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NEWSLETTER OF THE ASSOCIATION
FOR THE STUDY OF LANGUAGE
IN PREHISTORY

MAY 1989

MAY 1989

MOTHER TONGUE SEVEN

Newsletter of the ASSOCIATION for the STUDY of LANGUAGE IN PREHISTORY.

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EDITORIAL ESSAY

On Methods of Reconstruction

One of the most understated problems in the general discussions of language origins has been that of reconstruction. This seems to be because so much attention has been devoted to arguing that one must reconstruct in order to accomplish various things, as opposed to classifying or seeking more remote connections among reasonably well-established linguistic phyla. The emphasis on sound correspondences as the most solid and valid criterion of relationship depends naturally on the process of reconstruction. Indeed it is one of the vital parts of reconstruction. Part of the unsystematic or unfocused debate in which we are engaged concerns the lack of agreement, for example, between Aaron Dolgopolsky and Joseph Greenberg on WHEN reconstruction should be done and WHEN more intuitive searching and comparison should be done. Is it necessary, for example, to reconstruct proto-Niger-Congo and proto-Nilo-Saharan FIRST in order to try for a N-C connection with N-S or is it quite reasonable to compare N-C languages with N-S languages directly with each other BEFORE their ancestors have been reconstructed?? Aaron has argued long ago in Circular 2 that the proto-languages must be available before interphyletic connections can be sought. Greenberg would deny that necessity but probably would gladly accept the proto-languages were they to be available. Everyone does that, for example, when the phylum involved is Indo-European.

One question which arises concerns the QUALITY of the reconstructions. How good are they? When one deals with languages A,B,C,D in N-C and languages R,S,T,U in N-S, one deals with fairly reliable data, despite the occasional mishearings of phones by our colleagues. Are we dealing with the same level of reliability of data when we compare reconstructions like proto-N-C and proto-N-S or even p-IE itself? My awareness of this problem has been aroused by the numerous cases of proto-Afrasian forms presented by various authors, or indeed the cases of proto-Cushitic and proto-Chadic (or sub-divisions of each) which are used to propose proto-Afrasian forms. As many colleagues have agreed over the years our grasp of proto-Afrasian is a bit slippy and also the branches like proto-Chadic, proto-Cushitic and (even) proto-Semitic.(*)

So? Are the proto-Afrasian (p-AA hereafter) reconstructions that one is apt to see, or have to use, in comparing Afrasian with IE or Kartvelian or the rest of the Nostratic lot -- are these reconstructions as reliable as the average data on Coptic, Aramaic or Kafa? I would bet that most Afrasianists would join me in saying: "Hell, no!". Much of published p-AA suffers from sampling bias (using only Semitic or Egyptian data), arbitrariness, weakness of support, semantic implausibility, and so weiter. Usually one is given no detailed data with which to follow the author as she creates ancestral phonemes. More than anything else, in my opinion, it is difficult to know what kinds of rules the author followed herself. Some people seem to operate deductively, imposing a general phonological theory on the data, while some are strictly inductive and severely phonetic. Some people apparently see reconstruction as an art or the solving of puzzles, whilst some see their creations as exact and scientific.(**) Finally, malheureusement, some become so emotionally involved with their reconstructions that they display extreme defensiveness. Alas, that is a normal and expected phenomenon of scientists!

Well then, let us agree on some rules! If we can do that, then maybe it will come to pass that we probe deeper into antiquity with improved data bases. Although I try to model my own efforts at reconstructions on the teachings of Raimo Anttila, (***) still others may not agree. And let us be even franker than usual -- some of us cheat and some are incompetent! So let us try to get some international agreement on rules for reconstruction and with luck some international agreement on the role of reconstruction in long range comparisons. If that can be accomplished, then MOTHER TONGUE will have been worth it. In any case a serious portion of future MT issues will be devoted to problems of reconstruction. In my opinion it is our most salient problem, one which denies us consensus in strategy and tactics, and one which keeps us squabbling over who is right, who does it better, and so weiter.

But then reconstruction in the larger sense is WHAT ASLIP IS ALL ABOUT IN THE FIRST PLACE, while reconstruction in the narrower sense used by historical linguists is a CRUCIAL PART OF WHAT WE DO.

How shall it be done? Let me at once solicit comments & ideas on the subject from colleagues near and far. Let this topic assume priority in publication. Let us require, when examples of reconstruction are given, that they actually teach us, give us enough detail to see how the reasoning goes, and become convincing. Let me suggest some guide lines in the form of specific questions to be answered. These questions are not meant to impose my thinking on yours -- far from it. Rather they are based on two facts, (a) we have to start somewhere and c'est moi who writes this, and (b) it is not as hard as it might seem because there is an enormous amount of agreement already in place. In a fairly real sense, we need to rehearse things more than we need to discover new things.

One last prefatory remark, directed primarily at the "professional" historical linguists. One often hears from a scholar whose reconstructions have been criticised this reply: "How could I be wrong? I used all the standard techniques; I obeyed the standard rules of reconstruction." The rest naturally is an implicit demand that we believe her because she is a competent co-worker. What happens then will vary from place to place but what RARELY happens is the perfectly legitimate question of: "Well, tell me exactly how you got /*t/ and /*d/ in this language." We usually do not know in any detail whether our co-worker was trained at Harvard or Stanford, whether the teacher was Helimsky or Janhunen, or Dyen or Blust, or whether she even had a course in historical linguistics, and what text book she used. There are books and then there are training books. Check this one out. Most historical linguistic books don't tell you how to use the "comparative method" in the detail that Anttila does. None of the IE oriented books, including Anttila, answer questions about where the cognates come from. Most just run through a bunch of IE examples and assume that your teacher will be smart enough to make you actually do exercises. And then there are books that an average intelligent person cannot comprehend, like Henry Hoenigswald's or Raimo's first (some say).

Here are the questions which I claim need to be answered in order for us to agree on international rules. You are invited to take a shot at all of them or just one or just one part of one. It is assumed that whatever you write me on these questions WILL BE PUBLISHED. Good hunting!

- (1) There are no differences among the national schools of historical

linguistics, as far as reconstruction is concerned. Germans, Frenchmen, Yanks, and Soviets all do it the same way. Are these two statements true?

(2) There are no differences among the phylum-oriented or regional approaches. Indo-Europeanists, Semiticists, Sinologists, Americanists: they all do it the same way. Are these two statements true?

(3) There are some differences but that is only because some people have been improperly trained in IE methods. If everyone followed the definite and standard rules of IE studies, we would all do it right. Are either of these two statements false, or just misleading?

(4) There are differences among Indo-Europeanists. One example is the glottalic theory, another the extent of the laryngeals, another the taxonomic position of Hittite, Balto-Slavic, and Greek (even!), another the curious case of "tongue". What are the undetected problems within so-called IE standard procedures? How can we solve them in general principle? Or are they simply too historically specific to partake of general principles?

(5) How many matchings (match ups) between two sounds are needed before one can say that they correspond? The matchings in IE "tongue", for example, are said to be unique = one matching. Allowing that in English at least, with its hordes of Romance borrowings, it is possible to get several correspondences which do not go back to proto-Germanic but only to the Norman conquest or Louis XIV's time, would you agree that the principle ought to be the more match ups the better? But, really, can we ever permit just one match up to establish a sound correspondence? And if we unfortunately lacked the nine centuries of written history since William put an arrow in my ancestor's eye, how could we in principle overrule the spurious evidence (yes, spurious evidence!) provided by some of the sound correspondences between French and English? Or how could we tell that English borrowed huge amounts of French?

(6a) Granted that linguists hate statistics and the terminology thereof, still how well should match ups or correspondences represent a good SAMPLE of the phylum in question? For example, only Germanic has a /sn-/ root for "nose" in IE. Therefore, since Germanic can hardly be said to represent the other nine branches of IE, /sn-/ ought NOT be attributed to p-IE. Correct?

(6b) How much do we allow people to say that one or two languages can represent a phylum for comparative purposes? Should one not at least take the internal structure of a phylum into account when sampling? In IE, for example, is an etymology based on one word from Dutch and one from Italian as strong as one based on a word from Dutch and one from Wakhi? And this question is directed right straight at the Nostraticists. Isn't it a bit bold to take one word from Arabic and call it proto-Semitic and then take a word from Iraqw and call it proto-Cushitic and take them both and call them proto-Afrasian? And if the Arabic-Iraqw match up is the only one in over 200 Afrasian languages, then by what logic is the Arabic-Iraqw pair asserted to be proto-Afrasian? Heavens! why must we give our enemies the evidence to prove we are incompetents? (That is a real question, not a rhetorical one.)

(7a) There are no burdens of proof when someone says that a morpheme or root or word is onomatopoeic in origin? And therefore the word is just made up by natural processes and cannot be evidence of past states of the language? So /*sn-/ can be dismissed because it is onomatopoeic? Or can we

argue that words that imitate nature (moo, baa, woof, etc.) may also be inherited and be subject to regular sound change? If /*sn-/ is imitative of nasal sounds -- the /n/ is inherently nasal, while the /s/ imitates the passage of air --, then why do we not also reject /*nos/ "nose" because it has exactly the same imitations?

(7b) I think it obvious that linguists believe that whatever they can imagine to be sound imitative MUST really be so and scarcely anyone ever challenges them to show just what is imitated or asks how long this imitation has been present in one or more languages. Am I mistaken in this? For example, at Stanford Lyle Campbell used Mayan /ts'ub-/ "to suck" as an case study of sound imitation which we must all reject as a potential cognate. Two things about that are interesting. First, something like that form is fairly common in languages of the world but many of them are thought on other grounds to be related to Mayan. Second, [ts'] is not the implosive one would expect; it is a glottalic egressive (ejective) and the flow of air in its force is the opposite of sucking. What one would expect would be a dental click, as in common Khoisan [ʔ] or better yet the "kissing click" [ǀ] or in Swadesh [!b] or perhaps imploded [ɓ] common in central Africa and eastern India. The second consonant [b] of /ts'ub-/ is indeed bilabial but perhaps too common a sound to rule any thing out. Third, more importantly some people see (hear) in sounds like /ts'ub/ an action of spitting! If we can agree that spitting is roughly the opposite of sucking, then perhaps we can ask our colleagues to make up their minds.

Any sound that can be construed to be sound imitative must therefore be so in origin? Not only does a so-called imitation like /ts'ub/ ruin sets of potential cognates for spitting and other sets for sucking but also the rest of the assumptions seem to beg the question of the age of this imitation. Another question begged is the mutation one, i.e., how many known cases are there where children or others make up new words by imitating nature and those imitations catch on in the adult language and are passed down to later generations? We have been very cavalier on this topic of onomatopoeia and need to consult with psycholinguists more than we do. Does anyone agree with the critique in this question?

(8) If sound A in one language matches sound A in another language, is it not sufficient to say that they correspond and that their ancestor was *A? What other facts, if any, could over-rule this ostensibly clear fact? Well, think about two different problems. In AA [s] or /s/ in a number of branches seems to match up with an /s/ in a Semitic language. However, one scholar believes that the Semitic /s/ is derived from proto-Semitic /*th/ ('theta'). Thus he has a set of match ups like p-C /s/, p-Ch /s/, p-E /s/ but p-Sem. /th/ and therefore p-AA /c/. But on the face of it there were a number of AA languages (scores) with /s/ matching a Semitic language with /s/. Why wasn't the conclusion that p-AA was /*s/? Or take the interesting Chadic example of "eye". Many Chadic languages, and in several branches, have /ir-/ or its likeness, but many other Chadic languages have /id-/ or its likeness. Chadic scholars usually reconstruct /*id-/ or its likeness. But outside in AA "eye" occurs as /ir-/ or its likeness (including /il-/) in Egyptian and Cushitic (scores of languages). Should the p-AA form not be /*ir-/ and the p-Chadic form too? (Ah, mes amis, this one will make you grind teeth and bite tongue!)

(9) If sound A in ki-Linga matches sound A in ki-Langu, while sound B matches sound B in both, but sound A in ki-Linga also matches sound B in ki-Langu, is it not true that the ancestor of A-matching-B is something other

than *A or *B, either a different sound or a merging or a splitting? (Do you understand the question?) Formal presentations are sometimes better. Suppose that

A = A < *A

B = B < *B

but A = B < *X or all A + some B are < *A in the right hand language
or some *A > B in the left hand language.

If you will look back at #8, you will see that both the Semitic and Chadic examples could have been this sort of thing. Or not!

(10) The problem of semantic implausibility or how to have convincing semantic matches. It is the problem with perhaps the highest casualty rate of all historical hypotheses. (To say that English /fUt/ is cognate with East Armenian /vot'/ is an historical hypothesis.) Some colleagues seem to believe that cognation consists of showing that one set of sounds in ki-Linga matches another set of sounds in ki-Langu and that is all there is to it, fecklessly saying: "May the Devil take the poor semantic matching! Or who cares about the silly semantics -- just use your imagination a little, for heaven's sake, and you will see the connection proposed. What's the matter? Are you stupid or something?" Then we usually go right ahead and reject our colleague's phonetic masterpiece because we do not believe the semantics, i.e., the meanings are too far apart. Does that not happen often enough?

Let's take an example -- from French. There was a word "croupe" which meant the rear portion of a horse's back. So if one rider sat on the horse in the middle of the back, another humanoid might sit on the horse farther back -- on the croupe (rump). (In the Vespas of Italy the girl friend rides on the Vespa's "croupe"). In time the person who rode on the "croupe" was called the "croupier". That usage was then extended to a business partner, a so-called 'silent partner'. Eventually that last meaning was extended to the man who presided over the roulette wheel in Monte Carlo. Then English borrowed "croupier" as the 'dealer' in roulette. Now suppose one proposes to you that a sophisticated dealer in Monte Carlo had something to do with a horse's rear end. Without knowing this history, what would you believe? There is another example in French, how a wee table cloth begat the grand word "bureaucrat".

Greenberg's critics were scornful of any connections between the meanings of "green, black" and "dirt, night". Ruhlen, arguing for Greenberg, tried to show the connections by showing the intermediate stages, in effect creating a kind of history like that of "croupier". One can always appeal to the common human semantics which repeatedly link things like "black" to things like "dark, darkness", "evening", and "night". That isn't even hard. But the link between "green" and "dirt" is not common and so it must be demonstrated -- somehow. In Africa, for example, "green" interchanges with "black" very often and so in that realm "green, black" is not hard to establish. Elsewhere? But "dirt" gave me problems until he showed "dirty" connected both "dark" and "dirt".

The labor that Ruhlen spent on that semantic etymology shows perhaps where the source of semantic plausibility lies. One cannot simply say, as some of our colleagues do, that a tortured semantic connection which she proposes MUST be true because tortured semantic connections are known to exist. Older French "croupe" to modern English "croupier" is a tortured semantic connection and therefore you must believe that "green" connects up with "dirt". We have some vrai savants among ourselves!.... Again Robert Blust's lovely essay on semantic reconstruction in DIACHRONICA is recommended to one and all. Still what think you of this semantic matching business?

(11a) The only evidence for sub-classification within a phylum is the evidence provided by shared innovations? In order to know what things are shared innovations, is it not necessary to have pretty careful reconstructions? For example, Germanic, Greek, Armenian, and Old Persian share the phoneme /th/ (which is "theta" in all but Armenian where it is an aspirated [t] = [th]) and they share the phoneme /h/ which has allophones in [x] in Germanic sometimes. Does this not imply a special relationship among these four branches of IE? Particularly since there is no /*"theta"/ or /*h/ in p-IE? (Forget the laryngeals for now, please.)

Watkins from whom I took these data would point out that the Germanic /th/ was descended from p-IE /*t/, as was the Armenian /th/ but that the Armenian sound was different from the Germanic so that they are not the same innovation. The Greek /th/ comes from p-IE /*dh/, while the Old Persian is from p-IE /*k/, so none of the /th/s are proper shared innovations. Similarly the Germanic /h/ comes from p-IE /*k/, while the Greek, Armenian, and Old Persian are from p-IE /*s/. So why don't most people classify Greek in a special branch with Armenian and Iranian? Also modern Persian, modern French, and Armenian share a phoneme /s/ which in each case comes from p-IE /*k/. Why is French not an "eastern" language?

The answer is two-fold: (a) some reflexes of p-IE /*k/ in French are still not /s/ and (b) time makes a difference, i.e., French did not split its ancestral (Latin) /*k/ into [s] and [ʃ] until quite recently, while some "easterners" have had their /s/ for so long that they turned it into something else. Hence they did not share an innovation with French at some old IE dialect stage or whatever. It is a case of "accidental" convergence.

(11b) Does it make any difference whether the shared innovations are lexical, phonetic, phonemic, or morphological? I don't think many of us think that syntax is useful in this context because of its stylistic variability, the fluidity of transformational outcomes. Well, what is the logic of this shared innovations maxim? It means basically a common historical period when what became later entities were originally one and the same thing. The ancestor had an experience unique to itself, a new thing from the standpoint of its relatives, and this innovation was passed on to the later forms of itself -- its daughter languages. They share the descent from this ancestor with the singular experience, hence they share an innovation. It is almost the exact analog of an inherited mutation in genetics (biology).

