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

Issue 25 (MT-25) Summer 1995

**MOTHER TONGUE : NEWSLETTER of the Association for the Study of
Language In Prehistory. Issue 25. Summer 1995**

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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LEADING DISCOVERIES AND NEWS, 2nd Quarter 1995

A Very Old Englishman Unearthed

Remains of one of the oldest inhabitants of the British Isles were recently found in an excavation at Boxgrove (spelling not secure) in southern England. As displayed on American television recently (June), a hand-axe wielding hominid standing perhaps six feet tall and quite robust had inhabited that part of England around 500,000 years ago. George Wainwright of University College, London, led a team of what looked to be scores of young archeologists in the endeavour. Boxgrove man was said, by the program, to be ancestral to Neanderthal. That analysis was based on a piece of his lower leg, perhaps most of a calf bone. We regret that nothing else was said on the squib and there are no published (written) accounts that we know of to confirm the brief report.

But Many, Even Older, Basques

More recent but more solidly reported and analyzed than the first Brit, some 36 fossils (skull, dental and jaw fragments from four persons) were found in a cave near Burgos in the Atapuerca region of northwest Spain. The team leader is Eudald Carbonello of U/Tarragona but dating was done by Josep Pares of Institute of Earth Sciences, Barcelona. Comments have been made by F. Clark Howell (U/C-Berkeley). Science (8/11/95) has the report as does Associated Press. The import is very heavy and will be pursued here in, with luck by Günter Bräuer or another long ranger. 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. To stress this: Howell, a world class paleoanthropologist, is quoted saying: "These are not Homo erectus. These are something different." Amen, a hot topic!

Most Important Find in the Nile-Congo Zwischengebiet

John Yellen and colleague archeologists have unearthed a key piece of evidence for putative early modern man around 90,000 years ago. It was found during excavations in the highlands between Lakes Edward and Albert along the Congo (Zaire) border with East Africa (Uganda). This area abuts what early German explorers called the Nil-Kongo Zwischengebiet -- between the watershed of the mighty Congo and that of the majestic Nile. Although technically in political Zaire, the site basically is in the east African highlands. One might also call the area the Pygmy-Bushman Zwischengebiet too. And the country is beautiful.

Yellen's team found harpoons (and more) which in details of their manufacture are considerably more like the work of early modern technologies such as the Upper Paleolithic of Europe than like the cruder technologies of the Middle Stone Age or earlier periods -- but with some African cultures like Lupemban sometimes sophisticated too. In brief, they thought it was the work of Homo sapiens sapiens, i.e., anatomically modern man. The thinking is basically correlational, of course, because cruder stuff mostly is associated with non-modern humans, while the more specialized, more finely turned-out stuff mostly is associated with modern humans. Clarifying the reasoning does not, of course, in any way refute it. But it does mean that statistically oriented thinking would insist that the harpoons could have been made by non-modern humans.

Harpoons have been found in later periods in Uganda where their dating unfortunately was bedeviled by contamination of shells (used in the dating) and one can only say that a kind of harpooning tradition persisted around the east African lakes for many millennia. What it led to or who was linked to it cannot be said, although John

Sutton's 'aquatic civilization' circa 7000BC might link it to Nilo-Saharan. (Dan McCall suggested it.)

However, modern peoples in several east African lakes still hunt hippos with fire-hardened wooden spears, use hooks for lesser fish and possibly harpoons for the very large fish endemic to those lakes. (It's only a question of memory and looking up the ethnographies - sorry!)

Long rangers need hardly be reminded that this is the first archeological site in Africa and practically everywhere else in the world where this putative association of advanced tools and modern man can be found so early. True the Levant has earlier moderns (90-100k) but the tools are not so spiffy. Given the far far greater intensity of archeological excavation in the Levant, compared to eastern Africa, then it is quite reasonable to expect that more sites like this will be found in the four great highland countries (Uganda, Kenya, Tanzania, and Ethiopia).

Yellen's discoveries do not threaten the thesis that early modern mankind either began in eastern Africa or got there soon after their origin elsewhere.

