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NEWSLETTER OF THE ASSOCIATION
FOR THE STUDY OF LANGUAGE
IN PREHISTORY

Issue 26 (MT-26) Spring 1996

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

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INTRODUCTION TO MT-26: The Newsletter (Editor this issue: H. Fleming)

THE HOTTEST AND THE LATEST NEWS.

While the very hottest, most recent, news is often fairly old for some scholars close to the action, particularly in biogenetics, for colleagues in other disciplines the news may be absolutely fresh and exciting. Indeed for others the news ordinarily never would have reached them, working in the depths of another field.

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.

Our traditional mode of presentation features fossils first or at least archeological developments.

SOMEWHAT COOLER NEWS.

With some guilt we include portions of last year's hot news, chosen for their newsworthy qualities for scholars not likely to have heard it. Again we remind you that cool news may turn out to be of unrealized importance. With great gratitude to Alvah 'Pardner' Hicks: we are not able to publish large parts of his heroic efforts in 1995 to abstract the key developments in biogenetics and archeology for us.

DELAYED REBUTTALS TO TRASK'S CRITIQUE OF BASQUE contra MACRO-CAUCASIC

John Bengtson and Merritt Ruhlen have written partial rebuttals. We call them partial because both were restricted to 10 pages. Considering the very ample space Trask had for his critique and rebuttal, we see why Bengtson / Ruhlen are called partial. The controls on debate do not mean that the Basque business is finished; rather they show the limits of our space. We include belated Muscovite comments (Nikolaev).

->> >>> All below this will be in MT-27 (July or August). Space! <<<<-

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

Our first loyalty is to John Kerns, a stalwart colleague of long-standing. Egerod, Winter, Haas, and Stopa were not members of ASLIP but 3 of them were important historical linguists in specific regions. Wilfried Schuhmacher, Søren's countryman, has given a brief commentary on Egerod. Eric de Grolier has written on his colleagues, Winter (co-founder of LANGUAGE ORIGINS SOCIETY) and Stopa. Victor Golla published an obituary of Mary Haas on the Internet recently.

NEWS OF MEMBERS' ACTIVITIES, INCLUDING LETTERS OF COMMENT

Jungraithmayr's proto-Chadic lexicon, Bar-Yosef's trips to Choukoutien + Brooks digs in Ethiopia, Hayes subdues Austric, und so weiter.

ASLIP BUSINESS

The Annual Meeting was well attended, a Board of Directors was elected, Officers were elected, and general strategy was discussed. The innovation with the biggest impact was the polling of members by mail which produced a substantial total vote and virtual unanimity with the votes cast at the Annual Meeting. Those elected feel firmer.

ANNOUNCEMENTS & ADVERTISEMENTS, IN SUPPORT OF COLLEAGUES.

Reference will be made to Alvah Hicks summaries from time to time, labeled (AMH). Given the volume of research being done in archeology and biogenetics relevant to our common enterprise, we must be quite selective in what we report. Of course, that may expose us to the charge of being biased. We accept that charge because we are pragmatic; we try to report on positive developments but also negative arguments of **substance** directed against some hypothesis. Still we do not think that most of the quibbling and niggling and doubting -- sans doute, many scholars do nothing more than that -- has much to do with proper falsification of hypotheses. So we report very little of said bickering. That means we have to ignore 90% of linguistic work. Now

THE HOTTEST & LATEST NEWS

Maginot Line Punctured in Amazon

The first serious breakdown of the Clovis time line occurs without smashing new dates but rather via a general probability too strong to be blown away. At the same time that the Clovis horizon of big game hunters arose in the great plains of North America a very distinct forest-adapted small game hunting horizon existed in the middle of the Amazon system in northern Brazil. Very careful 'high tech' excavations block the usual dismissal of poorly separated strata or geo-facts too primitive to be called human tools. This set of sites was too well dug and analyzed to be blown away by scepticism. Or so it appears.

The major source is in SCIENCE, vol.272, 19 April/96, p.373-384. A.C.Roosevelt, et al "Paleoindian Cave Dwellers in the Amazon: The Peopling of the Americas." The abstract says: "A Paleoindian campsite has been uncovered in stratified pre-historic deposits in Caverna da Pedra Pintada at Monte Alegre in the Brazilian Amazon. Fifty-six radiocarbon dates on carbonized plant remains and 13 luminescence dates on lithics and sediment indicate a late Pleisto-

cene age contemporary with North American Paleoindians. Paintings, triangular bifacial spear points, and other tools in the cave document a culture distinct from North American cultures. Carbonized tree fruits and wood and faunal remains reveal a broad-spectrum economy of humid tropical forest and riveraine foraging. The existence of this and related cultures east of the Andes changes understanding of the migrations and ecological adaptations of early foragers." Among other things, the spear points -- triangular, stemmed & bifacial -- hardly look like geo-facts.

Apropos of the famous North American scepticism about pre-Clovis sites, (AMH) reports Allan Bryan (1991) saying this: "In order to cast doubt on any 'pre-Clovis' report, sceptics operating with the conviction that Clovis constitutes the only demonstrated evidence for Pleistocene humans in the Americas and therefore must be the earliest apply what they claim is the scientific method of multiple working hypotheses by raising any imaginable question about the validity of the reported radiocarbon dates, the reported

stratigraphy, or the report that artifacts and/or human-made features were recovered in proper contexts. In order to put a cloud over any reported 'pre-Clovis' site, sceptics, most of whom have never visited the sites in question, suggest remote possibilities that might conceivably be true, such as that an object that looks like an artifact might have been flaked during a flood or an earthquake, or is the product of a waterfall; that the radio-carbon samples might have been contaminated by coal or ground water containing ancient carbonates; that people might have collected old wood to use in their fires; or that the artifacts might have been intruded from later deposits. Although all these 'alternative hypotheses' might conceivably be true, in fact, the sceptics present no actual evidence to support their claim that they are true. Nevertheless, the sceptics insist that as long as at least one alternative hypothesis has been presented, then the original report must be considered as 'equivocal'. A reader usually interprets this statement to mean that the original report is probably in error and therefore should be dismissed." Bryan also argued that Andean people from 14,000-11,000 BP "appear to have been general foragers who occasionally took advantage of locally available large mammals in addition to smaller mammals." (AMH source is Allan L. Bryan, 1991. "The fluted-point Tradition in the Americas--One of Several Adaptions to Late Pleistocene American Environments." In R. Bonnichsen & Karen Turnmire, eds., CURRENT RESEARCH IN THE PLEISTOCENE. Oregon State

University.

Rising Tide Theory Fails Tests.

The multi-regional theory of anatomically modern humans & their world distribution has been battling the 'Eve' or 'out-of-Africa' or 'Noah's Ark' theory for about a decade. Based on the premise that *Homo erectus* was the last global diaspora from Africa and that regional varieties of *Homo sapiens* arose from regionally-evolved *Homo erectus* varieties, this theory denied an African diaspora or migration of modern man. One of its key points was that modern crania from China were more like ancient local crania than like modern African or European ones.

Biogenetic work has tended to be critical of multi-regional proposals. A recent nuclear DNA study (from the nuclear autosomal genome) supports the 'Eve' theory -- or fails to support the rising tide and finds that the diversity in Africa exceeds that in the rest of the world. In this there is no support for east Asian separateness, since the entire mass of Eurasians is more homogeneous than Africa. Thus a blow to multi-regionalism. (Source: S.A. Tishkoff, et al, SCIENCE, vol. 271, 8 March 1996: 1380-87. "Global Patterns of Linkage Disequilibrium at the CD4 locus and Modern Human Origins".) Their abstract says: "Haplotypes consisting of alleles at a short tandem repeat polymorphism (STRP) and an Alu deletion polymorphism at the CD4 locus on chromosome 12 were analyzed in more than 1600 individuals sampled from 42 geographically dispersed populations (13 African, 2 Middle Eastern, 7 European, 9 Asian, 3 Pacific, and 8 Amerindian). Sub-Saharan

African populations had more haplotypes and exhibited more variability in frequencies of haplotypes than the Northeast African or non-African populations. The Alu deletion was nearly always associated with a single STRP allele in non-African and Northeast African populations but was associated with a wide range of STRP alleles in the sub-Saharan African populations. This global pattern of haplotype variation and linkage disequilibrium suggests a common and recent African origin for all non-African human populations."

When did this biogenetic diaspora happen? The Tishkoff team reckoned 100,000 years ago or less. Few believe nowadays that such dates are very precise but the regular clusterings of dates circa 100 KYA is striking.

There is an excellent editorial summary of Tishkoff, et al, on page 1364 of the same issue of SCIENCE by Joshua Fischman. A number of critics are quoted; most are not deadly focused critiques. They resemble the Americanist sceptics more than anything else but John Clegg of Oxford's team is running a test on **beta-globin** genes which may produce different results.

Of more detailed interest to us is that **Agau Cushites** (Ethiopian Jews) and **Somalis** seem closest to the ancestral population of the non-African world; their genes are intermediate between sub-Saharan Africa and the rest of the world. Or the great diaspora is most likely to have departed from northeast Africa, especially the Horn. Of course, the

geography virtually dictates that probability. The Egyptians are closer to Middle Easterners (Druze + Yemenite Jews).

Also the first serious gene study with Caucasic speakers in it occurred! The 98 **Adyghe** folk sampled are West Caucasic, most likely Circassians. Simplement dit, they are Europeans! Still a bit frustrating because it is not said whether they **assumed** the Adyghe were Europeans or **concluded** that it was so. Finally, it may not always be useful to lump Africans together as such -- Wolof, Yoruba, Biaka & Mbuti pigmies, Kikuyu & Herero Bantu, Khoi & San Bushmen. Their differences are significant; some of us are interested in that too!

Tishkoff, et al, say in their footnote 20 that "Information on origins of population samples, sampling procedures, and preparation of DNA samples can be found by checking from the Kidd Lab Home Page on the World Wide Web at <<http://info.med.yale.edu/genetics/kkidd>>." Check it out!

Multi-regionalism took its **second blow** from fossil-oriented comparative study of crania, including faces (naturally). Here was centered the hard core resistance to 'Eve' theory and the anatomists from Weidenreich thru Coon to Aigner to Wolpoff. **Marta Lahr's** highly sophisticated, careful compilation and computations concluded that the flat faces (and other features) of Mongoloids were not local developments but present in older crania found to the west, especially in North Africa, and a robusticity in Pacific peoples was also matched in old Africa.

Highly interesting is the added (almost casual) finding that the antique Africans differ from modern Africans clearly -- to make the point that modern Eurasians are not derived from modern Africans but rather from ancient ones. We have known this in general terms for years now.

By analogy native Americans speak to the same point. Nearly universally derived from Asia in scholarly minds and most often seen as examples of archaic or earlier Mongoloids, these native Americans differ from modern east Asians in the direction of ancient east Asians. Some biogenetic studies find specific links between Amerinds and Pacific peoples. Perhaps wisely, multi-regional theory has not tried to account for the native Americans in any way other than migration from Asia.

