals at this moment.

For further information on the first three issues or the last three, please contact the Secretary-Treasurer (addressed above). Some arrangements may also be made for reproducing back issues.

In the Index some very slight editorial changes have been made, mostly about spellings of non-English names. Members are invited to spot any errors of ours that they can. If you do chance upon a mistake, please so inform the Secretary-Treasurer. Or better still so inform the author who may wish to revise and update the Index¹. Her address is:

Mary Ellen Lepionka
94 Clay Street
Cambridge, MA 02140-1710
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¹ It has not been Mary Ellen's aim to attain perfection in indexing. The indexing project was undertaken with an eye to being useful -- both for members and for assessing what we have covered. Consequently, we welcome suggestions about increasing the utility of the Index. We do not intend to fuss over spelling or punctuation.

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8/26/89 John Bengtson on Fleming's editorial in *Mother Tongue* Issue 7 (November/December 1989).

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2/16/91 A. Murtonen on Kulturwörter, Issue 13 (April 1991), 40.

2/4/91 M. Lionel Bender on long rangers, with reply by Harold Fleming, and rejoinder by Bender, Issue 13 (April 1991), 46-48.

3/23/91 Karl Krippes on Altaic linguistics, Issue 14 (August 1991).

5/31/91 Kay Williamson on Mother Tongue articles and Niger-Congo, Issue 14 (August 1991).

Roger Blench on map of African language families, Issue 14 (August 1991).

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4/27/92 Lionel Bender on Bengtson's *Language* Rejects "Global Etymologies", Issue 17 (August 1992), 79.

5/5/92 Sarah Thomason on Bender's response to Bengtson's *Language* Rejects "Global Etymologies", Issue 17 (August 1992), 80.

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12/20/92 Paul Benedict on Austronesian and Tai-Kadai, Issue 19 (Spring 1993), 6-8.

Robert Blust on Austronesian and Tai-Kadai, Issue 19 (Spring 1993), 9-11.

11/5/92 George Grace on Austronesian and Tai-Kadai, Issue 19 (Spring 1993), 12.

10/23/92 Jurgen Pinnow [in German], Issue 19 (Spring 1993), 28.

1/21/93 Paul Sidwell on the classification of Japanese, Issue 19 (Spring 1993), 29-31.

1/24/93 Eric Schiller on Austro-Tai, Issue 19 (Spring 1993), 32.

9/9/92 Carleton Hodge on methods of reconstruction, Issue 19 (Spring 1993), 49-51.

10/12/91 Patrick Ryan on Aihenyald-Angenot taxonomy, Issue 19 (Spring 1993), 57-60.

2/28/93 Alvah Hicks on fossil evidence for human origins, Issue 19 (Spring 1993), 61

6/26/92 Karl Menges on Benedict's views, Issue 19 (Spring 1993), 87-88.

12/11/92 Victor Shnirelman on additions to Hegedus's bibliography, Issue 19 (Spring 1993), 89.

11/30/92 John Bowden on Hungarian historical linguists, Issue 19 (Spring 1993), 90-91.

5/19/93 W. P. Lehmann on IE studies, Issue 19 (Spring 1993), 92.

7/29/92 Juha Janhunen on the work of *Mother Tongue*, Issue 19 (Spring 1993), 93.

4/1/93 John Bengtson on IE linguists, Issue 19 (Spring 1993), 94-95.

3/29/93 E. Morgan Kelley on *The Metaphorical Basis of Language*, Issue 19 (Spring 1993), 96.

2/6/92 Carroll Riley on American dating, Issue 19 (Spring 1993), 97.

1994

6/21/93 John Colarusso to James P. Mallory on the Caucasus and PIE, Issue 21 (January 1994), 20-22.

1/94 William Poser defending criticism of the work of Greenberg and Ruhlen, Issue 21 (January 1994), 65.

3/7/94 Henry Harpending on mtDNA and language origins, Issue 22 (May 1994), 58-60.

Joseph Salmons on Fleming's view of the Eastern Michigan workshop on Nostratic [with rejoinder by Fleming], Issue 22 (May 1994), 60.

1995

12/1/94 Paul Whitehouse on massive comparisons, Issue 24 (March 1995), 67-68.

