

HISTORIANS, ARE ARCHEOLOGISTS YOUR SIBLINGS?

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The historian of pre-nineteenth century Africa...cannot get far without the aid of archaeology.¹

Nevertheless, historians have good reason to be cautious about historical generalisations by archaeologists and about their own use of archaeological material...: it would be a rash historian who totally accepted the conclusions of Garlake and Huffman with the same simple-minded trust as I myself accepted the conclusions of Summers and Robinson.²

I

In the beginning, historians of Africa put great store by archeology. Was its great time depth not one of the distinctive features of the history of Africa, a condition that cannot be put aside without seriously distorting the flavor of all its history? Did not the relative scarcity and the foreign authorship of most precolonial written records render archeological sources all the more precious? Did not history and archeology both deal with the reconstruction of human societies in the past? Was the difference between them not merely the result of a division of labor based on sources, so that historical reconstruction follows in time and flows from archeological reconstruction? Such considerations explain why the *Journal of African History* has regularly published regional archeological surveys in order to keep historians up to date.

Since then some disillusion has set in. First, in spite of all the declarations of principle, most historians are simply not interested in the results of archeology, and for the most part they remain unaware of what is going on in their sister discipline. Perhaps the last discovery that truly made an impact on them was the excavation in 1977 of Jenne-Jeno, because the locality became a city well before any Muslim North African influences were felt in West Africa.³ One does not have to look far to find reasons for this lack of interest. Most historians focus on more recent periods than archeologists, on issues other than material culture or technology, and few of them are knowledgeable enough about archeological practice to follow its literature effectively. Most historians feel hopelessly lost when reading debates about the fine print of the seriation of pottery styles or the different interpretations

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of C¹⁴ dates, which so often provide the intellectual excitement at archeological get-togethers. But the foremost problem may well be that historians have too touching a faith in archeology as a “scientific” discipline, and hence misunderstand some basic realities about it. Mesmerized by the observation that archeology deals with concrete objective data, they fail to perceive the role played by interpretation—and hence subjectivity—both in the recovery and in the interpretation of its data. Yet when former aficionados discover that archeologists are after all only human, they like Ranger, tend to be disillusioned and throw out both baby and bathwater.

But there is more to it than that, as attempts to blend an archeological reconstruction of an earlier period with a subsequent historical one reveal. Unexpectedly, these have tended to produce major dissonances at the juncture between the two fields. This situation suggests that perhaps archeological and historical reconstructions are mutually incompatible. A fine example of this situation is Andrew Roberts’ otherwise excellent *History of Zambia*.⁴ Its archeological chapters tell us about stone tools and ceramics, while the following historical chapters deal with ethnic groups, government, and trade. The subject matter of each part is so different that the two cannot dovetail: even a transitional chapter fails to weld the two parts together.

This seeming incompatibility is of course linked to the difference in the sources used by each discipline. *Mute* artefacts such as stone tools and pots are the bread and butter of archeologists, while few historians of Africa care about material culture. Most historians deal with written or oral *messages*. Most archeological findings document *situations*, while historians often focus on sources which document *events*. The characteristics of the sources they use obviously exert a strong influence on how scholars imagine their historical reconstructions, and hence on their basic assumptions and theories. Their respective reconstructions are difficult to reconcile because they incorporate differences more fundamental than a difference in subject matter alone.

The use of archeology in the reconstruction of African history is therefore far from being as straightforward as it might seem at first.⁵ One cannot simply borrow specific contributions. One must begin by considering the context in which they were made: the epistemology and the practices of archeology, a discipline in its own right. As long as historians overlook these issues, they will also remain unable to make good use of its specific contributions. Hence something must be said first about both theory and practice in archeology before one can turn to a discussion of its concrete contribution to African history.

II

By far the most common theoretical approach in archeology since the late 1950s is the neo-evolutionist theory developed by Julian Steward and Leslie White.⁶ Its fundamental assumption is that biological evolution, driven by genetic mutation, was succeeded by a multilinear evolution driven by cultural innovation: cultural selection followed natural selection. The mission of

archeology is to provide a detailed account of this evolutionary progress for the whole human race. Worldwide comparisons are facilitated by the notion of successive social stages from “band” over “tribe” and “chiefdom” to “state,” allied to the notion of universal technological sequences of development from “stone age” to “iron age” and beyond, as rungs on an evolutionary ladder of progress. Archeologists seek to discover “laws” (i.e., recurrent regularities) to explain how a more advanced stage emerges from a preceding one. Thus one seeks to establish the “origins” of chiefdoms, cities, or states by citing conditions recurrent in different parts of the world that are then said to cause the appearance of the next stage. Historical specificity and contingency are erased in this pursuit, although for Steward, at least, cultural evolution is supposed to be multilineal: i.e., different pathways of development can, in different parts of the world, lead from the same simple stage to the next higher stage of evolution. Needless to say, this approach strikes historians as profoundly teleological and hence antihistorical.

Most archeologists who work in Africa tend to downplay this approach. Yet it still provides an epistemological foundation. Even those such as Phillipson, who are not vocal in expressing support for the theory, are in fact still governed by it.⁷ Since Phillipson’s work is the most widely used and fully informed textbook in the field, it is the one which historians are the most likely to use. Hence it is well suited to illustrate the general assumptions and themes which underpin multilineal neo-evolutionism. The general processes of change it uses are evolution, technology, environment, migration, and diffusion. The core concept is evolution. Phrases such as “from simple to complex” and “from nomadic to sedentary to complex,” which were once quite common, imply an evolutionary view.⁸ For instance, “from simple to complex” presupposes a standard of measurement (complexity and diversity of technology or degree of sedentism, later degree of urbanization and state formation) which implicitly ignores all other aspects of culture or society as irrelevant. When used as a *leitmotif* recurring from epoch to epoch (i.e., chapter to chapter), such phrases depict a predestined future as if the outcome of human history could not have been different from what it has been.

It is telling that, when Phillipson rejects the use of “Late Stone Age,” “Neolithic,” or “Iron Age,” he sees them merely as ill-defined technological-chronological subdivisions, he does not reject them because they are units on an evolutionary scale.⁹ In fact his book cannot escape them altogether: chapter 2 deals with hominids and earliest toolmakers, chapter 3 with the Acheulian, chapter 4 with the Middle Stone Age and the Late Stone Age, chapter 5 with incipient sedentism (and pottery), chapter 6 with early farming (for which “Neolithic” often stands), and chapter 7 with “iron-using peoples.” Hence an index of technological innovation underpins his neo-evolutionism and succeeds to the index of biological speciation.

Once a technology is acquired, it is then inherited unchanged, just as an organ, once it appears, remains unchanged within a species. Usually invented only once at a single spot, like a genetic mutation, the technological innovation then diffuses from there. But once in a while “parallel evolution”

occurs, when the same technique is invented twice or three times as an identical adaptation to pressures of the environment, just as happens when similar organs evolve in different species under similar environmental pressure.¹⁰ Once invented, the technological innovation diffuses from its cradle because of its superior survival value; just like an advantageous genetic feature spreads over a number of generations to become dominant in a population.

And so from chapter to chapter epoch-making innovations succeed each other: from crude stone tools to better stone tools, ceramics (an index of sedentism), farming and the smelting of metals.¹¹ The outlook is so centered on these specific technological innovations that other material objects or technologies (e.g., weaving or basketry) are barely mentioned in such works of synthesis.¹² Once an epoch-making innovation such as ceramics, farming, or metallurgy has been introduced, it tends to be presented as perfect. The new stage has been reached and that is that. There is no further discussion of later changes in such technologies, nor of their variability over time.

Still Phillipson does reject an extension of the evolutionary paradigm beyond material culture. Unlike some others, he avoids treating urbanization and state formation as the next higher stages after "metals." He discusses "political centralization" and urbanization in his last chapter, but refrains from using these concepts as a *leitmotif*.¹³ In an evolutionary scheme the superiority of certain technologies over others is functional: they are better adapted than others to the particular natural and human environment. Phillipson argues in the case of Egypt, for instance, that the natural environment and its changes explain where, why, and when a complex society arose and why it exhibited its typical technological features.

No one doubts that the environment plays a major role, nor that some reconstructions involving environmental variables, such as the one proposed by Vogel for southern Zambia, are quite persuasive.¹⁴ Nevertheless, adaptation is too often used as a human Pavlovian response to a natural environment. It is a reductionist determinism because it leaves no room for a human choice based on a store of knowledge, for the serendipity of invention, or for fancies such as changes in stylistic decoration.¹⁵ After all, it is hard to accept that all changes in ceramic decor (for instance) are either designed for better prehension or as ethnic markers to increase social cohesion.

The spread of the new technologies is tacitly assumed to be proof of their superior efficiency in tackling a task determined by a natural or social environment. In a given place, the environment can produce an innovation as an independent invention, which is similar to a genetic mutation. When Phillipson's occasionally refers to "parallel evolution," he invokes a repetition of an independent invention of the same feature in response to the pressures of nearly identical environments. Yet features which are perceived as a quantum leap in technological complexity and efficiency, thereby constituting passage to a higher evolutionary stage, are held to have been invented only once, and to have diffused later from their cradle of origin over the whole continent. Most archeologists still favor this interpretation. The archetypal example for Africa has been the treatment of metallurgy: too complex, it is said, to have

been invented more than once, and too advantageous not to diffuse. In fact, that was Phillipson's initial position.¹⁶ By 1993 he hedged his bet by saying that "it was perfectly possible" that future evidence would demonstrate an independent development of metallurgical technology south of the Sahara, but that unfortunately the C¹⁴ dates for this time period are "exceptionally uncertain."¹⁷

The spread of technological innovation used to be attributed primarily to population migration rather than borrowing, perhaps by analogy to the "survival of the fittest." Migration appears in the archeological record as a clear break between two successive occupations of a site, accompanied by major differences in the shape and assemblage of objects found on different occupation levels. Yet what exactly constitutes a major change that could not be explained by a series of gradual internal changes, and hence must be attributed to migration, remains a matter of interpretation.

Appeals to migration as an evolutionary device were exceedingly popular until recently.¹⁸ Now, though, most archeologists reject migration as a facile *deus ex machina*. Wherever possible, the record has now been reinterpreted as the result of borrowing or even parallel independent invention. Migration has become the principle of last resort—only applied to cases that cannot otherwise be explained. Thus all archeologists still postulate migrations from an increasingly arid Sahara into West Africa, although no longer as a dramatic mass migration, but rather as a gradual drift of small communities occurring over very long periods of time.¹⁹

Phillipson still postulates a single rapid Early Iron Age mass migration from eastern to southern Africa. He stresses that the "sites and artefacts" of his "Chifumbaze complex" contain the first evidence for "the cultivation of crops, for the herding of domestic animals, for settled village life, for metallurgy, and south of Tanzania, for the manufacture of pottery." All these features appear simultaneously and form a package carried by the migrants.²⁰ Yet this interpretation can be challenged: perhaps all the features were not simultaneous, and perhaps the ceramics do not all derive from a single prototype style.²¹ But apart from this, Phillipson, like the vast majority of his colleagues, favors borrowing rather than migration, and thus stands opposed to a minority, which still argues for migration rather than borrowing in other cases.²²

Innovation, of course, can also spread by borrowing from neighbors, a process for which there is no direct parallel in natural evolution: genes or organs are not borrowed. Perhaps for this reason it was unfashionable until recently to invoke this process, except as an interpretation of last resort. That situation is now reversed. Borrowing is now interpreted not as a passive undergoing, but as a variant of active internal innovation, which is triggered off by a functional need to adapt better to changing human or natural environments. Hence arguments for borrowing are accompanied by suppositions concerning the functional usefulness of the feature borrowed, despite the fact that, while this approach may well apply to some items, it is

patently untenable *as a general rule*. What, after all, is the *functional* usefulness of a borrowed musical instrument or melody?

The change in emphasis from migration or passive borrowing to internal innovation is one of the results of the rise of the New Archaeology, a term which refers to a new goal: to go beyond a narrow interpretation of artefacts to elaborate hypotheses about the organization of the societies which apparently created them, by constructing and testing models based on general systems theory, locational theory, and sometimes game theory.²³ S. Keech McIntosh has been particularly enthusiastic about the use of models. In a single paragraph of a recent article, she mentions with regard to the Inner Niger Delta a cooperation model, a coercion model, Central Place Theory and Peer Polity Interaction Theory.²⁴ She is not alone among English-speaking archeologists of Africa.

The impact of the New Archaeology on southern Africa has been spectacular. There studies of the typical layout of settlements and of the hierarchies of settlement have led to the construction of well-grounded models to describe both rather egalitarian communities and the structure of centralizing societies.²⁵ Several attempts were also made to understand the ideological underpinnings of early societies better. Even though the reconstruction was not accepted by many scholars, still a bold reinterpretation of various parts of the urban sites of Zimbabwe in terms of a presumed social system and its ideological (symbolic) concomitants remains a remarkable instance of such efforts.²⁶ The appeal of the McIntoshes to interpret the meaning of features in terms of a common "symbolic reservoir" goes in the same direction, and has also run into opposition.²⁷

Recently the application of locational analysis in Southern Africa has led to a direct clash with neo-evolutionary archeologists. Denbow and Wilmsen stressed that San-speakers had been part of herding and farming societies for the last two thousand years, and that the behavior of San foragers today is therefore not a valid source to infer paleolithic behavior, a thesis that is still bitterly contested.²⁸ The acrimony of the debate indicates how deeply embedded neo-evolutionary premises remain in the epistemology of African archeology.

Most archeologists may well feel comfortable with the neo-evolutionary approach because it allows them to construct a single narrative encompassing the whole world and encompassing what used to be called "the ascent of man," and hence to reinforce their place in the wider discipline of anthropology. Yet another reason is certainly as important—in this scheme of things gaps or lacunae in the record are only a minor hindrance. One looks for epochal innovations and orders them in sequence of complexity. That yields the ladder of ineluctable and irreversible progress. Later finds will either add information about the spread of diagnostic evolutionary items or can even lead to the discovery of hitherto unsuspected intermediary rungs on the evolutionary ladder.

