MOTHER TONGUE

NEWSLETTER OF THE ASSOCIATION FOR THE STUDY OF LANGUAGE IN PREHISTORY

Issue 24, March 1995

CONTENTS

OFFICERS OF ASLIP

(Address appropriate correspondence to each.)

President: Harold C. Fleming
16 Butman Avenue
Gloucester, MA 01930
U.S.A.
Telephone: (508) 282-0603

Vice President: Allan R. Bomhard
73 Phillips Street
Boston, MA 02114
U.S.A.
Telephone: (617) 227-4923
E-mail: bomhard@aol.com

Secretary: Anne W. Beaman
P.O. Box 583
Brookline, MA 02146
U.S.A.

BOARD OF DIRECTORS

COUNCIL OF FELLOWS

ASLIP BUSINESS
PLUS IMPORTANT ANNOUNCEMENTS

We are putting the business of the Association at the front of this issue because of the importance of its being read. We do not wish to have scholars writing us later, saying they did not know what we were doing.

Please read on. It is important to you and to us.

The Board of Directors met on January 7, 1995, and had long discussions on all the points brought up in ASLIP Business in MT-23. Before reviewing that meeting, we want to state at once the things that concern members the most.

1. *Everyone's membership in ASLIP expires upon receipt of this issue.* If you see your membership as a subscription only, then that ended too.

2. *Everyone can apply for new membership in ASLIP under a new contract.*

3. *Mother Tongue has changed. It becomes a new Journal plus 4 newsletters.*

4. *Annual dues change too. Annual membership will be $25.00 everywhere.*

BOARD OF DIRECTORS MEETING

The Board met under the threat of ASLIP's demise. The key problem was how to liberate Allan Bomhard and Harold Fleming from the excessive labor involved in running ASLIP and producing *Mother Tongue*, while at the same time preserving the good work already achieved and increasing the quality of our enterprise. The discussions were long and the concentration intense. However, the Board and the officers are satisfied that the crucial problems have been solved. Here are the most important results:

First, we decided that Fleming's contribution was excessive; not only in the amount of time he spent but also in too much personal, colorful, or idiosyncratic style. The best solution to that would be a journal edited by someone else and playing more closely "by the rules".

Second, the main scientific thrust of ASLIP would be presented in a new *annual journal* which had peer-reviewed articles — that includes MT* Treatment articles — basically along the lines of *Current Anthropology*. The editors of that entity, *Mother Tongue: The Journal*, will be Allan Bomhard and Roger Wescott for the next two years at least. The journal will publish original articles, full data reports without peer review, and reports or summaries of important symposia. While it is published annually for now, it is possible that it will expand in the future, depending entirely on the flow of articles. The Journal aims to be on the frontiers of human origins science and will hesitate to publish articles which merely tidy up small points.

Third, the more newsy parts of *Mother Tongue* will be presented in a new short publication, *Mother Tongue: the Newsletter*, to be published four times a year, roughly Spring, Summer, Fall, and Winter. Letters and sundry comments from members, small arguments and discussions, announcements of things of interest to members (from other members), and short blurbs on significant discoveries in human origins science — all will be included. This will be edited by Harold Fleming, with luck soberly or at least not too flamboyantly. The Newsletter is intended to be short and will not publish long heavy articles. Since it is essentially dependent on information given by colleagues, it may be very short indeed if nobody tells the editor what s/he is doing or thinking.

Fourth, what one member has described as "trawling the media for curiosa", i.e., attempts to keep members informed on developments in a wide range of fields, will not be a feature of the newsletter. That feature was aimed primarily at those who had little access to major journals in our key disciplines and at those excessively blind to other disciplines. It has proven too time consuming for one person. *Significant* discoveries still will be reported briefly in the Newsletter.

Fifth, however, individual members are encouraged to send in summary reports on developments in some discipline or part of one. These can be published in either the Journal or the Newsletter, depending on size and significance.

Other matters of importance decided at the Board meeting include these:

Membership. The meeting rejected the suggestion that ASLIP should discourage "passive members" and encourage active researchers and their communication. The changes made in our plans (see above) removed the strains on time and effort which prompted the suggestion in the first place. Still, for three reasons, we decided to ask for application forms for new members. They are:

(a) The information on the applications is vital to the operation of our peer review system. In order to determine what experts are available to review or "critique" a submitted article, we must know our collective expertise.

(b) The application form when filled out is a tacit acceptance of a new contract with the new member. So many publications are expected and so much money is asked in dues.

(c) Since ASLIP is a scientific organization, we reserve the right to reject some applications — for whatever reasons. We still do not want to be mistaken for a magazine subscription.

Dues. The meeting was caught in a mild dilemma. Apparently we were offering less but charging more, as a Japanese comrade kindly pointed out. Here again there were three reasons for increasing the dues by two-thirds.

(a) Members would be getting more in publications.

(b) The quality of the Journal increased and thus its value.

(c) USA postage costs have gone up sharply. Domestic mail now must pay 45% more for an issue the size of MT-23 and it must go First Class. International mail has doubled in price to some countries. Old members may recall that postage costs have always bedeviled *Mother Tongue*. For the Journal, we may resort to Surface mail, possibly, but not for the Newsletter.

Research. The Board acknowledged that most research was individual, except in biogenetics and archeology (often). While ASLIP should encourage the publication of research, especially in our Journal, still we would have to increase our monies tremendously in order to support actual research. But we will be searching for grants, gifts, and other sources of funding to support some research and our data base and other things. We do ask members to think about money they have left over, or someone they know who has extra money, for possible donations to our research. It is tax deductible for Americans, of course, since we are a non-profit corporation.

Apropos of gifts: Hal wants to thank individual members for their donations to the "printer fund". We made the full $300.00 and paid off the debt. Merci!

Data Base. Paul Whitehouse of London has a fine idea for building up a data base which will be portable and salable. Rather than attempting to follow the increasingly high standards of contemporary linguistics, he suggests that we do an introductory and practical thing, instead of trying to share huge dictionaries and full grammars. Like many really good ideas its virtue is simplicity, utility, and universality. He suggests we gather together in one accessible place approximately 5000 Swadesh lists to begin with. That is either 100 or 200 words from each of the different languages of the world. Later on we could add as much as we wanted to individual languages or add dialects for more insight or make universal lists of other things (e.g., body parts, primary numbers, etc.) as desired. His suggestion is printed below.

One advantage immediately pops into mind. If we were following the debate between Ruhlen and Goddard, for example, most of us could turn on our computers and *look to see* if certain pronouns occur in certain areas or not. Would that not be useful?

European Membership. We began in fellowship with Europe and Israel, then extended that around the world. However, due to the printing trouble our new journal would cause Ekkehard Wolff, we are relieving him of this chore. There will no longer be a European distributor as such, unless at some later date we arrange to send copies en masse to one person for distribution by mail. Our European colleagues who wish to re-join ASLIP are naturally welcome; but they must send their applications to the Secretary of ASLIP, Dr. Anne Beaman, P.O.Box 583, Brookline, MA 02146-0005, USA. Europe in this context includes the Czech Republic, Hungary, and Israel. We hope to find a central bank or means for Europeans to pay their dues to ASLIP without losing 50% of that money to banks around the Atlantic Rim. The money transfer problem is worldwide, of course. Greedy little buggers, these bankers and their fees!

Russian Membership. Except for Igor Diakonoff, our eight-plus years special relationship with the Muscovites appears to have ended. We send, but they do not respond. Accordingly, if someone from Moscow or Georgia wants to be a member of ASLIP, let him/her apply for it. Payments or the equivalents can be discussed and arranged, but the free lunch is over. For Lithuania and Ukraine, let our old arrangement prevail, but do send in your applications.

The Rest of the World. In the ten other countries where members reside, the existing arrangements shall prevail, except for the applications and dues. We would very much like to encourage recruitment in the Middle East, India, and China, providing dues arrangements can be made. Our previous policies prevented our expansion in those areas because we were too poor to extend free memberships to large numbers. We hope that members will help us recruit in, for example, India and China, where many interested scholars live. Again, if anyone has extra money to give away, we could expand ASLIP in Asia greatly — given enough dollars, DM, francs, quid, or yen.

•••

The ASLIP Annual Meeting and Board of Directors Meeting, including election of the Board and officers of ASLIP, will take place as follows:

DATE:	Saturday, 6 May 1995
TIME:	12:00 noon
PLACE:	African Studies Center , 4th floor Boston University 264 Bay State Road Boston, MA

OBITUARIES

We regret that we have to report the deaths of two good colleagues, Professor Abraham Demoz and Professor John Rittershofer. Ethiopian studies noted the loss of Susan Park, Eike Haberland, and Vinigi Grottanelli. We were unable to obtain the obituary of Dr, Demoz on time for this issue, but we do present the obit prepared on Dr. Rittershofer by his wife and Susan Lederer. We only knew him through our correspondence, but he seemed an estimable fellow.

JOHN SWING RITTERSHOFER (1941-1994)

Susan Lederer wrote in November, 1994, to do John's obituary at the request of his widow, Gloria Rittershofer of Yonkers, New York. Susan's remarks make a suitable introduction to the more formal statement.

"The request honors me as well as saddens me because John has entered history and cannot laugh and quip and tell me that what I am about to submit to you is just trimming, just froth ..."

"John and I were colleagues on the faculty of a community college where we both taught. His friendship was a rare gift, as, indeed John was a rare man. He was a scholar, a gentleman, as fair-minded and deep-thinking a man as you might hope to find anywhere the world over. Fact is, he had been the world over, or through most of it, and had shared his keen intellectual curiosity, his learning, his wisdom, his wit, and his spirituality with people in many corners of the globe. The enclosed page is one that was distributed at his memorial service after his death this past April. That was what John had wanted; therein resides John's spirit. (A practicing Buddhist, John had studied Zen at Dai Tokuji temple for three years.)"

"Born and raised in the Cincinnati, Ohio, area, John earned a Bachelor of Arts degree in English literature in 1963 at Dartmouth College. After a graduation tour of Europe, he traveled widely in the Pacific area between 1965 and 1972, beginning his teaching career in Papua-New Guinea, then in Kyoto, Japan. He returned to America in 1972 to begin graduate studies. John earned Master's and Doctoral degrees in Applied Linguistics from Columbia University, Teachers' College, publishing his dissertation in 1987 with the title of 'The Nominal Reference System in the Interlanguage of Writing in English by Japanese Students: A Discourse Analysis'."

"John was a linguist. He had formal training in Latin, French, Russian, Japanese, and Mandarin Chinese. He also had a speaking familiarity with Amharic, Korean, Demotic Greek and three New Guinea languages. He was a member of the Linguistic Society of America, of the International Linguistics Association (NY), of the Royal Society of Asian Affairs (London), and of the New York Academy of Sciences. He enormously enjoyed receiving *Mother Tongue* and felt that its mission was part of his quest too."

"At the time of his death, John was a professor of English at Hostos Community College, CUNY [City University of New York — HF]. He is survived by his wife Gloria, by his young daughter, Katrina, by his mother, and his sister. His memory is cherished. We miss him. He was one of the best."

Respectfully submitted,
Susan M. Lederer
45 East 74th Street
New York, NY 10021

[End of Susan Lederer's letter] The following poem was read at John's memorial service.

Bedside Remarks

(Instructions from *The Tibetan Book of the Dead* to a New England Boatman)

*The lights will draw lower until in the last breath
No sense of light or darkness remains at all;
The ever-steady hum of mundane motions
Will dwindle and disappear into muteness;
Breezes brushing at the door will calm,
The inner drumming of pulse will be stilled;
Heat will dissipate, the system disconnect —
Nerves, tissue, organs, all in descending spirals.
Grow accustomed to this envelope of quiet,
It will be your vessel these few short days.
Once you have entered the next passage,
Intimations of sense may appear to you,
Each form is its other, yet neither is the same.
Church bells will resonate as temple gongs,
A foghorn at sea will sound like an alphorn on the heights,
Waves slapping at a boat will be foaming currents
Carrying a yakskin boat down a mountain gorge,
Ice skates on a pond will become sleds on a glacier,
Fresh caught fish will smell like buttered salt tea,
The finish gun from a regatta judge's boat
Will resemble logs popping in a monastery fireplace,
Spring daffodils will fill the rooms with scent
As incense sticks at an altar alert body and mind.
These illusions are mirrors from earlier visits,
Do not pause to nourish them along the way;
Trust your inner logic not for action
But for the radiance of your clear true mind.
These instructions may seem difficult at first
And the path impossible to follow,
But you must banish your doubts and go on
As you take the final steps toward being free:
I am with you, you are with us; the cycle continues."*

One can detect underneath the words and sentiments the presence of a high-quality human life, now gone or now gone elsewhere, as you like. Thank you, Susan, for this obituary. One could hardly expect to have such evident love and admiration expressed in most cases; more evidence indeed that John must have been a very fine man.

REVIEWS OF CAVALLI-SFORZA ET AL'S *HISTORY AND GEOGRAPHY OF HUMAN GENES*

Preface to Reviews

THE REVIEWERS

Before we go into the essentials of our praise and criticism of this gigantic book (wgt = 3.4 kilos), we want to make it quite clear to everyone who reads our comments that we are not hostile to Luca or his colleagues. In fact, there is warm friendly feeling in each of our personal cases towards Cavalli-Sforza. Some of us do not know his co-authors personally, but that makes no difference because we all perceive *HGHG* as Luca's Meisterwerk. (Hal opts to call that [hâgâg] similar to "hug hug".) This may be the largest and densest book any of us has ever reviewed. It is packed with information like the Tokyo telephone directory or the Christian Bible. Dense! It is global in spatial reach and evolutionary in its deep time depth of 100,000 years. It makes a significant effort to account for major parts of human evolution all over the world since modern humanity emerged. There is a great deal of archeology and history, as well as linguistic taxonomy, in it. Rare is the book that tries seriously to document a subject of such scope.

Reviewers are taxed by such an enterprise. Go write a four page review of *War and Peace* sometime! A doctoral dissertation would be an appropriate scope for a review of *HGHG*. Clearly, any reviewer short of that standard has to be selective. And, in deference to the reader, she should say what she is reviewing and what she is not. Neither Rebecca Cann nor Frank Livingstone nor Hal Fleming will do a complete review. We say this in advance to save each other the bother of having to say it again inside. Becky concentrates on the view from the frontiers of biogenetics; Frank wants to examine questions of theory and interpretation, especially vis-à-vis hemoglobins and natural selection. Hal eschews their areas for the best of reasons — their greatly superior knowledge. While his forte is thought to be historical linguistics, it is actually culture history or the emerging synthesis. Fundamentally he is an ethnologist.

We also have a science to run. *HGHG* may very well turn out to be the bible of the emerging synthesis, especially in the next millennium. Therefore, we are obliged to be critical and forcefully so. Out of a sense of Popperian love, we need to bite and claw at and chew on *HGHG* — the better to suggest improvements in it and to depict sections worthy of being erased. We have no fear that our criticisms will defeat this magnificent book. (One should see the violent reviews of Charles Dickens works, now classics of English.) But to repeat, the criticisms have to be understood as friendly or our affection for the Meisterwerker will stifle all comment.

The first review is by Becky Cann and the second by Frank Livingstone; they are meant to show the opinions of colleagues in Cavalli-Sfora's own realm of biogenetics. The third review by Hal Fleming concentrates on historical reconstruction or the retrieval of prehistory; it shows some difficulties facing general readers.

•••

This following review was submitted to the *American Journal of Human Genetics* and published in *AJHG* 56:349-50, 1995. Becky also sent a copy to *Mother Tongue* with permission to publish it. *AJHG* permits its re-publication in *Mother Tongue.*

HGHG reviewed by REBECCA L. CANN, University of Hawaii

This book represents a landmark in biology. There is nothing of its kind, in either zoology or botany, where the evolutionary history of a single species possessing a cosmopolitan distribution is distilled from genetic, morphological, and cultural data. It represents an essential historical source for all human biologists, guaranteeing its importance in evolutionary biology. It will, no doubt, also become an essential tool for researchers in linguistics, demography, and anthropology. In reading it, I could only compare my excitement with that I remember feeling in 1982, when the first edition of *Molecular Cloning*, the Cold Spring Harbor Laboratory manual (Maniatis et al. 1982), arrived in the mail. Possibilities for experiments, comparisons, simulations, and papers arise instantly as you thumb through the chapters, largely organized by five major geographic regions (Africa, Asia, Europe, the Americas, and Australia and New Guinea).

Genetic data discussed in this volume are heavily weighted by methodologies for the indirect detection of nuclear gene diversity. One hundred twenty-eight alleles for classic proteins and other secreted products are scored in 42 "populations." The authors closed data collection in the summer of 1986, rendering their discussions of new DNA-based polymorphisms very spotty. In contrast to the compendium of allelic frequencies and the coverage of DNA polymorphisms by Roychoudhury and Nei (1988), this volume also relies rather heavily on unpublished DNA-based work of their colleagues, and it attempts to include discussions of mtDNA studies when relevant. The authors do not make use of Roychoudury and Nei's summary, citing the late publication date, and this makes their coverage of some classic polymorphisms redundant and difficult to evaluate for accuracy. (All loci are summarized in tables included in Appendix 1.)

The authors excel in their discussion of methods of analysis of geographic patterning, listing the advantages and disadvantages of using phylogenetic trees versus principal components (PC) analysis and the biases intrinsic in each ("Trees are fallible friends" [p. 30]). Trees for populations and their PCs are well integrated in the relevant text sections, while the synthetic gene maps take up the final half of the book. Color maps revealing population outliers are also included.

In more global discussions of major patterns, the authors spell out their rationale for interpolating from disparate local groups a hypothetical regional population, and what may be lost in this approach. They also include information about linguistics and archaeology for all geographic areas of the world, a feature not generally found in the previous summaries of allele frequency data. This multidisciplinary approach is essential to the interpretations of historical population subdivisions, and their close collaboration with linguists Joseph Greenberg and Merritt Ruhlen is evident in their preference for global linguistic comparisons that lead to efficient "lumping" and to broader interpretations of population movement.

Geneticists may wonder at this point why data that leave a laboratory with such a clean interpretation should become controversial in another setting, especially when coupled with linguistic or settlement patterns. Our desire to explain human differences as accidents of history, versus adaptations with value, lies at the root. In a broader evolutionary vein, Williams (1992) pointed out the problem as the danger of using dendrograms in populations where genotypic and phenotypic categories recur rather than persist. The central focus of this volume is the attempt to use genetics to evaluate persistence due to adaptation and natural selection, versus migration, drift, and recurrent mutations, with implications for the development of uniquely human cultural attributes and/or morphological changes over the past 100,000 years. Our uncertainty about mutational processes and the level of migration connecting once-separated populations forms the basis for asking one of the most fundamental questions in anthropology, i.e., what is the origin of anatomically modern humans? Geneticists should play a central role in answering this question, a point the authors clearly demonstrate by their continual effort to find new methodologies sufficient to answer it.

I have two major problems with their treatment of human phylogeny and with the perspective that human geneticists bring to reconstructing human history. The first is their statement, found on page 93, that "the most important difference in the human gene pool is clearly that between Africans and non-Africans. . ." Some biologists hold, in contrast, that the most important genetic difference in any mammalian species is of sex, linked to certain loci on the Y chromosome in males, their absence in females, and the inherent potential of females to differentially modulate gene expression in 5.3% of the haploid human genome, perhaps as many as 5,300 X loci, by chromosome inactivation.

Eventually, the context of the paragraph in which this statement occurs makes it clear that the authors are merely discussing historical subdivisions in the human gene pool. However, their statement begins in such a vague manner that some may be left to imagine a great evolutionary gulf that therefore could exist between Africans and non-Africans, in spite of any later qualification and interpretation of this point. If the data warranted such imagery, then the statement is valid as it stands. However, one fundamental insight of molecular evolution is the extraordinary relatedness of all human populations, including Africans, relative to genetic diversity found in our closest nonhuman ape relatives (Kocher and Wilson 1991). This insight is trivialized by overemphasizing the genetic subdivision that appears to have occurred between some humans of African ancestry and all other people, including some Africans.

This discrepancy is further emphasized in their diagram on page 157, which shows a series of boxes indicating the evolution of genetic diversity found in each of the successive major groupings of living humans (fig. 2.15.2). If one believes that a particular group is unrelated genetically to modern Africans, this diagram reinforces such a view. It shows a common ancestral population (all humans) giving rise to one box (Africans) and to a separate box (non-Africans) from which all other planetary populations stem. The non-African box gives rise to two boxes, Europeans/north Asians and southeast Asia/Australians. In order to be consistent with hypervariable mtDNA sequences, as well as with earlier RFLP studies, it needs to give rise to yet a third box, containing some Africans. Second, we have known for >20 years (Lewontin 1972) that the simple, nonanastomosing depictions of human continental groups must be wrong, and the authors state why in numerous places in the text, but this faulty notion is unfortunately perpetuated by figure 2.15.2.

The volume closes with an epilogue conveying a noble vision that the authors have for the next generation of human population geneticists. A recurrent theme is the need to sample large numbers of loci, do it systematically, and cover the remaining religious, geographic, and linguistic isolates before they disappear through acculturation or disease. Changing technologies offer us this opportunity, and the gauntlet has now been thrown down.

References

Kocher, T.D., Wilson, A.C. (1991) "Sequence evolution of mitochondrial DNA in humans and chimpanzees: control region and a protein-coding region." In Osawa, S., Honjo, T. (eds.) *Evolution of life: fossils, molecules, and culture*. Springer, Berlin, pp. 391-413.

Lewontin, R.C. (1974) *The genetic basis of evolutionary change*. Columbia University Press, New York.

Maniatis, T., Fritsch, E.F., Sambrook, J. (1982) *Molecular cloning: a laboratory manual*. Cold Spring Harbor Laboratory, New York.

Roychoudury, A.K., Nei, M. (1988) *Human polymorphic genes: world distribution*. Oxford University Press, New York.

Williams, G.C. (1992) *Natural selection: domains, levels, and challenges*. Oxford University Press, New York.

[This term "nonanastomosing" is new to most of us. It segments to non + ana + stoma > non-ana-stom-osis-ing > anastomosis "the union or connection of branches, as of rivers, veins of leaves, or blood vessels" > anastomose "to join or connect by anastomosis, as blood vessels" + not. Now I am lost. I have no idea what this word means in the sentence Becky used it in.] [Unsolicited remark by Hal Fleming].

•••

HGHG reviewed by FRANK B. LIVINGSTONE, University of Michigan

This is surely the biggest, and heaviest, book I have ever reviewed. It certainly merits the title of magnum opus, and the subject, the complete history of the human species, is just as grandiose. It is appropriate for it to be titled the history and not the evolution of the species since it attempts to reconstruct the unique events in the past that led to the present distribution of the species and does not consider in any detail the causes or processes behind them. Cultural variation especially linguistic is included but the major task is the use of gene frequencies to decipher human history and prehistory.

There is relatively little interest in or discussion of the specific genes involved, but instead all are stuffed into several number-crunching distance programs that measure the differences among the populations and then draw supposedly phylogenetic trees from the distances. I have always been skeptical of these trees regardless of what variation they are based on, and I think the trees reconstructed from anthropometrics (p. 71) or skull metrics (p. 72) provide ample support for this skepticism. Are gene frequencies any better? I don't think so.

The major reason for the obvious inconsistencies in these trees is natural selection. Africans and Melanesians are alike because of adaptation to a tropical environment. Are the genes and gene frequencies and their trees subject to the same caveat? Yes. The authors discuss selection and show even classical loci such as the ABO blood groups must have balancing selection as Cavalli-Sforza pointed out in 1971. There is a discussion of the hemoglobin variants but as usual they are not included in the tree analysis since they are strongly associated with malaria. But the G6PD locus, which is equally associated with malaria is included. Nevertheless, despite all the discussion, they assert "it would appear that most of the genes used by us for tree analysis are neutral" (p. 121).

The justification for this analysis that is surely based on false assumptions is that it works. The very close correlation between the genetic tree and the linguistic one was previously published with great fanfare. But the close association of these two trees is simply a reflection of geographic variation. A cline for any single gene results in a perfect tree and the combination of several clines is all that is necessary for a good "phylogeny" that in reality reflects geographical variation. Thus, the ultimate tree for the human species is only a result of the fact that human genetic variation is clinal, as can be seen from the 518 pages of maps that make up the last half of the book and many others interspersed in the text.

In addition to the dubious data behind them, phylogenetic trees are an oversimplification of human history. Gene frequency change can be very independent from locus to locus, and there can be exchange between populations. As an example, the linguists would have little trouble reconstructing the phylogeny of English, French, and German, but what would a genetic tree look like? Of course, there has been so much intermixing that there is more variation within many of these populations than there is between them. A genetic phylogeny is of little help for our understanding of the history of these populations. And I would go even further and assert that genetic variation is of little value in understanding human history or prehistory. Instead of trying to understand their own data, geneticists have continually used genetic variation to draw phylogenies under the egregious assumption that it is neutral. Investigating neutrality should be our primary endeavor in the attempt to understand or interpret the variations of individual loci as they occur within a common population structure and population history.

•••

HGHG reviewed by HAL FLEMING, Gloucester, Massachusetts

The book's format resembles those of A.E. Mourant's *Distribution of the Human Blood Groups* (in various editions), and Arthur G. Steinberg's *The Distribution of the Human Immunoglobulin Allotypes* 1981 (with Charles E. Cook). There are introductory theoretical discussions, large tables of data arranged by genes and their alleles, maps of distributions of particular genes, and sections on what it all means for global regions like Africa, Europe, the Americas, etc. However, *HGHG* has a huge section of very high tech innovative maps — ca 500 pages or roughly half the book. These are cleverly done, show clines and isometrics nicely, may often implicate certain areas as the sources of things, but can be quite misleading.

The human body has thousands of genes. Only a few (120 ±) have the properties of *marker genes*, i.e., they can be measured, have a range of alleles (varieties) and tend to differ systematically from population to population and/or region to region. Theoretically, perhaps in the on-rushing millennium, pure DNA sequences and their bodily or behavioral correlates will be so thoroughly known that a number of populations can be compared in terms of DNA (mtDNA or Y Chromosome) segments. To achieve that is basically a matter of money and labor, people with grants and research assistants and lots of computers and lab equipment. We are getting there, but *HGHG* does not represent that accomplishment of definitive genetic knowledge. *HGHG* is still late 2nd millennium AD.

In comparing the patterns and numbers of alleles in various populations, one cannot compare them with each other and simply derive an ancestor — it is not quite like linguistics. Differing percentages (gene frequencies) of alleles may be accounted for by well-known processes of genetic change, the three most important of which are *natural selection* , *drift or genetic drift* and *gene flow.* A linguist would call the last "borrowing", an ethnologist might say "diffusion". A large number of alleles held in common at the same locus (locus of gene) does not necessarily indicate a close phylogenetic or taxonomic relationship with respect to that gene at least or even in general. But it often does. For example, two tribes of quite different origins may live together in the same malarial area (or other disease). The disease puts them under severe adaptive pressure. One tribe has had a mutation that copes rather well with the disease (as many hemoglobins do). Over time that mutation will thrive in that tribe because its carriers have a selective advantage over those who do not — they who lack are liable to die of the disease. At some point, due to intermarriage or casual sex, the mutant with its advantages enters the gene pool of the second tribe. There it will also thrive, and for the same reasons, because it functions just like a mutant of local origin. In time, the two tribes come to have a high percentage of the mutant gene in common. At that gene locus, they will tend to look like closely related populations, and may appear quite dissimilar to their own "real" relative at that locus. Naturally ,a close similarity at one locus may also indicate genetic closeness. If four South American tribes all show "high O", especially in very different environments, common inheritance is more likely.

So any one gene is fraught with all this ability to confuse things. Thus Cavalli-Sforza (C-S from now on) has consistently advocated the use of the greatest number of genes to overcome the distorting effects of a highly adaptive mutation. Yet some of the genes (including haplotypes and allotypes in this discussion) upon which he relies a great deal are known or strongly suspected of being subject to disease selection (natural selection via disease). Such as the Immunoglobulin systems (GM, KM, et al) and HLA. Both seem to have a part to play in the human body's defenses against diseases. But they are mentioned over and over again as some of the best marker genes for phylogenetic purposes, especially HLA. On the other hand, most of the hemoglobins, especially those involved with malaria, are flamboyantly selective. They tend to be discriminated against for phylogenetic purposes, on the grounds that they are less neutral than others. "Neutral" here means less prone to selection. Of course, knowing about hemoglobins and diseases may be useful for medical reasons in protecting people from illnesses.

It is almost as if HLA and GM had been assigned to basic vocabulary because they are quite conservative, even if they are ultimately subject to natural selection. Just as innovations, even including borrowings, can occur in basic vocabulary. But hemoglobins are like cultural vocabulary, like the ultimate instability found in American teenager slang. They mutate quickly and spread rapidly and widely through fads. There may be good reason to favor immunity genes over hemoglobins but it might also be pertinent to prehistory to count *all* the genes, not just those thought (perhaps erroneously) to be more neutral and stable. It is a matter for the experts in biogenetics to decide. Yet my linguistic instincts, based on the high value of basic vocabulary, prefer to think of some marker genes as more useful taxonomically than others. The standard argument in biogenetics for such favoritism is that alleles like GM haplotypes reflect ancient continental type selective forces leading to old mutations which have been passed on by ordinary inheritance ever since.

Genetic drift does not seem to have a counterpart in linguistics, unless it be the tendency of some dialects accidentally to contribute more to the future of a language than others. Drift is a phenomenon of small populations and those that migrate. It is not adaptation as such. It's more like lotteries or other forms of gambling. For example, take the population of Awajishima, a Japanese island recently the epicenter of a very powerful earthquake. Suppose that before the disaster they had 100 people, of whom 60 had ABO*r, 30 had ABO*p, and 10 had ABO*q. Since the alleles clung to particular families differently, then survival or not of particular families would affect the frequencies of ABO alleles on Awajishima. Suppose that one district had the most families destroyed, say 50 people were killed. By the luck of the draw there were more *p* and *q* people killed than *r*. Say 8*q*, 22*p* but only 20*r*. After the earthquake, Awajishima would have only 50 people left but the gene frequencies would be ABO*r 80%, ABO*p 16% and ABO*q 04%. The island now would be a "high O" area, almost like South America where virtually nothing but O can be found. Drift seems to bedevil gene frequencies in the New World, especially South American in greater Amazonia, and Pacific islands.

Gene flow is an easy concept for linguists. What is usually the most difficult part is figuring what percentages of specific alleles in a population flowed into it, were borrowed from others. Biogeneticists are good at the mathematics of this business and have generated some important corollaries. Most important are the effects of gene flow into a small population, as opposed to a large one. Gene flow from 100 Tocharians would hardly be noted in the large populations of archaic China but the same amount would alter the composition of a small Turkic population in the Altai. But sometimes the assumptions made by biogeneticists about donor peoples are hard to accept on ordinary prehistoric grounds. (More on this, below).

Who are the people of the world whose genes are compared? Who are left out? C-S has the goal to record and compare the genes of those populations indigenous to various regions of the world *before* the great expansions of the modern colonial era — before 1500 AD. The effects of this are most dramatic in the Americas and Australia. There, millions of European Caucasoids have replaced earlier native American and native Australian peoples. Millions of Africans, primarily from West Africa, were transported brutally to the New World and there mixed in varying degrees with Europeans and native Americans. These millions of Canadians, Yankees, Criollos, and Aussies are not examined. Nor are the modern Hawaiians or the Pitcairn Islanders nor, above all, the 30 million African

Americans. Not to mention the scores of millions of Mestisos in Latin America plus practically everybody in the Caribbean. But these exclusions are completely logical. The past 500 years are pretty well known and can be peeled back so we can look at a more pristine global situation.

But the Jews and Gypsies are excluded too. All the fascinating research done by Israeli biogeneticists is saluted and put aside. It has been or will be published by Batsheba Bonné-Tamir. Yet the Jews are different because, unlike other colonial groups, they lack a homeland population against which the colonials (diasporees) can be compared. The primary or original Jewish population must itself be reconstructed from the evidence of the scattered parts.

The reasons for ignoring the Gypsies are not convincing; it might have been difficult to sort out the alien genes picked up in their travels. But the Gypsies could be handled just as easily as the Jews, indeed more easily because there are fewer colonials, and the original Gypsy population is of interest to us. They derived from the early Aryan peoples of western India and could give us some clues to ancient Aryan genetics. The several Gypsy languages are not hard to classify as western or northwestern Indic, despite the borrowings from half of western Eurasia. Surely the genes are not insurmountable either.

Two General Comments

HGHG as High Tech Methodology

It is always disheartening for a reader to hear a reviewer admit that he does not fully understand half of the book being reviewed. Yet it is important to be candid about this because the book is probably beyond the developed skills and knowledge of any one reviewer. Yet it is also true that *HGHG* generally is user-friendly because it repeats its points in charts, figures, maps, and finally in summary paragraphs. Genetic trees and distribution maps and genetic distances are easy enough. The magnificent maps show high points and low points of most alleles around the world and clines in between. There are around 500 maps, each a page in size. But one would like to check things, especially a reviewer would, and that can be difficult.

First, the mathematics was too much in some cases. "Bootstrapping" was interesting but impossible for me to check; it gave alternatives to particular trees and sometimes better than the first trees. Then the ideas in "Principal Components" (PC) were not too hard to understand but how they were all calculated was beyond me. They had to be taken on faith or checked against good sense or other data. Since there were often four or five PCs and they were mapped, one could look at an area from the angle of its most salient characteristics and then some of its secondary, tertiary, etc. These are the basis for those maps showing source of migrations and routes.

Finally, there was the "raw data" itself, actually slightly cooked. If one could compare the actual allele frequencies in the hundreds of "tribes", one could somehow check the whole book by one's own methods. Well, some researchers may try to do that. Certainly a reviewer cannot take on all that data in a review. So one tackles a piece of it, preferably an area one already knows something about. As expected, I picked Africa and immediately got in trouble.

It wasn't high tech math or genetic theory. I could not determine from *HGHG*'s instructions how to determine which tribe some of the data belonged to. The very first tribe listed — the data came in columns — had no label other than * on it. Such things were supposed to be identified by a general category label in the line above. This one had only "Africa" as its category. Alas, the data were interesting too. There were also overlaps so that a number of times an entry might be, for example, "East African" or "Khoisan" or "Bantu". Being able to figure from the data what group the data probably belonged to didn't help too much because in a region of great diversity the particular tribe makes a difference.

A last point would be that the data themselves were a problem. In Africa, for example, two Ethiopid groups who were very similar (Amhara and Tigre, called Tigri by *HGHG*) with nearly 95% of most genes in common turned out to have a large difference in GM. Why? Because of refinements in the analysis of haplotypes with base *za* had occurred between two data collections. So one had 39% *za;b* and 39% *za;bc3*, while the other had no *za;b* but 63% *za;bc3*, so their genetic difference was 45 instead of 36 points. But Tigre's total was 130.4% for GM instead of the expected 100%. The GM scores between Amhara and Tigre could not be checked without going back to the original publications. Since the Tigre data was about 96% similar to a composite Cushitic group (which did total 100%), it appeared that the Amhara *za;bc3* of 63% was seriously mistaken somehow. But the Tigre was still 30% out of whack (having 130.4% total); thus Tigre GM data were also off. Since *HGHG*'s calculated overall genetic difference between Amhara and Tigre was .0046 and that between Tigre and Cushitic was even less at .0020, my crude figures were basically correct in stipulating a close genetic connection among the three groups. So the GM data table was flawed. Later on, *HGHG* contemplated the meaning of the very high Amhara *za;bc3* in terms of drift and natural selection. Yet most likely the data were wrong.

Just to add to the confusion, next to Amhara was a column of data headed by an *. It looked Cushitic and interesting. Indeed it was because it turned out to be Tigre — again, redundantly. I suspect that in such a massive book, proof readers and data compilers had their hands full. Surely, that was the source of the errors in data.

More serious is the matter of misleading maps. Perhaps because the computer or the cartographers abhorred vacuums, gene frequencies on maps moved in "isogloss" lines. Since the lines filled out the continents they were addressed to, it appeared as if we knew the data from all the continents. Actually, of course, as we have mentioned, there are very large gaps in the data and not just in Africa. The maps absolutely fail to reveal the lacunae and thus seriously distort reality. Whatever good reasons the authors might have had to draw the lines that give these impressions, they should change the maps to show the lacunae. To do otherwise is lamentable. Again, one can suspect the publisher of wanting to have "nice" maps.

HGHG as Prehistory: In General Terms

When we consider globally what *HGHG* meant to accomplish and what it actually accomplished, we may choose to be critical or laudatory. The glass is half full, as they say. However, I cannot say often enough that what was accomplished is more than anyone else has ever accomplished. There is no serious competition from the other great compilations in biogenetics because none of them took the linguistic and archeological aspects of the problem seriously. There is strong competition from several books written in what one might call "the grand tradition of physical anthropology", such as William Howell's *Mankind in the Making* and Carleton Coon's *The Living Races of Man* — both excellent books which inspired me in my younger days. Their limitations were due more to their reliance on phenotypic anthropometrics, and a corresponding paucity of good biogenetic data, than to anything else. Coon who has been attacked as a racist — I doubt that he was — had the poor fortune to publish during the wrong Zeitgeist. But he retired to Gloucester which shows what good sense he really had!

We might add that excellent university courses in world prehistory, such as that taught by Ben Rouse at Yale, offer true competition to *HGHG*. But those are unpublished and not strictly comparable. Still Rouse's sharp distinction between archeology and prehistory is very useful to us. Prehistory includes archeology plus historical linguistic plus biogenetics plus other stuff; the past does not belong to just one discipline. If some archeologists do not make or accept such a distinction, it may be due to their lack of experience in multi-disciplinary pursuits (other than the so-called "hard science" assistance in analyzing field data). Let us, just for the sake of spelling out exactly what the "emerging synthesis" is, look at this seriously. Some kinds of synthesis may be stipulated, as follows:

Simple Prehistories ⇒ Archeology only or biogenetics only or historical linguistics only. Here the limitations of each discipline are profound. To put it poetically, one is stones and bones without the souls; another is disembodied souls or ungrounded mentalist speculation; and another is Zombies or bodies with no souls or tools.

Compound Prehistories ⇒ Any two of the primary four disciplines. (For our purposes, paleoanthropology cum anthropometry is a separate discipline from biogenetics. Dental taxonomics à la Turner, for example, is properly both biogenetic and anthropometric.) Archeology cum paleoanthropology is perhaps the most common. As a general rule, biogeneticists and archeologists are fearful of linguistics, except for using somebody's taxonomy in (usually) crude ways, and tend to avoid such cooperation. Nevertheless, superb examples of archeologists and historical linguists in cooperation exist, especially in Bantu Africa and Polynesia and the Indo-European problem.

Complex Prehistories ⇒ All four primary disciplines or at least three of them. These are the best and the fullest but also most difficult and usually most frustrating. Superb examples would include *HGHG* plus the Greenberg-Zegura-Turner hypothesis. These are more vulnerable than the other types because they can be overthrown by developments in any single field. For example, the G-Z-T hypothesis has been attacked on each of the three legs it stands on.

HGHG would be classified without dispute as a Complex Prehistory but in a special class — the whole world. It is equally clearly a tripartite type — biogenetics, linguistics, and archeology in orders of importance — but with important assistance from written (document) history. Anthropometry is lacking. A few fossils reported. In places where large gaps in the genetic data base are present, e.g., central Africa, western Ethiopia, greater China (north and west), former Soviet areas generally, etc., there is often much anthropometric data of useful type. For example, in most of the Himalayan region, barely any biogenetic data are reported, but the anthropometric data strongly suggest that the populations resemble the Gurkhas and/or Tibetans, for whom we do have genetic data. Thus, also, in the western end of the Himalayas, peoples like the Burusho of Hunza quite clearly resemble Afghans, northwest Indians, and Iranians. The limited genetic data strongly support that conclusion.

Why is the glass then half empty? *HGHG* does not reach its full potential because it does not generate hypotheses of much scope or chronological specificity for the bulk of the 100 millennia involved. Much of the blame for that can be assigned to the undated linguistic taxa, a major problem for any prehistorian. Some more blame to the unsettled questions of linguistic homelands. Where and when was proto-Sino-Tibetan or proto-Altaic? But much of the fault lies with archeology. Not its methods, not its thinking, but its *luck* or even skillful-survey luck in finding sites. Generally, the older the rarer. So for scores of millennia in many regions, there is not enough fossil cultural evidence to help anybody very much. Then there are areas where few archeologists ever dig. Then they have a hundred different reasons for digging and only a few of them are deep prehistory. Besides, there are not that many archeologists anyway. Naturally, if we could get the bulk of them to concentrate on our deeper prehistory, they would make an enormous difference! But why should they?

The conclusion is that most of the credit for this enormous contribution to human prehistory must be given to Luca and his colleagues, while most of the blame goes to the two weaker legs of the tripod. But we must disagree with some of *HGHG*'s conclusions and data in a number of prehistoric points, and we will sound shrill at times, but the principal thing to bear in mind is that the authors should remember the criticisms and perhaps change things in the next edition of our "long ranger's bible".

Case by Case Commentary

Case # 1: Africa

C-S has traditionally looked at Africa outwards from his favorite populations, the western (Aka) and eastern (Mbuti) pigmies of the central African rain forest. This has been a most valuable counter weight to other tendencies to emphasize the Bushmen of southern/eastern Africa or the "handsome Hamites" of northeastern Africa and the Sahara. At the outset, we can stipulate that these three clusters are well marked off from each other and from the non-African world. Pigmies, Bushmen, and Ethiopids[1] are clusters which are basically supported by mtDNA research too. But a major part of the African prehistory problem is what to do with all those other people, i.e., most Africans. *HGHG* does a pretty good job of trying and succeeds quite well, especially with the southern third of the continent inhabited by Bantu-speaking peoples. But, considering the huge gaps in our collective data base for African populations, *HGHG*'s good job is not as good as one would wish.

Biogenetically, the pigmies are fairly clear. They represent foci of basic central and west African tendencies, usually having the highest frequencies of alleles most identified with African-ness and Negritude. They also represent a likely adaptation to life in the great rain forest; they get around easily with their wee bodies. They also seem to have specific genetic constraints on growth hormones. What would be an adolescent growth spurt in most Africans, indeed most humans, is inhibited biochemically, except for sexual apparatus which is normal human. In essence, they become larger children with adult brains and sexual equipment. Their health is excellent except for parasites which plague them.

