MOTHER TONGUE

NEWSLETTER OF THE ASSOCIATION FOR THE STUDY OF LANGUAGE IN PREHISTORY

Issue 28 (MT-28) Spring 1997

MOTHER TONGUE : NEWSLETTER of the Association for the Study of Language In Prehistory. Issue 28. Spring/Summer 1997

The Association for the Study of Language In Prehistory (ASLIP) is a nonprofit organization, incorporated under the laws of the Commonwealth of Massachusetts. Its purpose is to encourage and support the study of language in prehistory in all fields and by all means, including research on the early evolution of human language, supporting conferences, setting up a data bank, and publishing a newsletter and a journal to report these activities.

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INTRODUCTION TO MT-28: The Newsletter (Editor this issue: H. Fleming)

THE HOTTEST AND THE LATEST NEWS, AS OF END-MAY, 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 hottest news came from **the Americas** where a major scientific breakthrough has occurred, until May 30th when the hot focus switched to **Spain** and the **new ß-globin (DNA)** analysis by **Rosalind Harding et al** of **John Clegg's** Oxford team (biogenetics).

Had we finished when we should have, we would have missed this; thanks to **Dan McCall** and the brouhaha in the press which helped us to see the bond between the fossils and the proposed biogenetic dates.

NEWS OF MEMBERS' ACTIVITIES, INCLUDING LETTERS OF COMMENT

This rich lode of material -- too much for MT-27 -- was promised for February. So our promises are none too good! But some of it appears herein, especially **Carleton Hodge's** plea for a re-appraisal of a key Old Egyptian sign; it is often involved in Afro-Asiatic and Nostratic etymologies some of which are now endangered.

ANNOUNCEMENTS & ADVERTISEMENTS: THE MEMBERSHIP (PERMITTED) LIST.

Continuing the list of members who permit their names to be made public. It is good to distinguish between an ASLIPer and a long ranger. The former is a member of ASLIP -- simplement. The latter most probably is both an ASLIPer and one who favors long range work. There exist ASLIPers who are actually profoundly opposed to long range work!

Do remember: the list of permissions **still** does not equal the full membership. Many people don't bother to mail in the questionnaire

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. We regret that the obituaries have been deferred yet another while longer. Again we ask forgiveness for 'bumping' their obituaries in order to present the hottest news first. Said news is very 'time sensitive' as they say nowadays, while sad news (the obituaries) isn't

ASLIP BUSINESS

There is much. John Bengtson has a brief report on the Annual Meeting (Boston, April 19th). List of libraries is growing, including seven major university libraries plus the two biggest public ones. Still nothing in Europe. The 3rd issue of the Journal should be on the cutting edge again, with follow-ups on Nihali plus several serious biogenetic discussions, mayhap also new findings in paleo-linguistics. Our production editor, Bomhard, has resigned. Our Web Site was also in a state of flux until **Mary Ellen** Lepionka rescued it recently.

Et, bien sur, now is time for most ASLIPers to pay their 1997 dues. See the attached colored sheet. We also ask if there are any **volunteers** out there to help John Bengtson and the Bostonians run this operation of ours. Your **financial** help or advice on it, is solicited!

What has Homo antecessor got to do with the new ß-globin problem?

What indeed! Some old bones from Spain threaten to re-write the skeletal procession from **Homo erectus** to **Homo neanderthal (or Homo sapiens neanderthalelsis**) and to **Homo sapiens sapiens.** A new DNA analysis by an Oxford team throws the biogenetic side of the equation out of whack, causing us to suppose that humanity's common ancestor lived about 800 kya -- instead of 100-200 kya -- and that there was plenty of human gene flow around the Old World in those times, enough to raise serious doubts about the 'Out of Africa' replacement theory. We will give the particulars of the DNA problem in a moment. For now let us hark back to MT-25 in 1995 when we reported the following things about Spanish bones (+ some English) not mentioned recently.

Quoting now from MT-25, p.1.

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 Neandertal 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! [End of quoting MT-25]

In the intervening 21 months 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.**

Although the classification may be doubted, and may be rejected, because the fragments of an immature male's head had to be carefully pieced together, one may not readily dismiss two years of patient forensic work. Nevertheless an awful lot of prehistory seems to be resting on one lad's shattered head. So it is legitimate to have serious doubts!

Sources. Although an article on this has appeared in SCIENCE on May 30th, my copy did not reach me yet. So this is based on TV reports. However, a young paleoanthropologist from Smithsonian, Rick Potts, who also tends to accept the Spanish classification, is rushing off to Kenya where he knows of some 800 kya sites with lots of artifacts in association with which he may find evidence of **Homo antecessor** or the alternate **erectus** forebearers of Homo sapiens sapiens.

As is our custom in fossil matters, we asked **David Pilbeam** (Peabody Museum, Harvard) his opinion on all this. Although he had not received his copy of SCIENCE yet either, he knew a lot from the 1995 phase. David said to distinguish between the species classification of **Homo antecessor** and its evolutionary position. There was an excellent chance that a new species had been found but its position relative to H.n. and H.s.s. was much more difficult to figure, unless Potts found a contemporary in Kenya. Since Potts was directing his attention to the highlands (around the Rift Valley) -- due to a shortage of fossils in the lowlands --, his success seemed less likely to both of us. (But it might be found in Ethiopia where **Alison Brooks** and some keen Ethiopian archeologists have been looking)

Then the NY Times web site kicked in with the opinions of two more experts, **Phil Rightmire** (SUNY Binghamton) and **Fred Smith** (Northern Illinois U.), both of whom expressed scepticism about the new species. Said Rightmire: "I am reluctant to endorse this new species. I wonder if the facial characteristics of one juvenile are really diagnostic. It's tricky to compare children to adults and on that basis establish a new species." The reader will note that this scepticism is based on data, not on theories of the impossible. The Spanish proposers of the new **Homo** species, **Jose Bermudez de Castro** (National Museum, Madrid) and **Juan Luis Arsuaga** (Complutense U., Madrid) stood their ground, saying inter alia: "(the boy's facial traits) are exactly the morphology we would imagine in the common ancestor of modern humans and Neanderthals." **Eudald Carbonello** (U/Tarragona) was the original excavator & reporter.

Clearly odd in terms of our usual image of these hominids is the height of 6 ft (1.8m) given for some males at Atapuerca. See the 'Boxgrove' report above for similar height, very robust too.

David said Wainwright's team had only found that one bone, just too little to go on, but this led us to discuss the amazing find from Schoningen, Germany of long **wooden** spears of **400,000** BP. Excavator **Thieme** (first name has slipped away) believed the spears were **thrown** not jabbed or thrust. The center of gravity was a third of the way from the sharp or fire-hardened end. (I'd call that the hefting point.) **Robin Dennell** (U/Sheffield) said: "The craftsmanship of the three spears

shows considerable depth of planning, sophistication of design and patience in carving wood, all of which have been attributed only to modern humans." The context appeared to be group hunting of horses. That also was taken as a sign of modernness. Yet one must demur. Everyone knows that both African lions and wild dogs show strategy and tactics in hunting. So it ain't necessarily so human or modern.

The signs of robustness in Atapuerca, hinted at (Boxgrove), and compatible with the German spears, could be seen in the well-known musculature of Neanderthal. David has long argued that H.n.'s powerful muscles must have been useful in hunting by spear. If he played American football, Neanderthal would be a star quarterback, setting new records at throwing the football. David thought that the muscular line from **antecessor** to Neanderthal was adaptively linked to spear throwing, i.e., in 700 ky some natural selection involved in all that muscle. (One can easily imagine society or women 'preferring' able pike men who brought home more bacon or fended off cave bears. I know this sounds 'romantic' but the scenario is not so unlikely.)

Some light was thrown on this thesis by **Stephen Jay Gould** (Harvard) and **Christopher Ruff** (Johns Hopkins) and colleagues who argue that earlier humans, especially of the 600 ky-30ky period, were taller and brawnier than we have been since then. Neanderthal was 30% heavier than us, albeit not as tall as some, and with a larger brain than ours. But his/her brain-to-body ratio was 10% less than ours, giving us an edge in brain power. Given the size difference between BaMbuti and Nuer, Semai and Samoans, or Sicilians and MacDonalds, one can be a bit sceptical of the inferences. But our spearman theory probably merits even more doubt.

Reminding everyone of some obvious things. In old forested areas like Europe and China the presence of wooden tools ought to be **assumed.** Archeology has an unavoidable but inherent bias in its data against perishable things. The digger fraternity knows this perfectly well but finds it easier, not better, to frame things in lithic terms. No meat, just bones; no wood, just stones. Try to remember that!

Ofer Bar-Yosef once said a guy with just a crude hand axe may still have a sophisticated tool kit in bamboo or bone. Thieme seems to prove him right.

Sources: The remarkably preserved wooden spears or javelins were reported in NATURE, Feb.27, 1997. NY Times web site of May 30, 1997 was written by John Noble Wilford (thanks to Allan Bomhard). Gould's part was in NATURE, May 8, 1997, while Ruff et al reported in SCIENCE, May 9, 1997. The sizes in modern humans are well-known generally in anthropology.

Inevitably **Homo antecessor** provokes renewed interest in the so-called 'archaic sapiens' or 'pre-sapients' of Europe, viz., **Homo heidelbergensis, Steinheim, and Swanscombe.** H.h. was the earliest (circa 500 kya) a good candidate for pikeman of yore, while Sw. was circa 225 kya. All likely lived in 2nd Interglacial

Rightmire believes that H.h is "the last common ancestor of" both Neanderthal and modern men. Smith sees **antecessor** as likely to be an early Heidelberg man or

late **Homo erectus.**

Let us sketch briefly the competing schemes proposed til now in our phylogeny. Let H. = Homo, n = neanderthal, s = sap., 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
(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

Stay tuned to this station!

