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

Issue 27 (MT-27) Fall 1996

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.

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INTRODUCTION TO MT-27: The Newsletter (Editor this issue: H. Fleming)

THE HOTTEST AND THE LATEST NEWS, AS OF MID-NOVEMBER, 1996.

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 hottest news comes from Australia and Virginia but both badly need some/more confirmation.

Our traditional mode of presentation features fossils first.

DELAYED COMMENTS ON THE BASQUE CRITIQUE + NIHALI IN MT-II, COMING UP

The father of Sino-Caucasic, Sergei Starostin, has spoken, saying enough to be put in the Journal (MT-II) rather than the Newsletter. The editorial board of Bengtson, Wescott, and McCall will soon produce MT-II whose central focus will be on Nihali of India, with MT-Treatment of new Nihali data and the classification of this most difficult language, so potentially vital to South Asian prehistory.

NEWS OF MEMBERS' ACTIVITIES, INCLUDING LETTERS OF COMMENT

This rich lode of material has proven too much for MT-27, if we are to publish before New Years. We promise MT-28 with this lode by February.

ANNOUNCEMENTS & ADVERTISEMENTS: THE MEMBERSHIP (PERMITTED) LIST.

The eagerly awaited list of members who permit their names to be listed. While not necessarily an act of singular courage at a time of rampant conformity, giving permission to publish asserts something, like "This is no Communist cell. Let them know I belong! And I want to discuss prehistory with good colleagues!"

(Many members plan to face this issue, answer the questionnaire and pay their dues in their own good time -- later. So remember: the list of permissions does not equal the whole list of members, even including some Directors and Fellows. Will half of you please pay your dues? We cannot afford to mail out Journals to those who hoard money.)

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

We regret that the presentation of these has been deferred a while longer. Since we have noted the deaths of these valuable colleagues, we may be forgiven for 'bumping' their obituaries in order to present the hottest news first. Said news is very 'time sensitive' as they say nowadays, while the obituaries lack that attribute.

ASLIP BUSINESS

There is not much. A brief report on the Utah meetings. A small grant in aid from Boston University. List of libraries is growing. February!

INFORMATIONAL EDITORIAL: BORROWING SOME WISDOM FROM KINDRED SCIENCES.

We find remarkable similarities or analogies in our sister sciences. Michael Day (paleoanthropology), Lynn Margolis (deep taxonomy biology)

APOLOGIES! I was sick too much, traveled too much, etc. Sorry tardy!

Amazing New Data from Australia

Other than the vexing uncertainties of Chinese dating and the continuous resistance of Michigan's Wolpoff, no serious empirical challenge to the 'Africa first' hypothesis of modern human origins has remained upright. New excavations in Australia at one swell foop threaten the whole notion of an African diaspora for *Homo sapiens* s. Modern humans able to produce art and artifacts lived in northwest Australia (Jinmium, in the Kimberley area) a long time before most consensus dates for the African exodus, like many thousands of years earlier! More exactly, a site with presumed artifacts dates to 116,000 to 176,000 ya. The art consists of thousands of circles (all about 3 centimeters across) carved into boulders. But the art is apparently dated only to 66,500 ya. Or more precisely "a chunk of boulder bearing the circles lay in a stratum dated to between 58,000 and 75,000 years old." While that is very old for art, still it doesn't threaten the Africa hypothesis. We reported on similar Aussie art dates in MT-25.

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? One easily imagines a population of late *Homo erectus* type humans who did crude stuff which just barely qualifies as art. 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

old. Yes, but they have also been right! And we do not know which it is this time -- right or wrong!

By any reckoning the new Aussie material is a major contribution to global pre-history, unless of course it turns out to be 'bad data'. If the dates are only 50k or 60k too old, then as corrected they would be nearly contemporaneous with the African consensus dates of circa 100 kya. If the artists so-called were not *Homo sap sap*, they would constitute an advanced *Homo erectus* occupancy of Australia which we have known nothing about before this. It would roughly equal the older discovery in Europe of advanced *Homo erectus* / archaic *Homo sapiens*, i.e., Neanderthal with Mousterian culture. Again these early Australians would mean significant sea-faring for non-modern humans because Australia cannot be reached any other way. We do have to assume that there was a population of sorts, as Henry Harpending would wish, rather than one pregnant female accidentally floating over on a piece of driftwood.

Full reports of exactly what was excavated and how old various strata are and how well stratified the site(s) is -- all that is supposed to be coming out in Antiquity in December. We are operating off reports from (a) the New York Times (Sept. 22), (b) Allan Bomhard reporting it from e-mail, and (c) a brief summary in Science (Oct.4, 33-4)

For those impatient to hear more: the authors of the Antiquity paper are Richard Fullagar of the Australian Museum (Sydney) + Lesley Head

and David Price, both of the University of Wollongong, New South Wales, in the School of Geosciences. For the absolutely latest progress in dating with new OSL methods contact Rhys Jones (Australian National University, Canberra) or Richard Roberts (La Trobe University, Melbourne). OSL = optically stimulated luminescence, a slightly different dating technique. They mean to test the TL dates. David Price says that C^{14} dates of 'upper levels' already confirm the TL dates. However, with dates of this magnitude involved, C^{14} with its own limitations may weakly support but cannot confirm much of the deep stuff.