Because of their small populations of nomadic hunters, the pigmies perhaps maximized the effects of genetic drift in a context of sylvan natural selection. With small increments from Bantu gene flow their gene frequencies could be altered substantially over time. For these reasons, *HGHG* hesitates to see Pygmy patterns as necessarily archaic or the most archaic. Alleles which normally have low frequencies in, say, west African populations might be lost altogether under Pygmy conditions. But still, it is hard to avoid the conclusion that what is extreme in Pygmies was probably at least predominant in ancient central/west African populations. Yet how much of Africa and its prehistory do they represent?

Pygmies speak no languages inherited from their earlier prehistory. The usual statement is that they have no language of their own. Some have, in fact, been speaking Bantu for two or three millennia; some have been speaking languages akin to Mangbetu of the Central Sudanic branch of Nilo-Saharan for at least part of that time, while some have been speaking Adamawa-Ubangian languages of Niger-Congo. Yet there is a definite consensus among interested scholars that none of those are very old in the forest, having entered not so long ago with Bantu pioneers or Mangbetu expansions.

Since we do not have a clue to original Pygmy Sprache, we cannot assign Pygmy genetics to either Niger-Congo or Nilo-Saharan. Technically, *HGHG* is correct in correlating Pygmy genetics with Niger-Congo as they do on their master dendrogram (below). But the real prehistoric problem has been dodged, so salient are the Pygmies. Strangely enough, a good guess can be made on the basis of genetic distances. If we can find out what clusters of peoples the two primary Pygmy groups are closest to, we will have a basis for hazarding a guess about most likely linguistic affiliations. Who are their closest relatives? The data are in *HGHG*, albeit neglected from our new viewpoint.

It depends on which group of Pygmies is being discussed. *HGHG* carefully reviews the matter, separating out three clusters — western, eastern, and Pygmoids[2]. The last group is most like eastern Bantu (Northeast and Central East), to whom the Pygmoids are

[1] Ethiopid is a term of my own coinage. Cf. Fleming 1980. It embraces those peoples whose gene frequencies cluster together, including most non-Omotic Ethiopians, Somali, northern Sudanese and/or Nubians, Beja, Tuareg, some of the Fulani (Peul), and the outliers in Kenya, Tanzania (Iraqw), and Rwanda-Burundi (some Tutsi). It is not the same as Hamite because it does not include the Egyptians or Berbers but does include Semiticized Cushites (Amhara, Falasha, and Tigre), and it does not include any Omotic or Chadic peoples, save some Ometo probably. The only data on the 150+ Chadic peoples are those on the Hausa, who are clearly not Ethiopid.

[2] There are Pygmoids in Cameroon, Gabon, Congo, Zaire, Rwanda, and Zambia. Usually, at least in Bantu areas, these are known as Twa or local versions of that. Some of these Twa overlap with the Bushmen as the second shortest group of humans in Africa. In 1960, Herbert and Marcia Lewis and I observed that the Hadza were not so much larger than mixed Pygmy cum villager people we saw in the

closest socially and geographically. Thus, we discount Pygmoids due to gene flow. The western group (Aka) are closest to the Sara, a member of the Bongo-Bagirmi group of Central Sudanic, and to the Northwest Bantu, i.e., both their neighbors. The Aka are the third closest to the Pygmoids. While there is a reconstructed social history which purports to show that the Aka have moved from the southwestern Sudan across the Central African Republic to its southwestern borders with Cameroon and Congo, I remain skeptical because names with Ak- abound for hunter groups in eastern Africa. In any case, the Aka are closer to the average sub-Saharan African than the Mbuti are. Indeed, the Aka are not particularly close to the Mbuti.

HGHG brings the question down to the Mbuti, who "are the most different genetically from the remainder of the Pygmies and other Africans are also the smallest in stature. They are unquestionably the most representative Pygmies in most respects. . ." (p. 179). Well, it turns out that the Mbuti are coordinate to all the other sub-Saharan Africans, except for (what I call) Ethiopids and northern peoples! The Khoi and San[3] of southern Africa are somewhat closer to the main mass, albeit still coordinate, while their linguistic cousins, the Hadza, are even closer to the main mass.

This essentially leaves the Pygmies as outsiders. If the Mbuti are *less* like West Africans and central Africans than the Khoisan or Hadza are, they must have a special phylogenetic status in Africa — along with the Ethiopids. Without pursuing that problem, which has no apparent solution, we can at least conclude that there are no longer good grounds for correlating the Mbuti with *any* of the four linguistic phyla extant in Africa. By the same token, we no longer need burden Niger-Congo or Nilo-Saharan prehistory with our efforts to relate them to ancient people of the rain forest. For it is more than evident that both great phyla are focused on the savannahs to the north, not on the rain forest.

In the case of the Bushmen (Khoisans), a generally cogent argument that the Khoi are San who mixed with southward moving Bantu with cattle and that the basic San hunter is indigenous to eastern Africa is presented. Most of this is old stuff, some of it due to the labor of archeologists and linguists. The further proposal that the San — the basic population — has connections with Ethiopids and beyond them to the Caucasoid realm is a newer idea and more interesting. A fuller realization of this theory is thwarted by the biogenetics of the northern Khoisan speakers — Hadza and Sandawe — and by the erroneous assumptions *HGHG* makes about a number of things.

Initially, the genetic distances calculated for the Hadza and Sandawe are probably faulty for two reasons. First, there are no HLA nor GM data for the Hadza, a very important point when GM is one system where the San differ most markedly from others. HLA data are important in themselves as apparently very sensitive marker genes but also because HLA alleles are so numerous; there are 29 listed in the data appendices (out of 120 ±). Along with the 10 GM haplotypes (including KM), some 27 other alleles are not reported. That means around 55% of the possible significant evidence is lacking. Of course, hardly any sampling has all the alleles in question. But the Hadza number is low for such an important group. Furthermore, Sandawe lacks HLA data but not GM. Yet since it has 98% concentrated on one haplotype *za;b* and almost no one fails to have other *za;b* type haplotypes, it is possible that the field workers missed *za;bs* the "Bushman gene" or others.

Secondly, what is more certain is that the field workers apparently missed the "real" Sandawe. Eric ten Raa[4] suggested that the blood was taken from the predominantly mixed Nilote and Bantu population of the market town in Sandawe, while the smaller yellower natives lived in the hills outside of town. Both of these factors suggest that the Sandawe field results ought to be checked.

However, in discussing the Khoisan problem, *HGHG* seems to try to make Hadza and Sandawe go away. First, their linguistic connection with each other is doubted: "Surprisingly, the two languages have almost no reciprocal similarity, even though they both show similarity to the Khoisan languages (Greenberg 1963)". That problem has been considerably reduced now that more data have become available through the efforts of Derek Elderkin and Christopher Ehret. I do not doubt that they are related to each other, but I agree that their links are remote in time. Other than trying to account for the Sandawe in terms of either Bantu gene flow "hybridizing" them or possibly moving "considerably before reaching their present location", *HGHG* implies that Hadza and Sandawe may be aliens to Khoisan-ness. Maybe like the Dama (Bergdama) of Namibia they had been slaves brought in by the Khoi from farther north. But

---

former Belgian Congo, although the Pygmoids of Zaire seemed much lighter in skin color than the Hadza. *HGHG*'s map (p. 179) shows Pygmoids in Ethiopia. They are discussed separately in this issue.

[3] Oddly enough the Somali also end up in the same cluster with the San and Khoi. *HGHG* thinks that "This may be an error, but there are other similarities between Khoisans and other West Asians." It surely is an error, due mostly to insufficient data on Somali (no HLA and no GM, among other gaps). It is remarkable: famous peoples like Chechen and Somali, with populations in millions, involved in wars on television, bleeding profusely in the daily press, and yet one cannot adequately compare them because nobody bothered to do the modest chore of sampling their blood scientifically! The Somali also are not West Asians, despite fictitious kinship with Mohammed.

[4] For Eric ten Raa's observations, see Fleming 1987. Eric is the world expert on the Sandawe, among social anthropologists. Hadza blood has been immortalized in the sense that samples of it have been stored in London in liquid nitrogen. Its release has been blocked by a British social anthropologist who fears for the Hadza.

they are so far from the San that this idea won't do, so it is suggested that their links with Khoisan must be very ancient. Finally, "The possibility may also be entertained that their genetic similarity with Khoisans never existed, and their language was acquired because of political imposition or social influence in Khoisan milieu in a fairly distant past, as hypothesized for the Dama."

Our friends have been caught with their preconceptions hanging out. The notion of Khoisan is popular from social anthropology; it is named after two south African groups. The linguistic phylum of the same name was undoubtedly oriented around the many very exotic Bushmen whose languages full of clicks meshed with the hunter cultures and the wee yellowish bodies. It was a package which became some kind of standard fare in anthropology, like the Eskimos. However, linguistic taxonomy, when it is correct, is a bit more demanding than the Khoisan package was. The phylum, actually called "Click languages" by Greenberg, has three mutually remote sub-phyla. One (SAK) is in southern Africa; it probably has been spoken as far north as Zimbabwe and Malawi, judging by the gene flow to the Bantu. Or a kindred branch of it was spoken in those regions. The two others (Hadza and Sandawe) are both spoken near to each other in northern Tanzania. It is possible to show that they lived farther north at some time in the past, judging by linguistic borrowings or shared words in South Cushitic languages. Indeed, on the borders of Somalia, so to speak, a South Cushitic language with clear verified clicks can be found — Dahalo or Sanye. While there is almost no genetic data on the Dahalo, there is some ABO. They have a very high incidence of "p" which reminds one of Hadza, which also has a very high incidence (36%). Both are salient in the area. The key point is that Hadza and Sandawe have respectable antiquity in the north and a distribution perhaps through most of the lowland Horn.

The assumption that the south Africans are the real Khoisan and that the northerners must somehow be explained lies at the heart of *HGHG*'s inability to decide what to do with Hadza and Sandawe. But linguistically, it is easiest to argue that *Tanzania* is the Khoisan homeland and that SAK moved down south or just spread down south at some remote date. It is also possible to argue that SAK was an immigrant group in the south who converted the real natives to speaking Khoisan[5]. Why does the burden of proof have to lie with the northerners? Might it also be easier to solve the problem of ancient Caucasoid gene flow to Bushmen by proposing that, in their Tanzanian homeland, they had much greater opportunities to exchange genes with partly Caucasoid peoples like early Cushites or even archaic Afroasiatic? As Arthur Steinberg has pointed out, it is hard to explain the presence of *za;bs* in Ethiopians and European Jews without proposing some ancient contact at a convenient spot, like the Nile Valley or Ethiopia.

Let the Khoisan case rest!

Looking at the "Ethiopians" as *HGHG* calls them, there is close agreement with older hypotheses, namely that most Ethiopians are closer genetically to Somali, to Beja of the Sudan, and to many north Sudanese. Moreover, they are distinct from both the main mass of sub-Saharan Africans and the Caucasoid realm of North Africa, West Asia, and Europe. In varying degrees, they are intermediate between those two large clusters. These main points are not in dispute.

There is an interesting connection which extends to the Sahara, *HGHG* proposes. That is to be discussed. A disagreement about preconceptions comes next. Finally, a few things about the Ethiopids and what they mean to African prehistory are discussed. *HGHG* just missed this aspect of things.

It is exciting to contemplate the hypothesis which C-S is generating. The *Tuareg* of the west central Sahara have an unexpected affinity with the *Beja* of the eastern Sudan. *HGHG* wants to contrast the Tuareg with the Berbers of North Africa, well-known Mediterranean Caucasoid peoples. But that is cheating because the Tuareg probably speak the "purest" Berber, i.e., fewer Arabic borrowings, have kept older Berber kinship patterns better (probably), and have a clear history of moving south from Libya into the central Sahara. They even write in their own script, their men (not the women) wear the veil, and their tents differ from Bedouin Arab tents.

Again, we have to argue a little with preconceptions. Why do the mountain Berbers of, say, Morocco represent the old Berber gene pool better than the Tuareg? Well, the Tuareg keep black slaves/serfs in oases and so deviate in the direction of Negritude. Not true, if we believe the ethnographers of Tuareg. Their gene flow seems to be *down* from the upper strata to the lower strata; this may be

---

[5] Let us think more like long rangers. The Pygmy versus Bushman problem is quite important for world prehistory, if we are to entertain the African origins hypothesis. Either or both of these "races" could have lived in Africa since 100 kya. If the Pygmies are strikingly correlated with the water shed of the Congo river, the Bushmen are evidently correlated with the great uplands of eastern Africa, the savannahs and prairies and highlands from Somalia to Namibia. No strong or convincing archeological support for either group exists in Africanist consensus, although a case can be made for the Bushmen. The Pygmy presence in the Congo basin is virtually unknown archeologically. Most of the concept of Pygmy is physical, not cultural, certainly not linguistic. Yet one striking cultural feature joins Pygmies emphatically to other Africans, especially West Africans. That is their *music*.

Alan Lomax (personal communication 1965) has said that the basic pattern of Pygmy music is archetypical vis-à-vis west and central Africa *and* in basic ways is "rather like" the patterns of the Bushmen. Since the Pygmy-Bushman interface continues for hundreds of miles, with great overlap in Zambia, is it possible that both of them are chips off the same old African block?

the rationale for their matrilineal descent and caste *hypogamy*. A bit like the American South. Anyway, the mountaineers have been interbreeding with Romans, Phoenicians, Frenchmen, Visigoths, Spaniards, but above all Arabs, for many many centuries. So they deviate in the direction of Blanchitude, n'est-ce pas?

Nevertheless, C-S may be on to something quite significant because his linking of Beja and Tuareg may very well extend to the Teda/Tubu of east central Sahara and the pastoral Fulani over much of the Sahel from Lake Chad to the Atlantic. It would be logical to examine the Chadic peoples who occupy the key area in between Beja, Tuareg, and Teda; we can do that linguistically but not biogenetically. From appearances, we can suppose that, however Afroasiatic arrived in the Lake Chad area, it was adopted by a local population rather than being the language of colonists[6]. Still, it would be nice to test this hypothesis! For the non-Africanist, it must be remembered that neither Fulani nor Teda/Tubu are Afroasiatic languages. Fulani is from the West Atlantic branch of Niger-Congo; Teda/Tubu are in the Saharan branch of Nilo-Saharan. So these correlations do not have a significant linguistic aspect, it seems. Indeed, Beja is a Cushitic language, while Tuareg is a Berber one; they come from different sub-phyla in Afroasiatic.

Part of this problem may derive from data insufficiency again — in this case, the Beja. (C-S has gotten new data on the Beja since publication of *HGHG*. They should help decide things.) Not only do we lack any HLA data on the Beja, but very few of the Ethiopid populations have any recorded. It is a serious lack when some peoples are compared with full data and others with inadequate data. Inadequate because HLA appears to make a great deal of difference world-wide. However, in order to compensate for inadequacies, I fell into the habit of comparing peoples in terms of those genes which are known to all. And in those terms I agree that the Beja have an affinity towards the Tuareg and pastoral Fulani, as well as with most Ethiopians and north Sudanese.

A whole series of physical anthropologists, and ultimately a prominent ethnologist (G.P. Murdock), have confused prehistory by merging several kinds of Africans into a so-called local race based essentially on linearity and height. But cattle possession is probably an undetected criterion too. By using, indeed featuring, Jean Hiernaux's African classification (p. 168), which explicitly mixes criteria, *HGHG* might pass the falsehood on to the next generation. Murdock's well-known *Africa* book reckoned that the tall Tutsi of Rwanda and Burundi were undoubtedly descended from the tall Nilotic Lwoo who were known to have invaded northern Uganda. Stanley Garn, in a very sophisticated classification of local races around the world, lumped the linear folk together. It's like saying that basketball players are genetically distinct from farmers. Yet the lumpees, Nilotes and Ethiopids, are not only distinct each from the other; they are not close at all, if the criteria are simply biogenetic.[7] Some parts of the data are reported in *HGHG*.

One recent phenotypical report from Rwanda describes a woman as "a sterotypical Tutsi, tall, light-skinned and slender." I thought the photo in the story (*Manchester Guardian Weekly*, 5 February, 1995) was of an Ethiopian. It is also apparent phenotypically in those rare scenes when "specimens" of the groups come together. In a number of places in western Ethiopia, where Sudanese refugees are sheltered, one can see Nuer, Dinka, Anuak, and sometimes Shilluk; one can easily observe their differences from

[6] There are scattered populations of hunters in the western Sahara. In Tuareg lands, there is a tradition of people called "Issebeten" who were hunters of the mouflon sheep who also kept sheep and goats but knew not the camel. The Tuareg said these Issebeten were responsible for the prehistoric rock art in the Sahara and, as vassals in the Tuareg caste system, had hereditary rights to hunt in the Ahaggar region. They were patrilineal.

However, in the far western Sahara lived a hunter group, called *Nemadi* and on the coasts of the Atlantic fishermen called *Imragen*. As quoted in Briggs (1960), few anthropologists had ever seen the *Nemadi* but one "Jean Gabus, a Swiss ethnologist who has spent some time with the Nemadi, has described them as being very variable in stature and black haired, but lighter skinned and straighter haired than their Moorish neighbors. They have good teeth and big, flat feet, says Gabus, and a smooth, fast, gliding walk (as do most desert nomads). Several French authors have remarked that the women are often strikingly handsome and well built." In fact, some Nemadi women have French husbands. They do not live in Tuareg country but in the Arab-Berber region of Mauretania.

Briggs describes the Teda of Tibesti as being like the Harratin but more slender, shorter, normally very dark, but with ABO frequencies quite distinct from Harratin or Arabs or Sudanese Negroes yet much like Berber patterns. We need some biogenetic data! Harratin vary but mostly are like most West Africans.

[7] The Tutsi and the less-known Hima of the Lacustrine Bantu area (Rwanda, Burundi, and Uganda) have been a famous problem in African prehistory for some time. Although Murdock is still being quoted as proving that the Tutsi are really Lwoo type Nilotes, he himself accepted a different reading of their history in 1965. As senior reader of my dissertation, he confronted the biogenetic evidence that the Tutsi and Hima were even less like the Nilotes than the Hutus and other ordinary Bantu were. He gave in with grace and style — much to my relief! Years later, more evidence came in; C-S in his *African Pygmies* showed Tutsi with 10% *f;b*, a very good index of some Ethiopid connection in that part of Africa. Then Christopher Ehret proposed a set of South Cushitic loan words, especially pertaining to livestock, in the Bantu languages of Rwanda-Burundi and indeed in most of the Lacustrine area. Murdock had thought the pre-Bantu population of that area was Cushitic but "Sidamo" (his misnomer for Omotic) from southwestern Ethiopia. Finally, the discovery of numerous Omotic and South Cushitic loan words in Kuliak, a Nilo-Saharan group of northern Uganda, tended to confirm these hypotheses. Both Heine and Lamberti concluded that Kuliak was Afroasiatic. Cf. Murdock 1959, Heine 1976, and Fleming 1983.

Ethiopians. In Addis Ababa, it is not uncommon to see a Nilote walking along, towering over the local Ethiopids. When one is with a group of tall Ethiopids, for example Somalis, one is not apt to confuse them with Nuer, Dinka, or Lwoo[8]. While it is entirely possible that centuries of consuming dairy products have produced the linearity that supposedly links Nilotes and Ethiopids, most of highland Ethiopia has remained average in stature on about the same diet as the Nilotes. Farmers who keep cattle, sheep, and goats do not count as "cattle people"?

Malheureusement, *HGHG*'s marvelous maps and dendrograms failed to reveal one major aspect of East African prehistory, and it tended to pooh pooh another in southern Africa. Both occurred in what are now mostly Bantu areas but areas where significant numbers of pre-Bantu peoples still live. In the first, roughly former British East Africa plus Rwanda-Burundi, several generations of scholars going back at least to Carl Meinhof have proposed a significant Hamitic (Meinhof, Biasutti) or Semitic (Merker) or Cushitic (Greenberg, Murdock, Fleming, Ehret) presence. Some have these Ethiopids coming in as waves of conquering Caucasoid cattlemen (handsome Hamitic herders), while others see them as the autochthones (after the Bushmen) who were conquered or ground down or replaced or absorbed by immigrant Bantus and Nilotes. Nowadays, we can see that probably all of these things happened because a number of different clusters of Cushites have been involved over many millennia.

HGHG missed this point largely due to data insufficiency once again. For reasons peculiar to the strange lack of discipline called anthropology, East African peoples famous to ethnology (e.g., Masai, Nandi, Kikuyu, Hima, Turkana, Ik) have magically repelled physical anthropologists. Are there genes that particularly suggest Ethiopid or Bushman connections in Bantu or Nilotic populations? Sure, plenty of them. Rhesus *cde* and *CDe*; Duffy Fy^a and Fy^b; P1 in <60% amounts; GM *f;b*, *za;g*, *za;xg* and *za;bs*; not to mention the possibilities with HLA. But for most of the ethnographically famous, we do not know the pertinent allele frequencies. For example, the Masai, whose interaction with Cushitic Rendile and El Molo is well-documented, and many of whom look Ethiopid, are poorly known biogenetically. Still, they are well-described when compared with the whole mass of South Nilotic peoples from Nandi in the north to Barabaig in the south. Totally untapped genetically, as far as I know, but easily the crucial peoples mediating the interaction between Cushites and the new in-migrant peoples. Again the deficiency might have been made up for by data from the neighboring Bantu. Yet, alas, they are not very well known either. And then *HGHG* has the custom of lumping large sectors of Bantu together and giving average figures for the lumps. For example, in their primary appendix of tribes, they list 26 Bantu clusters, some of which have many peoples in them. Then, *HGHG* lumps some of the clusters together so as to present data on only 6 primary sectors like Northeast, Southeast, etc. There is no way in this data presentation to find out what gene frequencies are found in such salient clots in the soup as the Tutsi, Ganda, Kikuyu, Chagga, Swahili, or Pokomo, each of whom have a different set of non-Bantu populations they have interacted with. Most of the Northeast Bantu were not tested for GM or HLA. The data presented under the heading of NE Bantu are quite misleading. One of those populations — or is it five averaged? — has 1.5% *f;b*. That's not very helpful information.[9]

Nevertheless, *HGHG* does a good job analyzing Bantu prehistory as a whole from the Benue river to the Cape of Good Hope. There may still be conservative linguists or ethnologists who doubt that Bantu belongs in Niger-Congo or that the homeland is in the Nigeria-Cameroon borderlands, rather than Zambia or Uganda, but, if there are any, they derive no support from *HGHG*. The work of Greenberg, Phillipson, Vansina, Heine, and others has meshed and established a consensus. We know a lot about Bantu and, outside of East Africa, *HGHG*'s data compilations show it. Their discussion of the Bantu takes up a major part of their Africa section. It is good

[8] When C-S's new data on the *Nuba* of Kordofan are analysed and published, they may prove to be quite close to the Nilotes. There is a singular robustness (robusticity?) about the Nuba that reminds me of Nilotes. While Ethiopids tend towards delicacy and slenderness, both Nuba and Nilotes tend to be the opposite. The question of which Nuba, of the score or so of distinct tribes, and how many Nuba (tribes) were sampled remains. There are at least three very different clusters of Nuba, to wit, (a) those Niger-Congo in speech, (b) those recently re-classified as Nilo-Saharan by Thilo Schadeberg, and (c) those on the outside of the Nuba onion who were long ago classified as East Sudanic (Nilo-Saharan).

One has to admire Luca for going to the Sudan to get important data. He is at an age where most scholars have long since quit doing field work. Luca, like Scotty MacNeish, is still going strong in his seventies! Bravo!

[9] Paleoanthropologists flock to East Africa. Biogeneticists seem to go everywhere else. It is an oversight by a whole branch of science. Contemporary East Africans are a very diverse lot! North Tanzania is, by linguistic and cultural criteria, the most diverse region in Africa. All four linguistic phyla are represented there in strength. Why doesn't all this interest biogeneticists?

In passing, we must note that *HGHG*, following a good idea of Renfrew's, gives too much credit to the Bantu farmers in bringing agriculture to the southern third of Africa. The Neolithic was already in East Africa when the Bantu got there. If that can be debated, certainly pastoralism cannot. C-S assumes a lot about the Bantu, which he picked up from his sources. The pastoralism loaned to the Khoi by the Bantu, plus many crops found in eastern and southern Africa, were acquired by the Bantu in East Africa — from South Nilotic and South Cushitic peoples primarily. *HGHG* also cites Murdock's *Africa* book but ignores GPM's better theories about Southeast Asian crops and East African crops plus herding.

stuff, even including their discussion of the (Berg) Dama of Namibia, who are seen as a basically, nay typically, Negro population like the main mass of sub-Saharan Africans. Their Khoi language (Khoisan) was undoubtedly acquired.

They only have a small failing, and that is in the far south. For reasons of technical genetics and unexpected caution, they shy away from acceptance of Trevor Jenkins cum Arthur Steinberg's calculations of amounts of Bushman genes in southern Bantu populations. From a regional high of 60% in Xhosa to respectable 10-11% just north of the Zambezi, it seems clear that southern Bantu are different people from those in, say, Zaire because many of them are as much Bushmen as Bantu. And the Khoi cattlemen are nearly as much Bantu as they are Bushmen. Gene flow both ways has been substantial, but less so in the case of the San hunters.

In deeper prehistorical terms, *HGHG* thinks about the homelands or basic foci of the three predominant "types" — Negro, Bushman, and Caucasoid. While they are extreme, Pygmies are still in the mainstream of Negritude, as are most Africans. *HGHG* never decides what to do with the Bushmen but ends up (p. 194) considering that they might have come from Arabia. With the Caucasoids, *HGHG* has little doubt that the 175 million of them in northern and eastern Africa came from western Asia, probably Arabia or the Levant, although they do say that it is known that Caucasoids crossed over from Spain to Morocco about 20,000 years ago. This fact, if true, probably solves their Tuareg-Beja problem. The mountain Berbers are the most likely descendants of Iberian immigrants of 20 kya and were in situ long before the linguistic Berbers got to the Maghreb. (I wish I had known this before!) Caucasoid-hood in Africa and Arabia is so strongly correlated with Afroasiatic languages that evidence of one causes a search for the other. Only the Chadic and Omotic realms are truly exceptional to that statement, although it appears that Nomotic is partly Caucasoid. It is also mildly surprising that *HGHG* ends up doubting the closeness between Ethiopians and Beja, suggesting that the Beja may have come from Arabia (too).

I would like to enter once more a mild protest against the tendency, which *HGHG* shares, among biogeneticists to see evidence of Caucasoid things as evidence of somebody's migration from western Asia. It sometimes got downright simple-minded in Mourant when Cushites deep in the interior were explained in terms of Arab movements, trade or what-have-you. It is not the case that I believe the "all whites come from Arabia" hypothesis is false. It may be and it may not be. My quarrel is with the ostensibly paradigmatic *presumption* of things flowing out of the Near East or over from Arabia whenever comparable things are found in eastern Africa. In at least one epoch in earlier prehistory, the flow was wholly in reverse, I believe.

Here is a small collection of debatable specifics on the Africa section of *HGHG*. (1) On page 188 ,they say that Rhesus *cde* or *r* is of European origin with a "maximum in the north; the peak in Libya may be due to drift or contact via Sicily." High *r* has always been a Berber trait, but *HGHG*'s implication is probably wrong. Or very interesting. Substantial amounts of *r* adhere to Afroasiatic speakers everywhere. But Bantu "tribes" always have a respectable amount too. Did everybody else get *r* from the Berbers? That's doubtful on the face of it. But it is possible that the famous Bantu expansion was triggered by an important contact with Chadic peoples in Nigeria and/or Cameroon; or by whatever tides were bringing iron smelting south across the Sahara or west from Meroe. Did they bring *r* too? (2) On page 191, their synthetic maps of the 2nd and 3rd PC (principal components) are actually reversed. A simple error but one which could mislead scholars. (3) On page 192, the south Arabian contribution to the Ethiopian gene pool is stressed. A consensus grows among Ethiopianists that some language and culture crossed from South Arabia to Eritrea but not large colonies of old South Arabians (Sheba and her Sabeans et al). One has only to compare genes of Ethiopian Semites with those of modern South Arabians (Yemeni or Soqotrans) with those of Cushites to see that the first is like the third. Sure, many Sabeans et al settled in Eritrea, as traders probably, but they didn't affect local gene pools that much. Remember that the locals were already farmers. (4) On page 183 and Fig. 3.9.3 on page 185, *HGHG* adheres generally to a notion that Nilotes had a strong influence on the early Bantu at Urewe which shows up particularly in the Central West Bantu (mostly of Angola). This is to be based on archeological and linguistic information, they say. Given the paucity of genetic data from the Nile (southern White Nile) over to Lake Chad, and the fragility of the linguistic hypotheses, this theory should be held in deep abeyance.

A final point about Africa, this place we love and love to talk about. *HGHG* is surprised at the small differences among sub-Saharan Africans other than the Khoisanids (Bushmen) and Pygmies. Of course, the greatest genetic distance on earth lies between all these Africans and all external folks. But, having somehow split off from the rest of mankind a long time ago, Africans should have developed more differences by now. One would assume. Yet what is most striking in the interface of *HGHG*'s traditional genetic factors and the recent mtDNA reports is that *HGHG confirms* the mtDNA in the great African/non-African split but *contradicts* it about Africa's internal diversity. Even before they got good data on Bushmen, the mtDNA researchers were reporting such diversity *among* Africans that it nearly matched the internal diversity among the non-Africans. When the wee yellowish people were added to the mix, they became the most deviant population on earth! So much so that they caused researchers to doubt the mutation rates.

After all has been said, the reconciling of the traditional with the mtDNA results seems to be the most important problem that remains.

Case # 2: Europe and SW Asia

HGHG has Asia following Africa, then Europe following Asia. I think the decision to follow the geographical entity called Asia was a serious strategic mistake — from both a reader's and a researcher's point of view. Europe and western Asia interdigitate prehistorically far more than either does with eastern Asia. A category called Western Eurasia would have been much more user-friendly. Therefore in this section Europe will be treated together with southwestern Asia (SW Asia). In the south, the purview will run over to India; in the north, across the Urals up to Sinkiang. It's a natural area. It is also the case that in map after map focusing on Europe, *HGHG* almost always includes much of SW Asia and adjacent north Africa.

This is what Carleton Coon aptly called the *Caucasoid Realm*. This is the area whose strong genetic patterns induce *HGHG* to see it as the source of similar but attenuated patterns in Africa. The heart of the matter is SW Asia, not Europe, judging from the number of hypotheses which derive major settlements of most of Europe from adjacent SW Asian areas, e.g., Upper Paleolithic from the Levant, Neolithic from Anatolia, and many invasions from the steppes on the east.

HGHG's discussion of Europe seems hindered or constrained by their lack of focus on the larger area. The best example is Sardinia, whose population is the outlier of Europe and not included in the calculations at some points lest they skew the results. Yet the Sardinians have some of the highest frequencies of alleles which associate strongly with the Caucasoid Realm, e.g., GM *f;b* of 86%, Duffy Fy^b of 61%, HLA*A2 of 34%, HLA*B18 of 28%, etc. Their strongest resemblance is to the modern Lebanese, says *HGHG*, although their high GM *f;b* is matched in the Balkans by the Romanians and Greeks. Some of the unreported Jewish groups present very similar frequencies. Plainly, the Sardinians are part of an eastern Mediterranean focus which extends into the Balkans. Yet this never quite gets reported, given *HGHG* preference for treating the Sardinians as European deviants. Although the Soqotri (off the south coast of Yemen) are not reported in *HGHG*, their available data compare so well with Sardinian that it is arresting — but not amazing if both had origins in the eastern Mediterranean.

HGHG concludes that most Europeans are very much alike. In fact, most northern and western Europeans were alike enough that their data could be collapsed into one category, which *HGHG* used in their global calculations or the global dendrogram. That category was *Europeans*. For our purposes here, we can show *HGHG*'s breakdown of the major branchings *within* Europeans. Then we can get on to the more important work of examining the whole Caucasoid Realm. The six important outlier groups of Lapps, Sardinians, Greeks, Yugoslavs, Basques, and Icelanders (!) form the outer layers of the European onion, decreasing in distinctiveness from Lapps inward. Then Finns and Celts are the next inner layers before the main onion or the core — the rest of the Europeans. By the onion analogy, naturally, the basic or original Europeans should be seen as that "rest of them" group. Since that analogy might be misleading, as the Basque or the Sardinians might be the originals, I use it only to convey the primary conclusions of *HGHG*'s calculations. Nobody thinks the Lapps are the originals — which is probably an unwarranted conclusion. The Lapps may not be the population ancestral to most modern Europeans, still they might represent what was left of the "real" original inhabitants. While most people can readily see that Europeans are highly *polymorphic*, the various morphs seem to appear in many places. Most of us are probably glad for that too because it gives us more variety to look at. But some scholars in the past often saw differing degrees of various types of head, for example, as the bases of talking about *race* differences. I recommend that one just take the regular train from Paris to Moscow to clear one's head of notions of local *races* in Europe.

Yes, of course, there are some differences. Since most of us long rangers come from one or another of the European sub-regions, it is probably useful to rehearse what *HGHG* is teaching us. In this, the most densely researched continent on earth, the bases for proposing major genetic differences between peoples — the rationales for some wars and a lot of social inequality — are just not there, biogenetically.[10] Nor do the genetic clusters within Europeans strictly adhere to linguistic sub-grouping. The Czechs, for example, are not in the same cluster with some other Slavs, nor are the Yugoslavs close at all. Romance and Germanic peoples do form clusters but not exclusive ones, since Czechs, Frenchmen, and Belgians are also in the Germanic cluster. Here is a more detailed statement of all those clusters: (p. 268)

"1. Celtics (Scots and Irish)
2. Eastern Europeans (Russians, Hungarians, Poles)
3. Southwestern Europeans (Spaniards, Portuguese, and Italians[11]

[10] I do not know whether *mtDNA* researchers would agree with this statement or not. I imagine that they agree in finding the Basques and Lapps distinct from other Europeans but there is no source to cite here.

[11] Technically, Scots and Irishmen nowadays are overwhelmingly Germanic speakers. Sardinians are politically Italian but their dialects normally are considered as a different part of Italic from Italian dialects. Hungarians speak a Uralic language, as everyone knows. The name for the French was originally Frank, for a Germanic tribe. Belgians even now are divided into Romance (Walloon) and Germanic (Flemish) languages.

4. Czechs and Slovaks, who do not join other Slav speakers, but are approximately intermediate between them and the central subcluster of the Germanic group below, to whom they are geographically close.
5. Northwestern Scandinavians (Norwegians and Swedes)
6. French, who are related to the Germanic group below.
7. Germanic populations, including two subclusters, northern and central; the first subcluster is made up of Dutch, Danish and English people; the second of Austrians, Swiss, Germans, and Belgians."

"It is clear that there is a basic linguistic association within these subclusters, with exceptions worth examining."

One gets some of this correlation by declaring that Sardinians are not Italic speakers, Yugoslavs are not Slavs, and Icelanders, Scots, and the Irish are not Germanic speakers. Lowland Scots, for example, have been speaking English as long as Yorkshiremen; they aren't really Celts. One may realize that Yugoslavs were originally Illyrians and Romans, or the Irish were originally Celts, or the French were named after a Germanic tribe, but still there is some value in being stricter in making these correlations. We are correlating languages with genes, not with the known histories of various peoples. Even if it is sensible to consider backgrounds, and certainly Irishmen and Highlanders consider themselves to be Celts, we are looking for correlations to see what their *true value* is. In some other areas, where we do not know what the backgrounds of the particular tribes are, we will have to rely on those language-genes correlations — or not.

I don't know if this point will be understood. Obviously, there are few if any one-to-one correlations between language and genetic cluster in the world, except for the so-called "linguistic isolates". All Germanic-speaking peoples are not in the same genetic cluster, nor is that genetic cluster exclusively Germanic-speaking. Also obviously in the world, there is some correlation between linguistic and biogenetic clusters. Let's say the correlation probably ranges between .1 and .9. What we might establish empirically is *how much* of a correlation there is globally, so that we can say things like: "language cluster correlates with genetic cluster usually" or "normally" or "rarely" or "only occasionally". What *HGHG* does generally is establish or argue for a world-wide correlation something like "normally" or "for the most part". The mathematically satisfying numbers do not mean much more than that. That is a substantial contribution to prehistory. What this reviewer argues for here is a stricter analysis of these correlations so we can strengthen them and make them a bit more precise.

In Europe itself, we note that the Lapps are figured (p. 273) to be 47.5% Mongoloid and 52.5% Caucasoid in one calculation, but 82% European and 18% Samoyed in a second calculation. The first used Komi and Mari, western Uralics (i.e., west of the Urals) as an ancestral Uralic population but with limited data. The second used only Samoyeds from east of the Urals as the base or supposed ancestral Uralic population. Yet all those west of the Urals are in the Finno-Ugrian sub-phylum of Uralic. With very early and strong contacts with Indo-European, especially Indo-Iranian, it would seem most appropriate to assume that the ancestral Finno-Ugrians were at least partly Caucasoid already. So I would prefer the first calculations. The unraveling of the Uralic versus Indo-European problem is not within the scope of this review. Incidental to that, we should add that *HGHG* finds non-trivial differences among Norwegian, Swedish, and Finnish Lapps; as great as that between Dutch and Sardinians is found between Finnish Lapps and the others.

The Sardinians also manifest considerable internal diversity, since there is a known core of oldest settlers and then several blocks of newer arrivals elsewhere. How the Sardinian sample was chosen, i.e., which local population was chosen, is unknown to me, but it was probably the core, in the "darkest area in the geographic presentation of the genetic tree corresponds with the area of highest concentration of pre-Latin place names (Contini et al. 1989)" (p. 275). That seems to be central and south-central Sardinia, the highlands. Italian anthropologists have done beautiful detailed work on Sardinia, particularly co-author Piazza, so it is better known than most places. *HGHG* notes that Sardinia was settled before the Neolithic, circa 9120 BP, the "earliest date for any island in the Mediterranean". An alleged non-Indo-European language (they call it "pre-") was spoken in Sardinia before Latin, which was apparently the first Indo-European language there. Later, a culture developed which built the famous *nuraghi*, stone forts and/or houses, from 1500 BC to 400 BC. Still later, Punic and Roman influences culminated in permanent Roman occupation and the earliest split off from Latin. We are not told when the Neolithic got to Sardinia.

So I checked with a expert Bostonian, Dr. Miriam Balmuth of Tufts University, and was told that the Neolithic arrived around 7000 years ago, from whence not known clearly but probably ultimately from Anatolia. She said that recent digs have established that first settlement was about 11,000 years ago, and one excavator had a new site with dates circa 20,000 BP. So I would conclude that there may have been Upper Paleolithic folks on Sardinia, more clearly Mesolithic (11,000-7,000 BP), Neolithic farmers certainly, and a Bronze Age culture or two for a millennium before the Romans got there with the first Indo-European language.

It is the Neolithic which grabs our attention. Since those peoples closest to the Sardinians (p. 274) in total genetic distance (not just GM) are (1) Greeks, (2) Italians, (3) Basques, (5) most central Europeans[12], and (4) Lebanese. Even allowing for the exaggerated percentages of many genes, due to genetic drift on the island, the Sardinians seem to fit inside the population picture most

[12] This was not reported in the text. I had to figure it out from the tables. Perhaps they were pre-occupied with the peoples of the Mediterranean and so neglected the central Europeans.

associated with the movement of the Neolithic from Anatolia up into the Balkans and across the northern Mediterranean to Iberia. When we add the evidence given by *HGHG* and elsewhere by C-S to the older evidence of Neolithic farmers crossing the Hellespont to expand through the Balkans to become the Danubian Neolithic, it is all very convincing. It would be useful if we conceded this prehistorical theory because it seems to be the most parsimonious explanation of the spread of the Neolithic from Anatolia to far corners of Europe. Nobody debates it!

However, what both C-S and Colin Renfrew do with this "fact" is to assert that it equals the spread of the Indo-European languages. We hardly need rehearse this point because *Mother Tongue* has been full of it. The bearing of Sardinia on the question is this: in the logic of these things, the pre-Latin language spoken on Sardinia should have been Indo-European because the Neolithic supposedly arrived speaking Indo-European. (Presumably, the language remnants found by Piazza to be pre-Latin were not Phoenician either. That would confound things.) Yet, presuming that *HGHG* is correct in asserting that the pre-Latin language was not even Indo-European, the Neolithic had not brought an Indo-European language after all. Many of us, of course, do not put much stock in place names as evidence of a previous language. But toponyms can be very useful, as anyone from Massachusetts, Connecticut, Iowa, or Arkansas could tell you. We will try to get the recovered toponymic data and display it for ASLIP's linguists. I'll bet it is from John Bengtson's Macro-Caucasic.

HGHG reckons the Basques are descendants of the Upper Paleolithic. Partly this is due to their proximity to the wonderful cave art of 20 kya. I doubt that the data demand such a conclusion, since the Basques turn out to be much less special than they were — biogenetically. By *HGHG*'s own calculations the Basque genetic distance from most Europeans is *less* than that of the Sardinians, Greeks, and Yugoslavs. What *HGHG* is probably thinking of is that the Basques are the prototypical "mainstream" Europeans who were in residence before the Neolithic farmers came pouring in. Their residence in Iberia is thought to be pre-Neolithic and they may have Upper Paleolithic links to North "Caucasians" because they seem to have genetic links to those woefully underdescribed but currently beleaguered people. That will be good news to Bengtson. Naturally, the linguistic link between Basque and Caucasic may be reflective of the Neolithic, while the genetic close ties to mainstream Europe may be reflective of the Paleolithic.

We cannot stay in Europe, despite the richness of the material assembled by *HGHG*[13]. But it is necessary to stress that the Europeans are only a piece of the Caucasoid Realm, albeit a large piece. *HGHG* stresses the West Asians as prime Caucasoid peoples again and again. I think they are probably right. Since the Caucasoid Realm has one foot firmly planted in northeast Africa and another foot solidly set in India, west Asia as the area in between the two seems a logical pick for homeland. But let us look at the whole Realm and see how *HGHG* set up the trees. There are several ways to confront the Caucasoids: (1) by looking at the synthetic maps and literally going by the darkest hues, or the densest lines and shading on the colorless maps, (2) reading a taxonomy off *HGHG*'s "Genetic Tree" (p. 99) of the world (for perspective's sake), or (3) deriving the most popular genes in Caucasoid areas and seeing which peoples exemplify those the most. This is very much like (1) except it is simpler and does not involve the formidable mathematics and model-simulations at *HGHG*'s disposal.

