MOTHER TONGUE

NEWSLETTER OF THE LONG RANGE COMPARISON CLUB

JANUARY 1989

MOTHER TONGUE SIX JANUARY, 1989

Newsletter of the Long Range Comparison Club.
Soon to be legally incorporated and re-named as ASLIP :

ASSOCIATION for the STUDY OF LANGUAGE IN PREHISTORY

Address correspondance or write checks to MOTHER TONGUE
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USA

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MENU for the COMPUTER ISSUE

COMPUTERS AND DIACHRONIC LINGUISTICS

J. Joseph Pia
September 1988

The question is, can computers make our work go more efficiently and effectively? This question is really several questions. For example,

- Can computers take the tedium out of the sorting and sifting of data that take so much of our time?
- We know that two heads are usually better than one in avoiding confusion. Can computers make it possible to engage others in conversation about our hypotheses, thereby steering us straight and keeping us out of dead ends and other troubles?
- Can computers enable us to get from hypothesis to publication more quickly than is now the case?
- Can our mutual labors move along faster, thus keeping our enthusiasm high and avoiding the discouragement that comes from spending our lives only with file cards? (Sometimes with weekends off, of course.)

The answers depend in part on what we know about computers and how they operate. For example, presumably we are talking about microcomputers or personal computers. These are the kind that sit on our desks or laps. There are two other kinds — mainframes, which are the large devices used by institutions, and minicomputers, which lie somewhere between the huge capacities of mainframes and the lesser capabilities of micros. As time goes on, though, the separation of powers between minis and micros grows more blurred as the power of micros increases while the price continues to go down.

These questions arise because the growth in readily available and affordable computing power over the last fifteen years or so is simply incredible. I am sitting, for instance, at an unenhanced Apple IIe, a vintage machine now more than five years old, that has 64Kb of memory and many other capabilities. That's more memory and other things than the University of Michigan's mainframe had when I was a graduate student there in the late fifties. And my Apple cost a miniscule portion of the several million dollars that IBM charged the University thirty years ago. So, we find ourselves faced with a growing technology, fearing that we may allow ourselves to be passed by, on the one hand, and, on the other, that we may be missing a real opportunity to do more and better work and do it faster.

I raised some of these questions with Hal Fleming in several letters. In an effort to get some discussion underway, he has sent around for comment, remark, and, in some cases, rebuttal many of the comments, observations, and suggestions he has received from me along with responses and some other original material from Stanley Cushingham. In response, Claude Boisson of Lyon (France, not Lyons, Michigan) has sent along some of

his latest thoughts on the subject. All in all, it looks to me like there is considerable interest and already substantial experience among a subset of us in the issues which center on computer usage.

However, judging from the reactions of some us, it also looks like some number, although quite competently able to do our own work, are less than secure in our understanding of "computerology" and want therefore to stand back and let others carry on in public or in print. It follows, then, that though only a few of us have responded on paper, we can be sure that there are more of us in the wings, as it were, looking on, sagely stroking our chins, and taking it all in. It is in part to those folks that all this is addressed. For those experienced in matters computerological most of what I have to say will be old hat, boring even. But to those who have come only recently to the world of hi-tech these comments are intended to be enlightening.

Untangling the Issues

In the course of correspondence a number of issues have emerged. Some of them are very closely related to each other; so, it's easy for us to get them mixed up and find ourselves as tangled in them as a kitten in a ball of yarn. Interestingly, it's hard to talk directly to the questions we started with because they ultimately turn into issues concerning the capabilities of the machines and the software that runs them. Hence, this piece is organized around several issues, but our underlying concern is with those questions we started with. We hope this approach will keep us and our thinking straight and free from entanglement.

1. Transcription.

The issue here is getting onto paper via our computer printers and monitor screens the various shapes and images we use to represent the sounds our specially trained ears have learned to hear. This takes us into issues of computer systems, software packages, and such.

2. Sorting Data

Since a major part of our work involves sorting and sifting through great piles of data, we can legitimately ask how or if computers can help us with these tasks. As readers might imagine, hardware and software issues are critical here as well.

3. Electronic Networks and Bulletin Boards.

Networks and bulletin boards are very powerful devices. Learning to use them to our advantage will take some time and effort. But they offer a lot of opportunity. A prerequisite is understanding how they "work" — not in any technical sense but from a user's perspective. Hardware and software become matters of concern here, too.

4. Why Use Computers at All?

Putting this question here may seem to beg the issue. But some things need to be said explicitly. Some of our colleagues, as we all know, have yet to take the plunge "into computers." They view the devices as distractions from the real comparative work they are doing and don't want to take the time to learn how to use the machines. Some folks are simply scared half to death of computers, fearing that (1) computers will eventually replace humans in some of these important tasks or (2) the machines are easily broken, or (3) stupid mistakes can be made publicly by the hand still ignorant and, therefore, too heavy. These folks need some reassurance about the general sturdiness of computers and some non-patronizing instruction that will let them make their own choices of how to carry on their work.

The first three matters represent substantial segments of this paper. The fourth is addressed more or less throughout, as noted above. Let us proceed now to each section in turn.

Transcription

Its Importance.

At first blush transcription appears to be not really substantive at all, but after looking at it for a while, we find that it is. The substance lies in the fact that transcription is the mechanical part of the trade that, when all is said and done, lies at the very core of our enterprise. Without good transcription we can not have decent comparative scholarship. Or synchronic scholarship either, for that matter.

Transcription Schemes and Approaches.

Power of the Typewriter.

In the days when typewriters were the pinnacle of technological advance, here in the US we used to prepare our manuscripts on a typewriter to send to an editor. That arrangement meant that we were limited to the characters on our typewriters. Consequently, we in the US learned to make do with those characters.

In Europe, However...

In Europe, of course, things were different. After the war, typewriters were rare so handwritten MSS were the norm. They may even still be acceptable. I remember being surprised when given a look at a MS about to be sent off to find it handwritten and in beautiful penmanship. Scholars also used the IPA for transcription purposes. Those who were able to type their MSS also drew in IPA symbols by hand if they needed to.

There simply wasn't the motivation in Europe to move to a set of symbols that could be reproduced on the typewriter. In contrast, neither US scholars nor editors could be bothered with handwritten MSS or with putting in squiggles by hand. So we here in parochial North America typed tš instead of tʃ, etc.

One might argue also that the availability of characters on typewriters was the motivation for developing things like č for tʃ. The rationale was fairly sophisticated — two phonetic elements made a single phoneme in the minds of speakers of some languages. The ultimate reason, however, might well have been the pool of characters available. Even when Camwil created its special phonetic typing element for the IBM Selectric, North Americans continued to use what they were accustomed to. Some phoneticians, particularly those trained in Europe, used the IPA, but they are few in number.

Computers and the IPA.

Suddenly, however, the world is different. Computers and fancy software packages allow us now to use almost any symbol we can imagine. The secret is what are known as "user-defined" characters, the capacity of a software package to permit the development of unusual symbols and assign them to keys on the keyboard. As a result, IPA symbols can be produced by a user or purchased in a software package. Consequently, the IPA is enjoying growing popularity in the US. People who were staunch champions of č now tout tʃ, and so on.

Emic and Etic.

Kenneth Pike's terms refer to the two levels of phonological representation, both of which must somehow be represented in our MSS. Another, and probably parallel, constraint was the need felt by most of us to make our phonological, as distinct from phonetic, transcriptions as easy for the typesetter as possible. That constraint meant keeping the number of atypical symbols down and making them not so different from the "normal" set of Latin

characters. The Somalis went this same route when they settled on the Roman writing system. For example, they write c instead of ʕ; x for ħ; etc.

The Upshot

The ultimate result of all this is that now we can use computers, word processing software, with "user-defined" character capacity, and dot matrix printers to produce whatever we want in the way of unusual symbols. Consequently, the IPA is now available everywhere, even to us here in the colonies.

Several Kinds of Computers

The world has seen three kinds of computers achieve the status of industry standards: Apple, Apple Macintosh, and MS-DOS machines, often known as IBMs and/or compatibles and/or clones. A fourth line, the Amiga by Commodore, has great power and speed but not yet the reputation of the others.

Each kind of computer uses what is called an Operating System, sometimes known as a Disk Operating System or DOS (rhymes with English 'boss'). The Operating System is a software program that governs the operation of the machine in general and determines how data are saved to and read from diskettes. Operating Systems include all kinds of features, some more useful to one kind of user than another. Apple uses what is called Pro-DOS; it used to use DOS 3.3 (my machine still does). Macs and Amigas use proprietary DOSes that are not widely available to anyone outside the development world. IBM, in contrast, contracted with a company called MicroSoft to develop the Operating System which comes with the IBM machines. Hence the name MS-DOS (MS for MicroSoft). When it comes in IBM's own blue package, the system is known as PC-DOS. IBM did not contractually tie MicroSoft to distribute its MS-DOS exclusively to IBM. Consequently, it is quite widely available in its several versions to other manufacturers. Using MS-DOS is what makes a computer compatible with IBM equipment.

Each Operating System and computer has its own limitations and strengths. Cushingham reports in some detail on MS-DOS machines; Boisson talks about Macintoshes; and I can speak to some extent about Apples. The which I now propose to do.

Apple Computers

General Information

The Apple II "family" of computers is now represented by three machines. The IIe replaced the II+. Then came the IIc and most recently the IIgs.

Some people who use MS-DOS machines look down their noses at users of Apples. They are convinced that Apple computers are "toys" and not suitable for serious work. They are welcome to their attitudes. But many of us use both kinds of machines and are unaware of limitations posed by the "toy"-like character of the Apple II machines. Too little software of a power that is now standard for MS-DOS machines is available for Apple II machines. But that situation obtains because developers have decided pretty much that owners of Apple II machines are unwilling to pay the relatively high prices the developers want to charge for their new software packages. Apple continues, however, to be able to sell IIe machines faster than they can be manufactured. In addition the market is big enough to justify developers and manufacturers in creating enhancement products for Apple II machines. These days it is possible to have as much memory and as many fancy peripherals, like hard drives, in an Apple II as in a high powered IBM AT, an upscale Compaq, or whatever. With those possibilities in addition to the other factors of cost and relative ease of use at least a few people recognize a good thing when they see it On the other hand, if one is a developer of software or a very serious

hobbyist, one will find Apple II machines less challenging than the run-of-the-mill MS-DOS machine.

Word Processing

The term 'word processing' was invented by some IBM marketing person. It's an abomination, but what can we do? Word processing software packages come with a variety of capacities. The simplest ones include only an 'editor.'

An editor allows a user to insert and delete text and to move blocks of text around. Some also have a 'global' search-and-replace feature that permits changing occurrences of one string to another. So, for example, if a user spelled it 'Indo-European' sometimes but Indoeuropean other times, the search-and-replace feature permits a user to change all occurrences of the one to the other or to another string altogether.

More "fully featured" word processors include such capacities as user-defined characters, multiple typefaces, and print commands. Packages such as these are more expensive than the simpler ones.

Another feature of many word processing programs is WYSIWYG, pronounced [wiziwig] (stress is on the first i). This acronym means What You See Is What You Get. That is, the final shape of the document appears on the screen. This allows one to format information just the way one likes it. But, if the particular format one wants is not available in that package, then it is not available to the user.

Another factor comes into play here. By accident of history most software development has taken place in the US, which is notoriously parochial. Further, software developers are interested in making products for the largest possible market. When an academic explains to a salesperson that s/he wants to produce copy in a foreign language, write chemical formulae, or print equations, the salesperson often wrinkles his or her forehead and asks, "Why would you want to do something like that?" The most straightforward letters or reports, sometimes including tables, are within the purview of the software sales person. But almost anything else is beyond the pale. The reason is that we academics are perceived as too small a market. We aren't, but we are perceived that way. We also tend to avoid spending lots of money because we don't have it to spend. Consequently, the number of software packages available to scholars is very small.

Gutenberg: Word Processing on the IIe.

I use the software package called *Gutenberg* to do all my word processing. I use it, instead of one of the zillion other packages available, because it does what I want to do. And that, friends, is the only reason for choosing any kind of software.

Lest anyone think that I am pushing *Gutenberg* for personal gain, let me assure everyone that though I wish it were so, it isn't. It's just that *Gutenberg* is available, and it does what we want it to do. If another package were to come along to do the same things (1) on an MS-DOS machine or (2) less expensively or (3) more efficiently, I would plug it, too. In the meantime, *Gutenberg* is here, and it would make many scholarly lives simpler and easier. Struggling with other machines and with less powerful word processing packages seems like such a waste of scholarly effort. If we, for instance, encounter a pencil with a defective lead or a pen that won't write properly, we throw them away and grab a new one. Unfortunately, the prices we pay for computers and software preclude lightly casting aside our current equipment and software. However, the price to be paid in frustration and anguish seems often too high for what we get. Look closely, therefore, at *Gutenberg* and see what comes to mind.

Better Than WYSIWYG.

Gutenberg is not a WYSIWYG package. The screen simply fills up with lines of type. Users insert commands which determine how the text will appear on the page. So, there is some premium placed on an ability to map from commands to formatting.

That means that any document can be put into any one of several formats which come standard with the package. One time the document can be printed as this is. Another time in a single, page-wide column. Single, double, or triple, as well as some in between, spacings are all possible on different occasions. In addition we can change the font to larger type, if we will be reading the document at a lecturn, or smaller or just different for whatever reason we want.

This arrangement also means that the user can define his or her own formats. Learning how to produce new formats is not especially easy; so, I haven't done it yet. However, the capacity is there, making the *Gutenberg* package considerably more flexible than most others on the market.

Precursor to Desktop Publishing.

Desktop publishing is the name given to the process of producing camera ready copy from one's personal computer. The two major programs available are Aldus' *PageMaker* and *Ventura* which is marketed by Xerox Corporation. *PageMaker* comes in versions for both MS-DOS machines and Macintosh. If *Ventura* isn't already available for Macs, it soon will be.

These programs are the ultimate in WYSIWYG. Users can put text or whatever on any page and at anyplace on the page. But there are disadvantages, too. For example, inserting a new paragraph near the front of a document requires users to move forward everything following that new paragraph, sometimes onto new pages. So, the pagination changes and therefore the Table of Contents also. Etc.

Gutenberg is designed to produce camera ready copy, too. But it does so with a relatively inexpensive printer. Unlike the desktop publishing programs, *Gutenberg* will calculate where the page breaks are and put them in automatically. Users have to print out a whole document before they can see where the page breaks are and make any changes to avoid widows or orphans. This is a decided disadvantage, but on the other hand, the advantages are many. And in my book they override this disadvantage. And, as noted below, those other advantages handle very well the kinds of material we deal with.

Transcription with *Gutenberg*.

Gutenberg handles transcription, and it handles it well. It does so because of its ability to "download" fonts, including any user-defined characters, from a floppy disk and make the Apple Dot Matrix or the Apple Imagewriter II printers print all the unusual characters. The only limitation on the number of fonts that can appear in any given text is the number of fonts that can be stored on a single floppy. And, we hope, the good taste of the producer of the text.