* * * * * * * * * * * * * * * *

Here is the abstract of the article which has disturbed the serenity of the 'Out of Africa' scholars. The authors are Rosalind Harding, et al. [Now begin quoting]

Title: **Archaic African and Asian Lineages in the Genetic Ancestry of Modern Humans.**

Summary. A 3-kb region encompassing the ß-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 ß-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, and an effective population size of ~10,000 has been maintained. Modest differences in levels of ß-globin diversity between Africa and Asia are better explained by greater African effective population size than by greater time depth. There may have been a reduction of Asian effective population size in recent evolutionary history. Characteristically Asian ancestry is estimated to be older than 200,000 years, suggesting that the ancestral hominid population at this time was widely dispersed across Africa and Asia. Patterns of ß-globin diversity suggest extensive worldwide late Pleistocene gene flow and are not easily reconciled with a unidirectional migration out of Africa 100,000 years ago and total replacement of archaic populations in Asia. [End quote]

Remembering that the DNA used was **autosomal (nuclear)**, not Y, not X, not mtDNA. It reflects mating and gene flow, just as the others do not. Since Harding et al's conclusion is 'heavy', we repeat it here. [Now quoting] "The new methods used here to estimate the ages of mutations in gene trees challenge some of the currently favored interpretations of human genetic diversity regarding the ancestral history of contemporary populations. These and other population genetic methods will be applied to nuclear sequence data as they become increasingly available, and it is likely that there is much more to learn about the evolutionary history of modern humans. Our conclusions from this study of allelic ß-globin sequences are that there has been substantial multidirectional global gene flow within the last 100,000

years and that modern humans have both African and Asian ancestry dating to >200,000 years ago. We infer an earlier evolution and dispersal out of Africa by the ancestors of modern humans than indicated by some interpretations of the fossil data (Stringer and Andrews 1988) and, therefore, inclusion in the ancestral gene pool of non-African population groups identified morphologically as archaic or pre-sapiens." [End quote]

Sources. Am.J.Hum.Gent. 60:772-789, 1997. Also summarized in SCIENCE vol.27, 25 April 1997, 536 which also reported the misgivings of **Sarah Tischkoff** about the adequacy of sampling of African haplotypes. Since Africa's genetic diversity tends to have four foci (Mbuti Pigmies + Central Africans [the bulk of west and central Africa], Khoisan speakers, and Ethiopids of the Horn & Sahara), then half of it was neglected. What Harding et al called Asian was preponderantly Oceanic -- a good choice for prehistory, in my opinion.

Another criticism has been made, i.e., Harding et al basically use **one locus** or one gene to infer the prehistory of everything. That has its problems. Cf the lad's broken head.

Rebecca Cann made editorial comment on Harding et al in the same issue of AJHG, pp.755-757. It is an excellent overview of biogenetic strategy which also rehearses the differing results from using different loci. A wry comment on the increasingly hard mathematics of biogenetics will delight non-geneticists! One should also mention that the Harding et al article is very complex and technical, thus very hard for the rest of us to evaluate. We are now seriously addicted to the experts whose writings we can barely savvy! Yes, yes, we know that linguistics is quite difficult to understand too! Our old problem.

Two other studies impinged on these discussions. One by **Lynn Jorde et al** (including **Henry Harpending**) tested a large number of nuclear loci -- 60 microsatellite loci -- in 14 populations from Africa, Asia, and Europe. To test the hypothesis of greater African diversity. As their abstract says: "On average, African populations have ≈20% greater microsatellite diversity than do Asian and European populations. A comparison of continental diversity differences in microsatellites and mtDNA sequences suggests earlier demographic expansion of the ancestors of Africans."

The second study by **Michael Hammer et al** (he of U/Arizona) claims to show that earlier emigrations of Africans were followed by a big backflow from Asia. He believes that African migration happened twice but that a "major component of African diversity is derived from Asia." Hammer studied Y-chromosomes, especially the "YAP" region, a 2600-base-pair segment. Inheritance is strictly patrilineal. The general conclusion is that Hammer's team found one (haplotype) showing up "far more often in Asians than Africans. Its sequence shows more diversity in Asians, implying that the haplotype had more time to acquire mutations in Asia than in Afri[illegible] -- and, therefore that it aro[illegible] in Asia" Exciting but debatab[illegible] later on! Jorde et al, PROC.N[illegible] ACAD.SCI, vol.94: 3100-3. Not[illegible] read: Hammer's paper in GENE[illegible]

ARCHEOLOGY IN THE AMERICAS: MONTE VERDE (CHILE) SCENE OF SURRENDER.

"The mills of the gods grind slowly but they grind exceeding fine."

The press (USA) has been full of reports on the 'new discovery' that Amerinds (native Americans) had settled in the Americas before the famous Clovis Line date of 11,200 BP. The scholars who had been blocking this discovery (owing to inappropriately high standards) gave up en masse by accepting the Monte Verde dates of 12,500 BP for higher strata at least, maybe 33,000 for the lowest. Also accepted at **Tom Dillehay**'s site was a human foot print and lashings on hut poles.

Less noticed subsequently was an effort to undermine the Amazonian discoveries of Anne Roosevelt and her team which had established a contemporary culture far removed from the main Clovis-Folsom areas and one with little affinity to the main cultural focus of Clovis. Using very elegant criteria, the critics attempted to show that (a) Roosevelt's site was younger than she thought and (b) the artifacts could be derived from local Amazonian cultures of significantly later age. Their critique was pitched to such a rarified and technical level that it was truly difficult to read -- all the more reason to believe 'it must be really scientific!'. If one can endure wading through scientific esoterica, then Roosevelt's marvelous rebuttal will be most rewarding to read. Basically, she and her team threw the critic's argument back at them "Tu quoque!" by observing that their own sites had never been held to such high standards -- so why was the Amazonian material criticized in such inappropriate terms? -- plus in technical details her critics were wrong anyway. It was a tour de force!

Opinion. This could spell the end of **Technocracy** in American archeology. Or so one can hope. More precisely one can hope for **normal science** wherein we do have high standards but only in so far as they affect data gathering and analysis. We ought not smother venturesome hypotheses, so frequently fruitful and valuable, in a dense thicket of technicalities. While the inventors of Technocrat archeology believe they adhere to a sound philosophy of science (Hempelian), and emulate physics faithfully, their performance suggests otherwise. They would have stifled Planck and Einstein in their teens. Can you imagine Charles Darwin presenting his theses to an audience of Technocrat archeologists? They would have murdered him.

If we may borrow the concept of **spin control** from political theory, with the associated notion of **spin doctors**, we may understand a most recent statement by a distinguished archeologist (**D.Meltzer**) which accepted the verdict from Monte Verde but sought immediately to govern its interpretation -- control its spin -- by stating that many other sites had also broken the Clovis time line but that they would each have to be examined painstakingly before one believed in their authenticity. Either their dates were 'off' or their artifacts were really only geofacts -- the essence of his message. In other words, one should not spin out of control and believe such things as Guidon's hypothesis in Brazil or MacNeish's in Texas with their 20K, 30K or even 40K dates. The good spin doctor would have us believe that Mon

Verde is only a **salient** along a battle front, not a real breakthrough like Sédan or a true reversal like El Alamein, in military terms. (The particulars of this page are given below in our regular format.)

Just as Scotty MacNeish had been nearly forsaken by American archeologists, especially after being shredded in a review of his Mexican Neolithic work, some of his theses received a little confirmation. Smithsonian's **Bruce Smith** found firm evidence of domesticated crops -- squash; **Cucurbita pepo** -- in Oaxaca about 9000 BP or twice as old as recent revisions of the Mexican Neolithic had it. One might say that MacNeish was right but for the wrong reasons. One might prefer to say that his sites were bad but someone had recently done the job properly. Or one might say that fault could be found with his excavations but that his basic hypothesis was right after all, or despite it all. But with whimsy one might add that Scotty's critics were wrong but for all the right reasons. Their technique and analysis is superb but their hypothesis is, malheureusement, false. Hélas!

At the famous / infamous site of Pendejo Cave (also called Orogrande) in Texas some new things to report. Careful lab tests have established that **human hair** in some strata are circa 10 kya, not recently shed from the heads of diggers. While not breaking the Clovis Line, this supports the authenticity of the whole enterprise or at least the relevant stratum. And mtDNA tests suggest that the humans at Pendejo are linked to some in Siberia circa 35 kya.

More importantly, a recent article accepted by AMERICAN ANTIQUITY, 61(2), 1996:357-376, makes strong points which need to be mentioned in today's climate of opinion. **Donald Chrisman** et al's summary is:

"In the excavation of Pendejo Cave ... 16 friction skin imprints were found in five stratified zones on clay nodules, baked at over 120°C. After careful analysis, expert dermatoglyphologists determined that these imprints had positive primate characteristics. The imprints are probably of human origin, since no other primates are known to have existed in prehistoric New Mexico. Eight of the imprints occurred in three well-dated zones falling in the late Pleistocene. These zones have direct radiocarbon determinations between 12,000 and 37,000 BP. In addition to their association with radiocarbon determinations, the prints come from three of 24 stratified zones, intensively studied by geologists and pedologists, that are dated in sequence by 34 other radiocarbon determinations acquired from four different laboratories. The imprints are associated with a column of over 35,000 paleontological specimens and more than 15,000 botanical remains. These specimens indicate Pleistocene changes and supply evidence of human transportation and modification of various materials. The prints are also associated with artifacts, ecofacts, features of human construction, and human remains. The imprint specimens therefore provide evidence of Pleistocene human occupation in the New World." [End of quote].

What do you have to do to get a site taken seriously, any way? The answer seems to be --

you have to be trusted and considered reliable by your colleagues! That is what the grapevine is saying. So the testing of a major hypothesis is done by judging the excavator's character? Mama mia! e stupido!

Sources. Early reports on the consensus re Monte Verde were actually on American TV. Then in the NY Times and the Boston Globe (one owns the other). All in mid-February. The critique of team Roosevelt was in Science, vol.275:1948-52, 28 March 1997 as was the team's rebuttal (last 2 pages).
The shredding of MacNeish occurred in Current Anthropology, vol.37:700-16, 1996, by **Karen Hardy** (Newcastle U.), while the discovery of domesticated crops in Mexico was reported in Science vol.276:932-34, 9 May 1997. Recent finds at Pendejo Cave are in manuscripts furnished by **Donald Chrisman** (Yale U.); a field report on Scotty's dig in China, hence new Neolithic data, are in PUBLICATIONS IN ANTHROPOLOGY No.13, EL PASO CENTENNIAL MUSEUM, U/Texas at El Paso, 1995. One might get a copy by contacting **Lorraine Farmer** at **AFAR**, Box 83, Andover, MA 01810, USA.
Prior discussions in our Newsletter of the dates for the peopling of the Americas go back at least to 1987. The most recent was in MT-27, pp.2-7. We have been accused of excessive attention to American archeology but that is the only reliable source of American **chronology** without which nothing can proceed.

