

# THE LIMITATIONS OF INFERENCE IN ARCHAEOLOGY

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**M**OST of the time of this Conference is being devoted to assessments by experts of different types, or aspects, of archaeological evidence. From their intimate knowledge of the particular fields they are able to point out to us both the shortcomings and the potentialities of the data which they study. An archaeologist, in interpreting his material, takes into account the results of all these, and of other particular studies which are relevant to his problem. What I want to do, alongside these specialist accounts, is to try to consider, in general, what sort of inferences an archaeologist can expect to make when he utilises all this evidence to the full. We are here to discuss the reliability of archaeological evidence, but obviously before we can decide how far any type of evidence is reliable we have to know what it is supposed to be reliable for; in other words, we have to be clear what sort of problems archaeological evidence can legitimately be expected to answer.

I shall not need, here, to give an account of the history of archaeological research in this country. Our progress in field-work dates from the general adoption, in the present generation, of standards which General Pitt-Rivers was already applying, as a pioneer, in his excavations at the end of the 19th century. Accurate and exhaustive recording of sites raised archaeological excavation to the status of a science. Then, in the 20th century, in the period between the two wars, there was the further assimilation of archaeological to scientific practice, with the increasingly active co-operation of natural scientists themselves in archaeological problems. Scientists began to provide the archaeologist with precise determinations, not only of his finds, but also of their contexts. Quite rightly today we stress the value of this co-operation, which helps the archaeologist to

make use of every potentially significant fact about his material.

But, on the other hand, the very precision of scientific analyses, and the elegance of a well-laid-out excavation, do tend to have a rather hypnotic effect on the mind, and some people have thought that there was a danger that archaeology might come to be equated simply with a competence in handling a set of techniques. The result has been that when archaeologists in recent years have had occasion to stand back from their detailed studies and review the subject as a whole, what they have tended to do is to reassert its essentially human aspect against a too persuasive "scientific" approach. We are reminded that the relics we study are the result of human activities. As archaeologists we classify these remains, and the recurrent combinations of material we term "cultures", in a technical sense — the Maglemose culture, the Windmill Hill culture, the Deverel-Rimbury culture, and so on. But, if I may quote from Professor Childe, prehistoric artifacts, being made by men, have "their significant features... determined, not by mechanical processes, but by social traditions and purposes. Accordingly," he concludes, "to be significant archaeological classification must embrace the cultures, that is, the societies, whose aims and needs determine the artifacts' characters."<sup>1</sup> Again, Professor Clark, in his inaugural lecture, declared: "As prehistorians we deal not with individuals... but with societies, including their internal stratification and their local organisation, and their relations to one another and to the world of nature... Prehistory is essentially a social study and as such it has obvious affinities to social anthropology."<sup>2</sup> Finally, an eloquent expression of the humanistic approach from the pen of Sir Mortimer Wheeler: "the archaeological excavator is not digging up things, he is digging up people; however much he may analyse and desiccate his discoveries in the laboratory, the ultimate appeal across the ages...

is from mind to intelligent mind, from man to sentient man.”<sup>3</sup> “The only thing that really matters in our work,” as he said on another occasion, “is the *re-creation of the past* . . . archaeology and history are alike frustrate unless they contribute to a vital *reconstruction of man's past achievement*, in other words aspire to interpretation as well as to mere transliteration.”<sup>4</sup>

We have then here assertions that an archaeologist should try to re-create the past, or at least that in his cultures he is dealing with societies, and therefore claims to apprehend societies which existed before the times of written record from their material remains. If this is correct, prehistory must in some sense approximate to history. But alongside this view of the nature of the subject we have to keep in mind the caution which Sir Mortimer Wheeler himself appended to the passage I have just quoted: “The archaeologist may find the tub,” he said, “but altogether miss Diogenes.”<sup>5</sup>

It is admittedly often the case that the conclusions of an archaeological study can be expressed in the form of a narrative about past events. The statement: “Gold lunulae were exported from Ireland to the Continent in the Early Bronze Age” may be taken as an example. Now the essential character of this conclusion is that it translates back into the observations which gave rise to it. We map the distribution of known lunulae, and find a marked concentration in Ireland, a country rich in mineral gold. The evidence of other metal types found in that country suggests that the Irish were in fact skilled in metal work in this period, so that it is plausible that the lunulae were made there. Furthermore, other distributions show that a connection with the Continent across England or Scotland did exist. The conclusion that lunulae also were exported along this route seems to follow from these observations, and it is the sum of these relevant considerations which we have in mind in asserting it.

We should, I think, all agree that in such instances the conclusions are legitimate inferences from the archaeological material. All the evidence used in them can be empirically verified, and nothing has been added to it.