Academically Oriented

Gutenberg was produced by academics for academics. The major programmer is Johann Wagner, a typesetter. But his colleagues in this endeavor have been Dr David Stermole, an anthropologist whose degree is from the U of Toronto, and Prof Al (H.A.) Gleason, Jr., he of the textbook *An Introduction to Descriptive Linguistics* which was the standard before *Syntactic Structures* changed the face of the linguistic universe.

Al did the tutorial and some of the documentation; Dave did the rest of the documentation and some of the programming; John did by far the major portion of the programming. *Gutenberg* is available from Gutenberg Software Ltd, 47

Lewiston Road, Scarborough, Ontario M1P 1X8. Canada, of course. When last I looked, the program cost US$325, but that may have changed.

That price includes a number of fonts, making it unnecessary for a user to redevelop the wheel. Not only Latin alphabets but also Cyrillic sufficient for Ukrainian, Massoretic Hebrew characters, and Greek are a standard part of the package. It includes also for each Latin alphabet font a bold, a slant (Italic), a light weight, and a regular face. An additional fee brings a disk of more Latin alphabet fonts, each in its several varieties — bold, slant, etc.

Overall Expense.

So, for the cost of the Apple IIe, two disk drives, and a printer together with the cost of *Gutenberg* I can do whatever I need to. Without worrying about graphics cards, kinds of monitors, and limitations on the number of fonts available at any one time. Etc.

Of course, printers wear out, and the one printing this document has been doing yeoman service every day for five years. Its age shows here and there. And, without going to an enormous amount of trouble, I can't hook my machine up to a laser printer. But that's okay by me for the forseeable future since my dot matrix printer gives me much more flexibility than a laser printer can give.

Upgrades.

Wagner has produced a couple of revisions of the program. The most recent revision needs 1 Megabyte of memory and works best with a 20 Meg hard disk. I don't have either with my IIe, and so I haven't gone on to the new version yet. It sits quietly on the shelf waiting for me to get a IIgs with the requisite additional periphernalia.

In the meantime, Gleason and Stermole, no longer involved with Wagner's company, have gone ahead on their own with another program which does *Gutenberg* one better. This new program, and I don't know its name (if it even has one yet), has been written in the programming language called C; so, it will work on any MS-DOS machine and also on the Amiga. A note to Gleason will probably get the information: 144 Cummer Ave., Willowdale, Ontario M2M 2E4, Canada. (When I talked with him on the phone last week, on 22 Sep 88, he let me know that he, his son, Henry, and Dave were still at a standstill on developing his new program. The hang-up is a new job for Henry that is keeping him from getting to the print driver part of the program. So, stay tuned.)

Interpreting Transcription

Even though we are now able to produce almost any symbol we want, we are still faced with the problem that plagues our students: What do the bloody squiggles mean? No amount of fancy character work will get us by this problem.

Our collective experience suggests that we can seldom simply look at a transcription and be sure we know what all the characters mean. We have to ask the producer of the transcription. And, if he or she has written well, there will be a table or legend that will tell us exactly what the transcriber means by this or that symbol. For example, in certain transcriptions of southern Bantu languages the symbols ty and dy are used for what would appear elsewhere as tʃ and dʒ. The choice of ty is made to represent the connections in a pattern of palatization that runs throughout the languages in question. Okay. But, unless we are told of the equivalence of "local" ty and dy with symbols used elsewhere for sounds which are phonetically similar, we would never have any idea what we are seeing.

The exhortations we always give our students are (1) to define our symbols very

carefully and (2) to be consistent in their use. We have to follow our own instructions or all our hard work in defining characters that will print is for naught.

Sorting Data

Another facet of our comparative work is lining up lists of forms so that we can see differences and similarities. For this kind of labor we really need a different kind of software package, namely a database management system.

A DBMS, as they are affectionally called in the trade, is essentially an electronic file. In each disk file material can be stored in what are often called records. Each record has some number of fields or categories defined by the user. Developing a file and defining the fields of each record can be time consuming. Then, after that work is done, all the data have to be entered into those records. Few jobs are more tedious and therefore so prone to error. However, after some data are pounded into the records, all the records are available for sorting according to whatever criteria the user wishes — given the limitations of the package. So, we can ask things like, 'Give me all the forms from Lgs X and Y with o and e.' And the program will dutifully put all the stuff on the screen.

Two kinds of DBMSs are available. One is called several things, usually hierarchical. An example of a hierarchical database is a telephone directory. All the data are there, and they can be sorted. Electronic databases can be sorted electronically; otherwise it's by hand.

The second kind is known as relational. None exist for Apple II machines. Relational database packages are far more flexible, usually allowing for the creation of new files from old and for moving or copying data from one file to another. This capacity means that we can ask the program to create a file with all the forms exhibiting some sound or other from several other files. For example, give me all the forms with the so-called uvular stop, q, in Arabic, Hebrew, Akkadian, etc. If the forms for each language are in separate files, the program will create a new file with all the requested forms in it. That new file can be manipulated just like any other.

Each DBMS has varying "reporting" capacities, too. Reporting means printing out a list. Usually information can be printed out as labels or in tables. In some very powerful database management systems the data can be called up and placed in other documents, like letters and such.

The DBMS I use is *Revelation*. It is quite expensive, but very powerful. "Industrial strength" is what one reviewer called it. It works only on an MS-DOS machine with a minimum of 640 Kb of memory and a hard disk of at least 20 Mgb. I use it for a number of things, though I haven't yet had the gumption to set up an "account" for comparative linguistic work.

Now the sad part. DBMSs have been created for businesses. Lists of customers, records of performance by individual sales people, customers' payment records, and such are the kinds of things DBMSs are used for. You won't see in a list of applications anything about linguists using the systems for comparative work. The fact is that DBMSs are limited to the rather standard characters found on any keyboard. And there is no perceived need on the part of the developers of DBMSs to add any capacity for anything else.

We find ourselves, then, right back where we started. If we are going to store data in DBMSs, then we are back to $ for ʃ and such. Obviously what we need is a DBMS that is both very powerful and includes the ability to produce and print user defined characters. Anybody out there interested in doing that for the trade? The market may be small, but it should be loyal.

One's reward will be the virtue of devotion to duty rather than riches. In the meantime we can use our word processors to make lists, and we can manipulate the data "by hand." Tedious, but better that dealing with thousands of tiny slips of paper that fall on the office floor, get caught in vacuum cleaner nozzles, or fly all over when we sneeze.

Electronic Networks and Bulletin Boards

The question addressed by this topic is, Can we be in touch with each other as we work through our hypotheses, and can we do it quickly? This becomes an issue for those whose work is proceeding so rapidly that the postal system is a frustration rather than a help. Some of us by-pass the mails and resort to the phone. But even the phone fails because we cannot look at data in the course of our conversation. Conceivably we could prepare a page containing the relevant forms and send it to our colleagues overnight by one of the express services — and pay a hefty price to do so. Even if we were willing and able to pay the price, preparing a page of data can take a long time, and then the phone conversation must be limited pretty much to what is on the page unless we are willing to spell forms verbally to each other.

A corrollary question is the one I first raised with Hal: Can we distribute *Mother Tongue* more cheaply by doing so electronically instead of by the mails? My thought is to reduce printing and postage costs for as many folks as can get to an electronic network. We would need to print only those, then, that go overseas, especially to Eastern Europe, including the Soviet Union. Whether using an electronic net is in fact less expensive is not clear, but we need to look into it.

The issue of carrying on discussion via an electronic network raised in the minds of some people great concern about the privacy of data. Such concern is really irrelevant to the issue. One reason is that no one would be forced to carry on discussion electronically. The issue is not must we but can we. Another reason the matter is irrelevant is that we can "talk" privately even over an electronic network if privacy is what we want.

We begin with a look at how electronic networks seem to work from a user's perspective. Then we look at "information utilities" and electronic bulletin boards. We bring this section to a close by examining how we might use these electronic marvels to our own advantage.

Computer to Computer Talk.

Computers can be made to talk each other. We mean, of course, that users can type at their own keyboards and have those characters appear on the screen of the receiving user. The equipment and software needed depend on the situation. Described here is a situation in which the computers in question are at some physical distance from each other — perhaps as far as in another country — but can be linked by the telephone system.

In order to communicate in this circumstance computer users need in addition to their computers and a telephone line (1) a modem and (2) communications software.

A **modem** (from MOdulator-DEModulator) is a device which takes the digital signals a computer produces and modulates them to the analog signals which the phone system uses. When receiving signals from the phone system, a modem demodulates the signals from analog to digital and then sends them to the computer's innards.

Many modems are now on the market. Finding one that works well with one's own computer is not always easy, but it is possible. A knowledgeable and good sales person as well as colleagues willing to give advice can be very helpful. Further, modems

are still relatively expensive, though the price has come down considerably in the last couple of years.

Using a modem requires communications software, a special package that has its own price. *Gutenberg* has its own internal communications module that allows Gutenberg users to swap data in forms, i.e., use of special characters, common to both Gutenberg users.

Commercially available communications software packages enable a user to dial a telephone number from the keyboard or from a list stored by the software on a disk. They also allow a modem to answer the phone. In addition they require a user to set certain "parameters." For example, the rate at which signals are sent down the wire (the baud [bod] rate, and the "duplex" status. (Full duplex means that we see on our screens only what our interlocuter sends us, half duplex that we see our own signals echoed on our screens. Sometimes this mmeeaannss ddoouubbllee oocccccuurrrreennccceess of each character. Other times not.) Other parameters usually must be set as well, but these are best understood when dealing directly with a particular package.

With a modem, a computer user can call any telephone number in the world. However, if there is no modem at the other end, the modem will hang up after a few seconds of trying to make connections. Making a connection is called "shaking hands." If there is no handshake, the connection is severed. If there is a modem, one can begin communicating as soon as the modem says it's okay to start sending. If one dials into a modem expecting to make a voice connection, one will hear a godawful scream on the line. That scream is the modem trying to shake hands with the non-existent modem at the dialer's end. The phone is probably not out of order. But hanging up is the only solution, unless we want to try screaming back at the modem on the correct pitch. But even if we are successful, what would a modem have to say of interest to us?

Another advantage of connecting machines by modem is that data can be swapped back and forth by computers that use different Operating Systems. So, my partner in New York who has a Leading Edge machine which uses MS-DOS sends me documents and other information even though I am using an Apple IIe. (I don't have a modem yet for my MS-DOS machine.) The data come through just fine; and I can massage them here with my equipment. Similarly, when I send him stuff, it gets saved onto his disks in accordance with MS-DOS.

There exist at least two possible consequences of the possibility of linking up computers to talk to each other. One is a Local Area Network (LAN); the other is a Wide Area Network (WAN). We discuss each in turn.

Local Area Networks

In a LAN the machines are hooked together by a special cable called a null-modem cable. The software enables signals to travel over or through those cables from machine to machine.

Two major configurations are possible: Chain or Net(work) and Star. In a Chain or Net configuration everything is hooked to everything else. Data are stored in memories or on hard-disk drives at each machine, and each user has access to the data stored at other machines in the network.

In a Star configuration all the machines are hooked up to a central machine, called the Host or File Server, where information is stored. Each user has access to those data. If user A wishes to communicate with user B, the message goes from A to the central unit and out to B. Vice versa when B wants to talk to A.

Some networks customized for particular applications or companies use combinations of net and star configurations all hooked together. The people who are in charge of those combination nets keep very busy ensuring that they run smoothly, and they always have a little bit of their attention devoted to the net — even when talking to someone else. They don't just seem preoccupied; they are.

Wide Area Networks or Hooking Up to the World

The default meaning of the term "network" in most settings is that of a WAN (Wide Area Network). Computers which are at a distance from each other can talk to each other over the telephone lines that now encircle the globe. In addition companies have figured out that they can offer services to people who regularly use their modems. Some of those services are now thought of as fairly standard network functions. Here are some of them.

Network Functions.

A network makes possible the following kinds of functions.

- Direct connection between computers.

  Each user has a unique address, analogous to a phone number, where s/he can be reached.

- Electronic mail.

  Each user on the net has an electronic mailbox where messages can be left. Only the addressee can receive the message.

- Electronic bulletin board.

  Information can be left where any user can get to it. The messages may be read in any one of several orders: by date, by author/sender, or by any other criterion the network has set up.

  Electronic bulletin board software is available also to individuals. So, not all bulletin boards need be associated with or be part of a network. One publicizes the phone number where one's bulletin board may be reached; subscribers or users dial up as they wish to. In some parts of the country public libraries operate bulletin boards.

Other functions are possible, but these are the ones that may be useful to us.

Electronic Networks

Lots of companies use such WANs to keep and gather data of importance to them. Two examples show that their uses aren't so different from ours, though the kinds of data aren't like ours at all. Marriott, a hotel and motel chain, every night takes a data dump from the central computer-cash register at every one of their properties. The central Marriott office can tell anyone who needs to know just what the room occupancy rate was for last night and what the gross income was for each inn or for each management region or for the chain as a whole.

A second example is from the insurance industry. A single nationwide file of all people holding a life insurance policy with any licensed company in the US is available to all such companies. All the information about medical records, claims information, payment records and such are in that file. Should someone apply for another policy, the company simply checks out the applicant's data in the file. If someone is being devious about his or her medical or other history, the facts are in the file. The company then proceeds to decide whether to provide coverage or at what price. Obviously there are security precautions which are intended to make it impossible for non-authorized people to tap into this file.

While our data might be quite different from the business information mentioned here, we too could use an electronic network to collect and make available such things as (1) a bank of forms from languages of interest to us, (2) a list of reconstructions cited in the literature, (3) some reconstruction hypotheses that we're working on right now, (4) etc. Any one of us could dial into the data bank at any hour of the day or night to leave messages or to retrieve an essay that someone is working on about a particular set of forms. Or, not a one of us would have to dial in ever if we didn't want to.

Information "Utilities"

One change rung on the electronic network idea is the information utility. Several exist in North America: CompuServe, the Source, and Genie. Several academically oriented bibliographical utilities also exist. Dialog is one, BRS and Orbit are others. The commercial market for the utilities is quite wide, though they can also serve academics and researchers. How they work is the important point here.

CompuServe is a useful example. The company stores all kinds of information on its mainframe hosts. Only some of it is professionally useful to us. Investors, for example, can obtain stock quotes and the trading histories of almost any security registered with the Securities Exchange Commission. A newspaper clipping service is also available. The subscriber indicates what s/he wants to gather information about, and the service puts the information in the subscriber's electronic mailbox each day.

The company makes its money by selling subscriptions to people. If one is a subscriber to CompuServe, or to any of the other utilities, one receives a subscriber number and a password. In return the subscriber provides a credit card account number to which all charges may be billed. The billing is for what is called "connect time." Currently the charge for the most widely used modem speed, 300 baud, is US$6.25 per hour plus a 25 cent charge per hour for use of the telephone network.