By the first criterion (1), the prime Caucasoid area is western Asia in particular but not all of it. Plainly or not so plainly, since the maps are not actually clear always, the coreland is greater Caucasia or circum-Black Sea. The first outer layer of this onion would include northern Arabia cum Levant, the Balkans, Sardinia, and greater Iran (l'Iran extérieur). The second outer layer would have most of Europe, the rest of Arabia, northern India, Egypt, and North Africa. Finally, the Lapps and some of their poorly known relatives, northeast Africa, the Sahara, and most of the rest of India would more or less constitute a third layer. Oddly enough, the blond Caucasoids and the dark brown Caucasoids are a bit less typical than brunet Caucasoids. The central concentration area includes strong representation from five linguistic phyla — Caucasic, Kartvelian, Indo-European, Altaic, and Afroasiatic (Semitic). Dravidian, Burushaski, and Egyptian are close by, while Basque and the rest of Afroasiatic are more external. Ancient extinctions such as Akkadian, Sumerian, Elamitic, Hittite et al, Hattic, Hurrian-Urartean, Minoan, Kassitic, and Qutian or Gutian are *all* in the *core* area. Etruscan, Iberian, Dilmun, and Indus Valley are close by, and Meroitic is on the fringes. Finally, the speakers of all of those extinct languages as depicted on stone or papyrus appear to be Caucasoid, with the possible exception of Meroitic[14].

By the second criterion (2), Ethiopids would be coordinate to all the rest. Then Lapps would be coordinate to the remainder. Then Indians (both Indic and Dravidian) would be coordinate to the rest. Then Sardinians would be coordinate to a cluster of (a) North Africans and (b) Europeans versus Iranians and so-called Southwest Asians, which is called Near East later on and seems to be data from Lebanese and/or Jordanians and/or Druze or all combined.[15] I will shorten their name to Levantine, although Palestinian would be

[13] On two of their synthetic maps, in the 2nd and 3rd PC maps of Italy, *HGHG* locates a region whose patterns can be most closely associated with the old Etruscan civilization in central Italy, especially Tuscany. Then it locates another which it associates with the Ligurians, possibly pre-Indo-European people around Genoa. This is all very impressive. I hope it is true.

[14] It is only a question of memory and of being unsure whether the Egyptians depicted the Meroeans as red people or black.

[15] The categories of data presentation became a major irritant as I confronted the book deeper. The Near East here displayed yet another * column. Somewhere in this mighty tome the answer may lie!

just as appropriate. Essentially, the tree tells us that the far south (Ethiopid) and the far north (Lapps) are outliers, with the Indians a little closer in. Unlike a linguistic tree, however, the outliers are not necessarily the first split off the old tree but rather the groups which have had the most gene flow from outsiders. The central concentration and most of the outer layers equal almost exactly the ethnographic region called "Circum-Mediterranean" by G.P. Murdock.[16]

By the second criterion, but done a little differently[17], there are some changes. Ethiopians are still the most outlying group but now Dravidians are more "outer" than the Lapps. Then North Africans would be coordinate to all the rest, with Sardinians the next inner coordinate to the rest. Finally, the core group is better defined. (1) North Indians vs. (2) SW Asians and Europeans. Within SW Asia, Near Easterners versus Iranians and *Greeks* ; within Europe, Basques versus Italians and the North Sea folk. The latter are Danes and Englishmen. Here the Greeks represent to some extent the other Balkan peoples. Sadly, there are no Albanian data anywhere that I know of. I'll bet that they too would be in the Caucasoid core.

By the third criterion (3), one cannot tell for sure, that is if one is checking the basic data. Again that is due to very large gaps in the data in key areas. Synthetic, shminthetic, the maps show areas of intensity of Caucasoid popular genes — in the absence of supporting field data! There are 35 different languages spoken in the former Soviet Caucasus. Field data exist for a few. It appears that *no* HLA or GM data exist for *any* population in the Caucasus, not to mention most of the rest of the 120 genes examined globally. This is also true of former Soviet Central Asia, where nicely drawn maps imply data on the Uzbeks, Turkomen (Turkmen-lar), Kazakh, Kirghiz, and others, when in fact only a few gene frequencies have been obtained[18]. Much the same is true all the way across Siberia. Biogenetic research in the former USSR was none too active. It is beginning to grow, but *HGHG* closed its data base in 1986, they say, so none of the more current research is included.

Why does *HGHG* gloss over things like this? Who knows? Nevertheless, the custom is reprehensible scientifically and in their revised edition in the 3rd millennium, they should stop taking such liberties with the truth. It is *the* major fault with the mighty book. But perhaps I am wrong because some of the high tech presentation is beyond me? I doubt it. This is basic.

Still the fundamental concentration of popular Caucasoid genes from the Balkans to Iran, with or without the Caucasus, cannot be denied. Despite the data gaps, the premise that West Asia is primary for the Caucasoid Realm holds up. Much of this is confirmed by data on the Turks who occupy the middle of that range. While lacking GM data, we have most of the other popular genes, including HLA, on them. The Turks are close to both Greeks and Palestinians but very close to the latter in HLA. Comparing the HLA of the Turks with the Sardinians, we find them to be farther away and about the same distance as the Turks are from the Iranians. So it seems that the Turks would fall into the SW Asian group mentioned in (2) above.

Case # 3 Asia (minus SW Asia)

The transition to the east can be made through Siberia or through India. We will go to India in a bit. First, however, let us address the largest of all Asian questions. This was necessitated by the mtDNA studies which proposed an east Asian entity to be called Mongoloid for want of a better name and which was the source ultimately for native Americans. In their 1988 genetic trees, the authors proposed that North Mongoloids were linked to the Caucasoid Realm *before* they were linked to Southeast Asians and Pacific islanders — rather than the reverse as in mtDNA theories and, one must add, in traditional taxonomies in physical anthropology. So there are major issues in Asia.

Neither mtDNA research nor traditional taxonomies treated Papuans and Australians generally as members of the greater Mongoloid Realm. But dental taxonomies did suggest that those "Australoids" (my term not theirs) had a root connection with the Sundadonts and more remotely the Sinodonts, as opposed to the rest of the world. The main theorems are roughly as follows:

---

[16] In his Ethnographic Atlas which was published as part of a journal, *Ethnology*, for many years, perhaps still is. The journal is in many libraries. From the University of Pittsburgh Press.

[17] The first genetic tree used 42 populations and 120 allele frequencies and calculated genetic distance by methods used by Nei, while the second tree used "F_{ST}" and did not lump Europeans as one. There are differences between the first tree which is the one first published in *Science* in 1988 and the second one which should be regarded as more finely tuned.

[18] On page 400-402 of the Table of Allele Frequencies, another mystery population appears; good data but an unknown provenience. Located just in between West African and East Altaic, its governing category is "Asia". It is probably Indian, having GM *f;b* of 38.6% alongside GM *fa;b* of 36.7% and Rhesus *r* of 15%. What a confusing way to present data! The data are better on Uzbek than others, but the GM totals only about 64%. The remaining 36% is likely to be GM *f;b*. For the Hazara Tajiks, the same applies, i.e., GM totals 62.2%, including 22% *fa;b*, with the unstipulated 38% likely to be *f;b*. Both Tajik and Uzbek live in old Iranian country. Yet another mystery people shows up in the Caucasus under "Caucasian". In a place of such diversity, one cannot guess who they could be.

(1) C-S et al 1988 had two gross genetic categories, [A] and [B].

[A] $\Rightarrow$ {Africans} $\Rightarrow$ /San, Pygmies, Main Group, Ethiopids/[19]

[B] $\Rightarrow$ {Non-Africans} $\Rightarrow$ {B1} and {B2}

{B1} $\Rightarrow$ {B1-a} and {B1-b}

{B1-a} $\Rightarrow$ /Caucasoids/

{B1-b} $\Rightarrow$ /North Mongoloids (Asian & Amerind)/

{B2} $\Rightarrow$ {B2-a} and {B2-b}

{B2-a} $\Rightarrow$ {S} $\Rightarrow$ /Sundadonts or Southeast Asians, Pacific islanders, including Micro-, Poly-, Mela-nesians/

{B2-b} $\Rightarrow$ / Papuans or New Guineans + Australians/

(2) mtDNA proposals lacked a global taxonomy like (1) but did at least set up the equivalent to [A] and [B] above. Then proposals, focused more on the Pacific Rim, both mtDNA and dental, essentially ignored the Caucasoids, except implicitly treating them as a major category other than the big one in the Pacific area. Teasing out the logic of those proposals seems to lead to the following:

Humanity $\Rightarrow$ [A] and [B], as in general above.

[B] $\Rightarrow$ {B1-a} and {P}. {B1-a} is as above.

{P} $\Rightarrow$ {M} and {B2-b}, where {P} is Pacific Rim and {B2-b} is as before.

{M} $\Rightarrow$ {B1-b} and {S}, where {M} stands for Mongoloid.

At the crucial point, the two differ in what they do with the northern East Asians and the native Americans. One puts them with the Caucasoids and the other puts them with the "other" Mongoloids of southern East Asia and the adjacent islands. No substantial resistance to the joining of northern East Asians and native Americans seems to exist. Restricting the American links to the Arctic peoples does not follow, of course, and that is not the view of most physical anthropologists.

(3) I repeat that the first hypothesis (1) was C-S et al of 1988. That was based on "modified Nei's" calculations of genetic distance. In *HGHG*, they specifically modify their 1988 thesis, since they are using the new improved F_{ST} system of making the calculations, and they propose some changes to (1) above. First, it is still [A] and [B]. The rest is like this:

[B] $\Rightarrow$ {P1} and {C}

{P1} $\Rightarrow$ New Guinea and Australia

{C} $\Rightarrow$ {S} and {B1}. (Note that {S} = {B2a} as above)

{B1} $\Rightarrow$ {B1-a} and {B1-b} which in detail is

{B1-b} $\Rightarrow$ N.E. Asia + Arctic (versus) Americans

In plain language, this means that the Southeast Asians have been separated from the "Australoids" and put into a category with Northern Eurasians and Americans but as a coordinate sub-category. Eskimos as part of Arctic are still joined to North East Asians as separated from other Americans. But most of all "Australoids" have a status almost as separate as the Africans.

HGHG reminds us (p. 79) that in 1988 the "modified Nei tree was tested by bootstrap. . ." and they wished to point out that "the second bifurcation shown in Fig. 1, separating North Eurasians from Southeast Asians" was of special interest. "This split occurs most often among bootstraps, but two alternative partitions are also fairly frequent: one separates Caucasoid from all Asian, Oceanian, and American populations, and the second separates New Guinea and Australia from all other non-African populations." Their new work via F_{ST} favors the last partition.

After devoting two pages (80-81) to genetic distances and bootstrap results — a meticulous analysis I might add —, they allow as how the two views about the Southeast Asians are almost equally weak. They believe that gene flow from Northeast Asians to Southeast Asians has distorted things somewhat, and the best analysis would be one like (1) above, which joins all the peoples of the southern Pacific and mainland Southeast Asia against the north Chinese, Korean, Japanese, and other northern Mongoloids and the Caucasoids. We will skip over their presentation of mtDNA research which MT has mostly covered and focus on their exciting reports

[19] We can borrow a very productive and useful way of doing categories from transformational grammar. The symbol $\Rightarrow$ means "re-write as, is composed of". What follows it on the right are sub-categories. It is brief and effecient.

of Y Chromosome and nuclear DNA research. Some of their results are vitiated by their choice of Melanesians and general Chinese, both especially intermediate and dubious choices.[20]

The Y Chromosome research evokes their comment: "The molecular basis of the variation [in "male-specific bands" — HF] is not clearly understood, but the system is powerful in distinguishing ethnic origins. The A_1 band is virtually absent in 900 Caucasoids examined . . . and present in 70%-80% of West and South Africans examined. A variant D band was found in all 22 populations examined. In studies on Mediterranean populations . . . several haplotypes showed unusually large variations in frequency among neighboring populations (Algerians, Tunisians, northern Italians, southern Italians, and Sardinians). Clearly, variation here is of considerable interest, and parallel studies with mtDNA and nuclear genes in the same populations might be especially informative."

In nuclear DNA ("DNA Polymorphisms in Nuclear (Chromosomal) Genes") (p. 89-90), two tables are given which very clearly show the split between Africans and non-Africans. We can stipulate that, but the other results may be more germane to our Asian problems. The first table (#2.4.1) shows the relative haplotypes of five RFLP in the b-Globin region. They warn that this is a limited number of genes in the hemoglobin region, which is "subject to intense natural selection in malarial areas where most samples investigated for hemoglobin originate." The table follows:

Haplotypes (Number)	European (169)	Indian (111)	Thai (32)	Melanesian (173)	Polynesian (55)	African (61)
+ - - - -	63.3	52.2	90.6	67.6*	78.2	1.8**
- + - + +	26.0	25.2	0.0	16.7	10.9	9.8
- + + - +	8.9	13.5	6.2	1.7	0.0	1.6
- - - - -	1.8	2.7	0.0	0.0	0.0	0.0
Several other haplotypes	0.0	6.3	3.1	4.0	3.6	0.0
- + - - -	0.0	0.0	0.0	0.0	0.0	3.3
- + - - +	0.0	0.0	0.0	2.3	7.3	19.7
- - - - +	0.0	0.0	0.0	1.2	0.0	60.7

*Only totals 93.5%
** only totals 96.9%

European and Indian form a group, as do Thai, Melanesians, and Polynesians. The Africans get closest to Polynesians, but that is still far less than the islanders get with any non-African. However, Melanesians upset the pattern a bit because they are actually a bit closer to Caucasoids than to Thai or Polynesians. One could suspect malaria, since Melanesia has plenty of it, or some gene flow from Caucasoids. European colonials lived there, and World War II passed through Melanesia, with Japanese against a Caucasoid mélange.

Table 2.4.2 "Matrix of Genetic Distances of Five World Populations Tested for 100 DNA Polymorphisms". (Standard error figures not shown in our table)

[20] While the Melanesians in general terms embrace peoples who are nearly all "voyager" type Austronesians and some who are mostly diluted Papuans, the term also is extended sometimes to all Papuans. In the two cases reported, one has unknown provenience and the other is from Bougainville in the Solomon islands. Most of that island is "voyager" type and a few populations are more Papuan. In the case of Chinese, through whose country the main split between Northern Mongoloids and Southeast Asians seems to run, lumping north and south Chinese together is unfortunate. The nuclear DNA results reflect some of this ambivalence.

	Aka	Mbuti	Melanesia	Chinese	European
Aka Pygmies	---	0.023	0.133	0.144	0.088
Mbuti Pygmies	0.043	---	0.139	0.139	0.084
Melanesians*	0.242	0.265	---	0.094	0.086
Chinese**	0.235	0.235	0.171	---	0.058
Europeans	0.141	0.142	0.148	0.093	---

* Sampled at Bougainville in the Solomons.
** Both north and south China.

[Note: in this table, **low** numbers are **closer**, higher numbers more distant.]

The upper right triangle corresponds to Nei's distances. The lower left corresponds to F_{ST} distances. Some of the variation in the table is "unacceptable", according to *HGHG*. The Aka Pygmies are cited in their table as Central African Republic and the Mbuti as Zaire Pygmies. The latter might be Efe.

There is an interesting calculation involved in their attempts to make trees out of this data. In Fig. 2.4.4 (p. 91), they seem to put chronological values on the nodes of the "rooted tree". The common period is 100,000, which I take as an initial assumption. The next figures probably are reckoned from the genetic distances. They are:

(A) common Pygmy period = 20 kya (they do not say kya)
(B) common non-African period = about 73 kya or Melanesians split off from Chinese and Europeans.
(C) common Chinese-European period = about 45 kya.

The Melanesians here figure to be more Papuan than Southeast Asian, especially since they are just as far away from Europeans as they are from Africans. If the Chinese here are northerners, then this 45 kya could date *Borean*. If they are southerners, then it practically demands that the Melanesians be "Australoids". I believe Stoneking's mtDNA calculations proposed very similar dates, reported in *Mother Tongue*. In future, research should seek Japanese or Koreans to counterpose to Cambodians or Hmong, numerous in the USA.

However, we are left with no firm conclusion about Southeast Asia and the "Australoids". Taking into account that both mtDNA and dental research lean towards integrating the Sinodonts and the Sundadonts, we may suggest that the *HGHG*'s alternative that non-Africans are divided into "Australoids" and non-"Australoids" is closer to the truth. It is on the whole an unsatisfying conclusion. But considering the great complexity of the southwestern Pacific, it's the best we can do — for now anyway.

But we have to be salient about a key point. The *language and genes correlation* is seriously affected by the outcome of this uncertainty about the classification of the Southeast Asians and the slightly more secondary question of Caucasoids versus northern Mongoloids[21]. Starting with the lesser question, it bodes poorly for a Nostratic correlation with genetic north Eurasians if northern Mongoloids seem fundamentally more akin to southern Mongoloids than to Caucasoids. Most of Greenberg's Eurasiatic is spoken by Mongoloids. *HGHG* stands firm on the commonality of north Eurasia and the New World. But some mtDNA work denies that commonality. Others find a descent group in Amerinds, Na-Dene, and part of the Arctic group, i.e., Eskimos. That would seem to override *HGHG*'s separating them. But that does not necessarily imply that they do not link up with the rest of the northern Mongoloids, i.e., they might still be northern Mongoloids.

Nor is the Nostratic hypothesis a kind of weak concept waiting to be confirmed or denied by solid biogenetic work. Nostratic has become a *strong* hypothesis and its basic notion that northern Eurasians largely fall into one genetic group, a linguistic one, has a bearing on the biogenetic stuff. Nostratic argues that, at a considerable time depth, such peoples as the French and the Chuckchi share common ancestors — at least a linguistic community and its culture. And that is not so trivial, mes amis.

The Dene-Caucasic hypothesis is another that bridges the two ends of Eurasia and adds a piece of North America. If Bengtson is correct in his Macro-Caucasic amendment to Dene-Caucasic (D-C) theory, then D-C may have had two clumps of phyla or sub-macro-phyla, *x* and *y*. The first spoken in western Eurasia (Basque, Caucasic, Burushaski) and the second spoken in eastern Eurasia (Yeniseian, Sino-Tibetan, Na-Dene). The Himalayas would be the logical candidate for the reason for the split. But if the *y* portion of

[21] Having tried to shout down the use of the terms Mongoloid and Caucasoid because of their lingering adherance to race labels, I have to give up. Neither term is really so pernicious as a taxon label because each is so *apt*. We just do not forget that they are gross labels for huge chunks of mankind but not the bases for "us" against "them" or "who is superior?" type thinking.

D-C is spoken by people affiliated genetically with somebody other than the *x* people, then a language and genes correlation no longer exists.

Please note that I do not deny other explanations for this kind of situation. A language spoken by an eastern Eurasian population of Mongoloids could have been acquired by a population of Caucasoids in the west. Or vice versa. I worry only about the language and genes correlation.

Language and genes correlations can also be encouraging to new theories. What I have called *heurisms*. Linguists are encouraged to seek genetic links between such as Austro-Thai and Austroasiatic. The biogenetic links are powerful. Similar links between Papuan and Australian are indicated, though they are remote. Or one can just contemplate the Indo-Pacific hypothesis, which correlates powerfully with half of the "Australoid" world. Finally, the uncertain links of (eventually) validated Austric with Indo-Pacific and/or Australian and/or Sino-Tibetan should be hard to find and few in number. This would be an exercise only for the bravest scholars, like Bengtson and Blažek, were it not for the fact that more than 2500 languages are involved. You can find a lot of scarce nuggets in 2500 languages!

Asia cannot be surveyed in detail in this issue. Nor can the Pacific or the Americas. It is possible to criticize *HGHG* as one colleague has done for "not paying enough attention to Southeast Asia (SEA)". Yes, SEA and the southwestern Pacific, except for southeastern Australia and New Zealand, lie right in the middle of the tropical zone. Natural selection is more variegated in the tropics, I suppose. In addition, this whole area is a world of islands. Local isolations leading to more serious genetic drift and larger consequences for gene flow should cause great heterogeneity, possibly the greatest on earth. All this should impede gross continental type classifications. In fact, the gross ones are what can be obtained and the finer details are harder to obtain. Yet the insular tropics are not the only place where such difficulties face us.

India[22] has created a kind of social insularity with its caste system. It is also a difficult place for more detailed classifications. In gross terms, most of India is in the Caucasoid Realm or, if not, has one foot in that realm. All of Pakistan, save a small bunch of Tibetans in the far north, is solidly Caucasoid. That includes the Burusho of Hunza, whose published data were overlooked in *HGHG*. Much of India is also Caucasoid, but central and eastern India proper have many peoples who are far less Caucasoid than they are something else. Rather like some Ethiopids. Those exceptional populations correlate strongly with Dravidian and Munda languages. Roughly the western third of the Himalayas is predominantly Caucasoid, while the eastern two-thirds goes from Tibetan type Mongoloid to Burmese type Mongoloid from central Nepal to Nagaland on the Burmese side.

HGHG agrees with an Indian anthropologist, Vidyarthi, about the "four major components of the genetic structure of India..." Their theses are briefly summarized here below:

1) First were Australoids or Veddoids from the Paleolithic, surviving today possibly in a few tribals or in Burusho or Nahali. They have darker skin, sometimes frizzy hair and small stature. No trace of their original language exists, except possibly Andamanese in the Bay of Bengal. The *Kadar* of Kerala, nomadic laborers, are an Australoid remnant, they think. At least the Kadar are outliers in the genetic structure of India.

2) A major migration of Caucasoid Neolithic farmers with Dravidian languages (proto-Dravidian, they say) set a dominant pattern in India. They still form a major segment of central and southern India. We have no data on the Brahui, northern Dravidians west of the Indus river.

3) Then came the Aryans. Bronze Age Caucasoids with Indic languages entered India "from their original location north of the Caspian Sea, via Turkmenia and northern Iran, Afghanistan, and Pakistan." We are not told what the Caucasoid differences are, between Dravidians and Aryans. Not said is the Aryan predominance in northern and western India.

4) In the northeast and central India, a mixed bag of Austroasiatic and Sino-Tibetan speakers. They "are a witness to other major migrations and infiltrations, mostly from the east and northeast. They are even less well known than the other three components and probably more diverse. In the case of Munda, their genetic similarity with the Dravidians indicates that their migration may have taken place before the Aryan expansion to the eastern part of India." None of these groups is very predictable in genes, which may be partially Caucasoid, partly akin to SEA, and partly like Tibetans (who are northern Mongoloids). Do the authors really think the Australoids are better known than the Munda? Heavens!

First, we can stipulate the Indic-speakers and many of the Dravidians as being mostly in the Caucasoid Realm. Secondly, forget about the Australoids; that is an old tradition which talks about survivals of such great time depth — theoretically — that

[22] I use India here in the old British sense of South Asia, including Pakistan, India, Bengaladesh, Sri Lanka, Nepal, Bhutan, et al. But not Afghanistan or Burma. To *HGHG*, "Indians" seem to be north Indians, Aryans, Indic-speakers. "South Indian" = Dravidian.

evidence would be too skimpy. Furthermore, *HGHG* puts the Australoids in India and Philippines (Negrito) notion to sleep. On page 356, *HGHG* declares that both Kadar and Negrito resemble peoples closer to themselves than either does Papuans or Australians. Other physical anthropologists have reached this conclusion. But there is more. The Kadar, according to *HGHG*'s data, are very similar to the ethnologically famous dairymen, the Toda, who live near by (unreported in *HGHG*'s genetic trees). The Toda have GM *f;b* of 75%, while the Kadar have *f;b* of 79%. Both groups have about 20% of *za;g* but no *fa;b* at all. If the field data are correct, and there is some doubt about that, then these south Indian Dravidian-speaking aboriginal populations are fully Caucasoid with virtually no ties to Southeast Asia. That is remarkable, but it is also one of Carleton Coon's surprising conclusions. He also said the Veddah of Sri Lanka were Caucasoid.

Thirdly, *HGHG*'s genetic trees distinguish clearly between Himalayan, mostly Tibetan, peoples (Gurkha and Tharu) and eastern India's Munda (with the Dravidian Oraon very close to them). Indeed, the Himalayans are the second clearest outlier in India after the Kadar. The Munda themselves are part of the Austroasiatic "genetic and linguistic" class along with the Viet Muong, Khasi, and Semai. But in India the Munda do cluster with a primarily Dravidian group (with some Indic) from central India. The suggestion is strong that both groups have exchanged genes, perhaps in the past before the caste system developed.[23] Incidentally, the assumption that the Munda migrated from southeast Asia occurs *passim*, most especially on pages 239 and 241, and this is based on nothing more than a traditional preconception. Linguistic data suggest most strongly that the Nahali in central India and the Burusho in the northwest are the most likely to be authochthonous. But the Munda are the next most likely. If we can discard fuzzy notions like the archaic Australoids, we might see the real natives more clearly. But there is precious little supporting evidence for people much like the Papuans or Australians.[24]

A Kind of Summing Up

The size of the book has finally overcome the review. I will stop at this point roughly half way around the world, except for summing up some general points of great interest to all long rangers. *Mother Tongue* 24 must be put out, after all! The key findings will simply be numbered — no special order.

1) *HGHG* supports the two related hypotheses in Southeast Asia and the insular Pacific. Austronesians and Daic peoples do have a genetic reason as well as a linguistic reason to be classified together. Furthermore, various Austroasiatic populations in Southeast Asia and India do form a class unto themselves. The combining of both clusters supports the notion of Austric too.

2) On page 232: "The tree clearly shows that northern and southern Chinese have different genetic backgrounds. Here, as in other investigations discussed later, the northern group always associates with Mongols or in general with speakers of Altaic languages, and the southern group with Southeast Asia." This fact was first discovered by GM research but has lately been confirmed by a large HLA study, unpublished, by Saitou. It remains to remind everyone that *HGHG*'s *Altaic* includes Japanese, Ainu, and Korean, as well as Mongols and Tungus. Indeed, their Altaic comes close to being more than half of Greenberg's Eurasiatic.

[23] However, again the shrill critic, it seems that *HGHG* show no HLA or GM data at all for the Munda and only GM for the Oraon who, however, lack Duffy. Too much missing data vitiates much of the discussion of these Indian peoples. And to base a grand Austro-asiatic scheme on so little data is still reprehensible.

[24] There is anecdotal evidence, as one might call it, in photos and phenotypical descriptions of some Indians, usually tribal, who do resemble Papuans and Australians quite a lot. The most famous photo, often mentioned, is of former Indian Foreign Minister, M. Krishna Menon. I believe he was a Dravidian and from the south. And such a face can find resemblances among Caucasoids. But if we have learned anything from the famed hairy Ainu, it is that appearances can be deceiving!

3) There seems little doubt that Japanese, Korean, and Ainu are united in a cluster. Lord knows how the Ainu came to differ so markedly in hairiness. Still, northern Mongoloid body hair is the skimpiest on earth, but perhaps a series of mutations which functioned as depilatories failed to do well in the Ainu gene pool. These biogenetic findings do lend strong support to the notion that Japanese is an Altaic language (or Eurasiatic), rather than a member of Austro-Tai. Perhaps more interesting is the bearing of Ainu genetics on this question and the question of Jomon archeological culture. The Ainu are outliers vis-à-vis the Japanese, Koreans, and Tibetans but their closest ties are with the Japanese of Hokkaido (old Ainu land) and with the Ryukyus. This is consistent with the "Ainu and Ryukyuans are derived from Jomon" hypothesis. More telling, however, is that the Ainu are closer to North Chinese than to South Chinese and that the Ainu have much less GM*fa;b* than Ryukyuans, other Japanese, Koreans, or most northern Mongoloids do. This is not a good token of a Southeast Asian remnant population..

4) On page 327, they say: ". . . America, in particular South America, is genetically the most variable part of the world. . . An independent approach that leads to the same conclusion is the study of mitochondrial DNA." What bedevils linguists is the same thing that bedevils geneticists. There are hundreds of distantly related populations with plenty of time to nurture their differences since the time of their common origins. Every reason for becoming different seems to be maximized. We can sympathize with Ives Goddard in exclaiming that there was a terrific diversity among the Amerinds. We can empathize with Greenberg who saw the common origins underneath the diversity.

5) Of great interest to us all on New World questions. Later in the same paragraph of page 327 they say: "Analyzing DNA markers makes it easier to identify specific mutants and may help us to follow specific migrations more closely. Inferences about the number of migrants [sic — HF] to America that have been made in some mtDNA papers, even with techniques allowing of higher resolution than those above, seem largely unwarranted at this stage of our knowledge." They meant to say "migrations", but the editor missed that. It is difficult to know what to do with a statement with the word "unwarranted" in it. So we will leave the matter at that.

6) On page 327, also they say: "In North America the major divergence is between Eskimos and non-Eskimos, with Na-Dene showing more similarities to the former than to the latter." Moreover, in the eastern part of North America where contact with Caucasoids has been long and intense, there is evidence of Caucasoid admixture. Naturally, if the Mexicans had been included, even more evidence of Caucasoid admixture would have been found.

There is so much about the Pacific peoples to discuss that to stop now is sad. But we must end the review here. I would urge all who can afford it to buy this book because no review can come close to revealing what is in it. It is unlikely that any evaluation can do justice to such a gigantic undertaking. I have tried and my failure is evident to me. Congratulations again to the authors! Do it again, good colleagues! But stress new data, heed the criticisms, and down grade the high tech math and computer stuff.

Literature Cited

Biasutti, Renato, et al. 1955. *Razze e populi della terra*, vol. 3, Africa. Torino. (Perhaps the closest to G.P. Murdock in approach but with more archeology, anthropometrics, religion and art.)

Bonné-Tamir, Batsheba. 1980. "The Samaritans — A Living Ancient Isolate." In W. Eriksson et al, eds. *Population Structure and Genetic Disorders*. Pp. 27-41. Academic Press, London.

Bonné-Tamir, Batsheba and A.Adam., eds. 1992. *Genetic Diversity among Jews: Diseases and Markers at the DNA Level*. Oxford University Press, New York.

Briggs, Lloyd Cabot. 1960. *Tribes of the Sahara*. Harvard University Press, Cambridge.

Cavalli-Sforza, Luigi Luca., ed., 1986. *African Pygmies*. Academic Press, New York, etc.

Cavalli-Sforza, L. L., A. Piazza, P. Menozzi, and J. Mountain. 1988. "Reconstruction of human evolution; bringing together genetic, archeological, and linguistic data." *Proceedings of the National Academy of Sciences* (USA) 85: 6002-6006.

Coon, Carleton. 1965. *The Living Races of Man*. Alfred A. Knopf, New York.

Ehret, Christopher. 1971. *Southern Nilotic History: Linguistic Approaches to the Study of the Past*. Evanston, Illinois.

Ehret, Christopher. 1977. *Ethiopians and East Africans: The Problem of Contacts*. East Africa Publishing House, Nairobi.

Fleming, Harold C. 1965. *The Age-Grading Cultures of East Africa: An Historical Inquiry*. Unpublished doctoral dissertation, University of Pittsburgh.

Fleming, Harold C. 1969. "Asa and Aramanik: Cushitic Hunters in Masailand." *Ethnology* VIII: 1-36.

Fleming, Harold C. 1978. "Ethiopians and East Africans." Review of Ehret 1977 (listed above). *International Journal of African Historical Studies* xi, 2: 267-282.

Fleming, Harold C. 1980. "Microtaxonomy: Language and Blood Groups in the Horn of Africa." In Robert Hess, ed., *Proceedings of the Fifth International Conference of Ethiopian Studies, Session B. APRIL 13-16, 1978*. Pp. 25-49.

Fleming, Harold C. 1983. "Kuliak External Relations: Step One." In Rainer Vossen and Marianne Bechhaus-Gerst, eds., *Nilotic Studies. Proceedings of the International Symposium on Languages and History of the Nilotic Peoples, Cologne, January 4-6, 1982.* Dietrich Reimer Verlag, Berlin. Pp. 423-478.

Fleming, Harold C. 1987. "Hadza and Sandawe Genetic Relations." In Franz Rottland, ed., *Proceedings of the International Symposium on African Hunters and Gatherers, Sugia*, Band 7, 2: 157-189.

Garn, Stanley M. 1971. *Human Races*. 3. Charles C. Thomas, Springfield, IL.

Heine, Bernd. 1976. *The Kuliak Languages of Eastern Uganda.* East African Publishing House, Nairobi.

Hiernaux, Jean. 1975. *The People of Africa.* Scribner's, New York.

Howells, William W. 1967. *Mankind in the Making.* Doubleday, New York.

Meinhof, Carl. 1906. "Linguistische Studien in Ostafrika." *Mitteilungen des Seminars für orientalische Sprachen*, IX. 3: 294-323. (Meinhof had many other well-known writings on Africa, including *Die Sprachen von Hamiten.* This one is specifically on Tanzanian Cushites.)

Merker, M. 1904. *Die Masai: ethnographisches Monographie eines ostafrikanischen Semitenvolkes.* Dietrich Reimer Verlag, Berlin. (Includes the first data on Asa, a South Cushitic language spoken by hunter-gatherers in Tanzania. Merker recognized their temporal priority over the Masai and the Tatoga who had also preceded the Masai but he thought the Asa must also be Semites. G.W.B. Huntingford was critical of Merker's "valueless discussion of Masai racial affinities.")

Mourant, A.E. 1954. *Distribution of the Human Blood Groups.* Blackwell Scientific, Oxford. The first of four volumes in 24 years with colleagues.

Murdock, George Peter. 1959. *Africa: Its Peoples and Their Culture History.* McGraw-Hill, New York.

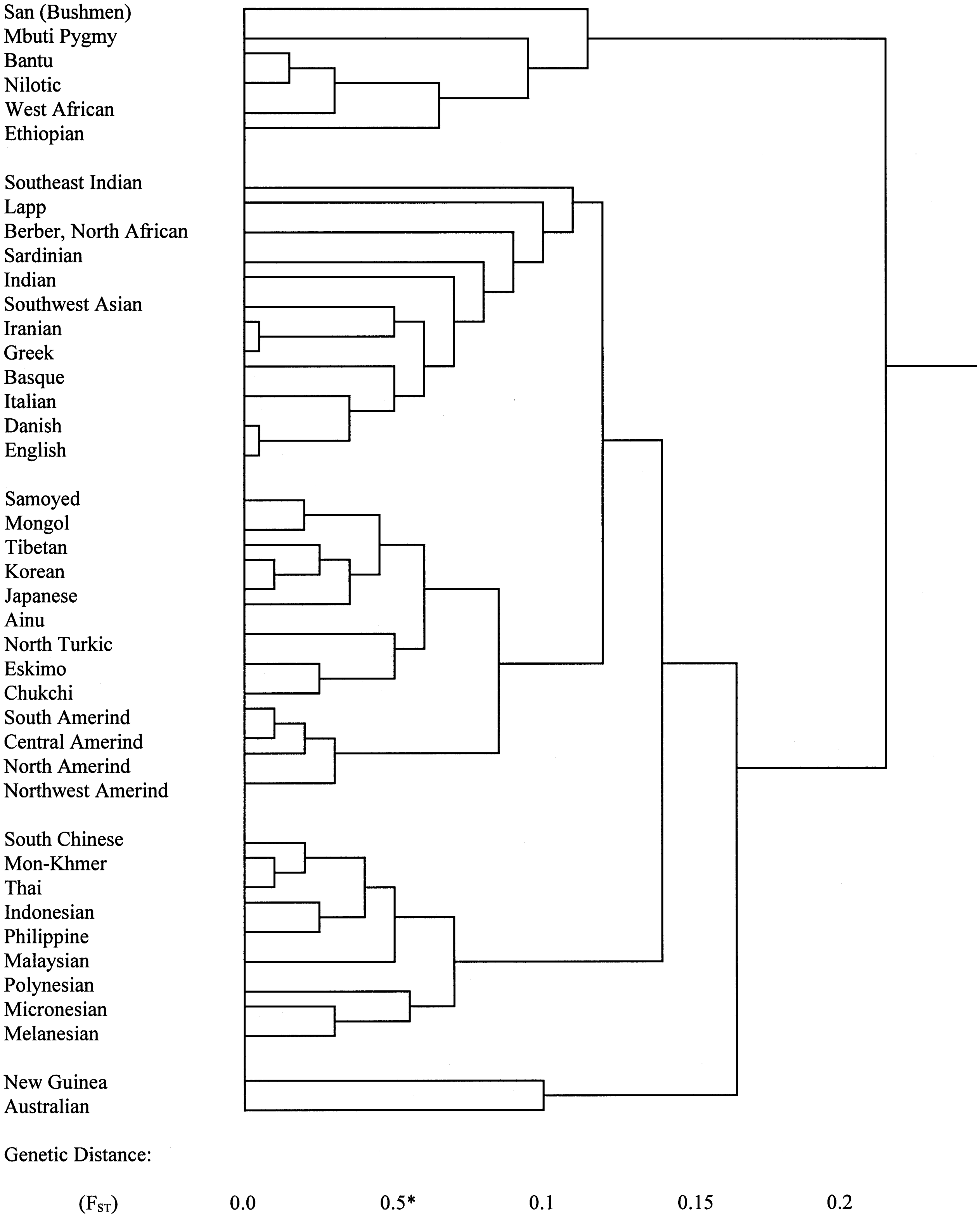
Sherratt, A., ed. 1980. *The Cambridge Encyclopedia of Archeology.* Crown, New York. (Not referred to in the review. Cited very frequently in *HGHG.*)

Steinberg, Arthur G. and C.E. Cook. 1981. *The Distribution of the Human Immunoglobulin Allotypes.* Oxford University Press, Oxford.

Vansina, Jan M. 1966. *Kingdoms of the Savannah.* University of Wisconsin Press, Madison and Milwaukee.

The master dendrogram referred to earlier is, as follows:

[We have adapted the master dendrogram on page 78 of *HGHG*. In *HGHG*, it is Fig. 2.3.2.B "Average linkage tree for 42 populations. The abscissa shows the genetic distances (F_{ST}) calculated on the basis of 120 allele frequencies from the systems listed in for Fig. 2.3.2.A. They are not clustered here and are kept separate." (Their diagram follows here, but reversed from right to left.)]



[*Note: this is an error. It should be 0.05 — HF.]

THE "SOGENANNTEN" ETHIOPIAN PYGMOIDS

HAROLD C. FLEMING
Gloucester, Massachusetts

Subsidiary information on so-called Ethiopian Pygmoids; data first gathered by Pampiglione and Cavalli-Sforza in 1970 and still unknown to Africanists! These are interesting data because the sample size was 62, and a number of genes were sampled, including GM and Duffy. The attribution of Pygmy status can best be ignored, yet there are other valuable things about this group.

They are the famous Manjo, the hunter outcast group, or the autochthones, as Kafa say. Not only were they found in the hills around Bonga in southwestern Ethiopia, but C-S himself heard them referred to as Manjo. Although we do not know if their language was more akin to proper Kafa or to the so-called jargons recorded by Cerulli, any of those will do as Omotic speakers — almost certainly Nomotic speakers. Since they are the Manjo, rather than the Minjo, we can see them as outside of the circle of gene flow and cultural borrowing in west central Ethiopia from which the Kafa proper (Minjo) and Gongan in general + the Yemna (Janjero) + the eastern Ometo (at least) are derived. The Manjo are more like the Mao, Dizi, and Ari clusters to their northwest and south. Or as the Amhara would put it, the Manjo are a variety of Shanqilla (pagan and/or Negro and/or both).[1]

This information is great because we have some biogenetic data on the eastern Ometo, GM data reported by Steinberg as partly Sidamo (Kambata more precisely) and Wolayta (Wallamo) or Highland East Cushitic and Ometo of Nomotic respectively. When we present that limited data below, we will call it Ometo. Along with the Manjo data, the Ometo give us the first glimmerings of old Omotic genetic patterns. And it is *very interesting*!

For comparison, we throw in some of C-S's data on Tutsi of Rwanda and Mbuti of Zaire. Plus Amhara who, despite being Semitic in speech, represent the Agau, a neighboring highland Cushitic population. While data on the Koman peoples of Nilo-Saharan, who abut Nomotic peoples to the west, would be most valuable, there is none anywhere. The closest we can get are the Funji (Dar Fung) who are Berta speakers but influenced by Nubians and/or Arabs. And assorted Nilotes called Nilo-Saharan by C-S. There is more variability in the assortment than one would like. Finally, data on northern Bushmen (Hadza) are added because of their suspected links to Ethiopia and the Horn. In *GM*, San data are used.

In the ABO system of blood groups, the genes are p, q, and r with varieties p1 and p2 recognized. The relevant gene frequencies are: (A1, A2, B, O are phenotypes; genes p1,p2,q,r underlie them)

	p1	*p2*	*q*	*r* (the most common gene)
Manjo	.152	.105	.075	.669
Ometo	—	—	—	—
Tutsi	.097	.021	.109	.773
Mbuti	.228	.023	.119	.630
Amhara	.127	.096	.158	.618
Funji	.054	.097	.152	.697
Nilotes	.116	.044	.132	.690
Hadza	.197	.166	.058	.579

In the MNSs system, there are four primary genes M, N, S, s. They are often cited as *haplotypes* MS, Ms, NS, Ns, but the frequencies of M and N are instructive in themselves. An allele very common in central Africa (S^U) is merged in these data with *s* for technical reasons; I cannot determine if any were found among the Manjo. This is unfortunate. Moreover, the Henshaw allele cannot be determined.

[1] The source of information about their name being Manjo and their physical size was Cavalli-Sforza (personal communication, 1995); it is much appreciated. For a discussion of Kafa ethnography, see Ernesta Cerulli 1956. For the only published field data on Manjo varieties, called jargons by the author, see Enrico Cerulli 1951. For a recent discussion of the Manjo in relation to the Mao and the "acculturated" or "ta/ne" branch of Nomotic, see Fleming 1984. For a recent discussion of possible pigmies in farther southern Ethiopia, see Fleming et al 1994.