Biogenetic dates are fairly wild, albeit interesting. Linguistic dating cannot be tried, not so much due to irate controversy as it is due to the probable ancestors being beyond the limits of glottochronology -- too old. That is only my opinion; Ruhlen doesn't agree, while Greenberg has been loath to depend on that dating method -- preferring the simpler logic of holding archeology's date of the first Amerinds as definitive.

Antique Sites in the Americas

We have enclosed a **nice colored map** of the New World which shows all or most of the controversial sites and their proposed dates. This is reproduced by permission of the Boston Globe whose reporter, David Chandler, had an artist draw the whole scene. Who knows how many of these dates will survive the mills of the gods?

To the map's group add the sites listed in MT-27, p.4; the first two are in Virginia:

Saltville (USA) 15,000
Cactus Hill (USA) 14,000
Nebraska mammoth site, northeast of Haystack, Colorado, 18,000
+ Monte Alegre (Amazon, Brazil) (Roosevelt team) 11,075 ± 110

Eventually younger bolder archeologists will dig vigorously in the promising terrain of earlier prehistory and we will be able to sort out how these numerous old sites relate to each other and to the cultural sequences which will become apparent later. In time we will know whether the Ur-Amerinds came by sea (straight across) or coastwise skipping along the Aleutians or across Beringia to pick their way south through the glaciers. Twill be fun to figure these puzzles out! Then ultimately to find the archaic Na-Dene and trace their routes from Asia then go beyond to their purported junction with Yeniseian! Yes!

↓ INTERESTING BUT COOLER NEWS ↓

Oldest Human Tools: Ethiopia

Perhaps better to say 'Ethiopia again'. Nearly 3000 stone tools, including round, fist-size artifacts and small sharp-edged flakes, possibly used to crack nuts and sharpen sticks for digging up tubers, were found in the Gona (Awash) region. Dating to 2,500,000 years ago, they are the oldest known in the world. No association with any fossil hominid was found but a strong presumption that **Homo** did it has been made, in this case **habilis**. The report was by **Sileshi Semaw et al** (he of Rutgers U.) in NATURE, 23 January, 1997. They said that the tools showed surprising control of the flaking process and were much like tools made a million years later. Precocious!

Music: the neglected aspect

Most of us do not know what to do about music, an old, very dear, and universal part of our human repertoire. Music is normally included in the cultural side of things, rather than linguistic or biological, yet it is usually as ephemeral as talk. Neuro-anatomists may study the locus and functions of music in the human brain but what Alan Lomax would call 'song style' or what we might call music tradition has no serious prehistoric research associated with it. I could argue that, by ordinary ethnological criteria, music has just as good a probability as language to be associated with our founding mothers. Just like kinship terms. But how can we tease some evidence out of archeology on these things?

Well, some people do this, a bit indirectly, it is true, but still bearing on the topic. Witness a recent discovery that Neanderthal probably played a flute! That is between 43 ky and 67 kya or on average 55 kya. The association is with a Mousterian culture of Slovenia. The flute a piece of bear thigh bone with 4 holes in/on it, with more holes on lost fragments of the bone. **Bob Fink**, musicologist in Saskatoon, Saskatchewan, inferred the presence of a **minor diatonic** scale, much like the seven tone scale supposedly used in the Near East 4 kya. That last bit according to **Anne Kilmer** (Berkeley). Fink added that **one-note** whistle type artifacts dated to maybe 20 ky to 30 kya. Where and by whom was not mentioned. A minor diatonic scale would have half tones and whole tones or roughly the same seven note scale on which Western music is (supposedly) based. (I don't think about music this way, so I am unsure of my rendering of the musicological opinions.)

Fink added something more interesting to me: "that the human brain's perception of musical tones and what constitutes harmony is at least partly hard-wired -- a view bolstered by recent research showing that young babies can distinguish discordant from harmonious combinations of notes." Everyone probably knows that **hard-wired** means that this musical ability is part of the hardware, part of the genetic endowment of babies, rather than enviromentally given.

Nevertheless, it is useful to issue challenges to readers from time to time. Everyone knows that birds sing, dogs howl and monkeys chatter. Chimps do chatter and grunt, but I bet no

one can produce an example of any higher primate, any ape, who sings in any fashion similar to the crudest human singing (e.g., the music of the Amami Islands). Unless my proposition is refuted, I will state that music is another important and **exclusive** human trait. Anyone care to bet? (Starting at 1000 Italian lire.)

Source. SCIENCE Vol.276: 205, 11 April 1997, "Random Samples".

And an after thought. Birds are hard-wired to sing, squawk, scream, etc., but may also be able to learn new material. The bright and gifted parrots can do almost anything; they seem to be unique themselves. Like humans they can speak any language and sing any song in any style, if the normal apparatus is present. Again human beings do not come with hard-wiring for any particular language or any song or song style. A girl of Samoan parents can be raised in Tbilisi naturally to speak a language and sing music very unlike the same things in gentle Samoa. (Just rehearsing some basic points and adding music to it.)

How Many Genes do You have?

With so much research and so many conclusions resting on the genetic composition of **Homo sap. sap.** -- and with so many corporations competing in the biotech sphere of business -- it would be useful to know just how many genes there are in average bodies. Call the total -- **genome** as they do nowadays. We find that there is less than perfect agreement among experts. At first it is better to clear up one question. Is all of our DNA the same as our genome? Is all of our DNA composed of genes only? One answer is, no! Why? "The essential problem is that our genes are hidden in a haystack of apparently meaningless genetic information. Only about 3% of the 3 billion individual units known as 'bases' that make up DNA actually code for proteins, which is the simplest definition of a gene." That useless stuff must be like the 'dark matter', the main component of the universe. So say astronomers. Ex nihilis 'dark matter'?

Everyone can calculate that, ergo, we have 90 million bases. Is that the same as genes? No, a number of bases make up a particular gene. How many? There's the rub! We do not know for sure. The genome of the Japanese /fugu/ or puffer fish has 360 million bases and about 6000 bases for each gene, for a total of 60,000 genes. Some think the human genome = 60,000 too. Anyway the estimates vary from as low as **42,000** to **150,000** at the highest. People are counting furiously at different labs and the answer is expected by the year 2003 when the Human Genome Project is completed. By that time it is expected that we will know something about what each gene does, as well as the total.

Does anyone else remember back when -- Soviet scientists and American creationists both challenged the **concept** of gene because no one had ever seen a gene under a microscope? 1950s? It flustered some Western scientists because they could only argue that the concept was valuable in itself -- sight unseen. Then one was finally seen under a more powerful microscope. Yet we have believed in atoms for ages and teeny electrons for

much longer than genes. Not to mention gods and goddesses.

Source. SCIENCE had a marvelous special on the human genome on 7 February 1997 from which the above discussion is taken.

On Dying Vikings & Doing Homework

It is no secret that some scholars do little bibliographic work at all, while some cite a few key sources and then quit. Most of us are conscientious but not perfect; we sometimes miss some one else's work. And they often resent it or at least set the record straight lest their own work be forgotten or neglected.

So it is with the exciting news that the mystery of the Viking settlements on Greenland in the Middle Ages has been solved. As the blurb in SCIENCE said "By combining data from ice cores, archaeological digs, and fossil flies, researchers have shown how increasing cold and an inflexible culture could have doomed this medieval Norse outpost." It was a nice article. The research had involved support and/or collaboration between big entities like CUNY (New York), National Science Foundation, Sheffield University and comments by noteworthys. The primary work was done by **Paul Buckland** (Sheffield) and **Tom McGovern** (CUNY), with the latter having spent nearly 20 years surveying data, digging, etc.

The Norse settlements had begun in the 10th century AD and died out in the 14th, becoming invisible ('vanished') by the 16th. The first three hundred or so years were spent with a mild climate. Then climate changed to the 'Little Ice Age'. While the neighboring Thule Eskimaux adjusted to the cold, the Norse were culturally rigid and ultimately the settlement died out. Conjecture is that the settlers may have gone back home to Iceland or Denmark, rather than dying out on the spot. All reported by science writer **Heather Pringle** in SCIENCE vol. 275:924-26, 14 February 1997.

On May 16th 1997 SCIENCE printed a letter from **W. Dansgaard** (Niels Bohr Institute, Dep't. of Geophysics, U/Copenhagen) who stated that he and his colleagues had reached a similar conclusion 22 years before and published it in NATURE 255, 24, 1975. Since the piece in SCIENCE did not have a bibliography in it, what may have triggered Dangaard's response was a comment inside the piece by a paleoclimatologist that "the ice-core data are at last being applied to human history." It is unlikely that nobody knew of Dangaard's work but those who knew did not write the report. Maybe.

For the rest of us it is good that McGovern et al confirm Dangaard et al in a neat analysis of one 'event' in prehistory. This lends credibility to Dangaard's other ice-core hypotheses, including the precise date (1643 ± 7 years BC) of the eruption of Thera which doomed Minoan civilization.

China's Zhoukoudian: New Dates

A key place in the debate over modern human origins has been, and remains, **China.** Some have doubted the dates and interpretation of fossils, particularly those of Choukoutien, modern Zhoukoudian, the cave and the inhabitant, often called **Peking Man.** New research with

ESR dating has broadly confirmed the pattern of older dates and in fact given a fairly firm set of dates which my best informant (who commutes to China almost monthly) says are credible. The conclusion of the recent study by **Rainer Grün et al** is: "The ESR age estimates presented in this study confirm the results of the previous multidating study (**Wu et al**, 1985). They indicate that **H. erectus** occupied the Peking Man Cave at Zhoukoudian in the range of about 300,000-550,000 years." Grün (Australian National U.) had **Pei-Hua Huang** (U/Science and Technology of China, Hefei), **Xinzhi Wu** (Academia Sinica, Beijing), and **Chris Stringer** (Natural History Museum, London) on his team. Two Australians, **Alan Thorne** (Australian National U.) and **Malcolm McCulloch** (same) completed the team. The team is all listed here because of its international character and because some of them had doubted Chinese dates in the past.