(Source: Marta Mirazon Lahr, 1994. "The Multiregional Model of modern human origins: a reassessment of its morphological basis". JOURNAL OF HUMAN EVOLUTION 26, 23-56. Her abstract says: "The Multiregional Model of modern human origins predicts that a group of features, recognized as characterizing the evolution of regional populations from their archaic regional ancestors, will consistently show higher incidence in those regions. This model also predicts that regional morphological patterns are stable, reflecting absence of geographical isolation. In order to test these assumptions, the incidence and distribution of the regional features proposed to characterize the evolution of Chinese and Javanese *Homo erectus* into modern Chinese and Australian aborigines respectively were examined. The material studied

were five recent populations and a fossil sample of anatomically modern *Homo sapiens* from the sites of Afalou and Taforalt. For this, a scoring system of grades was developed, so as to allow quantification and statistical testing. These analyses showed first, that although the majority of the features studied represent a regional pattern, this pattern does not always correspond to that proposed by the model; and a second, that some of these features occur in other populations with a higher frequency. Furthermore, the lack of special resemblance between the North African fossils and recent Africans suggest high levels of population differentiation. These results indicate that these features do not support a multiregional origin, giving further support to the existing fossil, chronological and genetic evidence for a single African origin of all modern humans."

In both articles Dr. Lahr uses a new word (for me), to wit, **plesiomorph-** or near-form. Normally applied to crystals which look alike but which have different chemical bases, it refers to forms which resemble each other for some reason but which do not necessarily have the same origin or basis. Thus resemblances between some fossil faces and some modern ones may be due to a shared feature, e.g., robusticity, which has similar effects on other shared features. It reminds me either of biology's **analogous parts** or the **spurious similarities** of historical linguistics. However, following Michael Day's definition, a linguist would call it a **shared retention**. In any case plesiomorphy seems to have

misled multi-regional theorists.

Marta Lahr has another fine paper on the same subject but focused on Amerinds. Her conclusions are rich and thought stimulating. They are in part, as follows: "1. The late appearance of regional morphological patterns suggests that the last common ancestor between Asian peoples and Amerindians did not conform to the typical Mongoloid description. Furthermore, it should be considered that earlier typological studies did not observe character origin and polarity when characterizing populations, and instead defined such terms as Mongoloids on the basis of the most typical or derived group. Within an evolutionary framework, such groups are the most autapomorphic, and therefore the least likely to throw any light into population relationships."

"2. The populations from Tierra del Fuego and Patagonia, inhabiting the southern periphery of the Amerindian geographical range and showing a very robust morphology that departs from a typical Mongoloid pattern, may be seen as a group that has retained to a greater degree the morphology of the first inhabitants of the continent. As such, they provide evidence of heterogeneity within native Americans, and suggest that independent of the timing of first occupation, it is not possible to derive all the South American aboriginal populations from a morphologically derived ancestral source."

"3. The long-observed relationships between certain fossil (European Upper Paleolithic, Upper Cave Zhoukoudian, Lagoa

Santa) and recent (Ainu, Fuegian /Patagonian) populations and Australian aborigines should be given an interpretation in terms of differential retention of levels of robusticity rather than in terms of close phylogenetic distances." End of Lahr.

Aut-apo-morph is Greek to me, even knowing what the parts mean (self-awayfrom-form). All dictionaries failed me and most physical anthropology texts. Michael Day's (GUIDE TO FOSSIL MAN) rescued us. The word means "A new morphological feature confined to one group in an evolving lineage". An example is severe cold adapted faces of so-called 'specialized Mongoloids' of the Arctic, only one lineage within a larger set of Mongoloid lineages. This all makes Coon a guilty 'autapomorphophile'. Heh, heh. (My humor is oft obscure.) In MT-27 (hopefully) we shall return to Michael Day's marvelous book and see some fruitful links between biological systematics & historical linguistics.

SINO-TIBETAN --> TIBETO-BURMAN !

It is not routine procedure for a major linguistic phylum to be drastically altered internally, especially if reconstruction is well under way. Still it does happen from time to time; usually to the benefit of all. Such revisions are inevitably controversial -- at least at first -- and the heat often begets light.

So it may be with S-T (Sino-Tibetan), long viewed as having two primary sub-phyla: Chinese in one and all the rest in the other or T-B (Tibeto-Burman). Paul Benedict pioneered a first revision of 'Indo-Chinese', containing S-T + Thai & its kin.

That was a half century ago. Since then, Paul's classification has become more or less standard; others resemble his.

Now **George van Driem** of Leiden (PhD from Berkeley) has changed, one must say totally changed, S-T. Chinese has been de-throned at least as much as Semitic has been in Afrasian, if not more so. George properly gives credit for initial steps towards re-classification to **Nicholas Cleaveland Bodman** and **David Bradley**. Their schemes and references are in van Driem 1995. (Source: George van Driem, 1995. "Black Mountain Conjugational Morphology, Proto-Tibeto-Burman Morphosyntax, and the Linguistic Position of Chinese", SENRI ETHNOLOGICAL STUDIES 41: 229-259) It is nearly amazing that in a phylum noted for its tendency towards word isolation such a grammar-oriented study should overthrow the standard classification!

Here is the basic taxonomy of his new T-B :

T-B --> Western + Eastern

Western--> Baric, Sal, Kamarupan

Kamarupan --> Bodo-Konyak, Abor-

Miri-Dafla, Kuki-Naga, Mikir-

Meithei (India-Burma borders)

Eastern--> Northern + Southern

Northern --> Sino-Bodic or Bodic

+ Himalayan vs Sinitic or

Northwestern vs Northeastern

Southern --> SW vs SE

Southwestern --> Burmic, Karenic

Burmic ---> Lolo-Burmese

Karenic --> Karenic

Southeastern --> Qiangic, Rung

Qiangic --> Tangut, Qiang, Primi

Rung --> Nung + rGya-rong + Naxi

[Note: the exact membership of Qiangic and Rung groups was not given. Above is HF's guess.]

Fundamentally, George's new scheme reflects greater weight being given to the Himalayan

languages and, accordingly, less to Chinese, but also Karenic.

This is very exciting and we hope to have a **MT*Treatment** of this whole question, with the lead article obviously going to George van Driem's taxonomy.

FIRST DOGS, NOW COWS, WHAT NEXT?

Some long rangers were amused at the dog genealogies in MT-25, but it was relevant. More lively information concerns the age of African cattle. *Bos taurus* or roughly the long-horned **humpless** kind split from west Eurasian bovines 22,000-27,000 years ago! And from *Bos indicus* or **zebu** cattle even earlier. So what? yawned a bored grammarian in Ann Arbor? Well, cattle are involved deeply in proto-Afrasian, as well as proto-Nilo-Saharan, and all debates on African Neolithics. Dates on probable domestication differ; African *Bos taurus* circa 9000 BP and European circa 5000 Bp. Knowing that cattle did not come in with the Levantine Neolithic + knowing that cattle herding might be older than said agricultural revolution, we may learn something else, to put it mildly. (Source: Bradley et al, 1996. "Mitochondrial diversity and the origins of African and European cattle". PROCEEDINGS OF THE NATIONAL ACADEMY OF SCIENCES 93: 5131-5135). Thanks also to Alison Brooks for her personal communication and Ofer Bar-Yosef for a copy of the paper.

What next? We hear that someone has done the same for **sheep**, not yet published. They are interesting because of their solid ties to SWAsian Neolithic. Indeed Bradley et al deny that **shoats** (sheep & goats) are native to Africa and date their appearance in Africa to 6500-

7700 BP or within the range for the arrival of Levantine crops. For those reckoning that wider distributions mean older things we put some African facts on the record: Widest (global) = lice. Next widest = dogs. Nearly as wide = goats + chickens. Less wide = sheep + donkeys. Almost as wide = cattle. Still less wide = fat-tailed sheep + Zebu cattle. Limited distributions = camels + horses + pigs (tame). Given the paucity of sheep and donkeys in the great forests, it seems that Bantu crossed the rain forest without them, later borrowing them from resident East Africans. Thence southward.

Just to rehearse this point -- we can see clearly that shoats came long before cattle in the Levant but cattle came long before shoats in Africa; in terms of domestication. It is very unlikely that the pastoral Neolithic (so-called) in Africa derives from the Levant. But the question of farming & crops from the Levant remains quite active.

REINCARNATION AT ISHANGO.

Three decades ago a promising site at Ishango (lacustrine East Africa) was denigrated when 'contaminated shells' ruined its dates. Recently, Alison Brooks solved its problems, bringing back a splendid site with boats, harpoons, and mathematical bones all dated to 25,000 BP. Our Ms. Brooks was the one this writer forgot to credit (MT-25) for her work with John Yellin in finding the 90,000 BP harpoons in the same general area. The Ishango report was in AFRICAN ARCHEOLOGICAL REVIEW in 1987 & 1995.

Also at Ishango was skeletal material which has not been

published formally but was listed in the Abstracts of the AJPA. Some ideosyncrasies of shoulder and arm bones are said to be peculiar to modern Nilotic peoples to the north. (Additional source: Alison Brooks, personal communication, 1996) This is very interesting because Ishango ipso facto becomes a major candidate for association with old or proto-Nilo-Saharan, even if its location is too far south to be completely credible. Apropos of Nilo-Saharan, it seems that no tooth evulsion is found with the skeletons. Evulsion of two or four lower incisors is widely practiced by Nilo-Saharan peoples, as circumcision is by Afrasians. Check it out!

AFRICA'S VERY DRY SPELLS

Another valuable communication from Alison Brooks, reinforced by several books, is that there appears to be a reason for the dearth or seeming dearth of African sites with evidence of evolving modern people of any sort. In a word or two -- bone dry. During the long period from 60,000 to 20,000 BP there were very arid conditions prevailing over much of Africa -- in the Sahara, 5000km south in the Kalahari, and even in the cool highlands of East Africa. These arid periods were long and extreme, even though some small periods of moisture occurred too. That they had some effect on migrations and adaptations of peoples is self-evident. How they relate to the prehistory we are all working to reconstruct is not yet known in detail. But there will be some consequences for this knowledge later on.

FINNISH ROOTS DIFFER FROM LAPPS?

(Source: Thanks to Paul Benedict for sending this to me) A certain willingness to distance themselves from Lapps has been observed in the Finns. Yet linguists keep lumping them together as Finno-Ugrians. Their manifest physical differences are usually explained by contacts with I-E speakers leading to the obviously European Finns.

DISCOVER magazine in its May 1996 issue, under 'Ethnography' entitled "Saami I Am Not", reports two biogeneticists, Antti Sajantila and Svante Pääbo, have found that "Finns are more likely to share identical 'microsatellites' -- repetitive DNA sequences -- with other Europeans than with the Saami (Lapps). Meanwhile, more than a third of the Saami in the study group carried three specific genetic 'motifs' that were found in only 1 in 50 Finns and in none of the other Europeans studied."

Instead of making the more ordinary assumption that "more recent European immigrants who mixed with an ancient populace of eastern origin", they think that "a better interpretation of the genetic evidence is that Finns colonized the land from the south some 2,000 to 4,000 years ago, adopting a proto-Saami tongue in the process."

And why would (civilized) Finns adopt the language of nomadic herders they had already pushed up into the Arctic? Well, "Sajantila points out that the Finns, who now outnumber the Saami by 100 to 1, may not always have been in the majority. In fact, studies of genetic diseases unique to the Finns indicate that at some point they went through a squeeze in

numbers that, when the population later began to expand, resulted in the spread of rare, mutant genes. If this bottleneck occurred during their colonization of eastern Scandinavia, then the Finns might once have lived as a minority among the Saami."

"We know from history that the Finns have been pushing the Saami northward .. so it seems that the Finns were more powerful. But if it's true that the Finns have changed their language and obtained it from the Saami, it shows that the power game was not necessarily so simple or that the Saami were always the underdogs."

Well, Americans always cheer for the underdog, but I really like the Finns, so let us cheer for the truth. What is it in this matter? From over here the bias of the two Finns is breath-taking, while their moral support of the Saami is laudable.

