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Newsletter of the Association for the Study of Language in Prehistory
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In a partial return to our old customs ASLIP will be producing occasional small newsletters on topics which are not usually covered in MOTHER TONGUE: THE JOURNAL and which are not original contributions like those which appear as articles in the Journal. We offer up three such small reports which are meant to stimulate responses or reactions which may guide us in future mini-newsletters. Tell us how you like the format and topic!

Editor for this issue is Harold C. Fleming

July 2003.

Just a little less than seventeen years ago we began our journey to the sources of *Homo sapiens sapiens* or *Homo loquax*. Upon exposure to the excited young linguists of Moscow (summer 1986), I wrote my own excitement to my friend, Aharon Dolgopolsky, and invited 80 other people to listen in. Soon Aharon and I started a club, the Long Range Comparison Club, which quickly evolved into the Association for the Study of Language In Prehistory and which periodically produced a newsletter called MOTHER TONGUE. Right from the start we were affected by the excitement generated by the paleoanthropological work of Chris Stringer and his many colleagues who were generating the OUT OF AFRICA hypothesis of anatomically modern human origins. A solid base in fossils and dated sites and traditional physical anthropology anchored that whole bit. Simultaneously, or virtually so, Rebecca Cann and her colleagues (mostly Californians) gave a tremendous leg up to the OUT OF AFRICA hypothesis when they did a world-wide survey of mitochondrial DNA and concluded that the Africans were not only the most diverse group of modern humans but they also showed signs of being (technically) the African STAY-AT-HOMES. The dominant African clusters of Pigmies, Bushmen, mainline Negroes, and Ethiopians could not be derived from each other, yet everybody in the rest of the world seemed to be derived from Africa and had greater affinities to mainline Negroes and Ethiopians than to Pigmies and Bushmen who looked to be the most divergent folk on Earth.

Charles Darwin himself had anticipated that Africa would probably turn out to be the homeland of mankind, meaning modern man, of course, since barely any of our ancestors were known to him It would be interesting to follow his reasoning on this score but I have not yet been motivated enough to go look up the source and read it. In modern times Rebecca Cann's thesis was advanced perhaps in a less powerful way by geneticists and physical anthropologists, most saliently by Cavalli-Sforza well before he and his colleagues produced their gigantic HGHG book and indeed before Becky Cann's hypothesis was first presented.

Opposed to this OUT OF AFRICA hypothesis was the perfectly reasonable alternative that modern man did not originate in any one place and spread around the world. Indeed a strong anti-migration ideology in archeology also inhibited world-wide expansions. There was no Garden of Eden except in Christian mythology. In the 1970s and 1980s, teaching basic anthropology and examining textbooks for classes, I noticed several things. First, there was quite strong opinion that Neanderthal was in our family tree, that Europeans at least if not much of the rest of mankind had Neanderthal as the last ancestral stage before Cro-Magnon and ourselves. Many anthropologists vigorously attacked the stereotype of Neanderthal the brute, the dummy, the savage who was knocked off by tall handsome Cro-Magnons. Secondly, the evidence kept mounting that varieties of Homo erectus lived in much of the Old World before Homo sapiens sprang up there. Again the strong bias against migrations in archeology affected everyone else, so that there was a kind of supposition that local invention was preferable to diffusion and local development was preferable to

migration from the outside. One heard frequently that, if mankind had a homeland, it was probably in Central Asia, a convenient place about which little was known.

Moreover there was a major and respected intellectual tradition in physical anthropology that favored local invention or native (autochthonous) evolution. In the 1960s Carleton Coon had published his ORIGIN OF RACES and later THE LIVING RACES OF MAN. Their main thrust was to show that groups established in Homo erectus' realm plus Neanderthal in Europe were the bases for the geographical varieties of modern man more or less associated with various regions of the world. In this Coon continued or confirmed the hypotheses first ventured by Franz Weidenreich among others in the 1940s. Despite the book titles, Coon was playing down rigid categories of race without actually abandoning them because a powerful movement was afoot in physical anthropology to dispense with the concept of race altogether.

