

Social Issues in Historical Linguistics in Africa

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SOCIAL ISSUES IN HISTORICAL LINGUISTICS IN AFRICA

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1. My purposes here are to outline (1) the way in which I think of the term "social issues in historical (and comparative) linguistics in Africa", and (2) some of the lessons we have learned over the last few years and the directions in which I think we are moving.¹

I use the term "social issues in historical and comparative linguistics" very broadly to refer to any attempt to relate demonstrable linguistic events to real or assumed human or historical events. Historical linguistics has been characterized since its inception by a tendency to regard linguistic phenomena as if they occurred or existed independently of people. I think we are now trying to put people back into the formula. This is happening world wide but also in Africa. A forthcoming issue of the journal *Sprache und Geschichte in Afrika* consists entirely of some 15 articles on Language Contact and most are concerned, to some extent, with the human component.

What time period is being referred to in general? Historical linguistics effortlessly covers from yesterday back to the establishment of today's language families. With the exception of Khoesan, that would probably not exceed 10,000BP in Africa. At that distant time remove, given today's methods, historical linguistics will not yield us much worthwhile detail. In practice, historical and comparative linguistics in Africa have proved most useful in interpreting the events of approximately the last five millenia.

SOCIAL ISSUES IN HISTORICAL AND COMPARATIVE LINGUISTICS
IN AFRICA

2. I would like to start with a case study. Recently T. Hinnebusch and I wrote a history of Swahili. It covers roughly the first fifteen centuries AD. We proceeded in the time-honored way using the Comparative Method: triangulation back from the present to the past, from current data to reconstruction of the past. Today Swahili is used across East Africa but it is known that prior to about 1800AD it was just a coastal phenomenon, stretching from southern Somalia down along the Kenyan coast, the Tanzanian coast, to central Mozambique - earlier it might have gone down to southern Mozambique. Our data came from all the old coastal dialects and we reconstructed aspects of Proto-Swahili (PSW: a partial lexicon, a C and V system, certain morphophonemic processes, the nominal system, and the verb paradigm, concentrating on tense and aspect categories). We did not stop at PSW but went a little further back in time. We did that for a number of reasons. Partly because we had comparable and comparative data for all of Swahili's nearest relatives and why not use it? Partly because Swahili's relatives are not very much farther along the continuum of variation evident in the Swahili dialects and so, chronologically, probably did not come into existence very long before the Swahili dialects. Partly because considering the relatives threw light on the development of Swahili itself. For example, reconstruction of suprasegmental features for PSW using only data from Swahili today would be difficult. Most Bantu languages are tonal, Proto-

Bantu was also tonal, it seems likely that PSW was too, but only two or three of today's 15 or so Swahili dialects are tonal and the systems are vestigial so they don't much help with reconstruction. But all of Swahili's relatives are tonal so including them threw more light on earlier conditions in Swahili.

The phrase "It covers roughly the first 15 centuries AD" is used advisedly. Where it stops is clear enough - about 1500AD - partly because we assume that change after that point was relatively small and partly because that marks the entry of the Swahili into history. The Portuguese arrived at that time, described what they saw in some detail, and events after that are less interesting and challenging to an historical linguist. We think that we started around the start of the Christian era because we were able to tentatively link our linguistic picture to a very few known or assumed archaeological facts. The co-author and myself disagreed on a few matters. One was how we viewed the limit of our job. Both of us thought of ourselves as comparative and historical linguists, that is, as comparativists we were interested in moving backwards to reconstruction and as historical linguists we were interested in charting the forward movement of language development. But whereas my co-author viewed his role as more or less stopping with that, I was also interested in linking purely linguistic evolution to real or assumed non-linguistic, external, events. I would go further, and say that I am actively interested in using linguistics as a tool for reconstructing prehistory. I would regard as incomplete linguistic explanations that do not take into account relevant non-linguistic variables. Although it would be an overstatement to claim that all linguistic change is externally driven - in most cases, for example, palatalization happens without any external cause - some kinds of linguistic change, and not just lexical, relate to events in the external world.

Because of this interest, a few years ago, another co-author, a historian, T. Spear, and myself produced a book on the history of the Swahili people. What we attempted to do there was put together a history of the coastal Swahili from ca. 800 to 1500AD.