If, then, we allow that a certain sort of narrative about undocumented events does emerge from archaeological studies, we can now go on to consider how far this corresponds to, or is different from, the re-creation of the past, which is said to be our proper aim as archaeologists.

It is often pointed out that the non-perishable material remains of prehistoric peoples imply fairly clearly how their economic life was carried on, and how it was adjusted to fit the environment in which they lived. Certain aspects of religious

and social practices also are reflected in what remains for us to study. Among the megalithic tombs of the Marne, in France, for example, some are found to contain only a few skeletons, and rich grave-goods, while in others of a similar plan there are many skeletons and only poor offerings. Professor Childe concluded from this that the former “must have been reserved for chiefs”, and that the others were “presumably graves of commoners”. Of course, it might be another distinction which is represented here, say between priests and laity, but in its main lines this conclusion is acceptable, because it is easy to see how it is related to its evidence. It is very similar in sort to the conclusion about the Irish lunulae because, like that, it is in fact virtually only a *paraphrase* of empirical observations.

Arguments to the social and religious practices of prehistoric peoples, when there is no direct or indirect written evidence, however, are not always so simple. Let us consider an example which Professor Hawkes uses to illustrate this: “If you excavate a settlement in which one hut is bigger than all the others, is it a chief's hut, so that you can infer chieftainship, or is it really a medicine lodge, or a meeting hut for initiates, or a temple?”<sup>6</sup> Obviously it will be important to know which of these interpretations is correct if in fact we are engaged in re-creating the past, or in studying *societies* “including their internal stratification,” as Professor Clark said.

How should such problems be tackled? Clearly, the conclusions of prehistory cannot even approximate to history unless there is some way of deciding between the various possible interpretations in Professor Hawkes' example. We do not get very far in re-creating the past, or in understanding a prehistoric society, if we cannot be sure whether the largest hut in a village is a temple or a storehouse.

R. G. Collingwood used to say that the way to interpret a piece of archaeological evidence was to try to appreciate the problem of the man who was responsible for creating it. This method is all very well when one already knows, or can guess with reasonable certainty, what that problem was likely to be: the explanation of multivallate hill-forts, for example, follows from our *knowing* that a defence was needed against sling-stones. But even if we decide to allow that prehistoric peoples were as intelligent as ourselves — if not more astute — when faced with problems which we *know* confronted them, we must not make the mistake of assuming that they *could* have been aiming *only* at things which seem justifiable to us. The Western European standards of value which condition our own thought are not absolute. It used to be thought that studies of surviving primitive peoples would provide the necessary

analogies for interpreting prehistoric societies; but in the event the extension of ethnological studies has only served to show what an incredible variety of codes of behaviour in fact actuate human conduct.

Malinowski was the first to undertake a thorough investigation of the life of a primitive people, and his study of the Trobriand Islanders of New Guinea is the obvious one from which to quote.<sup>7</sup> He found that the growing of the staple yam crop was the central activity of the community, but that it was an activity shot through with magical, religious, and social implications, which cannot, in Trobriand society, be thought of as features additional to the economic aspect of the work, which is never considered there in abstraction. To take only one example: at harvest the crop is divided according to the rule that a man must give his best yams to his married sister and her family, often carrying them over long distances to where she lives in her husband's village. Thus the distribution of the staple crop, which we should think of, and tend to interpret, in purely economic terms, is there entirely governed by the requirements of a complicated kinship system.

Moreover, to have any understanding of the central activity of Trobriand society we must take into account what Malinowski called the "aesthetics of gardening". He reported that considerable energy is spent on purely aesthetic effects. "The ground before planting is cleared with a meticulousness far beyond what would be necessary on technical grounds. The cleared soil is divided into neat rectangles about 4-10 metres long and 2-5 metres wide by means of sticks laid on the ground. These rectangles have little practical purpose, but much value is attached to their proportions and to the quality of the sticks which mark their boundaries."

Again, far more yams are grown than are needed for sustenance. During the harvest, work is twice interrupted while the yams are scrupulously cleaned and arranged on show, with temporary structures built over them. In short, in order to understand what the people were *doing* when engaged in growing their subsistence crop, it would be essential to realise that at all stages aesthetic considerations were important, and that an interchange of visits for mutual admiration was a constant feature of village life.

If we imagine a perfectly conducted excavation of an extinct Trobriand group, supposing that conditions were at their most favourable for the preservation of evidence, and that the potential significance of the remaining material was fully appreciated, how far could we expect to get towards *re-creating* Trobriand society? How far could the archaeological classification of the remains incorporate the "aims and needs" of the

society, which is what Professor Childe requires if the classifications are to be significant?