The cognitive descendant of the Weidenreich-Coon theory, albeit modified and not claiming descent, has been called the "multiregional hypothesis" whose principal proponent in fossil studies has been Milton Wolpoff (Michigan) and Alex Templeton in genetics (Utah). Both have routinely challenged new discoveries and conclusions of the OUT OF AFRICA school, while each seems to have fewer and fewer followers per year. For example, last October I sat in a room at the Cold Spring Harbor Laboratory with 100 or so geneticists and paleoanthropologists when one speaker stated that he wished to know, so that he might do justice to the controversy, if there were any present who believed in "multiregionalism". If there were, we might discuss the problem. No one rose to defend "multiregionalism". No one raised a hand to show that he was at least in favor of that theory. I was impressed.

One serious problem adhered to both theories of Homo sapiens and his distribution. The problem of the physical unity of mankind. Along with the psychic unity of mankind espoused by most anthropologists the simple fact was that from one end of the globe to the other human beings were one intra-breeding species; they could and they did breed with each other. Sailors had always known this. The key solution to the theories of Weidenreich, Coon, and their modern followers was GENE FLOW. That was the mechanism by which `evolutionary grade changes' could be passed around the species, enabling all varieties in all regions to advance together at least in physique, if not in culture or language. Even though the general theory seemed to freeze races into their regions and their differences, still it was egalitarian in its presumption that all peoples of the world were equally human. From Pigmies and Bushmen to Australian aborigines to Eskimo hunters nobody was primitive or backward physically, even if their cultures sometimes were pictured as primitive. Oddly enough, except in central Europe, the idea of primitive languages never caught on among linguists.

In the years since we started there has been much resolution of the two basic hypotheses. We are approaching a final decision on the veracity of each one. Except for the settlement of the Homo loquax question, we are almost finished in writing the history of our species in broad outline. Recently two research reports in major scientific journals have nearly sealed the fate of the "multiregional hypothesis" and nearly proclaimed the victory of the OUT OF AFRICA hypothesis. Let's see what happened.

Writing in SCIENCE, a group of Italian geneticists presented data and analysis which said two important things. First, Neanderthal DNA again indicated that the Neanderthals were quite distinct from all or any modern human populations in mtDNA (at least). Second, the near contemporaries of Neanderthal, namely Cro-Magnon (in this case of Italy), were very much within the range or central tendency of modern humans. In short Cro-Magnon was simply a modern human in DNA, while Neanderthal was not. This confirmed the view from fossil studies which had always maintained that Cro-Magnon represented modern human beings, while Neanderthal was something else, possibly distantly ancestral to moderns and possibly an offshoot of a much more remote common ancestor. Some anthropologists objected to the Italian study on the grounds that the Cro-Magnon DNA had been contaminated by the DNA exuded (?) by the modern human lab personnel. Some said it was impossible to ever solve this problem. Most of the protests seemed like die hards grasping at straws, or anything to protect their hypotheses. My own reaction was that the protesters represented the degree of dishonesty evoked by losing a long hard debate. Nobody likes to admit they were

wrong, that their favorite theory was kaput.

What put the rest of the nails in the coffin of Weidenreich-Coon theory was another important fossil find. This one was in Ethiopia in the same region (Afar) which had produced Lucy of global renown. The field workers were even the same or at least some of them were, especially Tim White who must be one of the luckiest fossil hunters in existence. His intelligent preparation and search in the designated region was not luck, of course, but planning. But the actual site was chanced upon because of a rain storm. They practically tripped over the skull of their fossil in an area they would have overlooked or delayed examining.

The archeological content was described thusly: “The archeological assemblages contain elements of both Acheulean and Middle Stone Age technocomplexes. Associated faunal remains indicate repeated, systematic butchery of hippopotamus carcasses. Contemporary adult and juvenile Homo sapiens fossil crania manifest bone modifications indicative of deliberate mortuary practices.” Three quick comments. The African Middle Stone Age as the general `complex’ had its European counterpart in the Mousterian in which Qahzeh was later found but it was also generally associated with Neanderthal. These are typologically intermediate between the earlier hand-axe and later blade `complexes’ (traditions). The hunting of hippos is still practiced by several tribes in the Ethiopian lakes and by the El Molo of Lake Rudolf (Turkana) in Kenya. Theoretically, one could argue that the mortuary practices are indicative of cannibalism, even if the site report thinks otherwise. The `whatever for?’ question naturally arises but there is ethnographic evidence of such mortuary practices, they say.