Remarkable Australian Paintings

Only the magnitude of John Yellen's African finds can overshadow the highly significant but quite controversial proposals by Australian archeologist, Rhys Jones. With new dating techniques and bold assertions about ores and paintings, Jones' new hypothesis is that modern man reached Australia around 60,000 years ago and was able to paint pictures in a very credible way.

In a word -- 'good' art some 30,000 years ± before Europe's renowned Upper Paleolithic art. (Recently, new cave art in the Chauvet cave in southern France has pushed their dates from 27k to 32k) Those 'primitive' Australians were

certainly early (one meaning of primitive) but not so backward (a second meaning of primitive). Now that the Africans of 90,000 years ago were whittling bone and the Abos of 60,000 were painting on walls, surely we can harbor a revolutionary notion -- that Europe was actually a backward place for many many millennia after the great human diaspora had begun.

It would seem logically hard to keep the Eurocentric view that our Neanderthal kin were the immediate ancestors of modern people, when, during Neanderthals' heyday in Europe, technologically or culturally more advanced moderns were living in the great tropical realm from Africa to Australia.

Jones' sites, Nauwalabila I and Malakunanja II, both rock shelters in northern Australia, are not new, being known from the 1970s but the dating is new -- 'optical dating' and thermoluminescence.

Ancient DNA: A Third Time at Oxford

This refers not to some tenured mandarins but rather to an interesting conference held recently at Oxford University. The third conference on ancient DNA was held, with a fine group of scholars in attendance. It is important to point out that the ancient DNA in question is that derived from direct examination of fossils, relics of the tissues of the formerly living, not from any inferences made about ancient populations from modern field data.

Whatever their intentions, however, some papers did project back in time the data from modern folks.

Thanks to reporters, Becky Cann and Andy Merriwether, some items of interest. First, great scepticism currently awaits any conclusions based on fossil data older than, say, 10,000 years. Too much tissue loss --> unreliable data.

Second, despite that, many people are trying to extract some things from Neanderthal bones. It is very hard going technically but the

potential rewards are great. Of course, as everyone knows, finding some clear nuclear DNA or mtDNA from Neanderthal fossils will help test Cann's thesis that *Homo sapiens s.* is quite distinct from Neanderthal.

Third, so great is Cavalli-Sforza's prestige that many at the conference were surprised to hear that HGHG had not actually covered the whole world -- properly.

Fourth, Basques are not so very different from west Europeans, especially British, French, and Iberian. Nor did HGHG show a great chasm between the Basques and their fellow Atlantic fringers either. If there remain doubts about this, the mtDNA results from western Europe will dispel them. Words like 'quite similar' described Basques in relation to the others. We have all been eager to show Basque affinity with the folks of the Caucasus, which no one can truly refute because we actually know little genetically about Caucasian-speaking peoples. Will someone please send a group of graduate students to New Jersey where quite a few West Caucasian-speakers live! Or to Israel where many Circassians are found! Can't the many reporters doing the Chechen war bring back some vials of blood? Or purses stuffed full of hair follicles?

Fifth, Andy's paper itself was a major event. He has more data from South American Amerinds, Yamomami in particular, which show the presence of even more haplogroups in the New World than were proposed before. His previous paper in MT-23 is stronger now. There was basically one migration to the New World, bringing Amerinds, Na-Dene and Eskaleuts from an Asian homeland or dispersal point in or around Mongolia. Actually from his own remarks it seems that Tibet is a bit more likely. Andy's data are mtDNA, as you know, and his research is right on the 'cutting edge' as they say, judged by the people he cites and who cite him. It is also amusing that the peoples who link together in Andy's analysis are elsewhere

called Mongoloids. In Cavalli-Sforza's dendrograms the branch called 'Northern Mongoloid' fits Andy's group very well.

Sixth, another spin-off of Andy's research, also mentioned by others at Oxford, is a retreat from the notion of dating by biogenetic analysis. It is not in principle impossible; it is too uncertain at the moment to be trusted. So Andy and some others, at least, are avoiding the chance to date migrations into the New World. Some say they heard him propose a date of 15,000 BP for the basic migration from Asia but Andy denies that. He might have said it at the conference but his considered opinion is still negative about dating the migration.