[Note: ESR = Electron Spin Resonance. Having no real understanding of it, we report ESR as a 'social fact'.]

We must stress, however, that this paper does not bear on the **Homo s.s.** remains in the famous 'Upper Cave' or their dates or their interpretation. My informant, **Ofer Bar-Yosef**, has participated in the evaluations of the Upper Cave and its dates and its human remains. He is serious about the whole matter being as yet **unresolved**. We will honor that view here and not guess. Source. JOURNAL OF HUMAN EVOLUTION 32, 83-91, 1997.
Also in a late SCIENCE, Grün and Thorne attack Swisher et al for their ESR dating of **Ngandong Man** of Java, a H.s.s. circa 27 kya.

Puzzling mtDNA Diversity: Africa

Sometimes research accounts are too puzzling to report on properly. The reasons obviously are our failure to savvy the jargon but also our failure to understand the conclusions.

Last year **Elizabeth Watson** (U/Munich) **et al** (including **Svante Pääbo** also of Munich) published "mtDNA Sequence Diversity in Africa". The study was initially aimed at reporting on some previously unknown African data in the hypervariable region I (HVR I) of the mitochondrial control region which would bear on questions of African diversity as evidence of genetic prehistory. While the study did report a great deal of diversity among the 9 peoples bled, this was not related to the global question -- unless it was by some undetected implication. Instead Watson et al seemed more interested in the results from an African perspective and in terms of subsistence economy.

In terms of **genetic distance** this set of mtDNA samples set the populations apart like this:

(1) !Kung
(2) (a) Biaka (Pigmy)
(b)(1) Mbuti (Pigmy)
(2)(a) Turkana
(b) Kikuyu vs
the rest

The 'rest' consists of Somali, Tuareg, Kanuri, Hausa, Songhai, Fulbe (Fulani), Mandenka (Senegal) and Yoruba. The outstanding position of Turkana has been the source of remarks about them having as much diversity as the rest of humanity.

Straight away, this pattern is not good news for biogenetic vs linguistic correlations, except for the Khoisan-speaking !Kung. Presumably the Watson

team found language correlations so unrewarding that they deserted them. Anyway they are never mentioned. That was probably a wise move! What then was this pattern all about? **Subsistence economies!** They call this food producing vs food-gathering. If the people is relatively isolated and small and foraging, then their internal diversity is less but their distance from others is greater. The pastoral or agricultural peoples on the other hand show evidence of expansion in size, apparently with more gene flow from others, and show the greatest internal diversity.

[Note: These conclusions are slightly troublesome. Previous discussions herein had stressed the record-setting internal diversity of the San (of which the !Kung is often chosen as representative). Mark Seielstad wondered about this too.]

Two examples of the food-producers are the Turkana and the Somali. Both have terrorized their neighborhoods and moved other peoples out of the way during their expansions. Both are heavily pastoral & warlike; both have incorporated other peoples such that some of the present Turkana and Somali populations used to be somebody else (e.g, Galab, Oromo, et al).

Another example, the Kikuyu, represent an older expansion of Bantu across Kenya from the west to the center where their absorption of former Nilotic and Cushitic peoples is easily demonstrable.

Thus, the testimony of the Watson paper seems to be that much **social prehistory** can be obtained from mtDNA research, even if linguistic correlations cannot always be found. But maybe we did not understand the paper! (But see below)

Source. AMERICAN JOURNAL OF HUMAN GENETICS (AJHG) 59:437-44, 1996. (Thanks to Maryellen Ruvolo for recommending it.)

Thinking about Prehistory

Perplexed by the Watson paper, I discussed this business with **Mark Seielstad**, soon to be Harvard PhD in anthropology cum molecular biology. Student at both famous labs (C-S at Stanford and Lewontin's at Harvard), Mark is very smart but also very thoughtful. For a biological, he knows an extraordinary amount of African ethnology and history. His recent field work in south Ethiopia was quite unfairly maligned in MT-27 when I said he had walked right by a group of Shabo. It were false, mes amis. My friend P.U. had unfairly planted that story in my head. The truth was that Mark was not allowed by local authorities to enter a plantation where some Shabo were living. Full apology!

Later on in MT-III we will, or we hope to, feature Mark's work, including the first Y-chromosome data and indeed the first biogenetic data ever on 9 peoples of southwest Ethiopia; from Surma, Nilote, Cushite and Omotic populations. Yes, Ongota too! But, alas, no Shabo yet. (That is more than compensated for by the new data on the Nuba of Kordofan which Mark and Luca are helping others to acquire.)

Anyway our discussions led me to revamp my thinking. Mark is not responsible for what follows but it does reflect his cognitive influence.

It behooves us who seek biogenetic correlates with language to realize and/or predict that DNA research picks up much more than language links. It

picks up **social and economic history, including castes, kinship and marriage patterns** and **disease** too. Some think that above all it picks up **geography**, often old co-residence in the same area. But that will not amaze or confound linguists who have always found language taxa correlated with territory (not perfectly but largely). The reasons for geographical factors like area, bodies of water, islands, mountains, river valleys, etc. to correlate with linguistic taxa are obvious or at least well-known. What does not follow is the often dismissive remark that such taxa are 'merely geographical'. There are many geographical areas on earth where language distributions are complex and we must know more than mere location to make sense of a region. Some examples are British Columbia, the Algic and Iriquoian areas of North America, Mexico, north Tanzania, Kordofan, north Pakistan, the Caucasus, and ancient Anatolia. Oh, yes, the north coast of New Guinea + insular Melanesia.

The role of borrowings and gene flow in our prehistory, and the labor of reconstructing it, are-- well, quite important. We have been misled by the critics of our genetic hypotheses, and their intolerant onesidedness, to play down the importance of **word flow** and **loan genes.** As a consequence such exchanges and our knowledge of them are used as clubs to beat down genetic hypotheses, even when the argument for the borrowing is none too strong itself. Even true borrowings are not enough. Oft cited in arguments are mysterious <u>sub-strata</u> which negate linguistic similarities, even when the sub-stratum in question is barely attested. Southeast Asia has probably become the epi-center for borrowing theories wherein most lexical resemblances generated by the Austric hypothesis are reduced to ashes. Americanists increasingly use borrowing proposals to explain away Amerind lexical similarites, or 'influence' to negate grammatical cognations. Rarely, it seems, are they challenged on their proposed borrowings. Simply to say "it's borrowed" is enough to do the job.

Many years ago a prominent Semiticist used to do this to virtually every proposed cognation between Semitic and Cushitic to such an extent that the reconstruction of Semitic, Cushitic, and Afrasian were hindered. One day at a conference after he had swept through a set of Cushitic etymologies, destroying half with his Semitic borrowing beliefs, a senior Cushiticist politely said: "I doubt very much that what you say is true." A second scholar, no doubt emboldened by the brave action of the first, declared: "Yes, I doubt it too." Whereupon the Semiticist relented and remained cautious in this realm for ever after.

But what decided the fate of the alleged Semitic loan words in this case? Strict phonological rules? Exact phonetic correspondences? No, it was decided by **chutzpah** (strong self-confidence)! Amazing, what!

However, when we come to evaluate loans more prudently, we shall find that they contain a mass of vital information about prehistory in addition to the equally vital information given by genetic taxonomy. Our reconstruction of overall human prehistory will become a richer

thing, not just tracing the family trees of language groups or seeing who evolved together in a region. All kinds of evidence for contact and influence, movement and adaptation, spheres of cultural influence, und so weiter, will be available.

So let us demand better arguments and 'proof' from the 'it's all borrowed' school of historical linguistics or biogenetics. Then let us incorporate the real exchanges into our book of prehistory.

Actually we come back again to ancient debates in ethnology & anthropology. We can simply label the 'it's all borrowed' school as DIFFUSIONISM and identify a major source in the teachings of **Franz Boas**. 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.). We probably should link Heritage to **Edward Sapir** who struggled with Boas over Amerind taxonomy.

To illustrate these doctrines in action, let us take the Japanese word /fugu/ for puffer fish (see above). An inventionist like Trask would say that /fugu/ is naturally onomatopoeic -- the word imitates puffing. A diffusionist like Campbell would say that /fugu/ was borrowed from or had diffused from South Omotic where it is common for 'blow, puff', or 'bladder'. An inheritist like Bomhard or Greenberg would say that /fugu/ was an ancient word which Japanese and Omotic both inherited from Nostratic.

So who is right? Well, we get our rich prehistory perhaps best of all if some things are invented, some borrowed, & some inherited. Like English and our bodies (mutations = inventions).

Apropos of these points: Lemba

As if tailor-made to show the prehistoric treasures in DNA research, we have the case of the **Lemba**, a dispersed caste of the Bantu **Venda** of far northern South Africa + adjacent Zimbabwe. Metal workers in gold, silver, copper and iron, Lemba have always claimed special status as ancient colonist miners whose ancestors came ages ago from far away (Arabia?), becoming trapped in Africa because a mighty kingdom had conquered their home city, a major center of civilization, trade, and metal working. Lemba spoke a language different from Venda, circumcized their boys at puberty, and had other customs which suggested Jews but also Muslim Arabs. Moreover the Lemba tried mightily to keep their patri-lines pure, i.e., free of Bantu or other local males. But they perforce married local = Bantu women. Well, the women might have been Bushmen or even Twa, since their distributions are not so far away.

These traditions, and the fascinating Zimbabwe archeological civilization near by, caused some interest in the Lemba, especially since metal working or trade in metals with the Indian Ocean seemed to be part of the Zimbabwe scene. Since the old tradition of the 'lost tribes of Israel' was always present in scholarly heads, one working

theory could always be linked to the 'Jewish' aspect of the Lemba. After all, there was the precedent of the **Falasha** Jews in Ethiopia, even some connection with a lost Sabean civilization. For the physical anthropologists another factor was the reports of 'Jewish' or 'Semitic' faces on the men. More precisely, some men had hooked (beaked) noses and long or narrow faces. One might suppose that such faces were an 'invention' of local Bantus, caused by something in their diet or just **chance**. Silly genes just mutated for no good reason! After all such faces were not uncommon in India (the Krishna Menon look) and very much the rage in New Guinea.