In any case new discoveries do not require gut-wrenching revisions of the whole narrative edifice that had already been constructed. Consider, for instance, the discovery of Jenne-Jeno in this light. All it means is that parts

of West Africa had reached the urban stage a few centuries earlier than foreseen and that the idea of a city had not been borrowed from Muslim North Africa, but should now be attributed to parallel evolution already well-documented from various other parts of the world. In Phillipson's first edition Jenne-Jeno appears as a site.²⁹ In the second all that was needed was the addition of a paragraph saying that it became an urban trading center.³⁰ In contrast, the discovery has had an impact on historical reconstructions for the first millennium A.D. to such an extent that even now its significance has yet to be fully assimilated.

The theory of multilineal cultural evolution influences nearly all archeologists working in Africa, even though many among them, like Phillipson, are distancing themselves from it.³¹ Yet very few English-speaking archeologists have systematically rejected this approach by championing or inventing an alternative theoretical position, although one position labeled "direct historical approach" is now being developed.³² This viewpoint is inspired by the practice of historical archeologists—scholars working on sites also known from written sources, including most archeologists of classical antiquity. These scholars tend to adopt assumptions typical for historians—the importance of contingency and the specificity of change.³³ In African archeology, however, only Jean Devisse, a historian who turned into an archeologist, has elaborated this stand.³⁴ For him the role of archeology is to document the processes which created the historical landscape and background for the thoughts and actions of human agents living at a given time.

The theory underlying this view is that of the *Annales*. Any moment in history is the product of various temporal oscillations, including slow movements or *longue durée*, such as climatic or demographic change which provide a seemingly (but not really) stable matrix for faster changes (*conjonctures*) such as economic trends, which in turn are the matrices for current events. Thus, an incident in the history of the gold trade between West and North Africa, e.g., the striking of a gold *dinar* in Sijilmasa, must be understood against the backdrop of the trade at that time, itself part of a long nearly two-thousand-year-long story of the ebb and flow in gold production and trade. Ore bodies, mining technologies, labor processes, trading institutions, ideologies involving gold, political control, hoarding and the creation of wealth, currencies, and salaries are all involved, and all together illuminate an understanding of each of these features.³⁵

Secondly, Devisse recognizes that the concrete material character of artefacts has a special value. Archeological objects are concrete bits directly out of the past—not reconstructed—even if they also remain mute. The features of such objects cannot be generalized away, and each object can become the subject of a wide variety of technical investigations. Archeometry helps to unlock knowledge about the past hidden in the structure and makeup of objects.

Thirdly, for Devisse history is never finished. Not only does each age pose new questions and reinterprets the past, but new techniques and new

discoveries continue to expand the body of evidence. Hence one should not propose a single historical reconstruction—let alone adopt a single model—to account for the now-known evidence as long as a series of other plausible hypotheses can also explain the situation.³⁶ The scholar should put all these potential explanations on the table and test them by further excavation or laboratory work until most of them are weeded out. Then, and only then, can a reconstruction be proposed.

It is instructive to contrast the plots of Phillipson's or Shaw's books with that of the catalog (a huge book) of Devisse. The division by stages, a chronological progression, and an overarching narrative are all absent in the catalog. Devisse sets the stage by plotting the "human geographies" of successive ages. Lucien Fèbvre could not have done better. He then illustrates the present state of research by discussing methodologies, presenting case studies, and underlining specific achievements, failures, and problems before tying archeology and history together by a presentation of studies about objects from a past also documented by written records.

This work is about the state of research. Rightly, the reader learns much about the practical work of the archeologist, from surveys to archeometric analyses. At the same time the intertwining of long-term change with shorter-lived trends and nearly ephemeral events is apparent throughout, so that the reader sees how the rope of history is braided. Remarkably, this catalog by a historian pays much more attention to various sorts of artefacts than the archeologists Phillipson or Shaw do in their general books. They focus only on ceramics and the smelting of metals as evolutionary indicators. Thus, for instance, the unique collection of objects (textiles, ceramics, woodwork, leatherwork, metal objects) found in caves on the Bandiagara cliffs, many of which have now been studied in depth, are given the ample space they deserve in Devisse but do not rate a mention in either Phillipson or Shaw's volumes.³⁷ It is as though, unlike historians, archeologists were not interested at all in reconstituting the daily circumstances of life as a major goal.

Historical archeologists have worked for the most part in northern and western Africa and in the coastal cities of east Africa, and their work had no effect on practitioners of the theoretical mainstream. Indeed, the latter often reacted with hostility to the exertions of the former. Thus Phillipson found it necessary to add the following critique of research in West Africa in the new edition of his textbook.³⁸ After stating that a coherent overview of iron-using peoples in West Africa is particularly hampered by the very incomplete and uneven coverage of the research, he continues:

Such research as has taken place has been concentrated on sites which have yielded art objects or which are connected with the trading states mentioned in foreign written accounts. There has been virtually no integrated investigation of technological and economic development, or of state-centralisation processes, although pioneering work around the Inner Niger Delta amply demonstrates the potential for such work. As a result, whole areas—especially in

the western part of the region—remain effectively unknown archaeologically.³⁹

Passages like this reveal how deeply entrenched neo-evolutionism remains, even in the thought of those who have begun to question it. What are these “integrated investigations” and what is meant by “economic development” and “state-centralisation processes?” The charges themselves sound rather hollow. The Bandiagara caves, for instance, were neither renowned for their art, nor connected with trading. The study of various technologies, ceramics as well as of other objects, is certainly as advanced in large parts of West Africa as elsewhere in tropical Africa, while surveys have been as thorough in parts of West Africa as in southern Africa.⁴⁰

One should not attribute the differences in approach to a Anglo-French rivalry among archeologists, because this quarrel is so obviously about the goals and methods of the discipline, opposing historical to neo-evolutionary approaches, even if in Africa the first tendency is more common among French-speaking archeologists and the second more so among English-speaking scholars. As to methods, Devisse, along with nearly all archeologists trained in France, rejects most of the tenets of the New Archaeology, including the use of a general model as an explanation. As to the goal, the peroration of Susan Keech McIntosh—“In the future we can expect that West Africa will continue to offer highly original insights into the development of archaeological theory and culture process”—is but an annoying irrelevancy for them.⁴¹ For anglophones, archeology is part of the wider discipline of anthropology; for many francophones and most other European scholars it is not.

III

It is just as important for a reader of archeological accounts to be somewhat familiar with the practices of the discipline as it is to know something about its epistemology. While it is correct to think of archeologists as people who find and excavate sites, it is quite misleading to imagine that this is all they do. The process of research begins with funding requests, which are justified either by the claim that the area to be studied is “virgin territory” or, more commonly, that further work at an already-known site or at new sites in a known area will throw further light on a well-known general problem. Archeological research is much more expensive than historical research and therefore the level of approved funding also shapes the outcome much more—for instance, by limiting the time available for surveys or digging, the size of an individual dig, the number of experts who can be taken into the field, the diversity and the number of laboratory analyses that can later be undertaken, and the planning for excavations extending for several years in the future.

Unlike funding provided for research in the Middle East or in Middle America, most of the work undertaken in tropical Africa has been carried out

on the proverbial shoestring. Adequate and long-term funding has been available only for very few sites such as Tegdaoust in Mauritania; Kilwa, Gedi, Manda, and Shanga on the east African coast; and (in installments) for Zimbabwe. Everywhere else funding has left the indelible stamp of inadequacy, whether at urban sites such as Gao or Kumbi Saleh, or at humble places in eastern or central Africa, where only a few cubic meters per site could be dug up.⁴²

Once funding has been secured, survey or excavation can take place. Any place where human artefacts are visible is a potential site (except for the occurrence of a single isolated object), while a confirmed site is one where excavation has taken place. Sites are not necessarily permanent settlements and historians should beware of treating them as such. Nor are maps indicating sites directly comparable to historical maps. Many a historian has been puzzled by the fact that once familiar places on an older archeological map are no longer listed on a later map, where other places now appear. Each map simply reflects the state of play at the time the map is made: hence the saying that sites do not exist because they are an artefact of archeological research.

Typically, archeologists, having heard about traces of old human activity in an area tour several potential sites, then look at the visible remains or conduct auger tests and choose a site for digging. This choice is often quite subjective, and all sorts of variables play a role. A Late Stone Age specialist chooses sites where typical stone tools abound, a ceramic fiend one where the diversity of shards is thickest on the ground. One person will be attracted by the potential to recover trade goods, another by organic remains (often in very wet or very dry conditions), others by traces of slag or trash heaps or living floors or bones, all depending on their evaluation of the potential of the site to contribute to the solution of one or another outstanding question in the professional literature. Digging techniques can also differ very much according to the type of site: living sites on open terrain,⁴³ cemeteries,⁴⁴ caves,⁴⁵ hoards or single structures,⁴⁶ industrial sites,⁴⁷ mounds or tells,⁴⁸ urban sites,⁴⁹ even underwater sites. All sorts of other variables such as soil conditions and climatic conditions, which can be crucial for the preservation of remains, as well as the topography of the site itself, must be taken into account. Readers of site reports must be aware of such conditions in order to assess what was recoverable at the place excavated and how orderly the recovery could be.

Once a site has been chosen, digging can proceed. Nowadays scholars use standardized approaches in setting up a grid for reference and in digging according to arbitrary geometric shapes (often cubes in square meters, separated from each other by berms) or by following visible structures such as walls, pits, furnaces, or graves. Such techniques facilitate stratigraphic control and indicate the exact location of each artefact found, including its association—or not—with others. It is well-known that stratigraphy and relations between features are of paramount importance for the later interpretation of what the site was, and that they should be fully discussed in the site report. Less often realized is the fact that the actual size of the excavations in relation to the whole known surface occupied is also quite important, as is the actual

number, and not just the diversity, of artefacts recovered from the site. Thus the McIntoshes claim that there was no elite center in early Jenne-Jeno because they found none, but the proportion of that site that was excavated by them is so small that their negative finding cannot be taken seriously.⁵⁰ Claims for an exceptional importance of the finds at Twickenham Road in Zambia are not substantiated because of the exiguous size of the site, its poor stratigraphic conditions, ambiguous dating, and the tiny scraps of most artefacts actually found there.⁵¹

In the early days most sites were found accidentally. Eventually though, and especially during the last decade, scholars have been conducting systematic surveys to trace the potential sites extant in a given area and thus to reduce some of the serendipity associated with earlier finds. Unfortunately such surveys are not often published in detail, nor are they mentioned in textbooks or syntheses, yet they are crucial. For instance, thanks to a plant, *Cenchrus ciliaris*, which prefers to grow on vitrified dung and which shows up on aerial photographs, Denbow was able to locate some 80% of all potential sites of former cattle kraals over a large portion of Botswana.⁵² The stone walls of settlements in much of Transvaal and Natal also show up on aerial photographs, and again have allowed a very high percentage of potential sites to be identified.⁵³ Anthropic mounds in the Inner Niger Delta have now also been mapped.⁵⁴

Surveys are now common in many parts of tropical Africa, although altogether they cover only a small fraction of that huge territory. Surveys are crucial because they help to indicate how representative of the whole an excavated site actually is. In addition they uncover correlations between site distribution and natural features such as soils (fertile or not), topography (e.g., hill refuge areas), ecotones (transhumance), and bodies of water, thereby yielding clues as to the predilections and occupations of the populations involved. Thus in southern Uganda sites with Urewe ceramics occur only on fertile soils, suggesting that the people who used those ceramics were farmers.⁵⁵ Sometimes they also show hierarchies of sites which can then be confirmed and extended by further excavation. These techniques have now proven to be very successful all over southern Africa.

Mapping potential and excavated sites is also a first-rate tool to show exactly how extensive lacunae actually are, and how considerable spatial extrapolation from the findings of a *single* excavated site actually is. Such spatial extrapolation varies from a few kilometers, as in the Nile valley of upper Egypt in the case of late predynastic or dynastic settlements, to huge distances in central Africa. Recently some authors have postulated a "western Bantu stream" in this area, which links Benfica near Luanda to northwestern Botswana or the upper Zambezi valley without any intervening site at all, a long 800 miles as the tireless crow flies!⁵⁶

What an excavation yields is in part conditioned by earlier expectations: first, the choice of the site itself and then the tactical choice of how and what to dig up. The case of the east African coastal cities has become famous. Until recently digging there focused entirely on the remains of stone buildings in the

belief that these cities had been built by foreign merchants. Yet the results did not show much of a city plan: on some plans a stone mosque is a mile or so away from a "palace" itself and just as far away from the nearest stone house.⁵⁷ The plans fairly shouted at one that something essential was missed. Yet biases were so strong that the obvious inference that there had to be other types of dwellings filling up the missing spaces was not drawn.⁵⁸

Similarly, until recently scholars in eastern or southern Africa expected a site to yield only stone tools, pottery, charcoal, and perhaps a trace of metallurgy, imported beads or shells, and some bone. Once soundings or small excavations yielded the expected stone, pottery and charcoal, the dig was considered a success and one did not continue with it. As a result much of the available record for most of Zambia for instance (southern province excepted) still remains quite limited. Archeological interpretation for the "Iron Age" in Zambia therefore naturally came to be based almost entirely on an interpretation of ceramic styles which in turn reinforced the actual excavation practices that had produced this over-reliance on ceramic style to begin with.⁵⁹

Expectation and interpretation thus reinforce each other and drive archeologists to propose ever-more far-fetched links between ceramic style, the language of its makers, artists, and a host of social and cultural features.⁶⁰ Part of the difficulty Roberts encountered in his *History of Zambia* was due to precisely this situation. The straw of the archeological record did not suffice to make bricks. Where were plans of whole settlements or even of single houses, reports on old land surfaces, vegetal remains, analyses of human bones for information on diet and disease? Where were the laboratory and comparative studies on other artefacts, such as the metal objects which had been found? No doubt financial constraints played a large role in the slow adoption of new techniques of digging such as wet-sieving or flotation to find plant remains or excavating large surfaces, but one also suspects that scholars here became only gradually aware of what more could be expected from the humble sites of the region.

