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NEWSLETTER OF THE  
ASSOCIATION FOR THE  
STUDY OF LANGUAGE  
IN PREHISTORY

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NEWSLETTER of the ASSOCIATION for the STUDY of LANGUAGE IN PREHISTORY  
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 Editor: 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.

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ELECTION OF FELLOWS

The members have voted in impressive numbers and we have a Council of Fellows. Being chosen for the council means that one is liked, respected, and/or thought to be an important participant in our common endeavour.

Unfortunately, an unforeseen consequence of the rules laid down by the Board of Directors caused many good people to be left off the list of the elected. Because the rule was that only 5% of members could be elected at this year's election, only 7 Fellows could be elected this year. Fourteen nominees each received 10 or more votes, two more got 9 votes each, and so on, but only 7 could be chosen.

Since the President and Vice President both were elected to the Council, and since they were already officers of ASLIP, they felt their presence blocked two respected members from being Fellows and therefore declined their own elections. The Fellows of the Council are listed in the box at left. Congratulations, good colleagues!

I hope many other colleagues will be nominated next year and that the ranks of Fellows will grow. It should not detract from the honor given to this year's Fellows to note that some deserving colleagues were accidentally omitted from the list of nominees.

Any member can find out the number of votes each person received by writing to the Secretary of ASLIP.

## Archeology and the Americas: MacNeish Strikes Again

Concerns about the prehistory of the Americas have been warming up for several years. Now they are boiling. What with Greenberg's volcanic linguistic hypotheses and the embattled chronology of human settling of the two continents an Americanist could become disoriented. Some dissident or lucky archeologists have started to overturn the satisfying consensus of a few years ago --that early man came to North America first and from Siberia and not more than perhaps 12,000 years ago. We talked about these archeological matters in several earlier issues of MOTHER TONGUE, forewarning people that the established chronology was threatened. But now it is wobbling on its pedestal!

Richard S. "Scotty" MacNeish has struck again. Previously he has had sites that were supposedly too old.

Although MacNeish has been quite respected in American archeology, one of his sites in Mexico has been criticized by some archeologists because of alleged insufficiency of evidence. Moreover, his belief that the site showed 20,000 years of Amerind presence in Mexico has not been accepted generally to mean that Homo sapiens got to Mexico 8000 years earlier than expected.

In a brief communique to the MAMMOTH TRUMPET last summer (1990) MacNeish described his new site and gave some dates for various levels. Quoting from his letter, we hear that: "Pendejo Cave (Fb9366) is located on the Fort Bliss military base, approximately 13 miles east of Orogrande, New Mexico, and 30 miles north of El Paso, Texas. The site itself lies about 50m above an arroyo in a small cliff that rises to a low mesa. Pendejo Cave is a relatively small limestone cavern, measuring about 5m wide and 12m deep, with a maximum height of 3m.

A talus slope drops off steeply in front of the cave's mouth." There were 14 levels in the site, that show evidence of different climatic periods, different faunal assemblages, Clovis points in some levels and unmistakable pre-Clovis artifacts and animals below the Clovis levels. Not only do the site's fauna seem to refute the theory that big game hunting Clovis point wielding Amerinds were responsible for the demise of certain kinds of animals in North America, but it also shows clear evidence of cultural artifacts at the lowest levels. These at "Zone 0, the lowest artifact block, 'is heavy on choppers and pebble tools. These are mainly... split-pebble, side-scraper chopper, flaked chopper, plus .. a bone awl made from the scapula of a horse. There's no way you can make a horse scapula pointy without whittling it. And this has a lot of whittle marks on it!' (close of quote within a quote)"

"The radiocarbon date (MacNeish said) is 24,420 +/-560 UCR-2499A. Now the date is not from the lowest level... but from Zone L. There are at least four levels below L -- Zones M, M1, N, and O." Both the editors of MAMMOTH TRUMPET and Scotty MacNeish implicitly worried about the criticisms which would explain away either the dates or the finds or both. But Scotty argued, in conclusion, that "I don't know how ... the site can be refused, but I think that of a dozen or so sites in the New World. I don't expect easy acceptance. But I think we've got all the ingredients here that Vance Haynes said are necessary to prove this is an out and out site. There's no doubt about the date, there's no doubt about the stratigraphy, and there's no doubt in my mind about the tools, particularly the worked bone!"

## Observations on the Comments on Kaiser's and Bomhard's Nostratic

Under the inspired editorship of Allan Bomhard, a fruitful discussion of the specific etymologies which support the Nostratic hypotheses of Illich-Svitych and Bomhard took place in MOTHER TONGUE last year. We are all indebted to Mark Kaiser, Allan Bomhard, Vitalij Shevoroshkin and Adam Murtonen for honoring us with the amounts of effort obviously entailed in their presentations. For linguists who do not know IE, AA or Altaic, it was perhaps difficult to relate to the discussion, but everyone probably could at least get a look at the kinds of data and conjectures which underlie the whole subject. In the absence of comments from other long rangers, I'll pass on a few of my own.

a) Surely enough evidence has been presented in support of the Nostratic hypotheses for them to be taken seriously by now? It is not the case that everyone will agree on each and every proposed etymology linking IE, Uralic, Kartvelian and Dravidian, for examples. Or AA, Altaic and Eskimaleut. And so forth. A number of proposed etymologies have something wrong with them, as has amply been indicated by Murtonen and others. Since good etymologies are the bases of good proto-forms, then the converse is true too, viz., in the presence of BAD ones, some of the proto-forms and some of the vaunted sound correspondences and rules are mistaken. How could it be otherwise? But by the same logic, surely the good ones argue strongly that the various Nostratic languages are verily related to each other. Scores of etymologies survived Murtonen's frosty Finnish eyes. And Alice Faber's too. It is good to have Nostratic hypotheses --with their drastic overemphasis on Semitic --examined by two competent Semiticists.

b) Just in this context, please recall MT-1 (Circular One) and the remarks about Aaron Dolgopolsky's Nostratic pronouns. His presentation was the most convincing argument I have ever seen for Nostratic. His conception was that the super-phylum extended from AA to Eskimaleut, embracing Japanese, Korean and Chukchee in the far east as well as the traditional core of IE, Uralic, Altaic, Kartvelian and Dravidian. If this vast array of languages from Berber of Mauretania to Inuit of Greenland was a genetic taxon, a class of things sharing a common inheritance, still Aaron's own pronouns suggested that "some of these these things are less like the others" (to paraphrase a child's TV lesson). He himself reacted against inequalities among the parts of Nostratic, denying that there was a 'Western' Nostratic as opposed to an 'Eastern'. This requires discussing a possible sub-grouping of Nostratic.

c) Does Nostratic have clumps (above the level of the individual phyla) within it? Or is each traditional phylum within it equally related to each other? For example, is AA as close phylogenetically to Altaic of Siberia as it is to Kartvelian of the Caucasus? Some FIVE of us now believe that Nostratic lacks internal homogeneity; either there is a true 'Western' group (AA, Kartvelian, Dravidian, IE) versus all the others or AA is coordinate to all the rest of Nostratic or AA is outside of Nostratic but still related to it as a distinct entity. Taking the pronouns again, it seems to me that the basic AA pronoun sets are as close to Amerind as they are to Nostratic proper (= all the others). (The specifics cannot be argued here). Greenberg has set up his Eurasiatic (IE to Eskimaleut but

including Ainu and Gilyak) but separates Kartvelian, Dravidian and AA out as related but not in the same genetic category. In more kinship-oriented terms, we might say that IE is a clan, Eurasiatic (IE-Uralic-Altaic-etc.) is a clan group or phratry (somewhat loosely speaking), while AA is a moiety over against the rest as a moiety (Eurasiatic + Kartvelian + Dravidian). This whole TRIBE, which I suggest really might be called Afro-Eurasiatic, has a relationship to Amerind that cannot be conceived of accurately at the moment. We will return to these matters later (below).

Also apropos of Nostratic sub-grouping, Starostin reckons that AA is historically distinct from the other Nostratic languages and that the true analysis of glottalized consonants in Nostratic cannot be made until the sub-grouping is worked out. (This at the Moscow Conference, 1989). Bomhard also now feels that AA is distinct from the rest of Nostratic (personal communication, 1990). And Ruhlen in the updated version of his Guide (in Press) will report similar views.

d) Authoritarian and ad hominem arguments: Do they do us any good? "You must believe X is true! Why? Because I say so! And I say so because I know. Or "You must believe that X is true! Why? Because George says so and George knows though you don't know!" This seems to be the basic structure of an authoritarian argument. As such, it is actually the most likely source of our beliefs about various things in the world. Who can be an expert on every thing? We must transmit the culture to the kiddies. And so forth. However, in a scientific or philosophical debate, or even in an ordinary group of sceptics, the appeal to authority is no more effective than whistling in the wind. Shevoroshkin's attacks on Bomhard and Murtonen have been heavily authoritarian, based on

Illich-Svitych's prestige and analyses or the assertion that the opinions of the Moscow Circle are unassailable. The rest of us don't know nuttin.

Authoritarian arguments rapidly become abusive and when used in debate may carry an inherently ad hominem component, as we saw above. After all you must be stupid if you don't agree with the authority. Well, who is this authority? From what we have heard he was a very bright and hard-working young Russian linguist who died more than 20 years ago. Vladimir Illich-Svitych quite possibly would have changed some of his hypotheses had his genius not been snuffed out by a truck. Perhaps like his comrades, Aaron Dolgopolsky and Vladimir Dybo, he would have been growing and evolving and somewhat embarrassed at being canonized. We are free to suppose that he would not settle arguments by appeal to authority or by showering abuse on his opponent. None of the Americans have been abusive to the Muscovites, while none of the other Muscovites have been abusive to the rest of us. For the sake of our collective survival, let the crap cease!

e) Murtonen's extrinsic assumptions & admirable reviews: In the course of perusing the proposed Semitic reflexes of old Nostratic morphemes, Adam Murtonen did us all a major favor. As most scholars know, there is a great expenditure of time and effort involved in checking such long lists of proposed cognates. As he evaluated, he had recourse to arguments that do need to be disputed, not because he was mistaken but because he exceeded the limits on being cautious, in my opinion. There is an appreciable issue involved too in that scores of potential Nostratic cognates were at stake. One criterion for rejecting an etymology was that X was a 'wandering word', another criterion was that Y was a

'culture word.' I would only argue that although such words have a good likelihood of being innovations (that is what the issue is about at root) a matching of two culture words does not always or even necessarily mean that they are not cognates. The question of that cognation still is, and always has been, empirical.

