MOTHER TONGUE

NEWSLETTER OF THE LONG RANGE COMPARISON CLUB

NOVEMBER 1987

>>>> Contents of Circular 4 of MOTHER TONGUE <<<<

NOTE. Starting with this issue, the distribution of copies to Long Rangers is facilitated greatly by the kindness of several members. A large part of the heavy expense of transoceanic mailing will be mitigated because David Appleyard, Georgio Banti, Franz Rottland will be mailing within Europe; each will make a dozen copies and mail them to a sub-set of European members. Ekkehard Wolff will take on a European dozen in January, 1989 when he returns from Niamey. In addition Allan Bomhard, Fred Gamst, Sheila Embleton will each do a dozen to North American members. Since they are so generous, I have asked them each to include in their dozen some four Soviet Long Rangers. Since the greatest postal costs are between the USA and the USSR, and the longest time of delivery bar none, it would be very kind of a person or two in western Europe to arrange to mail a dozen Soviet copies.

ANOTHER NOTE. Quite a few people have been generous, making money contributions to Mother Tongue. I hesitate to name everyone who has given some money but I am totally unwilling to say how much each person gave. But I would like to single out Professor Seto of Tokyo because he not only sent the second highest amount but also sent it in ¥en. Had I the foresight to see how much the ¥en would RISE against the US$ I would still be hanging onto his ¥en! Right now the LRC Club has about $241 in the bank. Much of that will be consumed by the next two issues. But, a small number of volunteers to COPY and MAIL will make a large difference. In fact just 7 more mailers would make our distribution nearly painless to all.

THIRD NOTE. We are late, very late, in starting this round of circulars. The reasons are: I drove to California, wrote a long article about Ruhlen's book, spent much time overhauling our ship, been teaching hard this semester. SORRY!

Hal

<<< SOME SAD NEWS >>>

Our esteemed colleagues, CLAUS BAER and KAREL PETRACEK, have died. I will let the enclosed materials speak for themselves, except to say that I was very fond of each of them and deeply regret their passing.

THE UNIVERSITY OF CHICAGO
THE ORIENTAL INSTITUTE
CHICAGO • ILLINOIS 60637

Cables: ORINST CHICAGO

1155 EAST FIFTY-EIGHTH STREET

June 12, 1987

The Oriental Institute regrets to announce the death of Klaus Baer, of a heart attack, on May 14, 1987 in Chicago. Mr. Baer was a Professor of Egyptology at the Oriental Institute and in the Department of Near Eastern Languages and Civilizations at the University of Chicago.

His wife's address is:

Miriam Reitz
5530 South Shore Drive
Chicago, Illinois 60637

+ IN MEMORIAM +

KLAUS BAER,
1930-1987

SATURDAY, MAY 16, 1987
2:00 P.M.

CHRIST THE MEDIATOR LUTHERAN CHURCH
3100 S. Calumet Avenue
Chicago, Illinois 60616

KLAUS BAER

June 22, 1930 - May 14, 1987

Klaus Baer was born in Halle, Germany, the son of Marianne and Reinhold Baer. In 1933, the Baers emigrated to the United States, where the son remained but the parents eventually returned to Europe.

Mr. Baer received a B.A. in classical Greek from the University of Illinois in 1948. Immediately thereafter, at the age of 17, he entered the University of Chicago as a graduate student in Egyptology, having already taught himself classical Egyptian. From 1952 to 1954 he was a Fulbright Fellow in Egypt working on excavation projects at Saqqara and Giza. He received a Ph.D. from the University of Chicago in 1958.

Subsequently, Mr. Baer spent six years at the University of California at Berkeley. He returned to the University of Chicago in 1965 where he became an Associate Professor at the Oriental Institute and in the Department of Near Eastern Language and Civilizations. He was named Professor in 1970 and served as Department Chairman from 1972 to 1976. Klaus Baer was an internationally known Egyptologist, especially known as an expert on ancient Egyptian languages.

On July 20, 1985 Klaus Baer was married to Miriam Reitz, who survives him. They have been members of Christ the Mediator Lutheran Church since September, 1986 and in January, 1987, he was elected to the church council. Mr. Baer served for many years on the board of the Rocky Ridge Music Camp in Estes Park, Colorado.

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10 Section 2 Chicago Tribune, Friday, May 15, 1987

Obituaries

Klaus Baer, 56, an expert on Egypt

By Kenan Heise

Klaus Baer, 56, an Egyptologist at the Oriental Institute of the University of Chicago, was president of the American Research Center in Egypt from 1981 to 1984.

A memorial service for Mr. Baer, of Hyde Park, will be held at 2 p.m. Saturday in Christ the Mediator Lutheran Church, 3100 S. Calumet Ave. He died Thursday in Bernard Mitchell Hospital at the University of Chicago.

"He knew classical Egyptian when he came to the university as a graduate student at the age of 17 in 1948," a university spokesman said. "He had his bachelor's degree from the University of Illinois in classical Greek, but also knew Egyptian."

Mr. Baer, a native of Halle, Germany, immigrated to the United States with his family at the age of 3. His father, Reinhold, became a University of Illinois professor.

Though he was only 17 when he was graduated from the University of Illinois, Mr. Baer was co-salutatorian of his class.

From 1952 to 1954, he was a Fulbright Scholar in Egypt and worked on excavations at Giza and Saqqara. He received his doctorate from the U. of C. in 1958.

He became a lecturer and professor of Near Eastern languages at the University of California at Berkeley.

In 1965 he returned to the Oriental Institute as an assistant professor, and in 1970 he became professor of Near Eastern languages. In 1972 he was appointed chairman of the department.

He wrote the book "Rank and Title in the Old Kingdom" and had largely completed a grammar of the Coptic language and an in-depth ancient Egyptian chronology. The latter fixes the dates of the reigns of the Pharaohs by comparing references in a number of ancient texts.

Survivors include his wife, Miriam Reitz.

CHICAGO SUN-TIMES, Friday, May 15, 1987

OBITUARIES

Klaus Baer

Klaus Baer, 56, an internationally known Egyptologist and a professor at the Oriental Institute at the University of Chicago, died yesterday at the university's Mitchell Hospital.

Mr. Baer, who taught himself classical Egyptian, was an expert on ancient Egpyt and worked on several excavations at Saqqara and Giza.

Survivors include his wife, Miriam Reitz.

Services will be at 2 p.m. tomorrow at Christ the Mediator Church, 3100 S. Calumet. Burial will be private.

hp ns the herald, wednesday, may 20, 1987

Services were held Saturday, May 16, at Christ the Mediator Church for Klaus **Baer**, who died of a heart attack at the age of 56 on Thursday, May 16, at Bernard Mitchell Hospital.

Baer was a professor of Near Eastern languages and civilizations at the University of Chicago and chairman of that department at the Oriental Institute. An expert on ancient Eyptian languages, Baer came to the University of Chicago in 1948. He was a Fulbright Fellow from 1952 to 1954, when he worked on excavation projects at Saqquara and Giza, Egypt.

Baer is survived by his wife, Miriam Reitz.

See Also.

a) News & Notes, No. 110 September-October, 1987 The Oriental Institute, pp. 2-3.

b) Journal of Von Siden Books, VA 3 (1987) pp 101-102 (Has complete Bibliography)

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September 4, 1097

Mr. Harold Fleming
Mother Tongue Newsletter
69 High Street
Rockport, Mass. 01966

Re Klaus Baer

Dear Mr. Fleming,

Thank you for your kind letter of a few weeks ago expressing sympathy over our loss of Klaus. While I am acutely aware of having lost a good and generous human being, to judge by responses such as yours, the academic world has lost a fine scholar.

I would be very pleased to have him recognized in your Newsletter. Enclosed are several pieces that will give you some information about him: the funeral folder, the newspaper obituaries, and an article that has just come out in the Oriental Institute's newsletter.

You will notice that each of the pieces adds some different personal note. There is one such note not mentioned in the other pieces but which will be of interest to you. My husband's interests were indeed very broad but ion the academic sphere, right after Egyptology came languages and linguistics. In his very extensive professional library, (which will be given to the Department of Near Eastern Studies at Berkeley), there is a whole section on linguistics plus a couple hundred grammars. A little known fact about my husband was that he enjoyed reading grammar books and considered them light bed-time reading. His collection includes a range from Pawnee to Islandic to Gothic to Japanese.

I hope that this information reaches you in time for your next newsletter. Would you be so kind as to send me a copy? Thank you so much for your interest.

Sincerely,

Miriam Reitz

Miriam Reitz (Baer)

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Boston University

Department of Archaeology
675 Commonwealth Avenue
Boston, Massachusetts 02215
617/353-3415, 3417

Nov. 19, 1987

Dear Hal,

I hope the following is the kind of thing you had in mind. In some ways I feel a little awkward writing this, since my memories of Klaus Baer largely center around my first years in graduate school and many people have told me that he underwent a complete personality change after his first heart attack several years ago. I never met this 'new' Klaus, any many of your readers may not have known the guy I remember. But anyway...

The loss of Klaus Baer will profoundly change Chicago's Oriental Institute and, I assume, American Egyptology, although in ways that might not be immediately apparent to remote observers. Certainly his publications--few in number and all brilliant--are inadequate indicators of his influence. Klaus was above all a presence. He was generally the first professor students got to know when they began doing graduate work and he was one to whom they frequently talked and listened for the rest of their careers. It didn't matter if your were studying Hittite or Mesopotamian archaeology or Coptic, and it didn't matter if Klaus happened to be the departmental advisor or Chairman or whatever; his door was always open and he was always there.

And what a source of information he was! I don't believe I have ever met anyone who was so widely read, both in his own field and in everyone else's. I remember an encounter with him in a bookstore one evening. We were both browsing largely for entertainment, pulling various used books off the shelves and thumbing through them as we talked. After about fifteen minutes of this, during which I had been looking at some very obscure things, it dawned on me that Klaus had actually read every book I handled. His personal library was probably one of the best Egyptological collections in the country, and he made it available to his students. But if you wanted to know something quickly, it was easier just to ask him. He'd read all his books, remembered everything, and I don't remember him ever being wrong about a source or a quotation; he was, in short, a bibliographical marvel.

Klaus was also a wonderful teacher and ran one of the best seminars I have ever taken. He had a way of challenging students, of drawing on their creative abilities and welding these to accurate scholarship, that I have never seen equalled. His talents for organization were apparent both in his courses and in the overall structure of Chicago's Egyptology program. One cannot help feeling that whole field has now been wounded, having lost one of its quintessential professors.

Sincerely yours,

Paul Zimansky

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Není smrti, zůstává věčně živý,
kdo spravedlivý byl a dobrotivý.
F. Halas

Hluboce zarmouceni oznamujeme všem přátelům a známým, že nás navždy opustil náš drahý a milovaný manžel, tatínek, tchán, bratr, švagr, strýc

pan

PhDr. Karel Petráček, CSc.

UNIVERZITNÍ PROFESOR

Jeho srdce dotlouklo 1. července 1987 ve věku 61 let.

Poslední rozloučení s naším zesnulým se koná
ve středu 8. července 1987 v 10 hodin
ve velké obřadní síni krematoria v Praze-Strašnicích.

Letohradská 20, Praha 7

Jménem rodiny:

Eva Petráčková
manželka

Vojtěch Petráček
syn

Markéta Petráčková
snacha

Milena Zýmová-Davidovičová
sestra

Vladimir Davidovič s manželkou
synovec

Vytiskly Tiskařské závody, n. p., Praha, závod 1, provoz 14, Praha 2, Lípová 6, telefon 29 67 97

AFROEUR ASIAN FLEMING'S NETWORK : PETRÁČEK /PRAGUE/ SPEAKING

Hello Harold,

This is Carl calling from Prague using Your AFROEURASIAN network.

The idea is excellent and the results of our future conversations /in AFROEURASIAN NEWSLETTER/ could be useful for us as well as for the next generation of linguists and cultural anthropologists.

You appreciate highly the Soviet initiative in this field of studies and You are right.

In our land where Slavonic languages are widely understood, we are good acquinted with all works by Illič-Svityč, Dolgopol'skij, Dybo, Palmaitis, etc., not to speak about Afroasian writings by Diakonoff, Militarev, Stolbova, Porkhomovskij, etc.We could also add some annotations and criticism to their theses /cf. my articles [1]/. Really, they do not sometimes mention works from abroad /e.g. American studies about North-Eastern Africa, Fleming, Ehret/.

We also have a good bibliography of Nostratic studies in Slovo and slovesnost [2] that could be useful for You but it is in Czech. The Nostratic these has found in my land some open minded linguists who seem to accept the general idea and apply it in IE studies /esp. in phonology/ [3].

As for me, I am rather critical but my position is /like Your position / not negative. I would like to limit my speculations only to the relations of AA to N; I have written some pages on this problem [4]. The African hypothesis of the AA origins /from Reinisch,Lepsius,Nöldeke to Diakonoff,Bender and Your works/ makes it difficult to connect the AA family directly with other families of N /of Asian origins/. Your idea of treating both parts of N, i.e. AA and the other families in one super-phylum could help us to understand better the relations among AA and other families.

The Soviet position indicated in Your letter /two coordinate branches, one of them is AA/ seems to support Your conception.

I would like to study in the future and in coordination with AEA NEWSLETTER and Your tasks the relations of AA to other African families, especially to Saharan and then perhaps Benue-Congo/ Niger-Congo and Nilo-Saharan. I am also interested in the relations between AA and IE.

My last studies concerning the Saharan and AA 5/ have some positive results /in the verbal system - the verbal extension; in the reconstruction of some roots 6/ /. AA seems to be merely tied with the African soil and with the linguistic situation there in prehistorical times. I am very happy that I can use Your and other American works for the anthropological and prehistorical interpretation of that situation.

You are also right when stating that our colleagues who write in Russian have some gaps in the western literature and current theories. This is also my case because it is sometimes difficult to gain the whole production /say of American linguists and anthropologists working at our problems/. AEA NEWSLETTER could help us to overbridge our gaps. It could realize contacts among different groups of linguists, anthropologists and archaeologists /cf. Your network/, discussion about strategy and theory of our complex problem, presentation of pertinent results of our progress in research and perhaps bibliographical notes. It could enable us the exchange of written and printed material.

I am ready to join the group and to collaborate in the field sketched above / AA: Nilo-Saharan,Benue-Congo,Niger-Congo; AA : IE/ and perhaps in some general or theoretical problems.

The annotations to this letter contain the whole Czech literature about the Nostratic problem.

I am sending some of my papers on Your address /publications and manuscripts/.

If necessary , I could also send copies of the Soviet works which interest You.

Yours sincere

Karel Petráček K. Petráček

Prague 7.1.1987.

Address: Department of Asian and African Studies
Charles University, Prague 1 120 00
Celetná 20
Czechoslovakia
/the institution in Czech: Katedra věd o zemích Asie a Afriky/

my home address:
Prague 7 170 00
Letohradská 20
Czechoslovakia

x x x

P.S.