Any reader of archeological literature must be keenly aware of continuing technical innovations in the field. The archeologist is like the conductor of an orchestra composed of geologists, chemists, soil specialists, paleobotanists and zoologists, human biologists, specialists in ceramics, metallurgists, specialists in textiles, and as many more as the score being played requires. He or she turns over much of what is found at a site to such specialists for archeometry. What laboratory work actually is done and how well it is done depends first on the availability of funding to finance all the work. But it also depends on the imagination and the predilections of the digger. It is the archeologist who calls in other specialists for consultation, and it is the archeologist who must know beforehand what others can do with a bit of bone, a hank of hair, or a handful of dust. In practice that knowledge is informed both by what has been done hitherto and by the particular interest of the archeologist in question. Thus someone interested in intercontinental trade will be well aware of what can be done with bits of glass, while someone interested in diets will know which specialists can deduce what from bone and

how they do it. Thus a good deal of subjectivity is involved in the choices made of what to send for analysis and for which kind of analysis. Anyone assessing site reports should be aware at least of which analyses seemed obviously to be called for and which ones were actually carried out.

In the fullness of time excavation reports are published—usually. They indicate how the excavation proceeded, what was found, what laboratory analyses revealed, and they conclude with an interpretation of the evidence produced.⁶¹ Reasoning from stratigraphy and interpretation establishes whether the site represents single or multiple occupations and what associations of artefacts and features occur at each level. A chronology is established usually by C¹⁴ or by thermoluminescence dating claimed to be in close proximity to a given association of artefacts at a given level.⁶² Then follows a discussion of the relationship between successive occupations to make the case either for continuity or for a break between successive occupations by assessing the stratigraphy, by evaluating the degree of similarity between successive assemblages, or by considering similarities and differences in successive ceramic styles. Often the evidence can be interpreted in several ways, depending on how much one is willing to credit gradual internal innovation. Frequently, theoretical preference or some other bias of the researcher decides the choice of interpretation.

Beyond this, reports make inferences in three major ways. The findings at one spot are often extrapolated to a more or less large area surrounding it. The presence and quality of available site surveys and other excavations allows one to evaluate the extent of the extrapolation involved.⁶³ Secondly, one infers from the presence of one feature that others existed as well, because the feature found belongs to a supposedly indissoluble complex of objects. It has, for instance, often been concluded from the presence of pottery shards that the inhabitants of the site had been sedentary (“nomads have no pots”) and were farmers. When these are found without any stone tools being associated with them, it has sometimes been further deduced that these putative farmers were also users of iron, even if no remains of iron occur.⁶⁴ Such extrapolations from present to absent features very often are unwarranted.

Thirdly, artefacts are imbued with significance by inference from recent or contemporary practices and from recent experiments, e.g., in stone knapping or the smelting of metals. This approach has become common enough to give birth to a new specialty called “ethnoarchaeology”⁶⁵ Such extrapolations deal with the manufacture, use, or even the symbolic meaning of objects.⁶⁶ The danger of anachronisms and of a relapse in neo-evolutionary habits of thought are obvious, as illustrated by a famous interpretation of a Saharan rock painting in which the Fulani scholar Hampaté Ba recognized a Fulani initiation scene. The six thousand-year gap between the date of the painting and the Fulani practice observed has not prevented scholars from accepting his interpretation, and to derive the conclusion that Saharan rock art was executed by the ancestors of the Fulani!⁶⁷

Inferences from objects to social conditions are not unusual either. Rich grave goods in some graves rather than in others point to the presence of

elites; walls or ditches allow for calculations as to the amount of labor needed to build them and from there, assuming a set time to execute the work, to the size of the labor force needed, and from there to the size of the population and the scale of social control available to leaders in that society. Or the presence of shards on a site with stone tools leads to the claim that foragers there were clients of farmer patrons from whom they obtained the shards.⁶⁸

Site reports then are not easy to assess. The choice of the site dug, the excavation technique used, the placement of the pits or trenches, the choice of laboratory analyses carried out or omitted, the association of features, the stratigraphy, the extrapolations and inferences made are *all* sensitive to potentially systematic bias, while the proposed interpretation may or may not be as plausible as alternative interpretations. It takes considerable familiarity with the relevant literature truly to understand such reports and to be aware of possible alternative interpretations. A site report then—always and unavoidably—includes a subjective component. It is no more fully objective than a historical monograph is.

This is of course even more true for syntheses which include several site reports. Interpretations at this level include the three types of inferences mentioned. Some of these seem quite persuasive. Thus the presence of a hierarchy of sites accompanied by systematic differences in the presence of luxury goods and in diet between various settlements have led to what appears to be a well-founded claim for a large-scale and fairly centralized society there. The claim is well-founded because it is based on a thorough survey, has been tested by examining further evidence, and includes both differences and similarities between assemblages at different sites.⁶⁹ Moreover, the proposed reconstruction is fruitful because it has led subsequent researchers to find its limits and to elaborate on it.

Similarly, the presence of a settlement plan in which similar circular houses surround a central circular kraal (the so-called SBCP pattern) from the late first millennium A.D. onwards recalls the plans of recent Nguni and Sotho settlements and has led to attributing basic features of Sotho and Nguni social organization linked with such plans to these early populations. This may well be so, but the proof so far is less substantial than in the previous case.⁷⁰

In another case the unusual distribution of ancillary potential sites around Jenne-Jeno, linked to the absence (hitherto) of an elite center in that city, has been tentatively attributed to the existence of casted and cooperative, but non-centralized, societies in the Inner Niger Delta. The case is much weaker than the preceding ones because the links between the social organization postulated and the spatial distribution of the archeological features is much less evident and, as was mentioned earlier, because the still-limited scale of excavation does not allow one even to be certain that there was no elite center at Jenne-Jeno.⁷¹

Attempts to infer social organization from archeological features have unfortunately not been limited to plausible links relating to the distributions and plans of settlements. A recent fashion—without, I believe, any

plausibility whatsoever—involves the deduction of social organization and associated cultural features from ceramic style. The premises are: (i) a ceramic style is representative of the whole stylistic *corpus* in a given culture; (ii) graphic style, a mainly conscious but arbitrary system of signs, is closely correlated with language, another arbitrary but unconscious system of signs; (iii) language shapes world views; iv) worldview includes a precise type of social structure. Hence ceramic decor allows one to differentiate between languages, ethnicities, social structures, and ideologies. Once the language involved with a particular style has been identified, all the rest follows.

Language can be linked to sites from long ago by plotting the present distribution of languages and language families on a map of sites. From language, ethnoarcheology then leads to specific social and cultural features: *X* is matrilineal, with an initiation for girls, bridewealth service, an organization in chiefdoms with titled officials, and a belief in both ancestors and nature spirits...whereas *Y* is patrilineal, with a boys' initiation, bridewealth in livestock, a segmentary lineage system and an ancestor cult, capped by a High God. Apart from the first premise, which is often correct, none of the others holds at all. Even the cherished link between language and worldview (the Whorf-Sapir hypothesis) has been totally discredited. If this hypothesis were correct, how then could we expound a complex worldview in any language in which we choose to do so? The lesson is that one cannot milk ceramic evidence for more than it is worth.

Potential bias is not only as common in archeology as in history, but just as diverse as well. Theoretical bias has been highlighted in this paper only because historians may not be aware of it. Among others, the effects of the many familiar -isms, such as colonialism, nationalism, and Marxism are easily detected.⁷² And so are various familiar idiosyncratic preferences with which historians are familiar from their own experience. There is therefore no need to expatiate. But it is useful to draw attention to a disguised expression of bias which appears as a dispute over C^{14} dates. At first it looks as if frequent disagreements over dates flows naturally from the uncertainties of the method: the source of the carbon, its association with artefacts, the statistical frequency of false results, the number of samples required, the bracket of time (the "resolution") involved in the date and the accuracy of the corrections required to convert a radiocarbon date to a calendar date. On the surface the arguments for acceptance or rejection of a date are always highly technical.

But it soon becomes evident that larger issues often lurk behind such debates, because disagreements among scholars do not occur at random. The acceptance or rejection of dates is conditioned by prior expectations as to what is acceptable or not. For instance, French scholars systematically accept earlier dates for iron-smelting both in West Africa and in the Great Lakes area than their English-speaking colleagues do, irrespective of the nationality of the archeologist who obtained such dates. Indeed, some English-speakers have discarded early dates which they acquired themselves, claiming that the material must have been intrusive or that there was a laboratory error. Perhaps the French have been more influenced by African nationalism and the English-

speakers more by neo-evolutionary theory? Be that as it may, bias is certain. It cannot be an accident that almost every early date proposed by one group is dismissed by the other. One who follows the debate closely discovers that the reason for accepting or rejecting proposed dating usually is that they fit or do not fit with the chronological bracket that seems "reasonable," given a belief that there was—or was not—diffusion involved.⁷³ Precisely because debates about chronology are both reasonable and frequent, they should attract attention as a litmus test for bias.

The relentless exposé of the subjectivities involved in archeological theory and practice is not intended as a dismissal of the discipline, any more than a similar exposé about subjectivity in historical research would be. The task is necessary in order to understand the contributions archeology can make to African history. If Africa archeology can and has documented such subjects as long term *climatic change*, including short term oscillations, whether anthropic or edaphic in origin; *population* (growth, spatial distribution, stability, and the layout of settlements) and by inference about the economy, the spatial scale, and the degree of stratification of society; *technology*, including food production and by inference labor processes and the daily rhythms of life; *diet and health* (from the analysis of human remains and kitchen rubbish); *material standards of living* as well as the movement of goods and, by inference, the social and economic uses of goods, trade, and contacts between communities; *ritual practices*, usually funerary, but sometimes dealing with ritual localities or objects and, by inference, some elements of ritual, ideology, and the social uses of wealth; *art history*, including style and iconographic material rendering attitudes, costume, or various objects and, by inference, conclusions about a wide variety of human activities and aspirations.

This whole range of data is almost never available on a single site or small set of sites, however. In addition, archeology does not directly contribute data about the non-material aspects of human thought and activity such as ideology, ritual gesture, or the practices of social organization. Even many economic features such as manual labor motor habits are not documented. True, further inferences can be made about some of these features, but unsubstantiated inferences alone are conjecture, not evidence. Because artefacts are mute objects, not messages, they do not allow for a reconstruction of history in the same way as written documents or oral data can—a history without named agents, much less detailed than messages, yet also more direct and more concrete. Therefore historians should not expect a reconstruction similar to the one that messages allow for. Yet for all that, they should not underestimate the contributions of archeology either, as they have also done.

IV

Specific archeological findings expand existing historical reconstructions; they require a re-evaluation of such reconstructions or they allow

reconstructions to be made for periods for which there was hitherto insufficient evidence. Using mainly data which have accumulated over the last decade or so, we examine such contributions first for inland West Africa during the long first millennium A.D. (from *ca.* A.D. 1-*ca.*1250), and then for eastern and southern Africa for the half-millennium from *ca.* 750 to *ca.*1250 A.D.

In the western part of the western Sudan new findings concerning the long first millennium A.D. are gradually leading to a complete re-evaluation of the previously accepted historical reconstruction for that period. At the outset of our era the long population drift from the southern Sahara into the lands south of the Senegal river and the great bend of the Niger was ending, while both the Middle Senegal Valley (MSV) and the Inner Niger Delta (IND) were being settled.⁷⁴ Agriculture and pastoralism were in general use, while iron-smelting and ironworking were practiced in most areas. As far as this goes, these data mostly strengthen existing views, but the historical picture that results still remains quite sketchy. Little is known, for instance, about trade and communication, both within various regions of the western Sudan and among them.⁷⁵

Until recently very little was known about the first three-quarters of the period, apart from the gradual growth of a walled town at Jenne-Jeno.⁷⁶ Recently that situation has changed dramatically. First there was the discovery of an impressive necropolis at Asinda-Sikka (Bura) in Niger, which was in use at some time between 200 and 1000 A.D.⁷⁷ This site contained some 400 large figurative ceramics. Other sites, some with ceramics as well, tell us that the nearby middle reaches of the Niger valley were also inhabited during the same period. Although the ceramics at Asinda-Sikka and other sites have not yet been studied in detail, it is evident that the society which used this necropolis was complex and rich. There are indications of pronounced social stratification, of different "ethnic[?]" markings, of the importance of cavalry and of a wealth of jewelry, some of which was made out of cupreous metal.

All of this comes as a total surprise to the historian. Art historians will be fascinated by the cocktail of styles and the number of sculptures which fill a period between late Nok and the mostly undated—but presumed to be later—figurative ceramics from the Inland Niger Delta and upstream. To me the most significant result of the finds in Bure is that some of the styles recall those of the coeval figures at Yelwa, some recall coeval or later Inner Niger Delta figures, and some remind one of the subactual funerary potteries near the coasts of Togo and Benin.⁷⁸ Asinda-Sikka underlines the probability of farflung communications over much of the western Sudan during the first millennium.⁷⁹ The Cyrenaican statuette from the second century A.D. also points to communication with the Mediterranean world from the beginning of this period as well. Such communications probably vehiculated a trade in commodities about which nothing as yet is known.

Was gold one of these? It may be worth recalling Garrard's finding that the gold trade from West Africa first became significant in Tunisia during the fourth century B.C.⁸⁰ The sources of this gold remain unknown. Hitherto one only invoked the goldfields of Bure and Bambuk. The fact that the only early

gold object found so far in West Africa is an earring from Jenne-Jeno dating to *ca.* 800 A.D. fits well with this view.⁸¹ But these goldfields are much further from Tunisia than from Morocco, while other potential goldfields in Niger and northern Nigeria are much closer to Carthage than Bure or Bambuk. In 1992 Regnault discovered that there had formerly been significant gold-mining activities in the Sirba valley of Niger, i.e., in the Bura area.⁸² While it is far too early to conclude without further research and dating, it now looks quite possible that the gold of Carthage came from here and that Gao grew rich by trading it.⁸³

A second recent major finding concerning the period before 750/800 A.D. has been that developments in the Middle Senegal Valley and the Inner Niger Delta were radically different. In the former little change occurred either in the size or in the distribution of settlements, although some population growth did occur, at least until around 900 A.D. Then in less than a single century, the valley was suddenly unified into the kingdom of Takrur and just as suddenly dragged into the wider world. Imported copper, textile technology, glass, and other exotic imports appear and by 1000 A.D. at least some inhabitants were Muslims. Meanwhile the population of the IND grew more substantially as the millennium wore on, and settlements began to cluster around larger centers such as Jenne-Jeno or Dia, which became substantial towns. By 1000 A.D. the IND harbored an estimated tenfold its present population. Concomitantly, trading networks expanded and fused the whole IND into a single network running from the goldfields on the Upper Niger to the region of the later Timbuktu. Yet by 700 A.D. and later, the kingdom of Ghana developed not in the populous IND, but in a nearly empty area to its northwest.⁸⁴

Population growth peaked around or just before 1000 A.D. in the IND followed by a decline in population, perhaps already from the twelfth century. This growth was probably due not just to the effect of natural increase but also to immigration from further away to places in and around the delta. The population known as Tellem are a good example of this. They were immigrants from further south who settled around 1000 A.D. in the inhospitable Bandiagara region east of the delta, and were probably only one of the groups attracted to the delta and its margins, thereby contributing to the *general buildup of population.*⁸⁵

A dramatic reversal becomes very visible during the thirteenth century, when settlements west of the delta and northwards in the heart of Ghana were suddenly abandoned because, it is claimed, of increasing aridity. A precipitous decline in the size of the Tellem population during the same century is hardly coincidental. Here aridity alone cannot be blamed. Nor can it be blamed for the concomitant decline in population, leading even to the desertion of urban sites, in the IND.⁸⁶ Such a thorough redeployment of population over such a large region suggests effects induced not just by climatic change but by political upheaval as well. After all this was the century of Ghana's decline and the rise of Mali.

