CORRESPONDENCE

Archaeology and theory

I am grateful to the editor of MAN for allowing me to respond to Binford (*Man* N.S. **22**, 391–404). A response seems particularly appropriate given the wider public debate about his article (Times Higher Educational Supplement 2.10.1987 and 16.10.1987).

At the end of the THES report of 2.10.1987 it is concluded, wryly perhaps, that according to Binford 'archaeology's only hope is to treat things as things and nothing more'. This may be an inaccurate summary of Binford's position. After all, he has elsewhere made a strong case for theory building and testing of theory against data. But on the other hand, the comment captures some of the implications of Binford's position. Amongst archaeologists still heavily influenced by the direction of the New Archaeology of the 1960's in which Binford played such an important role, the over-riding emphasis is on methodology—statistics, sampling, site formation processes, settlement patterns and the like. There is agreement with Binford that one must remain objective, explicit and rigorous. There is a belief that it does not matter where theory comes from. As long as one gets the methodology right, all will be well.

At least in the early days of New Archaeology it was claimed that archaeology was anthropology, and as a result interesting attempts at generalisation were made. Because constructed within a narrow, ecological and functional view of cultural variety, these generalisations tended to fail or appear trivial. The retreat was into methodology, in accordance with the view that 'the external world exists in its own right, and that includes the properties of the archaeological record' (p. 403). It is entirely consistent with this retreat that Binford should now be retreating from ethnography as well—indeed from all the social sciences. According to Binford, many of the major debates in the social sciences are 'metaphysical issues that are not really appropriate to science in fundamental ways' (p. 391). The data of social scientists (except archaeologists) are 'inadmissible for scientific research' (p. 393), and ethnographers 'are still not operating in a scientific role' (p. 395).

This is a sorry state of affairs for the rest of the social sciences, perhaps. But it is even sadder for those archaeologists who espouse Binford's views. For archaeology to be a science it must disassociate itself from ethnography and the social sciences. Archaeology is reduced to dry methodology, isolated from the great contemporary debates in social theory, unable to contribute to our understanding of history, culture and the construction of meaning. Indeed the extent to which Binford's programme for archaeology has diverged from anthropological concerns is distinctive. On the anthropological side there is human agency, structure and event, culture and economy, the reactions to deconstruction and critical perspectives. On Binford's side there is bone taphonomy, traces of use wear on flint, and hunter-gatherer foraging strategies.

In fact Binford, in other works (e.g. 1983), has shown how careful study of these latter middle level concerns can produce important implications for our understanding of early humanity. On the other hand, as soon as one gets beyond the purview of primary depositional arguments, there is little that can be contributed to broader debates by adhering to the rigid view of science advocated by Binford. As a result of the failure of that view to deal with complex contemporary issues, many archaeologists are beginning to seek alternatives.

Binford seems unclear about these alternatives. He lumps divergent positions in contemporary archaeology together. I do not know what a 'Marxist contextual' (p. 403) approach is, for example. However, underlying his criticisms is a general misunderstanding which I presume results from an assumption that in discussing cultural meanings, what people say is of primary importance. Binford privileges speech.

The first result of this assumption is that all ethnography is condemned because Binford sees the ethnographer dependent on information received from informants. The anthropologist can thus never seek explanations, only understanding in others' terms. It is certainly true that some ethnographies can be criticised for over-reliance on authoritative informants. But most contemporary ethnography goes beyond what is said by informants to structures or systems of meaning which make sense of what is said. Certainly in many modern material

culture studies, ethnographers or ethnoarchaeologists would not get very far if they relied on indigenous exegesis. A common response to the question 'why did you make this pot this way?' is 'because it is tradition'. The scientific ethnographer and anthropologist is concerned to move beyond spoken words in order to explain them within a wider framework, and to move beyond material objects and historical events to discover generative processes. Binford is incorrect in implying that ethnographers simply record what is said and leave it at that. Whatever happened to symbolic anthropology? Or anthropology more generally? Or history?