Your question concerning Slavonic dialects and mutual understanding of them is to be answered that they are languages /perhaps with the esception of Czech and Slovak, both in Czechoslovakia - but Slovak is officially a language in our Confederation/. Linguists, say Zaborski and myself, can easily understand the other language when speaking his own language. This was the case of our conversation with Zaborski in Polish and Czech in Vienna.

The problem of Russian is another problem because there were many historical, cultural and linguistic contacts between Polish and Czech that make conversation between two learned linguist quite easy. This is not possible with Czech and other Slavonic languages /Russian etc./.

But I must add that e.g. my son, without any special linguistic training /he studies electronics at University/ also understands Polish quite well. That's all.

Yours K.P. KP.

Annotations

1/ K.Petráček, K problematice nostratické teorie /z hlediska fonologie semitohamitských jazyků/, Slovo a slovesnost 37,1976,60-61 ;Indoevropský a semitohamitský kořen a perspektivy jejich srovnávání, Slovo a slovesnost 42,1981,216-218 = in French La racine en indoeuropéen et en chamitosémitique et leurs perspectives comparatives, AIOUN 42,1982,381-402; Semitohamitské jazyky a nostratická hypotéza, Slovo a slovesnost 44,1983,57-63; K teorii laryngál, Slovo a slovesnost 42,1981,262-268.

2/ V.Blažek, Současný stav nostratické hypotézy /fonologie a gramatika/, Slovo a slovesnost 44,1983,235-247 /• The contemporary situation in the Nostratic hypothese, 91 bibliographical items /

3/ cf. in the Bibliography of V.Blažek, Ann.2 the following authors: Čejka ,M., Erhart,A., Lamprecht,A., Lamprecht,A.-Čejka,M., Petráček, K., Skalička,Vl., Vacek ,J., and the author of the bibliography, together 18 items of Czech authors.

4/ Cf. ann. 1.

5/ Saharisch und die Nilo-Saharanische Sprachfamilie, into press for Acta Universitatis Carolinae; Saharisch und Hamitosemitisch, Paper presented to the XXIII Deutscher Orientalistentag, Würzburg 1985, into press; Altägyptisch, Hamitosemitisch und ihre Beziehungen zu einigen Sprachfamilien in Afrika und Asien, Monograph , Charles University into press /Ch. 3.2 Hamitosemitisch in Asien /; Indoeuropäisch, Afroasiatisch und Nostratisch. Randbemerkungen zu A.R.Bomhard, Toward Proto-Nostratic. A New Approach, into press ArchOr; Leo Reinisch: Der einheitliche Ursprung der Sprachen der Alten Welt und die afrikanische Urheimat der Semitohamitischen und der semitischen Sprachen, Leo-Reinisch Symposium Wien ,1982, into press /1987,p.309-332/.

6/ One of them shows what danger is in the long range comparison like Your reconstruction of the root for NOSE,SMELL etc.: +SN , cf. here also Saharan forms in Zaghawa sina, Berti sano, Bideyat šena /= all Eastern languages/, b u t the common Saharan root is to be reconstructed clearly as +KINA /cf. forms with k,c,č in Teda,Daza,Goran; +K > palatalisation/. I feel we need a new theory of phonological Super-Phylum reconstruction.

P.S.Now I am preparing two studies of our problem: Die velare Lokalisierungsserie im Saharischen, and Saharan and Nilo-Saharan Phonological Reconstruction / discussion of the Saharan items in the reconstruction of Ehret, in Nilo-Saharan II, ed.R.Thelwall. My conclusion is negative: Saharan is not a part of Nilo-Saharan, cf. also my elder studies. I have also finished my Berti or Sagato-a Vocabulary /to appear in Afrika und Übersee/ and The BER-Group of Saharan Languages /to appear in ArchOr in Prague/ and at last a comparative vocabulary of the BER / Eastern group of / Saharan languages /with phonological correspondences/.

K.P.

P.S.P.S. cf. now:

V. BLAŽEK, GENETICKÁ KLASIFIKACE JAZYKŮ SVĚTA VE SVĚTLE REKONSTRUOVANÝCH PRAJAZYKŮ NEOLITICKÝCH AŽ PALEOLITICKÝCH JAZYKOVÝCH JEDNOT. theses of a conference, in: JAZYKOVĚDNÉ AKTUALITY, XXII, 1986, 1-2, p. 41-44

FOR PUBLICATION | 13

9.V.1987 Victory Day

Karel Petráček, Prague

Afroasiatic and Nostratic (IE) in Geographical View

(Note on Mother Tongue 3,p.VII)

H.Fleming quotes in MT 3,VII: " If AA is related to IE or Nostratic, it means that they (IE and Nostratic) came from Africa originally. And I am positive that is what Carl Hodge and Karl Petráček think."

This is all right b u t there is a difference between the position of Carl and Karl (=Karel).Carl Hodge would like to derive IE from Africa (like Merlingen;cf.the paper of Carl read on Leo Reinisch Symposium ,Wien 1982), I am ready to suppose (with Gamkrelidze,Ivanov,1984 a.o., cf. esp. Garbini-cf.my article in Slovo a slovesnost 44,1983,57-63:Hamitosemitské jazyky a nostratická hypotéza = Hamitosemitic Languages and the Nostratic Hypothesis) that the similarities (in the lexicon) of both families (HS and IE) are due to specific areal contacts in the North of the AA "Sprachwelt".

The last position of Soviet colleagues (AA is a coordinate branch to all other nostratic families,cf.also Fleming MT Circular 2,p.2) and the results of Greenberg's study of EUROASIAN (cf. MT 3: AA+Kartvelian and Dravidian o u t of Euroasian) seem to point at the direction of our hypothesis that AA has a special status in the world of languages and that it could be -perhaps- linked rather to the African phylas. Its nostratic filiation seems to be weaker now than it was before.

But cf.the opposite meaning of Militarev who again derives AA from Asia (in view of the contacts with North Caucasian).

Another type of explanation of AA and IE geographical problems could be in the hypothesis (near to what Olderogge said before many years) that the original home of AA was situated in a large area from Sahara to the Arabian Peninsula.

In this respects we can also note that the supposed presence of Cushites (some of proto-Cushitic branches) in the Peninsula is not out of possibility (cf.elder views of Diakonoff and Dolgopolsky and now the argumentation of Militarev in suppoert of teh Asian home of AA).

The last quoted model reduces the a geographical dynamics of AA supposing its earlier presence in the large area and admits also possibilities of contacts with IE (and Kartvelian ?) in the North of this area. Archaeological evidence (rock paintings from Arabia, Ethiopia and Egypt) supports the hypothesis of a large original area (Saharo-Arabian) of AA. In this respect cf. the studies by P.Červíček (Neue Felsbildstation im südlichen Hidschaz, Paideuma XVII,1971,21-31; Rock Painting of Lago Oda (Ethiopia), ibid.121-126; Rock Engravings from the Hamasen Region,Eritrea, ibid.22,1976,237-56; P.Červíček ,U.Braukämper, Rock Paintings of Lago Gafra (Ethiopia),ibid. 21,1975,47-60;prehistorical documentation for the presence of the Cushites in the Peninsula cf. E.Anati, Rock-Art in Central Arabia, Louvain 1968,2 vols. - but this evidence seems not to be conclusive).

Kf.

Reprinted with permission from Natural History, June 1987

Bushes All the Way Down

We are all products of a recent African twig

by Stephen Jay Gould

An old English rhyme captures, quaintly but succinctly, a central truth of nature's dilemma:

> Pale Ebeneezer thought it
> wrong to fight
> Puffing Bill who killed him
> thought it right.

Or, in American translation, "There ain't room enough here for the both of us."

The tale of Ebeneezer and Bill epitomizes a rule of thumb in ecological and evolutionary theory called the principle of "competitive exclusion." This doctrine holds that if two coexisting species are "too close" in their ecologies and mode of life, they cannot both persist in the same area. We cannot imagine that both will pursue their common modes of life with an absolutely equal efficiency; one must perform at least ever so slightly better, and this species will, in course of time, eventually supplant the other (so long as space and resources are limited, as they always are in our finite world).

Yet, manifestly, species of similar form and relationship do often coexist in stability. In these cases, biologists argue that the domain of ecological difference is large enough to permit joint survival. (The principle can become meaningless if we use the fact of coexistence as a priori evidence for sufficient difference, and evolutionists have often so erred. But if we search for such cases of coexistence in order to test the principle by a subsequent study of ecological disparity, then competitive exclusion may have scientific value.)

In any case, the principle of competitive exclusion became the centerpiece of an explicit hypothesis about human evolution that enjoyed a great vogue in the 1960s and 1970s but has now been disproved—the "single species hypothesis," the last bastion for the metaphor of the ladder in studies of human evolution.

In the classic statement of the single species hypothesis ("Competitive Exclusion Among Lower Pleistocene Hominids: The Single Species Hypothesis," *Man*, vol. 6, 1971, pp. 601–14), M.H. Wolpoff quoted Ernst Mayr, our greatest living evolutionary theorist, on the interpretation of competitive exclusion:

> The logical consequence of competition is that the potential coexistence of two ecologically similar species allows three alternatives: (1) the two species are sufficiently similar in their needs and abilities to fulfill these needs so that one of the two species becomes extinct, either (*a*) because it is "competitively inferior" or has a smaller capacity to increase or (*b*) because it has an initial numerical disadvantage; (2) there is a sufficiently large zone of ecological nonoverlap (area of reduced or absent competition) to permit the two species to coexist indefinitely.

The single species hypothesis held that no two human species ever coexisted and that our evolution has progressed as a series of successive stages on a single pathway leading to modern *Homo sapiens*. Wolpoff and his colleague C.L. Brace applied their single species hypothesis particularly to the record of early human evolution in Africa—arguing that the two classic lineages of australopithecines, the so-called graciles and robusts, must belong to a single species, with pronounced geographic and sexual variation previously misinterpreted as evidence for multiple lineages.

But why did Wolpoff and Brace hold so strongly to this view of competitive exclusion, especially since the principle permits coexistence of two species if their domain of ecological overlap is small enough? The single species hypothesis rested upon the specific argument that the uniqueness of human life styles precluded such small overlap between coexisting species. Wolpoff identified culture as the reason for necessary competition to the point of exclusion. Other animals can become narrow specialists on a particular type of food or within a limited space in a rich environment. Such specializations can minimize competition with relatives committed to different foods and spaces—and permit close evolutionary cousins to dwell together in stability.

But culture defines human uniqueness, and culture is, by definition, expansive. We become learning animals and develop ways to exploit more kinds of foods and places. Our evolution must proceed toward greater generality—that is, toward the domain of overlap, where competitive exclusion must operate if two human species inhabit the same area. Even though australopithecine culture scarcely rivaled our own, Wolpoff deemed it rich enough to build an ecological niche so broad that only one hominid species could inhabit Africa at any time. Wolpoff wrote:

> Culture acts to multiply, rather than to restrict, the number of usable environmental resources. Because of this hominid adaptive characteristic implemented by culture it is unlikely that different hominid species could have been maintained.... Competition would most likely cause each hominid species to develop the ability to utilize a wider range of resources and thus increase the amount of competition. One surely must succeed at the expense of the other.

As an extension of the single species hypothesis, Wolpoff and Brace sought to interpret other supposed cases of apparent interaction between two differing peoples as evolutionary sequences of direct transformation—in particular, Neanderthal evolving into modern humans, rather than Neanderthal interacting with, and replaced by, a discrete group of invaders (Cro-Magnons of modern type), as dramatized in the popular novels of Jean Auel. (If Brace is right, then Ayla's struggle is fiction in more ways than one.)

In fact, Brace often derided hypotheses of interaction and replacement, labeling all such ideas as "hominid catastrophism"—a reversion to the bad, old preevolutionary habits of special pleading: to avoid an interpretation of direct evolutionary transformation, we suppose that a

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new species migrates in from elsewhere and wipes the "primitives" out.

If the single species hypothesis be valid, then Brace's ridicule is justified—for no other species can exist to form the phalanx of an invasion, and all temporal sequences should be interpreted as cases of evolutionary transformation. But if the single species hypothesis is wrong, and if human evolution follows nature's conventional topology of the bush (rather than our culturally bound hope for a ladder of progress), then "hominid catastrophism" should be an anticipated consequence of evolution, not a term of reproach. If splitting and twigginess are primary themes of human evolution, then different species may exist to meet and interact.

As the single species hypothesis had set its roots in a claim about our long African prehistory (from our split with the chimpanzee lineage some five to eight million years ago to the exodus of *Homo erectus* from Africa about a million years ago), so too did it fall in Africa. By 1976, the hypothesis had already faded, since most paleontologists had concluded that gracile and robust australopithecines represented separate lineages, not males and females of a single species. In that year Richard Leakey and Alan Walker described two hominids from the same geological formation (about 1.5 million years old) so different in appearance that no one could dispute their separate status ("*Australopithecus, Homo erectus,* and the Single Species Hypothesis," *Nature,* vol. 261, pp. 572–74). Fortunately (for clarity in conclusion, but not for the single species hypothesis), these two skulls displayed extremes of gracile and robust tendencies—thus accentuating differences to the point of resolution.

One skull represents the so-called hyperrobust form *Australopithecus boisei*, a small-brained creature with a protruding face and massive brow ridges. The other, quite modern in appearance, has been placed in *Homo erectus*, the species supposedly ancestral to modern humans. Thus, much of human prehistory in Africa included at least two coexisting lineages—our own and the surviving robust australopithecines. (Richard Leakey sees even more bushiness in our African story, for he argues that three hominid species coexisted just before this time—*H. habilis*, presumed ancestor of *H. erectus*; the robust lineage; and surviving populations of the gracile lineage, *A. africanus*. As with the apes of last month's column, our knowledge may not be near the asymptote of hominid bushiness.) So Africa has fallen to bushiness, but how far can we extend this favored metaphor? Surely, at some point we must reach a twig that grows straight out without further branching to modern *Homo sapiens*. Where is the teeny ladder of this ultimate twig?

About a million years ago, after our long and exclusively African prehistory, some populations of *H. erectus* migrated out of Africa (while others stayed) to colonize parts of Europe and Asia. (As Java man and Peking man, we knew about these Asian *H. erectus* even before we had discovered their australopithecine forebears in Africa.) Some paleontologists have identified *H. erectus* as a bottom rung of the ultimate ladder, arguing that this ancestral species transformed itself, in toto and in various places, into modern humans (*H. erectus* and *H. sapiens* become, in this interpretation, grades of structural improvement within a single evolving lineage, not proper species by the usual criterion of branching). Carleton Coon advanced the extreme form of this argument when he claimed, in his popular book *The Origin of Races* (1962), that five separate groups of *H. erectus* had independently evolved in parallel, in Africa, Europe, and Asia, to *H. sapiens*.