The results of many probes were these: (a) the secret language was a variety of Bantu from farther north, (b) but it contained some names quite similar to Muslim ones, (c) circumcision of boys at puberty also is practiced by those Bantus farther north, (d) regular blood group studies showed Lemba to be like the Bantus of their region, (e) mtDNA showed the same thing! but (f) **Y-chromosome** studies now show the Lemba to be quite distinct from all southern Bantus! + Bushmen and Twa; but 'Caucasoid' yet not like Falasha or Europeans so much as like (you guessed it) -- Jews! either Sephardim or Ashkenazim. [As is well known by now, the Falasha are **genetically** Agau **Cushites** like Tigre/Amhara]

Furthermore, circumcision at puberty is not, nor has it been, the Jewish custom. Eight days after birth, unless the baby is weak, is the rule for Jews. Moslems may circumcize at puberty -- a few do, most don't. There is no Quranic rule and most local rules fall in childhood. The MSA Soqotri do it at puberty, explicitly as preparation for marriage, and probably did before Islam. Most of the people of East Africa + the Horn + much of the Bantu realm do it at puberty or <u>later</u>, primarily as a definition of adulthood or introduction to age sets. These African cases are most likely to pre-date Semitic influence by many millennia.

But what this case shows is that one DNA factor which links with a patrilineal tradition may show us that a Jewish or very similar Semitic group was a key element in the formation of a Bantu caste, giving clues to the traders & miners involved in old Zimbabwe. This was <u>diffusion</u>, in origin, while the many Bantuisms plus the tradition itself plus the preservation of Y-chromosome features were <u>heritage</u>. We ought to add that many of these things came together many years ago to <u>invent</u> great Zimbabwe. QED, no?

<u>Source</u>. Many older ones. The key recent one is by **Amanda Spurdle** and **Trefor Jenkins** (U/Witwatersrand), AJHG 59:1126-33, 1996. It was part of Spurdle's dissertation. Trefor is a world class biogeneticist, maybe most famous for his Gamma Globulin studies on Bushmen and southern Bantu. Also **Akbar Jafri**, Said of Lahore (personal communication), **Cyrus Gordon** (also p.c.), **Danielle Bar-Yosef** (also p.c.). It is fascinating in terms of the sociology of science that Jewish scholars cited <u>Biblical</u> sources for Muslim practice, while Muslim sources cited <u>custom</u> for Muslim practice. In the time of Ishmael Arabs may have circumcized boys at puberty; however, custom not sanctioned in Qur'an. Also see **G.P.Murdock's** AFRICA

book (1959) and **H.C.Fleming**'s 1965 doctoral dissertation.

Y-Chromosomes & Migrations

An arresting piece of evidence for what some call the 'incredible power' of Y-DNA analyses at retrieving human prehistory comes from the Maya! In a case which cannot be viewed as either an innovation (mutation) or diffusion (gene flow) -- these for technnical reasons --, an 'African' haplotype in a Mayan sample was interpreted as a residue from much earlier times. To quote the relevant passage: "That a single Mayan individual carries the G allele on a chromosome with the **Alu** insertion suggests an African origin for this Y chromosome. However, comparisons of this Mayan with Africans and other Maya at 30 autosomal microsatellite loci ... fail to show significant African admixture. This discrepancy can be explained by considering the rapid decrease of an ancestor's autosomal contribution to succeeding generations. While a particular Y chromosome would remain unchanged over many generations, an ancestor's contribution to the autosomes declines exponentially with each generation. This observation suggests that Y-omosome haplotypes may be more useful in identifying ancient population admixture than autosomal loci such as microsatellites. Based on our data alone, it is not possible to determine whether the G transition occurred independently on a YAP containing chromosome that reached the Americas from Asia. We consider such an event unlikely, but the discovery and application of additional well defined polymorphisms may clarify the issue. Interestingly, a high frequency of the YAP insertion in the Japanese has been observed in this and another study (5). A recent massive infusion of African Y chromosomes in Japan or Japanese Y chromosomes in Africa can be excluded. Presumably the **Alu** insertion existed (or still does) in some mainland Asian populations which colonized Japan." [End quote]. Reference (5) is to a 1994 paper by Michael Hammer in MOL.BIOL. EVOL.,11, 749-761. (See above for more about Hammer's work.) Source. **Mark T. Seielstad et al**, HUMAN MOLECULAR GENETICS, 1994, vol.3, no.12, 2159-61. The last author listed was Luca C-S.

Y-Chromosomes and Geography

Following on Mark's 1994 paper, a 1996 paper by **A.Ruiz Linares** (U/Antioquia, Colombia) **et al** (which included Mark and Luca) produced a dendrogram, strongly reflecting geography. As their summary says: [Quote] "Five polymorphic markers on the Y-chromosome (mostly microsatellites) were typed in 121 individuals from 13 populations around the world. With these markers 78 different haplotypes were detected. Haplotypes present more than once tend to be shared by individuals from the same population or continent. A reconstruction of haplotype phylogeny also indicates significant geographic structure in the data. Based on the similarity of the haplotypes, population relationships were examined and found to be largely concordant with those obtained with other markers. Even though the sample size and the number of markers are small, there is very significant clustering of the haplotypes by cont-

inent of origin." [End quote]

The summary forgot to mention another finding: "As with other markers, the greatest genetic distance is seen between African and non-African populations; a finding compatible with an African origin for humans. Based on Y-chromosome data, there seems to be a particularly close relationship between Asian (mostly Chinese) and Oceanian populations." And later "Linkage of Y-specific markers will make phylogenetic trees very sensitive to migration apart from the fact that haplotype and population trees need not agree, as has been seen with mtDNA haplotypes ... The fact that geographic clustering has been detected with only five markers suggests that microsatellite-based Y-haplotype trees might have more structure than those seen with mitochondrial DNA."

[To update some terms for our colleagues -- the kinds of genes used in the grand opus of Luca C-S et al which we reviewed in MT-24 are now being called 'classical' or 'autosomal' in the literature. The terms are not, however, exactly the same. Autosomal refers to those chromosomes which are <u>not</u> sex-linked; most of them = non-Y, non-X, non-mtDNA. Another term 'nuclear DNA' refers to pretty much the same thing.]

In their dendrogram of 10 populations they merged data from the Lisongo, a non-Pygmy people from Central African Republic (CAR) with that of the local Pygmies (probably Biaka). They also combined Australian, Papuan, and Melanesian data to make up an Oceanian cluster. The question they did not answer, as few of their colleagues ever do either, was <u>which Chinese</u> they sampled. North or South? We already know that the south Chinese join Southeast Asians, while north Chinese join Mongols and Eskimaux. If the great power of the Y-chromosome analysis does show north Chinese drawn to 'Australoids', that is news! Here is their scheme:

Base --> Pigmies vs the Rest
Pigmies --> Mbuti vs CAR
Rest --> Brazil-Ind vs Other
Brazil-Ind--> Surui vs Kartiana
Other --> Pacific Rim vs E-M
Pacific Rim --> Japan vs CCO
CCO --> Cambodia vs Sino-Oceani
Sino-Oceani --> China + Oceania
E-M ---> Europeans vs Mayas

As they say, a picture is worth a thousand words. A diagram would be much better!
<u>Source</u>. ANN. HUM. GENET (1996), 60,401-408

<u>The Oldest Family Tree</u>

It surprised a man in England to find that his remote ancestor on his mother's side had lived maybe a mile from the Englishman's home. His mtDNA was judged to be descended from that of Cheddar Man in southwestern England. The fossil ancestor had lived 9000 years earlier.

It was a cute little story that some geneticists say was ridiculous because there were probably thousands of people in northwest Europe who qualified. <u>Source</u>. The Boston Globe, March 9, 1997.

<u>UBAR: Old Semites in South Arabia? Or an old Semitic date?</u>

Ubar in south Arabia, on the edge of the Rub al Khali, the huge desert, was rediscovered by astute archeological work,

aided by 'remote sensing' and NASA and keen historical work and some good luck on the ground. It was a city which traded with the civilized north but fell victim to tremendous sand storms and was thus buried. Moslem tradition has it that Ubar was called **Irem** and was punished by Allah for its sins. The people are said to have been called **Ad**. The site by one account existed 2500 years ago but by another it was 5000 years ago. The problem is that good information is hard to track down because everything has either been on TV or was on the Web -- lightly.

We need help with this because Ubar potentially is to be identified as a locus of the Modern South Arabian (MSA) prime branch of Semitic, not to be confused with either Arabic or the Sabean civilization and its Ethiopian colonies. MSA includes languages such as Soqotri, Jibbali, et al. (Some Muscovites and Bostonians agree that MSA merits a higher place, closer to the initial or top node in Semitic.)

Dogs and Cows - Again! Pigs too!

What's a nice linguist like you doing in a topic like this? No mystery. Prehistory is great fun! But we have reasons for returning to our symbiotic pals. First, we actually scoop SCIENCE this time. Next week they will announce that dogs were domesticated about 100 kya! That is based on mtDNA and 6 times older than the usual archeological age of 14 kya or 3 times the biogenetic date of 15-30 kya which we announced in MT-25. With all due respect for biogenetic dating, the 70 ky to 80 ky spread between the two dog dates is a bit off-putting. Never having been happy with the previous dates for our marriage with our best friends, I embrace the 100,000 date enthusiastically because it "stands to reason" in prehistory. Since the new dates differ so much from R. Wayne's dates in MT-25, perhaps we are also freed from his insistence that all domestic dogs are descended from grey wolves? Mayhap it happened several times, given the ease of raising puppies?

Secondly, **Roger Blench** was disturbed by the comments on cattle in MT-27 which he labeled as 'bizarre' and probably taken from Gautier's (presumably) mistaken theories. I do not know what Gautier's theories are; though I've learned a lot from French archeological research in the Sahara and North Africa. Even though not overly impressed by mtDNA dating, and in MT-27 we corrected one such date by 20%, still they are scientific hypotheses not to be discarded so lightly. Yet my espoused dates for a Saharan Neolithic are based on archeology -- French, American, British, & Polish.