There are quite a few culture words in various sets of proposed cognates; many of them seem plausible. Moreover others have argued that 'early man' had diverse cultures and that we needed to learn about early cultures too. Admittedly we cannot use a borrowed culture word in a genetic proof but we are interested in ancient cultural borrowings too. Who would be indifferent to the finding that old Dene-Caucasic borrowed words from AA, for example (as Militariiev has proposed recently)? For another example, ethnologists have said that Altaic horse cultures of northern Eurasia were basically borrowed from IE horse culture, even if later developed more individually. Might there be linguistic confirmation or falsification of this theory? Or Murtonen's belief that proto-Australian would have no numbers in it and that therefore one should not use numbers as cognates elsewhere -- what if other sectors of mankind DID evolve numbers? N-C almost certainly had a word for '3', while AA had at least a word for '4', as did Sino-Tibetan. P-IE counted to '10' for sure, if not to '100'. Can we rethink this point, Adam?

f) The debate as seen from the southern sector of Afrasian, with apologies for not publishing more Omotic data: Despite the bias towards Semitic found in Nostratic studies, one cannot reject Nostratic just for that reason. Looking at AA with the seat of my pants on the lower Omo of Ethiopia, instead of Babylon, I still cannot dispute the

finding that AA including Omotic has many points of cognation with the languages of Eurasia. For the many critics who despair that proto-Omotic will ever be done more fully (pace Lionel) or that Omotic field data will ever come out -- there is hope! Not only has Richard Hayward (SOAS, London) published a huge book on Omotic (see News, below) but also last year's field work greatly enriched the Omotic data base. We promise that proto-Omotic will come out promptly. A taste of the new Omotic data plus the major new branch of AA called Ongota can be found at the end of this issue under 'Omotica, Afrasiana and More', q.v.

Some time it would be useful to spell out in detail what is wrong with basing most external comparisons of AA primarily or solely on Semitic. Semitic or especially Arabic really is a conservative group of AA languages both in grammar and in phonology. When one deals with proto-Semitic, enriched by the inclusion of modern data from South Arabian languages, one has a valuable set of potential proto-AA forms. But the classic fallacy of many comparativists is to assume that all or most of proto-Semitic is the same as proto-AA. Some kind of mental mist descends upon them when they contemplate the hoary Semites preserving forever ancient Bedawin ways and words. Yet we have to dispel the fog in their heads because Semites actually change over time, just like other people. But the full argument will have to wait.

Please give credit where credit is due. If you get ideas and data from Mother Tongue, why not mention them in your writings? If you don't help us, we will not collectively prosper and, judging from the Amerind crowd, you may need some help from time to time.



-5-

Although Merritt Ruhlen has contributed an article on this subject for us (next issue), let me review the topic separately both as editor and as advocate of a viewpoint. I am certainly no one's idea of what a neutral is. As editor I try to be fair and even-handed but neutrality is impossible.

of mass comparison. Indeed Greenberg himself has always insisted that, in effect, his strategy has ample precedent in the history of linguistics. Not only not unique to his work on Amerind but also, one must say, the strategy which gave us most of the modern taxa, including his African and southwest Pacific phyla. Even in the land of splitter mania (the Americas) the strategy had produced large taxa more than 20 years ago. Not everyone realizes how much we have progressed BACKWARDS in the Americas between 1966 and 1988.

The Reconstruction First strategy has recently made mighty efforts and contributed greatly to our joint push, primarily in Eurasia. In addition the concern with reconstruction has fleshed out the bodies of the new taxonomic hypotheses impressively. Perhaps most importantly the advocates of this approach have given an aura of respectability to long range comparisons by assuring our more timid colleagues that only 'exact' methods, i.e., Indo-European (= Neo-grammarian) methods, were being used. You need not fear us for, lo, we are competent professionals just like you -- only we try a little harder.

Yet in fact much of the recent Muscovite work resembles the Old Americanist hypotheses very much, both in the boldness

and the 'piling up of etymologies' to link various taxa to each other. The difference is that the Muscovites have this partially concealed under layers of reconstructions which sometimes look like a mass of hasty, arbitrary and phonetically incomprehensible proto-forms. How can one trust an etymology linking phylum X to phylum Z, if all you get to compare are a set of proto-forms which do not, frankly, look too trustworthy?

Nor is this appraisal original with me; several European and American long-rangers have said as much to me privately. The comments of Grover Hudson, Adam Murtonen and Saul Levin, previously published in MOTHER TONGUE, have argued passionately for the display of etymologies (proposed cognates,

in this case) to back up the reconstructions. Why do the Muscovites need all this hocus-pocus nowadays, especially since their foreign colleagues may not believe the proto-forms anyway? One (Russian) friend has told me that 'we do things this way' because their own conservative Indo-Europeanists will attack them for deviationism otherwise.

Still, underneath the unnecessary and unconvincing pseudo-data one finds a mass of good judgments of cognation, heroic amounts of work, and fruitful taxonomic hypotheses. That is why this section is sub-titled: "Differences Decrease." It is heart-warming and exciting to know that we are getting somewhere despite our official ideologies and mythologies

## Dene-Caucasic, Nostratic and Eurasiatic or Vasco-Dene?

a) Let's be careful with the nomenclature. If IE is related to N-C, should we say that N-C is Nostratic? As Ruhlen as argued several times, the term Nostratic does not simply mean 'ours'; it really means a group of languages related to us Europeans. The term has been Euro-centric and because all comparisons begin and end with IE it tends to distort what might be a proper taxonomy of Eurasian languages. Because so much attention was devoted to IE and Semitic and all those historical connections in religion (and race too, I hesitate to add), for example, the classification of Eurasia came out crooked, until more recent work began to straighten it out. Some of the terms for various ancestral entities, as well as our terms for taxonomic units at different levels of inclusiveness, are

beginning to clash with each other. We should begin to think seriously about a Committee to handle problems of NOMENCLATURE.

b) The continuing problems with Japanese, Sumerian, Nahali, Caucasian and Etruscan, not to mention others like Quechua, Finnish or Almosan. Or Songhai, Mende and Basque. Each suffers from conflicting classifications in two different phyla. Sooner or later we will be able to count these problems in the scores because scholars will compare practically every language in the world with every other. (They are starting to do it!) Are some good scholars wrong or are our conceptions too limited? Short rangers and long rangers themselves have generated conflicts in the classification of several languages. There are at least three interpretations of these conflicts or mutual



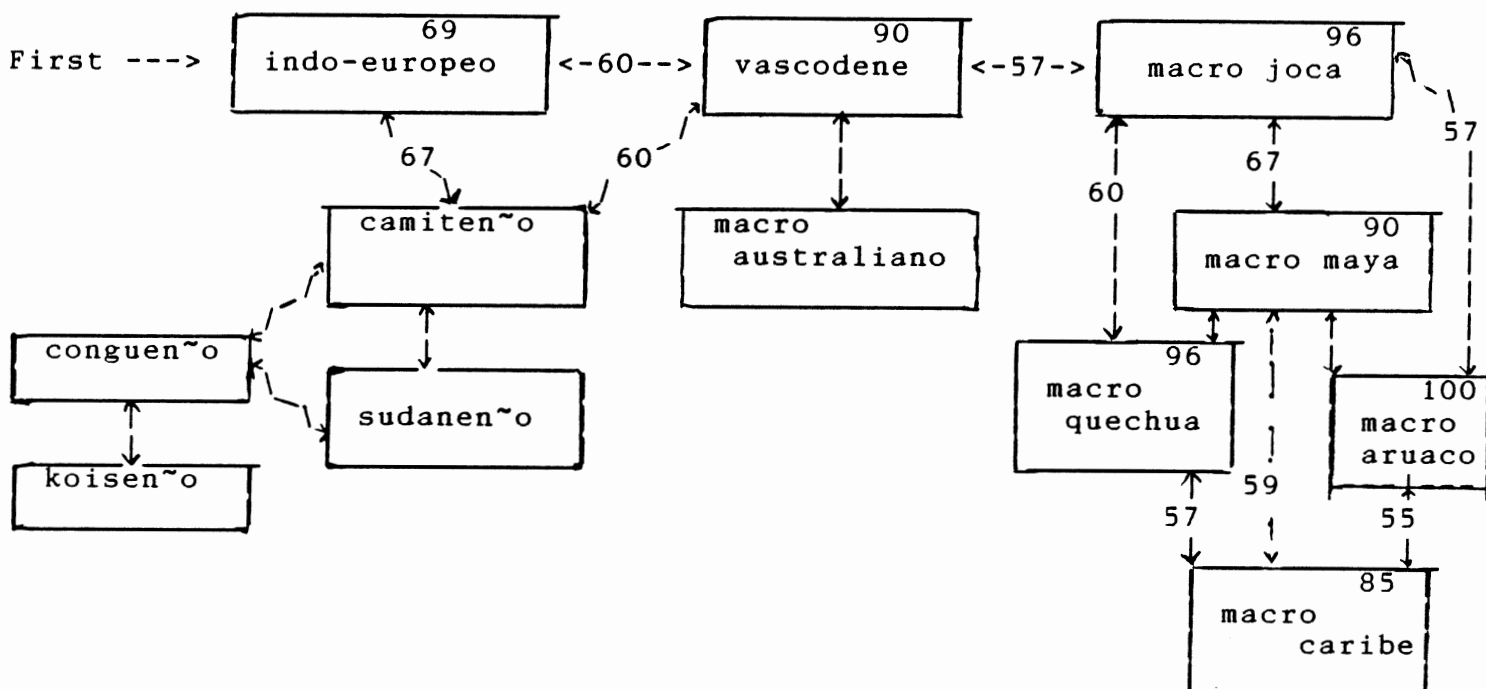
challenges. One is that somebody is wrong; a second is that the conflicts prove how arbitrary and fruitless long range comparison or deep taxonomy is: period. And a third is that it is the old sub-grouping problem only at a higher/deeper level. That is to say that our conceptions of relationships have not caught up to our abilities to see genetic relationships.