The alternative viewpoint, following the metaphor of the bush, still interprets *H. erectus* as our ancestral species but seeks a later and local point of origin for modern humans. After all, *H. erectus* thrived on three continents. Why insist that all its populations moved upward and onward to our current glory? Why not argue that *H. sapiens*, like most species, branched from one of these populations and then spread out, eventually to displace *H. erectus* populations (or their descendants) in other parts of the world—a classic case of "hominid catastrophism" as a legitimate pattern of evolution?

The hints have been with us for a decade, but strong evidence has just emerged for a radical version of bushiness to this bitter end. To summarize the conclusions baldly (the evidence follows in a moment): all modern humans are products of a very recent twig that lived exclusively in Africa until 90,000 to 180,000 years ago. We therefore branched from *H. erectus* in Africa, the center of origin for all hominid species discovered so far. Modern *H. sapiens* migrated from Africa to the rest of the world (reaching Europe and Asia quickly, Australia some 40,000 years ago, and the Americas some 10,000 to 20,000 years ago). All modern humans are a product of this split and migration; the previous emigration of *H. erectus* to Asia left no descendants. (Lest this seem improbable or complex, consider the story of horses, told in this forum two months ago in the first column of this trilogy. Remember that T.H. Huxley mistakenly concocted a European ladder of horses from four separate lineages that migrated sequentially to Europe, where each became extinct without issue.) Fossil hominids older than this date of splitting for *H. sapiens* in Africa—including the Asian *H. erectus* and probably the famous Neanderthals of Europe—are separate lineages on the hominid bush and played no role in our ancestry. For African *H. sapiens*—the forebears of us all—as for Judah the Maccabee:

> See the conquering hero comes!
> Sound the trumpet, beat the drums!

(although we have no evidence for martial replacement by African invaders; the indigenous people of Europe and Asia may have disappeared earlier or for other reasons).

The hints are in stone and bone. Sophisticated blade tools appeared in Africa nearly 100,000 years ago, long before they replaced simpler flake tools in Europe or Asia. Concomitantly, the oldest modern humans have been found in African sediments some 100,000 to 140,000 years old. Moreover, some paleontologists are now arguing that the Asian populations of *H. erectus* developed a suite of anatomical specializations absent both from modern humans and from African fossils usually called *H. erectus*. If this tentative claim is affirmed, then Asian *H. erectus* would be debarred from the ancestry of modern humans, while African forms remain admissible. (I leave for another time the interesting implication for taxonomic realignment—that African populations now placed in *H. erectus* may require redesignation as a separate species. The name *Homo erectus* must, by rules of nomenclature, remain with the Asian forms that first received this label.)

The firmer evidence lies in molecules, for we all carry genetic tracers of our ancestry. During the past decade, molecular evolutionists have recognized the power of mitochondrial DNA for unraveling the histories of recently evolved groups. Mitochondria are the energy factories of all complex (eukaryotic) cells. They presumably originated, more than a billion years ago, as entire cells of primitive (prokaryotic) type that began living as symbionts within the ancestors of eukaryotic cells. As a heritage of their independent origin, mitochondria have their own DNA—arranged as a short, circular molecule.

Mitochondrial DNA has two favorable features for the reconstruction of evolutionary histories. First, it evolves about ten times faster, on average, than nuclear DNA—thus permitting sufficient resolu-

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tion for such recent and rapid events as the origin and spread of modern humans. Second, compared with nuclear DNA, its pattern of inheritance is simple and direct. Since the business end of a sperm is all nucleus, mitochondrial DNA is strictly maternally inherited. We can therefore trace lineal paths of descent, rather than the complex crisscrossing of family lines for nuclear genes that may come from either parent. Moreover, the entire mitochondrial genome is inherited as a unit. Prokaryotic cells (like modern bacteria and the precursors of mitochondria) do not have paired chromosomes; DNA is arranged instead as a single continuous molecule. When chromosomes pair, as in all nuclear DNA of eukaryotic cells, exchanges occur between the two members in each generation. Nuclear chromosomes are, therefore, continually fractured and reconstituted. But the mitochondrial genome is a stable entity, passed intact from mother to offspring and altered only by mutation. It is therefore an ideal tracer for genealogical histories.

Rebecca L. Cann, Mark Stoneking, and Allan C. Wilson have just published our most extensive data on variation in human mitochondrial DNA ("Mitochondrial DNA and Human Evolution," *Nature*, January 1987, pp. 31–36). They studied 147 people drawn from five geographic populations (Africans, Asians, Caucasians, aboriginal Australians, and New Guineans) and succeeded in surveying about 9 percent of the entire mitochondrial genome of 16,569 base pairs.

Cann and her colleagues found 133 variants among the 147 subjects (most people are unique, but very little different from many others). As the next (and crucial) step, they arranged these 133 mitochondrial types into an evolutionary tree. We now encounter an important property of such molecular information: the data themselves are abundant and "hard"; but interpretations rest upon assumptions that, although reasonable and proper, must be stated and evaluated. In principle, a vast number of evolutionary trees may be constructed from 133 variants. How shall we decide which to prefer?

In such cases, we generally invoke the assumption of parsimony—that is, we build the evolutionary tree that requires the minimal number of mutational changes to link the 133 variants. (This procedure matches our intuitions: confronted with mouse, rat, and human, we would assume a closer tie between mouse and rat rather than the unparsimonious solution that mouse evolved to human and human back to rat—for this second, unparsimonious tree would require a much longer pathway of linkages, namely, a double run both up and down the long rodent-to-human road, rather than a single excursion, as in the first solution. But parsimony is a procedural assumption that might be wrong in any particular case, not an a priori truth of nature.) In the mitochondrial example, we may worry less about the parsimony assumption because conclusions are, in the profession's jargon, so "robust"—that is, a large family of most parsimonious and nearly parsimonious alternative trees all yield the same basic solution.

The minimal length tree for 147 humans has a simple and striking topology. It includes two major branches joining at the base. One contains only Africans, the second includes other Africans plus everybody else. Cann and colleagues compared this most parsimonious tree with several alternatives. The conceptually opposite tree for example—one that links each of the five geographic groups to an independent root and corresponds to Coon's old theory about separate origins from different stocks of *H. erectus*—would require fifty-one more mutations to make all the linkages.

These data provide two strong reasons for viewing Africa as the unique source of modern humans: first, of course, the form of the tree itself, with its African root; second, the greater mitochondrial diversity maintained by peoples of African descent. The older a group, the longer the time available for generating diversity. Cann found as much variation *within* the African populations as between Africans and any other geographic group.

The tree's form tells us "where," but not "when." Since mitochondrial trees say nothing about the anatomy of our common African ancestor, we need subsidiary information from paleontology—and this requires knowledge of timing. If the two great branches of the mitochondrial tree joined in Africa more than a million years ago, then our most recent common ancestor would presumably have looked like *H. erectus*. If the joining occurred much later, then our common roots are much more shallow—and we all probably branched from a subset of a population that had already become *H. sapiens*.

To derive such an estimate of timing, we must make an additional assumption, more tenuous than the previous statement about parsimony. We assume that mitochondrial DNA changes by mutation at a constant average rate over considerable stretches of time. Such an assumption is not required by evolutionary theory, and alternative ideas of greatly variable rates (due to differing intensities of natural selection) can easily be defended. The justifications for this assumption are primarily twofold: first, the presupposition of constancy, though initially derided by many evolutionary theorists, has worked in many cases where we can check a molecular tree against known dates of branching from the fossil record. Second, the tree derived under this assumption is also robust; large departures from constancy would be required to change its form or its timings substantially. In any case, the figures reached under the principle of constancy must be viewed as ballpark numbers tied to their assumptions, not as established facts.

Many studies of diverse animal groups yield the same estimate of 2 to 4 percent change in mitochondrial DNA per million years. Combining this figure with measured distances among the 147 people, we derive a time scale for diversification and spread of modern humans. This exercise suggests a conclusion surprising to many (though not to me and other devotees of the bush) and stunning in its implications about human unity: despite our external differences of skin color, hair form, and size, all modern humans have a remarkably recent, or "shallow," common ancestry, occurring well after our anatomical transformation to *H. sapiens* in Africa.

The assumption of constancy at 2 to 4 percent suggests that the common ancestor for all existing human mitochondrial DNAs lived in Africa between 140,000 and 290,000 years ago. This branch then split into the two main limbs of Cann's tree, and members of the second limb left Africa later—only 90,000 to 180,000 years ago. All non-African racial diversity arose within this geological millisecond, and the underlying unity of all humans is, as I have argued before (November 1984), a "contingent fact of history," not a hope of liberal ideology.

If these dates are right, we must also accept the conclusion that older inhabitants of Europe and Asia died out without contributing anything to our genetic heritage. European Neanderthals, for example, predate this time of migration from Africa. If the invading Cro-Magnons had hybridized with Neanderthals or if Neanderthals had simply evolved to humans of modern form (both hypotheses have been popular), then the mitochondrial tree would not have its unique and shallow African root—for older mitochondria from Neanderthals would be found in European populations. Of course, a larger sample of humans might yield different mitochondrial variants of greater distinction, but the data as now known suggest no such heterogeneity in human ancestry.

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Before leaving this subject, I must correct one striking misinterpretation that has begun to flood popular accounts of this discovery. Noting that all human mitochondrial DNA can be traced to a single African type, some have dubbed this conclusion the "Eve hypothesis" and have actually claimed an implication that we all owe our ancestry to a single female who lived about a quarter of a million years ago. The data do mean that all modern humans may contain, in their genealogical ancestry, one African female (or a few with the same mitochondrial type), but such a perfectly orthodox, almost necessary conclusion says little about the *size* of our ancestral population at this time of origin. To say that we all include one woman in our ancestry is not to claim that only a single woman existed at that time—although this is the ludicrous misinterpretation that has spawned some lurid press accounts. After all, the ancestral human population may always have included, say, 50,000 people during the time of its African origin, but all modern humans may still trace a mitochondrial genealogy to just one female among these 50,000.

In fact, such a pattern of boom for one and bust for everyone else is not at all surprising but an expected and predicted result in our tough and random world, exposing each and every one of us to the continuous slings and arrows of outrageous fortune. Most genealogical processes work this way. Consider human family lines, for example. If we started with a population of twenty family names, with twenty people per name, and maintained the population at constant size for many generations under uncertain conditions of human life (disease, conquest, infertility), most names would eventually die out and we would all be Smiths or Goldsteins (if we didn't confound the process by adopting new names as the old lines expired). Yet this later uniformity would permit no conclusion that a certain Ms. Goldstein had lived alone in Eden way back when—for the population had always numbered 400.

This principle rests upon a well-established mathematics beyond the scope of this column and its author. Its conclusions are firm, though surprising to those (most of us, alas) who do not understand the nature and power of random processes. For example, in a purely random system even for a large population begun with 15,000 unrelated females, we can calculate a 50 percent probability that, 18,000 generations later, all members of the population would be descendants of but one female among these 15,000.

This stunning demonstration of the temporal shallowness of our roots has a precious property shared by very few of the new discoveries that inundate us daily. It provides one of those rare items of information that might make us think in a fundamentally different way about a subject of great importance—our own origins and the nature of evolution. First, the generality: no matter how high we tune the power of our microscope, we cannot escape an evolutionary topology of branching and bushiness. We are all products of a recent African twig, not termini of a general evolutionary advance. The metaphor of the bush (and the falsity of the ladder) permeates evolution at all genealogical scales, from the history of a species to the unfolding of life's entire tree. Bushiness is a pattern of self-similarity that emerges whenever we magnify successively smaller segments of life's tree.

We might have anticipated a different conclusion—a change from bushes to ladders once we looked at sufficiently small segments of life's history. We might have supposed that while life, in toto, must be a bush, each little twig might grow straight. Since the human lineage is a tiny twig, why not hold that *H. sapiens* might be the top rung of a tiny ladder, even while the history of all primates forms a bush. But life's tree is a fractal, and tiny parts, when magnified, look much like the whole.

This shallowness of ancestry also teaches a more particular lesson for us as a species. Modern *H. sapiens* is an entity, not an evolutionary tendency. We have a definite point of recent origin and a history of later spread. We are not a grade of structural advance in mentality, the expected termination of the hope of ages; we are a discrete historical thing, a fragile little twig of recent origin and unparalleled subsequent success. Our unities of mythology, of what we call human "essence" or "nature," perhaps even of language (if the Indo-European branch can be connected, as some scholars maintain, with other families of language to a single rooted tree), need not reflect mysterious immanences of the soul or deep archetypes of the psyche, but need only record a recent history of common origin. We are close enough to our African origins to hope for the preservation of unity in both action and artifact. We are used to thinking of ourselves as an essence, or a type—one, moreover, that holds hegemony over nature by virtue of evolved superiority. We are no such thing; we are an item of history—an entity, not a tendency.

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Please see Michael Day's 'Neanderthal Problem' for a fossil-based scheme. Page 31

← Underlining not in the original

Vitalij Shevoroshkin's Comments L 19

THE UNIVERSITY OF MICHIGAN • ANN ARBOR
DEPARTMENT OF SLAVIC LANGUAGES AND LITERATURES

May 15, 1987

Dear Hal,

I'd like to comment on Merritt's letter (see recent Circular): it's sad to realize how much the Nostratic reconstruction, and the necessity of precise recunstruction is underestimated in the West.

But first, the language grouping. Merritt thinks that Illič-Svityč did not include Korean and Jap. into Nostr.; no, he did, and he regularly used Korean data in his Nostr. Dict. As for Chukchi-Kamchatkan, Dolgopolsky started to use it as a Nostr. daughter language from the very beginning (see his studies published in 1964). He also thaught (already at that time) that EskAleutian is Nostr.; this was proved later (last work I know about is O.Mudrak's comparison in the materials of the 1984 conference in Moscow). In a recent letter to me Greenberg writes that I.-S.'s Dictionary has persuaded him that Afro-As. belongs to the big Phylum he calls Eurasiatic: it's almost identical to Nostr. (as for Dravid., Greenberg compares it with Nilo-Sah.; Ivanov writes about genetic unity of Nilo-Sah., Niger-Kordof. and Afro-As., and Starostin, a few years ago, made a report about Nostr. character of Niger-Kordof. I see no serious objection against including of all these languages into Nostr., especilly in the light of I.-S.'s good sets [he includes Drav. into East-Nostr., alongside with Alt. and Uralic], Tyler's Uralo-Drav. sets, etc.) As for IE-Uralic grouping (Greenberg, and earlier scholars): it seems, Uralic is archaic and transparent; I.-S. gives very many Ur.-Drav.-Alt. isoglosses showing closer relations of these three languages; what disturbs me somewhat, is the lack of the 1st pers. *mV in Drav. And now about alleged lack of close relations betw. IE and Kartv.: if we look through the preliminary list of Nostr. comparisons (found in I-S's files: 1st vol. of the Dict., pp. 5-37) we already find many stablest Kartv. forms as having closest connections with IE: K. *me/*mi 'I' : IE *me; K. *se-/*si- 'thou' oblique stem : IE *-s ending of the 2nd pers. sg.; K. *m- 1st pl. inclus. (obj. marker) : IE *me-s 'we'; K. *naj 'we' : IE *ne-/*nō- 'we' (in oblique cases); K.,IE *te- 'this'; K. *(h)e : IE *He- demonstr.; Kartv. *maj : IE *mo- interrog. (note parallelism of the forms: maj : mo- = naj 'we' : nō ,etc, betw. K. and IE);K. *mā/ō : IE *mē prohib., etc. K.-IE parallelism in stem structures (ablaut, etc.) which Gamkrelidze explains as substratum or adstratum is, in reality, common inherited feauture typical for West Nostr. languages. Many, allegedely borrowed from IE, Kartv. words (see Gamkr. and Ivanov's IE and IE-s) is, no doubt, Nostr. inheritance in both K. and IE: these words do not become subject to borrowing, they are stable, and their phonetic correspondences fit Nostratic.