Third, **Roger Blench** wanted us to know that a major study of pigs in Africa has been made by himself. Entitled "A History of Pigs in Africa", its roughly 10 pages will appear in R.M.Blench and **K.MacDonald**, eds. THE EVOLUTION AND DEVELOPMENT OF AFRICAN LIVESTOCK: ARCHAEOLOGY, LINGUISTICS AND DNA. UCL Press, London. 1997. Since Roger sent a draft, not the final pages, we can say only limited things. But, since it is his book, surely our words can be taken as a sort of publicity. Let his abstract suffice:
[Begin quote]
"Almost all texts concerning the domestic pig argue that the pigs

of Africa are a recent introduction, with the exception of those in the Nile Valley. This appears to be contradicted by both linguistic and cultural evidence which suggests that an ancient pig-keeping culture was spread from the Sudan-Ethiopian borderlands to eastern Burkina Faso. The Nile Valley, however, is likely to have been the primary corridor of transmission to Sub-Saharan Africa since pigs cannot be herded with other pastoral animals. Pigs were introduced by the Portuguese in the early period of contact and seem to have spread rapidly inland and crossbred with 'native' pigs There is additional fragmentary evidence that pre-European pig-keeping may have extended into the Congo and Angola. An ancient centre of pig breeding is also noted in the Senegambia, although its origin is difficult to explain. Pigs in eastern and southern Africa, by contrast, seem to be derived either from Portuguese introductions on the east coast, or later introductions by missionaries."
[End of quoting]

A nice paper! We might suggest to the author that eastern and southern Africa once had a major contact with one of the great pig-keeping areas of the world -- Southeast Asia. In the form of old Indian Ocean trade which eventually brought the ancestors of the Malagasy to the coasts of East Africa, then the Comorros, then Madagascar. Some by-products of that contact did spread to West Africa (e.g. the xylophone, taro, some yams). Why did this leave no pigs in East Africa? Just an amiable query.

We Must Rethink What We Know About Pigmies

With the title "ARE THE AFRICAN PYGMIES AN ETHNOGRAPHIC FICTION" **Roger Blench** has mounted a root and branches attack on much settled prehistory, or ostensibly settled prehistory, involving the inhabitants of the great rain forests of central Africa. More specifically, he argues that the pigmies are the product of natural selection for small size but that this diminishing of normal central Africans is not an ancient event. Nor are the pigmies the ancient inhabitants of the rain forest. For people who like to be up to date, going with the flow, challenging things, breaking new ground -- this will be an exciting thesis.
Certainly the thesis is wide-ranging, and very ambitious, and a classic use of biological, linguistic, and cultural data (some of the last from archeology). For the most part, however, Roger is scornful of biogenetic conclusions and quite rough on Cavalli-Sforza's hypotheses, at least about pigmies. Although one might say a great deal about the Blenchian thesis, we confront again the fact that this too is a draft. The paper will be published, presumably, in **K.Biesbrouck, S.Elders and G.Rossel, eds.** PROCEEDINGS of the LEIDEN CONFERENCE on HUNTER-GATHERS of EQUATORIAL AFRICA. Publisher is Research School, Centre for Non-Western Studies, Leiden.

Since it is unfair to criticize an unpublished work prematurely, I make a single recommendation. Before final publishing Roger should re-check his facts and re-write some hypotheses. An item: in repeating the belief

that diversity in Papua and the New World each exceeds that of Africa, he gives no basis for believing so. One does not know how he figures this out. In any case it is impossible to accept. It seems all backwards, opposite to truth.

Australia and New Guinea: Unity

An unread article in AJHG has the provocative title: "An Ancient Common Origin of Aboriginal Australians and New Guinea Highlanders is Supported by α-Globin Haplotype Analysis." Its authors: **J.M.Roberts-Thompson et al.** We regret that we could not get to it but it sounds like it is based on autosomal DNA. As everyone probably knows by now, the settling of New Guinea by modern humans is almost as old as that of Australia. It figures that new settling was by their common ancestor, assuming that this new study is correct. Source. AJHG, vol.58:1017-24

More News from Ofer Bar-Yosef

Thanks again to my best informant who I perpetually trouble with my questions. He is kind.

First, from Japan, news that could have been reported in the hottest section alongside the spear from Germany. No excuse except that there is no source save 'personal communication'. In effect this is a conference & consulting report. Early paleolithic artifacts from a site in Japan date to 400-500 kya. No fossil hominids were found. One could suspect **Homo erectus** but the context induces caution -- it could have been the proposed **Homo antecessor** because some stones in caches seem to show long range planning or the kind of rational coping that correlates well with the German spear.

While there are no bones, there are **biface adzes** of stone. It is generally the case that hand axes or biface axes are rather more associated with the western part of Eurasia than with the east. And biface adzes more with the east. Ofer wondered if these Japanese adzes and the **chopper tools** at the Diring site in Siberia testified in favor of the oft refuted and oft proposed **Movius Line.** For those who may not have tuned in a few years ago, the famous Line was invented by **Hallam Movius** (late of Harvard) to explain a hemispheric (nearly global) tendency for tools of axe type to be concentrated in the west and tools of adze type to be concentrated in the east. The border ran roughly through eastern India.

Ralph Linton used to say that "chasing adze types around the Pacific" was an ethnological game he did not want to play. Even if Movius Line thinking applied to the times of **Homo erectus**, still the typological split between east and west seems to have some ethnological validity in the modern world, i.e., it's ± factual. Hmmm!

Second, a **Tabun female**, a Neanderthal, has dates of probably 60-70 kya. We are now pretty sure that Neanderthals are late comers to Israel and the Middle East. The site of Shanidar (Iran) is now crucial for testing this notion, when that site is re-examined. The key question is: when did the Neanderthals disappear from the Middle East? Was it earlier or

later than their disappearance from Europe? [This again from Ofer Bar-Yosef].

Third, we now have a little better understanding of the date of the Near Eastern Neolithic moving into adjacent areas from the Levantine Corridor (Euphrates to Jericho) where earliest agriculture is found. In the second stage (after the Levant) farmers expanded into central Anatolia around 9000 BP. By 8000 the technology, but not necessarily the people, moved to the Zagros and then all the way to Khuzistan (Susiana). Through or around the Caucasus to Turkmenistan by 8000 also. Finally into Baluchistan over the mountains by 8000 too.

Even though Ofer is an acknowledged authority on the Near Eastern Neolithic, he recommended a source: **David R.Harris** 's book on the Neolithic (from Smithsonian). I'm sorry the book title escaped me. There also was no name given for the site in Japan; twas my oversight.

Our Fellow Animals: Our Cousins

Something like a definitive taxonomy and dating of the ape family, whose tallest and meanest members we are, has been worked out by **Maryellen Ruvolo.** The means are molecular rather than morphological but it is hard to see how her results would displease a morphologist very much. We present a crude version of her scheme forthwith:

↓
Proto-Ape at 11 my (11,000,000)
Proto-Ape --> Orangutan + Rest
Ur-Orangutan at 3.5 my --> B + S
B + S ---> Bornean + Sumatran
Rest of us at 8.7 my --> A + B
A --> Gorillas --> East + West
Ur-Gorilla at 2.2 my
West = Western Lowland at 705 ky
(split to two branches)
East = Eastern Lowland + Mountain Gor. split @ 400 ky
B --> Chimps and Humans @ 6.9 my
Chimps --> Pygmy + Common @ 2 my
Pygmy Chimps halved @ 400 ky
Common halved @ 900 ky, then
1 half halved again 600 ky
Humans halved @ 298 ky

[Note: Her humans = **Homo s.s.** or modern humans. Our fossil forebearers occurred between 6.9 my and 298 ky. Since we lack mtDNA for them, their position on the scheme cannot be calculated. Their dated bones (morphology) surely lie along the human line after 6,900,000 years ago.]

We cannot reproduce her neat scheme because it is still a draft. To appear in ANNUAL REVIEW OF ANTHROPOLOGY, vol.25, in October, 1997, entitled "Genetic Diversity in Hominoid Primates". We will give her Abstract because it sums up so well the general strategy of molecular work. [Begin quoting]

"Humans are only one of the species produced by the hominoid evolutionary radiation. Common and pygmy chimpanzees (our closest relatives), gorillas, orangutans, and the lesser apes also belong to this group. In humans, patterns of genetic variation are becoming increasingly better characterized by modern molecular methods. But understanding human variation in an evolutionary context requires comparison of human patterns with those of other hominoids, in order to reveal features shared among hominoids and those unique to humans Genetic variation among chimpanzees, gorillas, and orangutans is beginning to be characteriz-

ed, so that comparisons are now possible.

From genetic data, several different kinds of information can be reconstructed, including the evolutionary relatedness of subspecies and populations, time estimates for evolutionary divergences, past population dynamics, extent of gene flow over geographical landscapes, and group social structure. Knowledge of hominoid genetic variation also has relevance to applied fields such as primate conservation and medicine." [End quote].

In the text around her scheme (Fig.3) she adds that: "Humans show the least variation within species and thus have a common ancestor which is more recent than that of all other hominoid species. Orangutan subspecies are more different from one another than are the two species of chimpanzees; the same is true for western lowland gorillas versus the other two goriilla subspecies. The tree is calibrated using 298,000 years as the divergence date for modern humans (Ruvolo et al 1993), and tree proportions are inferred from the cytochrome oxidase subunit II gene data (Ruvolo et al 1994, Ruvolo 1996) ..." Additional Sources. Her references are found in (1993) MOL.BIOL EVOL. 10:1115-35; (1994) PROC. NATL.ACAD.SCI.USA 91 :8900-8904; (1996) MOL.PHYLOGENET.EVOL.5: 202-19.

Hominoids: Definitive Review of Issues & More Details

I rarely use the word brilliant of scholars, assuming that most long rangers are already pretty bright. But this scholar causes me to dust off the old word and apply it to her. **Maryellen Ruvolo** has written what appears to be the last word on an issue -- which of us apes is closer to which? Man may be alone in the vast cold universe but there is a wee bonobo tugging at his sleeve, saying: "Hey, what about me?"

Ever since **Becky Cann** first amazed us with her mtDNA work in the high 1980s, we have been reporting biogenetic results in this Newsletter. Frankly, it is remarkable how tolerant the pros were of our reports! But we staggered along, trying to give the gist of things to the non-biologicals. In the process we mentioned a number of different kinds of genes and DNA loci, many of which gave conflicting results, so that the estimates of dates and homelands and migrations kept varying from issue to issue. But a enormous amount of work was being done by large and cooperative groups of smart scientists and sooner or later it was all bound to be integrated by someone -- at least with respect to some key hypotheses and issues.

