

**M
O
T
H
E
R**



**T
O
N
G
U
E**

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

ISSUE 14 AUGUST 1991

NEWSLETTER of the ASSOCIATION for the STUDY of LANGUAGE IN PREHISTORY
Editor (August): Harold C. Fleming

ASLIP is a non-profit organization, incorporated under the laws of the Commonwealth of Massachusetts. Its purpose is to encourage and support the study of language in prehistory in all fields and by all means, including research on the early evolution of human language, supporting conferences, setting up a data bank, and publishing a newsletter and/or journal to report these activities.

CONTENTS

OFFICERS AND COUNCIL OF FELLOWS OF ASLIP:
(Address appropriate correspondence to each)

President: Harold C. Fleming
5240 Forbes Avenue
Pittsburgh, PA 15217

Vice Pres: Allan R. Bomhard
73 Phillips Street
Boston, MA 02114

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

FELLOWS:

Raimo Anttila
U/California, Los Angeles (USA)

Aharon Dolgopolsky
University of Haifa (Israel)

Ben Ohiomamhe Elugbe
University of Port Harcourt (Nigeria)

Joseph H. Greenberg
Stanford University (USA)

Carleton Hodge
Indiana University (USA)

Winfred P. Lehmann
University of Texas (USA)

Karl-Heinrich Menges
Döblinger Hauptstrasse 64, Wien (Austria)

Hans Mukarovsky
Inst. für Afrikanistik, U/Wien (Austria)

Vitalij Shevoroshkin
University of Michigan (USA)

Sergei Starostin
Academy of Sciences of the USSR

John Stewart
7 East Barnton Gardens, Edinburgh (Scotland)

PREFACE.

OBITUARIES: Allan Wilson (by Rebecca Cann),
Otto Rössler (by Yoël Arbeitman)

Emmanuel Laroche (by Yoël Arbeitman)

A GREAT AND FRIENDLY DEBATE : Act II

Vaclav Blazek on BASQUE et alter

"NOSTRATIC" AND OTHER LANGUAGE "FAMILIES"
(OR "MACROFAMILIES"). by Eric de Grolier.

AMERIND KINSHIP TERMS and Other Essays:

by Merritt Ruhlen (one with John Bengtson)

!La lucha continua! : the NEWS.

Four recent books:

LOS meetings.

More books

Announcements

Petracek's critique of 'nose'.

LETTERS:

EDITORIAL.

ASLIP BUSINESS.

A TAXONOMIC HYPOTHESIS : BOREAN
by Harold C. Fleming

Annual dues for ASLIP membership and MOTHER TONGUE subscription are US \$10 in all countries except those with currency problems. In those countries the dues are ZERO. All members can help by making donations to defray these costs.

BOARD OF DIRECTORS

M. Lionel Bender, Southern Illinois University,
Carbondale, Illinois 62901.
Sherwin J. Feinhandler, Social Systems Analysts,
Cambridge, Massachusetts 02238.
Frederick Gamst, U/Massachusetts, Harbor Campus,
Boston, Massachusetts 02125
Mark Kaiser, Illinois State University,
Normal, Illinois 61761
Saul Levin, State University of New York,
Binghamton, New York 13901
Daniel McCall, 7 Wigglesworth St.,
Boston, Massachusetts 02120

European distribution: All members living in Europe, the USSR, and Israel will pay their annual dues to, and receive MOTHER TONGUE from:

Professor Dr. Ekkehard Wolff
Seminar für Afrikanische Sprachen und Kulturen,
Universität Hamburg,
Mittelweg 177,
D - 2000 Hamburg 13,
DEUTSCHLAND (Germany)

PREFACE. Some introductory remarks.

"Free at last, free at last! Thank God Almighty! We're free at last!"

Martin Luther King, Jr. August 27, 1963

To the People of Ethiopia & the Soviet Union:
Well done! Way to go! Enjoy it! Guard it!

Congratulations to our Muscovites and our Ethiopian colleagues for courage and survival stamina. Special mention to Herbert S. Lewis for courage above and beyond the requirements of research. Bravo, Herb!

x x x x x x x n o w b a c k t o w o r k x x x x x x

Gy. Hegedüs Irén's bibliography of Nostratic will be presented in parts, starting with MT-15, because the whole exceeds our capacity.

Monsieur Eric de Grolier points out that our differences in taxonomy just may reflect our biases, due to where we start looking and where the easy data are and so forth. Interesting thesis!

Vaclav Blažek continues the amicable debate with John Bengtson over the classification of Basque. New Ongota data save one etymology.

Some of John Bengtson and Merritt Ruhlen's global etymologies have been, as they themselves observe, underpowering. However, one of their very good ones -- the 'milk' etymology -- was rejected for publication by the editor of LANGUAGE supported by the Executive Committee of the LSA, no less. They never said it might be worthwhile for linguists to inspect such a formidable looking etymology. They rejected it because it might not be true. Has anyone ever seen such an attitude in a journal of a scientific society? That is what we are up against, not rational behavior! B & R's 'milk' is shown so that our readers can judge for themselves. Also a beautiful table of Amerind kinship terms which are at least as strong as Sir William Jones's first IE set. Plus a set of D-C (Dene-Caucasic) etymologies to help those starved for the sight of real data in that realm. All courtesy of Merritt Ruhlen.

There is some sad news about some good people. There is also rather exciting news from the biogenetic and archeological fronts. And some interesting letters. Roger Blench has given us a high quality map of African languages, useful in our upcoming tropical endeavours!

Finally, there is a new taxonomic hypothesis, written by yours truly. It may not be true but it is a serious effort. If it is not true, no harm done because we can then examine Alexandra Aihenvald's (with Jean Pierre Angenot). If it turns out to be true, please note that it takes us "half way home". After Borean, one has to cope with the big tropical phyla. Peaceful reading!

OBITUARY

Dr. Allan C. Wilson, Professor of Biochemistry and Molecular Biology at the University of California, Berkeley, died in Seattle on Sunday, 21 July 1991, at the age of 56. He died while undergoing bone-marrow transplantation for leukemia, diagnosed only late last November, at the Fred Hutchinson Memorial Cancer Institute. Allan felt that the brain was much more important than environmental change in driving morphological evolution of species with learning capabilities. He hypothesized that humans had become uncoupled from the demographic and environmental catastrophes that limit the expansion of new species due to the evolution of language. He personally trained more than 200 graduate students and post-doctoral fellows in molecular evolution in his laboratory. He was a Fellow of the Royal Society, received 2 Guggenheim awards, numerous science prizes, a MacArthur Foundation "genius" award, and was a member of the American Academy of Arts and Sciences.

Allan's principle interest in evolution came from his observation that rates of morphological change and rates of mutation seemed to be uncoupled in many lineages. He was widely knowledgeable about the diversity and behaviour of birds, fish, primates, rodents, bacteria, bats, amphibians, ruminants, and insects. How did behaviour drive evolution? What was the true phylogenetic picture that related species to each other, and when did these behavioural changes occur? He was interested in real examples. He had a way of encouraging students to work on projects likely to pay off with the application of new technologies, and a phenomenal memory for who had stored what samples in what freezer.

Allan thought that evolutionary biologists needed a more objective way of measuring rates of change, since they were often misled by convergence due to natural selection. He also thought that they needed a clear phylogenetic perspective for the group under study, because hypothesis testing would be impossible if family relationships among taxa were not known. The fossil record might be incomplete, but that should not stop biologists in the modern world from reconstructing evolutionary history. He felt that in order to really study evolutionary change, it was best to concentrate on genes and proteins of living animals in a simple system, and to make the fewest number of assumptions about how DNA mutated over time. What mattered to him was data, and the name or fame of the laboratory producing the data was always secondary.

Allan's contributions to evolutionary biology include 1) the primate molecular clock, showing humans and African apes shared a common ancestor as short as 5-7 million years ago, in contrast to the 25 million

year divergence once favored by paleoanthropologists; 2) the Lucky Mother hypothesis to account for mtDNA divergence in modern people from an African ancestor who lived about 200,000 years ago; 3) the demonstration that convergence due to natural selection can occur at the molecular level in proteins, as evidenced in the same structure and function of a stomach enzyme (lysozyme) in cows and leaf-eating monkeys; 4) the discovery that the immense adaptive radiation of Hawaiian flies in the genus Drosophila must have taken place before the current Hawaiian islands were formed, leading him to hypothesize that insects island-hopped down the now-submerged seamounts and atolls of the Hawaiian chain as new islands were formed; 5) the demonstration that the survival of DNA in ancient species is common and that it can be sequenced and compared to modern relatives; and 6) a universal framework for measuring rates of morphological evolution in animals, and the observation that morphology appears to change faster in species that utilize observational learning in their social behaviour (birds and primates). This last discovery formed the core of his interest in big brains and language, and the contribution these factors made to increasing the rate of morphological evolution in humans.

Allan wanted to have an statistically robust, universal yardstick for measuring morphological change, because he was struck by how subjective the opinions of anatomists were when describing evolutionary change within and between taxa. He was particularly put off by primate anatomists, who attempted to dazzle their audiences with detail but neglected to deal with real issues of biology, notably, low sample sizes, no real understanding of sexual dimorphism, and the unknown link between genes and anatomical structures. He thought that they hid behind jargon, and constantly attempted to put readers off by speaking in code about particular fossils by number instead of clearly identified terms. Allan spent part of a sabbatical in Kenya in the early 1970's, where he was finally able to see some of the fossil evidence for human evolution first-hand due to the help of Alan Walker, now at Johns Hopkins University.

This experience helped solidify his perception that behaviour must have been changing fast in human evolutionary history, and his interest in the reorganization of the brain as a response to the demands of language grew. He thought that all modern people shared similar language characteristics, because they shared a recent, common origin. He also felt that until there was a real understanding of modern human genetic diversity and its origins, linguists would not be able to make much headway in the problem of when modern languages arose and where. Up until he went to Seattle for further treatment, he was looking forward to future discussions with

Bickerton and others about the links between the expansion of modern humans about 100,000 years ago and the spread of particular language groups.

Allan felt that words had power beyond their obvious communication potential. He knew that words could hurt, and that they could also heal. His first years as a professor at Berkeley had embroiled him in some bitter debates, and he understood the necessity of funding long-shots if they were likely to yield new evidence. When our Lucky Mother hypothesis was subjected to scathing ridicule after the first publication in 1987, he reminded me how sure the anthropological establishment had been that he was wrong too in 1967. Although it seemed impossible to get funding for research and I was almost ready to quit, he encouraged me to keep trying. I am now glad that he had faith in me, and this story could be repeated by a large number of his former students. He used his last years as a MacArthur fellow, when he was released from formal teaching duties, to travel extensively so that he could discuss, explore, and attempt to bridge gaps between biologists and social scientists. His overriding concern was the nature of the evidence that could be gathered, the potential bias in gathering it, the scientist(s) involved, and who would use the data for what eventual purpose.

I knew Allan as an intense, gentle, humble, and humorous individual who packed 28 hours of activity into a 24 hour day. As a teacher, he stressed rigor and elegance in presentation of data. Allan was a math wiz, but he appreciated that concepts were worth talking about too, because you never knew who was likely to have an important insight into a problem. He disliked pretension of any kind, political expediency, and departmental games. Berkeley was not always a comfortable place for him in this regard. Most students began their stay with him by thinking that he talked too simply about evolution, and left his lab understanding how important it is to talk and think clearly about this complicated subject. He promoted the interests of his students with vigilance, and was constantly pressing conference organizers to include women and minorities on their speakers' lists. Gender or race were irrelevant to him, and it was no accident that at one time, his laboratory had almost all the women students enrolled in the Department of Biochemistry at UCB.

As a friend, Allan enjoyed discussions about science with good food and fine wine, Mozart, single malts, sheep jokes, and simple technology. He and Leona, his wife of 32 years, often hosted memorable parties for hungry students and visiting dignitaries. He would promote these by saying, "Come to dinner, you'll learn what it's like to talk to someone who really thinks like a ... ex) bat, monkey, camel, mouse, etc." His family in California

including Leona, his daughter Ruth, and his son David, often tolerated intrusions into their family life by needy, visiting biologists. Allan was a proud citizen of New Zealand, who grew up on a cattle ranch. His family there include his mother Eunice, sister Coleen, and brother Gary. We students were often told to visit New Zealand before it became trendy, and see the one cattle farm in a land of 10 million sheep. Those of us who had the pleasure of working with Allan will always remember his warm smile, and his ready questions, "So, what are you doing lately? Can you stay a minute? I've got a new idea I need to talk over with you."

7 August 1991

Rebecca L. Cann

Assoc. Prof. of Genetics and Molecular Biology

University of Hawaii at Manoa

Honolulu, Hawaii, 96822

There will be a memorial service, to be held on SEPTEMBER 12, 1991, for Allan Wilson. It will be at Berkeley (U/California at Berkeley), exactly where is not yet known. One who wished to attend the service could probably find out by calling the Department of Biochemistry.

There will also be a student scholarship fund set up in his name.

OBITUARY

Prof. Dr. Otto Rössler of Marburg (Lahn), Germany, died on 9 July 1991. He was born on 6 February 1907 and was, thus, 84 years old. A man like Prof. Rössler cannot be just given circumscribing dates and locations for his epitaph. I can think of no better defining word for his position than to say that he stood at the summit of all that was best in "classical" Hamito-Semitic linguistics, and he trained a generation that was to go far beyond him into wild forests he himself could not enter. Words to this effect he wrote me himself also at the time we collaborated on the English translation and slight updating of his magisterial work — in English, "The Structure and Inflexion of the Verb in the Semito-Hamitic Languages", in the *Gedenkschrift* for J. Alexander Kerns ז"ל (1981:679-748). Prof. Rössler's main updatings there consisted of excising some of what he had said on the Chadic languages, because the field had gotten away from him in the strides made, among others, by his disciple, Prof. Dr. Hermann Jungraithmayr, and that integrating the new material would require an entirely new article, one conceived differently. In fnn. 13 and 14 of my 1981 translation, I listed some of Prof. Rössler's most important articles and appended to these a further listing on p. 748 there. I would only wish to point out here that, most likely, the course of time will deem his "Verbalbau und Verbalflexion in den Semito-Hamitischen Sprachen" (the 1950 original of the article I translated and which first appeared in *ZDMG* 100/2:461-514) and his, by far later (1971), "Das Ägyptische als semitische Sprache", in the volume *Christentum am Roten Meer* (I:263-326), in which he established a very stringent and symmetrical manifesto of the Lautgesetz-System obtaining between Egyptian and the Semitic languages proper, as his most important.

I should like to add a few personal notes: in 1965-66, while living in Israel, in the age before photocopies (and washing machines), I sat — as a monk — a certain number of hours per diem and copied out the above-mentioned German original of "Verbalbau und Verbalflexion..." by hand into a small notebook at the Library of the Hebrew University of Jerusalem. To jump some years, in 1978, when I first approached him by post concerning publishing the translation, many months flowed away, and, when his letter of response finally came, I was lying — having just come back from Death's door — in a hospital, where — inter alia — I had had fevers up to 110°. I read the letter my Mother ז"ל brought to the hospital and took it seriously until I got to the Master's request that perhaps we could conduct all future correspondence in Yiddish. Since I can read Yiddish fairly well, but ONLY as a side-prize of knowing Hebrew and German and some Slavic, this request at first registered in my mind, but then I later came to the conclusion that this was all part of a fever-induced delirium. Time and the document show now that he did write this. My last letter from Prof. Rössler came in April 1989 in response to 82nd birthday wishes I had sent him. I should like to cite a single sentence from this delightful letter: "Continually I have a lot of things to do: To prepare for my 'postumous' [sic] paralipomena is the one major activity."

I personally will very much miss this fascinating man who lived on the other side of the ocean. Scholarship, whatever road its young and new researchers will take it, will continue to honor the light shed by this brilliant man, especially his work with the Lybico-Berber and the Beḍaue (Beja) verbs. *Sit ei terra levis!*

Yoël L. Arbeitman
Institute of Semitic Studies
195 Nassau Street (#16)
Princeton, NJ 08542

OBITUARY

To recall the life-work of Prof. Emmanuel Laroche permits one to say, that which, if applied to most of us, would be but wanton hyperbole. But here is an appropriate place to say this: In few, if indeed any, other sciences can it so easily be said that one individual was THE nonpareil in his field. Yet, in Anatolian Comparative Philology, in its ever-expanding divisions and subdivisions, it is easy to pronounce that Prof. Laroche simply had no peer. If he did, he would still be minimally *primus inter pares* — a very few *pares* indeed. Prof. Laroche died on 16 June 1991, at the age of 77.

True, Anatoliology has been blessed with a plethora of major figures who have striven and overcome mighty roadblocks, many of these latter being due to the several ill-adapted writing systems and the hardships they engender for a recognition of the sounds behind them.

During the many years that he ran *Revue hittite et asianique* (RHA) almost single-handedly, he had, so to speak, a "regular column" in it, "Études de vocabulaire", in which he regularly elucidated meaning and/or etymology, primarily of Cuneiform Hittite lexemes and particles. Later on, his major work-area moved primarily into examining the Luwic languages and establishing (for many of us fellow-workers, firmly) the family tree: with Cuneiform Luwian, then with what had been known as "Hieroglyphic Hittite", this latter especially with his volume, used by so many of us, *Les hiéroglyphes hittites* (1960). In equal measure, all who tried their hand at Cuneiform Luwian employed his 1959 *Dictionnaire de la langue louvite*. This modestly entitled volume was actually dictionary, chrestomathy, and outline of grammar all in one — had it been published in German, it most certainly would have been a **Handbuch der luwischen Sprache!* That the later work of Hawkins/Murpurgodavies/Neumann, building on the basis of the discoveries and proposals of Laroche and his contemporary equal, Piero Meriggi, proved that there was no "Hieroglyphic Hittite" language and that these inscriptions, assigned more precise sign-values, were actually in another form of the Luwian language (with Cuneiform Luwian being an older and a Western dialect and Hieroglyphic Luwian being a much later — separated, in the main, by at least 500 years — and an Eastern dialect, used in an area where many refugees of the destruction of the Hittite world ca. 1200 BCE had settled), in no way detracted from its extreme proximity to Cuneiform Luwian.

Yet another part of Laroche's formation of the Luwic family of Indo-Hittite/Indo-European, was his series of articles "Comparaison du louvite et du lycien", in *BSL* 53 (1957-1958), 55 (1960), and 62 (1967). One can say, in part at least, that this area of his work peaked in the preliminary publication of the newly-found "Trilingual Stele of Xanthos", with a long philological analysis by M. Laroche and with the same manner of work done on the Greek and Aramaic texts by eminent scholars in those fields. This work later on culminated in the volume *Feuilles de Xanthos. Tome VI: La stèle de Xanthos*, done by the same three savants.

Just to skim our lamented colleague's areas of work requires mentioning two further areas: his long interest and continuous work with theonyms and anthroponyms of the Anatolian world, as demonstrated, respectively, in *Recherches sur les noms des dieux hittites*,

which constituted the 1946-1947 volume of *RHA* and was reprinted as a book in 1975 (Swets and Zeitlinger B.V., Amsterdam), and his 1966 book *Les noms des hittites*.

Finally, in his older years, he devoted himself to Hurrian lexicography, with this work appearing as the 1976 and 1977 volumes of *RHA* and entitled *Glossaire de la langue hourrite* (*première partie, A-L*, and *deuxième partie, M-Z, index*, respectively). The 1978 volume of *RHA* published the *Actes de la XXIVe rencontre assyriologique internationale, Paris 1977: Les Hourrites* and, with the loss of the various underwriting "commissions", this became the "swan-volume" of this great journal.

I have here to add but one semi-personal note: quite by chance, as our respective articles were submitted a year apart from one another, the Vice President of ASLIP, Allan R. Bomhard, and I had back-to-back articles in *RHA* XXXI (1973), both dedicated to the memory of our common teacher and mentor, Prof. J(ames) Alexander Kerns. The health of our now-prospering field is manifested by the contents of the remainder of that issue: S. R. Bin-Nun of Israel, Ahmet Ünal of Turkey and Germany, H. Craig Melchert of North Carolina, Françoise Bader of Paris, and Emmanuel Laroche himself, with an article entitled "Études de linguistique anatolienne". The issue concluded with a group of book reviews by the Maître.

Yoël L. Arbeitman
Institute of Semitic Studies
195 Nassau Street (#16)
Princeton, NJ 08542

We also regret that Professor SAMUEL NOAH KRAMER died late last year. He was often thought to be the greatest Sumerologist in the world, at least by Americans.

In MT-15 we will have a short statement on Kramer's life and work by long ranger Stephen J. Lieberman of Penn and Philadelphia, himself highly regarded in Sumerology.

BASQUE AND NORTH CAUCASIAN OR AFROASIATIC ?

Václav BLAŽEK (Příbram, Czechoslovakia)

The question of genetic classification of Basque language stays open because only synchronic data from Basque can be used for comparisons. But the progress in reconstruction of proto-languages of neighbour language families allows at least to differentiate among hopeful and less hopeful hypotheses.

Two solutions of the problem of the genetic affiliation of Basque language seem to be the most promising: (1) North Caucasian, (2) Afroasiatic. Both points of view have been interpreted usually as mutually excluding each other (with exception of "omnicomparativist" A. Trombetti and D. Woelfel, who understood the all common features connecting Basque with Afroasiatic and Caucasian as a cultural heritage of Mediterranean Sprachbund).

The following material represents 30 parallels connecting Basque with both North Caucasian and Afroasiatic. This list is not exhausting, only illustrative. A real number of promising cognates including pronominal and verbal roots is, of course, higher. The restriction is motivated not only by a lack of space, but also by greater informative value of nouns and adjectives for semantical analysis and following interpretation.

The number of 30 parallels is too low for establishing the set of regular phonetic responses. On the other hand, the numbers of bilateral parallels among Basque and North Caucasian, Basque and Afroasiatic and North Caucasian and Afroasiatic, are significantly higher to allow formulating of the sets of regular correspondences, if they exist.