Now about precision of Nostr. reconstructions; even confronted with new data they are precise (because I-S and D made them on real correspondences betw. languages they analysed): see excellent pro-Nostr. article by Xelimskij in VJa 1986 (should be translated into Engl.; he shows how clumsy the critics of Nostr. are: his article is directed against Sčerbak's objections concerning the Altaic unity, etc.). Dybo wrote several methodically important article showing precision of I-S's reconstructions; he showed, e.g., that I-S correctly explained the origin of IE triad of the type $k:\acute{k}:k^w$ (velar : palat. : labiovelar) on the basis of Nostr.: when compared with East Nostr. languages, IE words beginning with *k,*g, gh correspond to Ur.-Drav.-Alt. words in Ka-; IE *ḱ-, *ǵ- *ǵh- correspond to East-Nostr. words in KE- (E = front vowel); IE $*k^w$-, $*g^w$-, $*g^wh$- - to East KU- (U = lab. vowel). This is because East-Nostr. languages (more archaic in this respect) show the underlying Nostr. structure Ka-, KE-, KU- accordingly. IE had this change: Ka- >Ke-; KE- >ḱe, KU->K^we-.

Not less important was the realization that IE voiceless consonants correspond to Kartv. and Afro-As. (Sem. etc.) glottal stops (sic!); IE voiced - to Kartv. and Afro-As. voiceless stops, and IE voiced aspirated stops - to Kartv. and Afro-As. plain voiced (cf. the corresponding triad in reconstructed Altaic: Tˁ- T- D- in anlaut). This is one of the Nostr. theses of paramount importance, supported by hundreds of excellent sets of correspondences (and one may propose better reconstructions for IE, e.g. T [tense] : T [lax] : D instead of T : D : Dh, but the correspondences will stay as they are: Nostr. T' > Kartv./Afro-As. T' : IE T [or T, for that matter] : Alt. Tˁ-, etc.; see M.Kaiser's and mine paper in the last issue of General Linguistics. I invite anybody to discussion on this subject (one must have in mind I-S's and D's statements about deglottalization in certain cases in Afro-As., as well as the known rule about impossibility of T-Dh, Dh-T in one root in IE : hence Nostr. *K'erd- > IE *kerdh- > *kerd-).

Now, if we take Nostr. words with, say, initial glottal stops and compare them with apparently cognate words of other (macro)families, we should try and find out if the non-Nostr. words have T' as well. So, for instance, North Amerind languages show, indeed, glottal stops: and this might be the common Amerind archaism preserved in North Amerind. And when Merritt and myself started to compare Nostr. and Amerind words having glottal stops, they matched. One of many examples is Nostr. *K'ujnV 'wolf, dog' (I-S reconstructs *K'üjnA, see Dict. I, p. 361) and Amerind *K'uan 'dog'. In both languages we must postulate K' = k' or q' because it is not clear what consonant (k' or q') was here; for Nostr., only Kartvelian has preserved the pair k' : q' ; in Amerind, many languages have this distinction but they did not preserve the word.

This is one of many examples which show how much more precise can be comparisons between phyla if exact sound correspondences are established. In Greenberg-Ruhlen's preliminary reconstruction of Amerind (which is highly important in itself) there is no distinction between many "individual" phonemes: e.g., k and g, k and q, l and ɬ, l and ƛ' , n and ń etc etc etc. And it is also clear why: many intermidiate reconstructions (Penutian, Hokan, Uto-Aztekan and many, many more) are lacking. This job will require a lot of efforts (that's why I'm trying, for a decade or so, to create a research group to reconstruct Am. Indian languages, etc. But now it is clear that Americans won't do this job, but Russians would). I am not against global comparisons: but we must have in mind at least two things: 1) language groupings (try to look up each time same languages or those belonging to certain groups; so we'll have material for further reconstructions), 2) try to establish sound correspondences (take again the example with the dog-word: it is important to limit the range of words under comparison: if, say, Nostr. has t', than Amerind should show t' - unless we establish a different correspondence). This is not a very difficult task, but it is important: it'll make our comparisons less amorphous.

Still, I would prefer to make even more systematic comparisons first, to establish isoglosses (important for further groupings and reconstructions of proto-proto-languages). So, I'm waiting with interest for Nikolaev's Amerind-Macroasiatic (Austric) comparisons; he thinks it was a dialectal grouping (may be with some other languages?). The other important development is Starostin's comparisons betw. Nostr. and Dene-Cauc.; another possible grouping. It is more important to establish such ancient groupings first (preliminary as they are), to reconstruct their proto-languages, and then, on the basis of these reconstructions, we can try and reconstruct the "proto-proto-proto". Three ancient families which remain beside the four above phyla should be considered as having phyla-status each (Australian, Indo-Pacific, Khoisan). In this way we'll be able to penetrate as far into the past as 30,000 years, or so. Making just global comparisons for comparisons' sake, without trying to establish sound correspondences, taking each time different languages, will force us to stay on Trombetti's level.

Yours, Vitaly

Allan Bomhard's view of Stanford Conference L21

WORKSHOP ON LINGUISTIC CHANGE AND RECONSTRUCTION METHODOLOGY

July 28, 1987, to August 1, 1987
Stanford University, Stanford, CA
Linguistic Society of America 1987 Summer Institute

The Workshop on Linguistic Change and Reconstruction Methodology organized by Professor Philip Baldi and held at Stanford University from July 28, 1987, through August 1, 1987, brought together nearly 40 scholars representing the following language families: Indo-European, Afro-asiatic, Altaic, Native American, Austronesian, and Australian. These scholars were asked to discuss important issues in the reconstruction of the linguistic history of the particular language family in which they specialized, focusing on the following issues:

1. What are the patterns of linguistic change and the factors influencing linguistic change in each language family?

2. How useful are such notions as phonetic regularity, morphological conditioning of sound change, analogy, borrowing, areal influences, etc.?

3. What techniques of reconstruction (comparative, morphological, internal, etc.) are most useful or not useful at all?

4. How far back can one reasonably expect reconstruction to reach?

5. What about distant linguistic relationship? Can it be established for particular language families, and with what techniques?

The following is a listing of the invited specialists broken down by language family:

1. Indo-European:

   Alfred Bammesberger, Eichstaett, West Germany
   Eric Hamp, University of Chicago, USA
   Robert Beekes, University of Leiden, The Netherlands
   Allan Bomhard, Boston, USA
   Henry Hoenigswald, University of Pennsylvania, USA
   William Schmalstieg, The Pennsylvania State University, USA
   Calvert Watkins, Harvard University, USA

2. Afroasiatic:

   Lionel Bender, Southern Illinois University, USA
   Alice Faber, University of Florida, USA
   Robert Hetzron, University of California, Santa Barbara, USA
   Carleton Hodge, Indiana University, USA
   Stephen Lieberman, Philadelphia, USA
   Paul Newman, Indiana University, USA
   Russell Schuh, University of California, USA

3. Altaic:

   Robert Austerlitz, Columbia University, USA
   Larry Clark, Sacramento, CA, USA
   Samuel Martin, Yale University, USA
   Marshall Unger, University of Hawaii at Manoa, USA
   John Whitman, Harvard University, USA

4. Native American:

   Lyle Campbell, SUNY, Albany, USA
   Ives Goddard, Smithsonian Institution, USA
   Michael Krauss, Alaska Native Language Center, USA
   Margaret Langdon, University of California, San Diego, USA
   Jeffrey Leer, University of Alaska, USA
   Marianne Mithune, University of California, Santa Barbara, USA
   Pamela Munro, University of California, Los Angeles, USA

5. Austronesian:

   Robert Blust, University of Hawaii at Manoa, USA
   James Collins, University of Hawaii at Manoa, USA
   Isidore Dyen, University of Hawaii at Manoa, USA
   George Grace, University of Hawaii at Manoa, USA
   David Zorc, Washington, DC, USA

6. Australian:

   Barry Blake, Monash University, Australia
   Robert Dixon, The Australian National University, Australia
   Jeffrey Heath, Exeter, NH, USA
   Steve Johnson, University of New England, Australia
   Geoffrey O'Grady, University of Victoria, Canada

The workshop sessions lasted three full days: Tuesday through Thursday. A general session was then held all day Saturday, during which time the section leaders summarized the salient points brought up by the speakers, relating how each of the papers addressed the main issues around which the workshop was organized. The general consensus was that the time-honored methodologies of Diachronic Linguistics (that is, the Comparative Method and Internal Reconstruction), established first in the Indo-European domain, were not family-specific but, rather, were applicable to all language families.

There were, it almost goes without saying, many points of dispute as well. For instance, within Indo-European, a heated discussion developed around the Glottalic Theory, with various participants taking a strongly favorable position and others taking an equally strong opposing position. The Native American group, on the other hand, devoted considerable attention to Joseph Greenberg's new book **Language in the Americas**, with almost all of the group taking a rather negative view of Greenberg's proposals. Then there was a sharply-worded clash between two members of the Austronesian group on how the subgrouping of the Austronesion languages should be approached. The Altaic group, in contrast, was almost bland in its unanimity of opinion -- to a person, each of the Altaic specialists argued against setting up an Altaic language family. (This does not mean that the individual papers presented by the Altaic specialists were bland or uninteresting -- on the contrary, they were all first-rate and highly stimulating.)

Though not a part of the workshop itself, the Collitz Lecture by Joseph Greenberg took place on the evening of the first day of the workshop. His presentation, entitled "The Prehistory of the Indo-European Vowel System in Comparative and Typological Perspective", aroused considerable interest and discussion.

One of the contributions presented at the workshop dealt directly with distant linguistic relationship. This was Allan Bomhard's paper on "Lexical Parallels between Proto-Indo-European and Other Languages". This paper, consisted of a discussion of the common vocabulary shared by Proto-Indo-European with five other language families, namely, Afroasiatic, Kartvelian, Uralic, Dravidian, and Altaic. After a brief description of the phonology of the parent languages of each of these families, there was a discussion of methodology and the applicability of the Comparative Method as envisioned by Joseph Greenberg to the problem of distant linguistic relationship, an analysis of root structure patterning in Afroasiatic, and a discussion of the 405 lexical parallels proposed by Bomhard. The paper aroused both interest and skepticism.

THE STANFORD CONFERENCE: As seen by Hal Fleming.

It is an "ambush" when one blunders into a situation through ignorance or whatever and one gets attacked. Of course, there is no ambush without someone setting it up secretly. If one is caught in an ambush and one's group is slaughtered, then one uses the word "massacre". So it was at Stanford. While most of the other scholars were tending to the task at hand, the evaluation of Indo-European historical methodology as it applied to their respective phyla, the Americanists had an ambush in mind. They came to attack Greenberg and the other Lumpers and to establish -- at this fairly prestigious conference -- that distant genetic relations could not be attained and ought not be sought after. I regret that this is not a florid or inaccurate description of five days on that lovely campus. It is an ethnographic conclusion, from participant observation.

There seem to be two underlying reasons for the massacre. First, the Amerind Border Patrol, especially Campbell, Goddard and Mithun, were well organized in advance, co-ordinated their activities (papers), worked hard, attacked very aggressively, and found their opposition virtually speechless and unprepared. Campbell's attacks on Greenberg became personal and vile. For example, I heard a quote something like this..." Greenberg is lucky that he had Stanford University Press to publish his Amerind book because no one else would have touched it!" This was not said in the corridors between formal sessions, or over cocktails after hours; it was said during a formal session -- and it shocked the audience.

Secondly, the attack had two primary scientific facets to it and both of them could have been rebutted. However, Greenberg would not defend himself. He's not a conference brawler and does not like confrontations. (Who does?) His friends could not defend him, or indeed counter-attack, because of intellectual confusion and simple social fear. (Rare indeed is the scholar who will argue publicly with a loud, aggressive expert, especially on the expert's own turf!) The two prongs of the attack were actually discussed in Ruhlen's book where he devoted much space to refuting them. Had everyone read his book before the conference the massacre might have been a more ordinary, albeit loud, scholarly debate. But Greenberg's potential defenders were stupified most of all by their own beliefs in the validity of the Border Patrol's argument!

In brief, they argued that: (1) reconstructions of obvious families are necessary before distant genetic connections can be proposed or believed in. Nobody knows what "distant" means because what is distant to one of the current Americanists would not be so to others. Moreover, it is not modern Indo-European methodology with its stress on reconstruction, appropriate to its maturation, which actually created the great phyla of the world, nor is it reconstruction which causes scholars to believe in them. Also (2) they said that: anyone can pile up bunches of etymologies as between any two languages. So bunches of etymologies prove nothing. "One can throw mud at a barn and some of it will stick to the barn." Therefore etymologies are useless or something like that. I've written about this in a forthcoming article in DIACHRONICA. You are invited to read it. My conclusion is that the etymological argument is ridiculous, when one thinks about it, and should have been challenged when Dyen first used it against Benedict.

More or less by accident, there was a massacre in the Altaic section. Those who favored the Altaic hypothesis did not come to the conference; those who vigorously opposed it did come. At the end there was in fact no Altaic hypothesis left. The Altaic section had reached a consensus to abolish itself. Yet this was not what I would call an ambush, just a massacre. Some of the former Altaicists did become excited and sought to start a fit of phylum bashing in the other sections. Luckily, in my opinion, the other scholars lacked any good reasons for demolishing their phyla, even though three of them were very large and wobbly entities (Austronesian, Australian, Afro-Asiatic). So the local massacre did not become a general phylocide.

ANOTHER "TOO ANCIENT" SITE FROM SOUTH AMERICA.

There is more archeology to report -- from the remarkable series on "The First Americans" which has been running for two years now in NATURAL HISTORY, published by the American Museum of Natural History, New York City. This is another site from South America, and quite far south, in a relatively untouched region (archeologically). It too suggests that the MacNeish cum Gorman hypothesis of over 30,000 years of human residence in the New World is true or that the "received" or "conservative" or "orthodox" archeological first entry dates of 13,000 years are false.

While there has been good and useful input from archeological Long Rangers (e.g., Wilmsen, Petruso, Trigger, Rouse, Zimansky), we still have not received a single comment on the Amerind dating questions raised in Mother Tongue-3. Therein, Ed Wilmsen commented on a wide range of sites and dates; I can hardly ask him to repeat himself. How about other archeologists? Speak!