The proportions of the various tool types may shed some light. For example, there were some hand axes but most tools were not those. A very diagnostic technique, I’m told, was the Levallois method of flaking; that was common. There were blades too, but they were not common. Mostly we’re talking about flakes in this culture. But also, given the hunting tools of the modern hippo hunters of Lake Margarita (Abbaya) in Ethiopia, it is possible to kill hippos with fire-hardened spears, i.e., no hafted stone or metal points. Since hippos are very large and very dangerous beasts, hunting technique includes a great deal of skill at sneaking up on them and stabbing them when they are vulnerable. Or so I was told by the Ganjule of Lake Chamo who practice hippo hunting. (Oddly enough, contrary to `rational’ expectations, the Ganjule venerate or worship crocodiles!)

Tim White and his colleagues found that Herto man was not quite modern human, but almost, and at a date and location that powerfully suggested that this almost modern human was most likely to be the ancestor of all modern humans, including those found in eastern Africa around 125 kya and those found in Israel around 90-110 kya and the Cro-Magnon themselves of Europe of 25-40 kya. We now had an increasingly clear descent line or evolutionary progression from *Homo sapiens idaltu* (the man of Herto in Afar) via Omo Kibish (Gemu-Gofa, Ethiopia), Jebel Irhoud (Morocco) to Qafzeh (Israel) to Cro-Magnon to ourselves. But, since we had a line of modern human ancestors stretching back 120,000 years before Cro-Magnon or at least humans who looked a great deal like our probable ancestors, then it became very hard to believe that Neanderthals of that same period were our ancestors or even our closest cousins. Our line was distinct from the Neanderthal line and had been for quite a long time.

More precisely, in the Middle Awash valley of northeast Ethiopia in the territory of the Afar (Afar to the linguists) there is progression of sites with genus *Homo* skeletons from 1,000,000 (Daka) to 500,000 (Bodo) to 160,000 (Herto) years ago. That is not the same as finding one stratified site with that range of dates but it is still very arresting! That progression is also not quite the same as the evident series of anatomical changes which can be inferred from the larger African distribution of genus *Homo* over a million years. Thus the anatomical progression or evolutionary grade changes (if you will) from early genus *Homo* (or early *Archaic H*omo) to early *Homo sapiens sapiens* (Qahzeh) can be found in various parts of Africa, although mostly in the east.

To put it another way, the Middle Awash valley has better preserved the record of changes taking place over the wider area of eastern Africa than any other region. Clearly not all the changes took place in northeastern Ethiopia. Nor can environmental conditions in northeastern Ethiopia be cited as determinative or causative, since people were evidently moving around, circulating over the variable terrains of eastern Africa. The people who ended up living around Herto may have arrived there from Kenya many years before. This is an important part of the prehistoric record that we don't know about yet.

And for this reason Chris Stringer has raised the possibility of a kind of "multiregionalism" within Africa where genetic changes or mutations first occur in (for example) West Africa and spread from there, while other changes occurring first in (for example) Zambia spread from there. The resultant of the convention or merging of various mutations would be newer and newer versions of *Homo,* culminating in WHO? The process is still going on and human populations are still passing genes around. Naturally, given the greater mobility of people in the past half millennium gene pools long separated or marginal to each other have been thrown together to create distinctive new kinds of humans, like African-Americans, Latinos, or Hawaians. Or Tiger Wood.

Another statement by the excavators is important (p.744): "Among the global sample of modern humans {that were examined – Ed.}, the Herto crania, both metrically and non-metrically, lack any derived affinity with modern African crania or with any other modern group, confirming earlier suggestions {fn11-Ed.}. Instead, the closest approximations among modern individuals to the overall morphology , size, and facial robusticity are found in some Australian and Oceanic individuals, although these are also clearly distinct from the Herto hominids." This has been found before by Marta Lahr and Robert Foley and bears to be repeated. An old fossil from Ethiopia is not directly ancestral to Ethiopians, not the same as them in appearance, and not necessarily living in the place that they came from. Not necessarily.