The Jinmium site provokes more thinking. First, one can not help comparing MacNeish's cave in New Mexico which also had startling dates and novel artifacts but was doubted to death by archeologists. The second is the parallel story of New Guinea and proper Melanesia where we find modern humans in the islands by 38,000 ya. Homo sap sap didn't get to Europe much earlier but had been on New Guinea maybe 10,000 years before that. So modern humans have lived east of Wallace's Line longer than most places, even if the Jinmium site fails to live up to its publicity.

Maginot Line: The Shredding.

The news from Virginia, reported by Mammoth Trumpet, is not good for advocates of the Clovis Horizon as first Amerind settlement of North America. Two sites with clear stratigraphy contained levels below the Clovis Horizon and with dates

just too old to be accommodated by Clovis theory. Just as the Rockefeller team's Amazonian sites could not be crammed into the Procrustean Bed of acceptable dates for early humans in the Americas. Americanist archeologists now must surround two more sites with the sticky bubble of their scepticism. Let us pray for them for truly their labors become too demanding! Or maybe the image of Peter at the Dike would be more suitable?

What are the sites? In ecologically distinct areas of Virginia. The first is **Cactus Hill** in the tidewater ('Niederländer') area of southeastern Virginia, 45 miles south of Richmond. There were found one or two cultural levels beneath an established Clovis level. The finds were "simple stone tools - blades and cores"; the pre-Clovis level has been dated to 15,070 BP by AMS procedures (accelerator mass spectrometer) but the Clovis levels were also dated by C^{14} (10,920 ya) and by the presence of white pine charcoal. That tree has not been in the tidewater for 10,000 years, said a Yale paleobotanist (McWheeny). Negative aspects of the site include sand dunes and the extensive looting of the upper levels or over 400 of 1400 square feet of site or 30% ±.

The other site is **Saltville** in southwestern Virginia in a valley of the Appalachian Mts. Here were found "stone and bone tools, mastodon bones and ivory, fire-cracked rock, and other evidence". While the site was dated by C^{14} to 13,990 ± 70 on a "twig in a sand lens that integrates a variety of presumably cultural material", it also had a C^{13} -adjusted date of 13,950 ± 70. That is the better part of

14kya. While the principal investigator was not an archeologist, his credentials sound fairly good, i.e., research associate in paleobiology at the Smithsonian Institution's National Museum of Natural History. His name is Jerry N. McDonald. His team has been laboring away at the site for fifteen years.

Sources are: MammothTrumpet October, 1996, and the chief of the Cactus Hill operations, Joseph M. McAvoy, Archeological Society of Virginia, 5861 White Oak Road, Sandston, VA 23150, USA.

That same issue of the 'Strumpet had some wonderfully interesting cranial/facial reconstructions of a 9900 year old American woman from near modern Austin, Texas. She has been lovingly named 'Wilson-Leonard II' by excavation logic but surely we can improve on that, as we did in Lucy's case. There are two casts, one made by forensic artist Betty P. Gatliff and the other by Arthur H. Rathjens and colleagues of Dow Corning Corp. The results are different to my eye, albeit much like sisters. Although Amerinds have often had a nondescript or unspecialized look, I am hard put to locate either model in a 'race'. Gatliff's could easily be an ancestor common to both Caucasoids and Mongoloids, or simply Caucasoid. The other looks strikingly like a forest tribal person from Southeast Asia, as pictured in Coon's Living Races of Man, or just as easily a pastoral Cushite from the Horn of Africa. Remarkable!

Also in the 'Strumpet was the second part of an extended interview they did with Marta Mirazon Lahr. Since we have

already reported much of her work (in MT-26), I confine myself to pointing out that her dispersal map of early humans entering the New World has them traveling by a coastal route, rather than through the Rocky Mountain passes, and via the Japan-Aleutian route. I point out humbly that we proposed just that several years ago herein.

Critical Mass: It's Been Reached

As this is not nuclear physics, nothing will explode. Yet we surely have reached a point where it's getting obvious that too many pre-Clovis sites in North and South America have been found. Too many, that is, for a stubborn doctrine that nothing before 11,500 ya can stand up to the scrutiny of expert archeologists. (Does this ever sound like Americanist linguistics!)

Let us rehearse a bit. The following sites with their dates have been published by competent professional archeologists. All are earlier than the Clovis level of 11.5kya. Here they are:

Saltville (USA) 15,000
Cactus Hill (USA) 14,000
Meadowcroft (USA) 14-17,000
Nebraska (mammoth site) 18,000
Alberta (Canada) > 20,000
Pendejo Cave (USA) 28,000 ±
Monte Alegre (Brazil) late
Pleistocene contemporary
Pedra Furada (Brazil) > 20,000
Monte Verde (Chile) > 13,000
excavator has receded from deeper levels and dates.