A subscriber dials a number, usually a local one, via the modem and is quickly hooked into the CompuServe net. The screen displays prompts asking for the subscriber's number and password. Then the charges start. Navigating through the service the first few times is not particularly easy, but comfort comes quickly as one learns one's way around. Also, one learns to use reduced prompts and therefore to get to the service desired more quickly and consequently more cheaply.

In order to appeal to the widest possible audience CompuServe has added all sorts of things to its services. The one most significant for us is its special interest group Forums. They are in addition to all the other services available. Subscribers may join Forums, usually simply by asking to do so.

A Forum is a collection of services provided mostly by the membership. The services usually include (1) data libraries — information deemed of significance to the membership at large, (2) a message area — provision for electronic mail and a bulletin board, and (3) a conference area where members may "talk" to each other. Forums will schedule meeting times when members dial up and join in. Conversations go on, sometimes more than one at a time. But individuals who want to talk to each other can separate themselves from the mob and carry on quiet discussions.

Sometimes a Forum will schedule a talk by a celebrity in the field. For example, the Apple User Group usually has Steve Wozniak, the inventor of the Apple computer, in at least once a year. Steve tells everyone what is on his mind, what his plans are, etc. And people can ask him questions. The idea is to be at a conference except that there is no one physically

present in one location. And, the price is somewhat less than what one would have to pay for a conference call. Further, if one has diagrams or other data s/he would like others to react to, those data can be displayed on the screen.

Our Use

My original thought was to ask Hal to put a copy of the newsletter in a data library in a Forum that we would together develop. We could each then "download," as they say, a copy, saving printing and postage costs charged to us all. Instead, downloading might take as much as ten minutes, probably less, and would cost each of us one sixth of $6.50. Further, we could probably get our institutions to pay for it. This arrangement would mean that Hal would have to print and mail a relatively few copies of the newsletter, mostly to people who can't or don't want to get on an electronic net.

BITNET

Correspondence with colleagues in preparation for this issue of the newsletter has revealed the existence of BITNET. BITNET does much of what CompuServe does and at no cost to the user. The real users are the institutions that put the net together and are paying the phone and other bills. But individual faculty members are given leave, as it were, to sign on and off the net as they please.

Each BITNET user has an address/phone number at which s/he can be reached and a mail box in which messages can be left. This arrangement is also suitable for the distribution of the newsletter. Hal simply signs on at his terminal at BU or at home, uploads the newsletter, and orders it distributed to the mailboxes of everyone on a list that he has previously put together and uploaded.

The disadvantages of using BITNET for people like me who by choice do not have university appointments is that I can't get on it. No capacity for obtaining a password or access code, as they are sometimes called, and no provision for paying a bill. Which we can guarantee is going to be a lot more than US$6.50 per hour.

Another disadvantage of all the services, BITNET included, is of course the one we started with, namely transcription. Funny symbols that are not mathematical will not function well in one of these services. So, we're back to using whatever a keyboard will provide and making equivalence lists for interpreting the oddball symbols.

No doubt there are other advantages and disdvantages to BITNET. Perhaps our colleagues who are experienced in its use can enlighten us further.

Finally

These are my comments and observations on the issues of transcription, the sorting of data, and some possible uses for us of electronic networks and bulletin boards. No doubt the rest of us are like me in eagerly awaiting word from our colleagues who also have informative things to say about these issues and opportunities.

A committee was asked to react to long thoughtful letters by Pia and Cushingham on computers and their usefulness to us. Joe Pia's letter (article!) you have seen. Stanley Cushingham decided that there was too much redundance as between the two contributions and so withdrew his. He has retained the option of reporting to us from time to time on interesting new developments in the always changing field of computers. This will be a benefit to us. For those interested in following these things the SSILA newsletter has a regular section on computer use.

Committee members asked to help were Black, Blench, Bomhard, Boisson, DeRose, Feinhandler, Gorman, Gragg, Loos, and Willcox. The comments of those who responded (after Pia's) follow serially below. (I lost Boisson's letter) In addition we had a bonus in Mark Kaiser's efforts which are reported below. Since some see communication as unnecessary. it is very likely that many others could have something to say too.

o o o o o

PAUL BLACK. October 10, 1988. ..."People may want to note a paper by C.M. NAIM on 'A Program for Partial Automation of Comparative Reconstruction' in ANTHROPOLOGICAL LINGUISTICS, Vol.4, no.9, pp.1-10 (1962). This computerized an algorithm proposed by Gleason in a talk in 1955 -- I think Gleason ran his "program" by hiring a room full of Indians (of India)...I extended Naim's program a bit in work at Yale in 1967; I wonder if others have picked up on it in recent years. Unfortunately, since long-range reconstruction is based on relatively few cognates, the statistically-based Gleason/Naim approach may not work well unless you can apply it in clever ways to huge masses of data.

With regard to huge masses of data, there is currently a lexicography project running at the Australian Institute of Aboriginal Studies (A.I.A.S.) in Canberra (P.O.Box 553, Canberra City, A.C.T. 2601 Australia). The goal is to get together computerized lexicons on as many Australian languages as possible. Obviously the availability of this and similar databases elsewhere in the world is potentially a great boon to long-range comparison by computer. Anyone interested should contact either Dr. Jane Simpson or Dr. David Nash via A.I.A.S. Kenneth Hale at M.I.T. should also be familiar with the project...(my regards to Pia)...Paul".

o o o o o

ALLAN BOMHARD. June 12, 1988. ..."First, the best way to get the ball rolling on Militariev's request (for a computer - HF) is to contact one of the big computer companies and find out what can be done. Most have marketing offices in or near Boston .....Also we would need to get specific instructions from our Soviet colleagues on how to proceed should we clear away the obstacles on this end. .... No doubt, arrangements would have to be made with a designated and properly approved institution.

In general, I am opposed to using either BITNET or COMPUSERVE as the primary means of disseminating the newsletter, though it could serve as a very, very useful communication tool between individual members. My main reasons for being opposed have already been clearly enunciated by Stanley Cushingham.

So, what options are there? Well, that depends to a large extent on you. I remember you saying that you did not want a formal structure -- you wanted to keep everything open and informal. At the same time, the job of running the whole show by yourself is time-consuming and expensive. Perhaps it is time to look at other options. Why not keep the newsletter informal but change its format and shorten it greatly? It is a lot easier and less costly to prepare a 4 or 5 page newsletter on, say, a quarterly basis. This would mean being very selective in what is sent out. Also, why not have

others get more deeply involved in editing, production, and mailing?

On the other hand, you may find that the time has arrived to become more formally organized. Then, why not go after (with help from other members) funding? There are a lot less worthy undertakings that have gotten money. Why not set up a formal organization with dues to help defray the costs? Now, I know you do not take kindly to such suggestions, but these options are worth considering....(hope it's helpful)...Allan."

<< Most of what Allan said was convincing and to a great extent will now be followed, especially the more formal organization. Allan and four other members of the Board of Directors, ad interim (Beaman, McCall, Hutchison, and Lepionka) clearly supported this notion when we met (at our lawyer's suggestion) recently. Most of the conclusions of substance are listed on the Menu page, t'other side of the cover. The Board also voted to discard the term "Long Rangers". We are now "members". What can I say? -- they were all Bostonians! -- HF >>

o o o o o

SHERWIN FEINHANDLER. No date. A frequent discussant of computer matters with me over the years. "No particular comment for MOTHER TONGUE 6, other than to say that I agree with Joe Pia for the most part."

o o o o o

EUGENE LOOS. July 21, 1988....."I am quite ignorant about how to get on Bitnet. As far as I know our institution (SIL -- HF) is not connected in with it yet. I do use Compuserve, but because my modem got sick and died I have been out of contact for quite a while. One strong motivation for getting back into active contact would be the availability there of something more relevant than most of their fare. Like, for example, things of linguistic interest.....Eugene."

o o o o o

MARK KAISER. Two contributions. First, after saying that, ..." of course you have my permission to publish my translations of Illich-Svitych's work (which will probably appear as an appendix to a forthcoming article or book)," he went on to say: "Re: computers. I have Illich-Svitych's Nostratic Dictionary (that is, the header words) and Bird's list of IE roots and their distribution in the daughter languages in computer databases. (The latter gives me the ability to perform queries such as 'List all IE roots with reflexes in Greek, Anatolian and Slavic.' The actual Slavic forms, or Greek forms, etc. are not available.) Yes, I am willing to share."..Mark.

Then, at the Michigan conference on the last day, Mark announced that he had been able to get and make copies of Starostin's computer program for Root Dating. I believe that he said that it was compatible with an IBM PC or similar machine. He also said that "one can get a copy of the Starostin program by sending $100 to Mark Kaiser, Department of Foreign Languages, Illinois State University, Normal, Illinois 61761-6901."

<< Oh, ho! What goodies Mark is offering to you and me! It is also the case that I almost forgot what an act of generosity it was on Sergei Starostin's part. You guys are terrific! -- HF >>

o o o o o

But it gets even better now.

Flash. A dramatic upturn in our international exchange of information and mutual help has occurred. GENE GRAGG has been active in important ways and deserves our thanks. Through his efforts the renowned Oriental Institute of the University of Chicago has seen fit to GIVE an IBM XT type computer to the Oriental Institute in Leningrad, care of Professor Diakonoff. Since Igor's generosity is well known, it is very likely that our Muscovite colleagues will get many chances to use it. The IBM is

expected to arrive in the USSR this spring or early summer.

The XT model is the one that I have lusted after for many years because it has 20MB (= 20 megabytes) or twenty million bytes of information storage capacity. << Voici, six bytes ----> abcdef >> Consider what that means to a comparativist. Let's use my own software on this. With my THOR program (= one floppy disk) I can store about 900 words of, say, Kafa on one disk and file each word according to source, name of language, general category like "core vocabulary" or grammar" or "kinship terms" or whatever, specific sub-categories like noun or verb or grammeme, other comments, and a phonetic category, e.g., C t' or it is a consonant and it is [t']. Since each word has its own "file", I can put any information I choose alongside the word. For example, along with the entry [nibbo] = "heart" goes the information that [nibbo] may have been borrowed from old South Ethiopic [*libb] but that it might be cognate to [*libb], not borrowed. Then I can ask THOR to give me all the words whose roots begin with [n] that are in cultural vocabulary (for example) and cited by the Italian linguist, Enrico Cerulli, and thought to have been borrowed from Amharic or old South Ethiopic. Or whatever else we want to search for. These 900 words and their category markers plus the formatted part of the disk occupy roughly 350 K (K = kilo = 1000) of memory or 350,000 bytes. By filing words as lists, of course, I can get many more than 900 on a floppy disk but the category retrieval power of THOR is then much reduced. In any case I can only compare what is on the one floppy disk at any one time, usually those 900 words.

But with an XT which has a HARD DISK there are 20,000 K to work with. On that I can store or file away 51,426 Kafa words or 20,000 divided by 350 (times 900). Since I have only about 3000 Kafa words in my data banks, there is room on the hard disk for 17 other languages each with 3000 words. Then we can ask for "heart" in 17 languages all at once or all the words in 17 languages which begin with [n]. Or all "body parts" which begin with [n] or [l]. Or we can file 17,333 selected IE morphemes, the same for Kartvelian and the same for AA; then ask to see all words with initial aspirates [bh-, dh-, gh-, etc.], final aspirates [bh-, dh-, gh-, etc.], medial aspirates [-bh-, -dh-, -gh-, etc.] and glottalized consonants [p', 'b, t', 'd, k', 'g, etc.] in the three phyla and see which correspond to each other. A hard disk is very useful! BRAVO again to Gene Gragg and the Oriental Institute!

.....

COMPUTER USE SURVEY and SURVEY OF LANGUAGES KNOWN.

Members were asked in earlier issues to answer some questions about their use of computers, as well as what languages they knew. Near final results are in and they show that -- the average member "couldna care less" (Scots) for the surveys. A minority did respond, however; there are surprises too. Very few respondents own Apples or Macs. Very few possess Modems or know how to use them. Despite fears of being "ripped off" in data exchanges, a few want to share electronically. Not much support at all for electronic distribution.

English, French, German or Russian for MOTHER TONGUE seems indicated by what we can read, and in that order. Sorry! they don't agree with you, Juha.... One can feel the European nationalisms throbbing in the languages questions. But ASLIP is not an aspect of Yankee imperialism or Anglo-Saxon ethnocentrism; please my friends! We just need a good medium! Esperanto ??

UNIVERSITY OF CALIFORNIA, SANTA BARBARA

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DEPARTMENT OF ANTHROPOLOGY SANTA BARBARA, CALIFORNIA 93106

April 13 1988

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

Dear Professor Fleming:

Reference your March 1988 Newsletter, I think there is an important point to be made about claims of 30,000 year-old human settlement in South America.

The point is this..Yes, there are claims, and important ones that are worth taking very seriously, in both north-east Brazil and in northern Chile. A generation ago, the artifacts, dates, and artifact associations would probably have been accepted very much more quickly than they are today. The reason is a simple one. Many of our common assumptions about artifact manufacture, the formation of archaeological deposits, and about animal bone fracture, to mention only a few, have been shown to be simplistic, and often wrong.

As research at very early sites at Olduvai Gorge and at many more recent locations elsewhere in the Old World (and in the New) has shown, we need to develop highly sophisticated understandings of how archaeological sites are formed, eliminate all possible explanations for natural fracture of stone artifacts or splitting of bones, and so on before a claim of 30,000 year-old occupation can be treated with anything but extreme caution. It is no longer just enough to find an association, and to reply on intuition to state that simple, modified stones are actually of human manufacture. We have to account for the phenomenon, remove all possible doubts as to their human origin by meticulous scientific observation and analysis. We are searching for tiny, primeval populations in a vast continent, people who left most transitory signatures in the archaeological record--just as the very first hominids did on the other side of the world.

Few very early sites anywhere, let alone in the Americas, have been subjected to this kind of searching, long-term analysis. Meadowcroft is an exemplary excavation, a site that has been dissected with meticulous care, unfortunately with some lingering doubts remaining about the precise date of its first occupation. Tom Dillehay's years-long excavations at Monte Verde in northern

Chile involve not only conventional archaeological analysis, but detailed reconstruction of the complex site formation processes and natural phenomena that acted on this creek-side environment. Both James Adovasio at Meadowcroft and Dillehay at Monte Verde have shown admirable, cautious restraint in their dissection of very early occupation. As respected scientists with a multidisciplinary perspective and a lively awareness of site formation process research, they are presenting and studying their excavated evidence with measured care. And we can hope that the recent Brazilian discoveries will be subjected to similar, careful analysis. It will be on the basis of the final reports on these sites, and others hopefully still to be found, that the precise chronology of first settlement will be based.

Some of the comments in Mother Tongue suggest that scientists are making life more difficult by raising their standards for accepting very early settlement. In a sense, they are, in a sense they are not:

- The basic criteria...sound association, unimpeachable dating, impeccable stratigraphy, and absolutely certain artifact identification...are the same as they were in the early years of this century when Holmes and Hrdlicka laid out the criteria for dating early settlement.
- What modern archaeological science has done is to develop elaborate technologies and multidisciplinary approaches to dissect archaeological sites in fine-grained detail, approaches that were non-existent even 20 years ago. This is an entirely new approach to first settlement that draws heavily on paleoanthropological research methods developed elsewhere in the world, as well as new methodologies in the Americas.