OBITUARIES

Klaus Baer (Issue 4, November 1987, 2-6)

Karel Petraček (Issue 4, November 1987, 7)

Peter Behrens (Issue 7, May 1989, 42; Issue 8, August 1989, 3-6)

Allan C. Wilson (Issue 14, August 1991)

Otto Rossler (Issue 14, August 1991)

Emmanuel Laroche (Issue 14, August 1991)

Samuel Noah Kramer (Issue 15, December 1991, 1-2)

* Hans-Jürgen Pinnow (Issue 17, August 1992)

Zelig Harris (Issue 17, August 1992)

Steve Johnson (Issue 17, August 1992)

Stephen Lieberman (Issue 19, Spring 1993, 1-2)

Hans Gunther Mukarovsky (Issue 19, Spring 1993, 3-4)

Marija Gimbutas (Issue 22, May 1994)

Sherwin J. Feinhandler (Issue 22, May 1994)

Susan Park (Issue 22, May 1994)

John Swing Rittershofer (Issue 24, March 1995)

* This was an error!

Age of sites
shown as years
before present

23,000
years ago
BLUEFISH
CAVES, YUKON

15,000
WILSON BUTTE

15,000
HAYSTACK, COLO.

38,000
OROGRANDE CAVE, N.M.

11,200
CLOVIS, N.M.

23,000
TLAPACOYA, MEXICO

18,000
MEADOWCROFT, PA.

15,000
TAIMA-TAIMA,
VENEZUELA

45,000
PEDRA FURADA,
BRAZIL

13,000
TIBITO, COLOMBIA

18,000
PIKI MACHAY, PERU

**12,500 to
33,000**
MONTE VERDE, CHILE

11,000
TRES ARROYOS,
TIERRA DEL
FUEGO

12,500
LOS TOLDOS,
ARGENTINA

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S. McNAUGHTON



The Egyptian Vulture, Reedleaf, and Now

Carleton T. Hodge, Indiana University

Loret (1945), after reminding us that Coptic (and therefore Egyptian) had an l, showed on the basis of comparative evidence that the hieroglyph for this l was the Egyptian vulture (3; Sign List G1). Initially unaware of Loret's work, I independently came to the same conclusion many years later (e.g., 1992). A complicating factor is the comparative evidence that relates l, r and n to each other. This has been neatly clarified by the consonant ablaut hypothesis. Plain l is the basic consonant (Ar. li- 'to, for'), r is the pharyngealized ablaut of l (**lH) (Eg. r 'to, toward') and n is the nasalized ablaut of l (**NL) (Eg. n 'to, for'). Where we have l in an attested lexeme, it comes from *l; where we have r, it comes from *lH; where we have n, it comes from *NL.

The situation is also simple in the case of reedleaf (i, M17). As this is used to write a prothetic alif, i.e., a glottal stop, and is used to transcribe ? in loans, it is clearly ?.

These carefully arrived at conclusions (3 is [l], i is [?]) have not been recognized by most AAs scholars. Two notable examples of the resulting confusion are the reconstructions in Ehret (1995) and Orel and Stolbova (1995).

Ehret finds the l-r-n correspondences 'extraordinarily puzzling'. The result is a complicated set of rules, with l becoming r, r becoming l or 3 (which he takes to be [?]), etc. (1995:391-94). In his etymologies we find 3 equated with ? (e.g. 34 [89], r (20 [85]), c (157 [137]), h (274 [183]), h (275 [183]), nothing (298 [190]), γ (745 [369]), x (? 380 [222]). (References are to the entry numbers, followed in brackets by the page numbers.) See also his discussion of laryngeals, pp. 338-40. Not a single example under *l has older Egyptian 3 corresponding to l elsewhere, though Coptic examples with l are cited (396-408).

His treatment of reedleaf is comparable. It is taken to be ? (719 [361]), y (992 [470]), i- (an affix, 621 [320]) and c (667 [342]).

