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Ways of looking at the past

Early in the present century a British professor of history, A. P. Newton, a man who had specialized in the history of those parts of the world into which Europe had expanded, made the astounding claim that Africa had had no history before it was colonized by Europeans. Apparently, he believed that there was no such thing as African history because, he said, 'history only begins when men take to writing' (Newton 1923). This attitude showed not only an ignorance of those written sources that undoubtedly exist for some parts of pre-colonial Africa but it demonstrated also an incredibly narrow approach to the study of Man's past. Happily, the stark Eurocentrism of such a view is now less common and one should recollect that it was not Africa alone that suffered from such an attitude. The myopic insistence that text-bound history is the only sort of history still has many adherents, however. Even in African universities, the concept of history as being only the study of written sources about the past is still so strong, that the attention of students is often focussed on the last few centuries and they acquire little appreciation of the prehistoric heritage of African peoples. This, in a continent where the known history of Man is longer than that of any other part of the world, is astonishing but it is true.

It all depends, of course, on what you mean by the word 'history'. Over the last century and a half it has become common to distinguish carefully between history and prehistory. To some extent this has been a separation of methodological necessity; the methods of the historian are very different from those of the prehistorian and in some parts of the world prehistory has become almost the sole prerogative of the archaeologist. It is the difference between historical and archaeological research techniques that creates the real division and at least one writer, D. P. Dymond (1974), has called for what he terms a 'reconciliation' between archaeology and history. He has pleaded for what he calls 'total archaeology'; I would change this to a plea for total history. He has written from the point of view of the study of the British past; I am approaching the problem from the point of view of the study of the African past. In some parts of the African continent prehistory ended long before it did in north-western Europe, in other parts it ended little more than a lifetime ago. The African historian who ignores prehistory denies a vast wealth of cultural heritage,

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the African prehistorian who shuns history misses the true relevance of his or her findings. Archaeological evidence is capable of knitting these two aspects of our study of the past together: the trader's gin bottle is as much archaeological evidence as the ground stone axe. Written evidence, particularly that of ethnohistory, can provide a vital control against which the archaeologist can compare his or her evidence. The study of Man's past should be indivisible and there can be no such thing as an historical time before history: providing that we define history as the whole natural history of Man and not his written history only. Thus to write about Africa's past it is essential to step over some of the traditional academic boundaries.

The varied nature of our sources about the African past dictates this state of affairs. The variety of techniques that can be used for exploring Africa's past have been discussed in a series of essays edited by Creighton Gabel and Norman R. Bennett which resulted from a symposium held at Northwestern University in the United States of America, in November 1962 (Gabel & Bennett 1967). Those essays expressly excluded historiography, not because it was thought to be worthless but because so much had been written about it elsewhere. Obviously, where we have written records, either those of outsiders such as travellers, traders, missionaries, and colonial government officers, or those of indigenous observers, we will make grateful use of them, but alone they will not be enough. Africa's written history is geographically patchy and often of limited time-depth. The essays in question explored, therefore, an astonishing variety of other disciplines: archaeological sources were given pride of place as the first essay but there followed studies of ethnology, linguistics, oral tradition, music, art, the sickle-cell gene, botany, the biosystematics of cultivated plants, and food economies, and the editors regretted their omission of human ecology, epidemiology, physical anthropology other than genetics, zoology, palynology, and folklore. It appears then, that the writing of African history in the broadest sense requires the synthesis of extraordinarily diverse data from a variety of sources. Inevitably the result will be a mixture of good and bad and perhaps will ignore certain types of evidence altogether. Thus, I have attempted to reconstruct the whole of human history for a particular part of Africa using whatever evidence is available. Because I am an archaeologist, my handling of some of the other types of evidence possibly falls short of what a specialist in any one of those subject areas would wish to see. Also, I will be found to give more emphasis to archaeological data than to other types of evidence. Nevertheless, I have cast my evidential net as wide as I am able, because it is my wish not to write an archaeology of the area, nor even a prehistory of the area, but an account, in so far as we can construct it at the moment, of Man's experiences in the area. This book concerns roughly 3000 years in the Lake Chad region of Nigeria; it is an attempt to understand the past, it is an

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account of what happened and how it happened. It is at least partly an answer to one historian of Africa, who some years ago is reported to have told his students that archaeologists were merely technicians, who would never contribute anything new to our historical knowledge of the past.