Make no mistake, this is sophisticated research with staggering potential. For example, to take only two instances, lithic experts can now identify left-handed stoneworkers 1.75 million years ago. And mammoth bone specialists are studying trampling patterns among modern elephant herds as a way of better understanding Paleo-Indian bone accumulations. This type of research is infinitely painstaking, extremely measured and slow, and often very frustrating, often yielding inconclusive results.

Given these new approaches, it will be years before we can be sure that Monte Verde was occupied by human beings 30,000 years ago, or that there were Stone Age bands in north-eastern Brazil who were artists by 17,000 BP. But when the thorough dissection of these sites is completed, everyone should be satisfied with the final result--even if it is a negative one. At least every reasonable alternative hypothesis will have been weighed.

There is a still a tendency in the literature for claims of early human settlement to be bandied around and accepted on the basis of a scatter of claimed artifacts, a radiocarbon date or two, and a preliminary description of an excavation. To report such claims in scientific journals is not only admirable, but expected. But, as Old Crow and other famous instances have shown, such claims

must be treated with great caution until site formation processes and other factors have been investigated, in months, often years, of subsequent investigation.

It is very tempting to accept preliminary claims as firmly established archaeological fact, and even easier to fall back on cries of unreasonable scholarly rigor when such claims are challenged on scientific grounds. But we must resist such temptations simply because the stakes are so high and the evidence so thin on the ground. The new, much more rigorous approaches may try our patience, but they offer tremendous hope for the future, hope that, one day, we will have a relatively firm chronology for the first human settlement of the Americas.

At the moment, most American archaeologists would accept a date of after 15,000 years ago for first settlement without a murmur. Most are very cautious about any earlier dates not because they are taking academic sides (something of which they are sometimes accused), but because they feel the evidence is still questionable. Science is a cumulative process, and the new methods of today and tomorrow may well produce substantive, and introcontrovertible, evidence for eariler settlement. When they do, I suspect all of us will be very happy. There is, after all, no THEORETICAL reason why human beings should not have been living in the New World during the Late Wisconsin glaciation. The problem is that the scientific evidence for such settlement is still elusive.

With kind regards,

Sincerely,

BRIAN M. FAGAN
Professor

Comment -- HF

THE BASIC PROBLEM : AMERIND PRE-HISTORY

We need to spend a great deal of time and MT space on the problems of prehistory in the Americas. This will be followed in the future... For now, in brief, American archeology has given us an established date of 11,000 BC for the first human beings in the New World. Granted that the date is more likely to be 12,500 BC (Meadowcroft) or 13,000 BC (Fagan) and could turn out to be as early as 30,000 BC. Still 11,000 BC is what is ACCEPTED in contemporary American archeology. And I definitely defer to that analytic discipline in this respect. Let us assume for the moment that 13,000 years is all we have for the peopling of the Americas, at least until some other date is accepted by most American archeologists.

European archeologists on the other hand are prone to disagree with their American colleagues on the dating. How many disagree is not known, of course, or even how many are well informed about Americanist matters. But one hears that some European archeologists, especially some in France, maintain that the Americans have a "Maginot line mentality" about the 11,000 BC date. At the Michigan conference (Language and Prehistory, November, 1988) a Soviet archeologist, Shnirelman, hazarded the guess that the peopling of the New World would be closer to 40,000 than to the much older dates preferred by "many" European archeologists.

JOHN BENDOR-SAMUEL wrote saying that the new and long-awaited and definitive book on Niger-Kordofanian, called Niger-Congo in the actual title, is due to come out in February -- about the time when half of our members actually get this issue. In Africanist terms this will indeed be a great moment. The rest of the network may want to pause in our/your preoccupation with the battles over Amerind and Nostratic and what is just the right methodology --and have a look at this book. Niger-Kordofanian is a rather large affair, containing 1200 languages at least, grander by far than the sum total of all the Amerind and Nostratic languages proposed by anyone. Within Niger-Congo proper there is diversity enough to persuade the most conservative person that hundreds of separate phyla exist and that several generations of dedicated scholars will be required to reduce the perplexity and complexity of it all.

Most of us who interacted with Sarah Thomason, editor of LANGUAGE, at the African linguistic meetings last year, besides being impressed by her intelligence and warmth, were startled by her assumptions about Niger-Congo and other African phyla -- namely that these phyla were fairly homogeneous and lacked great time depth. It may be that this is the standard view from Amerindistics and IE studies. It would go far towards explaining why they are not so impressed by Greenberg's African classification. One recommends the work of John Bendor-Samuel and his colleagues to those ignorant of Africa. May they realize!

Not less than eight of our members are featured in the Table of Contents below. .....We are also experimenting with small type to save space. Let our Africanists not think we are trying to belittle them! HF

Publisher = The University Press of America

THE NIGER-CONGO LANGUAGES

A Classification and Description of Africa's Largest Language Family

John Bendor-Samuel, ed. 1989.

The Niger-Congo family of languages accounts for some 1,200 languages of Africa, spread from Dakar on the westernmost tip of the continent right across to the Indian Ocean and south to the Cape of Good Hope.

This book updates J.H. Greenberg's classification of African languages (The Languages of Africa, 1963), taking into account research that has been carried out since that book was published. Each main branch of the family is described by a scholar who has specialized in that particular group of languages.

The book begins with an overview chapter outlining the classification of the whole family, touching on previous classifications and discussing the various factors which are relevant to the proposed classification. There are two other overview chapters, one on the Benue-Congo branch and the other on the Bantoid branch.

The chapter on each branch of Niger-Congo gives a brief account of earlier scholarship and then classifies and lists all the languages thought to be members of that branch, discussing the basis for setting up the appropriate sub-groupings in that branch.

Each chapter also provides a sketch of the outstanding phonological and grammatical characteristics of the languages in that branch. A bibliography and map are included.

An index of authors and languages completes the book.

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DEPARTMENT OF ANTHROPOLOGY
STANFORD UNIVERSITY, STANFORD, CALIFORNIA 94305

JOSEPH H. GREENBERG
RAY LYMAN WILBUR PROFESSOR
OF SOCIAL SCIENCE

April 26, 1988

Professor Harold Fleming
Anthropology Department
232 Bay State Road
Boston, MA 02215

Dear Hal,

This in in belated reaction to your letter of January 31 to Lyle Campbell in which you say that I didn't mention more than about 32 sources in my work on Oceania. I suppose this was based on impressions, which can be quite wrong after a lapse of time.

At any rate I looked at my article "The Indo-Pacific Hypothesis" (Current Trends in Linguistics 8). In order to save space since there was an excellent bibliography by Klieneberger, I cited items in his book by K+number. As I note on page 808 "Hence the bibliography consists entirely of items either overlooked by Klieneberger, stemming from geographical areas not covered in his bibliography or published subsequently to it." Obviously to have repeated the sources already listed in K could have added enormously to the text length and above all, the length of the bibliography. I went through the article using a word processor to avoid duplication of numbers. So far from "not being into bibliography" I used 216 sources from Klieneberger, 130 additional ones in the bibliography and five manuscript sources. The latter involved writing to SIL and getting materials on the Dutch area in photographed form from the work of Anceaux, none of which was in published sources. This latter I got through my former student George Grace.

Every language name is followed by the source or sources I used in the article. The material was assembled in 12 notebooks for vocabulary and three for grammar. Even now I think it has value since in many of the areas no subsequent work was done and I plan to have them photocopied and made available from the Stanford Library.

In regard to Africa I was following Westermann's example in his West Sudanic. Moreover my articles in their earlier form used precious journal space as I was constantly reminded by the editor Spier. I am now sorry I

didn't give sources for every language in my Amerind book but it would have added significantly to the length and cost of the book. I should add in supplement to what Merritt wrote in his letter in Mother Tongue, that the ms. of Language in the Americas was submitted in 1983. You know that University presses are slow. I spent part of 1981 and virtually all of 1982 writing it. Hence I did not usually use sources earlier than that.

Yours sincerely,

Joseph H. Greenberg

1 enclosure
cc Lyle Campbell

These are the 216 sources listed in Klieneberger, H.R. "Bibliography of Oceanic Linguistics" in Greenberg "the Indo-Pacific Hypothesis" (Current Trends in Linguistics 8.) + 130 in ~~the~~ bibliography of the article + 5 unpublished sources. (i.e. 341 sources)

Comments -- MF.

The following numbers, presumably from Klieneberger's system of numbering sources, were included in the enclosure. I am not positive that the assumption is correct, of course, but it probably is. Since Greenberg's printout was too faint to photocopy, I have reproduced the numbers immediately below. 359, 360, 421, 427, 430, 432, 434, 437, 438, 444, 451, 454, 457, 461, 462, 465, 466, 468, 470, 471, 472, 476, 478, 480, 481, 482, 484, 501, 502, 503, 504, 506, 507, 508, 509, 511, 514, 517, 520, 524, 533, 535, 540, 542, 547, 548, 549, 550, 554, 560, 562, 563, 565, 567, 568, 569, 570, 571, 572, 573, 574, 575, 576, 578, 579, 580, 581, 582, 583, 584, 585, 586, 587, 588, 595, 596, 601, 602, 603, 604, 606, 608, 611, 612, 613, 614, 615, 616, 618, 620, 621, 622, 624, 631, 636, 645, 651, 652, 653, 658, 662, 663, 664, 666, 667, 668, 669, 675, 676, 677, 678, 682, 683, 685, 686, 687, 690, 691, 692, 693, 695, 696, 698, 700, 704, 706, 708, 709, 710, 719, 721, 722, 723, 724, 729, 733, 735, 742, 743, 745, 747, 750, 751, 765, 766, 767, 769, 771, 772, 785, 786, 789, 791, 794, 795, 806, 808, 809, 810, 811, 814, 815, 822, 824, 830, 837, 839, 841, 845, 846, 847, 850, 854, 855, 858, 859, 861, 867, 885, 887, 892, 896, 899, 900, 902, 903, 906, 910, 913, 924, 925, 926, 928, 929, 930, 966, 968, 980, 981, 1007, 1009, 1010, 1011, 1032, 1035, 1036, 1037, 1057, 1063, 1069, 1070, 1096, 1132, 1162.

Polysemic!

There appear to be 214, not 216, but who is counting? In his letter Joe seems to misunderstand what I said in MT5, page 20. It was not that "I didn't mention more than about 32 sources in my work on Oceania." What was actually said was that Joe had "neglected to mention many more than 32 sources in Africa and Oceania", i.e., he didn't mention many sources that he could have mentioned. That statement still is true about Africa but it is clearly false for Oceania. For that I apologize. More importantly, I apologize for the clumsiness of my tactics. In trying to argue that -- basically -- the bibliographic question was not important, I managed to damage a scholar's reputation and in a careless manner. That really is unforgiveable!

Partially belittled -- H

The University of Michigan

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Jan. 4, 89.

Dear Colleague:

The Russians will have their Conference II * (= Linguistic Reconstruction & ~~the Ancient~~ ~~Pt~~ Prehistory of the Orient, II) at the very end of May, '89. They have no money but they can provide cheap [cheap] housing (a hotel of the USSR Ac. of Sc.) ~5 days

If you go, send them your title, & a short, or long, version of your presentation, or both. ASAP — they want it by mid-Febr. $ now

They'll invite 12 foreigners. (I'll try to go by all means).

Address your stuff to:
Dr. A. Militarëv, Inst. of Oriental Studies, USSR Academy of Sciences, Moscow.

See you there,

Yours, V. S.

* The 1st (in '84) was outstanding.

somewhat more belittled -- H

Slavic Dept. THE UNIVERSITY OF MICHIGAN Ann Arbor MI 48109

May 1, 1988.

Dear Hal:

Thank you for reproducing my letter alongside with clippings from our aborted newsletter, issue 1 (1984); the UM was "not able" to provide us with mailing funds, etc. - Our work on translation of Russian articles (for a collection Ancient Homelands and Migrations) came to an abrupt halt, despite generous donations to the fund Language and Prehistory from our colleagues T.Kaufman, T.Walsh and L.Cavalli-Sforza: there is no money left to pay our students for their translations.Here is a fact that has transformed a well-wisher into a donor (and that might influence other colleagues in the same way): a penniless lady has donated, some time ago, $ 10 to promote our work. And this donation was not just "symbolic": it enabled a translation of 1,5 pages of first-rate Russian stuff (from the materials of the 1984 conf.) into English.

It was an excellent idea to reproduce Mark's translations of I-S's Nostr. comparisons and reconstructions extracted from the published issues of his posthumous dictionary; note that some 250 more roots can be extracted from I-S's long article in Etimologija 1965 (M.'67) pp.330-73. To appriciate the whole significance of I-S's comparisons and reconstructions one should also use the comparative tables: pp.147-171 from the 1st vol. of his dictionary. As Dolgopolsky wrote in 1971, "[I-S] ... was able to reveal phonetic correspondences betw. IE, Ural.,Turkic,Mong.,Tungus, Kartv.,Afro-As. and Drav. languages, and to formulate laws of phonetic changes from Common Nostr. to its daughter-languages. Thus, a comparative-historical phonetics of Nostr. languages has been developed." The fact that recent progress allows for corrections in I-S's reconstructions (especially as far as Afro-As. l-ges are concerned) does not diminish the significance of his discovery. It still is a mighty tool of compar. linguistics, even if we should consider Nostratic not a phylum but a macrophylum consisting of Afro-As., IE-Kartv., Uralo-Drav. and Altaic sister languages, or alike. I-S's data help us in formulating global correspondences: say, Nostr. glottal stops usually correspond glottal stops in North Caucasian and Amerind (provided we deal with languages where these archaic consonants have been preserved), etc. Variants are secondary.

In connection with the above, I should say that I disagree, in many points, with my friend and colleague John Bengtson (see his letter in MT 5, pp.7-8). I'm fully convinced that chaotic comparisons made by Trombetti, Swadesh (and, on a narrower scale, by Möller, Cuny et al.- with their pseudo-"correspondences") have alienated many potential long-rangers. And today, some long-rangers use I-S's material without proper attention to the sound correspondences revealed by him - and, independently, by Dolgopolsky . Not only phonetic, but also semantic correspondences are frequently disregarded, alongside with the fact that many ancient roots are, actually, not "ordinary" roots but compounds.

I could use many examples from most recent works to demonstrate my point. Let us take one from an important paper by my friends John and Merritt (especially because this very example is used by Merritt in his report The Origin of Language: Retrospect and Prospective to be presented at a Conference on Language and Biological Evolution in Torino). I should like to underline that I quite agee with most data in the paper Global Etymologies by John and Merritt; after all, I myself proposed to publish this paper in our second collection of articles Genetic Classification of Languages ..., and, I'm sure, this will be the most-discussed paper of the collection; but I'd like to insist on the necessity not only to take into consideration the sound correspondences, etc, but also not to ignore correspondences already revealed by both I-S and Dolgopolsky).