The presentation of various semantical fields doubtless correlates with an origin of the cognates, if they are the result of a common heritage or they represent mutual borrowings.

Thus, the basic lexicon is represented by terms from body/plant parts names (forehead, tooth, root), human society (male, child), nature (stone, water, creek, firewood), darkness/light (day, morning, tomorrow, shadow, star) and by some adjectives or adverbs (dry, old, little, few). The cultural meaning is characteristic for zoonyms (goat/Mid, ram, ass, dog) and other words (milk; city) while the other zoonyms represent rather the substratum lexicon (fox, rat, mouse).

Any definitive explanation is very far now. We can only for-

mulate some more or less probable hypotheses explaining presented data.

1. Common genetic unity of Basque, North Caucasian and Afroasiatic. (It is improbable, a lot of quoted roots from the last two families have external parallels in other Sino-Caucasian, resp. Nostratic languages.)

2. Distant genetic relationship of Nostratic including Afroasiatic and Sino-Caucasian including North Caucasian (Starostin). The classification of Basque in this hypothetical super-phylum implicates the relationship with all daughter families, including Afroasiatic and North Caucasian.

(It is possible, but it doesn't explain, why just Afroasiatic among the other Nostratic families has so much common in lexicon and also in grammar with Basque.)

3. Basque is related to North Caucasian. E.g. J. Bengtson (1990) supposes the Macro-Caucasian sub-phylum (Basque, North Caucasian, Durushaski).

(It is possible, but how to explain the numerous parallels among Basque and Afroasiatic and among North Caucasian and Afroasiatic?)

4. Basque is related to Afroasiatic (Mukarovsky).

(It is possible, but how to explain the remarkable parallels with North Caucasian, Durushaski and Yenisseian?)

5. Basque and North Caucasian (or hypothetical Mediterranean substratum related to them) influenced Afroasiatic before its disintegration.

6. Afroasiatic influenced Basque and North Caucasian before their disintegration.

(Both the last versions are plausible, but they must be localized in time and space.)

7. Basque (related to North Caucasian) influenced Berber.

(But how to explain the parallels in other Afroasiatic languages?)

8. Berber influenced Basque.

(But how to explain the fact, that Berber data are not often presented among a lot of Basque-Afroasiatic parallels?)

We believe in the highest probability of the versions 2. and 3. on deeper time level and later 5. and/or 6. But definitive solution does not manage without the synthesis of full paleo-linguistic data together with anthropological and archeological verification.

Basque	North Caucasian	Afroasiatic
1. agorr, igarr, ihar "dry"	'iGGwVr "(to) dry"	'k/aw/r- "(to) dry"
2. ahunna "kid"	'H ^W nxV "sheep, lamb"	'H ^W an- "sheep"
a(h)untz "goat"	'hI ^W 5nxV "ram"	
3. arr "male"	'Hir ^k WV "man"	'ar/y/- "man, kinsman"
4. asto "ass"	(W)'Č ^W Vdə "ass"	'3at- "ass"
5. aza(ga)ri, axari "fox"	'c ^W 3H3le "fox"	'CuHar- "wild feline, hyena"
6. bel(h)arr "forehead"	pLezg 'bel "forehead"	'bal-/ 'bar- "forehead"
7. bigar, biharr "tomorrow"	'pəkv "dawn"	'bak ^W -(ar-) "morning"
8. egun "day"	'wəGGInV "day"	'(Hi)gan- "sky; morning"
9. egurr "firewood"	'karV "stick, log"	'kar-/ 'kur- "wood, beam"
10. ezne "milk, juice"	'sinwV "milk; udder"	'Sin- "butter, oil"
11. garrathoin "rat, mouse"	'q ^W IrdV "hedghehog"	'kVrd-/ 'g ^W Vr(d)- "rat, mouse"
12. goiz, gotx, gox "morning"	'qqV ^W sV/ 'qqVc ^W V "morning, evening"	'kVS- "morning, sun"
13. gose "hungry"	'kašI/ 'ggašI "hunger"	'ga(wa)C-/ 'ka(wa)C- "hunger"
14. guti "few"	'ko ^W tV "short"	'k/u/t-/ 'k/u/t- "small, short, few"
15. harr(i) "stone"	'q3rQV "stone"	'kar- "rock"
16. haur "child"	'k ^W arnV "young of animals"	'k/u/r- "child"
17. herro "root"	'qurI "stalk, root"	'k/i/war- "root"
18. hir(r)i "city"	'x ^W 3IrV "village, farm"	'čyar- "city, house"
19. hor "dog"	'xwar "dog"	'hw/ar- "dog"
20. hortz "tooth"	'gwar3i/ 'k/war3i "tooth"	'k ^W ar-/ 'karw- "tooth"
21. hur "water"	'hwirə "river, lake"	'hVr- "lake, river, well"
22. itzal "shadow"	'/cVn/čč ^W ilV "green, gray, blue"	'čil- "shadow, dark, green"
23. izarr "star"	'3wHar "star"	'3uHar-/ '3iHar- "star, moon(-light)"
24. keretz, geriza "shadow"	'kärV "black; coal"	'kar-/ 'kar- "black; night"
25. marro "ram"	'maIr33V "sheep"	'mar- "sheep, ram"
26. sagu, sabu "mouse"	'cwar33gV "weasel, marten"	'Cig/a/r- "mouse, rat"
27. samin "sour"	'cwenIV "salt"	'čam- "sour"
28. txiki "little"	(W)'č ^W k ^W - "little, young, boy"	'čik ^W - "little"
29. za(ha)r, zagar "old"	'swärHo "old"	'Siwar- "old"
30. zorrotz "creek, brook"	'šorV "lake, river"	'šVry- "river; to flow"

Notes:

1. T 125/131: Ba+NCc+Be; L 13 and M₃ n.20: Ba+AA; G 210-210 and W 52/7: Ba+Be; Č 99/41: Ba+NCc.
AA: 7Se: Akk karūru "drying", krr "to burn, dry"// Eg krr "to heat the kiln" (EG V 61)// ECu: Som karah, ? Oromo kora "dry"; SCu karah "dry, hard" (Eh 331)// NOM: Šako korints "dry"// Berb: Siwa qqor, Fodjaha eqqur, Ahaggar igar/ekkar "to be dry", Qabyle aquran "dry" (Cn.127; OS n.215)// WCh qawr- "to set fire" (St 222/715), CCh: Musgu keraq "dry".
2. W 57/12: Ba+Gu.
AA: Eg hnmw "a god with a head of ram" (V 244)// Beja ano "sheep"; ECu: Afar aneCā "lamb", Rendille onó "sheep" (pl.), Som wan "ram"// Gu ana, haña, jana "sheep".
3. T 113/35 and G 146-7: Ba+Be; B n.75: Ba+NCc.
AA: Se: Ug 'ary "kinsman" (Ai 35)// Eg 3r(j) "fellow", Copt ēr "companion" (V 53-4)// CCu: Awngi nārā< 'ni-ara "her husband"// ECu: Sidamo arō "man", arē "woman", Hadiya arō'o/arētē id. (cf. NOM: Anfillo arō "man") (D 201); ? SCu: Mbugu m-'aro "neighbour, kind" (Eh 286)// Be 'argaz/3 "man"> Qabyle argaz, etc., Baamrani argaz, where the second component '-gaz/3 can correspond with Ba gizon, giza- "man", cf. the opposite order of the Ba composite giz arra "person of the male sex", and WCC 'qaça "man" (T 123/119: Ba+WCC+Be; G 146-7 and M₀ 180/17: Ba+Be; Č 100/63: Ba+WCC).
4. T 144/304 and W 61/33-34: Ba+WCC+Be; G 136-7 and M 40/20 4: Ba+Be; Č 101/70: Ba+WCC.
AA: WCh: NB '3/y/at- "ass" (Sk 18)// Be: Siwa ezet, Ahaggar aheḍ, Zwawa ized, Ait Halfun ižžeḍ, Zenaga ažžej "ass".
5. T 114/39 and Č 101/72: Ba+NCc.
AA: 30m: Burunge 3i'erare, Alagwa je'ira "wild cat"// NOM: Kafa 3āro "Viverra abessinica" (cf. Amhara 3ari id.), Čara curā "cat" (D 300)// CCh: Bačama žāra "leopard"; ? Gulfei sar "lion"// Be: Ahaggar tāhūri, Iullemeden tazuri "hyena" (P 405).
6. T 116/56: Ba+Lezg, cf. Ta 274.
AA: ECu: Daseneč bal "chest, front" (S₂ 33-4)// SOM: Dime balte, Galila-Ubamer bələ "forehead", NOM: Kafa barō, Moča barō, Šinaša bara id., Amuru bəl "formerly" (F 318).
7. T 116/62: Ba+NCc+Se+eg, B n.39: Ba+NCc; M₂ n.6: Ba+Se+eg.
AA: Se 'bukar-/ 'bukar- "morning"> Arab bukrat-, bakar- "early morning", Hbr bōqār "(tomorrow) morning" (MM 177)// Eg bk; "mor-

ning"// ECu: Som bakkal, Rendille bakal, Oromo bakkalča, Dume bikil "morning star"// NOm: Kafa bākura, bak^wra "star" (D₁ 261)
// CCh: Lame bükā, Fave bēka "morning", ECh: Sokoro bīkī "day".

8. T 126-140 and Č 102/86: Ba+NCc; G 104-5: Ba+Be.

AA: WCh: NB: Siri lǵánī "sky"; ECh: Sokoro igindī "morning"(JSh 181: pCh *-gN- "morning")// Be: Qabyle agenni, Šilh igenna, Zena-ga gumuen (pl.) "sky".

9. T 126/146: Ba+NCc(Agul kur "tree")+ ECu; G 122-3: Ba+Be.

AA: Se: Hbr kōrā "beam", Arab qarīyat- "stick" (D₂ 135)// ECu *kor- "wood, tree" (S₁ 48, 49); SCu: Dahalo kōro-e "tree, stick, wood" (Eh 253)// Be: Qabyle aqerum "firewood".

10. T 121/97 and B n.66: Ba+NCc.

AA: CCu *sən- "butter" (A 42); ECu: Dullay šinan id.// WCh: NB: Diri šinama "oil".

11. L 424: Ba+AA; G 134-5: Ba+Be.

AA: Se: Arab ġirdaun, ġuraḍ, pl. ġirdān/ġurdān "muris campestris species", Mehri ġirēt "rat"// ECu: Som ġir "mouse, rat" (Co n. 208), Boni šir "rat", ? Yaaku koīro' id.; SCu: Iraqw garaṇ, Asa gwarank "rat" (Eh 371)// ? WCh: NB *g^wa/rā/- "rat" > Warji gwarana, Diri agwara, Jimbin ragwaḍa (Sk 36)// Be: Tamazight (Sened) ġarda, Wargla aḡērda, Šenwa aḡērda "rat", B.Menacer aḡērda "mouse", etc.

Note: The semantical shift "mouse" → "hedgehog" is plausible, cf. Lak ccac-kulu "hedgehog" vs. kulu "mouse" or Ba sagarrōi vs. sagu id. The Romance word for "rat", e.g. Spanish ratón, French rat, etc. can be borrowed from the pre-Indo-European substratum related to Basque.

12. AA: CCu *kes- "dawn" (A 47)// NOm: Zayse kos "sun".

13. T 125/135: Ba+NCc+Som; M₁ 41/23-4: Ba+Logone.

AA: ? Se: Arab ġāc "to wish strongly"// ECu: Som gājo "hunger", Oromo kačo id.// NOm: Ometo košā "hunger", Kačama kēwāci "hungry" (D₁ 305)// WCh: Hausa kīšīrwa "thirst"; Bolewa kuzum, Kirfi kušūṅu "hunger"; SB: Geji kuzun, Saya kwusun id., etc. (St 221/707: *quši); CCh: Logone ḡozem "hunger".

14. T 126/147: Ba+NCc+Be+Eg.

AA: Se *ktn "to be small, thin", but Akk katnu, kattanu (Ai 275) // Eg kt "small"// CCu *qqt- "to be thin" (A 41)// WCh: Hausa kādān "few"; Angas kwīt, Sura kat "small", Kulung kede "thin"; Be:

Ghadames iktu "few", Zayau ketṭin "to be small, short", Ahaggar ektem "to make smaller" (OS n.205).

15. T 127/153: Ba+NCc+CCu; L 488: Ba+Be (Šilh aḡarraṇ "heap of stones").

AA: Se: Arab qāra "solitary rock, hill"// Eg kṭt, kṭt "hill"; ? Demotic kr, Copt kra "bank" (V 85)// CCu *karṇ "stone" (A 45) or Awngi qar "stone"; ECu: Som kār "high mountain", Oromo kara "peak, edge"// Be: Ntifa iḡir "rock", Šilh iḡar "hill" (OS n.216).

16. T 127/154: Ba+Beja; M₂ n.19: Ba+Cu.

AA: Beja *ōr, pl. *ar "boy, son"; CCu *q^war- "child" (A 37, 53; D₁ 83)// CCh: Mafa kuṅgorà "boy", Daba kṛti "son, boy", Masa gora "son".

17. T 128/162: Ba+Se.

AA: Se *cikkar- "root" (MM 177)// CCh: Bačama kwīrey, pl. kwērye "root"; ECh: Jegu kyāro id.

18. T 130/184: Ba+Se.

AA: Se: Hbr *ir, Ugar *r "city", ESA *r "castle" (Ai 291)// ? Eg *rw.t, *rrw.t "gate" (EG I 210)// ECu: Afar *ārī "house, kin, family", Saho *ārī "house"// SOM: Ubamer eri, Bakko eri "house"// WCh: ? Hausa gari "town", Geruma yōrō "place, town"; CCh: Gisaiga hirwi "village".

19. T 129/68 and Č 101/72a: Ba+NCc (the reconstruction *xwar - see Gu 103), W 62/40 and G 132-3: Ba+Eg.

AA: Eg whr "dog", Copt uhor (EG I 346; V 242)// Be: Senhaja iwhar, pl. iwhariwen or iuharen, B. Urjagal uhar, Ait Tuzin awha(r)/ iuha(r)en "dog"; ? Ahaggar āhōrhi/iḥorhiyan, Ūhat aḥurhay "fennec" (P 381).

20. T 129/172: Ba+NCc+Beja; M 39/15.5: Ba+Be.

AA: Beja kwire "tooth"// SOM: Ari-Jinka kari "tusk"// WCh *hV-q^wari "tooth" (St 222/717) > Hausa hakōrī; Ron: Kulere *agwér, Bokkos *ā-gūr, Fyer hēgor, Daffo-Butura gur// Be: Senhaja a-qarruṣ "tooth". Note: Gi 78, 85 reconstructs NECc gwarj.

21. Č 102/83: Ba+NCc.

AA: ? Se: Arab hawr- "lac de décharge de marais" (Co n.162)// ECu *har- "pond creek" (S₁ 39, 40)// ? CCh: Munjuk èrè "river"//Be: Ahaggar ahir "source d'un débit extrêmement faible"; Gu hero "a weak source".

Note: W 157/17 and M 37/9.2 compare with Ba herio, erion, etc.

"outflow".

22. T 148/353: Ba+Se+Be; M₂ n.11: Ba+Se+Ron.

AA: Se *till- "shadow", *tīm "to be dark" (Le 52, 51, 157)// NOM: Wolaita (Beke) cilālea "black", Gimirā čil-/čil, Nao, Mači čil-, Kafa močočillo "green" (D₁ 113)// WCh *č/Vla "shadow" (St 194/455)// Be: Semlal idili "black"; Ahaggar ədilu "to be green and profuse (of vegetation)".

23. T 147/337, Č 102/84, B n.26 and W 147/6: Ba+NCC.

AA: Se: Aram zhr "moon", Arab zhr "to shine", suharat- "planet Venus", *al-'azhar-ān (du.) "sun and moon" (MM 213)// WCh: Sura zhr, Montol zayi, Gerka dar-kir; SB: čāā-šur, Zem šasur (St 201/519); Ron: Daffo jóret, ? Kulere sīriri "star"// Be: Nefusi ziri "moon", Siwa taziri, Ghadames, Wargla taziri id., B. Snus taziri "moon-light" (W 146).

24. G 170-1.

AA: Beja *iru, eru "yesterday (evening)"; CCu *qar-/qir- "night" (A 49, 52); ? ECu: Oromo gurračča, gur'ačča "black"; ? SCu: Iraqw xwēra "evening"// NOM: Janjero kara, Čara kartā, Dače karats, Wolaita karta, etc. "black" (D₁ 206)// WCh: Gerka kurrum "black"; CCh: Kilba kékeru, Margi kyankyer, etc. "black".

25. T 136/232: Ba+NCC+ECu.

AA: ECu: Afar mārū "ram", Saho^(mārū) "ram, sheep", Rendille mār, pl. maaró "calf"// NOM: Šinaša mēra, Basketo, Dokka marzi "ram", Koyra marre "sheep"// WCh: SB *mar "buck"; Ron: Bokkos maray "ram"; ? CCh: Mafa mari "bull".

26. T 142/287, Č 101/71 and B n.52: Ba+NCC; L 1136 and Pa 65: Ba+Be.

AA: Be: Libyan zegeries (pl.) "mouse" by Herodotos (IV, 192), Augila éqzer, yāqzer, pl. aqzar (metath.); Ahaggar eg'ig'ier "dark grey big rat" < *eziger, Ghat t-aššier-t "rat, mouse" < *t-aziger-t; Šerwa azērgug, pl. izērgug "mouse".

Note: Akk zirkatu "lynx, caracal" can belong here tpp (a NCC loan?).

27. T 142-3/293: Ba+NCC+AA; M₂ n.8: Ba+NOM+Hausa; W 53/8: Ba+Kafa.

AA: NOM: Kačama čām, Wolaita čam, Moča čammó "sour", Kafa čamm- "be sour"// WCh *čami "sour" (St 184/358); ČCh: Tera čomčom "sour", Fidlīmī šomšom "sourness"; Logone semsem "sour".

Note: Be: Jullemiden tesmit, Ahaggar tesemt /tēmīn "salt", vs. esmam "to be sour" represent rather only a typological parallel

because the correspondence of the initials is problematic.

28. T 118/78: Ba+NCC+AA; Č 99/40: Ba+NCC.

AA: CCu *cag- "small" > Bilin šeg-/šej-, Kemant šag(g)-, Qwara šeg-, Xamir čik-; ECu: Konso šaka, Gato ižakki "few, little"; SCu: Durunge čoko "little" (D₁ 119)// Be: Sus mezzī, pl. mezzuket "small", ? Nefusa mešek/mešuket id. (W 52/5).

Note: Č 99/40 compares the Ba word also with NECC *ččēk-/V "kid; young of animal" while B n.45 prefers the comparison with NCC *čči/kk/V "short".

29. T 147/338; W 51-2/1-2 and M 37/10.8: Ba+Be; Č 99/36 and B n.

43: Ba+NCC.

AA: ? ECu: Yaaku -sirgin, pl. -siragde "old"// WCh: Ankwe sir id., Sura siyir, Angas nsir id. < *si/wir ?; CCh: Daba musir id.// Be: Sokna ausser/wussuret, Ghadames wesser, Ghat auššar "old", uššer, Nefusa usser, B. Snus ússer "to be old".

30. G 120-1.

AA: Se: Arab sry "to flow, circulate", sarīy "a little brook"// WCh: NB *sirV "river, creek" (St 254) > Siri sarrangi, Jimbin sirya.

Abbreviations of languages: AA Afroasiatic, Akk Akkadian, Arab Arabic, Aram Aramaic, B Bauči, Ba Basque, Be Berber, C Central, Ch Chadīc, Copt Coptic, Cu Cushitic, E East, Eg Egyptian, Gu Guanche, Hbr Hebrew, Lezg Lezghinian, N North, Om Omotic, p proto, S South, Se Semitic, Som Somali, Ug Ugaritic, W West.

Abbreviations of authors and basic bibliography:

A APPLEBYARD D., 1984: The internal classification of the Agaw languages: a comparative and historical phonology. In: BYNON J., ed., Current Progress in Afroasiatic Linguistics. Papers of the 3rd International Hamito-Semitic Congress. Amsterdam - Philadelphia: Benjamins, 33-67.

AI AISTLEITNER J., 1963: Wörterbuch der ugaritischen Sprache. Berlin: Akademie-Verlag.

B BENGTON J., 1990: An end to splendid isolation: the Macro-Caucasian phylum. Mother Tongue 10.

Co COHEN M., 1947: Essai comparatif sur le vocabulaire et la phonétique du chamito-sémitique. Paris: Champion.

Č ČIRIKBA V.A., 1985: Baakakij i severokavkazskie jazyki. In: Drevnjaja Anatolija. Moskva: Nauka, 95-105.