Marta Lahr mentioned three sites in far southern South America with dates close to the Clovis line. Again was there a 'forced march' migration from North America? Can all of the above be dismissed as not up to standard?

Clovis Culture in Siberia!

Presumably the vaunted Clovis horizon in North America was the cultural appearance of the first Amerind and/or the first modern humans in the New World. (It seems that no one believes the intruders went to South America first.) That culture dates to 11,200 to 8500 years ago (ya) or Before Present (BP) and has been the bedrock of certitude for American archeologists for at least a decade now. Presumably said Clovis people arrived from Asia as colonial immigrants, since Pardner Hicks has not yet persuaded scholars that Asians were emigrants from America. The search has been going on then, quietly, for the residues of that Asian or pre-proto-Amerind colonial culture and population in Siberia or Eastern Asia.

Just as final shrugs of regret for not finding anything relevant were being made -- lo and behold! voila! -- a similar culture or at least a similar stone industry has been found in the Russian Far East at Uptar, Magadan Oblast, northeastern Siberia between the Kolyma upland and the Sea of Okhotsk about 1920 km west of the Bering Straits. Its firm dates are from a stratum above the Clovis type stratum, 8260 ± 330 ya by C^{14} , so the "Clovis" level at Uptar has to be older, maybe much older. At a rough guess it seems a virtual contemporary of the North American Clovis strata.

Let us back track just for a moment because the reporters on Uptar have a nice summary of their regrets mentioned above. "the earliest firmly documented tradition in eastern Siberia (the Upper Paleolithic Diuktai

from the Aldan basin, 35,000 to 10,000 years B.P.) .. is thought .. to bear little resemblance to Paleoindian traditions." Let us go over this point for non-archeologists. Fossil cultures as old as 35,000 ya but not much like Clovis have been found in the Lena river basin of Siberia. So it is hard to derive the later emigrants to America from Diuktai culture.

However, Uptar provides the 'marker tool' (my coinage) that shows serious similarity with Clovis, namely lanceolate bifacial points, especially fluted points. Most of the 3100 artifacts collected were chipped stone; of them shaped artifacts are mostly bifaces or fragments of bifaces. Also a few cores, flake tools, some blades, with some microblades, were found. Yet the points were not identical to Clovis, being smaller in size and lack grinding on the edges and base. Nor would they fall exactly within the Nenana complex of Alaska or other cultures of Siberia or Alaska. The authors do not believe that Uptar is evidence for a source whence Clovis culture came, nor for a counter-emigration from North America. In linguistic terms we could say that there was a typological similarity without genetic links or evidence of borrowing. Or to quote the authors more directly: "The Uptar site shows that the early prehistory of northeastern Siberia is more diverse than traditional colonization models imply .. The focus on defining technologically distinct migratory groups (for example, pre-microblade versus microblade complexes) may neglect important aspects of assemblage variability, especially as they pertain

to issues of the peopling of Beringia."

Source: Maureen L. King and Sergei B. Slobodin. 1996. "A Fluted Point from the Uptar Site, Northeastern Siberia". SCIENCE 273, 2 August, 634-6. She is at Dept. of Anthropology, U/Washington or Desert Research Institute, Quarternary Sciences Center, P.O.Box 19040, Las Vegas, Nevada 89132 USA. He is at Dept. of Education, Apartment 19, 14 Dzerzhinsky Street, Magadan, 68500 Russia. We also recommend previous issues of Mother Tongue in which many of these Siberian and Alaskan sites were discussed more thoroughly. We should also remark that Uptar is entirely compatible with the coastal route to the Americas which Marta Lahr advocated above and ourselves earlier on. The same for the Ushki site on Kamchatka, but not for Diuktai.

Why Stress Clovis Horizons?

Some may have forgotten why we stress this argument about the Clovis horizon, since we started it a decade ago and have been bringing it up again and again. Several reasons exist: first, for archeologists with very high standards and strongly empirical orientations it is a **quality** problem. Brian Fagin argued eloquently for quality many issues ago in MT. This is the archeological equivalent of the linguist wanting only the best descriptive grammar before venturing an opinion. It cannot be faulted in its own terms because: who would prefer lousy data to the bright and shiny stuff? We can say, however, that such does not justify ignoring decent or suggestive data, albeit possibly defective, that

point to inferences different from the tightly-laced conclusions considered to be correct at the time. Weak and wobbly data or seemingly speculative theory may in fact presage major paradigm shifts soon to be required of the discipline. There still is no better model for this than Wegner's drifting continents which so revolutionized geology.

Second, many long rangers find in the Clovis horizon, itself spread over so much of the New World at the earliest date, a neat counterpart for the ancestor of the linguistic super-phylum = **proto-Amerind**. It is extremely tempting to settle for such a correlation! It warms the scientific heart to solve puzzles and the Clovis=Amerind solution verily warms some hearts.