The size and lay-out of the yam gardens would confuse us, if we imagined that an increase in crop production was the only aim of work undertaken. Calculations from the estimated yield of yams would suggest a far larger population than in fact existed. We could not know that the structures built in the gardens were intended only for temporary display of the harvested crop, or that the coral walls round the gardens had magical significance only. And if yams from village A turned up in village B, how could it be guessed that the transaction was not commercial? Even if it were suspected that a token offering was represented, we should be more likely to think of it in terms of a tribute to political authority, than the expression of a kinship tie.

The complexity that was Trobriand society, the sort of considerations which would enter into a reconstruction of it which was in any way comparable with the study of the anthropologist, simply would not follow from the observations an archaeologist would be able to make.

I have not chosen to quote the Trobriand society because the principles which govern it are especially aberrant from European values, so that it would be a particularly difficult instance to infer to its life and institutions from the material remains. That would be special pleading. I hope, rather, that it will illustrate and bring home a general point.

I am thinking of the problem of Diogenes and his tub, which will have to be met by anyone who maintains that archaeology aims to reconstruct man's past achievement, and of the comparable problems involved in asserting that archaeological cultures represent societies, i.e. the essential social divisions of prehistoric peoples. It would obviously be impossible to understand the relics of the Trobrianders from the evidence of the material remains alone: they don't even act rationally, on standards we can comprehend, when engaged in growing their subsistence crop. By considering why even the closest possible attention to the material remains reveals so little of what is significant, I think we can get a clearer idea of the sort of questions we should expect archaeology to answer.

If, as in Professor Hawkes' example, you were to infer from the single large hut to the institution of chieftainship, this conclusion would not be comparable with the one about the export of Irish lunulae, which we examined earlier. This conclusion will not translate back exactly into empirically verifiable statements which are supposed to imply it; it is not merely a paraphrase of a set of observations. It contains an element of *conjecture*, which cannot be tested. If you decide

to call the large hut a chief's hut, and not a meeting-house, or a temple, this is an *assertion*, not an argument. You can't really say that you *know* that it is, and if someone criticises your assertion, it is impossible to produce *sufficient* evidence to convince him you are necessarily right. This is not due to any fortuitous incompleteness in the archaeological record: the position couldn't be improved by better excavation, by finding a more favourable site, or by the invention of a new technique for analysis.

It arises from the sort of relation which holds between the human activities we should like to know about, and the visible results which survive from them; between these two things there is logically no necessary link, so that it is a hopeless task to try to get from what remains to the activities by argument. This is what I mean by the limitation of inference in archaeology. A code of behaviour, or the idea of chieftainship which prevailed in a particular tribe, cannot be expressed adequately by things like the lay-out of yam gardens, or large huts, so that, conversely, the garden and hut plans, from which the archaeologist has to work, cannot imply them. It seems to me important to realise that trying to establish the existence of, say, chieftainship in the prehistoric past is something quite different from reaching the conclusion that Irish lunulae were exported in the Bronze Age. It is not related to its evidence in the same way, and can never attain to the same status in knowledge. You can infer, i.e. proceed by argument, from archaeological remains to some aspects of prehistoric economics, and reasoning will take you to a limited range of social practices, like the distinction of rite perceptible in the Marne tombs. But to expect an archaeologist to infer from a hut to chieftainship, or from the tub to Diogenes, is nothing less than a demand for logical alchemy.

This means that the accounts of the past we can offer in prehistory are necessarily different in kind from the accounts of history, where at least something of what was truly significant in the period is recorded. There is, of course, no distinction in the subject matter: human experience runs back unchanged from historic to prehistoric times. The difference arises from the nature of archaeological evidence, that is from the means we have of *coming to know* about this human experience and activity in undocumented phases of the past. This in turn requires some caution in assertion about how far archaeological interpretation can take us in knowing about the past, and what the proper aim of prehistory should be.

It is often said that archaeological cultures represent prehistoric societies, and by way of example I propose to examine this one assertion about what is revealed about the past by

archaeology. Now the minimum definition of a society in anthropology or sociology is a group of people acknowledging a single political authority, obedient to a single system of law, and in some degree organised to resist attack from other such societies. How is an archaeologist to be able to judge when these conditions hold? Are we to think that the Windmill Hill culture was a society in this sense? Was there one system of law and a single political authority throughout, or perhaps a single legal system common to all users of plain Western Neolithic pottery, and a separate political authority for each of the variant styles of ceramic decoration? a Hembury, an Abingdon, a Whitehawk and an East Anglian authority? These questions are soon seen to be meaningless, and indeed how could it be otherwise? Recognition of political authority and of systems of law, like Diogenes, do not find expression in material things, and so evade the prehistorian. If we ask ourselves what archaeological material we should accept as an indisputable indication that a single political authority was in force, it becomes obvious that, when we are working from material remains alone, there could never be the sort of evidence necessary to substantiate a claim that the Windmill Hill culture represents one, or alternatively several, of the communal groups which an anthropologist or sociologist calls a society. We may wonder whether it does, but we can never know.