	M	*N*	*MS*	*Ms*	*NS*	*Ns*
Manjo	.726	.274	.151	.575	.122	.152
Ometo	—	—	—	—	—	—
Tutsi	.611	.389	.278	.333	.080	.309
Mbuti	.440	.560	.037	.403	.073	.487
Amhara	.647	.353	.247	.400	.037	.316
Funji	.611	.389	.073	.499	.096	.196
Nilotes	.570	.430	.099	.440	.075	.347
Hadza	.596	.404	.105	.491	.034	.370

In the Rhesus system of blood groups, at least nine haplotypes are distinguished in the *African Pygmies*; only six are reported here because the relevant populations lack the three, viz., $CD^{U}e$, cdE, and $cD^{U}E$. The most famous of these haplotypes, CDe, cDe, and cde, are sometimes called R1, R0, and r respectively. R1 is most common from Europe to Papua, R0 in Africa, and r or rh-negative among the Basques and other "Caucasoids" from the Atlantic to the Bay of Bengal.

	cde	*$cD^{U}e$*	*cDe*	*$C^{*}de$*	*CDe*	*cDE*
Manjo	.178	.040	.661	.000	.081	.040
Ometo						
Tutsi	.340	.063	.460	.089	.007	.024
Mbuti	.000	.272	.653	.000	.007	.067
Amhara	.256	\ .497	/	?	.185	.043
Funji	.259	\ .731	/	?	.010	.000
Nilotes	.181	\ .749	/	?	.035	.023
Hadza	.043	\ .888	/	?	.013	.056

Note: the Tutsi also have *$CD^{U}e$* at frequency .017 . Several Nilotic populations have no *cde* but 90%> of *cDe*.

In other blood groups for the Manjo, there are recordings for P1, Lutheran, Kell, and Duffy. P1 and Duffy are useful in distinguishing between Africans and non-Africans, but also between Bushmen and other Africans. P1 frequencies are normally high in Africa, and in many populations the dominant world alleles of Duffy, Fy^{a}, and Fy^{b} are absent or virtually so. The so-called "silent" allele for those African populations is usually called Fy or Fy^{0} or non-a and non-b. Thus the Manjo frequency of Fy^{a} is highly significant.

	P1	*Lu^{a}*	*K*	*Fy^{a}*	*Fy^{b}*	*$^{*}Fy^{0}$*
Manjo	.716	.093	.024	.328	.000	*67%
Ometo						
Tutsi	.564	.000	.032	.041	.000	*96%
Mbuti	.728	.019	.000	.034	.004	*96%
Amhara	.622	.051	.030	.187	.213	60%
Beja	.588	.025	.005	.127	.086	79%
Funji	. . .	. . .	.005	.005	.000	*99%
Nilotes	.779	.090	.000	.012	.000	*99%
Hadza	.760	.066	.004	.000	.004	.996

Note: Figures with * are my calculations, hence hypothetical

Immunoglobulin Markers or Gamma Globulin + Km and a few others. These data are difficult to present because of differences in nomenclature between sources and because the genetic analyses were not completed for the Manjo. Suffice it to say that the major issues are the presence of and varying percentages of what we can call loosely *za;b*, *za;g* and above all *f;b* or simply *f*. There are a number of allotypes whose first segments are *za* + varieties of *b* and *c*. These are found in great strength in Africa but also in Papua. Unfortunately *f* together with *b* occurs as the predominant allotype segment among Caucasoids but also among Southeast Asians and speakers of Austronesian generally. In clearly presented allotypes, they are always different, as *fb* for Caucasoids and *fanb* for Southeast Asians. Since anything with *f* is rare in central or southern Africa, and absent among "genuine Negroid populations" (p. 206), the varieties with allotype *f* are significant. C-S reports that the Manjo have about 24% of *f*. As a number, it has to be taken to mean simply that the Manjo have about 24% non-Negroid allotypes which relate to either the Caucasoid realm or the southwest Pacific area. Although the Caucasoid option is far more likely than the other, because of the proximity of *f*-bearing Ethiopian populations, still the emigrant southeast Asians on their way to Madagascar did touch East Africa, and their crops and some of their material culture did reach southwestern Ethiopia. So ancient Malgache cannot be absolutely ruled out.

We will present the GM allotypes in standard forms and hope that everyone can make the proper inferences from the data.

Frequencies of Immunoglobulin Haplotypes

	za;b	*za;b1c3c5*	*za;b1c3b5*	*za;g*	*f;/n*
Manjo	\	52% of (za-)	/	.246	.237
Ometo	.393	.011	.125	.236	.157
Tutsi	.556	.114	.163	.064	.104
Mbuti	.916	.046	.034	.000	.000
Amhara	. . .	.629	.000	.113	.240
Beja	. . .	.407	.105	.234	.235
Nilotes	.744	. . .	.171	.074	.011
San	.282	.042	.012	.137	.000

Note: the first two columns are likely to be in error and ought to be merged. Mark the dissimilarity between Mbuti and San or Pygmy versus Bushman. The Beja are thrown in to offset the Amhara, who appear to be more remote from Ometo than Beja are.

Note: The San also have about 50% *zabs* = *zab0b3b5s*, a noted "Bushman" haplotype, so called. Both Amharic and Ometo have low percentages of *zabs* (Ometo .061), which is also present among Ashkenazi Jews (Steinberg 1981). Ometo is called "Sidamo" by Steinberg; they also have a small amount of *zaxg* (.018). Nilotic GM data were substituted for Funji because of linguistic connection and geographic proximity. However, one is warned that these Funji are very different from Nilotes because some of them derive from the complex mix that is "northern Sudanese", i.e., Nubians partially derived from Meroeans plus Beja plus Hilali Arab Bedawin plus Aksumite Ethiopians. The greater bulk of the Funji are presumed to be Berta who are distantly related to the Nilotes within East Sudanic of N-S in the older Greenberg classification but even more distant in recent revisions of N-S internal taxonomy.

We really do not know who the Funji (from Dar Fung) are ethnically. Basically they are a population from the eastern savannahs and low hills of the Sudan, rather than the forested highlands of Ethiopia where the Manjo live.

As the Manjo appear to have some affinity to Amharas and other Afroasiatic speakers, but some too with Nilotes, one might add a final quote from the *African Pygmies*, pp. 198-99, from Nijenhuis and Hendrikse's "Blood Group Studies in Pygmies and in a Few Bantu Populations" (pp. 181-99) in *African Pygmies*. Quoting:

"The Pygmoid character of the Ethiopian Pygmoid series is shown by the presence of a rather high percentage of *GM* haplotypes with *c* variants (see van Loghem, Chap. 14, this volume). The most reasonable conclusion seems to be that the Ethiopian Pygmoids are a population originating from intermixing of a Pygmoid and a non-African component, the same non-African component that is present in other Ethiopian populations. The inconsistencies (e.g., lower *CDe* haplotype frequency than that which would agree with the higher FY^A and non-African *GM* haplotype frequencies) could be explained if it is assumed that the racial intermixing occurred long ago, and that since then, genetic drift or natural selection caused relatively sharp shifts in the gene frequencies. By analogy, we cannot exclude the possibility that other African Pygmy populations originated from racial mixtures of ancient Pygmoids and African non-Pygmy populations."

The last sentence seems completely uncalled for. How about this as an alternative ⇒ Pygmy ancestral populations arose through natural selection — probably mutations affecting growth hormones — in or near the vast tropical forests of central/west Africa and in earlier times the rain forests of highland Ethiopia? *HGHG* called this non-African component "Asian", a term with a very wide range of meanings. Given their geographical location and linguistic affiliation, it seems easier just to say that the Manjo have a Caucasoid component in their genes, like all the other Ethiopian populations whose blood has been tapped for biogenetic purposes.

However, van Loghem (p. 218, same volume) has another view. In his conclusion of "Immunoglobin Markers" he says:

"When the findings in the various populations are compared, it is clear that there was less admixture in the Pygmies investigated than in non-Pygmies, with the probable exception of the Ethiopians [i.e., Manjo — HF]. New rare Negroid alleles occur in several populations."

"Immunoglobulin markers can contribute greatly to our understanding of the relationship between populations and in tracing migration. The genetic makeup of the proto-African remains an open question. It is conceivable that the Ethiopian Pygmies, rather than the other tribes, are the representatives of the first human inhabitants of Africa and that selected groups migrated from here to different parts of the world, resulting in deviating gene distributions caused by selection and genetic drift."

Well, the Kafa would surely agree that the Manjo are ancient natives. However, if the Manjo are that important, then how come no one has gone back to draw some more of their blood — in 25 years?

Literature Cited

Cavalli-Sforza, Luigi Luca, ed. 1986. *African Pygmies*. Academic Press, Orlando, San Diego, New York, etc.

Cerulli, Enrico. 1951. *Studi etiopici 4. La lingua caffina.* (Including "Il linguaggio speciale dei cacciatore Mangio di bassa casta nel Caffa"). Rome.

Cerulli, Ernesta. 1956. *The Peoples of Southwestern Ethiopia and Its Borderlands*. International African Institute. Ethnographic Survey of Africa. Oxford.

Fleming, Harold C. 1984. "The Importance of Mao in Ethiopian History". In Sven Rubenson, ed. *Proceedings of the Seventh International Conference of Ethiopian Studies, Lund (Sweden)*, April 25-29, 1982. Pp.31-38.

Fleming, Harold C., Aklilu Yilma, Ayyalew Mitiku, Richard Hayward, Yukio Miyawaki, Pavel Mikeš, J.Michael Seelig. 1994. "Ongota or Birale: A moribund language of Gemu-Gofa (Ethiopia)". *Journal of Afroasiatic Languages* 3, No.3, 181-225. 1992-93. (But came out in 1994).

Steinberg, Arthur G. and C.E. Cook. 1981. *The Distribution of the Human Immunoglobulin Allotypes*. Oxford University Press, Oxford.

[We are honored by Igor Diakonoff, who has written a significant article for *Mother Tongue*. Perhaps influenced by his 80th birthday, Igor wished to see this article in *MT* while he was "on this side of eternity." We wish him a happy 81st year and more!

Hal particularly likes Igor's approach because their minds are so much alike — thinking in a multi-disciplinary way is truly fun! Here is the article.]

LONG-RANGE LINGUISTIC RELATIONS: CULTURAL TRANSMISSION OR CONSANGUINITY?

IGOR M. DIAKONOFF
St. Petersburg

The problem of the diffusion of languages, and the formation of families of related languages, has hitherto practically interested only linguists; what kind of historical processes were responsible for such diffusion, has not been sufficiently clarified. Meanwhile, in linguistic science, there has in recent years appeared numerous studies on so-called "long-range comparison": linguistic affinity is now being ascertained not only inside each individual family (like Semitic, Germanic, Indo-Iranian, etc.), but also between different but ultimately also related families, constituting linguistic superfamilies (like Indo-European or Afrasian — also called Afro-Asiatic or Semito-Hamitic) and between linguistic phyla. One of such phyla is Nostratic, first discovered by V.S. Illich-Svitych [1], and including at least the Indo-European superfamily, the Kartvelian (or Georgian) language group in the Caucasus (but not the other languages of the Caucasus and Transcaucasia) [2], the Afrasian superfamily, the Dravidian (Elamo-Dravidian?) language group, the Uralo-Altaic (including the Finno-Ugrian family), the Turko-Mongol, the Tunguso-Manchu, and the Korean linguistic groups — whether the status of family or superfamily should be attached to these groups is to be decided in the future; with less certainty, to the same Nostratic phylum may belong such languages as Japanese, Nivkh, Yukagir, the Chukchi-Kamchatkan family and, further, the Na-Dene linguistic family of North-Western America, and perhaps the Eskimo-Aleut family [3].

Other linguistic phyla beside the Nostratic, which can be ascertained at present, are:

The Sino-Caucasian phylum (thus according to S. Starostin and S. Nikolaev), including the languages: Adyghian, Abkhazian, Abaza, and the extinct languages Ubykh and probably Hattic [4] and Kaskian along the Black Sea coast of the Anatolian peninsula; the Alarodian family (including the languages of the Ingushs and Chechens, most of the Dagestani, the Hurrians and the Urartians, and perhaps the Qutians (the last supposition is based on archaeological evidence); the Yenisei, the Tibeto-Burman family, and Chinese [5]. It is characteristic that here, just as in the case of Nostratic, there seems to be no one-to-one correlation between the linguistic and the physical anthropological entities.

There follow:

The Niger-Congo phylum, including the Bantu (super?) family and some others;

The Nilo-Saharan phylum (?);

The Amerind phylum.

Unfinished is the classification of the languages of Southern Asia, Australia, Tasmania, and New Guinea, where the limits of phyla, superfamilies, and families have not yet been sufficiently clarified; the Malay-Polynesian group of languages, stretching from Hawaii to Madagascar, is certain enough, but its taxonomic rank is not clear. An Austric phylum has been postulated, or else an Austronesian plus some other not finally classified languages.

Moreover, on nearly all continents and in some island groups, there exist relic languages which may have survived from Paleolithic times. Such are the languages Burushaski in Kashmir, Basque in the Pyrenees and surrounding area and, in antiquity, Etruscan and Lemnian in the Mediterranean region, and Sumerian in Mesopotamia.

As a rule, all languages of the world can be subdivided into two main types as regards the character of their syntactic construction: (I) ergative languages, where the subject of action has a special marker, while the subject of a state and the object of an action (regarded as the subject of an appearing state) have one marker in common (usually a zero marker); and (II) nominative languages, where the subject of action and of state have a common marker (nominative), and the object of action has a different marker (accusative); here there often exist an active and a passive voice.

There also exist a few languages which have lost all markers for the cases of the noun (the genitive marker survives longest), but these are, as it seems, always descendants of nominative-type languages.

Most of the languages currently spoken use the nominative construction, but often it can be proved that the ergative construction has also here preceded the nominative one.

Among the Nostratic languages, the ergative construction seems to be preserved in languages which had lost contact with the hypothetical Nostratic proto-language at an extremely early date (Chukchi, Aleut, Kartvelian); isolated languages (Sumerian, Burushaski, Basque) tend to preserve it. The ergative construction is also attested in other linguistic groups.

Leaving other phyla aside, we are going to dwell on the Nostratic phylum, and mainly on that part of it which includes languages spoken in the Ancient Near East. The linguistic proofs for the existence of the Nostratic phylum seem sufficient, at least as regards the classification of the superfamilies Indo-European, Afrasian and, to my mind, also Elamo-Dravidian. However, there arises the problem of the historical identification of the speakers of the languages in question with any given physical anthropological and archeological entity, of their date, their origin, and its placement on the map. Here a number of factors should be taken into consideration:

First of all, there are the questions of the rationale for the present-day broad distribution of kindred languages, and of dating their movements. It would seem natural to connect unilaterally the distribution of languages with the movement of population groups speaking these languages. But in order to adopt this solution, one has to show that the linguistic facts proving the spread of languages coincide with the facts supplied by the physical anthropology of the populations in question; and one has to check the results by comparing both series of facts with the diffusion of archeological cultures. However, one must take into consideration that spontaneous changes in the type of archaeological objects occur not only due to ethnic changes among the bearers of any given araeological culture, but more often due to technical innovations and sometimes simply to a change in fashions (this is frequently the case of the forms and/or the ornamentation of pottery etc.). Furthermore, we must take into account the character of population growth — a factor especially important at the initial stage of ethnic diffusion; thus, it is known that the transition to an agricultural and cattle rearing economy, and the introduction of meat and milk food greatly diminishes children mortality and may lead to a hundredfold growth in population [6], and therefore, naturally, to an extension of the area inhabited by the population in question. Also, it is necessary to consider the changing climatic conditions, i.e., the paleo-climatology of the anthropogenic period of Earth's history.

It should be stated at once, that the assumption of a more or less close correlation between the distribution of related languages and the distribution of the physical anthropological features of the population in question, is unacceptable. Basic, for instance, for the Turkic subfamily of languages, is the important study by L.V. Oshanin [7], who, in his time, worked not with mitochondria and DNA, but with the easily observable genetic feature typical of the Mongoloid race — the epicanthic fold of the upper eyelid. He was able to demonstrate that inside the population using languages of the Turkic subfamily (and, to a considerable degree, speaking mutually intelligible dialects), the epicanthic fold can be observed in 50% of the male and 80% of the female individuals speaking Kirghiz (near the Chinese border), the percentage being considerably lower for the Uzbeks and quite low for the Turkmen (who, as shown by this author, are, anthropologically, descendants, in the main, of the Indo-European speaking Sacae or Scythians); about 2%-0% in the Azerbaijani Turks and approaching zero in the Turks of Asia Minor. Thus, the spread of a language does not coincide with the spread of a physical anthropological type and, accordingly, of a certain set of genes. The same picture can be easily observed, even without counting genes, in peoples belonging to the Indo-European linguistic (super)family, from blue-eyed tall blond Scandinavians to black-eyed and rather dark, moderately tall black-haired Indians. Or, to take another example, the Finno-Ugrian linguistic family includes Finns who are tall blonds, and Hungarians who are moderately tall brunets. There may be a lack of coincidence not only between the limits of distribution of one (say Indo-European) linguistic family, but even between the limits of the spread of a racial type, and any one branch of a linguistic family; thus, the Slavonic branch does include both the blond Russians of Karelia and the Arkhangelsk oblast, who are indistinguishable from their Finno-Ugrians neighbors the Karels and the Vepses, and the Bulgarians, who are brunets, are hardly distinguishable from their neighbors the Greeks; also any amount of intermediate types can be observed inside the Slavonic linguistic branch.

A historian operating with the notion of ethnicity should take into consideration the *ethnic selfconsciousness*, which may unite a population with a common historical past. As often as not, such a population is, at the present time, united by a common language, but this is not a universal rule. Thus, the population of the island of Ireland is almost completely English-speaking (the Celtic Irish is retained as a living language in a few villages on the Atlantic coast and partly, artificially, in the cities); however, the English-speaking Protestant population of Ulster, having a different historical experience, does not perceive any ethnic identity with the English-speaking Catholic population in other parts of the island. Norway has two related but different official languages, and a multitude of spoken dialects but a common ethnic self-consciousness. The Swiss are united by a common historical past but not by a common language; here there are no less than four languages with official status, one of them being the German literary language, while the spoken dialects of the "Germanophone" Swiss are on the verge of being unintelligible to the inhabitants of Austria and Germany. Austria has a historical past different from that of Germany, but the inhabitants of both have a language in common; note, however, that not only Austrians but also, e.g., the Bavarians are practically bilingual (as was only a short time ago also the case of the population of the Baltic coast of Germany); locally they converse in their own specific dialects which are not so easily understood by the rest of the Germans. On the other hand, nearly all of the speakers of Turkic dialects, except the most outlying, can make themselves mutually understood without great difficulty.

Thus the limits of the diffusion of a language — or a linguistic family descending from a proto-language — do not coincide with the limits of physical anthropological types. A good example is furnished by the studies of the aboriginal population of America presented by certain biogeneticists and linguists and published in *Mother Tongue*, 23 (1994), pp. 21-31 (D.A. Merriwether, R.L. Cann, and others): the geneticists find four distinct genetic population groups (not clearly contraposed but partly overlapping), where the linguists identify three families (Eskaleut, Na-Dene, and Amerind; note, however, that Amerind is certainly not a family but a phylum. There has been a suggestion to abolish Amerind as a singe taxon, and to substitute it by no less than 145 independent phyla. Of course, this means simply that one may not have found the necessary diagnostic features connecting the language groups, because one can hardly envisage the separate passage of 145 independent population groups across the Bering bridge in the short time when such a passage was possible). Note the situation with the Khoisan phylum: the linguists distinguish different branches or even families of Hottentot and Bushmen (Khoisan) languages, all totally unrelated to Bantu; also, the Bushmen differ quite strongly externally from the Bantu; but there appears to be no serological difference between Bushmen and Bantu.

It can be concluded that the diffusion of languages from a certain center cannot, as a general rule, be identified with anthropological migration; we may rather envisage the possibility of spreading of a proto-language, not uncommonly with the accompanying cultural and socio-psychological manifestations, to a physically new anthropological population.

We may define ethnic unity as a conscious unity of culture (as, e.g., in religion but not only in religion) *a n d* of language. Therefore, in order to get results suitable and comprehensible for the historian, one has to date the movements of the archaeological cultures, comparing them with the possibly different picture of the diffusion of languages, and the dates of the displacement of linguistic branches, families, superfamilies and phyla. We should also take into consideration the fact that all these movements are not necessarily connected with movements of the bearers of different genes, but may, inter alia, be connected with climatic changes.

A historian has to deal with ethnic units. Such a unit involves a clear apprehension by a certain population of the unity of their language *and* culture (as, e.g., religion but not religion alone). Therefore, in order to achieve historically meaningful conclusions, we have to date the changing of the limits of archaeological cultures, checking the resulting picture by comparing the diffusion of languages, and, accordingly, to date the movements of languages, linguistic branches, subfamilies, families, and superfamilies, as well as phyla. Culture is very strongly bound to language, much less to anthropological features. One must also take into consideration the fact that anthropological types move slowly in comparison with the movements of languages and cultures, and that the secular climatic changes strongly affect the whole process of ethnic mobility in all its different aspects.

Each linguistic branch, each subfamily, family, superfamily, or phylum, is bound to have had a proto-language. The proto-languages of orders as comprehensive as a superfamily or a phylum cannot be dated to a later period than the Late Paleolithic or Mesolithic, i.e., not later than ca. 10,000 BC. Any proto-language can be dated archaeologically and historically by analyzing the semantics of the lexical stock which can be identified as having already existed in this proto-language. Note that, for the earliest periods of the existence of mankind, one should postulate the existence of local linguistic means of communication that cannot always be identified with the proto-languages which we are able to reconstruct; some of them may be supposed to have disappeared, perhaps leaving traces, e.g., in the form of unidentified languages. Dating a proto-language is thus possible; it is far more difficult to reconstruct the biological history of mankind, and it cannot be stressed too emphatically that by reconstructing a proto-language we may get some notion of the proto-culture of the proto-population — but we cannot reconstruct the physical anthropology of the speakers of a proto-language except by inference and indirect evidence.

Mostly, a nation, whatever its language, is physically continuing the population originally inhabiting the country in question, except in rare cases when the population which had lived in that country had been totally ousted or killed off (as has been the case in the Caribbean and in parts of South and most of North America).

Above we have posited eight linguistic phyla (leaving aside a few isolated languages): Nostratic, Sino-Caucasian (?), Niger-Congo, Nilo-Saharan (?), Khoisan, Amerind, Australian (?), and Austric (?). At the same time, e.g., W.N. Boyd has suggested a subdivision of mankind into six physical anthropological races, according to gene frequencies: 1) Early Europoid, now preserved only in the nation of the Basques, who, however, do not appreciably differ from Mediterranean Europoids externally; 2) Europoid; 3) Negroid; 4) Mongoloid; 5) Amerind; 6) Australoid. We should like to add still another race, viz., 7) the Bushmen race. It is not included in Boyd's list, probably because serological data do not differentiate the Bushmen from the Bantu. But anyway, this biogenetic classification differs completely from the linguistic one. Language displacements tend to mix the races, this process beginning probably already as early as the Mesolithic/Late Paleolithic period.

These six or seven races are to be subdivided into subraces, as e.g., the Europoids can be subdivided into the tall and blond Atlantic Europoids, the moderately tall, black-haired and rather dark Indo-Mediterranean Europoids, and a number of others.

Below we shall specifically discuss the hypothetic picture of the diffusion of the Nostratic linguistic phylum:

The original scenario is to be dated to the period approximately between 10,000 and 7,000 BC — the period following the last Glacial period. We hypothesize that the ancestors of at least the western group of the Nostratics occupied, about this time, the "Fertile Crescent" in the Near East, mainly the inner slopes of the mountain ridges of the "Crescent" (bordering on three sides on the region of what was later to develop into a desert zone), and also the neighboring regions, especially the Asia Minor peninsula. It was inside this region that the oldest remains of what can be described as Early Modern Man have first been found (in the caves of Skhul in Palestine and Qafzeh in Jordan); this species ousted the Neanderthal man completely. Here is attested the first archaeological culture in which man succeeded in mastering the harvesting of wildly growing grain with primitive reaping knives, and perhaps started primitive agriculture (the Natufian culture, so called after Wadi an Natuf, a site near Jerusalem dated to the Xth-VIIIth millennia BC); it might have spread to the territories of Lebanon and Syria; anthropologically, the Natufians seem to have belonged to a type similar to the Indo-Mediterranean Europoids. About the time of the transition from the VIIIth to the VIIth millennium BC, primitive agriculture is attested on the site of Beidha in Jordan (emmer, *Triticum dicoccum*, and barley, *Hordeum spontaneum*, were cultivated). On the opposite side of the "Crescent", rearing of sheep is attested at Zawi Chemi Shanidar at about 9000 BC; in Deh Luran (about 7000 BC) the marginal use of sown barley and wheat is attested; in Jarmo (VIIth millennium BC) near modern Kirkuk in Iranian Kurdistan traces of agriculture have been found (wheat and barley; sickles with inserted sharp cutting stones), as well as of goat rearing, etc. At that same time, not inside the "Fertile Crescent" but still in the fertile zone, in Asia Minor, the highly developed culture of Çatal Hüyük is attested (ca. 6600-5600 BC), with real dwelling houses, and sanctuaries with wall paintings, with fairly developed agriculture and cattle rearing; primitive pottery and weaving appeared. The end of the Çatal Hüyük culture may be supposed to have been caused by the end of the warm pluvial "Atlantic" period in the history of the climate in the Mediterranean and adjoining regions and the coming of the cooler and drier Sub-Boreal period, which may have begun with a spell of drought. However, another site in Asia Minor, Hacilar, was also later (again?) occupied (from the late VIIIth to the VIIth, and from the second part of the VIth to the early Vth millennium); in the later period, the site was fortified. However, neither Hacilar, nor another important site, Çeyönü-tepe, seem to have been connected with the later Hittito-Luwian culture of Asia Minor. The early culture of Asia Minor may perhaps have been connected with the Balkans; the men of Çatal Hüyük may even have wandered over to the Balkans, at that period covered with lush vegetation. Agriculture was known here (as e.g., in Thessaly) from the beginning of the VIth millennium BC. The Karanovo culture (in Bulgaria) may be directly akin to Çatal Hüyük. — The later highly developed cultures of Asia Minor (in the IIIrd-IInd millennia BC) were due to immigrants, viz., the Hittito-Luwians probably arrived here from the Balkans — certainly not over the Caucasus as hypothesized by A. Kammenhuber [8] and some other scholars, because the archaeology of the Caucasus does not suggest that any such migration occurred here [9]. Professor Kammenhuber points out herself that the Hittito-Luwian language group was nearer to the Western than the Eastern Indo-European languages (I would suggest that it may be classed as not belonging to either of them but representing a separate branch — or separate family if Indo-European can be classed as a superfamily). The Balkans are, to my mind, to be regarded as the homeland of the Indo-Europeans; to this problem I shall return below.

In one of my former detailed publications [10], I had drawn the reader's attention to two facts: first, that the Semitic word for 'earth' means actually 'red', while white soil is characteristic of certain parts of Africa but not of South-West Asia, and that the names of wild animals are usually not identical in Semitic and in the African branches of the Afrasian superfamily; hence, I hypothesized that the origin of Semites was in Africa, from whence they had migrated to Asia; however, now I don't think these arguments are decisive. Already earlier [11] I had shown by linguistic means that the Proto-Cushitic is considerably older than Proto-Semitic.

In spite of my former objections, perhaps we should agree with A.Yu. Militarëv in connecting the Afrasian superfamily with the Natufian-Beidha culture in Palestine-Syria [12].

It seems hardly possible to locate the *w h o l e* Nostratic phylum inside the "Fertile Crescent"; its origins must predate the first agriculturalists. Unfortunately, we have hitherto little information on the Upper Paleolithic period outside of Europe. Hence, it is difficult to pinpoint the origin of the eastern part of the Nostratic linguistic phylum, but, if the hypothesis of Starostin and Nikolaev is to be accepted, there must have been contacts during the Upper Paleolithic period between the ancestors of the Indo-Europeans, the Afrasians, and the eastern groups of Nostratic (whether the origin of the phylum can be dated to the Upper Paleolithic can be verified by analyzing the word-stock identified as proto-Nostratic). It is possible that we should in this case introduce a taxon or "class" between "superfamily" and "phylum" to distinguish between Western and Eastern Nostratic (and also perhaps between Western and Eastern Sino-Caucasian); however, the problem of one proto-language which it is necessary to postulate for each phylum, a proto-language with a certain geographical placement, still remains.

Although my historical reconstruction is purely hypothetical, I suggest the following interpretation of our data: The Natufian-Beidha culture may be regarded as created by the ancestors of the Afrasian superfamily. Under the conditions of a comparatively favorable climate and unprecedented population growth, which would necessarily follow the introduction of agriculture and stock-rearing, most of the Afrasians moved southwards. Inside the "Fertile Crescent" and on the Arabian peninsula, the part of those Afrasians were left who constituted the Semitic linguistic family. In spite of some objections that have been made, it seems probable that, during the Neolithic epoch, water still ran in the numerous dry river beds (*wadi*) of Arabia, and the climate was much more favorable than at present.

Not counting the peoples speaking the languages of the Ethiopian branch of Semitic, having probably settled in Ethiopia not earlier than the VIIth century BC, and the Arabs who settled in Egypt and beyond together with the spread of Islam, nearly all of Northern Africa is now inhabited by peoples and tribes speaking other languages which all belong to the Afrasian superfamily and have come to Africa before the Ethiopians and the Arabs (here we list them geographically, not chronologically): the Beja, or Bedawye along the western coast of the Red Sea; tribes of the Cushitic and Omotic families, living in the southern part of Ethiopia, in Eritrea, Somalia, and the north-eastern part of Kenya; the Egyptians and their descendants the Copts, now completely Arabicized, who occupied the northern part of the Nile Valley; the tribes of the Berbero-Libyan linguistic family, which probably stand closest to the Semitic family, and who settled in present-day Libya, Tunis, Algeria, Morocco, Western Sahara, and the Canary Islands (the Guanche tribes, now absorbed by the Spaniards) [13], Niger and Mauretania (a considerable part of the population of these countries has been Arabicized); and the numerous peoples and tribes of the Chadic family (the most important of which are the Hausa), inhabiting mainly northern Nigeria. It goes without saying that although all these tribes and peoples speak languages that belong to the Afrasian superfamily, anthropologically and genetically they differ.

An important problem which needs solution is that of the routes by which the Afrasian languages moved from the region of the Natufian culture into Africa. It would seem that the most natural way would be up the valley of the Nile, towards Ethiopia southwards, and towards the Sahara westwards. But it seems that this solution is untenable: the humid period which lasted from ca. 5000 BC to the end of the IVth - early IIIrd millennium BC (as evidenced by rock carvings and paintings depicting animals requiring moist climate in Arabia and in Sahara — especially on the plateau of Tassili n-Ajjer in the center of the Sahara [14]) caused a swampy lake to appear in the lower part of the Nile valley; at the same time, the now desert land to the west of the valley was at that period uninhabitable. But it is quite possible that the territory to the east of the valley was climatically more similar to the conditions at Tassili, and here an ethnic movement southwards was possible, e.g., for the ancestors of the Bedauye and others; if the level of the ocean was low at some period, it was also possible to cross the Bab al-Mandab straits on rafts from South Arabia to Africa.

It should be noted that, inside the Afrasian superfamily, Semitic is nearer to Berbero-Libyan and Bedawye, while Egyptian seems nearer to Chadic [15]. Cushitic may not be a single family but several related ones, and its position relative to the others is uncertain (nearer to Semitic than to Chadic?).

Subsequent periods of drought (ca. 2200-1900, 1200-1000, 700-500 BC) created impassable desert zones between the different families of the Afrasian superfamily.

There was a wholesale racial mixture between the Afrasian tribes moving into Africa, and the local population. While the Egyptians and the Berbero-Libyans are most Europoid (of a southern sub-race), the Cushites and especially the tribes of the Chadic group emerged as Negroid.

I suggest that the tribes speaking languages of the Indo-European superfamily have had their origin in the Balkans and the Danubian region; it was there, so I think, that a demographic explosion took place which was conditioned by the adopting of milk and meat food, and the lowering of child mortality; from here the speakers of the Indo-European languages — divided into two, or, more probably, into three groups (the Western, the Hittito-Luwian, and the Eastern) — moved; the first northwestwards, absorbing the aboriginals with their Maglemose culture in Northern Europe; the second, back to Asia Minor to create the Hittito-Luwian culture; much later the Phrygians and the Armenians went this way; and the third was responsible for the Cucuteni-Tripolye culture (IVth-IIIrd millennia BC), where perhaps the Kartvelian members of the Nostratic phylum were the vanguard, creating first the Maikop culture north of the Caucasus, and then mixing with the aboriginal population of Transcaucasia, preserving a large percentage of Nostratic word-stock but adopting the Caucasian grammatical structure and a number of local words from the Western Caucasian and the Alarodian (Chechen-Daghestani-Hurro-Urartian) language families; these, according to S.A. Starostin, belonged not to the Nostratic but to the Sino-Caucasian phylum [14]. The wave pushing eastwards and following the Kartvelians from west to east until they disappeared behind the Caucasus, consisted of the speakers of Indo-Iranian languages (perhaps with a Slavo-Baltic group branching up the Dnieper); according to archaeological, historico-cultural and linguistic data the proto-Indo-Iranians can be traced to later Indo-Iranian speakers — the Scythians, Sacae, and the Sarmatians, as well as later Iranians and Indians; they inhabited the Ukraine and Southern Russia, and later Middle Asia and Iran, entering Northern India in the first part of the IInd millennium BC.

It remains unexplained why such families as the Turko-Mongol or the Turkic and the Mongol, now situated in the center of Asia, and their neighbors in North-Eastern Asia and North-Western America, should be a part of the Nostratic phylum. The separation of these (super?) families can hardly be dated later than the last Glacial period; the date and the historico-cultural implications may be found through a semantic analysis of the words that go back to the period before the division of the Nostratic into different (super?) families. This is an urgent problem for the linguists specializing in these language groups. Unfortunately, the existing dictionaries are not exhaustive, and proto-forms are not indicated. There is much that scholars failed to record before it was too late, and little has been done by way of glottochronological and semantic analysis of the vocabularies.

The task for the linguists and the historians is to find out about cultural transmissions; the physical anthropologist and the geneticists should find out all about consanguinities.

NOTES

1. V.M. Illich-Svitych, 1971. *Opyt sravneniya nostraticheskikh yazykov.* [An Essay in Comparison of the Nostratic Languages], vol. I, Moscow.

2. Kartvelian seems to be a result of ethnic unification of a native Caucasian and an originally extraneous Nostratic element. An "Ibero-Caucasian" linguistic family as postulated by A.S. Chikobava, does not exist.

3. The Na-Dene linguistic family apparently belongs, in America, to the second wave of migration over the Bering straits across the Beringian land bridge which, according to Rebecca L. Cann (*Mother Tongue* 23, November 1994), was closed for ~6000 years during the late Wisconsin Glacial (which lasted between ca. 21,000-8000 BC), but the straits could also be passed over winter ice before 65,000 BC and after 13,000 BC. The latest date should be assigned to the crossing of the Eskimo + Aleut, while the Na-Dene would have crossed at a somewhat earlier period; there is also a disagreement on whether there was only one passage from Asia to America, and then at an early date, or whether there were three or even four passages. Anyway, the passage of the Na-Dene into America (say, about 13,000 BC) would be the *terminus ante quem* for the existence of a Nostratic family in North-Eastern Asia.

4. I.M. Dunayevskaya, 1964. *O strukturnom skhodstve khattskogo yazyka s yazykami severo-zapadnogo Kavkaza.* [On the Structural Similarity of Hattic and the Languages of NW Caucasus], "Issledovaniya po istorii kul'tury narodov Vostoka", Moscow-Leningrad, pp. 71-77.

5. Note, however, that some authoritative Sinologists deny the Sino-Caucasian unity; however, there is an archaeological link between ancient Daghestan to the West of the Caspian and the regions east of the Caspian, cf. K.Kh. Kushnareva, 1994. *Kavkaz i Zakavkaz'ye v IX-II tysyacheletiyakh do n.e,* [Caucasus and Transcaucasia in the IXth -IInd millennia BC]. StPb, p. 16.

6. Kushnareva, op. cit., p. 19.

7. L.V. Oshanin, 1957-1959. *Antropologichekiy sostav naseleniya Sredney Azii i ethnogenez yego narodov*, vols. I-III, Yerevan.

8. A. Kammenhuber, 1993. *Kleine Schriften zum Altanatolischen und Indogermanischen*, 1. Teilband, Heidelberg, p. 166.

9. Cf. Kushnareva, op. cit., pp. 51 sqq.

10. I.M. Diakonoff, 1981. *Earliest Semites in Asia. Agriculture and Animal Husbandry According to Linguistic Data (VIIIth-IVth Millennia BC)* Altorientalische Forschungen VIII, Schriften zur Geschichte und Kultur des Alten Orients, Adademie Verlag, Berlin, pp. 23-74.

11. I.M. Diakonoff, 1975. *Lingvisticheskiye dannyye k istorii drevneyshikh nositeley Afraziyskikh yazykov*. [Linguistic Data for the History of the Earliest Speakers of Afrasian Languages], Africana, Afrikanskiy etnograficheskiy sbornik, Nauka, Leningrad, pp. 118-130.

12. I.M. Diakonoff, 1989. *Yazykovyye kontakty na Kavkaze i Blizhnem Vostoke* [Linguistic Contacts in the Caucasus, and the Civilizations of the Ancient Orient] in: Kavkaz i tsivilizatsii Drevnego Vostoka, Ordzhonikidze, 15 sqq.

13. I.M. Diakonoff, 1988. *Afrasian Languages*, Moscow, Nauka. Pp. 21-22.

14. I.M. Diakonoff, S.A. Starostin, 1986. *Hurro-Urartean as an Eastern Caucasian Language*, Kissinger, München. S.A. Starostin, *On the Yeniseian and North Caucasian Languages*; S. Starostin, S. Nikolaev, Sino-Caucasian Languages in America — Preliminary Report, both in: V. Shevoroshkin (ed.), 1991. *Dene-Sino-Caucasian Languages*, Bochum. Cf also: S.A. Starostin, 1991. *Altayskaya problema i proiskhozhdeniye yaponskogo yazyka* [The Altaic Problem and the Origin of the Japanese Language], Moscow, Nauka.

[Editor's final note: Professor Diakonoff's text has been lightly edited. It has not, however, been reviewed by peers nor commented on in letters to him. The time needed to communicate by letter with Russia is nowadays long, and the receipt of mail is uncertain. Furthermore, the e-mail option could not be used because of time pressures for publication. The article was desired for this issue. For long-rangers eager to discuss the paper with the author, Diakonoff's e-mail address is: diakonov@lnpi.spb.su (we don't guarantee that the spacings are correct. So if you get no results, vary the spacing between segments.) — HF]

STATISTICS AND HISTORICAL LINGUISTICS: SOME COMMENTS

SHEILA EMBLETON
York University, Toronto

[Editor's foreword. Sheila was a discussant of a series of papers given at the Linguistic Society of America's meetings in New Orleans, January, 1995. It was entitled: "A Symposium Against Multilateral Comparisons". Sheila's remarks followed 30-minute papers by Lionel Bender ("An African test case in comparative methodology"), William Poser ("The mathematics of multilateral comparison"), Donald Ringe ("Testing a basic evaluation metric"), and Johanna Nichols ("Multilateral comparison and linguistic geography"), as well as commentary by Alan Kaye (read by Bender) and William Baxter. By agreement with Sheila, we will not publish the first three pages of her remarks because we have not shown the original papers, and so it would be unfair to comment on unseen papers which might be revised by the authors if publication was imminent. Since Sheila is known to be a member of ASLIP, but a friend to all parties in "the dispute", she might be described as a "moderate" if it were politics, neither a lumper nor a splitter. In her own terms (page 1) she is neither an Amerindianist nor a Nostraticist, "but a specialist in mathematical, more specifically statistical and probabilistic, techniques and modelling in linguistics, specifically in historical linguistics, dialectology, and stylistics".]

(Hereinafter quoting from Sheila's remarks. Beginning with her page 4.)

Now the more general comments

I would like to make a few remarks about the utility of statistical tests, and perhaps mathematical or probabilistic techniques in general, for linguistics. Not because I think that linguists are naive about statistics — some are and some aren't — or because I think that the use of statistics will cause anyone to be convinced of anything, or to switch sides in any debate, such as the current one, but because often doing statistical tests properly forces you to make explicit, and maybe even re-examine, some of your assumptions, makes you change/re-consider assumptions, see some things in new perspectives. Some examples:

- You are forced to consider seriously the size of your sample, and whether it's adequate. Also what to do about missing data points, or data that is hard to categorize in some way (e.g., languages where it's hard to determine a basic word order type).

- You are forced to consider whether the features you are examining are statistically independent, something required for most statistical tests to be applicable/valid (e.g., chi-square).

- You are forced to lay out *all* the assumptions underlying your test, and (as mentioned earlier) to realize that a "significant" result can often simply mean an invalid assumption has been made, not that the data are significant in some way.

- Checking the significance of inter-group differences forces you to explicitly look at intra-group differences (you may see new things!), to make sure that inter-group differences really are bigger than the intra-group differences.

- It forces you to consider that there are, after all, TWO types of error that you can make with regard to the null hypothesis — that statisticians refer to as Type I and Type II errors. Essentially that rejecting a true hypothesis (Type I error) and accepting a false hypothesis (Type II error) are both errors. This forces you to think carefully about which is actually the greater/worse error — or at the very least to realize that both *are* errors. In short, it clarifies your thinking. I think this particular fact, that there are TWO types of error, is not adequately addressed in linguistics in general, nor specifically in the consideration of long distance relationships. We seem to spend all our time worrying about one type of error, Type II error — phrased differently, we are being very careful not to accept the null hypothesis when it's false, and seem to worry less about rejecting a null hypothesis that might be true. Statisticians would take this as a sign of conservatism, by the way. Usually, such types of conservatism are considered reasonable by statisticians only in some contexts, where the "cost" of an error is very high — e.g., studies of the *safety* of a new medicine (as opposed to studies of the *effectiveness* of a new medicine). In other words, when a certain type of error is devastating and must be avoided at all costs. I'm not saying that we shouldn't be conservative — just that we should be aware of what we are doing, and of the other type of error. And I'm not going to go into the whole issue of decision-theory, and how one can try to optimize the expected gains and losses vis-à-vis both types of error.