There were other topics at the Oxford conference but we will report them elsewhere (e.g., Robert Wayne's work on dogs -- below). We also do not know if or where or when the conference will be published.

Adam Comes to Eve's Aid

The truest complement to mtDNA and its matrilineal descent is the Y chromosome and its patrilineal descent. Three geneticists, Robert Dorit (Yale), Nobel-laureate William Gilbert (Harvard) and Hiroshi Akashi (Chicago), examining a segment of the male chromosome, found little genetic variation among 38 males from many 'racial' and geographical origins and concluded that their common origin was quite recent, say 270,000 years ago. The data were drawn from 'cheek scrapings and hair follicles.

However, Milford Wolpoff of Michigan said the Adam study and the Eve study were quite different and no conclusions should be drawn. Conversely we might add that nothing about the Adam study should be seen as supportive to multi-regional theory, just as in the case of the Eve study.

Significant activity in linguistics

Except for the fascinating case of a stalled paradigm whose followers grow ever so slightly more modest,

nothing interesting has come out of linguistics in too many years. The great impact of Chomskyite theory on psychology and philosophy is now history, as the Americans say. Historical seems to be the only branch of 'scientific' linguistics still bearing fruit. It is also the oldest branch. Extraordinaire, n'est-ce pa?

One of our basic taxonomic regions -- Southeast Asia-Oceania -- is bearing large fruit these days. Austric is finally coming out of the doldrums of scholarly caution. A super-phylum whose reach is from south India to Easter Island but whose core lies between Formosa and Bengal will enlighten prehistory in those parts. Accounting for at least 1/4 of human languages, it is very exciting!

Robert Blust has tipped the balance between cautious and very cautious scholars, in favor of Pater Schmidt's original notion that Austroasiatic (AA) was linked to Austronesian (AN). Robert is very clear about what he has done; reviewing the literature and making decisions in debates.

First, he has supported his colleague at U/Hawaii, Lawrence Reid, in Reid's analysis of the problem. Step one was to break up Paul Benedict's Austro-Thai (AT), while disparaging Benedict's methods and reconstructions. Step two is to relate the newly liberated AN to AA by affirming the only evidence that Benedict has agreed seemed to exist -- morphological. Tacitly perhaps, although no credit is given, this seems to be a confirmation of Pinnow's work stressing morphology in Austric.

Second, he stressed the rarity of the specific bound forms used to bind the phyla together, rather like marker genes in biogenetics. As many linguists are aware, there are three common verbal affixes; infix *-um-*, prefixes **pa-* and **ka-* (1st usually 'inchoative', the others causatives) in Indonesian languages and Mon-Khmer and

elsewhere in AN. In one AA language, Katu, the two prefixes combine in a double causative *paka-*. Although some use the word 'infix' rather loosely (e.g., in Indo-European, Afrasian), their 'infixes' are aspects of the phonology involved. A true infix has to be like other bound morphemes, a particle with ascertainable meaning. The only true infixes I know about in Africa are found in Koman of Nilo-Saharan where verb roots are split in two and pronouns infixed = put in the middle. But such is rare.

Anyway the *-um-* infix is an accepted part of Proto-Austronesian (PAN), so far as I know. Blust extends the three affixes to key parts of AA (e.g., Nicobar Islands) and draws the simple conclusion that such rare but verbally significant morphemes occurring in widely separated languages is per force evidence of kinship.

What about borrowing? Well, let me be Blust's advocate here. We will offer a prize to the first person who can demonstrate the borrowing of a true infix between any languages of the world. If some of us think that the borrowing of pronouns is rare or non-existent, that is still inherently more likely than the case of the Austric infix. It is not part of universal semantics like the pronouns are; rather it is part of specialized verbal behavior. Who will take up my wager? Who will win?