The notion that the Finns are really Europeans but the Saami are not is quaint. Bodies are more basic than languages? This kind of assumption used to plague Omotic studies too. There, since so many Somotic speakers look more like central Africans than most Ethiopians do, European scholars were wont to believe that Somotic was Nilo-Saharan or such but surely not Afrasian. We got the same resistance about Chadic. Or how to explain the tall handsome 'Hamitic' Fulani speaking a Niger-Congo language.

Are north Russia and Scandinavia parts of Europe or not? Is Finnic not full of borrowings from Germanic, Baltic and Slavic? Have the Finns and Estonians not waged war and peace with Lithuanians, Swedes,

and Russians for many centuries? Is much of southwestern Finland not still full of Swedes? Why do we need a migration from the south anyway? Twas gene flow, mes amis! Sexual folk contact.

Is Suomi (Finland) cognate with Saami (Lapp)? A spurious cognate?

Methinks biogeneticists are starting to use 'bottleneck' as a **deus ex machina**, as a convenient device to explain things they cannot explain in normal terms. Shouldn't there be some rules for invoking this business of **bottlenecks**?

USA MARCHES INTO LA LA LAND.

Onward, Christian soldiers! Down with humanists & down with Bible-doubting science! Well, it is hot news, and for most of us, deplorable news that one giant TV network (NBC) showed a program 'The Mysterious Origins of Man' on 'prime time' (when the largest audience is watching) which starred Charlton Heston, a patriarchal figure who once played Moses in a movie. Why deplorable? Because the program argued seriously that there was hard evidence that mankind had co-existed 200 mya with dinosaurs; they even showed giant footprints purportedly from these very very old humans. They also claimed that the lost city of Atlantis was in Antarctica and that technologically advanced men existed before history began. Dio mio!

Methinks the program was a happy merger of New Age thinkers who divorced science in the '60s and evangelical creationists of Texas Baptist persuasion. Nevertheless SCIENCE was in a dither about it all, as the bio-

logy community was also reported to be.

The amusing aspects of the situation disappeared ten weeks later when pollsters measuring American scientific knowledge in general found that a majority of adult Americans believed that mankind had lived with the dinosaurs. In fact NBC cannot claim all the credit for that because Hollywood has been showing so-called 'B' movies for scores of years now which feature some poor explorers having to battle horrifying ersatz dinosaurs. No doubt Japanese colleagues will think of Godzilla-san.

VICTOR MAIR'S SUPERB CONFERENCE

A most valuable example of one ASLIP ideal, mutually stimulating interdisciplinary work, was shown to what I want to call perfection in Philadelphia in April. The convening of experts on all aspects of the prehistory and ethnology of East Turkestan (Sinkiang) was managed by Victor and his friendly team of Pennsylvanians.

A fair number of long rangers were there, and it was a pleasure to meet some of them, yet the conference was a short range one. Rather than focusing on a long range topic, we all were bound by the time limits set by Indo-European and the archeology of Sinkiang -- 6 kya. Malheureusement, there are too many excellent papers to report on, not even the abstracts which we had hoped to put in MT-26. We will perforce be most selective in what we report. My apologies! But only things relevant to common concerns can be included.

Sensa dubbio, the peak of it all came at the end -- the

semi-debate between Renfrew and Mallory over I-E dates and homelands. Sophisticated but amiable and a treat to the audience, the pair taught us much prehistory. Their talks were separate, not in a formal debate format, yet each disputed the other's past commentary on the questions. No clear victor (save Mair!) but each scored some heavy points. JPM scored against CR's chronology; CR scored against JPM & Anthony re horses. Great debate!

Here are other key points:

(1) **Tongmao Zhao** (NIH) showed that Uighur Turks, now dominant people in Sinkiang, were 55% European in blood/serological groups. Kazakh were 35%, Hui 14% and Dongxiang 26%. Someone pointed out that such was already obvious in Uighur faces. We all reflected on this 'simple' truth.

(2) **Paolo Francalacci** (Sassari) tested DNA from 'Tocharian' mummies, got enough mtDNA to tell something, and concluded that the mummies were certainly Europeans, adding that Euros were very homogeneous actually but included Turks and Lebanese. [Probably Jews too-HF]

(3) Chinese scholar, **AN Zhimin**, concluded that the Bronze Age came to North China via the Tocharians; this startled us.

(4) **Donald Ringe** talked about his new 'computational cladistics' which frankly mystified most listeners. He also made a statement which caused me to doubt his usefulness to prehistorians. A rough quote is: "What matters about all this is the method. The results are not important." I would say that meant he was playing theoretical games in preference to history. Still his results interested the conference because he 'found'

Indo-Hittite to be valid, i.e., Hittite is the first split off or coordinate to the rest of IE.

(5) **Eric Hamp** was a traditional methodological opposite to Ringe but also, unbelievably, found Indo-Hittite to be valid. Sturtevant would have loved this.

Hamp also produced a leit motif for his paradigm. Again a rough quote: "Our job is to produce an absolutely spotless reconstruction of proto-Indo-European; nothing else really matters."

Does that sound like the voice of Neo-Grammarians in the 20th century? Nothing in this about taxonomy or prehistory. And since the perfect reconstruction is forever eluding our grasp, his brand of linguistics looks more and more like the Arthurian pursuit of The Holy Grail. Anyone for Camelot?

(6) **Michael Puet** (Harvard, East Asian Studies) gave a paper full of wisdom in which he advised the 'New Archeologists' or the 'Process Archeologists' to admit some diffusion sometimes in their explanations. Local process does not always explain things, he said. (We hope to get more of his paper another time.) During questions, Colin Renfrew supported Puet's main argument.

Michael had written his paper before a big discussion of Bronze Age metallurgy in which the amazingly rapid spread of that technology from the Atlantic to the Pacific (in the 'civilized' world) begged for explanation. During our talks, I was very surprised to be twice attacked by irritated colleagues for being so 'old-fashioned' as to suggest diffusion as a model.

I guess I forgot how much basic ethnological theory has been lost by so many. In any 'theory course' with diachronic

problems in it the traditional dichotomy of **invention versus diffusion** remains valid. Oddly enough, historical linguistics is not confused at all on this point; nor is biogenetics or paleoanthropology. But archeology has allowed its children to believe that only **invention** counts, for isn't that what 'process' archeology is all about? (Plus high tech digs)

(7) While no single paper proved that the mummies spoke Tocharian or whatever, and while a great deal of evidence for an Iranian presence in Sinkiang surfaced, still we generally reached a consensus -- the initial conclusion that the mummies spoke Tocharian was probably true. And these Tochari had come from the west off the great steppes, had come from PIE, had greatly influenced ancient Turkic folk, living on in the modern Uighurs.

>> SOMEWHAT COOLER NEWS <<

SOUTH AMERINDS AND OCEANIANS

(AMH) reports on Marco Zago et al, 1995. "a-Globin Gene Haplotypes in South American Indians." HUMAN BIOLOGY 67:535. The principal findings are: "The similarities of a- and b-globin haplotypes between South American Indians and Southeast Asian and Oceanic populations suggest substantial genetic affinity between these populations and support the notion of a predominantly Asian origin of native Americans." [As we go to press, the editor has only just found out which genetic system is being discussed here. Many labs in Boston had never heard of it! Anyway a-globin pertains to nuclear DNA. Look it up on

MEDLINE EXPRESS if you have that or see journals **Nucleic-Acids-Res.** 1995 May 25; 23(10):1790-4 or **Gene.** 1995 Apr 24; 156(2): 277-81. Cutting edge stuff!]

(AMH) wonders if Zago et al (who include J.B.Clegg) have not got things backwards. He asks: "What about an Amerindian origin for Asians and an Asian dispersal for modern humans?" He knows his is a minority opinion!

POLYNESIAN MTDNA LINEAGE CLUSTER

(AMH) reports on J.Koji Lum et al (include Becky Cann), 1994 "Polynesian Mitochondrial DNAs Reveal Three Deep Maternal Lineage Clusters". HUMAN BIOLOGY 66: 567-590. (AMH) reports: "Major group I lineages are common in Remote Oceania and include about 95% of the Native Hawaiian, 90% of the Samoan, and 100% of the Tongan donors in our sample. They contain the region V deletion and generally share three control region transition substitutions. This group also contains non-Polynesian individuals, such as Indonesians, Native Americans, Micronesians, Malaysians, Japanese, and Chinese. . . ."

"Fully modern humans colonized all of Sahul, inhabiting Australia by 50,000 yr BP (Roberts et al 1990), New Guinea by 40,000 BP (Groube et al, 1986), New Ireland by 33,000 BP (Allen et al, 1988), and the Solomon Islands by 29,000 BP (Allen et al 1989). The strings of atolls and islands of Remote Oceania were apparently beyond the simple navigational skill of the first modern people. Increasing distance between islands corresponds to a long pause of almost 30,000 years, documented in the

archeological record, for the intentional spread of humans further across the ocean."

"The ancestors of Polynesians and the Lapita complex attributed to them appear to be the first human culture to develop a system of navigation and two-way sailing sufficient to ensure successful deep-water passage over thousands of miles. Some of these passages may have been the result of drift voyages, but computer simulation (Irwin 1992) and direct experimentation have refuted the likelihood that settlement was primarily accidental (Finney et al, 1989)"

"Their presumed route, based on archeological and linguistic evidence, appears to be from Mongoloid centers in East Asia south into Australo-Melanesia, then east across the Pacific."

"Lapita-associated skeletons from Mussau and Fiji are similar to Polynesians in nonmetric traits and are similar to Melanesians in skeletal dimensions (Kirch et al, 1989; Pietrusewsky 1989). Although direct gene flow is a possibility, the Lapita cultural complex could have been shared without mate exchange. The expansion of the Lapita people from the Bismarck Archipelago to western Polynesia is archeologically instantaneous (Kirch and Hunt 1988)."

"Finally, the South American sweet potato is found throughout Polynesia, raising the question of two-way voyaging from Polynesia or drift voyaging from the west coast of South America (Heyerdahl 1950; Yen 1974; Irwin 1989). The presence of flotsam from North America in Polynesia and demonstration rafting from South America indicate that some limited contact may have been possible between

eastern Polynesia & the Americas.

Linkage of the DRB1*1,5,0,2 and DRB5*0,1,0,1 alleles (Gao & Sarjeantson 1991) which are fixed in Papua New Guinea Highlanders and Polynesians but are absent in Chinese, suggests a New Guinea origin of Polynesian group II." (End of Lum et al & AHM report)

Ethnologically primitive, but interesting. Still Oceanian prehistoric studies have now reached a level above this paper and that includes much agreement on Austronesian language groups and their movements. The clear interaction with NAN or Non-Austronesian people along the north New Guinea coast + Melanesian islands should not be a mystery anymore and this significant truth should not be buried under 'Melanesian' a term which is now properly equivocal. See below where the term is extended to native Australians.

MORE POLYNESIAN TIES WESTWARD

(AMH) reports on Melton et al, 1995. "Polynesian Genetic Affinities with Southeast Asian Populations as Identified by mtDNA Analysis". AM.J.HUM.GENET. 57:404-409. (with M. Stoneking) (AMH)'s summary: "This 9-bp deletion is largely absent in Melanesian populations -- for example, aboriginal groups of Australia and highland Papua New Guinea (PNG) -- while it is present in coastal populations of PNG that are thought to be more recent arrivals to the island (Hertzberg et al 1989; Stoneking et al 1992). While the frequency of this deletion has been reported for many populations throughout Asia, the frequency alone does not reveal either the source of the deletion or the origin of Polynes-

ians."