Now most historical linguists seem to share another assumption about the maxim, namely that some shared innovations are more important or more mutant (?) than others. There is a distinct preference for phonological or morphological innovation over lexical. Why? Mostly because we think that phonemes and grammemes are closer to the heart of language than other things, especially "mere vocabulary". But so what? What is the relevance of being at the heart of things to a criterion which is defined as an historical event? The Hapsburgs shared an innovation which was at the heart of things -- their blood -- but it is not more diagnostic of relationship than the four facial moles inherited in my family.

Consider also an interesting documented case which seems to show that all is not well with our criterion. Because of the influence of the Sun King of France it became the custom of Frenchmen around the Ile de France to pronounce /r/ as /R/ and so it has come down to modern standard French. e.g., /maRi/ not /mari/. So great was the influence of the great Louis on the Germans that it became their custom too to pronounce /r/ "a la francaise" as /R/ and so it has come down to modern standard German, e.g., /hierR/ not /hier/. So modern standard French and modern standard German have shared a unique historical experience, imitating the grand monarch, and therefore they have a special closeness in taxonomy? Is this not a shared innovation? And in phonology no less? (We will have to rule out Dutch, which seems to be

crawling with [R]s, because its change was of an older /g/ not an /r/ (of course, the Irish English of Dublin does just that too.....). What shall we make of all this?

(12) It is not necessary to know the sub-classification exactly in order to reconstruct the common ancestor. Right? It doesn't make any difference in the reconstruction of p-IE that Anatolian (Hittite, et al) is only one of ten or eleven branches instead of a sub-phylum coordinate to all the rest? Or to take a more timely example, does it make a difference to Nostratic studies whether AA and Kartvelian are (a) simply two out of eight or nine branches or (b) they are coordinate to all the rest? Since they have all of the actual (living) glottalics and pharyngeals in Nostratic, their status must make some difference? No? Yes?

(13) Segmentation. How much latitude does one have in making cuts in words, in segmenting, for the purpose of comparing ostensible roots, when the ordinary grammars of the languages involved do not justify them? The usual examples involve treating parts of the right hand sides of morphemes or lexemes as if they were suffixes. Thus if dealing with English "brightness", I first segment the lexeme into its two constituent morphemes "bright" + "-ness". (I put the /-/ before the suffix to show that it is a bound form.). But then suppose I segment "bright" which derives from /*brixt/ into "bri-" and "gt" but then ignore the "gt" because I really want to compare ROOTS like /ber, bar, bra, br-, bri-/ etc. (One can find this example in print.) Either our original "bright" has an undetected suffix "-gt" or it was a compound of two original morphemes {bri} + {xt}. But what justifies splitting a morpheme in two because one wants to use half of it for a comparison? You think they are amoebas?

More theoretically, an unjustified cut can be criticized as comparing apples and oranges. How does one know that there ever was a morpheme {bri}? Should not an unjustified cut be stoutly resisted as destroying primary data? Well, anyway and nevertheless it is done and often accepted! Do you remember Germanic /sn-/ "nose"? That is an undetected morpheme PREFIX in a series of compounds with "-eeze", "-iffle", "-ot", "-out", "-ore", and so weiter; realized as "sneeze, snuffle, snot, snout, snore, etc." in modern English. Since everyone is focused on the /sn-/, how about asking what the justification for the right hand morphemes (e.g., -eeze, -iffle, etc.) is? Christopher Ehret, in his new p-AA manuscript, has taken the "-iffle" type problem seriously. As Paul Benedict has remarked several times, even quite small and generally likely morphemes like proto-Sino-Tibetan /*sna/ "nose", can be broken down even farther. In p-ST "nose" comes from /s/ + /na/. With something like /s-n/ so common outside of Sino-Tibetan for "nose" it seems unlikely that the ancestral Chinese could have re-invented such an old form. But probability and internal reconstruction do not have to agree and do not necessarily! But there is an important question here in #13 and your responses to it are desired!

(14) Then there is the problem of where cognates come from. As I argued angrily at Michigan and in MT6, neither Indo-Europeanists nor their imitators seem to know that a problem exists. But those who incessantly fry Greenberg on their verbal fires have a kind of logic, backed by the alleged experience of a century of IE studies, whose conclusion seems inexorable. I don't want to present the full syllogism but it starts with sound correspondences and ends triumphantly with something dear to Indian philosophy -- Maya or illusion. Things are not what they seem and one must look behind the flickering mirages of the empirical world to grasp the truth. If one seeks similarities between languages and their morphemes, one will be

deceived. IE studies have learned that change is so powerful that TRUE cognates -- which is those having sound correspondences -- are usually or frequently or always dissimilar. So only fools look for similarities!

As everyone knows that I think this statement is the reductio ad absurdum of IE-dominated historical linguistics, no more in this cranky vein need be said. Let us discuss the rational parts of the "Maya premise" and see if our problems have gone away. First, does the search for similarities actually lead to illusion? or does it in fact lead to something else to be called PROFIT? Profit = gross truth minus false truth, as any merchant would calculate. Or gross similarities minus false similarities yield profit or true cognations. False similarities = borrowings plus accidental similarities (e.g., English /bad/ and Farsi /bad/). Second, since we are doing science rather than Hindu philosophy, which of MAYA or PROFIT is supported empirically? Third, it is possible theoretically that IE languages have changed more PHONETICALLY than the average human language or at least more than some languages in other parts of the world, presumably due to the historical turbulence of western Eurasia. Judging from the appearances of modern English and French as opposed to Lithuanian and Latvian or Akkadian and Mbugu versus Arabic and Dahalo, amounts of sound change differ from language to language rather than from phylum to phylum. What do you all think? Fourth, is it not true that there is a lot of similarity among IE languages which is not illusory but true cognation? This question is obviously very biased towards my viewpoint and so hearty and contrary responses are expected. But after the heat of argument cools let us see what we can agree on!

(15a) Cheating or ignoring data is something that no true scientist would ever do, contrary to the excitement in the American press about biologists faking data and lying about conclusions. I really believe that we all try to be true to this calling. But there is a shady area in historical linguistics where the uncertainties of our inquiry give us much leeway to ignore data which do not fit our hypotheses. Nobody can account for everything and so data are frequently brushed aside or just flatly ignored. But we are not gifted with enough foresight into the ultimate truths (= the real proto-forms) and so we do not know that the stuff we sweep aside truly is without value. This question is quite like the one in #13 in its concern for segments of words or morphemes. One example will suffice to illustrate but not to define the real question. Thus in a number of Nostratic and global etymologies part of the actual data on "nose, smell" in four phyla is consistently ignored, due quite clearly to a belief that the "real root" is /*sn-/ or /*s-n/. Not everyone ignored the same things, of course. Myself tended to ignore the prefixed (?) or initial vowels in AA, while others tended to ignore the Omotic and N.E.Caucasic data with final /-t'/ or Khoisan with final /-ʔ/, and a few others the Altaic data with final /-g/. Yet look what we have overlooked (besides initial a-):

Chadic	-N	(but very rare & maybe conditioned)
Cushitic	-N or -m	(" " " & " ")
Omotic	-t' or -d'	
Northeast Caucasian	-t'	
Khoisan	-ʔ	
Altaic	-N or -g	

Does that look like a promising set of correspondences? Perhaps if Paul Benedict and the Southeast Asianists had been involved, they would have reconstructed /*asingut'/ or /*asunt'iN/ or the like, with a meaning like "place on the face where water flows" or "this place smells". (Heh, heh!)

(15b) When one ignores or sweeps aside data in order to protect one's hypothesis, or to attack another hypothesis, then we are closer to what we

ordinarily mean by cheating. Still such may be motivated by a profound belief that one is truly correct in doing so because one's hypothesis is really true -- for higher reasons than just these little facts -- or because one is sure that the alternative theory is truly false. One colleague swept aside a matching between [d.] and [r] in Cushitic because he "knew" that [d.] was derived from [*dl]. Another twice forced Semitic data into two pan-AA etymologies, distorting both reconstructions seriously, because he "knew" that the Semitic data had to fit and that no AA etymology could possibly stand, unsupported by Semitic cognates. Four people ignored or overruled the data from Dahalo and Dullay in Cushitic which supported a /*'b/ in certain words because they denigrated the testimony of such isolated languages.

But the real question of this #15 is -- should we demand that people stop ignoring data or should we shrug it off as inevitable?

(16a) This question concerns GOALS, the goals of narrow reconstruction only. Should the goal of reconstruction be phonology or proto-morphemes or syntax or 'everything'? Is the resurrection of the proto-language an end in itself or is it clearly meant for comparison with other languages? Is the study of a group of languages a "Ding an sich", a thing onto itself, a goal in itself, or is it part of a more general inquiry? The science answer would undoubtedly be : the more general inquiry. But many of us are embedded in the Humanities, not the sciences, and feel quite emphatically that we are not obliged to shed light on anything outside of our chosen, and beloved, specialty. Another kind of answer which I received recently was basically PHILOLOGICAL or ETHNOHISTORICAL, i.e., we are interested in the cultural things which surround our linguistic reconstructions. Rather than push our linguistic inquiry any farther into the past we prefer to explore the archeological and ethnological data which impinge on the linguistic reconstructions. I reckon that stance is very very common in the Americas and reflects perhaps the long association between linguistics and cultural anthropology there/here. I surely can identify with it because it was my orientation towards Afrasian and Nilo-Saharan for thirty years. It certainly came to me from Sapir and Kroeber, and my teacher Murdock. But it is so much like classical German Ethnologie, and from that >> most European ethnology outside of France and Britain, that the difference is not apparent to me.

(16b) Alright, fellow humanists and ethnologists, why can we not do BOTH? What is often not appreciated by the outside world of academia and the media is the amount of philology or ethnohistory present among linguists. It is not the case that everyone follows or ever has followed the "scientific" linguists like Bloomfield and Chomsky. They have all been influenced by the "scientists" but have persistently not really followed the leaders towards the goals of description and theory and mathematical elegance. They/we have had other fish to fry, as the Americans say. Now I put the word "science" in quotes when talking about Bloomfield and Chomsky only to highlight the term. There are many models of what science should be and linguistics (plus ethnology and archeology) has had several imposed on it. Yet one of the most powerful scientific models of all time, and the dominant one of the 19th century, was the diachronic or evolutionary or historical orientation. It is still powerful in biology, geology, and astronomy. In the 19th century it was dominant in linguistics. What has persisted or remained in the 20th century has not been always identified as "linguistics", certainly not as relics of the old paradigm. All those little pockets of diachronic linguistic interests (e.g., Sumerology, Sinitics, Semitics, IE, Egyptology, Dravidology, Mayan studies, etc.) fail to display a common agenda, seem each to be a Ding an sich, and do not agree on methodology. Put them all together and they constitute a multitude. Direct our energies in part, only in part, towards a

common goal and we could constitute a SIGNIFICANT scientific force, a different paradigm.

One illustration. Steven Lieberman and colleagues at Pennsylvania once told me that a conference on Assyriology held in Italy attracted HUNDREDS of interested people. How could that be? Assyriology! A very specialized part of Semitics should draw like a weak magnet! Maybe twenty or thirty bearded and half-blind museum moles but not hundreds of people! No doubt part of the answer was that some of them wanted to go to Italy in the summer but what about the Italians and other Europeans?

So this question, again one obviously biased towards a long ranger point of view, still is WHY NOT DO BOTH? Why not work with the anthropologists and archeologists to see how the languages of Oregon relate to each other and to cultural things? (Thanks to Victor Golla for this example) But then why not spend some of our time looking outside of Oregon towards Alaska or Mexico? Or, if one is an Oregonist, why not take the Amerind hypothesis seriously and spend some time looking at the big picture? If one is a solid and secure scholar of Indo-European, why not take a hard look at Uralic and Altaic? Why not spend some time confronting that bold Russian work on Nostratic? After all, although your teacher may not have told you, God never said she created Indo-European to live by itself. Don't you think IE probably had kinfolk at some "earlier date" and don't you think that looking for your lost kin has scientific validity? And if your teachers have so brain-washed you that you cannot imagine scientific validity, then how about HUMAN validity? If someone said that IE-speaking peoples were directly descended from lemurs, you would look into that. Right?

(17) Not least but certainly last is the Americanist claim that such efforts as these are "a waste of time". That is how a Semiticist colleague recently summarized her reading of Americanist opinion. At the Stanford conference (1987) I encountered a younger colleague in the bookstore. He looked at the Greenberg book on Amerind and snorted (= /sn-ort-ed/) that "it was a pity that people throw their money away on such a book. What a waste of time? With all his brains and energy he (Greenberg) could have contributed so much to linguistics like finishing the Penutian hypothesis or Algic or Mayan or something worthwhile like that. What a shame!" So common is the notion of worthwhileness (i.e., worth) and time wasting among the Americanist critics of Greenberg (+ Sapir and Kroeber) that I suppose it all must come from one teacher or a closely knit group strongly influenced by one teacher. And I am morally certain that teacher or influence reside-s/-d at Harvard University. Beyond that I do not know the history of its science. (We must ask George Stocking, or Hymes, or Hodge, or Joel Sherzer.) But I suspect that there is some sort of relationship to those anthropologists who, according to George Stocking, tried to destroy Franz Boas during the First World War.

Instead of ranting and raving about this Americanist mind set, as you would expect me to do, let me quote an eminent Harvard professor on the general pursuit of human origins. Writing in NATURAL HISTORY not long ago (Feb. 1989, pp.20-28), S.J.Gould said, among other things:

"The reconstruction of the human family tree -- its branching order, its timing, and its geography -- may be within our grasp. Since this tree is the basic datum of history, hardly anything in intellectual life could be more important."

Here (page 22) he talks of both language and our genes. Later, speaking more exclusively of language (page 28), he concludes:

"Our original linguistic unity is only historical happenstance, not crafted perfection. We were once a small group of Africans, and the mother tongue is whatever these folks said to each other,

not the Holy Grail."

"This research has great importance for the obvious and most joyously legitimate parochial reason -- our intense fascination with ourselves and the details of our history. We really do care that our species arose closer to 250,000 than to 2 million years ago, that Basque is the odd man out of European languages, and that the peopling of the Americas is not mysterious for its supposed 'delay', but part of a regular process of expansion from an African center, and basically 'on time' after all."

"But I also sense a deeper importance in this remarkable correlation among all major criteria for reconstructing our family tree. This high correspondence can only mean that a great deal of human diversity, far more than we ever dared hope, achieves a remarkably simple explanation in history itself. If you know when a group split off and where it spread, you have the basic outline (in most cases) of its relationship with others. The primary signature of time and history is not effaced, or even strongly overlain in most cases, by immediate adaptation to prevailing circumstances or by recent episodes of conquest and amalgamation. We remain the children of our past -- and we might even be able to pool our differences and to extract from inferred pathways of change a blurred portrait of our ultimate parents."

"The path is tortuous and hard to trace,History is also a hard taskmaster, for she covers her paths by erasing so much evidence from her records -- as Hansel and Gretel discovered when birds ate their Ariadne's thread of bread crumbs. Yet the potential rewards are great, for we may recover the original state so hidden by our later stages -- the prince behind the frog..."

Quod erat demonstrandum.

.....

- (*) I assume that everyone knows that my specialty has been Afrasian, primarily Omotic and Cushitic. Militariiev is producing proto-Berber. No one seems to think proto-Egyptian needs to be done, since we have epigraphic early Egyptian of 3100 BC. Proto-Omotic still is absent but is the object of my field trip to Ethiopia in the Fall. Christopher Ehret has just finished a late draft of a massive try at p-AA; in it he eschews proto-Berber or Berber-to-other-AA correspondences as a matter of singular difficulty. And just as we go to press I find in my mailbox a copy of Igor Diakonoff's AFRASIAN LANGUAGES (1988). Moscow. Nauka. Central Department of Oriental Literature. Translated from Russian into English by A.A.Korolev and V. Ya.Porkhomovsky. "This edition is a revised edition of SEMITO-HAMITIC LANGUAGES published in 1965 both in Russian and in English." Many post-1965 authors from Aihenvald to Zaborski are in the bibliography, as well as some treatment of Omotic. It contrasts very markedly with Ehret's draft in content and style -- that is interesting.
- (**) Linguists love words like "rigor, exact, elegant, precise", instead of probabilistic words like "tendency, mode, likely". Does anyone know why?
- (***) A new edition of Anttila, joined by Sheila Embleton, is said to be coming out or has just come out. Sight unseen, I recommend it to all!

7 East Barnton Gardens,
Edinburgh EH4 6AR,
Scotland, UK.

27 February 1989.

Dr. H.C. Fleming,
Mother Tongue,
69 High Street,
Rockport, Mass. 01966, USA.

Dear Dr. Fleming,

I am sorry about the further delay. The long article I mentioned took a lot more work than I had bargained for.

I don't know if my initial reaction to MT would have been better described as an identity crisis or just plain shock. If I had been asked "Do you see yourself as a long ranger?" before I had set eyes on MT, I would probably have said "Yes" without much thought. MT drove me to a review of the historical source and present defensibility of that "Yes".