Orel and Stolbova (1995) do not have the elaborate explanations displayed by Ehret, but their correspondences are just as erratic. 3 is most often taken to be ? (35 [10]). It also corresponds to l (21 [6]) and r (1024 [231]). Contrary to the consonantal nature of the Egyptian script, established over 100 years ago, it is not infrequently taken to represent a vowel (901 [204]) and even the specific vowel a (1051 [236]). Entries 1-153 (all ?-) take 3 as ? sixteen times and i (reedleaf) as ? twenty-eight times. Reedleaf is also taken to represent y (1202 [267]; read h²w), as reflecting *r (2121 [450]), and as a 'front vowel' (78 [21]).

Such confusion characterizes both volumes, and scores of other examples could be cited from each. It is highly unfortunate that works of such magnitude are so unnecessarily and extremely flawed.

Ehret, Christopher. 1995. *Reconstructing Proto-Afroasiatic (Proto-Afrasian)*. (University of California publications in linguistics, 126) Berkeley: University of California Press.

Hodge, Carleton T. 1992 [1994]. 'Tooth and Claw.' *Anthropological Linguistics* 34.202-32.

Loret, Victor. 1945. 'La lettre l dans l'alphabet hiéroglyphique.' *Academie des Inscriptions et Belles-Lettres. Comptes Rendus*. Paris: Henri Didier.

Orel, Vladimir E. and Olga V. Stolbova. 1995. *Hamito-Semitic Etymological Dictionary*. (Handbuch der Orientalistik, 1.18) Leiden: E. J. Brill.

ANNUAL MEETING OF the ASSOCIATION for the STUDY of LANGUAGE IN PREHISTORY
Reported by John D. Bengtson, President (edited slightly for brevity -HF)

The Annual Meeting was held on April 19, 1997 at the African Studies Center of Boston University (room 415), Boston, MA. USA. Those present at the meeting were: Officers (Bengtson, McCall, Fleming); Directors (Bar-Yosef, Christensen, Gamst, Harmatz, Hutchison, Lepionka, and Lieberman). Members present were: Seielstad, Denofsky, Janet Hong Lee, and Marcia Lieberman.

The meeting was called to order at 1 pm. Routine business was carried out, including the reelection of the 1996 roster of officers and board members. [Secretarial note: M.E.Lepionka and K.Hale also elected to Board]

V.P. Roger Wescott's concerns were brought up by President Bengtson (in Roger's absence). These were namely (a) the unnecessarily strong invective in some of the recent debates in Mother Tongue (newsletters and journals), and (b) the technical linguistic tone (e.g., assuming that all readers know what "Nostratic" and other deep classifications mean.) Both concerns were affirmed by a consensus of those present. P.Lieberman suggested that Mother Tongue include "tutorials" explaining some of the topics paleo-linguists take for granted.

The latter topic flowed together with a discussion of the Mother Tongue web page, which Bengtson (and others?) had complained to S.-T. Fleming about. The web page (when reviewed by Bengtson in March 1997) still listed the pre-1996 officers and Board; and the address printed on the front inside cover of MT II < <http://www.leonline.com/aslip/index.html> > was obsolete, and is now (as far as I know): ↓

< <http://www.tiac.net/users/aslip/> >

Hal informed us that the web page had been designed by his daughters, at no cost to ASLIP. Lepionka volunteered to act as a "Webmaster" (the alternative "Webmistress" was declined) and do what she could to revise and otherwise improve the ASLIP web page.

Dan McCall requested that the duties of Vice President be better defined. After discussion, it was determined that Bengtson and Wescott act as editors of Mother Tongue, McCall continue (as in 1996) as a fund-raising specialist, and Wescott continue also with membership recruitment.

Priorities for the Mother Tongue journal were discussed. From the view-point of non-linguists (the majority of those attending) a consensus emerged that linguistic dating (glottochronology, lexicostatistics) was the topic that generated the most interest, in contrast to some others, such as the classification of Japanese, Ainu, or Sumerian.

After business was concluded, those at the meeting were treated to three overviews: Bar-Yosef on recent archeological developments; Lieberman on linguistic neural 'hardware'; and Seielstad on his genetic studies of Africans.

Though I am a newcomer to the job of President, I thought we had a good meeting, and a good year. Last August, at the LACUS meeting, I had the pleasure of being in the company of two Council Fellows, Joseph Greenberg and Sydney Lamb. In December I was with two others in Ann Arbor, Vitaly Shevoroshkin and Sergei Starostin, and then in April being with Hal, Dan, and the others (who were all new to me.) As hinted above, I think ASLIP is growing into new possibilities. It is an exciting time to be President.