Further details about the anatomy of the three skeletons at Herto and the site itself and its archeological content can be found in SOURCES. Suffice it for now to list some of the other sites and their dates to help the reader in her research. These sites are not uniformly revealing so their deficiencies and more helpful traits are not listed.

Nariokotome	Kenya	1.6 mya	H. erectus / H.ergaster. Turkana boy
Daka	Afar, Ethiopia	1 mya	H. erectus
Bodo	Afar, Ethiopia	500 kya	H. heidelbergensis
Kabwe	Zambia	125 or 250 kya	H. rhodesiensis, H.erectus/sapiens
Kapthurin / Baringo,	Kenya	230 kya	Mandibles & limbs only
Herto	Afar, Ethiopia	160 kya	H. sapiens idaltu
Florisbad	South Africa	38-41 kya	Disputed taxon. H.helmei, H.sapiens?
Ngaloba	Tanzania	(Cannot find the primary sources.)	
Singa	Sudan	" " " " "	
Eliye Springs	Kenya	(" " " " ")	
Border Cave1	South Africa	49-115 kya	Much disputed dates, sapiens
Omo Kibish	Ethiopia	3 to 125 kya	Most like Herto. Also called Omo I
Jebel Irhoud	Morocco	125 kya ?	Most like Herto. Also Jebel Ighoud.
Qafzeh	Israel	90-115 kya	Most like Herto
Skhul	Israel	same ?	Most like Herto
Cro-Magnon	many sites in Europe	25-40 kya	Us = anatomically modern man.

Chris Stringer drew a more formal chart of the longer term human descent lines (in his OUT OF ETHIOPIA article), going back basically to the two million year mark and the advent of genus Homo. The world is divided into three primary regions. The taxonomy reflects two different schemes, not necessarily tied to any one scholar.

Scheme A has *Homo erectus* ancestral to all and found in Africa almost 2 mya.

They persist steadily in Africa until roughly 900 kya. During their long African residence they also bud off settlements to Asia early on, i.e. circa 1.7 mya. Their Asian cohort persists longer than the African one until circa 100 kya or the advent of modern man. *Homo erectus* may have gone to Europe 1.1 mya and 0.9 mya but there is a question about this on the chart.

Meanwhile in Africa *Homo erectus* turns into, becomes, *Homo heidelbergensis* around 850 kya and fairly quickly invades Europe. They possibly invade Asia between 500 kya and 250 kya but that is also questionable. More importantly, they (H.h.) evolve into *Homo neanderthalensis* in Europe around 375 kya and also stay in Africa to evolve into *Homo sapiens* around 200 kya. Thereafter *Homo sapiens* expands in Africa and around 100 kya begins their conquest of the world, in the process eliminating Neanderthal in Europe and whatever late Homo erectus were present in Asia.

Scheme B is fundamentally similar to Scheme A in Asia but different in Africa and Europe. *Homo erectus* starts out in Africa at the same date as in Scheme A , then moves to Asia about 1.8 mya (to accommodate some very old dates from Indonesia and Georgia), then leaves Asia apparently circa 1.6 mya only to return maybe 20,000 years later, there to persist until the advent of *Homo sapiens* in the last 100,000 years.

However, in Africa *Homo erectus* evolves into *Homo antecessor/Mauritanicus* which persists in Africa and extends into Europe from 1.2 mya to 0.8 mya. Then comes a crucial point, looking for all the world like a bottle-neck, around 0.7 mya or 700, 000 years ago on the borders of Africa and Europe. At this point they change into *Homo rhodesiensis* which comes to dominate Africa and extend briefly into Asia. The Asian intrusion dates from 600 kya to 300 kya when it expires. It is also associated with a question mark. The European invasion 0.7 mya onwards leads to the development of *Homo neanderthalensis* which persists in Europe until the coming of Cro-Magnon. Meanwhile in Africa, Rhodesian man, so-called, evolves into *Homo sapiens* around 200 kya and follows the same scenario of world conquest as outlined in Scheme A.