Third, three considerations impinge on the above heart-felt theory. (a) Since Americanist linguists have no hypotheses of much serious time depth, piously insisting that no relationships as old as 10 kya can be known at all, their influence does not come to bear on the Clovis question. (b) It has always seemed to this Africanist that the time depths discernible in the many obviously divergent Amerind languages could not be contained within 11 millennia, (the Clovis framework) but had to be much older than that. Take as one example the 500 Bantu tongues of Africa whose genetic unity is one of the 'obvious' things in historical linguistics but whose time depth has at least 3000 years (with strong archeological correlates) in it. Only a few sectors of Amerind have such an obvious quality as Bantu, e.g., Iriquoian, Algonkian, etc, but a brief look at

Mexican languages will quickly dispel the notion of obvious. (c) The dates generated by biogenetic theory have always exceeded 10 kya when discussing native American populations. No one has to remind geneticists that their dates remain unsubstantiated; they admit that themselves. But the dates remain scientific hypotheses which have not been falsified -- yet. So they do bear on questions of Amerind origins. (For one false calculation see below: 'Cows')

Hot Linguistic News from Africa

Well known are the four increasingly solid linguistic phyla in Africa, courtesy of the Greenberg classification. What is often not understood by non-Africanists is the profound diversity within all of the African phyla. Anyone of them would be extremely controversial if their languages were spoken within the I-E or Americanist realms. I was amazed at one conference by a senior Oceanist who pooh poohed Greenberg's methods because the African languages were so much alike, he believed. In effect anybody could classify them -- peas in a pod they are. Simple stuff, not like the diversity of Oceania!

How do you explain to an Australian that his country is actually upside down? It is that kind of problem. Along with that ignorant old Oceanist there have been several Indo-Europeanists who have taken the same stance. Some have published as much, although the references do not come to mind right now. I find such people astonishing in their provincialism. Everyone has to know something about I-E, since all the text books fashionably

focus on I-E, yet so many of these parochial Aryanologists really know very little about Africa.

Well then, how do we explain to these Australians that their country is actually upside down? Hmmm. Might we use a universal measure of congruence versus diversity? like percentages of basic lexicon or whatever one can think of? One measure might be how many phyla a master taxonomist (e.g., Jos. Greenberg) could find in any area. If languages can't be classified together, then they are surely remote from each other -- at least. In the home of novice taxonomists -- the Americas -- we can at once infer diversity since the specialists can find no less than 100 phyla. Indeed in one tour de force they make South America the most likely source of modern humans; it has no less than 16 times as many phyla as any other region on earth. If Pardner Hicks is listening, he will rejoice for this is the best evidence he can get for an American homeland for Homo sap sap. However, by the criterion we started with above, South America has one phylum.

Of course in an area like India where we find the record for the Old World -- eight phyla -- still five of them appear to be intrusive (I-E, Dravidian, Munda, S-T, and Daic) or have very strong kin links on the outside. So let us count only those which are totally native, cannot arguably be derived from any outside group. Then India's 3 are matched by SouthEast Asia & Oceania's 3 and by Africa's 3. That is, Burushaski, Kusunda, Nihali are India's natives, while Australian, Indo-Pacific and Austric do for the Pacific

Rim. If one does not accept S-T as Sino-Caucasic, and by our criterion we cannot, then the Rim has 4 phyla and is champ. Africa loses Afrasian as part of Nostratic, keeping Khoisan, Nilo-Saharan and Niger-Congo. Again by our criterion, JHG has several times agreed informally that Afrasian was a member of Nostratic, but maybe coordinate. And in these terms Europe, of course, has only one -- Basque.

Now the news. Christopher Ehret has proposed that Africa in reality has six phyla, adding **Shabo** and **Krongo** to the list. As he announced this obscurely four years ago, when I was not tracking the subject, it never got into MT. Sorry for the delay! But thanks to Paul Whitehouse (London) for pointing out Ehret's article of 1995.

Shabo is spoken by a small group of hunters inside of Majang society, itself hunter-farmers. We reported Shabo in earlier issues. Fleming is on record, postulating Shabo's kinship with Koman inside Nilo-Saharan (N-S). Krongo is a new name for the group associated with Kadugli, i.e., formerly a part of Kordofanian of Niger-Congo. Thilo Schadeberg excised that group from Niger-Congo and put it in N-S instead. Both Fleming's and Schadeberg's hypotheses were reported in MT.

Ehret's new phyla are debatable, even as he sees them. But his main argument is that both Shabo and Kadugli are so far from N-S and N-C that they cannot be incorporated in either phylum. Rather they should be seen as independent phyla, possibly related of course to N-S or N-C ultimately at a very deep level. It seems to me that we both agree that Shabo is very

remote from anyone, while I see it as belonging to N-S in a primary branch or moiety with Koman at the root of the tree. In reality these are very similar views and reflect the great diversity within N-S or within Africa.

