

There has long been a need for archaeologists and anthropologists to correlate their ideas and methods for interpreting the material culture of past civilisations. Archaeological interpretation of the past is inevitably based on the ideas and experiences of the present and the use of such ethnographic analogy has been widely adapted—and criticised.

In this challenging study, Ian Hodder questions the existing assumptions, values and methods and shows how anthropology can be applied to archaeology. He examines the criteria for the proper use of analogy and emphasises the need to consider the meaning and interpretation of material cultures within the total social and cultural contexts. Throughout, he emphasises the need for a truly scientific approach and a critical self-awareness by archaeologists, who should be prepared to study their own attitudes to the present-day material world.

Dr Hodder is Lecturer in Archaeology at the University of Cambridge.

"...this is a powerful and well written book that will clearly become the standard university textbook on this subject."
Current Archaeology

"...Ian Hodder questions the application of the techniques of anthropology as they have hitherto been applied in archaeology and establishes basic rules for their proper utilisation. It is a salutary yet constructive corrective to some recent excesses and abuses." *BCA Newsletter*

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IAN HODDER

THE PRESENT PAST

Introduction to Anthropology for Archaeologists



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For the joy of little Greg

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research does not involve confusing our methods with our aims. Rather, the programme is concerned to identify, in dissatisfaction with the evolutionary approaches seeking laws of societal change, new aims for archaeology.

I The use of analogy

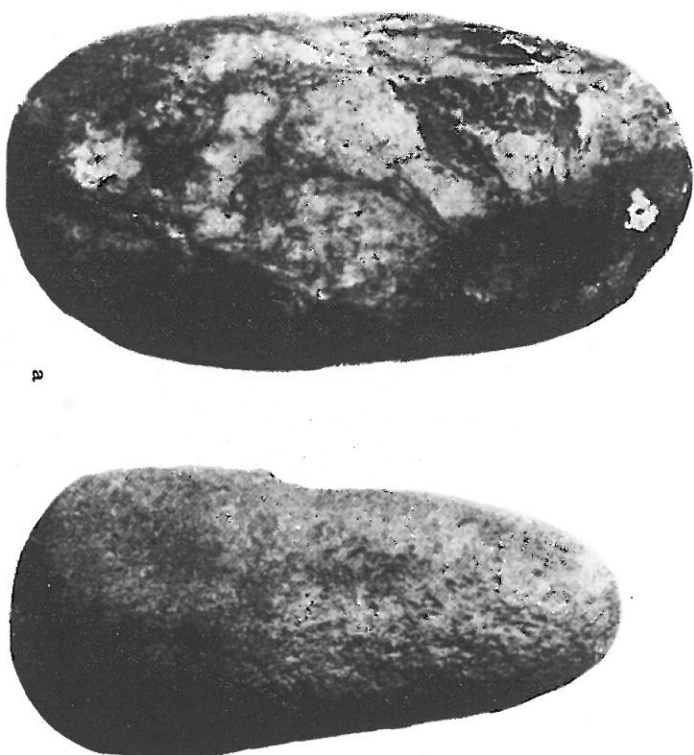
When an archaeologist digs an object out of the ground and says 'this is an axe', how does he know? He may have dated the site from which the object comes to millennia before the first written records, so how does he know it is an axe? The answer is, really, that he doesn't. All he can do is make a reasonable guess based on the fact that the object he has in his hand from the past looks like axes he has seen in his own or in other contemporary societies. In the first half of the nineteenth century, the Danish scientist P. W. Lund sent back polished stone axes from Brazil to be compared with Danish antiquities (*fig. 1a*). This information supported early Scandinavian archaeologists such as Nilsson in their notion that similar artifacts found on their soil were indeed 'primitive' axes. Polished stone objects found widely in Europe (*fig. 1b*) have the shapes of axes with sharp edges; they are called axes on analogy with modern objects.

To support the axe interpretation the archaeologist might examine the edge of the artifact with a microscope to show that it has traces of wear from cutting; he might conduct experiments to show that it is possible to cut down a tree with such an object; and he could conduct pollen studies of the environment of the prehistoric site where the object was found to show that trees had been cut down in the vicinity. Yet all these subsidiary studies are developed to support or weaken the initial analogy.

Much the same could be said for nearly every other interpretation that the archaeologist makes about the past. He is rarely shown directly what prehistoric objects were used for. He has to guess, using analogies. So, when he finds a circle of post holes in the ground and says 'this is a house', he is influenced by evidence of modern round houses lived in by many Africans and American Indians (*fig. 2*).

Many such interpretations may seem obvious to the archaeologist and he may be unaware that he is using an analogy at all. It needs no specialist knowledge to describe an object as an axe, pin, dagger, sword, shield or helmet. And the notion that circles of post-holes indicate houses is so deeply enshrined in archaeological teaching that the archaeologist may not question the ethnographic origin of the idea. Other uses of ethnographic analogies are more conscious and calculated. When an attempt is made to reconstruct from fragments of archaeological evidence the organisation of prehistoric social relations, exchange, burial ritual and ideologies, the archaeologist searches among traditional societies today, in Africa, Asia and America, or in historic Europe, for suitable parallels and analogies.

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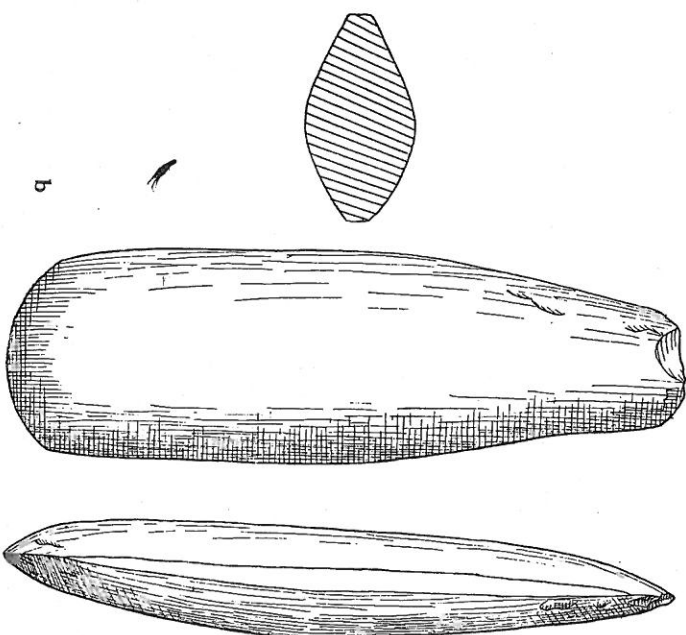
1 Archaeological and ethnographic stone axes. **a** Polished axes from Brazil sent by Lund to the Royal Society of Northern Antiquaries in Copenhagen (Klindt-Jensen 1976). **b** Neolithic stone axe from Cambridgeshire, England

So, in nearly every interpretation he makes of the past, the archaeologist has to draw on knowledge of his own or other contemporary societies in order to clothe the skeleton remains from the past in the flesh and blood of living, functioning and acting people. The drawing of analogies would thus seem to play a central role in archaeological reasoning. Yet recently, many archaeologists have tried to diminish the use of analogies because they are unreliable, unscientific and limiting. Why is this?

Problems with analogies

An interpretation of the past using an analogy is thought to be unreliable because, if things and societies in the present and past are similar in some aspects, this does not necessarily mean they are similar in others. As an example of this difficulty we might take Grahame Clark's (1954) reconstruction of life at the Mesolithic site of Star Carr

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in Yorkshire, England. Clark suggests (1954: 10-12) that the evidence of skin-working at Star Carr argues for the presence of women on analogy with the hunting peoples of North America and Greenland. Amongst the Caribou Eskimos women are mainly responsible for flaying the kill and preparing the skins for use, and Clark suggests the same for his Mesolithic hunter-gatherers from Yorkshire. On the basis of the presence of women, Clark further postulates the number of people living at Star Carr, and the form of subdivision of labour. The environment, technology and hunting-gathering economy of the Star Carr inhabitants and the Eskimos are, very vaguely and broadly, comparable. But are these general similarities in some spheres sufficient to allow us to infer correlations in others? There seems no reason to suppose that skin working amongst modern Eskimos and at 7200 bc Star Carr was carried out by the same sex just because they both had broadly the same economy and environment. Clark's interpretation seems highly unreliable.

Clark, in the Star Carr example, seems to be assuming a deterministic 'uniformitarianism'; he supposes that societies and cultures similar in some aspects are uniformly similar. Such a view is unreliable, especially given the great spans of time over which the comparisons are being made and given the great diversity of present-day cultural forms.

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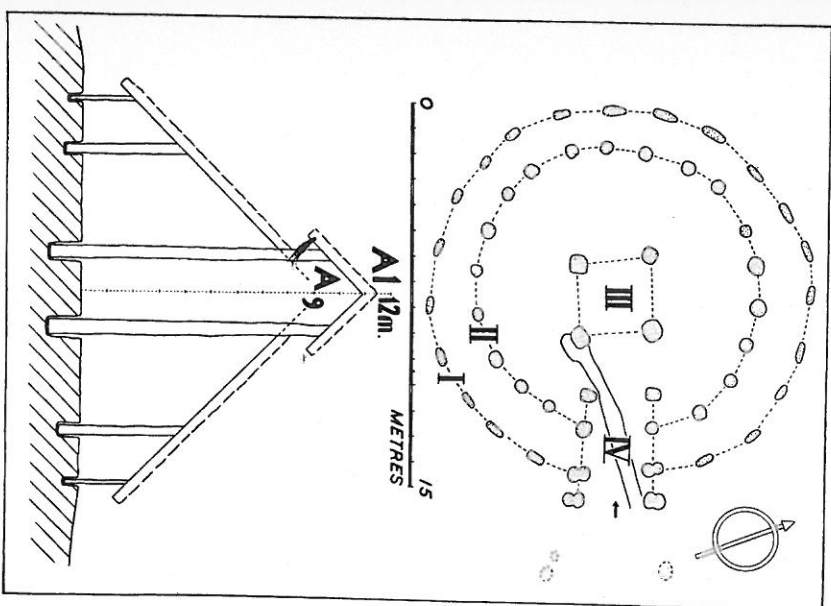
Clark happened to use the North American Eskimos, but he could have chosen other societies where men prepare the skins.

If analogies are unreliable, they must also be unscientific. Or so the argument goes. It has been implied by many archaeologists recently that we can never check or prove analogies because we can always find alternative analogies which would fit the data from the past equally well. Because similarities in some aspects do not necessarily, certainly or logically imply similarities in others, we can never prove interpretations such as Grahame Clark's at Star Carr. Such archaeological interpretation is seen as subjective story-writing, pure speculation without any scientific basis.