As a second result of Binford's privileging of speech, Binford suggests repeatedly in his Huxley Memorial Lecture that because archaeologists have no informants they cannot make progress in making inferences about cultural meanings. I do not understand why Binford thinks that the material world was not patterned meaningfully in the past and that traces of these past patterns do not survive in the archaeological record. The material world is organised in ways comparable to, if in detail different from, spoken sounds. Its meaning can thus be debated critically and rigorously in ways similar to the methods of ethnography or history even though it is clear that within this wider framework archaeology must develop its own method and theory.

There is a danger, in following Binford's line, of the death of archaeological theory. There is a danger in retracting from the increasingly critical perspective of archaeologists. Binford accuses archaeologists of being 'theologians' (p. 404) and of making arbitrary value judgements. He accuses me of 'socio-political moralising' (p. 402). The main reason I have taken such a stance is that in order to be scientific (i.e. 'objective' or explicit) it is necessary to be theoretically critical. It became clear that in the New Archaeology there was a hidden political and moral agenda which was used unscientifically because it was not subject to critical examination. These deep assumptions, Binford's own 'theology' are clear in his Lecture, in statements such as 'The external world exists in its own right' or 'The claim that our cognitive devices insulate us from the external world is false' (p. 403). These value judgements are presented without substantiation. It is difficult to see how they could be verified. They are presented as *a priori* knowledge, as ideas flowing 'self evidently' from the 'human' experience, and they encapsulate assumptions, which are social and political in nature, about the role of the scientist in society. These are exactly the points for which Binford criticises post-processual archaeology.

Should archaeology as a science be about knowledge or should it also be about meaning? Oversimplified, this is the question at the hub of the contemporary debate. Binford wishes to move in an orderly manner 'to an accurate appreciation of the past' (p. 404). Others realise that any such aim must involve the critical interpretation of past and present meanings in relation to each other. Indeed, if Binford would look at his own work with the same powers of criticism that he brings to the work of others, he would find that his own writing and research about the present and past are prime examples of the social construction of meaning within which we all play a role.

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Binford, L. R. 1983. *In pursuit of the past*. London: Thames & Hudson.

It does not surprise me to learn that Dr Hodder takes exception to virtually the entire content of the 1986 Huxley Memorial Lecture [published in *Man* as 'Data, relativism and archaeological science'], for he and I have been disputing the utility of one another's views, in person and in print, for nearly a decade. What does concern me is Hodder's apparent assumption that the words 'criticism', 'theoretically critical', and 'critical examination' represent the same intellectual processes as the words 'scientific research', 'argument', and 'knowledge'. It is the equation of one set of terms with the other that permits Hodder to claim: 'in order to be scientific (i.e. "objective" or explicit) it is necessary to be theoretically critical', and it is from this critical posture that a 'hidden political and moral agenda' was perceived to be embedded in the New Archaeology, with which I have been associated.

I would argue that the pursuit of *any* intellectual goal, and this would include the goal of discovering a putative hidden political and moral agenda, involves not only an appropriate procedure but also an awareness among those participating in the quest that the procedure appears appropriate to their knowledge goals. If there is disagreement about the goal or the procedure, or the fit between the two, then evidence must be organised in the form of an argument that has reference to, and can be evaluated in terms of, the aspect of the external world that is being debated. This is what science is and does, and if Hodder were ever to approach my work from this perspective, I am certain that I would benefit from his attention.

Within the field of archaeology, unfortunately, there is considerable controversy with respect to even the most basic issues, with the result that in his last paragraph, apropos of disciplinary goals, Hodder asks: 'Should archaeology as a science be about knowledge or should it also be about meaning?' One is left to assume that he would place himself among

those who hold that 'any such aim [an accurate appreciation of the past] must involve the critical interpretation of past and present meanings in relation to each other'.