In discussing Shabo in 1989 I mentioned the complete lack of fit between its grammar, here to mean **morphology**, and those of neighboring languages. By now it has become apparent that all field workers have used the same informant and he is unreliable for grammar. The lexica recorded are not too dissimilar. So we may pray for better field work.

In the case of Krongo we frankly solicit the opinions of other Africanists or globalists, since my own expertise in Kordofan is too limited. Chris Ehret may be right cause he is a fine taxonomist. But Thilo's sharp eyes caught the problem in the first place, so his may be the right answer.

As far as African diversity is concerned, we rest our case. The implications for the Greenberg classification need to be specified. They do not prove the inadequacy of that work as might be thought. Rather the advent of this new data (Shabo) could not be predicted in 1948-63. Plus the Krongo question arises from newer field data, more intensive study, and uncertainty. When the dust settles, we may discover that Greenberg was mistaken about the total membership of Kordofanian. Or he was not. It's not a big deal, at least not for a mature science. For absolute truth seekers -- it's a problem.

Sources: Christopher Ehret. 1995. "Do Krongo and Shabo belong in Nilo-Saharan?". Robert Nicolaï et Franz Rottland, eds.,

Fifth Nilo-Saharan Linguistics Colloquium. Nice, 24-29 août 1992. Proceedings, pp.169-193. Köln, Köppe Verlag. Also Ehret's advanced work on N-S is coming out soon as a book called Reconstructing Nilo-Saharan. Publisher to be announced.

Since the Shabo live in the rain forests of southwestern Ethiopia, speaking a language too remote to be satisfactorily classified, then one senses that biogeneticists will be just as interested in Shabo bodies as they are in Pigmies or Bushmen. Thus it is a bit amusing but too sad to report that a trained biogeneticist walked right by a group of Shabo in Ethiopia last year, never knowing who they were. Thus reported Peter Unseth who would have told the fellow!

COWS & GENES: A miscalculation?

We reported biogenetic dates for cattle domestication in MT-26. Well, the European date of 5000 is false, since domestic cows in Turkey are 1000+ years earlier than that. (Bar-Yosef, p.c.) All this spells concern for African cattle theories and dates. If anything, judging by the Turkish test, the African domesticates will turn out to be **even older** than 9 kya! Say 11,000 years ago by proportional assumptions. Hmm

Deep Biogenetic Calculations

Having shown biogenetic errors, we want to highlight some recent & exciting work done by **Stephen Sherry** in his doctoral dissertation at Penn State (with Mark Stoneking as Thesis Advisor). Steve will present a fuller version of his conclusions next year in the Journal along with **MT*Treatment**. This

will feature an English translation from the BioGenetic Speak dialect of English.

Most crucially, the dating involved in Steve's thesis is more relational than absolute. That is, he produces time periods during which crucial events of human evolution and prehistory occurred, rather than a specific absolute date. Since the orientation is both towards DNA and population genetics, the result is a sort of progression of events happening to human populations. We present here his formal abstract from his dissertation, knowing full well that many long rangers will not understand it, because his thesis is important. We all will extend ourselves to try to savvy what is said, so that next year the effort will be easier to grasp. Thus saith his abstract:

"Data on human genetic diversity for two independently evolving genetic systems, mitochondrial DNA (mtDNA) and nuclear Alu elements, and mtDNA diversity in common chimpanzees were analyzed by three low dimension distributions methods which fit models of demographic history to the data by intensive computer simulation: mismatch distribution analysis, multi-variate crossmatch and segregating sites analysis, and spectrum of heterozygosity. Although the three methods use different properties of coalescent theory to derive their respective probabilities of gene identity and patterns of genetic divergence under demographic flux, they suggest a common hour-glass shaped pattern of human demographic history. We conclude that hominid numbers fell from a long-term effective size of 100,000 down to 20,000 by

400,000 years ago, and to a minimum size of approximately 5000 total individuals by 100,000 years ago before recovery at 50,000 years ago. Common chimpanzees show a similar episode of collapse from 55,000 to 85,000 years ago." (Quote ends)

Sherry's research has been submitted to several journals for publication.

Preliminary discussion with colleagues has indicated that the locus of populations referred to may be critical to understanding and evaluating Steve's argument. For example, 5000 may be a substitute for Cann's "Eve" or proto-Mother. See Henry Harpending's arguments in MT-19. Or 5000 may stand for all *Homo* on earth in 100,000 BP -- in which case we may doubt the 5000 on empirical grounds. Some think that known populations of *Homo erectus* plus archaic *Homo s.s.* greatly exceeded 5000 in total at that date. Und so weiter.

Paleolithic Paradigm Rocking

On the frontiers. Tomorrow we may see some of the familiar verities of the Upper Paleolithic of western Asia, especially the circum-Mediterranean area, somewhat differently. New archeological data from western Asia indicate that four 'facts' will be changed. First, that *Neanderthal* may be earlier there than in Europe. Second, the **Middle Paleolithic** is much older than expected, i.e., 250 kya. Third, that things usually associated with Cro-Magnon or Aurignacian culture may also be present in Mousterian, e.g., art (minimally figurines) and blades. Fourth, that the Upper Paleolithic was very slow to spread from either the Levant or Europe to the Caucasus or even

the interior of Syria. Slow = 35 kya, not 40 kya. Yet Aurignacian persisted in Greece until 20 kya which may be the source of the same in Israel in 18 kya.