- D₁ DOLGOPOL'SKIY A.B., 1973: Sravnitel'no-istoričeskaja fonetika kušitskix jazykov. Moskva: Nauka.
- D₂ DOLGOPOL'SKIY A.B., 1983: Semitic and East Cushitic: Sound Correspondences and Cognate Sets. In: SEGERT S., BODROGLIGETI A.J.E., Ethiopian Studies, dedicated to W. Leslau on the occasion of his 75th birthday. Wiesbaden: Harrassowitz, 123-142.
- EG ERMAN A., GRATOW W., 1971: Wörterbuch der ägyptischen Sprache, I-VI, Berlin (originally Leipzig 1928-31): Akademie-Verlag.
- EH EHRET Ch., 1980: The Historical Reconstruction of Southern Cushitic Phonology and Vocabulary. Berlin: Reimer.
- F FLEMING H., 1976: Omotie Overview. In: BENDER M.L., ed., The Non-Semitic Languages of Ethiopia. East Lansing: Michigan State University, 222-231.
- G GABELENTZ G. von der, 1894: Verwandtschaft des Baskischen mit den Berbersprachen Nord-Afrikas. Braunschweig: Sattler.
- GI GIGINEJŠVILI B.K., 1977: Sravnitel'naja fonetika dagestanskix jazykov. Tbilisi: Izdatel'stvo Tbilis. Gos. universiteta.
- GU GUDAVA T.Je., 1979: Istoriko-sravnitel'nyj analiz konsonantizma didojkskix jazykov. Tbilisi: Mecnierba.
- JSH JUNGRAITHMAYR H., SHIMIZU K., 1981: Chadic lexical roots. Vol. II: Tentative reconstruction, grading and distribution. Berlin: Reimer.
- L LÖPELMANN M., 1968: Etymologisches Wörterbuch der baskischen Sprache. Berlin: Gruyter.
- Le LESLAU W., 1963: Etymological dictionary of Harari. Berkeley - Los Angeles: University of California Press.
- M MUKAROVSKY H., 1969: Baskisch-Berberische Entsprechungen. Wiener Zeitschrift für die Kunde des Morgenlandes 62, 32-51.
- M₀ MUKAROVSKY H., 1963-66: Les rapports du basque et du berbère. Comptes rendus du Groupe Linguistique d'études chamito-sémitiques. 10, 177-184.
- M₁ MUKAROVSKY H., 1972: El Vascuense y el Bereber. Euskera 17, 5-49.
- M₂ MUKAROVSKY H., 1980: Einige hamito-semitische und baskische Wortstämme. XXI. Deutscher Orientalistentag. Berlin (March 25-29, 1980).
- M₃ MUKAROVSKY H., 1980: Common Hamito-Semitic and Basque with examples for a proto-phoneme /t b/. Bascologists International Meeting at Bilbao, August 25-29, 1980.
- MM MAJZEL' S.A., MILITAREV A.Ju., ed., 1983: Puti razvitiya kornevogo fonda semitskix jazykov. Moskva: Nauka.

- OS ILLIČ-SVITYČ V.M., 1971, 1976, 1984: Opyt sravnenija nostratičeskix jazykov I, II, III. Moskva: Nauka.
- P PRASSE K.-G., 1969: A propos de l'origine de H touareg (tahaggart). Kobenhavn: Det Kongelige Danske Videnskabernes Selskab Hist.-fil. Meddelelser 43,3.
- Pa PARADISI U., 1963: Sul nome del "topo" nel berbero di augila e una voce libica citata da Erodoto. Rivista degli studi orientali 38, 61-5.
- S₁ SASSE H.-J., 1979: The consonant phonemes of Proto-East Cushitic (PEC): A first approximation. Afroasiatic Linguistics 7/1, 1-67.
- S₂ SASSE H.-J., 1982: An etymological dictionary of Burji. Hamburg: Buske.
- Sk SKINNER N., 1977: The Northern Bauchi Chadic Languages. Common Roots. Afroasiatic Linguistics 4/1, 1-49.
- St STOLDOVA O.V., 1987: Sravnitel'no-istoričeskaja fonetika i slovar' zapadnočadskix jazykov. In: Afrikanское istoričeskoe jazykoznanie. Moskva: Nauka, 30-268.
- T TROMBETTI A., 1926: Le origini della lingua basca. Bologna: Accademia delle Scienze dell'Istituto di Bologna.
- Ta TALIDOV B.B., 1980: Sravnitel'naja fonetika lezginskix jazykov. Moskva: Nauka.
- V VICICHL W., 1983: Dictionnaire étymologique de la langue copte. Leuven - Paris: Peeters.
- W WOELFEL D., 1955: Eurafrikanische Wortschichten als Kulturschichten. Salamanca: Acta Salmanticensia, Fil. y Letr. T. IX, num.1.

The Basque data are quoted mainly by von Gabelentz and Löpelmann (in standard transcription). The North Caucasian reconstructions are borrowed from the unpublished list of reconstructed proto-forms of S. Starostin and S. Nikolaev with certain modifications of the same authors by later papers (n.10, 14, 23), except ionally from the other authors (n.19, 20). The reconstructions of North Caucasian (Abxaz-Adyghean and Nax-Daghestanian), Nax-Daghestanian and Daghestanian levels are not differed.

Add these comments by HF

#2 "kid, goat". Add Ongota hóóna/hoona/
xuuna

Otherwise weak. sheep, ram
#7 "tomorrow", proto-AA *bkr/bkl. No fair cheating
#4 "ass" = "donkey" by dropping -r

Scratch the Caucasian * form.
Substitute: Kabardian šd (more
Abzax šid, convincing)

And the proto-AA form is obviously wrong
Just look at the Berber forms.

Very doubtful that this is from proto-AA

#18. Quite good! Delete Chadic forms.
Meaning more like 'house' than 'city'

Generally, the Caucasian forms are tortured.
Unconvincing phonetically.

No back up in real language forms.

More convincing to cite "

Generally, the AA forms are localized;
rarely going back to proto-AA in
a strong way. Often debatable.

Eric de Gralier

"NOSTRATIC" AND OTHER LANGUAGE "FAMILIES"
(OR "MACROFAMILIES")

During the last five years or so, the question of languages classification has become a major subject for debate not only among linguists, but also between (some of) them and (some) specialists of other disciplines - mainly in molecular biology.

If we accept the principle that the first step for establishing a certain field of knowledge as a "science" is to reach an agreement on a classification of the "objects" (phenomena, entities...) in that field, then we must conclude that linguistics is just beginning to justify its pretention to be a "science". Indeed, it is at the stage where botany and zoology were at the time of Linné (and the Linné of linguistics could have been Swadesh, if he was not prematurely dead, leaving this honor to somebody fortunately still alive: Joseph Greenberg).

In a paper presented at Bochum in 1986 (but published only in 1990), I expressed some scepticism concerning certain groupings of languages pertaining to different "families".

Since then, new data have been available, including revisions of the "classic" (i.e. Illich-Svityč's) "Nostratic" by Starostin and Aikhenvald, attempts for correlating "Nostratic" with "Sino-Caucasian" (Starostin) and the latter with Austro-Thai and Mon-Khmer (Pejros), and Fleming's edition of previously unpublished versions of Swadesh's classification - which, interestingly, provide some support to Shevo-

roshkin's proposal for disconnecting Almosan-Keresiouan from Greenberg's "Amerindian" and joining it to Sino-Caucasian. But we are still awaiting the publication of Greenberg's Eurasiatic material, except for the revised version of his Stanford 1987 lecture.

Four months ago, I tried to supplement the rather crude statistical analysis contained in my Bochum paper, correlating the data provided by the three published parts of Illič-Svityč book, his two posthumously published papers in Etimologija; that given by Starostin's and Pejros' 1989 papers; and (separately) the new (1990) version of Bomhard's own "Nostratic". The results are summarized in the four graphs presented here.

A word of caution is appropriate concerning the Illič-Svityč data: for 1965-84, I added the items provided by the Etimologija 1965 (published in 1967) paper to those included in the book edited by Dybo and his team - insofar as they provided really different sets, and not earlier versions duplicating the new ones. This is not always clear-cut: there are some dubious cases (about ten) and one duplicate entry in the 1965 material (puskat'/rozdat' 4), but these are without real influence on the global statistics. More important, from that point of view, are the items marked as "uncertain", either by I.-S. himself or his posthumous editors. I had to take a somewhat arbitrary decision: I accepted all the items marked by (?) in I.-S.'s own paper (1965/67) and all the cognates listed in the headings of the published book (with or without ?), but not those marked ? in the text and not included in the heading (for an example, see item 5 in the 1971 part 1, p. 174: I did not include the Uralian putative cognate).

The 710 items in I.-S. 1965-84 represent the total made from 378 items in the three parts of the Opyt (1971-84), plus 319 non-duplicated items in the 1965 (1967) paper, plus 13 "morphological" items which

the Dybo team did not include in the numbered items of Opyt.

The 1968 I.-S. material is not complementary to the 1965-84 data: it comes from the same corpus, envisaged from another point of view.

There is no need here for detailed comments on the graphs. One very clear conclusion emerges from comparing the two I.-S. graphs with the Bomhard one: Bomhard's "Nostratic" is not the same entity as Illic-Svityč's "Nostratic". Bomhard's statistics is heavily "loaded" by the origin of the author's endeavour: a comparison between Indo-European and Semitic. By contrast, I.-S. appears more "balanced" - but it leaves the sceptically oriented "external" (i.e. not "committed" to one "school" or the other) observer with some question-marks. It is, for instance, rather surprising that the number of "cognates" between Indo-European and Afrasian is superior to that between I.-E. and Uralian.

Without agreeing to all the objections presented by Murtonen to I.-S. "Afrasian" material (which, in fact, are more directly pertinent concerning his Semitic data), one must admit that a good number of I.-S. Afrasian "cognates" with the three families pertaining to Greenberg's Eurasiatic (I.-E., Uralian and Altaic) are at least debatable.¹

Ruhlen recently (1990:10) wrote a rather harsh remark on "Nostraticists" - that "they have reconstructed, with regular sound correspondences, a linguistic family that never existed". On the other hand, Job (1990:362-3), criticizing the Gamqrelidze-Mačavariani (1965) reconstruction of their "Nostratic" phonological system, observed that its structures "strikingly resemble the Proto-Kartvelian type and

make the impression of a blend based on the latter and the pharyngolaryngeal part of Proto-Semitic plus two lateral fricatives which can also be interpreted as continued in the Proto-Semitic coronal fricatives that are assumed to have been lateralised". This observation can, mutatis mutandis, be also directed to other versions of "Nostratic" phonology.

The Starostin-Pejros graph could be compared with the Illic-Svityč ones, as the "Nostratic" material used by these authors is practically the same as that on which Illic-Svityč "reconstruction" was based. It is rather interesting to compare the figures concerning Ienissei and North Caucasian relations with Sino-Tibetan (as these three families are supposedly genetically connected according to Starostin's hypothesis) with those on their relations with Indo-European (part of "Nostratic"): they are indeed very much in the same range.

If I would dare a general conclusion on the relations shown by these graphs, it would be that the present "long-range" classifications reflect more the peculiar¹ interests, preferences (or idiosyncrasies) of their (and the span of their specialized knowledge) respective authors² than the real relationships between the language families concerned.

Éric de Grolier

ISSC, Paris

April 20, 1991

Notes

1

The same observation is valid concerning Bomhard's Afrasian data. On the other hand, Blažek has recently supplied a rather large number of putative cognates between Afrasian and Dravidian.

2

A similar judgment was made by Greenberg (1987:332) on Ramstedt's classification of Korean as "Altaic".

References

- Aikhenvald-Mangenot, Alexandra Y. and Jean-Pierre Angenot. 1989. Etimoestatística multilateral para morfologia comparada de Proto-Noscau et Proto-Amerind. Mimeo report.
- Blazek, Václav. 1989. The new Dravidian-Afroasiatic lexical parallels. Handout distributed at the Moscow 1989 Conference. 10 pages (115 items).
- Bomhard, Allan R. 1990. The Nostratic macrofamily. Mimeo.
- Fleming, Harold C. 1990. Dene-Caucasic, Nostratic, and Eurasiatic or Vasco-Dene? Mother Tongue (12) 6-12.
- Greenberg, Joseph H. 1987. Language in the Americas. Stanford: Stanford University Press.
- 1990. The prehistory of the Indo-European vowel system in comparative and typological perspective. In Proto-languages and proto-cultures, p. 77-136, ed. by Vitaly Shevoroshkin. Bochum: Brockmeyer.
- Grolhier, Eric de. 1990. Towards a tentative "reconstruction" of Homo sapiens sapiens language(s)? In Geneses of language, p. 135-163 (see p. 138-142), ed. by Walter A. Koch. Bochum: Brockmeyer.
- Job, Michael. 1990. Comparative linguistics and the genesis of language. In Geneses of language, p. 339-373. Bochum: Brockmeyer.
- Illič-Svityč, V.M. 1967. Materialy k sravnitel'nomu slovarju nostratičeskich jazykov. Etimologičeskij žurnal 65 321-373.
- 1968. Sootvetsvijsa smyčnyh i razryvnyh jazykah. Etimologija 1966, 304-355.
- 1971-84. Opyt sravnenija nostratičeskich jazykov. Moskva: Nauka.
- Murtonen, A. 1989. Comments on the Nostratic reconstructions of Illič-Svityč (revised edit.). Mother Tongue (9) 15 p.
- 1990. Comments on Bomhard's "Lexical parallels between Proto-Indo-European and other languages". Idem (11) 20 p.
- Pejros, I.I. 1989. Dopolnenie k gipoteze S.A. Starostina o rodstve nostratičeskich i sinokavkazskih jazykov. In Lingvističeskaja rekonstrukcija i drevnejšaja istorija vostoka, čast' 1, p. 125-130.
- Ruhlen, Merritt. 1990. An overview of genetic classification. In The evolution of human languages, p. 1-27, ed. by J.A. Hawkins and M. Gell-Mann.
- Starostin, S.A. 1989. Nostratic and Sino-Caucasian. In Lingvističeskaja rekonstrukcija i drevnejšaja istorija vostoka, čast' 1, p. 106-124.
- 1990. A statistical evaluation of the time-depth and subgrouping of the Nostratic macrofamily. Abstract of a talk delivered at Evolution: from molecules to culture symposium, Cold Spring Harbor Laboratory, p. 57.