Similarly, we find ourselves speculating about the relation of the succession of cultures we establish in archaeology to the historical realities of the period. But once again, we can never know whether the events which were really decisive in the history of the times we are studying got reflected in material equipment at all, or in activities which left material results. It looks very much as though questions like these, how far archaeological cultures correspond to actual societies, or what is the relation of archaeologically recorded events to historical realities, are not only meaningless but irrelevant to the archaeologist, because there is no means for him even to begin to answer them.

I began this evening by recalling how much the natural sciences have contributed to the progress of archaeology. By providing minute analyses they have recovered much significant information, which in the days of haphazard excavation and less critical studies was destroyed or overlooked. We may say that science has taught the archaeologist a due respect for his material and for his sites.

It has to be acknowledged that there is no logical relation between human activity in some of its aspects and the evidence left for the archaeologist. Accordingly there are real and insuperable limits to what can legitimately be

inferred from archaeological material. Some time ago, a competition in the periodical "The Spectator" asked for definitions of "prehistory". One of the suggestions commended by the adjudicator was "The cradle of mankind rocked by the mother of invention." Now if as archaeologists we purport to be trying to re-create the past, or to be *able* to apprehend prehistoric societies, this type of comment becomes embarrassingly pertinent. I want therefore to end by suggesting that, having been taught by science a due respect for the material we study, we might equally learn from its methods a respect for our subject itself.

I am not suggesting that the archaeologist has anything useful to learn from the intricate conceptual apparatus in which current scientific research is conducted. But it is well worth while to turn back to the 18th century, to the discussions leading to the establishment of the method which has been basic to the scientific progress of the last two centuries.

Bishop Berkeley is the classic commentator on the unsuccessful efforts of earlier science to comprehend the essence of material things in the way of Greek philosophy; in posing themselves this problem, he said, natural philosophers were first "raising a dust, and then complaining that they could not see." Now in many respects the problem which Berkeley was dealing with here is, methodologically, analogous to the one of deciding what we can expect archaeological evidence to be reliable *for*. Of course we are quite conscious that there must have been rich historical realities behind the material relics we study, whereas Berkeley denied that material substance existed at all. But nonetheless his recommendations to his contemporary scientists are directly relevant to us. He urged that, since it was proving impossible to come to any knowledge of the essence of material things and real causes from the data available, this could only mean that it was wrong for science to aim at finding out about them. Scientists should content themselves with arranging the information which they *could* come by, by working correlations between events, and so on; by establishing "observable regularities" as he put it. Unobtainable ends cannot be the proper ends for any subject. What any subject aims to do must be appropriate to the means at its disposal.

The subsequent acceptance of Berkeley's teaching was to provide the philosophical basis for the natural sciences in the progress they have since made.

If we apply his axiom to archaeology, it follows that, since historical events and the essential social divisions of prehistoric peoples don't find an adequate expression in material remains, it cannot be right to try to arrive at a knowledge of them in

archaeological interpretation. A recognition that archaeological evidence, when it is confined to material remains, demonstrably supports only a limited range of conclusions about human activity, is incompatible with too ambitious a programme for archaeology. It is incompatible, as I see it, with an attempt to "re-create the past" in any real sense, or with a claim to recognise prehistoric societies from their surviving relics, so that the subject could be compared either to history or to social anthropology. Moreover, if it appears that archaeologists do make such a claim, or are engaged in such an attempt, it may seem to an outsider, conscious of the weak logic this involves, that the subject has no sound intellectual basis at all.

I shall therefore end by suggesting that a respect for the subject requires that we should consciously adopt Berkeley's method of defining the aim of our interpretations by strict reference to the potentialities of the evidence. It is only the problems to which the evidence can provide answers which can legitimately be asked. The fact that we know that human societies and their histories were responsible for the remains we study tends to obscure this, but it should not affect a realisation that attempts to establish prehistoric societies, and events, in the historical sense, must rest on conjecture, not on argument. I should prefer not to say that archaeology "re-creates", or "reconstructs" at all; it merely *recovers* what it can. That of itself is a sufficient programme of research. And we should not be tempted to clothe the results of this research with more significance than they can legitimately carry. There is no point in asserting that an archaeological culture which we have established represents a prehistoric society if we can never *know* that it does.

When Bishop Berkeley repudiated the search for material substance in natural science, in favour of the "observable regularities" he could validly establish, he said he determined to know "what I know, and not some other thing". And, in face of any exaggerated claims about what archaeology can tell us about the past, I think this would be a good motto for the archaeologist.

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