In the 2-nd version of Global Etymologies (now widely circulated among linguists) there is an entry (No.2) BU(N)KA 'knee'.To support this etymology, many words in many languages are cited.But they represent different roots:

1)**wank'V 'bend; crooked';J. and M. cite only N-Cauc. *wV̈nk'WV 'corner, bend', but cf. Nostr. *wankV 'bend': so I-S in Ètym.1965 (M.'67,p.336) basing on Ur. *waŋka 'crooked', Drav. *vāṅk- 'bend' (intrans.) and IE *weng-,- but IE *wenk-, a variant with *k, indicates Nostr. *k' (cf. IE *t<Nostr.*t'). Important: N-Cauc. corresponds (through Sino-Cauc., or Dene-Cauc. as its mother-tongue) to Nostr. in a certain way, e.g., *w to *w; *b- to *b- etc. N-Cauc. (and Sino-Cauc.) *w does not correspond to Nostr. *b (or Amer. *b):

2)**bongV 'lump'; J. and M. cite only Ur. languages (Ostyak pongəl 'knob, protuberance' etc), but this is a part of Nostr. root *bongä 'thick; to swell' which is present in the 1-st vol. of I-S's dictionary (> Ur. *po/uŋka 'thick; a swelling'; Drav. *poṅk- 'swell';(?)Alt.*boŋV 'thick,big';IE *bhengh- 'thick, dense, solid'; Nostr. *-ä indicated by IE palatal *ǵh). We can add Austrones.: PWMP *bu(ŋ)kul 'lump, knob, protuberance', following R.Blust's suggestion.

3)**buKa 'bend,bent': Nostr. *büK'a id. (see I-S vol.1, from where J. and M. cite Turkic *bü/ök(ä)- 'bend', Mong. *böke- id., Tung. *bök(ä)- id [all from Alt. *bü/ökä- according to I-S];IE *bheug(h)- 'bend'). They correctly add Ainu he-poki-ki 'bow down' etc; Amerind (as in Hokan: Chumash si-buk 'elbow',Macro-Tucan.: Iranshe poku 'bow' - a noun, etc).Maybe IP: Halm.:Tobelo buku knee etc

4)**kujNA 'bend,bent;a joint': Tai *gun 'circuit of a river' etc; Mak *guk 'to be bent/crooked'; maybe Miao-Yao *ŋkho? 'bent',*gu? 'bent over, stooped'; *maybe also* Austrones. *-kuŋ 'bend, curve', *-kuk 'bent, crooked' which, according to Blust, should be extracted from *bə(ŋ)kuŋ 'to be arched', *bi/uŋkuk 'crooked' (cited by J. and M.). The above root **kujNA is present in Nostr. *kŭjñA (I-S, vol. 1) > Ur. *kŭjñä- (in the word for elbow bone,etc), Drav. *kūn 'hump' and IE *ǵenu/ǵneu- 'knee'. As for Amer.,cf.Yokuts *kʰuyo-tʰul id.

5) Either a separate word for 'knee' - represented only in Bantu *bóngó 'knee' and Austral. *puŋku id. (but this might be a compound 'leg-bender' or the like - to a root for 'leg' present in Nostr. *PoNV 'leg' on the basis of Drav. and Tungus languages) or a part of the above set No. 2. (reconstr-d by Dolg.)

So, not too much remains from the putative BU(N)KA 'knee'.Watch your sounds.

On a related matter: A detailed review (by Anttila and Embleton) appeared in Canad. Journ. of Ling. 1988, 33 (1), pp.79-89: of our book (or, rather,collection of translated papers) Typology, Rel-ship and Time. So, we have more and more publications on Nostr. in English (add Mark's and mine papers in General Linguistics 1987 and JIES 1985 and 1986, - I think). - As for the German review by K.H.Schmidt in Kratylos 1987, 32, pp.12-16, this is a formidable disservice to German readers: Schmidt falsifies Dolgopolsky's data from his experimental paper (first published in '64) saying that Dolg. compares 15 stable lexemes/morphemes from Nostr. languages. Wrong, Dolg. shows how the very first steps of comparisons of languages can be made which might - or might not - be related. Schmidt reproaches Dolg. who compared - in this initial, preliminary approach - IE *ḱerd- 'heart' with Kartv.: Georg. guli;Schmidt does not mention Dolg.'s prefatory note to his paper where he discards this comparison and indicates that the Nostr. reconstruction [*k'ErdV (cf. I-S vol.1) is based on IE *ḱerd- and Kartv. *k'erd- adding also Afro-As. cognates (*k'Vrd→ As for Georg. gul- (< Kartv. *gul- 'heart'), cf. Alt. *gŏl(V)'middle' etc,both from Nostr. *golHV 'heart' + maybe Af-As.:Chad.:Musgu a-gul id. and Drav.*kunṯ 'heart': see I-S vol.1, pp.231-2. - Schmidt reproaches Dolg. for his preliminary comparison of Sumer. eme 'tongue' and Georg. ena id. indicating that the Kartv. is rather *nena. He should consult I-S lists (why is this so difficult to do?) where the Nostr. reconstr. *ńan[g]V is based on this Kartv. root and IE *d/jnǵhū and Ur.*ńaŋkəm 'tongue' [cf. also Amerind]; see Ètym.'65 (M.'67),373.

Why people try to downgrade Nostratics, this mighty and elegant tool of comparative research? Schmidt's revue reminds me on Doerfer's falsifications in his infamous Lautgesetz und Zufall (see Typology ...).It is no wonder why there are no Nostraticists in W.Europe, except Karl Menges.

Good luck with the Mother, cordially, Vitaly.

Ist das recht? Colleagues in FRG -H

(4) ◇ URALO-ALTAISTIKA. Arxeologija. Ètnografija. Jazyk. [Papers on archaeology, ethnography and language] edited by E.Ubr'atova. Nauka,Novosibirsk, 1985. 36 papers all in all; 9 on linguistics. 217 p.,neither index nor summaries.

◆ V.Ivanov. O predpolagaemyx sootnošenijax meždu vostočno-nostratičeskimi i zapadno-nostratičeskimi jazykami [On Probable Correlations between East- and West-Nostratic Languages], pp. 147 - 150.

Phon. innovations in West-Nostr. languages (IE, Kartv., Afro-Asiatic): restructuring of polysyllabic stems (hence much less phonemes in each, now shortened, stem), decreasing amount of vowels, formation of sonorants as a phon. sub-system; in IE also: differentiation of back consonants (palatal versus non-palatal; labialized versus non-lab.).

In East-Nostr. (Ural., Drav. and 5 Altaic): vanishing of laryngeals in the post-vocalic position [some remained as -ɣ-,-ǧ-,-k-; a more radical innovation: disappearance of all initial "laryngeals"; q and ǧ. - V.S.] which lead to the lengthening of following vowels: same in certain IE dialects which, in this and some other respects, behaved similar to East-Nostr. languages [all this covers both "lar." h, ḫ, ʕ, x, χ, ʔ,and uvulars q,ǧ.-V.S.]

Morph. innovation in West-Nostr.: restructuring of nominal paradigms; this process was particularly active in the Western area, i.e., in Afro-Asiatic.

Lexics: many words appeared in East-Nostr. only, e.g. *ɨükV 'pierce, shove/thrust'(No. 261 in Illič-Svityč's Nostr. Dict.; cognates in Ural.,Alt. and Drav.); *nülV 'pull/tear out, strip/scratch off' (No. 329; cognates in Ur., Alt., Drav.); *duli 'fire' (No. 71; Ur., Alt., Drav.); *k'aλa 'leave' (Ur., Alt., Drav., - but cf. also Kartv. *k'el- 'leave; stay': No. 194); *k'/q'awingV 'armpit' (No 220: Ur. *kajŋa-la [Ivanov: *kaina-la], Alt. *kᵊawiŋi, Drav. *kavuńkV; considered as only tri-syllabic word in Nostr. [and therefore very doubtful; apparently an archaic compound *k'/q'awin--gVLa, with partial preservation of the second stem in Uralic - Mari koŋ-gêla etc.,- and Dravidian - Telugu kaun-gili 'embrace, bosom', Kannada kavuń-kur 'armpit', Tula kań-kuḷa, - as well as in Altaic - Tungus *xawaŋin, not *xawäŋi, Solon. shows ognin < *xoŋin. Apparently, Drav. shows the archaic vocalism of the second stem: -gula; I would suggest Nostr. *k'/q'awin--guLa, with -L- = l or ɬ. This finds corroboration in M.Ruhlen's comparison of above Drav. words with Nilo-Saharan: Kunama ukun-kula 'elbow, armpit', Nandi kul-kul 'armpit' etc. This is, actually, Greenberg's comparison; he reconstructs Nilo-Sah. *kun-kul 'armpit' < *kan-kul 'arm/shoulder'+'hole'.-V.S.

Ivanov compares IE *dh-sg-e/o- (from *dheH- 'put') with Kartv. *d-esg [rather *de-sg-; in any case, a nice IE-Kartv. isogloss, indicating, among many others, that there was a Kartv-IE dialect, contemporary with Afro-As.-- V.S.]. Ivanov considers -eH- in IE *dheH- as an archaic suffix [which is in no way possible: the root is, actually, IE *dheiHʷ-, with a "real" labiolaryngeal, cf. -w- in Luw. tuwa- 'put' etc.; it originates from Nostr. *diʕu 'put' and shows labial elements in all West-Nostr. languages: cf. Kartv. *dw- and Afro-As. *wdjʕ. - V.S.] - See review of Nikolaev below (about *Hʷ,*)

◆ G.Kornilov. Nekotorye novye aspekty podgotovki i interpretacii isxodnyx dannyx dl'a rešenija problem uralo-altaistiki [Some New Aspects of Processing and Interpretation of Basic Data for Resolving Problems of Uralo-Altaic Linguistics], pp. 172-179. [I'm not sure if the translation is correct because I don't understand the title, in the first place].

All archaic words originate from "imitatives", so one should look for these latter everywhere. K. considers Illič-Svityč's semantic reconstruction 'inside of the thorax' as underlying meaning of Nostr. *k'ErdV 'chest, heart' too abstract (see I.-S. No. 200: IE *ḱerd- 'heart' < *ḱerdh-, according to the rule; Kartv. *m-k'erd- 'chest'). K. compares Chuvash imitative-singulative kart- (about pulling a cord/string; heartbeat; pulsation); he speaks about "Uralo-Alt. parallels" of this latter [Sounds interesting; but note that I.-S.'s suggestion is now, in principle, confirmed by Afro-As.: Chadic *k'Vrd- 'chest', - see Dolgopolsky in: Typology, Relationship and Time, Ann Arbor, 1986, p.28. So we have a West-Nostr. word, - which, by the way, totally refutes Gamkrelidze's suggestion about a borrowing of Kartv. *-k'erd- < IE

(2)

Nostr. ?*mälgi 'female breast, udder' (only in Af.-As.*mlg id. +'suckle'; IE *melǵ- 'to milk'; Ur. *mälɣe 'breast/chest/thorax': I.-S. No. 291 [vol. II] is regarded as "descriptive" and compared with Chuv. mál(k)-, imitation of a suckling sound. [Doubtful: Chuvash would have b-, not m-, from Alt. *m-< Ns.*m- [What comes next is highly improbable, - e.g., "reconstructing" an underlying meaning '(mish)mash' for Chuvash kodzuv-kot, Mari kutko 'ant' (note that this word has parallels not only in Drav., but also in Nilo-Saharan, according to Greenberg and Ruhlen), etc. The author arrogantly accuses I.-S. of having committed many mistakes and proclaims existence of new field of linguistics - Imitativics (which ignores laws of phonetics and established sound correspondences, as far as I can understand). He sais that "Imitativics can supplement the Comparative Linguistics, to say the least",- I wonder what is implied?]

D.Nasilov. Nekotorye voprosy izučenija vidovremennoj sistemy v uralo-altajskix jazykax [Some Problems of Studying the Tense-Aspect System in Uralo-Altaic Languages], pp. 188-193.
These languages are highly archaic allowing us to reconstruct a state where aspects existed, but there were no tenses yet (cf. lack of a present marker; presence of past-present or present-future markers in different languages). Citations from [philosopher?] M.Axundov about an archaic stage of "atemporal perception of the world" and other exotic stages which follow. [All this is refuted by I.-S.'s reconstruction of Nostr. marker of the past tense *-di > Kartv. *di (Impf.), Drav. *-tt-/-t- (Pret.), Alt. *di (Pret. in Turkic, Mong., Tung. and Kor.) and ?IE: Germanic *-da (Pret.). I.-S. sais: "The underlying meaning of the [Nostr.] marker is apparently purely temporal" (I.-S. No. 65)]

E.Helimsky. Samodijsko-tungusskie leksičeskie sv'azi i ix ètnoistoričeskie implikacii [Samodian-Tungusic Lexical Parallels and their Ethno-Historic Implications], pp. 206-213.
Such parallels are many, they are very important, and they are of quite different age: some belong to the epoch when both Samodian and Tungusic existed as individual languages. Such archaic isoglosses are: Samod. *ńä 'man, somebody' : Tung. *niä; Sam. *kə̑jwə̑ 'side', *kə̑jwə̑tə̑ 'rib' : Tung. *xew, *xewtə; Sam. *op 'one' : Tung. *up-kat/l 'all, whole'; Sam. *jeppə̑- 'be hot' : Tung. *jeppö 'warm'; Sam. *mə̑ncV 'to look' : North Tung. *munsi-, etc.
Some isoglosses precede the shift Samod. *t < Ur. *S, *Š, for instance: Sam. *tə̑jə̑ (< *sə́jə̑ < Ur. **suša) : Tung. *saja(n) 'finger'; Sam. *tErV (< *sErV < Ur. *še/ärä) : Tung. *s/širę 'spring' (water), etc. Since the above shift took place some 3,000 years ago in Samod., the isoglosses with Samod. *t- versus Tung. *s, *š should be at least that old. [Many other solid data].

◇ [Paper, pt. 3] S.Nikolaev K ISTORIČESKOJ MORFONOLOGII DREVNEGREČESKOGO GLAGOLA [On Historical Morphonology of the Verb in Old Greek], in: Balto-slav'anskie issledovanija 1984, Nauka, M. 1986, pp. 157-208 [Pt. 1 in ...'82, M.'83].