Figure 2

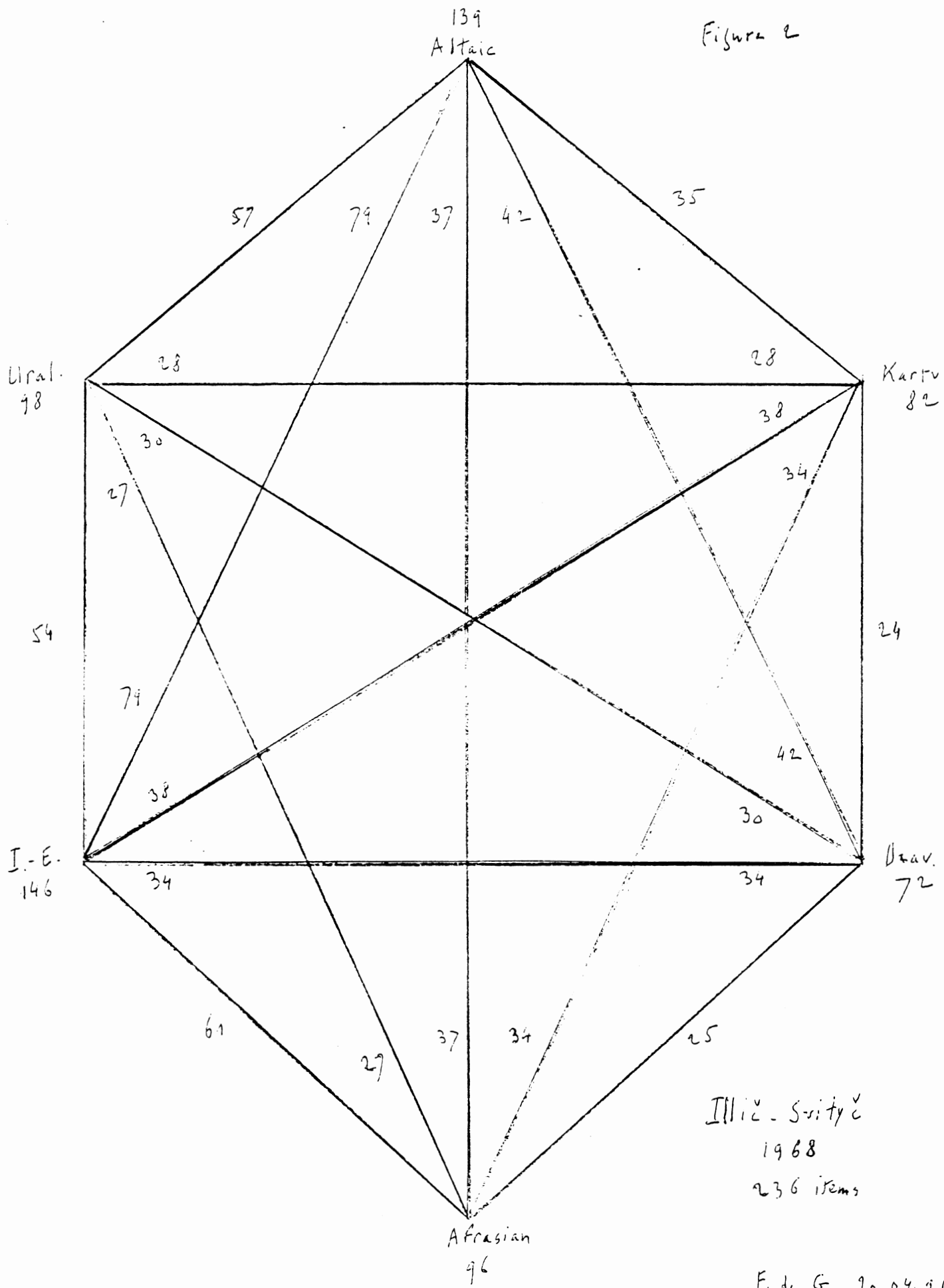


Figure 3

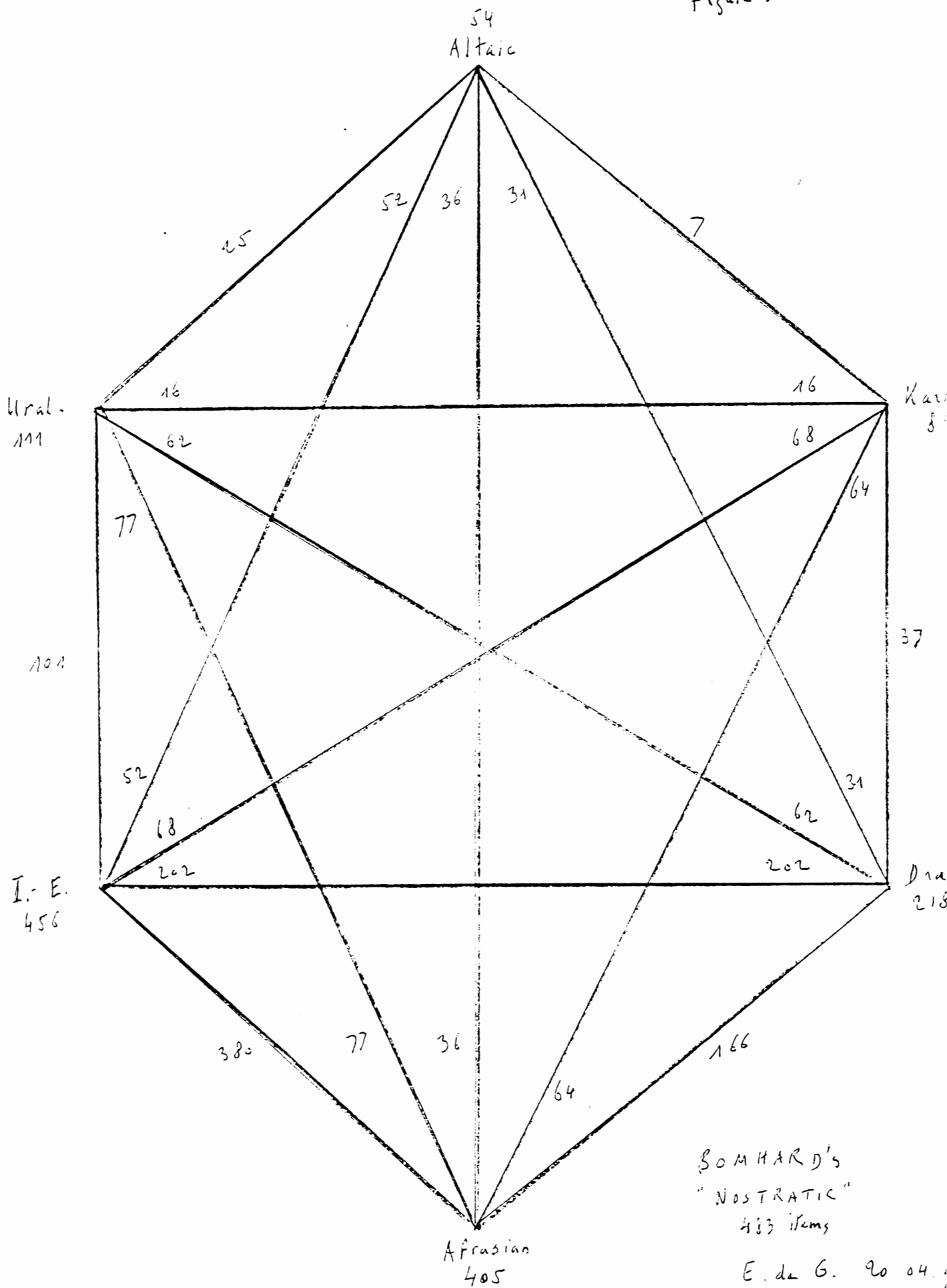


Figure 4

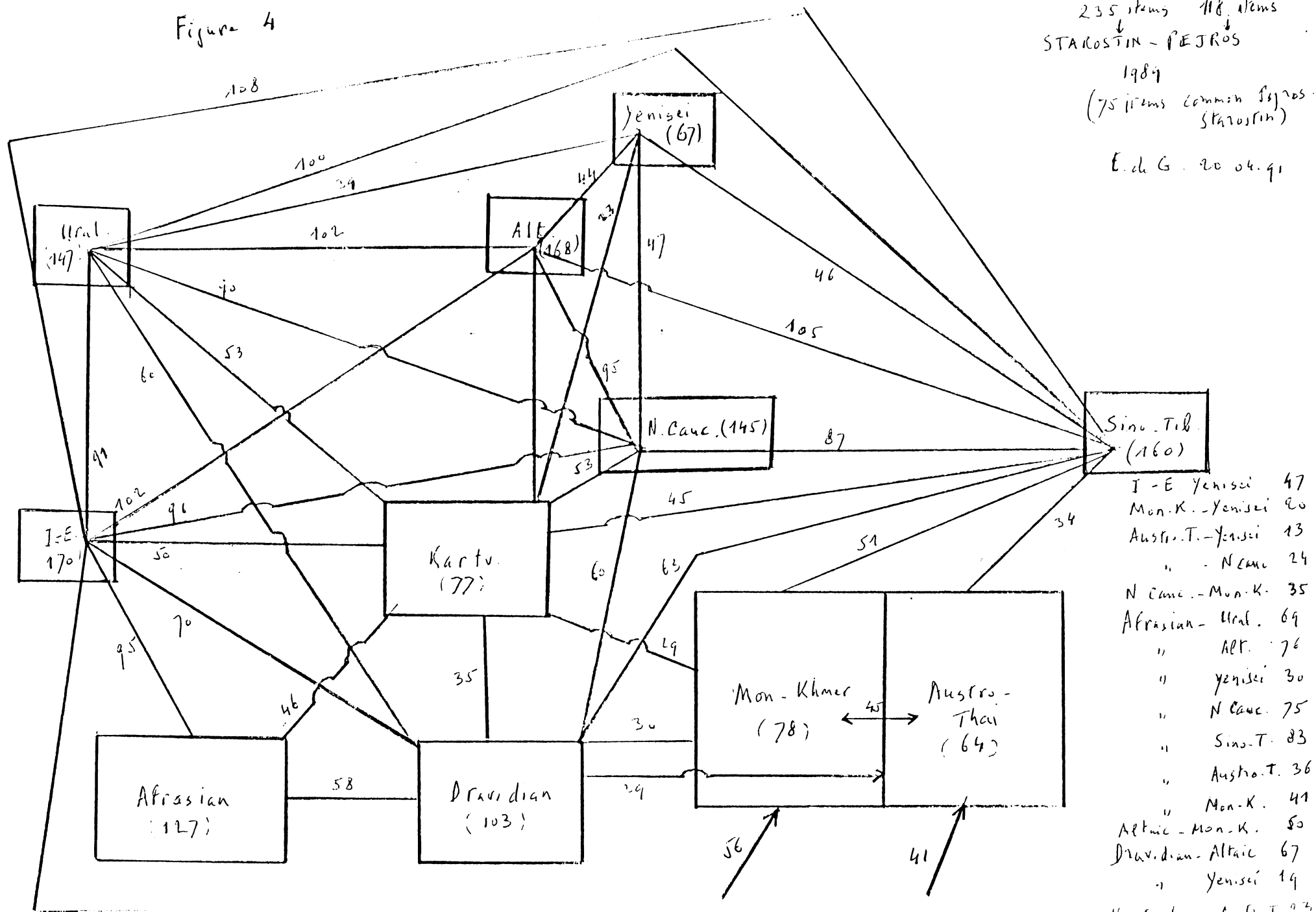


Table 3. Amerind T'ĀNA 'child, sibling'*

PROTO-AMERIND	*T'ANA 'CHILD, SIBLING'	*T'INA 'SON, BROTHER, BOY'	*T'UNA 'DAUGHTER, SISTER, GIRL'
Almosan	t'an'a 'child' (Nootka)	t ^s in 'young man' (Yurok)	tune 'niece' (Coeur d'Alene)
Keresiouan	tane 'brother' (Yuchi)	-ʔtsin 'male, boy' (Mohawk)	t ^s one 'daughter' (Yuchi)
Penutian	t'āna-t 'grandchild' (Totonac)	pnē-t'in 'my brother' (Cayuse)	-tūne 'daughter' (Central Sierra Miwok)
Hokan	t'an-pam 'child' (Coahuilteco)	t'ini-si 'child, sibling' (Yana)	wi-t ^s u-k 'younger sister' (Washo)
Central Amerind	*tana 'daughter, son' (Proto-Uto-Aztecan)	pil-tsin 'child, boy' (Pipil)	tutuna 'older sister' (Taos)
Chibchan	tuk-tan 'child, boy' (Miskito)	a-te-gwa 'nephew' (Motilon)	tuntu-rusko 'younger sister' (Lenca)
Paezan	t ^s āna 'son' (Cayapa)		t ^s uh-ki 'sister' (Cayapa)
Andean	tane-ngh 'maternal uncle' (Ona)	den 'brother' (Tehuelche)	ke-tun 'sister' (Tsoneka)
Macro-Tucanoan	tata 'child' (Nadobo)	ten 'son' (Tiquie)	ton 'daughter' (Tiquie)
Equatorial	dan 'baby' (Atoroi)	tin-gwa 'son, boy' (Mocochi)	t'uana 'woman' (Chipaya)
Macro-Carib	tane 'my son' (Pavishana)	dēnu 'male child' (Yagua)	ho-tone 'nephew' (Nonuya)
Macro-Panoan	đana 'younger sister' (Tacana)	u-tse-kwa 'grandchild' (Tacana)	tuna-ni 'woman' (Towithli)
Macro-Ge	tog-tan 'girl' (Tibagi)	čina 'older brother' (Guato)	a-ton-kā 'younger sister' (Piokobyé)

*Adapted from "Amerind *t'āna 'child, sibling,'" by Merritt Ruhlen, 1991.

Table 4. Dene-Caucasian Cognates

Family	I, MY	WE, US	THOU, YOU	WHAT?, WHO?	FOOT, PAW, CLAW	ELBOW, KNEE	SQUIRREL, MARTEN, RAT	SUN, DAY (LIGHT)	CHILD, SON	ICE, FROST, SNOW	YELLOW, GOLD, WHITE	HUNGRY	BITE
Basque			-k	ze-	hatz	u-kondo	sagu	e-gun		-tzig-		gose	
Caucasian	*zō	*iλi	*gu	*sa	*kʷāčʰē	*qʷantV	*tʰārgwi	*-ginV	*kəntʰV	tʰiq	*tʰakʷV	*gāšī	*qʰātʰi
Burushaski	ža		gu-	be-sa-n	qʌš		čʰarge	gōn			šik-ark		
Sino-Tibetan	ži		*kʷVj	*su			*srāiŋ	*kʷāŋ			*tsyak	kʰusʃi	
Yeniseian	*ʔaʒ		*kV-	*sV-	*kiʔs	*gid	*saʔqa	*gəʔn	*gʌʔt	*tiχ	*täk-		
Na-Dene: Haida		iλʰ		gū-su			-tʰʰākʷ	kūŋ	gyitʰ				qʰus-gat
Tlingit			yi	ah-sa			tʰʌlk	-gan	gitʰa	tʰiqʰ			kʷač
Eyak	chuu				-kaša	guhɗ	tʰaɬkʰ	ʒah	qēčʰ	tʰitʰʰ	tʰeʔq		qʰətʰʰ
Athabaskan	*ši	*-iʰd	-hʷi	-šāʔ	*-kečʰ	gūd	tʰolq-	ʒanih	γəʔ	λʰitʰʰ	tʰək	gas	qəɬʰ

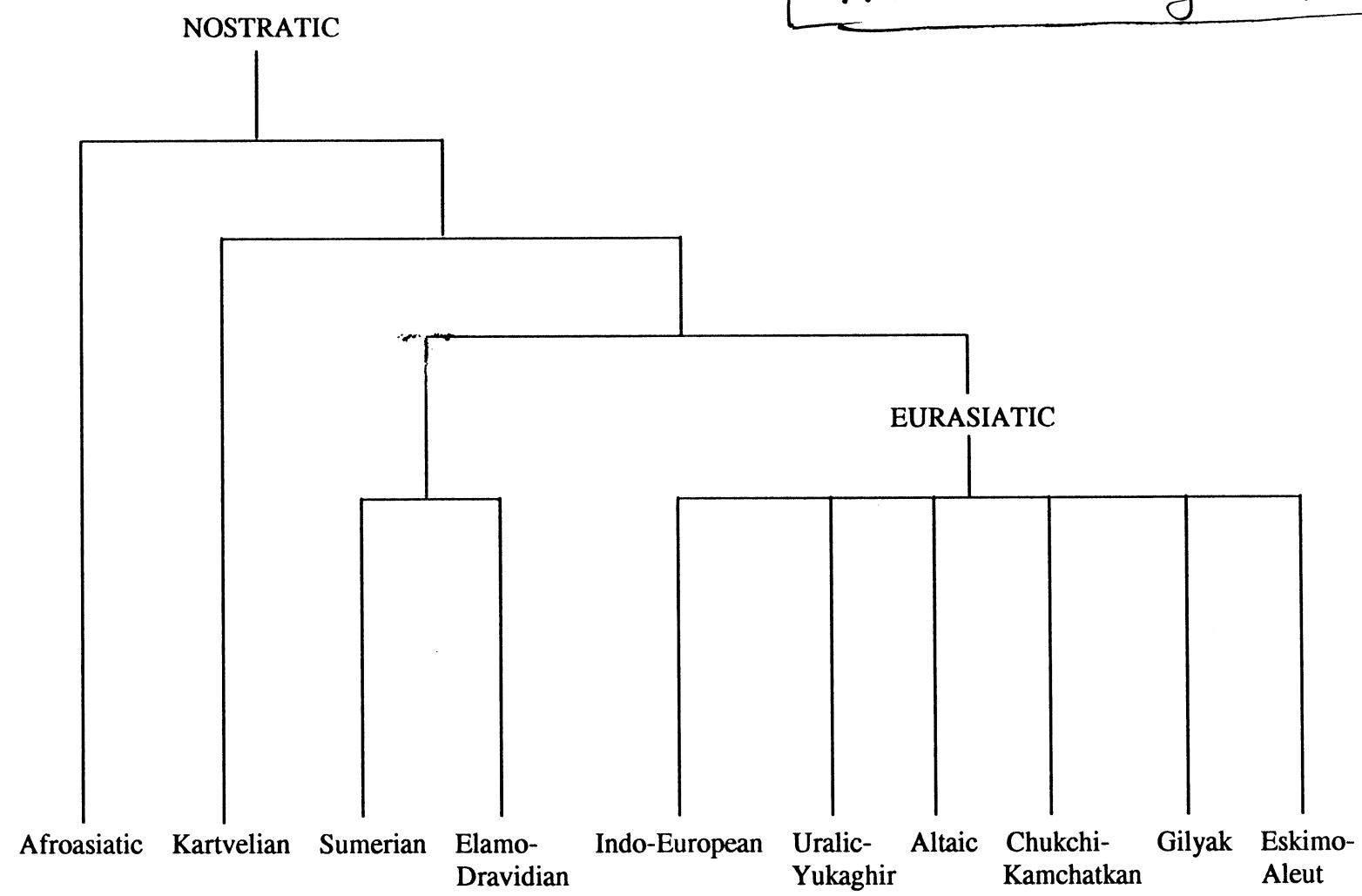
Adapted from the work of Sergei Starostin, Sergei Nikolaev, and John D. Bengtson.

Table 5. MALIQ'A 'suck, nurse, swallow'.

Language Family	Language	Form	Meaning
AFRO-ASIATIC	Proto-Afro-Asiatic	*mlg	'to suck, breast, udder'
	Arabic	mlġ	'to suck the breast'
	Old Egyptian	mnd	'woman's breast, udder'
INDO-EUROPEAN	Proto-Indo-European	*melg-	'to milk'
	English	milk	'to milk, milk'
	Latin	mulg-ēre	'to milk'
	Tocharian A	malke	'milk'
URALIC	Proto-Finno-Ugric	*mälke	'breast'
	Saami	mielgä	'breast'
	Mordvin	mälhkä	'breast'
	Hungarian	mell	'breast'
DRAVIDIAN	Tamil	melku	'to chew'
	Malayalam	melluka	'to chew'
	Kurux	melkhā	'throat'
ESKIMO-ALEUT	Kuskokwim	milugā	'sucks it out'
	Aleut	umlīx	'chest'
AMERIND	Proto-Amerind	*malīq'a	'swallow, throat'
	Almosan		
	Halkomelem	məlq ^w	'throat'
	Chehalis	mq ^w	'swallow'
	Nootka	m'uk ^w	'swallow'
	Kwakwala	m'lx ^w -ʔid	'chew food for the baby'
	Kutenai	uʔmqoʔ	'swallow'
	Yurok	mik'olum	'swallow'
	Penutian		
	Chinook	mlq ^w -tan	'cheek'
	Takeima	mülk'	'swallow'
	Tfaltik	milq	'swallow'
	Wishram	ō-mēql ⁱ	'lick'
	Yokuts	mōk'i	'swallow'
	Mixe	amuʔul	'suck'
	Zoque	muʔk	'suck'
	Hokan		
	Mohave	mal ⁱ aqé	'throat'
	Walapai	malqiʔ	'throat, neck'
	Akwa'ala	milqi	'neck'
Chibchan	Cuna	murki-	'swallow'
	Cuitlatec	eʔmeli	'eat'
Andean	Quechua (Cochabamba)	malq'a	'throat'
	Quechua (Huaraz)	mallaqä	'be hungry'
Macro-Tucanoan	Aymara	mal ⁱ q'a	'swallow, throat'
	Iranshe	mokeʔi	'neck'
Equatorial	Curetu	mouku	'drink'
	Guamo	mirko	'drink'
Macro-Carib	Arawak	amüküddun	'swallow'
	Surinam	eʔmōkī	'swallow'
	Faai	mekeli	'nape of the neck'
	Kaliana	imukulali	'throat'

*Adapted from "The Amerind Root *MALIQ'A 'swallow, throat' and Its Origin in the Old World," by Merritt Ruhlen, 1991.

Allan Bomhard's
vision in August, 1991



THE NOSTRATIC MACROFAMILY

Four recent books:

A key set of books to help one get a grasp on the combined efforts in biology and biological anthropology and archeology to document and/or explain the emergence of modern humanity or Homo sapiens sapiens. One book on Indo-European is included in that same spirit.

I. Paul Mellars, ed., THE EMERGENCE OF MODERN HUMANS: AN ARCHEOLOGICAL PERSPECTIVE. Edinburgh University Press. 1990. A tremendous compilation of articles by some experts, like Stanley Ambrose, Karl Lorenz, Sandra Bowdler, John Parkington, Anthony Marks, Arthur Jelinek, Philip Van Peer, Pierre Vermeersch, P. Allsworth-Jones, and 14 others. Primarily focused on the transitions from Homo sapiens Neanderthalensis to Homo sapiens sapiens or more generally the Upper Paleolithic (Europe) or Middle Stone Age (Africa) changes or flake to blade lithic transitions or broad correlations with some aspects of these. Paul Mellars (Cambridge University, England) did a good job of keeping people focused, although individual conclusions are not always clear nor arguments understandable. Two of my favorite articles were on the interesting developments in eastern Europe, summarized by P. Allsworth Jones (Dept. of Archeology, U/Ibadan, Ibadan, Nigeria), and Australiasia with its coastal settlement patterns, proposed by Sandra Bowdler (Centre for Prehistory, U of Western Australia, Nedlands, Western Australia 6009, Australia). Long ranger Stanley Ambrose had the 'lead' article but was severely technical and ultimately murky as to dates and events in South Africa. I believe that the book is close to definitive for its period, except for the complete lack of coverage of China, India, Japan and Southeast Asia. Tut, tut, one must add that they forgot to cover East Africa, Ethiopia, and Morocco where the most likely candidates for earliest modern woman can be found. But can one expect too much from 'conference books' where participants are often lacking at key logical points?

II. Eric Trinkaus, ed., THE EMERGENCE OF MODERN HUMANS: BIOCULTURAL ADAPTATIONS IN THE LATER PLEISTOCENE. Cambridge University Press, Cambridge, New York, etc. 1989. This is also strongly focused on the archeology and paleoanthropology of the same period as the first book, but even more on Neanderthal (now spelled Neandertal) and the debate on his/ their classification and 'meaning'. Authors such as Trinkaus himself, Lewis Binford (Himself), C.B. Stringer, Milford Wolpoff, Jean-Philippe Rigaud, Ofer Bar-Yosef, Fred Smith, Steven Paquette, and Randall White. A heavy cast of characters! Suffice it to say that Rebecca Cann was referred to on 15 different pages, Allan Wilson on 5 pages, and Douglas Wallace on 2; it was not Douglas who was being attacked and defended! As long as Stringer and Wolpoff have each other to contend with at conferences and in books, neither is going to prevail! Formidable discussants of what might be called the Big Bang Theory (or Garden of Eden) and the Steady State (Evolution Across the Board or a Rising Tide Lifts All Boats) Theory of modern human origins.

There is not a single reference to Greenberg, Goddard, Starostin, Campbell, Thomason, or even Sapir; but there are 2 for Jeffrey Laitman and Phil Lieberman. Trinkaus's authors are the hardware people and even linguistics is outside of their purview. Have any of you noticed this: that biologicals know some theoretical linguistics, but no historical, while European archeologists are the opposite?

III. J.P. Mallory. IN SEARCH OF THE INDO-EUROPEANS: LANGUAGE, ARCHEOLOGY AND MYTH. Thames and Hudson Ltd., London. 1989. First recommended to me by James Egan to whom I am most grateful for finding this wonderful book! Mallory goes at the question of the IE homeland and subsequent migrations therefrom by simply reversing the process and taking the various IE daughters step by step from their modern locations back to where they must have come from. While some good and true colleagues detest Mallory's book or disagree with his conclusions, I found myself shocked by a kind of déjà vu. His reasoning and conclusions were such that I thought he had been auditing my remarks to my classes, above all on dispersal theory, over the past 25 years. Naturally, I have to agree with Mallory and admire him because he's my psychic twin! (But very dour [duur] and sober-sided) Although his last chapter got all fussy about ostensible minutiae in European archeology, his main finding is that south Russia or the Pontic-Caspian or North Pontic area is most likely to be the IE homeland. I agree with him. Moreover, he is opposed to Renfrew's theory, Ivanov & Gamkrelidze's theory, and the new biogenetic 'wave of advance' theory (i.e., that farmers from Anatolia brought IE language with them). Me too! He even concluded that what I call the old Danubian Neolithic, spreading farming northwestward in Europe, was actually associated with ". . . a language similar perhaps to a distant ancestor of those that are historically attested in Anatolia such as Hattic, and that this would provide us with our earliest retrievable evidence of substrate languages in Anatolia and the Aegean. This language may well have spread through Southeast Europe where, in the course of several millennia, it underwent regional differentiation in the farming villages of the various cultures of this region. . . . Only Etruscan, Tartessian, Iberian, Basque and Indo-European survived into the written record." The genetic findings that Robert Sokal announced in NATURE (see MT-13) were probably accurate but associated with the wrong linguistic group. It was not IE but rather Macro-Caucasic!

IV. Michael H. Brown. THE SEARCH FOR EVE. Harper and Row, New York. 1990. It was written a bit before Mellars' book. Brown as a science writer like Roger Lewin (the SCIENCE article) or Robert Wright (the ATLANTIC article) was encouraged by a senior editor (Craig D. Nelson) at Harper & Row to do a book on the Eve business and Rebecca Cann's theory (cum Wilson and Stoneking) of an African homeland for modern humanity. The result is 367 pages of comment and reportage, falsehood and remarkable fact, gossip & journalism & science, all mixed up together. Like Wright's, eh?

Allan Wilson avoided Brown who wanted to focus on Wilson, hence Brown's evident hostility to Wilson which positively gleams throughout the book. He was also probably a pain in the brain to kindly Rebecca

Cann who talked to Brown a lot, thus becoming one focus of the book. Scores of big name paleoanthropologists and other biologicals were interviewed. The old Johanson-Leakey rivalry and bitterness was played over several times. Bombastic Wolpoff was featured as a kindly cave bear attacking Cann and Wilson. Und so weiter. But the book contains a very good general summary of the issues involved in the emergence of modern man debates and a lot of relaxed, easy-to-understand de-mystifying of the tiresome technicalities of anthropology. If you want a short cut to understanding much of what has been talked about for the past 10 years in our business, this book is for you. It is not really stupid and Michael Brown basically understands what is going on, even if the gossip and repetitions are irritating.

Brown became a vacuum cleaner in the last few chapters, turning finally to a survey of the biologicals' literature. There are references to the important French work on chromosome Y, to other French genetic work, to Arensburg's refutation of Lieberman on the speech of Neanderthals (in NATURE, vol.338, April, 1989) but not to Lieberman & Laitman's counter to that also in NATURE (DATE??). Also to much Japanese genetic work and the "thermoluminescence dating" which changed the dates of Homo sapiens sapiens fossils at Qafzeh in Israel from 92,000 to 115,000 BP. Much of this work had been reported to me by colleague, Eric de Grolier, including the name of the French geneticist, Gérard Lucotte, whose work on chromosome Y had led him to the conclusion that Adam lived in Africa too, réellement parmi les Babinga pigmies in the heart of central Africa. Go read Brown's book or write to Eric de Grolier!

As a final footnote to these books I'll try to give some dates and sites more au courant than mentioned before. Combining various finds in South Africa (Florisbad), Tanzania (Laetoli 18), Ethiopia (Omo I and Omo II), Israel (Qafzeh), and Morocco (Jebel Irhoud), we find at least 5 cases of anatomically modern humans older than 100,000 years in eastern Africa and the Middle East. While dating continues to be a problem, it seems that Florisbad shows the oldest Homo sapiens of all of them -- perhaps 200,000 years old. May I remind everyone that this specimen -- just "one cranium with a relatively high forehead" -- is 75,000 years OLDER than the European Neanderthals of 125,000 BP? This is according to Günter Bräuer (Institut für Humanbiologie, Universität Hamburg) and Richard Klein (Dept. of Anthropology, U/Chicago), as reported by Brown. Eric de Grolier also said that Neanderthal is 'quite certainly' descended from the archaic Homo sapiens or advanced Homo erectus of Europe of 400,000 BP and thus a parallel branch | / not leading to modern men. But the Steady State theorists, according to Geoffrey Pope (U/Illinois), have a clearly Asian or Mongoloid modern human from far northeastern China, dated from 100,000 to 280,000 years old. It is called Jinniushan. There is another group of 6-8 fossil Asians from Shanxi, also dated to more than 100,000 years. However, Chinese dates are said to be subject to change. Well, heavens, we have seen Amerind dates go from 30,000 to 1000 and Israeli dates go from 40,000 to 115,000! Who is kidding whom? Dates simply cannot be believed until they get tested a few times and argued over! Finally, there is a very interesting postcranial piece of Homo sapiens sapiens from Crete with a

date of 50,000 +, according to Bräuer. That may match the now oldest Australian, reported by Sandra Bowdler in the Mellars' book and by Eric de Grolier from other sources. First peopling of Australia in 55,000 BP.