All of this frontier stuff awaits the publication of sites now being excavated. Source: Ofer Bar-Yosef's extensive survey of sites this year and personal communication to me. He is not responsible for any errors I made in reporting or interpreting his communication.

Homo Gets Older in Ethiopia

Absolutely the latest news, reported in Boston Globe and NY Times Nov. 19th before its publication in the Journal of Human Evolution in December. For our purposes it is not the hottest kind of news, however. The jaw of a hominid, called the first of **genus Homo** by taxonomists, was found in Hadar of the Danakil lowlands of northeast Ethiopia (Afar) by an international team of scientists. Being an upper jaw, it had inferential value for the face which was broader than *Australopithecus afarensis* with a parabolic dental arch, a short flat nose and no "projecting, apelike" face, i.e. no prognathism. The dates of 2.33 mya are about a half million years earlier than other *Homo* genus forms, making this the oldest known form of that genus and by implication the oldest evidence of the start of our human line. In a sense the old 'missing link' which Haile Selassie feared. Alleged stone tools came with the jaw, but remember MacNeish's troubles with geofacts! Fossils pour in now: an area 5 times as big as Texas is being searched. Tribute to Berkeley's team: W.H. Kimball, R.C. Walter, and D.C. Johanson.

Some Members of Association for the Study of Language In Prehistory

Introductory Note: The following list is not a true list of the members of ASLIP for two reasons. First, some did not want their names listed (for all kinds of reasons). Second, barely 35% of the members returned the questionnaires, so 65% never said whether they would permit their names to be published or not. This even includes a number of Board members and Council Fellows, as well as some very well known long rangers. Consequently, the list is a true list only of ASLIPers who returned the questionnaire and allowed their names to be published. The label USA is omitted for American members.

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Informational Editorial:

Borrowing Some Wisdom from Kindred Sciences

Diachronically oriented researchers tend to come upon similar general problems and react to such in remarkably similar ways. I mean here to exclude documentary history and theoretical evolutionism. The first tends towards similarity but is usually so engrossed in details as to lose the general picture, while the latter as part of general theory in anthropology is much more interested in explanatory factors of cultural evolution with a marked tendency towards economic cum ecological determinants. In more general philosophy of science terms both of these aim towards **outlining the narrative and explaining the narrative**, differing markedly in preferences for one or the other.

Biology in general, paleontology and systematics in particular, as well as paleoanthropology, much of archeology, and traditional historical linguistics are more oriented towards the narratives of large areas, whilst making some explanatory efforts, because the narrative is so critical to their efforts. In a word we cannot generate explanatory hypotheses until we know what happened. A corollary to that is -- until we know what the relationships among organisms have been. It is a striking feature of both theoretical evolutionism and the hypotheses about language origins that both basically lack interest in the narratives, especially after the original great creative epoch (in the case of the latter). Or to paraphrase their mind sets -- who cares what happened after the Garden of Eden? The creation is everything!

We prehistorians in a general sense can learn from our colleagues in neighboring fields. Two examples are given here to show this. First, in biology, Lynn Margulis with Karlene V. Schwartz shows one of our problems clearly. In her FIVE KINGDOMS: An Illustrated Guide to the Phyla of Life on Earth. 1982. p.163, discussing "ANIMALIA", she wrote: "For many years (and even today), biologists divided the animals, protozoans and metazoans together, into two large groups: the invertebrates, those without backbones, and the vertebrates, those

with. In fact, all animals except the Craniata, a subphylum of Phylum Chordata, belong to the invertebrate group. This invertebrate / vertebrate dichotomy amply represents the skewed perspective we have as members of Phylum Chordata. Our pets, beasts of burden, sources of food, leather, and bone -- that is, the animals closest to our size and best known to us -- are members of our own phylum. We now realize that, from a less species-centered point of view, characteristics other than backbones are more basic and reflect much earlier evolutionary divergences." (End of quote.)

In fact the overwhelming pre-occupation of linguists with European languages, and historical linguists with Indo-European, looks to be the true example of a "species-centered point of view". It surely hinders our conquest of human prehistory. The viewpoint has been labeled "Euro-centric" by Ruhlen. The major narrative of that to which I-E belongs -- I-E's evolutionary background -- cannot be achieved by focusing on the reconstruction of PIE. No matter how elegant and satisfying that reconstruction may be it contributes little to the whole narrative because PIE is still only a twig on a giant bush, just as our vertebrates are only a small branch of the Animalia bush. Or do we really want to maintain that I-E has no relatives and thus an utterly unknown evolutionary past? I hope not!