"It is interesting that the six southern Indians with the 9-bp deletion found in this study share their mtDNA types most closely with those from China and Borneo, suggesting that migration from these regions west to India and Sri Lanka may be a possibility."

"We observed that the Polynesian motif, this trio of nucleotide changes in the control region at positions 16217, 16247, and 16261 (CGT), occurred exclusively on the background of the 9-bp deletion. This motif, seen in 79.2% of Samoans and 73.9% of coastal Papua New Guineans, was observed in 20% of east Indonesians with the 9-bp deletion. These east Indonesians were from the islands of Alor, Flores, Hiri, Ternate, and Timor. Remarkably, it was not observed elsewhere in Southeast Asia (including Borneo and Java in Indonesia), except in 1 of 81 Malays and probably 1 of 176 Filipinos." (End of Melton et al)

When will biogeneticists pay some attention to taxonomies other than their own? Three of the east Indonesian islands have Indo-Pacific languages on them (Timor, Alor, Ternate) and the other two are in the same area. That these should be connected to coastal New Guinea and the Polynesian route eastward is to be expected. Melton et al have found a special areal genetic trait, probably an initial mutation found in some Indo-Pacific peoples but not in highland New Guinea. Sans doute the insular NAN-folk have been distinct from Papuan highlanders for a long long time. OR the mutation occurred within the

ancestral Oceanic branch of the Eastern Malayo-Polynesian subphylum of Austronesian. One also wonders when and what will be found when mtDNA is taken from Andamanese and not-so-old fossil Tasmanians.

We also record that (AMH) believes the 9-bp deletion began among native Americans who bore it to Polynesia and beyond.

MAYBE NOT OUT OF AFRICA?

(AMH) reports L.B.Jorde et al, 1995. "Origins and Affinities of Modern Humans: A Comparison of Mitochondrial and Nuclear Genetic Data". AM.J.HUMAN GENETICS 57: 523-528. (includes T.Jenkins, AR Rogers & 7 others) This is not simply quibbling but has substance. (AMH) summarizes: "An evolutionary tree based on mtDNA displays deep African branches, indicating greater genetic diversity for African populations. This finding, which is consistent with previous mtDNA analyses, has been interpreted as evidence for an African origin of modern humans. Both sets of nuclear polymorphisms, as well as a third set of trinucleotide polymorphisms, are highly consistent with one another but fail to show deep branches for African populations. These results, which represent the first direct comparison of mtDNA and nuclear genetic data in major continental populations, undermine the genetic evidence for an African origin of modern humans. ..."

"Long branch lengths are seen for most of the African populations. This pattern has been observed in most other mtDNA analyses and has been a major component of the argument

for an African origin of modern humans [references deleted -HF]. The non-African populations have comparatively short branch lengths and the nodes separating these populations are very close to one other. . . ."

"The HVS-2 data analyzed here show a similar departure from neutrality in Asians and Europeans, although it is not statistically significant. These departures may reflect the action of natural selection, or they could be the results of past population expansions (Rogers & Harpending 1992). Since there is no recombination in the mitochondrial genome, natural selection on a coding gene will exert a substantial genetic 'hitchhiking' effect, even on polymorphisms in the non-coding D loop. It is thus possible that the differences seen here in mtDNA and nuclear DNA may be produced by natural selection rather than population history."

"Increased mtDNA diversity in Africans has been a linchpin of the argument that modern humans originated in Africa and then replaced existing archaic populations on other continents. Proponents of this view argue that since Africa is more diverse genetically, its population must be older (Stoneking 1993). However, diversity can be strongly affected by events in a population's history, such as the timing of major bottlenecks, and therefore does not necessarily reflect a population's age (Rogers & Jorde 1995). Our findings further compromise the diversity argument by showing that nuclear DNA trees lack the deep branches (and thus the excess genetic diversity) observed in mtDNA trees. These results do not disprove the African replac-

ement hypothesis. However, they weaken the genetic evidence in its favor." (End of Jorde et al)

In another summary (AMH) gives this lively & quotable thought: "Eve is from Kansas!"

The reader is referred back to Tishkoff et al (above ↑) who basically refute or at least rebut Jorde et al.

THE PARAMETERS OF CAUCASOIDS

(AMH) reports Antonio Torroni et al (incl DC Wallace), 1994. "mtDNA and the Origin of Caucasians: Identification of Ancient Caucasian-specific Haplogroups, One of Which is Prone to a Recurrent Somatic Duplication in the D-Loop Region".

AM.J.HUM.GENETICS 55: 760-776.

"Consequently, though human evolution is brief, a large number of mtDNA variants distinguish the major racial groups, yielding powerful genetic markers for inferring the ethnic background of human subjects. For instance, 70%-100% of the mtDNAs from sub-Saharan African populations belong to an mtDNA group defined by a HpaI site at nucleotide pair (np) 3592 . . . which is found at very low frequencies outside Africa and only in Caucasian populations that historically have admixed with Africans . . . Approximately 60% of Asian Mongoloid mtDNAs have an AluI site at np 10397 . . . which is absent in Africans and Europeans."

"Parsimony analysis suggest that all Caucasian mtDNAs are grouped into two major lineages . . . distinguished by the presence or absence of a DdeI site at np 10394. The NJ tree provides a less clear-cut subdivision of the haplotypes into these two lineages. However, it maintains the clustering of the

large majority of the haplotypes lacking the 10394 DdeI site. The 10394 DdeI site is found in 26.3% of the Caucasian mtDNAs, a lower frequency than either the 66% observed in Asians . . or the 91% observed in sub-Saharan Africans . . This polymorphism is probably very ancient, since it also subdivides Mongoloid . . and African phylogenies into two major lineages."

"All Native American mtDNAs belong to one of only four haplogroups defined by a HaeIII site at np 663, a 9-bp deletion in the COII^{trNA}_{LYS} intergenic region, an AluI site at np 13262 or the absence of an AluI site at np 5176 . . These Native American mutations and the associated haplotypes have never been observed in Africans and Caucasians but are found in northeastern Asia and Siberia, from which the ancestral Native Americans derived . ."

"Our survey revealed that 64% of European mtDNAs fell into four Haplogroups: H-K. Since these haplogroups are characterized by Caucasian-specific mutations, they probably originated after ancestral Caucasians separated from the ancestors of modern Asians and Africans. Hence the ages of these haplogroups could provide lower and upper limits to the age of modern Europeans." (End of Torroni et al). [Note: all references are deleted. See the source]

NEW PRIMATE FROM CHINA

This news is very recent but with weak relevance to our main concerns. (Source: K. Christopher Beard, et al. April 1996. "Earliest Complete Dentition of an Anthropoid Primate

from the Late Middle Eocene of Shanxi Province, China".

SCIENCE 272: 83-85.) Their abstract reads:

"The complete lower dentition of a new species of the basal anthropoid genus *Eosimias* shows a combination of primitive and derived traits unknown in other living or fossil primates. Although certain dental traits are decidedly more primitive in *Eosimias* than in other basal anthropoids, numerous derived aspects of jaw and dental morphology support the anthropoid affinities of *Eosimiidae*. *Eosimiids* document an early structural phase of the evolution of higher primates. Phylogenies that derive early anthropoids from cercamoniine adapiforms are inconsistent with *eosimiid* anatomy. Because early fossil anthropoids are known from both Asia and Africa, the fossil record is presently insufficient to specify the continent on which this clade originated." [End of Abstract]

NEW AUSTRALOPITHECUS FROM CHAD

The range and number of species of *Australopithecus* was increased considerably by the discovery of a jaw in Chad. The jaw was found last year by Dr. Michel Brunet (U/Poitiers) in central Chad Republic and named *Australopithecus bahrelghazali* after the province in Chad. The name is somewhat confusing, since the main river system -- Bahr el Ghazal -- lies much further east and south of this.

The jaw's owner lived from 3mya to 3.5mya, making it one of the oldest *Australopithecines*. The teeth were described by Yves Coppens as a "combination of the

evolved human trait of molars with the three-root teeth typical of chimpanzees and other apes."

David Pilbeam added that this one also had fair-sized canines. And that its presence in Chad showed a characteristic of normal wide-ranging animals, speciation in a large region. No one mentioned any peculiar traits that would suggest that this was close to the line that developed *Homo habilis* and us. (Sources: THE BOSTON GLOBE, May 21, 1996, p.2. And David Pilbeam personal communication. He also had remarks in NATURE in January.

THE EMERGING SYNTHESIS: WE ERRED

Shucks, we got caught by a historian of science! It seems, says Roger Wescott, that our felicitous term 'emerging synthesis' is not so new and avant garde after all. We borrowed it from Colin Renfrew who may have had a disclaimer about its history in his article. In any case the term was used in the 1940s by theorists of the new synthetic theory of evolution. It is not quite appropriate for us to use because of the heavy freight of prior meaning attached to it. Our 'emerging synthesis' means only the collaboration of three disciplines pursuing a joint goal.

We therefore abandon the label 'emerging synthesis' right now. Forthwith. While we hope our members will suggest a term of our own to replace it, we propose a new label -- just for now -- until a better one can be found. Our proposal = *Mutter-sprachwissenschaft*, for short, or *Metraglottogonics*. We took part of that from Eric de Grolier's original "Glottogonics", his term for language

origins study. We added on Greek 'metra' (womb) (Latin 'matrix') just to see if anybody salutes it.

FURIOUS TALK ON THE INTERNET

We do not intend ever to try summarizing events on the ever-expanding electronic avenues of communication. Much of it is a waste of time. Some of the problems were reviewed by Joe Pia years before 'e-mail' & 'Web sites' became buzz words and the terrible social pressure to 'get with it' became apparent. More than half of our members did reject, and still reject, the opportunity to chat electronically with half the world.

But we do have a place on WWWeb where people can read the newsletter and we have an e-mail address for those who wish to write to us. All this courtesy of two nice young women who did pity the father's backwardness.

Yet some members have been alarmed by recent Internet skirmishes among linguists. Quite bitter exchanges, eventually become hateful in one case, have caused some sectors to shut down and cries of protest from other linguists to be 'heard'.

Some long rangers were engaged: Allan Bomhard was heavily attacked by Alexis M-R, while LV Hayes had a long polite exchange with Sasha Vovin over the validity of Paul Benedict's Austro-Thai and other matters. Patrick Ryan took on Alexis and Larry Trask in a debate that became so furious that Patrick was told to refrain from hyper-aggressive remarks or be barred from the sector ('list'). Or so I have been informed.

What has been striking on the Internet for some time now is the passive acceptance of the

sacred **Comparative Method** by all. It is used like a club by conservatives to beat long rangers into line and used like a shield by long rangers to show that they really are orthodox and not wild radicals. C.M.? It goes unquestioned, nicht wahr? So either there are a lot of timorous long rangers who fear saying unorthodox things or they really believe themselves that all questions relate to **reconstruction** and therefore the sacred C.M., rather than **taxonomy**, and cannot be conceived in any manner other than via Indo-European orthodoxy. In other words by people who haven't done any significant taxonomy in many many moons. Why don't they, hmm?

We have been nattering on about these issues for almost a decade now but apparently few long rangers, so-called, have heard what the core long rangers have said. So I will repeat just one key point. **Indo-baloney** will never ever get us back to Mother Tongue, so there is no point in worshipping it. If you never want to transcend 10,000 years, then stick with Indo-baloney. If you want to get back to Mother Tongue, then you ought to follow the **taxonomy** first scholars. Period.