But, isn't it reasonable to ask how such a goal is to be accomplished? Where do past and present meanings come from? I have no difficulty identifying the source of present meanings: they come from us, from our ideas about how the world works and why it works the way we think it does. In fact, the quintessential concern of the activity that Leslie White has termed 'sciencing' (1969) is with an evaluation of our ideas—the terms in which we think and with which we interpret the world—according to strictly defined rules and procedures.

On the other hand, I have considerable difficulty when it comes to the subject of past meanings. I don't know of one person in the history of archaeology who would dispute Hodder's claim that past human 'material culture was meaningfully constituted' (Hodder 1986: 1). The problem for archaeologists is how do we extract from the fragments of artefacts and features remaining for us to observe in the archaeological record an accurate view of the meanings these objects had for participants in ancient cultural systems? After all, inherent in what is meant by a symbol is the arbitrariness with which it is invested with meaning. As we know, a circle or a square can be given any meaning. Unless there is some way of connecting the symbol from the past to something in the present for which the meaning is arguably secure, then any meaning that we give to the past symbol is inescapably a contemporary meaning.

Notwithstanding what some, including myself, consider to be the epistemological difficulties inherent in the search for past values, Hodder has elsewhere espoused an interpretive procedure that was given expression and applied by Collingwood. Hodder has justified the latter's method in the following terms:

> The mind is able to imagine and criticize other subjectivities, the 'inside' of other historical events. . . . Although each context is unique in that it derives from a particular historical circumstance, we can have an identity or common feeling with it; each event, though unique, has a universality in that it possesses a significance which can be comprehended by all people at all times (Hodder 1986: 95).

At a further point in the same text, Dr Hodder writes approvingly of the perspectives on the past assumed by some contemporary achaeologists whom he classifies for the reader into groups with common ideological goals. The taxonomy consists of 'indigenous archaelogies, feminist archaeology, and working-class and other perspectives within the contemporary West' (p. 157). All these groups approach the methodology of archaeology similarly' 'the past is subjectively constructed in the present, and secondly, the subjective past is involved in power strategies' (p. 157).

According to Hodder, it is never possible to separate our ideas about the world from our perceptions of it, and therefore the goals of science are unattainable (Hodder 1985: 12). The 'dry methodology' that isolates archaeologists 'from the great contemporary debates in social theory' is to be abandoned, and in its place a methodology of critical detate will be substituted. Those who persist in demanding that their intellectual goals and methods be compatible with the nature of the phenomena that they investigate are, according to Hodder, afflicted with 'a concern with scientific control and the desire to make a relevant contribution to the running and administration of a modern world' (1985: 8).

To anyone who takes these issues seriously, reading Hodder's work is confusing and responding to his complaints is frustrating. On the one hand he maintains that by empathetic projection it is possible to have an insider's view of attitudes and concerns held by persons in the very distant past, yet on the other he argues that human perceptions, at least those of archaeologists whose work he disparages, are culturally biased and passé. Some of us are told that the methods we use to evaluate and correct our subjectivity are misguided and that our motives are anti-humanistic, yet others are told that the road to understanding passes through the less-than-rigorously-defined arena of critical debate.

Hodder's ability to entertain radically different standards for archaeologists with different intellectual goals reminds me of the 'pretty taste for paradox' affected by Gilbert and Sullivan's modern major general. I will, however, venture into the arena of critical debate under the assumption that Hodder intends that his own work be subjected to the kind of scrutiny he recommends.

I should like to examine briefly the attempt at meaning reconstruction that is contained in 'Burials, houses, women and men in the European Neolithic' (in Miller & Tilley 1984). In this chapter Hodder maintains that western European megalithic mortuary structures dating to between 4 and 2 thousand years ago 'referred symbolically to earlier and contemporary houses in central Europe and, to a lesser extent, in western Europe. . . . The tombs signified houses' (p. 53). It is further asserted that 'the existence of the tombs, their form and function can *only* be adequately considered by assessing their value-laden meanings within European Neolithic society' (emphasis added).