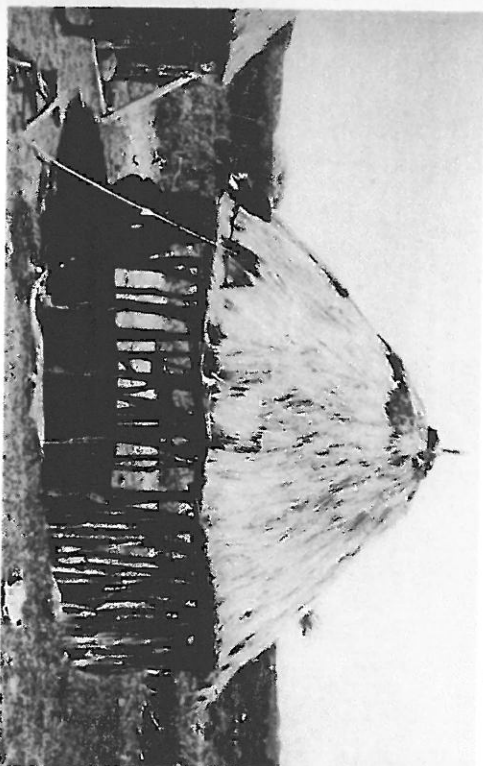
While it will be explained below why charges of unreliability and anti-science are only really relevant to the misuse of analogy, a charge that analogical reasoning limits and restricts our knowledge of the past also needs to be considered. It seems logically to be the case that, if we interpret the past by analogy to the present, we can never find out about forms of society and culture which do not exist today (Dalton 1981). We are limited in our interpretation to our knowledge of present-day societies. Since these societies, industrialised and non-industrialised, represent a highly specialised set of interlinked economic, social and cultural adaptations, it seems unlikely that they adequately represent the full range of social forms that existed in the past. So we would never be able to understand the full variety of prehistoric societies. Anyway, what would be the point of repeating our knowledge of contemporary societies by tagging labels on to societies in the past?

The various criticisms of the use of analogy in archaeology have led to some extreme statements rejecting analogical reasoning in archaeological interpretation. Freeman (1968, 262) claimed that 'the most serious failings in present models for interpreting archaeological evidence are directly related to the fact that they incorporate numerous analogies with modern groups'. While few archaeologists would now go as far as Freeman, it is widely accepted that the use of analogies should be limited. Ucko (1969) has indicated that the use of analogies in interpreting past burial practices using analogies, and Binford (1967) has indicated that analogical reasoning is of little value unless associated with a rigorous, scientific testing procedure. Tringham (1978, 185) also suggests that we should try and move away from discovering analogies which can be fitted to archaeological data, while Gould (1978a) has attempted to move 'beyond analogy'. I will try to show that these assessments of the value of analogy in archaeology are clouded by a misconception of the nature and proper use of analogy. It will then be possible to reassess the supposed unreliable, unscientific and limiting features of analogies.

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2 Circles of post-holes and round houses. a Circles of post holes from the Iron Age site of Little Woodbury, England, reconstructed by Bersu (1940). b A modern round house in Kenya



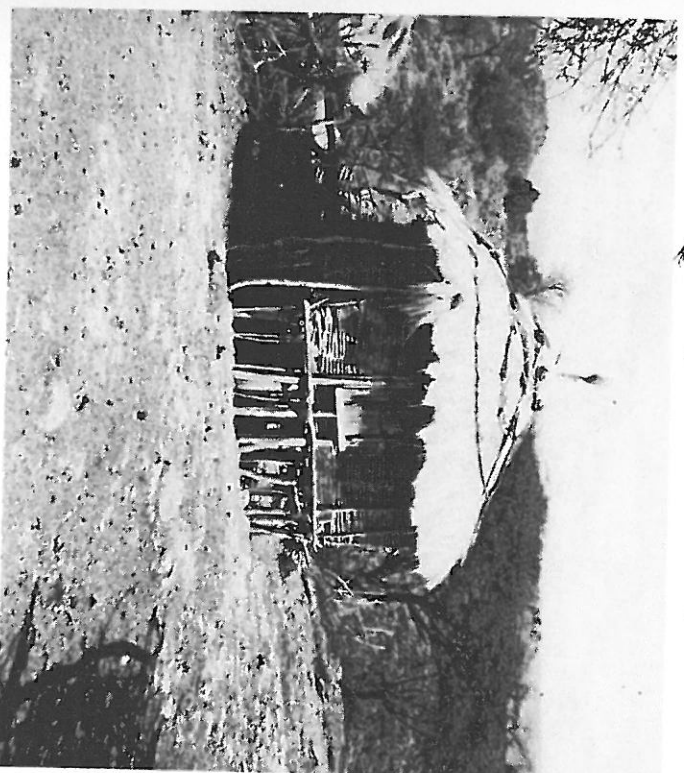
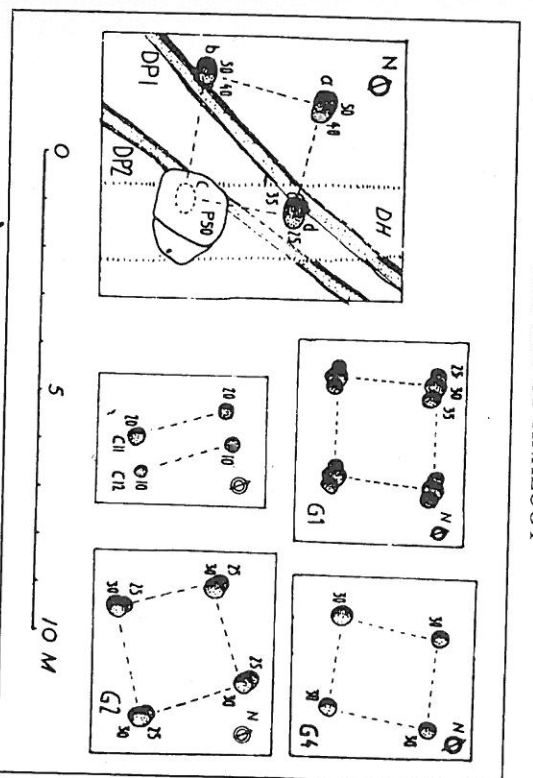
The proper use of analogy

As a form of inference, 'analogy' can be defined as the 'transportation of information from one object to another on the basis of some relation of comparability between them' (Umeov 1970; Wylie 1980). The proper use of such analogies has been discussed by philosophers of science (Hesse 1974; Copi 1954; Umeov 1970) and from their work we can draw some general conclusions (see Wylie 1980).

A distinction can be made between *formal* and *relational* analogies. According to a formal analogy it is suggested that, if two objects or situations have some common properties, they probably also have other similarities. Such analogies are weak in that the observed association of characteristics of the objects or situations may be fortuitous or accidental. So other analogies, of the relational kind, seek to determine some natural or cultural link between the different aspects in the analogy. The various things associated within the analogy are said to be interdependent and not accidentally linked.

There is in fact a continuum of variation from the more formal to the more relational analogy. Unfortunately, most archaeological uses of analogy have tended to cluster at the more formal end of the scale. Clark's interpretation at Star Carr is really a formal analogy in that the Mesolithic site and the Eskimos are said to be similar in certain aspects (environment, technology and economy) and are 'therefore' similar in other aspects (women do the skin preparation). No link is suggested by Clark between the two parts of his analogy; we are not told why females prepare skins in such environments, technologies and economies. Other examples abound. In the late Iron Age in southern England, four or six post-holes are frequently found on hillforts and settlement sites arranged in a square averaging 3m by 3m. Analogies with contemporary and recent societies suggested to Bersu (1940) that these post-holes contained posts holding up granaries with raised floors (fig. 3). The formal similarity in the square of posts in past and present agricultural societies was taken as indicating a similar above-ground function.

How can such formal analogies be strengthened? Most obviously, the more similarities that can be identified between the two situations being compared, the more likely are other similarities to be expected. A formal analogy becomes more reasonable as the number of similarities increases. At some point, the volume and detail of comparable points become so great that it is unreasonable to suppose differences in the few aspects for which comparability is unknown. So, in the Star Carr example, if we could list a very large number of ways in which the prehistoric site was a replica of a modern Eskimo site, similarities in the 'unknowns', including who prepared the skins, might be thought to be expected. Similarities between the 'squareness' of post-hole arrangements in past and present sites could be augmented by examining the detailed dimensions of the post-holes, their



3 The interpretation of four post arrangements as granaries.
a Arrangements of four post-holes at the Iron Age site at Little Woodbury, England (Bersu 1940). b A modern raised granary and store of the Tugen in Kenya. For other modern examples see fig. 30.

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widths and depths, by considering the soil conditions, the presence of vermin and damp which might cause the above-ground storage of grain to be necessary, and by searching for evidence of grain in the post-holes. As the number of similarities between past post-hole arrangements and present above-ground granaries increases, the interpretation of the Iron Age feature in terms of a granary becomes more acceptable.

One way in which the number of formal similarities between a past and present situation can be increased is by comparing a recent archaeological site with modern sites in the same area. Thus, in North America it is sometimes possible to interpret prehistoric palaeo-Indian sites on the basis of analogy with modern or historically recorded surviving Indian groups in the same vicinity. In Africa, also, continuity between archaeological and ethnographic data can frequently be assumed. Such comparisons make up what is called the *direct historical approach* in archaeology. Where continuity between the past and present can be assumed, it is easier to acknowledge many formal similarities between the information being compared.

If the direct historical approach cannot be followed, formal analogies can be further strengthened by a very different strategy. Rather than simply increasing the number of similarities between the source and the subject of the analogy, the range of instances in which various characteristics of the analogy are associated can be emphasised. Thus, if it could be shown that, whatever the environment or economy, skin working was always carried out by women, then Clark's interpretation at Star Carr would have greater validity. Similarly, if it could be shown that patterns of post-holes with particular detailed dimensions were, in modern and historical examples, always associated with above-ground granaries, then the Iron Age interpretation would be more believable. In fact, neither of these two correlations can be made. Yet it remains possible to substantiate the use of an analogy by demonstrating its wide range. In recent archaeology, such wide-ranging statements, valid across different cultures and environments, have sometimes been declared *cross-cultural laws*.