L.O.S. MEETINGS.

Due to an unfortunate breakdown in communications, we did not notify long rangers about the meetings of the Language Origins Society. It would have been announced but nobody ever told the editor! Just one of those oversights. In any event the L.O.S. meetings were held in De Kalb, Illinois, USA on July 18th and thereafter for a few days. Some long rangers attended and gave papers, including Gordon Hewes, Eric de Grolier, Vitalij Shevoroshkin, and others. I only saw the list of papers once. Eric and Gordon are charter members of L.O.S.

Professor Dr. Walter A. Koch (Englisches Seminar, Ruhr-Universität Bochum, Universitätsstrasse 150, Postfach 102148, D-4630 Bochum 1, Germany) has written a book on language origins, focusing on the great European glacial periods. It will be reviewed by Eric de Grolier in MT-15. I apologize to Professor Koch that I gave the book and the announcement to Professor de Grolier and so it is not at hand now to give to our readers. One may write to Professor Koch or the publisher (Brockmeyer of Bochum) if one is impatient to read the book.

Professor Gyula Decsy in 1990 wrote a book, entitled THE URALIC PROTOLANGUAGE: A Comprehensive Reconstruction. Now in 1991 he has written THE INDO-EUROPEAN PROTOLANGUAGE: a Computational Reconstruction. The first has 147 pages, the second 240. Both were published by Eurolingua, P.O.Box 101, Bloomington, IN 47402-0101, USA. The Uralic is paperback (good quality), costing \$26, while the IE is hardback \$42. They are Volumes 9 and 10, respectively, of the Bibliotheca Nostratica (ISBN 0342-4871) which are "Monographs on Interphyletic Linguistics, Language Origins Research, Protolinguistics, Preprotolinguistics, Long Range Language Comparison, Global and Intercontinental Linguistics." I should have mentioned this series to you before. There is a powerhouse of long ranger activity under Gyula Décsy's general supervision, with a tendency to focus on north Eurasian languages. Eurolingua also publishes the URAL-ALTAISCHE JAHRBÜCHER or URAL-ALTAIC YEARBOOK which has been the source of many articles on Nostratic topics. It is also a place where long rangers can comfortably send their long range articles for publication without fighting their way past a thicket of myopic conformists. The two books will be reviewed in MOTHER TONGUE; they are good and solid things. The rule is first come, first served. While it might be preferable to get an expert's opinion, it might be interesting to see what an Africanist or Papuanist thought of them. But you must actually review the book or I will come take it back from you. If no one responds before October 15th, I'll review them myself!

Going back to Petraček's critique of my 'nose'. Part of the awesome de Grolier mind is his memory. While I often cannot remember what I did two days ago, Eric remembered that I had never responded to our late great colleague, Karel Petraček. He had said that my 'nose' cognates (Circular 1) ran afoul of the Saharan data, especially since the group had both

/sina/ and /kina/. Supposedly the /sina/ could be derived from /kina/, and one would assume so, then therefore the /sina/ were misleading. His implication seemed to be that therefore one should beware of going around the world looking for /sina/ when it might be derived from /kina/ at various places. If you look for similar rocks, some of them may turn out to be hard balls of mud. So what, Karel? I should have said to him. Maybe we will find other /kina/ and determine that the world famous /sina/ root for 'nose' or 'smell' was palatalized down from an original /*kina/. If one started with French and Spanish, and switched to Slavic, one could find many sets of /s/ or /č/ matches until you ran into Sardinian /k/ and others. Or the Nostratic examples that match /b/ or /bh/ until they met /bʰ/ in southern Ethiopia. They handled that problem by denying the /bʰ/ or forgetting it like I did the /kina/. We do our best, if we can remember to.

W. Wilfried Schuhmacher published an article in ZEITSCHRIFT PHON. SPRACHWISS. KOMMUN. FORSCH (ZPSK), Berlin 44 (1991) 3, 290-294. Its title " 'Ado about nothing' or 'Evidence': Austronesian and Eskaleut" is backed up by 26 etymologies. Let the reader judge for herself. As is the fate of most such ventures, as we all find out, some etymologies seem very promising and some do not. He also has an interesting review of the literature, as follows:

"There have earlier been attempts to relate Austronesian (languages) to other families/languages (cf., e.g., DYEN 1971: 17-19). In 1889, Ernst Kuhn proved a genetic relationship between Munda, Mon-Khmer, and Vietnamese; to this Austro-Asiatic family, WILHELM SCHMIDT (1906) and KURT WULFF in 1908-10 added Austronesian (Malayo-Polynesian) resulting in the new language class "Austrie". Later, in a work published posthumously, WULFF (1942) proved genetic correspondences between Austrie and Thai-Chinese. On the same level, PAUL K. BENEDICT (1942, 1966, 1967) worked out his Austro-Thai studies where he showed correspondences of the Kadai and the Thai groups with Austronesian.

As for the more "untraditional" attempts, for more than a century, beginning with FRANZ BOPP in 1840, scholars have been toying with the idea of a remote genetic relationship between Austronesian and Indo-European. One of the more serious studies was carried out by ISIDORE DYEN (1970), gathering 78 'matchings' between the two families. The latest study in this field is that of BRUNNER and SCHAFER (1982) setting up an Austronesian vocabulary 'with Semitic and Indo-European roots'."

"Not only an Austronesian-Indo-European relationship but also one between Eskimo and Indo-European has been proposed in the course of time (cf HAMMERICH 1951 for a critical state of the art). BERGSLAND (1959) has pointed to Eskimo similarities with Uralic. Indeed, since the days of RASMUS RASK, Eskimo has been a used tool for building up possible genetic relationships with other language groups. MORRIS SWADESH may have reached a temporary climax putting forward the theory 'that Wakashan, Eskimo-Aleut, Chukchian, Yukagir, Ket, Uralian and Altaian form a phylum, "Altai-Wakashan", whose common period dates back about 9,000 years. Relationships with Indo-European, Koreo-Ainuan, Penutian, Salishan and others in both Eurasia and America must be attributed to a still older time' (1972:706)." (Dear me, I knew that Morris enjoyed his work but this ...! HF)

"Supposing a relationship of Indo-European with Austronesian and Eskimo respectively, the logical next step must be to propose an Austronesian-Eskimo -- more correct: Austronesian-Eskaleut hypothesis." And so Wilfried does, concluding finally : "having in mind recent macrolinguistic attempts, a comparison at a higher level, viz. between Nostratic (including Indo-European and Eskaleut) and Austric (including Austronesian), may yield additional evidence." And so it may!

Wilfried Schuhmacher struck again; in ZPSK (1991) 3, 372-388, he reviewed Paul Benedict's JAPANESE/AUSTRO-TAI. Noting that Benedict stressed the importance of Austronesian in reconstructing proto-Austro-Tai and followed Blust in also stressing the importance of the Formosan sector of Indonesian, Wilfried had these mildly critical things to say: "CHARLES DARWIN had a contemporary companion in ALFRED RUSSEL WALLACE; mutatis mutandis, PAUL BENEDICT had his in the Dane KURT WULFF who (also in 1942, not referred to in this book) demonstrated the existence of a special relationship between Tai and Austronesian (and also between Tai and Chinese). In his unfinished and posthumously published work, WULFF utilized the old hypothesis of RENWARD BRANDSTETTER that Austronesian polysyllabic, mainly disyllabic, words originated from monosyllables, as a result of affixation or, to some extent, duplication; recently recapitulated by ROBERT A. BLUST in his AUSTRONESIAN ROOT THEORY (1988; not referred to by BENEDICT). It seems also that within the framework of Proto-World or global etymologies the Austronesian monosyllable theory does find support; ... (he cites evidence. HF)."

"As for Austronesian-Japanese proper, the works by SHICHIRO-MURAYAMA and EVGENU D. POLIVANOV are missing here, and not referred to are also the various articles of SØREN EGEROD on Tai-Austronesian vs. Tai-Chinese relationships."

"Before making expeditions into the neighbourhood, it seems the Austronesian house has to be cleaned up first, following the prescription as given by BLUST."

The bibliographic items listed here only by name are available in the original article or by writing to Wilfried (Kirkebakken 13, 4621 Gadstrup, DENMARK). The same kinds of questions were raised by Lyle Campbell in his assault on Greenberg who replied that he had too much bibliography to publish, i.e., his publisher would forbid it. But also there is another general comment to make: Suppose that Paul has already rejected Wulff's and Brandstatter's monosyllable argument, which I bet he did long ago, then is he obliged to mention them? People do not always like to say negative things unless it is important. Hmm?

Our esteemed FELLOW and general guru, IGOR DIAKONOFF, has published an article entitled "The Importance of Ebla for History and Linguistics" in Cyrus H. Gordon, Editor, and Gary A. Rendsburg, Associate Editor, EBLAITICA: ESSAYS ON THE EBLA ARCHIVES AND EBLAITE LANGUAGE, Volume 2, 3-29. The year is 1990; the publisher Eisenbrauns, Winona Lake, Indiana. Since there was great excitement back in the 1970s about this 'new old' Semitic language and its very antique civilization (3rd millennium BC), followed by repeated rumors that 'someone blew it' meaning that we might

not ever get good information, it is gratifying to read uncle Igor's rich article. Too much to summarize here, except for a piece of his conclusions:

"According to the present data, Eblaite is a very archaic language of the Semitic family in the Afrasian linguistic phylum. It seems that the number of isoglosses connecting it with Northeast Semitic Akkadian is almost equal to the number of isoglosses with West Semitic languages (according to M. Dahood with Canaanite, according to E. Lipin'ski with Aramaic; however note the important glosses with Arabic and South Semitic). All this proves that Eblaite was a part of the Common Proto-Semitic dialect continuum,³⁷ with a position between East and West Semitic. It seems to have separated from the continuum for a relatively short time, but its isoglosses with West Semitic (including Arabic) seem to be more innovative and hence more significant for classification."

³⁷ Like all "proto-languages," Proto-Semitic was, of course, not a "language", but a dialect cluster. End of quote.

Well, my friend, I feel myself in the presence of dogma. Here's a question for you. Do all modern languages constitute dialect clusters? If they do not, then why assume that all protolanguages were dialect clusters?

Thanks to Dan McCall for sending a few pages of RESEARCH NEWS, from SCIENCE, vol. 252, 1614-1617. Therein "A GENETIC SURVEY OF VANISHING PEOPLES" is discussed. Sub-title is "Racing the clock, two leaders in genetics and evolution are calling for an urgent effort to collect DNA from rapidly disappearing indigenous populations." The two leaders are now one, unfortunately, since they were L.L. Cavalli-Sforza and Allan Wilson. We wish him well. Most of us would agree that the project is worthwhile and most of us would help out, if asked to. If Luca wants, members can send lists of which peoples they think most urgently need to have their DNA collected.

Thanks to Claude Boisson we're in possession of an important article for long rangers. The author is KAMIL V. ZVELEBIL (University of Utrecht, The Netherlands). He published "Long-Range Language Comparison in New Models of Language Development: The Case of Dravidian", in PILC JOURNAL OF DRAVIDIC STUDIES, 1:1/January 1991, 21-31. PILC stands for Pondicherry Institute of Linguistics and Culture, Pondicherry, Madras (state), INDIA. Since the cover of the journal features Chomsky's sayings as its logo (!), one can hardly think of a less likely place for an article with such a title (except for LANGUAGE). Zvelebil being a Dravidianist or Dravidologist of rank, the omens look good. Before his data presentation he discusses some theories of language development and how many different people had proposed relating Dravidian to "every possible candidate at kinship". Following that is his EDUAJ hypothesis. Instead of drawing his diagram, we'll reproduce it this way:

```
EDUAJ ----> J + EDUA      (J)(EDUA)
EDUA ----> ED + UA      (ED)(UA)
ED -----> ELAMITE + DRAVIDIAN
UA -----> URALIAN + ALTAIC
J -----> JAPANESE
```

One point which we can note in his long discussion (q.v.) is that the Japanese connection with Dravidian is a product of the labors of Susumu Ohno, who has persevered "in spite of a vicious attack by Roy Andrew Miller". The latest Ohno publication on this subject is "The Genealogy of the Japanese Language -- Tamil and Japanese." In GENGO KENKYU 95: 32-63. But one cannot help wondering when serious scholars will quit wasting so much time on binaristic comparisons. It is too primitive an approach for long range comparison. People spend so much time and effort and typically get nothing from it. Then they condemn long range work -- in their frustration.

ANNOUNCEMENTS

Overleaf please find two announcements of coming conferences. One will be very soon. The other is next year on the Riviera, ho ho! This week I have received five conference announcements for events which will be held within two weeks. It would be a waste of time to mention them. It is finally dawned on me that the reason so few conference announcements get to me these days is that I've moved twice in two years. Those who do the lists have lost me!

EXCHANGE

Since there have been no responses to the advertisements that five different, but beautiful, people were looking for jobs or positions in graduate schools, we will abandon this feature. The reality is that more and more people tell me they need work but cannot find it. The new people on the list are very competent and surely interesting. What they have to offer include Semitics, Burushaski, Sumerian, Indic of the far northwest, Afroasiatic, und so weiter.

APPENDIX

Parasession : "Classification of African Languages and Researchs of Genetics Relations"

The research of genetics relations will be discussed, more specifically situations where languages are in contact.

"Nilo-saharanistists" as well as "comparatists" working on other African linguistics families are invited to participate.

General overview :

1. "classical" researchs,
2. researchs which deal with crucial questions concerning languages in contact and their evolution,
3. researchs on the interaction of language function and linguistic structure.

Themes developed :

1. theorization in comparative research,
2. language contact phenomena and formation of new languages,
3. classical methodologies and new prospectives,
4. research tools,
5. influence of empirical data on theory and methodology.

Fifth Nilo-Saharan Linguistics Colloquium Cinquième Colloque de Linguistique Nilo-Saharienne

URA 1235 - CNRS
University of Nice-Sophia Antipolis

Notice n° 2
July 1991

The Colloquium will take place at the

University of Nice-Sophia Antipolis
August 25-29, 1992

Deadline :

We request an extended summary by

April 30, 1992

to the following address :

IDERIC - URA 1235 CNRS
Cinquième Colloque Nilo-Saharien
63 Bd de la Madeleine - Bat. A - 06000 NICE - France
(Tél. : 93.44.82.44 - Fax : 93.37.55.36 - Earm : Nicolai @ FRNICE 51)

After receipt, all summaries will be sent to each participant and the program of the Colloquium will be definitely set up.

Plan to Attend

the 11th Annual International Conference on Native American Language Issues

The purpose of the Native American Language Issues Institute conference is to protect, preserve, and promote the indigenous languages of the Americas. This is achieved by providing a facility where education professionals, language specialists, and people of the First Nation communities gather to discuss indigenous language concerns.

Presenters and workshop facilitators from across Canada, the United States (including Hawaii and Alaska), and New Zealand have already committed to attend this special conference.

Attached is a request form for more information. If you would like more information about the NALI '91 Institute conference, complete the attached form and return it in the self-addressed envelope provided.

Applications to make a presentation, or to facilitate a conference workshop will be accepted until June 15, 1991. For more enquiries write to:

NALI '91,
P.O. Bag 7000,
Vanderhoof, British Columbia,
Canada, VOJ 3A0.

Or telephone (604) 567-9236

Conference Opening Sunday, September 22, 1991

Displays of the school program, teacher training, and technology for languages will be open. This multi-media display centre will remain open from Sunday noon until 3:00 p.m. on Wednesday.

Cultural exhibits and professional displays are welcome.

Contact:

NALI '91
Sharon Bird
Conference Coordinator
P.O. Bag 7000
Vanderhoof, British Columbia
Canada, VOJ 3A0
Telephone (604) 567-9236 or
Fax (604) 567-3851



N.A.L.I. '91 Institute is hosted by
Yinka Dene Language Institute

Monday, September 23, 1991

- Opening Ceremonies
- Keynote Address
- Workshops
- NALI '91 Business Meeting
- Exhibits and Displays

Tuesday, September 24, 1991

- Opening Plenary
- Workshops
- Exhibits and Displays
- Banquet and Entertainment

Wednesday, September 25, 1991

- Opening Plenary
- Workshops
- Exhibits and Displays
- Closing ceremonies



L E T T E R S

ROGER BLENCH had been in Nigeria for some time, counting all the cows and other livestock, when he wrote in February. His letter got bumped in MT-13 because of space. Back in England he will be at the London School of Economics for two years from this October, doing Developmental Anthropology. Among other things he reports:

"Something that has been on my agenda for a very long time, the revised Index of Nigerian Languages (now re-christened Atlas) is finally being put to bed. S.I.L. is doing the publishing and getting the map (a colour masterpiece) printed. It might just be available at the end of the year. Since this thing summarizes a decade of wandering around Nigeria inquiring into the roots and stems of things, I shall be pleased to see it in print and get on with Edition Three. I have also been asked to help put together a map of African Languages for some UNESCO project..."

"Mother Tongue seems to have reverted to something like it was before under your revived editorship. It seemed to have gone from a genuine investigation of World Languages to a rather ingrown journal for a very particular set of debates. I hope you can recover some interest from the Africanists who I suspect may have tended to go off in other directions"

"Enclosed is a map that I have done for .. Well, you maybe remember the World Archeological Congress in Southampton a few years back? I was invited to give a paper in the Neolithic section on the significance of new developments in African Language classification for hypotheses about prehistory. The paper was given and duly sent in for publication -- but nothing happened for six years. Then two months ago, Peter Ucko suddenly asked me to revise the paper! Six years is a long time and I needed it like I needed horseflies. Nevertheless, I redid the map on computer and the paper by hand, since I had by then lost the original computer file. Anyway the map is just for comment and interest. I'll send you the article when it is published. What is your present view of Ongota, Shabo, Hadza and Sandawe. As you see, I have marked them isolates but I'm still not clear exactly what your view is. Is Shabo the same as Mekeyir? Confused. There is a language, Laal, in Chad that does not seem to fit into any of the major families, though it has borrowed so extensively from Adamawa and Chadic that you cannot say this for certain. Still, I think it needs to be on certain lists."

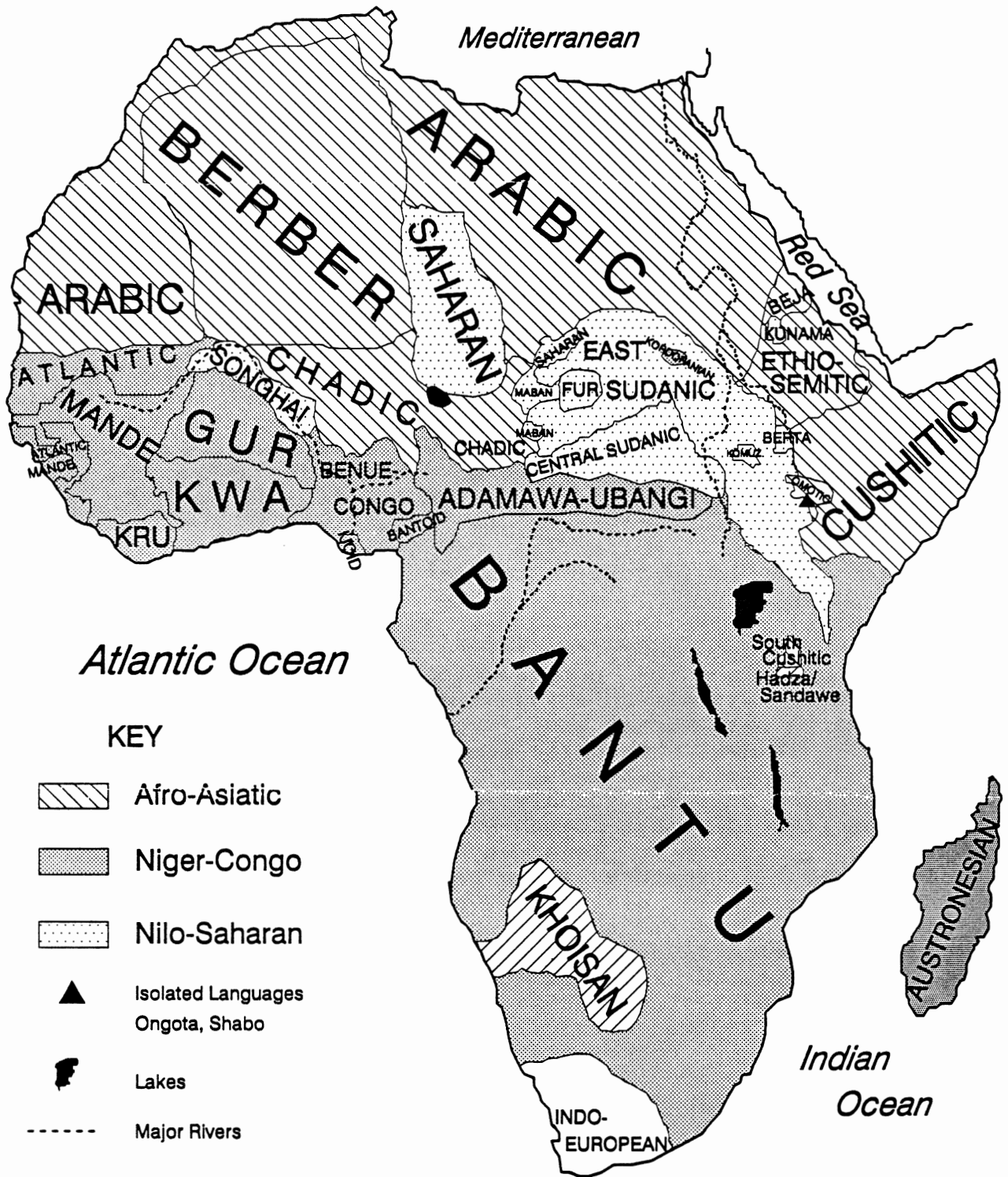
"I am glad you have fixed up a means of paying in Europe ... but as you know it costs you nearly as much to transmit dollars to America as the total amount of the subscription..."