There are two other details about these schemes which it is useful to mention. *Homo antecessor* (which was earlier reported in MOTHER TONGUE) is the only West African contribution to the family tree. And in the naming of Neanderthal's taxon, many paleoanthropologists prefer to call them *Homo sapiens neanderthalensis*. By this reckoning *Homo sapiens* had two distinct descent lines developing by 500,000 years ago. This would suggest that *Homo sapiens* is not such a young species. The other question would pertain to *Homo erectus* in Asia. How come, after living in the world's largest continent with a wide range of environmental conditions for more than a million years, Homo erectus did not evolve into one or two other species? It is hard to believe that H.e. of say 1.5 mya in Arabia had not evolved into something quite different in say 0,5 mya in Java.

What about *Homo loquax*? How does he fit into these fossil schemes? Man the talkative. That is a goodly label for our species, as chattering as magpies or parrots, far more inclined to jabber than mongooses or chipmunks, but blessed with an enormous capacity to internalize this talk and connect it to our cognition, as proposed by Vigotsky. This reminds me of a story told by my friend Willard Park who was working with an ancient shaman (Paiute, methinks) via an interpreter. Willard asked the old man if his tribe ever followed a particular custom. The ancient one leaned forward and spoke eloquently for half an hour in response to the question. So what did he say, asked Willard of the interpreter. He said `yes', was the answer. Very disappointed was my friend who did, however, learn that English might not be the vehicle for probing the complexity of shamanly thought.

Was *Homo sapiens* the one who initiated our jabbering? Was Neanderthal a strong silent type or did he pioneer a crude or impoverished version of language? Over the years these questions have been addressed in several ways. Overall, however, there seem to be two basic approaches. One has been called the **hardware approach** before, i.e., an effort to understand the human brain, its evolution and relationship to the evolving

`machinery' of sound production and breathing and drinking, and evidence to be inferred from various fossils about the hardware. This approach has attracted those who follow the **physics model**, looking for variables and generating hypotheses about the condition of the hardware at various periods, mostly the `beginning'. The adherents have tended to be the more theoretically inclined linguists, as well as anatomists, psychologists, and paleo-anthropologists. It is probably fair to say that this group has evoked far more interest among social scientists, has produced a great number of untested or unconfirmed hypotheses, and has reached no great amount of agreement on what happened.

The second group can be called the **historical or prehistorical approach**. Dominated by the basic viewpoints and techniques of traditional historical linguistics, it has been aimed at achieving the two primary products of that field, i.e., reconstruction of earlier languages and taxonomy or classification of languages into genetic groups. Since the notion of primitiveness finds little support in this approach, the goals of research have been conceived of as regular languages which were themselves parents to later languages but themselves also daughters of earlier languages. Nothing extraordinary is expected, i.e., any language at the parental level was still expected to be within the range of known languages in grammar and phonology. However the normal expectation did not extend to the lexicon where no one expected ancestral vocabularies to be as full of cultural things as modern vocabularies are. Moreover, given fundamental changes in society and culture over time, the contents of cultural vocabulary could be quite different between ancestor and offspring. Witness, for example, Latin of Caesar's time and modern French.

The two approaches differ in other ways too. Their empirical bases are very different, their `popularity' is very unequal, and their methods of dating ancestors are wholly separate. The hardware school draws on the rich data of paleoanthropology, anatomy, neurology, psychology, evolutionary biology and general linguistics; it draws upon the analyses and theories of those fields to help in solving the puzzle of human speech and its origins. But it uses little specific data from the 5000 plus known languages. The historical school draws heavily upon the specific language data. It uses little theory outside of that associated with historical linguistics. In a word one approach is highly theoretical, while its counterpart is highly empirical. The hardware approach is probably much more popular with both the general public and the scientific public than is the historical approach. I cannot prove this, having taken no surveys, but that seems to be our experience of the past two decades. A general reader can understand, for example, the arguments for and against Neanderthal language easier than she can the proofs that proto-Indo-European was glottalized or not or the genetic links between Basque and Burushaski. Eyes tend to glaze over and nap time approach when most scientists are confronted with the specific empirical arguments so beloved of the historical school. Not to mention the intelligent educated general public. What these publics do understand are the conclusions that the historical school reaches but those have to be taken on faith for the most part. And, when two `authorities' disagree with each other, the general public is lost, as is the scientific public for the most part.