Professor Niede Guidon, lecturer at the Ecole des Hautes Etudes en Sciences Sociales (Paris), is an expert on prehistoric art. "She is now directing a French-Brazilian interdisciplinary project to trace the interaction of humans with the environment in that part of Bazil from Ice Age times to the present." That part of Brazil is in the Piauí-Bahia 'zwischen Gebiet', near the village of Sa~o Raimundo Nonato, circa 500 miles nor'nor'east of Brasilia. On my map that looks like the continental divide between the Rio Parnaiba watershed and the watershed of the Sa~o Francisco river. In more general terms, while the area is in northeastern Brazil, it is basically on the eastern flanks of the great Amazonian basin. If one follows Brazilian Indian culture area maps, this is the northern part of the large eastern area with few or no ethnographic Indians in modern times.

The excavations reported here are at Pedra Furada rock-shelter and represent one of a series of efforts to date the several rock art styles associated with 240 sites spread along 120 miles of the cliffs (sometimes 800 feet high) forming a "spectacular border between two contrasting geological zones". There were six rock art styles recognized, three of painted figures and three of engravings. She has been working on this problem since 1970 and now there are 35 archeologists, geologists, ecologists, etc., on the French & Brazilian team. By 1980 they had dates ranging from 12,000 to 25,000 years ago, which "challenged the generally accepted notion that people entered the New World by way of the Bering land bridge shortly before 12,000 years ago."

One art style, called Serra Talhada or "Northeast rock art tradition", yields expected end-Pleistocene dates of 12,000 to 6000 years ago. But underneath that they found a Pedra Furada cultural phase which lasted from 32,000 to 17,000 years. There was a 5000 year gap between Pedra Furada and Serra Talhada which represents lack of occupation of site, rather than absence of humans from the area, because a nearby rock-shelter (Toca do Sítio do Meio) yielded artifacts between 15,000 and 12,000 years old.

Finding hearths associated with art and human artifacts is better than just finding hearths. Part of the report says: "The most ancient possible vestiges of painting dated so far are some red marks found on chunks that fell from the rock-shelter wall and were found within layers 32,000 to 27,000 years old. These pale traces cannot be deciphered because they are too fragmentary and damaged by the elements. More clear-cut dated evidence of painting comes from the end of the Pedra Furada phase, 17,000 years ago. This is a single hearth around which have been found a few stone artifacts. Some pieces of wall with red stain were found in this layer. One of the fallen chunks, used to border the hearth, bore two straight, parallel lines on its underside. Several pieces of red ocher and yellow ochre have also been found in the Pedra Furada phase. Based on these finds we can say that the antiquity of art in the Americas approaches that of Europe, Africa, and Australia."

Physical Anthropology

What Is Lost with Skeletal Reburial? II. Affinity Assessment.

By Christy G. Turner II

Prehistoric Hunter-Gatherers in Japan. New Research Methods. (1986) TAKERU AKAZAWA and C. MELVIN AIKENS, editors. University of Tokyo Museum Bulletin No. 27. Tokyo, Japan. 234 pp., 66 figs., 13 plates. ISBN 0-86008-395-0. ISSN 0910-481X. $62.50. In English.

This review is the second of a series of three that illustrates some of the sorts of information that would be lost with human skeletal reburial. The foremost concern of this series is with the content of the literature reviewed but at the same time the reader's attention is drawn to the issue of reburial. The first review (*QRA*, 1986, 7[1]) considered published works that focused chiefly on skeletal elements that can be classified within the theoretical framework of adaptation. The third will discuss evolution.

The issue of reburial transcends local interest groups and their politics. It strikes this reviewer as belonging to that rapidly growing class of important worldscale environmental issues that we are encountering more and more, namely, when do finite resources belong to all peoples, not just local, regional, or national divisions? When should a group or nation have to cease an activity because it is damaging, destroying, or using up something that logically (although not necessarily ethically or legally) belongs to all peoples, not just those asserting ownership? This reviewer maintains that because prehistoric human skeletons are the only record of past human evolution, and because all humans are members of one species, that this record rightly belongs to all peoples.

Affinity assessment is a rapidly growing aspect of physical anthropology concerned with objectively and precisely measuring the degree of similarity between populations. In most morphological comparisons the degrees of similarity can be interpreted as estimates of genetic relatedness. As will be seen in the following, it is important to use traits for affinity assessment that have a high genetic component in their expression, otherwise the similarity matrix may be little more than measures of environmental similarity. This may serve some archaeological purpose, but for reconstructions of population history little is gained from knowing non-genetic similarities. These and other ideas about affinity assessment will be developed in the course of the present review of Akazawa and Aikens.

Prehistoric Hunter-Gatherers in Japan, a carefully edited and cleanly produced collection of papers, is the latest in a series of very high quality monographs from the University Museum, University of Tokyo, of which Yukio Nose is Editor-in-Chief. The volume results from a symposium organized by Takeru Akazawa for the XIth International Congress of Anthropological and Ethnological Sciences in Vancouver, British Columbia, 1983. Fumiko Ikawa-Smith served as symposium commentator, but her lively contribution is not included.

The volume contains two sections plus the editors' introduction and summary conclusions. Section I contains archaeological contributions on Jomon hunter-gatherer subsistence and settlement. There are four papers here by the editors, K.M. Ames, D. Sanger, H. Koike, and K. Suzuki dealing with comparative north-temperate settlement evolution, palaeobiomass, midden nutritional analysis, and regional variation of food procurement. Section II has five physical anthropological contributions involving various live body, skeletal, and dental observations on Ainu, Japanese, and Jomonese secular changes, epigenetic relationships, origins, and oral health. The authors are M. Kouchi, Y. Mizoguchi, Y. Dodo, N. Inoue, G. Ito, T. Kamegai, and N.S. Ossenberg.

The stated objectives of the collection are (1) to apply new analytical methods to the gigantic body of anthropological and archaeological data in Japan, (2) to let the international community of scholars know about archaeological and anthropological research in Japan, and (3) to improve our understanding of Japanese genetic origins and affinities. The editors and conributors do a fine job with objectives 1 and 2, but number 3 has rough edges, so this review will focus on it.

The majority of the *QRA* readership is undoubtedly familiar with Japanese prehistory and aware of (a) the vast amount of archaeological work that has accompanied industrialization, and (b) the long-standing controversy caused by the presence in Japan of two biologically and linguistically distinct populations — the numerically dominant Mongoloid Japanese, and the remnant Paleo-Asiatic Ainu. The morphogenetic differences between the Japanese and "unadmixed" Ainu are about as great as those between, say, Australian Aborigines and recent Australian Whites, and certainly more than the differences between Aleut-Eskimos and South American Indians — populations that have been genetically separated at least 10,000 years. The White migration to Australia is so recent and historically well documented that no one would ever think of claiming that Australian Whites evolved in place and share a common ancestor with Aborigines. But, just such a view is held by some scholars of Japanese prehistory. That is, some workers believe that recent Japanese *and* Ainu are descended from the aboriginal prehistoric Jomonese despite the major Japanese-Ainu differences. The only human skeletal remains found in Japan that date before 300 BC are Jomonese. These remains are like unadmixed Ainu in every character which can be compared. Moreover, both Jomon culture and biology changed very little over thousands of years until 300 BC when Yayoi culture begins in southwestern Japan. This culture is patently Sinomorphic, that is, mainland in origin, and is focused on the complicated but highly productive wet rice agricultural system. The Yayoi people were biologically distinct from the Jomonese, being much more Mongoloid in skeletal and dental characteristics. In most respects Yayoi bones and teeth are indistinguishable from those of recent Japanese. After 300 BC Yayoi culture and people spread northward, replacing the Jomon cultural and population system. Japanese archaeologists have unequivocally demonstrated a direct cultural linkage from Yayoi to recent Japanese. As already noted, the Ainu can be linked biologically to the Jomon population. Thus, the parallel with Australian population history is almost exact except in timing and sourcing. Both Australia and Japan had Holocene aboriginal hunting and gathering populations that were largely replaced by later unrelated agriculturally-based immigrants. In both areas some admixture between the aborigines and the later immigrants occurred as has happened with dispersal and contact events everywhere in the world from Greenland to Tasmania. So why all the excitement and controversy about Japanese origins? Perhaps the historians of Japanese science will someday find the answer. For now it is sufficient to recognize the simple fact that there are scholars who deny the evidence for a basically dual biocultural heritage of recent Japanese and Ainu. As the editors note in their introduction (p. x) this dual origin hypothesis is the one most favored by international scholars, including this reviewer. But, the editors reject it on the grounds that it "does not accord well with many of the actual facts of the case," and the dual origin hypothesis "fails to provide any significant illumination of cultural process and the interaction between human biology and culture." These are strong words. Let's see if they are backed up by the material in this volume.

Co-editor Aikens and associates K.M. Ames and D. Sanger start the papers with a north-temperate comparison of affluent collectors in Japan, New England, Northwest Coast, and the Baltic of Europe — all well separated so that no cultural exchange could have occurred. Cultural similarities are striking and parallel evolution is reasonably invoked. It is concluded that Japan and the Baltic area took up agriculture because these regions had more time for population growth than did those in the New World. Omissions abound. There is not one word on the fact that agriculture was unquestionably introduced to the Baltic from the south, and possibly by populations biologically and linguistically unrelated to the earlier Baltic hunters and gatherers (Riquet, 1970; Ammerman and Cavalli-Sforza, 1984 and elsewhere). There is no mention of the dual origin hypothesis or how the very complicated wet rice agricultural system began in Japan. Fatally, the authors ignore their own review of Jomon prehistory which they abundantly document as remaining completely stable for thousands of years. If stability was the rule for so long in Japan, why did agriculture explosively expand at the moment a new population can be recognized bearing mainland-evolved exotic seeds, new settlement patterns, metal, other mainland features, and possessing genetic characteristics never before seen in Japan? Facts such as these are needed to evaluate the authors' thesis that agriculture can be expected to develop in the north-temperate zone if there is enough time and people. In my view, there is nothing in this essay that overthrows the dual origin hypothesis (it is not even mentioned), nothing that betters our understanding of how biology and culture interact, very little is proposed about cultural process that doesn't already exist in introductory anthropology textbooks, and absolutely nothing that helps our understanding of the initiation of agriculture in Japan or anywhere else in the world. Finally, I find it misleading, if not worse, to conclude "...Japanese and north European societies went on to even higher levels of societal complexity, ultimately to develop feudal states and embrace agricultural economies" (p. 21) without any discussion about the strong possibility these so-called advances were mainland introductions, not independent transformations of Jomon society and economics.

The next paper by H. Koike assesses Jomon middens near Tokyo by powerful quantitative methods. Jomonese predation on clams and terrestrial vertebrates is found to have increased from earlier to later Jomon times, reaching levels comparable to those of modern commercial shellfishery in the research area and on Hokkaido wild deer populations of today. Koike's paper is must reading for anyone needing methods for estimating human economic activity from food refuse. However, there is nothing in this paper that addresses the third objective of bettering our understanding of Japanese origins and affiliation.

K. Suzuki provides another midden analysis paper, which, like that by Koike, is a most valuable contribution for evaluating prehistoric human economic activity, but, like Koike's work, there is nothing here that deals with Japanese origins and affiliation.

The last archaeological paper is by co-editor, T. Akazawa. Here, we finally get some discussion of origins and affiliation. Akazawa nicely demonstrates the regional variation in Jomon procurement practices as evidenced by varying tool kits, other lines of evidence, and at the same time sets up a reasonable basis for arguing that the idea of rice agriculture would have been more quickly *received* by western Jomonese because of their experience with laurel forest zone plant products than by eastern Jomonese who were likely more rigidly regulated by seasonal scheduling of maritime resources. That is, sea products were mainly available when rice plants would have required much attention.

Akazawa concludes that the "transition to rice agriculture in Japan" was related to "the extent of cultural readjustment that was needed" (p. 86). In other words, he accepts that rice agriculture originated on the Asian mainland but its *adoption* in Japan was accomplished variously by the indigenous Jomon population. This places him squarely in the camp of those who reject the Ainu-Japanese dual origin hypothesis. As far as I can tell, the leading proponent of a single-origin view is H. Suzuki who has argued that the Ainu-Japanese differences arose by environmental effects. Since there can be only two ways for the differences to arise — new genes were either introduced or they were not — Akazawa has joined the environmental determinists.

Secular change in stature and head shape (length x breadth) of almost 3000 young adult male students is examined by M. Kouchi and related to geography throughout Kyushu and Honshu. Some significant geographic variation is found and it differs from earlier findings supporting the view that bodily changes are occurring. Kouchi suggests that similar changes may have taken place in the past but at a much slower rate.

Is there anything in this collection that addresses the origin and affinity issue? Clearly not. The traits chosen for study have been long known to be highly plastic and respondent to environmental effects, a classic study being the work of Shapiro (1939) on Japanese migrants to Hawaii. Modern workers concerned with affinity usually avoid these sorts of traits. Furthermore, all contemporary studies on affinity are based on much larger numbers of traits, and increasingly with traits whose genetic bases have been assessed by at least one or more Mendelian or quantitative genetics techniques. Where batteries of traits have not had some manner of inheritance assessment, such as those used by Howells (1966) or Ossenberg (1976) there are other lines of evidence to suggest that, as a set, they manifest more genetic than environmental information. Kouchi's findings have important implications for national health considerations, but they are of but limited utility for taxonomic purposes.

The next paper, by Y. Mizoguchi, employs a powerful statistic called path analysis, developed 50 years ago by S. Wright, to assess Japanese affinity. Using published values for eight cranial measurements in more than 2000 individuals belonging to 26 Asian populations, Mizoguchi compares these groups by path analysis and other statistics applicable for assessing affinity. All three methods agree that the Jomonese and Ainu have a strong similarity. Also, the Chinese and Japanese are very similar. Mizoguchi also finds that the Yayoi resemble mainland Asians (Neolithic Baikal) more than they resemble Jomonese. Diachronic Japanese series post-dating Yayoi times show, by path analysis,

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ongoing change.

Despite what amounts to being the most sophisticated analysis in this volume, Mizoguchi concludes that the origin of modern Japanese is in the Jomon population. He attributes the differences between Japanese and Jomon-Ainu as due to environmental influences. Could not the increasing Mongolization he sees as occurring after Yayoi times be due to sexual selection? Hulse (1967) demonstrated that Japanese mate preference existed for skin pigmentation. Suzuki (1956) showed the same thing for facial features. As Ossenberg will splendidly demonstrate in her contribution to this volume there must have been both temporal and latitudinal gradients for the Jomon gene pool, as well as puddles of greater concentrations of Jomon genes in mountainous areas less easily adapted to wet rice agriculture. Mizoguchi simply has not faced up the the issue of whether mainland Asian genes were introduced in Yayoi times. If it is admitted that they were, then Mizoguchi's entire analysis matches perfectly what has happened in the United States with the westward spread of European genes. The residual western Indian populations correspond to the remnant northern Ainu. Some white communities have more Indian admixture, and there are pockets of Indian genes here and there all across the United States. The proto-historic and historic genetic history of the United States, Siberia, Australia, Canada, and probably elsewhere, offers perfect and overwhelmingly documented examples of inter-population competition with associated population replacement, displacement, gene flow, admixture, and gene pocketing. It is not parsimonious to call on some unknown environmental effect to explain the differences between modern Japanese and Ainu. There are simply too many examples around the world that match the genetic prehistory and history of Japan, and in each it is perfectly natural inter-group competition for resources — what evolution in the past has been all about. It is more logical to have an accurate understanding of the past so that we can learn from it, not to continue making the same "mistakes" that arise from historic revisionism. Such revisionism could easily occur in the absence of prehistoric or historic skeletal remains.