Wait! one may say. What about Nostratic? It is probably at least 20 kya and yet was made by Indo-baloney devotees, e.g., Muscovites + Bomhard. Surely it is looking very strong? Yes, it was and yes, it does. Even though it had been discovered several times by taxonomic types, the major work was done by believers in Indo-baloney like Dolgopolsky, I-S, and Bomhard. No doubt some of them

applied the Comparative Method successfully in searching for additional cognates. However, it is unlikely that the original sets of etymologies were obtained by reconstruction-driven approaches; rather much simpler comparisons of words and grammemes found in a number of languages gave birth to the etymologies to which the C.M. could **then** be applied. One must not entirely believe the Nostraticists in their protestations of orthodoxy. Those are their shields!

Moreover in some etymologies C.M. reconstruction methods lead to distortions in the etymologies: (1) based on faulty or premature proto-forms and (2) excessive reliance on happenstance historical work, e.g., the grossly excessive use of Semitic forms in Afrasian to the neglect of **most** of Afrasian. Even though (1) is a greater problem in Dene-Caucasic than Nostratic, still many starred forms in etymologies are quite questionable (to be polite).

It is also necessary to point out **again** that the so-called rigorous sound laws are no better than the etymologies upon which they are based. As they say, "garbage in, garbage out".

There is also the usual linguist's cry for greater and greater **precision, rigor, etc.** These cries seem to be part of the anality resident in the culture of linguistics, derived from 19th century Prussian militarism. Right? At least no one questions these **values** but no one seems to say why they are more important than anything else. (We could say more here!)

DELAYED REBUTTALS TO TRASK'S CRITIQUE OF BASQUE contra MACRO-CAUCASIC
Preliminary Note: We maintain our use of **Caucasic** as a word for the great phylum of languages spoken north of Kartvelian in the Caucasus. There is no other term that does not get confused with 'Caucasian' as used in biogenetics for both 'white people' in general and peoples of the Caucasus in particular; and the same goes for newspapers & novels. The Russian specialists on the Caucasus ought not dictate such a term as Caucasian to English, even if they do know more about the Caucasus. Their own Russian word is more like **Caucasic**!

For the record: the short statements which follow below cannot be understood without reference to MT-1: THE JOURNAL, Dec.1995, because the entire critique of Basque as a Macro-Caucasic language is found there, including John Bengtson's first reply to Trask, Merritt Ruhlen the same, and Larry Trask's long rebuttal to them and others. However, the letter from Sergei Nikolaev is his first comment on Trask's critique and also the only response yet forthcoming from the Muscovites. We wish to stress that Nikolaev's letter was written in Russian, thus enabling him to express himself most fully in his native code without the troubles given by an alien one. We wish to thank **Mary Sibbalds** (Rockport, MA), a retired US Foreign Service person, and **Merritt Ruhlen**, well-known to all, for their hard work in translating Nikolaev's letter. It was not thankless work because we are thanking them now. But it was unpaid! If Nikolaev's comments seem a bit murky at times in English, it was also the case in the original Russian, say the translators. We note also that S.N.'s letter took 3 months to reach us! Translators' footnotes mostly are omitted, save a few.

- - - - -

Moscow, 2 September 1995
Dear Hal!

Today I received your 'circular letter' regarding discussions between Bengtson and Trask, and decided (despite some reluctance, the reason for which is explained below) to say a few words about the topic, which in and of itself is important (especially in perspective) but which for now needs nursemaids more than bodyguards.

The fact is that linguistic comparison (when a linguistic relationship is assumed) is comprised of two factors¹. 1) claim to discovery: a demonstration of general consensus for form's sake which must win over the scientific public to the problem proposed (I'm sure there's gold here - let's dig!); 2) evidence of the relationship by means of classical application (in the sense of being tested by time and brains) as opposed to historical procedures which necessarily demand comparison by means of phased-in reconstructions (as would be the case if Indoeuropean were based on direct comparison of Hindi, Albanian and English in oral form!).

If we move to a Sino-Caucasic relationship (or if one likes, "Deno-

¹ Literally "views", says translator.

Caucasic": This is my occasional facetious term, so to speak, in memory of the Swadesh's bizarre "Dene-Finnish" theory, but it caught Shevoroshkin's fancy; I prefer the term "Sino-Caucasic". Thus:

1. **Sufficient verifiable North Caucasian Material** - the same with Eastern Caucasian, Western Caucasian = the Abkhazian-Adygei language is completely destroyed, and if it is possible to share with them, then only sign² their data to the Eastern Caucasian. There is a great deal of material on the North Caucasian; phonetically it is archaic, reconstruction is in general not bad (although I, like an 'ancient', see in it a pile of raisins) as far as suitability for external comparison is concerned. But of course there is no benefit from "exclusive" comparison of such languages as Abkhaz/Adygei (especially without the reconstruction of their phrases³), with the languages of the Basque, Burushaski, Athapaskan, Etruscan, and other isolated peoples? whatever their relationship with Caucasian languages.

Slava Chirikba in his time simply compared head on the native Abkhaz language (Abkhaz-Adygei reconstruction did not exist) with the Basque which was very little known to him; the result was nonsense. The elder generation were really taken with him (Chirikba), naturally having in mind general Caucasian "phonics"⁴ and reconstruction. I have no doubt that something exists in common (most likely a kinship) between the Basque and Sino-Caucasic languages, but to determine the place of Basque within Sino-Caucasic (or possibly "para-Sino-Caucasic") family is simply impossible and discussions (based) on a contemporary level of knowledge is totally absurd: what if we were to begin to struggle with the hierarchical relations within the Nostratic family, when all that remained were languages of the Finno-Ugric family, Semitic, French Creole on some atoll, Korean and Somali? This also applies equally to a Sino-Caucasic-Burushaski comparison: yes, the cited material most probably speaks in favor of a genetic relationship (distant? -- we cannot even measure the distance: there are no etymologies, no glottochronology!), but the true evidence to add Burushaski directly into this macrofamily is lacking and, I fear, for the time being will remain so.

Where even is the reconstruction of Basque? (there are Basque dialects, as well as early Latin loans, and in the Ibero-Romance

² "assign" would make more sense but it is not a meaning of the word as far as I know. Translator's note.

³ I assume this is "phrases" although it [the Russian word - HF] is not in my dictionary. The written correction [in the Russian original -HF] is his. Translator's note.

⁴ I assume this is "phonics" although it is not in my dictionary. Translator's note.

languages there are many old borrowings from Basque and other extinct "Iberian" languages -- it is possible, you see, to "archaicize" the Basque material!). It seems to me that the relationship of Basque to Sino-Caucasic (or its admission into this macrofamily) will, all the same, one day be possible to prove.

2. **The Sino-Tibetan material is vast** (but on the whole less archaic than Caucasian, although who knows it all?). **To prove a Sino-Caucasic relationship one must be fully prepared** since there are such excellent "units of comparison" as Starostin's reconstruction of Old (Proto-) Chinese, written Tibetan, quite archaic "mountain" languages of the Burmese-Lolo group, etc. Unfortunately, Ilya Peyros and Starostin (and not just for these reasons) did not have the time to complete their Sino-Tibetan reconstruction, but even that which is completed is sufficient for external comparison. **The hypothesis of a relationship between the North Caucasian and Sino-Tibetan families was proved on the basis of enormous materials, including the establishment and publication of regular sound correspondences.**

3. Starostin's Yeniseian reconstruction is completely correct, even though many data are lacking. For all that **the Sino-Yeniseian-Caucasic comparison of Starostin seems to me methodologically and factually irreproachable** (I'm not talking about a few debatable lexical and phonological correspondences and their interpretation).

4. **The reconstruction of Proto-Na-Dene** (whether Haida is inside or beside it I don't know -- just a month ago I received Dimy Leshchiner's dictionary of this language) is not nearly at the same level: I did what I could on the basis of scanty materials, but since then I have not returned to it: I didn't have the time because of my main concerns -- Indo-European, especially accentology, sometimes Caucasian, and my Amerindian studies -- all in all Proto-Na-Dene is just a scientific hobby or relaxation. In spite of all this the Dene-Caucasic relationship is obvious to me. It has been shown ("announced") rather than proved, although I have no doubts that Na-Dene is to be included in the Sino- ("Dene-") Caucasian macrofamily. **The Sino-Caucasic relationship is already for a long time not just an hypothesis: all the objections come either from the "principles" of the sceptics, or -- and this is the majority -- from not having access to the primary data on the languages of the peoples. It is for this reason that Starostin and I brought out our COMPARATIVE DICTIONARY OF NORTH CAUCASIAN LANGUAGES in English, so that we can play this game with our cards on the table, face up. However, the "Dene-Caucasic" relationship is not proved, but an hypothesis. Whether I will prove this hypothesis, or someone else, it will be done in time, but for the moment there is a lack of necessary materials. . .⁵**

⁵ Editor's Note: it may be that the 'necessary materials' are lacking only in Moscow. It behooves us as a collectivity to shower lots of data on this creative, hard-working Muscovite! If thou hast solid data (e.g., good reconstructions of branches), send him them.

So consultation regarding "accidents" with Bengtson and Trask, unfortunately is doubtful: Some say the patient lives, others that he is dead, but they can't prove it to each other because they don't see the patients at all. To show the relationship (attractively enough) is possible for many distant languages: for Algonquin (and other native American languages) with Chukchi-Kamchatkan and Nivx [Gilyak -HF] (which we did with Oleg Mudrak), for Amerind, for many African (Greenberg) but for **proof** one needs to **work**: collect sufficient relative material (not ten but One hundred comparisons), to establish regular conformity,⁶ etc. Unfortunately -- alas! Both Bengtson's results and Chirikba's results (like our comparison done with Mudrak and to a large extent my "Dene-Caucasic") all are only interesting claims. When I recorded Russian dialect material in one village at an old woman's home, she kept asking me "why do you need this -- for practical use⁷ or to break a head? If it was for use, it was fine with her -- with me also. But to break a head over dozens of resemblances?? half of which clearly play the role of "filler" is of course interesting but of doubtful promise in the long run and even **unprofessional**.

Regards! /privet/!

Sergei Nikolaev

[If one has data to send Sergei, contact Bomhard, Bengtson, Ruhlen or Shevoroshkin to get Starostin's e-mail address = quick access to Nikolaev. Go send the stuff!]

REPLY to TRASK

by Merritt Ruhlen,
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In my original critique of Trask's "demolition" of Dene-Caucasian I made a few simple points that seemed to me to call into serious question Trask's conclusion that Basque is an isolate -- a language without perceptible relatives. First, Trask's capricious and arbitrary decisions to ignore evidence from two of Dene-Caucasian's six branches -- Sino-Tibetan and Na-Dene -- rendered his paper not an appraisal of Dene-Caucasian, as the title of his paper seemed to promise, but simply a judgment of the evidence connecting Basque with three other families -- Caucasian, Burushaski, and Yeniseian. Second, in some cases the evidence from one of the two omitted branches is strongly

⁶ Editor's note: the thought occurs that real long rangers, i.e., those apt to make taxonomic advances, habitually sift through very large amounts of data. Put another way, it seems that in this field being gifted usually means **knowing** a lot, rather than being mathematically or analytically very quick. Maybe short rangers use their brilliance for theoretical advances, while long rangers pan vast areas for gold (nuggets).