This raises the question of how to do this: how does one write the history of a language and a community whose earliest written records date from only some three centuries ago? That could be expressed in more general terms. Africa is said to be home to some 1500 languages, which is said to represent about 25% of the world's languages. If we exclude languages spoken north of the Sahara such as Coptic or Arabic, none of those African languages were written down by indigenous people or by outsiders more than a very few centuries ago. Or if they were, no records have come down to us. That could also be couched more generally. We are used to work with languages such as a few in Europe, the Middle East, and India, a very few in Asia, and perhaps one or two in Central America, that have a written tradition or written documents that go back a millenium or more. How many are they? Out of the world's 6000 languages, only a handful go back two millenia or longer and a few dozen exceed one millenium. So writing a history covering a millenium or two or three for a language whose written records go back only a century or two or three has implications for most of the world's languages.

Parallel to the history of Swahili is the history of the coastal Swahili-speaking community. If one wanted to write a history of, say, the country or the nation that speaks French, that is, France or the French, where would one start and how would one set about it? One might decide to go back as far as, say, 800AD, because that is about when French documents start, when Charles the Great thrived. A linguist might find it hard to tell where Late Latin finished and French started, so might claim that France is an extension of Rome and that it is necessary

to go back to, 300BC, which is about when Latin documents started. In either case the writer would have access to, firstly, a long, continuous, and fairly substantial set of written documents by the community itself, secondly, another set of documents, by outsiders, which would be less detailed or substantial but nevertheless informative, especially for the earlier period, and thirdly, a fairly systematic and detailed archaeological record covering a long period. One would not have to rely on oral traditions to any extent, one would not have to rely on historical linguistics because the kinds of things they reveal are known already. We all know that Latin and the French replaced Celtic and the Gauls and that Latin grew into French.

None of that would be true for the Swahili-speaking community. Written literature in Swahili goes back just some three centuries, is locally based, and deals mainly with religious themes. It is not much help with uncovering two millennia of history. Accounts by outsiders are mainly also of local areas, and are geographically and chronologically spasmodic. Although they span nearly two thousand years, there are frequent gaps of several centuries, they refer only to a few places (Muqdishu, the Green Island, Rhapta, the Comoro Islands) that are often many miles apart and sometimes by names that no longer exist, and the detail they provide is haphazard and does not afford anything like a complete picture. The archaeological coverage is also incomplete and somewhat haphazard. The amount of money available to dig holes on the East African coast is paltry compared to that for other parts of the world. A small group of individuals has done splendid work over the last thirty or forty years and built up a basic archaeological profile of Swahili history, but I think all would say they are just picking at the surface of the task. More is undone than done.

Most other African communities are poorly served compared to Swahili and the Swahili. Most have no written records at all or have records that are even shorter than Swahili's. They are unmentioned by outsiders: as various outsiders sailed down the East African littoral from southern Arabia during the first fifteen hundred years AD, they must have passed dozens of discrete communities but not a single one is mentioned by name. Similarly, while an archaeologist on the Swahili coast can be reasonably sure of the general correlation between linguistic and archaeological profile, that is not true of most other places in Africa or the world. While archaeologists can be fairly sure in some cases of the correlation between, say, potsherds and language family a millennium or two ago, they are usually far from being able to identify with certainty the individual communities within a language family responsible for the sherds.

A source not much used by communities in the world that do have lengthy written records is oral traditions. Many African communities have - or did until they broke down in the twentieth century - oral traditions passed on from generation to generation, traditions often covering the events of the last several centuries. For the Swahili these were either traditions of individual towns or traditions of clans, or apocryphal stories, inspired by religion, of how they came from southern Arabia in general and Mecca in particular. For other societies in Africa, the traditions cover groups larger than clans. All are of limited use, partly because they explicitly cover only local areas or local groups, and especially because as historical accounts they are only reliable back for a few centuries (many observers would say maybe five centuries before present). Some cover events before that but then they are no longer "objective" accounts of history, nor are they intended to be.