Next, Y. Dodo examines metrical and non-metrical traits in crania from Jomon sites in the Tohoku district of northern Honshu and southern Hokkaido. The battery of 21 non-metrical traits used is one that has been found to produce highly reliable affinity assessments elsewhere in the world (Ossenberg in this volume provides useful commentary), that is, environmental effects on trait frequency are minimal, probably random, and almost certainly canceled out by the assembling of regional series as Dodo has done here.

Dodo's principal finding is that Jomon and Ainu are more alike than either is like modern Japanese. The Ainu are slightly less dissimilar to the Japanese than are the Jomonese, a finding wholly consistent with the recent settlement of modern Japanese in Hokkaido and intermarriage with Ainu there. A cluster analysis of non-metrical traits in the Ainu, Jomon, Japanese, and mainland Asian population samples is definitive. The Ainu and Jomon cluster together, and very distantly so, from all the other groups which form their own distinct and separate branch. Dodo concludes that the Ainu are most likely descended from the Jomon population, while the modern Japanese belong to the same clear-cut Mongoloid category as other mainland Asian populations. Both eastern and western Japanese are greatly more like non-agricultural Mongols than like Ainu or Jomonese — hardly a relationship that can be attributed to similar environmental effects. Rather, it is one that must be due to the possession of many identical genes.

The most advanced study in population dental pathology that I have ever seen is provided by N. Inoue, G. Ito, and T. Kamegai. These workers examine almost 20 features such as malocclusion, caries, attrition, periodontal disease, etc. in more than 200 crania, ranging in age from Early Jomon (>5000 yBP) to Kofun times (1800-1400 yBP), obtained from scores of sites on Honshu and Kyushu. This huge amount of data is then reduced by various multivariate statistics to eventually produce clusters of archaeological sites based on dental pathology. In addition to findings of special interest to dental anthropology, they can suggest that the Yayoi immigrants did not arrive all at once in a large group, but could have reached Japan as a number of small groups, apparently during the interval between the archaeologically defined older and newer Yayoi population.

This is fine-grain analysis, and addresses the issue of Japanese origins. Other clustering algorithms might produce slightly different results, but the matrices are clear. Late Jomon and Yayoi are vastly different. Such differences could not possibly have arisen without external influences in the short period of time between Late Jomon and Yayoi. This is not a gradualistic epidemiological shift; instead, it represents a wholly new but mature configuration.

The last paper is by N.S. Ossenberg who uses about two dozen non-metric cranial traits to assess the dual origin hypothesis. As did Dodo, Ossenberg also finds that the Ainu and Jomon are quite similar but vastly different from the Japanese who are very much like mainland Asians. Ossenberg refines her basic analysis and comes up with some beautiful clines that must represent the temporal and geographic wave front of the spreading Sinomorphic Yayoi-Japanese gene and culture pool. Ossenberg concludes that the Ainu "have retained the largest genetic endowment from Jomon," and her clines show that "people of western Honshu are more closely descended from continental immigrants" (p. 212). In other words, Ossenberg fully supports the dual origin hypothesis. (For archaeologists interested in the value of non-metrical skeletal traits for affinity studies, I cannot think of a better paper to recommend than this extremely well-written effort by Ossenberg.)

Now, what do the editors make of all this? In their introduction they assert that they and the contributors do not agree that the "original Jomon population of Japan [had to be] the direct ancestors of the remnant Ainu people, and the modern Japanese [are] descendants of continental immigrants who swept over the country as the bringers of the Yayoi culture" (p. x). In their conclusions (p. 221) they begrudgingly allow that mainland gene flow had occurred, but now they assert that it could have been earlier than late Jomon times and continued on into the present. Where, we must ask, do they get this new information? Certainly it is nothing that was presented in any of the papers of this volume. Where the burr seems to be sticking is in the simple but fundamental proposition that during Yayoi times there was definite migration by a sufficient number of mainlanders to forever change the prehistoric culture of Japan. Whether this was caused by 10,000 or 1,000,000 immigrants hardly makes any difference at this stage in our understanding of East Asian prehistory. (I use "East Asian" intentionally because the transformation of Japan's prehistoric culture, physical anthropology, and apparently language [Japanese is unrelated to Ainu], was not an event unique to Japan.) China was in political turmoil during Yayoi times. Had this turmoil spilled over the seas to affect Japan? More than just a chance boatload of mainland Asians underlies the fact that Yayoi people severely impacted the Jomon culture, people, and language, all of which had evolved in greenhouse fashion, isolated from the rest of the world for thousands of years. How could just a few people bring about the near extinction of all Jomonese, replace them, and change a way of life that had worked so well for thousands of years? In my view the central problem of Japanese archaeology is now to learn precisely how the mainland change stimulus was delivered.

It is assumed on little more than geographic proximity that Korea was the source of the Yayoi immigrants, their rice, and other mainland products, skills, and traditions. But now that the editors admit the fundamental (simple) truth of the dual origin hypothesis, they should be asking all the corollary questions. For example, where did the Yayoi folk actually come from? P.K. Benedict (1986) has recently published his linguistic analysis of Japanese and proposes that it originated in South China, rejecting Miller's (1971) Altai-Japanese hypothesis. This new linguistic proposition suggests a far more important historic event than what we might imagine, had a few Korean farmer-fishermen accidentally beached a storm-broken boat in southwestern Japan and settled down to begin the Yayoi lifeway.

Consistent with Benedict's new view on the South China origin of Japanese language is a recent analysis of the reviewer's on several samples of East Asian teeth. Frequencies of 26 largely independent crown and root traits such as incisor shoveling, molar cusp and root numbers, and others, were used in the multivariate Mean Measure of Divergence statistic to calculate the degree of similarity between various pairs of Asian populations. A small MMD value indicates greater similarity than a large one. As can be seen in Table 1, the Yayoi dentitions are most like those of South China and Hong Kong, and unike Jomon teeth. Similarly, Recent Japanese teeth are much like those of Hong Kong and South China, and least like Jomon teeth. In other words, there is no dental support for Yayoi or Recent Japanese having arisen from the Jomon population, and slightly more support for both having had a South China rather than a North China origin. It is even possible that other mainland Asians moved into Japan after Yayoi times since the Recent Japanese and Yayoi MMD is rather large. However, my Yayoi sample is very small and must be enlarged before such fine-grain interpretation can be considered statistically sound.

Table 1. Mean Measures of Divergence in Asian Populations*

	Yayoi	Ainu	Jomon	Hong Kong	S China	N China	Japanese
Yayoi	[illegible]						
Ainu	[illegible]	[illegible]					
Jomon	[illegible]	[illegible]	[illegible]				
Hong Kong	[illegible]	[illegible]	[illegible]	[illegible]			
S China	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]		
N China	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	
Japanese	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]

*[illegible]

I believe *Prehistoric Hunter-Gatherers in Japan* is a landmark volume because it definitively shows the dual origin hypothesis to be correct. No more time needs to be spent on this question. The Ainu are descended from the Jomonese, and the Japanese origin was somewhere on the Asian mainland beginning in Yayoi times. Work can now shift to determining precisely where the Japanese immigrated from, figuring out the magnitude of the initial Yayoi migration, working out the mechanism(s) of Yayoi expansion, estimating the exact amount and direction of admixture that occurred between Jomon and Yayoi folk, and similar processual, adaptive, and biohistorical questions. If researchers will turn to these sorts of problems then the editors' plea for advancing our understanding of cultural process and the interaction between human biology and culture will be responded to with richly detailed, bountiful, and exciting results. Japan specialists, worldscale prehistorians, and methodologists should all be as grateful to the editors as I am for making available these important papers on Japanese archaeology and physical anthropology.

The difference between the editors' introduction and conclusions concerning the dual origin hypothesis nicely illustrates the power of affinity assessment in skeletal populations for aiding the reconstruction of culture history. By now it hardly needs to be said that skeletal reburial would prevent or limit the use of this methodology to understanding our past evolution, dispersal, and group formation. ☐

Note. **Table 1 is based on research made possible by grants and other assistance to me from the National Science Foundation (BNS 83-03786), National Geographic Society, IREX (International Research and Exchanges Board), USSR Academy of Sciences, and very many cooperating institutions and individuals in Japan, USSR, USA, and Hong Kong. I am particularly indebted to K. Hanihara (University of Tokyo), T. Suzuki (Sapporo Medical College), and J. Ikeda (Kyoto University) for recent assistance.*

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GUESSING GAME or What is STAROSTIN's ROOT DATING all about ANYWAY?

Here is a short summary of what I think Starostin's dating method is all about, mostly because I am tired of waiting for Starostin and Militariev to send me a summary or anything. My hunch is that they want to publish the whole book at once and get full credit for it. Perhaps my version of their Root Dating will be so distorted that they will feel a pressing need to correct my errors -- by writing to me!

Root Dating, as I call it, seems to be based on counting true cognates instead of counting how many ancestral forms have been retained in the 100-200 arbitrary meaning slots set up by Swadesh. Morris himself did not intend that his system would evolve as it did, such that one held his 100 basic words as 100 basic meanings against which change was measured. As his title implied, he was interested in "archaic residues" as well as "diffusional cumulations". But because his method got involved in glottochronology and fixed formulas and efforts to standardize his list, so that comparisons could be more exact, his list got to be frozen and items such as "All, ashes, bark, belly,..." became fixed meanings and not just the current American words for those meanings. So, if a current American word became different from the original on Swadesh's list, one could also describe change lexicostatistically in American English. For example, one could argue that {guy}, {a lot of}, {rock} had replaced the earlier {person}, {many}, {stone} on Swadesh's list in the frozen meanings of "person, many, stone". {Person} itself is becoming feminized in American English and may yet displace {woman} in the meaning of "woman". The key point is that we were forced to distinguish between a fixed meaning like "stone" and the actual morpheme in use for it, like {rock}. And hence we would have to say that "retention" is negative or zero in the case of (frozen meaning) "stone", even though the old Germanic word {stone} was still very much alive in American English.

The difference between a retention in a fixed meaning and a retention somewhere of an old word was missed by many scholars. It led to the erroneous statements that, since any language will lose 16-20 % of its basic vocabulary every millennium, after 15 millennia any language will have only about 7.3% of its basic vocabulary left (at 16%) or as little as 3.5% (at 20%). Since that is the "chance similarity" level established by Greenberg in 1953, ergo there is no hope of finding deep genetic connections, i.e., those older than 15,000 years old. Worse than that, however, are the chances of TWO languages retaining the same word. If 80-84% are kept per 1000 years (because 20-16% are lost) in one language, two languages will only keep 64-71% per 1000 years. (That is 80% times 80% or 84% times 84%). So after 11,000 years two languages will have only 0.7% in common (at 64%) or at the most 2.3% (at 71%). And indeed at 7000 years the two will have only 4.4% or 9% left in common. Therefore long range comparison is impossible! Or so they say.

But, of course, the proposition above is false because it contains two serious covert statements which are false. The first is that a language will lose 16-20% of its basic vocabulary every millennium and each millennium it will lose 16-20% of its ORIGINAL 100 morphemes of basic vocabulary. Oh, no, no. It doesn't do that. It loses 16-20% of whatever it starts out each millennium with, AS MEASURED ON SWADESH's LIST. Let us say that English starts out in 100 AD with 100 Germanic morphemes on its basic vocabulary list. By 1100 AD it will have lost say 20 Germanic morphemes which will have been replaced by 20 English morphemes. Between 1100 AD and 2100 AD English loses another 20 morphemes but only 16 of them are Germanic; 4 of them are English. So after two millennia

English on its basic vocabulary list has 64 Germanic words, 16 English words, and 20 innovations or replacements which we can call American. At 3100 AD our language will have lost another 20 words but only 12.8 of them will be Germanic, while 3.2 will be English and 4 American. So after 3000 years we will still have 51.2 Germanic words. Everyone agrees to that. And we will have lost 48.8 Germanic words, n'est-ce pas?

Wrong! It will have lost 48.8 words FROM SWADESH's LIST. Most or many of those will still exist in the English language but OFF SWADESH's LIST. It seems that the problem arose because of the use of words like "loss" or "retention". When a word is lost from the Swadesh list, it may have died out in the language altogether, but it may have only moved slightly away from the position of dominant form in the frozen meaning. Thus, if {rock} becomes more common or popular for the meaning "stone", then {stone} becomes an alternative morpheme or it may acquire a specialized meaning like "be stoned" or indeed it may itself displace some other morpheme from a frozen meaning position, like {die} displaced {starve} which had indeed displaced {*die} from the same position in earlier times. What is retained in English, as opposed to the Swadesh list, is 51.2 still on the list plus X which are alive but off the list. Can X be calculated in advance? I don't know but in principle it can be COUNTED just as readily as the items on a Swadesh list. Let us look at English again and also consider German. One must also be careful to specify Standard American English and Standard Hoch Deutsch. The results would be different if we used dialects or varieties as different as Lowland Scots of Bobby Burns and local Swiss valley dialects.

Using etymological dictionaries carefully, I find (as many others have found) 71 English-German cognates on the Swadesh 100-list. More precisely, the following happens 71 times. I begin with the frozen meaning "all". English {all} occupies the position. So does German {alle}. They are cognates so we count one positive or one common retention on the Swadesh list. Later I come to frozen meaning "belly". English {belly} occupies that position, while German has {Bauch}. They are not cognate so we count one negative or one lack of common retention on the Swadesh list. So eventually I end up with 29 "losses" on the Swadesh list, i.e., when I examine 29 frozen meanings I do not find a German morpheme or word which is cognate with the English occupant of the position. Yet in a number of cases there is a German cognate known but it is not on the Swadesh list or there is an English cognate off-list which matches the German form on-list. If there were 29 English cognates known for the 29 unmatched German Swadesh items, as well as 29 German cognates known for the 29 unmatched English Swadesh items, I might actually find 58 English-German cognates still alive in the two languages! Theoretically, then X = 58 is a possibility and 51.2 + X = 109.2 Germanic cognates still alive is another. It is wonderful after 3000 years to end up with more than we began with!