Michael Day, one of my favorite writers on prehistory, discusses nomenclature and taxonomy in his useful Guide to Fossil Man, 4th Ed. 1986, pp.12-13. Two of his points are germane to our inquiry. First, he cites nomenclature rules in biology. "The naming of a new taxon must follow the rules of nomenclature as set out in the International Code of Zoological Nomenclature (Code Zoologique), a code adopted by successive International Congresses of Zoology and since 1973 by the Division of Zoology of the International Union of Biological Sciences.

"The internationally accepted object of the code is to promote stability and universality in the scientific names of animals, and to ensure that the name of each taxon is unique and distinct. Duplications must be avoided and the international understanding and acceptance of names is a prime objective of the Code. Priority is the basic principle of zoological nomenclature. It follows that nomenclature, the naming of a taxon, is not simply a matter of choice and taste; if the rules of the Code are not followed the world of zoology will not accept the new name for the good reason that it will inevitably cause confusion and controversy. The history of paleoanthropology is littered with nomenclatural solecisms not all of which are in the distant past." (End of quote)

The situation is so bad in linguistics that a custom has arisen which says that each field worker may change the name of his language if he wishes to or thinks the old one not quite right phonetically. The reader is referred to the closest approximation to chaos we have, David Dalby's Thesaurus of African Languages, 1989; herein almost anything goes -- there are numerous words for (e.g.) Kikuyu and even normal print is forsaken for a kind of handwritten text and ordinary phonetic symbols are replaced by an innovative Africanist set. It is all terribly avant garde and guaranteed to reflect accurately its internal taxonomy -- numerous twigs and branches in search of trees.

On taxonomy Day is even more useful. Quoting now: "The original Linnean binomial system was designed for modern forms whose distinctiveness could be determined by their inability to breed with other species and produce like and fertile offspring. In palaeontology this possibility is denied to us yet the affinity of a modern hippopotamus to a fossil hippopotamus that was living some thousands of years ago cannot be denied. The Linnean system of classification based on similarity encourages natural classification, yet small evolutionary changes make precise delineation between successive fossil species difficult to determine. The concept of the palaeo-species, or chrono-species, evolving through time, is an attempt to deal with the problem of the application of the Linnean system to palaeontology."

"An alternative approach is that of Hennig (1966). In this system taxonomic significance is attached to the appearance of new morphological features in an evolving lineage that may indicate a branch point on an evolutionary radiation of taxa. Features shared with previous evolutionary stages in a lineage are discounted as features of common inheritance. The sharing of new features is regarded as significant evidence of close kinship. The codification of these schemes allows diagrams, termed cladograms, to indicate relationships that are in no sense 'family trees' since a cladogram has no time axis. Cladistic or phylogenetic analysis can throw interesting and significant new light on the subject of primate taxonomy, particularly at the higher levels of taxonomic resolution; it is less effective when dealing with genera, species and subspecies. It can also produce inconsistent and confusing results when misused."

"Linnean taxonomy, based on similarity, and Hennigian taxonomy, based on difference, are complementary approaches to the problems of understanding the natural world; they each have a contribution to make, they each have their difficulties. Neither one system nor the other is wholly right or wholly wrong, only the dogmatic application of either will lead to false conclusions; indeed, the literature has examples already." (End of quote). The Hennig reference is to W. Hennig. 1966. Phylogenetic Systematics. Urbana: University of Illinois.

All of our taxonomic endeavours partake of the Linnean and Hennigian, it seems. Not just in historical linguistics but also in archeology and biogenetics. Concepts like **shared mutations** and **shared innovations** are particularly close to the Hennigian. Notions of continuity or genetic connection in archeology and much ordinary linguistic classification are more Linnean. Much of what is 'obvious' is Linnean. What is different is the one-sided scorn for the Linnean among 'mainstream' linguists, who nevertheless accept the lower level (Linnean) groupings virtually without question and stop there. Such impeccable logic! Day's arguments suggest that for long range taxonomy more Hennigian approaches will work better, specifically because similarities do get fewer and the obvious is no longer so. Much depends, of course, on how many languages are compared. Although it seems that historical linguists are cognitively challenged in the area of numbers, still even they can see that seeking similarities across 20 word lists is almost always going to produce more of them than you

can get between 2 lists.

We need to look briefly at the ostensible dismissal of common inheritance above in the Linnean discussion. What Day is arguing is that features of the common inheritance can be discounted -- as evidence for close relationships. If one will recall Trask's critique of the Basque-Caucasic hypothesis, he argued that a 'genitive in -n' for example was not evidence that Basque was related to Caucasian because such a grammeme is fairly common in human languages. But this is tricky reasoning since it is only closeness that is denied by this argument. Bengtson was not arguing that the two languages were close, but rather that they were related, i.e., they shared an inheritance. All those other languages which shared the grammeme were probably related too! 'Not close' does not equal or mean 'not related at all'. Day's point might be exemplified by stating that bats, cats, rats, and beavers share warm blood and suckle their young, yet only beavers share the special teeth with rats. Cats do not, so they are unrelated? And the flying bats? Well, since warm blood and mother's milk are quite common, they cannot show that bats and cats are related, either to each other or to rats or beavers? Those who believe in taxon called Mammalia will deny this and point out that these common features of mammals were once innovative among animals. So rats and beavers have a closer relationship but along with cats and bats they belong to a large taxon, itself part of Craniata, part of Animalia. Enough said.