Given this general situation, historical and comparative linguistics becomes more prominent as a tool for history. Distinguishing language families and their

subgroups is useful because it provides a framework on which historians can hang details of early events. It implies the existence of earlier communities which historians can elaborate. Consideration of the geographical distribution of language communities and language families gives broad hints about the probable earlier movements of peoples. Reconstruction of parts of earlier languages - mainly vocabulary - affords historians details of earlier culture and its possible sources. Analysis of which components of individual languages or groups of languages are inherited and which derive from contact with other peoples often tells historians about early events they only suspected or did not even suspect. I am constantly amazed how often historians accept statements by linguists about historical and comparative matters, and often without questioning them because they do not feel confident enough to deal with the methodology.

That is why historical and comparative linguistics is useful for historians in Africa and elsewhere, particularly for periods of history before the advent of written records. There are limits to its role. This is not Martha's Vineyard, or the Lower East Side, or Panama City, or Norwich, or Turkish communities in Germany, or the mobile women of Belfast, or a systematic dialect survey. We can never go into the linguistic communities that have covered most of the world during most of its history and conduct investigations into the progress of vowels or socioeconomic status or attitudes to language, nor can we repeat the investigations a generation later just to make sure change is on schedule. Social issues in historical linguistics in most of the world's languages are not the issues of contemporary sociolinguistics.

I would like to turn now to some of the lessons we have learned and have picked just a few out of a long possible list. Some are unique to Africa, some are familiar from work elsewhere in the world over the last two or three decades.

SOME LESSONS

3. IN PRACTICE ALMOST ANYTHING CAN BE TRANSFERRED IN CONTACT SITUATIONS. In what follows the general term "transfer" is preferred to "borrow/loan" to refer to any situation where linguistic material has moved from one community to another. Borrowing/loaning is a subform of transferring, usually resulting from transfer due to lengthy geographical adjacency. It used to be said there was a hierarchy of features that could be borrowed from one language to another, an order in which they were likely to be borrowed. Cultural vocabulary would be borrowed before core vocabulary, content words before function words. Vocabulary in general was borrowed before phonemic units, followed by phonetic processes, followed by phonological processes, followed by morphological processes in a certain order, followed by syntactic processes, marked features were likely to be eliminated in favor of unmarked features, and so on.

Languages which did not fit into this hierarchy were regarded as exceptional. One such language is Ma'a in Tanzania. It has a largely Bantu grammar and a largely Cushitic vocabulary. Over the last few decades many have tried to unravel the mystery of how this came about. The explanations are various but all used to agree that this was a strange case.²

We have started to learn in the last decade that it is not so strange. The process started back in the 1970s with studies such as that by Gumperz (1971) of the case

of Kupwar village in India, where several language communities have lived closely together for five or six centuries. Each language in the area has developed a local variant, and all these local variants share a similar or identical surface structure. The main differences are lexical. The result is that a person there slots one set of lexis into the surface structure if they want to communicate with people A and exclude people B, but if they want to exclude A and include B, they slot in a second set of lexis and so on. This is an effective and economical strategy and a model which can be usefully applied elsewhere.

Then came Thomason and Kaufman's book (1988, henceforth TK) which recognizes that historical collisions between languages are common and tries to set up a typology of what happens in different sets of circumstances. Ma'a is there and does not look quite so exceptional.

Most recently there appeared a book edited by Bakker and Mous (1994) called simply "Mixed Languages". Mixed languages, ostracized for many decades, have come in from the cold. It is becoming acceptable again to speak of mixed languages, as they appear more often. It contains 15 case studies of relatively unknown languages from all over the world. Ma'a is there and starts to look almost mainstream. One finds what one looks for. If one looks for regular and exceptionless sound change one will find it. If one looks for cases where what is most obviously transferred is vocabulary one will find them. If one starts to look for a different set of priorities, if one looks for mixed languages, if one starts to look for numerous cases of things other than vocabulary being transferred, they will be there. Highly marked features such as clicks or ejectives can be transferred, articulation habits can be changed, inflectional morphemes can be transferred, TA categories and systems, basic word order, and much else, can be transferred.