Granted that our task is the reconstruction of the past in the broadest possible sense, how are we to go about it? There is no self-evident truth about the past but merely a series of possible truths depending on the way we employ our 'historical imagination', as R. G. Collingwood called it. It was Collingwood also who compared historians to landscape-painters and remarked that 'it is the artist, and not nature, that is responsible for what goes into the picture' (Collingwood 1946:236). The way that we paint our picture will clearly depend on the governing concepts that we use to explain the past. In recent years it has become usual to identify particular conceptual structures that we call 'models' (Chorley & Haggett 1967) and at least the recognition of the model that we adopt should prevent us from falling hopelessly in love with the results of its application. The late David Clarke, who contributed so much to the application of models to archaeology, defined models as 'pieces of machinery that relate observations to theoretical ideas' (Clarke 1972:1). He went on to say:

> 'In general, models serve as heuristic devices for manipulating observations and hypotheses; they may also act as visualizing devices, comparative devices, organizational devices, explanatory devices or devices for the construction and development of theory. Models are usually idealized representations of observations, they are structured, they are selective, they simplify, they specify a field of interest and they offer a partially accurate predictive framework.' (Clarke 1972:2)

One of the reasons why archaeologists should concern themselves with models, according to Clarke, was:

> 'The construction, testing, verification or refutation and modification of explicit models is the essence of the empirical and scientific approaches – providing the progressive cycle by means of which fresh information and insight are gained and theory is accumulated. Observations, hypothesis, experiment, conclusions, fresh hypothesis, fresh observations...' (Clarke 1972:3)

The way in which we reconstruct the past from our collection of diverse evidence will, in fact, depend on the model we adopt. We should not merely accept the model which happens to be currently popular, it is up to us to select the model which seems best suited for the task in hand, and it is essential that we make it quite clear which model we are using.

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It is necessary to understand what David Clarke meant when, in a characteristic pun, he claimed that the history of archaeology is 'a history of "paradigms lost"' (Clarke 1972:8). A man who has written much about the development of archaeology is Glyn Daniel and it is interesting to see how he has applied the models approach to the writing of prehistory (Daniel 1971). It is instructive to examine some of the models discussed by Daniel and observe how they have been employed in African archaeology, particularly in the archaeology of Nigeria.

In his writings Daniel has given considerable space to the discussion of the Three-Age technological model of C. J. Thomsen and of its various modifications during the century and a half that it has remained with us. With hindsight, we can now see that this techno-epochal approach was an understandable conceptual tool in the hands of scholars who were steeped in the notion of technological progress that emanated from the so-called Industrial Revolution and were fired with the evolutionary zeal of Darwin's followers. In its day the Thomsen model had enormous influence because it produced an ordered time-depth where previously there had been chaos. In 1921 R. A. S. Macalister could refer to it as 'the corner-stone of modern Archaeology' (Macalister 1921:11) but even he realized that it was 'nothing more than a working hypothesis' and not the historical fact that it virtually became in much archaeological literature. Many years ago Daniel recognized the difficulty that this model meant different things to different people and he issued a warning concerning its future use: 'This system has indeed been the foundation stone of modern archaeology; it is for us to say whether it is to become a mill-stone hanging around the necks of future archaeologists' (Daniel 1943:60). A glance at African archaeology is sufficient to show that the mill-stone is very much with us. Archaeologists still waste time debating what they mean by terms such as 'Neolithic', 'Middle Stone Age', 'épipaléolithique' and so on, and at one time we nearly got saddled with 'siderolithic' (Elisofon & W. Fagg 1958:58; W. Fagg 1963:24). Some authors still write about 'Stone Age assemblages' (Swartz 1974) or 'skeletons of late Iron Age date' (Wade 1971:61). In fact the use of this techno-epochal model is still so widespread that many writers retain its nomenclature because of the confusion that might result from abandoning it. Thus, the first monograph on Nigerian archaeology makes a courageous attempt to combine this model with others of a more flexible nature (Shaw 1978) and, indeed, in my own writings also there can be found the characteristic terminology and thought patterns. In this book I intend to escape from them; as Glyn Daniel has written, 'The technological model produced depth in prehistory, an approach to the prehistoric past, but it did not, of itself, explain the changes in the material culture of man' (Daniel 1971:141). Surely, our business is with explanation? If we are to be forced to make that choice between the 'culture history' and 'cultural process' approach that was offered us by