Exceedingly important conclusion: In IE, there were bivocalic roots of the type CVCHV [rather CVCXV. -V.S.] with laryngeal > Anat. *ḫ [x] > Hitt., Luw. ḫ. [So, there were some roots structurally identical to Nostr. CVCCV; cf. M.Peters' conclusion that roots of the type HaiC-, HweC originated from HiC-, HuC-P. didn't use Nostr. but for us it is important that in such form the IE root were much closer to Nostr. roots (words) of the type CVCV than IE HaiC-, HauC-HweC-]. - Finals of some IE reconstructed as *-wə by V.Dybo might be re-interpreted as *-H^{W}V where *H^{W} "is not an abstract 'labyalized laryngeal' supposedly disappearing in most IE languages, after having changed [neighboring] *e into *o, but a real IE *H^{W} reflected as ḫw in Hitt. [and as -w- in other IE]" Same *H^{W} [i.e., *X^{W}] should be reconstructed in the anlaut of IE *H^{W}es- 'live' (> Hitt. ḫues-), *H^{W}Vl- 'wool' > Hitt. ḫulana-), *H^{W}ert- 'twirl' etc. [In some cases, we might deal with underlying XuRC-, though. - Nikolaev does not mention the possibility of reconstructing an unstable *H^{W}, different from the stable *X^{W}; the former would yield Anat. *w, the latter - Anat. *ḫw. In this way, we would have an exact counterpart to IE *H and *X discribed by M.Kaiser and myself in JIES 1985, pp. 377-408. IE *H originates from Nostr. *ʔ, *ḫ, *ʕ where as IE *X originates from Nostr. *q, *ʡ, *χ, *ɣ (and maybe *h). - Since H.Eichner's data show change of *ə̆ to *a in some roots in the neighborhood of IE *h [= our *X] we can modify our statement in JIES: Nostr. *ä/*ę/*i became IE *a in the neighborhood of IE *X (< Nostr. uvulars), cf. Hitt. paḫḫur 'fire' < IE *paXw-(V)r < Nostr. *p'iɣwe; IE *saXwel 'sun' < N. *ʒeqLU 'shine': *-e- in Ural.

[From Helsinki -- H-]

ANY CHANCES FOR LONG-RANGE COMPARISONS IN NORTH ASIA?

This is my answer to Hal Fleming, who asked me to explain briefly why I «think Ural-Altaic or Nostratic are 'not scientific'». Well, I prefer to view the question in the framework of my North Asian specialization, since I do not claim to have competence in all of the fields relevant to, in particular, Nostratic comparisons.

Indeed, I do have serious doubts concerning the validity of the long-range comparisons carried out *so far* using North Asian material. To be exact, however, I do not think the comparisons *as such* are 'not scientific', for they represent a rather logical continuation of previous work in comparative linguistics. No one can determine the limits of knowledge without raising questions and making hypotheses, and this is what the long-rangers are doing in my opinion. This is also why I think it is fruitful to continue the discussion with the long-rangers, although I cannot for the most part share their optimism.

So, I agree that long-range comparisons are an inevitable step on our way towards a more profound understanding of the diachronic situation in any given area, such as North Asia. In a not-too-remote past, some of the genetic connections today recognized beyond hesitation, for instance that between Samoyedic and Finno-Ugric, used to be at the level of tentative long-range comparisons, and it is only thanks to people who had the determination to continue the work that the present-day level of knowledge was finally reached.

It is perhaps not out of place to conclude these positive remarks by the personal observation that many of the long-rangers I happen to know, notably the Muscovite Nostratics, are, so far as I can judge, brilliant specialists in their fields, so there is every reason to take their work seriously. Nevertheless, I think the distant relationships they are so enthusiastically propagating are not real. Why?

(1) *Unconvincing comparisons*. This is both the most trivial and the most unfortunate problem with all long-rangers: their work simply fails to convince a number of critical people, including myself. The essence of the problem is that most long-rangers allow themselves a rather extravagant negligence of the established principles of comparative and diachronic linguistics. It is clear that the looser the theoretical apparatus is, the easier it is to «establish» new distant relationships. The problem is further complicated by the fact that not all long-rangers belong to those brilliant scholars referred to above. Typically, some of the boldest long-range comparisons, such as, for instance, those underlying the Uralo-Japanese or the Ob-Ugric-Penutian hypotheses, have mainly been propagated by people who can hardly qualify as serious linguists. Unfortunately, the difference between professionals and non-professionals is not always easy to determine.

(2) *Contradictory claims*. A fact which even the most fervent long-rangers cannot deny is that the conclusions drawn from long-range comparisons are often mutually contradictory. Understandably, everybody likes to defend his own conclusions, but

for an outsider the choice is not automatic. Moreover, it happens quite often that mutually contradictory claims derive from people who, by all available criteria, may be considered as equally competent, or incompetent, in their fields. Is, for instance, Uralic related to Indo-European, or is it related to Esk-Aleutic, or to some other language family? There are so many alternatives. It is as if one had to choose a religion: which prophet speaks for the true god? A critical observer has only one choice: to remain a nonbeliever, or at least an agnostic.

(3) *Omnicomparativism*. The Nostratic answer to questions of the type mentioned above is simple: Uralic is related to both Indo-European and Esk-Aleutic, and to many other language families as well. This is the famous omnicomparativist approach. The Altaic hypothesis with its extended versions, all of them normally included in the Nostratic framework, is another good example of this approach. Here we have basically three language families: Turkic, Mongolic and Tungusic. The Altaic hypothesis claims that they are mutually related. If this claim is accepted as a dogma, it is not difficult to «find» other languages that are also «Altaic», such as Korean and Japanese. After all, it is three times easier to compare a language with three language families than with one. Now, the problem is that it has already been shown that the comparative evidence once presented in favour of the genetic relationship between the three basic «Altaic» language families can only be explained in terms of a complicated network of areal interaction. What can remain of omnicomparativist fantasies based on an incorrect understanding of basic facts?

(4) *The naturalness of isolates*. It often seems that people active in making long-range comparisons view the whole world as a playground full of undetected genetic connections. However, the greatest challenge to their comparativist eager seems to be presented by genetic isolates, such as Ainu, Gilyak, or Yukagir. The algorithm seems to be: the genetic identity of any isolate can be determined, if only a sufficient amount of intuitive capacity is available. I disagree. While in some rare cases long-range comparisons may really yield important new identifications, the result is much more often negative. This belongs to the nature of the problem: long-range comparisons are inquiries into the limits of comparative knowledge, and it is only realistic not to expect too much of them. This is particularly true of isolates. There is really nothing mysterious about languages being isolates, so why not accept the situation that they stand apart from other languages?

(5) *The limited lifespan of language families*. This is another triviality probably never to be recognized by most long-rangers, but language change is really so rapid that genetic relationships inevitably fade away in a relatively short span of time. My experience from the North Asian languages makes me assume that the maximum lifespan of any language family is some 10.000 years. This brings us back to the early Holocene and the late Palaeolithic at best. In fact, the earliest proto-language that can be reconstructed in North Asia dates in my opinion to the early Neolithic: it is Proto-Uralic, about which we have some 150 lexical reconstructions and a

certain amount of structural information. By simple quantitative criteria I doubt, whether it is ever possible to go farther than that.

(6) *The recent expansion of large language families.* Uralic is an example of an expansive language family. Other expansive families in and around North Asia include Indo-European, Turkic, Mongolic, and Tungusic. In view of both linguistic and archaeological material, the expansiveness of these families seems to be due to favourable cultural and demographic conditions in the past, which allowed the corresponding proto-language speech-communities to grow sufficiently large and strong, so as to assimilate other speech communities. This was only made possible by the increasingly rapid cultural evolution of human communities since the Neolithic and through all the subsequent cultural stages. Thus, although linguistic evolution in its modern forms must be as old as human language itself, I doubt whether linguistic expansions of the modern type could have been possible before the Neolithic. This is also the explanation to the presence of isolates: they represent marginal remnants of the numerous Palaeolithic speech communities that once covered the world but remained under the recent expansions of the large language families, relatively few in number.

To answer the question of the title, I do not think there are any great chances to identify previously unknown genetic relationships in North Asia. However, it is clear that our comparative understanding of the region is still quite insufficient. It would be very welcome if some of those brilliant Nostratics, instead of working with the eternal prolems of distant relationships, started to work seriously with the concrete comparative analysis of, say, Gilyak or Yukagir dialects.

Of course, it cannot be excluded that some new concrete genetic relationship may finally emerge from the long-range comparisons in North Asia. An example of a case that I would personally not yet dismiss as hopeless is the Yeniseic-Bodic hypothesis, today included in the framework of the Sino-Caucasian comparisons of certain long-rangers. However, much remains to be done in this as well as in other fields, and the ultimate result may be a disappointment. I am afraid many long-rangers are not ready to accept a disappointing solution, and here lies one of the most obvious dangers of their approach.

The late Swedish omni-comparativist Björn Collinder used to put it this way: in long-range comparisons it is ultimately a question of temperament. His point was that some of us are more enthuasiastic about new ideas than others. I disagree again. I think that in long-range comparisons, as in any comparative work, it is a question of the presence or absence of factual evidence. Personally I am ready to believe in any distant relationship, if only the facts can convince me. So far no sufficiently convincing facts have been presented.

Juha Janhunen (Helsinki)

PLUS

PAUL FRIEDRICH. (Jan. 14, 1988). Paul has published a very useful model of adpreps or locatives as they relate to each other in a set. In addition to that there is an interesting discussion of relevent semantic and grammatical theory, culminating in a final set of proto-IE adpreps. The article is useful in three ways, besides being a typically valuable Friedrich product in its own right. First, it will help those non-native users of English who must sometimes struggle to discriminate among our prepositions, as well as helping anyone learning any language. Second, it will be useful to IE studies, since no one else seems to have pulled the adpreps together in a coherent way. Third, it should be dynamite for comparisons outside of IE, including those between IE and some other phyla. It is not reproduced here because permission has not been obtained from the publisher, although Paul himself permits it. See "The Proto-Indo-European adpreps (Spatio-temporal auxiliaries)" in George Cardona and Norman Zide, eds., FESTSCHRIFT FOR HENRY HOENIGSWALD: ON THE OCCASION OF HIS SEVENTIETH BIRTHDAY. 1987. Pp. 131-142. Gunter Narr Verlag Tu"bingen.

o o o o o

LUCA CAVALLI-SFORZA (with colleagues Alberto Piazza, Paolo Menozzi, and Joanna Mountain) has pulled together the bulk of bio-genetic data, basically serogenetic and other non-DNA polymorphisms, on the peoples of the world and have produced something close to a definitive taxonomy of our bodies seen in gross population terms. He does not agree in every detail with the mitochondrial DNA (mtDNA) studies of Rebecca Cann, Alan Wilson, and Mark Stoneking; nor with those of Douglas Wallace. But his study is intuitively more convincing because it includes scores of genes (120) and inheritance by both males and females, even though mtDNA studies have a strong logic of their own. What is then so arresting about Luca's study is that it too (like Cann, Wilson & Stoneking) posits a fundamental split between most Africans and the rest of the world. But it also segregates Ethiopians and Khoisan populations from other Africans -- and from the rest of the world too -- but puts these eastern/southern African groups TOGETHER as a taxon. (Cann, Wilson & Stoneking also segregated !Kung, for example, from other Africans) One hastens to add that the Ethiopian-Khoisaner togetherness is not very close -- being roughly equivalent in taxonomic distance to the Ainu-Eskimo relationship or Cantonese-Polynesian.

After the African vs others split, the next basic split is between "Northeurasian" and "Southeast Asian". That is estimated (by my ruler) to be circa 50,000 years old, while the first split was 92,000 years. The next oldest split is between Austric type peoples (mainland Southeast Asia, Sundaland, and the Pacific) on the one hand and New Guinea-Australia on the other; that is dated at 40,000 years. This is a relief after the usual textbook which overstresses the distinctiveness of the Polynesians, Micronesians, and Melanesians. Yet neither the Austric type people nor the (let me say) Australoids have any other affinity less than 50,000 years old.

The split between the Australians and Papuans (say 32,000) is almost as old as the primary split within "Northeurasians" -- that between Caucasoids and what we used to call northern Mongoloids, i.e., north Asians, east Asians, Arctic people, and Amerinds. It dates to 35,000 and might be construed as a date for proto-Nostratic cum proto-Amerind. However, most of what the Muscovites call Dene-Caucasic -- except for Sino-Tibetan -- falls within the same Northeurasian taxon. It ties in closely with the replacement of the Neanderthals of Europe by outsiders, us all -- Homo sapiens sapiens. Hmmm. How much of Soviet Central Asia or western Siberia was habitable around 33,000 BC ? Could our ancestors have easily walked around or through Sinkiang on their way -- so to speak -- to north China from Iran?

The correlation of Southeast Asians, Sundalanders, and Pacific islanders with the proposed Austric super-phylum is really stunning. But the more ordinary Sino-Tibetan phylum correlates poorly with anything. That is obviously because the two S-T populations included were the Tibetans and the Cantonese (South Chinese). The mistake seems to be that such S-T peoples as the Burmese, Naga, Nepalese, but most of all the North Chinese were not included. Tibetans are fairly close to Koreans and Japanese (Northeast Asians), while Cantonese are closer to Khmer, Thai, and Malays (Southeast Asians). The Gammaglobulin data have always shown a chasm between north and south China, hence uncertainty about Sino-Tibetan affinities, but Luca's conclusions do not help us figure who crossed the bridge. Chinese history and tradition does speak to this point, fairly definitely in fact, but it speaks with a forked tongue. True, Chinese historians say that the Tibetans are derived from northern pastoral nomads (Mongols?). Also true that Chinese tradition and history finds the real Chinese moving south, crossing the great river into a land of aliens (Daic? Miao-Yao?). Does anyone have a good well-chewed hypothesis about the Sino-Tibetan homeland? One that accounts for the Himalayans as well as the pastoral Tibetans?

Also exciting are his conclusions that (a) Ethiopians, Khoisaners (San or Bushmen), and Lapps show evidence of old admixture (gene flow) between more basic taxa; Ethiopians more recent and Khoisaners less recent overlap with Caucasoids; Lapps who are basically Caucasoids overlap with North Asians; (b) Australians and Papuans have their own taxon; their mutual differences are greater than Ethiopian-Khoisan ones and nearly as great as the Caucasoid vs northern Mongoloid one; (c) the gross taxonomic units correlate to some substantial degree with the venturesome linguistic taxa,,especially the super-phyla like Nostratic, Austric, and Niger-Kordofanian.

Very clearly, Luca's analyses show some of our old expectations to be true, viz., (a) that the two old tropical areas of Africa and Southeast Asia cum southwest Pacific contain the greatest diversity of biological humanity, as well as linguistic and cultural; (b) that the distinctiveness of Africans and Australian-Papuans is consistent with their statuses as earliest arrivals in one or the other area (from the other) or as the old folks at home, those who remained in the original human homeland; (c) that Europeans are ultimately more relatable to north Asians and Amerinds than to the tropical peoples. Yet the unexpected is that northern Eurasia also has great bio-genetic diversity; this is also true linguistically.