It might be objected that this new scenario is based on recipient languages which are small, languages with relatively few speakers - which would be true - and on donor languages which are large or larger, and so is not a model usable elsewhere. I think this is a non-objection. First, most language communities during most of the world's history have been relatively small: consider Britain which has a population today of just over 50 million, but had just over one million inhabitants a thousand years ago at the time of the Norman invasion - what if we went back another millenium or five or ten ? So these contact phenomena in a place such as Africa, where most communities were small until recently or are still small, are typical of most language communities during most of the world's linguistic development over tens of thousands of years. But second, from the work of Labov and others in big, very crowded, American cities, it is clear that change, once established, is spreading across large communities almost before our eyes. Size of community is not too significant. Or maybe the distinction between large and small communities is misleading. Any large community - the USA, or Pennsylvania - consists of a row of small communities. An individual or group of individuals regard themselves as members of several interlocking communities - a smaller local community, and some number of larger communities. Language is a badge of group membership. How quickly and how far change spreads reflects, inter alia, how speakers feel about membership.

In summary it may be true that there is a probability hierarchy of features that can be transferred. But it is not as fixed as we once thought and the incidence of features other than lexicon being transferred is higher than we once thought.

One question posed at the BLS meeting was: can the outcome of contact between two, or more, languages be predicted on the basis of linguistic inputs alone ? The predictive power of any model depends on how good the model is. Most of

our current models are based on an inadequate analysis of partially described or partially understood languages and situations. When we get to the point where our linguistic models are adequate, will we be able to predict the outcome of language contact? This is like trying to predict what English or any other language will look like three centuries from now. In purely linguistic terms, we can talk of probabilities. We cannot talk of good probabilities yet, because no one has put the contact question in those terms or sought an answer. TK, for example, examines many cases of contact and makes generalisations. That is different from trying to develop a predictive model. I think a probability model with good predictive value could be developed if we focused on that.

The obvious obstacle, however, to predicting the outcome of linguistic outcome is the non-linguistic component, the actual circumstances under which language or dialect communities meet. We know generally what the variables are: relative size of the communities, degree of bi- or multi-lingualism/-dialectism, language use, prestige of the languages involved, length of contact, and so on. Any particular situation contains its own particular mix of the linguistic and the non-linguistic variables and at present we are some distance from being able to predict the total outcome with any degree of certainty.

4. TRANSFER; BORROWING, LANGUAGE SHIFT, PIDGINS, CREOLES. Traditionally, through much of the twentieth century, the main contact model was that which led to "borrowing", although notions such as substratum implied the possibility of shift. What was not inherited was borrowed but, since the historical field was dominated by Indo-Europeanists, explanations relying on borrowing - other than of vocabulary - were regarded as dubious. The advent of pidgin and creole studies in the 1960s and the discovery of a growing number of cases unlikely to result from borrowing made these assumptions increasingly untenable.

TK outlines three or four types of contact situation: those that lead to borrowing, to language shift, to pidgins, and maybe to instant creoles. Since they say that after some time pidgins and instant creoles will probably be indistinguishable, that reduces the task of the historical linguist to looking for traces of three only.

TK raised at least two exciting possibilities - that of finding new cases of pidgins or creoles in areas other than those well known (West Africa, the Caribbean, Papua New Guinea, etc), and that of finding numerous cases of language shift. After all, during the decades prior to TK, new creoles had been found in Africa. It was known that the main dialect or form of several languages was or was until recently a pidgin and that many well known languages had pidgin varieties. Cases of language shift had been documented.

However, it seems to me that by and large the number of cases in Africa uncovered since TK and pointing unambiguously to pidginization, creolization, or language shift has been disappointingly small. In my own project, mentioned above, where I actively looked for such cases, I found it hard to identify any clearly. As far as I am aware, the literature in general on Africa in recent years has produced, and continues to produce, new cases of contact but explanations pointing to language shift or pidginization as the exclusive or main mechanism have been few.

If these observations are correct, then why is this? It may be that TK is a recent work, and that not enough time has passed for researchers to unearth new cases, especially in a continent where active field work is not expanding. Or it may be that we are looking at the wrong languages. In the case of my own investigation, for

example, I was aware of Bantu languages whose genetic affiliation had been clearly distorted and thought that examining them would lead to uncovering evidence of new historical situations. It did but it did not lead to clear cases of pidgins, creoles, or language shift. Or it may be that our general models are still not powerful enough and thus we still have to know the historical circumstances in individual cases in order to identify the correct transfer process - but we usually do not know them.