However, English is one of the great borrowing languages of the world. Many of our potential 58 cognations are defeated by the presence of a Romance or Scandinavian loan word on the English Swadesh list or among the English alternatives or off-list potential cognates. Often the older Germanic form has been chased out of the spoken language, although one can still find it in dictionaries. But I count words not found in spoken language as DEAD or REALLY LOST because this exercise is aimed at a global inquiry where most languages do not have dictionaries of their old lost words. The loan words in question are 10 in number: {bark, big, die, egg, give, mountain, person, root, round, skin}, most of them showing the impact of the Vikings on English. Of the 19 non-loan items some of them are innovations in English whose sources are unknown or whose German cognate is difficult to unmask, e.g., {bird, black, dog, cloud, kill, meat, road, snake, tail, tree, walk (?), woman (but not {wife}), thou (replaced by {you})}. Still 9 of the non-loan items in English find German off-list cognates, to wit, {Balg, Bein, haupt, kennen, Laub, Nacken, schmal, Schmauch, das}. Conversely, when we look at the 29 German Swadesh words which lack an English Swadesh cognate, we find that 17 of them find English non-Swadesh or off-list cognates, to wit, {rind, great, fowl, knuckle, starve, hound, cup, dead (for "toten"), wise/wis-dom, blade, flesh, way or street, hide, reek, beam, thou, yon}. Some other English cognates like {barrow} for {Berg}, {wort} or {wale} for {Wurzel}, and {clean} for {klein} are not counted because it is reckoned that these very specialized or archaic words should not be counted or like "clean" for "small" are too hard to believe semantically.

What do we have then? Empirically, English and German have 71 Swadesh 100-list cognate pairs after 1200-1600 years of separation (I cannot seem to get it settled whether it was closer to 400 AD or 800 AD when the split began.) But we also easily find 9 English-German and 17 German-English sets or 26 cognate off-list pairs. Therefore, there are 97 detectable, indeed easily detectable, etymologies on the Swadesh list. This is roughly 37% more than we find by using the usual Swadesh list definition of "retention". This also means that long range comparisons ought to be possible or more productive at dates older than 15 millennia. And, naturally, if we are dealing with 5 or 50 languages, the amount which can be retrieved is much greater than it is for just two languages. (See Greenberg's Amerind book, p.341-44, for some numbers when one compares up to 20 languages.)

Something like this can then be used for dating too. If English and German turn up 97 detectable cognates on a Swadesh 100-list after 1200-1600 years of separation, then maybe this is true of other languages too. This may be the key to Starostin's ROOT DATING. One could calculate that in one millennium they would display even more cognates, say 1200-1600 divided by 1000 times 97, or 135.8. Yet that appears to be ridiculous because the maximum number of cognates on a Swadesh 100-list at ZERO years of separation is 100! The answer probably is then that Starostin has abandoned the Swadesh list altogether and counts ALL detectable cognate pairs in the languages. I do not know more than this and that itself may be way off. We will have to wait for his eventual publication or response to this. Or maybe someone else can figure out how to do this Root Dating.

If Root Dating works something like this, it has some advantages over Swadesh's glottochronology. Most of all Root Dating does not favor quick trips across lists of similarities in frozen meaning positions; on the contrary it favors the knowledge of cognation or at least careful study of what are likely to be cognates. Its etymologies improve with age, as reconstructions make it more and more possible to reject or accept proposed cognates. Almost as importantly, it escapes the arbitrariness of decisions about which morpheme or word is currently dominant or more popular in some frozen meaning slot. We don't have to debate {rock} versus {stone} anymore. Both are in the language, but {rock} is always negative as far as Germanic is concerned.

However, there is an enormous uncertainty in this, again assuming the whole procedure is roughly that sketched above. How far apart can two cognates drift apart SEMANTICALLY before we stop counting them as cognate? It takes some knowledge of history to be able to say that {clean} and {klein} are cognate. But we do not have that kind of information as between, for example, Kafa and Anfillo (North Omotic). And therefore KNOWLEDGE of the languages becomes an even more significant factor in determining THE COUNT OF COGNATES, not just the "real" number of cognates. IE languages would always be closer than Omotic?

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The Neandertal Problem

From the first recognition of Neandertal man in 1856 his precise evolutionary position has been a source of debate, and the discussion still continues. One of the long-standing puzzles has been their supposed sudden disappearance from the fossil record: their origins and their relationship to a group that has come to be known as anatomically modern man (a.m. *Homo sapiens sapiens*) have been equally difficult problems.

So called 'classic' Neandertalers were first known from European sites and were associated with the Mousterian culture. Their sudden or 'catastrophic' disappearance has been variously attributed to epidemics, conflicts with more advanced peoples, changes in the climate, or to their absorption into the gene pool of incoming migratory peoples. If the Neandertalers did disappear suddenly, and the idea has been seriously questioned (Brace, 1964), there seems no imperative reason to look for a single cause: a combination of circumstances seems possible, even reasonable.

The number of Neandertal fossils from Europe and the Near East has grown remarkably and now includes several hundred specimens. Sites such as Krapina (*q.v.*), Amud (*q.v.*) and Shanidar (*q.v.*) have added enormously to the sample known from the classic sites such as Neandertal (*q.v.*), La Chapelle-aux-Saints (*q.v.*), and La Ferrassie (*q.v.*). The new site of St Césaire (*q.v.*) has revealed a Neandertal burial with a Châtelperronian industry that seems to be of the most recent Neandertaler yet known, at about 30,000–35,000 years B.P.

Clearly the question of the use and definition of terms is of crucial importance when a group such as Neandertal man is being discussed. Hrdlicka (1930) took a cultural view stating that Neandertal man and his period was 'the man and period of the Neandertal culture'. Brace (1964) added a morphological dimension to this later by stating that Neandertalers were 'the men of the Mousterian culture prior to the reduction in size and form of the Middle Pleistocene face'. The dangers of associating hominid categories with cultural traditions, however, have now been exposed by finds such as St Césaire and Jebel Qafzeh (*q.v.*).

Morphological definitions given in the past by Boule and Vallois (1957), Thoma (1965), Le Gros Clark (1966), and Vandermeersch (1972) would limit the term Neandertal to Western European examples of 'classic' morphology. Brose and Wolpoff (1971), however, gave a temporal and morphological definition that included 'all hominid specimens from the end of the Riss to the appearance of a.m. *Homo sapiens*.' This view has been criticized by Howells (1974) and Stringer (1974). Howells used the term 'Neandertal' to include only European 'classic' Neandertal sites plus Tabūn, Shanidar and Amud. He excluded Skhūl (*q.v.*), Jebel Qafzeh (*q.v.*), Jebel Ighoud (*q.v.*) and Petralona (*q.v.*) as well as sub-Saharan finds such as Kabwe (*q.v.*) and those from the Far East such as Ngandong (*q.v.*). In general this view was shared by Stringer (1974) and has become widely accepted.

Trinkaus (1983) defined the Neandertals in general terms as 'a group of Archaic *Homo sapiens* from Europe and western Asia who lived from the end of the last inter-glacial to the middle of the last glacial and shared a set of morphological characteristics that have traditionally been called "classic Neandertal". Lists of these morphological characteristics have been given by Vandermeersch (1972), Heim (1978), Le Gros Clark and Campbell (1978) and Stringer, Hublin and Vandermeersch (1984). These can be epitomized as follows:

1. An inflated skull form with its maximum transverse diameter mid-parietal, a low frontal bone, a suprainiac fossa as well as an occipitomastoid crest. The face is large with voluminous orbits and nasal cavities; the skull is also extensively pneumatized. The mid-face is prognathic showing a retromolar space and the supraorbital torus is divided centrally. There is no chin and the teeth are frequently taurodont. Postcranially the distal limb segments are short with large extremities, the scapula has a dorsal axillary groove and the superior pubic ramus is flat and elongate. In general the skeleton provides evidence of a short, thick-set, muscular individual with large hands and feet and a body form not unlike that of cold-adapted modern man.

2. Some of the features mentioned above share a common inheritance with earlier forms such as *Homo erectus*; others are new characters that are shared between contemporary hominids which, on this basis, can be defined as Neandertal. In the interpretation of Neandertal body form in relation to the environment, however, all of the information is of importance and its combination of features is of taxonomic significance.

3. The history of the Neandertals and their evolutionary position has been given recently (Spencer, 1984). The story is revealing in showing how discoveries and events have influenced the views of scholars over the years. In general terms the phylogeny of Neandertal man can be summarized in three ways at present, the *Neandertal Phase of Man Hypothesis*, the *Preneandertal Hypothesis* and the *Presapiens Hypothesis*. The first two of these are widely held whilst the third is less well supported and is losing ground.

The Neandertal Phase of Man Hypothesis

This view is unilinear and gradualist. It sees the Neandertalers as arising from a Middle Pleistocene predecessor by successive evolution and passing through a Neandertal phase to become modern man. This suggestion was first made by Schwalbe (1904) who saw the Neandertalers as a separate species intermediate between ape and man. Later supporters of this hypothesis (although not quite in the same terms) included Hrdlička (1930) and Weidenreich (1943, 1949). After a period when other views prevailed, a new impetus was given to this theory. Brace (1964 et seq. to Brace *et al.*, 1984) suggested that dental and masticatory evolutionary changes were brought about by tool use which led in turn to cranial morphological changes from Neandertal to modern sapient forms. Others who accept this general hypothesis include Brose and Wolpoff (1971), Wolpoff, (1980), Frayer (1978, 1984) and Smith and Ranyard (1980). In central and eastern Europe Smith also sees local continuity and change between Neandertal and later modern sapients (Smith, 1982, 1984); he discounts the other two current hypotheses and regards the Neandertalers as reasonable candidates for the ancestors of modern Europeans.

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The Preneandertal Hypothesis

This view suggests that the Neandertals arose from a 'Preneandertal' stock that became progressively specialized for resisting cold, underwent severe natural selection and restricted gene flow that led to 'classic' Neandertal isolates exemplified by La Chapelle, La Ferrassie, Neandertal and many others. This specialized Neandertal offshoot represents a group sharing new traits of subspecific taxonomic value. Supporters of this approach include Sergi (1953), Howell (1957), Breitinger (1957), Le Gros Clark (1966), Howells (1975), Hublin (1978), Santa Luca (1978), Stringer (1974, 1978), Trinkaus and Howells (1979), Stringer and Trinkaus (1981). The most recent supporters of the bilinear approach often see that the 'parent' line may have developed outside Europe, and at present Africa is the best candidate for the origin of the Preneandertal line on the basis of early examples of *Homo sapiens* known from Omo (*q.v.*), Laetoli (*q.v.*) and Border Cave (*q.v.*) (Bräuer, 1984a & b, Stringer, Hublin and Vandermeersch, 1984).

The Presapiens Hypothesis

This view holds that a European modern sapient lineage, as exemplified by Swanscombe (*q.v.*) and Steinheim (*q.v.*), existed quite separately from the Neandertals and ultimately gave rise to modern Europeans. The Neandertals then became extinct at the end of the Early Würm Glaciation. The hypothesis originated with Boule (1911/13, 1923) and was taken on by his successor Vallois (1954) as well as others including Weiner (1958), Thoma (1965), Leakey (1972), Vlček (1978) and Saban (1982). Gradually it has become apparent, however, that the Swanscombe and Steinheim skulls also possess Neandertal traits and are no longer widely acceptable as 'anatomically modern' and separate as a lineage (Stringer, 1974; Hublin, 1982; Bräuer, 1984a; Smith, 1984).

These three views of the origins of a.m. *Homo sapiens sapiens* are clearly a simplification—perhaps an oversimplification (Spencer, 1984)—of the various theoretical approaches that have been put forward by the authors cited, but they provide a framework within which to consider the phylogeny of this phase of human evolution.

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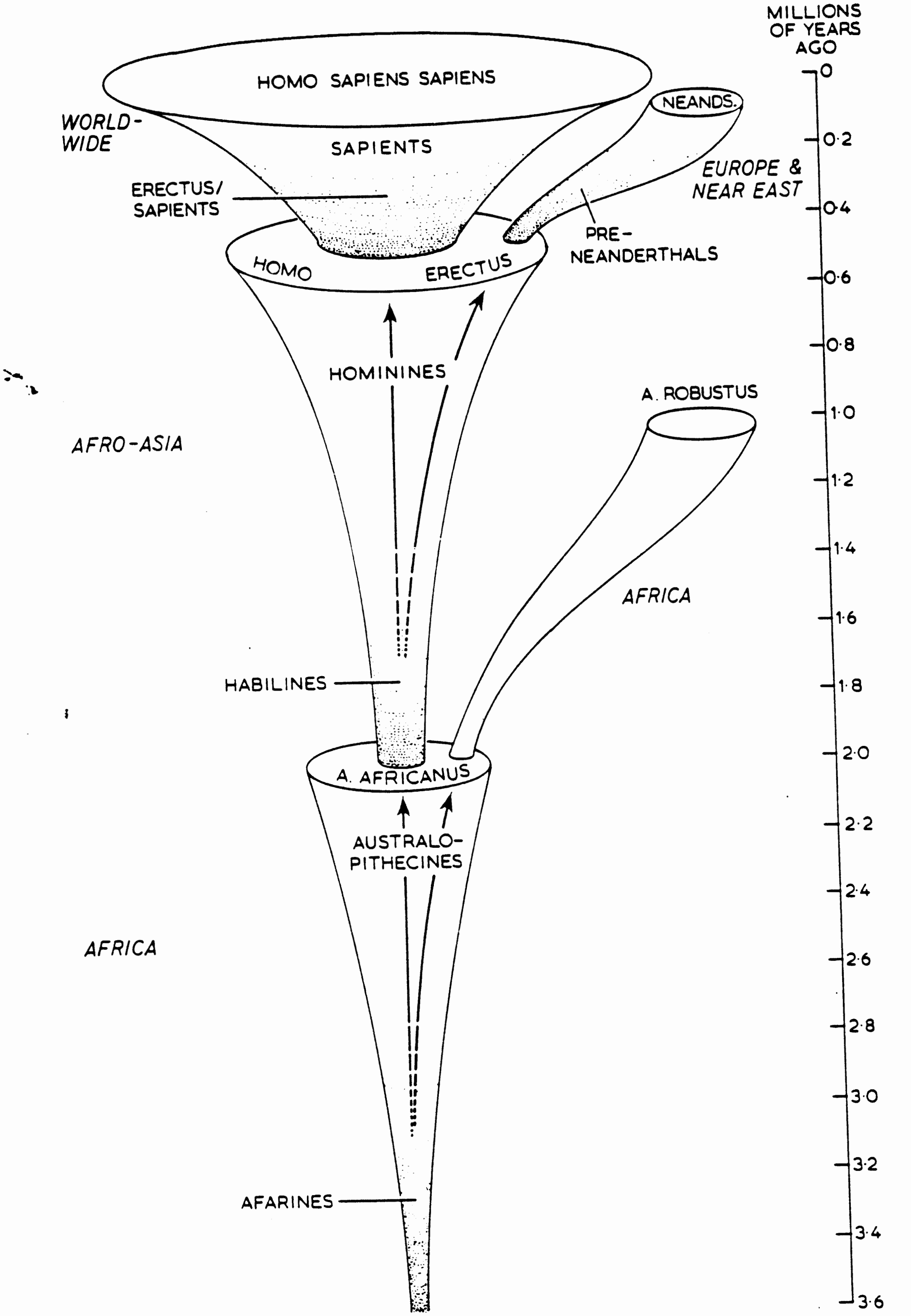

Fig. 133 A phylogenetic representation of the hominid groups of the Plio-Pleistocene period and their likely evolutionary relationships.