The historical source is clear enough. My career in African languages began with ten years (1951-61) in Guthrie's department at S.O.A.S. in London, and that was at the time something of a den of lions for any West Africanist who, like myself, saw in Greenberg's Niger-Congo hypothesis the obvious explanation of the obvious similarities between Western Sudanic languages on the one hand and the Bantu languages on the other. (For an excellent and eminently readable account of the Africa Department at S.O.A.S. in those days see Colin Flight, (i) 'The Bantu expansion and the SOAS network', History in Africa 15 (1988): 261-301, and (ii) 'Malcolm Guthrie and the reconstruction of Bantu prehistory', History in Africa 7 (1980): 81-118.) In that context, a long ranger would have been anyone who subscribed to the Niger-Congo hypothesis.

Now, however, I live in a world in which the Niger-Congo hypothesis is generally accepted, and I am engaged in an enterprise which, I have to admit, can no longer, in the light of the results achieved so far, be regarded as long range: the comparative study of certain Volta-Congo (VCg) languages (VCg is roughly Greenberg's Niger-Congo (NCg) minus his Mande and West Atlantic).

I certainly had a much more positive initial reaction, decades ago, to the NCg hypothesis than I had, more recently, to the Nostratic hypothesis. I might, however, have been less positive about NCg if the West African language with which I was mainly concerned had belonged to either the Mande branch or the West Atlantic branch, which are now seen as falling outside the core which I call VCg and which Bennett and Sterk call Central NCg.

Even within NCg, I am reluctant to devote much time to non-VCg languages. It is not that I am not interested - far from it - but rather that I believe the best contribution I can make to comparative NCg studies as a whole is to carry on with

the reconstruction of pVCg. I would expect the main drive towards pNCg to come from scholars working primarily on non-VCg NCg languages who felt compelled to sort out the regular correspondences with pVCg the way I have long felt compelled to sort out those between the Akan (Twi-Fante) language of Ghana and pBantu.

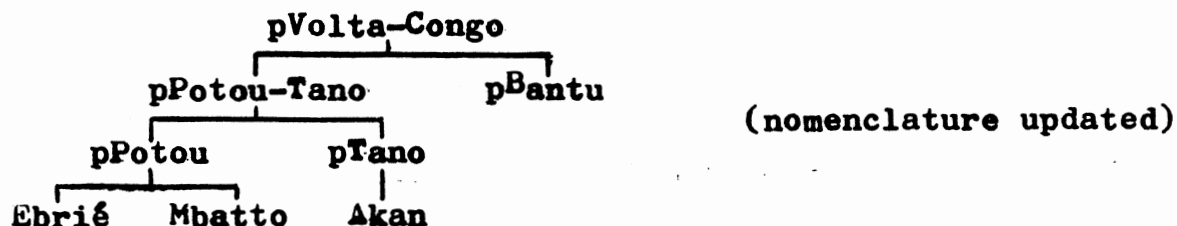
My attitude to Nostratic is essentially the same as it is to NCg beyond VCg: I am open-mindedly interested in keeping an eye on what is going on and in making comments, but in the long run I believe I best serve long range studies by conserving the bulk of my energies for VCg.

The comments on non-VCg languages that I see myself making most readily would reflect my interest in the various phonological changes that one has to posit within VCg. The assumptions I now make about what constitutes a possible or plausible phonological change differ quite drastically from those made by the average Indo-Europeanist. Let me illustrate, as I do in my chapter in Bender-Samuel's volume on the NCg languages (April 1989?), with an account of the way I see (i) the manner-of-articulation system displayed by the pIE stops, and (ii) the effect that the Germanic consonant shift had on it.

In a 1970 paper on 'The lenis stops of the Potou Lagoon languages and their significance for pre-Bantu reconstruction' I noted the following manner-of-articulation correspondences:

Ebrié	Mbatto	Akan	'Common Bantu' (Guthrie)
voiceless fortis stop	voiceless fortis stop	voiceless fortis ^{strident} distal continuant	voiceless fortis stop
voiceless lenis stop	voiced lenis stop	voiceless fortis stop	voiceless fortis stop
voiced fortis stop	voiced fortis stop	voiceless fortis stop	voiced fortis stop
voiced lenis stop	voiced lenis stop	voiced fortis stop	voiced fortis stop

The languages are assumed to be related as follows:



The four series of stops in Ebrié, which is spoken around Abidjan, the Ivory Coast capital, are presumed to go back to pVCg. The voiced lenis stop series has been heavily eroded, however, by the substitution of nasals and other sonorants: 'b (lenis b)

survives only in the absence of any adjacent nasal segment, and 'd, the only other survivor, only in the absence of any adjacent nasal segment and only before i, u, y, w, r.

Ebrié and Mbatto make up the Potou (Greenberg's 1955 Eastern Ivory Coast Lagoon) group, and the situation in pPotou appears to have been essentially as in Ebrié. In Mbatto, however, the original voiceless lenis stops have become voiced, merging with what was left of the original voiced lenis stops, with the result that it has a three-way stop system t/d/'d which invites comparison with pIE *t(h)/*dh/*d.

The Akan sounds I now derive as follows from those of pPotou-Tano (pPT), which are of course presumed to be the same as those of Ebrié and pVCG:

pPT	t	't	d	'd
1. Fortis stops become fricatives:	θ		ð	
2. Lenis stops become fortis:		t		d
3. Voiceless fortis continuants become distributed strident	s			
4. Voiced fortis continuants become voiceless:				θ
5. voiceless fortis continuants ^(Non strident) voiceless fortis continuants generally become stops, though not all at the same time and not always in all contexts:			t	
Akan	s	t	t	d

I see the first two changes as a push-chain which eliminates the relatively highly marked lenis stops. Similarly, I see 3 and 4 as a push-chain which eliminates the relatively highly marked voiced fortis continuants.

The neutralization of the fortis/lenis distinction in 'Common Bantu' is a complex matter which need not concern us here.

Now if we posit the Mbatto system in pIE (i.e. if we substitute fortis/lenis for aspirated/unaspirated and treat the voiceless stops as fortes), the Germanic consonant shift starts off with a push-chain very like the first of the two in PT-to-Akan:

pIE	t	d	'd
1. Fortis stops become fricatives:	θ	ð	
2. (Redundantly voiced) lenis stops become (redundantly voiceless) fortis stops:			t
3. voiceless fortis continuants ^(Non strident) voiceless fortis continuants become stops in certain circumstances:		ð	d
pGermanic	θ	ð	d t

(Note that my representation is systematic-phonetic throughout.)

The claim that the devoicing under 2 is automatic rests on the

assumptions that where there is only one series of lenis stops, these are voiced, and that where there is only one series of fortis stops, these are voiceless.

The main thing in IE-to-Germanic as in PT-to-Akan, by the above derivations, is that a push-chain trades in fortis/lenis for fricative/stop. PIE is presumed to have had only one fricative before the shift, namely *s; pPT, like pVCg and pBantu, is presumed to have had none at all.

I would emphasize that Mbatto, with its two series of voiced stops as against only one of voiceless stops, is a real present-day language, and that any observations about pIE which rest on the assumption that languages such as Mbatto do not exist are therefore invalid.

The same goes for observations about pIE which rest on the assumption that there is no such thing as a fortis/lenis contrast. I discuss this contrast at some length in the 'Characteristics' section of my 'Kwa' chapter in John Bendor-Samuel's forthcoming book. In particular I mention Ben Elugbe's work on the nature of the fortis/lenis contrast in the Edoid languages of Nigeria, and his finding that "muscular tension or force of articulation ... is a less tractable and less significant aspect than duration or length" ('On the wider application of the term 'tap'', Journal of Phonetics 6 (1978): 137). I was not convinced, at the time I wrote my 'Kwa' chapter, that fortis/lenis should be subsumed under long/short for pVCg, but subsequent work, on which I report in an as yet unpublished article, has produced an unexpected piece of evidence that has won me over. Now who can claim that there is no such thing as a long/short contrast?

Two contributors to MT, Dolgopolsky (MT 2) and Pulleyblank (MT 6: 33), report sound correspondences involving the pIE 'plain' voiced stops, i.e. the ones that look like lenis, or short, voiced stops to me.

Dolgopolsky quotes Illic-Svitic as deriving them from pNostratic voiceless stops. This is in line with what I might have expected, as I derive their Mbatto counterparts from pPotou voiceless lenis stops in most cases.

Pulleyblank derives them from sonorants, thus: *N > *g, *Nw > *gw. I find the correspondence with nasals interesting as in real, present-day Ebrié (Mbatto's closest relative), as well as in my pVCg as reported in John Bendor-Samuel's book (see below), nasal consonants occur prevocally only as variants of oral voiced lenes in nasal environments. The nasalized vowels of pVCg are almost certainly ancient; the nasal consonants don't seem to go back much further than pVCg. So while the correspondence of the pIE consonants with nasals is in line with what I might have expected, their derivation from nasals is not.

Kay Williamson, in her 'Niger-Congo overview' in John Bendor-Samuel's book, gives my version of the pVCg consonant system as follows:

5

p	b	t	d	e	j	k	g	kp	gb
'p	'b	't	'd		'j	'k		'kp	'gb
	[m]		[n]		[ɲ]				[ŋm]
			l						
			[ɭ]						

All the consonants enclosed in square brackets are contextually conditioned nasal(ized) variants of the consonants directly above. I would now add that *'j and [*ɲ] could perhaps go back to pre-VCg velars (note that no *'g or [*ŋ] is posited in pVCg), and that the evidence for the remaining two palatals *c, *j is much weaker than that for any of the other four voiceless/voiced fortis pairs.

It would not surprise me too greatly to come across a VCg language with a consonant system identical to that of pIE, for the system would be derivable from that of pVCg in quite a straightforward manner. I would not however claim that this means anything more than that the diachronic phonology of VCg can be expected to shed light on problems arising in the diachronic phonology of IE or of any larger grouping of which it may form a part.

I have comments on two things you say in connection with the forthcoming publication of John Bendor-Samuel's The Niger-Congo Languages (MT 6: 20). First: "1200 languages at least, grander by far than the sum total of all the Amerind and Nostratic languages proposed by anyone". I would add that the VCg core alone accounts for the great majority, and that I am satisfied that much of the phonology of pVCg is reconstructible even from the few languages that I have been working on. One day, I suggest, the reconstruction of pNCg or something approaching it could overtake that of pIE to become the greatest achievement of comparative linguistics.

Second: "Niger-Kordofanian, called Niger-Congo in the actual title". It is not that Niger-Kordofanian is called Niger-Congo, but rather that the Kordofanian languages are treated as falling not outside but inside Niger-Congo. Greenberg devotes most of his chapter on Niger-Kordofanian in The Languages of Africa to what the Niger-Congo and Kordofanian languages have in common, and none of it to what distinguishes them. There seems to be no reason at present to regard the Kordofanian languages as any more remote than the Mande languages from the remaining Niger-Congo languages; see (in due course) the section on 'External classification within Niger-Congo' in Thilo Schadeberg's chapter on 'Kordofanian'.

Yours sincerely,


John M. Stewart.

cc: Bendor-Samuel, Elugbe, Schadeberg, Williamson.

P.S. Thank you for the circulars you have kept sending in the

PHONOLOGICAL SAMENESSES AND DIFFERENCES, THEIR ROLE IN COMPARISON

AN EMBRYO PAPER

J.J. Pia
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In a recent issue of *Mother Tongue* someone asked, How can we compare two languages when they are so typologically different in their phonologies? The question has occurred before. It usually centers on the presence or absence of "exotic" segments in one or more of the languages. By exotic we mean things like glottalized, imploded, or doubly articulated stops, e.g., the kp of Kpelle (Liberia), or like clicks, etc.

My aim is to show that the presence or absence alone of such exotic phonic segments argues neither (a) against nor (b) for a possible genetic relationship between or among the languages in question. The exotic segments often in fact have nothing to say about the question of relationship. Their presence or absence should not in the least hinder comparisons, either casual or detailed. I suspect most linguists understand this principle; but many scholars working in related disciplines using language data do not. Such scholars are my audience for this piece.

An example case are the Khoisan clicks borrowed into Xhosa and other southern Bantu languages. We know that the clicks are borrowed and their presence in Xhosa does not argue for a genetic relationship between Xhosa and its Khoisan neighbors. Their presence does argue for a close social relationship, one in which "Khoisan chic" played an important part for a period long enough for Xhosa to borrow the clicks. Beyond that, however, the Xhosa genetic record includes no Khoisan transcripts.

Another such case is Hungarian and its borrowing of palatalization from Slavic. We

know that Hungarian is not Indo-European and, therefore, not Slavic. We know also that other Finno-Ugric languages which are not in contact with Slavic do not exhibit palatalization processes. Even one that is, Finnish, is free of the kind of palatalization phenomena that so characterizes Slavic. It follows that finding exotic phonic phenomena in any two or more languages does not in itself suggest genetic relationship.

We need to understand how such segments can emerge or disappear. With such an understanding in hand, we can go on to consider what kinds of testimony such exotic segments, or their absence, might give about matters other than genetic relationship. The reference above to "Khoisan chic" is a hint at the directions our investigations may take us.

Three major ideas are required to understand the processes involved in the rise and disappearance of exotic segments.

- (1) The idea of phonological feature is fundamental.
- (2) That features can have different phonetic realizations in different settings is critical.
- (3) The principle of group membership provides the final touch.

Taken together these three ideas constitute a process by means of which phonological segments can develop or wither. Spelling this all out will be tedious at points. Please bear with me as we slog our way together through some heavy underbrush.

Phonological Features

Linguists will find this section tedious and boring. Please feel free to skip to the next section.

When we study phonetics, we learn to assign certain symbols to certain noises and to their descriptions. For example, [b] is a bilabial stop voiced. We often come to think of items like [b] as units, indivisible. In fact, we come to think of each speech sound as an indivisible unit. So we consider a phonological inventory a collection of indivisible units.

Then we learn about things such as aspirated stops in languages like English. We can still write them with single letters, e.g., [p], [t], and [k]. We know also, though, that the aspect they share — in this case aspiration — can be written right along with the "main" character, e.g., [k^h]. We find ourselves, then, transcribing a number of speech sounds with two characters, one for the "main" sound and another for the aspect they all share.

This situation leads us to wonder if we can eliminate writing part of the transcription, that is, those "extra" characters that we seem to be repeating all the time. Maybe we can handle them in some sort of "rule." Maybe the rule should state that all segments of such and such a character also have the element *z*.

In the English case we want to say, "The voiceless stops are all aspirated." Of course, if we can say it in Natural Language (English in this case), we ought to be able to say it in Linguistics too. And, lo, we can. (Just look at what happens below.)

Once we have separated out aspiration from the sound segments, we have started down a path of great potential. We can in fact consider all segments to be composed of elements. Now, instead of understanding speech sounds to be indivisible units, we perceive them as collections, "bundles" is the term in the trade, of those elements.

Further, when those elements are properly combined, they yield the segments in question. This arrangement makes it possible to specify quite a number of sounds, including many that do not occur in the language we are studying at the moment. Off hand this seems like a significant advantage. It turns out to be so.

What might those elements be? Some may be articulatory. For example, we can say that the segments [p, b, t, d, k, g] all share stop-hood. We have, by so stating, separated stop-hood out of the segments. We can identify some of the segments as voiced and others as voiceless. Arrangements like this allow us to see that [p] and [b] can function very similarly in a language. Their only difference may be their voicing.

Of course [k] and [g] can share this same similarity of function as well. But, now we ask ourselves, "Whatever happened to aspiration? It is there with the [p, t, k], but not with the [b, d, g]." We figure some more and decide that we can say somewhere in the grammar that, "Voiceless stops are aspirated in environments *x*, *y*, and *z*." If we can say that, then we have permitted ourselves to think of the sounds in much the same way as native speakers do.

Native speakers may very well understand that under certain circumstances [p] alternates with [b], [t] with [d], and [k] with [g]. But they know also that when they choose [p, t, k], they must also check the environment to see if they must add aspiration to their pronunciation. The checks are quite rapid, and, as we say in the computer biz, transparent. That is, the user is unaware that the activity has taken place. The native speaker unwittingly either puts in or leaves out aspiration as the circumstances require.

The elements we've been referring to are known to linguists as "phonological

features." Each phonic segment is said to be composed of them. Und so weiter, to quote someone we all know.

This is the logic behind the phonological feature system developed first by Trubetzkoy and his colleagues in the Prague Circle. It was taken to the US and worked on still further by Jakobson and his students. It reached its culmination in the generative phonology put forward by Morris Halle and his colleagues and students at MIT.

Let us carry the logic a bit further. The features can be combined by a language to produce a wide range of segments. Further, those features help us see similarities and differences in the operation of phonological processes on and in certain segments, especially those which seem at first blush quite unrelated.

Feature specifications of each segment will vary to some extent from language to language. Segments on the periphery of the phonological system, e.g., semi-vowels or glides, exhibit the greatest variance across languages. For example, in Language A the glide [w] alternates with [b] in certain circumstances. That alternation tells us that the specification for [w] is that of a labial. In language B, on the other hand, [w] alternates only with [k]. In language B, then, [w] is specified as a velar.