Schiffer, one of the main exponents of the development of cross-cultural laws, or 'correlates', has provided many examples. He describes (1978, 233) the law taken from Yellen's detailed ethnographic studies that the diversity of maintenance activities performed at a settlement varies directly with the length of occupation. Thus the 'law', which in this case is little more than an expression of association, states that the longer a group of people stays in the same place, the more different types of activities it will perform. Schiffer (*ibid.*, 244) shows that this observation has been made independently in a range of very different situations. In suggesting the credibility of the analogy, appeal is made to the great range of societies and cultures

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where the suggested relationship between length of occupation and number of activities holds true.

A third aspect of the strengthening of formal analogies can be identified. The conclusions drawn from the analogy should not be too ambitious in relation to the number of similarities between the source and subject. In Clark's example, very little attention is paid to listing the similarities between Star Carr and Eskimo hunters and yet the interpretations made are highly ambitious. However, a cautious and modest interpretation based on a large number of correspondences may be seen to be more acceptable.

However much we might reinforce our analogies by increasing the number and range of similarities between past and present objects and situations, comparisons made at the formal end of the scale will always remain shrouded in unreliability and will always remain easy prey for ridicule and for charges of anti-science. In particular, it will always be possible to provide examples where the suggested association, or correlation breaks down. The depressing tone of Ucko's (1966) article on analogies for the interpretation of burial remains derives from the ease with which he can show that various aspects of burial, such as cremation, the body lying looking to the east, or the presence of rich graves, may mean very different things in different societies. The recent archaeological literature abounds with 'cautionary tales' which show how easily archaeologists can make mistakes in using particular analogies since alternative interpretations can be found. Archaeologists have delighted in visiting recently abandoned camps, interpreting the remains as archaeologists, and then asking the inhabitants of the site what actually occurred there. Invariably, the archaeologists' interpretations are shown to be wrong.

All such pessimism and caution derive from an overemphasis on formal as opposed to relational analogies. Of course, different analogies can be found to fit the same set of archaeological data if we simply look for formal similarities in the arrangement of post-holes and if we simply list associations of traits. Analogies, from wherever they are derived, must be deemed to be relevant to the subject of study in the past. I have applied Central Place Theory, derived from modern locational studies of the arrangement of towns in Germany, to Roman Britain. This use of analogy has been rightly criticised (Hodder 1975) because it is irrelevant to pre-industrialised societies such as existed in Roman Britain.

Relational analogies are thus concerned to demonstrate that similarities between past and present situations are relevant to the 'unknowns' that are being interpreted, whereas the differences that can be observed do not really matter; they are not really relevant because there is little link between what is different and what is suggested as being the same. Archaeological uses of analogy often do imply, although they rarely state, some necessary relationship between

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the various aspects of the analogies. But the relationships that are suggested are nearly always functional. Examination of four-post above-ground granaries indicates that they may function to keep out vermin and to keep the grain dry. The longer a site is occupied, the more functions are necessarily carried out there and so a wider range of debris and waste products is found.

The attempts which have been made in archaeology to use and develop relational analogies have aimed to define the contexts in which different properties of societies and cultures are functionally interdependent. It is thought to be possible to demonstrate that 'in certain conditions C, if A, then necessarily B'. The supposition is made that the material culture which the archaeologist digs up functioned in a particular way. So if, in the past, a group of people had a certain style of life, then material culture was used in particular ways; material culture patterning can be predicted because of its functional relationships with other aspects of life.

For many, the existence of such predictable functional links has meant that analogies can be scientifically tested against archaeological data. It is suggested (e.g. Gould 1978; Tringham 1978) that we can find present-day analogies for our data from the past, but that the unreliability of these analogies can be decreased by 'testing' their consequences in the material remains. This widely held illusion that the charge of anti-science can be avoided by a particular form of 'testing' of analogy against data derives from Binford's much quoted example of the use of analogy in archaeology. It is worth briefly considering Binford's (1967) account in order to demonstrate the false character of the claims for the 'hypothetico-deductive' testing of analogies and in order to show the nature of relational analogies.

Binford discusses the interpretation of 'smudge pits', 'caches', small pits filled with carbonised corn cobs in Mississippian sites and dated from AD 470 until recent times. He describes their archaeological distribution, shape and size, and he then lists ethnographic examples of similar pits which are used for smoking hides. He demonstrates that the archaeological and ethnographic pits occur in approximately the same region in North America, that they have the same form, and that there is possible continuity between the archaeological and ethnographic examples (so the direct historical approach is being used). Binford proposes that the archaeological pits were used for hide smoking.

It is of interest to note that, up to this point in Binford's analysis, he has used a largely formal analogy. The relationship between the archaeological pits and hide smoking is based on the formal similarity of the sizes, shapes and contents of the ethnographic pits, on the formal similarity in their distribution, and presumably on other similarities implied by the continuity of societies through time. But Binford then says that he can 'test' his interpretation by drawing deductively a set of secondary expectations.

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The deductive predictions are based on functional links between hide smoking and other aspects of life. Binford suggests that other activities involved in hide smoking might be identified archaeologically in the vicinity of the 'smudge pits'. He notes that, ethnographically, hide smoking was carried out in a spring and summer 'base camp' so that the 'smudge pits' should occur, archaeologically, in sites occupied during these seasons. In addition, in the ethnographic cases, hide smoking was women's work, so stylistic variation in 'smudge pits' should vary directly with variation in other female-produced items such as pots.

Binford presents his secondary predictions as testable hypotheses within the framework of a scientific deductive and positivist explanation, breaking away from subjective interpretations which use analogies. But in view of the discussion above it is apparent that Binford, in the second part of his analysis, has simply shown how the formal analogy could be supported by increasing the number of similarities. It could also be claimed that various functional links are being assumed between hide smoking and other activities. By searching for such additional and functionally relevant similarities between the past and present situations which are being compared, Binford strengthens his interpretation and moves towards a relational analogy.

Binford's interpretation of 'smudge pits' is not a good example of the use of the hypothetico-deductive method. If a true deductive argument had been followed there would have to be some logically necessary link (Binford 1972, 70 calls it a logico-deductive argument) between his predictions and the use of 'smudge pits' as hide smoking facilities. But no such logical necessity can be demonstrated in this instance. For example, it does not seem at all logical to me that all artifacts, including 'smudge pits', associated with women should show the same stylistic variation. It would need to be explained to me *why* pots made by women should be linked in an argument to 'smudge pits' made by women. In other words, I would search for a relational analogy which examined more adequately the relevant causal links between the different parts of the analogy. When archaeologists claim a 'logically deduced' argument, they are usually simply imposing their own assumptions on the data. In a relational analogy all the linking arguments must be examined in relation to a clearly defined and explained cultural context, in which the various functional activities take their place. More generally, archaeologists need to examine why one variable (such as pot variation) is relevant to another (such as smudge pits) when using an analogy. There must be greater concern with causal relationships rather than simply with associations. But our assessment of cause and relevance is always likely to be influenced by our own cultural and personal preconceptions. The use by archaeologists of the hypothetico-deductive method recognised this problem of

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subjectivity and tried to overcome the biases by rigorous testing of hypotheses against data. But this notion is misconstrued because, as well as considering the data, we consider or assume causal relationships lying behind the data.

If Binford was really using a logico-deductive argument, we would expect the secondary predictions to lead to valid independent tests of the initial proposition that 'smudge pits' were used for hide smoking. In fact, the second tests of data are not independent of the first. The identification of other activities involved in hide smoking near the 'smudge pits' would support the analogy, but they do not 'test' the proposition. The archaeologists' interpretation of tools as used for hide smoking activities may itself be influenced by the proposition that is to be tested. In addition, no matter how many times pits and tools are found in association, the pits may still have been used for some other purpose, with the hide smoking activities carried out around them. We need to know much more of the cultural framework within which these various activities were carried out and why they were associated together. The analogy can be supported by examining the cultural interdependence between the different aspects of data, but it cannot be 'tested' on independent data. The whole process of inference is one of building up an edifice of hypotheses, adding one to another and moving beyond the data in order to explain them. In such a procedure there is always the possibility of error, incorrect assumptions, faulty logic, biased observation and so on. We cannot prove or disprove, partly because our predictions and expectations (the 'test') may themselves be incorrectly construed, but also because there are no independent data, and a great deal of subjectivity is involved.

I have discussed little the disproof of theories, and yet refutation is seen by many archaeologists as an important aspect of their scientific methods, derived from Karl Popper. But in my view one cannot disprove in an absolute sense in archaeology any more than one can prove. I have shown how we can support a hypothesis by considering (a) relevance, (b) generality, (c) goodness-of-fit (number of similarities between object and analogy). Many archaeologists would agree that one cannot prove a hypothesis because other hypothetical processes could produce the same observed pattern. But they would say we can disprove a hypothesis if the predictions of the hypothesis are not verified in empirical data. However, the prediction is itself based on a hypothesis which may be incorrect. Any disproof is itself a hypothesis. For example, say I have made the hypothesis that, on analogy with modern societies and environments, a past shell-collecting society concentrated on the larger and more nutritious shell type A in order to maximise the resource potential in the environment. Consider the situation in which I find that on the prehistoric site there is mostly the smaller shell type B and few shells of type A. Can I say I have disproved the maximisation hypothesis? Clearly not, since there

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may be other reasons why few type A shells were found on the site. Type A shells may have been discarded elsewhere even though they were the most abundant species collected. The general problem here is that the predictions are not necessary outcomes of the behavioural hypothesis. In Binford's smudge pit example, if the styles of pots made by women do not correlate with the styles of pits, the hypothesis that the pits were used for hide smoking is not disproved. The hypothesis may, if the reader accepts Binford's assumptions about the causes of stylistic variability, be weakened but not disproved since the hypotheses (that the pits were for hide smoking and that women made the pits and the pots) may be correct but the prediction (that the styles of pits and pots should correlate) may be incorrect. The archaeologist can never test the validity of the predictions themselves because there are no data available from the past concerning the relationship between material culture and human activity. In the shell-collecting example, we could never be certain that shells of a certain type had been deposited in such a way that archaeologists would find them. So, although we can support or weaken hypotheses by arguments of relevance, generality and goodness-of-fit, we cannot test or refute in any absolute or final way.

The 'smudge pits' example does illustrate that it would be possible to be rigorous in the use of analogies if the number and range of similarities are great and if relationships between the different aspects of the analogy could be adequately specified. After the publication of Binford's interpretation of the pits as for hide smoking, Munson (1969) suggested an alternative hypothesis that they were used in the smudging of ceramics. In his reply to Munson's suggestion, Binford (1972) rightly indicated that it would be possible to differentiate between the two interpretations by examining other linked aspects of the use of the pits for the two alternative functions. Thus, by examining the causal relationships in a particular cultural context between the pits and other activities which also might be identifiable in the archaeological record, it becomes possible to differentiate between competing analogies.