Third, having shoved my friend Paul aside -- maybe even denting his famous self-confidence --, Blust then allows as how Reid has shown AN cognates with Daic (Thai-Kadai) by properly correlating CVCV forms of PAN with CV forms of Daic. (In fact both Benedict and Matisoff had done this before; it was even mentioned in *Mother Tongue*), thus in fact bringing all of the former AT into relation with AA in the new Austric. This is not Robert's finest collegial hour.

Fourth, at no point is Miao-Yao ('Hmong-Mien') brought into this new Austric; mentioned but not included. And naturally the new field data

coming back with Gérard Diffloth have not yet been incorporated into the mix either.

Fifth, it is quite important to stress how important a few scholarly decisions are when opinions and arguments are in conflict. Some good people have supported Austric; some good people have denied it. Blust tips the balance because he has great prestige as the finalizer of AN taxonomy and as a very competent but careful law-abiding 'professional'. With Greenberg, Diffloth and Yakhontov already backing Schmidt, Blust and Reid are enough to surmount the opposition of Dyen and Benedict. Add Pinnow and Matisoff to the scales on different sides. Only Norman Zide can tilt the balance back to level -- or even more one-sided.

Sixth, incidentally Japanese does not get included in Austric either. Its membership is rejected. (References at end of next topic)

Austric Hypothesis as Prehistory

In the fine tradition of Oceanian anthropology Blust puts his linguistic taxonomy to work in prehistory. Having agreed that the new super-phylum (words he eschews) was vast and complex, he set out to find its dispersal point and time depth. One will have to read his paper to follow his reasoning and get his more specific conclusions. Here I sum up his primary findings:

1) The homeland of PAN must be on or near Formosa. That is settled prehistory nowadays, it seems. Blust moves it there from the Fujian coast & oldest rice-farming areas in mainland China.

2) In order to join up with AA ultimately, it has to go to older highland areas to the west, where agro-ecological factors put it. Most likely the Yangtze-Salween Zwischengebiet, the highland area from which rice farmers spreading out would have access to most of Southeast Asia.

3) The homeland of AA lies not so far away in the same highlands to the west near Nagaland. Rice

farmers or wild-rice reapers again.

4) Proto-Austric (PAU) is essentially a foregone conclusion after the premises of 2) and 3). It is located to the south somewhat, again in the highlands along the Burma-Thailand border area. The Ur-Austricans were getting ready to be farmers, harvesting wild rice.

5) The dates of these proto-languages in their homelands, though informed by linguistic dates, will be governed by agricultural dates obtained from archeology; that seems clear. We get back to 15,000-10,000 for a kind of pre-Austric incipient rice cultivation stage, then 7000 BC \pm for PAU in its homeland, 5500 BC for PAA, and one millennium later for PAN. At one point Blust cites a lexical retention count (Swadesh list) of 7% between AN branches. This he believes fits nicely within his general estimates of the time depth of PAN on Taiwan of 4500 BC, i.e. 6500 BP. He exemplifies the statistics by Cebuano and Roviana, from the Philippines and Melanesia respectively, which he believes were separated only 5000 years ago, despite their 7% retention which should imply 6000-8500 years ago by standard formulae. Why not? The retention rates varied. (Probably because of Melanesia)

All of this is derived from a paper Robert gave in 1993 at a conference sponsored by Ward Goodenough, famous Oceanist anthropologist, at the University of Pennsylvania. It should be published soon. Since Blust sent me a copy of the paper and since it was delivered publicly at Penn, I presume that it is kosher for us to publicize its main points here in. If this presumption vexes anyone at Penn, please so inform me.

A Comment on the Austric Prehistory

Blust's theory of Austric prehistory strikes me as highly sophisticated, in the Oceanist tradition. Still it is similar to those of Renfrew, Cavalli-Sforza, and Ruhlen on Indo-European. The reasoning is profoundly ecological and archeological, but seriously

linguistic too, i.e., informed by Sapir and Swadesh! Yet it does seem that he had a Procrustean Bed -- the probable Neolithic revolution in southeast Asia -- into which he molded the sub-groups of the Ur-superphylum, their homelands, their movements, and the dates for all of these. Maybe 'molded' is too strong a verb?