Well, we owe that to Ekkehard Wolff. Re Mekeyir. It = Shabo. Hadza and Sandawe have been stable since that conference where we decided to assume for the nonce that they were not all related. Those two are both Khoisan, in my opinion. See MT-12 for opinions about Ongota and Shabo.

Calling all Africanists! Can anyone get some data on Laal? To swap? Especially data where the borrowings are marked? Or publish in MT?

Roger Blench's

OUTLINE MAP OF AFRICAN LANGUAGE FAMILIES



5/23/91
Karl Krippes
BBHN C206 Hepburn House
Bloomington, IN 47406

Harold C. Fleming
President
ASLIP
5240 Forbes Ave.
Pittsburgh, PA 15217

Dear professor Fleming,

thank you for issue 13 of Mother Tongue. I have been busy doing proto-Mongolian reconstructions for my Ph.D. dissertation, and haven't had time to polish up any of the projects which might be suitable for Mother Tongue. Having completed the first draft, I can take a moment to write to you.

I assume, perhaps wrongly, that specialists in other language groups are vaguely interested in an insider's view on problems in Altaic linguistics. In the Department of Uralic & Altaic Studies, Indiana University, where I am about to finish my degree, it's okay to use the word "Altaic" as long as you don't prefix the word "proto-". Of the few faculty members, past and present, who can claim to be "Altaicists", two prefer to be identified or labelled as "Mongolists" and the third as a "Turcologist". In this situation, and considering the focus of my dissertation, I must content myself with the label of "Mongolist", even though I have any equally solid background in Uzbek and Manchu (not from any relatives though).

I entered the program to investigate the Korean-Altaic linguistic affinity, a question which interested me upon my initiation into Korean some five years ago - I have been speaking Korean on a daily basis for at least that long.

Having said that much, I must apologize for not having any such depth or even acquaintance with Japanese and Ainu. But because the Japanese-Korean-Ainu affinity is another long-time interest, I try to keep abreast of the Japanese and Ainu linguistic literature, while having an active interest in Middle Korean and Korean dialects. So, at least in the case of Korean, to "have studied the subject sufficiently", takes second place to actually being fluent in the spoken and written forms.

However, in response to Karl Menges, I wish to politely disagree about the necessity to clarify my opinion of the relatedness of the Altaic languages (or that my opinion should reflect on my competence in comparative linguistics and the comparative method). Nonetheless, I usually find readers and referees insistent on telling me where I stand on the issue no matter how much I attempt to remain open-minded and willing to weigh arguments from both sides of the issue, or to present both sides so that the reader/referee may make his/her own judgement.

I agree with professor Menges that quotation out of context can lead to misrepresentation of another person's ideas. On the point of quoting the source of an linguistic comparison, who, for

example, has taken the time to find out that Meillet first compared Armenian kamurj 'bridge' and all the dialect variants of Greek gephura id. ? I strive to give scholarly credit where it is due, as in the "Altaic Component of a Nostratic Dictionary" (Mother Tongue issue 11). It would be a completely different type of academic exercise to find out which comparisons Ramstedt borrowed from Sanzheev (1930) and vice versa, or which comparisons Sanzheev borrowed from Schmidt (1898-1928) and so forth.

In my contribution to Mother Tongue issue 11, I tried to insist more on what linguistic and philological facts a reconstruction is based on, rather than the mere fact that a certain form is asterisked.

Up till now I was fond of relying of Martti Räsänen (1969) for proto-Turkic. Having learned that his work is a compendium of Radloff, and repeats many philological mistakes and inaccuracies, I use both Shcherbak and Clauson for proto-Turkic. Because of the "puerile" arguments (as Miller has called them) which Clauson is notorious for in his anti-Altaic articles, I cannot help but feel a sense of embarrassment at quoting Clauson's Etymological Dictionary of Pre-Thirteenth Century Turkish (1972), which by itself is a more solid piece of linguistic and philological work.

At the risk to boring you further, let me thank you for welcoming my two contributions to MT and for keeping MT as a forum. I'm saddened that only the smaller of the two papers evoked even a passing reaction from one of the more prominent ASLIP members. I hope that whether I'm welcomed as a Mongolist or an Altaicist (the dying breed), that I may continue to offer corrections and clarifications on Altaic and Korean as they concern Nostratic.

Sincerely yours,

Karl Krippes

Karl Krippes

P.S. You'll be pleased to know that in a forthcoming review article of Benedict, I had the occasion to quote from two issues of MT.

P.O.Box 142
Uniport Post Office
Port Harcourt
Nigeria

31 May, 1991

Harold C. Fleming
5240 Forbes Avenue
Pittsburgh, PA 15217
U.S.A.

Dear Hal,

MT 13 arrived today and as usual I have read and skipped thru it with fascination. Sorry I don't reply more often -- I'm just a bad letter-writer! I think you asked me last time if I was about to retire; the answer is NO, not until I reach the Nigerian retiring age in 1995. I am desperately overworked and am really looking forward to my sabbatical next year; I shall be in Oxford in 1992 to finish writing up my Proto-Ijo and Proto-Igboid -- the latter jointly with Chinyere Ohiri-Aniche, who is in the last stages of producing an exciting Ph.D in which she reconstructs the proto-language of Yoruboid, Igboid and Edoid. There are also here a bunch of MA theses I have supervised which reconstruct subgroups of (New) Benue-Congo; Proto-Akpes (a previously barely-known dialect cluster), by Femi Ibrahim, Proto-Kegboid, alias Ogoni, by Suanu Ikoro, Proto-Central Delta, by Inoma Alex, and Proto-Bakor, an Ekoid Bantu language, by Osbert Asinya; as well as Proto-KOIN (Kalabari-Okrika-Ibani-Nkoro), a part of Ijo, by O.G. Harry. It is so sad that we have nothing like University Microfilms here; some years ago we found it costs \$50.00 to put ~~your~~ *one's* dissertation there, and that's a month's salary for a junior academic here. This is all crucial data.

I find the Niger-Congo scene really exciting now. There is also a reconstruction of Proto-Lower Cross coming up by Bruce Connell; have you invited him to become a member of ASLIP? (The Phonetics Lab, 41 Wellington Square, Oxford OX1 2JF, England.) We keep reconstructing lenis consonants in one group after another, and John Stewart keeps propounding new hypotheses about their development.

Incidentally, re your remarks on pp. 33-34 about the extreme parochialism of historical linguistics, I teach it mostly with Niger-Congo examples -- tho I do usually mention Grimm's Law briefly in case my students ever get to a European/American conference! I enclose a copy of my "inaugural" (many years after) lecture as a sample. *I meant to send you this before, but I guess I didn't?*

I am delighted by the deeper time depths proposed for Amerind (pp.3, 5, etc.). I've always felt but have no way of proving that in Niger-Congo we need much more than 10,000-12,000 years, and of course if we link up Nilo-Saharan, or perhaps take Niger-Congo as one of the younger branches of Nilo-Saharan, we shall get much deeper again.

I must register a protest over your remarks on the IPA (p.17)! The new IPA was discussed quite widely before the conference, and it has many features which are a great improvement on the old one. And it IS international -- altho there are a lot of Americans they do only occupy one country!

Glad you've elected Ben Elugbe -- but he's at Ibadan, not Port Harcourt, as I suppose you've discovered by now.

I have a feeling I should have paid some dues last time, so enclose a cheque for \$10.00. Why not introduce life membership? It would give you some lump sums which you could invest to the benefit of ASLIP, and is convenient for people like me who have money right now but might not in future.

With best wishes,

Kay
Kay Williamson

EDITORIAL: Thoughts on a few topics.

Eric de Grolier paid me a visit this summer and stuffed my head full of new thoughts and information. He may be the smartest and/or best-informed long ranger of all! His thoughts confirmed a substantial portion of what I had been reading and gave me a better grasp of French thinking in particular on prehistory. Sparks flew from time to time, as they always do when 'Anglo-Saxon' and 'Gallic' minds engage each other seriously, but we agreed on more things than not. Nevertheless I am in awe of Monsieur de Grolier!

However, on two vital matters we disagreed flatly. His stance would, I think, please the greater number of ethnologists, especially symbolic anthropologists. Our definitions of language were different and what followed from that was crucial. We agreed on the usual set of attributes: language is a social phenomenon, it embraces concepts, it is highly structured or has syntax and morphology and rules, it saturates human minds, it changes over time, and so forth. And, of course, one of its critical defining characteristics is that it is SPOKEN, that it critically involves a mouth-to-ear channel, that its arbitrary phonemes are vital to the arbitrary semantics, and so forth. Or human natural language is different from other forms of communication most crucially because it is SPOKEN.

Eric did not agree that the spoken part was crucial, nor did he agree that language was limited to human natural languages, or roughly whatever it is we mean by French, Tibetan, Ongota, and Tlingit, but not written Chinese. Therefore it was possible for him to see 'language' as present in the new evidence that various animals have concepts or use symbols or think or the like.

Our second flat disagreement followed upon the first. Could one hope to find the origin(s) of language? Yes, said I, thinking of a spoken language and an anatomically modern human, we can find when we were able to speak and compare that to other evidence of symbolic qualities and our reconstructions back to earliest proto-human. No, we cannot, said Eric, thinking of how old abilities to think and use symbols and communicate must be in various animals and surely in early human animals like the Australopithecines. We will probably never know, (I would add) because it is not a matter of some changes in the bones of the throat.

Thus we realized to our mutual surprise that we differed on the definition of the phenomena in question and whether their origins were retrievable (discoverable) or not. We also realized that many other long rangers shared the same disagreements or lacked agreement on these essential or basic points.

There is no way, normally, in science to force definitions on people, unless you fail them in graduate school or don't give them tenure or beat them with sticks. So Eric and I agreed to live with our definitions, respecting the other's right to remain disinformed. Still it is important for all of us to be quite clear about our conceptions of language. Mine is a fairly standard definition used by linguists. Eric's is more like an ethnologist's or archeologist's or paleoanthropologist's (who think you "have language" because you can make a tool. Notice that they hardly ever say you must be able to speak because your hands can make tools.)

This all leads to profoundly different views on language origins. For those who do not define language as part of culture, like me (but not Dell Hymes or Raimo Anttila), language is very unlikely to occur before Homo erectus, possible to occur during archaic Homo sapiens and/or Neanderthal times, and likely to occur during proper Homo sapiens sapiens times. Culture or symbolic behavior could occur much before that, say well before Homo erectus, and even in a rudimentary sense among other apes (see Jane Goodall's THROUGH A WINDOW). It could be communicated through gestures (kinesics), as Gordon Hewes has maintained. Language in my sense of it is obviously intimately connected to culture in a great symbol system in our brains (but also) in our interactions. There is a heavy area of overlap between linguistic and cultural symbols -- in the lexicon. But other aspects of both language and culture are basically independent of each other (e.g., phonology, music, dance, syntax, many kinds of art, behavioremes, etc.). Two related and linked sets of phenomena do not need to be defined as parts of each other. How about saying that culture is part of language? And so forth.

Naturally, language could still be defined as a part of culture, as Raimo defines it, but nevertheless a new part of it, one essentially embodying a much richer set of icons & symbols (morphemes), based on the non-symbolic (non-representational) set of sounds (phonemes), which carry the meanings of a much richer culture. However, but, although: If earlier humanity used gestures instead of spoken language, it does not necessarily follow that humanity was poorer mentally just for that reason. Or has American Sign Language been found to be much poorer than spoken English in transmitting information? (It may have; I just don't know.) With the advent of spoken language, one hears it said frequently, human communication got richer, culture got richer, and we got smarter. Was it because gesture "language" was inadequate or was it because technology kept quietly growing or was it because we accumulated and learned from a richer human experience? Or what can a spoken language do that a gesture "language" cannot?

As most anthropologists know, once you open up the bag called 'culture' definitions become impossible and arbitrary things. But one sage once observed that, despite "arid definitionism", most of us agreed on a primitive conception of culture. I'm assuming that here, except for the archeologists who do use one more appropriate for their work, but adding that in 1991 at least everybody's 'culture' contains the word 'symbol' somewhere. I would prefer that Eric argue his own case but semantics are crucial to his viewpoint. I think he would say something like this: language is critical, symbolic, mental, conceptual and characteristic of the highest life forms on earth. It is not important that it use speaking as its channel; it could use and does use gestures, body 'language', calls, cries, paralanguage or what have you. The crux of it is communication of symbols and concepts from mind to mind, even between species. The swinging of my cat's tail and my dog's tell me very different things. So language is the communication of meanings, ergo it is the communication of culture -- the system of meanings. Perhaps he might even say: language is merely/simply/just the channel for messages with meanings to be exchanged. (I hope I got Eric's meanings straight!)

Confronting cultural theories, especially those stressing the symbolic, and their supposed relevance to language origins. It is something we must do. L.O.S. people do find evidences of symbolic behavior in archeological prehistory and they do tend to interpret such as evidence or proof of language. So if there is, for example, a rock painting with a wavy line on it ~~~ and it seems to mean "water", then ergo that means the people then had language and a word for "water". Alexander Marshack is a brilliant exponent of this approach. But I think the wavy line ~~~ proves most of all that he follows Monsieur de Grolier's definition of language. Since I believe that most rock paintings have been done by Homo sapiens type artists, I reckon they also had human natural languages and words for "water". But the drawing on the rock proves ZERO about their possession of spoken language. Why should it? If Brutus can gesture ~~~ with his hands, does it prove that he can say "aqua"? Or even knows that there is "aqua" to say? Ah, yes, but Brutus is communicating, using a symbol (an icon, Raimo) to convey the concept of water. That IS language, n'est-ce pas?

When we all agree on what we are talking about, and arguing about, it will help us immensely. Don't you think so? Above all the poor biologicals and archeologists who hated taking linguistics in college and often do not know what they are talking about when they mention language -- literally.

ASLIP BUSINESS.

It will be a good thing that Mark Kaiser does! When he takes over MOTHER TONGUE for a while. Not only in relieving the present editor but also in healing the wounds in our body politic. It is no secret that Muscovite and Bostonian long rangers have drifted seriously apart. As they say in Moscow: "Everything is secret but nothing is mysterious." So let Mark play the role of Brahma while Vicalij and I play Shiva! And while Igor Diakonoff is our Vishnu. (Just a bit of Hindu theology.)

In Ann Arbor, Michigan, a short informal annual meeting of ASLIP occurred on August 15, 1991. It was a response to a suggestion made by Sydney Lamb that we 'piggy back' on the LACUS FORUM meetings. Ours was set up quite abruptly and thus non-North American members were not invited; they could hardly adjust their schedules so quickly. Since we had just enough members present to 'do business', the six of us voted to ask LACUS to give us a piece of their action annually. Otherwise the meeting was a waste of time and money. No long rangers from the state of Michigan showed up; for some a ten minute walk away! The money I spent on this meeting could have produced an extra issue of MOTHER TONGUE, if given for that purpose instead. Tsk, tsk!

We will discuss the matter of 'piggy back' meetings more fully in future at our Board of Directors meeting. As something for members to think about, let me suggest the following:

There may be many 'piggy back' meetings, both in North America and Europe, in any given year.

Neither the President nor the Vice President can be expected to attend all these meetings. Money alone prevents them.

Let the suggester of a meeting also be the volunteer convener of said meeting. Let me invent an example. Suppose that Douglas Wallace wanted to have an ASLIP 'piggy back' session as part of the meetings of the mitochondrial experts of America in Cancun in January. Then he should make the arrangements. We should think of ASLIP as something like a 'daddy long legs' (Phalangium sp). A tiny center with very very long slender fragile legs; enough to frighten the status quo but easy to blow over with a good puff of wind. Try to think of our 'piggy back' meetings as Phalangium's footprints. An ever so light and delicate touch!

Let each 'piggy back' meeting be reported briefly in advance to our officers so that they can assist, if possible. Otherwise let the volunteer conveners send out notices to ASLIP members world wide and far enough in advance so that people can make plans and even submit papers or suggest topics. Our members are great travelers after all.

Let each volunteer convener try to negotiate terms with the host meeting (e.g., LACUS FORUM) such that long rangers are spared the cost of registering for the whole host meeting which can be formidable (e.g., Soc. Am.Geneticists get \$250).

Above all let us all be realistic about ASLIP and MOTHER TONGUE. There is as yet no natural constituency. Our activities are on the outer fringes of awareness for most of our members. Judging from the lessons of Ann Arbor, not even our core members will trouble themselves in any serious way to help out. Loyalty is thinner than condom skin. We have elite Directors and Fellows who don't even believe in our goals, not to mention lower level taxonomic hypotheses like AA!

Alternative Formats

One of our true long rangers has suggested that we stop trying to be a organization which attracts a large membership, stop trying to be a newsletter to a society whose members don't report their activities, and stop worrying about the opinions of those thousands of drone linguists who produce all that trivia. He thinks we should concentrate on productivity and communication among the true workers in this domain. Let us be a small group of as few as 20, he says, who pay actual attention to each other's work, who feed each other data and ideas, and who support each other morally and scientifically. It sounds great and there are two models in existence: (a) the Muscovites and (b) the core group of the Americanist border patrol (Ives, Lyle, Terry, Sally, and Al). (The positivists and the negativists) Let us one and all think about this proposal and the few who give a damn write to me. You will define the potential new ASLIP just by doing that!

Consider another alternative, however. We could become a big association too. My recruiting activity has been confined to serious 'professionals' and personal friends. There are ways of becoming much larger, with a membership in the thousands. And still be a newsletter, albeit altered for style and glossiness of presentation. We could hire a bright editor and turn out a spiffy product which is what many readers now long for. That great financial success could help support

the true long rangers in their work. (Due to economies of large scale production and lowering of unit costs. For you, John) Think about this too.

The format of small elitism above probably would have worked a few years ago. It thrived on good will and cooperation. George Steppenwolff did plenty to stamp out those qualities. Nowadays the "good ole spirit of free enterprise" and competitive individualism (me first) has replaced it. Our 'heroes' rarely communicate outside of little circles and on the whole are quite self-centered. There is some doubt that the elitist format could work at all.

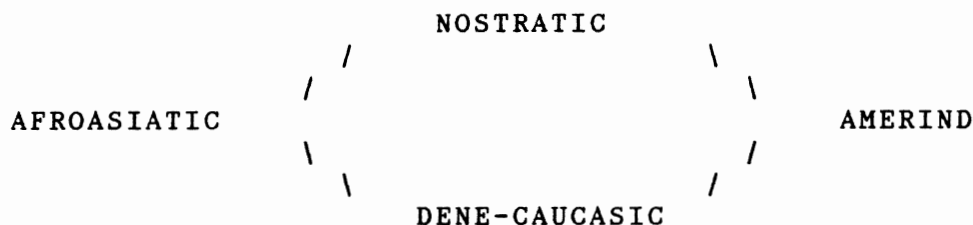
But it does not matter too much. As Kroeber might have said, the cultural impact of long range comparison has happened, even if individuals like Fleming are unhappy about the details. Attitudes towards language origins are changing and there is a huge drive towards a synthesis, 'emerging' in Renfrew's words, drawing together archeologists, biologicals, and a few linguists. The emergence of modern humanity consensus will explain the origins of language, whether we participate or not. So be it!

A NEW TAXONOMIC HYPOTHESIS

BOREAN / BORALEAN by H.C.F.

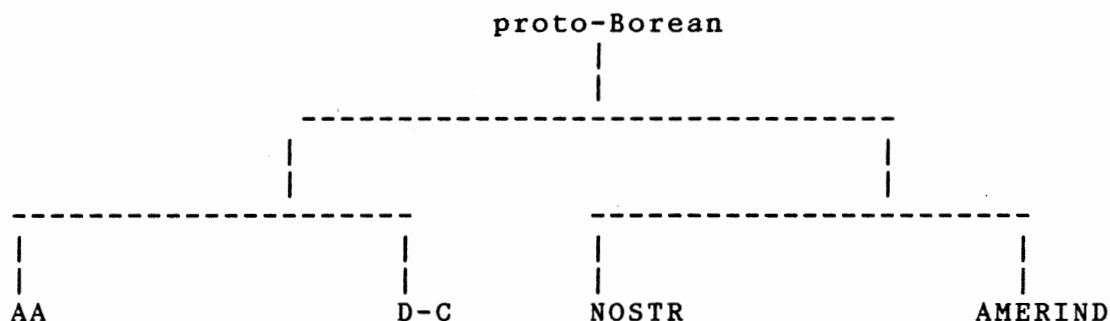
Or AFRO-INDIAN or AA-VASCODENE-AMERIND or
AA :: DENE-CAUCASIC :: NOSTRATIC :: AMERIND

In non-arboreal diagram it has the following shape:



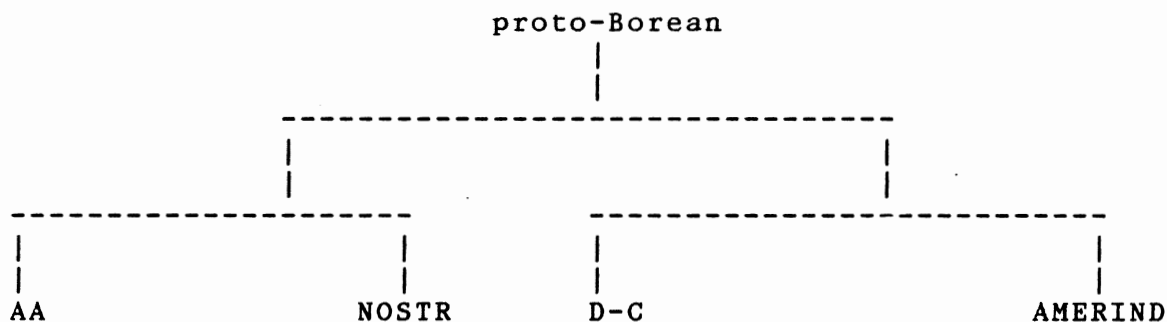
In arboreal diagram it has one of these three shapes:

A.