⁷ In the Russian 'dela' literally means 'business' or 'matter'. Translator's note.

supportive of an etymology (e.g., Basque *odol* 'blood' and Proto-Athabaskan *dehl*⁸ 'blood', and possibly Burushaski *del* 'oil, contents of an egg'). Third, even if one were to eliminate all the Basque evidence to which Trask objects -- which would not be a wise decision, as Bengtson has shown -- there is still abundant evidence connecting Basque with these particular families rather than with others. Fourth, if Basque is not more closely related to the other members of Dene-Caucasian, then one should be able to come up with equal evidence for the Austro-Basque hypothesis, which would connect Basque with Australian, Khoisan, Gilyak, Algonquian, and Quechuan. It is interesting to see how Trask addresses each of these criticisms -- or rather fails to address them.

(1) Despite the inaccurate title of his paper, Trask now pleads *ex post facto* that he ran out of space for Sino-Tibetan and Na-Dene! Just poor planning, I guess, but it did entail ignoring the data on these two families that were contained in the very papers he was criticizing.⁹

(2) Furthermore, according to Trask, these additional families cannot save etymologies that have already been dismissed on other grounds. But what about Proto-Athabaskan **dehl* 'blood'? For Trask, this resemblance between Basque and Athabaskan is just an accidental coincidence, of the kind one supposedly finds between any two languages. So too apparently would be the similarity between Basque *gose* 'hungry', Proto-Caucasian **ggaši*¹⁰ 'hunger', Hruso (Tibeto-Burman) *k^hussī* 'hungry', and Galice (Athabaskan) *gas* 'become hungry'. So too the fact that Basque just happens to share the Dene-Caucasian interrogative pronouns in N and S. And so on. The question Trask never asks is why all of these "accidental" resemblances constantly fall among the **same set of families**, rather than a different set of families.

(3) With regard to the 68 etymologies that I cited as surviving Trask's demolition, Trask is silent. No doubt they are all just accidents. In fact it is clear that any proposed Basque comparison that cannot be dismissed on other grounds will be eliminated by appeal to accidental resemblance. In a bizarre twist on normal comparative linguistics, accidental resemblance, rather than evolution from a common source, is taken as the default explanation, a practice that has been aptly ridiculed by Vince Sarich (1994).

⁸ [hl] is the lateral fricative, written as something close to [ɬ] in the original, i.e., an [l] with a belt on.

⁹ Editor's note: although it was unprecedented in most journals, MT-1 placed no limits on the size of Trask's contribution. In terms of journal pages, including his rebuttal, Trask was allowed 110 pages or 55% of an article shared with 12 other people or 46% of the entire Journal. He has acknowledged our generosity.

¹⁰ The original had a [g] with a line over it. This is interpreted as a familiar Caucasian sign for a strong or geminate consonant, 'fortis' according to Ian Catford. Since this computer cannot write the original, we write an ordinary double consonant.

(4) Trask assures us that he is "confident" that he could, if he were so inclined, come up with evidence for Austro-Basque equally as good as that which has been offered for Dene-Caucasian. This is reminiscent of Goddard and Campbell's claims that the M/T 'I,you'¹¹ pronoun pattern is just as widespread as the N/M pattern in the Americas. When asked to produce their evidence for this claim, neither Goddard nor Campbell could come up with any, in stark contrast to the evidence I presented for the N/M pattern in Amerind. This great pronoun hoax turned out to be little more than bluff and bluster (Ruhlen 1995). Surely no one should consider the Austro-Basque hypothesis any differently. Science does not involve weighing what scholars from Trombetti to Bengtson **have done** with what Trask says he **could do** if he were so inclined.

In the final analysis, Trask's defense of splendid isolation for Basque is the work more of a lawyer than a scientist. Like O.J.'s lawyers he has chosen to defend the indefensible, and, like O.J.'s lawyers, he has to manufacture evidence to buttress his arguments, while dismissing the DNA evidence as accidental. The manufactured evidence is the following: On page 189 of his rebuttal he asserts that "a fine example of this [i.e., the importance of considering a wider context] is Basque **gorotz** 'dung', which is explicitly singled out by Ruhlen as a case in which the data from Sino-Tibetan and Na-Dene would prop up the comparison. But . . . it is hardly likely that **gorotz** is a native Basque word. And, if the Basque word is borrowed from Romance, who cares what the Tibetan or Apache words for 'dung' might look like?" The reader who bothers to go back to my paper will find (page 154) that no evidence from Tibetan or Apache is adduced, explicit or otherwise. In fact, this triconsonantal root -- apparently restricted to Basque, Caucasian, and Burushaski -- is a strong piece of evidence in favor of Macro-Caucasian, Bengtson's proposed subgrouping of these three families within the larger Dene-Caucasian complex. I have recently argued that Yeniseian and Na-Dene should also be grouped together as another branch of Dene-Caucasian (Ruhlen 1996). The third branch of Dene-Caucasian would then be Sino-Tibetan.¹²

¹¹ Editor's note: One would prefer to call this 'I/thou', since 'you' remains ambivalent and 'thou' is the proper cognate. 'Thou' apparently still survives in one village in New Hampshire.

¹² Editor's note: always on the look-out for taxonomic change, whether external or internal, we see here that one of the principal champions of Vasco-Dene has proposed an interesting sub-division. Assuming that these three sub-phyla (A, B, C) are equidistant, in a taxonomic sense, then the greater likelihood is that B is closer to the homeland than the other two. Since A is western Eurasia, B is eastern Asia, and C is Siberia-Canada (basically), then eastern Asia is a bit more likely as a dispersal area. If, however, the whole lot (A B C) is derived from Borean, then group B originally

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A FINAL (?) RESPONSE TO THE BASQUE DEBATE IN MOTHER TONGUE-1

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[Editor's Prefatory Note: it is characteristic of the group of languages, here called Dene-Caucasic, that they contain some of the most exotic or 'harsh' consonants on Earth. Caucasian by itself has already strained international transcription conventions maximally; proto-Caucasic with its proposed 180 consonants is simply over the hill, outside the bounds, or what-shall-we-call-it. For the purposes of the Internet and our WWW site most of proto-Caucasic is out-of-sight. If someone thinks that Na-Dene is relatively easy, she should look hard at Haida, Tlingit and many other Northwest Coast languages! We simply cannot share the exotica with you the same way in which we can simply zerox the strange transcriptions to send you a copy. Not on the Web! What to do about this?

We reach a compromise by telling you what the exotic sound is similar to. But sometimes we do not even know what articulatory principles underlie a symbol used. So we describe it -- literally. So, for example, in the first paragraph we write [gh] to show that the sound is like a voiced velar fricative. Then we put a ^ next to it to show that the original had a dot over a Greek gamma sign.

For all this we use footnotes. As sparingly as possible. We do also beg writers on Dene-Caucasic to show a little mercy and give us a script which vastly improves on Starostin & Nikolaev's incredible complexity. You cannot imagine what will happen when we come to put much Khoisan on the Web. Caucasian will look relatively simple! -- HF]

Shed No Tears -- for the Vasco-Caucasic Hypothesis

After all the dust has settled, what is the upshot of the "Great Basque Debate" in the first issue of Mother Tongue (the Journal)? In our sports-minded world, the first crude question would be 'who won?' If the objective was to prove that paleo-linguists like me, Chirikba, and others have made mistakes in comparing Basque with other languages, Trask and the other vasconists have clearly "won".

If the objective was to disprove or "destroy" the Dene-Caucasic

entered China from the west, quite possibly pioneering the trail that Tocharian later followed. Speculation is a fun sport!

and Vasco-Caucasic (Macro-Caucasic) hypotheses, I believe Trask has lost that battle. A prominent historical linguist recently wrote me (March 13, 1996) that "I believe you came out very well in the exchange with Trask and the others . . . I am accustomed to attacks by specialists who find a few real errors, a number of imaginary errors and never ask what's left over." Some of the other respondents asked the same question: even assuming that all of Trask's and Jacobsen's objections are correct (which we do not), how can they totally overlook the comparisons that are left over?

As an example, I will discuss one comparison here that, as far as I can see, was passed over without comment by Trask (p.55) and the other vasconists. It begins with a Basque word, *negar* ~ *nigar*, that can be generally translated as 'weeping, tears', Spanish 'llanto', French 'pleurs, larmes' according to Azkue (1905). The *e* form (*negar*) predominates in western dialects, and the *i* form (*nigar*) in eastern dialects. One Bizkaian community (Ubidea) also has the odd meaning 'rennet / cuajo / présure', and there is a curiously similar word *negal* ~ *negel* ~ *negelar* (in some eastern dialects) with the meaning 'rash, scurf, herpes, skin eruption' ('sarpullido, herpe, erupción de la cara/dartre, herpès, éruption de la peau' in Azkue).

As far as I know, Trombetti (1925: 249) was the first to compare these Basque words with similar Caucasian words for 'tear' (or 'pus'), found in every branch except Khinalug and West Caucasian (NCED 848-49). Some of the words have initial *n*-: Dargwa *nergh^hw* 'tear', Lezgi *nagh^hw* Tabasaran *niwq* / *niwgh^h*, Archi *nabq*, (oblique) *nibqi-* (all 'tear'), Chechen *not'q'a* 'pus', Batsbi *not'q'* 'pus' (but plural *nat'q'ajrī* 'tears'¹³); some have initial *m*-: Avar *mā9u* 'tear'¹⁴, Akhwakh *maq'a*, Lak *maq'*, and others. Nikolaev and Starostin reconstruct a protoform with initial *n*-: **nēwq'ū* (from which the *m*- forms derive by anticipatory assimilation). They also propose that this **nēwq'ū* had an ablaut variant in the oblique form reconstructed as **niwq'V-*, reflecting an alternation which is preserved in a few languages, e.g., Bezhta *maq'o* / *miq'a-* 'tear', and possibly Archi *nabq* / *nibqi-* 'tear'. This Caucasian *e/i* ablaut reminds us naturally of the *e/i* alternation in Basque, though in the latter the morphological alternation was apparently generalized as dialectal variation.

Thirdly, let us look at some interesting words in Burushaski, recorded by Colonel Lorimer (1935-38) as *nāgei* 'a boil'¹⁵ (i.e., a septic skin eruption), which has the variant *māgei* in the Nagir dialect. The *n/m* alternation reminds us of the Caucasian words, and the

¹³ The final vowel /i/ is written as [i] with a wedgie over it in the original.

¹⁴ The sign for a voiced pharyngeal, looking like a backwards [?], we write as /9/.

¹⁵ First vowel in the original is an upside down [v], a usual British rendering for the 'u' in English 'but'; we write as [â].

meaning calls to mind the meaning in Chechen, Ingush, and Batsbi ('pus'), and also Basque *negal* 'rash' etc., another form of eruption, apparently seen by the ancients as the skin 'weeping'. (This would also explain the Ubidea Basque *negar* 'rennet' [above], seen as 'tears' of gastric juice excreted in the fourth stomach of a calf). So the Burushaski words are fully in the Basque-Caucasic tradition, except that the meaning 'tear' (of the eye) has been lost to the gain of 'pus' > 'boil'. Though I have not found this word in any of the other Buushaski dictionaries (Berger, Morin, Pesot, Tiffou, Zarubin), I suspect a more phonemic transcription might be /nagé/ ~ /magé/ 'boil', which can be derived from Proto-Burushic *nagwé 'pus > boil', with regressive labialization in the Nagir dialect, exactly as in Lak, Bezhta, etc.