Relational analogies and the notion of the context of the things being compared can now be seen to be essential aspects of the proper use of analogy in archaeology. We can avoid the charges of unreliability and anti-science by increasing the number and range of points of comparison between past and present, but also by identifying the relevance of the comparisons. We have to understand the variables which are relevant to the interpretation of particular features of cultural evidence; we must have a better idea of the links between the properties we are interested in and their context. As I have already indicated, the links and the contexts in which archaeologists have recently been most interested concern function. What I wish to do now is develop the idea, to be examined more fully later in this book, that

the notion of context must include not only function, but also the ideational realm.

Context

It may be helpful to begin with an example. We shall see in Chapters 3 and 5 that many archaeologists have recently attempted to make generalisations about the way people living in settlements organise and deposit their refuse. Schiffer (1976) and Binford (1978), for example, have suggested several cross-cultural relationships between discard behaviour and size of site, length of occupation, amount of effort put into making a tool, and so on. For example, it has been suggested that, with longer and more intense occupation on a site, there is more secondary removal and clearance of refuse away from activity areas. This is a functional relationship concerned with the need to keep areas habitable and clean, and with the likelihood of secondary kicking and removal as population densities increase. Now, while the theoretical statements concerning such cross-cultural generalisations do note the importance of describing the conditions under which the relationship holds, in practice the conditions, the context, are not well defined. The 'law' is supposed to be universal, or the context is simply pushed off to the side under the heading 'other things being equal' (Binford 1976). There is rarely any analysis of what the 'other things' might be except in so far as analysis is made of technological, economic and environmental constraints. We have already seen how dangerous it is to assume that aspects of the material environment are sufficient to define a context for the relationship between variables.

In the studies of refuse, no emphasis has been placed on how peoples' attitudes to dirt and rubbish might affect the functional relationships between discard patterns and site size and occupation intensity. Yet there are many anthropological studies, which will be discussed in Chapter 5, that demonstrate how our conceptions of what is dirt and, our concern with keeping things clean are very much dependent on local frameworks of meaning. Attitudes to refuse vary from society to society, and from group to group within societies; just consider the dirt and rubbish piling up around a gypsy camp site in England and the offence that is given to many of the hygienic, sparkling pillars of non-gypsy society; or the contrived dirtiness and dishevelled aspect of hippies breaking away from the attitudes, standards and interests of their middle-class parents who are concerned with tidiness, manners and impeccably clean self-presentation. There can be no simple functional links between refuse and types of site, lengths of occupation or forms of society, because attitudes and conceptions intervene.

The notion of context, then, must concern both functional and ideological aspects of life. In developing the use of analogy in

archaeology the emphasis must be on examining *all* the different aspects of context which might impinge upon particular characteristics of material culture. But this more developed notion of context implies that every context is unique. The framework of meaning within which we carry out day-to-day actions is a unique combination of ideas, strategies and attitudes. And the symbolism given to material culture within such a meaningful context is likely to be peculiar to that context.

In the discussion of relational analogies it was suggested that it is possible to answer the criticisms that analogies are unreliable and unscientific. But a third problem with analogies was also identified in the first part of this chapter: analogies from the present limit our understanding of the past. The emphasis on the unique nature of each context aggravates the problem. If each context in which material culture is used represents a peculiar arrangement of meanings and ideas and personal strategies, how can we ever use a present context to interpret evidence from the past?

This is not such a difficult question to answer as it might seem at first sight. The problem of the contextual nature of material culture patterning can be examined in two ways. First, each unique cultural context is assembled from general principles of meaning and symbolism, which are often used in comparable ways. Thus, in many societies dirt is used as a symbol of rejection of the control and authority of dominant groups. So, Gypsies use dirt to contradict the non-Gypsy society in which they live in much the same way as hippies use dirt and disorder to defy the generation and society of their parents. One aim of the development of the use of analogies is to increase our understanding of how aspects of material culture patterning such as dirt and disorder can be used in personal and group strategies. The general principles of material symbolism must be examined. But in developing generalisations about symbolism it is essential to pay particular attention to the links with other variables, and to the conditions under which a particular type of symbolism might be used. Clearly not all minority groups use dirt as a part of their rejection of authority. We need to examine carefully what other factors are relevant and how they are linked. If we know a lot about how aspects of material culture (such as dirt or burial) are used and given meaning, if we know a lot about the links between material culture and its functional and meaningful context, then we can interpret new situations, unique cultural arrangements beyond present-day experience. We can interpret how the new cultural 'whole' has been assembled from general principles which are well understood.

Second, in all uses of analogy it is necessary to define what aspects of the things being compared are similar, dissimilar or of uncertain likeness. The aim is to throw light on what is not known (the uncertain likeness) by demonstrating the strength and relevance of the positive

analogy, while showing that the negative likenesses are of limited consequence. So, all analogical reasoning accepts that there will be some differences between the things being compared. We can set the past beside the present even if some aspects of the contexts do differ. Indeed, Gould (1978) has suggested a 'contrastive' approach which moves 'beyond analogy'. He indicates that we can use modern situations as base lines *against* which to compare evidence from the past. In fact Gould's contrasts do not lead to a different approach, they are an integral part of the use of analogy.

It is important, then, to be able to decide whether the differences between a past and present situation are enough to invalidate the use of a particular positive analogy. In making such a decision it is essential to understand the factors which link variables of material culture to their context, as suggested in the first point above. For example, I have compared the use of material culture to mark ethnic group distinctions in modern Kenya and Iron Age Britain (Blackmore, Braithwaite and Hodder 1979) even though the pattern of annual rainfall differs considerably in the two places. I feel it is valid to make such a comparison because my studies of ethnic group expression identified no direct link to rainfall patterns. However, it would be invalid to draw analogies from the Kenyan area in order to interpret Iron Age farming practices in Britain, since rainfall regime and agricultural systems are closely linked. Depending on the analogy being made, different aspects of context are more, or less, relevant to an assessment of the validity of the analogy and interpretation.

It is possible to interpret the manipulation of material items in a unique context in the past by (a) referring to general principles of symbolism and to generalisations about the links between those principles and the context in which they are used, and by (b) carefully using selected analogies for different aspects of the evidence, identifying similarities and differences, and gradually building up from bits and pieces to an understanding of how the whole picture is composed.

In my answer to the problem of how to use analogies if each situation, past and present, is unique, it has become evident that two types of analogy are necessary; (a) it is valid to suggest general theories about the relationship between material culture and social and economic aspects of life as long as the links to context are understood; (b) with that general theoretical knowledge, it is possible to assess which aspects of each particular analogy are relevant. The use of relational analogies depends on a good theoretical framework within which one can identify what is relevant in a particular case.

Examples of the peculiar and particular nature of modern material culture patterning are important as archaeologists carefully select analogies to build up, from bits and pieces, to an assessment of the whole in each context. An interest in the particular as opposed to the general is also essential since the traditional societies that remain for us

to study today are often highly specialised to particular environments and ways of life. Indeed it is probable that types of society which were common in the past are represented by only one or a few examples today. Particular modes of manipulation of material culture may be extremely rare today. There will always be a place for the unique and the odd, even if only to provide a contrast.

Conclusion

On the whole the criticisms that have been made of the use of analogy in archaeology should be restricted to its misuse. Interpretation with the aid of analogies is unreliable and non-rigorous when the similarities between the things being compared are few in number and when the relevance of the comparison cannot be adequately demonstrated. The proper use of analogy in archaeology must pay special attention to context; that is, to the functional and ideological framework within which material items are used in everyday life. Most recent discussions of analogy in archaeology have considered cross-cultural laws which note the co-occurrence of different aspects of material culture and society. Such studies pay too little attention to when and why covariation occurs. It is necessary to examine not only the *existence* and *strength* of covariation, but also its *nature* and *cause*. It is only when archaeologists understand more fully the conditions and context in which certain things hold true that the use of analogies can be seen to be reliable and rigorous.

Several archaeologists have recently attempted to diminish the role of analogies by suggesting that analogies should simply be used for the forming of propositions about the past. The real guts of a scientific archaeology is then said to be in the deduction of the consequences of the proposition and in the testing of those implications in independent archaeological evidence. In fact, the nearest the archaeologist can get to a rigorous method is the careful use of a relational analogy. The analogy is situated within its relevant context.

For the moment it is difficult to see how archaeologists might break away from their dependence on analogies with the present; or why they should want to do so. I have tried to show in this chapter the importance of analogy in archaeology, and I have attempted to demonstrate that the use of present analogies does not entirely limit our interpretations of the past. While it is true that all our interpretations of human behaviour and thought are affected by the society in which we live, it is also true that we can comprehend different societies, cultures and economies in other parts of the world, and that we can use this additional information in describing and explaining the past. As archaeologists our dependence on other societies, other behaviour and ways of thought always has been, and remains, immense. The links with ethnography and anthropology are close, as Chapter 2 shows.

2 Ethnoarchaeology

An important source of analogies, although as we shall see not the only source, is difficult to label since almost every word which is commonly used brings with it offence. But I hope that, if I talk of traditional, less-industrialised contemporary societies, my meaning will be understood. In the last 25 years archaeologists have begun to carry out field studies amongst traditional societies in order to answer questions of archaeological interpretation and in order to develop and examine analogies. It is this type of work which is called ethnoarchaeology.

First, some definitions. Although the word 'ethnoarchaeology' was used in 1900 by Fewkes, it has only recently become a popular term. Yet there is a divergence of opinion as to its meaning. Gould (1978b) and Stiles (1977) define it as the comparison of ethnographic and archaeological data. This definition seems hopelessly broad, especially if one accepts that all archaeological interpretation involves making analogies with present-day societies. Nearly all archaeological interpretation could then be described as ethnoarchaeology. Stanislawski (1974), on the other hand, defines ethnoarchaeology as being a field study and it is easier to concur with such a viewpoint. Ethnoarchaeology is the collection of original ethnographic data in order to aid archaeological interpretation. This second definition coincides more closely with current usage than does the first. The armchair archaeologist leafing through ethnographic tomes is less likely to retort 'I am doing some ethnoarchaeology', than is an archaeologist conducting his own ethnographic field research.