I aim to support the third alternative. To borrow Greenberg's examples from the Amerind book -- let us imagine that someone detected that Polish was related to Hindi and so sets up a P-H class. Another scholar sees that Hindi is related to Ossetian (neo-Scythian) and sets up a H-O class. Now the two conflict, two giant egos get involved, and battle begins. Possibly the H-O advocate will summon up more evidence and people come to think she is the winner or at least right. But people scratch their heads and wonder what, after all, was wrong with the P-H theory? We, with our superior wisdom of hindsight, know that they were both right because there is a P-H-O class at a higher/deeper level. Call it IE. We know that Hindi (Indic) and Ossetian (Iranian) are closer to each other as Indo-Iranian than either is to Polish (Slavic), but none of them is easily or obviously related to the other two in a binaristic (paired) comparison.

Japanese is the epitome of the difficult language to classify. Its archeological prehistory shows conflicts between north Asian and southeast Asian origins; so do its biological data (Turner vs Cavalli-Sforza). Japanese colleagues tell me that their traditions show the same conflict. Well, now we have Paul Benedict in flat-footed conflict with Roy Andrew Miller (+ some others including Dolgopolsky and Greenberg). Two of us at least think that Benedict's argument

(in JAPANESE/AUSTRO-THAI, 1990) is better than Miller's (in JAPANESE AND THE OTHER ALTAIC LANGUAGES, 1971) but both make a good case! Also Claude Boisson sees Sumerian as Nostratic, while John Bengtson has been working hard at showing that it belongs with Basque, Burushaski and Caucasian as part of Dene-Caucasic. Yet Hans Mukarovsky has compiled a very large corpus to show that Basque is related to Berber of AA, while Pat Bennett sees Basque related to Mende and Songhai (if I remember correctly). Does this all show how crazy we taxonomists are? Sure, if you want to believe such. However, the views of Morris Swadesh are relevant once more.

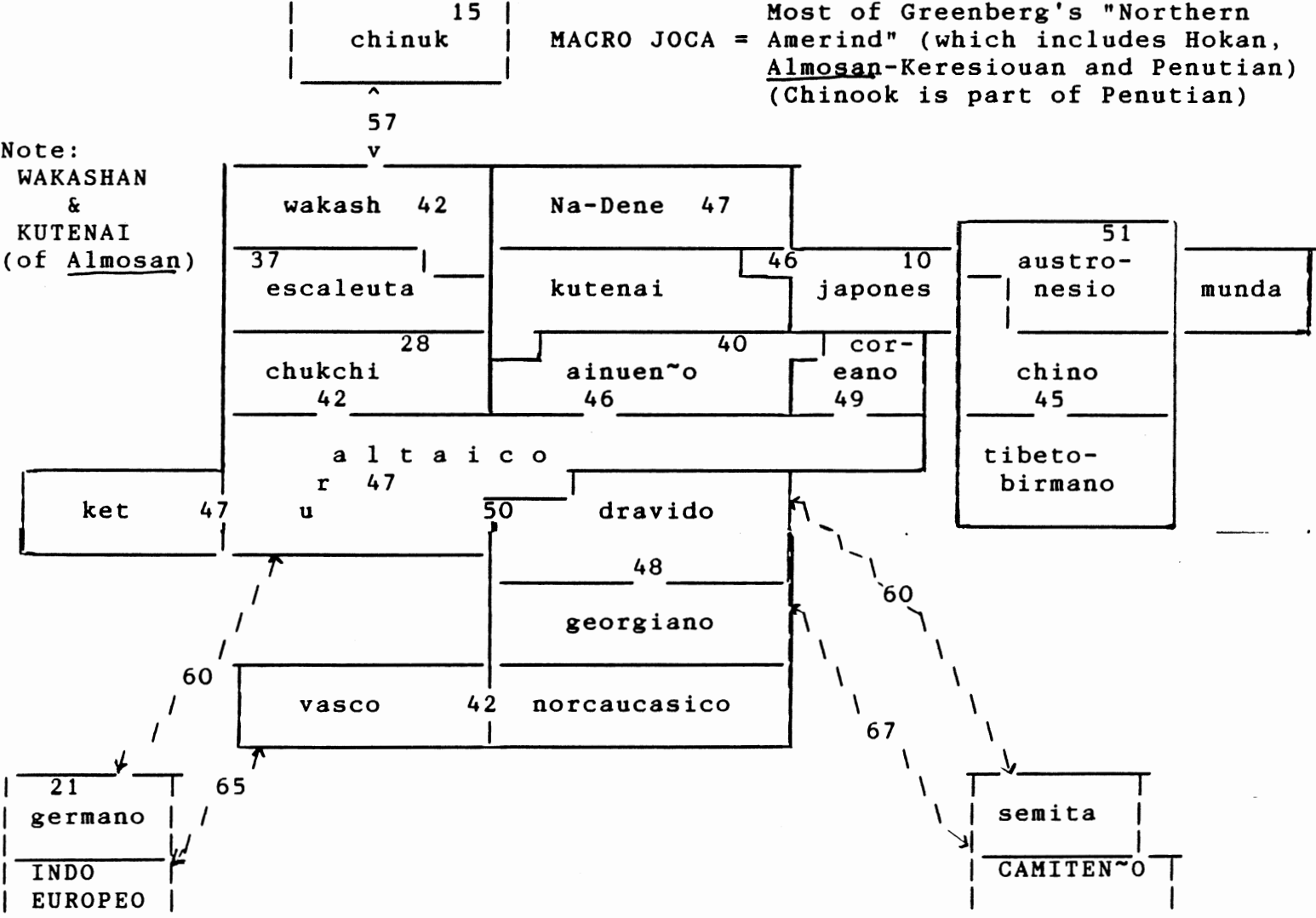
c) SWADESH SURFACES AGAIN. I discovered some old notes on his Vasco-Dene and its relations with other super-phyla or other phyla too large or independent to be members of Vasco-Dene. Two diagrams follow. One is a scheme of his ideas about the gross relationships in the world. It is similar to his scheme as reported earlier in MT 3. The second is his only statement of the membership of Vasco-Dene (that I know of). It is one of the few efforts in existence of a sub-classification of the overall class of human languages. Swadesh's model should be viewed as a TRELLIS, not an ordinary family tree, because his network (La red linguistica) is not simply that but also has some aspects of a tree, i.e., there are major trunks or branches or clumps within the overall network. Here they are:



The numbers within blocks (e.g., Vascodene 90) or between blocks (e.g., <60> as between indo-europeo and vascodene) stand for centuries of separation or divergence as calculated by Swadesh. It is a bit ironic that the calculations by the inventor of glottochronology could be described as grossly inadequate but relatively adequate, i.e., relative to each other the numbers are probably adequate or correct, but the number of centuries are likely to be too small for the percentages of retention which he found among various languages. In Africa Swadesh was only feeling his way around unfamiliar material, using the Greenberg classification as his model taxonomy, when he abruptly died. I had worked with him for a few days in 1965 and found that his sense of cognation was first rate but he was usually misled by Semitic borrowings into African languages and so his counts would normally be too HIGH in Africa. More importantly the fine tuning of glottochronology done since his day by such as Hattori, Gudshinsky, Joos, Sankoff, van der Mere, Dobson, Kruskal, Dyen, Black and many others, has resulted in much greater time depths being proposed for the lower percentages. Assuming that 90 centuries by his (old-fashioned) formulae (summarized recently by Raimo Anttila, 1989, p.397) result from percentages around 2%, the implied time depth of Vascodene would be much more than 90 centuries (say 22,000 years) by the new calculations but the comparability of the super-phylum to the African phyla would be most interesting.

A short translation is in order. "Koisen~o" = Khoisan or Bushman, "Sudanen~o" = Nilo-Saharan, "Conguen~o" = Niger-Congo, "Camiten~o" = Hamitic or Hamito-Semitic or Afrasian, "Vascodene" = Eurasiatic + Dene-Caucasic or Nostratic minus I-E and Afrasian but plus Dene-Caucasic, "Macro australiano" includes both Australian and Indo-Pacific, "Macro joca" = Macro-Hokan or most of North American Amerind, "Macro maya" = most of central American Amerind, "Macro quechua" = Amerind of western South America, "Macro aruaco" = Macro aruacanian or Amerind of southern South America, while "Macro caribe" = Amerind of the Amazon and adjoining Caribbean. However, as we shall see below, some northern Amerind languages are included in Vascodene rather than Macro joca. One is reminded that Swadesh was most of his life an Americanist, in fact one of the leaders of Amerind historical linguistics, and his Amerind taxonomies and reconstructions should be seen as a direct continuation of Sapir's work. So his opinions on Amerind are not lightly brushed aside. Furthermore his clearly higher numbers for the Amerind blocks reflect greater knowledge of cognation in the Americas, as well as the very considerable antiquity of Amerind in the New World.

The second or VASCODENE (Basque-Dene or from Basque to Navaho) scheme follows below:



Again, for purposes of identification, escaleuta = Eskimo-Aleut or Eskimoan, munda = Munda of India and probably Austroasiatic, chino = Sino or Chinese, ainuen~o = Ainu, coreano = Korean, ket = Yeniseian (Ket, Kot), uraltaico = Uralic + Altaic, dravido = Dravidian, georgiano = Kartvelian, and norcaucasico = North Caucasian (Caucasian). Some percentages in "minimum centuries" which would not fit in the diagram include the following:

Wakashan-Na-Dene = 43	Kutenai-Na-Dene = 43
Kutenai-Japanese = 46	Kutenai-Eskimoan = 45
Kutenai-Ainu = 43	Kutenai-Chukchi = 46
Kutenai-Wakashan = 49	Ainu-Japanese = 46
Ainu-Korean = 47	Korean-Japanese = 46
Chukchi-Eskimoan = 49	Japanese-Austronesian = 49
Japanese-Chino = 47	Chino-Austronesian = 47
Austronesian-Munda = 7_ (70?)	Georgiano-Norcaucasico = 49

Since Swadesh's inter-block percentages are strange looking, they require some explication. Operating with a trellis model, Swadesh could tolerate anomolous numbers such as I-E having some internal cases where 6900 years separated some I-E languages while only 6000 years separated some I-E and Ural-Altaic ones. Or Japanese is closer (4900) to some Austronesian languages than some of them are to others of their block (5100). In the case of the 60 centuries between Dravidian and Hamitic (AA) the figure is based on Arabic compared with some Dravidian languages. In a letter to me he noted that "if Egyptian had continued to modern times, it would now be about 60 minimum centuries divergent from Arabic,.....Unless the Cushitic languages provide an intermediate link, a question I have yet to study, Hamito-Semitic will have to be divided in two (at least) on the level of division employed in the diagram (first scheme - HF). Some data I have seems to leave open the possibility that Hamitic may unite with my present "Basco-Dennean". I am waiting for some vocabularies from a Baskist which should clarify this and certain other problems."