That is what Maryellen has done and in the process summed up the strengths and limitations of many kinds of locus & method. We cannot give the whole paper which is hot off the press; we have only permission to quote from it. One is sincerely advised to read it, even though it is bloody technical, because her English translation is first rate. Just skip the pages on statistics. But we believe that she is right to hold firm to her conclusions which we believe to be scientific truth. For a long time to come. [Note: 'clade' is a branch < Greek /klados/.] Here is her abstract: [Quoting now] ↓

"Consensus on the evolutionary relationships of humans, chimpanzees, and gorillas has not been reached, despite the existence of a number of DNA sequence data sets relating to the phylogeny, partly because not all gene trees from these data sets agree. However, given the well-known phenomenon of gene tree-species tree mismatch, agreement among gene trees is not expected. A majority of gene trees from available DNA sequence data support one hypothesis, but is this evidence sufficient for statistical confidence in the majority hypothesis?

All available DNA sequence data sets showing phylogenetic resolution among the hominoids are grouped according to genetic linkage of their corresponding genes to form independent data sets. Of the 14 independent data sets defined in this way, 11 support a human-chimpanzee clade, 2 support a chimpanzee-gorilla clade, and one supports a human-gorilla clade. The hypothesis of a trichotomous speciation event leading to **Homo**, **Pan**, and **Gorilla** can be firmly rejected on the basis of this data set distribution. The multiple-locus test (Wu, 1991), which evaluates hypotheses using gene tree-species tree mismatch probabilities in a likelihood ratio test, favors the phylogeny with a **Homo-Pan** clade and rejects the other alternatives with a p-value of 0.002. When the probabilities are modified to reflect effective population size differences among different types of genetic loci, the observed data set distribution is even more likely under the **Homo-Pan** clade hypothesis. Maximum likelihood estimates for the time between successive hominoid divergences are in the range of 300,000-2,800,000 years, based on a reasonable range of estimates for long-term hominoid effective population size and for generation time.

The implication of the multiple-locus test is that existing DNA sequence data sets provide overwhelming and sufficient suport for a human-chimpanzee clade; no additional DNA data sets need to be generated for the purpose of estimating hominoid phylogeny. Because DNA hybridization evidence (Caccone and Powell, 1989) also supports a **Homo-Pan** clade, the problem of hominoid phylogeny can be confidently considered solved."

[End quote]

A few subsidiary points -- not minor details -- about the power of different data sets will give us some perspective on other articles. On page 17 she says:

"Taking all of the DNA sequence data sets which provide some phylogenetic discrimination among hominoids and placing them into linkage groups, we have 14 independent data sets (Table 1). Of these, 11 independent data sets support the phylogeny with a **Homo-Pan** clade (one mitochondrial, one Y-specific, one X-specific, and eight autosomal), 2 autosomal data sets support the phylogeny with a **Pan-Gorilla** clade, and one autosomal data set supports a **Homo-Gorilla** clade."

In case this point was not understood, she makes it again on page 23, in the midst of a math blizzard (harder to savvy than Sumerian texts!), thusly:

"A consequence of these differing probabilities is that Y-linked genes (like mitochondrial genes, Moore, 1995) have a higher probability of producing

a gene tree-species tree match than do nuclear autosomal genes. X-linked genes also have a higher matching probability than do nuclear autosomal genes, but this probability is less than for mitochondrial and Y-linked genes (Figure 1). These probabilities thus define a hierarchy of loci for phylogenetic reliability, with mitochondrial and Y-linked genes most reliable, X-linked genes intermediately reliable, and nuclear autosomal genes least reliable."

By linguistic analogy this is similar to: basic or core lexicon + grammemes is more reliable than outer core lexicon which is better than general or cultural lexicon for classification.

On page 46 Maryellen sums up a number of estimates of so-called 'coalescence times' or roughly age of common ancestor, in relation to population size estimates. While she herself has chosen 298,000 years as the age for proto-**Homo**, the various studies have a huge range from as low as 37 ky to 60 my ! There are some reasons for not taking most of these dates too seriously. One is the effect of assumptions about population size, years in a generation, and mutation rates. Another is the outside or non-genetic date they measure against. We have traditionally supported biogenetic dates when there is some archeological support for them, hence the clustering around 100-130 kya which we have promulgated. Some of the 'coalescence times' are sans doute juste, spot on, just right. Our problem is telling the good dates from the crazy ones.

More perspective coming up. It is no surprise to find a theory of kinship betwixt man and ape. Surely paleoanthropologists and mammalian taxonomists have been proposing some kind of close relationship for many generations now. We hear that Darwin thought as much 140 years ago. Linnaeus noted our similarities before that. Again surely the idea goes back at least to Greek science and/or philosophy. This idea is not new.

Yes, but! We are very proud primates, uppity apes, haughty. The idea of relationship has <u>not</u> been popular. One way to escape it is to do the mind versus body thing. Or body versus soul. Christian theology has tried to live with evolution by letting the bodies evolve but reserving to God the right to put souls into humans. Of course, this denies souls to animals, something that many religions never did, especially native American ones. And nowadays new religions, integrating ideas from Buddhism, Christianity and Buck Rogers, have people descending from unseen space stations or celestial homes to enter human bodies or 'vehicles'. Some 30 of them in California abandoned their vehicles thus to return home -- somewhere near the tail of a passing comet. To them it was <u>not</u> mass suicide, just going home.

What modern biogenetic work does is to bind our bodies even more closely to those of our kin but in a context where an explosion in neuroscience is drawing so much of human psychology ever closer to the brain with an ever increasing set of genetic determinants of human behavior. In a word it becomes harder to talk about mind or soul as independent of brain, body, and evolu-

ionary background. Not to mention those 'mind' things like language and culture which do not easily reduce to individual psychology or to the brain either for that matter.

The Explosion in Neuroscience

Biogenetics is not the only hot area in science these days. Biology as a whole is thriving, yet the fascinating interface between biology and psychology is booming. The reason? New and remarkable methods for examining the human brain and correlating its impulses and activities with psychic states, thoughts, feelings, memories, und so weiter.

We cannot begin to report on psychobiology and its hot spot -- neuroscience. It is where the 'hardware' issues of our whole enterprise are centered, yet the sheer volume and complexity of research is beyond our capacity. So sorry!

Still we can recommend a few good sources for members to poke around in. First, within a short time, **Philip Lieberman**'s new book EVE SPOKE should be or was published. It will be one of the most comprehensive efforts to discuss the 'hardware' problem and make sense of it. We look forward to seeing it, along with everyone else. Conversation with Phil leads me to say this, as well as remarks he made at our Annual Meeting. As many know he offers an alternative to the more orthodox Chomskyite theory of language and brain, and child learning of language. Phil was Noam Chomsky's first doctoral student; thus his subsequent break with Chomsky was not a misunderstanding of difficult theory, but rather disagreement.

Second, SCIENCE recently devoted a major section of one issue to Neuroscience, including a section on language. This was followed later by an exchange of letters in SCIENCE about details of child acquisition vs learning of language.

Source. SCIENCE, vol.275, 14 March 1997 from page 1580 to 1611 had COGNITIVE NEUROSCIENCE with this Table of Contents:

News

Getting a Grasp on Working Memory + Working Memory Linked to Intelligence 1580-82

Visual System Provides Clues to How the Brain Perceives 1583-85

Articles

Linking Mind and Brain in the Study of Mental Illnesses: A Project for a Scientific Psychopathology 1586-92
by Nancy C. Andreasen

A Neural Substrate of Prediction and Reward 1593-98
by W. Schultz, et al

Language Acquisition and Use: Learning and Applying Probabilistic Constraints 1599-03
by Mark S. Seidenberg

Optimality: From Neural Networks to Universal Grammar 1604-10
by Alan Prince & Paul Smolensky

Book Reviews

Language in Cognitive Development: Emergence of the Mediated Mind. by Katherine Nelson. 1996. Cambridge University Press. Reviewed by Catherine Snow 1611

The letters which displayed to the maximum abilities to quibble were in SCIENCE, vol.276, 1177-1181, 1276, 23 May 1997. The issue was basically learning vs acquisition of language.

Taxonomy & Phylogeny: Interest

Increasing interest in classification in evolution was a fo[illegible] in SCIENCE, VOL.276, 11 Apri[illegible] 1997, 218-9, 227-32, 253-59.

SOME TARDY NEWS IN THE WEST, ONCE HOT NEWS IN JAPAN

At the very last moment (mid-June) ANTHROPOLOGICAL SCIENCE: JOURNAL OF THE ANTHROPOLOGICAL SOCIETY OF NIPPON, vol.105, arrived. It had a set of abstracts from the Society's annual meeting last October. **Kenichi Aoki** (U/Tokyo) is editor-in-chief; he's particularly interested in sharing information and views. What we shall do then is to list a scholar's name, university, and a very brief statement of the gist of her/his paper; in so far as long rangers may find interest in it. Only some of the papers reported. Apologies for haste & brevity.

[Note: allow 5 months for Surface Mail to cross the blue Pacific; now that is Whale Mail!]

Osamu Kondo. (Tohoku U.). Re the Dederiyeh Neanderthal child in Syria. Its limbs are close to being cold-adapted but maybe tropical.

G. Suwa (U/Tokyo), **B.Asfaw, Y.Beyene** (Addis Ababa). At the Konso-Gardula site (sw Ethiopia) "A total of 11 hominid specimens have been recovered as of the end of the 1995 field season. These are attributed to **Homo** and **Australopithecus boisei.** All hominid specimens derive from the circa 1.4 to 1.5 myr horizons as determined by Ar/Ar dating of volcanic tuffs."