As concerns the dispersal of the various sub-groups and the testimony they bear about the ultimate homeland, I suggest that Robert take another look at J.P. Mallory's analysis of Indo-European. Since taxonomy makes such a big difference to dispersal theory, he ought also consider Gérard Diffloth's last remarks in Mother Tongue, worrying about the divergence of Munda. Such would force PAA much closer to India, it seems.

More importantly, if he accepts AT and Miao-Yao is half of AT, residing in eastern China as he says, then AT's homeland might lie smack in the middle of the Neolithic unveiled recently in east China. Of course, Miao-Yao has been a problem for every taxonomist in the region, so Robert has our sympathy here.

On the question of dates we have a case of archeologically probable dates and a big 'epistemic correlation'; leaping to their linkage with proto-languages. Since that is what we do all the time, the real question is where his linguistic dates come from. The archeology? So the archeology is the source of the archeological dates and the linguistic dates? As one can see the circularity of that reasoning quite easily, once it is pointed out, let us point it out! Blust unfortunately seems not to trust linguistic dating other than Paul Thieme style, which we might call 'the logic of reconstruction'. If PAN had a word for rice and settled villages, then the date of rice's domestication in settled villages gives the date of the proto-language. This is the

Afrasian problem with Natufian all over again; the linguistic date is chosen because it fits the archeology well. But said linguistic date has no integrity of its own.

If we use glottochronological estimates, a percentage as low as 7% argues for 13+k years of separation, as Kruskal, Dyen and Black see it, or between 6k and 8.5k depending on the use of 80% vs 84% (using " $t = \log C \div 2 \log r$ ") in 'standard formulations' or by Greenberg's calculations. (8500 is Joos-based). So PAN may be as young as 6000 years or as old as 13,000 years. Given the greater realism of the Joos formula, I would guess that PAN is 8500 years old -- at least. It may be that linguistic dating usually fails in Melanesia because of severe borrowing problems with Papuan but that does not necessarily mean that it fails in the empty Pacific, especially Micronesia where we can also get down to 7% or thereabouts.

Just as a hunch, seeing that Benedict has established that just about no common vocabulary exists between AN and AA, I would guess that lexical retention between any two samples from either phylum would show percentages down around zero. Since Afrasian gets almost that low, but usually not quite, then I would also guess that PAU is older than Ur-Afrasian. If you will recall Swadesh's calculation in an early issue of Mother Tongue, note that 1% (one percent) retention should yield 22,000 years of separation. That is by the inventor's standard calculations. Actually by Joos, Greenberg only gets 20,000 years at 1%.

Even if these calculations are wrong by a quarter, say 5000 years, still the result of subtracting that from 20-22,000 would be 15-17,000 years. Surely that is much older than any Neolithic we know of. And surely one must be more wary of 'epistemic correlations' (F.S.C. Northrop, 195-. Original date and definition lost to memory. It is like 'circumstantial evidence', only more refined.)

Finally, quibbling aside, I think Blust's paper is a powerful boon to our endeavours. By moving Austric out of the traffic jam and by proposing matching prehistory, he will keep lots of scholars busy checking it all out. Assuming for the nonce that Miao-Yao was supposed to be included in his Austric, we can say that Robert has enormously simplified the linguistic picture in a crucial part of the world. Oceania and its mainland now have no less than three major superphyla, Austric, Indo-Pacific and Australian. Plus the major phylum (Sino-Tibetan) which may or may not belong to an outside super-phylum, Dene-Caucasic. Congratulations!

New York Times Notices Linguistics!

During the past decade of activity in human origins science the leading American newspaper has conspicuously ignored most of the developments, but especially the linguistic ones. Thus it is noteworthy that the Times suddenly printed a big spread on Nostratic in its Tuesday 'Science News' section (June 27, 1995, C1, C13). Little new stuff in it, except that Bomhard got some due recognition and Manaster-Ramer unexpectedly appeared as a prime researcher in this field. Don Ringe seemed to be saying that the Uralic+Indo-Aryan hypothesis passed the 'Ringe test'. The tree of Nostratic presented was not accurate but it was Eurocentric to an extreme.