But we still have a "leftover element" -- the -r in Basque. Here let me recall the Dargwa *nergh^w 'tear', realized as *nergh^* (uvular gh^) ~ *nirgh^*, and even *mergh^* ~ *mēgh^* in the diverse dialects. Nikolaev and Starostin, without any reference to Basque, propose that the Dargwa form probably comes from "an original plural form in *-r," thus *nergh^w* from **negh^w-r* 'tears'. Thus, I suggest, we now have an explanation for the whole Basque word:

Basque *niga-r* ~ *nega-r* 'tears'
= Pre-Proto-Dargwa **negh^w-r* 'tears'

The old plural -r in Caucasian is preserved in some languages, such as Hunzib: *koma* 'kidney', plural *koma-r*, *âp'â* 'paw', plural *âp'â-r*, and elsewhere only in fossilized remnants, as in the Dargwa word for 'tear(s)', and some of the Lezgian words for 'ear': Rutul *ubur*, Kryz *ibîr*, Budukh *ibir* 'ear' < '*ears' (NCED 240); and Khinalug *culoz* 'tooth' < **cul-or* 'teeth'¹⁶ (NCED 326). There are also a number of Basque words with a "leftover element" when compared with Caucasian, e.g.:

Basque <i>an-tziga-r</i> 'frost'	Caucasic *3'igV 'hail, rain' ¹⁷ (NCED 1102)
Basque <i>hama-r</i> 'ten' (from '*handfuls')	Lezgian *XXamm 'handful' (NCED 819)
Basque <i>bulha-r</i> 'chest, breast, mother's milk'	Caucasic *GwâlHe 'udder, breast' ¹⁸ (NCED 376)
Basque <i>ziga-r</i> 'mite'	Caucasic *ċ'ääkwê 'biting insect' ¹⁹

¹⁶ The editor suspects that **culo-r* would be more convincing. In Kryz the [î] stands for 'barred i' or high mid unrounded vowel.

¹⁷ Our [3'] represents the original's [ž] but with a tail on it, giving the appearance of a [3] partially sub-script. This is usually read as [zh] as in French 'je' but the [''] modifies that.

¹⁸ Our [H] represents a voiceless pharyngeal fricative which John writes as an [h] with a right-leaning flag on top.

¹⁹ Our [ää] represents an [a] with two dots over it plus a semi-circle over that. The final vowel [ê] represents a 'schwa' with a semi-circle over it. Annoying phonetic hyper-precision!

	(NCED 376)
Basque e-lhu-r 'snow'	Caucasic *hliwV : Chechen lō 'snow'
	(NCED 684)

I suggest that at least some of these "leftover r"s could be fossilized remnants of an old plural form that lost productivity in early Basque. This hypothesis helps solve not only the 'tears' etymology, but also several others. All of these words are consistent with a plural or collective meaning, e.g., '*snowflakes > snow', '*handfuls > 10', etc.

So in sum, we have :

Basque nega-r ~ niga-r 'tears, weeping' (? nega-l 'rash, scurf')

Caucasic *nēwq'ŭ / *niwq'V- 'tear(s) ~ pus'

Dargwa nergh^, Lezgi nagh^w, Bezhta maq'o

Burushaski nagé ~ magé 'boil < pus'

This is an etymology that explains the whole Basque word, tying together lexicon and morphology (cf. the Basque-Dargwa comparison, above). I have also shown that the words from all of these languages form a semantic continuum. As with many other of the strongest Vasco-Caucasic comparisons, the more facts we gather, the more the etymology is cross-confirmed.

Do you see now why all the vasconists totally ignore this etymology? Because it is a powerful witness to Vasco-Caucasic unity, they try to sweep it under the rug by not mentioning it. It is this kind of intentional neglect of promising evidence that shows Trask and some other vasconists do not proceed as scientists, interpreting facts in an objective manner.

As one of my colleagues reminded me, "science is not a football game" -- a debate is not decided by who writes the most pages, or how many scholars gang up against other scholars. It is ultimately decided by the interpretation that best explains the facts. I think we have given a good interpretation of the facts in this 'tear' etymology, and in many others that demonstrate and cross-confirm the original unity of Basque with Caucasian and Burushaski.

So I am not really interested in who is thought to "win" the Basque debate. If the reader is content to accept all of the phonological assumptions and etymologies put forward by Trask (who assumes a totally isolated language), by all means join him, and Jacobsen and Zabaltza, as they endlessly speculate (see "Pre-Scientific Etymology", below), with no possibility of external comparison.

But if the reader is interested in a scientific approach to the question, I suggest not taking Trask, Jacobsen, and Zabaltza's comments at face value. When they say things like "it is universally accepted among specialists," remember that this is irrelevant to the scientist. (It was once universally accepted that the earth was flat.) And when they make claims like "no native Basque word can begin with x-," bear in mind that any statement of this type is not fact, but hypothesis, as more moderate vasconists recognize (see Hualde's

comments in MT-1). When they confidently proclaim that "x is transparently a compound of y and z," recall that nothing can be taken for granted as "transparent" in historical linguistics (see "Pre-Scientific Etymology," below). And when they dismiss all Basque-Caucasic and Basque-Burushaski comparisons as mere "chance resemblances," yet accept virtually all Basque-Latin comparisons (see my "Is It Romance" in MT-1), it becomes clear that this is not objectivity, but ideology.

My debate with Trask has now continued for over a year, during which I have spent countless hours working on rebuttals, time I otherwise could have spent doing positive research and writing. I could have spent much more time, had I chosen to defend all the twelve-score comparisons I still consider worth defending (MT-1, p.94). As can be seen in the 'tear' etymology, a proper discussion of a comparison can take a page or two of space, or more. I never intended to write several hundred pages of rebutting arguments that all too often seem to me to be patently specious and even sophomoric, and furthermore, the time and space constraints of MT-1 did not permit this. Instead, I urge anyone who is interested in pursuing this discussion to (a) read carefully my responses to Trask (Bengtson 1994a, 1994b, 1994c, 1996; Blažek & Bengtson 1995), and (c) contact me personally, by conventional mail, e-mail, or telephone, to discuss general or specific questions.

So shed no tears for the Vasco-Caucasic hypothesis! In spite of Trask's negative efforts, it is alive and well. In fact, Trask has unwittingly helped us by eliminating some of our weakest evidence (in the few cases where we agree he is right), and also by information that in many cases actually strengthens our case. To that extent I am grateful for this debate, but it is time to move on. We are going to continue work on Basque and its Dene-Caucasic cousins, because we believe this is a viable, indeed a robust, hypothesis. The Splendid Isolation of Basque, like Stalinism in Eastern Europe and Apartheid in South Africa, is an idea whose time has run out. I believe it is only a matter of time (though perhaps not in my lifetime) before Vasco-Caucasic is as widely accepted as Sapir's Algic or Jones' Indo-European are now.

PRE-SCIENTIFIC ETYMOLOGY

You may recall reading about the state of etymological science in ancient times, when "a Roman could imagine that *vulpes* 'fox', genitive *vulp-is*, really was 'fly-foot', compounded of *volo* 'I fly' and *pes* 'foot', genitive *ped-is*; and that *lepus* 'hare' . . . was 'lightfoot', from *levis* 'light' and *pes* 'foot'. It did not occur to the Roman that the stems were entirely different in these words. And he would not have understood that there could be any objection to this procedure" (Pedersen 1962: 3-4).

Of course, this was before modern linguistic science, and the idea that external comparison with other languages, when analyzed correctly, leads to real etymologies. In this context I strongly question many of the etymological methods used by some of the vasconists, as evidence by the exchange in MT-1. In many ways they function at the pre-scientific stage. Thus when Dr. Zabaltza sees Basque *sabel* 'belly, stomach' and *gibel* 'liver', he proposes that the element *-bel* is the same as *bel-* in *beltz* 'black'. Of course, if you believe that

Basque is totally isolated (as Zabaltza, Trask, and Jacobsen do), there is no possibility of external comparison, and this kind of 'etymology' is all one can hope to propose. With those assumptions, the examples given by Zabaltza are perfectly logical, and these vasconists, like the ancients, do not understand "that there could be any objection to this procedure."

But Zabaltza's solution raises more questions than it answers. If -bel- really is a discrete element, what then are sa- (in *sabel* 'belly, stomach'), gi- (in *gibel* 'liver') and -tz (in *beltz* 'black')? This is left unanswered.

Similarly, when Trask analyzes Basque *ukondo* (~ *ukhondo*, *ukhundo*, *ukando*) 'elbow' as *uk(a)- 'arm' + *ondo* 'bottom', it all seems perfectly logical from the point of view that Basque is totally isolated. But again, more problems are raised. (See my discussion, p.87)

My procedure, on the other hand, and that of Cirikba, Blažek, et al., accepts that Basque is demonstrably related to other languages, and that comparison with them allows us to formulate real etymologies. Thus the solution for Basque *ukhondo* is to compare with other Dene-Caucasic languages, and most immediately we come across some Caucasian words that mean 'elbow' or 'knee' in the Lezgian and Tsezian languages. Note especially:

Basque *ukhondo* (Basse Navarre) 'elbow'

Lezgi *q'üntū-* (oblique form) 'elbow'

The meanings are identical, and phonetic forms easily correspond in a striking fashion. Any historical linguist who is ignorant of the 'correct' vasconist analysis would probably accept this as a plausible comparison, the more so since there are other basic parallels between Basque and the same languages (see my Table 1, MT-1, pp.96-97). But the vasconists tell us it cannot be right: they already have a pre-determined solution.

I shall not take the space to discuss the alternative etymologies for all the words discussed above, but as to *sabel* 'belly, stomach', I propose the segmentation *sabe-l* and connect it with words such as (Caucasic) Bezhta *šebo* 'liver', etc., Tibeto-Burman *čwap* 'lungs', and Yeniseian words of the type *tVp-Vl 'spleen' (Bengtson 1991b: 131), where we come to a possible parallel to Basque -el as well:

Basque *sabel* 'belly, stomach'

Kott *tebel'a* 'spleen'

I suggest that it is really the isolationist vasconists who "chop words up" arbitrarily. In most cases the Dene-Caucasic etymologies explain the whole word.

Whenever Trask uses the phrase "x is transparently a compound of y and z," I counter with "yes, just like crawfish, crayfish is transparently a compound of craw and fish," which would be a respectable etymology if we only had English material to draw from and we considered it isolated from all other languages. (In reality, we now know crayfish to be a folk-etymology reshaping of French *écrevisse*.)

For example, Trask confidently states that the second component of Basque *emakume* 'woman' is identical with (h)ume 'infant, young', (in spite of the phonetic and semantic obstacles), so that for Trask -

kume cannot possibly have anything to do with Caucasian words like Archi Xom 'woman', Lak qami 'women', etc. I simply offer an alternative solution, i.e., that comparison with Caucasian offers semantically and phonetically plausible parallels for the element -kume 'woman', and that Trask's analysis may well be a sophisticated folk-etymology, like English bridegroom, which is "transparently" a compound of bride and groom. (In fact the element groom was originally *guma, an old word for 'man'. Comparison with related languages, e.g., German Brautigam, Swedish Brudgum, confirms that the second r in bridegroom is secondary, and influenced by groom, of distinct origin.) Things that seem transparent at the synchronic level may not necessarily be transparent diachronically!

We are well aware that we often call traditional vasconist etymologies into question, and not out of ignorance. I have noted several disagreements with traditional etymologies in my responses to Trask. It should go without saying that when external comparison is correctly applied, at least some "universally accepted" vasconist etymologies will be found to be false. This is the natural outcome of scientific etymology.

Trask will no doubt stick with his segmentation of uk-ondo, and Zabaltza with his sa-bel, etc., and it is of course their right to cling to those pre-scientific ideas. I would hope that sensible historical linguists will see that external comparison combined with internal evidence can, and do, lead us to correct solutions in Basque etymology.