So, quite apart from what the statistics may (or may not) tell you/others, it's good for a general re-examination of assumptions, goals, conclusions, new perspective in general.

People often ask me what WOULD constitute statistical proof, or beg me to set up some formula for them which would, e.g., calculate the probability of chance resemblances in these etymologies/long-distance look-alikes. These issues have already been touched upon by Ringe, but I'll touch on them briefly again. Just think of all that has to go into such a formula, to even come close to approximating reality — and remember, if you don't put in all these factors, people will criticize you for it, claim your formula is invalid because it doesn't include whatever, and therefore disregard your conclusions, particularly if they don't fit into THEIR views. So, you have to have not just the phoneme inventory, but also information on the phoneme frequencies, their phonotactic restriction/cooccurrence restrictions on their distributions, should probably take account of factors related to persistence/universality, maybe factors related to acquisition, maybe the TYPE of morpheme it's in, and so on. And that's just the phonology. What about when you get to the meaning side of the whole thing? There simply is not the theoretical apparatus or even plain practical knowledge here that there is in phonology. Lexicostatistics/glottochronology handled this semantic problem by allowing no latitude whatsoever — which is of course easy to criticize, for all sorts of reasons — and it certainly *was* criticized! — but the most obvious is the sort of thing like missing English *hound* as being cognate to German *Hund*, because you insist on only considering *dog* in English. But as soon as you allow some latitude, NOBODY is going to agree on just how much latitude, and you've got yourself a whole new can of worms, and a whole new set of reasons as to why people won't accept your method, if they don't like your method, if they don't like what your method concludes. [I could add parenthetically that semantics is the trickiest part even when using "traditional" methods, even, e.g., in very traditional approaches to reconstruction or etymology.] In any case, such formulas quickly become hopelessly complex — and then they criticize Don for not tackling more than TWO languages! And another thing you quickly learn when you work in statistics/mathematical methods — whatever formula is eventually constructed, most especially if it looks complicated, will be in general met with one of two reactions. People may be very impressed and immediately convinced by whatever you have to say, but much more likely — it won't convince people at all. Any reasonable formula, with any pretense of accuracy, will be so complicated that skeptics will take it as hocus pocus, obfuscation from "the other side". — Mary Clayton, 1993, *Language* 69:604, in another context (review of an NWAV volume): "[S]tatistical methods can be seductive. They always produce an answer, leaving even the naive or dull of mind with a feeling of accomplishment. Whether that answer is valid, important, or relevant lies in the skill of the linguist — not only in one's prowess in manipulating numbers, but even more in the knowledge, insight, and imagination that one brings to the initial formulation of questions and to the interpretation of the resulting data."

Or perhaps, in the words of Henry Clay (US statesman and orator, 1777-1852), "Statistics are no substitute for judgment."

Of course, after hearing all this from me, you will probably wonder why it is that *I* persist in doing mathematical methods! Am I particularly stubborn, or thick-skinned? I'm sure there are those that would accuse me of that, but I go back to my first reason, the one that I offered you earlier as the reason for doing statistical tests at all — the fact that it sharpens up your assumptions, goals, etc., and your general approach to the problem at hand. And I would say that our papers here tonight, whatever else you might think of them in general or in detail, have at least done that admirably. And I will come back to another use of statistical methods later ...

Another point — one which is relevant to any statistical application in linguistics, but has special relevance to some of our topics tonight. It's hardly a new idea, and you would find it in any basic statistics course, but people sometimes lose sight of it, maybe just in the heat of the argument, so I'll say it again here. Suppose you have:

A is related to A' with .99 probability

B is related to B' with .99 probability

C is related to C' with .99 probability, and so on.

Each of these individually looks pretty secure at 99% certainty. But note that the probability that A is related to A' AND B is related to B' is .99 x .99 = .9801. And the probability that A is related to A' AND B is related to B' AND C is related to C' is .99 x .99 x .99 = .9703, etc. With 4 such relationships, the probability of all 4 being correct is .9606, with 5 .951 and so on, with 11, .8953. So the important point, in our context tonight, is that as you increase the size of the number of hypotheses, you get two things — both the additional weight/security of a "package" of hypotheses, but concomitantly an increase in the chance of any one item (or even more than one item) being wrong. It is absolutely important to note that you don't know WHICH one (or more than one) item without a *careful painstaking* examination of the data.

To speak now just briefly about one point of more specific relevance to diachronic linguistics and to the reconstruction of trees/relationships . . . Languages change over time — no matter what one wants to say about rates of changes or types of change, they CHANGE, as an undeniable fact. There is a progressive loss of the data that we depend on for reconstruction. So at large time depths, it is absolutely inevitable that one is going to have to be dealing with residues (of the pre-existing similarity) that are so small the "chance" and "borrowing" and "universals" and any other "non-genetic" factors that anybody can think of are going to loom large, be very important — possibly even dominant. We have seen tonight some important attempts to overcome this problem (e.g., Nichols), but no matter what, it still comes down to making the best of what is in effect a bad situation, trying to detect the signal amongst the considerable noise, by using the most sophisticated techniques available to us. Or as Ringe put it — "Reality is intractable. Get used to it."

To go back for a moment to another use for statistical methods ... They can be good, especially in huge uncharted fields with a wealth of data, for hypothesis generation and/or hypothesis testing. As a general claim, made by many others, I would endorse that for mathematical methods in general in linguistics, whether it be historical linguistics, dialectology, stylostatistics, etc. They are good for generating *PROVISIONAL* hypotheses, *PENDING* the results of full-scale painstaking investigations by traditional and more detailed methods. They are NOT a quick and easy short-cut to a FINAL result. Thus, whatever you may think of Multilateral Comparison, it can only produce provisional results, in my view. Bender already said this, by saying it's "not really a method of doing genetic language classification", "it is a pre-theoretical step preceding Comparative/Historical ... Reconstruction, which is the real method". If our mathematical methods are good, those interim results should be good, and may eventually be shown to agree with the final consensus (if there ever is such a thing). But if they're bad, the interim results will probably also be bad (unless of course your data are so robust, the trends are so strong, that no matter what lousy method you use, the results will come out right anyway). [There were allusions to this in Bender's comments on Greenberg's Nilo-Saharan work, which as I've said before, I'm in no position to judge. And actually also in Nichol's comment at one point "Good evidence, in short, is very robust".] To return to the production of interim results and hypothesis generation ... What is the harm in this? Isn't hypothesis generation always good? Can there be harm in this? Well, yes, there *can* be harm. Bad interim results can end up diverting a lot of research time that might have been better spent in other pursuits, not barking up the wrong tree. And another way in which interim results can be harmful is that it may prematurely generate a "received" view — which then means that "the truth" will have an even harder time getting itself established., because it will first have to combat this false "received" view. And I would like to emphasize that, from this point of view, statistical methods and the conclusions reached by statistical methods are no different from any others in linguistics — methods are always open to improvement, and conclusions reached by any method, statistical or not, are ALWAYS provisional, subject to revision in the light of further evidence.

Another, perhaps more philosophical point — statistics and probability will probably never outright convince anybody anyway. (Why else would people still buy lottery tickets?) Suppose I tell you that language A is 95% certain to be related to language B. That might be good enough for some of you, perhaps many of you. Anything that's 95% likely can probably be shown without statistics anyway, by the way . . . But those of you who for whatever reason don't *want* language A to be related to language B, or maybe are just very conservative by nature, will point to the 5% probability that I am wrong, and prefer not to accept the relationship. And supposing I move the cut-off to 99% or even 99.9% — it's not likely to have any real effect anyway, on those who for whatever reason, valid or invalid, don't want to be convinced. Statistics is unlikely to ever be able to PROVE anything in the real sense (the sense of the rest of mathematics!) of PROVE — which would require 100%. So what do we mean by "establishing proof", anyway? "Beyond a reasonable doubt"? But then we are back to fighting over just how much doubt can be allowed, or 5% vs. 1% vs. .1%, etc. Maybe instead of chasing elusive proofs we should instead look towards establishing the hypothesis which, at this particular point in our investigations and our state of knowledge in general, most fully satisfies as many criteria as possible. Maybe we should start using phrases such as "count as evidence for" and "count as evidence against" rather than the *absolute* terms "prove" and "disprove". Or, looked at another way, maybe we need a third possibility, besides decreeing languages either "related" or "not related" — we need a category for "possible, or promising, but not proven". Compare the Scottish legal system, which allows verdicts of "guilty", "not proven", and "not guilty" — so with "not proven" in addition to the more familiar "guilty" and "not guilty". We would need something similar for more distant comparisons — at the moment, we have to say either "yes" or "no", and don't seem to allow ourselves to say "maybe", or "this looks promising, and bears further investigating." Compare also Raimo Anttila's analogy to medicine — doctors don't just attempt to treat patients who are (almost certain) to recover. Similarly, our methodology should be applicable to any problem, and should produce some sort of prognosis, not just be able to deal with cases that are more certainly related or most certainly unrelated.

I would just like to conclude with a plea for open minds — that open discussion from both sides should ideally continue until a consensus is reached.

[Editor's note: this is not the end of Sheila's article.]

Useful information and quotes, in case any need for this arises in the discussion.

- Justeson & Stephens (1980) give distributions for statistical assessment of apparent resemblances (multiple phonetic and semantic resemblances). Still doesn't/can't adequately treat phonological side (inadequate on inventories, frequencies, let alone phonotactic/positional questions), let along semantic side.

  "As the criteria for phonetic resemblance are weakened it becomes more likely that a single form on one list will resemble more than one on another. This obviously increases the probability of getting a chance cognate for that item, and the expected number of chance cognates rises accordingly. The same argument holds as criteria for semantic agreement are relaxed. In both cases, multiple resemblances alter the combinatorial model" (42). "Thus we can expect the number of chance cognates to increase approximately in proportion to the average size of the similarity sets" (43). "[this paper] quantifies the dramatic decrease in the likelihood of chance cognation under mass comparison and its rapid increase when criteria for phonetic or semantic similarity are weakened to the point that many items on one list are similar to more than one on another" (45). [Editor's note: we presume the numbers are for pages].

- David Sankof 1973 (in Sebeok, p. 95): "Swadesh himself repeatedly indicated that he considered these methods additions, not replacements, with regard to other methods of historical linguistics, and that interpretation of a particular case should always use all lines of evidence available".

- Starostin's 35-word list, due to Yakontov (according to Laurent Sagart and Bill Baxter), has the following meanings: [Editor's note: we have not shown the quotation marks of the original] blood, bone, die, ear, egg, fire, fish, full, give, hand, horn, I, know, louse, moon, name, new, nose, one, salt, stone, sun, tail, this, thou, tongue, tooth, two, water, what, who, wind, year. He claims that there are statistical limits to borrowing within basic vocabulary, and that, therefore, genetic relationships can be deduced from basic vocabulary retention. Dolgopolsky was using 15 (see Shevoroshkin & Markey 1986), in order of decreasing stability, based on 140 languages: 1st person marker, two, 2nd person marker, who/what, tongue, name, eye, heart, tooth, verbal negative, finger/toe nail, louse, tear [noun], water, dead. Dryer used 20, at least in 1987: I, you sg., who, two, three, not, arm, hand, eye, ear, tooth, blood, brother, sun, moon, night, water, die, drink, see.

- Comparative method in syntax.

- No syntactic analogue to the regularity of sound change. Some sentences are actually stored, e.g., proverbs and idioms, and these often show syntactic archaisms. Also, earlier syntax often survives in fossilized form in later morphology, we have another rich source of data for diachronic syntax.

- Problems with non-independence of "features". Also, how many features is, e.g., SVO — relative order of subject and verb; relative order of verb and object; relative order of subject and object where necessary to disambiguate.

- Lack of "tertium comparationis" ("basis for comparison"). Cf. phonology, where you can compare Greek *pater*, *pod-*, with English *father*, *foot*, because these pairs have the same MEANING. Since "the sign is arbitrary", it's unlikely the *p-f* correspondence in initial position could be due to chance. Basic word order — only 6 possibilities. Sharing a rare syntactic trait (e.g., postposed articles in Romanian, Bulgarian, Scandinavian) no proof of genetic relationship.

- Relative stability of different parts of the grammar. Jacques Guy says "From my experience with languages of Vanuatu, morphological paradigms are the LEAST stable features, followed by phonology, then, most stable, lexical.

- Sally Thomason: "Structures do get borrowed, sometimes. So, for instance, there is general agreement that the Tanzanian language Ma'a (also called Mbugu) was not originally a Bantu language ... dramatically mixed structure ... it has few structural features that are clearly of Cushitic origin, and it has an entire inflectional morphology (as well as other features) adopted wholesale from Bantu languages ... One of these features is the irregular negative + 1sg prefix, which (as in some Bantu languages) contrasts with other members of the negative paradigm, which have separate negative and person/number prefixes. This is just the same type of feature that Teeter cites as obvious evidence of the relationship between (say) German & Latin." cf. Nichol's "individual-identifying evidence". [Editor's note: According to Sheila, Thomason's statement was made on an electronic bulletin board called LINGUIST on Dec. 14, 1994, vol. 5, no. 1448. That bulletin board seems to resemble a conservative talk show, as on political radio.]

BIBLIOGRAPHY from Embleton's remarks

Anttila, Raimo & Sheila Embleton. 1988. Review of Vitalij V. Shevoroshkin & T.L. Markey, eds. *Typology, Relationship and Time*, 1986. *Canadian Journal of Linguistics* 33:79-89.

Hamp, Eric P. 1992. "On Misusing Similarity". *Explanation in Historical Linguistics*, ed. by Garry W. Davis & Gregory K. Iverson. Amsterdam & Philadelphia: John Benjamins. Pp. 95-103

Justeson, John S. & Laurence D. Stephens. 1980. "Chance Cognation: A Probabilistic model and decision procedure for historical inference". *Proceedings of the Fourth International Conference on Historical Linguistics*, ed. by Elizabeth Closs Traugott, Rebecca Labrum & Susan Shepherd. Amsterdam & Philadelphia: John Benjamins. Pp. 37-46 (?).

Nichols, Johanna. 1992. *Linguistic Diversity in Space and Time.* Chicago & London: University of Chicago Press.

Ruhlen, Merritt. 1994. *On the Origin of Languages: Studies in linguistic taxonomy*. Stanford: Stanford University Press.

Salmons, Joe. 1992. "A Look at the Data for a Global Etymology: **tik* 'finger'." *Explanation in Historical Linguistics* (above).

A FEW REMARKS ON EMBLETON'S COMMENTS

HAL FLEMING

I hate to criticize people I really like. But "this is business", as they say, and we are honest, open people. In the same spirit as comments about Cavalli-Sforza's book, I offer these remarks. Since Sheila is one of the nicest and most fair-minded humans I've ever met, I must try to match her in those qualities. At fair-minded at least, I have a chance. The most important part of being fair-minded here is to state loudly that Sheila did not prepare her remarks as a paper submitted to *Mother Tongue*. Her remarks were addressed to a specific group of linguists at a specific time and place. My criticisms are only about those remarks. Sheila is free to rebut anything and everything I criticize. The pages of either our Journal or our Newsletter are open to her, if she chooses to use them.

When Bender set up his symposium in New Orleans, he managed (perhaps by accident) to get a set of participants none of whom could accurately be called a long ranger. The symposium was loaded for negative comments of a conservative nature. This conclusion I derive from the list of participants with the possible exception of Wm. Baxter and from Sheila's own remarks. Since Bender did not invite me to the symposium, telling a mutual friend that I wouldn't be interested, I can vouch for some of the selectivity at least. As the papers by Bender, Poser, Ringe, and Nichols would not be expected to be documents of flaming liberalism or radical rebellion, one can appreciate the bias built into the symposium.

I had expected Sheila to take a position midway between the two camps in the dispute, as she called it, and I was disappointed in my good friend. In her statistical statements, in her own specialty, she was more even-handed, but when she made statements that were basically linguistic judgments, not really statistical matters, she came out — a virtual conservative. If there truly were a field between two camps, she was not in the middle of it. However, if there had been just one long ranger style paper in the lot, or some in the audience during the question period, I am sure she would have been more even-handed because all of her remarks were per force reactions to strongly conservative papers. She was moderate in relation to their positions.

Presuming that people have read her remarks, let me ask the questions I might have asked in New Orleans, had I been invited:

1. How are we defining "long distance" (our long range) comparisons? Nobody challenges anyone on that subject. It seems that a long range comparison to an Indo-Europeanist would be X, where X = 5 or 6 kya or the most commonly accepted dates for Proto-Indo-European. However, a long range comparison to an Afrasianist would be Y, where Y = 10-15 kya. But a long range comparison to an Australianist would be Z, where Z = 20-40 kya. So what does a long range comparison mean? X or Y or Z ? Does that matter? You better believe it!

2. Are you discussing taxonomy or reconstruction? With all the emphasis on "painstaking" examinations of the data, is it reconstruction you are doing or are you trying to classify languages? Do you think they are the same thing? Well, have you ever read *Mother Tongue*? We've been discussing that difference for at least six years.

3. It seems to be the case that mass comparison (multilateral comparison) will produce *provisional* hypotheses, as you say on page 4 (your #7). That was close to the method used by the inventors of Indo-European. Is it still provisional or has that paragon of linguistic sophistication been finally proven? Is it 99% certain? Or only 85%? Or even 62% ? And how do you know? And why does it matter anyway? Is anybody challenging it?

4. The two types of error are interesting and her points are well taken. One question seems directed at her LSA audience (page 2) "I'm not saying that we shouldn't be conservative . . ." I wonder, do you think it is good time to be conservative when there is great excitement in the scientific air about human origins?

5. In your discussion of probabilities (page 3), where the chances of a single item being wrong *increases* with the number of "hypotheses", why didn't you give more attention to the "weight/security" derived from a "package" of them? Actually, I am not sure what this particular discussion is about, so I assume it is about a word in language A relating to a word in language A', etc. This relates to the next question.

6. Why do you ignore Greenberg's calculations of increased retrieval of cognates (really cognate) when more languages are used? Since I have mentioned these to you before, perhaps you have forgotten them? But they seem (to a mathematical innumerate like me) to be highly encouraging, so that in comparing *20 languages* after 20,000 years of separation one can still retrieve *20%* of the original vocabulary or 44 words on a 200-word list. Do you think Greenberg's calculations are wrong? Do you not think this point is quite important? This relates crucially to the next point.

7. Not a question but a comment. Starting in lower page 3, you have a section on CHANGE, culminating in a quote from Ringe worthy of Newt Gringe:

   "Reality is intractable. Get used to it." Remarkably enough, many of us have known that languages change and we have known it for some time now. Even from the time of the Greek philosopher, Heraclitus, we have known that things change. Perhaps the most defining characteristic of a conservative is that s/he wishes for things *not* to change. Or to quit changing so much. But I find this section vague, oddly enough. Great time depths are undefined. Changes in some things do take a lot longer than changes in other things. The goal is said to be reconstruction, not taxonomy, and they are not the same things. The attitude is pure Indo-European and hardly mathematical at all. Gringe's comment is very much au courant. "Get used to it", meaning get used to our policies, is much used nowadays by those taking away welfare benefits from poor women and children.

   In the debate between the two camps, who has ever said that nothing changes? So the real point of this section is to reinforce the — I hate to use this word again — conservative view that languages change so much that after a while you cannot find enough evidence to get deeper relationships or reconstruct more ancient ancestors. It is about time you guys stopped claiming this attitude as a "reality". It is not a reality. It is the product of certain assumptions about change. And those assumptions are *not* well founded, *not* well based *empirically*. A group of theoreticians sit around on their butts, imagining what results they would ever get if they tried to relate all the languages of the world and, overwhelming each other with their ideas, conclude that it is impossible. What would have happened if they had really tried to look for the evidence? And how do they know that their theoretical assumptions are true when they are untested?

8. I suspect that Nostratic theory — either Muscovite, Bomhardian, or Greenbergian — and the evidence mounting up in support of it will prove to be a major falsification of the premises just discussed in (7) above. Maybe it has already done so. Starostin-Nikolaev's Dene-Caucasic is not as well established and may need some alterations. But there is much agreement among long rangers that Dene-Caucasic is *older* than Nostratic, hence should prove harder to flesh out. But, if we can dispense with the altogether inadequate notions of the ages of the two super-phyla and just look at them globally, we can see that they take us a long way back towards the mother tongue, perhaps a third or more of the distance in time. Never mind Borean which, as it includes Amerind and Dene-Caucasic too, probably takes us almost *halfway* back to La Mama. Again forgetting about Borean, will those who already believe that Nostratic is related to Amerind consider that their common ancestor is very likely to be in excess of 25,000 years — an archeologically-based judgment — or a quarter of the way back?

9. Are we to conclude that we must either "reconstruct" all the way back to proto-human or we are stuck with the trivial Indo-European time depth? Either all or nearly nothing? Invoking the Scottish custom of a type in between "guilty" and "not guilty", may we not suppose that we can make it halfway back to proto-human? Some would be unsatisfied with that result, but what an accomplishment it would be for our science! Don't statisticians have categories midway between all and nothing? Consider that two linguists in good standing — Colarusso and Pulleyblank — have proposed hypotheses that connect two parts of Dene-Caucasic to the central part of Nostratic, although both were looking binaristically at only Indo-European and one other family. Isn't that encouraging at the Borean level? Doing all of Eurasia, save the tropics, plus the Americas — doesn't that figure to be about halfway back?

10. On a more picayune level Bender's remarks — that multilateral comparison is really not a method of doing genetic language classification but only a pre-theoretical step preceding the real method (the comparative method) — are ludicrous and will amaze any Africanist who thinks about them. A harsh thing to say? Yes, it is, but look at the facts. Bender has operated for about 30 years in African historical linguistics, happily using the four phyla established by Greenberg's "pre-theoretical" method of mass comparison. None of the four phyla are well reconstructed in the purist terms he advocates, but they are much better off than they were 30 years ago. In taxonomic terms, they have been altered in places, especially Kordofanian and Omotic, but not basically, yet are now pretty solid. How can four phyla be pretty solid, in the judgments of most Africanists, but not be terribly well reconstructed? That may be an intellectual quandry for an Indo-European theorist but it is not particularly for an Africanist. Taxonomy came first, it comes first, albeit provisional, and it gets better as people work on it. Reconstruction comes later and is not crucial to the taxonomy. Bender, for reasons all his own, has chosen to throw away his birth right, so to speak, for a mess of theoretical potage. Does anyone know what a "pre-theoretical step" means? Multilateral comparison leads to taxonomic hypotheses. What is pre-theoretical about generating hypotheses?

11. The famous case of Mbugu (Ma'a) arose again. It does indeed show what Sally Thomason is quoted as saying. A language can borrow almost an entire alien morphological system, while still keeping the greater part of its native vocabulary. But Bantu grammaticalization probably did reduce the phonology of Ma'a to conform more with Bantu phonology — loss of two pharyngeals, several glottalized consonants, and two lateral affricates seems indicated. I argued this case in 1969 but published it

in the wrong journal. Few people noticed it, except Floyd Lounsbury. However, as with any coin that has two equally valuable sides, Ma'a has recently been declared to be a Bantu language which has happened to borrow a few Cushitic words. So the entire basic vocabulary, pronouns, most body part terms, and numbers *can* be borrowed, while the grammar is virtually untouched? One way or another Mbugu is going to hurt somebody's theory! (I am indebted to Roger Blench for reporting that Maartin Mous of University of Leiden has presented the new hypothesis about Mbugu.) I suspect that Mous is mistaken, but I have not seen his paper; he may be right.

12. Perhaps we should resist what is pre-statistical thinking implied in the discussions of Mbugu. Linguists and anthropologists are prone to dismiss theories that admit to one or two exceptions, sometimes in the face of hundreds of counter-examples. Although Mbugu has borrowed most of a grammar (or most of its basic vocabulary), it does not follow that most languages do and it does not follow that morphology (or basic vocabulary) is not pretty good stuff to work with and sometimes quite decisive in taxonomy. If morphological evidence is so very useful in classifying hundreds of Afroasiatic languages, as Greenberg found it to be, why should one Afroasiatic language, Mbugu, undermine that valuable evidence? And the same goes for basic vocabulary. Is it not the experience of most working taxonomists that *most of the time* basic vocabulary evidence will point in the same direction as morphological evidence? The endless debate about the relative superiority of morphology or basic vocabulary is one of the silly aspects of historical linguistics.

A Note on Chance, Likelihoods, and Probabilities

In the physical sciences, there is an intimate and comfortable working relationship between science and mathematics. In the more humanistic social sciences, like linguistics and cultural anthropology, the relationship is nowhere near as intimate or comfortable. Many social scientists don't "do" mathematics, even statistics, which is kind of easy, but many are quite sophisticated in mathematics. When physical scientists dislike a conclusion with a mathematical component, they probably check the math carefully and the science part separately. Many social scientists accept a conclusion just because it has a mathematical component — in the belief that it must be true because of the math, mysterious and prestigious as it is supposed to be. Others reject for about the same reasons because they cannot check the math and so they do not understand the argument. Some very bright people have used statistics in historical linguistics but some very bright innumerates have rejected their conclusions — sometimes.

Now is the time to realize that science and mathematics are not the same thing, as philosophers of science argue regularly, for a number of reasons. One, mathematics is obviously not empirical. Two, its operations are more like those of formal logic than of empirical science. Three, the concept of proof is different in mathematics, being absolute so to speak, as opposed to science, where it is degrees of proximity to final truth — which no one really expects to ever actually reach. Four, while math is highly regarded in the physical sciences for its role in spelling out the implications of theory and in "model-building", still it is reported that many scientists really build their models in pictures/images, which they then translate into mathematical models for formal presentations. Five, the notions of likelihood, chance and probability are different sometimes in science and mathematics. This does have implications for the use of statistics in historical linguistics. Let us look at that.

Each of the notions has long since been formalized in mathematics, i.e., formal statements about probability; these in the sense of statistics dominate the interactions with historical linguistics. And indeed in sports and other parts of general culture. "What are his chances of winning the Pennsylvania state lottery?" can be formulated with precision by ordinary folk, who are "laymen" to the statisticians. We can say that the concepts of likelihood, chance, and probability have been *numeralized*, in the sense that statements about the likelihood of something can be expressed in numbers.

Yet there is a clear scientific sense of likelihood or probability that is not ordinarily or easily numeralized. That is because expressing the probabilities in formal mathematics (statistics) is too difficult or impossible or irrelevant to the discussion. In those cases, the statements about likelihoods are not innumerate — illiterate in math, so to speak — but rather are *judgments* about the relative truths of things or prophecies about outcomes. Or *predictions and postdictions*. Or to put it differently, there are statements which pertain more to the logic of a situation or discussion than to numeralized likelihoods. Consider these examples:

1. George thinks that the "Big Bang" theory is probably right.
2. Relax, Mrs. Jones, you are not likely to die of the mumps!
3. History is likely to declare President Reagan a winner.
4. President Yeltsin probably suffers from hangovers.
5. Hans will probably marry Gretchen because she is very pretty.
6. Darwin was wrong; we are unlikely descendants of monkeys.
7. "Do unlikely events ever happen?" (said a physicist to another).
8. Mexico's peso will probably rise in value now that ...

9. Poltergeists exist. What happened tonight was no accident.

Und so weiter. Let us consider some "probability" statements made by linguists. These should not be numeralized because they are judgments.

1. "The *Sanscrit* language ... bearing to both ... a stronger affinity ... than could possibly have been produced by accident ..." (Excerpts from Sir Wm. Jones' famous speech, proposing Indo-European).

2. "I find it so unlikely that English and German appear similar only by accident. Just look at those very idiosyncratic morphological patterns, like /-r/ versus /-st/ in adjectives. How could that happen by chance?" (A distillation of a number of scholary comments, put in quotes for display)

3. "I think proto-Indo-European probably had a series of glottalized consonants because that makes more typological sense of the phonology." (Another distillation of scholarly statements)

4. "I think the patterns of 1st and 2nd person pronouns in Nostratic and Amerind are so striking and different/separate from each other that they must be two distinct super-phyla. You just don't get this sort of thing by chance." (Another distillation of scholarly statements). QED. Can formal numeralizing really refute or prove such statements?

ON THE NATURE OF THE ALGONQUIAN EVIDENCE FOR GLOBAL ETYMOLOGIES

MARC PICARD
Concordia University, Montréal, Québec

1. Introduction

As proponents of the hypothesis that "the world's language families . . . all derive from a single source" (Ruhlen 1994:283), and that consequently "all of the world's populations are linguistically connected" (289), John D. Bengtson and Merritt Ruhlen (henceforth B&R) have recently proposed twenty-seven global etymologies to bolster their case for monogenesis of extant languages.[1] Given what they acknowledge to be "the generally antipathetic or agnostic stance of most linguists" (277) toward this type of endeavor in general and, more specifically, vis-à-vis the technique of mass comparison on which it is based, they have also presented a series of arguments in defense of some of the charges that have been leveled at their methodology, which owes much to the work of Joseph Greenberg (and against whom most of the attacks have been directed, especially as regards his controversial [1987] classification of Amerindian languages).

Basically, the general complaint on the part of historical linguists has been that there is an "incompatibility between Greenberg's method of multilateral comparison and the traditional methods of comparative linguistics" (283) mainly because of the disregard for reconstructions based on systematic sound correspondences. B&R's response is that they are working on linguistic taxonomy, and that discovering and classifying language families is something that must be done *before* any "family-internal questions such as sound correspondences and reconstruction" (284) can be addressed.

Two other criticisms of this type of long-range comparison that have often been expressed are that "such liberties are taken with semantic change that literally anything can be connected with anything else" and that "the presence of errors in the data . . . invalidate the overall hypothesis" (289). With an interest in trying to ascertain the legitimacy of such reproofs, on the one hand, but with neither the wherewithal nor the competence to check the plausibility of the semantic connections and the accuracy of all the forms that were used in setting up B&R's global etymologies, I thought it might still be possible to get some measure of the tenability and reliability of their methodology by examining the data from one language family very thoroughly. The family I will look at is Algonquian.

Out of the twenty-seven global etymologies proposed by B&R, nine contain Algonquian forms. These are:

(1) BU(N)KA 'knee, to bend'

a. Proto-Algonquian (PA) **wāk-* 'bend'
b. Blackfoot *woxos* 'shin'

(2) KANO 'arm'

Blackfoot *kin(-ists)* 'hand'

(3) KU(N) 'who?'

Passamaquoddy *kek*ʷ 'what'

(4) KUNA 'woman'

Shawnee *kwan-iswa* 'girl'

(5) MAKO 'child'

Natick *mukketchouks* 'boy'

(6) MANO 'man'

Blackfoot *no-ma* 'husband'

(7) MENA 'to think (about)'

Shawnee *menw* 'prefer, like'

(8) PUTI 'vulva'

a. Delaware *saputti* 'anus'
b. Mohegan *sebud* 'vagina'

(9) ʔAQ'WA 'water'

Proto-Central-Algonquian (PCA) **akwā* 'from water'

2. A review of the Algonquian forms

In this section, I will examine each of these correspondences in terms of their phonological, morphological, and semantic proximation so as to evaluate their potential relatedness.

(1) BU(N)KA 'knee, to bend' > (a) Proto-Algonquian **wāk-* 'bend', (b) Blackfoot *woxos* 'shin'

The PA form **wāk-* is accurate and, like BU(N)KA, it means 'to bend' (also 'to be bent, crooked, rounded').[2] In order to link the two, however, one would have to be able to explain how /b/ became /w/, for although /w/ > /b/ (via / β/) is common enough, e.g., Latin /weni:re/ > Spanish /benir/, /b/ > /w/ does not appear to be attested, at least in initial position.[3] Therefore, any claim that BU(N)KA and **wāk-* are related would depend crucially on a plausible account of this /b/:/w/ correspondence, especially since it is so rare in this series of comparisons. In other words, out of the eleven stocks in which reflexes of BU(N)KA are allegedly found, only Amerind has any instances of /w/, and among the fifty-odd Amerind forms that are cited, only the widely disparate Bella Bella *wak-* 'bent' and Tiatinagua *waku* 'elbow' also have this segment.

I have not counted what B&R list as Blackfoot *woxos* 'shin' among the latter for two reasons. First of all, it is not Blackfoot at all, as a check of Frantz (1989) has confirmed, but Arapaho /wóxos/, and, secondly, it is derived from PA **meɬkaɬkwana* (cf. Picard 1994),[4] which looks nothing like **wāk-*. Note also that 'knee' and 'elbow', which are the most frequent nominal glosses for the proposed derivatives of BU(N)KA, have no similarity to this form nor to **wāk-* in Algonquian. The first has been reconstructed as **meketekwi/a* (cf. Michelson 1935:144), and the second as **metoškwani.*

(2) KANO 'arm' > Blackfoot *kin(-ists)* 'hand'

The word for 'hand' in Blackfoot is /moʔtsís/, and it stems from PA **meɬentyi* (> **meɬenči*) (cf. Proulx 1989:63). Donald Frantz has informed me that he does not recognize the form *kin(-ists)* at all. Not only does it look nothing like the PA reconstruction above but neither does it bear any resemblance to other PA etymons like 'arm' and 'shoulder', which are other frequent glosses associated with this global etymology, e.g., **meneɬki* 'hand, arm', **meʔpetoni* 'hand, arm', and **meteɬemani* 'shoulder'. Moreover, even if *kin(-ists),* which Greenberg gives as *-kinist*ˢ (cf. 1987:165), were a real Blackfoot form, it might very well be segmentable as *ki-nists* with *ki-* being derived from **ke-* 'your' (cf. Proulx 1989:45-6).[5]

(3) KU(N) 'who?' > Passamaquoddy *kek*ʷ 'what'

It is difficult to see why this particular form has been chosen to represent Algonquian since a PA reconstruction is available, viz. **keekw-* 'something'. B&R have assembled a rather impressive collection of supposedly related forms from twenty-four different stocks for this global etymology, but the semantic and phonological criteria are so loose that almost anything would seem to qualify. Basically, any interrogative or relative pronoun containing a (preferably but not necessarily) word-initial velar or postvelar obstruent — and even then there are a number of exceptions — is deemed to be derivable from KU(N) 'who?'. How this form might have developed into PA **keekw-* 'something' is virtually inconceivable.

(4) KUNA 'woman' > Shawnee *kwan-iswa* 'girl'

To begin with, two corrections must be made in the Algonquian data:

1. the form is not *kwaniswa* but *kwaaniswa,*
2. the language is not Shawnee (where 'girl' is /škwee?θeeθa/) but Miami.[6]

Now, when one comes across any form that is glossed either 'woman' or 'girl' in Algonquian, one immediately looks for some possible connection with PA **eɬkweew-* since reflexes of this root can be found in every language group, e.g.. Abenaki (Eastern) /(a)skwa/, Cree (Central) /iskweew/, Arapaho (Western) /hísei/. Although a connection with /kwaan-/ (where /-iswa/ includes a diminutive suffix derived from **-ihs-*) is not immediately obvious, it becomes much more discernible when a check of the oldest records reveals that the word was formerly /ahkwaaniswa/ since /a-/ is one of the possible reflexes of initial **e*, e.g., Ojibwa /akkweeseenss/ 'girl', and also because **ɬk* > /hk/ in Miami-Illinois (cf. Costa 1991:376). In sum, when one now compares KUNA with **eɬkweewa*, no plausible phonological link between the two can be seriously entertained.

(5) MAKO 'child' > Natick *mukketchouks* 'boy'

Trumbull (1903) is the source of this form, but the gloss he gives is 'son, man child' rather than 'boy'. The Algonquianists I have consulted about this form have all professed their total ignorance of its structure and origin,[7] nor have they been able to provide any cognates outside of the immediate vicinity (Narragansett, Massachusett). The general consensus seems to be that this is a neologism rather than the isolated remnant of a global etymon. Algonquian languages of every group have a word for 'son' which stems from **-kwiʔs-* so that 'my son' is /ngwis/ in Micmac (Eastern), /nikwiʔθa/ in Shawnee (Central), and /naeʔhḁ/ in Cheyenne (Western).

(6) MANO 'man' > Blackfoot *no-ma* 'husband'

There are three errors in the Blackfoot form. Phonologically, it is /nóoma/, morphologically it is /n-óoma/, and semantically it is 'my husband'. There is no universal Algonkian form for 'husband'. A number of Central and Eastern languages have a reflex of **naapee-* 'male, man' (cf. Blackfoot /naapi/), but the Western languages have widely divergent forms, e.g., Arapaho /nééс/, Cheyenne /naéhame/; the Blackfoot word is unlikely to be related to any of these. At any rate, whatever /-óoma/ may be derived from, chances are its ancestor will resemble MANO even less.

(7) MENA 'to think (about)' > Shawnee *menw* 'prefer, like'

The situation here is similar to that of (4) in that a PA reconstruction is available, but there is also a serious problem of accuracy involved. Judging from the gloss, it is obvious that this root has been "lifted" from such forms as **melweelemeewa* 'he likes him' and **melweelentamwa* 'he likes it'. However, **melw-* shows up in a host of other forms with meanings that have nothing to do with 'like', e.g., **melwaačyemowa* 'he speaks well', **melwehtawekwesiwa* 'he sounds good', **melwaapaminaakwesiwa* 'he looks fine', etc. What this clearly shows is that **melw-* means 'well, good, fine, nice',[8] and I doubt if even B&R would propose that it could be semantically related to a root that means 'to think (about)'

(8) PUTI 'vulva' > (a) Delaware *saputti* 'anus', (b) Mohegan *sebud* 'vagina'

First of all, it is difficult to understand why the Mohegan form is glossed as 'vagina' since Prince & Speck (1904), which is clearly the source of the Algonquian data, gloss both of these forms as 'anus', which makes the semantic connection to 'vulva' less transitional, as it were. More importantly, however, the morphology is all wrong. According to John O'Meara (personal communication), who has done extensive work on Delaware (cf. O'Meara 1990), *saputti* /səpó:təy/ is a dependent noun which is composed of *səp-* 'closed' and *-təy* 'ass, backside'. This is easily confirmed by the existence of such forms as Ojibwa *n+diy* 'my ass', on the one hand, and Ojibwa *spo-,* Micmac *sep-* 'be closed', on the other (cf. Rhodes 1993, DeBlois & Metallic 1984). In sum, it looks like Delaware *saputti* and Mohegan *sebud* mean something like 'assplug', which is certainly as interesting a conception of 'anus' as 'asshole'.

(9) ?AQ'WA 'water' > Proto-Central-Algonquian (PCA) **akwā* 'from water'

This form has been taken from Siebert's reconstruction **akwa·ška·wi* 'breaker, wave dashing on the shore' (cf. 1975:413), but there are two inaccuracies: (1) it is a PA and not a PCA reconstruction; and (2) the pertinent root is **akw-* 'ashore, out of the water' with the following long vowel belonging to **-a·ška·-* 'wave'. According to Siebert (personal communication), "PA /*akw-/ 'ashore, out of the water' is a far cry semantically from any noun meaning water" and "actual semantics offers no support for such naive extensions of meaning and etymology".

Note that the universal Algonquian form for 'water' is **nepyi*, with offshoots appearing in every group, e.g., Natick (Eastern) /nəpi/, Fox (Central) /nepi/, Cheyenne /mahpe̥/. This, combined with the fact that there exists another PA form **kwa·p-* with the meaning 'out of water' (cf. Hockett 1957:267), makes it quite unlikely that **akw-* would constitute yet another root for 'water'.

3. Conclusion

In genetic classification, according to B&R, "the cumulative weight of all the evidence completely swamps the effects of whatever random errors may be scattered through the work" (1994:290). The errors in the Algonquian data are far from random, however, and this does not bode well for the procedure as a whole. As we have seen, every single etymology is beset with some sort of factual and/or analytical inaccuracy, as shown in Table 1 (next page). Some errors, such as those involving faulty glosses, erroneous transcriptions, and misidentified languages, may be considered trivial, but they are at the very least indicative of a general lack of concern for precision and rigor.

Others, however, are more serious and consequential, as when morphologically complex structures are misanalyzed so as to invalidate any proposed link between Algonquian and Proto-Amerind; or when contemporary forms (which may often be mere phonetic approximations) are adduced instead of available older forms or reconstructions which, upon inspection, reveal themselves to be totally unlike their putative global etymons, either phonologically or semantically (this is what I have termed 'ancestral disparity' in Table 1), and therefore useless as sustentative or corroborative evidence for these etymons. In sum, B&R may claim that "historical linguists . . . do not demand that the evidence be complete or immaculate" (1994:290), but neither will they accept that it be distorted, misrepresented, and error-ridden.

FOOTNOTES

1. Lest there be any confusion, Bengston has co-authored the chapter entitled "Global Etymologies" in Ruhlen (1994).

2. Unless otherwise indicated, PA reconstructions are from Aubin 1975 and/or Hewson 1993.

3. Examples of the types of changes that /b/ normally undergoes can all be found in the history of French: (1) deletion (*rubeum* > *rouge*), (2) insertion (*cameram* > *chambre*), devoicing (**corbu* (< **corwum*) > OF *corp*), and fricativization to /β/ followed by either deletion (**taβone* (> *tabānum*) > *taon*) or strengthening (*habēre* > *avoir*).

4. Note that **me(t)-* is a prefix meaning 'somebody's' in this and a number of other PA forms below. For the substitution of **ɬ* for **θ* in PA, see Picard 1984.

5. Greenberg is somewhat inconsistent in his use of morpheme boundaries. For example, what he gives as "Proto-Algonquian **nexkee,* Northern Arapaho *nes*" (1987: 184) should in reality be **me-neɬki* and /bé-nes/.

6. I am grateful to David Costa for his assistance with almost everything dealing with Miami-Illinois and Shawnee.

7. An intriguing possibility is **mačihkiwehsa* 'first born, eldest son', though this would entail, inter alia, that Natick had undergone metathesis.

8. Shawnee /menw-/ is apparently aberrant in that one would expect */melw-/, given that PA **l* is continued in this language (cf. Miller 1959:17). However, the form may very well be from another Central language which *has* undergone nasalization since B&R have misidentified languages in other cases.