Over time a language can change its specification of some segments; but in general those specifications tend to stay put, and other things change around them. We will see some of that in the examples in the following sections.

Ethiopic Languages, A Sample Case.

Amharic and Tigrinya, to name just two of the languages, have their well known glottalized stops. We write them with a following [ʔ]: [tʔ]. We find ourselves writing a lot of double character transcriptions for the glottalized segments in Amharic, Tigrinya, and all the other Ethiopic

languages that have them.

In our example from English, we solved this problem by taking several steps. First, we decided to identify aspiration as a feature. Second, we were able to say that all voiceless stops were aspirated in certain environments.

We find with the Ethiopic data, that we can consider the glottalization a feature. However, because the segments with the glottalization feature do not share a unique set of other features, we can't set them apart like we could the voiceless stops of a language like English. For example, in Ethiopic [t, ɕ, k] are all voiceless stops all right, but we also have glottalization appearing with [s]. Since the languages also have [f], we can't say that glottalization appears with voiceless stops and voiceless fricatives. That statement would give us a glottalized [f], which is a no-no. It is a no-no not because it is impossible; it clearly is. It is a no-no because it does not occur in the languages.

It appears, then, there is no way out of writing two characters for the glottalized segments in the Ethiopic languages.

(Some people write the glottalized segments with upper case or capital letters and the others with small or lower case letters. However, that arrangement means that some sentences will start with small letters instead of capitals. try reading some Text whiC uSes capiTals only in this way. iT's a bummer.)

Even so, we can identify a series of segments in Amharic or in Tigrinya as consisting of all the regular articulatory stuff which yields the stop or fricative "part" of the segment. In addition, they have the glottalization feature. So, we have "plain" [t, ɕ, k, s] and glottalized [tʔ, ɕʔ, kʔ, sʔ].

If this approach is correct, then we could expect to find other segments possibly

appearing with the glottalization feature. That happens in Amharic. The English or French word post, 'mail,' appears in Amharic with a glottalized p as in [pʔost]. To be sure, only cosmopolitan Amharas use the term, but what is important is that they know how to produce a segment when they need one. They do it by combining features to yield what they need or want or both.

So, we have developed the notion of feature. The linguists can stop yawning now.

Phonetic Realization of Phonological Features

In our discussion of Amharic and Tigrinya we noted that [t, ɕ, k] and [s] can be glottalized. We know that these languages are Semitic. We also know that other Semitic languages show some interesting parallels. Hebrew, for example, has its so-called "emphatic" consonants, ṭ, ḏ, ḳ, ṣ, etc. Arabic has a similar set of "emphatics," consisting of the set [t̤, d̤, k̤, ʂ]. (They are usually transliterated with a dot under them, but this typeface won't do that without a lot of trouble.) The emphatic k is usually transliterated with a q.

In addition, Arabic and Biblical Hebrew show two other "pharyngealized" sounds, the 'ayin and the che. In Hebrew the latter is the n of the toast ḥay 'to life.' These two segments may somehow be involved, too.

Anyone working in Semitic notices very quickly that the glottalized segments of Ethiopic are parallel to the "emphatics" of Arabic and Hebrew. Other Semitic languages not named here show the same parallels. Clearly, the parallels are not exact — Arabic has an emphatic d while Hebrew doesn't. But the parallels are striking just the same.


The usual approach in comparative Semitic studies is to view the sounds as indivisible units. One finds statements in the literature like "Arabic q corresponds

to Ethiopic glottalized k." Those statements are all right as far as they go, but they seem to miss a point.

The point is that all these languages share a set of features which has come down through the ages from the parent proto-Semitic. One of those features specifies certain consonants as "emphatic," whatever that means. Each language has made some changes, assigning that feature to a slightly different set of consonants.

The feature could be assigned to all the consonants in principle, yielding a consonant inventory roughly twice the size of the "normal" Semitic inventory. Would any language do that? Modern Colloquial Egyptian Arabic has come pretty close. It has "emphatic" b, f, m, n, l, and r if Ferguson is to be believed. And I expect he is.

Here is the Important Point:

 Each language has not only assigned the feature to different consonants. Each has also "selected" a different phonetic rendering of that feature. So, Arabic "emphatic" t is rendered phonetically one way while the "emphatic" t of Amharic is rendered as glottalized.

When we take this same approach to the wider connections of Afroasiatic, we find some very interesting things. For example, Hausa shows "emphasis," however we ultimately define that, with k and d, at least. Implosion is the distinguishing element with the d. Again, Somali also has "emphatic" counterparts to k and d. We could list the languages one by one, identifying the consonants with the "emphatic" feature. But, you get the point by now, I'm sure, that this is an Afroasiatic characteristic, not just Semitic. That makes it very old indeed. Does it precede the split-off of what we now call Indo-European or were such features added later? If older, how would we expect this set of features

to appear in Indo-European? We could go on with this stimulating line of thought, but instead let us return to the issue at hand, namely the "emphatic" feature in Afroasiatic.

Each Afroasiatic language has assigned it to one or more of its consonants, usually a subset of what appears to be an original set, and developed pronunciations for each such consonant in accordance with some other principles to be outlined below.

"Selecting" a Phonetic Rendering.

To account for the variety in phonetic renderings we must become amateur social psychologists for a bit. Also better than average linguists, ones with an unusual eye for patterns that most scholars miss.

1. The Principle of Phonic Potential.

Let's look first at Akkadian, the language that preceded both Assyrian and Babylonian. Most people know it as the language written with cuneiform characters on clay tablets. Also, they may know that Hammurabi's code was written in Akkadian.

In Akkadian certain verb forms show an alternation between /l/ and /š/ in the causative prefix. This was not an extensive alternation nor one that has caused anyone much worry. It does, however, make manifest a certain principle. Namely, sounds must share some elements in order to alternate. It is clear, for example, that the Akkadian /š/ was not the standard Western European /š/ produced with either the tip or the blade of the tongue against the palate. Instead, the Akkadian /š/ must have had the option of being produced by releasing the air along the side of the tongue. In other words, it must have been a lateral /š/.

If it had not had such a lateral potential, it would not have been able to alternate with /l/. At some point in the history of Akkadian, scribes began to hear

the lateral /š/ as /l/; so, they wrote it that way.

Remember, too, our noting above that the specification of w can vary from language to language. Clearly, the [w] has the inherent potential for being either labial or velar. The labial designation focuses on the rounded lips aspect of the sound. The velar designation focuses on the [u] character of the sound.

The designation can go either way, but it must rest on something inherent in the sound. Similarly, a given sound can alternate with another in certain environments, but the pair of them must share some inherent phonetic aspect. The same is true for historical changes. One sound does not arbitrarily become another. Instead, there must be a phonic link from one to the other.

Most historical or comparative linguists up to this point in time have operated pretty much the way geologists did until the theory of plate tectonics was developed. The geologist says, "We got mountains here that used to be sea bottom. The fossils are all sea bottom critters." The interviewer asks, "How did this old sea bottom get lifted into these mountains, Dr. Geologist?" Answer, "We don't know, my dear. Just happened. How about a cuppa coffee?"

The literature is full of X's corresponding to Z's, but no attempt was made usually to account for the correspondence. Now, it seems to me, we are forced to. So, we must develop the ideas which will support such efforts. This is the first one, the principle of phonetic potential.

2. Group Membership and its Linguistic Reflections.

All God's chillun gotta belong to a group. We all have an ethnic identity in addition to our familial one. If we lived in Lichtenstein chances are we would know everyone, since there are so few

Lichtenstinians. We would all live in the same town, more or less, shop at the same markets, get our mail from the same postoffice, etc. When looked at from outside, we would appear as homogenized as the milk at our favorite supermarket.

Now, returning to our own identities, we are aware of a few "them - us" feelings. We know who we are and who we aren't. We know who belongs to our group and who doesn't. And there are times when we want to be able to distinguish between ourselves and the "others," whomever they may be.

If our group is small enough, we can, like the Lichtenstinians, recognize each other by sight. But, if our group is too large for us to know everyone by sight, we need to develop some other techniques or badges of membership.

Bill Labov's famous studies of Martha's Vinyard are the classics in the field of linguistic badges. He was able to show that certain pronunciations signalled membership in the islander communities. People who did not use those pronunciations were outsiders, the scorned "summer people."

In the Martha's Vinyard case islanders raised the first member of the diphthongs /aw/ and /ay/. If a customer at the checkout counter didn't say at least /ay/ in my or /aw/ in house, s/he paid the "regular," i.e., higher, price.

Though the community was too large in number and too diverse for everyone to recognize everyone by sight, the use of special pronunciations was quite satisfactory as a badge of membership. And, importantly, most outsiders couldn't figure out what it was that so clearly labelled them as required to pay more. Labov, being a linguist, figured it out quickly.

Not every group, ethnic or other, consciously develops ways to distinguish themselves linguistically from other groups. Sometimes such differences arise quite without conscious awareness. At other

times, of course, no such differences seem to arise at all. Instead, people's affiliations are revealed only by the content of their conversation. For example, in the old days, i.e., before Vatican II, references to first Fridays marked one as a Roman Catholic involved in a Novena. Terms like "preaching charge" came only out of Methodist mouths in the town I grew up in. "White baptismal robes" identified the Baptists. Etc. In other words, if the investigator listens long enough and in enough different environments or social settings, the identifying linguistic signals will come to light.

Affiliation and Phonetic Potential.

Records from the Rhennish Fan during the late Renaissance detail a story of high fashion and a linguistic chase. It seems that the wealthy folks decided to identify themselves to each other by the use of certain pronunciations of what historically was /s/. The rising and increasingly wealthy middle class folks decided to emulate the higher and wealthier classes. One way was to adopt their speech.

Apparently, when the bourgeoisie, to use modern terminology, found their pronunciations adopted, they changed them. So, at some point /s/ became /ʃ/. When the burghers, too, came to use /ʃ/ for /s/, the fancy folks moved back to something close to /θ/. The aspiring middle class people followed them. Hence, the notion of chase. Wherever the high class people went along the line of apical fricatives, the middle class folks were sure to go.

Notice that the sounds used all shared phonological features and what we call the phonetic potential. The /ʃ/ is implicit in the specification of the /s/. It requires very little movement conceptually to move the tongue back a bit phonetically. Likewise, the move forward to the region of /θ/ involves simply moving the tip of the tongue forward toward the teeth. The phonetic

potential for each of the changes was present in each case. There was no big jump from one sound to another entirely different.

Phonetic Potential in Semitic.

Now let's look at the Semitic situation. With regard to the "emphatic" feature, all the languages were faced with doing something to the standard consonants to make them different if "emphatic." Working with the phonetic potential of each sound yields a range of possibilities.

One possible range can be seen with [t], for example. With most voiceless stops there are usually two closures. The obvious one is in the mouth. The second occurs simultaneously at the glottis. When [t] is aspirated, both closures are released simultaneously and there is a delay before the vocal bands start vibrating for the following vowel.

One minor change produces what we call "glottalized" [t]. Instead of releasing the glottal closure simultaneously and allowing the air to flow during the delay, we can do two other things. First, we keep the glottis closed, allowing no air to flow when the tip of the tongue is pulled away from the roof of the mouth. Second, we hold the glottal closure a bit longer, finally allowing the vocal bands to vibrate.

This means that we can release the stop in any number of ways into the period of the delay. A simple pulling of the tongue away from the roof of the mouth will yield one sound. Another way is to tighten the muscles in the tongue, compressing the air in the mouth and the throat above the glottis. Then, when the tongue tip is pulled away from the roof of the mouth, the compressed air produces a little pop as it rushes out beyond the tongue tip closure.

So, we have phonetic potential in a "plain" [t] which can yield aspiration or glottalization. We also have potential for another membership badge. We can use a

strong popping noise to accompany the release of the tongue tip, or we can have a relatively weak pop. Women speakers of Amharic use the weaker noise; men the stronger. The stronger pop is also used in public speaking, while the weaker sound is used in more casual conversation.

Egyptians in Cairo consciously expect women to use a softer version of "emphatic" sounds and men to use a stronger version. Some women are said to "talk like men" if their renderings of the "emphatics" are strong. My guess is that we would find similar gender differences throughout Afroasiatic were we to study this topic thoroughly.

Linguistic Badges of Affiliation

These last two paragraphs lead us to the point of recognizing phonetic renderings as badges of membership. What is required is that the rendering mark one as a speaker of one or another of the languages. One group can decide to render the "emphatic" [k] as a glottalized [k]; another can simply move it back to just before the uvula, producing the sound that we write with a q. This latter solution has been the traditional one in Semitic, but it clearly is not the only non-glottalized solution, as witnessed by the Hausa "emphatic" [k].

The need to identify oneself as a member of Group A as distinguished from Group B can arise under a number of circumstances. We look here at three examples.

The Need for Coze.

Once a group achieves a certain size and geographical spread, people often feel a need to be part of something smaller, more immediate, and more direct. Not to say more cozy. Dialects may serve as linguistic badges which allow folks to identify with a particular subgroup within a society.

Dialect studies, we remember, make use of the concepts of focal and relic areas. Both reveal something of the society's history. Focal areas result when people want to affiliate, however loosely, with the folks in a particular location. The affiliators adopt as linguistic badges elements or characteristics of the speech of the affiliates. The speech of some people in eastern New Jersey, for example, is almost more "New York-ish" than what can be heard on the streets of Manhattan from natives. This same kind of affiliating lay behind the linguistic chase in the Rhennish Fan. In that case, however, the emulated speech was that of a social class rather than that of a location.

Relic areas testify to the diverting of attention away from a former focal area toward a new one. No one outside of it talks like the people in a relic area. People have disaffiliated themselves from the folks there.

Fracturing.

Groups which are breaking up into smaller groups also use linguistic badges. Each new or smaller group may be identified with a particular geographical location or, as in nomadic cases, e.g., Somali, with ethnic affiliation. As people feel themselves different from "them," they may employ linguistic differences to mark themselves off. Those linguistic differences in turn emphasize the notion of differentness which may lead to more badges of affiliation. Und so weiter.

Affiliation across Linguistic Boundaries.

What happens when the affiliators are culturally or linguistically different from the affiliates? The Hungarian and Xhosa cases are examples of what can happen.

Hungarians and Slavic Palatalizing.

Hungarians attempted to show their affiliating efforts with Slavic speakers by

adopting the feature of palatalization. Even their name for themselves shows the effects: Magyar, pronounced [majar]. [I don't know when or under what circumstances. Please inform me if you know.]

Xhosa and Khoisan Clicks.

Xhosa is a Bantu language, but the clicks are borrowed from Khoisan languages. The Khoisan people have not enjoyed high social status in the eyes of the Xhosa; so, we have to explain the borrowing of the clicks.

At some point in their history Xhosas attributed a high value to Khoisan speakers and imitated their speech as much as they could. That high value might be called "Khoisan chic." The social circumstances have been documented by Lanham [from one of the South African universities but I don't remember the details anymore. He reported his findings at Michigan in the late Fifties during an LSA summer institute talk. Does anyone know if the work was ever published?]

"Khoisan chic" parallels the kinds of "Black chic" that occupied many white upper middle class liberal Americans in the late sixties and early seventies. White people tried to talk like Blacks; so, they said things like, "Hey, man," in order to show their affiliation with Blacks.

The fact that Blacks did not talk quite like the whites imagined served only to make the whites appear foolish when trying to talk with Blacks. The socially important part of this period was the clear effort on the part of many whites to affiliate with Blacks and show solidarity with a political program energized by Lyndon Johnson's Great Society civil rights legislation. Certainly, no less striking is the Xhosa effort at affiliation with Khoisan speakers.

Disappearing "Exotics".

Affiliation efforts can, in principle, also cause the so-called "exotics" to

disappear. If the affiliates attribute very low or negative value to the exotic elements, affiliators can be expected to eliminate them from their speech.

A close, but not exactly parallel, example is presented by Michener in his novel *Hawaii*. A young Hawaiian boy is tested in order to get into a haoli (Anglo) school. The examiner tears a piece of paper in half and asks the Hawaiian lad to describe what happened. The boy referred to it as breaking the paper. That misuse of vocabulary rendered him unfit for the haoli school.

Though the incident described is fictional, it represents the kind of pressure an affiliate group can put on the language of the would-be affiliators. Hawaiian children and young people interested in assimilating into the dominating European society have to change their language to fit that of the dominant group.

Linguistic and Other Badges.

One final word to keep things in perspective. People use all sorts of things as badges of membership in groups. Clothing, accessories of a certain design, hair-do's, teeth filing, tatoos, and a host of other things can be and are used as badges of membership. Linguistic badges can be one more element in the mix of signs of group membership. Or, it can be used as a last resort, when none of the other things are sufficient. Miriam Makeba's famous "Click Song" refers to using language, actually the clicks of Xhosa, as passwords

in the dark when none of the other things visible in the light are available. A correct perspective sees linguistic badges as only one mode of signalling affiliation.

Summary

People can identify themselves as belonging to a given group by linguistic, as well as other, means. The linguistic means used will depend on and arise from the phonetic potential of the sounds focused on.