Another severe attack on Ruhlen

Some linguists criticized me last year for being too harsh in some of my comments on linguists. I said I was sorry. But now, good colleagues one and all can read something very very harsh from the other side. Even Lyle Campbell and Ives Goddard are pussy cats compared to some of the critics of Greenberg, and more recently Ruhlen.

Get a copy of Anthony Grant's review of Merritt Ruhlen's On the Origin of Languages: Studies in

Linguistic Taxonomy, 1994 which appears in Anthropological Linguistics 37, number 1, 1995, 93-96. After reading that piece of academic Schadenfreude, no one will ever again accuse me or Lyle Campbell of being harsh. By the way, that journal(AL) seems to have joined Language and IJAL in being totally biased. Like the three famous monkeys: see no evil, hear no evil and speak no evil -- where the Amerind theory is evil incarnate, in the body of Joe Greenberg. Heavens!

Important Activity in Biogenetics

Cavalli-Sforza and his son Francesco have published a more compact and less technical version of HGHG. Put out by Addison Wesley (Reading, Mass.), entitled The Great Human Diasporas, it may be valuable for most long rangers. We will review it in the next Newsletter. A non-biologist, ethnologist/historian, Dan McCall will do the review.

Fresh data on human phylogeny

Careful readers of HGHG will recall that HGHG's research was 'up through 1985'. In a fast-moving field like biogenetics new data keep piling up even after masterful summaries. It's like shoveling your sidewalk during a blizzard; as soon as you finish, you must shovel it again!

We reported above some important new researches. Others are put off until MT-26, especially new Japanese research on DNA and Meave Leakey's new hominid ancestor to Lucy. Thanks to excited colleagues for mentioning these to us!

New Dating of Biogenetic Phylogeny?

New analyses, new data, new dates for anatomically modern man, starting the diaspora from Africa. This are mostly confirming type conclusions but the analysis is new. David Goldstein (Pennsylvania State) and colleagues reported in Proceedings of the National Academy of Sciences, July 18, 1995, that they had determined a date of 156,000 years for the African diaspora. The

technique involved microsatellites, (nuclear DNA particles) and assumptions about mutation rates and the length of generations. Theirs was 27 years. Masatoshi Nei at Penn State said that small, isolated groups of early humans evolved independent mutations which can complicate calculations. Also he favors assuming 20 years per generation, making 115,000 the date of diaspora. However, it seems moderately culture-bound to assume a generation of 27 years, modern middle class Euroamericans. I would assume about 16 years, making the diaspora about 92,000 years. David Pilbeam (Harvard), when asked, saw 15-20 years as the best assumption for chimpanzee generations. So perhaps Nei's estimate fits best.

Alan Templeton (Washington U.) thinks the new techniques are interesting but, of course, the assumed split between Africans and non-Africans never occurred. Our common humanity goes back nearly a million years and genes flowed between continents, he says.

Mutation Rate Slower for Hominids?

Wen-Hsiung Li (U/Texas-Houston) has found that mutation rates, at least at nucleotide sites, go slower for humans (1.2 changes per billion years) than Old World monkeys (1.8 ...) and New World Monkeys (2.1 ...). All are faster than rats (4.8 ...). Morris Goodman who has proposed that such would be the case was delighted, while Vince Sarich was unhappy initially at least. I wonder if the size of the animal makes any difference, since there is a size progression from rats to Romans.

One is reminded of the earlier discussions of Bushmen mutation rates where some proposed that the mutation rates were faster for bush people than for others, thus denying the genetic divergence of the Khoi and the San. If Wen-Hsiung Li is right, then Bushmen are even more divergent than thought.

Dog Domesticated Man: When? Where?

Our canine friends are not the only animals we have persuaded to share our lives -- pigs, horses, chickens, cows, asses, shoats, and camels are not trivial -- but *Canis familiaris* figures to share a great part of our prehistory too. More than the others. Actually the domesticated animals who potentially may tell us even more about our prehistory are the lice and fleas who have been with us everywhere and whose own taxonomic evolutions will correlate with our own. Then dogs have their own fleas but who will we ever get to do research on that!