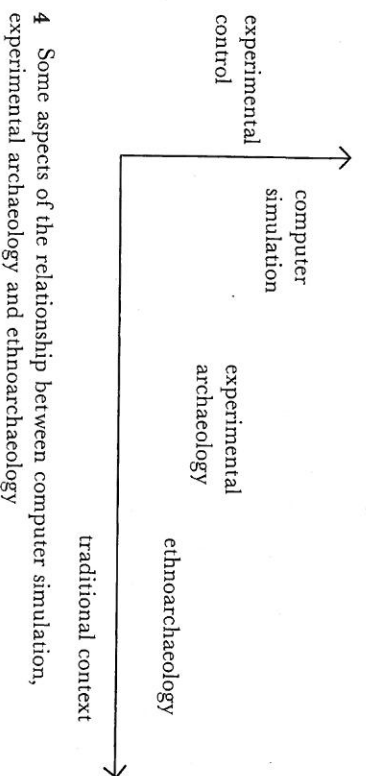
There is often confusion about the relationship between ethnography, ethnology and anthropology and it is perhaps necessary to clarify these terms. Ethnography is the analytical study of contemporary ethnic groups; an examination of their material, social and linguistic characteristics. Ethnology, on the other hand, is the development of theories about the relationships between the characteristics of ethnic groups and about why they change. Ethnology has tended to be concerned with simpler, non-literate peoples, so leaving the more complex industrialised societies to sociology, geography, etc. Anthropology is a wider set of sciences, which in North America includes archaeology, but which in Britain does not, concerned with the study of man, usually simpler, less industrialised man. It is a generalising discipline which includes both ethnography and ethnology. For further definitions and distinctions between European and American terminologies the reader is referred to Rouse (1972)

ETHNOARCHAEOLOGY

Relations with experimental approaches in archaeology

Contemporary traditional societies are not the only source of analogies for the archaeologist. Analogies can also be derived from studies of material culture in our own, highly industrialised societies as will be shown in Chapter 9, and such work is also called ethnoarchaeology. But in our own societies archaeologists can also set up artificial experiments in order to draw further analogies with the past. Both experimental archaeology (Coles 1973; 1979) and computer simulation (Hodder 1978b) are concerned to test the feasibility of particular archaeological interpretations. They differ from ethnoarchaeology in several ways. In the first place, an experiment such as firing pots in a kiln in order to see what methods and kilns could have been used in the past can be truly experimental in that different trials can be made, the conditions and temperature in the kiln can be accurately monitored and recorded, and variables can be controlled. The simulation of a hypothesised process of prehistoric exchange on the computer can be even more carefully controlled, with huge numbers of different trials being 'run' and the effects of different variables carefully examined and experimented with. In an ethnoarchaeological study, on the other hand, there is little experimental control. One is above all an observer and an asker of questions; one cannot try this and that and see what happens.

What ethnoarchaeology loses in experimental control it gains in 'realism'. The ethnoarchaeologist does not study an artificial environment that he has created (although we shall see in the discussion of field methods that some qualification is needed here). He can fit the firing of pottery in a kiln into its total social, cultural and economic context, and he can observe all the possibly relevant variables. The experimental archaeologist, on the other hand, has created an artificial situation, even though trying to duplicate as closely as possible the prehistoric information. The experimental archaeologist tries to learn and implement craft techniques to which he is unaccustomed and which his society has long forgotten. Apart from these practical difficulties, the experiment is carried out in a vacuum so that links with the social, cultural and ideological aspects of life are difficult to identify. While he may be able to examine the effects of technological and natural variables in his experiments, the experimental archaeologist is at a loss to assess the broader context which, in Chapter 1, was suggested as being of such importance. Computer simulation is totally artificial. Any analogies that derive from such work depend entirely on the assumptions that the archaeologist has fed into the computer. The working of the computed model depends on the second-hand ethnographic and experimental information.



4 Some aspects of the relationship between computer simulation, experimental archaeology and ethnoarchaeology

Experimental archaeology involves the carrying out of experiments in order to test beliefs about archaeological data. We have seen that it is less realistic, but more controlled than ethnoarchaeology. But ethnoarchaeology has one possible additional advantage over its rival. It can be claimed to be less ethnocentric. Any experiment carried out within our own society is bound by the assumptions and knowledge of that society. The extent to which we can broaden our ideas about the past by conducting experiments in modern Western society is limited. Ethnoarchaeology, on the other hand, is able more radically to confront the Western archaeologist with alternative behaviour. The ethnocentric bias can more readily be broken. However, there is a danger that we substitute one type of ethnocentrism with another, and I have already shown in Chapter 1 that analogies must not be taken over wholesale from one modern traditional society to a prehistoric society. There is a need for careful selection and control of analogies.

Attempts have been made by some archaeologists to combine the advantages of ethnoarchaeology and experimental archaeology by carrying out limited but controlled experiments in traditional contexts. One example is the experiment conducted by White and Thomas (1972) in which native New Guineans were asked to carry out experiments in stone tool manufacture. Duna-speaking people from the Central Highlands of Papua-New Guinea were asked by the ethnoarchaeologists to make traditional stone tools and the variation in form of the tools made by men in different parishes could then be analysed. Such an experiment carried out by ethnoarchaeologists has the advantage that the analysis, and the discovery of the variables which are thought to have most affect on tool variation, can be controlled and sufficiently large samples can be obtained. But there are also disadvantages. In particular, the ethnoarchaeologist is no longer an observer of traditional behaviour. He isolates activities from their context and he intrudes himself starkly onto the scene. He brings an artificiality and the context, normally such an important constituent

of ethnoarchaeological work, is eroded. We end up knowing very little about the conditions under which the relationships identified by White and Thomas might hold true, so that it is difficult to use their results in other contexts.

Experimental archaeology is of greatest value in understanding how things could have been made and used, and in identifying the natural properties of materials such as flint when flaked by man or stone when used to cut down trees. But in answering 'why' questions which seek to relate technologies and economies to broader social and cultural contexts, the approach is limited.

History of the use of ethnographic analogies in archaeology

Ethnoarchaeology has been given a more restricted definition than the general use of ethnographic analogies. But it is necessary to chart, even if sketchily, the history of the use made by archaeologists of ethnography and anthropology in order to assess the present importance and purpose of ethnoarchaeology. The past use of ethnographic analogies can be followed in both Europe and America.

In Europe, the Renaissance revival of interest in the Classical world and its literature led to an awareness of ancient societies existing prior to literary civilisations. Indeed the earliest ethnographies which were to prove of service to archaeologists may well have been the accounts of Caesar and Tacitus, who in *De Bello Gallico* and the *Germania* described the native inhabitants of France, England and the lower Rhine area. But at the same time as the increasing interest in early ethnographies led to a picture of a barbarous past, pictures of a very different kind from the contemporary world were also arousing attention.

Explorations beyond the known world introduced the sixteenth-century European to altogether novel cultures. John White, the Elizabethan explorer in Virginia, described and illustrated the villages, ceremonies, arrowhead manufacture and the hunting and farming activities of the Indians of Pomeiock. Some of his watercolours were printed in de Bry's (1590) edition of Harriot's *Brief and true report of the new found land of Virginia*. But de Bry also attached to the volume the first representation of past Britons as other than Tudors, Romans or mythical heroes. The pictures were accompanied by the following caption. 'The painter of whom I have had the first of the Inhabitants of Virginia, give me also thees 5 Figures followinge, fownde as hy did assured my in a colld English cronicle, the which I wold well sett to the ende of thees first Figures, for to shoue how that the Inhabitants of the great Britannie have been in times past as sauauage as those of Virginia' (from Orme 1973, 488). So here an ethnographic parallel was used in order to add information to what was known about the past. In 1611 John Speed referred to the Virginians in supporting his theory that the

Ancient Britons went naked. Again ethnography was used to shock the civilised members of seventeenth-century society about their primitive past.

Another instance in which ethnography was used to change romantic and mythical interpretations of the past was in the debate about early stone implements. In the mid-seventeenth century Aldrovandus described stone tools as 'due to an admixture of a certain exhalation of thunder and lightning with metallic matter, chiefly in dark clouds, which is coagulated by the circumfused moisture and conglutinated into a mass (like flour with water) and subsequently indurated by heat, like a brick', and at about the same time Tullius suggested that chipped flints were 'generated in the sky by a fulgurous exhalation conglobed in a cloud by the circumposed humour' (Daniel 1950, 26). Other interpretations of early stone tools were that they were thunderbolts or elf arrows. Such theories were put to an end in Oxford by Dr Robert Plot and his father-in-law Dugdale and his assistant Lhwyd in 1686, 1656 and 1713 respectively, incorporating the aid of ethnographic parallels. By 1800, John Frere could describe Palaeolithic hand axes as 'weapons of war'.

In the eighteenth century many parallels were built between ethnographies and the European past, as is clear from the titles of J.T. Laftau's 1724 Paris publication *Moëurs des Sauvages Amérindiens, Comparées aux Moëurs des Premiers Temps*, and J.Kraft's 1760 Copenhagen publication *Brief account of the principal institutions, customs and ideas of the Savage Peoples illustrating the human origin and evolution generally* (Klindt - Jensen 1976). Laftau had been a French missionary in Canada and he drew parallels between Indians and the Bible and classical literature. Kraft's more general work was translated into German and Dutch, but remained largely unnoticed by archaeologists. He explicitly drew parallels between stone tools found in the soil in Europe, and primitive stages of man, on analogy with existing primitive societies.

All these instances of the use of ethnographic parallels in the sixteenth to eighteenth centuries were concerned with making simple formal analogies for the immediate purposes of clarification and for the derivation of alternative interpretations. A lack of concern with the dangers of ethnographic parallels continued in the nineteenth century with the exception of such archaeologists as Nilsson in Denmark and Pitt Rivers in England.

Nilsson in *Primitive Inhabitants of Scandinavia* (1843) used a 'comparative method' by which prehistoric artifacts were compared with formally and functionally identical objects used by modern peoples. He claimed that by using the comparative method archaeology ought to be able 'to collect the remains of human races long since passed away, and of the works which they have left behind, to draw a parallel between them and similar ones which still exist on earth, and thus cut

out a way to the knowledge of the circumstances which have been, by comparing them with those which still exist'. But he also noted that care was needed in the use of ethnographic analogies. 'Similarities such as the presence of similar stone arrows in Scania and Tierra del Fuego do not always prove one and the same origin' (quoted by Daniel 1950, 49). The same caution is evident in Pitt Rivers' important and independent development of the use of ethnographic comparisons. General Pitt Rivers' involvement in the army led to an interest in the development and improvement of the rifle. His careful classification of categories and forms of rifle was followed by careful collections of a wide range of weapons, boats, musical instruments, religious symbols and so on. In arranging his material, Pitt Rivers was more concerned with types than their country of origin. Archaeological and ethnographic material could be compared, side by side. Yet, like Nilsson, he was wary of discussing only formal parallels. In making an analogy it was necessary to demonstrate functional as well as formal identity. This concern with function extends the context of the formal similarity and moves towards a relational analogy.