MIKE DiBLASI REPORTS ON HIS EXCAVATIONS AT AKSUM, ETHIOPIA

Consistent with our policy of occasionally focusing on matters of lesser time depth than Indo-European, we give you a short summary of recent research at Aksum. The importance of this research needs no profound justification -- it is simply interesting. As part of the slow but sure process of unveiling the prehistory of this most crucial of world areas for general human prehistory, probably also for Nostratic and Afrasian in particular, we're trying to publish almost everything that contributes to that goal. Aksum in Tigray is a critical interface between Semites and Cushites, Meroe and Aksum, N-S and Afrasian, and the trade of the Nile Valley cum Red Sea cum South Arabia cum Indian Ocean cum the Ethiopian interior. We have published some of S.Brandt's, A.Brooks', and K.Bard/R.Fattovici's work before. We also call attention to gifted younger colleagues, like Mike DiBlasi.

Because of format demands, we present the Bibliography first (!).

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Human-Environmental Relationships and the Aksumite State: Recent Archaeological Research at Beta Giyorgis, Aksum, Ethiopia

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Since 1993 Boston University (BU) and the Istituto Universitario Orientale, Naples (IUO) have been collaborating in a multidisciplinary research program on the amba of Beta Giyorgis near Aksum in northern Ethiopia. The project, directed by Kathryn Bard (BU) and Rodolfo Fattovich (IUO) is investigating the origins and development of the Aksumite state from an environmental perspective. Two issues that are of particular importance, and about which very little is known, are the impact of Aksumite settlement on the environment of the region and the characteristics of the Aksumite subsistence economy.

Excavations on Beta Giyorgis began in 1993 and two areas of the amba have been investigated: Ona Enda Aboi Zewgé (OAZ) and Ona Nagast. Ona Enda Aboi Zewgé is a large (ca. 17 ha) mortuary complex located on the eastern side of the amba. It consists of over 100 monolithic stelae and numerous rock-cut tombs and burials. The stelae, which include rough hewn as well as finely shaped types, mark burial/tomb areas rather than individual graves. Radiocarbon dates and associated pottery found at the six excavation units explored here thus far indicate that parts of the mortuary complex may have been used from late pre-Aksumite to Middle Aksumite times (ca. 300 BC–AD 700). The pre-Aksumite period is associated with the Kingdom of Daamat, a culture with strong Sabaean characteristics that flourished on the Tigray and Eritrean plateaux from ca. 700 to 300 BC (Fattovich 1990).

Ona Nagast is a large (> 12 ha) elite residential complex located approximately 600 meters west of OAZ. Exploratory excavations here in 1995 and 1996 have uncovered the remains of a substantial complex of stone structures distributed across three apparently artificial "terrace" levels. Radiocarbon dates and associated pottery from the excavations indicate that the settlement was occupied from ca. 390 BC to AD 660, making it contemporary with the mortuary complex at OAZ (Bard and Fattovich 1995). In 1995 sediments from the occupation levels at Ona Nagast were watersieved to recover macro-botanical remains. Carbonized remains of wheat and barley were identified from proto- to Early Aksumite deposits, and remains of wheat, barley, teff, lentil, and a grape seed were found in Middle Aksumite deposits. These represent the first domesticated plant remains to be found in Aksumite contexts. In addition to botanical remains, a large quantity of animal bone (cattle and sheep/goat) has been recovered from the settlement (Bard and Fattovich 1995). Future research will examine in greater detail the botanical and faunal components of the Aksumite subsistence base.

Exploratory pollen-analytical studies are being conducted to determine the characteristics of the vegetation cover of the Aksum region *prior* to Aksumite settlement and to assess the impact of Aksumite land-use practices on the vegetation through time. Plant ecologists and environmental historians believe that the Tigray plateau supported dry evergreen montane forest or deciduous woodland vegetation at some time in the past. It is generally assumed that population growth and intensification of land-use practices in Aksumite times was responsible for the transition from a forested or woodland pattern to the montane grassland vegetation that dominates the region today. Preliminary analyses of seven pollen samples from deposits of late pre-Aksumite (Daamat period) through Middle Aksumite times (i.e., late 1st millennium BC to ca. AD 700) at Ona Nagast and Ona Enda Aboi Zewgé suggest that herbaceous plants and shrubs were plentiful, but trees were not common components of the vegetation cover on or around Beta Giyorgis during these periods (DiBlasi 1996). Although they must be corroborated by more detailed palynological analyses, these preliminary data indicate that the transition from forest/woodland to grassland in the Aksum area took place well before the period of maximum population growth and land-use intensification as reflected in the archaeological record (Michels 1994).