Hiroki Oota (U/Tokyo) **et al.** Human remains from Linzi, Shandong (east China) compared to those from Takuta-Nishibun (Kyushu) by mtDNA extraction < teeth. Some resemblances & also to Mongols, Koreans, Ainus, and Amerinds. (From hyper-variable region of D-loop)

S.Horai (Nat.Inst.Gen., Japan) **et al.** mtDNA study of East Asians. "Of note is the finding that 50% of the mainland Japanese had continental specificity in which Chinese or Koreans were dominant, while less than 20% of either Ryukyuans or Ainu possessed continental specificity. Phylogenetic analysis of the entire human population revealed the closest genetic affinity between the mainland Japanese and Koreans. Thus the results of this study are compatible with the hybridization model on the origin of modern Japanese. It is suggested that about 65% of the gene pool in mainland Japanese was derived from the continental gene flow after the Yayoi Age." [Note: several important ambiguities - HF]

K.Shimizu (Naruto U.). ß-globin gene clusters in Colombia, viz. Wayuu, Kamsa and Inga (Amerinds). Links to both Asians and Europeans, but not Africans, were found. [Note: cf p.5 above for ß-globin]

Megumi Kondo & Shuji Matsu'ura (Ochanomizu U.). Dated remains of Mikkabi man and Hamakita man (cent.Japan). Both are pre-Jomon; 1st is 18,000 to 8000. 2nd is 18,000 or a bit less.

Shizuo Oda (Tokyo Metro.Govt.). Archeology primarily, reconstruction of prehistory of Jomon culture, especially "Maritime Jomon Culture". At end Pleistocene 2 peoples with microlithic culture entered the islands, one from northeast & one from southwest; both circa 14,000 BP. 3rd culture with polished adzes came 12,000±. These all > Jomon. After 7000 Jomon expanded seaward along north Ryukyus but also out to the Marianas (Micronesia!). Southern Ryukyus had non-Jomon culture like that on Taiwan and Philippines.

Hiroto Takamiya. Rehearses Okinawan history from Pleistocene at 20,000 (Minatogawa) at least to 'Late Jomon' circa 4000.

Y.Dodo, O.Kondo, N.Doi (Tohoku U., Ryukyu U.) shared 2 papers on the cranial affinities of the Amami islanders. 1st showed that Jomon and Ainu were a clade, whilst Amami was part of a Mongol to Okinawa clade but coordinate to the rest. Amami not like Jomon-Ainu. 2nd study says: "that the Amami-Okinawans, generally considered to have close affinity to the Ainu, have unique morphological characteristics ... They were clearly discriminated from the Ainu by flat faces." [Note: Doi's diagram sorts them & Japanese & Ainu into a triangular relationship.]

K.Ohnishi (Niigata U.). Oh, ho! a linguistic paper. Classifies Ainu, Eskimo, Japanese, and Korean as members of Austronesian language family. The basis? Basic vocabulary. Also interesting is his(?) conclusion that Japanese and Korean "most plausibly" have their common ancestor in Austronesian. [Note: details urgently wanted]

Michiko Intoh (Hokkaido Tokai U.). Article in AS, vol.105, no.1, January 1997. 15-28. "On Human Dispersals into Micronesia". Shows archeological inferences fighting linguistic ones. First site in Micronesia is Achugao, Saipan, southern Marianas at 3600 BP. Culture of "maritime-horticulturalists' distinct from, but similar to, Lapita (consensus source of Polynesians) came from the west. Three subsequent "dispersals" came from the west (2000 BP) and southeast (2000 and >1000). [Note: clear error = no use Blust!]

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We have to stop somewhere. This has gotten out of hand because we waited so long to produce the News that it accumulated & accumulated, until it has become almost unmanageable. There is such an explosion of research in our sector of science that more frequent and shorter issues of our Newsletter are necessary.

Therefore, we will try to hold to a schedule of quarterly issues but try to keep them more like letters and less like mini-journals. We will aim at Newsletters about the size of MT-25, about 10 pages, not like Mt-24 (100 pages) or even MT-26 (36 pages). Naturally we will do more when something extraordinary is involved. Examples might be the extraction of good DNA from a Neanderthal or the linguists at Berkeley hiring Ruhlen as a tenured professor, etc.

It is also apparent that many readers cannot cope with too much news. Long issues are off-putting. Why? Limited time, too hard, etc.

Finally, we are sorry not to report more news of members' activities, news about conferences, books to be reviewed in WORD, and other things of more personal concern to members. On the other hand few of you send much information in to us anyway. It is commonplace to hear that someone gave a major paper at a conference but told us naught of it. Still, if everyone did inform us, we couldn't handle it!

What we need is your opinion. Please tell us whether you want us to stress 'objective' news or 'personal' news. Or if you want a nice balance between the two conceptions of a proper newsletter.

Oh Dear, oh dear! Something to Try One's Patience

We will do this squib here instead of revising the whole issue because we want you-all to see what happens when one gets into a journalistic mode and popular magazines + ambitious science writers take over a familiar topic. We are referring to the June 16th issue of THE NEW YORKER, a popular magazine among the educated classes in USA. It featured the peopling of the Americas but in a bizarre way. I feel obliged to respond because that article will have wider circulation than everything we have said about Amerinds for the past decade.

Written by **Douglas Preston** and entitled "Reporter at Large. The Lost Man", p.70 et seq., sub-titled "Why are scientists suing the government over the nine-thousand-year-old Kennewick Man?". At issue is a legal dispute between the **Umatilla** tribe of Oregon + the U.S. Corps of Engineers on one side and a group of anthropologists on the other. Also supporting the Umatilla is an odd coalition of Christian fundamentalists and socio-political liberals. The Umatilla want to bury the remains, the Engineers want to help the Umatilla -- and so they seized the bones and locked them up --, and the anthropologists want the bones studied for prehistory reasons. The Umatilla say they already know their history. The anthropologists say the bones aren't Umatilla, but rather early immigrants from Asia. The Umatilla say that they have always lived here. We're the autochthones. The anthropologists say the bones are unmistakably "Caucasoid". The Umatilla say their tradition is that they have not always looked the way they do now. Und so weiter.

[Note1: remember the blurb at the end of MT-26? Israelis have met this kind of problem -- orthodox vs archeologists -- and solved it by discussion and negotiation. That might be tried in this case?]

[Note2: at no point in the discussion in the article was the language of the Umatilla mentioned. Looking it up in Greenberg, and Ruhlen, I found it -- absent! An editorial accident, no doubt. Umatilla is most likely to be either Penutian or Uto-Aztecan, but I can't reach any experts right now. If Penutian, it may be quite old **in situ.**]

A spin-off of this case is the remarkable discussion among anthropologists which followed, during the effort to show the bones did not belong to the Umatilla. You will have to read it to believe it. Drawing on long ranger **Robson Bonnichsen** and some famous short rangers usually opposed to pre-Clovis dates, the reporter absorbed two ideas from them which he joined together and ran with, spending much mental energy trying to explain. Idea 1 is that the original Americans were Caucasoid. Idea 2 is that Clovis technology is so close to Solutrean in Europe that it must have been made by the same people. So the problem is how these Solutreans got to Oregon thousands of miles away from France. Answer? They walked across the frozen sea from Norway to Newfoundland! Well, Caucasoids were very hardy in those days! Hmm, I thought Yasus, the Jew, was the first to walk on water. Or was He?

Do read the thing, friends. Send your comments to THE NEW YORKER.

NEWS OF MEMBERS' ACTIVITIES, INCLUDING LETTERS OF COMMENT

As we explained above, this report is extremely brief because MT-28 is overloaded. Just two small comments from esteemed scholars are included. Carleton Hodge's letter on Egyptian is zeroxed separately; we can't do his hieroglyphics on this computer.

Jürgen Pinnow, one of the few to write authoritatively on Nihali, had a comment on the Basque issue and some Muscovite remarks about Na-Dene. Since we want to gather in all the comments on that Basque issue above all, Professor Pinnow's comments have priority. He has exercised his option to write in German. His address is Gorch-Fock-Strasse 26, D-25980 Westerland/Sylt, GERMANY. (He reads & writes English well.)

Dated 20.4.1997 (Customary salutations)

"Leider mit Verspätung, aber mit grosser Dankbarkeit bestätige ich den Erhalt von MOTHER TONGUE Issue II, December 1996, mit dem Hauptthema Nihali.

Naturgemäss ist dies Thema für mich besonderem Interesse, und ich freue mich sehr, dass alle meine diesbezüglichen Arbeiten aufgeführt und z.T. behandelt werden.

Befremdlich hingegen ist, was Sergei A. Starostin p.101, "Comments on the Basque-Dene Comparisons" notiert: "... I only wish that comparative Na-Dene materials were published, too (by S. Nikolayev, or by his opponents), so that the supposed fourth branch of Dene-Caucasian could also be open to investigation."

Mir ist absolut unverständlich, warum meine umfangreichen sprachvergleichenden Arbeiten über das Na-Dene mit Schwerpunkt Haida ständig ignoriert werden, während die relativ unwesentlichen Bemerkungen und eine Buchbesprechung zum Thema "Nihali" im Rahmen der Austroasiatistik voll berücksichtig werden.

Für eine kurze Antwort zu diesem Komplex wäre ich sehr dankbar.

Mit freundlichen Grüssen und allen guten Wünschen,

J. Pinnow " [End of quote]

[Note: a long time ago in this Newsletter we announced the availability of Pinnow's Haida and comparative Na-Dene material. Only one person ever asked about it (Krauss of Alaska). Our material was obtained from // Uwe Johanson, Volkerkundliche Arbeitsgemeinschaft, Postfach 1142, 2353 Nortorf, GERMANY //. That address may now be wrong as the zip code lacks a 5th number. Uwe may have moved. But maybe now everyone can stop saying that there is no Na-Dene comparative work which includes Haida! On behalf of the officers and editors let me say that we are sorry that Pinnow's work has been so carelessly ignored. -HF]

F.B.J.Kuiper, the published authority on Nihali, sent a nice note: "I received Mother Tongue in good order and this is to thank you warmly. I would also compliment you on the way the Nihali material has been presented. I have long known of the existence of a Nihali vocabulary and at one time I have been considering trying to get it for publication in our Indo-Iranian Journal, but the way it has now been published is by far the best one. With my best wishes for the future of Mother Tongue. Sincerely, F.B.J.Kuiper." [End of quote]

ANNOUNCEMENTS & ADVERTISEMENTS: THE MEMBERSHIP (PERMITTED) LIST. Continuing the list of members who permit their names to be made public. The following list is not a true list of the members of ASLIP. It 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.

Members and their Addresses: Those Who Permit the Publication

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