The kinds of sounds selected can vary widely. We find fairly ordinary shifts from one point or manner of articulation to another. But we also find segments of a phonetically "exotic" nature in certain linguistic stocks. Those "exotic" segments can arise out of fairly "normal" sounds by processes which are well understood and well documented for other languages.

It follows that the presence or absence of "exotic" elements themselves do not argue for or against genetic relationship among or between languages. Similarly, they should not constitute a barrier to comparison between or among languages which appear at first blush to be quite different in their phonological typology. But in some cases attempting to account for the occurrence of exotics according to the normal canons of phonological change can reveal much about other, non-genetic relationships between and among the languages under study.

162 West Rock Avenue
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April 5, 1989

Harold Fleming
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Dear Hal,

You haven't heard from me in a long while as I have been busy preparing a complement of MS-DOS computer fonts for Africanists. When the first component of this project is finished sometime soon, the complement will consist of three corresponding sets of reference fonts, one for the screen (IBM standard screen typeface), one for dot-matrix printers (a century schoolbook/roman hybrid typeface), and one for laser printers (12-point swiss typeface). Fonts in other typefaces will become available in the future. Each corresponding set will consist of a minimum of about 2200 Roman characters (diacritics rapidly increase the number of characters) of past and current use by Africanists. These reference fonts, and accompanying software prepared by Rufus Hendon, Emeritus Professor of Linguistics and Southeast Asian Languages at Yale, will allow the user to construct and load with little trouble language-specific fonts as needed for display, dead-key entry, and printing. The fonts will be useful to MS-DOS computer users with EGA, Hercules Graphics Card Plus, or Everex Evergraphics Deluxe display cards; downloadable 24-pin Brother, Fujitsu, NEC, and (probably) Toshiba dot-matrix printers; downloadable Hewlett-Packard Laser Printers; and, optionally, general-purpose software allowing simultaneous display of multiple fonts on screen. The fonts and accompanying software will be freely available to Africanists through the Center for Applied Research in African Languages; non-Africanists may be charged a small fee for the fonts. More on these details when the fonts are finished.

I will also be preparing an article on the display, entry, printing, and use of nonstandard characters with MS-DOS computers for the benefit of the membership which can go in the August newsletter. An additional article on the transmission of computer files containing nonstandard characters via electronic network can also be prepared if deemed desirable (Joe Pia expressed interest in such information in his article).

If you wish to include the above information in the newsletter, please cut here. ~~Feel free to use it as you see fit.~~

Stanley

Stanley Lewis Cushingham

Character Translation

Import and Export Issues for Long Rangers An experience using the Hercules Graphics Card Plus

Eugene Loos, SIL

A common problem

The questions that arise when a source of language data is a restatement of an original source with an attempt to interpret or regularize the original author's transcription, but without providing the original transcription, is nothing new to comparativists. Sometimes I myself have been the source of my doubts: I attempt to standardize the representation of old sources because the original sources were in a different transcription tradition, or were done before there was a tradition, or were in a broader or narrower transcription than I use, then I forget what the original looked like.

When such doubts arise about the accuracy of the re-edit, I sometimes wish the re-edit had preserved the original's representation, or had at least provided the original along with the interpretation of it. The limitations of the computer screen prevented the inclusion of all the variants that I would have liked.

The problem now presents itself afresh because of the variety of hardware and software that is in use. MAC users do not find it too difficult to utilize symbols not commonly available in the MSDOS world, and even in MSDOS we have no firm tradition that standardizes phonetic representation on computer media. If I import data from someone else's computerized datafile, how do I make it compatible with my present system and yet preserve the original for reference?

I would like to share a relatively simple and inexpensive solution to both problems above. To preserve and show everything that I wanted required a system with considerable versatility, plus a data conversion method that would enable me both to import others' phonetic representations and export my own so that their systems would be able to handle my data their way. I think that handling some of this variety is an issue not foreign to many Long Rangers' concerns.

The hardware solution

The hardware solution was pleasantly inexpensive for me because all I had to start with was an old, standard IBM-PC with a simple monochrome screen. There wasn't much to lose! I simply installed a HERCULES GRAPHICS CARD PLUS (HGCP), which runs on my standard monochrome screen and costs less than an EGA at our local suppliers. The HGCP turned out to be **astounding** when I began to exploit it. It can display 3072 characters, all redefinable, giving the user plenty of capacity for phonetic characters of every variety, plus Ugaritic, Egyptian, Hebrew, and even Mayan petroglyphs too, simultaneously, if one wants.

The software solution

Of course, one needs software capable of addressing that many. I had long been using Microsoft WORD, so I was pleased to find that with WORD 4.0 I can access about 2000 of the full set of the HGCP. The limitations of version 4.0 will be detailed later in this article along with indications of how to get it to address the characters.

WORD is not the only word processor that can meet the need. I have experimented with WORD PERFECT and found that it will address all of the HGCP's 3072 characters, and SPRINT will address at least 2048. NOTA BENE and XYWRITE are listed as being able to handle many or all of them, but I have not had opportunity to test either one.

Creative author's concerns can conflict with character conversions

I wanted a data conversion and display system that would still allow a comparativist to address author concerns. Linguists need the ability to **use language to talk about language**, ways to differentiate readily for both the author and the reader the things **talked about** as well as standard ways to **express emphasis** and identify glosses by use of boldface and italics. Those typefaces should be easily visible on screen: I didn't want to have to clutter up the files with special codes for distinguishing the object language from the metalanguage. For me the needed author power includes handling charts, tables, windows, and redefinable characters. Those are part of the tools of the trade:

- Charts - with horizontal and vertical lines and tabbed columns and the ability to move either horizontal or vertical selections of material in the chart without upsetting the rest of the chart.
- Tables - the ability to insert, swap or extract columns in tabular arrangement, such as shifting one column of reflexes to compare more effectively with another column.
- Windows - while working on a linguistic article in one window, open up a second window and pull out fully formatted interlinearized examples to paste into the article in the first window.
- User-defined special characters - enable the user to reshape characters as needed, both for standardization in one's own data and to preserve an original's representation. At the same time, be able to distinguish easily and visually regular, bold, italics, bold italics, underlined and superscript, in order to differentiate lines of interlinearized text, differentiate examples from their glosses, regular text from examples, and isolate morphemes for focus in an example and associate those morphemes with corresponding parts in the gloss.

The conversion's first requirement: characters.

I use the Americanist tradition (APA) for transcription, so if I am keying in data from IPA sources I want to preserve on screen both the IPA source and my APA equivalent. To key in someone else's data using both their and my user-defined phonetic characters requires, of course, having a way to make and display those characters.

Getting special characters

The HGCP comes with FONTMAN, a software package for redesigning and loading twelve full sets of 256 characters each. FONTMAN is very powerful but not too easy for a newcomer to use. There are other character editors. I use FONTSHOP (FS20)¹ to create character sets, modify character sets, and swap characters between sets that have already been redefined.

With the HGCP, if WORD is loaded in text mode it automatically loads into the screen buffer its own character sets in the various typefaces of bold, italics, superscript, etc. The user gets the speed of text mode with the font variations he needs, that are with other adapters found only in graphics mode. That's fine, but unfortunately WORD wipes out any userdefined characters that might have already been loaded. The rescue is done by a wonderful little utility called XFERFONT², which allows the user to restore immediately any of the 12 sets of 256 characters each that make up the 3072 characters of the HGCP.

Here is how you can access the characters in WORD, WORD PERFECT, and SPRINT in each of the 12 types:

WORD	WORDPERFECT	SPRINT ³
type chr format to use		
t1 italics	3:	normal
t2 strikeital	2:3	ital
t3 superscr	2:4	large
t4 strikesuper	2:2	supersc
t5 italsupr	2:5	subscr
t6 strikitalsup	1:2	subscital
t7 subscr	1:1	largesub
t8 normal	1:3	strike
t9 strikethr	1:4	bold
t10 striksub	1:5	
t11 italsub	1:6	
t12 strikitalsub	1:7	

WORD PERFECT will not only display all of the characters of all 12 sets, it makes it easy to select any set. Microsoft WORD 4.0 will not display the upper ASCII characters 128-254 for sets t3, t4, t5, and t6, and will not display characters 175-222 for sets other than t8 and t9, which is why WORD will display only 2000 of the 3072 characters. Possibly version 5.0 of WORD will address the full capacity of the HGCP.

Importing

¹Available from JAARS COMPUTER SERVICES, JAARS, INC., BOX 248, WAXHAW, NC 28273.

²Available from HERCULES COMPUTER TECHNOLOGY, 921 Parker St., Berkeley, CA 94710, free of cost to users of a Hercules Graphic Card Plus, Hercules Network Card, or Hercules Incolor Card. The requester should send a formatted diskette with a self-addressed, stamped mailer for the return.

³This is only one possible set for SPRINT. SPRINT can be configured in almost any variation, and it might be possible to get more than 9 fonts accessed in one file.

Here is a very simple case of usage differences that I wanted to make conform to my system:

One author uses 'e (an apostrophe + vowel) to represent e with high tone,
another uses e' for e with high tone,
another uses the same sequence 'e to represent e with stress on the syllable,
yet another uses 'e to represent a glottal feature on the e.
and another uses 'e for e preceded by a glottal closure.

If I am keyboarding data, keeping track of the original sequence and adding my own representation is simple with the help of KEYSWAP⁴, a utility that allows one to have up to ten different keyboard mappings. It is compatible with WORD and other programs that we use for sorting and database building. There are other similar utilities available.

Using the Replace facility

I am a terrible typist. With the availability of others' linguistic databases, I want to avoid re-typing data. Wherever possible I want to import data directly, via diskette or other transfer means. Like any word processor of consequence these days, WORD has a controllable *REPLACE* command that can be used to convert any string to another string, and the replace command can effect global changes or be limited to any designated part of a file. Using *REPLACE* it is simple to convert the source 'e to my &. If one needs to control the replacements selectively and there are not too many conversions to test one's patience, *REPLACE* does the job well. To meet my need of having the original form in addition to my standardization of it, I make a copy of the original first, and replace characters in only one of the duplicate forms.

CC

If the source has many entries and the representation is rigorously consistent, conversion with CC⁵ (for "Consistent Changes") is handy. CC is a simple but powerful programming language that allows one to specify changes that incoming strings should undergo. It can change strings of any length to any other defined string, so a source that represents a character by a diacritic + vowel can easily be changed to a unitary character 'l', or whatever one prefers to use. The one requirement is that the source string be consistent and unique. Otherwise the glosses and any exposition of the source text will also be changed if they contain any of the character strings specified on the input side of the conversions.

Exporting

⁴Available from JAARS COMPUTER SERVICES

⁵Also available from JAARS COMPUTER SERVICES.

Converting a HGCP's rich set of characters to other strings so that humbler systems can use the data turns out to be quite simple. All that is necessary is to put into WORD's *plain* printer driver (called PLAIN.PRD) a definition of the unique strings that one wishes to convert each character to, and print to a file instead of sending the file to the printer. PLAIN.PRD contains no printer formatting codes that would clutter up the file, but it does have a character translation table for each font.

For example, if a friend uses @~+ for ASCII#139 (f') that we have in t8, we put into that font's character translation table an instruction to print #139 as @~+ . Then simply print to a file. It produces a file suitable for those needing different input, and can also serve as a means of converting files of data from other sources to a HGCP system when those source files consist of unitary characters (not combinations of characters) that are not unique.

In Panoan language sources "e" is koften found instead of "f" because there was no handy way to make f and e does not occur in the language data though it does in the glosses. By setting the font description for the language data to a different format from the glosses and printing to a file (having made a suitable change in the PLAIN.PRD printer driver), the conversion is quick and rigorously consistent.

This approach should be applicable for any word processor that allows the user to control the processor's printer driver and that will print to a file.

EEL

DEBATING THE ISSUES

1. As predicted before (MT4) the debates among Americanists about the Greenberg hypothesis have increased in frequency and perhaps in heat. Some specifics of the debates include confrontations, while others include amicable discussions among colleagues and more one-sided arguments emanating from journals and newsletters. Before stipulating some of the particulars, I should report a singular development which no rational person can do much about in the short run, even if rational argument has a chance to succeed in the long run. My reference is to the attitude which is spreading among linguists who have not read much directly about the debate and who have not read Greenberg's book. I was much struck by this when I first heard it from a colleague from Michigan. He said to me: "I suppose you know that Greenberg's Amerind hypothesis is no good, doesn't hold water, and is a major disappointment, coming from such a renowned scholar. Right? You knew this already too?" He had not read any part of the book, nor looked at any evidence, but simply assumed that the eminent Indo-Europeanist who gave him this attitude must be right. His eminence got his attitude from Campbell!

We all rely on colleagues for attitudes towards things -- from time to time and especially if the subject is relatively marginal to our concerns. What troubles me about this case of attitude diffusion is that the subject can hardly be marginal to the concerns of Americanists. They are confronted with the only major hypothesis in their topical area in years, a new and challenging theory that reduces the Americas to two phyla (Eskimo-Aleut and Na-Dene) and one super phylum (Amerind). You would think that Americanists, indeed most historical linguists, would want to read it and see for themselves if the Amerind hypothesis was such a stupid endeavour or not! The other thing that troubles me is the social pressure or social conformity aspect of this where groups of people hear "the word" and subtly shove each other into line = beget intellectual conformity from each other. It is sad to see that so much energy and time was spent to educate sheep!

2. The editorial policy of SSILA (Society for the Study of the Indigenous Languages of the Americas) is firmly set against the Greenberg hypothesis, according to the editor, Victor Golla. Most of the members of SSILA are Americanists but, luckily for them, they will not have to read Greenberg's book because their official newsletter keeps telling them what a bad book it is. The odd quirk in SSILA's routine is that Russian hypotheses about Amerind are treated with great respect. Once again it appears that a prophet cannot be honored in his own country -- but colleagues from a distant land who bear the same message are lauded.

3. SSILA will also hold a session on remote things at their Summer meeting in Tucson, Arizona (July 1-2, 1989). The Sunday afternoon (July 2nd) session includes the following speakers or topics: 1:30pm Robert Oswalt, "The Background of Chance Resemblances"; 2:00pm David Payne, "On Proposing Deep Genetic Relationships in Amazonian Languages: The Case of Candoshi and Maipuran Arawakan Languages"; 2:30pm Terrence Kaufman, "Hokan and Oto-Mangue as Examples of Mid-Range Comparison"; 3:15 pm Round-Table Discussion, "Deep Genetic Reconstruction." Chair: Terrence Kaufman. Panelists: Terrence Kaufman, Richard Diebold, Ives Goddard, Victor Golla, Geoffrey Kimball, Margaret Langdon, Wick Miller, Johanna Nichols. They hope that they can find people to uphold Greenbergian or similar ideas. Whether such people will be eaten alive on the spot or not is, as yet, unclear. But a clear majority of

- the panelists are already actively hostile to Greenbergism, while none of the others are known to be long range types at all, at least to me. The whole exercise strikes me as very similar to a hymn sing in a Baptist church. All together now let us rehearse the verities!

4. Joseph Greenberg and Lyle Campbell were permitted to air their respective viewpoints in LANGUAGE, the official journal of the Linguistic Society of America (for those who didn't know it). Campbell fired his broadside in the Fall of 1988 (LANGUAGE 64, 591); Greenberg replied this Spring. Many of us have already read the paired articles and drawn our own conclusions from them. Rather than undertake a formal presentation of their arguments in this issue, we will wait for Sydney Lamb's summary for MT8, provided he gives his final permission. Sydney prepared a lecture on this topic at Rice University recently and from the outline which he sent me seems to have an unusually clear grasp of the key points of argumentation.

5. Greenberg is also confronting some of his critics at a special conference recently funded by the National Endowment for the Humanities. It is to be held at the University of Colorado in the Fall under the general chairmanship of Allan Taylor (Dept. of Anthropology). One could inquire about opportunities to attend the conference and hear the arguments, maybe even participate, by writing to Professor Taylor or NEH. The line up of participants, provided they all attend, seems roughly fair and even-handed to me. The conditions for a good debate or argument, instead of a rout, seem to have been met and I recommend the meeting to one and all. I do not know what the rules of participation are, however, nor any other particulars such as the exact dates.

6. After L.L.Cavalli-Sforza et al published in SCIENCE their much compacted survey of genes in human populations -- plus taxonomy and correlations with linguistic taxa -- which we reported in MT6, a sharp counter-argument was published in SCIENCE on March 31, 1989 (Vol.243, 1651) by Richard T. O'Grady, Ives Goddard, Richard M. Bateman, William A. DiMichele, V.A. Funk, W. John Kress, Rich Mooi, and Peter F. Cannell. Except for Goddard, all are biologists; all are at the Smithsonian Institution. Their arguments seem convincing, although not always clear. They make a number of assumptions, one of which is interesting because it is surely misleading. Quote: "Any attempt to reconstruct global human history must deal with evidence that linguistic relationships reflect a much later period of human history than the genetic relationships among human populations." Why misleading? Since their linguist is known to oppose the search for language origins, then how do they know how old language is? Also misleading because it depends on whether you count the millions of years of shared human evolution from the time of our commonality with the baboons or you count the genes which have come to distinguish among modern populations in the years since the crucial African diaspora. If people carried early language and modern Homo sapiens genes out of Africa or Asia together, then language and later genes would be the same age. Shared mutations would be even younger than early language. Cavalli-Sforza's colleagues have submitted a rebuttal which one expects that SCIENCE will publish. For those not immediately conversant with physical anthropologists and their methods and ways of arguing it would be educational for you to read the set of three articles; or thesis, antithesis, and disantithesis.