The weight of the accumulating archeological evidence will soon force a far-reaching re-examination of the current historical views concerning the western Sudan before *ca.* 1200 A.D. These are still based practically exclusively on scarce scraps of written evidence. There are not enough of these to make much of Gao or Takrur, which are only cited in accounts of Islamization or trans-Saharan trade, but more has been said about the “empire of Ghana” and its successors.⁸⁷ One must confront written statements and archeological finds.

To start from a detail: according to al-Bakri, only the king of Ghana and his crown prince could wear sewn clothes.⁸⁸ But sewn shirts are found in the contemporary Tellem caves. Was al-Bakri misinformed? Was Tellem outside Ghana’s realm? Was the rule only valid at the court or in the capital? Elementary larger questions abound. It is now obvious that the vast IND was the demographic core and the economic dynamo of the whole region. Is this the Wangara/Palolus of the sources?⁸⁹ If it did not form part of Ghana, then Ghana was in fact a kingdom truly modest in population and economic production, much more modest than all the Arab authors tell us.⁹⁰

If the IND was part of the kingdom, why was its capital so eccentric and had been since *before* the upswing of the trans-Saharan trade? Why was not Dia or Jenne-Jeno the capital? If the delta was not part of Ghana, what was its political organization? In any case, it has now become impossible to believe the Arab authors who describe the kingdom as the dominant power in the whole of the western Sudan: the contrary archeological evidence is simply too overwhelming. The new evidence from the Senegal and Bura areas is also beginning to raise major questions as to the relative place of Ghana in the western Sudan, as well as to the history of population, economic, and sociopolitical dynamics elsewhere.

Archeological evidence now gives us a rather full picture of the standard of living in the marginal Bandiagara area, far away in the bush. Finds in the dry caves near Sanga, which date from the eleventh and twelfth centuries, include woodwork (among them two figurative sculptures), basketry, leatherwork, and metalwork, as well as an abundance of ceramics and textiles. This was a poor farming community, located on the far edge of the delta in a marginal environment for farming, the sort of place where iron tools other than arrowpoints were recovered rather than buried with the dead, where women wore basic pubic coverings in grass and men had neither trousers nor boots.⁹¹ Yet at the same time there were well-woven tailored shirts, wrappers, blankets, bonnets, and good sandals. Some inhabitants at least could afford textiles imported from both North Africa and Nubia. Perhaps not surprisingly, leather objects were abundant, but so were iron objects. Quartz jewels jostled carnelian beads, perhaps from the Tilemsi valley, glass beads from northern Africa, and the occasional expensive bronze pendant.

No, this was not a community merely eking out a subsistence survival! Ubiquitous headrests and metal hairpins tell about careful hairdos, certainly not for wear in the fields! Besides remnants of hoes or calabashes, one also finds musical instruments, even harps. Special ceramics were made for

funerary purposes. Indeed a type of footed bowl, one of two sorts of ritual pots, also occurs on contemporary sites at Kumbi Saleh and in the delta itself.⁹² If one finds such relative luxuries in a forgotten settlement in the bush on the periphery of the delta, what then were the standards of living of poor and rich in the major towns?

The Tellem of Sanga lived in a backwater, but they were not isolated from the delta. They could afford imports from far away, no doubt *via* a distribution point on the long-distance routes, probably in the delta. But what did they sell in exchange for these goods? So far we have no clue. Did they use any currency, perhaps similar to the small lengths of copper wire used at Kumbi Saleh? None has been found.

Moreover, the excellent state of preservation of most Tellem objects adds much evidence about technology and expertise, and thus indirectly draws our attention to the presence of industrial development, especially with regard to ceramics, textiles, and metals. The output of these industries all over the IND and its surroundings must have been considerable, absorbed much time and labor, involved economic specialization (certainly for metals, perhaps for textiles, leatherwork, and woodcarving), perhaps with the formation of castes, and obviously led to a flourishing trade in the finished products.

Hitherto, however, not much attention has been paid to the organization, size, localization, and political control, or to the economics of industrial labor and production, apart from a recent suggestion that a caste system may already have existed.⁹³ Yet industrial products were as essential to the inhabitants of the western Sudan, and especially those of the delta, as the production of food by foraging, fishing, and farming. Until now historians have focused almost exclusively on polities, Islam, or the trans-Saharan trade, and their descriptions retain a faint whiff of the fairy tale as a result. Grounding a political history on a basis of population dynamics and of the daily concerns of most inhabitants will teach us more about political possibilities, rooting the long-distance trade in the local trade in industrial products and foodstuffs which sustained it. It will help to think of Islam in terms of the ideologies, concerns, and rituals of daily life and thus will transform ethereal accounts of the past into a dense and multifaceted history.

V

Interpretations of the early history of eastern and southern Africa have been and still are dominated by the issue of the "Bantu expansion." The record shows a set of clearly related ceramic styles, traces of iron smelting or ironworking, and a settled life style stretching from coastal east Africa all the way to Natal. The sites are all dated between A.D. 100 and A.D. 250, the east African ones being a little earlier. This record strongly suggests a rapid migration from east Africa to Natal. Many archeologists and historians went much further in their interpretations. They still hold that the evidence cited is only part of a much wider picture which documents the spread of Bantu-speakers from the Great Lakes area all over eastern, central, and southern

Africa. These immigrants were culture heroes. They brought an "industrial complex," dubbed Chifumbake by Phillipson, which included ceramics, agriculture, herding, metallurgy, and settled life to this portion of the continent.⁹⁴ Yet it has become obvious that such sweeping views must be at least modified—for a large portion of the area there was no such single package. There is no evidence for the introduction of a full-fledged farming economy, and the relevant ceramic styles of central and inland eastern Africa may not be related at all.⁹⁵

Meanwhile, it has been evident for decades that between *ca.* 750 and *ca.* 1100 A.D. major changes occurred in eastern, east-central, and southern Africa, a phenomenon that has been given the unfortunate label of the Later Iron Age, a term which Phillipson wisely avoids in his textbook. What occurred during those centuries was the growth of regional systems which are the direct antecedents of the main subactual regional varieties of cultures and societies. Wherever systematic surveys have been made, they reveal that the number of sites dating to the Later Iron Age is so much greater than those for the preceding period that the increase cannot be attributed just to the fact that more recent sites are better preserved than older ones.

The most reasonable explanation for this situation is to accept that a significant increase of population did in fact occur during those centuries.⁹⁶ At the same time new ceramic styles appeared which developed into subactual pottery and rapidly spread over whole regions. In some instances clear hierarchies of settlements appear. On the east coast cities flourished and in western Uganda at least one complex town, Ntusi, appeared. The necropoles in the Lualaba depression became quite large and more richly appointed, while in the Limpopo area and westwards in Botswana large central settlements developed, culminating in the construction of Mapungubwe and later Great Zimbabwe itself.

The diversity and richness of assemblages in the highest-ranked settlements increased dramatically, while the manufacture of many artefacts, especially metalware, exhibits an amazing technological virtuosity.⁹⁷ In southern Africa and in Uganda cattle herding also became much more intensive than had previously been the case. Trade over longer distances between the different regions grew, trading became more frequent, and its effects, especially those of luxury goods, became more pronounced. Yet the innovations mentioned everywhere grew from local antecedents, and not by borrowing. Hence, and despite the rough synchronism of the appearance of the main regional traditions, their onset cannot be attributed to a single outside cause, such as the impact of trade from the Indian Ocean.

The maturation of farming economies many centuries after the introduction of the first elements of farming and centuries of local development was, I believe, the most significant common factor involved in the rise of more complex regional systems. For the first time agricultural and pastoral produce furnished the mainstay of the diet.⁹⁸ This maturation was intertwined with a concomitant growth in population which was essential to the development of complex regional systems. But with the exception of this

general underlying common dynamic, the specific processes by which various regional traditions grew were all different.⁹⁹

To start with the east coast, recent excavations at Shanga show that urban development there began around 775 A.D. and that a first mosque, built presumably for traders, is not much later in date. By the tenth century a sultanate, however modest, had appeared in the Lamu archipelago, and by 950 its rulers were beginning to mint coins.¹⁰⁰ A particular style of ceramics firmly associated with Swahili-speakers and their trading sites on the east coast has now also been documented in the near hinterland of Tanzania, as well as on one site each near Lake Tanganyika (Kanongo) and Lake Nyasa (Ruvuhu).¹⁰¹ That does not mean that coastal traders settled inland, but merely that a ceramic fashion spread inland, probably as a by-product of trade.

The extent of the reach of the Indian Ocean trade in eastern Africa by 800 A.D. has recently become more evident now that glass beads from the late eighth century, as well as domestic fowl, have appeared at Kwagandaganda near Durban,¹⁰² and rock crystal found on sites in the Lamu archipelago must have originated on the Laikipia plateau in Kenya.¹⁰³ By 1000 A.D. a web of trading routes may have spanned nearly the whole of southern Africa from the coast to the middle Limpopo, and from there to the Okavango delta area, where marine shells from the Atlantic Ocean have also been found.¹⁰⁴ By then the Indian Ocean trade was at least indirectly linked to the trading area centered on the copperbelt in Zambia and Shaba, where a few cowrie shells, conus shells, or glass beads occur on many sites.¹⁰⁵ In one way or another the core areas of all the new regional traditions, with the exception of western Uganda, were in at least in direct contact with each other.¹⁰⁶ Parts of this common web would develop in such a way that by the fourteenth century routes linked western Transvaal to Zimbabwe, to Ingombe Ilede, to the copper belt, to the Lualaba depression, and northwards probably as far as the rainforests, with "feeder" routes to the coast and probably westwards as well.¹⁰⁷

To establish the growing reach of the Indian Ocean trade links is especially relevant to the scholar concerned with the spread of all sorts of innovations, from new crops to xylophones.¹⁰⁸ Apart from the case of chickens—not necessarily from the Indian Ocean, however—archeology so far attests only directly to the introduction of cotton weaving. Spindle whorls are dated both on the middle Limpopo and the middle Zambezi to 1000–1200 A.D., but in contrast to West Africa weaving never became a major industry.¹⁰⁹

Recent research in western Uganda has shown that the grasslands of Mawogola were only occupied by *ca.* 1000 A.D. Cattle-herding on a large scale occurred there, along with agriculture. The new settlements fostered the exploitation of salt on an industrial scale, as well as of other resources.¹¹⁰ Site hierarchies developed. First a genuine town, Ntusi, in a landscape dotted with large concentrated villages. Later the town declined, small but walled centers took its place, and the general pattern of settlement changed from villages to dispersed homesteads.¹¹¹ This regional system is the direct forerunner of the

later kingdoms in the Great Lakes area. The famous tales about the Bacwezi refer to its last generations.¹¹²

In eastern central Africa the most informative group of sites remain the cemeteries of the Lualaba depression with their rich and varied deposits. The people who lived there before *ca.* 1300 A.D. were the forerunners of the later central Luba kingdom. No research could be undertaken there recently, but one wishes at least for more laboratory studies on objects already recovered. In the Copperbelt it has also long been known that standardized copper ingots were being produced by *ca.* 900 A.D., and that some of these were exported. By 1300 A.D. copper was clearly no longer exchanged only for use but also as a standard of value and payment in all sorts of transactions, so that a genuine currency, the copper cross, had developed.¹¹³ Regardless of whether or not most of this money was used in trade, it testifies to the existence of a very complex economic and sociopolitical system in which transactions were so numerous, for so many purposes, and between so many different people that a single fungible standard item of exchange was needed.

Further research in Zambia and Malawi continues to focus on identifying and classifying ceramic styles. The main result from a number of recent site reports seems to be that there is a period from *ca.* 750 to 1000 A.D., in which the number of ceramic styles increased greatly. This attests to a strong growth in local innovations as compared to the earlier situation. At the same time, contact between farmers and foragers seems to have increased during this period. A dramatic reversal of tendencies occurs *ca.* 1000 A.D., when the preponderance of centrifugal tendencies was suddenly reversed. A single one of the many earlier local styles was now rapidly adopted over most of Zambia and Malawi. While one feels that this sudden change somehow documents a major historical upheaval, it still remains quite unclear what exactly such a shift of ceramic style means in terms of economic, social, or political history.