LIST OF NEW MEMBERS INVITADO & OLD LISTEES DROP OFF

Mostly upon the recommendations of Long Rangers, some new people were invited to join. At the moment they are all listed, although some have not yet accepted membership. They have received the first three issues, except for a few whose addresses still have not been located.

Linda Arvanites. UCLA. Cushitic, especially Eastern, especially Oromo.
Philip Baldi. Pennsylvania State U. IE, hist. ling. Organized Stanford Conferen
John Bendor-Samuel. SIL, Dallas, Texas. N-K. Has new book on N-C coming out.
Vjaclav Blažek. Přibam, Czech. Historical linguistics. The world is his oyster.
Roger Blench. Oxford U. N-K and most of Africa. Econ. development, hist. ling.!
Claude Boisson. U/Lyon. Sumerian, Dravidian, especially. Hist. ling.
Remy Bole-Richard. Cote d'Ivoire. N-K, N-C
Eric de Grolier. Paris. LANGUAGE ORIGINS SOCIETY. Primus inter partes.
David Dwyer. Michigan State U. N-K, N-C.
Karen Ebert. U/Marburg. IE, Chadic & Sino-Tibetan ! Hist. ling.
James Egan. Riverside, California. M.D. Gynecology. Renaissance man otherwise.
Alice Faber. U/Florida. Semitic, Afroasiatic. Hist. ling.
Ludwig Gerhardt. U/Hamburg. Chadic and N-C.
Stephen Jay Gould. Harvard U. History of science, biology, geology.
Adrian Hill. Mitochondrial DNA. We want to invite him! Anyone know his address?
Ephraim Isaac. Princeton U. Semitic, Afroasiatic.
Frank Kammerzell. Göttingen. Egyptology and Koptology.
Mary Lisa Kazmierczak. Boston U. Anthropological linguistics. Caribbean, USA.
Stephen Lieberman. U/Pennsylvania. Semitic, Afroasiatic.
Antonio Loprieno. Göttingen. Egyptian et al. Descriptive, theoretical ling.
Gabriel Manessy. N-K and N-C. Temporarily lost address.
Robin, Count de Lalanne, Mirrlees. Isle of Lewis, Scotland. Renaissance man.
Geoff O'Grady. U/British Columbia. Australian. Historical linguistics.
Roman Raczyn'ski. Praha. Hist. ling. probably. (Not contacted yet).
David Sapir. U/Virginia. N-K and N-C.
Kiyoshi Shimizu. U/Wien. Chadic & N-C.
Victor Shnirelman. Moscow. Archeology. Collaborator c Militariev & Diakonoff
John Stewart. Edinburgh, Scotland. N-K and N-C.
Stephen Tyler. Rice U. India, cognitive anthropology, Dravidian-Uralic links.
Keating Willcox. Boston U. Computer science. Also catholic reading interests.
Andrew P. Wilson. U/California, Berkeley. Primates primarily.

Upon the recommendation of members, the following will also be invited shortly. If you happen to see one of them, ask them if they are interested. Positive replies in advance are appreciated. The process of inviting is quite expensive, since three issues are sent to each invitee.

Johanna Nichols. U/California, Berkeley. Slavic, IE. Said to be + and -.
Henrik Birnbaum. UCLA (?). Hist. ling. Slavic, IE, Nostratic and more.
Robert Blust. U/Hawaii. Austronesian.
Paul Benedict. New York. Austro-Thai. Needs no introduction, has been featured
George Grace. U/Hawaii. Polynesian, Austronesian.
Robert Harms. U/Texas. Uralic, Yukaghir. Hist. ling. + TG descriptive/theory
Winifred Lehmann. U/Texas. Germanic, IE. Hist. ling. & Theory of hist. ling.
Wm. Schmalstieg. Pennsylvania State U. IE, seeks evidence /C'/ > /C-voiced/.
Wm. Smalley. St. Paul, Minn (?). Descriptive/theoretical, hist.ling. Miao-Yao.
David Zorc. Washington, D.C. Austronesian.

OLD LISTEEs, who have been asked to send back a Hal-addressed post card and who have not sent it back after several months, are presumed NOT to be interested in staying on the list. However, some took longer than the several months I allowed and some may be in the field, so a few people on this negative list are in fact positive. (Two positives surfaced last week and one today!). There were two reasons for trimming the list, of course; one was not to bother people who did not want to be on it but did not want to tell me so. In some cases listees were restrained by strong rules of politeness from demanding that their names be dissociated from such a Club. The other was that mailing Mother Tongue to negatives is a waste of money.

Ed Brovarski, Don Brown, Desmond Clark, Bojan Čop, Nick David, Abraham Demos, Derek Elderkin, Hailu Fulass, John Gumperz, Bernd Heine, John Hutchison, Oswin Köhler, Bhadriraju Krishnamurti, Joeseph Kruskal, Lee Ki Moon, Floyd Lounsbury, Roy Andrew Miller, Wilhelm Möhlig, Paul Newman, Gérard Philipson, David Sankoff, Doug Schwartz, Joel Sherzer, Vladimir Skalička, Songmoo Kho, David Stampe, James Woodburn, and Norman Zide. If anyone sees Paul Newman or Nick David, please thank them for their friendly cancellations.

MEMBERS' COMMENTS

MARK KAISER. Mark sent a most valuable list of translations from the work of Illich-Svitch -- several hundred Nostratic etymologies. I will, with Mark's permission, reproduce all of it in MT5 or MT6. Mark's gift -- which by the way is a proper labor of love -- is available to members before that time. Some of the details are left out of the etymologies, so that one should write to Mark if one is interested in knowing more about a particular etymology. This would be true, for example, of any purported proto-AA forms because proto-AA is not at all a mature reconstruction like proto-IE. Mark is still at his Normal, Illinois address. (We will publish no pun before its time.)

CLAUDE BOISSON. Claude has worked up a very nice manuscript which shows a large number of high quality (proposed) cognations between Sumerian and Dravidian. I believe that he has shown that a Sumerian-Dravidian genetic connection exists. However, that is not what Claude thinks. His belief is that the connection should only be considered a good working hypothesis and he wants no grand announcement of his discovery to be made. I respect his wish to be modest and I heartily agree that it is dangerous to propose any connections with Sumerian since so many (ostensibly) bad proposals have been made in the past and the pros (or Sumerologists) are fed up with junk etymologies. Such is in many ways a general condition of working with ancient written languages in the Near East. The pros or experts have great problems with stating what the basic data are -- phonetics, lexicon, morphology -- and so they tend to be a truculent lot, resentful of outsiders running away with and misinterpreting "their" hard won facts. As Paul Zimansky said of the Diakonoff-Starostin proposal to classify Hurro-Urartean with Northeast Caucasic, it takes a lot of guts just to say what all the Hurrian words for various things are. Nevertheless, I too have a right to an opinion on this matter. I think Claude's work is superb and it shall be recorded here in public that in 1987 he offered good reason to believe that Sumerian is related genetically to Dravidian.

VJACLAV BLAŽEK. While we and his Czechoslovakian colleagues have lost a good Long Ranger in Karel Petraček, still I think Karel would approve the ascent of his young colleague, Vjaclav, to recognition as a suitable companion of Key, Seto, Bengtson, Ruhlen, Bomhard, Shevoroshkin, et al, as a global etymologist. (If any of the rest of you feel insufficiently recognized as merely part of "et al", then do notify me!) He has sent me several pages of quite good etymologies, including criticisms and extensions of my "nose/smell" and "lightning". What impresses me most about his work, however, is the "feel for the stuff" that he has. ("Feel for the stuff" = intuitive prowess, in 20th century American English). There are real idiots out there, doing global or regional etymologies, and our critics love to point them out. So it is good to have a new and competent colleague. He will not generate garbage!

NEXT ISSUE. MUCH MORE OF MEMBERS' COMMENTS. MUCH MUCH MORE. There is a significant number of good meaty letters which have not been reported. Also let scores of you please forgive me for not answering your letters or even not acknowledging your contributions of money. I am a lousy, nay shameful, correspondent but in defense let me say it is habitual!

TIDBITS

VITALIJ SHEVOROSHKIN. Vitalij is making serious efforts to raise funds for our collective pursuits and to popularize the general topic of language origins. His fund is not connected to the Long Range Comparison Club but rather is called LANGUAGE AND PREHISTORY. It is possible to send money to Language and Prehistory, Slavic Department, University of Michigan, Ann Arbor, Michigan, 48109. Vitalij says that "It now has over $300 ($200 from our wellwisher and supporter Mr. J. Parkinson, Glendale, California, $100 from myself, some more from friends and colleagues); the money is used for some, most urgent, needs: e.g., for translation of an excellent article by Xelemskij (Khelimsky) on Nostr. (Vopr.Jaz. 1986), etc. for the above book. If we gather a few more hundred, we'll immediately start translation (our grad. students do it quite well) of important Russian articles for one more book: ANCIENT HOMELANDS AND MIGRATIONS. I already spoke with processors & publishers: we do need a few hundred, indeed, and no foundation, no university, is gonna help us!..."

Drawing upon the Russian (or simply European?) model of popularizing science, Vitalij has energetically sought to publicize Nostratic, and related topics, in the press. He has had "pieces" published about these things in the Detroit News and most recently (Nov.24, 1987) in the New York Times (Science section). But he'll never catch up with the medical science reporting which, here in Boston at least, publishes virtually every thought each medical researcher has, be it raw or cooked.

EUGENE HELIMSKY. Apropos of Vitalij's project, Khelimsky also known as (=aka in the standard American system of abbreviations) Xelimskij, wishes the transliteration of his Russian name in English to have the form of Eugene Helimsky. Okay, Gene! Helimsky you shall be! (How many versions of Evgeni, Eugene, Eugen, Gene are there anyway? Does Modern Greek have *Efgen-?)

MARY RITCHIE KEY. Mary reports on two gatherings which will be of interest to Long Rangers. The first is reprinted overleaf in toto. The second is, of course, well known to North American linguists, being the LACUS Forum. This summer (August 16-20) their meeting will be held at Michigan State University and will feature a "WORKSHOP" on "global etymologies or whatever terminology one might use for the ideas of migration that some of us are interested in." One should write to LACUS, P.O.B. 101, Lake Bluff, Illinois 60044 for more information. (This attracts me very much; malheureusement, it also conflicts with the Ethiopianist meetings in Paris which our Russian colleagues may be attending.)

LANGUAGE ORIGINS SOCIETY. Some of us met Eric de Grolier this summer at Stanford. He is tres aimable, as they say, and informative. We agreed that the LOS was doing the same thing we're doing in a different way. We thought it would be a good idea to cooperate and so we shall. First of all, when we get around to it one of these days, we are going to exchange membership lists. Some people already overlap but mostly LOSers are the "hardware guys" while our task is showing the evolution of the "software" in its cultural context (?). Perhaps our meeting place ultimately will be in "paleoanthropology" (aka physical anthropology ?)

Hal, do you know of this?! Cheers. Mary Lue

THE WORLD CULTURES OF ANCIENT AMERICA

An International Conference

- Epigraphic Evidence from the Old and New Worlds
- Norse Tifinag and Runic Inscriptions
- Celtiberic and Arabic Inscriptions
- Pacific Voyages and Linguistics
- Archeoastronomy
- Native American Ethnology and Medicine
- Mythic Themes and New World Petroglyphs

This is a preliminary notice to members of the Epigraphic Societv. The meeting is tentatively scheduled for the Spring of 1988 in San Francisco. Please write to have your name placed on the conference mailing list, and to receive further details.

Dr. Jon R. Polansky, Room U-536
Conference Organizing Committee
University of California, San Francisco
San Francisco, CA 94143

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COMMENTS:

COMPUTER QUESTIONNAIRE.

We have a small sub-committee which is interested in computers and discussing computer sharing of data and mutual interaction or conversation by computer. Two members, Stanley Cushingham and Joe Pia, have devoted quite a bit of time to writing up some ideas on the subject. They will have a discussion in MT5, with comments also by Allan Bomhard, Sherwin Feinhandler, and perhaps Gene Gragg. To start with, there is a serious information gap. We do not know very much about you-all in terms of computers. Do you use them? Do you like them? Do you want to share data by computer? And so forth. Some people think that most scientists, especially linguists, like computers and that their minds are naturally atuned to them. Others, like me, think of computers as basically a typewriter and nothing to be interested in for its own sake. If it doesn't get the job done easily for you, why bother with it? Others actually hate or fear computers. Roughly we have Computer Freaks, Plain Users, and Computerphobes.

Will you please be kind enough to react directly to these questions and send it back to me PRONTO ? No doubt, a lack of response shows a Plain User or a Computerphobe but the two are different. Most importantly, however, you might be excited by a computer network, sharing data and ideas with other Long Rangers. If you don't answer, your interest will not be known. And that will no doubt help to ABORT a network or other cooperation .

Please/Bitte/Per Favore DETACH AND MAIL PORTION BELOW TO >>> Mother Tongue
69 High Street
Rockport, Mass. 01966

I OWN A COMPUTER ___ I USE SOMEONE ELSE'S COMPUTER___ I DON'T USE ANY____

I HATE COMPUTERS_____ I LOVE THEM _____ I JUST USE THEM_____

THE COMPUTER I OWN IS : IBM PC __ , IBM P/S ___, IBM CLONE ___

MINI: TYPE ______ MACINTOSH _______, APPLE_____, OTHER__________________

I MIGHT USE MY UNIVERSITY/INSTITUTION'S COMPUTER_______ IT IS A _______

I DO USE MY UNIVERSITY/INSTITUTION'S COMPUTER _______ IT IS A _________

I OWN A MODEM ______ I DON'T OWN ONE _____ I HAVE ACCESS TO A MODEM _______

WHAT IS A MODEM?______ I KNOW ALMOST NOTHING ABOUT COMPUTERS _______

I WOULD LIKE TO JOIN A COMPUTER NETWORK OF LRC CLUB ______ I WOULD NOT LIKE___

I WOULD BE WILLING TO SHARE DATA, VIA: COMPUTER NETWORK ___ ORDINARY WAYS ____

I DO NOT WANT TO SHARE DATA: VIA COMPUTER NETWORK ____ IN ANY WAY AT ALL ____

MY GENERAL OPINION ON THIS COMPUTER SUBJECT IS ________________________________

BY The Way: I AM Willing To Copy & Mail: To 6 Long Rangers in My Country___

I could do 12 in my part of the world ___ I could do 6 to the Soviet Union ___