Orme (1973, 487) suggests that the work of Wilson (1851), Evans (1860), Christy and Larret (1875) in recognising and identifying artifacts used comparisons with the material culture of contemporary traditional groups, while the more general interpretations of Lubbock (1865) and Tylor (1865) also relied heavily on ethnography. In 1870, Sir John Lubbock wrote in *The Origin of Civilisation and the Primitive Condition of Man* that 'the weapons and implements now used by the lower races of men throw much light on the significance and use of those discovered in ancient tumuli, or in the drift gravels: . . . a knowledge of modern savages and their modes of life enables us more accurately to picture, and more vividly to conceive, the manners and customs of our ancestors in bygone ages'. The influence of ethnography on archaeology in the second half of the nineteenth century increased as part of two additional developments. The first was the reference, in interpretations of the past, to the folklore survivals of 'primitive' antecedents (Daniel 1950, 185). Second, the 'culture' concept began to replace the previous emphasis on dividing archaeological material into epochs. The European school of 'anthropogeographers' started by Ratzel and his pupil Frobenius described cultural areas in West Africa and in Melanesia. The work of German ethnologists came to have a great impact on the development of the culture concept in archaeology as it was later to be adopted by Childe. Indeed, Daniel (1950, 246) has described the organisation of pre-history into cultures as the anthropological attitude to prehistory.

Though the debt to ethnography and anthropology in the second half of the nineteenth century was large, the cautions of Nilsson and Pitt Rivers went generally unheeded. Formal parallels were applied with little attention to context and the dangers were little discussed.

The trend of increasing links with anthropology continued in the first half of the twentieth century with, for example, the journal *Annals of Archaeology and Anthropology* first issued in 1908 under the auspices of the University of Liverpool Institute of Archaeology, but the increase in quantity was not always associated with an increase in quality.

The first issue of *Antiquity* (1927) included an article by Raymond Firth on Maori hillforts which was intended to aid British prehistorians in the interpretation of their own Iron Age earthworks. While the place of hillforts in Maori society is well discussed, comparisons with English hillforts are scanty and formal, based on such features as the size and shape of ramparts. Thomson, in his 1939 study of the Wik Munkan Aborigines of the Cape York Peninsula of Australia, recognised that the archaeologist would be confused and possibly misled by the surviving material culture. The Aborigines lead such different lives in the different seasons of the year that the archaeologist might be fooled into thinking the different assemblages were produced by separate peoples. In addition, Thomson noted that very little would survive to be found by archaeologists anyway. This is the first example of a pessimistic, negative type of analogy which became common later as 'cautionary tales'. Most such cautions have the characteristic that they are little concerned with the context which leads to the particular material remains: in the Australian example the context is highly specialised and generalisation would be difficult (see discussion in Chapter 5).

Parallels with societies closer at hand seemed to avoid some of these problems. In his report of the excavations at Skara Brae, a Neolithic settlement on the Orkney Islands, Childe (1931) used analogies with recent inhabitants of the northern isles in his interpretation of the functions of beds and other features inside the huts. Clark (1952) also used a wealth of information from recent and modern Scandinavian societies in order to provide formal analogies for prehistoric tools. But in the same book many formal comparisons were made over great distances to the Eskimos and tribes of Canada and North America, as was also seen (Chapter 1) in his account of the Star Carr site. More recently, a visit by Grahame Clark to Australasia led to an article (1965) comparing, with some concern for context and relevance, the axe trade there with the distribution of Neolithic axes in Britain. It was suggested, on the basis of the similarities of the axes, their very wide distributions, and the simple level of societies, that the Neolithic axes were traded by ceremonial gift exchange, much the same as in New Guinea and Australia.

An increasing concern with relevance and context has continued in the careful and informed analogies drawn in the more recent work of, for example, David (1971; 1972) and Rowlands (1971; 1976). But the emphasis on formal comparisons still remains. Clarke (1968) used evidence from Californian Indians to suggest that links could be set up

between the forms of regional artifact distributions and linguistic groupings, while other formal comparisons of cultural distributions have been made by Hodder (1978a). Such formal studies are now decreasing in frequency as warnings are made (e.g. Ucko 1969) and assessments published (Orme 1973; 1974; 1981). The strength of the link between archaeology, anthropology and ethnography has now reached a new peak in England and continental Europe with ethnographic archaeology posts in three departments of archaeology in English universities, several archaeology and anthropology joint degrees, numerous courses, and a host of articles, many of which will be referenced in the pages which follow. Nevertheless the fuss over the conscious rapprochement between archaeology and anthropology in Britain is slight when compared with the call to arms that has emanated from North American publications over the last 25 years. Indeed much of the recently increased popularity of ethnography for archaeologists in England and Europe must be seen as a result of influence from the other side of the Atlantic.

In America, the presence of living Indian societies in areas where excavations were being undertaken helped to make archaeologists aware of the possibilities of using the present to interpret the past. In a much more direct way than in Europe, American archaeologists have always been able to make use of ethnohistory and modern ethnographic accounts. An example of the early use of ethnography in America is provided by the resolution of the 'moundbuilders' controversy. By the late eighteenth century, a large number of mounds or ruins had been discovered in Ohio and in other frontier areas. Any idea that these monuments could have been built by the Indian 'savages' was rejected, and a 'lost race' was imagined which had been replaced by the Indians. The nature and origin of the lost race of moundbuilders were the subject of much fanciful speculation until finally, in the late nineteenth century, the myth was laid to rest by the work of Cyrus Thomas. Major John Wesley Powell (the first explorer to descend the Grand Canyon rapids of the Colorado River in a boat; see *fig. 6*) was Director of the Bureau of Ethnology and he picked Thomas to carry out research into the mounds in the 1880s and 1890s. Through Thomas' excavations and comparisons of the excavated material with contemporary Indian material culture in the same areas, Powell and Thomas were able to suggest that the moundbuilders were the same people as the modern 'savages'.

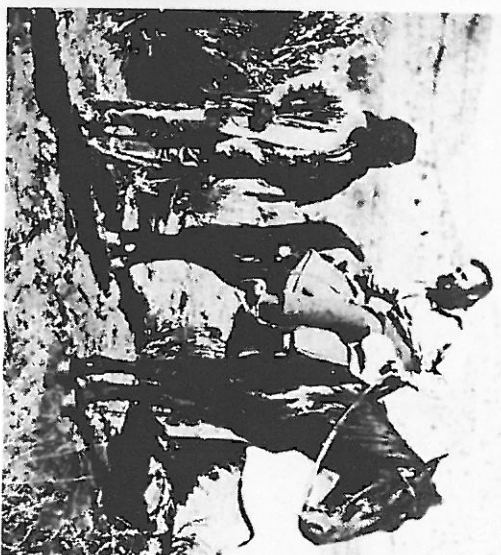
This direct historical approach, used by Thomas to throw light on archaeological information, was part of a more general emphasis on continuity. Around the turn of the century a number of studies (Fewkes 1893; Hodge 1897; Kroeber 1916) tried to use archaeological evidence to test Indian oral traditions or to find sites associated with particular clan myths. Analogies based on continuity in local areas were an important part of the ethnic identification of cultural



5 An important early European view of the south-eastern American Indians is provided in the illustrations of Jacques Le Moyne, who accompanied the French settlers to north-eastern Florida in the 1560s. This drawing shows a burial ceremony with the grave or small burial mound outlined in arrows and topped by a conch-shell drinking vessel. The mourners surround the mound, and in the background is a palisaded village. Pictures such as this were either unknown to or ignored by early writers on the Moundbuilder controversy (Willey and Sabloff 1974)

complexes throughout the early part of the twentieth century in North America. In particular, W.D. Strong (1935) and J.H. Steward (1942) developed the direct historical approach as a distinct procedure within archaeological interpretation.

Although the same emphasis on using continuity to support analogies is still seen in more recent American work (for example in the 'smudge pit' study discussed in Chapter 1), there has been an increasing concern over the last 30 years with generalisation, as part of the conscious expansion of what came to be termed 'anthropological archaeology'. In 1948 (p. 6) Taylor described the archaeologist as a 'Jekyll and Hyde, claiming to "do" history but "be" an anthropologist'. The use of direct, continuous analogies was more clearly associated with a historical emphasis, and the new concern with making general statements about man, society, ecological relationships and systems which has lasted to the present day, eschewed emphases on historical frameworks and sought to embrace more



6 Major John Wesley Powell consulting an Indian on the Kaibab Plateau, near the Grand Canyon of the Colorado River in northern Arizona (Willey and Sabloff 1974)

closely the generalising science of man. Associated with this development has been a decreasing concern with causal relationships within particular contexts when using ethnographic analogies. The aim has been to build and use general comparative analogies. Interpretations of the past are obtained through broadly comparative and quasi-universal generalisations about human cultural behaviour, rather than being confined to a specific historical context (Willey and Sabloff 1974, 207). But this view has not gone without its critics in America. According to Anderson (1969), 'logical analysis of form depends as much on perception of the object, which is conditioned by cultural background, as by any universal principles'. Those recent studies which do examine context and which use relational analogies have been more concerned with the functional and ecological context than with the realm of ideas.

It was in this more recent period, as 'anthropological archaeology' became taken up as the battle cry of the 'new archaeologists' (Meggers 1968; Longacre 1970) that ethnoarchaeology became defined as a distinct area of research. We have already seen that the word was used as early as 1900 by Fewkes, but it was in the '50s and '60s that the main development occurred. Various definitions and outlines of ethnoarchaeology and the equivalent terms 'action' or 'living' archaeology were suggested by Kleindienst and Watson (1956) and Ascher (1962) and specific studies appeared (Thompson 1958, Oswalt and Van Stone 1967). The *Man the Hunter* symposium (Lee and De Vore 1968) was important in bringing a large amount of information and general

statements about hunter-gatherers to the notice of archaeologists, while in the same year the collection of essays edited by the Binfords included important ethnoarchaeological studies (e.g. by Longacre and Ayres). Since then the number of studies, definitions and books has greatly increased (Donnan and Clewlow 1974; Stiles 1977; Yellen 1977; Ingersoll, Yellen and MacDonald 1977; Gould 1978b, 1980; Binford 1978; Kramer 1979). But it is important to realise that this rash of ethnoarchaeological work is not characterised by a uniformity of approach. Variation in views on definitions was described at the beginning of this chapter. There is also still considerable variation in the emphasis placed on context and relevance. For example, Wilmsen's (1979, 1980) careful study of the particular Bushman cultural and historical context and the patterning of material remains contrasts with Schiffer's (1978) call for the generation of cross-cultural 'laws'. While some studies are interested in only the ecological and functionally adaptive context within which material residues are produced (e.g. Binford 1978), others are concerned to examine the cognitive basis of behaviour (e.g. Hardin 1979).