K. V. Flannery (1967), then many of us would now incline towards process, providing we are allowed to write about it in plain English.

Daniel has also discussed, in some detail, the second great model of the prehistoric past that has influenced the development of archaeology. This is the Gordon Childe model of culture-groupings and modified diffusionism, a model that by the middle of the twentieth century had become 'orthodox archaeology' (Daniel 1971:149). As Daniel claimed: 'The idea of a culture and cultures was a methodological device which helped us over the gap from the discrediting of the epochal idea to a time when exact dates were possible.' The Childe model was an extremely important one: at its best it produced a structured and explanatory interpretation of the prehistoric past that the increasing numbers of interested readers could comprehend; at its worst it spawned an artefact-bound view of the past in which the common items of day-to-day life took on an almost anthropomorphic quality. The Childe model has gradually receded into the background since the late 1950s but for a while it also influenced the reconstruction of the African past, the traces of its influence can still be found. At a general level, archaeologists working in Africa tended to remain modified diffusionists, so that the traditional explanation for the development of food-production in any part of Africa for a long time tended to be one of external influence, and the adoption of iron-working in any particular place has often been explained in a similar fashion. Too often the question: 'How did it happen?' has been turned into 'Where did it come from?' At a more particular level, the culture-grouping aspect of the model had a certain amount of influence also. Thus, seeking to identify a group of Nigerian terracottas that seemed to have a distinctive distribution in space and time, Bernard Fagg invented the term 'Nok Culture' (B. E. B. Fagg 1956, 1959). Although inadequately supported by either stratigraphic or statistical data, the concept rapidly became entrenched in archaeological mythology, so that there is hardly a schoolchild in Nigeria who has not heard of the 'Nok Culture'. There are other examples scattered across the African continent: the 'Kintampo Culture' in Ghana (Davies 1962, 1967:216–22), the 'Kalomo Culture' in Zambia (Fagan 1967), and the 'Uelian Culture' in Zaïre (van Noten 1968). As long ago as 1967 it was proposed that the term 'Culture' should be replaced by the term 'Industrial Complex' as part of a general review of archaeological nomenclature in Africa (Bishop & J. D. Clark 1967:893). Some, like Inskeep (1967), protested that the subject was dynamic but the new interpretative model no less static than the old; others, like myself (Connah 1967a), merely expressed their preference for the devil they knew rather than the one they did not know. Indeed, as late as 1971, Willett was writing about an 'Ife Culture' and a 'Benin Culture' (Willett 1971a).