On dogs we have a biogenetic expert, Robert Wayne of UCLA. Again thanks to Becky and Andy. Professor Wayne has written several recent articles on canine phylogeny, using DNA, and told me the highlights of his research on the phone. (His writings are being pursued.)

Exposed to a linguistic view that early *Homo sapiens* probably had dogs as friends or co-hunters, he said that "there was more than one episode of domestication", i.e. it happened several times. Exposed to an Africanist scepticism about *Canis familiaris* being descended from wolves -- African dogs do not look so much like wolves --, his firm retort was that all domestic dogs derive from grey or timber wolves (*Canis lupus*). And the most divergent genetically are the "New Guinea singing dogs" and the famous dingo of Australia. Well! After being asked if he supported the usual archeological dates of, say 10-30k, for domestic canines, he agreed with those dates.

Finally, I penetrated his firm convictions by commenting on the likely age of the dingo -- 55-60k almost required with human partners. Well, yes, he grasped the logic but couldn't relate that to his data.

Biogeneticists are reliable more on taxonomy than on dating, it seems. (See above Oxford meeting) Good old dating -- everyone's most difficult problem!

For the sake of alternative hypotheses of canine ancestors. Dr. Wayne was also firm about coyotes being closest to grey wolves; then jackals, except the so-called 'Semien jackal' of northern Ethiopia which is really a wolf. Still farther away are vixen and most remote our beloved African wild dogs (*Lycaon pictus*) with their trumpet ears and ferocious hunting in packs; India's dhole (*Cuon alpinus*); and so-called bush dog (*Speothos venat.*) of Asia. It appears clear enough that early man in eastern Africa or Sundaland surely could not have domesticated grey wolves. North Africa, the Near East or northern Eurasia would have them. At a minimum the Borean sub-groups, such as Nostratic, Amerind or Afrasian, would have been in touch with 'real dogs'. Indeed those groups are the main support for the possible proto-Human reconstruction of *kuon or something like that. May we consider that sub-Saharan African experience with dogs is different and has different words? And ditto that of Sunda- / Sahul-land?

Chimpanzees are virtual carnivores!

Evolutionary theory has always had to cope with the dietary differences between humanoids and the great apes -- leaf and banana munching apes but a savagely carnivorous Cave Man type of human. We have known, of course, for a long time that ethnologically we gather as often as we hunt, except the Eskimo and some others. *Homo sapiens* is omnivorous.

We have also known that chimps do sometimes eat meat. What is new are the recent appraisals of chimp meat-eating. Not only is it much more common than thought but also the notion that chimpanzees have something like a lust for meat is now upon us. Craig Stanford (U / Southern California) was reported in the New York Times (June 27, 1995) with these new emphases, including the new observation that sharing meat plays an important part in chimpanzee social interactions.

Checking with David Pilbeam, I was reminded that male chimps do have 'large projecting canine teeth'. Those are not for cracking nuts or munching bananas!

Despite King Kong and Godzilla, are there any reports of gorillas or orangutans eating meat, or much meat?

Chariots in Syria in 3000 BC?

Chariots older than any likely advent of early Indo-Europeans are reported by D. Michael Fuller (St. Louis Community College at Florissant Valley, MO, USA) at Tel Tnir in Syria about 480 km northeast of Damascus. The site was a marketplace; a horse and two chariots were clay statues. Someone should look into this.

Grains and monuments at Aksum

At the most famous place in what was Haile Sellassie's empire -- Aksum -- archeologists Katherine Bard (Boston U.), Rudolfo Fattovici and team are settling the mystery of the famous 'phallic' monuments and the domestication of /t'eff/, a very important grain in Ethiopia/Eritrea. The Aksumite stelae are associated with big rock cut tombs which will help define their meaning.

Around the time of Christ, Aksum had wheat and barley, Near Eastern cultigens. By 500 AD /t'eff/ (*Eragrostis teff*) had been added to the grain supply, along with grape seed and lentils (at least). The source of Teff? Ethiopia, somewhere.