For dating – a most important aspect of archeological and paleoanthropological work – the hardware school has few problems because they use whatever the fossil folk give them. For the most part the dates are grounded in *Homo sapiens* or Neanderthal specifics. For the historical school dating problems are endemic and severe. Part of the problem lies in a growing contemporary reluctance to investigate linguistic taxa of any appreciable age at all. Six thousand years or ten thousand years are the usual cut off dates given nowadays by the dominant historical linguists in the USA. Other problems arise for the small minority of linguists and anthropologists who do `long range' comparisons. The specific problematic nub is glottochronology. Very strong disagreements have arisen between the Russian school and one American scholar (c'est moi) about what time depths can be reached and how to reckon the data and chronologies involved. Had this discussion taken place 35 years ago we might have spoken of an American school but so thoroughly did American linguistics pursue and seek to destroy glottochronology that rare indeed are the Americans who do glottochronology nowadays. In effect the historical school has no means of calculating the deep time depths needed to reach the origins of *Homo loquax*. So, if linguists manage to do the final taxonomy of the human language set and create at least the outlines of a final ancestor to be called **proto-Human,** they will only be

able to date it by arbitrary association with some archeological culture or horizon. In the great discussion of when the **human diaspora** left Africa to settle the world the undated linguistic proto-languages will be next to useless, even though the possession of human language must have been a crucial part of the human diaspora. (In the forthcoming issue of MOTHER TONGUE: THE JOURNAL there will be a long report on a debate in Washington among archeologists about the dates of the great diaspora; language was not important during that discussion and it seems clear that the other sciences have decided to settle these questions without much input from historical linguistics.)

Nevertheless, the historical school is not really limited by the fears of contemporary linguists. It has a small number of competent and dedicated scholars who know how to generate taxonomies, deep or shallow, which can reach `all the way' back. This is because taxonomy does not depend on time or superficial conclusions due to misunderstanding glottochronology (the bases of the fears of linguists). If you compare two languages –binarism, you know – which have been separated in all probability from each other for sixty or seventy thousand years, you expect to find next to nothing in common in the lexicon. Take Siwai, an Indo-Pacific tongue spoken on Bougainville in the Solomon Islands (well into the Pacific) , and line it up against Vai, a Mande language spoken in Liberia (West African coast). They would surely share a few loan words from English and French derived from their colonial histories or those of their neighbors. But not much else. And this is the point where most of contemporary linguistics is hung up – an inability to think outside of the bounds of binarism. They are trapped in a fable of their own invention!! When they finally figure out that they can actually break out of their own chronological barriers, they will naturally claim that they solved the problem themselves. Is it possible that they haven't heard the rest of us telling them how to do it? Io non credo. Das glaube ich nicht.

They are lucky in a way because genetic taxonomy is something that the hardware folks do not do and probably can not do because they don't know how to. Nor do they care to.

Joseph Greenberg long ago spelled out how to proceed; it is all on record and we have discussed it for many years now. To illustrate – let us go back to Siwai and Vai where we have no reasonable hope of finding any genuine cognates at all. So what then? This is not at all the same as saying that Siwai and Vai cannot be related. Siwai is member of a large phylum of more than 700 languages, including 23 in Siwai's own branch (East Papuan). Vai belongs to Niger-Congo, the largest phylum in the world with about 1100 languages, including the 29 in the Mande sub-phylum. Who is willing to say that we are unlikely to find genuine cognates when we compare 20 East Papuan to 20 Mande languages? Fewer will be nay-sayers here. But, when we compare 100 Papuan to 100 Niger-Congo languages, the nay-sayers will surely become a minority because "the odds" keep going up. Indeed just 20 languages, when compared in basic vocabulary, should show about 22 cognates after 20,000 years according to calculations incorporating the so-called "Joos factor" (Greenberg's Amerind book, page 342). And 80 languages after 40,000 years should show about 30 cognates on a Swadesh list. Of course that does not mean you cannot find more than that amongst these languages because many cognates exist in other parts of the vocabulary, not to mention grammatical morphemes some of which are phenomenally conservative. One example from Germanic: German /faar-en/ `to go, travel, drive, ride' is cognate with English /feer/ which is found only in `cost of a ticket, cost of travel' or in archaic greetings such as `Farewell' or `fare thee well'. Neither is on the Swadesh list and one is a specialized term probably assigned to cultural vocabulary by most analysts. Since this sort of linkage is certainly well known to historical linguists, one has to wonder why it is not allowed for when thinking about these matters.