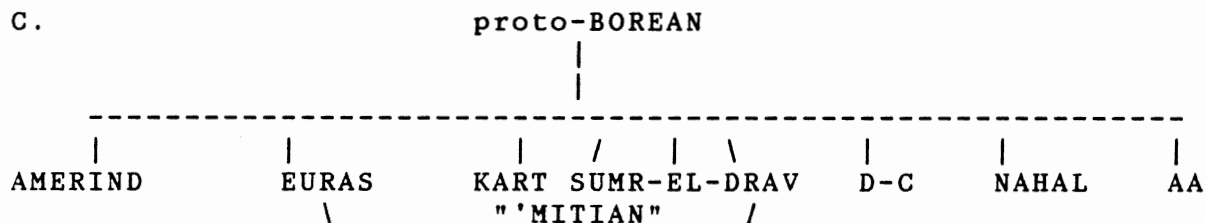
.....or its alternative

B.



.....the preferred alternative.....

C.



Sumerian, Elamitic and Dravidian are SUMR-EL-DRAV. That group +
Eurasian + Kartvelian could be called Nostratic or Mitian ('moi/toi'
pronouns) (my choice). While Nahali is implicitly in the Dene-Caucasic
realm, it is left as found today --distinctive and controversial.

In (C) Japanese is left unclassified out of respect for Paul Benedict's thesis. It has become the most controversial language to classify.

Some attributes of this scheme, whether implicit in one of these diagrams or now to be presented, are as follows:

(A) Whatever the outcome of the sub-grouping, i.e., whether diagram A, B, or C is correct, the group of languages tacitly included are believed to be a proper taxon in overall terms. Each of them is related to the others in BOREAN before it is related to any outside language, presuming that borrowings from outside languages have been identified. That stipulation becomes nearly impossible to sustain in the case of Nahali vis-a-vis the purported Austric super-phylum. It is probably difficult also in the case of Sino-Tibetan vis-a-vis Austro-Thai and it has become unmanageable in the case of Japanese.

Neither SUMR-EL-DRAVIDIAN nor KARTVELIAN are very close to each other. Since Kartvelian is already believed to be related to (North) Caucasic (Gamkrelidze 1974), it might go in D-C. Diagram C clearly states that Kartvelian is related to SUMR-EL-DRAV and to EURASIATIC before it relates to D-C. Ilych-Svitich, Bomhard, and Dolgopolsky have shown amply enough that Kartvelian is very likely to be linked to the old Nostratic bunch. That does not deny a relationship with Caucasic, but their rejection of Kartvelian's kinship with Caucasic puts one farther away. The real question which bedevils the Caucasus is who has seen how much data on the Nakh-Dagestan group? (Or Northeast Caucasic)

(B) There are locations and dates to propose for proto-Borean and the movements of the daughters (descendent branches). Despite the irrational archeological hang-up about migrations, there had to be some huge ones. The dispersal area and date are 1'Iran exterieur (greater Iran) around 45,000 BP. This is by hypothesis. From there the Amerind branch moved northeastward around Tien Shan and reached Alaska around 35,000-40,000 BP, reaching deeper South America quite rapidly after that. This had to be a sweepstakes kind of move with population bottlenecks at some points and it had to be during an interglacial interlude. The speed of their march across Mongolia and/or Siberia, while a mere 5000-10,000 years, was slow when compared with the few hundred years it took the Russians to do the same thing -- against opposition. Despite the stubborn insistence of some that the Bering Straits must have been crossed during a time of low seas (i.e., a glacial episode), there seems to be no good reason to deny the ancestral Amerinds the ability to use marine transportation (e.g., rafts, canoes, etc). See below (D) for Australo-Papuan boating 20,000 years earlier.

(C) Later on, the Dene-Caucasic eastern flank moved into and across northern India, then probably through some Himalayan passes up onto the Tibetan high plateau and so to north China, whence the ancestral Na-Dene crossed maritime Siberia, occupying Alaska and some of northwestern Canada, while the kindred ancestral Ket/Kot moved north into the Yenesei river system there to be cut down slowly over millennia by the various surges of Eurasiatic-speaking peoples across Siberia. From north China or eastern Tibet the Sino-Tibetans later began their slow steady expansion southwards at the expense of the Austric peoples.

Alternatively, ancestral Burushaski, Nahali (and probably Kusunda) eased over into the Indus system, there to be opposed by earlier arrivals (see (E) below, while the rest of the eastern flank of D-C went around Tien Shan (mts.) to the north, via Lake Balkash, thence through

the Dzungarian Gate into Sinkiang, thence northwest China. There is easy access to the Yenesei river system to the north (for Ket/Kot) or Manchuria to the east (for Na-Dene). This later became an Altaic route and most likely was the old Amerind route to the east.

(D) It is proposed that this still undated Dene-Caucasic movement to the east was the third movement into or across India by modern men but the first by a more or less cold-adapted people like other Boreans. It is seen as analogous to much later Indo-Aryan movements into India from the west where a previous set of populations remained partly in place. In this section the Borean hypothesis clearly includes another postulate: there was a much earlier movement of moderns (sapient) from either the Levant or the African Horn across Arabia to India, thence to southeast Asia for some of them. Certainly, these were the so-called Australoids.

These southeast Asians then crossed Wallace's Line somewhere around 55,000 BP and began settling greater Australia, first Australia and then New Guinea (30,000-40,000) and the Melanesian islands (e.g., Bismarcks, Solomons: 28,000-33,000). These movements represented Indo-Pacific and Australian, surely. The bits and pieces of Indo-Pacific, spread from the Andaman Islands to Timor to Helmahera to Papua to near Fiji suggest their earlier distribution and what eventually happened to their older territory (see E below). Despite the easy correlation of Australian languages and first settlement, that theory is specifically rejected. Following Bowdler's and Jones's analyses, it seems that early movements circumnavigated Australia first and probably New Guinea too before settling inland to any great depth. They clearly knew how to travel by sea (bamboo or pandanus rafts), even if only for short distances. It is this fact that makes sense of the massive concentration of Australian branches in far northern Australia, as well as the strange anomaly of the 22,000 year old Tasmanian settlement. There is much to unravel in the southwest Pacific, however, and that awaits another hypothesis.

(E) Throughout Sundaland the old 'Australoid' territory was occupied by movements of various Austriac groups, on the west Austro-Asiatic and in most of the island world by Austronesian. But proto-Austriac resided or at least began, it seems reasonable to say, in India. While Austriac may have remained from the original early settlement by sapient that took their kin to southeast Asia, it may instead be derived from peoples living farther west on the eastern or southern flanks of the Borean area, i.e., the Persian Gulf region or Pakistan.

Austriac movements into Sundaland and the Pacific came much later than their movements into southeast Asia and up into China from which Chinese language and culture mostly expelled them but still leaving most south Chinese with close genetic ties to Austriac peoples. (In this section I respect Paul Benedict but think an Austriac taxon is called for.) While Austriac represents the second movement from the west across India, proto-Austriac in the narrower sense was probably located in eastern India or may even itself represent feed-back from southeast Asia. Nahali may, of course, turn out to be closer to Austriac than to D-C, in which case it represents a more western version of old Austriac. It is likely that the original Austriac expansion into India began around 60,000 years ago, while the breaking up of proto-Austriac dates to much later than that, of course, but surely not later than 20,000 BP.

(F) Meanwhile back in greater Iran the same general surge that took the ancestral Amerinds across Siberia carried some close kin through Anatolia and around the Black Sea littoral into Moldavia (Bessarabia) and Romania, thence into south Russia and on to the Volga basin, then

everywhere west into Europe and east into Siberia that glaciers permitted them to go. It is almost certain that they were in eastern Europe 43,000 years ago and on the shores of the Atlantic by 35,000. I agree completely with Igor Diakonoff and Eric de Grolhier that this group had to be Aurignacian in culture, Cro-Magnon in physical type, and Mitian (narrow Nostratic) in speech, especially the Eurasiatic branch of Mitian. Those who went into Europe were, or developed into, the fairly closely knit European physical or racial cluster. They were not, however, all of those who were, or developed into, what are usually called Caucasoids. (More on them in a minute.) Since northern Europe was occupied even before the Neolithic by people of Caucasoid type, it seems likely that they were related to the Eurasiatic folks or at least Mitian linguistically. It is tempting to see some of them at least as resembling the modern Lapps. When Indo-European much much later spread from the east into northern Europe, it was probably kindred Eurasiatic speakers they absorbed or pushed out. Much of northern Europe remained glacial and inaccessible to settlement for millennia.

(G) Those Eurasiatic groups who moved into eastern Europe left behind kinsmen in the Russian forests and steppes (or tundra in the early days), probably the I-E and Uralic ancestors. I-E represents the old Cro-Magnons who lingered in eastern Russia, or as David Anthony has said (personal communication), I-E roots go all the way back to the Paleolithic thereabouts. Those who crossed the Urals and went on into Siberia and Mongolia remained localized for a long time but eventually reached the eastern seas, remaining as Gilyak, Ainu, Koreans, and possibly Japanese, or carrying a pronounced adaptation for frozen maritime areas finally got to the Bering Straits as the Eskimaleuts, or close by as Chukchee, Kamchadal and Yukaghir. When they crossed over to Alaska around 10,000 years ago, they found Na-Dene already there no doubt. Such groups as the Turkic and Mongolic of Altaic were sitting around quietly as fairly local groups in western Siberia when the new I-E horse culture hit them hard, pasturizing them and leading to their great expansions over Eurasia. But that is quite recent history.

Except for the I-E and Uralic peoples, all these Eurasiatic groups get classified as Mongoloids, indeed the archtypes they are. More of the Uralic peoples are 'mixed' or 'transitional' (e.g., Lapps) or simply Mongoloid than are simply Caucasoid. But even farther east the vast Mongoloid domain has enclaves of deviation in the direction of Caucasoids, especially the Ainu, some Gilyak, some Eskimos, and some Amerinds. Moreover in the west more than a few north Europeans have flat faces, small noses, 'puffy' eyes from fatty depositions on their eye lids, and chunky bodies with a tendency to get fat, more than a little reminiscent of those phenotypic qualities associated with cold adaptation and Mongoloids.

(H) The molecular biologist, Masatoshi Nei, has dated the common period or proto-Caucasoid-Mongoloid period to about 40,000 years ago. This figure is independent of the calculation that Cro-Magnon of 43,000 BP was an early Borean speaker invading Europe and eliminating his distant cousin, Neanderthal. His date is much later than the 53,000 years which one can deduce from Cavalli-Sforza's paper.

(I) While Dravidian ultimately ended up looking like a south Indian residue akin to Austro-Asiatic, its northern credentials are well established. It has walked a short distance south from the Borean dispersal area. Not only have Stephen Tyler and others argued for its

Uralic affinities but also its ties with Elamitic reinforce a northern thesis. Its own internal 'weight' is more northern than southern because of Brahui but also Kui and Malto. Some archeologists see Dravidian coming in from Iran maybe 7000 years ago. It was IE (Indic, of course) which pushed Dravidian into its cul de sac. Like many others, I think it obvious that Dravidians created Indus Valley civilization.

(J) The Sumerian language is not close to any others, be they ultimately D-C or ultimately in the same bag with Dravidian and Kartvelian. But the Sumerians lived very close to our proposed original Borean dispersal area. Some say they derive from eastern Anatolia, others say from the Zagros. Geoffrey Bibby in LOOKING FOR DILMUN reckons that they might be derived from the Persian Gulf area. Eric de Grolier is presently looking into some tantalizing clues to their possible affinity to Khoisan! Since Bibby thinks that eastern Arabia was cooler, wetter, and higher than now, say 7000 years ago when the Sumerians probably started entering Mesopotamia, I'll vote for the Kuwait-Qatar area as the Sumerian source, their Dilmun -- with the stipulation that much of their old territory is now under water.

(K) How Kartvelian, king of the Caucasus, got there is a bit of a mystery. It sits right in the middle of the dense Caucasic distribution, since IE Armenian is standing in for old Urartian a major (dead) branch of East Caucasic. The clever Georgians of yore might have talked their way into the D-C stronghold but from whence? It is most unlikely to have been central or eastern Anatolia, crawling with Hatti and Hurrians. To the farther southwest the aggressive Semites. To the farther southeast the Sumerians, Elamites and Dravidians. It pretty much had to be from one of three areas, to wit, (a) from the north, through the passes or by the Baku lowlands, hence into the sweet valleys of Georgia. Or (b) from the farther east beyond the Caspian Sea, hence across northern Iran. Or (c) perhaps simply from northern Iran. Since Kartvelian is most convincingly related to IE and Dravidian, one solid datum established by all those Nostratic etymologies, the most likely place to find pre-proto-Kartvelian would be near the path of the old Eurasiatic surge north from greater Iran. The Elamo-Dravidian homeland being most probably in southwestern Iran (the Zagros area), let's bring Kartvelian in from the El Burz mountains or the Mazandaran coastal plains + western Turkmenia. It is possible that such a movement has been detected already, archeologically, but has been ascribed to the early IE movements coming south from the Volga basin

(L) Surely Anatolia and the Caucasus have been solid Dene-Caucasic territory for a long time. Logically, we can derive them 'ages ago' from mountains farther east in Afghanistan or perhaps from Uzbekistan because of the need to connect up with the D-C eastern flank. 'Them' refers to the Caucasic phylum plus its new members Hurrian & Urartian, Hatti, and Gutian (of southern Kurdistan). All are extinct, except the 30 or so very diverse Caucasic languages in their mountain valleys. To show the strength of Caucasic in Anatolia there was still 'a living West Caucasic language, Pakhy, in the early 20th century in northwestern Turkey.

But this point has to be argued for -- briefly -- because a number of people see Anatolia as the IE homeland. Yet, contrary to Renfrew, Ivanov, Gamkrelidze and others, Anatolia of 7000 BC is about the most unlikely place to derive I-E from that one can find. It is about as silly as deriving Basque from Nigeria, another very crowded place. Despite Robert Sokal, et al, in this context the likelihood is that the

Danubian Neolithic, that which carried agriculture into most of Europe, was associated with languages of D-C type. Nobody doubts that Anatolia is the source of European agriculture. What Sokal and others did was to associate that farming with a linguistic phylum quite unlikely to have been in Anatolia during the 8th millennium BC -- Indo-European!

(M) What about the rest of D-C, the western flank? It carried Neolithic culture and farming across the Mediterranean to Iberia and up into France and (probably) up into the Maghreb (northern Tunisia, Algeria, and Morocco). Also to islands such as Crete, Sicily, and Sardinia and to southern Italy. Uncertainty and controversy shroud this entire concept, unfortunately, because of the number of dead and poorly known languages along the route. The western anchor of the D-C chain is Basque but it is questioned due to its links with AA. Another link in the D-C chain is Etruscan. If it is specially related to IE, then it may represent some of the left over Eurasiatic languages of Europe. If not, like Basque in my opinion, it is a D-C language spread around the Mediterranean basin by the Neolithic from the strong D-C realm in Anatolia. At this point in order to bolster the hypothesis -- and certainly without any proof at all -- four Mediterranean languages are classified as D-C. One is 'Iberian' of Iberia, one is old Sicilian, one Tartessian (SW Spain), and the last is Minoan (Linear A) on Crete. Moreover, the northern or mountainous Maghreb is stipulated as former D-C country, more precisely formerly Basque or some language similar to it.

SPECIAL NOTE: While it is not desirable to get into Africa too deeply, as it is (like the southwest Pacific) a deeper problem than the Borean hypothesis, still we have a huge difficulty staring at us in the Maghreb. It cannot be ignored. Modern humanity has resided in the Maghreb at least 100,000 years (Jebel Irhoud in Morocco). Later modern humanity has left sites in North Africa with dates much older (30-40,000 years BP) than anyone's wildest ideas of AA occupancy can accommodate. A comparable puzzle awaits us in south Africa. Some people lived in Morocco where you can see Iberia across the Gates of Hercules and which you could almost walk across during low sea episodes. Yet supposedly modern humanity did not get to Iberia until 65,000 years after they first gazed at it! That is truly remarkable indeed. Non credo! Nevertheless we have no explanation for this incredible anomaly and we are stuck with it. But who were the people of the Maghreb from 100,000 to 9000 BP when the very first Neolithic might have reached there? Four hypotheses are suggested but cannot be developed or argued here.

First, the old folks of the Maghreb were the ancestors of the great African super-phylum, Niger-Congo. I got this idea from Kay Williamson primarily, but partly from Hans Mukarovsky. There is supportive blood group and linguistic evidence. Second, the old North Africans were related to the Boreans as cousins who went west and were finally absorbed by later arrivals like the Basque and Berbers. Some of the old timers even looked like Cro-Magnon (e.g., at the site of Mechta el Arbi), it is said. Third, these antique northerners were the ancestors of the Bushmen (Khoisan phylum) which later moved south into eastern Africa. This idea comes from Carleton Coon and is not quite as crazy as it seems. Fourth, the old Maghrebians were the ancestors of the Basques who were finally absorbed or displaced by the incoming Berbers of AA before Neolithic times. At some point they had crossed over to Iberia and spread north into western Europe as Basque speakers. There IE Celts absorbed them finally, leaving only a residue around the Pyrenees. If the fourth hypothesis is right, it is most improbable that Basque is a D-C language -- no one thinks that D-C has 100,000 years time depth.

(N) AA (Afrasian) is almost as controversial as IE with respect to its homeland and its population genetic affinities. By now the consensus of long rangers has put AA linguistically in a much more independent status than it started out with in 1986. I would recommend that the word Nostratic not be associated with it anymore. Aihenvald agrees.

The Borean hypothesis says (above, first diagram) that AA is not closer to IE or Eurasiatic than it is to D-C. Is AA then in fact somewhat closer to D-C than to IE? NO! But it may be closer to Dravidian-Kartvelian. I'm not sure.

(O) The African side of dates and locations is much more difficult to postulate than the 'Oriental'. This is partly due to the fact that, once one has chosen Africa as an original dispersal area for modern humanity, the Amerind and 'Australoid' movements are rather obvious. The crowded area in and around Iran plus the Arab Middle East is much harder to figure out. Therefore I will just state the hypothesis and postpone explications of it.

Pre-proto-AA as the western member of Borean expanded from its sources around northern Iraq across the Fertile Crescent, down into the Levant, over into Egypt, and down the Nile valley and the Red Sea hills. This was not a migration in a strict sense but rather a long term or gradual expansion, starting sometime after 45,000 BP and culminating perhaps 20 or 30 millennia later. While this postulate is very similar to Militarev and Shnirelman's choice of the Natufian culture of Palestine as proto-AA in its homeland, Natufian is a way station on the route and too young to play the role of proto-AA. (Yet Natufian was my first choice in my dissertation in 1965!).

Beja (Tu-Bedawie), language of the famous Fuzzy Wuzzy warriors who were also the Blemmyes who troubled the ancient Egyptians, is sitting in the ultimate proto-AA homeland, east Sudan hills and Eritrea (northern Ethiopia). From Beja-land Omoti spread south into the western highlands of Ethiopia, followed by Cushitic into the eastern highlands and lowland Horn going all the way to central Tanzania (at least). Semitic took one of two possible routes to western Arabia, Egyptian moved down the Nile, while Libyan (Chadic + Berber) moved off to the west.

NOTES

(A.1) The term 'Boreal' has been proposed for Nostratic by Dolgopolsky and Palmitis, and probably others before. I have borrowed it from them because it is much more apt than 'Nostratic' with all its problems of Euro-centrism. 'Borean' is derived < Greek boreas and means "north or north wind". Fleming (1987) proposed "Eurasian" for a similar taxon but that is rejected here lest it be confused with "Eurasiatic". The Borean group of languages is the exclusive owner of the Arctic, sub-Arctic, and northern temperate zones of planet Earth, as well as all of South America, most of southern China, the entire Mediterranean area, the entire Middle East except for Nubia, northern Chad Republic, and the areas on the south Saharan fringes in western Africa.

(A.2) The Borean model is not the same as Alexandra Aihenvald's NOSCAU because it lacks Austric and is internally different in structure. But I have read her hypothesis and think she may be right where I am mistaken. Yet we do not owe these hypotheses to each other. Just a case of great minds thinking alike, n'est ce pas? Alexandra's scheme (plus Jean Pierre Angenot. Pardon!) is to some extent a consensus of Muscovite thinking as of 1989, as she says. We differ most strongly in the sub-grouping of AA where she/they include Meroitic and

Nilo-Saharan which no west European or American Afrasianist would include, at least none that I know of. She has Omotic as one of 8 branches, while I have it as one of two sub-phyla or a coordinate. However, we do agree in giving MSA (Modern South Arabian) languages a larger status within Semitic, as one of three branches. Her Dravidian includes Elamitic but has no Brahui; her I-E has four sub-groups: Indo-Aryan, Greek-Armenian, European, and Anatolian; on these we differ.

(A.3) These fairly small differences do not predict our major lack of agreement on basic branches at the super-phylum level. She has several articles on this topic and they do not all say exactly the same thing --hardly any of us ever do! But for the most part they want to make Amerind a part of Nostratic or tuck them in together in opposition to either SCAU or AA. SCAU is the interesting one: Sino-Caucasic (= D-C) + Austric. I do not say she is mistaken in proposing SCAU -- it has a catchy name -- but it is not a branch that I would propose.