Gone are the days, however, when archaeological students could dream of identifying and naming new cultures rather as a botanist might identify

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and name new species. Post-1960 archaeologists have been forced to rethink Childe's idea of prehistory and as Daniel has written:

> 'The idea of prehistoric cultures is collapsing under the new structure of accurately dated prehistoric contexts...now we wish to describe prehistoric communities in their geographical, environmental and historical contexts without giving them cultural labels.' (Daniel 1971:149)

During the 1960s archaeology passed through a period of more rapid change than had been the case for some decades. There were, no doubt, many reasons for this movement that became known as the 'New Archaeology', but there were two particular causes which seem to have been of notable significance. First, by about 1960 enough radiocarbon dates had become available to allow the first worthwhile attempt to write world prehistory; Grahame Clark published the first edition of his *World prehistory* in 1961 (J. G. D. Clark 1961). The radiocarbon revolution, as it has sometimes been called, released archaeologists from traditional theoretical frameworks which had primarily been concerned with the problem of dating. For the first time it was possible to give priority to the questions 'How?' and 'Why?' rather than 'When?'. Second, the 1950s to some extent, and the 1960s to a far greater extent, saw an enormous increase in the number of properly trained professional archaeologists. This affected many parts of the world and led to the sort of questioning of accepted ideas that a large infusion of young people into any subject is likely to bring about. This questioning was at first most evident in America, particularly with Lewis Binford and his followers (S. R. Binford & L. R. Binford 1968; L. R. Binford 1972:1–14) but the late David Clarke in Britain contributed also (Clarke 1968, 1972). Furthermore, one of the most successful strands of the 'New Archaeology' was the development of an ecologically-based 'economic prehistory' by the late Eric Higgs, an English archaeologist who was much older than most of the innovators (Higgs 1972, 1975). There have been many attempts to define the 'New Archaeology' and it should be apparent that I interpret the phrase very broadly. Clarke (1972:53–7) attempted to compare what he called the 'New' and 'Traditionalist' archaeologies and said of the 'New Archaeology' that:

> 'It should be remembered that a healthy discipline is defined not only by its particular body of observations but by the vigorous network of theory and methodology reticulating its parts. Indeed, it is the corpus of theory and methodology of greater than regional and sectarian importance that is the vital element that serves to coagulate those parts within the whole of one discipline.' (Clarke 1972:56–7)

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J. N. Hill (1972:62) went further and identified 'the crux of the methodological debate' as: 'the question of whether our research should proceed in a primarily *inductive* manner, or whether we should emphasize a *deductive* procedure'. In a convincing piece of argument Hill contrasted what he called the 'vacuum cleaner approach' of the inductive method with its rather rigid procedure of excavation – classification and dating – analysis – interpretation (Hill 1972:67) with the more flexible 'hypothetico-deductive method' in which there should be a 'continuous feedback between data and hypotheses, with each being modified as necessary' (Hill 1972:69).

How does all this affect our search for a model for the African past? Clarke (1972:6–7) thought that it was possible to identify four '"new" paradigms' in contemporary archaeology which were gradually replacing the old ones. He characterized these as (1) the morphological paradigm; (2) the anthropological paradigm; (3) the ecological paradigm; (4) the geographical paradigm. What are those of us who study archaeologically unknown areas to do? Certainly we ought to realize that it may have been some of the archaeology of new areas of research like Africa that Clarke (1972:8) was thinking of when he remarked: 'some archaeologists are still content to work in terms of nineteenth-century paradigms where only the inevitable freshness of new material prevents them from actually writing nineteenth-century textbooks.' Faced with a similar situation to ourselves, Higham (1972) gave an interesting example of what he termed 'initial model formulation *in terra incognita*'. He first affirmed the usefulness of a 'generalized cultural model' which stressed 'the importance of the interaction between core-cultural variables, such as the economy, technology and social structure on the one hand, and the constraining environment on the other' (Higham 1972:454). He then went on to develop a specific model from the research that had been conducted in the particular area that he was discussing, situated in his case in south-east Asia. I also would affirm the usefulness of the generalized cultural model and would agree that the specific model adopted for a particular piece of field research ought to arise from the circumstances in the area concerned.