There is also variation in views about the part played by ethnographic analogies in archaeological interpretation. Oddly, much of the recent ethnoarchaeological fervour has occurred at a time when the dominant consensus among American archaeologists has been that the role of analogy should be limited. Several archaeologists have emphasised that explanation should proceed by the testing of deductive hypotheses. The analogy derived from ethnoarchaeology is seen as playing only a small initial role in suggesting the hypothesis. In Chapter 1 we saw why this view is misleading. In fact, archaeologists do and can proceed rigorously by the careful use of relational analogies and it is not necessary to set up this procedure as 'hypothetico-deductive'.

The increase in ethnoarchaeology is a product of the anthropological emphasis in American archaeology, and of the growing interest in the formulation of cross-cultural generalisations about human behaviour. Ethnoarchaeology is also seen to provide particular hypotheses to be tested deductively against archaeological data. Whether carried out by Americans or Europeans, and however it is used, ethnoarchaeology now has a number of important functions. First, it became clear, as archaeologists turned more and more to social anthropologists for advice and inspiration, that existing ethnographic studies were inadequate. Ethnographers had rarely collected the type of data on material residues that is most relevant for archaeologists. They had concentrated on aspects of social and linguistic variability, and on general accounts of cultural material. Few ethnographic studies provided detailed information on the locations of settlements, size and shape variation of artifacts, or disposal processes. In particular, questions concerning the depositional and post-

depositional processes which result in distributions of artifacts and features on archaeological sites could not be answered using existing ethnographic studies. The increased interest in such questions in the last ten years (e.g. Schiffer 1976) has been one further factor encouraging the recent rise to importance of ethnoarchaeology. It became necessary for the archaeologist to collect his own ethnographic information, and in fact most ethnoarchaeology is today carried out by people trained as archaeologists, not by ethnographers or social anthropologists.

A second important function that ethnoarchaeology provides is to salvage relevant information from forms of society which are fast disappearing. The colonial destruction of numerous peoples from Tasmania to North America was as horrifyingly efficient as it was speedy. The technique of flint-knapping used by the last Californian Indian, Ishi, was recorded by Nelson (1916), but such information was usually lost or badly recorded. What we have left now are the remnants of our slaughter. The traditional societies which did survive are fast becoming incorporated into world-wide economies. Wobst (1978) notes that most ethnoarchaeologists now mainly consider the local adaptive aspects of hunter-gatherer groups. Regional and inter-regional links are sifted out because they are 'modern', 'post-contact' and 'distorting'. If we do think it is important to examine traditional societies, ethnoarchaeology must not lose time in placing them in their modern world context.

The desire to study traditional, less industrialised societies is understandable for those archaeologists who wish to examine flint-knapping and other techniques which are no longer to be found in Western society. Yet there is a possibility that, after having overrun and pillaged Australian, African and American peoples militarily, economically and socially, we follow this up by an 'intellectual colonialism'. I have said that ethnoarchaeology is important in that it helps to break away from a Western ethnocentrism. But there is a terrible risk that in examining 'primitive' societies we are really just repeating that ethnocentrism in a new and more sinister form. We assume that our own 'developed' society is less relevant in finding analogies for Neolithic Europe in 6000 BC than are 'undeveloped' Africans or Australians. But 'primitive' Africans might not see it in the same light. They are not devolved, reverted or stagnant societies. There is a potential danger in 'dressing up' European, Neolithic society like Kenyan tribesmen, in the same way that in the eighteenth century Ancient Britons were dressed like Virginians. This form of intellectual ethnocentrism is part of the evolutionary perspective which has gripped archaeology throughout this century. Breaking away from such biases leads to the third function of ethnoarchaeology today.

Ethnoarchaeological studies in Western industrialised societies are as equally valid as studies in the less industrialised world if the

importance of a control over context is accepted. I have tried to show that the drawing of analogies for the prehistoric past must move away from formal cross-cultural studies, to a more careful consideration of the links between material patterning and its functional and ideational context. We need to understand *why* material is patterned in a particular way in each cultural milieu. This emphasis on the links themselves means that Western society is as good a source for analogies as are less industrialised peoples. If we can understand all the functional and ideological factors which cause variation in how we bury our dead in modern England, that is if we understand the links between burial and its context, then we can assess whether the modern information is relevant to a particular prehistoric situation. While technological studies of particular, fast dying-out, activities such as flint-knapping remain important, the more generally applicable 'contextual' studies do not depend on finding 'primitive' societies. Rather there is a need to consider all forms of society in their own terms.

The third function of ethnoarchaeology, and in my view the most important, is to develop ethnographic analogies which concern the principles which relate material patterning to adaptive and cultural contexts. But in saying this it also becomes apparent that a change is needed in ethnoarchaeological methodology. For the moment nearly all the ethnoarchaeological studies of which I know have been carried out by people trained as archaeologists and I have already suggested why this should be so. Ethnoarchaeology simply involves the archaeologist going off to do a bit of ethnoarchaeology, often as a sideline or second interest. The archaeologist treats the ethnographic data as if they were archaeological data and he uses archaeological methods (sampling, recording, etc.). As far as I am aware, there is no detailed account in the ethnoarchaeological literature of field methods, interview techniques, and the problems of sampling live populations. This remarkable lack results from what Gould (1978b) has called the 'materialist' emphasis of ethnoarchaeology. There is an assumption that the ethnoarchaeologist is concerned with the actual products of behaviour, with plotting material remains and with objective descriptions. But as soon as it is accepted that the context of material activities is the major concern, then anthropological field methods take on a real importance. It we are to find out the social and cultural framework which informs the making of a pot or the knapping of a flint blade, then we must understand the limitations, difficulties and problems of interviewing, observing and understanding members of other societies and their material products. We shall see that not only people but even pots can lie.

Ethnoarchaeological field methods

Various published sources of world ethnographic data are listed by Stiles (1977), but since I have defined ethnoarchaeology more narrowly than the general use of ethnographic analogy, as a field study, I will examine here only field methods. For more general accounts of anthropological methodology see, for example, Naroll and Cohen (1973) and Brim and Spain (1974).

Perhaps the major problem to confront any ethnoarchaeologist working in a contemporary society is that what people do may bear little relation to what they say. For example, in my initial work in the Baringo District in Kenya, I was told that many of the clay pots in use in houses and compounds were up to 80 years old. In fact in *Man* (1977) I published several maps containing such information. In subsequent years, after talking to anthropologists who had worked longer in the region than I had, and after talking to the potters and observing the frequency of pot breakage, I realised that it was extremely rare for any pot to last more than eight years. Similarly, I was told by a number of informants that a group of pots came from a particular source. After measuring the pots it became clear that the information was almost certainly incorrect.

In modern western society individuals often mislead when asked questions concerning various aspects of material culture. For example, many may be embarrassed or ashamed to answer correctly questions concerning the amount of different types of refuse that they produce. Work in Tucson, Arizona and in Milwaukee has demonstrated that estimates of garbage disposal rates based on verbal information were often severely misjudged (Rathje 1979).

Perhaps the most widely discussed instance of a lack of correspondence between verbal information and actual behaviour concerns the notion of the 'type'. Are the categories and types of artifacts identified by the archaeologist purely of his own making, or do they correspond to a 'native' classification? Ford (1954) saw types as archaeological and imposed, while Spaulding (1953) suggested that native categories could be inferred by using various statistical techniques. There are now a large number of examples such as those given by Gould (1974), Hardin (1979) and White & Thomas (1972) which show that archaeological and native categories do not coincide. White and Thomas, for example, note that the primary archaeological division of stone artifacts into cores and flakes is not made by native New Guinea highlanders. Clearly, in studying the cultural context and ideational basis of material culture patterning, we may be examining meaningful behaviour which is not expressed at a particular verbal level. What are the reasons for this?

There is a wide range of factors which may cause a disjunction between what people do and say. One reason is that there is a real

difference between verbal and non-discursive knowledge. For example, we may know how to speak without being able to explain the grammatical rules of the language. So, in material culture, we may know what to do even if we cannot explain it verbally. This appears to be the case in modern western society where what people say about material culture is probably less thought out than their views on religion, sex, politics and the like. Material, everyday knowledge is not and need not be an area of discursive knowledge.

In some cases this undiscussed, hidden meaning of material things can be used strategically in social relations. For example, women may be able to make subtle and effective statements about their social position by using everyday artifacts which are not consciously discussed and regulated by dominant male members of society. But it is difficult to generalise. In other societies, true knowledge about the meaning of things and their spatial arrangements in settlements may be controlled by an elite minority. This dominant group may maintain its position partly by a withholding of knowledge about how the world is put together and the members of that group may intentionally mislead the uninitiated (cf. Barth 1975). How difficult it is going to be, in such a situation, for the ethnoarchaeologist to penetrate the relationship between what is said and what is done.

Much of the confusion may, however, result from simple and direct lying where the informant seeks immediate gain from the ethnoarchaeologist. For example, in my Baringo example, it is probable that the ages of pots were grossly exaggerated because it was thought that I might want to buy pots (in the footsteps of the anthropologists who had worked in the area before me). It became important to be known in the area as someone who did not have purchase in mind. Other reasons for intentional misleading are extremely numerous and include the possibility that the researcher is an inappropriate sex or age, that the area has had a history of maltreatment by white colonial powers, that missionaries have encouraged a feeling of guilt in relation to traditional activities, that the interpreter is not respected, or simply that the ethnoarchaeologist, wishing to measure pots and refuse, is perceived as a fool, unworthy of attention.

A further reason for a lack of correspondence between what is seen and heard may result from problems of comprehension. Whether the researcher is trying to learn the language or is using an interpreter, there may be many misunderstandings. In fact, the informant with the best will in the world may be unable to understand and comply with the questions asked. Binford (1978) records that, in retrospect, his initial questions of the Nunamut Eskimos could not be answered in a straightforward way, however much this frustrated him at the time.