(A.4) Bert Seto has recently sent me a large number of etymologies involving East Caucasian (Nakh-Dagestan) languages and others such as Basque. I have not been able to evaluate these data yet but his data and analyses are most gratefully received.

(C.1) While the hypothesis does not demand it, it is useful to propose that the eastern flank encountered remnants of erstwhile ancestral Amerinds in Sinkiang or Mongolia. The excellent conditions for natural selection for cold adaptation or just genetic drift may have already 'Mongoloid-ized' those presumably fairly small populations. Their absorption by old S-T and Na-Dene immigrants may have contributed to the cold adaptation of those populations, as they no doubt donated genes for the same qualities to the later Eurasiatic peoples coming from the west.

Coming originally from Africa, probably cool mountainous eastern Africa, the ancestors of the Boreans were presumably not well adapted to freezing cold, winter winds that kill, or frost-bite. In greater Iran which includes some of central Asia the Boreans probably did some cold adapting, especially culturally in terms of clothing and housing, but also biologically. Those who became the Amerinds would have become the most advanced of the Boreans in these respects before they even left for Siberia.

(E.1) Not only do we have Aihenvald's thesis that Austric shares a taxon with D-C but informal hypotheses linking Austric to old Nostratic or D-C (several of us). Swadesh explicitly put Munda and Austronesian in Vasco-Dene rather than closer to Australian or Indo-Pacific. However, some biogenetic data and studies fail to support those theses, lumping the Austric-speaking peoples with Papuans and Australians. Malheureusement, other studies place them with mainland Asians instead. 'Australoid' theories about Polynesians and Fijians having imbibed Papuan genes in large quantities in their travels seem, if anything, to have gained support.

(F.1) The choice of routes into eastern Europe and the choice of dates have been taken from archeology. It seems clear, and mildly surprising, that Upper Paleolithic type people occurred in the Moldavian and east Rumanian areas around 43,000 BP, substantially earlier than their occurrence in southern Russia.

(G.1) Genotypically, it has become reasonably clear that the Caucasoid cluster of peoples is most closely related to the so-called Mongoloid cluster. The reason for saying 'so-called' is --because the biologicals often lump everyone together as 'Asians' or 'Orientals' or 'Mongoloids' -- northern Mongoloids are not consistently distinguished from southeast Asians. South Chinese are lumped together with north Chinese. Und so weiter. Virtually every population from central India all the way across to Hawaii, if it is speaking an Austric language or a southern Sino-Tibetan language (including Cantonese), has a clear affinity to a marker gene, Gamma Globulin 'fanb', which barely occurs in highland New Guinea or Australia or is not very common as among northern Mongoloids (including north Chinese). When discriminations are made clear, as in Cavalli-Sforza's earlier paper reported two years ago, there is no doubt that the northern Mongoloids and the Amerinds are the ones closer to the Caucasoids. What is obscure is how far away Southeast Asians are. How useful can a label like 'Asians' be after all this?

(H.1) It is still possible theoretically that Neanderthal was absorbed by Cro-Magnon in Europe and adjacent areas but not in Siberia or India. Thus European physical types or Caucasoids generally may be basically the result of adding Neanderthal genes to erstwhile African genotypes. I myself do not believe this at all because of those other Caucasoids who lack the rugged or European features sometimes associated with the alleged Neanderthal genes.

From central India westward to Ireland, across North Africa, then down the Nile to Khartoum and down through Ethiopia almost to Kenya (where the research pooped out) -- in that whole vast area populations share a marker gene, a 'shared innovation', Gamma Globulin 'fab', which is otherwise present only in colonial European populations (like Americans and Siberian Russians) or as gene flow in small amounts (e.g., Quechua, Tibetans, etc.). Even the Omotic Wallamo and Cushitic Sidamo have a goodly amount of it. The large Caucasoid genetic presence among Ethiopians is not to be explained by recent gene flow from Moslem Arabs or Sabeian immigrants, as most of the biologicals automatically propose. Archeology has now rejected the notion of large Semitic migrations across the Red Sea. And there exist clearly Ethiopian type people in East Africa who show no signs of Arab contact, save recent trade.

(H.2) Not everyone knows that the Caucasoid realm is much larger than Europe, so important do small differences seem to parochial people. As the British say, the 'waags' begin at Calais. (Thanks to John DiCara for pointing out that the term originally meant Worthy And Honorable Gentleman.) If IE is about the only European population in Eurasiatic, there being none in Amerind, where do other Caucasoids reside?

First, much of D-C would be Caucasoid if Bengtson's new group were added to Starostin's old D-C; Basque, Etruscan, Caucasian, Sumerian, Burushaski, and probably Nahali, as opposed to Ket/Kot, S-T, and Na-Dene.

Secondly, Dravidians and along with them many millions of Indic speakers constitute the largest concentration of Caucasoids outside of Europe. They are 'gracile' and lack the features which remind people of Neanderthals. Predominantly, they have much Gamma Globulin 'fab' but in varying amounts. Consistent with their interaction with Austric peoples, both groups of Indians also have varying amounts of 'fanb'.

The third major concentration of Caucasoids is in the Middle East and African Horn, both among the IE-speaking Iranians, the Altaic-speaking Turks and the AA-speaking Arabs, Berbers, Ethiopians, and

Somalis. Most Afrasians are Caucasoid more than anything else, except for Chadic and Omotic speakers. And even some of them are partly Caucasoid. But our genotypic information is so limited on both groups that ...

(I) It is perhaps little appreciated that the first urban civilizations on earth linked Sumeria to the Persian Gulf but more strikingly to Susa (Elamitic) and Yahya (Elamitic probably) and the Indus Valley. The reason is not that the threesome of Sumerian, Elamitic, and Dravidian are genetically related in their own sub-taxon of Mitian. Rather it was probably sheer geographical propinquity that enabled them to influence each other, trade together, and develop similarly. That fact does after all apply to the 5th millennium BC and suggests that 7000 years ago they were neighbors -- minimally.

(J.1) It is interesting that a Semitic occupancy of eastern Arabia before Islam cannot be shown. Since much of that land has been slipping under water for ages now, there exists one of the world's prize opportunities for underwater archeology. Two others are the shelf under the Timor Sea and naturally the Beringian shelf.

(L.1) Before it became Kurdistan the northeast Tigris basin contained three more languages too skimpily known to classify (Subarean, Lullubian) or not easy to classify (Kassitic). In no way should it be assumed that any of them are Caucasian languages or even related to each other, not in that complicated ancient area with Elamitic and Sumerian neighbors to the south!

(M.1) Biogenetic data speak to this thesis with forked tongues. On the one hand Sardinia has unusually strong ties in Rhesus and MN to the eastern Mediterranean. On the other hand the Basques contrast quite smartly with them. Basque ties to the Berbers of the Magreb and to the Celtic peoples of western Europe in Rh, MN, and ABO are famous, of course -- serologically and linguistically. Thus an alternative theory would be that the Mediterranean Neolithic brought farming to Iberia and the Magreb but D-C type languages were already there. To put the physical differences on a phenotypic level where people can check their own observations, one might say that Sardinians, Sicilians, south Italians, and Greeks were much more 'Mediterranean' looking than Basques were, or Gascons or Bretons.

More support for D-C in the Mediterranean comes from John Bengtson's brief compilation of presumed D-C loan words in IE languages. Others would call these a 'sub-stratum'. There is a literature on the so-called Mediterranean sub-stratum but I have not read it. Of sub-strata I'm leery, preferring to talk more specifically about loans and influence.

(N.1) It may be the strong religious ties between Europeans and Jews that have kept the IE :: Semitic thesis going for so long or it may be that Semitic was the first easy place to look when comparing IE languages externally. In any case it now looks as if that IE-Semitic link has distorted things and it is time to think about AA, not just Semitic, and to re-do the question of AA's overall best ties to its external world.

But, having said that, one must stress that the distorted work did produce so many etymologies that the IE to Semitic to AA link is nearly incontrovertible by now! Furthermore that strong link, along with the historically powerful link with Uralic, constitutes the core of Borean.

The old traditional Nostratic is still the strongest hypothesis around in terms of etymologies. Consider only the work of Ilich-Svytich, Dybo, Dolgopolsky, Bomhard, Levin, and Hodge as a great piling up of good etymologies. One is not going to throw that baby out with the bathwater! We must simply try to separate the baby from the waste water.

(N.2) Phonetically AA languages are much more like Caucasian or Na-Dene or Amerind languages than they are like Eurasiatic. But they are most like Kartvelian. Phones by themselves count for nothing, however. Yes, but they do contribute to our intuitions (feelings) about languages.

Morphologically, the IE languages have been well analysed, as have many of the AA. Most scholars, I would bet, know far far less about Caucasian, Basque, or Na-Dene; and of course S-T has practically no morphology at all, except for standard Tibetan. Just syntax.

Lexically, the D-C group operates under the same disadvantage; this is especially true for Afrasianists outside of the USSR who are most unlikely to know Russian, or Spanish for that matter. The literature just ain't in them. But Russian is quite crucial for D-C because the key data are the Caucasian data and available only in Russian for the most part. Several times, trying myself to really compare AA and Caucasian, I've gotten nowhere because I had no Caucasian lexica. When on occasion there were Caucasian data, I've sometimes struck gold. One occasion of that sort was upon reading Diakonoff and Starostin's Hurrarrian book. My surprise helped start MOTHER TONGUE.

Most Nostraticists will strenuously object that all those etymologies prove that AA is closer to IE at least, if not Eurasiatic. Let me reply that an alternative not really considered is still an alternative. When people spend as much time and energy trying to show that AA is related to D-C as they have spent trying to show that IE is related to Semitic, then I will believe them.

We need a story to illustrate. Once Hodge told me that he had amassed a great pile of evidence showing that AA was related to IE. So I asked him: "Did you try to relate AA to Khoisan?" His reply: "No, that is your job." His meaning was that, yes, it would be better if we looked at the alternatives but he was not going to do it himself. And in the business of the D-C alternative to Eurasiatic -- well, I'm just doing my job, ma'am.

(N.3) First, we must distinguish between pre-proto-AA and proto-AA, or roughly AA before it began to break up and AA at the time of breaking up. Also in a phylum with six strong branches the internal taxonomy can be crucial. With apologies I must pass by some Soviet reconstructions of AA which are based on the old internal taxonomy. It seems faulty now. Supposed shared innovations based on the old taxonomy are not likely to be correct either. If the reader will consult DIACHRONICA IV: 1/2 (1987), pages 159-224, she can see the discussion of 7 expert opinions on AA sub-grouping. I will take off from the consensus therein. Four of seven experts thought Omotic a sub-phylum coordinate to the rest; another saw it as a coordinate half of Cushitic, not just one of five branches of Cushitic. A different tetrad thought Beja was coordinate to the rest of Cushitic or a separate stock within AA. Two plus two outsiders thought Berber had a special linkage with Chadic, but two thought Berber closer to Semitic. (It is awash in Arabic loanwords) So Berber is problematic. This loose consensus on some basic branches differs sharply from traditional views. Hence my problems with some reconstructions. (I owe this point to Paul Black.)

(0.1) Despite the powerful array of northern branches in AA spread along the Mediterranean shores, there are equally strong magnetisms pulling towards the south. Omotic is the strongest but also the southern reaches of Cushitic in East Africa show great diversity. Then there is Ongota. And Agau fundamentally located in the northern highlands south of Eritrea. Beja itself is an argument but its strong ties to Chadic (East) suggest an old dialect link, suggesting that Chadic is a western movement. If our Libyan thesis is correct, then Berber too is derived from the Chadic dispersal. If that is true, then the powerful northern tier is weakened decisively as an argument. Beja's 4000 years of documented contact with Egypt as a southeastern neighbor (perhaps) are an argument. Egyptian traditions of being linked to 'red' southern people are yet another. Their (Egyptians) own probable derivation from the southeast is not difficult to argue but also shows up faintly in their traditions.

There is an alternative, Renfrew-type, hypothesis. I find it rather attractive. That is that pre-proto-AA took its time going to Natufia and then about 20,000 years ago spread out in a great swoop across much of northern Africa and down the Nile and down through Ethiopia as far as Tanzania. This was associated with advanced hunting techniques, perhaps the invention of the bow and arrow. Then proto-AA settled down in its various areas and everybody just quietly got different. It is neat and makes a lot of sense. And I don't believe it for a minute.

General Note: The Borean hypothesis finds the scholar going back to work, replacing the editor. Many will applaud that for different reasons. The hypothesis will be presented with proper bibliography and the missing linguistic analyses or etymologies to proper journals, generating a cycle of submissions and rejections until it is finally published. Or not!

Many toes have been stepped on. Outrage will be heard. Some things will please some people at least. On the whole, however, it is likely that 5 to 10 per cent of long rangers will accept the Borean thesis. Another 35 per cent won't understand it, while yet another 35 per cent won't even read it. But 20 to 25 per cent will read it, understand it, and reject it. That is normal, I think, when the hypothesis is only outlined, supporting data and analyses are not given, and -- to a careful reader -- confusion appears to exist in D-C matters.

I accept all this because the hypothesis reflects the kind of philosophy of science I believe in. One advances hypotheses to make sense of a domain of data or a non-integrated bunch of hypotheses. The mental image of the solution to the problem presented by the data is what the hypothesis is. One marshalls data to support the hypothesis initially but that only establishes that one is familiar with the data. The crucial next step is to test the hypothesis against more data or just for its adequacy as a mental image of the solution. Usually a theory or model will be damaged by the testing so that a modified version of it is presented next for new testing. And so forth until we end up with a battle-tested theory which we can begin to believe is "really true". Let it be so with the Borean model of prehistory.

The confusion in the middle of Borean is due to D-C. That linkage of languages as far apart and ostensibly dissimilar as Basque and Navaho (or indeed Kabardian and Chinese) is inherently incredible. I have accepted D-C on faith alone. But I have struggled to make prehistoric sense of it; it just does not fall together so nicely as Mitian and Amerind do. There are too many unsettled phyla in the Caucasoid realm. Is Basque in or out? What is Etruscan anyway? Is Sumerian really in?

And what about Nahali? At times I am tempted to junk D-C altogether and substitute Swadesh's Vasco-Dene for Borean. At least Vasco-Dene made more sense prehistorically, if not linguistically.

In many ways the Sino-Caucasic hypothesis which is at the core of D-C is an extraordinary one. It is difficult to find languages as dissimilar as S-T and Caucasian both phonetically and morphologically, especially the latter. After a half lifetime spent arguing with other Africanists about the relative weight of morphology and the lexicon, I usually react apprehensively when morphology is so trivial to the proofs of relationship. Just look at Caucasian grammars sometime!

So next we take the null hypothesis with respect to D-C and try to see what linkages exist among all of them and the Mitian and AA phyla. There is not so much to worry about in the cases of Amerind and AA. I have no doubt that both are solid taxa, despite the stubborn resistance of some Americanists and Semiticists. Except for the strange case of Altaic and Japanese, Mitian looks pretty good too.

SUPPORT FOR THE HYPOTHESIS.

Inherently the Borean model is a three-fields type hypothesis, but focused on a linguistic taxon. So biological, archeological, historical (primarily in the ancient Near East), and linguistic data and analyses are assembled in it. However, the focus dictates unequal evidentiary value to various kinds of data. Unlike the typical paleoanthropological approach where linguistic conclusions count for almost nothing, herein the linguistic analyses are critical. Thus it matters little whether the early Homo sapiens sapiens entering Europe with Mitian language were Cro-Magnon or not. For example, it does not matter very much whether Neanderthals were pushed out of Europe, absorbed by the Cro-Magnon men, or had themselves simply become Cro-Magnon, as long as the languages known to exist in Europe have been Borean. Actually I doubt that any of the Rising Tide Lifts All Boats theorists have ever proposed that Indo-European or Basque or Uralic derives from (was inherited from) the speech of Neanderthals. But, of course, the logic of the traditional Indo-Europeanist is just that. Since IE is autochthonous to Europe, it could be a Neanderthaloid remnant language!

So the crucial support for Boreanism comes from linguistic work. Therein I have not tried to do everything by myself. That is no longer possible in long range comparison, except maybe for Greenberg who is on his second trip around the world. The underpinnings of Borean are these:

AFRASIAN. In rough alphabetic order these are a few who have published phylum-wide analyses stitching together the various groups. This list is short but there have been many other authors. M.L.Bender, M.Cohen, D.Cohen (de Grolier says), I.Diakonoff, A.Dolgopolsky, C.Ehret, J.Greenberg, R.Hetzron. C.Hodge, H.Jungraithmayr, S.Lieberman, P.Newman, et al. Alter goes back over 600 years to some Jewish scholars in the Maghreb.

TRADITIONAL NOSTRATIC. (AA + Mitian). Ignoring what is probably a centuries old linking of Hebrew, Arabic, and IE, modern pioneers are well known to ASLIP members. Pederson, Illich-Svtych, Dolgopolski, Dybo, Hodge, Levin, Bomhard, et al.

MITIAN or NARROW NOSTRATIC. Few as such that I can think of. S.Tyler (Dravidian and Uralic), H.Birnbaum, K-H.Menges, G.Deczy but the last two probably belong to the first category above. Mitian is a by-product of traditional Nostratic, created essentially by the separation of AA and the use of an older name for Nostratic.

EURASIATIC. Or its equivalent labeled 'eastern Nostratic'. Greenberg, young members of the Moscow circle (junior Muscovites), Bomhard (recently), and a large number of people (to be reported in Iren Hegedus's Nostratic Bibliography) who linked various dyads or tryads like Uralic and IE, IE-Uralic-Eskimo (16th century), Altaic & Japanese, etc. Including R.A. Miller, J. Street, N. Poppe, J. Patrie. 'Ural-Altaic' was widely accepted at mid-century, it seems.

AMERIND. Or major parts of it. Kroeber, Sapir, Swadesh, Greenberg, Lamb, Hymes (?), Ruhlen (improving on Greenberg), S. Nicolaev. A tough scene: the lumpers have been very bright and the hyper-splitters very numerous.

DENE-CAUCASIC. It is basically Starostin's baby but smaller links were made by Bouda, Trubetskoy (?), Dumezil, Shafer, Pinnow, Sapir, Lewis Gray (Sino-Tibetan & Ket), Bengtson, Ivanov, Diakonoff, Nicolaev, Cirikba, and of course Swadesh before all of them but with something closer to Borean. Many people have tried to relate Etruscan and/or Basque to Caucasian, but an almost equal number have tried to relate those two in other directions.

BOREAN. Those cases where the arguing over Etruscan, Basque, Sumerian, and Chinese showed that etymologies stitching them to both D-C and Nostratic suggested a larger garment existed. Mukarovsky's long work relating Basque to AA. Gamkrelidze's 'proof' or belief that Caucasian and Kartvelian were related was an improvement on the usual automatic assumption of 'North Caucasian' and 'South Caucasian', since he was an expert on languages of the Caucasus and a native Kartvelian speaker. Yet the demonstration by others that each could be related elsewhere again suggested a larger garment. Swadesh's VASCO-DENE separated AA and Amerind out clearly from the mass of D-C and Nostratic languages in between them (except for IE). That Vasco-Dene conception may yet prove superior to the joint notion of D-C and Mitian. The global etymologies of Trombetti, Swadesh, Bengtson, Blazhek, Kaiser, Shevoroshkin, F. Seto and Ruhlen piled up evidence linking all major phyla of human languages. But that also included evidence of a more narrowly Borean nature. Starostin's argument that D-C and Nostratic were related. Greenberg's statement that Amerind and Eurasiatic were too. My own constant experience of seeing AA 'match ups' frequently in Greenberg's Amerind etymologies or D-C etymologies, more in fact than I find when looking at data from the great tropical super-phyla (from an AA standpoint).

The interesting combination of O. Mudrak, V. Shevoroshkin, and M. Swadesh have tried to show that the Almosan branch of Amerind links up with Na-Dene and/or Sino-Caucasic of D-C. Some of their proposed cognates are arresting because of the special phonetic qualities of Caucasian, Na-Dene, and those Almosan languages on the Pacific Coast of Canada/USA. I must agree with Bengtson's review of their proposals, i.e., that local areal influences between Na-Dene and Almosan may account for many match-ups but that otherwise Greenberg's classification of Almosan as Amerind is more solid. However, it is also possible that both of them retain features of their common Borean past. They also by their existence challenge the 'special relationship' between Amerind and Mitian or Eurasiatic. As in the case of AA at the western end of Diagrams A and B, the Borean hypothesis holds that Amerind is equally close to D-C and 'Nostratic' (Mitian).

Aihenvald & Angenot's NOSCAU supports Borean in the sense that they assumed that AA, Amerind, and Mitian were in one taxon together but they negate Borean by taking D-C out and putting it in another taxon with Austric. Naturally, they may be right. We should try to get their article translated from Portuguese, mustering ample evidence as they do.

DATING: Some historical dates have been used, mostly in the Near East. Linguistic dating is used sometimes, recognizing that after 15-20,000 years it becomes statistically chaotic. At critical earlier periods dates have been taken from archeology, supplemented by a few molecular clock type dates. In the case of the New World where controversy resides my bet is that the early dates will prevail eventually over the young ones. A few dates are educated guesses, not necessarily wrong just for that reason.

QUOD ERAT DEMONSTRANDUM

FOLLOWING IS A ROUGH SKETCH OF THE AIHENVALD-ANGENOT TAXONOMY.

My apologies to them if I have distorted their thinking in any way.

Better yet, their sketch!

ÁRVORE INTEGRADA DAS EVIDÊNCIAS LINGÜÍSTICAS, BIO-GENÉTICAS E ARQUEOLÓGICAS

(Hipótese de investigação de A. AIKENVALD-ANGENOT & J.-P. ANGENOT, 1989)