REFERENCES

Aubin, George F. 1975. *A Proto-Algonquian Dictionary.* Ottawa: National Museums of Canada.
Costa, David J. 1991. "The historical phonology of Miami-Illinois consonants." *IJAL* 57:365-93.
DeBlois, Albert D., and Alphonse Metallic. 1984. *Micmac Lexicon.* Ottawa: National Museums of Canada.
Frantz, Donald G.. 1989. *Blackfoot Dictionary of Stems, Roots, and Affixes.* Toronto: University of Toronto Press.
Greenberg, Joseph. 1987. *Language in the Americas.* Stanford: Stanford University Press.
Hewson, John. 1993. *A Computer-Generated Dictionary of Proto-Algonquian.* Ottawa: Canadian Museum of Civilization.
Hockett, Charles F. 1957. "Central Algonquian vocabulary: stems in /k-/." *IJAL* 23: 247268.
Michelson, Truman. 1935. "Phonetic shifts in Algonquian languages." *IJAL* 8:131-71.
O'Meara, John. 1990. *Delaware Stem Morphology.* Ph.D. dissertation, McGill University.
Miller, Wick. 1959. "An outline of Shawnee historical phonology." *IJAL* 25:16-21.
Picard, Marc. 1984. "On the naturalness of Algonquian ɨ." *IJAL* 50:424-37.
Picard, Marc. 1994. *Principles and Methods in Historical Phonology: From Proto-Algonkian to Arapaho.* Montreal: McGill-Queen's University Press.
Prince, John D., and Frank G. Speck. 1904. *Glossary of the Mohegan-Pequot Language.* Lancaster, PA: New Era Printing Company.
Proulx, Paul. 1989. "A sketch of Blackfoot historical phonology." *IJAL* 55:43-82.
Rhodes, Richard A. 1993. *Eastern Ojibwa-Chippewa-Ottawa Dictionary.* Berlin: Mouton de Gruyter.
Ruhlen, Merritt. 1994. *On the Origin of Languages.* Stanford: Stanford University Press.
Siebert, Frank T., Jr. 1975. "Resurrecting Virginia Algonquian from the dead: the reconstituted and historical phonology of Powhatan." *Studies in Southeastern Indian Languages*, ed. James M. Crawford, pp. 285-453. Athens: University of Georgia Press.
Trumbull, James Hammond. 1903. *Natick Dictionary.* Bulletin 25. Bureau of American Ethnology.

TABLE 1

Types of errors in the Algonquian data in B&R's nine global etymologies

	1	2	3	4	5	6	7	8	9
WRONG LANGUAGE (GROUP)	x			x					x
WRONG GLOSS					x	x	x	x	
WRONG SEGMENTATION						x		x	x
WRONG TRANSCRIPTION				x		x			
UNRECOGNIZABLE FORM		x							
ANCESTRAL DISPARITY[1]	x		x	x			x	x	

1. Cases where an Algonquian form older than that given by B&R is phonologically or semantically more remote from its proposed origin.

GREENBERG COMMENTS ON CAMPBELL AND FLEMING

[Joseph Greenberg had the following response to MT-23. No quotations marks are used on his remarks.]

A few items in Lyle Campbell's "Inside the American Indian Classification Debate" in Issue 23, as well as one statement of Hal Fleming, as it turns out also, call for comment. In addition to the excellent remarks of Ruhlen on *n* and *m* first and second persons, I would add that Bright, who according to Thomason, originally assigned the review of *Language in the Americas* to Campbell in *Language*, in his book *American Indian Linguistics and Literature* (1984, p.15) talked about ". . . the widespread association of /n/ with the first person and of /m/ with the second person." He added "Campbell and Kaufman (1980) give the name of 'Pan Americanisms' to such similarities .." It was so widely known and commonplace among American Indianists that Dixon in his 1910 grammar of Chimariko stated (p. 322) "It will be seen that as in so many American languages, the pronominal stems of the first and second person are *n* and *m*." All this is mere obfuscation on Campbell's part.

With my regard to Swadesh, Campbell is not to be blamed for making some highly inaccurate remarks about my collaboration with Swadesh. I never received a joint grant with Swadesh nor did we do any systematic research together. I did use my influence to get him a grant from the Columbia University Research Fund after his appointment at CCNY was not renewed in 1950. He continued to live in New York until 1953 when he moved to Denver. Our joint research was confined to the *IJAL* paper on Jicaque (1953), where it is clearly stated that the idea was mine.

It happened as follows: Swadesh used to come to my office in Columbia. One day, probably in 1952, he looked over my shoulder in a notebook I was doing on Hokan and was surprised to see Jicaque included. I did not know it was considered a problem. He borrowed the notebook and come back convinced and suggest a joint article. He wrote the entire article. I deferred to him as I was quite young and relatively unknown. I was uncomfortable with a few of his etymologies, and I would not have included anything on glottochronology myself. I still have this early Hokan notebook and one can see the words on the glottochronological list checked off in another hand. I can produce this notebook if anyone wishes to see it. Today, Jicaque is generally accepted as Hokan. I discovered it, one of the extremely few new classificational ideas proposed in this period by what Campbell quotes as my method which is not a method. I might add that I am cited with quotations from my chapter in *Essays in Linguistics* by Campbell and Mithun in their *Native Languages of Native America* as an authority on the methodology of linguistic classification.

Campbell should abandon his notion that I had some predetermined scheme. One of the notebooks available in microfilm from Stanford is called "Unclassified South American Languages." These were added to larger groups when I had accumulated enough evidence to place them.

Finally, Fleming, a friendly critic, says I classified Mao as Cushitic instead of Coman. He considers this excusable because the material available at the time was so poor. But in *Languages of Africa* (p. 65, f.f. 13), I stated "I have employed Anfillo as the basic term here, since the northern Mao speak a different language which belongs to the Coman group." In the language index at the end, I list *two* Mao languages, one of which is listed as Western Cushitic and the other as Coman. As we can see, this is a mere confusion of names. Somewhere also (Ruhlen and I both remember this but have not found it), I said that Western Cushitic (now Omotic) was very different from the rest of Cushitic.

[End of Greenberg's comments]

A FEW DELAYED FINAL REMARKS ON CAMPBELL'S AFRICAN SECTION

HAL FLEMING

Lyle Campbell's attack on Greenberg in MT-23 drew my attention again, when I was checking out the errors Greenberg thinks I made. Before going to those, I must thank Campbell for crediting me with the first book I have ever written or even co-edited. That citation — Fleming and Bender 1976 — will be forever unique. Marvin L. Bender et al, eds., are the true authors of *Non-Semitic Languages of Ethiopia.* I had two articles in it. Bender and I made no joint remarks about Nilo-Saharan nor about how tough Omotic was to classify, although I said something like that — while on the way to making it a sixth branch of Afroasiatic. In other details, in this case Nilo-Saharan, Campbell made some more errors. Neither Surma nor Turkana are important branches/sub-phyla of Nilo-Saharan. Turkana is one Nilotic language, closely related to a number of others. Both Surma (older Beir-Didinga + Majang) and Nara (older Barea) were parts of East Sudanic which have been moved around a little in the re-shufflings of Nilo-Saharan internal taxonomy but neither would be called major branches of Nilo-Saharan. And again, a lot of new data have been reported since 1963. Who would be surprised if they changed internal taxonomy somewhat? Do be more reasonable, Professor Campbell!

Greenberg has a habit of making no comment on my efforts to defend his African classification, but will promptly criticize an error in detail. Do be more reasonable, Professor Greenberg! The Mao problem is unfortunate. I didn't say that he "classified Mao as Cushitic instead of Coman." [Coman = Koman]. Malheureusement, there are *three* Maos. Mao #1 is Anfillo, a member of the Gongan branch of North Omotic. Mao #2 is the supposed Koman peoples who are called Mao by themselves and others and recorded ethnographically in Vinigi Grottanelli's *I Mao.* This is the "Italian authority" who misled Greenberg but only in part. He was right about the other Koman peoples. Mao # 3 is a group of 7± languages in their own branch of North Omotic; one of them was the source of the linguistic data reported by Grottanelli in *I Mao* — it appears to have been the Gebsi language later recorded by Bender. It is right for Bender to publish on the Mao languages, since he recorded most of the field data until recently, but it is not right for him to imply that he classified them as Omotic. He was agonizing about them in the '70s because lexicostatistically they "didn't come out right". This caused me to look at the data and review Grottanelli's work, which I had seen before but not concentrated hard on. Then I classified them as Omotic and praised Bender for bringing up the matter. They escaped detection for two scholarly generations, most probably because they resemble the Koman physically and culturally.

It remains interesting that all along the interface between Afroasiatic and Niger-Congo or Nilo-Saharan the Afrasian languages spoken by "non-Hamitic looking" peoples have proven hard to classify or accept as Afrasian. It happened in the Chadic realm, and with Somotic, and with the Mao of Nomotic. It seems as if some scholars first classify people by eye and then try to make the languages fall in line with the physical/cultural categories.

SOME QUESTIONS AND THESES FOR THE AMERICAN INDIAN LANGUAGE CLASSIFICATION DEBATE (ad Campbell, 1994)

JOHN D. BENGTSON
Minneapolis, MN

1. RIGOR, OR RIGOR MORTIS? Lyle Campbell and Donald Ringe (1992) value "rigor," which is well and good, in moderation. But taken to an extreme, it becomes a tendency to be so rigorous that remote linguistic relationships are excluded by definition. The threshold is set so high that nothing beyond obvious relationships can possibly be accepted, resulting in an intellectual "rigor mortis" of accepting no fewer than 145 distinct language families in the Americas (Campbell, p. 41).

Edward Sapir's words of seven decades ago are still applicable to the situation today: "from an over-anxious desire to be right, [conservative historical linguists] generally succeed in being more hopelessly and fundamentally wrong, in the long run, than many more superficial minds who are not committed to 'principles"' (quoted in Bengtson 1994c:211).

The "splitterism" of Campbell and "most specialists today" is an *overreaction* to the "lumperism" of Sapir, Kroeber, and Swadesh. Note that splitting in itself has become a virtue. For example, in Na-Dene studies, Robert Levine (1979) is praised for his work attempting to "split" Haida from Na-Dene, while the positive and extensive work done by Jürgen Pinnow (1976, 1985, and much more) to restore Haida to Na-Dene is ignored.

This devotion to negativism should have been a short-lived corrective phase until things got back into balance, Instead, "splitterism" has become an end in itself, a dogma, and a cult.

I suggest that we try to get back to a more balanced view of science. Methods that exclude too much evidence (Greenberg 1993:89), keep languages apart that belong together, and result in an implausibly high number of language families, are *bad science*.

2. SPECIALISM VS. GENERALISM: Campbell repeatedly appeals to the opinion of specialists as a criterion of validity, even claiming (p. 48) that "no supporters among specialists" makes Greenberg's Indo-Pacific hypothesis "a total strike out."

In reality, the opinion of linguistic specialists as to the validity of a long-range hypothesis is largely *irrelevant*. Of course, specialists are the best source on matters of detail. But unless the specialist is also a generalist, s/he may well be unwilling and/or unable to make an informed judgment on the overall validity of a wide-ranging hypothesis.

All the specialists also rejected the ideas of Galileo, Copernicus, Newton, et al. This criterion should be eliminated from the debate.

3. CRITERIA OF GENETIC RELATIONSHIP: It is a relief to learn that Campbell (p. 47) does not insist on sound correspondences as the only indicator of genetic relationship, and that "patterned grammatical evidence may be sufficient in some cases." But by the latter rule, many long-range relationships are excluded *a priori*. For example, Harold Fleming (1974:84) notes that "Omotic languages usually lack grammatical gender and verb paradigms of classical or familiar type, i.e., not like standard Hamito-Semitic." Nonetheless, Fleming concludes that the Omotic languages are, indeed, Afroasiatic (= Hamito-Semitic) on the basis of "a number of morphemes which do find Afroasiatic fellows and these some of the most conservative items around." In Fleming's view "[morphologically] 'drifting' groups can sometimes be placed more easily and accurately in evolutionary terms from the evidence of the residues of the lexicon than from morphology (p. 87)."

Similarly, Sergei Starostin (1991:14), in his comparison of Caucasian with Sino-Tibetan, and Yeniseian, finds that "comparison with Sino-Tibetan is hampered by the almost complete elimination in these languages of a morphological system as such." In spite of that, Starostin (p. 13) deems the three families genetically related on the basis of parallels in basic vocabulary and "the presence in the majority of these comparisons of a system of regular phonetic correspondences".

What Starostin is really trying to say is that *diagnostic basic vocabulary* is the ultimate indicator of genetic relationship. The presence of a "system of regular phonetic correspondences" is intrinsic to genetically related languages, so saying "the languages are related because they share basic vocabulary with regular sound correspondences" is something like saying "it's the zebra with stripes," a restatement of the obvious.

So how do we make a genetic diagnosis with any certainty? Since grammatical paradigms erode away after long spans of time, as do sound correspondences, we are left with *diagnostic basic vocabulary* as the ultimate indicator of genetic relationship. When long time spans are involved, it is assumed that little of the original word-stock would remain, thus we do not compare just any words, but those which have been shown to be historically stable. I have found Dolgopolsky's (1964, 1986) list to be the most helpful, but there are also the well-known 100- and 200-word lists (in various versions) used in glottochronology.

When we focus in on these stable meanings, there is a better chance of finding deep cognates than in a random lexical comparison. Further, when repeated parallels are found (e.g., words for "eye, tongue, tooth", on the Dolgopolsky roster), genetic affinity is virtually assured, since "massive borrowing of basic vocabulary, not of separate basic lexical items, actually does not occur"

(Vovin 1994:96). When I applied the Dolgopolsky hierarchy to the problem of genetic affinity of Basque and Burushaski (Bengtson 1991, 1994a, 1994b), I found that there were about ten to a dozen parallels between the two languages and with proto-Caucasian. Finding this significant, I then went on to catalog all the traditional trappings of language families: grammatical parallels and regular sound correspondences (Bengtson 1990, 1992, 1993). It remains a mystery why this language family ("Macro-Caucasian," a subgroup of Dene-Caucasian), which has now been documented with all the traditional evidence of genetic relationship, continues to be ignored by most historical linguists.

4. PROOF: What is the burden of proof, if any? "'Proof' is for algebra or courts of justice" (Fleming 1994:70). Yet some linguists (e.g., Ringe) seem to think they must prove a hypothesis beyond a reasonable doubt. Isn't the idea rather to build a better model than we had before, then test it?

Greenberg has offered his model of the classification of American languages. If the methods of his critics are so much better, why don't they offer an alternative classification? Campbell and the others have not done this. (Sorry, 145 distinct families is not an acceptable alternative!)

5. MULTILATERAL COMPARISON VS. STANDARD COMPARATIVE METHOD: Campbell again repeats the supposed opposition between multilateral comparison and the standard comparative method. Greenberg (1993), Ruhlen (1987), and I (Bengtson & Ruhlen 1994) have repeatedly shown that this dichotomy is false, and that the two methods are in fact complementary processes of historical linguistics rather than contradictory.

Recently, certain long-ranger linguists of the Nostratic school (e.g., Vovin 1994) have taken up a similar claim, asserting the "traditional methods" as against Greenberg's. At a conference in Ypsilanti, Michigan, in 1993, this theme was discussed (see the report by Hegedus, 1994). Apparently, these Nostraticists have forgotten their own history: Aharon Dolgopolsky (1964) advocated a form of mass comparison in his pioneering article establishing the high probability of a Nostratic macrophylum. Only later were traditional methods applied, which, as Dolgopolsky (1986: 27-28) explained in a note written several years later, "rigorized the evidence" without altering the overall hypothesis. The same forgetfulness of history has afflicted Indo-Europeanists and Americanists.

6. POLARIZATION: Campbell characterizes the debate as between two extremes: Greenberg with three families, and "most specialists," who cannot bring themselves to accept fewer than 145 families. This degree of polarization, if it exists in reality, is strangely parallel to the political polarization of present day America. (I hesitate to take the political analogy too far: surely not all linguistic conservatives are politically conservative, and vice versa.) In the 1992 and 1994 U.S. elections, many voters felt alienated from both major parties, and expressed their alienation by voting for third party candidates, or not voting at all.

Can we not stake out a middle ground in this debate as well? Surely, there are some of Campbell's specialists who could accept, say, twenty or thirty families in the Americas. There may even be some who think the outlines of Greenberg's classification could be correct (as it turned out in his African classification), but reserve arguments about details of subgrouping, or about the need to "rigorize" the evidence (Ruhlen 1987:122). Let's hear more from these kinds of views.

I commend Professor Campbell for his call to "abandon rhetoric and to return to matters of substance" (p. 48), and for his willingness to present his case in a periodical whose editors and readers tend to be antagonistic to his viewpoint. I second his motion and hope that we can continue to discuss issues of method and evidence, without resorting to emotional and personal invective.

REFERENCES

Bengtson, John D. 1990. "An End to Splendid Isolation: The Macro-Caucasian Phylum." *Mother Tongue* 10 (April 1990).

Bengtson, John D. 1991. "Macro-Caucasian: A historical linguistic hypothesis," in Shevoroshkin (ed.) 1991, pp. 162-170.

Bengtson, John D. 1992. "Macro-Caucasian Phonology (Revised Version)," in *Nostratic, Dene-Caucasian, Austric and Amerind*, ed. by Vitaly Shevoroshkin, pp. 342-352. Bochum: Brockmeyer.

Bengtson, John D. 1993. "The Macro-Caucasic Hypothesis." *Dhumbadji!* 1/2:3-6.

Bengtson, John D. 1994a. "Comment on Colarusso 1994." *Mother Tongue* 22:13-16.

Bengtson, John D. 1994b. "On the Genetic Classification of Basque." *Mother Tongue* 22:31-36.

Bengtson, John D. 1994c. "Edward Sapir and the 'Sino-Dene' Hypothesis." *Anthropological Science* 102/3:207-230.

Bengtson, John D. and Merritt Ruhlen. 1994. "Global Etymologies," in *On the Origin of Languages*, by Merritt Ruhlen, pp. 277-336. Stanford, CA: Stanford University Press.

Campbell, Lyle. 1994. "Inside the American Indian Language Classification Debate." *Mother Tongue* 23:41-55.

Dolgopolsky, Aharon B. 1964. "Gipoteza drevnejsvego rodstva jazykovyx semej Severnoj Evrazii s verojatnostnoj točki zrenija." *Voprosy Jazykoznanija* 2:53-63.

Dolgopolsky, Aharon B. 1986. "A Probabilistic Hypothesis Concerning the Oldest Relationships among the Language Families of Northern Eurasia," in *Typology, Relationship, and Time*, ed. by Vitaly Shevoroshkin and Thomas L. Markey, pp. 27-50. Ann Arbor, MI: Karoma. [Translation of Dolgopolsky 1964, with additional notes.]

Fleming, Harold C. 1974. "Omotic as an Afroasiatic Family." *Studies in African Linguistics* (Supplement 5): 81-94.

Fleming, Harold C. 1994. "A Mild Rejoinder to Lyle Campbell." *Mother Tongue* 23:70-72.

Greenberg, Joseph H. 1987. *Language in the Americas*. Stanford, CA: Stanford University Press.

Greenberg, Joseph H. 1993. "Observations Concerning Ringe's *Calculating the Factor of Chance in Language Comparison*." *Proceedings of the American Philosophical Society* 137/1:79-90.

Hegedus, Iren. 1994. "Report on the 2nd Workshop on Comparative Linguistics: The Status of Nostratic: Evidence and Evaluation." *Mother Tongue* 21:5-8.

Levine, Robert D. 1979. "Haida and Na-Dene: a new look at the evidence." *International Journal of American Linguistics* 45/2:157-170.

Pinnow, Heinz-Jürgen. 1976. *Geschichte der Na-Dene Forschung*. Berlin: Indiana.

Pinnow, Heinz-Jurgen. 1985. "Sprachhistorische Untersuchung zur Stellung des Haida," in *Gedenkschrift Gerdt Kutscher*, vol. 2., pp. 25-76. Berlin: Indiana.

Ringe, Donald A. 1992. *Calculating the Factor of Chance in Language Comparison*. Philadelphia: American Philosophical Society.

Ruhlen, Merritt. 1987. *A Guide to the World's Languages*, vol. 1. Classification. Stanford, CA: Stanford University Press.

Shevoroshkin, Vitaly (ed.). 1991. *Dene-Sino-Caucasian Languages*. Bochum: Brockmeyer.

Starostin, Sergei A. 1991. "On the Hypothesis of a Genetic Connection Between the Sino-Tibetan Languages and the Yeniseian and North Caucasian Languages," in Shevoroshkin (ed.) 1991, pp. 12-41. [Translation by William H. Baxter III of the original article in Russian.]

Vovin, Alexander. 1994. "Long-Distance Relationships, Reconstruction Methodology, and the Origins of Japanese." *Diachronica* 11/1:95-114.

A NOTE ON AMERIND PRONOUNS

MERRITT RUHLEN
Palo Alto, California

In three recent publications, Lyle Campbell and Ives Goddard have once again charged that Joseph Greenberg and I have distorted the pronominal evidence for Native American languages. According to Campbell (1994:47), "the *n/m* ['I/you'] pattern is not nearly as common in the Americas as Greenberg claimed . . . [and] his supposed *m/t* ['I/you'] pattern for his Eurasiatic languages is also found abundantly in the Americas (despite his and Ruhlen's assertions to the contrary)." Elsewhere he claims that "several Amerind groups exhibit pronoun forms (*m/t* ['I/you']) that Greenberg attributes to Europe and Northern Asia" and "the *n* 'first person' / *m* 'second person' is by no means unique to, diagnostic of, or ubiquitous in American Indian languages" (Campbell 1993:3, 9).

It would seem that after eight years of debate, the infamous Amerind pronoun problem is still unresolved. We propose that it is time for both sides to lay their cards on the table, face up, and let the rest of the scientific community decide which side has the winning hand. I will present the evidence for the Amerind *n/m* pronoun pattern at the conclusion of this note, and I am asking the editors of *Mother Tongue* to print Campbell and Goddard's "abundant" evidence for the *m/t* pattern immediately following my note so that each reader can weigh their relative merits for himself. Should Campbell and Goddard decline to show their cards, this would be tantamount to an admission that they simply have been bluffing all along, and the Amerind pronoun controversy may then be considered resolved.

I put together the Amerind evidence by going through Greenberg's Amerindian notebooks (Greenberg 1983), looking for languages that have *both* first-person singular *n* and second-person singular *m;* plural pronouns were not used. Since Campbell and Goddard do not have easy access to Greenberg's notebooks, while I was looking for the Amerind pattern, I also looked for the Eurasiatic pattern *m/t,* that is, languages which had *both* first-person singular *m* and second-person singular *t.* I found neither the "abundance" of languages nor the "several Amerind groups" that Campbell claims possess these pronouns. In North America, I found two Siouan languages that could be considered to have the Eurasiatic pattern (Mandan *mi/da* and Hidatsa *ma/da),* while in South America, I found but two isolated languages, one Paezan (Millcayac *mioiñ/tœz)* and one Macro-Ge (Coroado *make/teke).*

For the Amerind pattern *n-/m-* 'I/you,' I found an entirely different story:[1]

Almosan-Keresiouan: Proto-Algic **-Vn/*-Vm*, Kutenai *-ən/-m*; **Penutian:** Tsimshian *n-/m-*, Chinook *n-/m-*, Takelma *-n/ma*, Nez Perce *ʔí·n/ʔí·m*, Klamath *ni-/mi-*, Wintu *ni-/mi-*, Colouse *nat/mit*, Patwin *na/mi-*, Yokuts *naʔ/maʔ*, Maidu *ni/mi*, Nisenan *ni/mi*, Tunica *-ni/ma*, Huave *-na-/me-*; **Hokan**: Chimariko *no-/mam-*, Karok *na/īm*, Arra-arra *na/im*, Pehtsik *naah/eehm*, Washo *le* (< **na*)/*mi*, Esselen *niš-/miš-*, Yuma *nnyep/mañ*, Mohave *inyeč/manč*, Walapai *ãn/ma*, Havasupai *inya/ma-a*, Yavapai *nya-a/ma-a*, Diegueño *ʔenyaa/maa*, Chontal *ni/mi*, Coahuilteco *na/mai*; **Central Amerind:** Kiowa *nã/am*, Jemez *ne/ũmiš*, Kawaiisu *nɨʔɨ/ʔimi*, Utah *ne/yim*, Opate *ʔina-po/ʔeméʔe*, Yaqui *ʔinapo/ʔemeʔe*, Tarahumara *ni-hé/ʔyemi*, Papago *-ñ/-m*, Hopi *nuʔ/uma*, Nahuatl *no-/mo-*, Pipil *-neč/-mit*^{s}*;* **Chibchan-Paezan**: Cogui *nə́s/má*, Ica *nən/ma*, Chimila *náari/ámma*, Bribri *ñõ/ma*, Rama *nā/mā*, Miskito *yan* (? < **ñan*)/*man*, Ulua *yan/man*, Sumu *yan/man*, Guamaca *nerra/ma*, Lenca *una/amna*; **Andean:** Mapudungu *ta-ñi/ta-mi*, Proto-Quechua **nuqa/*qam*, Jaqaru *na/huma*, Aymara *naya/huma*; **Equatorial:** Mocochi *an-/ma*, Guahibo *xánɨ/xamɨ*, Jitnu *kan/kam*, Cuiba *xan/xam*, Guayabero *xan/xam*, Itene *ana-/ma-*; **Macro-Panoan:** Moseten *ñu/mi*, Nocten *no-/em*, Pacaguara *no-/mi-*, Chacobo *no-/mina*, Arazaire *noena/mina*; **Macro-Ge:** Delbergia *nũ/ma*, Tibagi *in/ama*, Catarina *enha/ahama*.

REFERENCES

Campbell, Lyle. 1993. "Putting Pronouns in Proper Perspective in Proposals of Remote Relationships among Native American Languages," in *Survey of California and Other Indian Languages,* Report 8, Berkeley, Survey of California and Other Indian Languages, pp. 1-20.

Campbell, Lyle. 1994. "Inside the American Indian Language Classification Debate," *Mother Tongue* 24:41-55.

Goddard, Ives, and Lyle Campbell. 1994. "The History and Classification of American Indian Languages: What are the Implications for the Peopling of the Americas?," in Bonnichsen and Steele 1994, pp. 189-207.

Greenberg, Joseph H. 1983. *Amerindian Notebooks,* 23 vols. Mss. on file, Green Library, Stanford University, Stanford.

Greenberg, Joseph H. 1987. *Language in the Americas.* Stanford: Stanford University Press.

NOTE

1. If one were to take into account those languages which have one or the other pronoun, the evidence for the Amerind pattern would be even more overwhelming. As one example, there are no examples from the Macro-Tucanoan branch because the first-person pronoun in these languages is not based on *n-*. It is possible, however, that the Macro-Tucanoan first-person forms are cognate with the first person *n-* forms. The most common Macro-Tucanoan first-person pronoun is something like *yii/yeiʔe/ya.* It is not implausible that this initial *y-* derives from an earlier palatal nasal **ñ-*, an intermediate phonetic stage that is well attested in the Yuman family, in Bribri, Mapudungu, Moseten, and which I suggest is the source of the Miskito, Ulua, and Sumu first-person pronouns. When one compares Proto-Tucanoan **yɨʔɨ* 'I' with Kawaiisu (Uto-Aztecan) *nɨʔɨ* 'I,' there is certainly a suspicion that these forms might be cognate. When the comparative phonology of the Macro-Tucanoan branch is eventually worked out, it seems to me likely that the Tucanoan forms will turn out to be cognate with the other Amerind forms, in which case there will be a couple of dozen more languages attesting the Amerind pronominal pattern, since virtually every Macro-Tucanoan language has a second-person pronoun based on *m-*. Similarly, Macro-Carib languages, and Miwok languages, are absent because only second-person *m-* has survived. On the other hand, Oto-Manguean and Paezan languages are not cited because these families only show survivals of first-person *n-*.

REGARDING NATIVE AMERICAN PRONOUNS

IVES GODDARD
Smithsonian Institution

At the invitation of Allan Bomhard, I will respond briefly to Merritt Ruhlen's note (Ruhlen 1995).

What is at issue is claims such as the following: (1) "Amerind languages are characterized by first-person *n* and second-person *m*" (Ruhlen 1987:10); (2) "By itself ... a consideration of first- and second-person pronouns in New World languages leads directly to the Greenberg classification" (Ruhlen 1994a:178). Probably all that should be said about the first claim is that, as worded, it has no truth-value: the expression "characterized by" is too imprecise for it to be possible to determine whether the statement is empirically true or false. Certainly, however, the impressionable journalists and geneticists who seem to have taken it to mean that first-person *n* and second-person *m* are diagnostic of these languages have been misled.

The second of these claims is more interesting, because one can imagine how, in principle, it could be demonstrated. One would assemble the first- and second-person pronouns of the New World languages, classify them according to some replicable methodology, and then compare that classification to Greenberg's. Since Ruhlen has made this claim without actually presenting a comprehensive classification of the pronouns in question, it seems out of place for him to demand a showing of data from those who have expressed skepticism. If he expects his claim to be seriously discussed, he should present the data that would support it.

What Ruhlen (1995) does present is evidence that first-person *n* and second-person *m* can be found in a number of New World languages, a fact that has never been in dispute. The gulf between his methodology and the task necessary to support his claim is made clear by a comparison of the tabulations of "Almosan-Keresiouan" pronouns by Ruhlen (table 1 [below]) and by Goddard and Campbell (table 2 [next page]).

Table 1. "Almosan-Keresiouan" first-person-singular and second-person-singular pronouns as presented in a comparative table by Ruhlen.

	1sg.	**2sg.**
Almosan-Keresiouan	**ne-* "my"	*-m* "thou"

Source: Ruhlen (1994b:22).

I imagine that many readers will be astonished at how far Ruhlen's presentation is from the true picture of the extreme diversity of the pronouns actually to be found in the "Almosan-Keresiouan" languages. On the face of it, it is apparent that a consideration solely of the first and second-person pronouns of the "Almosan-Keresiouan" languages would not lead directly to Greenberg's classification of these languages, but if Ruhlen wishes to claim that it would, it is incumbent on him to demonstrate just how Greenberg's classification can be derived from these data.

The true diversity of these pronouns raises another, basic problem for Ruhlen and Greenberg's approach. On their hypothesis that the "Almosan-Keresiouan" languages are related fairly closely, at a time depth less than that of "Amerind" as a whole, how can the evident diversity of the pronouns in these languages be explained? On the hypothesis that these languages form a genetic subgroup, it must follow from this diversity that they have undergone a great deal of innovation in their pronominal systems. It would then further follow that there must exist commonly occurring processes of some sort by which pronouns are renewed and replaced. This conclusion, which follows inescapably from Ruhlen and Greenberg's hypothesis on the present facts, is, however, fundamentally inconsistent with the necessary premise of the use of pronouns in long-range classification: that "these two pronouns [the first- and second-person pronouns] are ... among the most stable items in language ... and are rarely borrowed" (Ruhlen 1994:123).

Table 2. "Almosan-Keresiouan" first-person-singular and second-person-singular pronouns as presented by Goddard and Campbell.

	Words and Prefixes		Suffixes	
	1sg	2sg	1sg	2sg
Almosan				
Alvic				
Algonquian	*ne-	*ke-	*-(y)a·n, *-ak[1]	*-(y)an-, *-at,[1] *-lwe[2]
Cheyenne	na-	ne-	-(t)ó, -o[1]	-(t)o, -os ~ ot,[1] -ce[2]
Ritwan				
Wiyot	d-	kh-	-Ø, -ak[3]	-t, -am[3]
Yurok	ʔne-	k'e-	-k'	-ʔm
Kutenai	hu-, ka-[4]	hin-	-a(·)p[5]	-i(·)s,[4, 5] -(e·)n,[2] -m[6]
Mosan				
Wakashan				
Kwakiutl			-ənł	-əns
Nootkan	*siy	*suw	*-s	*-suk
Chimakuan				
Quileute	lá·b; ʔal[7]	či; č[7]	-li, -s,[4] -sta[5]	-lič, -č,[4] -swo[5]
Salish	*ʔəncá; *n-[4]	*nəwí; *ʔən-[4]	*-(a)n, *-c[5], *-mx[8]	*-(a)x^w, *-ci,[5] *-mi[8]
Keresiouan				
Caddoan	*k-, *t-	*si-		
Caddo	ci-, ku-[5]	yahʔ-, si-[5]		
Iroquoian				
No. Iroquois	*k-, *wak-[5]	*(-h)s-, *(-e)s(a)-[5]		
Seneca	k(e)-, wak(e)-[5]	s(e)-, sa-[5]		
Keresan				
Santa Ana	hínʊ; s-, t^h-, k^h-, n-[9]	híṣʊ; ṣ- ~ š-, ç̣^{h}- ~ c^h-, p^h-[9]		
Siouan-Yuchi				
Siouan	*w-	*r-		
Catawba	d- (~ n-)	y-	-naʔ[4]	-yaʔ[4]
Sioux	wa-, mã-[4, 5]	ya-, nĩ-[4, 5]		
Yuchi	di; di-, cɛ-[5]	cɛ; nɛ- ~ yo-, nɛnʒɛ-,[5] so-[10]		

Intransitive subject markers given first in sets; others are:

1. Transitive subject.
2. Imperative.
3. Subjunctive.
4. Possessor.
5. Object (Siouan and Iroquoian: patient)
6. Reflexive imperative.
7. Conditional.
8. Causative object.
9. Indicative, dubitative, hortative, (first person) future hortative; single nonglottalized consonants only (there are many other class and modal allomorphs).
10. Indirect obiect.

Omitted: (1) Plurals, pluralizers, transitive combinations; in some languages these add many variants. (2) Minor variants, especially if only vowels are involved or if additional material looks segmentable.

Source: Goddard and Campbell (1994:197).

Finally, as always, there are the empirical questions regarding the accuracy of the data. Readers will notice that table 2 and Ruhlen (1995) disagree on what the pronouns are in Algic and Kutenai. The fact is, as table 2 shows, that nowhere in Algic (Algonquian + Wiyot + Yurok) and nowhere in Kutenai is there a paradigm with first-person *n* and second-person *m*. Second-person *m* is not found in Algonquian; its claimed existence goes back to Sapir, who later accepted Michelson's refutation of it (Goddard and Campbell 1994:199). The *m* in question is the formative of the absolute endings of the independent order and occurs in first person forms as well as second-person forms. Second-person *m* can be reconstructed for Proto-Ritwan (Wiyot + Yurok) on the basis of the agreement between Yurok and the Wiyot subjunctive, but Ritwan suffixal paradigms do not have first-person *n*. The Algonquian conjunct intransitive endings have *n* in the first-singular form but also in the second-singular form; the *n* cannot be segmented or analyzed as a distinctive first-person morpheme. It is worth noting that the claim of paradigmaticity for "Proto-Algic **-Vn/*-Vm*" now made by Ruhlen (1995) goes beyond the more modest claim implied in his earlier presentation (Ruhlen 1994; see table 1), where the differences in the glosses and in the ways the hyphens and the asterisk are used imply a realization that the two consonants come from different paradigms and different sets of languages.

Kutenai has both *n* and *m* in different imperative suffixes; I do not know the basis for saying that there is a distinctive first-person-singular *-ən* in Kutenai, as claimed by Ruhlen (1995). Some imperatives with first-person object have endings that contain an *n* (these are not given in table 2, which excludes transitive combinations). But since this *n* occurs in a transitive modal ending, it obviously cannot be simply assumed to be the distinctive mark of a first person. And, in fact, if it can be segmented, it is most likely to be identified with the *n* of the imperative ending and hence the second person. The first-person singular object suffix in Kutenai is *-a(·)p*.

The questions remain: How can the "Almosan-Keresiouan" languages be classified as part of "Amerind" on the basis of their first- and second-person pronouns? How is the diversity of the pronouns in the "Almosan-Keresiouan" languages to be accounted for? (I leave to others the consideration of the pronouns of the other putative branches of "Amerind.") These are questions that Ruhlen must answer if he expects his claim (the second of those quoted) to be taken seriously. It would be of absolutely no significance to the validity of this claim if it turned out that Campbell had exaggerated and there were only very few languages in the Americas with first-person *m* and second-person *t*. Meanwhile, some useful information is in Campbell (1994) in a discussion that will appear in expanded and updated form in Campbell (in press).

REFERENCES

Campbell, Lyle. 1994. "Putting Pronouns in Proper Perspective in Proposals of Remote Relationships among Native American Languages." In *Survey of California and Other Indian Languages*, Report 8, ed. Margaret Langdon, pp. 1-20. (*Proceedings of the Meeting of the Society for the Study of the Indigenous Languages of the Americas, July 2-4, 1993, and the Hokan-Penutian Workshop, July 3. 1993.*) Berkeley.

Campbell, Lyle. (In press.) *American Indian Languages: The Historical Linguistics of Native America*, Oxford University Press.

Goddard, Ives, and Lyle Campbell. "The History and Classification of American Indian Languages: What are the Implications for the Peopling of the Americas?" In *Method and Theory for Investigating the Peopling of the Americas*, ed. Robson Bonnichsen and D. Gentry Steele, pp. 189-207. Corvallis, Ore.: Center for the Study of the First Americans, 1994.

Ruhlen, Merritt. 1987. "Voices from the Past." *Natural History* 96(3):6-10.

Ruhlen, Merritt. 1994a. "Linguistic Evidence for the Peopling of the Americas." In *Method and Theory for Investigating the Peopling of the Americas*, ed. Robson Bonnichsen and D. Gentry Steele, pp. 177-188. Corvallis, Ore.: Center for the Study of the First Americans, 1994.

Ruhlen, Merritt. 1994b. *On the Origin of Languages: Studies in Linguistic Taxonomy.* Palo Alto: Stanford University Press.

Ruhlen, Merritt. 1995. "A Note on Amerind Pronouns." *Mother Tongue* (this issue).

TWO NEW ASPECTS OF MASSIVE COMPARISONS

First, *some new statistics from Joseph H. Greenberg.*

At the explicit request of *Mother Tongue*, Professor Greenberg agreed to extend the formulations of *recoverable vocabulary*, the lower levels of which he had first published in his *Language in the Americas*. What we had in mind were the dogma-driven statements[1], such as Kaufman's and Ringe's, that languages lost so much — changed so much — over time that older ancestors were not reconstructable, nor older taxa demonstrable. Finally, some have said that 5 millennia were all historical linguistics could manage. Others, seeking to avoid being trapped by their own chronology, suggested that 10 millennia were all that could be handled. The governing model compared two languages only, i.e., it was binaristic.

Again why do we have to be so binaristic? Sheila Embleton in her comments on the complexities of determining probabilities of cognation, seemed to exonerate Ringe from the criticism that he was only binaristic. Her statement is quite strong that one could hardly do more than compare *two languages* when it is so difficult. Well, it is difficult if you do it that way. If you address the problem that way, you create your own insurmountable mountains. To one who rarely works binaristically, the deep-seated reluctance to compare three or four or ten things seems irrational. Mathematical innumerates who use glottochronology are stuck with binarism because that is the way those formulae are set up. Par example, moi.

Herewith are those older formulations which Greenberg calculated and put in *Language in the Americas* plus some extensions which his colleague, James Fox of Stanford University, worked out on his computer (with a suitable program). One may look up the original formula in Greenberg's book (pp. 341-44). We take the constant to be *.80* and use the *Joos function.* Read the table as years on the left, number of languages compared on top, and percentages of recoverable vocabulary within the table, expressed to the right of decimal points. With constant *.84*, figures would be larger.

Number of Languages

Years	*2*	*3*	*4*	*5*	*6*	*7*	*8*
1,000	.648	.882	.959	.985	.994	.998	.999
2,000	.440	.700	.835	.906	.945	.967	.979
3,000	.310	.542	.693	.788	.851	.892	.921
4,000	.226	.420	.563	.666	.740	.794	.834
5,000	.168	.328	.456	.555	.631	.690	.737
6,000	.128	.259	.370	.461	.534	.594	.643
7,000	.100	.207	.302	.383	.451	.508	.556
8,000	.079	.167	.248	.319	.381	.433	.479
9,000	.063	.136	.206	.268	.323	.371	.413
10,000	.051	.112	.171	.226	.274	.318	.356
20,000	.010	.023	.038	.053	.068	.083	.096
30,000				.018			

[Note: if one used standard rates, assuming homogeneity of replacement — each word has the same likelihood of being retained/replaced, then the figures would be a bit lower. Joos function is a simple recognition that words on the list differ: some are lost easily, some are retained longer.]

Greenberg's calculations continue:

Number of Languages

Years	*9*	*10*	*20*	*30*	*40*	*50*	*80*
1,000	1.00	1.00	1.00	(et cetera, et cetera)			
2,000	.987	.991	1.00	(et cetera, et cetera)			
3,000	.941	.955	.995				
4,000	.865	.889	.976				
5,000	.775	.806	.937				
6,000	.683	.717	.883				
7,000	.597	.632	.819				
8,000	.519	.554	.751				
9,000	.450	.484	.683				
10,000	.391	.422	.618				
20,000	.110	.122	.220	.287	.336	.374	.455
30,000				.123	.150		
40,000		.020	.042	.061			.116*
50,000					.041	.048	
60,000							.140
70,000						.018	
80,000							.018

*I believe this figure is an error, probably a copyist's, because the true retention should compute to about 30%.

As a final note in his extended calculations, Greenberg adds that "Actually, as I'm sure you know, the recoverable* vocabulary is higher for the following reasons:

1. Cognates fall off the list because of semantic change but still survive and are recoverable by comparison.

2. I don't think the Swadesh list is normally distributed. I think it has a long tail (asymptote) of fantastically stable items like pronouns."

*Recoverable vocabulary has two related meanings, (a) the percentage of basic vocabulary on a Swadesh 100 or 200 word list and (b) the recoverable vocabulary in the general lexicon or the whole array of morphemes other than grammemes. In the case of (a), somewhat higher percentages pertain to the 100 word list, which is a bit more conservative than the 200 word list. My own experience in Cushitic is that the 200 word list yields 3% fewer cognates, yet it has more common retentions. Example, 20% on a 100 word list means that 20 words are recoverable (cognate) on *that list*, yet 17% of a 200 word list means *34* words are recoverable on the larger list. The comparativist is usually more interested in how many actual cognates she has to work with, rather than percentages which have no linguistic content.