In any case the few who are doing long range comparisons these days will tell you that they find lots of cognates when they examine the large phyla and try to relate them to others. In addition to the very active Russian workers, loosely grouped around Starostin and Gell-Mann at the Santa Fe Institute, all of whom are partially inhibited by Indo-European constraints, two young (essentially) Greenbergians are digging up cognates by the bushel in Australasia and India with connections to Africa. That would be Paul Whitehouse in London and Timothy Usher in San Francisco. Another group is working on African-Australasian linkages but prefers to not be noticed, i.e., not named. Another of our gifted amateurs, Philippe Burgisser of Geneva is

working on the ultimate African problems (Kadu and Shabo) and will be presenting some of his results in the next regular issue of our journal. Finally, myself has produced and given papers on Borean, stressing particularly the interesting cognates between the African anchor in Afrasian (especially Omotic, Ongota, and Cushitic) and Amerind. Under the inspired direction of Michael Witzel there are plans for massive computer comparisons involving hundreds of languages around the world. And so forth. Long ago I should have mentioned Pierre Bancel and Alain de l'Etang's superb article on KAKA, the kinship term, which appeared in MOTHER TONGUE: THE JOURNAL, Issue VII, pp.208-258. It pleases me immensely that three of our active scholars are French because of the great tradition and contribution of francophone scholarship to both anthropology and linguistics. Now, if we can only get the deutshophone scholars to come join us too!

When we get the taxonomy finished, and some of us have started a serious attempt to flesh out proto-human, then maybe we can come to grips with the dating problem. It certainly won't go away just because we ignore it.

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OBITUARY

Adolf Erhart (∗ May 31, 1926, Námìš nad Oslavou - † August 11, 2003, Brno)

Adolf Erhart, in the second half of the 20th century the most important representant of Indo-European linguistics in Czechoslovakia and from 1993 in Czech Republic, left us. This sad report was shocking even for those who was informed about his serious problems with his health in the recent time. Fortunately, his scientific work remains. Let us mention the most important points from his scholar's curriculum.

Adolf Erhart studied classical and Germanic philology and comparative Indo-European linguistics by professors V. Machek, F. Novotný, P. Trost, A. Beer and others at the Faculty of Arts of Brno University in 1945-49. On the basis of the rigorosum work *K problémùm tvoøení komparativù a superlativù v jazycích*

indoevropských a pùvodu primárních komparaèních suffixù ["Toward the formation of comparatives and superlatives in the Indo-European languages and origin of the primary comparative suffixes"], he became the Doctor of Arts. In 1959 he defended the dissertation: *Pøíspìvky k otázce vzniku a vývoje konjugace v indoevropských jazycích* ["Contributions to the question of origin and development of conjugation in the Indo-European languages"] and was nominated the Candidate of Sciences. On the basis of his habilitation *Nové pohledy na indoevropský konsonantismus* ["New views on the Indo-European consonantism"] defended in Brno 1962 he became the Docent of comparative-historical linguistics in 1964. These three studies remain unpublished, but were projected in his later books. The procedure leading to his professorship started in 1968, but thanks to the political situation during so called 'normalization' after the Soviet occupation, it was finished only 20 years later. Fortunately, it had no influence on the quality of Erhart's scientific results.