Hodder's confidence in this perception of European Neolithic social values is said to rest

on eight 'points of similarity between Neolithic houses in central Europe and long burial-mounds in Atlantic Europe' (p. 58); these include shape and orientation of the structure, location and orientation of the entrance, and the fact that both 'tombs and houses frequently have internal decoration' (p. 59). It is important to remember that Hodder is suggesting a tradition of identical meaning across time and space that spans more than two thousand years and five hundred miles, and also that the identification of this extraordinary continuity is based on eight points of similarity. Hodder observes, 'as in any analogical argument, any one point of comparison, on its own, could be seen as coincidental. But as the numbers of similarities increase it becomes unreasonable to argue for a lack of any significant relationship' (1984: 59).

But has Hodder really made his case? Did fourth century B.C. megalithic tombs 'mean' houses to the persons who built, used, and maintained them over long periods of time and in many different places? Perhaps the following example, which also deals with mortuary paraphernalia, will help us to answer these questions.

Imagine that a person visits a local car dealership and places an order for a new vehicle. The customer specifies that it is to be black, and it must be equipped with power steering and brakes, air conditioning, grey leather upholstery, electrically operated windows, a stereo system, and a clock. The salesperson promises that delivery will take place in seven days. Exactly one week later the customer appears at the dealership and to his surprise the salesperson hands him the keys to a brand new hearse. 'I didn't order a hearse', bellows the customer. 'I wanted a *car*.' In an attempt to justify the substitution of the hearse in place of the car, the salesperson responds, 'But sir, this vehicle has all eight of the options (points of similarity) that you specified in your order. Therefore, these two vehicles are equivalent (mean the same thing).'

My question is: how probable is it that our mythical customer took delivery of the mythical hearse, even though it is likely that the car that he thought he was ordering, and the hearse he was asked to accept, probably had more than a thousand 'points of similarity' between them?

Not surprisingly, because we disagree about the goal of archaeological research, Hodder and I are also unable to agree about the appropriate procedures to use in making inferences about the past. Hodder is led to observe that my work represents a 'retreat into methodology' and that in the Huxley Lecture I make the claim that 'for archaeology to be a science it must disassociate itself from ethnography and the social sciences'. This assertion is particularly baffling in view of the fact that I have spent many months doing ethnographic research in the Arctic and other settings and have published not only my observations but have argued for their relevance to the interpretation of the archaeological record.

What I did say is that how we go about doing archaeology matters, and that the methods of some ethnographers are inappropriate to the science of archaeology, and I attempted to present an argument in support of these assertions. Hodder's critical debate of my ideas demonstrates considerable misunderstanding of that argument, and links them to an unspecified but clearly odious 'hidden political and moral agenda'. If the subject were cuisine instead of archaeology, I can't help but wonder if Hodder would accuse Julia Child of harbouring a hidden agenda if she were to suggest that it is counter-productive to attempt to make a souffle using a vacuum cleaner.

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Ritual reinterpretations

Bruce Lincoln's provocative reinterpretation of the Swazi Ncwala (*Man* (N.S.) **22**, 132–56) enhances our understanding of this celebrated ritual by placing it in historical context. Lincoln argues that the ritual described so well by Kuper (1947: 197–225) and characterised by Gluckman (1963: 110–36) as a ritual of rebellion must be analysed in relation to colonial domination to be clearly understood. The strength of Lincoln's reinterpretation is that it relates a close reading of Ncwala symbolism to sociopolitical processes under colonialism. Three problems, however, detract from his otherwise illuminating essay. These are: 1) a misplaced critique of Gluckman's 'ritual of rebellion' thesis; 2) an inaccurate characterisation of the Swazi king under colonialism; and 3) the mistaking of a plural (Kuper & Smith 1968) for a segmentary society (Lincoln p. 151). Of these errors, (3) is perhaps the most serious for it misconstrues the