In addition some new implications arise. For example, South America has much linguistic and cultural diversity -- anthropologists have always treated it as a major sampling region for cross-cultural studies. Yet its biological sameness suggests that the linguistic diversity is relatively superficial, an old commonality exacerbated into the appearance of great diversity by many small and fairly isolated populations (peoples). Moreover the local differentiation of Amerinds, not counting Na-Denes, took place in 15,000 years, a date strikingly akin to Fagan's archeological ones. The fact that Na-Dene links up with other Americans only at the 25,000 year level (in his Fig.1, not in his calculations) suggests that Dene-Caucasic might relate to Amerind (at 25,000) before Amerind relates to Nostratic or Eurasiatic linguistic phyla at 35,000 years. This has always been a logic inherent in the evident earlier arrival of the Na-Dene in the Americas before the (Nostratic) Eskimo-Aleut or Chuckchi.

For another example, a major effort to compare Australian and Indo-Pacific languages might finally pay off -- partly because of the preservative qualities of 700 Papuan tongues -- and the resulting

super-phylum would represent "not less than" 40,000 years of time depth and be rather closer to our Mother's tongue than we usually get. For a last example the same could be done in Africa where there already are hypotheses linking Nilo-Kordofanian with Nilo-Saharan, as well as AA with Khoisan (maybe only me). But the peculiarity of AA bio-genetics is that attempts to link AA with Eurasiatic languages seems just as likely to pay off. Since most northern AA-speaking peoples clearly belong in the same physical taxon with Europeans, and they own the hoary antiquities like Egyptian and Akkadian, there is probably no way we can get a fair hearing for the Khoisan alternative. Nevertheless, it is now quite clear that most of the southern AA-speaking peoples are more like other Africans physically than they are like Europeans, only a bit more in the case of the Beja and northern/eastern Cushites but much more African in the case of Chadic and most Omotic peoples.

Finally, there is much to be cautious about. Within Africa the chart shows a piece of N-C, namely Bantu, much closer to Nilo-Saharans than to West Africans. Moreover, the Mbuti Pygmies are nevertheless Bantu in language. Their cousins speak an N-S language. No gap between N-C speakers and N-S speakers remotely resembling their great linguistic differences comes close to showing up. There are too many phyla which are found in two bio-genetic clusters (e.g., AA, S-T, Uralic) or cannot be located on the chart at all -- Dene-Caucasic. The Mbuti, Ethiopian, Lappish, and Cantonese cases are, in my opinion, language borrowing situations. Let us hope that Luca and his colleagues can factor in that sort of thing more in their next article which will have lots more detail -- and populations!

One can find Luca's very short but densely packed article in either PROCEEDINGS OF THE NATIONAL ACADEMY OF SCIENCES, USA, vol.85, 1988, pp.6002-6006 as "Reconstruction of human evolution: Bringing together genetic, archeological, and linguistic data". or a short summary (which includes the crucial dendrogram) by Roger Lewin in SCIENCE, Nov.18, 1988. One could have a whole conference discussing the article and its meaning.

o o o o o

EDWIN G. "TED" PULLEYBLANK. Nov. 8, 1988...."I see that you have put me down under "Language Origins Generalist" and "Sino-Tibetan". This is correct but you might also add "Indo-European". (See my paper on Indo-European ablaut in WORD 21, 1965) The particular bee in my bonnet as far as Long Range Comparisons is concerned is linking Sino-Tibetan and Indo-European, which I don't think is all that long-range in terms of pre-history, since they were next-door neighbors when historical records begin and probably a good deal earlier if Gimbutas is right about IE origins. My Sino-Tibetan, however, is quite different from Benedicts's and also from that of Starostin or Baxter, and my Indo-European is also different from, and incompatible with, the new model of Gamkrelidze, Hopper, et al. For both ST and IE, I start with a "two-vowel" or "vowelless" analysis and an A/a or zero ablaut (= IE *e/o) which is morphological, with "introvert" meanings as a function of infixed /a/, rather than the result of conditioned sound change as is usually assumed. For the consonants, I derive voiced aspirates in both Chinese and Indo-European from a prefix *a, cognate to Tibetan h.-, Burmese 7a-, which may be either morphological, with a meaning very much like that of infixed *a, or simply part of the root. The plain voiced series of IE comes, in my view, not from glottalized stops but from sonorants, thus *N > *g, *Nw > *gw (compare Early Middle Chinese Nuw "ox" <*...(it's too much for my printer -- HF) (many lines later, still quoting -- HF) ...I hope to have a monograph ready before too long."

As a "generalist" my interest is in universal distinctive features,

both from the point of view of their role in synchronic and diachronic phonology and from the point of view of their evolution as part of the innate language capacity of Homo sapiens. I think too little attention is being paid by historical linguists to the new theoretical advances in this area, especially as a result of the introduction of non-linear phonology into generative theory. A real revolution in distinctive feature theory seems to be foreshadowed in the joint article by Morris Halle and Peter Ladefoget in the latest issue of LANGUAGE.....Ted."

o o o o (!xo'õ) o

TONY TRAILL. Nov. 2, 1988..."I am busy doing a !Xo'o~ wordlist for publication in Vossen's QKF series put out by Helmuth Buske. It is really a taxing exercise, I find. When one gets beyond sand, stone, see, fall, etc. and has to provide finely tuned definitions one has respect for dictionary makers. Perhaps the final product will promote some more long range comparisons for the Khoisan area. I am finding that common food plants and technology almost never evince cognates in the Khoisan area. I find this particularly interesting since prima facie I would have expected stereotypy in the environment and its exploitation to be reflected in the lexicon IF we have genetic ties. Do you have any experience of similar situations elsewhere which would show that one cannot reach any strong conclusion like the one I am trying? Enjoy your Symposium....Tony"

Well, what about it, you guys? Let's give Tony a hand. Write to him Dep't. of Linguistics, University of the Witwatersrand, 1 Jan Smuts Avenue, Johannesburg, 2001 South Africa or send your comments to me if you want a lot of other people to hear your answer.... My own experience is that while economic and technical things are basic to life support and adaptation to the world, the words for such are not conservative. Words for "sand, stone, see, fall", etc. are conservative; lower numbers, pronouns, primary kinfolk and body parts too. See Dolgopolsky's famous listing of conservative words (the source is in MT2) or Paul Black's equally valuable list in Dyen 1973 (LEXICOSTATISTICS IN GENETIC LINGUISTICS). Or just the Swadesh list minus a few clinkers like "to lie, walk" is a good list for conservativism. But the cutting edge of culture in work life, I suppose, changes much faster, and hence its lexical symbol system, because life changes faster there. Yet, if two languages are not too far apart genetically, they may preserve such a mass of cognate terms for work and environment and technology that a marvelous picture of their ancestor's life situation may be reconstructed. (Anyone have examples of that outside of Germanic, Romance, etc.?) Or read Robert Blust's very fine article in the last issue, and I mean last, of DIACHRONICA for some good advice about semantic reconstruction.

o o o o o

SCOTT DeLANCEY. Sept. 6, 1988..."as my own comparative interests are at a shallower level (Tibeto-Burman, primarily; I also dabble in Penutian), but it's pleasant to be able to keep up with what's afoot....By the way, while I can't locate it/them right at the moment, I noted one or two allusions in MT to the fabled Austroasiatic substratum in the western Himalayas. Unless there's some new evidence floating around that I've missed, I would say that that notion can be safely dropped. The original impetus for it was the verb agreement systems of Tibeto-Burman languages of the area, which for entirely illegitimate reasons were once assumed to be secondary and probably contact-induced -- hence the hypothesis of the Munda substratum. In fact the agreement systems are clearly native (see James Bauman's 1975 UCBerkeley dissertation) and of Proto-Tibeto-Burman, if not Proto-Sino-Tibetan, age (see my forthcoming article in BSOAS). There is not, as far as I know, any compelling lexical evidence for the existence of an Austroasiatic population that far west. (There are supposed to be some

intriguing lexical traces in the eastern Himalayas, but that's not that far from contemporary Munda territory)....Scott". << Thanks a lot -- HF >> << Juha, you must meet Scott! He knows Japanese and Chinese, as well as French, German, and Russian. But not Finnish -- Hal >>

o o o o o

Now we are running out of space, so I must belittle our two good colleagues -- Frank Kammerzell and Petr Zemánek.

— — — — — — —

Frank Kammerzell - Seminar für Ägyptologie und Koptologie - Prinzenstraße 21
D-3400 Göttingen - Bundesrepublik Deutschland

7/I/1988

Dear Hal,

thank you for the letter and especially for the welcome among the Long Rangers. I didn't receive Mother Tongue 4 yet, but perhaps it will arrive one of the next days.

I'm able to answer your question quite immediately, for I have some free time just now thanks to a temporary defect of my right hand - a student pretended that I tried to show her a pseudoparticiple and during my desperate attempts to prevent such a calumny she got histerycally and hit me with a geminating root.

The original Egyptian word for 'knee' is *m?s.t* (since Pyramid Texts), in Dyn 18th and later one can find *p?d/pd* (no *prd*!), at first only as part of the compound *p?d-m?s.t* "head of the knee" (from *p?d* 'ball/small round cake') = 'patella', later also alone as expression for 'knee' (> ⲡⲁⲧ in Coptic).

But now to *q?b(.t)*: The different Egyptian nouns in the form of *q-?-b* (e.g. bend/coil; name of a snake; intestine; breast/nipple; parting) are generally thought to be derivations of *q?b* 'to fold/dopple', and this makes good sense semantically. Although I don't believe myself to be a "Belegnik" (our German term for Anglo-Slavic "referencenik"), I tried to look for the knee directly in the texts. As far as I could find out, Dolgopolsky's *q?b.t* 'knee' is not attested but once in the Book of the Dead, and it's nearly a century ago that the word *q?b.t* in that passage (Tb Naville, Spell 172,28) was translated with 'knee'. The Wörterbuch treated the topic rather strangely: On the one side *q?b.t* was thought worth an extra entry (Wb.V 11,9) with the remark "of a part of the leg, if 'knee'?", but on the other side they translated the same word of the same passage with 'nipple' (Wb.IV 410,9 explaining the immediately following - and also obscure - *š?b.ω n.ω zš.ωt*)!! In modern translations (e.g. E.Hornung, Das Totenbuch der Ägypter, Zürich & München 1979) *q?b.t* is rendered as 'breast/nipple' only. Forget the knee.

Cordially

Frk

Mr. H.C. Fleming
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232, Bay State Road
Boston, Mass. 02215
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Prague, 27th August 1988
Petr Zemánek
Dept. of Asian and African Studies
Celetná 20,
110 00 Praha 1
Czechoslovakia

Dear, Prof. Fleming,

enclosed you will find two offprints of Prof. Petráček's articles - "Sur le rôle des modalités sonantiques dans l'élaboration de la racine en sémitique" /Arabica 1987/ and "Berti or Sagato-a /Saharan/ Vocabulary" /Afrika und Übersee 1987/.

Mrs. Petráčkova, wife of Prof. Petráček, asked me to take care of the correspondence of Prof. Petráček, so in case you would need anything from Prague, you can turn to me. I studied under the leadership of Prof. Petráček Arabic and Hamito-Semitic linguistics /The theme of my diploma work was the comparison of the Semitic and Egyptian verbal systems/. Nowadays I work on the extended verbal stems in the Semitic languages.

From the posthumous works of Prof. Petráček will in September or October 1988 appear his "Altägyptisch, Hamitosemitisch und ihre Beziehungen zu einigen Sprachfamilien in Afrika und Asien. Vergleichende Studien." and probably during the next year /1989/ will appear his "Introduction to the Hamito-Semitic /Afroasiatic/ Linguistics" /- this title was announced - or more precisely - was to be announced in the Hamito - Semitic colloque in Vienna in September 1987 - it will be for the time being published in Czech in a form of a student's manual - Skriptum/.

Yours sincerely, (signed - H

Petr Zemánek

3000 BC ± | 2000 BC ± | 1000 BC ± | Cleopatra

∴ Ur-Egyptian /m?s.t/ >> Middle Eg. p?d-m?s.t >> (New) Late Eg. p?d >> pd >> Coptic pat

"Knee" | "Knee cap" | "Knee" | "Knee"

Beautiful example! Thanks, Frank -- H

Prof. Harold C. Fleming
69 High Street
Rockport, Mass. 01966 USA

Courtenay
30 avril,
1988

Cher Monsieur,

Ceci est un "follow-up" à ma lettre du 18 avril. 1) Voici la référence de l'article "contre Movius": Seonbok Yi et G.A. Clark, "Observation on the Lower Paleolithic of Northeast Asia", CURRENT ANTHROPOLOGY 24:181-202, avril 1983. Lisez attentivement les commentaires qui suivent l'article lui-même. Mais ne prenez pas au sérieux l'affirmation suivant laquelle il y aurait eu en Asie du nord-est (et en Amerique du nord-ouest) une "tradition" particuliere de "small tools" (Gai Pei, p. 192). En effet, comme l'ont montré de nouvelles recherches depuis 6 ou 7 ans, il y a des "small tools" depuis Olduvai, et des assemblages caractérisés par des "micro-outils" en Europe depuis peut-être 700 000 ans (Isernia, Italie). Voir un article tout recent à ce sujet dans CA, dont de nouveau malheureusement je ne peux vous donner la référence exacte (voilà ce que c'est de travailler en deux endroits différents: à Paris et dons cette maison de campagne d'où je vous écris, à 115 km de la capitale!) - mais vous le retrouverez facilement: il s agit d'une comparaison de outillages d'Arago (France), Vertesszöllös (Hongrie) et Bilzingsleber (RDA).
2) Pouvez-vous me communiquer l'adresse de Claude Boisson? Je ne la trouve à Paris...Je suis quelque peu sceptique sur un rapprochement spécifique entre Sumérien et Dravidien. La structure phonologique du sumerien me parait plus proche de celles des langues altaïques ...mais enfin, il faudrait voir les rapprochements de M. Boisson. Références des articles de Diakonov et Civil sur le sumérien: dans SUMEROLOGICAL STUDIES IN HONOR OF THORKILD JACOBSEN, ed. par Stephen J. Lieberman, Univ. of Chicago Press, 1976. Rivet, en 1928, avait voulu rapprocher le sumérien de "l'océanien". Pourquoi pas? Bouda, en 1938, avait tenté des comparaisons avec le basque, les langues caucasiques de l'ouest et le tibétain. J'ai moi-même relevé quelques correspondances curieuses par ex. sum. /erin/3 "entrepot, grenier" (Labat 49 = /arah/, que l on peut peut-être comparer avec /a-ru/ "donner", IE 2. /ar-, er-/ (Pok. 61) et avec ST /(b-)rin/ "distribuer, répartir, partager, recevoir, remettre", en chinois archaïque "recevoir de la part de supérieurs", "allocation de grains du grenier public", "grenier gouvernemental" (Benedict-Matisoff, p. 178, note 473). Il est assez intéressant de remarquer que /erin/erin/ en sumérien a aussi le sens de "serviteur, soldat, colon militaire, employé" et que, en grec, on trouve des dérivés du verge /árnumai/ (qui est dans la rac. /er-/) avec un sens très proche: /aistharnos/ "qui reçoit un salaire" (cf Chantraine, DICT. ÉTYM. GREC., 112, 705-6). Certes, sum. /mag/ "haut, élevé, grand" (Delitzsch 182, Labat 57) peut être rapproche de drav. 4841 dans le DED (DRAVIDIAN ETYMOLOGICAL DICTIONARY de Burrow et Emeneau): Tamul /micai/ "eminence", Brahui /bash/ "up" - mais on peut aussi bien le confronter à IE /meg(h)-/ (Pok. 708-9) "grand" - ce qu'avaient déjà fait Autran (SUMÉRIEN ET INDO-EUROPÉAN, 1925, p.17, note 1) et Gostony (DICT. ÉTYM. SUM., 1975, p.25, #163) et, pour cette même rac. IE, Bomhard (1984: 270, #271) donne des corresp. sémitiques plausible, auxquelle on peut ajouter celles fournies par M. Cohen (ESSAI COMPARATAIF...1947: 95, #87); cf. aussi Hodge "Indo-European consonant ablaut" #4. Dans ses LEXICAL PARALLELS...de 1987, Bomhard redonne sans changement (#344) sa comparaison de 1984, mais y ajoute (#372) une autre possibilité de rapprochement avec le dravidien: DED 4838, qui est en effet possible, - mais il faut alors admettre qu'il s'agit d'une seule et même racine "nostratique" /mg/mk'/...On pourra se référer aussi à Møller, VIM 158 (/m-ga/ "beaucoup"). Ce n'est pas tout! Diop (PARENTÉ GÉNÉTIQUE DE L'ÉGYPTIEN PHARAONIQUE ET DES LANGUES NÉGRO-AFRICAINES, 1978: 167-8) compare des mots égyptiens signifiant "les vieux" et "vénérable" à wolof /mag/ "vieux, vénérable" puis (p. 227) ce même /mag/ glosé cette fois "grand personnage, notable" a ég. /mk/ "protecteur" - mais ces termes wolof sembleraient pouvoir être rapprochés d'IE /meg(h)/. Par ailleurs, dans le domaine amérindien on trouve chez Mattison, COMPARATIVE STUDIES..., 1972, #86 une entree "big in size, area" /kV-ma-k?a/uC/ qui évoque tous les "putative cognates" énumerés ci-dessus, ainsi que ST /man/ "big, elder" (aussi "great" et "principal") (BM 189). En outre, on peut noter chez Greenberg (LANGUAGE IN THE AMERICAS), 1987: 237-8) un item étiqueté "large", qui, en fait, groupe des notions assez hétéroclites (fat, all, much, long, far) exprimées par des formes diverses, mais dont la structure de base semble être /mk/ et dont au moins une partie pourraient être rapprochés de celles que nous venons de passer en revue.