The case of (b) is more important. While much has been made of the inconstancy of cultural vocabulary, there are very useful sectors of the lexicon which are quite conservative yet are not listed in basic vocabulary. To put it another way, in 5000 years, English and Armenian would be expected to show 17 cognates on a 100 word list, yet the total lexicon which can be recovered just from these two is much more than that, as any Aryanologist will tell you. Thus, if we take one language from each primary branch of Indo-European, we would expect to get 81 cognates on a 100 list or about .807 when 10 languages of 5000 years separation are compared. Yet the total actually recovered vocabulary of Proto-Indo-European is more than 10 times greater than that. And naturally, some of that is cultural vocabulary because no one has ever argued all of that is lost quickly. Have they?

Once again, it is time to remind our linguistic tortoises that none of the calculations about lexical retention apply to *grammemes*. Swadesh was never able to get them on his list. They can often be very conservative!

Second, *Paul Whitehouse's proposal for massive comparisons.*

Long rangers have reached the point of diminishing returns empirically, and in this business that means logically too. One of the reasons that full scale tests have not been made on many propositions has been the sheer difficulty of amassing the huge amounts of necessary data and the long hours of analysis and comparison. Most people seem to prefer to make their judgments on the basis of theory/ideology; it is certainly a lot easier!

Would it not be easier — at least — to have all the data available in some handy place and be able, par example, to examine the Indo-Pacific hypothesis across its full range in all its languages (1000+)? Or settle the question of how many languages show the "Mitian" pattern of pronouns?

Someone has an idea about making it all easier. Paul Whitehouse's proposal follows below. It has *Mother Tongue*'s hearty endorsement. We would hope that reactions can be sent to us, as well as to Mr. Whitehouse. We especially hope that Mary Ritchie Key will react favorably and join in.

"Dear Professor Fleming, 2nd December 1994

[2nd P] I also enclose a copy of a letter which I would like you to print in *Mother Tongue*. It is an appeal to the linguistic community to make life easier for itself and make a lasting contribution to posterity by compiling a standard lexicon of all the world's languages and making it available to all who need to use it."

"Having read my modest proposal, I hope you will be sufficiently in sympathy with its good intentions to overlook its impertinence and consent to print it. After all, the worst that can happen is that everyone ignores it! I look forward to hearing from you. Yours sincerely, Paul Whitehouse."

"Earlier this year, impatient with the failure of any linguistic taxonomist to offer me a comprehensive sub-grouping of the Human language family, I decided that if my curiosity was ever to be satisfied I would have to do the job myself, even though this meant compiling word lists for all the world's languages."

"When I started, however, I was stunned and appalled to find how difficult this would actually be. I had assumed that the information I sought would be readily available and in digestible form. How different the reality of the situation! It soon became obvious that just to compile Swadesh 100 lists of any usable number of languages would take weeks and months of trawling through mountains of publications. I was looking at decades of work rather than years."

"What appalled me most, though, was the realization that every single linguist who wanted to carry out global research would have to perform exactly the same Herculean labour, and not only my contemporaries but every single linguist yet unborn; the linguistic community had fallen down in its most basic communal duty: to make its raw materials freely available. The omission staggers me still; it is as if I had discovered that all mathematicians were still expected to calculate their own log tables."

"Hence this appeal to the linguistic community: surely the time has come to make good our predecessors' neglect and establish the central register of linguistic data that could — and should — have been started a hundred years ago. Agree among yourselves what basic lexicon is needed, agree on a standard orthography and *stick to it*, parcel out the world's languages among however many expert volunteers will keep the individual workload down to a manageable level, and put the whole lot on CD-ROM and/or make it available on the internet."

"This is without doubt an immense job, but a job that only needs to be done once. The more linguists who man the pump, the sooner the job will be done. The alternative is to condemn us all, and all those who come after us, to an endless, needless reduplication of the same counter-productive drudgery. The world's lexicon is a basic resource. Surely we all owe it to ourselves to make it freely available, so that linguists will be free to proceed to that other great neglected duty: making their unique contribution to unraveling the Human story. It would be nice to finish this in the same century as the Human Genome Project is completed, for instance."

"I regret that I have neither time nor the expertise to contribute more to this enterprise than the initial appeal, but as it would not be fair of me to burden Professor Fleming with unwanted correspondence, I am happy to act as coordinator during the initial stages of the project. So, if this appeal has struck any sort of chord at all and you are interested in playing a part, please write to me at:"

Flat 3, Angel House
Pentonville Road
London, England N1 9HJ

"The next step can then be considered according to the response — or lack of it — received."

"As to finance, which will doubtless be uppermost in many people's minds, if we cannot find somebody to meet the costs of putting all this on CD (which surely cannot be *that* high), then for goodness sake let us pay for it ourselves. Once again, the more people involved the less of a burden it is to any individual. I will reach for my wallet if everyone else will reach for theirs."
[End of letter from Paul Whitehouse]

[Collegial postscript. Preliminary calculations suggest that we could get an astonishing amount on *CD-ROM.* Follow me through this. Say there are 5000 languages to get together. Add significant dialects and reconstructed proto-languages, say 1000 more. Total = 6000 languages or varieties. Say we aim at just a good Swadesh 200 list per language. For starters. Say that the attributes of my own data on my old Thor data base are a guide. It consumes about 490 bytes per word on average to enter the word in question (e.g., all, animal, ashes, etc.) as an entity complete with the name of its language, the source of the data, general category of its meaning, general and specific categories of its morphology and syntax (e.g., verb, imperative; noun suffix, plural; etc.), and its initial phone. Other remarks and phonetic details (e.g., [xórošo] contains [-r-] [-š-] and [-o], etc.) can be put in with the word itself. (These data are set up to be manipulated. E.g., find all the words for "nose" with initial [s]; find all the 1st person singular pronouns; etc.) Then we just multiply 6000 x 200 x 490 and we get 588 million bytes needed to do Swadesh 200 list 6000 times. That 588 million bytes ⇒ 588 megabytes. It can go easily on one CD-ROM! And with cooperation from your computer — it can be manipulated. Think about it comparativists, computer freaks and problem solvers. Write to one of us! Hal]

Afterthought. It seems good for everyone but for those who do not want to do the whole world — say they only want to do Niger-Congo — the CD-Rom would include all the Niger-Congo languages and in a form where they could be examined collectively. Also it would not be a necessary disadvantage to have only Swadesh basic vocabulary lists at hand. One could always increase the individual data sets into general lexicon or comparative morphology, depending on the disks. One might also convene a group of colleagues and make another CD-Rom with huge dictionaries of a selected bunch of languages. Also an advantage for those who live away from big universities.

End Notes

1. No bibliography is given because those writings are well-known. As are those of Oswalt and Bender. Of late, biologists have been getting in on the act; one reviewer of *HGHG* declared that language was so volatile that it was useless as an instrument of prehistory. Yet *HGHG* itself complained about the inconstancy of language. So what is one man's volatility is another's mutation rate! We should all go back to the Great Chain of Being, where differences are created by gods and evolution doesn't happen?

PROTO-AMERIND *QET^{S}' 'LEFT (HAND)'

MERRITT RUHLEN
Palo Alto, California

In *Language in the Americas*, Joseph Greenberg gave etymologies for 'left hand' in four Amerind subgroups (Almosan, Hokan, Andean, and Macro-Ge), but he proposed no general Amerind term for this concept. I have suggested (Ruhlen 1994:169) that two of Greenberg's etymologies — those for Almosan and Macro-Ge — represent reflexes of the same root and should be combined in a single etymology. Since Almosan and Macro-Ge are found at the northernmost and southernmost extremities of the Amerind territory — in northern North America and eastern South America, respectively —, it would seem likely that this particular root existed in the original Proto-Amerind language. Furthermore, the fact that this root is found at the extremities of Amerind territory suggests that perhaps other reflexes might be found in branches located between these two extremes. In the present paper. I would like, first, to review the evidence presented by Greenberg, and, second, to provide additional evidence from other branches of Amerind.

Greenberg's original Almosan-Keresiouan etymology for 'left' compared Yurok (Algic) *kes(-omewet),* Nootka (Wakashan) *qat^{s}-* 'on the left,' and Proto-Salish **t^{s}'iq^{w}-*, the latter form being a metathesized version of the root. All of these forms belong to the Almosan branch of Almosan-Keresiouan. Elsewhere, in Wakashan, one might add Nitinat *qat^{s}ās*; in Algic, perhaps Blackfoot *a:ksisauoxtsi* 'at the left' is cognate; and in the Chemakuan family, one might add Chemakum *k^{w}it^{s}aqu* and Quileute *t^{s}'uq^{w}*, the latter a metathesized version of the root similar to the Proto-Salish reconstruction. Furthermore, one should note that, while the Proto-Salish reconstruction, **t^{s}'iq^{w}-*, is supported by such Salish forms as Twana *t^{s}'iq^{w}a(-či)*, Shuswap *t^{s}'ək^{w}eʔ-ekeʔ*, Lower Fraser *st^{s}'ik^{w}a*, Colville *s-k-t^{s}'ik^{w}-aʔ*, Straits Salish *t^{s}'ək^{w}əʔiw's*, Upper Chehalis *t^{s}'íwq'*, Nootsack *θik^{w}a*, Lkungen *t^{s}'k^{w}aʔiwʔs,* and Coeur d'Alene *t^{s}'ik^{w}-eʔ*, Tillamook *yqači* appears to have preserved an unmetathesized variant. Though this is the only Salish language with an unmetathesized variant of which I am aware, outgroup comparison suggests that it, not the more common metathesized version, is the direct reflex of the original Proto-Salish word, which would then be **q^{w}it^{s}'-*. Greenberg's Macro-Ge etymology for 'left' compared Proto-Ge **-ket^{s}* with Mashakalí *-t^{s}at^{s}*, a comparison originally made by Irvine Davis (1968:43).

Additional evidence for this root can, however, be found in the Keresiouan, Hokan, Penutian, Chibchan, Andean, Macro-Tucanoan, and Equatorial branches of Amerind. Let us begin with Keresiouan, which is genetically closest to Almosan. In the Siouan family, one finds Hidatsa *ida-kiša* (cf. *ada* 'arm') and Biloxi *ḳaskani.* In Yuchi, the language genetically closest to the Siouan family, we find *kaš'ó.* Finally, Cherokee, which constitutes the Southern branch of Iroquoian, has *agasgani.* This form is virtually identical with the Biloxi form given above (*ḳaskani*) and also with Creek (Gulf Penutian) *kaskaná.* Mary Haas (1968: 82) noted the resemblance between the Biloxi and Creek forms and suggested borrowing as the explanation but was not able to determine in which direction. I suspect that the source was originally Cherokee since in this language the word for 'arm' is *kanoge,* while in Creek and Biloxi the second element in *kas-kani* has no identifiable meaning. An additional reflex of this root may occur in the Northern branch of Iroquoian in Onondaga *sketsíuka* 'left hand.'

For Hokan, Terrence Kaufman (1988:162) has reconstructed Proto-Hokan **Kisár-iK* 'left(handed)', where *K* represents either *q* or *k.* Kaufman gives no supporting evidnce for his reconstruction, but the root is general in the Yuman branch of Hokan, for which Alan Wares (1968) has reconstructed Proto-Yuman **kəsár* to account for forms such as Cocopa *kuṣar,* Maricopa *kusar,* Mohave *kuθar,* Walapai *kaθara,* Havasupai *kiθarika,* Yavapai *kθari,* Tipai *ksark,* Diegueño *kesárk,* Campo *ksa·rk,* and Akwa'ala *kasárk.* In northern California, perhaps Atsugewi *kawkosūʔkə* is related.

Penutian examples of the root in question include Morris Swadesh's reconstruction (Swadesh 1946:128) of Proto-Atakapa-Chitimacha **keʔs,* which accounts for Atakapa *ket^{s} ~ kut^{s}* and Chitimacha *ki·s.* Pamela Munro (1994:183) compares these Atakapa and Chitimacha forms with Tunica *ʔokešta* 'the left hand' and Yuki *me-kač'* 'left hand.' Karl-Heinz Gursky (1968:33) compared Swadesh's reconstruction with the Yuman reconstruction given above. In California Penutian, Lake Miwok *k'eṣíiliwa* 'left (direction)' and *k'eṣíili* 'to be left-handed' are probably cognate with the Gulf forms just examined, and perhaps also, in Oregon Penutian, Coos *a'qčíčunī.* Within the Mayan family, a branch of Mexican Penutian, Kaufman (1972) has reconstructed Proto-Tzeltal-Tzotzil **k'ešam* 'left hand,' a form strikingly similar to the Lake Miwok form. In the Chibchan branch of Amerind, there is Manare *kut^{s}(-maya)* 'left (hand),' and possibly Rama *kwik sak* 'left hand' (cf. *kwik baing* 'right hand'), which is strikingly similar to Chemakum *k^{w}it^{s}aqu,* cited above.

There are two additional cognate forms that have not previously been connected — either with each other or with the other forms for 'left hand.' These forms are Zuni *wešikkʔa* 'left hand' and Proto-Algonquian **-waašika,* a numeral suffix attached to numerals counted on the left hand by the Proto-Algonquians. In Ruhlen (1995), I have provided evidence that both of these forms contain two Proto-Amerind morphemes, **waši* 'hand' and **qet^s'* 'left hand.' The first morpheme enjoys a broad distribution in Amerind languages of North and South America; the second morpheme is the subject of the present paper.

South American examples are more sparse, but this may simply reflect the better documentation of North American languages. In the Andean branch of Amerind, we find Pehuelche *kesna*, and three forms which resemble the metathesized Salish forms discussed above: Jaqaru *t^s'iqa*, Aymara *č'eqaxar,* and Quechua *it^{s}ox*. In the Macro-Tucanoan branch, Papury *t^{s}oχ* could be a borrowing from Quechua, but in Tucanoan proper, Yabamasa *gat^{s}o-t^{s}atɨa* 'left hand' has apparently preserved the root in question (cf. *ríod^{z}oa-t^{s}atɨa* 'right hand'). In the Equatorial branch, Timote *kučumiya* is almost identical to the Manare form (*kut^s[-maya]*).

The evidence adduced above seems to me sufficient to suggest, with a high degree of probability, that Proto-Amerind, the language of the first immigrants to the Americas, possessed a word for 'left (hand)' that we might, as a first approximation, reconstruct as **qet^s'*. In two typologically similar, but no doubt historically independent, circumstances, metathesis has reversed the order of the consonants to *t^s'eq*. In a word for 'left hand,' the use of metathesis to disguise a word associated with a taboo subject is not surprising and is perhaps one instance of a socially-motivated sound change. In most cases, metathesis has, rather, a phonological basis (e.g., English 'wasp' derives from an earlier 'waps' by metathesis). The Salish, Chemakuan, Yuman, and Atakapa reflexes suggest that the Proto-Amerind initial consonant may have been labialized, so that a more accurate reconstruction might be **q^{w}et^s'*.

REFERENCES

Davis, Irvine. 1968. "Some Macro-Ge Relationships," *International Journal of American Linguistics* 34:42-47.

Greenberg, Joseph H. 1987. *Language in the Americas.* Stanford: Stanford University Press.

Gursky, Karl-Heinz. 1968. "Gulf and Hokan-Subtiaban: New Lexical Parallels," *International Journal of American Linguistics* 34:21-41.

Haas, Mary R. 1968. "The Last Words of Biloxi," *International Journal of American Linguistics* 34:77-84.

Huber, Randall Q., and Robert B. Reed, eds. 1992. *Vocabulario Comparativo: Palabras Selectas de Lenguas Indígenas de Colombia.* Santafé de Bogotá: Instituto Lingüístico de Verano.

Kaufman, Terrence. 1972. *Proto-Tzeltal-Tzotzil.* Mexico City: Universidad Nacional Autónoma de México.

Kaufman, Terrence. 1988. "A Research Program for Reconstructing Proto-Hokan: First Groupings," in *Papers from the 1988 Hokan-Penutian Languages Workshop,* ed. by Scott DeLancey, Eugene, Department of Linguistics, University of Oregon, 50-168.

Munro, Pamela. 1994. "Gulf and Yuki-Gulf," *Anthropological Linguistics* 36:125-222.

Payne, David L. 1991. "A Classification of Maipuran (Arawakan) Languages Based on Shared Lexical Retentions," in *Handbook of Amazonian Languages,* Vol. 3, ed. by Desmond C. Derbyshire and Geoffrey K. Pullum, Berlin, Mouton de Gruyter, 355-499.

Ruhlen, Merritt. 1994. "Additional Amerind Etymologies," in *On the Origin of Languages: Studies in Linguistic Taxonomy*. Stanford, Stanford University Press, 156-82.

Ruhlen, Merritt. 1995. "A Note on Proto-Algonquian Numerals," submitted to *International Journal of American Linguistics.*

Swadesh, Morris. 1946. "Phonologic Formulas for Atakapa-Chitimacha," *International Journal of American Linguistics* 12:113-32.

Wares, Alan C. 1968. *A Comparative Study of Yuman Consonantism.* The Hague: Mouton.

ARAPAHO, BLACKFOOT, AND BASQUE: A "SNOW" JOB

MARC PICARD
Concordia University, Montréal, Québec

It has recently been proposed by Schuhmacher (1993) that, since Arapaho (A) and Blackfoot (B), two Western Algonkian languages, have such different words for "snow", they might have different etymons, with the Arapaho word stemming from a putative Dene-Caucasian proto-form. In the following, I will try to show that both forms can be easily derived from the same Proto-Algonkian (PA) reconstruction, and that Schuhmacher's assumption is fanciful at best.

The PA form for "snow", as reconstructed by Siebert (1975), is **koona*, and it is obviously the source of B /kóni/ (cf. Prouix 1989:67). Much less obvious, however, is the correspondence between **koona* and A /hîîí/ (and not *hí* as Schuhmacher has it), though it is easy to show that it is wholly the product of regular sound change. First of all, although Schuhmacher is correct in stating that "Arapaho *-h-* < Proto-Algonquian **s* (and **h*)" (1993:37), this is only part of the story. Since we are dealing with word-initial /h/, it is important to know that, on the one hand, PA **s* yields A /n/ in this environment (cf. Picard 1994b), e.g.:

*siipiiwi "river"	*sakimeewa "fly"
niičíí	nó úbee

and that, on the other hand, A /#h/ stems from the insertion of this segment prevocalically, as in:

*ałi "arrow"	*ołakeši "intestine"
hóθ	hiθó óx

Moreover, Arapaho has undergone a general process of /k/-deletion, as shown in "fly" and "intestine" above, and a glottal fricative also appears in originally **k*-initial words since /k/-deletion feeds or, in other words, occurred before, /h/-insertion, e.g.:

*kaakiwa "crow"	*keʔtaapiči "canine"
h óu u	h éétookúθ

Another regular sound change which is pertinent to the case at hand is the fronting of high back vowels — traditionally written **o* — and glides, as shown below:

*kiišokooli "days"	*papikwanali "guns"
h iisí ií	kokú yono

Finally, we have the deletion of final vowels followed by that of secondarily-final nasal consonants:

*ohtehkoni "branch"	*metehkomi "louse"
hi té i	beté i

At this point, we have accounted for all the segmental correspondences between PA **koona* and A /hîîí/ save one, namely the "extra long" vowel in Arapaho. In Picard 1994a, it was proposed that A "snow" iṣ in fact a reflex of **okoona*, which would also explain Atsina /ʔíii/ (cf. Taylor 1967). More specifically, we have:

ARAPAHO	ATSINA
*okoona	*okoona
hí ìí	ʔí ii

Note that such a prothetic vowel in "snow" appears in other Algonkian languages as well, e.g., Fox /akooni/, Eastern Ojibwa (Odawa) /akoon/ (cf. Siebert 1975:387-8), and, more significantly, Kickapoo /okoona/ (cf. Voorhis 1988). In fact, as pointed out by David Costa (personal communication), "the real prothetic vowel is /o/, since Fox often changes initial /o/ to /a/, especially on nouns, and since 'eastern Ojibwa' . . . cannot be trusted for initial vowels that are now deleted in the dialect anyway".

In sum, any attempt to link the Arapaho form for "snow" with Basque *(in-)tzig(ar)* "frost" which, along with various Sino-Tibetan cognates for "cold" like Garo *-ćik-*, Lushei *śik*, and Kachin *-si*, supposedly reflects Dene-Caucasian **čɨG-(Vr)*, is purely illusory. As for Schuhmacher's suggestion that, as an Amerindianist and Bascologist working in the first half of the twentieth century. C.C. Uhlenbeck somehow missed the boat in failing to see such a connection because "his time had not come..." (1993:37), it would seem that, on the contrary, this was actually very much to his credit.

REFERENCES

Picard, Marc. 1994a. *Principles and Methods in Historical Phonology: From Proto-Algonkian to Arapaho.* Montréal: McGill-Queen's University Press.

Picard, Marc. 1994b. "On the Evolution of PA **s* to Arapaho /n/". *International Journal of American Linguistics* 60:295-9.

Prouix, Paul. 1989. "A Sketch of Blackfoot Historical Phonology". *InteMational JouMal of American Linguistics* 55:43-82.

Schuhmacher, W. Wilfried. 1993. "C.C. Uhlenbeck and Dene-Caucasian". *Mother Tongue* 20:36-7.

Siebert, Frank T., Jr. 1975. "Resurrecting Virginia Algonquian from the Dead: The Reconstituted and Historical Phonology of Powhatan". *Studies in SoutheasteM Indian Languages,* ed. by James M. Crawford. Athens: University of Georgia Press.

Taylor, Allan R. 1967. "Some Observations on a Comparative Arapaho-Atsina Lexicon". *Bulletin* 214. Ottawa: National Museum of Canada.

Voorhis, Paul. 1988. *Kickapoo Vocabulary.* Winnipeg: Algonquian and Iroquoian Linquistics.

WORLD ARCHAEOLOGICAL CONGRESS 3
New Delhi, India, 4-11 December 1994

MAJOR THEME 3: LANGUAGE, ANTHROPOLOGY, AND ARCHAEOLOGY

Summary by ROGER BLENCH

1. General

The World Archaeological Congress took place as planned ,and Theme 3 was the best-subscribed theme of the conference, which says something about the changing attitudes of both linguists and archaeologists. This is a strictly personal view of what occurred.

Before digging into the intellectual meat, some comment on the fraught politics of the conference is required. Outsiders such as myself, who were concerned mainly with the academic co-ordination, only learnt in the run-up to the conference that the Indian organizing committee had been hijacked by a number of interest groups whose priority was definitively not either broad coverage of the issues or indeed any sort of academic freedom. This led to the exclusion both of a group rather quaintly called by the Indian press the "Left Historians" as well as Muslims from outside India

Some of you may recall the destruction of the Ayodhya mosque a couple of years ago, at the hands of the BJP, an ultra-nationalist right-wing Hindu party. Instead of this being treated as an act of politically-motivated vandalism, it has been drawn into a debate fueled by various archaeological camps as to whether and what Hindu site might be beneath it. While most of the foreign delegates find the actual issues profoundly uninteresting, an instruction from the WAD Council committee *not* to discuss it was an unacceptable limitation on academic freedom at a supposedly international congress.

Unfortunately the local "co-ordinator" for the language theme, S.P. Gupta, was revealed to be a leading light of the Hindu nationalist camp. The effect on the linguistic sessions was that the rich linguistic diversity of India was obscured by an obsessive focus on Indo-Aryan and particularly claims for an Indian homeland for Indo-European. Since none of these papers were illuminated by any acquaintance with the recently-available data on minority Indo-European languages of the region, their contribution to the broader debate was somewhat limited.

These negative aspects should not obscure the fact that New Delhi represented the largest get-together of researchers on the Archaeology/Linguistics interface for many years. Four long days of sessions and much after-hours debate visibly led to fruitful interactions between scholars from all parts of the world and many different types of linguistics. In particular, there was considerable enthusiasm for a further meeting, and I am now actively considering when and how this might be.

2. The Sessions

The papers were divided into two groups, general and methodological, and regional. The main topics in the general sessions were:

1. Dating language spread and change
2. Macrophyla hypotheses and large-scale correlations
3. Origins of language.
4. Oral traditions, myths and archaeology

The regional sessions were:

1. Eurasia
2. Oceania including Australia
3. Sub-Saharan Africa
4. India

3. Comments

Patchy coverage. The regional coverage focused on the four areas headlined above. The New World, S.E. Asia, North Africa, and non-Indo-European India were sparsely represented. Such lacunae tend to reflect the organizers' contacts or lack of them; although persistent attempts to attract Americanists largely failed. The other topic under-represented was DNA/genetics material; this seems to have been largely accidental.

Austronesian. In terms of documentation, internal reconstruction and potential for correlation with archaeological data, the Austronesian phylum is becoming one of the best-known in the world. Unburdened by the unhelpful baggage that weighs down Indo-European, and aided by a progress from island to island that makes the process of language diversification more visible, great strides have been made in both producing a convincing Austronesian "tree" and linking it to linguistic reconstructions and archaeological data. More recently, a version of the "Austric" hypothesis has been revived and is being taken seriously by most of the major players. According to this, Austric (consisting only of Austronesian and Austroasiatic) are related at a coordinate branch level and may both have originated in South China. Expect much more on this in the coming year. Incidentally, it has recently been demonstrated that Daic-Austroasiatic "cognates" are most likely to be loanwords, one reason for excluding Daic from the new Austric.

Macrophyla. Personally, I have a neutral position on most macrophylist arguments, since I am painfully aware how poor documentation is in many regions of the world. However, at the level of conferences, there is a real gap between the enthusiasts for these approaches and those working in greater detail, either in archaeology or with small and more accepted linguistic subgroups. It is frequently asserted that such things are "proved" or "established" and this used as a base for still broader planet-spanning hypotheses. In the case of Afroasiatic, a language phylum with which I have personally have some acquaintance, I was startled to see several papers quoting Afroasiatic reconstructions as if these were established facts. There is a sort of "house of cards" phenomenon, where links between phyla are built on starred forms of questionable validity. What I do think is that we have a long way to go to establish convincing links between macrophyla hypotheses and archaeology. Given the controversy concerning the archaeological interpretation of Indo-European, how much less can we know about other more problematic phyla?

Comments on some regional papers

Elamite. Václav Blazhek has now argued in quite some detail that Elamitic is not, as has been widely argued, an outlier of Dravidian, but is instead related to Afroasiatic. This might be either as a "seventh" branch or alternatively as a co-ordinate branch in South Nostratic. He still accepts there are words of Dravidian origin, although these may be either loanwords or an inheritance from earlier common ancestors. Although the case is not fully accepted, his arguments are certainly strong enough to suggest that this is a hypothesis that has to be taken very seriously. Of course it would help if we had a convincing reconstruction of proto-Dravidian rather than just etymologies.

Bhutan. George van Dreim of the University of Leiden presented the results of his linguistic surveys of Bhutan, which has resulted in the recovery of many unsuspected Sino-Tibetan languages, including some that retain the morphology that has generally disappeared in this phylum. This has resulted in a major rethinking of the internal classification of Sino-Tibetan, in which Chinese is placed right in the center ("Sino-Bodic"), instead of out on a remote and rather doubtful branch.

Philippines. The discovery and then exposure of the Tasaday people as a hoax make world headlines at least among anthropologists. The author of the original book about the Tasaday, John Nance, was fortunately present to hear Laurence Reid talk about his linguistic fieldwork among the Tasaday, which establishes that they are in fact genuine and that the hoax was itself a hoax, mounted for political reasons. Although the Tasaday have now dispersed among neighboring peoples and no long speak the language they were using when "discovered", this can still be recovered and shows intriguing forms not found in neighboring languages but noted from other parts of Austronesian. The data does suggest that the Tasaday are, however, not survivors from an aboriginal stone-tool using culture, but a group that may have fled to the forest relatively recently. Analysis of the data is continuing.

Australia. Nick Evans and Patrick McConvell presented an overview of the Pama-Nyungan problem, looking at possible dating in relation to the expansion of Pama-Nyungan and trying to answer the all-important question; why would one hunter-gatherer group absorb or eliminate so many others over four-fifths of the continent? Although there is some archaeological correlation of the Pama-Nyungan hypothesis in terms of types of stone tools, the strictly economic explanation is now generally discounted. Nick and Patrick are looking for an explanation in terms of social and ritual organization. Hunter-gatherers displacing other hunter-gatherers is not so common in the world, and we would be interested in other examples as well as speculative explanations.

Australia II. Margaret Sharpe presented some striking evidence for the maintenance of oral traditions relating to both landholding patterns and extinct animals that appear to have been maintained over a period of up to 10,000 years. Given the usual skepticism with which we treat oral traditions that go back more than a few centuries, this is a remarkable case.

Nigeria. Chinyere Ohiri-Aniche has recently been surveying the small unclassified Benue-Congo languages close to the Niger-Benue Confluence. Improved data on these languages has not made it possible to assign them to any of the large language groupings of this area (Yoruboid, Edoid, Igboid, etc.), and it is looking as if Benue-Congo has rather a large number of branches with only one or two languages in them. Trying to group them is also made difficult by the fact that this is a region of intense multilingualism, a "language whirlpool", and patterns of isoglosses overlap dramatically. Indeed, the situation looks rather more like parts of Melanesia than Africa.

Ethno-ornithology. Didier Demolin raised an intriguing question in relation to his studies of bird names on the savannah/forest interface in northeastern Zaire. It turns out that onomatopoeic names for birds are strikingly more common in some environments than in others. Has anyone noticed this in other parts of the world? Why should it be so?

New World. Soren Wichmann has recently completed a new reconstruction of Mixe-Zoque and is proposing a revised interpretation of the Olmec script based on his findings. Watch this space.

•••

Roger Blench says: In the last issue [of MT], Hal puts a rather negative gloss on my report on Indo-Pacific, as if the only reason it has not been confirmed is that the Australians are simply conservative who don't want to investigate bold hypotheses. This is simply not true. The Australian academic establishment, together with SIL has made a remarkable contribution to the documentation of the Papuan. Few languages now remain unsurveyed. In addition, several scholars, notably Stefan Wurm and his predecessors, have made substantial efforts to group Papuan languages, which have resulted in a reduced roll-call of phyla and language isolates, but they have not confirmed the Indo-Pacific hypothesis. Indeed, it is fair to say that no-one who has examined the Papuan data in detail has confirmed it. The explanation may be simple. It may not be true.

COMMENT ON ROGER BLENCH'S REPORT ON WORLD ARCHEOLOGICAL CONGRESS

HAL FLEMING

Thanks for the report, nicely written and succinct. Some points are:

1. It is unfortunate when scientific discussion of homelands get involved in or swept up by political movements. Theoretically, every language group in the world (hundreds of them) could take political stands in these matters. We have not been so lucky in this area as one might think, contemplating only India. Prehistory in general is potential fodder for cultural and political dismay in many countries. The USA has plenty, Germany had a period, and then there's China. Und so weiter. Creationism, the kind of ultimate prehistorical theory, is battling Darwinism in the schools in many areas of USA. Just across the Bay from Gloucester, a wild dispute rages over the school board in a New Hampshire town. (We'll discuss the nationalisms now threatening long range comparisons — later.)

2. Van Dreim's research in Bhutan sounds fascinating. If someone can contact him, maybe we can share some of it in MT.

3. Austric yes, but Austro-Thai no? It is ironic that some are saying that Daic isn't really related to Austronesian; just a matter of borrowings, eh? Essentially, hypotheses about borrowing have carried the day in Southeast Asian-istics. Benedict used them to refute Sino-Thai. Then he used them to deny Austric, after once advocating Austric. Now they are using borrowings to undo his Austro-Thai theory too; but re-creating Austric. Stay tuned for the latest battle in the lending / borrowing wars of Southeast Asia. Is this area a triumph for diffusionist analysis?

4. My apologies for not defining my terms properly. Roger had his own reaction to an imprecise general term — conservative. To me linguists who do lots of field work, make detailed analyses, and limited comparisons are neither liberal, conservative, radical, nor reactionary. They are just good working linguists. A conservative linguist is one with a *mind set* that says you cannot go very far with comparisons. Australian linguists *do* have that mind set but, yes of course, they do lots of valuable field work too. If we gave all their field data to Vaclav Blazhek, John Bengtson, and Intina in Moscow, we would have a brand new Indo-Pacific in a few months. It's also not true that no one has examined the Indo-Pacific hypothesis and liked it. Several of us have, including George Grace of Austronesian fame who said so in *Mother Tongue* a while back. Sorry, Roger, we're not giving any ground on this point.

5. I forgot to excuse Geoff O'Grady from the above remarks. Geoff is a liberal Australian linguist. He and I were once talking with an Americanist about a group of native American languages. The latter was obviously an excellent linguist and very intelligent. He was convinced of the hopelessness of the Amerind hypothesis because he had compared several Amerind languages in his area and had been utterly unable to find them related. And he was most definite about it! We asked him *how* he had compared them and he blinked and said two by two — binaristically — and wondered how else anyone could compare them. We suggested that he compare them 4 or 5 at a time, as in "mass comparison". He was genuinely shocked. Never occurred to him. The languages that stymied him so had been classified, maybe even reconstructed, by Swadesh and others eons ago!

ANNOUNCEMENT & CALL FOR PAPERS

SEVENTH ANNUAL UCLA INDO-EUROPEAN CONFERENCE

PROGRAM IN INDO-EUROPEAN STUDIES
UNIVERSITY OF CALIFORNIA, LOS ANGELES

The Seventh Annual UCLA Indo-European Conference will be held on **26-27 May 1995** at the UCLA campus. As in the past, we invite papers on any aspect of Indo-European Studies: linguistics, archaeology, comparative mythology, and culture. Papers on both interdisciplinary and specific topics (e.g., typology, methodology, reconstruction, the relation of Indo-European to other language groups, the interpretation of material culture, etc.) are welcome.

Abstracts should be approximately *two* typewritten pages (double-spaced) and must be received by **10 March 1995**. A period of twenty minutes will be allotted for each paper, followed by a ten-minute discussion period.

Through the generosity of its donors, the Friends and Alumni of Indo-European Studies (FAIES) will offer two prizes for the best papers by a current student or recent Ph.D. (received 1990 or later): one prize for a paper in linguistics, and one for a paper in either archaeology or mythology. Please indicate your current status and year of Ph.D. if you qualify. No previous winners please.

Address all abstracts and inquiries to:

IE Conference Committee
Classics Department
7349 Bunche Hall, UCLA
405 Hilgard Avenue
Los Angeles, CA 90024-1475

e-mail: iepØrmr@mvs.oac.ucla.edu

For further information call: weekdays: (310) 825-4171

CONFERENCE ANNOUNCEMENT AND CALL FOR PAPERS
11TH ANNUAL MEETING OF THE LANGUAGE ORIGINS SOCIETY
JULY 28 — AUGUST 1, 1995
JANUS PANNONIUS UNIVERSITY, PECS, HUNGARY

Papers on all aspects of the origin and evolution of signed and spoken languages are welcome. Since progress in this field of research is best achieved through a multidisciplinary approach, the LOS and the organizers of this meeting are anxious to enlist the combined efforts of all interested scholars, especially those of anthropologists, archaeologists, biologists, cognitive scientists, ethologists, linguists, palaeontologists, and psychologists. A selection of the papers presented at this meeting will be published as soon as possible.

Those who wish to present a paper (LOS membership is not a prerequisite) are kindly requested to send a 300-word abstract to the organizers. Computer users are asked to send their texts in electronic format on 3.5" diskettes preferably processed with WordPerfect or MS-Word. Downloading electronic files is also possible (and indeed preferred), though authors using non-ASCII characters in their texts are advised to send their abstracts on diskettes.

The standard registration fee will be US$ 50.00, the student US$ 25.00.

Special arrangements have been made for lodging and some of the meals:
In the venue of the LOS Meeting:
The total cost for six nights starting July 27 plus breakfast and lunch each day will be:

US$ 310.00 for a single
US$ 215.00 per person for a double

N.B. These prices could vary slightly with the exchange rate.

Tentative plans are being made for a 3-day post-conference tour. The organizers will give out the details as they become available to them.

Correspondence should be addressed to:

Gábor Gyori
Janus Pannonius University
Department of English
Ifjúság útja 6
H-7624 Pécs
HUNGARY

Phone/Fax: (International) 36 72 314-714
E-mail: GYORIG@btk.jpte.hu

The organizers ask that those who can be reached electronically kindly indicate their e-mail addresses in their correspondence.

It is kindly requested that abstracts be in their possession no later than April 15, 1995, and that hotel reservations be made also before that date, by sending, along with the request, a deposit of US$ 50.00 or its equivalent in any other freely convertible currency. This sum should be in the form of a CERTIFIED BANK CHECK payable to Gábor Gyori.

QUICK NOTES

1. Our sibling journal, *Mammoth Trumpet* called *Mammoth Strumpet* by mistake earlier on, reports another nail for the coffin of the archeological Maginot Line — the earliest date for the settling of the New World. Even though the funeral has been delayed for a spell, still the nails are accumulating. This was reported in January in volume 10, Number 1, pp. 1, 4-6, as "Bones of Nebraska Mammoths Imply Early Human Presence". The dates are at least 18 kya. The finds are: "Mammoth bones from these sites reveal patterns of modification researchers believe could only have been caused by humans. Because both sites are in well-studied, primary deposits of Peoria loess that are confidently dated, the investigators are convinced people were there at least 18,000 years ago." Well, since Scotty MacNeish had an old bone with an arrow in it but was still ignored, how could the excavators be so sure of themselves?

   The answer was given by Steven R. Holen (University of Nebraska): "Our interpretations are based primarily on the fracture patterns of the bones... We see evidence of high-velocity impact points." Whatever broke the bone left impact scars that measured approximately four centimeters, added the *Trumpet*. Holen continued "You can tell by the way the impact fractured the bones — the negative bulbs of percussion — that these were hit with something that had to be fairly large and coming at a pretty high rate of speed." He also noted that studies of bone breaking (archeological and ethnographic) show that the bone marrow was the object of the activity. "We know that recent hunter-gatherer groups in Africa broke open elephant bones to extract marrow. We have good reason to think that people utilized marrow in prehistory ... People used marrow — it's a high-energy food." [Editor's note: it can be conceded!]

   Still the *Trumpet* worried about modification of bones by natural processes. "How can Holen be sure the mammoth bones from these two Nebraska sites were not modified by natural processes? Couldn't natural erosion have caused much of the breakage? And what about the teeth of those big Pleistocene carnivores, the massive feet of live mammoths or the hooves of bison? Holen's "most important evidence to the contrary may not be the bone itself but the stratigraphy which has held its secrets for so long. Loess — windblown silt from meltwater of the continental glacier, which would have been no more than a few hundred miles north and east of these sites at the time the mammoths died — covered the bones within a few years. Deposition of the loess began about 20,000 years ago and continued for approximately 10,000 years; by then 12 meters had been deposited on southwestern Nebraska. Both sites are solidly within the loess deposit; the bones have not been moved by running water or erosion." They added elsewhere that mammoths died out about 11,000 BP.

   Holen also found *hair* embedded in clay. He gave it to the Center for the Study of the First Americans (CSFA) to isolate and classify. With luck some of the hairs may be human, even with an mtDNA type!

2. *Expert archeologists evaluate Brazilian "too early" site.* Several archeologists, some with axes of their own to grind, were convened to evaluate the Brazilian site of Pedra Furada. Including Tom Dillehay of Monte Verde fame, James Adovasio of Meadowcroft fame, Scotty MacNeish, Robson Bonnichsen of CSFA, Claude Chauchat (University of Bordeaux), Dena Dincauze (University of Massachusetts), Claude Guérin (University of Claude Bernard of Lyon), David Meltzer (SMU), Fábio Parenti (University of Roma), Jacques Pelegrin (University of Bordeaux), J.C. Petheo (CWU), and Gustavo Politis (CONICET, La Plata, Argentina), the experts ruled the Brazilian finds to be inconclusive at best and mistaken at worst. It depends of which account one reads. The inconclusive finding was published about a year ago in the *Trumpet* but unfortunately not received until last month. The worst was a recent report in NY Times. The fires could have been caused by natural means and the stone tools were too primitive, one report said. We prefer the *Trumpet*'s more cautious tone. Fábio Parenti has written a four-volume dissertation on the site. Along with Guidon, the principal excavator, he was the strongest advocate for accepting dates as early as 40 kya for human artifacts at Pedra Furada. "Doubting archaeologists contend that reasonable scholars can disagree about what are geofacts and what are artifacts, and they suggest that since there can be ambiguity about artifacts from the site's old levels, all specimens must be considered geofacts. Thus, they insist, there is no credible evidence of human presence." We have discussed the vexing problem of "geofacts" before in *Mother Tongue*. Time for us to forget the archeological dates of first humans in the Americas for a while, save one thing. A book about the convention has come out, or is coming out, and we can all read it to see how the two or three viewpoints are argued.

3. *Scotty MacNeish digs in New Mexico.* Scotty and his team were reported to be hard at work at a new site in the Animas region of southwestern New Mexico, about 150 miles (248 km) west of his Pendejo Cave site. Since this report is now old news, the last year may have produced more results. But, since Scotty is quick to call the media, he may not have good archeological luck. It's a factor!

4. *"Fortiter calumniari, aliquia adhaerebit"* By accident some "ancient Latin advice" surfaced recently. Attention Lyle Campbell. Your saying was thought to derive from the folk wisdom of American farmers. But, lo, it was Roman wisdom! Translated roughly as "Throw plenty of dirt and some of it will be sure to stick." Ergo, the advice is a two-edged sword. Either it shows how any search will turn up look-alikes *or* it points the way to destroy someone's good reputation.

5. *Why do few of us take smells seriously?* This is not media trivia but some good questions have been raised by recent research into the olfactory sense and the brain. It turns out that humans recognize 10,000 different smells and that old smells in a person's life are remembered remarkably well and get associated with some of the most important events in a person's life. It has been well-known for many many moons that most mammals smell better than people (heh, heh) and that smell functions are in the more primitive parts of the human brain. Not true in their case, my dogs would say. But the thought also occurs that, outside of chemistry which really smells, these untapped abilities to discriminate 10,000 things could be useful to the rest of science, especially ours. One must also note the amazing lack of lexicon supporting the reportage of smells. (Perhaps we should add taste to this list.)