Research in southern Africa has by now uncovered and interpreted the main outlines of a nearly complete sequence of settlement layouts and of site hierarchies. After an initial period in which herding and then farming appear, the central cattle kraal plan is developed. This means that various families of nearly equal importance in a village herded their cattle together at night and cooperated with each other in other endeavors as well. Detailed reconstructions of such communities invoke recent Sotho and Nguni ethnography.¹¹⁴ This layout gradually developed into a plan where a separate royal establishment arose, as at Mapungubwe and Great Zimbabwe, accompanied by a well-demarcated and intricate hierarchy of sites.¹¹⁵

Meanwhile, two other territorial systems arose in Botswana, one being hierarchical and dominated by an elite at Toutswe, the other northwest of the Okavango swamp and later absorbed by the first one, being more egalitarian and based on mutual exchange.¹¹⁶ Further research is still refining, testing, and expanding the models used, as well as applying territorial analysis to other factors such as natural environments.¹¹⁷ Thus historical reconstructions of at least the commanding heights of political and social history are emerging. But scholars should remain prudent and be especially wary of untestable inferences

drawn directly from ethnoarcheology, as the dispute around the use and meaning of various buildings at Great Zimbabwe shows.¹¹⁸

If the findings from archeological research in eastern and southern Africa dealing with periods before 750 A.D. are still too rudimentary to be of much use to historians, findings for later periods have become the linchpin for any general reconstruction of southern African history after that date. In contrast, the findings for later periods in east-central and eastern Africa are still not rich enough to allow for any overall reconstruction, with the signal exceptions of western Uganda, the coastal cities, or the sites in the Lualaba depression. Yet little by little elements relating to the daily rhythms of life and standards of living are emerging, even for those periods and areas where the overall record is still too rudimentary to be connected into a single coherent picture.

VI

When a total consensus about some point exists in an allied discipline, historians naturally tend to accept it without question. And yet the consensus may turn out to be wrong. Perhaps the premier case of this in African history is the issue of “the Neolithic revolution.” Until recently the deeply-held consensus among archeologists about this question was that there had been such a revolution in Africa. Historians naturally accepted this and drew consequences from that supposed fact. The thesis originates with the archeology of Europe and the Middle East.¹¹⁹ It holds that the transition from a foraging to a farming and herding way of life had been very rapid (hence revolutionary) and constitutes a watershed (hence a revolution) in human history, comparable only to that following transition— “the birth of civilization”—and of course it is an evolutionary vision!

Farming created sedentism and hence was a prerequisite for any society more elaborate than a transient local community. Archeologists of Africa, along with all others, accepted this notion and historians imbibed it from them. Given the suddenness and the importance of this revolution, it was only reasonable for historians to think that the foundations of modern African societies and cultures were laid during or after this revolution. Meaningful African history began with the acquisition of farming.¹²⁰ The archeological consensus was that agriculture and herding had been introduced from the Middle East to northern Africa and resulted in the establishment of sedentary communities there. Then, after a delay to domesticate local foodcrops, the complex gradually spread over the continent. The knowledge of metallurgy later spread in a similar way from the Middle East to northern Africa and then further south. But today that consensus among archeologists has been shattered.

Now, it seems that, in Africa at least, and perhaps elsewhere too, there was no Neolithic revolution after all.¹²¹ It has become clear that sedentary communities are much older than any farming. A slight tendency towards

sedentism can be discerned about 25,000 years ago among the fishing folk of Ishango in the western Rift valley.¹²² A larger degree of sedentism was achieved when the inhabitants of Tamar Hat in Algeria began to exploit one or a few particularly abundant local species such as barbary sheep by 18,000 B.C., or when those of Wadi Kubbaniya by 16,000 B.C. began to eat the tubers of a species of marshy grass as a staple.¹²³ Then pottery appears in the eighth millennium B.C., both in the southern part of the central Sahara and, a little later, in the middle Nile valley.¹²⁴

For all we know, the appearance of pottery in these two locations may have been due to independent invention. Pots testify to at least some sedentism. Ceramics make cooking possible, allow one to use a much wider range of vegetable foods than was previously possible, provide improved dietary hygiene, and thus indirectly affect both health and population. Next, by *ca.* 6000 B.C. one finds semi-domesticated millets and sorghums at Nabta Playa.¹²⁵ Intensive foragers were now turning into farmers, at a time when communities in lower Egypt may or may not have been cultivating barley, perhaps of Egyptian origin, as well as wheat and emmer, probably of Asian origin.

The earliest date for the appearance of domestic cattle is still hotly disputed, although cattle were certainly herded before 4000 B.C.¹²⁶ From then on domesticated cattle spread in the Nile valley and across the Sahara.¹²⁷ These cattle need not have been imported from the Middle East, but may well have been domesticated locally.¹²⁸ Indeed, even goats and sheep need not have been introduced as domesticates, because the range of their wild ancestors may well have included parts of northeastern Africa.¹²⁹

Evidently then, there was no sharp transition from foraging to farming. The straightforward substitution of domestic plants and animals for wild grasses or tubers and game was not sudden. It is now thought, for instance, that when herding spread to the central Sahara the pastoralists there continued to harvest wild grasses and did not grow crops, a practice still observed in parts of the Sahel in this century.¹³⁰ So, while a sharp transition from collecting grasses to farming seems indeed to have occurred *ca.* 1000 B.C. at Tichitt (Mauritania), that case may well have been the exception.¹³¹ At Jenne-Jeno the consumption of wild cereals (including wild rice) exceeded that of cultivated cereals for 1600 years after *ca.* 250 B.C., and wild rice was also gathered elsewhere in the delta.¹³² Crops also constituted only a small portion of the food supply of the inhabitants of the equatorial rainforests, although domestic crops were introduced into the western part of this area by 3000 or 2000 B.C.¹³³

Similar situations seem to have been quite common in other parts of tropical Africa as well. Hence it no longer makes any sense to believe that a huge social and cultural divide yawns between foragers and farmers, nor to hold that African history really begins only after the introduction of full-fledged farming; some of the roots of modern African societies and cultures may go back much further into the past than that and with them a meaningful history of Africa also stretches back much further than was previously

thought. Rather than stress discontinuities between foraging communities and their successors, historians should now look more closely at the continuities between them.

The story of the demise of the Neolithic revolution teaches us that complex innovations never are single events, but processes. No single invention, technological or otherwise, was so momentous that it immediately and radically transformed daily routines. To the people involved, that would have been far too risky, nor would a change of this magnitude have been comprehensible. Major innovations occur incrementally over much time. It follows that what happened between the introduction of a particular innovation and the time when farming had become absolutely predominant is going to be more important than the event of a first small innovation itself.

The initial step or steps had to be followed by a series of experiments during a formative period that must be reckoned in many centuries, before a mature system could be fully in place. Where such intermediate steps did not occur, farming might not develop at all. Even when a mature phase was in place and farm products had become the mainstay of the diet, the history of farming does not end. New crops, animals, field techniques, and labor arrangements were—and continue even now to be—incorporated into the local system. Farming strategies also changed and still continue to change—e.g., when people switch over to another staple crop, such as a shift from a sorghum to a millet, or from cereals to maize, or from yams to manioc. There is no end to the possible changes. And what is true for the farming process also holds for any other major technological innovations, such as metallurgy or textiles, or indeed for complex social innovations such as “urbanization.” That major innovations are not events, but processes, is a point which until very recently has been overlooked until the demise of the Neolithic revolution focused the attention of scholars on the issue.

The demise of the Neolithic revolution also spells the end of the simple diffusionist model. In this view major innovations gradually and inexorably spread from a cradle of origin to the furthest reaches of a continent or beyond. That model is now shown to be false. Chronological anomalies in documented distributions have falsified it beyond redemption. The record is better explained by assuming that in similar circumstances parallel innovations were invented several times and in several different places. While some limited geographical diffusion may well have occurred from any or all of these places, such a diffusion was far from being always present, regular, and automatic. Instead, diffusion was an unpredictable and capricious process, contingent on the specific conditions obtaining in various localities at the time in question, just as any other historical development is.

Moreover, because each innovative event is only one step in a complex process, the next step may well occur in a place other than where the first one occurred, and might even have been invented several times independently in several localities. This new step could also—but need not—diffuse outward from its cradle. For each of the later steps the situation described for the second step would also obtain. To put it in a nutshell: the invention and

spreading of innovations is a historical process, subject to all the contingencies and vagaries of any historical process.

We have argued that during an innovation process a series of events occur over considerable periods of time and over a considerable area of intercommunicating settlements which stimulate one another in developing the process.¹³⁴ Lest it be thought that it is fanciful, especially for early times, to posit informational links between quite distant communities, consider the case of the amazonite beads. Amazonite from the Tibesti mountains over 1700 kilometers away was brought to Es Shaheinab (Sudan) on the Nile before 4000 B.C. It is therefore possible that new ideas traveled from the Nile to Tibesti or vice versa by that time as well.¹³⁵ This being so, it follows that there is no single place, nor is there a single event or date of origin, for any complex innovation. The search for origins is not only futile but deeply misleading as well: origins in the sense of a single locality at a single date do not—cannot—exist.¹³⁶

Various aspects of this more realistic approach to invention and diffusion are supported and can be supported, among other things, by evidence about the beginnings of intensive foraging, ceramics, agriculture, or metallurgy. But let us refer only to the case of the known spread of iron smelting as a splendid illustration of concrete situations. Iron smelting began in several places at about the same time, was technologically different from place to place, and did not rapidly diffuse from one place to another in any orderly pattern. Areally it spread in patches at chronologically different times. Thus, in the western great lakes region iron smelting appeared apparently *ca.* 800 B.C., but it did not spread to other places until about the turn of the Christian era.¹³⁷ In Gabon “Neolithic” farmers lived side by side with smelters and users of iron and also with foragers for at least four centuries.¹³⁸

In West Africa the earliest centers are found at Termit in northern Niger, perhaps by 1000 B.C., at Taruga, perhaps by 800 B.C., and in Igbo country, perhaps by 600 B.C.¹³⁹ But the lands west of lake Chad lying between Termit and Taruga did not begin to use iron for nearly a millennium after they did.¹⁴⁰ A simple dispersal even from Taruga to the Igbo sites not far away is excluded because different types of furnaces were used; indeed several sorts of furnaces occur on the Igbo sites themselves. One could find no better example to stress that for every community which adopts a diffusion, this adoption is also a new invention. The case of the invention and spreading of metallurgy shows that reconstructions on the lines of the simple diffusion of momentous innovations will not do. The diffusion model is mechanistic, ahistorical, and above all simply wrong.

The demise of the “Neolithic revolution” and of the diffusionist model which accompanied it is not the only case where a consensus among archeologists might have led historians astray. For example, all archeologists start human history with an account of the emergence of hominids, now traced back to well over four million years ago, and historians have accepted this position without serious demur.¹⁴¹ Yet *Homo sapiens sapiens*, people like us, appear only about 150,000 years ago. Any earlier species is simply not

human, but only hominid. The possibility of writing history requires that there be a sufficient common identity between the historian and the actors of the past for the historian to be able to understand the motivation of such actors in rational or emotional terms. That condition does not obtain with regard to hominids because they are not of our species. Their study should be the object of primatology and not history. The condition of sufficient common identity obtains only when true humans appear.¹⁴² These early people were just as intelligent, blessed with the same linguistic capacities, and endowed with same emotional makeup as people living now.

It follows from this line of reasoning that African and world history begins only in chapter four of Phillipson's work, when he coyly remarks: "What recent discoveries do emphasize is the central role of post Acheulian African societies in the development of modern human behavior."¹⁴³ One certainly must be an attentive reader to catch that this is the point at which human history begins. Refusing to accept the consensus in this case does not lead to major reassessments of the history of Africa later on, but it does underscore the essential difference between biological evolution and cultural development. It too is a consequence of the rejection of neo-evolutionary theories.

VII

In order to make good use of archeological evidence for historical reconstruction, scholars must first fully realize what its handicaps and strengths are. A nearly total adherence to neo-evolutionary theory (including various environmental determinisms), the refusal to recognize fully the role of contingency by sticking to the use of theoretical models, the extravagant use of extrapolation, and the lack of contemporary testimony to limit the free range of the imagination are the main handicaps of archeology. Its main advantages are that the evidence unearthed is concrete, usually documents situations, and often sheds light on the lives of ordinary people.

The main handicap of documentary (written or oral) evidence as used by historians, on the other hand, is that the bulk of that evidence consists of testimony nearly always deriving from leaders, and testimony which often consists of a narrative of successive events which happened to leaders of communities, thus leading historians both to focus their accounts on the exceptional doings of leaders and to do so from the point of view of these leaders. Its advantages are the limitations put on the imagination of the historian by the interpretation already provided in the testimony, the amount of detail often recovered, and the identification of historical actors, with their possible motivations. A fruitful integration of archeological data in a general historical reconstruction should first recognize the limits of its potential and then exploit its complementary strength to documentary history.

The main reason that Roberts was unable to achieve a fully satisfying historical reconstruction in his *History of Zambia* before a period for which documentary evidence becomes available was simple. The mass of evidence

available was too small, and especially not diverse enough, to allow for anything more than the most rudimentary sketch of the past. And for Zambia this is still true today. Historians should accept that in the present state of research one can begin to elaborate a meaningful (because complex enough) historical reconstruction for the western Sudan by the middle of the first millennium A.D. and for portions of eastern and southern Africa after *ca.* 750 A.D. Before those dates the lacunae in what we need to know, even for a still rudimentary reconstruction, are too overwhelming.

Having stated this, one must also underscore that this situation constitutes substantial progress over what could be done just twenty years ago. Even though the accumulation of evidence in archaeology is by nature—as well as by the realities of funding—quite slow and irregular, it nevertheless occurs, a fact which historians tend to overlook. They should not assume that because such data are not in hand now and progress is so slow that archaeology will never yield sufficient data to allow for a historical reconstruction of earlier periods. Nor should other impatient historians attempt imaginative reconstructions on the basis of a site or two. Historians must appreciate that a new find can *at any moment* completely overturn the conclusions drawn from such meager evidence. Therefore meaningful reconstructions can only be undertaken when enough sites have been excavated and when the diversity of artefacts and features recovered is sufficient to make such a surprise very unlikely. The second condition is as important as the first. In the case of Zambia, for instance, there are plenty of excavated sites, but the diversity of artefacts and features recovered is still too narrow to allow for a satisfactory reconstruction.