Or it may be that "borrowing" is simply more frequent than other forms of contact, or at least to other forms of contact that leave a trace. Language shift has occurred often in this world but for it to leave an identifiable trace, at least two conditions have to be met. One is that it is not enough for individuals or small groups to give up their language because typically such an action leaves no trace as the people involved sink anonymously into the mass of the host community. A large coherent group is a prerequisite. The second is that the shifting group must stay together as a cohesive entity for some time after the shift and retain something of the linguistic features they have brought with them. Thus, in North America, the Jewish community in New York and the Afro-American community are large and retain distinctive linguistic traces whereas the descendants of countless other millions of immigrants who moved as individuals or in families lost their former speech without trace. Likewise, pidgins are few because they derive from very particular circumstances which are/were relatively uncommon. By contrast, "borrowing" derives mainly from two - language or dialect - communities being side by side for a long time, this is a relatively common situation, thus borrowing is the commonest form of transfer.

Further, as pidgins develop into creoles and as creole systems become elaborated, it becomes increasingly hard or even impossible to identify their traces after the passage of some time, perhaps several centuries. Similarly with shifted languages. When a community shifts its primary language, the result will most often be a "dialect" form of the new language. As long as the historical circumstances are remembered, and as long as some record or form of the original language are kept, the origins of the new "dialect" will be recognized. But once the memory and the original language are gone, the new "dialect" will be wrongly thought to have derived genetically from an old protolanguage, just as the other dialects of the language.

J. McWhorter suggested (p.c.) that rather than looking for traces of fully blown pidgins or creoles, it would make more sense to look for traces of pidginization or creolization as processes, by identifying sets of features. In principle this makes sense but suffers from the practical drawback that the sets of features that characterize the different models often overlap and after a few millenia are hard to identify. One way to distinguish, for example, the results of borrowing from those of language shift is they follow different chronologies of transfer: for instance, structural transfer occurs rather late in the borrowing hierarchy but early in language shift. But since the historical circumstances surrounding most of the world's languages are unknown, and since they do not have written records, the chronology of transfer is a matter of conjecture. It is tricky to deduce from the linguistic facts. Finally, it may happen that several processes are involved. The small community at Brava in Somalia, which speaks Mwiini, the most northerly mainland Swahili dialect, has been surrounded by the much larger Somali-speaking community for a millenium or so. During that time, it repeatedly absorbed Somali-speakers - thus language shift - and was influenced by the surrounding community - so borrowing. There is at least a possibility that in earlier times it was used as an areal trade language - thus pidginized (Nurse 1991).

This perhaps overemphasizes the difficulties involved and sounds too pessimistic about the chances of success. Success is possible but it requires careful analysis of the linguistic features of a given contact situation and of as much as can be discovered about the real or assumed historical sociolinguistic circumstances.

A corollary to this is that our current models for analyzing historical language contact are inadequate and still evolving.³

5. CHANGE CAN SPREAD QUICKLY. We know now from the work of Labov and others that sound change can spread quickly across a monolingual community. A sound change or a set of changes, once established, can sweep across a community or communities in a generation or two.

Over the past few years I have been investigating various small communities in East Africa, communities speaking languages (Ilwana, Daiso, Sonjo, Mwiini) known to have undergone quite massive changes under the influence of neighboring languages. In some of these, not all, the period within which the changes took place can be established. In these languages changes can also apparently move quickly.

For two of these languages (Ilwana, Daiso) it can be reasonably shown that massive changes occurred in at most three centuries, changes so massive that if speakers of the two stages of the languages could be brought face to face they would hardly understand each other. While three centuries is of course more than Labov's "a generation or two", it is also possible that these changes took less than 300 years. It is clear that they could not have started before a certain point or finished after another certain point and that these points were 300 years apart but the changes may have been accomplished in part of that period.

Even if they did take as long as some part of three centuries, the total set of changes is much greater than that of Labov's vowel shifts. They include a radical enlargement of the phonemic inventory, a significant change of articulation habits, of phonological habits, a radical restructuring of the inherited TA categories using inherited morphology, and a host of smaller changes. In both these languages a large majority of their vocabulary has been borrowed (in one case over 50%, in the other 75%). All these changes push the languages involved in the direction of the surrounding donor language(s).

The changes also apparently differ from those of Labov and his colleagues in that they were externally induced by a "foreign" language.