We don't have names for many smells but we do describe them as being like some thing. Rather like the colors beyond the basic 3 to 5. Long years of interacting with thousands of humans from many parts of the globe cause one to know that different groups of people smell differently, i.e., they have different common odors. There is a Caucasoid smell. (Lord protect me from the outraged methodologists who will snort at that statement!) It has nothing to do with diet and nothing to do with gender. Of course, there are gender differences in smell but many of *those* can be attributed to situational factors. My own nose is a very poor instrument, sensing far less than 10,000 things, but this Caucasoid smell rests on the testimony of African and Asian students at American universities. One African described it as "rancid". Indonesian students have been known to beg for separate living quarters. Und so weiter. Even Caucasoids know this — for example, put four large pink Europeans in a hot jungle and you can smell them quickly enough (to escape).

Our canine companions, Canis familiaris, can tell us from our friends, neighbors and postmen plus what we ate, where we went, and who we met. My nose detects only a small portion of all that but it does also remember that *under similar conditions* most highland Ethiopians cum Somalis give off a different scent from Koman and Nilotic peoples or southern Sudanese in general. The same nose cannot detect any thing about east Asians. Apparently neither American nor African students complain about a Mongoloid smell. But now we have far outrun our data base and the speculation is wild.

The reason for continuing this theme is that the current research mentioned above does suggest that human smells may relate to the immunity system as a whole, or histocompatibility specially. Much of the data potential in the olfactory component has been blocked by the common response that "it is just due to what they eat." People smell like the foods they eat and that is all there is to it. That is the onion and garlic explanation and undoubtedly half true. But some kinds of disease states are detected by doctors with their noses. We will have to check with our local chemist or pharmacist or perhaps perfume connoiseuse. Incidentally a poor olfactory sense would be partial anosmia; complete inability is an-osm-ia (without-smell-state).

The research continues with these people: Susan S. Schiffman (Medical Psychology, Duke University Medical Center), Gary K. Beauchamp and Charles J. Wysocki (Monell Chemical Senses Center, Philadelphia), Richard Axel, Robert Vassar and colleagues at Columbia University College of Physicians and Surgeons, New York, and Linda B. Buck (Harvard Medical School, Boston). Check it out.

6. *The female brain versus the male brain.* There has been quite a bit of research reported this past year, suggesting that the organization and functioning of the human brain varies due to, or with respect to, *gender*. We are not solely concerned with language but language functions have been a big part of what is reported. The gist of it does not seem to be brand new, as they say, but perhaps re-assertions or affirmations of things discovered within the past decade. Women seem to have their language specific brain areas in both hemispheres — at least in part — while men overwhelmingly concentrate theirs in the left hemisphere. There are men, and this is old news, who handle language in their right hemispheres but this is a minority tendency. Left brain language which seems to relate to right-handedness may be genetically determined — or it may not be. Perhaps the female reluctance to put all the eggs in the left basket is genetically determined too — or it may not be. The answers to these alternatives are not presently clear. However, it does occur that, for various reasons, females and some males may use language differently than most males and thus determine the locations of things *by use* rather than by genes. We assure you that we are not advocating anything here but rather pointing out what alternative views are current.

7. Our old friend, Lucy's grandfather or *Australopithecus ramidus*, has been the target of continued research and some recent reporting. Tim White of Berkeley went back to Lucy's old habitat — the Danakil area of Ethiopia — and had the good fortune to bring along an Ethiopian graduate student, Yohannes Haile Sellassie, who promptly found what everyone was looking for — *feet and/or legs*. Remember the reports on *ramidus* in MT-23. The crux of his intermediate status as the real missing link between chimpanzees and humans is the evidence of upright posture and bipedal locomotion. Did he walk? Or did he locomote by knuckle walking? Yohannes found feet embedded in rocks, bone all shattered and difficult to analyze. So the long patient reconstruction in the lab will eventually reveal what kind of feet Lucy's ancestor had. Incidentally, the Leakey family who seem locked in some kind of perpetual competition with White and Johannsen are reported to have found limb bones of the same tribe in the Turkana region of Kenya. It is not yet official, being studied. But it was Mary Leakey's discovery of *afarensis* foot prints in Tanzania (Laetoli) which nailed down the question of bipedalism in Lucy's tribe. So the scholarly competition is fruitful.

A. ramidus may be a different species from Lucy's *A. afarensis* or it may be a different genus. As other paleoanthropologists have thought about *ramidus*, they have suggested that he might just as easily be ancestral to modern chimpanzees or gorillas or both them and humans. Or he is a distinct genus among the early hominoids of 4-5 mya, not necessarily ancestral to us or anybody. His teeth are said to show thin enamel which would contrast with most of the Australopithecines. Nobody should hold their breath or get too worked up over this question, however. This kind of research is like the mills of the gods. We might get some definitive answers before the 3rd millennium AD. Or hire more Ethiopian graduate students and quicken things.

8. *Pre-Islamic Semitic Loan Words in Bengali.* We take pleasure in announcing that Dr. Liny Srinivasan has discovered old Semitic loan words in Bengali (at least) of northeastern India. This is a remarkable find and explanations do not easily spring to mind. Perhaps trade ultimately from Mesopotamia but around India to the south and up the Bay of Bengal could be the reason. But speculation is irrelevant at the moment. Dr. Shrinivasin's article is being peer-reviewed by Cyrus Gordon himself, and their joint conclusion will be presented in the September/October issue of our new journal. Dr. Shrinivasin (University of Pittsburgh) presently lives in Eatontown, New Jersey 07724.

9. *The long-awaited article on Nahali.* A long lexicon and notes on various aspects of the very important Nahali language of central India have been prepared by Professor Norman Zide (University of Chicago). The original paper was a thesis by an Indian colleague, Asha Mundlay, with quite a large lexicon. In addition to being peer-reviewer, Professor Zide has been adding so many notes, particularly about Munda (his major interest), that the enterprise did not make it to this issue (MT-24). One of Zide's problems has been the sociolinguistic aspects of Nahali, which possibly has the world's record for borrowing and theoretically might be a jargon or a bandits' lingua franca. In any case, we promise the Nahali article for our new journal in September/October.

10. Perhaps Lamarck was right after all? about some things anyway? *"There is more to heredity than genes".* Thus spoke two Israeli biologists in the *New York Times*, January 3, 1995. There exist "such convergences of style and behavior" between parents and children in animals (including humans) that they "may be the result of genetic predisposition; or they could stem from children and parents coexisting in a similar environment that prompts the expression of like-minded characteristics." As argued by Eytan Avital and Eva Jablonka (both of David Yelin Teacher's College in Jerusalem), "parents could be masters of so-called phenotypic cloning: they impress their ways of doing things so firmly on their offspring that the behaviors, or phenotypes, practically look inherited in their strength. Indeed the strategy may work beautifully enough that the children will pass the behaviors on to progeny of their own as surely as they do their genes." Saying that the inheritance of these acquired characteristics can radiate rapidly through zoological populations and help explain rapid changes in populations under some conditions, they acknowledge that contemporary biology is overwhelmingly skeptical, if not hostile, to neo-Lamarckism.

    Yet that issue is not as interesting to long rangers as the statements about "group selection" made by David Sloan Wilson (SUNY-Binghamton) and Elliott Sober (University of Wisconsin), as reported in the same article in the *Times*. "Like Lamarckism, group selection has long been a scorned concept among evolutionary biologists, who have insisted that Darwinian forces of natural selection act on individuals competing for their own genetic primacy. Now [they] are trying to inspire researchers to consider that some traits observed among humans and other species may have evolved to benefit groups, rather than the individuals within the group." They also quote John Tyler Bonner (Princeton), saying "Yes, you can pass on social learning from generation to another, and yes, a particular behavior may be successful from the point of view of natural selection." But for a behavior that persists, he said it is almost certain that genes creep in and get involved. (Just hold that thought a moment.)

    Then Wilson said that "the majority of evolutionary biologists believe that standard theories of genetic evolution can explain the world's many riches, including human culture in all its cantankerous diversity. A minority are trying to devise theories that will liberate behavior from genes altogether, particularly when it comes to the evolution of human cultures. 'It's the middle ground that's most poorly explored ... and I'm convinced the middle ground is where it's at'." (Quote within a quote.)

    Why doesn't someone require biologists to take introductory anthropology? As long as these guys are talking about bodies and genes, we are all interested in their work because bodies are at the so-called biological level of explanation. When they start explaining cultures — and languages are included in this framework — in terms of genes, they have moved to a higher level of explanation where "environmental" factors are crucial to understanding the phenomena and genes are not very strong factors in the explanation. The most forceful attempt in 20th century social science to reduce important social phenomena to genes was Chomsky's famous built-in language acquisition device. Even if that theory is true — and some good people think it false —, Chomsky has never said that it included language evolution, particularly the histories of languages, nor that it explained the first language acquired by any human child. At its starkest, the theory will explain *how* a child acquires any language but can say nothing about *what* language any child acquires or its "surface" grammar or its lexicon. Isn't that so, good colleagues in linguistics? We must wake up Alfred L. Kroeber and tell him that Darwinists are attacking his *superorganic*. We can doubt that he'll be surprised.

11. Apropos of the last point, *Phil Lieberman* and colleagues address some of these issues in two articles sent to us in reaction to some of the Quick Notes in MT-23. The first and more recent article has a more direct bearing. The authors Bradley S. Seebach, Nathan Intrator, Phil Lieberman, and Leon N. Cooper wrote in *Proceedings of the Nation Academy of Science, USA*, Vol. 91, pp. 7473-7476, August 1994: "A model of prenatal acquisition of speech parameters". The ABSTRACT says:

"An unsupervised neural network model inductively acquires the ability to distinguish categorically the stop consonants of English, in a manner consistent with prenatal and early postnatal auditory experience, and without reference to any specialized knowledge of linguistic structure or the properties of speech. This argues against the common assumption that linguistic knowledge, and speech perception in particular, cannot be learned and must therefore be innately specified."

And in their first paragraph they explain a bit more:

"Chomsky's view that the 'core' features of human linguistic ability are innate is based in part on his assumption that linguistic knowledge, including the processes of speech perception, cannot be learned and thus must be preprogrammed. As this view is not universally accepted and as there is some evidence of early alteration of phonetic perception by linguistic experience, it appears useful to examine this assumption. In this paper we show that unsupervised neuron learning, as proposed to account for experimental data in visual cortex, can enable learned categorical perception of speech sounds with a reasonable approximation of the prenatal auditory environment. It thus follows that some aspects of early speech perception can be learned and therefore need not be innate."

[Footnote numbers were removed from the text.]

Since the source of this phonetic "fore-knowledge" seems to be the mother's voice reverberating in the child's place in her womb, all kinds of interesting questions are raised. Suppose mother was a Bushman and father a Bantu. Suppose that mother was speechless (mute) but the child was born with normal hearing. Etc.

Team Lieberman's other article has to be read carefully since it seems to be contradicted by the first (and later) article. Writing in the *Journal of Speech and Hearing Research*, vol. 28, 480-486, December 1985, Phil Lieberman with Robert H. Meskill, Mary Chatillon and Helaine Schupack wrote: "Phonetic Speech Perception Deficits in Dyslexia". Their summary of the article is:

"Adult development dyslexics showed deficits in the identification of the vowels of English when the sole acoustic cues were steady-state formant frequency patterns. Deficits in the identification of place of articulation of the English stop-consonants [b], [-d], and [g] in syllable-initial position were also observed. The average vowel error rate was 29%. The average consonantal error rate was 22%. These error rates are significantly different from those of nondyslexic control groups ($p < .01$). No single deficit characterized the entire group of dyslexic subjects. The pattern of errors with respect to place of articulation also varied for different groups of subjects. Three dyslexics have high vowel error rates and low consonantal error rates. The data are consistent with the premise that dyslexic subjects may have different perceptual deficits rather than a general auditory deficit involving the rate at which they can process perceptual information. The clinical histories of the present subjects suggest genetic transmission of these speech perception deficits. The presence of genetic variation in the biological substrate relevant to the perception of human speech should be further explored."

Their final statements speak to a kind of inheritance different from the Chomskyite model: [Footnotes again are ignored.]

"The original observations of Orton (1937) as well as recent chromosomal studies suggest that dyslexia is often genetically transmitted. The clinical records of subjects of these experiments indicate that they fall into this category. They had developmental dyslexia; they had close, genetically related relatives who were also dyslexic (e.g., parents, siblings, or children). The error patterns of these dyslexic subjects, moreover, fall into subgroups that differ with respect to particular discrete phonetic deficiencies — the type of pattern one would expect if human beings were equipped with genetically transmitted neural mechanisms that were the bases for the identification of phonetic features or classes of features."

"Genetic transmission of anatomical mechanisms is quantal and independent. There are no master genes that regulate the transmission of functionally related anatomical structures, like the mandible and maxilla that 'logically' should be under unitary or coordinated genetic regulation. Thus, even for dogs whose lives are literally built about the proper functioning of these anatomical structures, the lower and upper jaws are under independent genetic regulation and individual dogs can have undershot or overshot lower jaws. These principles apply to all genetically transmitted traits that vary in the human population and support the Synthetic Theory of Evolution, which is a synthesis of Darwinian Natural Selection and genetics. The pattern of errors for our dyslexic subjects is thus consistent with two hypotheses:"

"1. Human beings have innate, genetically transmitted neural mechanisms that structure the perception of phonetic features, like place of articulation and the identification of the vowels of human speech.

2. Alletic, genetic variation is present in human populations with respect to these neural mechanisms."

Since the two articles were written a decade apart, they may not be aware of each other, so to speak. It is perhaps a matter of mere verbal clarification to overcome the impression of mutual contradiction? Phil?

12. *Ken Hale* (M.I.T.) has an interesting article on another kind of universal, to wit, *universal fundamental concepts or conceptual primitives* and the *Misumalpan languages* of Central America (Nicaragua, Honduras). His article "Preliminary Observations on Lexical and Semantic Primitives in the Misumalpan Languages of Nicaragua" appeared on pp. 263-283 in Cliff Goddard and Anna Wierzbicka, eds., *Semantic and Lexical Universals*, John Benjamins Publishing Company, Amsterdam/Philadelphia, 1994. This has potential implications for any work done in glottochronology and, of course, semantics.

    Drawing on his many experiences working on Amerind and Australian languages, plus those of others like Ray Jackendoff, Ken concludes that these all "lead me to accept virtually without reservation the notion that there are universal fundamental concepts, or 'conceptual primitives'. I do have reservations about one aspect of the overall program which this short study of Misumalpan attempts to represent. Specifically, I doubt that all languages 'have words for' the conceptual primitives. This in no way challenges the idea of conceptual primitives, since concepts do not have to have names to be real. The 'reality' of the concepts can be determined in other ways. And I do not deny that 'shared words' exist, of course, nor do I deny the importance of determining what those shared words are or the importance of having a semantic metalanguage based on universal semantic primitives."

    "Nevertheless, it is well known that "mismatches" abound in comparative lexical semantics. It is immediately apparent, from the existence of such polysemous terms as English *know*, embracing both the universal KNOW_1 and the derived 'know'_2, for example, that the words of a language are not isomorphic with the universal semantic primitives. Observations of this nature, to my way of thinking at least, cast doubt on the strongest requirement — that is, the isomorphism requirement — on the naming of conceptual universals in the world's languages. It seems to me to be more interesting to yield in relation to the isomorphism requirement and to examine the evidence which remains in favour of the basic idea of conceptual universals." After examining the Misumalpan evidence, he returns for his final conclusion:

    "In summary, I think that a criterion of terminological isomorphy for universal concepts is too strong. While the proposed universality of fundamental concepts might be contradicted by empirical data at some point, it is not contradicted by the well-known fact that it is sometimes difficult or impossible to 'find a word' for some universal concept in a given language."

    One can wonder how one could possibly falsify a universal hypothesis like the one Ken is examining, when its advocates already seem to accept and dismiss negative evidence. How do they know these primitive concepts are universal when they don't find them in all languages they have examined, nor have they examined most languages (probably), and they certainly have not probed the psyches of speakers of most of them to see if they have unlabeled concepts either? There are thousands of human languages, n'est-ce pas? Long rangers can be skeptical too!

13. *Homo sapiens and his languages came from tropical Asia.* Such is the burden of a new monograph/book written by long rangers W. Wilfried Schuhmacher (Gadstrup, Denmark) and F. "Bert" Seto (Tokyo), with their colleague Juan R. Francisco. Published by Bochum as Volume 35 in their *Publications in Evolutionary Cultural Semiotics*, printed and distributed by Universitätsverlag Dr. Norbert Brockmeyer of Bochum, the book is entitled *The Emergence of Homo Sapiens and his Languages in Tropical Asia* and costs about 30 DM. We are unable to say more about the book because we do not have a copy. When Bochum or someone can get us a review copy, then we can assign a colleague to review it. Perhaps an expert on Southeast Asia or the Pacific would be best. Well, Herr Koch? Certainly, the apparent thesis of team Schuhmacher's book is one of the two most likely scenarios for the emergence of modern humans.

14. Although this notice will be too late to help anybody, we do announce that the *Sixth Nilo-Saharan Conference* will be held at UCLA on March 27-29, 1995. The theme is Genetic Classification, and the Plenary session on the morning of March 27th will feature four speakers, M. Lionel Bender (SIUC), Christopher Ehret (UCLA), Robert Nicolai (Nice), and Franz Rottland (Bayreuth). We hope that either long ranger Ehret or Rottland can give us a summary of the papers. Franz may also hum a Wagnerian tune. (Sorry about that; Bayreuth is famous for that kind of music.)

15. From a less musical but more industrial place, Frankfurt, *Herrmann Jungraithmayr* has reported on some of his recent research, particularly on proto-Chadic reconstructions. He also included a much fuller obituary on Hans Mukarovsky which we will publish in our first new newsletter this summer. While we would like to say more about them, we'll limit ourselves for the moment to citing just a very few of his reconstructions. These are for the full proto-Chadic (pCh), rather than proto-West Chadic (pWC) or some other branch. Considering Herrmann's expertise, we take his proto-forms very seriously. Thus for "bone" he arrives at /*k's_3 /, where the first is a velar emphatic/ejective and the second an aveolar fricative. This is essentially identical to the ancient Egyptian /k's/, also found in Omotic in nearly identical form /k'us, ʔus/. Thus for "ear/hear" he finds a general pCh root /*km/ which he believes is bi-modal in having /ɬm̥/, or what I would write as /hl-m/, as its alternative, given, for example, forms such as /súmo/ in Tubu. I think he is mistaken there, as the /hl-m/ forms seem to relate to external Afrasian forms like /sm'/ in Semitic and constituted one of Greenberg's original etymologies in setting up Chadic in the first place. Also /k'am-/ for "ear/hear" is dominant in Somotic.

Herrmann also chides me (Hal) for calling German colleagues "Herr Doktor Professor", while referring to myself as "Harold". "You are surely more than anybody else a Professor Doktor! There is no need at all to perpetuate — in an international context — a silly German tradition." Well, it is true that some people care a great deal about their titles — it's rather like counting coup among Plains Indians — and can be vitally offended if their titles are neglected. But I admit I was "having you on a bit", as the Brits say. But I'm just following my own culture into its own silliness — Americans want to call their President "Bill" and their local barber the same. A highly stratified society denying it all by egalitarian person names! Contemporary Germany seems much less stratified but not its titles. Fascinating!

16. *Mary Ritchie Key* has written a valuable — no, make that invaluable (heh heh) — history of hypotheses of a long range nature. Malheureusement, she gave it to another journal. We have not seen it yet but we hope to get her to allow us to reprint it, perhaps in the newsletter. If you all will recall Ruhlen's *Guide*, remember being struck by how many times someone had proposed a relationship before the present one (whatever it was) got accepted? To twist Lyle Campbell's famous saying: "people keep throwing mud at the same walls and some of it keeps sticking in the same places." One of the best places for a field linguist to lose a little arrogance is Africa, where you can discuss language relationships with the old folks (usually under a tree) and find that *they* already know the obvious relationships.

17. We close with a remarkable observation by a senior colleague who has not given us permission to use his name. At one point in his review of our activities and attitudes, he says: "On a more delicate issue, MT at times reflects a fortress mentality. For example, statements are made that suggest publications in other organs somehow carry more prestige than those in MT, not by their substance but rather by the locale of their appearance. Hoping to discuss the matter fairly, and not be negative to anyone involved, I would like to suggest that you take a look at the first article in *Language* of this year [1994]: 'The prosodic basis of the Tiberian Hebrew system of accents.' More than fifty pages long, it takes up a tenth of this year's space. I cite briefly its conclusions: 'the Tiberian Hebrew system of accents is best understood as a prosodic representation. — there is reason to suppose that the system described above is based on a careful transcription of a tradition of formal recitation.' The article was approved by Halle, McCarthy and 14 others."

"After reading it I turned to my Gesenius grammar printed in English about 150 years ago. In section 15 it deals with accents. A few excerpts: 'The design of the *accents* in general is, to show the rhythmical members of the verses in the Old Testament Texts ... As signs for the *tone*, they are all perfectly equivalent, for there is but one kind of accent in Hebrew.' In short, the *Language* article comes to a conclusion that was published at least a century and a half ago, probably also much earlier."

"Whether the publication succeeds in getting its author more money or prestige I leave to you. It would seem to me that articles published in MT would have similar benefits for authors."

18. We close on some general thoughts of a senior linguist. We forgot to get his permission to use his name, so we will not use it. Suffice it to say that generally he has the reputation of being a conservative historical linguist but friendly. All below this is quoted from a letter of his.

"You assume that long range research rests on a procedure that focuses on periods ten or more millennia ago, and relies for its data largely on lexical evidence. While concern for early periods of language is not prominent in linguistic study today, there are still many specialists. There are also organs that center on their research, among them *The Journal of Indo-European Studies* in this country and *Historische Sprachforschung* in Europe. The publications in these focus on periods two to five millennia ago, and have relied largely on morphological evidence, with some attention to lexical, phonological, and occasionally syntactic. Both procedures aim at understanding languages spoken in prehistoric periods.

"It may well be true that the second group are skeptical of the results of the first; but the first group has expressed reservations about the publications of the second, saying among other things that the recent work is sterile.

"Contrary to what is implied by members of the first group, those in the second, or at least some of them, are seriously interested in the language situation of ten, twenty or more millennia ago. Typically they assume that the best procedure for determining these is reconstruction of the proto-languages of recognized sub-families and families, and then further reconstruction of selected proto-languages from the data reconstructed for the more recent proto-languages.

"The two groups then may differ in procedures, but not in aims. It's unfortunate if one group pounces on the other."

[Today's pouncer was yesterday's pouncee. They started it. — HF]

"Statements in MT have been made to counter criticisms of procedures of the first group. Among these are items in Ruhlen's review of Nichol's book. I haven't read the book recently, nor taken the time to check his objections. When however he states that 'cognate words ...have allegedly disappeared after 8000 years' he implies that she assumes they have. At a point like this, one would like him to comment on the many tests of Swadesh's glottochronology on known languages, indicating where they were deficient. He might also have noted discussions like Meillet's on Armenian *erkun* 'two', in which Meillet demonstrates how the same proto-form yielded the distinct current reflexes. For not only are words lost, if Swadesh's theory is not disproved, but they also are changed so that they may no longer be recognized as cognate. Proponents of the procedures of the first group would do well to deal with these issues, and to avoid the scarcely convincing critiques of Nichol's undertaking."

[You can lead a horse to water but you can't make him drink. Every single one of his factoids have been refuted in MT and he read them. — HF]

"I might say that I did not adopt Nichol's conclusions and work further with them, but largely because that would require testing her procedures on a large number of additional languages, which would take a fair bit of time. I do however have great admiration for her work, and would not condemn it if its results were not in keeping with those of Greenberg. True scientific procedures would seek to determine where the problems like (sic) [he means: lie — HF].

"Since the work is innovative, it has its difficulties. But these are hardly grounds to disparage it and suggest that anyone concerned lacks the intelligence to learn a few technical terms and thereupon try to understand her procedures.

"In short, one would assume that MT would greet a large undertaking like Nichol's, examine her procedures and conclusions sympathetically, and be pleased that another method has been devised that might achieve some of the goals of the association."

[Poppycock! It was not his ox what was gored. Nichol's thesis is widely reported to be based on the twin assumptions that (a) real classification and reconstruction cannot be achieved after Indo-European time depths and (b) therefore a *non-genetic* method will suffice for all deeper taxonomy. It is similar to telling a Darwinian that natural selection is only true for the early periods of evolution; henceforth, one must be Lamarckian. Why would anyone want to examine carefully the basic assumptions of a genetically sterile model of prehistory? Furthermore the "senior linguist" holds dear the same assumptions that Nichols does. Why wouldn't he be ever so tolerant of her murky message? — Harold C. Fleming, let there be no doubt who said this!]

19. *The New World Neolithic was perhaps much younger than thought.* What is now cool news to archeologists, since it started coming out six years ago, may nevertheless be quite warm news to the rest of us. Gathering together a number of opinions and reports, John Noble Wilford reported in the *New York Times* on March 7, 1995, that paleobotanists had found a way to date the oldest known domesticated corn (Zea maize) in Mexico — qua maize and not by its surroundings. Using *accelerator mass spectrometry* and the assumption that the tiny cobs of maize found at lower levels might have been intrusive, they found that early maize had dates of 4700 years ago instead of 7000, as previously thought. The earliest domesticated beans, *lima beans*, found in the Andes were only about 3500 years old. Other beans, Phaseolus vulgaris, dated to 2400 years ago in the Andes and 2300 in Mexico at Tehuacán. While an intensified search for earlier "proto-maize" can be undertaken in the areas of *teosinte*, the wild relative of maize known to exist in highland Mexico, a valid contemporary conclusion might be that agriculture is nearly 4000 years later in Mexico than in Palestine. This was actually what they thought in an earlier generation. Moreover, a major difference between the New and Old Worlds in their agricultural revolutions has been that the role of sedentary living seemed to be opposite in the two regions. In the Levant, sedentary living came first. In Mexico, it was thought to have been second, i.e., that hunters domesticated maize and then settled down. Now that issue is up in the air again, with reports coming in from Louisiana and elsewhere of native American hunter-gatherers living a sedentary life. Unfortunately, it had to be Scotty MacNeish's dates for first maize that have been overturned!

A VALEDICTION OF SORTS: AGE GROUPS, JINGOISTS, AND STUFF

HAL FLEMING

It is also a sort of editorial but one of farewell. I am not yet passing over into Eternity, as our friend Igor puts it, but my future contribution to *Mother Tongue* will be quite limited. My baby has grown up and joined a group of friendly adults, so to speak.

But there are some observations to share about two or three cultural aspects of our science and an ominous social cloud on the horizon.

The first is about young guys, old guys, and the crucial age-grade in between. These are not real age grades but rather demographic aspects of international science, especially the sciences cooperating in the "Emerging Synthesis". (We will have to find a more appropriate label for Renfrew's felicitous initial term.)

Since ours are voluntary associations, they are age-graded only in the sense that scholars begin young, move through discernible stages, and eventually retire (from the science). It seems to me that, if you look at the whole science as a culture with goals, then different age-grades contribute to different goals of the common enterprise. This is all *by and large*, of course; some individuals are utterly different from the whole progression.

The age-grades that I have noticed in some 48 years as an academic, starting my freshman year in college, will mostly hold true for the English-speaking world. I think Germany and France have perhaps greater gaps between junior and senior grades in general than the Anglos do. This may be true for Japan and Italy too. In a global perspective, the Anglo world has perhaps less respect for elders in general than most cultures; the USA possibly worships the young and the new most of all.

Roughly these age-grades seem to exist. They are not equal in time span but differ in cultural qualities — their raison d'être. (Forgive my use of East African age-grading terms!)

Junior warriors. Undergraduates, non-academics learning the ropes, fresh minds encountering the knowledge and paradigms of the science. The second most creative group, the most eager for new ideas, most prone to enthusiasm for what the science does. The most open and frankest minds. The least terrified by the "standards" of the "profession", but, alas, the most likely to fly away.

Senior warriors. Graduate students and non-tenured faculty. In non-academics, these are the ones trying to "get accepted". Easily the most anxious group of scholars, the most afraid of rocking boats; they have imbibed the professional crap and want to be very very sure that everyone knows they are high quality people. But they have the most intense knowledge of some aspects of the science and often have the best total grasp of the science as it exists in their time slot. All become hyper-specialized for a while and almost all write a book, called a dissertation. Publishing in "proper" journals, especially peer-reviewed ones, using the most technical language and math if possible, and above all seeking to appear as superbly competent "professionals". Many qualify as what the Americans call "yuppies". Yet many senior warriors have the time and desire to examine their fields thoroughly. Many do original thinking, some of it profound, which tends to set their style for much of the rest of their careers. This age-grade may last a long time — 10 to 20 years — and actually embrace the bulk of the serious scientific work some individuals do in their entire career.

Nevertheless, the senior warrior grade has a cultural penchant. These are the people who are under the microscope all the time, constantly being judged — for admission to programs, for fellowships, for teaching or research jobs, for promotion to higher grades, and the like. These are the people who have to "make a mark on the world". They manifest ambition under pressure. Out of this crucible comes great creativity at times, but always within the grooves set up by some higher authorities — in the name of the high standards of the field. Many senior warriors find it easiest to attack the enemies of their professors or make a name by demolishing the work of some senior scholar (outside of their university).

Senior warriors often have "feeding frenzies" as when groups of them find a weakness in some current hypothesis, and they all tear it to bits. Yet others can take a "hot idea" and run with it and develop its potential very rapidly. In brief, they tend to be "natural selection" type forces, clearing away dead wood and weak theories and enthusiastically pushing the frontiers outwards in new directions. The most amazing event in this capacity, that I know of, was the rapid advance, nay explosion, of transformational grammar in the 1960s and 1970s. In social anthropology, this aptitude seems most often to lead to fad after fad, changing like the fashions in women's clothing. Other examples will occur to you.

But the last thing to say about the senior warriors is that they are a conservative force of some magnitude too. Not only in demolishing hypotheses in feeding frenzies but also in not joining in on things that are not to their immediate advantage. Senior warriors cannot waste too much time and effort on labors which are not published or rewarded academically. Au contraire, they can spend some energy attacking *if* they get recognition for it. Lots of young Americanists have been having feeding frenzies over the Greenberg hypothesis, motivated perhaps less by purely scientific concerns as by hopes of getting the approval of their senior professors and colleagues. Indeed, sometimes it looked as if those austere fellows had urged their juniors on (sic 'em, lad!). When it seemed as if the whole Establishment was ridding itself of a plague, when even the supposedly "open" and dominant journals printed few words to the contrary, then bright young scholars had a noble cause in which they could be heroes and get some recognition. Attack the dissidents and get ahead in life.

Looking at them in terms of natural selection, one may conclude that all the Angst of the senior warriors weeds out the dullards and chops up the poor ideas and thus is good for science. Or you may conclude that all the Angst fosters conformity and servility, which is not at all good for science. One might propose that giving more security to senior warriors could be fruitful.

The Managers. It doesn't seem useful to divide this grade into junior and senior grades. Basically, it is one period, a long one, of relative security and prosperity. The managers are tenured faculty or researchers whose positions are fairly secure because their reputations are established. The managers represent the great fly wheel of most sciences. They judge the juniors, they control the journals, convene the conferences, constitute the peers in peer reviews, run the departments, do the longer term planning, define what the discipline is all about, defend the paradigm, write the recommendations, and try not to be caught harassing students and staff sexually.

Membership in this grade is a privilege, an honor, and an opportunity. Much of the societal rationalizing of academia centers on the value of having senior scholars, secure and free, to produce new ideas and to advocate things which may not be popular but which might be good for society at large. Or for a science.

Yes, smugness and preoccupation with administrative trivia come along too. Many managers become dead wood; all of us know plenty of them. They pontificate at length about matters they haven't thought about in years. Except for the fierce demands of modern competitive economies (or is it societies?), managers may be secure without much Angst or even much effort.

Although proof of any of this is beyond our scope, I argue that most managers are pretty good scientists too. They tend to be productive too, but, oddly enough, they have less time for thinking and research than most of the other grades. They have to teach, maybe administer something, serve on all those bloody committees, do all that busy work and (most frequently) participate actively in a family and/or community. If the manager is female and married, then she will work as hard as anyone else, secure or not.

But this above all is what the manager grade is all about — they are the great central engine of the science. They really run it. When they want to change a paradigm, it gets changed. And vice versa. And mostly they must be persuaded, since they are usually too busy to join a feeding frenzy or to change their mental clothes. But when they do decide to go with a new thing, fad or real scientific advance, it is apt to fly.

Maybe gyroscope is a better analogy. Managers often have a strong feeling about what the field is supposed to be doing or ought to be doing. That is sort of a cultural "true north" setting.

For the study of human origins, including language evolution, we are actually lucky in one respect and unlucky in two others. The good luck is that managers of biogenetics *and* paleoanthropology are hot on the human origins question. A powerful force that! The bad luck is that the managers of archeology point more in the direction of technology and ecology than culture history. This is the time of the man in the white coat in his lab. More bad luck in linguistics: there the managers are preoccupied with theory and how many angels can dance on the head(s) of pin(s). But there is some good luck in this because the conservative historical linguists are not really managing linguistics. They too are a minority, despite all the bull about "mainstreams".

It is perhaps not an accident that the largest and stablest part of ASLIP's membership is comprised of managers. We are not too safe a place for senior warriors to play, while juniors hardly know we exist. (Is that true?) Managers have the time and security hence the patience to listen and appraise and question and listen, etc. When they begin to make up their minds finally, our fate will be sealed — for better or for worse.

The Elders. This category is essentially self-explanatory. Many elders still have university jobs or affiliations. Some retire to comfortable places like Florida or Hawaii, yet work as hard as ever. This category does *not* include the truly retired, the golfers and world traveling vacationers. Their relevance is gone, not because of age, but because they no longer participate in the work of the science. Yet some quite ancient colleagues of ours keep right on working and producing things. So age as such has less to do with the Elder grade than one might expect. Indeed I had a colleague in Pittsburgh who was truly retired while still in the manager grade in his 50s.

The crucial point about the Elders is that they are free — at last — to say what they want to say, do what they want to do, and have plenty of time and security to do it in. Still they never know just how much time they really have, so they tend to hurry up a bit. Of course, they may have lousy pensions, suffer from diseases, be "retired" far from good libraries and labs, be politely snubbed by younger colleagues, and suffer from hardening of the attitudes. Yes, everyone knows I am an Elder too. But *we* may escape some or even all of those disabilities, especially the last. And the creativity and mental resilience of some of ASLIP's elders belies the belief in on-rushing decrepitude expected by our juniors.

Although technically an amply wealthy person could become an Elder at almost any age, Elders do have valuable qualities. They have served their long apprenticeships, helped manage things for years, and carry scores of years of experience, data, and ideas into their dotage. Some Elders are simpletons, of course, while others semi-maniacal advocates of varieties of nonsense. But the capacity for wisdom is present in Elders, just as common cultures tend to say it is, and that has considerable scientific value too.

Just as social anthropology has been impressed with the reciprocal relationships between alternating generations in many societies, especially those with age-grades, our science too can see some of the qualities of the junior warrior in the elder. As they say, "the older the bolder". Some of the freshness of view and passion or enthusiasm of youth returns when the managers are freed of all their responsibilities and become elders. This begets much creativity in hypotheses and ventures, but in people often having enough data in their heads to test theories intuitively and effectively.

Tribalism: Our Eternal and Natural Enemy

Our emerging synthesis is part of a larger phenomenon, an emerging international culture. This has been growing apace for most of this century and before, fueled by awesome advances in transportation and communication. Perhaps one should add migrations too. Absolutely millions of people left their homelands and migrated to the two Americas and Australia. Or across the Russian Empire and around about in China and to refugee camps world wide.

Two savage world wars, many brutal smaller ones, and a massive struggle between the American and Soviet empires, have made the 20th century probably the most violent and brutal in human history. Yet still, this international spirit survives and flourishes, re-fueling itself on the prevailing loathing for war and longing for peace and security. Many millions of people look cynically on their political leaders and hate the very idea of following them into more pain and bloodshed. Americans have been balking at losing troops in tiny wars in Somalia and Haiti, while Russians curse their government for its remorseless pounding of the Chechens (East Caucasic speakers). Europeans agonize over poor Bosnia but hesitate to do much, lest a few of their troops be killed, while the Americans dither and blame the Europeans for dithering. Too much war!

But a powerful human interest, called nationalism, jingoism, nativism, or patriotism and love of country, has been with us for all this time and even before that in some places (France, USA). What is affecting us is not strong enough to be called jingoism; let us call it tribalism, knowing full well that it is not actual tribes who are involved. It is causing us some trouble now in the human origins science. How? What are its roots?

Herodotus said (somewhere) that any man would have to decide that his culture ultimately was the best, or at least preferable to others. Voltaire wrote caustically about nationalism. Sociology and anthropology have studied the general topic in a large variety of ways. Indeed, you might ask why an anthropologist doesn't expect peoples' heads to be organized to a great extent by their cultures, why he doesn't see that folks will tend to adhere to their own against the externals. Hmm? Well, the problem is that fascinating one of the anthropologist *living* in her own culture where, as an actor in the culture/society, she has to make moral decisions, be for or against things. In a word, she has to be a participant, not an observer. Then her ability to *understand* her social milieu is tempered by her desires to see some things happen and not others.

I don't have too much trouble figuring out why there is so much tribalism in the current world. But the problem with participating in an inter-national = inter-tribal culture, viz., human origins science, is that the understandable cannot be tolerated or ignored. Not if it threatens the common pursuit of recognized goals. And tribalism right now breeds conflict and breakdown of communication. We have enough problems as it is without having to cope with tribalism.

Perhaps because my mail tends to be international and aimed at human origins science matters — due to *Mother Tongue* — I hear a lot about tribalism. Those involved, as they come to mind, are Russian, Indian, French, Finnish, Slovak, and American. I must be quick to point out that the overwhelming consensus among ASLIPers is non-tribal, international. It is the exceptions who concern me, and I hope that their fellow tribesmen can talk them out of whatever current unhappiness it is that causes them to damn the outside and insist that only the views of their tribe are correct.

Tribalism can be a great poison. It can provoke anger and retaliation in those at whom it is directed. One small illustration from Somalia. The late and unlamented dictator Siad Barré gave a speech in 1978 that Joe Pia and I were invited to hear. Siad Barré carefully outlined the dangers to Somalia of tribalism. It threatened the nation literally and so it must be eliminated. (See what indeed happened 15 years later!) But, as Abdi Sheikh Abdi pointed out later, Siad Barré himself was a nepotist, he filled the government with people from his own clan and phratry. Small wonder that other Somalis thought him a hypocrite and began to struggle against him for the sake of their own clans and phratries. Voilà, tribalism won.

There is a separate but related matter. It was *not* involved in the above discussion. Let us look at the thing we talked about in the early days of ASLIP — the languages allowed in *Mother Tongue*. Some thought that we were imperialistic because we wrote in English. We did poll our members to see what languages they could read adequately. The vast majority, even of Europeans, had in common only French, German, and English, while practically everybody had some other single language that nobody else knew. It was and is an obvious fact — the scientific world at large communicates in English, French, and German, unless it is a purely local audience that is aimed at. Great scientific work is done in Russian, Chinese, and Japanese; potentially in Spanish, Arabic and, Hindi; but who reads it outside of their own countrymen? A few outsiders. We should be sorry about that but, for heaven's sake, it is a fact of life, not American tribalism.

This is a *practical matter*! Another practical matter is, for example, if Janhunen submits an article in Japanese or Finnish, how will the editor know what it is about or if it is good enough to publish? Right, he will have to send it to a colleague. If the colleague has time, he might translate it into English, after evaluating it. He might do that. But he might also request money for doing all that labor. It would be easier, for example, for Janhunen to submit the article in English in the first place because his English is superb. Perhaps we can get Jerry King to send Janhunen an article in Cherokee to see how he handles that problem?

We did make a mistake in a recent issue, saying that only English was acceptable. There was not space to spell the reasoning out. It is now much simpler just to specify that English, French, and German are languages to write articles in for *Mother Tongue: The Journal*. However, communications to members like letters or announcements — which will appear in the quarterly newsletters — can be in any number of languages, even Finnish. But when members do write in their preferred code, they ought to ask themselves: who will read this, I wonder? Who can read this?

An Ominous Social Cloud: American Nativism

Something similar is happening in Germany, France, and Italy in the form of anti-alien, anti-foreigner, anti-African, anti-immigrant sentiments. Much of the Middle East is doing similar things, only calling it religion — characteristically. In the USA, one can detect a full blown nativistic movement centered around fundamentalist Christians, political conservatives, armed private militias, a resurgent and triumphant South, an anti-abortion movement growing violent, politically explicit reactionaries who call themselves conservatives dominate one political moiety (the Republican Party), and a continuing massive media shift to the right. American colleagues may object to any or all parts of this assessment. Those I have talked to seem to agree with all of the above.

Traditional America, the Bible Belt, Middle America, the Heartland, Real America, and major segments of Corporate America are all involved. While these things are more characteristic of the Mid West and the South than they are of the Northeast or California, they do exist all over the country, especially in the lower middle class. The whole package is very familiar to me; it is the culture I grew up in, but now becoming even more intolerant than it was and getting angrier and buying guns.

Wow, this reminds me of the rise of Nazism in Germany. Many of us always wondered how a great country like Germany could go bananas like it did in the Nazi period. We read German history and talked to Germans and wondered. So much of it had parallels in Italy, France, England, and Spain. We wondered if "it could happen here"; the great Sinclair Lewis wrote a novel showing exactly how it would happen. And now I fear that it *is* happening.

Most of us in the Elder grade get nervous or truly alarmed when conditions called "great depression" or "fascism" are described as imminent. Both of them caused an enormous amount of human suffering. Neither are things that one would like to see repeated in one's life time.

How does this concern long rangers? Well, the modern world cannot do without the physical sciences. Fascism does not threaten them so much. But that is not true of the social sciences, even one with a physical science component in it. As I mentioned before, Darwin is being attacked nowadays —again — in America, just as Indo-European homelands have become politically relevant in India. Thought control in the guise of religion or conservative orthodoxy is a threat to an open international science like ours, or at least to its American component.

Come on, good colleagues, tell me I'm wrong, getting paranoid, or worse — senile. What do you think about all this? Yes, of course, MT is not supposed to be political. And it is not. But this particular matter is about a possible threat to our entire existence. That is permissible, right? Responses will not be published in the Journal, but those that permit it can be seen in the quarterly newsletter. Or just write to me. I'm the one who is spooked.