No doubt the Swadesh diagram will promote confusion among long rangers. This is a good thing, in my estimation, because we need to focus on the sub-grouping problems of an increasingly likely Super-Dooper phylum -- Common Human. There is a fantastic amount of valuable prehistory at stake in questions like "What groups are closest and closer to Na-Dene?" or "Is Kartvelian closer to Dravidian and Basque than it is to Semitic?" Or "Did the Cro-Magnon people who entered Europe 35,000 years speak proto-Macro-Caucasian (western Dene-Caucasic) or was it more like Swadesh's Vasco-Dene?" And so forth.

The Swadesh hypothesis is one of our few global taxonomies which explicitly stipulates that all its parts (most human languages) are genetically related. If I may quote Morris in a letter he wrote me in 1960, "I have evidence for the interrelationship of all American languages, the "BascoDennean" group, Indo-European and Semitic. For eight of the boxes shown on my diagram I have the network of constituent stocks and the point at which they link up with those of the next box. I lack this kind of data for "Macro-Australian", "Hamito-Semitic", Congan (Greenberg's "Niger-Congo"), Koisan and Sudanian." My interpretation of the last sentence is that he needed more data in order to compute the inter-block percentages, and he was asking many people for data at that time, rather than his not believing that they were related. In any case he had already stated his hunch that AA might need to be broken up (see above), so he had gone beyond Semitic. Some of our recent global etymologies, especially those by Bengtson and Ruhlen, have also made global taxonomies explicit. The output of the Muscovites is more amorphous, except for

Shevoroshkin's claims that Niger-Congo and Nilo-Saharan probably were part of Nostratic. A most recent taxonomy has appeared in the U.S. NEWS AND WORLD REPORT (November 5, 1990). Written by Wm.F. Allman and apparently reflecting Ruhlen's most recent ideas, it reduces the world to six major language families within an explicit statement of overall genetic relationship of world languages. The six major families are: (1) Australian & Indo-Pacific, (2) Austric, (3) Dene-Caucasic, (4) Amerind, (5) Nostratic, and (6) Khoisan & Congo-Saharan.

Clearly, Swadesh, Trombetti (?), Bengtson, Ruhlen, and Allman have quite different conceptions of the subordinate taxa (sub-classification) of Common Human. Let me throw myself into the discussion, since I did address these taxa three years ago (DIACHRONICA 1987, vol.IV, 159-223). It would be safe to say at the outset that no one's taxonomy is perfect or at least no one's taxonomy is wholly acceptable to other taxonomists. Swadesh conflicts severely with Allman and at some points (most especially the Americas) with Bengtson and/or Ruhlen. I disagree sharply with the Allman scheme, especially with respect to the old tropical phyla in Africa and the southwestern Pacific. I think Swadesh is wrong about IE being separate from Vascodene, just as he might criticize my groupings were he alive. And so forth. But there are some gains in these schemes too. are

Let us look at some of the agreements that Swadesh of 30 years ago had with the later Illich-Svitych, Bomhard, Hodge, Dolgopolsky, the modern Moscovites, or Benedict. Much of this lies in the Vascodene diagram. Naturally most Nostraticists would disagree with Swadesh about IE, Ket, Na-Dene, and (of course) Wakashan and Kutenai. But the main mass of Nostratic and Greenberg's Eurasiatic is also glued together by Swadesh, i.e., Kartvelian, Dravidian, Uralic, Altaic, Korean, Japanese, Chukchi, and Eskimaleut. Afrasian is linked but distinct, a view held by many contemporaries (see above). More important, and quite arresting on his diagram, is that Swadesh linked Afrasian specifically to Kartvelian ('georgiano') and Dravidian, as do Bomhard and Illich-Svitych. Swadesh's Basque has its closest tie to North Caucasic, supporting Bengtson among others.

Also the view that Japanese has equally strong links to Austronesian in part echos Paul Benedict's argument in favor of Japanese/Austro-Tai. His linking of Chinese with Austronesian is in accord with the opinion of Laurent Sagart (1990) reported by Merritt Ruhlen in his updated GUIDE. No one that I know of, however, agrees with Swadesh that Chinese is equally strongly linked to Japanese; I would wager that he was misled by the old Chinese loanwords in Japanese. Most of us would disagree with Swadesh about Sino-Tibetan; we would have Sino and Tibeto-Burman as one instead of his two blocks. And most would split his Uraltaico into two, while the Altaicists so-called would demolish that too! On his first diagram Swadesh agrees with Allman in the Pacific. His Macro-Australian equals Allman's Australian & Indo-Pacific. Swadesh never quite supports Austric of southeast Asia, even though he would have known about Pater Schmidt's original proposal, but Austroasiatic (his Munda) has its main link to Austronesian. However, as I reported in MT3, Swadesh linked Tasmanian to (today's) Indo-Pacific and to his Munda-Austronesian.

Perhaps Swadesh himself finally gains support from the slowly emerging notion that Dene-Caucasic and Nostratic are themselves related to each other and to Amerind. Since a scholar's strongest point usually lies in her specialty, and Swadesh's specialty was certainly Amerind, his notion that there is as much OR MORE diversity within each of FOUR out of five of his Amerind groups as there is in all of Vascodene ought to be given long and thoughtful consideration. Now that it is likely that Greenberg's original estimate of the

age of Amerind is wrong by a mere 24,000 years (see earlier issues of MT plus Scotty MacNeish on page 1 for further discussion) we can contemplate an Amerind of 35,000 years time depth with more equanimity. This makes it much more like the old tropical phyla like Nilo-Saharan or Indo-Pacific or Australian where perceiving the genetic unity of the phylum from the inside is down right difficult. I still agree with Greenberg and Swadesh that the native American languages are all related, excepting Na-Dene and Eskimoan of more distant affinity, but I can now understand the reluctance of many Americanists to accept that hypothesis.

The age of things has much to do with taxonomy. As an Africanist, used to the large time depths and/or diversity (at least) among our languages, I tend to see Amerind as probably a recent taxon -- perhaps just like some Americanists think African languages are closely related (!). (Personal communication, Sara Grey Thomason, 1988). In that I have been clearly wrong, as my own recent investigations have shown me. Tentatively, my estimate is that Greenberg's six primary sub-phyla of Amerind are roughly coevals of the six sub-phyla of Afrasian, except that all of the Amerind sub-phyla seem to be like Omotic, Cushitic and Chadic in their diversity and apparent internal time depth. Just off the cuff, just making a guess while in the middle of calculating things, I reckon that Amerind is at least as old as Afrasian, IF NOT SIGNIFICANTLY OLDER.

To make this point more explicit ... I presume that most of us would agree that proto-Kartvelian (p-K) of 1900 BC (according to Klimov) is younger than proto-IE which was close to being a contemporary of proto-Semitic and pre-written or pre-Pyramid Text Egyptian (circa 3500 BC). (Pre-dynastic Egyptian archeological cultures, Naqada I and II, from 3800-3200 BC). Yet proto-Afrasian (p-AA) was much older than p-IE, much much older than p-K, but itself probably younger than proto-Amerind (by my tentative estimate). Certainly I, and possibly others, have been misled by Swadesh's glottochronological estimates of Amerind time depth because he seems to have counted more cognates than Greenberg (for example) and used overly simple formulae for calculating time depth, thus UNDERESTIMATING THE DATES for low percentages of retentions.



## Schuhmacher on Bouda and 'New' Discoveries

DENE-CAUCASIAN: A LATE  
REHABILITATION  
OF PROFESSOR BOUDA  
By W. Wilfried Schuhmacher

Some of his colleagues used to laugh, others, no names, called the man "crazy" - when Karl Bouda (1901-1979), of Erlangen, West Germany, especially after World War II, made his numerous attempts to relate Basque to other languages/language groups (e.g., Bouda 1948, 1951). In his comparative work, also Caucasian played a prominent role, even leading to his establishment of a linguistic entity called "Euscaro-Caucasian". Bouda, von Hause aus finno-ugrist, did not concentrate on Basque-Caucasian but postulated also Basque-Caucasian relationships to Uralic, Sino-Tibetan, Austronesian (Malayo-Polynesian), Burushaski, and Chukchi-Kamchatkan, looking for linguistic evidence of a prehistoric culture of the Aurignac period that extended from the Atlantic to Lake Baikal.

Also in the beginning of the Fifties, Robert Shafer (e.g., 1952) demonstrated that the North American Na-Dene stock was linked with Sino-Tibetan, following in the wake of Edward Sapir. Both Bouda and Shafer, together with Morris Swadesh, may therefore be said to have been the forerunners of what today is called Dene-Caucasian (Basque, Caucasian, Burushaski, Sino-Tibetan, Yeniseian, Na-Dene, Salishan). Recently, Sagart

(1990), even has demonstrated that Chinese and Austronesian are genetically related, exemplifying among other things, that Old Chinese monosyllables correspond to the final syllable of Proto-Austronesian words: Austronesian, as already pointed out by Karl Bouda, as Dene-Caucasian? (Cf., e.g., Proto-Austronesian \*Da[m]pa 'palm of hand, sole of the foot' = Old Chinese \*pha/phuoA 'imprint of a horse's hoof' - Sagart 1990:28 [= Yeniseian \*boq 'hand, palm'; Salishan: Sechelt pəq- 'palm'; North East Caucasian \*p'weHq'wV 'wrist, palm' - Shevoroshkin 1990:88]).

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## ¡ LA LUTA CONTINUA! THE NEWS

News in the more ordinary sense that X has published a book on something, Y attended an important conference on something, Z has gotten a grant to excavate half of Burma, A has started up a journal on trivia,

etc. This will be a regular feature from on. Its success depends on you all sending the news!

According to our correspondents in the field, Sydney Lamb has a new book coming

out. He is editor (with E. Douglas Mitchell) of **SPRUNG FROM SOME COMMON SOURCE: INVESTIGATIONS INTO THE PREHISTORY OF LANGUAGES**, Stanford, California. More details are being sought.