It is apparently unimportant whether the source of the change is internal or external. Changes from both sources, or maybe change from any source, or maybe just "any change", can move quickly. In historical linguistics we do not necessarily have to assume that great changes took a great time. One thing that does need to be established is how far, across how large a community, or across how many adjacent communities, a change or a set of changes could spread quickly before running out of steam. A change that spread 100 miles in Western Europe or in post-Contact America would most often affect dialects of the same language whereas the same distance in Africa or pre-Contact North America would cross language boundaries.

6. THE TRANSFER OF MARKED FEATURES. An issue often raised in the literature is the fate of marked features in language contact. TK refer often to this and give a summary of the debate. A central difficulty is how to define the notion of

“marked” feature. Marked features are normally said to get lost in language contact. Consider two cases.

The first is Ilwana, spoken in north eastern Kenya, which at some point during the course of the three centuries just referred to, introduced two new features: a set of ejective consonants and a distinction between implosive and explosive stops. The second is the Bantu languages of South Africa, which at some point introduced clicks into their systems, where none had existed before.

From the viewpoint of the typologist, both these cases involve the introduction of sets of new sounds often considered typologically marked. Consider them rather from the point of view of the people speaking the language, people who had not heard of typology. In the one case, a community is surrounded by communities speaking languages with ejectives and having a distinction between implosive and non-implosive consonants. Not only that, there is good reason to think that the Ilwana became bilingual in the major - and prestigious - donor language. In the case of South African Bantu, two scenarios have been suggested. The traditional one is the offspring of Bantu fathers and Khoesan mothers being brought up by Khoesan mothers or nannies. Although the offspring eventually spoke the languages of their fathers, it is not surprising that they were much influenced lexically and phonetically by what they heard all around them as young children and also in later years as the same mothers or nannies would have continued to speak Khoesan. A newer suggestion (Herbert 1993) has (presumably) Khoesan-speaking women practising hlonipha in Nguni-speaking households: hlonipha is respect through avoidance, and in this case women substituted clicks in forbidden words or syllables. Both scenarios involve bilingualism over a lengthy period - Herbert speaks of "three to five centuries" of intense Bantu-Khoesan interaction.

In both cases, speakers were simply adjusting to their linguistic, their typological, surroundings. Whether or not these involved “marked” features, they were the local norm.

In thinking about all this, I remembered a conversation I had in the 1970s with a baker-poet in Lamu, Kenya. He spoke what was once a prestige dialect of Swahili, now encroached on by Standard Swahili. I was doing field work on Swahili dialects in the area and was rather naive. I expected linguistic purity, I wanted pure dialect forms. He allowed me to record some of his poetry and analyze it. When I found Standard, non-dialect, forms intruding, I reproached him with this and wanted to change it. He did not want it changed. He acknowledged the intrusion of Standard forms but said that his purpose was communication. He wanted to communicate with the people of his town, said they all increasingly spoken like that, and so be it. The mixture was becoming the local norm. I think he would have chuckled at some of our views of typology.

7. FAMILY TREES: WHAT IS SWAHILI (HISTORICALLY) ? Today’s Standard Swahili is a twentieth century development initially based on Unguja, a dialect of Zanzibar Island and a form of southern Swahili, one of the two historical branches of Swahili. Swahili in turn is one of the five or six branches of Sabaki, in turn one of the branches of the North East Coast (NEC) Bantu languages. Three nodes are involved: Proto-NEC, -Sabaki, and -Swahili. Within a group of related languages, a new branch is most reliably defined by some set of (non-lexical, phonological) innovations. We traced the line that leads down through these nodes to today’s Unguja and found that that line is hardly ever defined by its own innovations, it is most often defined by other parts of the group(s) innovating and by Swahili and southern Swahili having stood still. Both are historically largely

defined by what others did linguistically, not by what they did. Is this just an accident, or does it need an explanation ?

A branching in a family tree is normally taken to represent a language splitting into two or more, but how would it be interpreted in human terms, as a community dividing ? Or rather, if a community is already divided in some non-linguistic ways, how does subsequent linguistic change fit in ? One possible but speculative interpretation is via language as part of self identity. Part of how a community identifies itself as a community is by sharing some set of linguistic features, either internal innovations or transfers. This act of self-identification is not what induces the change in the first place but once the change has started, the community adopts it, as it were. Of all the NEC and Sabaki groups, that which spoke Swahili was demonstrably the most "successful". Could that be taken to mean "the most at ease with itself, the most content" ? If so, then perhaps it is a case of "let others redefine themselves by innovation, we stand here" ? Or maybe there is no connection between phonological conservatism and non-linguistic events ?