7. As mentioned earlier, Stephen J. Gould has written a long and very

trenchant accompaniment to the first thesis in #4 above. Composing both a review article of the original Cavalli-Sforza et al thesis and a kind of commentary on what he has heard in MOTHER TONGUE, but also drawing upon his much earlier college reading of the Grimm brothers and IE ideas, Gould gives us all a lively and wise picture of human evolution in both body and soul. Indeed we may be the only animals who have tongues in their souls and souls in their mouths. Whoever writes the next general book on Language Origins ought to reproduce most of Gould's marvelous article as an introduction to the book.

8. Professor A. Murtonen writes from University of Melbourne, Australia (Feb. 19, 1989) with reference to Juha Janhunen's piece in MT6 :

\\ As to Janhunen's opinions, of course, I am not competent in //
Uralic or North Asian, although I have dabbled a bit in them and have the impression that connection with Korean and Japanese is possible, but not proven and maybe not provable. I tend to agree with Janhunen's more general considerations, including a roughly 10,000 year time limit, and can add reasons for this. My work with some Central Australian languages indicated that in nomadic conditions, language change is more rapid than in settled ones (and written language tends to change still more slowly) and shows peculiarities which forestall lengthy backward reconstructions. The most important of these is confusion of phonetic correspondences due to copious borrowings and re-borrowings between languages spoken by tribes rarely larger than one thousand, often much less, as they repeatedly contact now this, now that tribe and part again. Therefore I don't think it is possible to go far beyond the "neolithic revolution" in the reconstruction of prehistoric language structures in the Middle East. In many comparisons, additional circumstances should be taken into account, such as wandering words and onomatopoeia. The former includes, e.g., numerals; for instance, Semitic numerals all have Semitic etymologies, and still more so in Egyptian and Berber, as far as attested at all; evidently, they were not created until after the dispersal of the proto-Semitic community, if there ever was one. Numeral system was hardly needed in nomadic conditions; the Australian languages I studied had exact terms for 1 and 2 only; 3 was also used for "a few" and beyond that, "many/all". Onomatopoeic words again can have more than one independent origin and still resemble each other.

I intended to write comments on Illic-Svityc's Semito-Hamitic comparisons, but still haven't found time for it; I'm afraid most of them are either dubious or plainly untenable. If I do find time later, I'll do part of them at least. You may publish anything I write, if you deem it worth it.

\\ All yours, A. Murtonen //

9. Professor Igor Diakonoff decided to speak his mind on a number of issues and seemed to want it published. If that judgment is an error, then I apologize. Writing from Leningrad on February 19, 1989, he says"

\\ Dear Hal, Receiving regularly your MOTHER TONGUE, I decided that //
I ought to respond to all the information it contains -- at least to do it once in a while.

As you know, I was not present at Shevoroshkin's conference in Michigan; but I have had reports from participants ranging from enthusiastic to completely negative. Conferences are all right for getting acquainted with other long-rangers and to get first information on what is going on. What we really want are good comparative vocabularies for individual families inside the phyla. It is easy to dismiss Starostin's Sino-Caucasian with a shrug

- because no one has seen the Caucasian vocabulary. However, it does exist on files compiled by Starostin and Nikolaev, and is faultless as to linguistic techniques. The difficulty is to publish the files: Proto-Caucasian has about 80 to 90 phonemes, and the vocabulary will require a tremendous amount of "squiggles" as you term them. Help is badly needed, and MOTHER TONGUE won't be able to give much help. Another point of crucial importance. Long-range comparisons will remain a matter of belief or disbelief until they are based on valid glottochronology. Here again Starostin's work is of the highest importance, because he seems to have created a new reliable system of glottochronology. So far as I know, he read a paper on it at Michigan, but no response has been forthcoming in MOTHER TONGUE or elsewhere.

Now a few words on Afrasian. Actually, we are prepared to publish a comparative vocabulary in the nearest years: we have a very good notion of proto-Semitic, and I hear that Stolbova has finished her comparative vocabulary of Chadic. Egyptian and even Berber do not present any insurmountable difficulties, and only Cushitic and Omotic are in a state of muddle, despite the work done hitherto by a number of scholars. Note that the book AFRIKANSKOE ISTORICHESKOE JAZYKOZNAIE, Moscow 1987, seems to have passed unnoticed; it contains Olga Stolbova's reconstruction of Common Western Chadic and what amounts to an introduction to the future COMP-HISTORICAL VOCABULARY written by A.Yu. Militarev, V.Ja. Porkhomovsky, O.V. Stolbova and myself. It is called THE COMMON AFRASIAN PHONOLOGICAL SYSTEM and needs only minor additions and corrections. I do not think that Afrasian, as a phylum, is a part of the Nostratic order (or class; by the way an elaboration of a common taxonomic system for language groups should be undertaken as soon as possible.) The main reasons for my scepticism as regards Afrasian-Nostratic relations are glottochronological.

This is PRO TEMPORE what I have to contribute. With best wishes,

Igor M. Diakonoff

P.S. DIE SIRENE DES GLEICHKLINGS remains the enemy No.1 in LR comparisons!!

\\

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(Rough translation = the temptress of like sounds = beware of similarities)

Despite my respect and affection for Igor Diakonoff, I shall express my opinion that The Siren is really a good temptress because her Gleichkla"nge lead to Ankla"nge or correspondences. It is my pleasure to agree on almost everything else he says, especially Afrasian not being a part of Nostratic and for the same reasons. (Cf Circulars 1 and 2). On the absence of discussion of Starostin's glottochronology in MT6: I agreed not to say much about the papers given at Michigan because they were going to be published. I have heard from a good source -- and this Vitalij can confirm or deny -- that the book may not be published after all. In addition to that, Starostin's paper "went over like a lead balloon" as my cruel countrymen say, i.e., it had little immediate impact because we could not study such an important matter from the hand-out (I missed getting one) and most people decided to wait for the book to come out so they could read his article in detail. It was also clear that Starostin's glottochronology was not at all like the one I speculated about in MT4. Mine was much more like something that Henry Gleason had written about long ago; Sergei's version seemed to be fairly similar to Swadesh's original but with borrowings taken out and methods cleaned up and everything altogether more reasonable. From what I heard it should be accepted as a good linguistic clock. And Igor is right, we surely need one of those!

10. Eric de Grolier and Claude Boisson sent copies of the same article

from a French news magazine (L'EXPRESS, 6 Janvier 1989), written by Francoise Monier, which discussed the theories of Andre Langaney and Laurent Excoffier about the origin and dispersal of Homo sapiens sapiens from the Middle East around 150,000 BC, as well as mentioning Allan Wilson and Merritt Ruhlen. Boisson also included a page from Langaney's book LES HOMMES, PASSE, PRESENT, CONDITIONNEL, Armand Colin, Paris, 1988. (Costs about 165 French francs) What is pointed out is that a Franco-Swiss theory of human origins exists and it tries to account for most of the same data that Cavalli-Sforza et al incorporate into their theory. But the material is far too rich to discuss here. We will try to get a summary for MT8. Andre Langaney is now the head of the Laboratoire d'anthropologie, Musee de l'Homme, Paris. He supervised Excoffier's dissertation which is said to be a "bombe dans le monde de la paleontologie" (in France presumably).

11. Esteemed and respected senior professor, Indo-Europeanist, cogitator, Austronesianist, math and logic oriented reviver of glottochronology, Isidore Dyen wrote from his Yale address, enclosing a reprint of a very interesting article from FESTSCHRIFT FOR HENRY HOENIGSWALD which will be discussed under Dessert in MT8. I take the liberty of publishing it because of the strength of his opinions and I hope he won't object. His views are quite provocative!

\\ Dear Harold,

May 15, 1989

//

Enclosed please find a copy of an article I wrote that concerns long-range linguistic hypotheses. I would not say that what has appeared in MOTHER TONGUE and elsewhere whether under the stimulus of Greenberg's views or not is fruitless, but then I do not have to. It is clear to me however that none of your authors and of the others understand what is meant by the strict application of the comparative method or, if understanding that, applies the method strictly. I should be only too happy to read something written by one who does apply the comparative method strictly. I do not mind receiving MOTHER TONGUE. However I am retired and I have to limit my expenditures, especially those for efforts in a direction that is unpromising. Please believe me when I say that nothing that I have said here is intended to be or is, as far as I can see, disrespectful. The pursuit of linguistic science is \\ of and by itself respectable. Cordially, Isidore Dyen //

With great respect I have to reply that his attitude shows precisely where many of our problems lie. The most stultifying aspect of long range comparisons and that which will indeed guarantee that this "direction" becomes "unpromising" is the demand that a strict application of the comparative method be made, must be made. It is like cutting your way through a bamboo forest with a surgeon's scalpel. Moi, I would prefer something less sharp but much bigger -- like an axe. Greenbergism is an axe. There are problems which require some kinds of tools, like those he used himself on Austronesian sub-classification, and there are problems which require other tools. On the other hand some of us who think we are already using the comparative method strictly will be rather provoked by this statement. Yo, Moscow! do you want to answer this? What is a STRICT application anyway?

12. The popular magazine, Der Spiegel, ran a short article on language origins and the Michigan conference in their January 2, 1989 issue (p.150-51). Although Der Spiegel is the Bundesrepublik's equivalent to Newsweek or Time, the coverage was not as extensive or as highlighted as Newsweek's earlier treatment of "Eve" had been. Its introductory head had this: "SPRACHFORSCHUNG : FROEHLICHES WURZELZIEHEN: Amerikanische

- Sprachforscher fahnden nach einem rund 15 000 Jahre alten Ur-Vokabular -- sie traueuen von einer Art Steinzeit-Duden." It included a picture of old Stone Age cave dwellers, a photo of Vitalij Shevoroshkin, and a reprint of Pieter Brueghel's famous Tower of Babel. The text led to controversy.

Vitalij Shevoroshkin soon thereafter wrote colleagues urging that protest letters be sent to Der Spiegel. John Bengtson did write such a letter and sent MT a copy. Der Spiegel replied to him. Later Winfred Lehman replied to Bengtson personally, rather than in Der Spiegel, and also sent MT a copy. The four are reproduced below without any comment except that -- from those whose native language is German -- some letters would be appreciated because a proper translation of the mood and style of Der Spiegel's piece became an issue in the discussion. I.e., was the piece vicious or was it playful?

//
THE UNIVERSITY OF MICHIGAN . ANN ARBOR
DEPARTMENT OF SLAVIC LANGUAGES AND LITERATURES

Jan.27, 1989

Dear Colleague :

I'm sending you a clipping from the West German magazine DER SPIEGEL (Jan.2, '89). The article on Ann Arbor Symposium on LANGUAGE AND PREHISTORY (Nov. '88) strongly distorts facts and is very damaging to the research in broad comparisons and deep reconstruction. The article cites only negative things (which are very far from reality); scholars who support Nostratic and similar research are not mentioned. The reader would think that our symposium was a witches' sabbath, -- whereas it was a highly successful meeting of first-rate scholars. Nostratic and Sino-Caucasic reconstructions are very precise. There is nothing mysterious in our methods which are regular methods used in comparative-historical linguistics. In many points, Nostratic reconstruction is more precise than that of the daughter languages. Nostratic reconstruction allows to separate archaisms from innovations and borrowings from inherited words in the daughter languages, -- and so on.

I urge you to write or call to SPIEGEL (USA address: DER SPIEGEL, 516 5th Ave., NY, NY 10036) and protest the above article. The journal should carry some kind of publication (maybe a letter to the editor, or something broader than that) to clear the matter. It is very unfortunate that exactly in Germany (a country with many comparatists) such damaging publications appear.

// Thank you. Sincerely, (V. Shevoroshkin)

//
DER SPIEGEL 743 Madison Street NE //
516 Fifth Avenue Minneapolis, Minnesota
New York, New York 10036 USA 55413

To the Editor:

This letter is to protest the article entitled "Froehliches Wurzelziehen" (2 January 1989: pp. 150-151), which was a highly biased and inaccurate "report" on the International Symposium on Language and Prehistory (Ann Arbor, Michigan, November 1988), at which this writer was a participant. The article gives the reader no real picture of what really went on at the Symposium. One would never guess that the participants included Joseph H. Greenberg (probably the most respected historical linguist of our time), Vjacheslav V. Ivanov (winner of the Lenin prize), Karl Menges (the senior authority on Altaic and Nostratic), Roger W. Wescott (holder of the Chair of Excellence in Humanities, University of Tennessee), and many other respected scholars, all supporting and pursuing research in remote linguistic

relationships.

While we highly respect Professor Winfred P. Lehmann for his work in Indo-European studies, we emphatically disagree with his dictum that research into relationships among major language families is impossible. Lehmann simply parrots the antiquated dogma of William Dwight Whitney (1867) that "The processes of linguistic change have obliterated all traces of any earlier unity." Present-day researchers into remote linguistic relationships have repeatedly demonstrated that this antique notion is totally without scientific basis. Lehmann's claim that "am Ende nichts mehr zu vergleichen gibt" is directly contradicted by the statement in your article that the Nostratic lexicon consists of about 1000 lexemes (a conservative estimate, which is sure to increase steadily as Nostratic research proceeds). The same claim was also scientifically refuted at the Symposium by this writer ("Global Etymologies and Linguistic Prehistory"), but since the writer of DER SPIEGEL's article obviously did not attend the Symposium, and in fact has only the vaguest notions of what actually went on, he had no way of knowing this. (1)

Even more invidious was the rather lengthy digression on the 'work' of Richard Fester, which was totally irrelevant, since Fester had no connection with the Symposium or any of its participants, as far as I know. Fester is the so-called "Begründer der Palaeolinguistik" only in the mind of the anonymous writer of the article, and his 'methods' have nothing whatever to do with the precise scientific methods of the Nostratists and other paleolinguists attending the Symposium.

Besides this clumsy attempt at 'guilt by association', the article is so full of distortions and misrepresentations, that it is now incumbent on DER SPIEGEL to give equal space (or more, since the distortions are so great) to a responsible report on the Symposium, as well as to a selection of the many letters you are sure to receive.

Sincerely yours, John D. Bengtson, Paleolinguist.....

(1) Professor Lehmann was initially expected to attend the Symposium as a discussant, but since he did not appear, we had no opportunity to discuss the issue with him.

.. ..
\\ DER SPIEGEL. Das Deutsche Nachrichten-Magazin. Hamburg, den 27.2.1989 //

Sehr geehrter Herr Bengtson,
wir danken Ihnen für Ihre Zuschrift vom 3. Februar. Die Geschichte 'Frohliches Wurzelziehen' beruhte auf einem Bericht im "Science" vom 25.11.1988, Auskünften von Sprachwissenschaftlern der Universität Hamburg und Informationen aus der SPIEGEL-Dokumentation. Im übrigen möchten wir klarstellen: dass die Autoren von SPIEGEL-Beiträgen nur in Ausnahmefällen namentlich genannt werden und dass Ihr Brief der einzige war, der sich auf diesem Beitrag bezog.

\\ Mit freundlichen Grüßen, DER SPIEGEL, Redaktion, Catherine Badger //

In his letter to Bengtson, Winfred Lehmann also pointed out that:

" I have checked the Spiegel article with current native-speaking citizens of Germany, who themselves have been quoted in the journal. They do not find the article malicious. They admit that it is written with a light touch, but that may reflect the author's uncertainty in dealing with a highly technical topic. I have written Professor Shevoroshkin as much. In my view the Nostratic position received a million dollars worth of free publicity through the article. With best wishes on your work, Sincerely yours, W.P. Lehmann"

.. ..

SPIEGEL on the Nostratic Problem

In the first issue of this year, 2 January 1989, the widely read journal **SPIEGEL** included a report on the Michigan conference dealing with the Nostratic problem last November. The journal gained early attention through insouciance parallel with that of **TIME** in its youthful days. Scholarly affairs, such as the Nostratic theory, are presented with a light touch, often to the dismay of persons discussed. Some Nostraticists were offended by the tone of presentation; the report was headed "Fröhliches Wurzelziehen", reminiscent of the recent season of "Fröhliche Weihnachten". Illustrations included a representation of paleolithic cave-dwellers 'babbling around a camp-fire' and also Pieter Brueghel's well-known picture of the tower of Babel. Yet comparing similarly lively portrayals in other popular journals on scientific conferences, I consider the article a great boon to Nostratic studies, which for the most part are not reported in widely circulated media for general readers.

Bordered by snappy remarks, the report accurately states that the conference was organized by Professor 'Vitali Scheworoschkin' and includes a picture of him. It indicates accurately that work on the theory involves consideration of 'hundreds of languages'; and it likens reconstruction to working in a linguistic tunnel. It also points out that over 6000 languages have been identified, and that these vary considerably. But it adds that Professor Shevoroshkin has stated that 'in the last analysis all languages, with possibility of a few exceptions, are related to one another.'