Archaeology was a late development in Nigeria (Shaw 1969). The first person to be employed as a professional archaeologist in the country was Bernard Fagg, who was appointed as Government Archaeologist in 1947 (Nigeria 1948). Fagg's appointment was to the staff of the Nigerian Government Antiquities Section which had grown from small beginnings in 1943 when K. C. Murray had been seconded from the Education Department to the Public Relations Office of the Nigerian Government 'in order to tour and make a report on the practical steps to be taken to preserve the known antiquities of Nigeria' (Nigeria 1947:1). That wording was to influence the shape of archaeological research in the country for some years to come. As early as 1939 K. C. Murray had been urging the

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creation of a Nigerian national museum to preserve items of artistic importance (Murray 1939) and it was the preservation and public presentation of cultural material, particularly artistic material, that demanded all the available energies of the people involved in the new Antiquities Section. Nigeria is a huge country and huge quantities of art objects were threatened with destruction as an acceleration in the rate of culture change occurred during the 1940s and 1950s: in 1947 it was predicted that for many things 'ten years from now will be too late' (Nigeria 1947:2). Known items had to be preserved, other items had to be rescued from mining, building and other development activities; museums had to be built, sites had to be protected and the public had to be persuaded of the importance of their own cultural heritage. When archaeological research could be done at all, it tended to be motivated by the same necessity to preserve or acquire items of artistic importance: thus fieldwork was concentrated on the tin-mining area which had produced the Nok terracottas and excavation was concentrated on Ife, Benin and Igbo-Ukwu. When ethnographic research could be done it tended to concentrate on wood-carving or 'bronze-casting' or other media of artistic expression. These remarks should not be thought of as a criticism, for these tendencies were the unavoidable first stage of a pioneer situation. So far as archaeology is concerned, there is no doubt at all that it was Bernard Fagg who provided the vital triggering mechanism for the development of the subject in Nigeria.

By the early 1960s Nigerian archaeology began to enter a new phase. The number of professional archaeologists in Nigeria rose suddenly in the first few years of Nigerian independence and the availability of radiocarbon dating improved at about the same time. By this time there were a number of public museums in Nigeria and a small but active staff to administer them in the form of the Federal Department of Antiquities of Nigeria. Not only did that staff take on more archaeologists but the new Nigerian universities began to show an interest also. One of the first to do so was the University of Ibadan which in 1963 appointed Thurstan Shaw to a research professorship in archaeology. I was one of those who arrived in Nigeria during those years, in 1961 to be precise, and I remember well the principal problem that required archaeological attention at that time. Quite simply there was very little that we could date with certainty and so for some years the emphasis of archaeological research in Nigeria became chronological: stratification, pottery sequences, datable imports, radiocarbon dating; anything that could help us to construct the vitally necessary timetables without which we could not begin to interpret the past. My own work in Benin City, of which the final study was not published until 1975 (Connah 1975), reflects this emphasis on chronology in the early 1960s: it represents an approach that may be characterized as a search for time-depth. At that time, however, one of the major difficulties

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we experienced in Nigerian archaeology was what appeared to be a lack of deep stratified occupation deposits. The shifting cultivation of much of West Africa seemed to deprive us of detailed chronological information about the development of human settlement. There seemed to be two main methods of dealing with this difficulty: first, to excavate rock-shelter sites where vertical accretion of deposits was assured, hence Thurstan Shaw's work at Iwo Eleru in 1965 (Shaw n.d. [1973]); second, to excavate settlement mounds in those relatively few areas where they were known to occur, hence the work of Priddy, Hartle and Breternitz in the Niger Valley and of myself in Borno (Priddy 1970a, b; Hartle 1970; Breternitz 1975; Connah 1976). It was, in fact, a search for settlement mounds that originally took me to Borno in 1963, following a conversation the year before with Hamo Sassoon, who had been a District Officer at Bama in the 1950s.