Il n'est pas évident que tous les mots reúnis par Greenberg dans cette entrée #160 soient réellement comparables. Par exemple, Greenberg cite Ona /mehes/, /makes/ "all". Najlis (DICCIONARIO SELKNAM, 60) donne /makeş/ (/mexe$/ dans le dialecte du sud) "todos", mais y voit une "racine" /ma/ "visible". Par contre, Beauvoir (LOS SELKNAM, p. 50-51) donne un /makes/ "convenir" (convenio de todos) puis /makesoyn/ "reunión", /maghes/ "comun a todos" et /mehenscherken/ "adunar, reunir". Mise à part la différence de transcription (que Najlis, LENGUA SELKNAM, p. 114, qualifie de "en extremo incoherente"), l'idée de base chez Beauvoir semble être celle de "réunion", d'ensemble: voir aussi /makeskoten/ "concierto, sonar todos". On ne voit pas bien comment on peut passer de cette idée a celle de "gras" (fat) dans les mots Penutian cités par Greenberg pour le même #160. Dans une entrée "additional", Greenberg donne un Uto-Aztecan /*meka/ "far" qui correspond à Selk'nam /mah/ "allí" et "lejano de la vista" (Najlis, DICC. 60, LENGUA 87 - à noter aussi /mah/ "tú", idem 21, et /ma7/ "ahora" idem 48). Malheureusement, comme apparemment il n'existe plus de locuteur Selk'nam, il y a peu de chances que l'on puisse eclaircir les relations entre ces mots...
Greenberg a cru pouvoir aussi rapprocher de cette série Kutenai /-ma/ "long object"; il n est pas sûr qu'il puisse réellement entrer i?-HF) dans la même serie, on observera qu'en IE on a une racine traditionnellement séparée de /meg(h)-/, /mak-/, /mēk-/ [j'emploie ici la transcription /e"/ pour le schwa] "long, thin" (Watkins 1985:38) Rivet ("Les malayo-polynesiens en Amerique, J. SOC. AMERICANISTES 18:200, 1926 #16) a comparé un /u-mau/ Dieguen~o (Hokan) à des mots austronésiens pour "beaucoup" commençant par /ma-/, qui me paraissent pouvoir être rapportés a /ma+(CtT(u(qso)a/

Encore apetissement! - HF

'strong' (HEAVENS! -- HF) (p-Aust. 'many' Blust, 1973 - Wurm-Wilson, 1983: 207) et à /mako/ (proto-Central Papuan, Pawley, 1969 -- Wurm-Wilson 127) -- mais /ma-/ doit être un préfix d'adjectif (Mohring in ORBIS 17:128, 1968) et, dans ce cas, la comparaison est sans valeur...
La morale de cette histoire est que l'on doit être circonspect en comparant des mots "piqués" un peu au hasard dans des stocks linguistiques différents, tant qu'une analyse morphologique détaillée n'aura pas été faite afin de savoir si l'on compare des morphèmes réellement comparables: j'avais déjà évoqué ce problème dans mon rapport au colloque de la LOS a Nashville de 1987 (p.14)

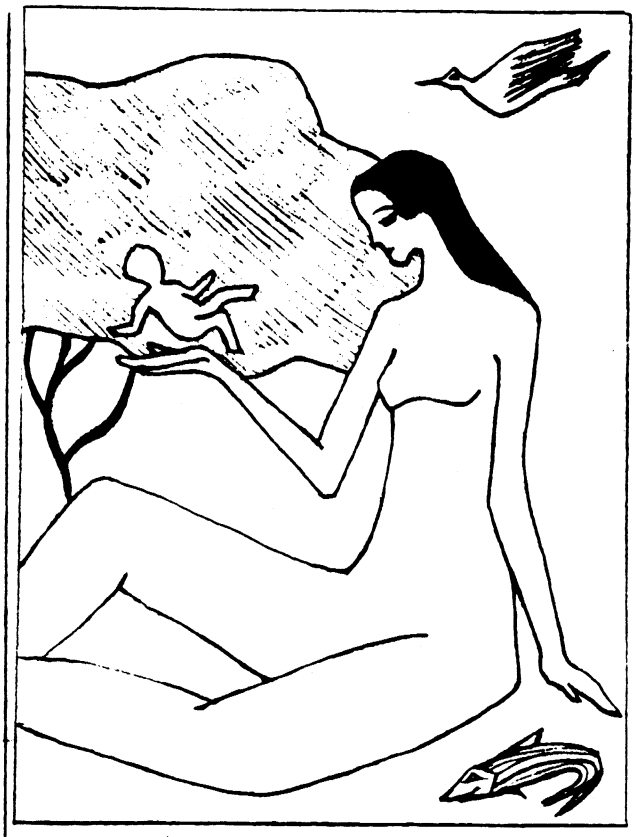
Pour en revenir au sumérien, voici un relevé du nombre de comparaisons qui ont été tentées:
sum. /indo-europ. (Autran) ca. 50; moi ca 40 (pas tous les mêmes)
sum. /austronésien (Rivet) 127 dont une vingtaine tres douteux
sum. /sino-tib. ca 20 (moi)
sum. /burushaski ca. 40 (moi)
sum. /altaïque et/ou v. turc ca. 60 (moi)

Je n'en conclurais nullement que le sumérien est plus proche de l'"austro-thai" que des autres stocks... Gostony (1975) a proposé un millier de rapprochements sum./hongrois - dont peut-être une vingtaine méritent d'être retenues comme correspondances sum./ouralien et/ou finno-ougrien. Sumer n'apparait comme une sorte de "melting pot" où se sont rencontrés, en donnant naissance à la premiere civilisation réellement "urbaine", des peuples venus sans doute à la fois du sud(-ouest) et nord(-est). Il pourrait en être résulté une sorte de "Mischsprache", le "substrat" étant plus probablement "altaïque". Les correspondances avec le dravidien -- il faudrait dire avec l'élamo- dravidien -- pourraient éventuellement être dues à des emprunts, ou bien encore s'expliquer par un "ancien fonds" SAPIENS... Mais, bien sûr, encore faudrait-il examiner les rapports possibles sum./basque, sum./nord-caucasien, sum./nigéro- kordof. et/ou nilo-sah. Alors, et alors seulement, on pourra dire de quel(s) stock(s) le sumérien est plus proche. C'est ce genre de comparaisons "multilatérales" que Greenberg propose comme tâche aux linguistes intéressés par les "long range relationships" et je pense qu'il a entièrement raison! (voir p. 337 de LANGUAGE IN THE AMERICAS)

Bien a vous (signed)

Eric de Grolier

P.S. Ref. of the CA paper (p.1): Svoboda, CA 28: 219-27, April 1987



Upper emblem is
Nü Wa = creatress (China)
Lower emblem is
Anči = Amharic 'thou, fem.'
Please write & tell us which you prefer for our symbol.

FROM HAL'S HEAD.

Vitalij Shevoroshkin. Ben Stoltz and their colleagues held a conference on LANGUAGE AND PREHISTORY at U/Michigan and it was even better than expected. By agreement with the sponsors I will refrain from much discussion of individual papers -- the whole proceedings will be published. While this dampens my urge to natter on about all the things said, it is useful to wait for the book and it saves space in MOTHER TONGUE. What can be discussed here are the issues raised in general at Michigan. Some parts of the conference discussion were summarized by Roger Lewin in SCIENCE (Nov. 25, 1988).

Two head-on clashes occurred. First, blistering attacks were mounted by Indo-Europeanists, quietly approved by the Soviet delegation, against the heresy of Greenbergisaus and related methodological errors. Eric Hamp from Chicago was most vociferous in trying almost literally to blow away the "search for similarities". John Bengston had ventured some global etymologies and during his discussion period he stood alone amidst the warm moist airs blowing from all directions. He held his ground quietly with dignity until a rather weak counter-attack was mounted by a small band of deviants, including myself. Second, there came a less heated but quite forceful attack on Nostratic itself, again by American Indo-Europeanists. Subjectively the visiting Soviet scholars saw themselves being attacked, as well as Dene-Caucasic and other hypotheses, at least by implication. They responded vigorously, particularly Gene Helimsky, who stood in the debating pit and blew back eloquent rebuttals into the thickening atmosphere. But we all stopped for breath and accepted a compromise notion that reasonable people had a right to see the Soviet data and analyses for themselves and did not have to accept such venturesome notions until they had time to study the proofs thoroughly.

I was struck by the force of the Indo-Europeanist belief system which also seemed an odd way of thinking, as alien cultural systems often do. Because it is a methodology without a beginning. If one assumes that Indo-Europeanists already have their cognates, like cats have genes, then one can understand the absurdity of "Seek the dissimilar! Do not look for similarities!" The comparative method of the rest of science has been stood on its head. They did not understand the rebuttal from the Greenbergites of "How in hell can you get cognates in the first place if you don't first sort out lists of similarities?" They had inherited their cognates and the chore of separating the wheat grains from the chaff had already been done. Clearly true cognates do not look like each other -- hence they are dissimilar -- because of phonetic change over time. Like English [tuu] and Armenian [yerk'u] (an Eastern dialect) for "2". Of course there still exist Armenian [vot'] and English [fUt] "foot", or German [du] versus Armenian [du] "thou". They should be thrown out because they are similar or identical? My goodness! As they say in politics: if it looks like nonsense and smells like nonsense and acts like nonsense -- it is nonsense!

But Hal's head is not an impartial observatory. I am an Africanist and I guarantee that we will never get anywhere disregarding and rejecting things that look similar. We still have to work for our cognates and the IE way is not helpful, not if it requires us to throw aside our common sense in the name of a semi-sacred law of dissimilarity. Say that we want to find out if English and Armenian are related to each other genetically. How does one go about that? Well, first we line up all the morphemes that are not alike, putting them in neat little piles. There will be a lot of neat little piles, of course, because English and Armenian do get rather unlike each other. Then what do we do -- after we have disdainfully shredded the residue heap of similar morphemes? Because we still do not have any cognates. So we can't establish any sound or unsound correspondances. Nobody knows what to do next, so we will have to declare that English and Armenian are not genetically related but the research does look promising -- after all we found thousands of dissimilarities!

ADDENDA (Members & Categories) As of January, 1989

Delete: Bernard Campbell, & from Physical/Biological.
Delete: Derek Strecker, & from Daic / Miao-Yao. He wants not LongRanger-hood.
Delete: Jack Cambell, & from Archeology. Very quiet for 15 months.
Delete: Rudolfo Fattovici, & from Archeology. Clearly not interested.
Delete: Paul Hopper, & from AA, IE. Very quiet for 15 months.
Delete: Mary Lisa Kazmierczak, & from Creoles/Pidgins. Silent for 15 months.

Add: Angela Della Volpe. (Address temporarily lost)
Add: Scholar's major work accidentally neglected! Apologies! Add Hetzron to Semitic, Cushitic, and Chadic.
Add: Scholar's full range not appreciated. Apologies! Add Bennett to Berber, Semitic, and above all to Mediterranean Isolates (Basque).
Add: Scholar's full range not known. Add Oryol to Mediterranean Isolates (Etruscan).
Add: Scholar's full range not known. Add Pejros to Dene-Caucasic and to Burushaski/Nahali/Kusunda (Burushaski). And he has linked them!
Add: Scholar's full range not known. Add Lamb to AMERIND (delete question mark), Add to LANGUAGE ORIGINS GENERALIST + EURASIATIC + COMPUTER WISE. Also says to add a new category = METHODS IN LONG RANGE COMPARISON.

Add: Herrmann Berger's address is Sudasien Institut der Universitaet Heidelburg, Abteilung fur Indologies, 6900 Heidelburg 1, (Im Neuenheimer Feld) (? - HF) FRG.
Change: Decsy. Delete from What Specialty?. Add to Uralic & Yukaghir, Altaic.
Change: Carroll T. Riley to Carroll (Cal) L. Riley. Change his address to: 1106 6th St., Las Vegas, New Mexico 87701.
Change: Abraham Demos's address to Dep't. of Linguistics, not African Studies.

Long Rangers Sorted by Country or Region, not necessarily by origin. At least fifteen of the USA scholars are European or Ethiopian in origin, some recently.

Western Europe	= 45	
Central Europe	= 8	
Eastern Europe	= 22	(USSR, including Caucasia; Finland)
Middle East	= 3	(all in Israel)
Africa	= 6	
Australia	= 7	
Eastern Asia	= 2	(both in Japan)
Latin America	= 1	
United States	= 113	
Canada	= 5	

About 25 new members joined up at the Michigan conference. Only a few of their names are yet on your lists.