As to complementary strength, an excellent illustration of this is the case of Daboya, a late urban site in Ghana. Daboya was already an old and sizeable settlement when it became part of the Gonja kingdom before 1600. In the later eighteenth century Gonja itself was overrun by Asante, to which it became tributary. Much of the known information about the kingdom stems from the *Kitab Ghunja*, compiled *ca.* 1751. Yet excavations at Gonja showed that both the Gonja and the Asante conquests remain invisible on the site.¹⁴⁴ One might conclude from this that the “resolution” of archaeological data is not good enough to capture even momentous political events. Be that as it may, what one should conclude is that even these momentous political events left little mark on the daily lives and living standards of the inhabitants of Daboya. In other words, the successive political upheavals were of little moment to the whole of the population. Hitherto historians had not appreciated how much the *Kitab Ghunja* had misrepresented the past by elevating the experience of a small political and religious minority to the level of a universal upheaval. The archaeological record in this case documents the fate of ordinary people and thereby substantially alters the accepted reconstruction.

Other excavations of historically well-documented sites bring similar lessons relating to the majority of the population by showing, for instance, to what degree and in what ways they were affected by European imports or the nearby presence of European settlements or by political upheavals.¹⁴⁵ One

lesson of archeology then is the danger of overstressing the importance of single events, usually political upheavals, and the need to focus more on daily life, daily routines, and the standards of living which affect the majority of the population—in short, on all those factors which are taken as known by most written or oral accounts and yet give substance and meaning to the events described in such accounts.

In general, archeology helps us give resonance to documentary evidence by placing it against its background, by eliciting the *longue durée* in which documented events and trends unfold. All too often we tend to take this background as a given, as if it were an unchanging backdrop to the action described in the foreground. And yet it is not. Natural surroundings, population movements, ways of making a living, the daily round of activities, and personal relationships all change. It will, for instance, no longer suffice merely to note that the inhabitants of the Middle Niger Valley grew grain crops during the first millennium A.D. and assume that it was then just as it is now.

There are those who do speculate that a grain surplus was produced in or south of the delta for export to the inhabitants of the desert edge. Yet during this whole period the inhabitants of Jenne-Jeno, as well as some others in the delta, relied as much on gathered food, especially wild rice, as on cultivated cereals.¹⁴⁶ What food then, if any was exported? Was there a substitution of wild rice and grass seeds for imported cultivated cereals further south so as to forward the latter to the desert edge? Or was it wild rice and grass seeds that were exported? One also assumes that both climate and population were stable, but they were not. The delta expanded or contracted and so did its population. Any historical reconstruction worth its salt will then be a description of the conjunction at a shorter or longer moment in time of all the various temporal movements from the majestic sweep of deep change to the oscillations of the time of events.

In reverse, archeology can benefit a great deal by the systematic input of written and oral documentary evidence to establish ethnoarcheological similes. The practice of using data from present times to illuminate an archeological situation assumes an unproven cultural continuity and is totally anachronistic.¹⁴⁷ The historian can help here by finding data relevant to the situation from documents and oral data at the earliest moment possible. Thus if one wanted to check on the shape and use of agricultural tools in Angola as a comparison to hoes found at Feti (certainly pre-1300), one is not limited to comparisons with twentieth century hoes and farming. One can turn to an abundant seventeenth-century record, including illustrations of hoes and descriptions of their use.¹⁴⁸

While reliable iconographic records for tropical Africa before *ca.*1880 are rare, there are more of them than most scholars realize, and the same holds true for objects which have survived in museums. It is a pity that historians have not shown much interest as yet in systematic studies of material culture, which would be both priceless to archeologists and of considerable relevance to historical reconstructions in general. Having these sorts of data will

certainly lessen, but not completely eliminate, the risk of anachronism in ethnoarcheology. This can often be done, however, by applying the linguistic technique called "words and things" to the vocabulary linked to the items studied, since that technique allows one to establish the relative age of each of the words used, and thus indicates how great the continuity, if any, has been between the archeological situation and the present-day one with which it is compared.¹⁴⁹

VIII

In a sense this paper has been but a gloss on the two quotations with which it opened. Yes archeology is indispensable for any worthwhile history of Africa and historians should be wary of conclusions drawn by archaeologists. Yet the paper also shows that these quotations are far too vague and too restricted to be useful. The task of the historian is to reconstruct history, and anyone of whatever discipline who does this is by definition a historian. This task requires two conditions: that there be enough evidence for a coherent reconstruction, and that the general rules of evidence be applied to that record.

Much of this paper has been devoted to the second condition. About the first condition it was said only that there must be a sufficient mass of diverse data. Some reconstructions are obviously much richer than others, yet there still exists a minimal threshold. In order to achieve a coherent reconstruction there should be a body of interconnected data ranging from background features affecting the whole population, such as climate, demography, material culture, the various technologies in use, daily routines, major social identities and cleavages, etc. to information allowing one to reconstruct at least a generally coherent narrative about the specific changes which a given society experienced during the period studied. For most periods in African history this requirement implies the use of a wide variety and types of sources, not just archeological data laid side by side with documentary testimony. In this sense, comparing just archeological and documentary sources is a mistake.

A general discussion of what is required to achieve a full reconstruction of history would take us too far. But we must at least conclude with the remark that when archeologists offer specific reconstructions of history, as they often do in their site reports, they are historians. More than most historians who use written or oral sources, they solicit and coordinate evidence from other disciplines relating to the sites they study, usually from the physical or natural sciences, but sometimes from other sources as well. They participate in the common task of reconstructing a vision of the past derived from a wide array of sources by a common method. All historians, whatever their disciplinary affiliation, can therefore learn a great deal from the practice of archeologists about the perils and the successes of reconstructing history from a varied lot of sources. Hence the contribution of archeology to the history of Africa is not limited to the discovery of new and complementary sources to be used by others, but goes to the very heart of the historical enterprise.

Notes

1. Roland Oliver, "Preface" in D.H. Jones, ed., *History and Archaeology* (London 1959).
2. T.O. Ranger, "Towards a Usable Past" in Christopher Fyfe, ed. *African Studies Since 1945* (Edinburgh, 1976), 21.
3. R.J. McIntosh and S.K. McIntosh, "The Inland Niger Delta Before the Empire of Mali: Evidence From Jenne-Jeno," *JAH* 22 (1981), 1-22.
4. (London 1976). For the transition see *ibid.*, 63-79.
5. This paper was triggered by the desire to discuss the concrete contributions of archeology in the following recent publications: David W. Phillipson, *African Archaeology* (2d. ed.: Cambridge, 1993); Thurstan Shaw, Paul Sinclair, Basseyy Andah, and Alex Okpoko, eds., *The Archaeology of Africa: Food, Metals, and Towns* (London, 1993); Marianne Cornevin, *Archéologie africaine* (Paris, 1993); Jean Devisse, ed., *Vallées du Niger* (Paris, 1993); and Susan Keech McIntosh, "Changing Perceptions of West Africa's Past," *Journal of Archaeological Research* 2(1994), 165-98.
6. Julian Steward, *Theory of Culture Change* (Urbana, 1955); Leslie White, *The Evolution of Culture* (New York, 1959). For an appreciation of the theory now see N. Yoffee, "Too Many Chiefs? (or Safe Texts for the '90's)" in Norman Yoffee and Andrew Sheratt, eds., *Archaeological Theory: Who Sets the Agenda?* (Cambridge, 1993), 60-77.
7. In both the first edition (1985) and the second edition of Phillipson's *African Archaeology*, 9 "Virtually every major subsequent stage in mankind's development may be illustrated from the African record" concludes the first paragraph of his discussion of "Africa in world prehistory." Two paragraphs later 1985's "were broadly similar" actually becomes "followed similar stages" in 1993, and both have the telling juxtaposition of evolution and technology in "...Upper Pleistocene Africa may have been a world leader both in the evolution of our species and in its technological development" (9 in 1985/10 in 1993). See also "various developmental stages..." *ibid.*, 60.
8. Phillipson avoids using them in his *African Archaeology*, 63, although he once says that "with the passage of time, human culture became more complex."
9. *Ibid.*, 5.
10. *Ibid.*, 99, 100.
11. Note the subtitle *Food, Metals, and Towns* of T. Shaw *et al.* eds., *Archaeology*, for the later part of the sequence, which also reflects the sequence of topics discussed in the work.
12. This is also true for research. Comparative research in various classes of objects other than stone or pots, such as metal farming tools, has been very much neglected.
13. Phillipson, *African Archaeology*, 208-09.
14. J. Vogel, "Microenvironments, Swidden and the Early Iron Age Settlement of South-Western Zambia," *Azania* 21(1986), 85, *idem.*, "Iron-Age Farmers in Southwestern Zambia: Some Aspects of Spatial Organisation," *African Archaeological Review* [hereafter *AAR* 5(1987) 159-70.
15. Jan Vansina, *Habitat, Economy and Society in the Central African Rainforest* (Providence/Oxford 1992).
16. Phillipson, *African Archaeology*/1985, 148-49.
17. Phillipson, *African Archaeology*/1993, 159-60.
18. B.G. Trigger "The History of African Archaeology in World Perspective" in P. Robertshaw, ed., *A History of African Archaeology* (London, 1990), 315, and "migration" in index; T.N. Huffman, "Ceramics, Settlements, and Late Iron Age Migrations," *AAR* 7(1989), 156.
19. McIntosh, "Changing Perceptions," 169-71, 180; R. Vernet, "Préhistoire des bassins affluents de la rive gauche du Niger" in Devisse, *Vallées du Niger*, 66, 67-69, for an elegant demonstration; Phillipson, *African Archaeology*, 127.

20. Ibid., 188.
21. D.P. Collett, "The Spread of Early Iron-Producing Communities in Eastern and Southern Africa" (Ph.D., Cambridge, 1985). A controversy over pottery found on forager sites in south-central Africa is still unresolved. Some archeologists even see the presence of pottery not only as an early independent introduction of ceramics among foragers, but as evidence for an early independent introduction of herding among stone-using communities.
22. Huffman, "Ceramics," 155-82. See his complaint about "deep-seated prejudice against migration hypotheses in general," 188. Also his *Iron Age Migrations: The Ceramic Sequence in Southern Zambia* (Johannesburg, 1989), esp. 5-9.
23. K.V. Flannery, "Culture History vs. Culture Process: a Debate in American Archaeology," *Scientific American* 217(1967), 119-22, and for Africa, Robertshaw, *History*, 53-54, 86, 314-16,
24. McIntosh, "Changing Perceptions," 179.
25. For instance, J. Denbow, "A New Look at the Later Prehistory of the Kalahari," *JAH* 27(1986), 3-28, and T.N. Huffman, "Southern Africa to the South of the Zambezi" in *UNESCO General History of Africa*, 3:643-63, for early studies in this vein. For a sample of quite elaborated and complex spatial analysis of a little more recent vintage see P.J.J. Sinclair, I. Pirikayi, G. Pwiti, and R. Soper "Urban Trajectories on the Zimbabwean Plateau" in Shaw, *Archaeology*, 705-31.
26. T.N. Huffman, *Symbols in Stone: Unravelling the Mystery of Great Zimbabwe* (Johannesburg, 1987), is the most general statement of arguments which he developed in several articles. Critics emphasize that he disregards the internal chronology of the urban site as it grew and reject the validity of his inferences about symbolic meaning by analogy with symbols used ca.1900 in southern Africa—e.g., D.P. Collett, A.E. Vines, and E.G. Hughes, "The Chronology of the Valley Enclosures: Implications for the Interpretation of Great Zimbabwe," *AAR* 10(1992), 139-61.
27. McIntosh, "Changing Perceptions," 185-86; R.J. McIntosh, "Middle Niger Terracottas Before the Symplegades Gateway," *African Arts* 22(1989), 74-83; Scott MacEachern, "'Symbolic Reservoirs' and Inter-Group Relations: West African Examples," *AAR* 12(1994), 205-24
28. For this acrimonious debate see the articles and comments in *Current Anthropology* 31-33(1990-92). More recent is R.B. Lee and M. Guenther, "Problems in Kalahari Historical Ethnography and the Tolerance of Error," *HA* 20(1993), 185-235, and E. Wilmsen, "On the Search for [Truth] and Authority: A Reply to Lee and Guenther," *Current Anthropology* 34 (1993), 715-21. A book by Wilmsen and Denbow devoted to the issue should be available soon.
29. Phillipson, *African Archaeology/1985*, 139,164.
30. Phillipson, *African Archaeology/1993*, 181 (paragraph), 182 (location and plan).
31. McIntosh, "Changing Perceptions," 165-67.
32. Ibid., 179, announcing A.B. Stahl, "Change and Continuity in the Banda Area, Ghana: The Direct Historical Approach," *Journal of Field Archaeology* 21 (1994), 181-203.
33. P. De Barros, "Changing Paradigms: Goals and Methods of the Archaeology of Francophone West Africa" in Robertshaw, *History*, 156. More generally, most atlases of archeology follow this approach—e.g., J. Bersani, ed., *Le grand atlas d'archéologie* (Paris, 1985)—adopt such views.
34. E.g. J. Devisse, "La recherche archéologique et sa contribution à l'histoire de l'Afrique," *Recherche de pédagogie et culture* 55(1981), 2-8.
35. For his treatment of gold see Devisse, *Vallées du Niger*, esp. 344-57, 503-11.
36. Devisse, "Recherche," esp. note 29, where he inveighs both against suited hypothesis, citing Phillipson on Bantu migration, and against models as perhaps suited for relativistic Anglo-minds, but as poison for absolutistic Franco-minds.
37. Devisse, *Vallées du Niger*, 280-86, 342, 426-30, 456-63.
38. Phillipson, *African Archaeology*, 173-74.
39. Ibid., 174.
40. Devisse, *Vallées du Niger*, 218-48; S. Sidibé, "Archéologie funéraire de l'Ouest africain: sépultures et rites" (Ph.D., Université de Paris I,1980); S. K. McIntosh and R.J.

McIntosh, "Field Survey in the Tumulus Zone of Senegal," *AAR* 11(1993), 73-107, and references there.