The article also points out that before the observation of Sir William Jones on the Indo-European languages no thought was given to their interrelationships. It then goes on to say that the picture we now have of them is the result of work by 'generations of word-detectives', and refers to the discovery of Hittite as giving rise to a view of Proto-Indo-European as probably spoken 7000 years ago near the Black Sea or in Anatolia.

Adopting the image of a family-tree, it goes on to say that the Nostraticists aren't content with stopping at this time. For them Proto-Indo-European is only one of nine further language families like Hamito-Semitic, Uralic and Kartvelian. It reports that for the proto- (ur-) language in question a thousand word-roots have been posited, at a time period of from

18,000 to 10,000 Before Christ., citing *kuni* 'woman' as one of the lexical elements.

After pointing out that these views have met with considerable criticism, the article goes on to say that this has not kept Nostraticists from further attention to the problem. They have proposed five 'proto-protolanguages', two of which are 'Dene-Caucasian--the ancestor of Chinese' and 'Amerind' as by Joseph Greenberg. Moreover, Professor Shevoroshkin is proceeding towards a 'pre-pre-pre-language' spoken by cavemen 25,000 years ago.

A few remarks on an amateur pre-linguist, Richard Fester, and on the phonetic equipment of Cro-Magnons 40,000 years ago lead to a final paragraph on Professor Shevoroshkin's expectations for pre-historic study on the basis of the Nostratic investigations.

Even though the article cannot resist a final quip on the lack of written records or recordings to supplement the assumptions, it does present a clear, if lively, picture of the general outlines of work on the Nostratic hypothesis. When one recalls the accounts in the popular media, if any, of papers given at annual meetings of the Modern Language Association or of the Linguistic Society, specialists in the field should be grateful for the attention to their work in *SPIEGEL*. A less engaging article might not have attracted many readers--and sober, scholarly accounts can be pursued in the learned journals or in *MOTHER TONGUE*.

Winfred P. Lehmann
Box 7247
Austin TX 78713-7247 USA

ANNOUNCEMENTS

1. THE FIRST WORLD SUMMIT CONFERENCE ON THE PEOPLING OF THE AMERICAS, sponsored by the Center for the Study of the First Americans (University of Maine, 495 College Avenue, Orono, Maine 04473). A world summit of experts on early Man in the New World has just ended. We mention it because access to the proceedings will be useful to anyone interested in that topic. Also the Center invites people to membership in its organization. For information on the conference or membership in the Center write to Dr. Robson Bonnicksen at the above address. Merritt Ruhlen attended and gave a paper. So did Ives Goddard. Details of their interaction will become available in due time. I was unable to attend, spending the week writhing in pain from lumbago instead. Also Christy Turner and Tom Dillehay among others gave papers. What was easily the most interesting potential gain from the conference in my opinion was the large set of papers on South American sites and early prehistory and by Latin American archeologists. I seriously regret that I missed them. I have been told that Dr. Nie'de Guydon was a star of the show (cf MT5 or 4 for discussion of her and her colleagues work in Brazil). The conference has outdone itself in publicity, being written up in the NY Times, Bangor Daily News, Maine Sunday Telegram, as well as mentioned on local radio and being televised by both BBC and Japanese television. Congratulations to Robson Bonnicksen and colleagues!

2. 4th NILO-SAHARAN LINGUISTICS COLLOQUIUM in Bayreuth. The very important topic of Nilo-Saharan and its deeply divided branches and difficult reconstruction is available this summer (August 30-Sept. 2, 1989) in Bayreuth, FRG. A number of good papers are scheduled but it can be argued that the greatest benefit obtainable would be from the conversations over refreshments and discussions after papers. This is because most of the world's N-S scholars will be there and none of them can give more than a smidgen of her knowledge in ordinary conference paper time. It is a great opportunity for talking about things and picking people's brains. Anything run by Franz Rottland is likely to be warm and friendly in atmosphere, so good communication is likely. For more details write to Professor Franz Rottland, Lehrstuhl AFRIKANISTIK II, Universitat Bayreuth, Postfach 10 12 51, 8580 Bayreuth, FRG.

3. CUSHITIC AND OMOTIC CONFERENCE IN TORINO. Under the inspired direction of Giorgio Banti a conference on Cushitic and Omotic languages and histories was arranged for Turin, Italy, for early summer. Due to the large response and the formidable logistics of a big international conference, Giorgio and his colleagues moved the date to around November 1, 1989. There will be many valuable contributions but perhaps most of all a rare opportunity for some of us to meet the Soviet delegation (Militariev, Belova, Aihenvald, Vetoshkina, Porkhomovsky, et al) whose prowess at Afrasian linguistics has not yet been fully realized by non-Europeans. For more information write Professor Giorgio Banti, Universita' di Roma "La Sapienza", Dipartimento di Studi Glottoantologici, Piazzale Aldo Moro 5, 00185 Roma, Italy.

4. INDO-EUROPEAN SUB-STRATUM CONFERENCE IN YUGOSLAVIA. The fascinating topic of Europe before the Indo-Europeans or who is/are the sub-strat-um/-a will be the focus of a conference in Dubrovnik, Yugoslavia in August. I have not yet gotten the particulars straight. One may write, as I shall, to Professor Maria Gimbutas, % Karleen Jones Bley, 2143 Kelton, West Los Angeles, California, 90025.

5. LANGUAGE ORIGINS SOCIETY 's 5th ANNUAL MEETING AT U/TEXAS. LOS which is our alter ego in academic matters eschews the journal/newsletter format for an annual conference and book publication option. We still have not seriously begun our discussions on merging but a combination of our formats into one might be a good thing since the two organizations are almost perfectly complementary, like Yin and Yang. They meet at Austin, Texas this year (August 10-12, 1989), indubitably including some sponsorship by Winfred Lehmann. For more information write: LOS Meeting, Center for Cognitive Science, Geography Building 220, University of Texas, Austin, Texas 78712

6. IT IS WITH INTENSE REGRET THAT WE HAVE TO ANNOUNCE THAT OUR GOOD FRIEND AND VALUED COLLEAGUE -- PETER BEHRENS -- HAS DIED. IT HAPPENED IN FEBRUARY IN EGYPT. THAT IS ALL THAT I KNOW AT THE MOMENT. WE WILL HAVE A PROPER OBITUARY IN MOTHER TONGUE 8 THIS SUMMER.

PETER WAS SUCH A NICE MAN! IT IS A TERRIBLE SHAME THAT HIS LIFE WAS LOST TO HIM !

DATING AND CHRONOLOGY IN THE LAKE CHAD BASIN
MEGA-CHAD SEMINAR
ORSTOM (Bondy), September 11-12, 1989
ANNOUNCEMENT AND CALL FOR PAPERS

In connection with the activities of the International and Multidisciplinary Network on the Lake Chad Basin (Mega-Chad), a seminar on "Dating and Chronology in the Lake Chad Basin" is to be held at ORSTOM in Bondy (FRANCE), on the 11th and 12th of September, 1989.

The seminar topic will be approached from various points of view: definition of different stages in the evolution of the geophysical context (geology, pollinology, hydrology...), in the development of material cultures (archaeology), of populations (history, geography, social anthropology, demography, biology) and of languages (lexicostatistics, glottochronology).

Priority will be given to methodological questions and to papers presenting a synthesis of various data (periods, areas, linguistic families).

In the historical sphere itself, issues such as the following would be of interest. What chronological reference points are available to us? What global observations can be made from studies of the history of centralised empires, as well as of dispersed population groups? History of colonisation. Economic history. Dating of natural disasters (periods of drought, locust invasions, epidemics) which resulted in famines and demographic shifts.

Annotated bibliographies (arranged by area, period or subject) would also be of great interest. These could deal either with archived documents and ancient works, or with recent writings on history and prehistory.

Verbal presentations - whether in English or in French - are to last 30 minutes, and will be followed by a fifteen-minute period of discussion. Written articles should not exceed 25 pages.

Film showings are not planned, although projectors will be available for the viewing of slides and overhead transparencies (such as for maps, tables and illustrations).

Authors should prepare an abstract of one or two pages. If the abstract is written in English, a French version would be most welcome, if possible. This should be placed on the reverse side of the English copy.

Authors are asked to

- submit the title of their presentation before March 31, 1989;
- send their abstract, as well as any maps, tables and illustrations for inclusion in the seminar handout materials, before May 31, 1989;
- submit, if possible, a copy of their article (hard copy and/or diskette), before July 31, 1989.

Please send all seminar-related correspondence to the following address:

Daniel BARRETEAU and Charlotte von GRAFFENRIED
Mega-Chad Seminar
ORSTOM-LATAH
70-74 route d'Aulnay
93140 Bondy (FRANCE)

DATING AND CHRONOLOGY IN THE LAKE CHAD BASIN
MEGA-CHAD SEMINAR
Bondy, ORSTOM, 11-12th September 1989
REGISTRATION FORM

SURNAME and First names:

Organisation or University:

Address:

Telephone:

Telex:

Will attend the seminar: yes / no

Will present a paper: yes / no

Title of the paper:

I desire accommodation in the University Residence (100 FF per night): yes / no

Arrival (day/hour):

Departure (day/hour):

Comments:

This registration form is to be sent before March 31, 1989, to:
Daniel BARRETEAU and Charlotte von GRAFFENRIED
MEGA-CHAD SEMINAR
ORSTOM-LATAH
70-74 route d'Aulnay
93140 Bondy (FRANCE)

ANNUAL MEETINGS AND ELECTIONS

The Association for the Study of Language In Prehistory (ASLIP) was officially established as a non-profit corporation in the Commonwealth of Massachusetts on April 15, 1989. This the date of the Annual Meeting is as good a date as any for our official beginning because that was when we elected our first officers, read the results of the election of the Board of Directors, duly stipulated who was elected to the Board, and voted in our By Laws.

Those elected to office were:

Harold C. Fleming / President
Allan W. Bomhard / Vice President
Anne W. Beaman / Secretary
Mary Ellen Lepionka / Treasurer

Their terms last through 1989 until the Annual Meeting of 1990. Since that meeting will probably be postponed until June/July of 1990, their terms will probably last that long de facto.

Some thirty-two persons were elected to the Board of Directors for 1989. That is because the original slate proposed was elected and a few others had write-in votes. Some elected Directors declined to serve on the Board and several have yet to respond at all. A final list of the membership of the Board will be made in the August issue (MT8) when those who have not accepted or responded will be excised. One may assume that most of those voted for will be on the 1989 Board and that they have accepted.

The Annual Meeting was very careful to separate the 1989 Board from all subsequent boards because of the special circumstances surrounding the 1989 elections. In the By Laws adopted at the April 15th meeting it is specified that 9 (nine) Directors will be elected in the future. This will be a practical, function-oriented Board whose quorum will be 5 (five) and whose ideal Director will live within a reasonable distance from Boston so that meetings can be held without great stress or expense to the Directors.

In the future there will also be a Council of Fellows, if the membership at large approves of this notion. The Council has been conceived of as a prestigious body, one that honors scholars for their work, one that picks new Fellows again on the basis of their meritorious work, and one which will adorn the covers of MOTHER TONGUE as a list of potential editors/referees for those articles which need serious discussion before publication. It is not conceived at the moment that the Council be primarily an editorial board. Rather it is seen now as primarily our equivalent to a council of Nobel laureates. The Council will be elected by the members at large in due course, according to strict rules and by official ballots.

It is hoped that the Council of Fellows will accomplish two important things for us; one, to assign true and worthy praise to those stalwarts who have labored in this field so bereft of ordinary reward and so prone to punishment from the outside, and two, to resolve the unfortunate confusion caused by our mixing up the practical functionaries with the prestigious pioneers all together in the Board of Directors slate voted on this Spring.

You all should be thinking about some of the colleagues you want to

nominate for the Council because we will be asking for those nominations fairly soon. Given the immense amounts of lead time needed to ask our membership any question and get a decent percentage of responses, the process of electing the first Council of Fellows should consume most of this year. The first election to the Council will be the last, at least as we conceive of it now, because the elections of future Fellows will be made by the Council itself.

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DESSERT

With the heavy regular courses finished we can indulge ourselves once again in sinful desserts: chocolat mousse, Apfel Strudel, lemon meringue pie or in thinking processes considered sinful in sober serious science. For our dessert this month three colleagues offer us delicacies which they have prepared by opening up their minds, suppressing the linear reasoning of the left hemisphere, and letting the creative but wise right hemisphere take over.

It is probably not an accident that none of them are linguists, though they do share either a love for languages or some training in linguistics.

Robin Mirrlees is one of our Renaissance Men, a businessman or merchant, and our only proper nobleman. Robin did not want his contribution to be so unfettered as to be embarrassing, so I have edited it a bit.

Anne Beaman has done ethnographic field work in, or has spent significant amounts of time in, Kenya, Ethiopia, and Nigeria. She holds the doctorate from Boston University, with her dissertation on the Rendile (not Rendille) of north Kenya, a Somaloid people, camel nomads with an age-grading republic, "pagan" or pre-Islamic religion and other intriguing attributes.

Mary Ellen Lepionka has done ethnographic field work in, or has spent significant amounts of time in, Botswana and Saudi Arabia. While primarily a cultural anthropologist, she has been strongly influenced by biological anthropology and archeology on the one hand and social science theory, especially sociology, on the other. Despite some striking similarities to their work, she has been innocent of influence from the major thinkers of the Language Origins Society (LOS), knowing nothing of them.

Readers should be offered some interpretation of what is going on here. First, the terms share an attitude. "Brain storming" in American English now means a conversation whose purpose is stimulation of ideas and hypotheses. Normally done in a group scene (e.g., workers on a project) it is intended to free co-workers from the normal and expected criticisms of fresh and undisciplined thinking so that the 'creative juices can flow' or shy people with new ideas can be heard.

"Free wheeling" is another term ultimately from the technology of transportation. It means the wheels are free -- of the gears which drive but also govern them. In my youth at least people all drove cars with "stick shifts" (gear shift, gear shifting lever). If one did not change or shift gears, one was stuck in some gear for the duration of the trip. (There was no automatic transmission) On hills or mountains one could disengage the gears altogether, or "put it into neutral" as we said, and then one could FLY down the hill, or COAST down the hill, unrestrained by the gears. It was very

thrilling and often quite dangerous. But in time "free wheeling" became attached to personality traits and style, particularly in business management, but could be applied to anything where one "put it into neutral" and tried to see how much fun one could have or how free one could be. Of course, as everyone knows, you cannot drive up hill in neutral!

"Unbridled speculation" is doubly sinful. "Speculation" or basically to SEE things in one's head or to imagine things has good hoary roots but in some sciences has come to be a term of opprobrium for those who don't use proper methods. "Unbridled" originates in horsemanship where one can remove the bridle or governor, or loosen it enough, so that the horse can be free of the restraints imposed by the governor (rider). An unbridled horse can run away to join the mustangs or just have a pleasant afternoon cavorting around a pasture. A bridled horse is a disciplined horse. So unbridled speculation is perhaps undisciplined imagination or simply drunkenness. Yet it also can be milder in the sense that the discipline has been set aside for a while so that the mind can use all its resources freely to solve a problem or imagine a situation.

We need to take our methods and disciplines quite seriously. But we need just as much to unleash the power of our imaginations. Some of us are more myopic or "professional" than we need to be. It can stifle our inquiry. How will we ever generate hypotheses/theories to account for the central problem -- how a bright and pedestrian primate acquired this incredible thing called human language -- if the eyes of our minds are too close to the ground? Methods which are misapplied can be self-crippling methods. For those worried about being "unscientific" let me urge them to spend some time with astronomers and physicists. They could be terrified at the boldness and brain storming resident in the so-called "hard sciences" or natural sciences. Spend a few days reading Stephen Hawking, or Charles Darwin for that matter. It would seem that there are times to be very careful and exact (in the lab) and times to use long precise mathematical calculations (testing hypotheses & deriving consequences) and times for inventing concepts and hypotheses. A good mature science has harnessed the power of unbridled speculation and to its own advantage!

COUNT ROBIN DE LALANNE MIRRLEES.

This has been permitted by Robin with the added fillip that he hopes not to be embarrassed in front of the experts because his letter to me was not written with an eye to publication. Part of the basis of his report is taken from a letter he wrote to Philip Lieberman and sent me a copy of.

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What started me off on this marvelous subject (though I have been interested in languages all my life) was to speculate whether all languages go back to ONE original "language", and that it would be possible for some courageous person to reconstruct it. The way I would set about it...would be to ask the question "What would be the minimum vocabulary which a primitive person would need, to communicate with his tribe and stay alive?" The list of words I invented ... was amazingly small, about one or two hundred grunts and growls, ouches, helps, give me's, watch outs, and whistles.

Surely bearing in mind the sound shifts, Grimms Law etc. and by comparing early forms of the present language systems, child talk, etc., we could hazard a brave guess at the probable hundred words in the dawn of

humankind? What a wonderful thing to attempt to do!!! People have reconstructed early Aryan (not difficult in my view) so have they not succeeded in reconstructing some of the other languages. From this base could they not reconstruct the first "language" of mankind?