Another possible, more concrete and more plausible explanation might have to do with geographical isolation. Original Swahili settlements were all or nearly all on islands or tucked away by the shore, remote from larger mainland population centers. Change(s) which started on the mainland didn't reach into Swahili. Did geographical isolation simply lead to linguistic insulation ?

8. CORRELATING LINGUISTIC AND ARCHAEOLOGICAL PROFILES.

An approach that has been used in Africa in uncovering details of earlier historical periods is the juxtaposition of linguistic and archaeological distributions. Plotting the geographical distribution of linguistic groups and archaeological strata on a map sometimes shows remarkable congruity between the two. If such congruence can be found, we can tentatively assume identity between the linguistic group(s) and the people who left behind their material remains. Progress can then be made. Linguistically, we can talk about spread, either of people or of language, and we can reconstruct details of earlier culture through reconstruction of vocabulary. This can complement the much greater detail provided by archaeologists. Combined use of the two may enable us to date linguistic events.

In today's circumstances this methodology may prove to be appealing. In several parts of the world we see ethnopolitical change. What were large political blocks are being replaced by local nationalisms. These local nationalisms are assertive and in search of an identity, of which a central part is their own history. Often local memories are short and this local history has no written record and has to be constructed. Linguistic and archaeological evidence provides a way forward.

In Africa, this approach has had some striking successes. One is with language families in general in Africa. Despite disagreement about many details and sometimes about major issues, there are some solid achievements in identifying earlier archaeological-linguistic communities at the level of family. Another is with Bantu in general, where there is almost a general one-to-one geographical correspondence between the overall domain of Bantu languages and certain archaeological distributions. A third is with Swahili. The present and former distribution of Swahili-speaking settlements along the East African coast correlates well with archaeological evidence for a culture with a specific building style, economic mode, maritime connection, and eventually religion. That allowed us to link the two and thus reveal details of past Swahili society. There have also been some striking failures. The history of the Bantu-speaking peoples has been characterized by well meant misjudgements. Guthrie's linguistic work in the 1950s

and 1960s was used by historians.⁴ as the basis for positing prehistoric migrations. Other linguistic work done in the 1960s and 1970s was then used by other historians to support another model of Bantu dispersal.⁵ In both cases the linguistic picture and the historical interpretation have been largely discredited. Most recently, a new model of dispersal is based on a third set of linguistic data.⁶ Common features run through these and other juxtapositions. One is the quality and nature of the linguistic material. This consisted most often of classifications based on lexical measurement alone, and on incomplete geographical coverage. A second is the archaeological coverage, which also often suffered from incomplete geographical coverage. A third was that the relationship between different ceramic types, often central to the archaeological interpretations, appears to have been not always well understood. The fourth and major problem area was in interpreting the relationship between linguistic and archaeological evidence. At the macro-level, the correlation between linguistic and archaeological distributions has worked reasonably well. But how to interpret whether this related to the movements of people or languages ? Should a split in a linguistic tree necessarily be interpreted as involving a movement of people ? At the micro-level, how to relate pot types to peoples ? There is no reason why ceramic and linguistic distributions should be coextensive. And how to interpret the history of sites which have a long settlement history and were archaeologically and maybe linguistically mixed ?

Despite the caveats above, these are in fact exciting times for historical linguists concerned with the human component. New cases appear frequently, new ideas are evolving and need testing. Old languages are disappearing and need recording. There is much to be done.

FOOTNOTES

¹ Comments by V. Bubenik, L. Hyman, J. McWhorter, H. Muzale, H. Paddock, and M. Silverstein have caused me to modify the original text. To them my thanks are due.

² See Brenzinger, M (1987) and Mous (forthcoming)

³ The work done in Africa by C. Myers-Scotton deserves more attention than I have given it here, for example.

⁴ Oliver 1966.

⁵ Phillipson 1977 and 1989.

⁶ Vansina 1995.

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