41. McIntosh, "Changing Perceptions," 186.
42. For the situation at Koumbi Saleh see J. Devisse and Boubacar Diallo, "Le seuil de Wagadu" in Devisse, *Vallées du Niger*, 108-14. For the latest foray in Gao, T. Ingersoll, "Archaeological Research in Gao, the Republic of Mali." *Saharan Studies Association Newsletter* 2/1(1994), 8-11.
43. E.g., Brian M. Fagan, *Iron Age Cultures in Zambia* (2 vols.: London, 1967-69).
44. E.g., P. de Maret, *Fouilles archéologiques dans la vallée du Haut-Lualaba, Zaire, 1. Textes* (Tervuren, 1985).
45. E.g., R. Bedeaux, "The Tellem Research Project: the Archaeological Context" in R. Bolland, ed., *Tellem Textiles* (Amsterdam, 1991).
46. E.g., T. Shaw, *Igbo Ukwu: An Account of Archaeological Discoveries in Eastern Nigeria* (2 vols.: Evanston, 1970).
47. E.g., P.R. Schmidt, *Historical Archaeology: A Structural Approach in an African Culture* (Westport, 1978), for smelting sites.
48. E.g., G. Connah, *Three Thousand Years in Africa* (Cambridge, 1981), the Daima mound in northern Nigeria.
49. E.g., N. Chittick, *Manda: Excavations at an Island Port on the Kenya Coast*, (Nairobi, 1984); or A.O. Babacar et al., *Tegdaoust III: Campagnes 1960/65: enquêtes générales* (Paris, 1983), one of six volumes devoted to Tegdaoust.
50. McIntosh, "Changing Perceptions," 179.
51. Phillipson, *African Archaeology*, 226, where he argues that a sharp break occurred there between earlier ceramics and the Luangwa-tradition ushering in a Later Iron Age in this region. But see his report, "Excavations at Twickenham Road, Lusaka," *Azania* 5(1970), 77-118. Most of the LIA material came from two adjacent pits (103-08) each about one meter in diameter and containing mostly tiny shards (shown on a scale of 10 cm). Twelve shards were EIA, and about 100 LIA. Pits mean a disturbed stratigraphy and small shards are susceptible to movement in the soil after deposit. Under these circumstances one cannot hope for a clear stratigraphy, and hence one cannot claim a sharp break between LIA and EIA. One should also be wary of claiming that objects at the same level in one of the pits were contemporary with each other. Two dates stemming from the waterlogged deposits at the bottom of one of these pits are later than two others from a pit adjudged to have been EIA, but all four dates can overlap at a tenth century (now after further correction an eleventh century) date, which makes all the material in all the pits contemporary.
52. J. Denbow, "Cenchrus ciliaris: An Archeological Indicator of Iron Age Middens Using Aerial Photography in Eastern Botswana," *South African Journal of Science* 76(1979), 405-08.
53. R. J. Mason, "Transvaal and Natal Iron Age Settlement Revealed by Aerial Photographs and Excavation," *African Studies* 27(1968), 167-80.
54. Mamadi Dembelé et al., "Prospections de sites archéologiques dans le delta intérieur du Niger" in Devisse, *Vallées du Niger*, 218-32; R. and S. McIntosh in *ibid.*, 234-48.
55. M. R. MacLean, "Late Stone Age and Early Iron Age Settlement Patterns in Rakai District, South-Western Uganda," *Azania*, forthcoming.
56. J. Denbow, "Congo to Kalahari: Data and Hypotheses About the Political Economy of the Western Stream of the Early Iron Age" *AAR* 8(1990), 140, figure 1; Huffman, "Ceramics," 161, figure 3.
57. J. Kirkman, *The Arab City of Gedi* (London, 1954) was the first among these volumes. Note also that until recently only the stone ruins of urban settlements in Zimbabwe were mapped, again showing odd gaps between structures, until further research plotted the clay floors of ordinary houses which filled most of the urban space.
58. Something similar also holds for Axum. Cf. J. W. Michels, "Review Article: Excavations at Axum," *AAR* 8(1990), 177-88, and figures 1-4.
59. The latest synthesis is that of N.M. Katanekwa, "The Iron Age in Zambia: Some New Evidence and Interpretations," *Azania* forthcoming, and is once again entirely devoted to

a discussion of ceramic styles.

60. Huffman, "Ceramics."

61. Shaw, *Igbo Ukwu*, is a model site report.

62. B. Clist, "A Critical Reappraisal of the Chronological Framework of the Early Urewe Iron Age Industry," *Muntu* 6(1987), 35-62, reviews the issues involved in establishing reliable carbon¹⁴ and thermoluminescence dates in a particular situation. It underlines the difficulties of establishing a secure association between a carbon sample and an artefact, as well as the uncertainties deriving from the techniques themselves and their conversion to calendar dates. In practice there is plenty of scope for subjective evaluations.

63. E.g., R. Haaland, "Excavations at Dakawa, an Early Iron Age Site in East-Central Tanzania," *Nyame Akuma* no. 40(1993), 50-51, argues that the type of furnace found there is similar to one at Samaru West in Nigeria "with no similar remains in intervening regions" and describes this as "puzzling."

64. Phillipson, *African Archaeology*, 194, about sites excavated by Katanekwa on the upper Zambezi, where pottery and bones of domestic animals were found. He generalizes (196) and the general rule then (202) allows him to claim that wherever ceramics and stone tools are associated, the sites belong to nomadic foragers who borrowed shards or pots for their curiosity or prestige value.

65. McIntosh, "Changing Perceptions," 182-86, for *West Africa*.

66. E.g., E. Herbert, *Iron, Gender, and Power: Rituals of Transformation in African Societies* (Bloomington, 1993). The work establishes a general paradigm of iron-smelting as ritual transformation, using ethnographic data, and discusses the application of the paradigm to earlier periods (*ibid.*, 121-26). For a list of recent experiments in iron-smelting, *ibid.*, 239-40.

67. Cornevin, *Archéologie africaine*, 76-79, still repeats this nonsense and defends it by an appeal to the "extraordinary social and religious conservatism of Africa societies" which she still observed in the 1950s.

68. Phillipson, *African Archaeology*, 202-03.

69. J. Denbow, "A New Look at the Later Prehistory of the Kalahari," *JAH* 27 (1986), 3-25. See also his "Congo to Kalahari" for a different reconstruction valid for northwestern Botswana, in which additional evidence for the eastern Kalahari reconstruction is presented.

70. T.N. Huffman, "Archaeology and Ethnohistory of the African Iron Age," *Annual Review of Anthropology* 5(1982), 133-50; *idem.*, "Archaeological Evidence and Conventional Explanations of Southern Bantu Settlement Patterns," *Africa* 56(1986), 280-98. The argument rests on Kuper's claim that the plans have symbolic dimensions and express fundamental features of social organization. A. Kuper, "Symbolic Dimensions of the Southern Bantu Homestead," *Africa* 50(1980), 8-23; *idem.*, "The House and Zulu Politics in the Nineteenth Century," *JAH* 34(1993), 473-74. M. Hall, "The Myth of the Zulu Homestead: Archaeology and Ethnography," *Africa* 54(1984), 65-79 and *idem.*, *The Changing Past* (Cape Town, 1987), 72, criticizes the approach as "ahistoric," i.e., anachronistic. Yet there exists continuity in the settlement plans since the seventh century or even earlier, which explains why most scholars accept the pattern and most of the social organization inferred from it. Studies using the technique of "words and things" could be quite helpful in checking the validity of such inferences.

71. McIntosh, "Changing Perceptions," 179; Tal Tamari, "Les castes au Soudan occidental: étude anthropologique et historique" (Ph.D., Université de Paris X, 1988), established on the basis of vocabulary studies that the caste system in western Sudan is quite old, although the date of its inception remains unknown. See also Tamari, "The Development of Caste Systems in West Africa," *JAH* 32(1991), 221-50, where she dates their appearance among the Malinke to no later than 1300.

72. E.g., Robertshaw, *History*.

73. For the great lakes see Clist, "Critical Reappraisal," 40-42: sources of possible discrepancies; 45: rejection of both late and early dates by the excavator; 48: refusal to accept early dates in Burundi because corresponding early dates for Tanzania had been rejected; 50-55: discussion of large numbers of dates treated together with the elimination of both isolated early and late dates. For *West Africa*, McIntosh "Changing Perceptions," 173, dismisses early

dates for iron-smelting in the Termit massif as due to "fossil charcoal," according to J.D. Killick, "On the Dating of African Metallurgical Sites," *Nyame Akuma*, no. 28(1987), 29-30. But F. Paris et al., "Peuplements et environnements holocènes du bassin de l'Azawagh oriental (Niger)" in Devisse, *Vallées du Niger*, 388, accept even earlier dates which were recently obtained from organic material included as temper in the clay of the tuyères. Was this organic material that turned to charcoal as the tuyère was baked or was it already "fossil charcoal" when used as temper? Apparently McIntosh believes the latter since she does not accept the dating.

74. McIntosh, "Changing Perceptions," 170, 178, and note the later disappearance of a distinct "Mechtoid" type of people.

75. Communication with Northern Africa existed since about the beginning of the Christian era, as the recovery of a few North African beads at Jenne-Jeno and in Niger of a metal figurine from the second century A.D., which was the base of a second-century A.D. mirror from Cyrenaica, tell us. Cf. Devisse, *Vallées du Niger*, 546, #81. Exports of gold to North Africa probably began by ca. 300 A.D. Cf. T.F. Garrard "Myth and Metrology: The Early Trans-Saharan Gold Trade," *JAH* 23(1982), 443-62.

76. R. J. McIntosh and S. K. McIntosh, "Jenne-Jeno and the Inland Niger Delta." *JAH* 22(1981), 16; idem., "Cities without Citadels: Understanding Urban Origins Along the Middle Niger" in Shaw, *Archaeology*, 627-34. The town covered 33 ha in ca. 800 A.D.

77. Boube Gado, "Un 'village des morts' à Bura en République du Niger" in Devisse, *Vallées du Niger*, 365-74, and illustrations, 550-56, #96-130. More dating is needed.

78. Ibid., 572, #209, for an illustration and bibliography.

79. The presence of domestic fowl at Jenne-Jeno, ca. 450-850 A.D. could be a further indication of pre-Islamic contacts with the Mediterranean world. Cf. K.C. MacDonald and D.E. Edwards, "Chicken in Africa: the Importance of Qasr Ibrim," *Antiquity* 67(1993), 588.

80. Garrard "Myth and Metrology."

81. R.J. McIntosh and S.K. McIntosh, "Finding West Africa's Oldest City," *National Geographic Magazine*, 162/3 (March 1982), 407 (graph and illustration).

82. Devisse, *Vallées du Niger*, 356-57. One date around 1400 A.D. has been reported so far.

83. T. Insoll, "Archaeological Research in Gao," *Saharan Studies Association Newsletter* 2/1(1994), 8-11, found a cache of hippopotamus ivory dated to before the eleventh century. Unaware of Regnault's findings, he suggests that Gao's wealth was at least in part derived from the export of ivory, but gold from the Sirba valley seems an even more likely candidate for this role.

84. McIntosh, "Changing Perceptions," 178-79.

85. Bedeaux, "Tellem Research Project," 34-36.

86. McIntosh "Changing Perceptions," 178 (Mema, Dia, Jenne-Jeno, Timbuktu).

87. N. Levtzion, *Ancient Ghana and Mali* (London 1973), 16-28, is still authoritative. Note that in part two of this work (105-217) his reconstructions fuse both Ghana and Mali. Levtzion's views on the beginnings of Mali and on the Susu kingdom are beginning to be challenged. Cf. Dierk Lange, "Das alte Mali und Ghana. Der Beitrag der Oraltraditionen zur Kritik einer Historiographischen Fiktion," *Historisches Zeitschrift* 255(1992), 587-623; Stephan Bühnen, "In Quest of Susu," *HA* 21(1994), 1-47.

88. Levtzion, *Ancient Ghana*, 109.

89. S.K. McIntosh, "A Reconsideration of Wangara/Palolus, Island of Gold," *JAH* 22(1981), 145-58, asserts this in convincing fashion.

90. The recent exploration of the closest Sahel area south of Ghana tells us that the valley was not inhabited in the first millennium A.D. K.C. MacDonald and P. Allsworth Jones, "A Reconsideration of the West African Macrolithic Conundrum: New Factory Sites and an Associated Settlement in the Vallée du Serpent, Mali," *AAR* 12(1994), 77. All around Kumbi Saleh in the other directions only very limited settlement was possible, whether in the few towns such as Awdaghost or not. Devisse/Diallo, "Seuil de Wagadu," 103-15.

91. The boots and trousers found are of a later date, contemporary with Mali. Is there a link between these articles of clothing and cavalry? They are not represented on equestrian

ceramic figures. C. B. de Grunne, "Heroic Riders and Divine Horses: an Analysis of Ancient Soninke and Dogon Equestrian Figures From the Inland Niger Delta Region in Mali," *Bulletin of the Minneapolis Institute of Art* 60(1983-86), 78-96.

92. Bedeaux, "Tellem Research Project," 16-34, for the objects. Many are also illustrated in his contributions in *Devisse, Vallées du Niger*, 280-86 (known distribution of footed bowls, map, 285), 425-30, and 456-63.

93. McIntosh/McIntosh "Cities Without Citadels," 631-34. The "craft associations" (632) are an expression of the caste system.

94. Phillipson, *African Archaeology*, 187-205. The second edition adds material relating to various "streams" of the Chifumbaze complex and on its further development in southern Africa (192-95), as well as some data about cultivated plants (188,197), and the layout of settlements (197,198), and it mentions the controversy over the technological sophistication of early iron smelters in Buhaya (188). It also accepts the alleged similarity of pottery at Benfica (near Luanda) with early potteries in Zambia, despite the exiguity of the evidence (193-94), and it takes no account of reservations made by others about the grouping of all the ceramics into a single stylistic whole.

95. J. Vansina, "A Slow Revolution: Farming in Subequatorial Africa," *Azania* 30(1994).

96. Phillipson, *African Archaeology*, 238, agrees and adds that "remnant populations" were now rapidly absorbed. The evidence is strongest for the Mawogola area in Uganda, the east African coast, the Manda area near lake Nyasa in Tanzania, central Zambia, the Lualaba depression in Shaba, eastern Botswana, and portions of Transvaal.

97. Especially in the Lualaba depression, at Feti in central Angola, and at Nqoma, west of the Okavango delta. Unfortunately, no serious comparative study of these metalwares has been undertaken.

98. Vansina, "Slow Revolution," details this process. The evidence involves the changing proportions of game/domestic animals in diets, a standardization of diet, the introduction of fowls and new crops from the Indian Ocean. The increase of the incidence in which remnants of domestic plants occur on sites is not stressed, however, because such remains occur on so few sites overall that to build a trend out of so few cases might be quite artificial.