One of my goals is to reconcile the western/American school of long-rangers with the eastern/Russian school (though it is a great oversimpli-

fication to think of these as monolithic groups.

Some main points of the meeting:

That there is a need, and ways and means, to improve MT-ASLIP's position in cyberspace (web-page).

That we must try to broaden the scope of Mother Tongue: The Journal to other than strictly linguistic topics, and offer more explanation of linguistic materials.

That linguistic dating is one of the urgent topics that should be addressed and discussed in our quest to coordinate linguistic findings with those of archeology and genetics.

John D. Bengtson / May 1997

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ERRATA: DO WE EVER MAKE MISTAKES?

You betcha. First, the following addresses on the 'permitted' list need to be substituted for those given there.

John D. Bengtson :: 1329 Adams St. NE, Minneapolis, MN 55413
Allan R. Bomhard :: 151 Wentworth St., #3B, Charleston, SC 29401-1743
Larry Lepionka :: 1405 Newcastle St., Beaufort, SC 29902. (803-424-0724)
Daniel McCall :: (summer only) P.O.Box 684, Edgartown, MA 02539
Aimo E. Murtonen :: My friend no longer lives there; or anywhere.
Peter Norquest :: 2525 N. Los Altos #315, Tucson, AZ 85705 (520-903-0648)
Stephen Sherry :: 1901 Perdido St, New Orleans, LA 70112 (504-568-8080)

Second, I used the wrong print-out from the Web on the ancient city of UBAR in Oman. There are better reports on Web sites. The city's location and dates (urban) are 18°N. latitude by 53°E longitude, the focal center of the NASA search, and 2800 BC to 300 AD (4800-1700 BP). That location is on the southern edges of the Rub al Khali. The city was apparently the last stop for caravans before they ventured to cross the desert (big as Texas). This all from < <http://www.jpl.nasa.gov/radar/sircxsar/ubar1.html> >. Then later, again thanks to Jennifer Fleming we found the site "The Not So Lost City of Ubar" with this incredible address: < http://www-dial.jpl.nasa.gov:80/kidsat/exploration/Explorations_TEAM/Russell_Moffitt/ubarpag/hi_res-text/text.html >. There is an e-mail address for Russell Moffitt, Project YES, at < rym@mpl7.jpl.nasa.gov >. In any case it turns out that there are "old Neolithic age" dates of 6000 BC to 3500 BC or 8 ky to 3.5 kya. Their association with the city is not suggested. They seem to be only artifacts, not from stratified sites, not radio-carbon dated. Yet the suggestion of 8000 BP is that the Neolithic reached southern Arabia about the same time it reached other SWAsian areas Bar-Yosef mentioned.

And that is news! And this date has a serious chance of being a date for proto-Semitic, there being no linguistic competitors for Semitic any place in southern Arabia. (Yes, the Persian Gulf may have competitors.)

Mid June, 1997

This is a very simple form. On the top part please find a polite request for 1997 dues. Please remember, if you paid your 1996 dues in 1997, that you only paid the 1996 dues. The march of dunning time is inexorable. We can't help it and neither can you.

If you already paid your 1997 dues, not 1996, but 1997 dues, just relax. Do nothing.

Please return this form with your dues payment.
Make any comments you like in the extra space.

Don't forget to sign your name.

= = = = = You can detach the bottom here = = = = =

The bottom part is an appeal for extra funds. It is more than some can bear to pay the \$25 dues, as it is. You are not the ones we are talking to here.

We are talking to that minority of members who have lots of money. We ask politely if you can spare more than you have been giving ASLIP. A gift to ASLIP is fervently requested. Why? Because we may not get the \$1500 grant from Boston University which pulled us through last year. **We may not make it financially. We'll QUIT.**

So please consider a gift of a serious amount, i.e., more than \$1k, more than \$100 for some people or at least \$50 from less affluent types.

We will give you full receipts (for income tax purposes) and we will list your name in the Newsletter as -- a good guy. What else?

Please enter here the amount of your gift _____

PLEASE !

You'll get your reward in heaven.