A very interesting book on **PREHISTORIC MONGOLOID DISPERSALS**, No. 7, Special Issue 1990, Newsletter of the "Prehistoric Mongoloid Dispersals" Project, published in Japanese for the duration of the Project, except for this issue (in English). Articles by C. Melvin Aikens on "From Asia to America: The First Peopling of the New World", Charles E. Schweger on "The Full-Glacial Ecosystem of Beringia", W.R. Powers on "The Peoples of Eastern Beringia", and finally Merritt Ruhlen on "Phylogenetic Relations of Native American Languages" are included. The articles are extremely interesting, especially since Powers offers evidence of Siberian cultures from which archaic Amerind cultures may be derived, while Ruhlen offers evidence to connect linguistic America with Asia.

Demitri Shimkin in an otherwise savage comment on long range type things in **SCIENCE**, pp. 345-6, 27 July 1990, mentioned a book in press by A.P. Derevyanko, et al, called **NEW DEVELOPMENTS IN THE SIBERIAN PALEOLITHIC: DISCOVERIES, FACTS, HYPOTHESES**, Champaign (Illinois), University of Illinois Press. On the lighter side Shimkin said: "More than this, the insistence by some American anthropologists on a postglacial entry of man in the New World has long been an anachronism, particularly since archeology in Siberia has established a wide-spread horizon of Mousterian-type cultures that have been dated at 35,000 to 50,000 years before present. Substantial evidence also exists of human occupancy in the Acheulian, 300,000 or more years ago. The most recent relevant

data have been developed by A.P. Derevyanko and his colleagues in the Siberian Division of the Academy of Sciences of the U.S.S.R." Siberia is a large place, as is well known, and these references are not necessarily to greater Beringia, some 4000 km east of the Ural Mountains. We have all hoped for years and years that a full prehistory of Siberia would emerge some day and help enlighten Amerind prehistory. Shimkin's vague suggestion of Neanderthal and possibly Homo erectus threatens EVERYBODY's schemes!

Thanks to Peter Hook (U/Michigan) for the report on the titles of talks given this past Fall at Michigan by some long rangers. On Oct. 25th Sergei Starostin spoke on "Methodology of Long-Range Comparisons"; on Oct. 31st William Baxter spoke on "HAL Looks at the Indo-Uralic Hypothesis: Long-range Comparison by Computer"; and on Nov. 7th Aleksandr Militarev discussed "Principles for Searching for Proto-Language Homelands". (The HAL was not me but rather a computer program. I knew one would replace me some day!)

Militarev and Larissa Babrova also gave a talk on "The Afrasian Substratum Underlying Sumerian" (Nov. 5th). She is from the Institute of Ethnography, Leningrad and struck me as a superbly competent Sumerologist. Her own separate topic was "Eme-Sal, the So-Called Women's Dialect of Sumerian" on Nov. 8th. Finally, on Nov. 9th Alexander (my spelling) spoke on "Work in Progress: The Linguistic and Ethno-Cultural History of Mesopotamia, Arabia, and the Levant, Fifth-First Millenia BC". This rich course of seriously important papers was arranged by Vitalij Shevoroshkin.

A new book by Terence Kaufman and Sarah Grey Thomason on **LANGUAGE CONTACT, CREOLIZATION, AND GENETIC**

LINGUISTICS, Berkeley, University of California Press, 1988. Costs \$48.50. It came out last year. I didn't see it until a colleague in Ethiopia showed it to me. A preliminary skim through indicates that it is a book of considerable merit and well worth reading. It has a section of Mischsprachen which is interesting, as well as many thoughts on Pidgins and Creoles. According to Paul Hopper's review (American Anthropologist, 91, 1989, pp.817-8), it would appear that I should have read it sooner for their effort is in part nothing less than an attempt to destroy long range comparison root and branch. I think their logic is quite faulty and will attempt to tell why in some appropriate place. But the way their minds work distresses me most of all.

A new book by Derek Bickerton on "LANGUAGE AND SPECIES" (University of Chicago Press) which is said to have caused Noam Chomsky to say: "Thought-provoking" is described by the publisher as a "strikingly original and absorbing history of mind and imagination." They say that "Bickerton seeks the origin of human consciousness in the evolution of our unique capacity for language. He cautions that language, having once assured our ascendancy as a species, may yet cause our downfall." It is supposed to cost \$24.95.

Cavalli-Sforza, et al, is said to have a huge new book (1000 pages more or less) coming out which will fully justify the statements they made in SCIENCE about human population taxonomy and linguistic taxonomy. I do not have the particulars just yet.

Several new books edited by Shevoroshkin which were announced in MT10 should be coming out or have come out. They contain many articles by long rangers on many taxonomic questions of interest to us. While I have seen most of the chapters of these books individually, I've not seen any of the full publications. One is invited to submit reviews for

publication. Probably a direct inquiry to Vitaly would be the fastest way of getting a review copy, since normal processes take much more time.

Richard Hayward has edited a new book on OMOTIC LANGUAGE STUDIES, 1990, S.O.A.S, University of London. Over 600 pages on Omotic matters, including some excellent analyses and a considerable amount of lexicon. The published price is 32 quid, but it is surely worth the price.

Allan R. Taylor (University of Colorado) who is shortly going to publish the results of the valuable conference he arranged last spring has written an interesting article on "Linguistics and Prehistory" for the August, 1990 issue of MAMMOTH TRUMPET. It is a long and quite sensible discussion of the linguistic and prehistorical issues attached to Greenberg's Amerind book. Near his conclusion he says: "As a specialist in American Indian linguistics with a great deal of interest in the larger picture in which American Indians belong, I accept the Greenberg classification with some reservations. It is interesting and provocative in the good sense of this word. But there is no question that caution will continue to be of the essence, because of the many problems of interpretation of evidence and because peoples can change their language very abruptly, often without any recollection of the fact only a few generations later. Thus, a genetically constant population may well belong to different linguistic groupings through time. Historical conclusions based on affiliation can therefore be quite risky." I recommend both his article and the journal (newsletter?) MAMMOTH TRUMPET (Center for the Study of the First Americans, 495 College Ave., Orono, Maine 04473).

See conference of the International Linguistic Association on next page.

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# Call for Papers

for the

## Thirty-Sixth Annual Conference

of the

## International Linguistic Association

The Thirty-Sixth Annual Conference of the International Linguistic Association will be held on Saturday and Sunday, 6-7 April 1991 in the Roosevelt Hotel, 46th and Madison Streets, in New York. The theme of the conference is "Indo-European and Indo-Europeans". Addressing the conference on these topics will be Winfred P Lehmann and Marija Gimbutas. As in the past, papers from all areas of linguistics, both theoretical and applied, are invited. However, those which are related to the theme of the conference would be particularly welcome.

If you wish to present a paper, send an abstract of no more than 200 words (1 page double-spaced) to:

Ms Johanna J Woltjer, Conference Secretary  
Columbia University Center for Computing Activities  
612 West 115th Street  
New York NY 10025 / USA

Submit the original and four copies. In your letter of transmittal, include your mailing address, telephone number, academic affiliation, and the amount of time you need for your presentation. Presentations of more than 20 minutes can be made only after prior approval by the Conference Committee. If your paper is accepted, you will be asked to resubmit your abstract on computer diskette; this version will be edited for publication in the Conference Handbook. The deadline for the submission of abstracts is 31 December 1990.

Conference registration fees are as follows:

before 1 March 1991 (preregistration by mail):  
regular, \$25; student, \$15;  
after 1 March 1991 by mail and on site:  
regular, \$30; student, \$20.

An information sheet with suggestions as to hotels and restaurants is available from the Conference Secretary. For further information, call the Conference Chair at 718-997-5587, -5890, or by E-Mail at bitnet%"fichtner@qcvaxa" or Compu-Serve 71630,700, or write at the address below.

Prof Edward G Fichtner, Conference Chair  
GSEE / Queens College CUNY  
Flushing NY 11367-0904 / USA

## THE EXCHANGE

Offprints or reprints given out. Let us move our papers around. Perhaps we can swap data, as we do in Africanistics, or perhaps we can just give some data to someone we know who needs it. To start the ball rolling, here is a partial list of some of my reprints I will be happy to send out as long as the supply lasts.

a) "Asa and Aramanik: Cushitic Hunters in Masai-land". 1969, pp36. This was the first formal presentation of the South Cushitic hypothesis.

b) "Towards a Definitive Classification of the World's Languages." 1988, PP47, Review article of Merritt Ruhlen's GUIDE TO THE WORLD'S LANGUAGES.

One of the functions of this exchange will be to assist colleagues in various countries to know about articles published elsewhere or to have access to articles which they know about but cannot obtain. We should not forget that this 'field' of ours publishes in a hundred different places -- and many languages.

## AN EDITORIAL IN THREE PARTS

### HOW ARE WE DOING? WHAT HAVE WE ACHIEVED?

At the time of writing this (December, 1990) it is the occasion of our fourth birthday, a time as good as any for taking stock, for counting our blessings as well as our curses. In goal-oriented long range terms --how are we doing? Are we, the long rangers, expanding scientific minds, illuminating more distant horizons or are we still struggling to be heard? Or are we just a bunch of "linguistic hustlers", as Paul Hopper has so charmingly put it? Are the gate keepers and border patrols still keeping us out of 'respectable' and 'prestigious' journals or have we gained some access? Has our rather impressive access to 'popular' media added to our strength or has it created more angry and disdainful opposition? And finally and crucially, are we adding to the

human storehouse of solid truths or are we piling up falsehoods and wrong ideas? Ought we to be "shouted down", as Lyle Campbell has said. (This is a different question from: "are they trying to shout us down?". Campbell certainly tries to do that, while the editors of LANGUAGE are said to block us far more than their duty requires.) It would be marvelous to know the answers to these questions because I certainly do not know what the truth is in this matter. Indeed I probably know less than the average long ranger because I've been in the field for the past year. Some people believe that things are going very well for us; others would argue the opposite. I want very much to hear what you Long Rangers think or what your colleagues think; and I want to publish what you think in the next issue. Please write! (For more in this vein, see below).