99. One may well ask whether such complex regional systems also developed concurrently in west-central and equatorial Africa. Far too little archeological research has been conducted there to speculate fruitfully, even in the parts of Gabon, Congo, Cameroun, and Zaire where some research has been conducted.

100. M. Horton, "The Swahili Corridor," *Scientific American* 25/2(Feb., 1987), 86-93. His full excavation report is in press.

101. G.K. Okello Abungu "Agriculture and Settlement Formation Along the East African Coast;" F.A. Chami "The First Millennium A.D. on the East Coast: A New Look at the Cultural Sequence and Interactions;" H.W. Mutoro "Tana Ware and the Settlement Archaeology of the Kenya Coastal Hinterland," all to be published in *Azania* 30(1994), as part of the proceedings of a conference held in July 1994 at Cambridge University on "The Growth of Farming Communities in Africa from the Equator Southwards." At the conference it was also asserted that this type of ceramics already occurred at Unguja Mkuu (Zanzibar) in the fifth century A.D. See also H.T. Wright, "Trade and Politics on the Eastern Littoral of Africa, AD 800-1300," 659-72; M. Horton and N. Mudida, "Exploitation of Marine Resources: Evidence for the Origin of Swahili Communities in East Africa," 673- 693; and G.H.O. Abungu and H.W. Mutoro "Coast-Interior Settlements and Social Relations in the Kenya Coastal Hinterland," 694-704, all in Shaw, *Archaeology*.

102. Tim Maggs and G. Whitelaw, "A Review of Recent Archaeological Research on Food-Producing Communities in Southern Africa," *JAH* 32(1991),15 (late eighth century), and G. Whitelaw, communication at the Cambridge conference.

103. J. de Vere Allen, *Swahili Origins: Swahili Culture and the Shungwaya Phenomenon* (Nairobi, 1993), 24-25.

104. Huffman, "Southern Africa," 673; Denbow, "From Congo to Kalahari," 166 (Nqoma) and personal communication.

105. E.g., D. Phillipson, *The Later Prehistory of Eastern and Southern Africa* (New York, 1977), 125, 129, 132, 136, 151. Since 1977 cowrie shells have also turned up in Malawi and at new sites in Zambia.

106. P. Robertshaw, "Archaeological Survey, Ceramic Analysis, and State Formation in Western Uganda," *AAR* 12(1994), 110. Beads and cowrie shells dating to around the thirteenth century were found at Ntusi, however, and one bead "of uncertain date" stems from Munsu.

107. M.S. Bisson, "Trade and Tribute: Archaeological Evidence for the Origin of States in South Central Africa," *Cahiers d'études africaines* 87/88(1982), 349-58, argues, however, for a limited copper trade and the use of copper both for trade and tribute payments. But the existence of tribute payments is only inferred and lacks any substantiation so far.

108. For crops see A.M. Watson, *Agricultural Innovation in the Early Islamic World: The Diffusion of Crops and Farming Techniques*, 700-1100 (Cambridge, 1983). For fowl see McDonald, "Chickens in Africa," 588. Known early dates for eastern and southern Africa are all around 800-1000, except for chickens at Kwagandaganda, ca. 630.

109. Roberts, *History*, 55; B.M. Fagan and D.W. Phillipson, "Sebanzi: The Iron Age Sequence at Lochinvar and the Tonga," *Journal of the Royal Anthropological Institute* 95(1965), 281; Hall, "Changing Past," 81-82.

110. Robertshaw, "Archaeological Survey," 105-32; G. Connah, "The Salt of Bunyoro: Seeking the Origins of an African Kingdom," *Antiquity* 65(1991), 479-94; A. Reid and J. Meredith, "Houses, Pots, and More Cows: The 1991 Excavation Season at Ntusi," *Nyame Akuma*, no. 40(1993), 58-61.

111. P. Robertshaw, "Recent Archaeological Surveys in Western Uganda," *Nyame Akuma*, no. 36(1991), 43-45.

112. R. Tantal, "The Early History of Kitara in Western Uganda: Process Models of Religious and Political Change" (Ph.D., University of Wisconsin, 1989).

113. M.S. Bisson, "Copper Currency in Central Africa: the Archaeological Evidence," *World Archaeology* 6(1975), 272-92, and, more in depth, P. de Maret, "L'évolution monétaire du Shaba central entre le 7^e et le 18^e siècle," *African Economic History* 10(1981), 117-49.

114. See note 70.

115. See note 70, and P. Sinclair, *Space, Time and Social Formation: a Territorial Approach to the Archaeology and Anthropology of Zimbabwe and Mozambique, c. 0-1700 A.D.* (Uppsala, 1987), 122-24, for site hierarchy.

116. Denbow, "Congo to Kalahari," 168-70.

117. Cf. the contributions in P.J. Sinclair and G. Pwiti, eds., *Urban Origins in Eastern Africa: Proceedings of the 1990 Workshop, Harare and Great Zimbabwe* (Stockholm, 1990), or A.K. Segobye, "Archaeological Survey and Excavations in Eastern Botswana," *Nyame Akuma*, no. 38(1992), 2-7.

118. D.P. Collett, A.E. Vines, and E.C. Hughes, "The Chronology of the Valley Enclosures: Implications for the Interpretation of Great Zimbabwe" *AAR* 10(1992), 139-62, a critique of Huffman Symbols in Stone. For diachronic mapping of Great Zimbabwe see Paul J. Sinclair, I. Pirikayi, G. Pwiti, and R. Soper, "Urban Trajectories on the Zimbabwean Plateau" in Shaw, *Archaeology*, 711-14.

119. The notion was fully developed by Gordon V. Childe. His major works such as *The Most Ancient East* (London, 1928) (rewritten as *New Light on the Most Ancient East* [London 1934]) and *Man Makes Himself* (London, 1936) enjoyed huge popularity among intellectuals in general. They accepted the "neolithic revolution" without so much as a murmur of dissent.

120. Cf. my own presentation in P. Curtin et al., *A History of Africa* (Boston, 1978), 1-2. By p. 2, "Once people became sedentary...A history of society and a history of culture now become meaningful."

121. E.g., R.W. Dennell, "The Origins of Crop Agriculture in Europe" in C.W. Cowan and P. J. Watson, eds., *The Origins of Agriculture: An International Perspective*, 71-100; cf. 92, "The concept of the "Neolithic" as signifying the appearance of agriculture probably has done more to obscure than to illuminate the nature of the processes involved." See also other contributions in this volume, and M. Zvelebil, "Les chasseurs pêcheurs de la Scandinavie préhistorique," *La recherche* 23(1992), 982-90.

122. A.S. Brooks and C.C. Smith, "Ishango Revisited: New Age Determinations and Cultural Interpretations," *AAR* 5(1987), 65-78. The relevant dates seem to lie between 20,000 and 30,000 bp.

123. E. Saxon et al., "Results of Recent Investigations at Tamar Hat," *Libyca* 22(1974), 49-51; F. Wendorf and R. Schild, eds., *The Prehistory of Wadi Kubbaniya* (Dallas, 1989). Incidentally, this seems to be earlier than comparable developments in the Middle East. By 12,000 B.C. wild barley was harvested on the floodplain of the Nile at Esna. Cf. Phillipson, *African Archaeology*, 104.

124. *Ibid.*, 112; Devisse, *Vallées du Niger*, 97 and 96 (illustration).

125. F. Wendorf et al., "Saharan Exploitation of Plants 8,000 Years BP," *Nature* 359 (22 October 1992), 721-24. W. Wetterstrom, "Foraging and Farming in Egypt: the Transition From Hunting and Gathering to Horticulture in the Nile Valley" in Shaw, *Archaeology*, 165-226, was written before this discovery occurred, as was K. Wasylkowa et al., "Examination of Botanical Elements from Early Neolithic Houses at Nabta Playa, Western Desert, Egypt, with Special Reference to Sorghum Grains" in *ibid.*, 154-64.

126. A. Muzzolini "Boeuf," *Encyclopédie berbère* 10(1991), 1547-54; *idem.*, "The Emergence of a Food-Producing Economy in the Sahara" in Shaw, *Archaeology*, 227-41.

127. For a map on the putative spread thereafter see P. Breunig et al., "Report on Excavations at Gajiganna, Borno State, Northeast Nigeria," *Nyame Akuma* no. 40(1993), 37.

128. Muzzolini, "Boeuf"

129. *Idem.*, "L'origine des chèvres et moutons domestiques en Afrique. Reconsidération de la thèse diffusioniste traditionnelle," *Empuries* 48/50(1993), 160-71. For a recent overview of animal domestication in general see J. Clutton-Brock, "The Spread of Domestic Animals to Africa" in Shaw, *Archaeology*, 61-70 (The title gives the diffusionist argument away) and R. Blench, "Ethnographic and Linguistic Evidence for the Prehistory of African Ruminant Livestock, Horses, and Ponies" in *ibid.*, 71-103. It is worth noting that Clutton-Brock is now more guarded about the issue of diffusion, given the first results of recent DNA studies on cattle from Senegal.

130. McIntosh, "Changing Perceptions," 169; J. Harlan, "Wild-Grass Seed Harvesting in the Sahara and Sub-Sahara of Africa" in D.R. Harris, and G.C. Hillman, eds., *Foraging and Farming: The Evolution of Plant Exploitation* (London, 1989), 79-96.

131. P.J. Munson, "About 'Economie et société neolithique du Dhar Tichitt (Mauritanie)," *Sahara* 2(1989), 106-08; Phillipson, *African Archaeology*, 129. (This passage does not occur in the first edition).

132. McIntosh "Changing Perceptions," 172-73.

133. J. Vansina, *Paths in the Rainforests* (Madison, 1990), 83-92.

134. These conclusions and this model are not new. Cf. M.T. Hodgen, *Anthropology, History, and Cultural Change* (Tucson, 1974), which sums up the comparison of the author's many studies of well-documented innovations and distributions in literate societies with existing models as a case of "Historical Process versus Natural Law" (65-93). But her demonstrations were not heeded.

135. Phillipson, *African Archaeology*, 130.

136. The debaters about the origins of Pharaonic Egypt and of Egypt's significance for leucoderms and melanoderms alike would do well to remember this. Known inputs into late predynastic Egypt, ca. 3500, involve time depths by then of 2500 years directly, and over 10,000 indirectly, and in areas as remote from each other as the central Sahara, the middle Nile, and Mesopotamia.

137. Controversies over the early dates reflect uneasiness for two reasons: that metallurgy would have been invented independently, and that it did not spread rapidly. But even those who favor late dates cannot avoid the issue completely. Thus Phillipson, *African Archaeology*, 188, speaks "of the middle of the last millennium B.C." for the first appearance, and dates the extension to "early in the Christian era," which allows him to avoid the conclusion that metallurgy was an independent invention here, but it does not address the issue of diffusion. The available dates are set forth by M.C. Van Grunderbeek "Chronologie de l'âge du fer ancien au Burundi, au Rwanda et dans la région des Grands Lacs," *Azania* 27(1992), 53-80.

138. B. Clist, "Synthèse régionale sur l'âge du fer ancien" in R. Lanfranchi and B. Clist, eds., *Aux origines de l'Afrique centrale* (Libreville, 1991), 225.

139. McIntosh, "Changing Perceptions," 173-77; Devisse, *Vallées du Niger*, 33n16; Okafor, "New Evidence on Early Iron-Smelting from Southeastern Nigeria" in Shaw, *Archaeology*, 432-48. On dating see S. Terry Childs and D. Killick, "Indigenous African Metallurgy: Nature and Culture," *Annual Review of Anthropology* 22(1993), 320, who hedge by dating all the early sites to "the interval 500-1000 ca. 1 BC but then (ibid., 321) doubt the possibility of independent invention (too difficult) and blithely invoke a reason (old wood) to distrust all dates older than 500 B.C.

140. Connah, *Three Thousand Years*, 146-47.

141. For archeology see, for instance, Phillipson, *African Archaeology*, 1-23, and, more blatantly, Cornevin, *Archéologie africaine*, 17-33, which jumps from a tale of biological evolution straight to ca. 9000 B.C. In historical textbooks and works of reference the practice is general, except for Curtin et al., *African History*. For a recent example see R. Oliver, *The African Experience* (London, 1991), 1-26.

142. One should remember that natural evolution occurs in discrete quantum jumps and not along a changing continuum. Although we do not exactly know what the last jump to full humanity entailed, the capacity to symbolize and the language instinct are certainly involved.

143. Phillipson, *African Archaeology*, 100. This concludes a paragraph summarizing results derived from mitochondrial DNA. Note his unwarranted leap from a biological category to a cultural (post-Acheulian") one.

144. McIntosh, "Changing Perceptions," 181; P.L. Shinnie and F. Kense, *Archaeology of Gonja, Ghana: Excavations at Daboya* (Calgary, 1989); A.B. Stahl, "Innovation, Diffusion, and Culture Contact: The Holocene Archaeology of Ghana," *Journal of World Prehistory* 8(1994), 51-112.

145. E.g., C. DeCorse, "Culture contact, Continuity, and Change on the Gold Coast, AD 1400-1900," *AAR* 10(1988), 163-96. For an instance of dramatic change see Mgungundlovu, Dingane's capital, and especially its military barracks. J.E. Parkington and M. Cronin, "The Size and Layout of Mgungundlovu, 1829-1838," *South African Archaeological Society: Goodwin Series*, 3(1979), 133-48.

146. McIntosh, "Changing Perceptions," 172-73.

147. E. Kofi Agorsah, "Ethnoarchaeology: The Search for a Self-Corrective Approach to the Study of Past Human Behaviour," *AAR* 8(1990), 189-208, sidesteps the whole question.

148. Cf E. Bassani, *Un Cappuccino nell'Africa nera del seicento: I disegni dei Manoscritti Araldi del Padre Giovanni Antonio Cavazzi da Montecuccolo* (Milan, 1987), which presents drawings executed from sketches made in Angola before 1667. For hoes see drawing #20, and #52 for a recent specimen. Cavazzi also drew a woman hoeing her field as an illustration in his *Istorica descrizione de tre Regni Congo, Matamba e Angola* (Bologna, 1687).

149. Vansina, *Paths in the Rainforests*, 11-16.