[Moving on to another topic - HF] Have you ever owned a talking parrot? My parrot (still alive) was absolutely extraordinary. He could say a hundred words accurately, BUT MORE THAN this, he used these words at the CORRECT TIME AND PLACE to an AMAZING degree. Up here [western isles - HF] the sheep dogs have an amazing intelligence, and seem to understand about ten or twenty different words. They cannot speak, but they can growl or bark or whine in four or five different consistent ways, which almost amounts to the possession of 4 or 5 different "words".

You mention neural mechanisms, well, I am convinced that parrots have highly developed ones. May I make a suggestion, or contribution? ... Parrots learn speech just for the pleasure of it, because they love sounds, and love imitating sounds, but while they imitate speech they are not in the least concerned with the Darwinian survival value at the time. May I suggest that EARLY HOMO SAPIENS became extremely skilled, PARROT WISE AT IMITATING SOUNDS, just because they enjoyed doing so! Particularly the children. In this sense birds sing because they enjoy it (skylarks etc.) and then the survival value comes later (in the sense of defense of territory, getting a mate, etc.).

When my parrot imitates so brilliantly, he does not intentionally send a message. Yet unintentionally, he does so. I.e., when he imitates running water, the telephone, the wireless, etc., you cannot help immediately thinking of these things by association Pavlovian. So in a sense, my parrot is truly talking, in a kind of way.

All modern races of man love music. I was almost going to say (but you will condemn me) that the more primitive the race, the higher their skills at Rhythm and harmony etc. [That must be false because the English have low skill at Rhythm and harmony -- HF] Therefore if we go back in time, did early mankind listen curiously to the sounds around him, and imitate them, skillfully, just for enjoyment? [some associated thoughts are ignored here -- HF] Gradually this great jumble of IMITATIVE sounds, barks, songs, whistles, hisses had a survival value; a "use" for conveying firstly mere emotions, then later on, specific messages, images, and so the sounds started to be used intentionally, logically, consistently.Don't forget to give a little praise to my dear parrot Mephistopheles.

Count Robin had many other thoughts but our space is running out. Too bad.
.....

ANNE W. BEAMAN

This has been permitted by Anne on the grounds that it is speculation here and NOT what she would normally submit for publication. Tis understood. Anne lived with the Rendile a long time, spending great portions of her time with women and children because her dissertation was directed at overcoming the potential bias in ethnographic descriptions of age-grading societies where male ethnographers interview male warriors and male elders about their male-oriented age-grading system. Nobody really knew very well what happened to women or what was a female view of such a society. Now we know.

P. O. Box 583
Brookline, MA 02146
March 7, 1988

Dear Hal,

(excised)

The search for the mother-tongue seems, at this stage, almost as ephemeral and far-fetched as the search for radio communications from extraterrestrial life forms. Everything that is known points to the possibility. A sort of internal kind of logic verging on hope-and-dream points to the probability. But putting the pieces together so they really fit is harder and less immediately satisfying than one might wish. The things that turn out not to be cognate but similarities based on onomatopoeia, the enormous time-depth and its effect on culture change . . . What a task!

(excised)

In the car, that day with John Segelan, you proposed a little scenario for the sudden emergence of human language, going along with S.J. Gould's "punctuated equilibrium" in evolution. Rather than gradual development over millennia, you envisioned a woman as the first Homo sapiens, rushing home to her pre-sapient husband in alarm at a hyena's whoop, and spontaneously telling him about it: the first human conversation. No doubt, the husband just stared at her in total non-comprehension . . . or whatever.

But I have a different scenario. Not a full-grown woman, but a child, and of either sex. (Mitochondria may be traceable only matrilineally, and passed on only through females, but that does not mean that a woman's sons do not inherit ~~the mitochondria too, as well as~~ ^{her} other characteristics. Therefore, while the first Homo sapiens to pass on her sapient heritage to us all may have been a woman, that does not mean that she was the only first Homo sapiens. I mean, she might have had a brother or two, too!

So I see a baby, age about 1 or 1½, lying on its back in the dirt in front of the windbreak of stones, playing with its toes, and gurgling and cooing in experimental fashion — experimenting with all the sounds it hears. It imitates the tone and pitch of its parents' grunts and exclamations. It pants and clicks and shrieks and just lets its tongue play around in its mouth. And when it cries, its mother picks it up and rocks it rhythmically on her hip, crooning to it in a kind of toneless chant while the child reaches for her breast and begins to suck. Rhythm soothes not only humans, and not only babies. I have seen caged baby

monkeys rocking themselves -- rocking, rocking, rocking -- on the floors of their iron cages in a desperation of loneliness and unmet need. And you know what they say about new puppies and a ticking clock. I am certain that pre-sapient mothers rocked their babies, just as I am certain that there were wordless chants and dances to lift both children and adults into that other level of consciousness and being that, ^{result} And I am certain that Baby sapiens's forebears had, if not all the mental and anatomical equipment necessary for language, at least very considerable preliminary capabilities for using sound as symbol: both the cries and utterances other primates have, and imitative, expressive sounds.

So here's Baby sapiens, born accidentally with the right vocal and mental equipment, growing up with what his/her elders see as a very unusual knack for imitating animal sounds. S/he and the other kids in the family play together, and because most truly evolutionary changes are not so large or significant as to set the new type wholly apart from its supporting population, the rest of the kids in the band -- perhaps four or five kids of a variety of ages -- play and scamper about in the bushes while their mothers gather nuts and roots, and they call to each other, imitate each other and the animals, and play hide and seek with ~~verbal~~ vocal signals. But only Baby sapiens's own true siblings share the same amazing new range of vocal capabilities and their symbolic possibilities. And as they grow big enough to be true playmates, they discover this as a secret means of communication they can use for each other alone: not sentences, really, but really good animal imitations, or sound associations (after all, even dogs recognize "W-a-l-k"). They become very skilled at using this as a kind of secret language between them.

But the elders see it only as a childish game, and they poo-poo it as childish activity. To conform, one needs to behave like a responsible adult eventually, so the thing never develops.

Then little Miss Sapiens marries and becomes a mother herself, and there she is, rocking her own first infant at her breast, staring down in love and amazement at this new life in her arms. And she talks "baby-talk" to it, reverting unconsciously to the sound-games she had used as a child with her siblings. And the baby begins to "talk" back! Still no major revolutions. She does not sit up in amazement, eyes round with an earth-shaking realization, and call to all the members of the band. No. She just gurgles and prattles right back to the baby. Yet, as the baby grows, she finds herself just automatically using vocal signals to tell it about the world, to call it to her, to warn it from danger, to point out the birds and animals around them. (As a Rendille mother held her 18-month-old in her arms and "mooed" for him at the cows entering the community. The cows were a novelty; camels and small stock were the norm. The child mooed too, and mother and child laughed.)

This new child is a boy and his brother and he become very skilled hunters, using vocal signals to plan and carry out hunts. They have a little sister who follows the boys into the bush, then comes home and "tells" her mother what the boys have done, and what they are bringing home for meat. Still no full sentences in the polished sense, ~~but~~ only the mother truly understands -- as mother always seem to understand their little children. No one thinks it at all unusual. The children, just playing, develop rather elaborate symbolic meanings in their child-language. They have a sound that means "run-like-an-antelope," and a name for most animals, and they can put these sounds together for fun -- perhaps to taunt a playmate in secret collusion:

They refer to the clumsy one as "runs-like-pig," then just "pig," then howl with laughter and roll in the dust, holding their sides. Or they sneak up on an adult, imitate a dangerous animal's sound, then laugh together about it later, describing the reaction in simple verbal symbols. Or they do as their elders have always done -- call to each other across a patch of bush to verify whereabouts, but then begin to use the calls symbolically, and then to send secret messages that way.

But within a generation or two, there is no secret any more. Just as every dog and cat knows the meaning of the sound of a can-opener, soon all the community knows what the sly few are transmitting, and the code is not only broken, but rapidly adopted. And then, like wildfire, it catches on and spreads.

The sentences come when single-utterance labels are fairly well understood, and then one day a child strings a few of these words together for his/her mother to describe not just a collection of nouns, but an event, and the mother understands exactly what her child meant.

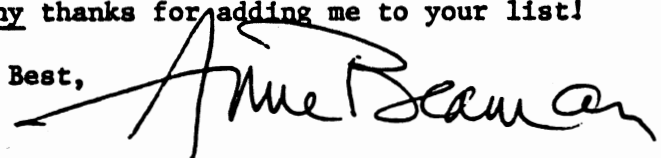
What I see is the language acquisition of a modern child mirroring the language acquisition of the first language-using sapiens. Phylogeny being replicated in ontogeny? Only I see the process as taking a number of generations, since a lot of what a child knows about language use is learned directly from his parents and others -- and before it would catch on enough to be passed on, it would have to be established as a capability in the first such child, then practiced as a childish game (since adults then "didn't do that"), then passed on as capability again, then gradually adopted as a game and a tool by adults as well -- perhaps adults who had used it as children.

But before language, I see dances and chants, I see imitations of animal sounds, I see hunting signals, I see elaborate calls for things like danger, anger, etc. And I see children, and then adults, amusing themselves and expressing themselves with the sounds they can make with their mouths as this capability increased. Every human child plays with sound, and in classes on language acquisition, you yourself pointed out how a child may replicate all the sounds humans everywhere are capable of, before beginning to select and adopt the phonemes of his/her parents' language. Why not the first Baby sapiens, then, lying in the dirt in front of the windbreak, playing with its toes, waving at the flies, and gurgling and cooing and playing little sound-games with its tongue, there in the shade of an acacia tree?

And just as with learning a language today, comprehension would come before ability to use a language. Therefore, those who could and did use verbal sounds symbolically would be fairly quickly understood by those who could not imitate or initiate such utterances. Comprehension exists in the apes now being taught sign-language. The work with Koko is mind-boggling, when you think of it, even with all the unanswered questions. These apes react with fear at the symbol of a danger -- even when the actual danger is not present.

Well -- other things to do, so I'll stop musing. This, by the way, is not intended for publication in your newsletter. It's just a series of musings on this whole subject. Many thanks for adding me to your list!

Best,



Dear Hal,

April 15, 1989

You asked, "How did people get language?" I have some suggestions based on a synthesis of old and new ideas from many different fields of study. I think language had to have begun as a complex potentiality. By that I mean the neurophysiological capacity for language and the vocal apparatus for speech. Fossil evidence and primate studies suggest that brain development for language preceeded the physical capacity for speech, and that the brain was part of a system of mutually reenforcing traits that changed together as a long-term evolutionary trend.

This potentiality had to have been characteristic of a population rather than an individual. Very likely all or most of the people presently classified as *Homo sapiens sapiens* had it, maybe beginning with the ancestral population (and the "mitochondrial mothers") before the African diaspora. I doubt very much that language originated as an individual genetic mutation. (Cf. physical anthropology, primatology, comparative anatomy and physiology, and population genetics.)

I think people had this potentiality for a long time before they used language. The neurophysiological capacity for language would have included concepts (i.e., repeatedly stimulated neural pathways acquired from experience) and automatic transformational mechanisms. The existence of a universal human grammar seems certain, as does the existence of a pan-human psychology. I think people had concepts, grammar, self-conscious thought, and "linguistic" systems of communication before they made languages. I.e., the egg came first. (Cf. neurophysiology, primate behavior studies, cognitive studies, structural linguistics, psychology, structural anthropology, and epistemology.)

This potentiality for language could not have been dormant or latent. It had to have been expressed spontaneously in the vocal experimentations of infants. Shared patterns of vocalization would have been routinely reinvented as adults responded to babies' babble. It seems likely that groups channeled expressions of potentiality into consensual vocalizations, including what might be referred to as singing and chanting. In this, people were probably not so much imitating nature (e.g., birds, crickets, frogs, wolves) as participating in the cacaphony of creatures to whom nature had given voices.

Singing would have been adaptive. It would have had selective value by enhancing the cohesion, cooperation, effectiveness, and morale of a group, which would have affected its chances of survival. This behavioral selection, however subtle, would have contributed to the growth of speech centers and interpretive centers in the brain. (Cf. anthropology, sociology, social psychology, human behavior and development, and human population genetics.)

So language was a precondition for languages, which I believe were invented. Possibly a "mother tongue" was the invention of an ancestral sapiens population. However, given the longstanding, geographically dispersed potentiality, it is more likely that limited independent invention took place. The intentional use of language as a means of communication was probably not the exclusive achievement of one group. It would be

far less surprising if it turned out that people fashioned language as an extension of the technology and skills for staying alive. In fact it might be useful to think of the origins of language as a technological revolution.

Like any other great technological breakthrough, language would have immediately become indispensable. It would have given its speakers vast technological superiority over those who lacked it. Languages and the idea of language would therefore have spread rapidly by cultural diffusion from the original center(s) of stimulus to all other groups that shared the same potentiality. Depending on the number and geographic distribution of centers of stimulus, it is conceivable that everybody who could get language within a very few generations. Contemporaneous groups that lacked the potentiality would just as fast have disappeared, and that might help account for neanderthalensis. (Cf. physical anthropology, archaeology, cultural anthropology, culture history, historical linguistics.)

What would it take for groups with the potentiality and precedents for a language to start generating it? I can think of two practical conditions that, taken together, would have amounted to probable cause: "critical mass" in population size and "critical mass" in the amount of information to be communicated. First, suppose there is a population size beyond which pre-linguistic means of communication become inadequate for a group's survival or success. Maybe group size exceeded an optimum for efficient or effective communication by traditional, learned means (i.e., calls, signals, gestures, songs, interpersonal contact, and imitation). Second, suppose there is an amount of cognitively salient information beyond which pre-linguistic means of differentiation will no longer work. Maybe there was more "necessary" information for the survival or success of a group than could be communicated by traditional, learned means. These two hypotheses have the benefit of being testable by experiment. (Cf. cultural anthropology, logic, physics, studies in human interaction and communication, and ethnoscience.)

What conditions might have given rise to increases in both population and cognitively salient information? The most likely causes would have related to environmental changes leading to changes in economic activity. The first intentional use of language as a means of communication in a group probably had to do with food-getting. Maybe the greater proliferation of plant and animal species during the first or second interglacial led to increased food supply and greater diversification of economic activity, which in turn supported larger groups and favored the invention of lexicons. If traditional ways of identifying food sources were insufficient (because there were many different local species or varieties of each and it mattered which one you were after), people might have resorted to naming them. It seems possible, but not necessary, that taxonomies preceded languages per se. (Cf. historical geology, paleontology, archaeology, paleo-demography, anthropology, and linguistics.)

In first inventing lexicons, people must have borrowed sounds from their songs and other consensual vocalizations, which

would have made vowels the stuff of the first language(s). Hence phonemes (as well as a proto-grammar; a shared system of concepts, meanings, and intentions; and shared patterns of social interaction--the basic content and rules for making and using language) were already in place. (Cf. linguistics, archaeology, anthropology, ethnology, and ethnomusicology.)

In naming, people probably wedded words to antecedent gestures, symbols, and images. These would have stood for each other interchangeably, and would have been expressed partly in elaborations of material culture, e.g., in decorative, symbolic, representational, and graphic art. The plants and animals that figure so prominently in prehistoric art surely had names. Maybe people secondarily gave names to themselves and their own doings and ultimately to all their social institutions by association with the appearance, characteristics, and behavior of plants and animals. Certainly plant and animal symbolism was embedded in the basic social institutions of upper paleolithic, early historic, and recent "primitive" cultures (e.g., in kinship, religion, voluntary associations, etc.) (Cf. anthropology, archaeology, aesthetics, ethnography, ethnohistory, and ethnosciences, and metaphysics.)

I think that individuals and small groups had to have played a significant role in the development of specific languages. Somebody had to have suggested "animal" for "that food source," "antelope" for "that animal," and "ibex" for "that antelope." (Or are we more likely to classify experience inductively from the specific to the broad?) In any event, through individual and group agency, vocabularies would have initially mushroomed in size. I think the first specific grammars were formalizations of conventions for organizing food-getting and other survival needs, including psychological needs. It seems likely, for example, that the earliest grammars contained narrative devices for storytelling.

Specific sounds, words, structures, and meanings would have become elaborated, regularized, changed, or lost by virtue of the same agencies of change that operate in languages today. The development of languages would have profoundly affected human relations. Linked with roles and statuses within a society, language would have had the potential of changing people's social structure and social organization. There could be men's and women's languages. There could be secret languages, sacred languages, and class dialects. (Cf. social psychology, ethics, ethnography, sociology, social anthropology, and linguistics.)

Please understand that in postulating a "grand theory" of the origins of language, I have intentionally ignored the boundaries between branches of knowledge--for example, including even ideas from physics and philosophy. I have also omitted references to specific evidence and sources in the fields of study I have cited, which in many cases I am not qualified to make. I'd be interested to learn if your readers find any merit in such an eclectic approach. I leave it to them to plug in the proper acknowledgements and any data supporting or refuting any of the ideas I've presented.

Sincerely, Mary Ellen Lepionka