# THE FUTURE OF MOTHER TONGUE, THE JOURNAL

This might be entitled instead: WANTED: JOURNAL EDITOR. As everyone knows there is a difference between editing a proper scientific journal and writing a newsletter. The latter can often be essentially an editing job but it may also involve a lot of personal, subjective and creative writing. There is no doubt that such subjectivity has been my style as editor of MOTHER TONGUE, the newsletter. It could hardly be otherwise when I believe that much of so-called "objectivity" in science is phoney and baloney. With such an attitude I would make an unusually poor editor for a good solid proper scientific journal. We need someone like Allan Bomhard or Viktor Golla to be the editor of MOTHER TONGUE! Did Allan not do a fine job this past year? And he has had years of experience as Review Editor of DIACHRONICA. Anyone who reads the SSILA newsletter can see the quality of Viktor's work.

Alas, Viktor works for the opposing team. And, unfortunately, Allan works full time at his job in Boston, even while churning out vast amounts of correspondence and Nostratic etymologies. It is not likely that he can become editor of MOTHER TONGUE, the journal. If we had enough money in our coffers to pay him or some other suitably informed and competent person to be editor, it would solve the problem created by me. Where are we going to get more money? At the moment who knows?

Theoretically, since I teach no longer at Boston University, I could solve our money problem by taking on the job of editing the journal. But, really, I am temperamentally unsuited for the job! Also my old love of Ethiopia and the Afrasian language phylum has been re-kindled by the year's field work and our discovery of a major new language, Ongota (Birale). (Our refers to five of us:

Hayward, Miyawaki, Aklilu, Ayyalew and me, initially stimulated by Ivo Strecker who stimulated Bender who...) Besides that my existence on planet Earth is no longer calculable in decades, more likely in single digits. So you can see the crunch, the logic of my situation?

If you will recall what Terence Kaufman said in MTX about the editor having a "thankless job", you may understand that there is a bit more to my reluctance to edit the journal. Being editor is not truly thankless; many long rangers have written seriously kind words to me and Allan. Yet, oddly enough, our whole common effort is being undercut by some of our own people who do not mention ASLIP or MOTHER TONGUE in their publications, even though in specific cases they have borrowed ideas (e.g., dates, hypotheses, etymologies, etc.) directly from our joint effort and even though they carefully cite every other publication, however miniscule, which is relevant to their articles. They may never write to us or never pay their dues (until begged repeatedly) and never mention us in print. Or they tell the media that they and one other guy are making all the discoveries by themselves. John Bengtson and Dell Hymes are the two exceptions to this and just because they do give credit where credit is due, they highlight our peculiar invisibility in the other cases.

Therefore, it is evident that we will have to wait for an editor to initiate MOTHER TONGUE, the journal. MOTHER TONGUE, the newsletter will continue as before. The decision not to become editor of the journal is my own, not subject to the approval of others. Some energy that might have been spent on the journal will be given to the pursuit of grants for conferences and research. The decision to try for grants and so forth is already legitimate, since those pursuits are



sanctioned in our By Laws. All these matters will be discussed at the next meeting of the Board of Directors. In the mean time all opinions on ASLIP business are welcomed. Those do not get published but will be discussed by the Board.

#### WHERE DO WE GO FROM HERE?

Well, to start with let us assume that the right to search for our initial language(s) is still intact. The right to publish or publicize the results of our inquiries has to be fought for, however. A few of the major prestigious scientific journals seem to be open or tolerant, although THE American linguistic journal appears to be closed to us -- but, naturally, unwilling to admit it publicly. But that is not news to us, or at least not to me. I still remember how Noam Chomsky was totally blocked by that same journal, as others had been, until he and some of the others resorted to book publication to get around the editors. That was why I started our newsletter in the first place. Now that our books are coming out, plus some articles in good non-linguistic journals, they won't be able to block our publication any more. They have lost that part of the struggle.

Some of the most valuable things that people can do now are quite easy to do. Well, it takes just a wee bit of courage. Enough to ask ever so politely at conferences or meetings of LSA why the editors will not permit favorable papers on distant genetic relationships or other long range comparison type questions, except by Greenberg who has the right of reply to attacks. Or, if you have a modest amount of courage, you might ask why Greenberg is being treated so unfairly by his own beloved LSA.

Most of us understand the trade-off that we make when we make a journal a "refereed

journal". It gets prestige that way. One's career is enhanced, hence the clamor from young scholars to make MOTHER TONGUE a refereed journal. Deans and chairmen of departments and all those who hire, fire and give raises play the prestige game via refereed journals. These journals have the virtue of keeping the lunatic fringe under control (they just block them) and of maintaining a certain level of quality in the articles in the journals. Thus they are supposed to perform an important scientific function, seeing that the discussion is disciplined, that we do not waste our time on poor data or stupid ideas, and that the dominant paradigm is adhered to. Or in the case of the crazy mixed-up field of Ethnology that some sort of paradigm is adhered to. But the thought control exercised by a refereed journal can be very tight. If the gate-keeper does not want you to say what you have to say, then, by God, you do not get to say it! Even if the referees themselves like what you say! So a 'good' journal would be one with smart but reasonable referees and the same kind of editors.

(More grist to the mill of the history and philosophy of science.). Thus too does the process work for many research grants, at least in the United States, where referees give their opinions to a panel which makes the final decision. Same thing happens, of course. We do not want to spend our (their) good money on stupid proposals so we will get the referees who represent the discipline to weed out the bad proposals and arrange the good ones in a hierarchy. But the panel makes the final decisions. One has been judged by one's peers.

Hah, but what if one is without a peer? This can be due to exceptional intelligence, like Chomsky or Sapir, or it may be due to the deficiencies of the

ostensible peers. Or what if you wish to seek truth in an area they never bother with? Either way, if they don't understand you, they won't go along with you. So built into the system of peer review with all of its virtues is an enormous potential conservatism. The present mind set of the peers will tend to perpetuate itself, until competition of ideas gets in somehow. As Barnes has concluded in his study of scientific revolutions, science is very much a social and cultural affair and very much less based on the logical rational individual thinker than the philosophers of science suppose. After all, when the deep prehistory revolution in linguistics is finally realized, how will people explain the extent to which rugged individual thinkers like Trombetti and Swadesh were reviled and brilliant but sweeter types like Sapir and Greenberg were heavily criticized and shouted down? Not only is the mind set of the average American linguist far from that of an average prehistorian, it isn't even terribly inclined towards ordinary history. Indeed some of our members understand this completely, since it's their heads I'm describing, but they stay with us as friends, recognizing our right to exist, or they find us amusing.

Let us have another discussion. Your ideas are wanted and I'll publish them. This is the question I submit to you:

In so far as contemporary (non-Russian) Linguistics is largely NOT interested in deep linguistic prehistory, it is surely a waste of time to try to interest most contemporary linguists in such a topic. Even most of those who are called 'historical linguists' have such shallow historical interests (e.g., a piece of Indo-European, one of the Amerind feudalities, Bantuistics, etc.) that it may be a waste of time to try to recruit them to our endeavour. Yes, a few

linguists who are personally interested in deeper prehistorical questions have been with us from the start and constitute our core. But we should not confuse them with contemporary Linguistics. We will not find a home in this discipline in this century. Linguistics itself is going to become a subfield of Mathematics, Artificial Intelligence and Computer Science anyway.

If we are to have some sort of society to which we belong, let us join that chaotic but tolerant area of science called Anthropology. Some few cultural anthropologists do have interests in what Stocking has called "the ethnological problem" but most social/culturals are possibly even less interested in prehistory than linguists are. However, there are two large sub-disciplines of Anthropology which are themselves derived from the 19th century concern about human origins and with whom we can work and thrive together -- Archeology and Physical Anthropology. Already that potential fruitfulness through cooperation or merging interests has paid off. Witness the labors of Cavalli-Sforza, Christy Turner II, Greenberg (with them), Ruhlen (with two archeologists), Militarev with Shnirelman, Chris Ehret with several archeologists, Ben Rouse with Douglas Taylor (the linguist), etc.. Even if one side's contribution is weaker than the other's in some specific case, still the general collaboration gets us somewhere.

Our good colleagues Eric de Grolier, Philip Lieberman, Gordon Hewes, and others also participate in LOS (Language Origins Society). As has been mentioned before, the LOS is dominated by biologically-oriented people, usually physical anthropologists. We have been invited to join them several times but have not yet done so. As part of a general movement towards Anthropology it would seem logical to join them,

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cooperate with them, or at least talk to them about possibilities. They have an annual conference, for example, which we have never had, and seem to publish their proceedings annually. Our risk is that they might swallow us up because they are very well

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organized and have lots of money. But we would always be free to leave them, if we wished to.

Come on! Write to me! We need to discuss these matters. If you do not want your opinions published, be certain to say so clearly in your letters.

## Potpourri

To this extra space let us add to an etymology. Starostin, 1989 (Moscow conference) has a "Nostratic and Sino-Caucasian" set for 'blood', to wit:

" PN \*SVXV / \*'VSXV 'blood' (MS 345, PI-PK,  
one should probably add PA \*sagV 'blood, health',  
SC \*SVHWV 'soul, life' (ST suā 'breathe, live',  
PY \*du'(x) 'smoke', NC \*S<sub>1</sub> ɹwV 'soul, breath  
--- "

Add to this: { Omotic: Oməto su-ts, suu-ts,  
AA: sugu-ts  
Ongota šoxo  
South Cushitic sako, saxo, sa'u-  
yaaku (E. Cushitic) sog'o  
N-S: kuliak seh, se?, se  
(Borrowed from AA?)

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- 22 -

**Omotica, Afrasiana and More:  
Ethiopia as the ever-flowing vase**

In order to show some comparative differences, to compare the diversity of language in one area or phylum with that in another, I enclose some eight word lists from recent field work in Ethiopia. Six of the languages are AA and two are N-S (but both full of AA loan words). One of the AA languages is a new major branch of AA, in my opinion, but some of my colleagues might say that it is too hard to classify = unrelated. It is Ongota (Birale) and it sits next to Tsamai from which it has borrowed a great deal -- mostly because all Ongota-speakers are fluent in Tsamai and their small community is evolving rapidly towards a new status as a variant of Tsamai with a local oral tradition of having been something else 'in our fathers' time' (1990). Ongota will be published in full shortly. The following have contributed to the field work: Richard Hayward (U/London), Yukio Miyawaki (U/Osaka), Aklilu Yilma (U/Addis Ababa), Ayyalew Mitiku (U/Addis Ababa), Pavel Mikeš (Charles University, Praha), Michael Seelig (Morehead State University), and Hal Fleming (Fulbright, Addis Ababa). This major increment to our knowledge of linguistic prehistory would have died out altogether in 15-20 years had it not been for the indefatigable Ivo Strecker who first notified his colleagues of its existence and M.L.Bender who relaid the message.