The following selected bibliography, reduced to monographs with exceptions of some of important articles which were not developed into the monographs), is arranged according to main subjects:

General linguistics:

Úvod do jazykovìdy ["Introduction into linguistics"]. Praha: SPN, 1962; Brno: Masarykova universita, 2001.

Základy obecné jazykovìdy ["Principles of general linguistics"]. Praha: SPN, 1965, 1969.

Úvod do obecné a srovnávací jazykovìdy ["Introduction into general and comparative linguistics"]. Praha: SPN, 1973.

Základy jazykovìdy ["Principles of linguistics"]. Praha: SPN, 1980, 1984, 1990.

Jak klasifikovat jazyky ["How to classify languages?"]? *Sborník prací Filosofické fakulty Brnìnské university* A 27, 1979, 21-33.

(together with J.M. Koøínek) *Úvod do fonologie* ["Introduction into phonology"]. Praha: Academia, 2000.

Indo-European phonology, morphology, etymology, glottogenesis:

(together with A. Lamprecht) K otázce vztahù indoevropských jazykù k jiným jazykovým rodinám ["Toward the question of relations of Indo-European to other language families"]. *Slovo a slovesnost* 28, 1967, 385-393.

Studien zur indoeuropäischen Morphologie. Brno: UJEP, 1970.

(together with R. Veèerka) *Úvod do etymologie* ["Introduction into etymology"]. Brno: UJEP 1975, Praha: SPN, 1981.

Geneze indoevropských jazykù - diferenciace èi integrace? ["Genesis of Indo-European - differentiation or integration?"] *Listy filologické* 99, 1976, 193-205.

Indoevropské jazyky (Srovnávací fonologie a morfologie) ["Indo-European languages (Comparative phonology and morphology)"]. Praha: Academia, 1982.

Zur Entwicklung der Kategorien Tempus und Modus im Indogermanischen. Innsbruck: IBS, Vorträge und Kleinere Schriften 35, 1985.

Das indoeuropäische Verbalsystem. Brno: UJEP, 1989.

Das indogermanische Nominalflexion und ihre Genese. Innsbruck: IBS 73, 1993.

Der indogermanische Mondname. *Linguistica Baltica* 7, 1998, 63-69.

Indo-Iranian languages:

Sanskrt I: *Popisná mluvnice* ["Sanskrit I: Descriptive grammar"]. Praha: SPN, 1967.

Sanskrt II: *Historickosrovnávací mluvnice* ["Sanskrit II: Comparative-historical grammar"]. Praha: SPN, 1971.

Struktura indoíránských jazykù ["Structure of the Indo-Iranian languages"]. Brno: UJEP, 1980.

Baltic languages:

Litevština ["Lithuanian language"]. Praha: SPN, 1956.

Litevské povídky ["Lithuanian tales"], ed. by P. Trost. Praha: Svìt sovìtù, 1956.

Baltské jazyky ["Baltic languages"]. Praha: SPN, 1984.

Slavic languages:

U kolébky slovanských jazykù ["Toward the cradle of the Slavic languages"]. *Slavia* 54, 1985, 337-345.
(co-editor and co-author) *Etymologický slovník jazyka staroslovìnského* ["Etymological dictionary of Old Church Slavonic"], 5-11. Praha: Academia, 1995-2002.
Odkud máme jméno? O pùvodu etnonyma Èech ["Where is our name from? On the origin of the ethonym Èech"]. *Slavia* 67, 1998, 289-294.

The curriculum vitae of Adolf Erhart and his exhaustive bibliography from 1949 to 2000 were published by Bohumil Vykypìl in Erhart's Festschrift devoted to his 75th birthday (*Grammaticus*, eds. O. Šefèík & B. Vykypìl, Brno: Masarykova univerzita 2001, 5-8, 14-23).

It is pity that some of the most fundamental studies of Adolf Erhart remain untranslated into some of the world's languages and so almost unknown. Especially his "*Introduction into etymology*" from 1981 (written together with R. Veèerka) and "*Indo-European languages*" from 1982 belong to the best what was written about these topics in the world's linguistic literature.
Václav Blazek

<with apologies for some missing Czech diacritrics; will be republished later on, M.W.>