Aksum also shows lots of cattle and shoats but no pigs or donkeys. Trade goods from Roman Egypt show up, as well as evidence of contact with Late Meroitic culture (Nubia).

Rare Evidence of Weaving at 27k BC

Textile fabric like potato sack, nets and baskets, impressed on clay which got fired somehow, showed up in Moravia around 27,000 BP. Olga Soffer (U/Illinois) and James Adovasio (Mercyhurst College, Erie, PA) collaborated on the discovery. While many have long assumed that

basketry and some sort of weaving was present during earlier periods, it has remained largely undetected. Many believed that it had to wait for the Neolithic. In fact the Czech site, originally excavated in the 1950s by Bohuslav Klima, shows some ceramics in figurines plus some ground stone, also thought to require a settled Neolithic life. The weaving technically is called 'open diagonal twining, with flexible vertical warp threads'; it isn't the same as 'plain' weaving.

Amerinds in Alberta before 20k ?

Mammoth Trumpet reports that a pre-glacial 'artifacts' site near the city of Calgary might be linked to mammoth sites ranging back to 40k in Alberta and showing human activity before the redoubtable Clovis horizon of 12k BP. However, the finds by Jeri Chlachula (U/Alberta) immediately ran into the usual archeological quarrel about whether they were artifacts or not. So strong is the opposition in American archeology to any pre-Clovis dates or cultures that our companion journal is becoming almost chary in its discussions. Why are their conservatives so much like the Americanist linguists? I always thought the Maginot Line was in France.

Ehret alters Nilo-Saharan taxonomy

Courtesy of Franz Rottland, we have a report of a new scheme for internal taxonomy of N-S. Chris Ehret presented it in Los Angeles in March. (Let -> mean 're-write')
 N-S -> Koman + Sudanic
 Sudanic -> Central + North
 North -> Kunama + Saharo-Sahelian
 Saharo-Sahelian -> Saharan + Sahel
 Sahelian -> For + Trans-Sahel
 Trans-Sahel -> Central Sahelian + Eastern Sudanic
 Central Sahelian -> Songhay + Maban
 Eastern Sudanic -> Astaborean + Kuliak + Kir/Abbaian
 Astaborean -> Nara + Nubian Tama
 Kir/Abbaian -> Jebel + Kir

Jebel -> Gaam + Bertha
 Kir -> Temein + Daju + Surma/Nilotic
 Surma/Nilotic -> Surma + Nilotic.

There are some surprises in this. Koman does deserve its special status, as do Central Sudanic and Kunama. Songhay is startling because so many people are trying to get it out of N-S into a group with Basque or Niger-Congo. Outside of Koman, the group I know best is Eastern Sudanic where I find Ehret's scheme entirely credible, especially the placing of the Kuliak group.

The weights of the branches in N-S suggest that the Sudan (Republic) and the Nile system are the homeland of this great phylum. This repeats a conclusion I reached in 1965; it was less well supported.

* * * * ARRIVEDERCI ! * * * *

Until we meet again in MT-26 this Fall. In that issue we will pick up the loose threads from this issue but most of all the large number of letters and announcements from members which were cruelly left out of this issue. There will be two obituaries (Egerod and Stopa), the one by W.W. Schuhmacher, tother by Eric de Grolier.

You all are reminded that the great debate on the classification of Basque will be presented in the near future in our Journal, issue 1. Featuring MT*Treatment of a main article by Larry Trask, with primary rebuttal by John Bengtson, we shall see serious comments by Ian Catford, Vaclav Blažek, Eric de Grolier, Étienne Tiffou, Jose Hualde, Xabier Zabaltza, William Jacobsen, Vitalij Shevoroshkin, Merritt Ruhlen, Roger Wescott and with luck Sergei Starostin.

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Treasurer's note: Many issues have been dispatched on faith. Those who have been reluctant to pay the \$25 dues are urged to become less anal. For the forgetful, a reminder.
 Viva Trombetti!