The very presence of the ethnoarchaeologist may mean that people behave in a different way so that what they say and do may be different from what is observed. The outsider always changes the situation that

is being observed. Yellen (1977, 286) discusses how food is distributed within a society, but this includes food distributed to an anthropologist. The ambiguous position and standing of the outsider may cause entirely novel ways of distributing food and of acting within the home environment.

Given that the verbal and non-verbal often do not coincide, and that the degree of coincidence varies with culture and context, we need to examine models which link the ideal and the actual together. Yellen (1977) provides an example from his work amongst the !Kung Bushmen. In practice, !Kung individuals move widely and social ties are loosely organised, but the !Kung see themselves as a series of loosely defined territorial groups. Yellen (*ibid.*, pp. 48-9) suggests a model according to which these two contradictory aspects are part of the same system. The formal band territorial model provides a mental framework for deciding on and justifying varying courses of individual action and for predicting in a loose way what others will do. But in the !Kung environment there is a need to be able to adapt rapidly and flexibly. So, over the long term, there is a practice of individual changes and movements in order to rearrange the demographic map.

During the chapters which follow, other examples will be given of the structured way in which idea and practice may not correspond. But, as Bloch (1971) demonstrates in his analysis of the ideal component of Madagascar burial which contrasts with the practice of daily life, it is incorrect to assume that the material world is more or less 'real' than the non-material. Both aspects are organised in relation to each other, as complementary components in a cultural unit.

Given the pervasive 'problem' that spoken and observed information may not coincide, for whatever reason, some archaeologists have suggested the particular 'solution' that was mentioned earlier (p. 40). The 'problem' is 'solved' if we accept Gould's materialist position. Wobst (1978) notes that ethnography involves the study of recorded behaviour, while archaeologists study actual behaviour. In a more extreme view, Schiffer (1978, 235) suggests that ethnoarchaeologists should only observe actual behaviour, and not study word-of-mouth reports. Such an approach would first of all be impossible since, as has been suggested, the very presence of the ethnoarchaeologist in a living situation affects and changes behaviour. The more the ethnoarchaeologist remains a remote outsider the more he/she changes what he/she observes. Also such a materialistic approach limits scientific research because it is difficult to find out basic questions concerning the observed data, such as where and when and by whom a pot was made, or who is the brother of the potter. Equally, it is impossible to examine the all-important two-way relationship between discursive and non-discursive knowledge, and to examine material behaviour within a social, ideological and cultural context.

The ethnoarchaeologist must not duck the issue and play at part-

time anthropology. While the ethnoarchaeologist does bring to anthropology the training to observe behaviour and its products, there will be few research problems for which such observation is sufficient. The ethnoarchaeologist must also face the issues of how to examine what is said, thought and explained by informants.

One procedure would be to arrive in the field with a questionnaire worked out in advance with advice from sociologists and others trained in interviewing techniques. Such an approach would frequently involve filling in forms with yes/no answers. While there may be some research questions and cultures in which this approach would be adequate, there are considerable dangers involved. For example, in many countries informants will answer yes or no depending on the tone of the question; the main concern of the informant may be to provide a pleasing answer rather than a correct one. Such problems could be countered by introducing cross-checks in the questionnaire. But it may be difficult to assess all the biases of age, sex, colour and so on mentioned above with such an inflexible technique.

In many cases the informant will find the questions ambiguous or unclear. This is especially the case in an alien culture where there may be many subtle differences in meaning which cannot be accounted for in a prepared checklist. Perhaps the most serious disadvantage of the questionnaire technique is that the researcher cannot be led down new avenues. By far the best way to find out about the unknown is to listen and learn.

One extreme, but highly effective, example of this was used by Guilbert Lewis (1980) in his work among the Gnaou of New Guinea. Even though one might use a tape recorder, the anthropologist is usually present, intruding into the scene. Lewis placed his tape recorder by groups of people talking and left. The people being studied had, by then, got used to the technical device being around and carried on their conversations and rituals regardless. The ethnoarchaeologist will usually want to record visual information, and since it will be impractical to film continuously and remotely, greater intrusion must occur. Indeed, there are many advantages in participating and becoming accepted, in so far as it is possible, within the study group. Understanding may come with closer contact, there will be time to learn the language, initial distrust may be allayed. During a long stay in which some participation is achieved, it is possible to use a variety of recording procedures; tape recordings of undirected discussion, notes or tape recordings of partially directed discussion in which questions from the researcher focus the informants on a particular topic, direct observation and recording.

Clearly the degree to which participant observation is used depends to a considerable extent on the nature of the research problem. Those interested in regional settlement patterns in relation to environmental features may have less need to understand verbal information than

those concerned with the meaning of ritual. However, I find it difficult to envisage topics that could successfully be completed without any reference to the explanations of the individuals themselves. It is sheer cultural and intellectual snobbery to allow no reality to informants' analyses of their own situation. It is important to examine the relationship between the spoken causes of events and the outsider's identification of correlations and interactions.

As much as the interview technique depends on the research questions being asked, so does the sampling design. How many villages, areas, houses, pots or people should be studied depends on the aspects of behaviour that the analyst is interested in and the degree of certainty that is required in the conclusions. If the questions are fairly straightforward, such as the relationship between size of settlement and duration of occupation, there are numerous equations in sampling theory (Mueller 1975 : Cherry, Gamble, Shennan 1978) which can be applied in order to determine an adequate random sample (for example, of settlements). Usually, however, there will be numerous questions and variables to be examined and a more subjective assessment of sample size will be required.

Major problems in the sampling design may concern the differences between an archaeological and a living settlement plan. In the living situation artifacts, and even houses, are constantly being moved and it may (although this depends on the particular culture) be impossible to produce a map of the settlement 'frozen' at one moment. The ethnoarchaeologist may be particularly interested in this mobility but his recording may be confounded not only by the speed and frequency of moves, but by changes in functions of artifacts, removals from the village, re-use of houses and so on. 'Tagging' of artifacts may often be impractical or prevented by informants. For the moment there are few guidelines available to the ethnoarchaeologists faced with such problems. The particular solution will depend on the questions being asked and the circumstances and ease of work.

In all societies there is likely to be great variation in activities and use of artifacts at different times of the day, season and year. There may also be variations at longer intervals related to environmental and climatic fluctuations and historical and external political events. The archaeologist may be particularly interested in recording these various ranges of behaviour so that the resulting palimpsest can be compared with an archaeological composite pattern. Long and repeated visits to the study area are, therefore, necessary and are in any case implied by the need to participate and learn languages for which no grammar or dictionary may have been published. In practical terms, the ethnoarchaeologist may have arrived in a study area at a time when women are busy in the fields so that they cannot talk easily and domestic tools and crafts cannot be observed. It will be desirable for the ethnoarchaeologist to spend a total of at least one year in the study area.

Various other practical aspects of conducting ethnoarchaeological fieldwork in a foreign country are so contingent on the laws of that country and its particular relations with the home country that little of a general nature can be prescribed. It is, of course, legally and ethically important to obtain the necessary research and travel permits, and embassies will often be a good starting point here. For many third world countries, applications may now take six months to a year to be processed, and the outcome may well be unsuccessful. Plans must, therefore, commence early and alternatives prepared. Many countries are now, understandably, highly sensitive to the attentions of anthropologists seeking a traditional 'primitiveness'. Success in applications is best assured through those already working in an area, and by a clear and honest statement of research aims.

In conclusion, in undertaking ethnoarchaeological fieldwork the archaeologist is aware of responsibilities to the country and people studied and to the anthropologists who, in the future, will wish to work in the same or similar areas. Their work depends on the good relations set up by earlier researchers.

Involvement in ethnoarchaeological fieldwork also requires recognition of the peculiarity of the task. One has to be more than an anthropologist and more than an archaeologist. The differences from archaeology are clear. There are also differences from anthropology. First, there is a particular concern with material culture. While the materially constructed world, including everything from fine painting to hearths, is a hopelessly broad category about which to generalise, there are certain aspects such as the ambiguity of material as opposed to verbal symbols, and the non-discursive nature of much material organisation, which do allow material culture to be defined as a distinct area of analysis (for further discussion of this point see Chapter 10). Second, ethnoarchaeology includes and focuses attention on everyday mundane behaviour, not just the rituals and myths of societies. Third, as has already been noted, the ethnoarchaeologist is trained to observe behaviour which he/she can then compare with written and spoken information. In all these, and probably many other ways, ethnoarchaeology is a distinctive area of field research with particular problems of its own. As well as being versed in anthropological field techniques, the ethnoarchaeologist must also be aware of additional problems, some of which I have tried to outline in this chapter. It is only as further ethnoarchaeology is completed that a full text of appropriate field techniques and biases can be written.

3 The formation of the archaeological record

Having seen some of the history of archaeological anthropology and of its practice as ethnoarchaeology, part of the range of results which have begun to emerge can be examined. It will be necessary to evaluate the analogies which have been suggested and to suggest the types of ethnoarchaeology which have been and will be most successful. In Chapter 1 various desirable aspects of analogies were identified, and will be sought for in discussing the formation of the archaeological record, the processes by which artifacts become deposited in the ground, and the post-depositional processes by which they are disturbed and survive.

It is often difficult, however, to draw a line between depositional and post-depositional processes. Is trampling of artifacts into the ground by human feet or domesticated animal hooves depositional or post-depositional, and, in Schiffer's (1976) terminology, is it a natural or a cultural transform? It is easier to provide adequate relational analogies when some natural link can be suggested between the various components of the model and so it may be appropriate to begin with post-depositional processes in which there is little human or cultural input.

Post-deposition

For example, once a house has been built, its decay and collapse may often be entirely natural processes. The houses in *fig. 7* are all collapsing in similar ways. Rather than simply falling over or inwards, the whole structure rotates and sinks to the ground. As the wall posts are twisted round they cause identifiable changes in the holes, trenches and moulds in which the posts are set. The ethnographic case suggests to the archaeologist that one might look for such patterning in post-hole shapes and angles while digging a prehistoric round house. Such information would suggest that the house had been left to fall down by natural processes as opposed to having been dug up intentionally as in the Zambian case in *fig. 8*. Here huts are dug up and pulled down when the senior male occupant dies. These post-depositional traces would be very different from those left by the Baringo huts. However, we cannot assume that rotational collapse always occurs with round houses. Such a process in the Kenyan case is encouraged by the shallowness of the post settings and by the method of lashing the roof to the wall posts. Nevertheless, it would be possible to examine the various ways in which round houses of different constructions collapse, with the