HALF EMPTY -- OR HALF FULL

As we follow Trask's arguments it becomes clear that Trask makes stringent demands on the pioneer paleolinguists: not only must they master all the languages they survey without making mistakes, they must also have every detail of the phonetic correspondences worked out, and have explained every grammatical feature in all the languages being compared. Anything short of this is "worthless", or "zero evidence", according to Trask.

It would indeed be wonderful if paleolinguists were specialists in all the languages they compared, and were so supremely talented that they could minutely analyze and explain every last detail of the proto-language (whether it be Dene-Caucasic, Nostratic, Austric, etc.) and its daughter languages. But in the world I live in (and Chirikba, and Blažek, etc.), one has to settle for mere mortals, who make some mistakes (not nearly as many as Trask alleges!), and do not yet claim to have all the answers about Dene-Caucasic.

One's overall assessment of the Basque debate may depend on whether one sees the Vasco-Caucasic and Dene-Caucasic hypotheses as half empty or half full. Trask and Jacobsen see only the errors and points that conflict with their models of proto-Basque, so for them the attempts to demonstrate Dene-Caucasic can only be thought of as half empty, therefore, "zero evidence".

On the other hand, if one can accept that scientific progress is made by pioneers who are also human beings, who by definition are exploring terra incognita, who make some mistakes along the way and learn from them, then perhaps one can look at our efforts to relate Basque to Dene-Caucasic as a matter of being half full.

Much of what the critics fault is found in the Dene-Caucasic work of six to twelve years ago. Since then we have learned a great deal about Basque as well as the other Dene-Caucasic languages. Our stock of Dene-Caucasic etymologies and paradigms is constantly growing as well as being corrected and amended.

For state-of-the-art Dene-Caucasic, I suggest taking a look at our latest publication, "Lexica Dene-Caucasica" (Blažek & Bengtson 1995, though mostly written in 1992-93). This article features the first synoptic table of provisional phonetic correspondences between and among Basque, Caucasian, Burushaski, Sino-Tibetan, Yeniseian, and Na-Dene, building on Starostin's (Sino-Caucasic) phonology. Of the 219 comparisons, 126 involve Basque. As always, we expect a certain number of these etymological proposals to be altered, corrected, conflated, split apart, and even "destroyed", as Trask puts it. But we believe most will stand. Dene-Caucasic has matured.

Table 1 : No Evidence at All?

meaning	Basque	A	B	C	D	E	F
'I'	ni	nu	-	-	na	-	-
'thou'	hi	Hu	Ho	ox	-	-	hu-n
'we'	gu	xxa	txo	ki-n	-	uxu	ja-n
'you-pl'	zu	šša	šu	zu-r	zu	uč ^v u	wa-n
'what?'	ze-r	se	stē(n)	-	-	-	-
'two'	bi	k'wi	-	k'u	k'i-a	q'u ²⁰	ppa ²¹
'fire'	su ²²	ts'a	ts'e	č'ä	ts'u	ts'a	a-
'hunger'	gose	gaši	-	-	kkaši	gaš	-

Here we have lined Basque up with six "mystery languages." The table is Basque-centric, and a dash (-) indicates that the language in question has a word (with a meaning in the left column) that is judged not comparable with the Basque word.

Note that the eight meanings are all highly basic, and among the most historically stable. If there is any realm of the Basque lexicon that is least likely to be subject to borrowing and neologisms, it is just these words. And if the mystery of the classification of Basque

²⁰ The [q'] has a wavy line, like a nasalization sign, under it. We've no idea what that means.

²¹ The original has [a] with a wavy line under it. This represents a pharyngealized vowel in Bengtson's system. Cf MT-1, p.102. There are a few errors on that page which we did not catch before and some of the assignments are arbitrary.

²² Reader is reminded that written Basque letters do not have precisely the same values as IPA symbols or ordinary expectations. Thus 's' = [š] an apico-alveolar fricative, while 'z' = a dorsal-alveolar fricative, not voiced as ordinary [z] is.

is ever going to be solved, it will be with these words, or with a few other sufficiently stable words (such as 'not', tear [lacruma], water, nit, hand, night': Bengtson 1994b).

Now just look at these 42 words as if they were cited from seven African or Southeast Asian languages. I think some historical linguists would waste little time in proposing that these seven languages belong to the same family, possibly even "an obvious family" on the basis of these eight words. It seems unlikely that such a tightly woven table could be drawn up for some accepted families, such as Afro-Asiatic or Nilo-Saharan.

Others, perhaps not so impetuous, might at least stop to ponder that there are quite a few basic resemblances here, and some are strikingly patterned, e.g., Basque *ni* : *hu* and language A *nu* : *Hu* with the same meanings 'I : thou'. Should this not, at the very least, be investigated further?

So why do so many linguists still believe that there is no answer to the genetic classification of Basque? I could mention at least two reasons, (a) the geographic argument, that Basque is "too far" from other languages being compared, and (b) the circumstance that one of the languages concerned (Basque) has a coterie of specialists, the *vasconists*, many of whom operate under the assumption that Basque is forever isolated, or at least that the linguistic relatives of Basque can never be detected.

The geographic argument, that Basque is located too far from a given language being compared, is the easiest to refute. We need only point out the geographic range of Indo-European, Austronesian, and other language families. Recall also the case of Algonquian, compared by Edward Sapir with the distant Wiyot and Yurok on the Pacific Coast, a relationship now accepted by even the most conservative Americanists

The 'mystery languages' are (A) Dargwa, (B) Chechen, (C) Khinalug, (D) Lak, (E) Tabasaran, and (F) Udi, all languages of the East Caucasian family, a family accepted by even the most conservative Caucasian specialists. The eight Caucasian etymologies are also generally accepted East Caucasian etymologies. The citations here follow Nikolaev & Starostin's NORTH CAUCASIAN ETYMOLOGICAL DICTIONARY (1994 = NCED).

To the isolationist ideology, the paleolinguists (Bengtson, Blažek, Chirikba, Ruhlen, Shevoroshkin, et al) respond that linguistic relatives of Basque can be found, and indeed have been found. Among these, we think, are the (North) Caucasian languages, as shown by abundant evidence, a small part of which appears in Table 1. This particular table is weighted (apart from Chechen) towards outlying languages of Eastern Daghestan: Lak, Dargwa, Khinalug, and the Lezgian outlier Udi (= "Albanian" of the Caucasus). These languages appear to retain some archaisms that coincide with Basque.

Note in particular the first four comparisons, where we see a rather high correlation between Basque and Caucasian personal pronouns: only Dargwa shares all four, but each language cited here has at least half of the four pronouns in common with Basque. The correspondences are sometimes patterned, as Basque *ni*, *hi* = Dargwa *nu*, *Hu*; Basque *gu*, *zu* = Tabasaran *uxu*, *uč'u*.

If these are just "isolated resemblances", as some vasconists are wont to claim, that will be borne out by the investigations of other basic vocabulary and morphology. As it turns out, Trombetti, Uhlenbeck, Bouda, Tailleur, Lafon, Chirikba, Blažek, and I have conducted such investigations, and have found that the more we study the Vasco-Caucasic (Macro-Caucasic) hypothesis the more it is confirmed. The latest publication (Blažek & Bengtson 1995) on this theme lists 126 lexical and grammatical comparisons connecting Basque with Caucasian and other Dene-Caucasic languages. How some vasconists can claim that all this is "zero evidence" and "insignificant" is difficult to understand, when even the eight-word table is strongly indicative. Note that the Basque words are all acknowledged by vasconists to be native, and that Trask either ignores these Basque-Caucasic comparisons in his critique, or makes minor comments that fail to "destroy" the comparisons (see the note to bi 'two' below.)

NOTES:

'I': Dargwa (A) and Lak (D) are the only Caucasian languages with this first person singular pronoun, most others having reflexes of *zō. (See the discussion in NCED 855.) Both first person singular 'I/me' and second person singular 'thou/thee' are thought to have been suppletive in Caucasian (and in proto-Dene-Caucasic).

'thou': Dargwa and Chechen have a pharyngeal H in this word. The Khinalug word (with a voiceless uvular fricative) is the dative form 'to thee'. The Udi word is cited from the Nidzh dialect (NCED 483). Basque hi is the eastern or 'French' form, which is simply i in the western or 'Spanish' dialects. Cf i 'thou' in the Kaitag dialect of Dargwa.

'we': The Dargwa word (cited from the Chirag dialect) begins with a tense velar fricative. The Caucasian words are all the inclusive 'we' ('you and I') except Chechen, which is the exclusive 'we' (NCED 786). Basque gu 'we', of course, has the usual European meaning.

'you': The Basque word is of course the polite singular 'you' (like French 'vous'), which was in ancient times the plural 'you', as all the Caucasian words still are. Basque z- here is a grapheme denoting an unvoiced sibilant [s̺], while the z in Caucasian languages denotes the voiced z as in English. The Dargwa word features a tense hushing fricative, and in Tabasaran we have a voiceless aspirated dentolabialized hushing affricate (!) (NCED 1086-87). Udi (F) is the most divergent Lezgian language, and has lost or simplified many consonants, here wa- from PNC *zwe .

'two': There are at least two possible explanations of the b in Basque bi 'two': (a) that it is a regular development of an earlier *Gw or the like. See the parallel development of a labial pp in Udi; or (b) Basque once had *bat 'one' - *Gwi 'two', assimilated to bat - bi in

counting (cf the reverse process in Latin quattuor - quinque, from earlier *kw- - *p-). Trask's claim that Michelena's proposed *biga 'two' (attested in eastern Basque) "destroys" this comparison, is of course false. Some Avar-Andian languages have a similar velar element, e.g., Avar k'i-go 'two', Andi č'e-gu 'two'. (NCED 924).

'fire': The full Udi form is arux 'fire', of which only the a- is cognate with Tabasaran ts'a, etc., another case of the phonetic reduction of Udi (NCED 354-5; see note to 'you').²³

'hunger': One of the clearest Vasco-Caucasic cognates (NCED 431).

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²³ [HF Note] But the problem of very short roots arises here. One would have to see more evidence before believing the loss of [ts'] and the original compound (presupposed here) [*ts'a-rux].

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(HF contribution):

We enclose a poem written by someone. It might have been Mark Twain.
In any case the name was lost. Ah, ha! It was New England's own James
Russell Lowell!

But it speaks to the spirit of long rangers who have not yet wet
their feet in the swirling waters of the Internet. Hyar tis.

"We will speak out, we will be heard,
Though all earth's systems crack;
We will not bate a single word,
Nor take a letter back."

"Let liars fear, let cowards shrink,
Let traitors turn away;
Whatever we have dared to think
That dare we also say."

"We speak the truth, and what care we
For hissing and for scorn,
While some faint gleamings we can see
Of Freedom's coming morn?
(Or Mother Human's tongue!) (Lowell forgot this part!)

Ciao! See you-all in August. VIVA TROMBETTI!

Subliminal suggestion. NOW is time to pay your DUES and send in questionnaire.

Late-breaking News. EXTRA!

One result of the Israeli elections could be that religious parties bring their influence to bear on Likud party and stop
ARCHAEOLOGICAL EXCAVATIONS IN ISRAEL! However, our informants do not believe such a dire outcome is to be expected. Why? Because there may
be a political compromise and the orthodox may get prohibitions on excavations involving BURIALS only.

HERRMANN JUNGRAITHMAYR
DYMISTR IBRISZIMOW

CHADIC LEXICAL ROOTS

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AN ETHNIC HISTORY

BY JUHA JANHUNEN