The Shabo language is very much a N-S counterpart to Ongota, being just about unclassifiable = non-relatable. We owe our knowledge of this forest hunters' language to Harvey Hoekstra (an American missionary), Bender, Peter Unseth (S.I.L, Addis Ababa), and Anbessa Teferra (U/Addis Ababa). Shabo is not moribund like Ongota but has borrowed a lot from Majang of the Surma group of East Sudanic (N-S). I reckon that Shabo is closest to Koman.

The four word lists below show the diversity of Nomotic. Jeba of the Dizi dialect cluster and Diddesa of the Mao language group each represent one of the two most divergent sub-branches of Nomotic. Shinasha of Dangur, found in NORTHERN Gojjam province, is the northernmost Omotic language, while Koyra east of Lake Abbaya (Margarita) is the easternmost Omotic language. The Koyra list is a merging of my field data with that of Richard Hayward and M.L.Bender. Aklilu Yilma and I worked together on Jeba. The other two lists are from my recent field work.

Data on all these languages will be published soon, except for that on Koyra which Hayward has already published (AFRIKA UND UBERSEE, Band LXV, 1982) and Shabo which is coming out soon in a book on Nilo-Saharan, edited by M.L.Bender. Suffice it to say that these Swadesh 'short lists' stand for only a small part of the existing data on these languages, except for Jeba where it is half of the corpus. My major work on Dizi was on the Adikas dialect.

The aim in all of this is to stimulate comparative evaluations of the existing established phyla which, despite Paul Hopper's scorn, include AA and N-S. No doubt the diversity in AA would show up a little better were the data from Berber or West Chadic or Modern South Arabian or Agau included, but the southern reaches of AA in Ethiopia and Kenya/Tanzania are already quite diverse. The same were Songhai or Central Sudanic of N-S also displayed. With some cooperation from colleagues we may publish some lists from the extremities of Amerind or Austronesian or Austro-Asiatic -- or some Eurasiatic to show (perhaps!) that north Asian diversity ain't so big! I bet IE and Uralic are closer than Jeba and Tsamai are!

## 4. Omotica, Afrasiana and More. Ethiopia as the ever-flowing vase.

ENGLISH	OONGOTA	TSAMAI (DULLAY)	SHABO	MAJANG
all	b'ad'd'e	xumme	ufebec yiNkapo hab'o	baaNe
ashes	táuni	dard'o	punk'wa / fuNk'a	têd'êfu
bark	kaad'a 7aqata	q'aq'-ay	ork'an \$ukuma	bukoy
belly	buusa	gara9	jukuma / sukuma	aamt
big	arba	d'amma	kiddi matti	beebêên
bird	karbo	7aag'g'-itte	hulut	tiiton
bite	ga9-	qayy-	p'iida / b'illa k'aw-ge	kaw-k
black	d'ak'a-muni	guma	c'iin	kôjôn
blood	\$oxo	c'eg'-de	(yêrom) caan	(yeerm)
bone	mic'a	meq'-te	(êmênan)	< êmeenên
breast	7aama	9ab'un-ko (sg) 9amb'e (pl)	kowan kokoN duh (=suck?)	aapôti
burn	k'ow- / qaw- k'oyka	lub'-	gooma goota	tûûjû-k
claw	soNq'e	g'o9-akko	kiik \$eenci	\$êpolkôy
cloud	(urate)	< urur-etti	guup'o	guumoy
(cirrus)	pfolo > ?	< poolo < Hamar?	wuri	
cold	s'anodi	nardaH-enko	k'êndi/k'end'i	\$alôôy
come	7ee hay- (come!)	xap- 7ogoy	am	waad'i-n kû\$
die	tiib-	par-	k'o	irerrî-k
dog	qaske	karo	(kaan) ka7al	warr
drink	c'a9aw-	d'ug- 9ug-	wo / woh / wuo	utû-k
dry	b'a9atuni	kaHa	c'ofa s'oto ici	palykn
ear	howwa	q'aame	k'iti / kithi	wiina
earth	biya < Oromo? >	biiya	boka	do
eat	c'ak-	ji7-	t'a	d'amû-k
egg	(9ugaxe)	< ukaHa-ite	(totokan)	< tutuka-n/k
eye	7aafa	7axe-te	se / Çe / \$e	taama
fat(n)	moora	goobi	s'ilaN / s'ilam ciime	(c'oome)

father	bááye	aabba	****	êpên
feather	siliti	kóólo	ceek'a	lom-
fire	7oxone	kaa-tte	c'owa / \$owa	máát
fish	kimi\$á	xaar-te	c'aNa / caNNa	olyt
fly (n)	(7innako)	< 7innako	(kayaN)	< kaayaak
			jefd / zefa	
			têr	
fly (v)	7axay	ha9- / xa9-	****	pir
foot	haaka	luq'-	d'uk	deer-
	meela (leg)		bica	
four	talaha	sálaH	aNan	aNan
full	****	dutsu-	****	enyaN
give	na7a	daH-	hanno	ga7
	bi7e (rare)			
go	roo / roota	zey-	no	mêk
		ac-	b'al	
good	7abba (non-self)	q'ay7a	daNka	mîntaan
	wanna (self)			
grass	(same as leaf)	9a\$-ko	camo	eebôy
				elt
green	c'ark'a-muni	raHa	****	\$olidiN
hair	(same as head)	gáz-ó	c'eeke / \$ek	aami
hand	7i7a / 7iyyA	Harko	if / epu	aari
head	b'ine	muga9-te	k'oy	ôôd'ô
hear	7aa\$-	q'abay-s-	7ot'om	tiik
			atcête	
heart	(za9ko)	< za9-ko	luunce	b'aaye
			(d'undet)	< dunde
horn	gatako	> ? < gasa-ko	kwete	koite
			kulba	kulba
I	kaa	áánu	tiNka / thin	eet
kill	ji7-	bog'-	ka / xa / ha	bokotu-k
	\$up-		Nahuma	
knee	giliba	gibil-ko	kutti < *kurN-ti >	korNa-n
know	s'i7-ni / s'iini	7ar-	d'e / d'ea	iigîri-k
leaf	(xaa\$e / Haa\$e)	< Haa\$e	c'am / caam	p'iiNôn
liver	tiré	tire	cukuma	nyai
long	orma >?	7orma	d'ama	jeed'o
		zigaba (far)		
louse	s'amis'a/t'amit'a	q'iske	nêna	nêêti
man	hintA/7inta	qawho	ulu	êt
person			ufa / upha	
many	gadahun	d'amma	kaaba/xaaba/haaba	b'ôôka
	nis'ina / s'ina			
meat	c'ata	sa7aN-ko	ha	taarr
moon	(le9a)	< le9o	ka\$ip / kacap	ééyê'n



mountain	ka\$ko (kotunko)	7a\$o < kotun-ko	goN / gom goort/gôônt	kurkûm
mouth	7iífa	bago / bage (pl)	k'aw	aatu
name	mi\$á	makaH	w-uNka / iNka-ye	tiriya
neck	d'iNge > ? (gomaro)(throat)	< d'eNge < goomaro (throat)	nôôdô	< Nâd'ê-k
new	(qawtito)	< qawtitto	numa-se	
night	-girim (verbal)	galaw	co / tso	tôôn
nose	siina / \$iina	sind'e	dippo	kulêêt
one	akala	dookko	\$ona / cona	êNên
			omo	< ômô
other	keesa	bile	iiNki/êNka	
		kubai	yuka	ôôm
		wana	ma7am	
rain (n)	haaje	ê'rô / erro	d'im / dim	tuuli
rain (v)	waak	d'ib'-	****	êk
red	romini	yid'a	c'aara / caara	de
road	kiti	záno	homa / khoma	goopa-n
root	(Hizete)	< Hizze (pl)	(tilt'il)	< tilti
round	mulqo	kiri	****	guNguN
sand	\$umaxa > ?	< \$umaH-to	k'êwê	jêêwe
			kiira	
say	7is	bey-	sum / com	êêrî-k
	alle (tell)	gaha	apho	ton
see	yop	em-	yiino	deenî-k
	noqota (look)	iyi < *hi7i	miimi	
seed	(b'od'aho)	Heorro (of gourd)	week'a-n	? < weiku-n
	d'a9asino (semen)	gore (semen)		(jáárè)
sit	7ááme	9akk-ad'-	mo	dokud'e
			maNka	
skin	d'arbo	q'uuro	wa	waa-n
		kontar-ko (human)	aku\$	taarman
		d'ol-te (cow)		
		kabo (sheep, goat)		
sleep	q'ade	raf-	t'ol7am	d'unguude
	katagam	g'ipp-	hab'a	
small	mod'd'one	takay	hêdêb	têêm
	/mo99one			
smoke	(9arto)	< 9a9ar- (v)	(tooru)	< tuur
		9arri-to (n)	ciimbi	
snake	gábare	dawo	(paar / phaar)	< opaaro
				k00'k0'
stand	yaw	ka99-	hitta	êtî-k
	7axay (+ fly)	cikar-	baalakit	
			poNka baala	

star	wal9ana	Hizge	(marion)	< mariôn
stone	c'a9a	gaH-ko	roga maana naama	gîd'e
sun	7ak'ac'o / xac'o	kali-ko	oka / oxa / oha	id'i
swim	zogiy- . > ? < \$ab- (cross river)	zog'iy-	lieet	leyêt lii-k
tail	la7akA	kirim- (+ cow's) dub-de (sheep)	sundum/cundum Çooça	kûûî
that	7ad'ate	kwattay kaysa	Na	\$oy
thin	xarqa	q'onne	kêêjê	têNêêli
this	hinda	7ulo	NaNum ney ma- kuku kuku/kungu (m/f)	i\$in
thou	jááme / jan-ta	7ato	(jiita)	< jiit
three	(zaha)	< zeeH	k'add handa	kâd'a
tongue	9aadaba / 9adaba 9aade (to lick)	9arraaf-ko	k'aw (+ mouth) k'onna / konnâ	ñida kêêt
tooth	7itima	7ilg'a-ko	baba	pee
tree	hanc'a goite	gaar-ko	t'eema	paakôn
two	lama	lakki	\$uuvu	
warm/hot	s'antuni /-\$ooní	lub'asa	wuâ / wud' yiiNa yiN / aN (m/f)	maw/mau îtîNk
water	c'a9awa	9and'e / 9and'i	mundi	dodon
we	joo / Zoo	7ine	waadi (+ green) k'inna	
wet	(-c'ab'i) c'ark'a-muni	< c'ab'-no	nambi	jiik
what?	nééni	moo	d'aaca / d'ada	kopulku
white	7at / atto-muni	bi9e	ne7ebe	wôôrr
who?	haakà	7aHa	naafe kukne umb'a kort'o	
woman	7ayma	gaante Hisk-	***** Naw peyêro subâk / Çubâk sitalak / subak (masc.) (fem.)	Naay
yellow	arate	> ? < arate		*****
you (pl)	gîda	7atune		înêêku