Time is hardly a model in itself, however, and it quickly became apparent that in the Lake Chad region of Nigeria the most important things to be learnt from the past concerned the dynamic relationship of Man and environment. The region is an agriculturally marginal one, it is a region where stress and opportunity intertwine in a complex manner. The very existence of the mounds that I set out to investigate indicated this, indicated situations where Man's settlements had been focussed on the same occupation sites for as long as three millennia in some cases. Within the archaeological evidence lay hidden a fascinating account of Man's successful exploitation of several remarkably trying environments. Neither the techno-epochal model nor the culture-grouping model, both of them artefact-bound approaches to the past, seemed likely to extract that account in any coherent fashion. The model that must be employed was, it appeared, dictated by the circumstances of the evidence: it must be drawn at least partly from the ecological paradigm that Clarke (1972:7) has identified as one of the four 'new' paradigms of archaeology. An appreciation of the importance of environment and of Man's interaction with it is nothing new in archaeology, however. O. G. S. Crawford in his book *Man and his past*, written as long ago as 1921, included several chapters on environment (Crawford 1921). Cyril Fox, in a seminal work of 1932 called *The personality of Britain*, produced an almost deterministic environmental theory that has had long-standing and extensive influence (Fox 1932). The model that I propose to employ in this book is ecological rather than environmental, however. It is the *interaction* of Man with his environment that is important. Although Dimbleby (1977:1) has complained of the failure of some ecologists when writing introductory books to see Man 'as a component organism in the ecosystem' there are nevertheless those ecologists who are under no such misapprehension. Kormondy (1976:191), for instance, has written: 'Humans are but one of the interacting structures in ecosystems – contributors, participants, and

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receivers in ecological processes, just another species functioning in the flow of energy and the cycling of nutrients, whose population is subject to growth as well as regulation.' Neither the nature of my fieldwork, spread over some fifteen years in the area under discussion, nor the nature of my own skills, will allow me to embark on such things as quantified catchment analyses, locational analyses, or measurements of energy flow and activity efficiency. Nevertheless, my model for the past will be partly an ecological one.

I wish to emphasize also the importance of an ethnographic approach to the study of the past and within the term 'ethnographic' I include the use of ethnohistorical material. At one time, particularly in the 1950s, archaeologists shied away from the use of 'ethnographic analogy', and they had good reason for there had been many examples of its gross misuse. However, I have no intention of being drawn into what Clarke (1972:40) has called 'the morass of debate about the proper and improper use of historical and ethnographic "parallels" in archaeological interpretation'. Like him I see ethnography as a useful way of developing models about the past, and it is particularly useful in an area such as the one under study in this book. For the last 150 years a succession of observant Europeans have travelled through this area and recorded the manner of life of its peoples: Denham, Clapperton and Oudney (1831); Barth (1857–8); Rohlfs (1868–72); Nachtigal (1879–89); B. Alexander (1907); Macleod (1912); Migeod (1924); these and others have left us a mass of ethnohistorical information. Ethnohistorical writings are not confined to European visitors, however; the sixteenth-century Borno scholar Ahmed ibn Fartua (ibn Fartua 1926) in his account of the first twelve years of the reign of the Mai Idris Aloma supplies us with quite a lot of circumstqntial ethnographic information. Not only this but also the fact that the traditional Man/land relationships persist, at least in their essentials, in most parts of the Lake Chad region of Nigeria have made it possible for me to study them directly with my own archaeological problems in mind.

In recent years an interest has developed in what is termed 'ethnoarchaeology'. A particularly important contribution has been made to this study by R. A. Gould working in Western Australia (Gould 1971; Gould 1980). In this approach ethnography is seen as a means not only of helping to ask the right questions but also of helping to answer those questions. It is not entirely a new development: O. G. S. Crawford was almost obsessed by what he called 'the past in the present' (Crawford 1953), wording that he probably borrowed from Arthur Mitchell's writing of the late nineteenth century (Mitchell 1881). Crawford saw the main task of the archaeologist as being 'the reconstruction of past life' (Crawford 1953:225) and seemed to see 'a continuum in time and space' in human society (Crawford 1953:33). In his book *Archaeology in the field*, he