ENGLISH	JEBA (DIZI)	KOYRA (OMETO)	DIDDESA (MAO)	DANGUR
all	hêla	umba	muk	unná
				jâ'ma
ashes	s'yákn	muk'o	puuse	túla
bark	goran	uro7	k'ok'aše	gook'a
(inner)			šoole	jóka
belly	çoN / çuón	gawwo	tiile	máác'a
big	7ák-âs	órje	keme	ééna
bird	kabi	kaffo	kape	kâ'fá
bite	wos'-n	sats'	taas	šac'
black	s'an-us	kartsi	t'išine	áák'a
blood	yârm	suu-tsi	haande	s'ása
bone	7úús-u <*k'us	mak'ê-ti	maalt'e	mak'i-sa
breast	t'yam	d'an-se	aare	s'áása
burn	7áts-n	mic'	k'ees	mits'a
claw	hela	ts'uNke	wanzube	s'úNgúsa
cloud	di-w	d'uma	šeele	(dáwna)
cold	k'eç'u	toyaa	kyakame	ak'a
come	yo	yoo	ki	wa-
come!	yon	yowwa		wo
die	šub-o	hay7-	heek'	k'irha
dog	kyan-u	kana	kaane	kana
drink	bey- (Maji)	u\$	i\$	u\$-
drink!	wí			
dry	kola (Maji)	mela	kaNgile	šúúk'a
ear	7aai	waše	waale	waaza
earth	yell-u (Maji)	saha	k'es'e	dasa
eat	m-	muu- / mii-	mi	ma-
egg	myak'-u	bubuli	kyale	kéésa
eye	7aab	afi	áápe	7áwa
fat	kaw-u	malla	maale	k'osa
father	baba	aade	*****	bonga
feather	bi (Maji)	(baale)	kap kwiint'e	báNga
fire	aal-u	tama	kaame	táwa
fish	wargitsi	gults'e	k'ook'e	músa
fly (n)	wâNg-u	wutsuutse	s'iNe	zánzá
fly (v)	gaNg (Maji)	dênd	páns-	bííd-a
foot	aa\$>-u	toke	tuge	tuha/tufa
four	kubm	7oyde / 7odd-	myets'e	áwdda
full	ts'os (Maji)	kume	s'on-te	s'ééna
give	ta-N	iN <*img	ta	im-
go	to-N	haN <*hamg	hiy7	7am-
good	je\$	mod'e	nook	šenga
grass	7yaba	maata	póre	moc'a
green	c'íl- (Maji)	mata biša	twak twak	moc'ómandá
hair	saar-u	k'ini	kwiint'e	s'ira
hand	kuç-u	kucce	kuse	kíša

head	gâli	k'ine	tóoke	tóoka
hear	sis-	s-	k'éw	Si\$-a
heart	çon	mutsure	eeNa	níba
horn	u\$>um	7u\$ume	ii\$ime	(k'ála)
I	nây	tamba / ta	ti\$á / ti-	tana / ta
kill	debu\$-un	wâd'	piy	ud'i
knee	kola	bohe	tulk'ume	gubra
know	t'us-	er-	ald-	dán-a
leaf	tar\$á	mitsi wa\$e	yats' waale	maara
liver	bah-u	mayye	meele	7afára
long	gami-s	gallala	kwaas'a	génzá
louse	c'uc'-u	c'ucci	k'ii\$e	s'us'a
man	yab	adi	ent'e	nuNgu\$á
many	s'ó	laga	gyay-	ayá
meat	acko	acco	o\$ke	méésa
moon	7atsum	agunna	anse	a\$ísa
mountain	t'uum	bâk'a	démpe	gúrá
mouth	edu	nuna	poonse	nona
name	sum	sun-tse	tuge ('foot')	\$úúsa
neck	kùm	onta	kidi\$e	bimba
new	k'alâz	kille	tuume	andra
night	góta-	k'ama	kulkeeme	t'uwa
nose	sín	sid'i	\$int'e	\$int'a
one	k'oy	bidzo	i\$ki	ikka
other	manâs	fêêda	aw	k'ósa
person	*****	atse	7eesa	a\$a
rain (n)	iirù	ira	ump'ut'e	áw\$a
rain (v)	*****	buk-	*****	bus-/buts-
red	sub-us	zo7o <*zok'o	zyaNke	bíra
road	kók <sup>n</sup>	oge	poombe	wééra
root	táli	ts'abo	\$aa\$e	s'ap'a
round	*****	*****	pulde7	gúúra
sand	kásá	sise	\$áo	siya
say	gyio-N	oh-	wi	êt-a
		zer-		êr-á
see	so-N	be	7int'	bek'a
				s'iil-a
seed	búkùma	(gosa)	hookore	\$óoka
sit	ál-n	ut-	ku-	bee-
skin	haad	k'om7e	goonk'e	góók'a
sleep	sóg-N'	woy7-	haal-	k'e-
small	yasun	7od'd'e	amt	muk'a
smoke	ç'ubu	c'uwo	s'uuwe	s'uwa
snake	çwaZ-u	\$o\$	\$oo\$	dáwza
stand	aa\$-un	e7 <*ek'-	nuNk'	ad'-(rise)
				tu-(stand up)
				need'-a

star	buzí	s'olinte	balge	k'ééna
stone	lyál-u	succe	\$oowe	\$úsa
sun	\$yáZì	awwa	aw kare	7ááwa
			aw (God)	
swim	bor-o (Maji)	kaka7	paN	waak-
tail	c'ira	natse	yoNke	dú\$á
that	eeke	sei <*seki	ye\$ì	êkê
thin	yasun	heego	saali-te	c'ic'a
this	áá	hai / ha	na	han
thou	yeta	nemba / ne	hiiya	néna / ne
three	kâ`du	háydze / hazz-	teeze / tyaze	kezza
tongue	7yabâla / 7ebêla	urtsume	ants'ile	albéra
tooth	áč'-u	gâggo	7aats'e	gâ'sa
tree	inc	mitsi	ints	mita
two	t'agN	lam7e	numbu	gitta
warm/hot	k'es'i-	micca	k'are	k'eesa
water	7áái	watse	haats'e	aassa
we	ina	numba / nu	ham-bile	noona / no
(we two = I + thou)			han-bile	
wet	k'eZu-Z (Maji)	tiima	mále	moosa
what?	naka	am	konsiya	7eega
white	gaydn-s	bootse	káwe	(nê'sa)
who?	ik	one	kiyá	kone
woman	kocin	inde	muns'e	máása
		mac'i		
yellow	c'aNk (Maji)	botsi bi\$á	ukulye gas'el	
	DANGUR 'yellow' = nana7i ac'uwok'o (it seems like baby shit)			
you (pl)	iti	hinumba / hin	haw-le	7itna/7it

TRANSCRIPTION EQUIVALENCIES: The letters used stand for phonetic values which need no explanation for the most part. In vowels the primary values of [ a e i o u ] are "as in Italian" as the cliché says. Five more primary vowels are obtained through modifying the first set by a circumflex, so the outcomes are close to the values of the first set. Thus [ â ê î ô û ] represent what used to be written herein as [ A E I O U ] or the vowels as usually found in Standard American "butt, bet, bit, bought, foot". No cases of [æ] (English 'bat') or the unlauded vowels [ü] or [ö] occur in the corpus. High tone and/or stress are shown on the vowels, e.g. [á ó] or, where the computer cannot do that, by a circumflexed vowel plus ['], e.g. [â'] as in [bâ'tr] or roughly English "butter". Low tones are shown as [à], for example, but does not associate with low stress as high tones do with heavy stress so frequently in Ethiopian languages.

Consonant varieties: Glottalization is marked with ['] after the consonant in question, e.g. [t'] = glottalic egressive [t] (= ejective) and [b'] = glottalic ingressive [b] (= implosive). Pharyngeals voiceless and voiced are [H] and [9], and the glottal stop [ʔ], thus King [fæ'Hîd] or Fahd of [saʔudi] [9ærâbíiya]. The new retroflex sounds of Omotic are found here by [Ç] = [s.], [ç] = [c.] and [ç'] is glottalized or [c']. If you imagine that the 'dot' is under the [s] or [c] you will have IPA conventions. There is no special letter for the retroflex 'sh' other than [\$>] where [\$] is the ordinary fricative of 'bush' or 'Shiite'. The [zh] of French 'je' is [Z] and the 'ng' of German 'Finger' = [N], not occasional English [Ng] as in 'finger'. While the international linguistic community is divided in how it writes the first phones of English 'yoke, joke, choke', herein [y j c] are used. The [ts] of 'hats' or 'Katz' or 'Zaun' is written [ts], a geminate [ts] is [ts:]; it could be written as [tɕ] as Americanists often do and its geminate would be [tɕtɕ]. To repeat, [c] = 'ch' or 'tsch', not 'ts'. When it is glottalized, it is [c'] which is very common in Ethiopia. Syllabic nasals are very common in Jeba and other Dizi languages; most consonants followed by [-n] or [-N] display them in final position.

Loan words are shown inside of caps, thus (arate) 'cloud' is borrowed from < urur-etti. Where the direction of borrowing is less clear, it is shown by > ? < . Alternative forms are separated by a slash /, while cuts are shown by a hyphen -. This Jeba 'big' = [7ák-âs] where the root is separated from the adjective formant {s} ---> [-âs] by '-'.

Ongota (Birale) and Shabo are unclassified languages of extreme southern Ethiopia. The first is AA and the second N-S, I think. Tsamai is an Eastern Cushitic (AA) language, while Majang is a Surman (N-S). The four others are North Omotic, showing its taxonomic parameters.