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## 40 Years of Theoretical Engagement: A Conversation with Ian Hodder

IAN HODDER, interviewed by NAR editors HÅKAN KARLSSON & BJØRNAR OLSEN

*Ian Hodder* was born in Bristol, England, in 1948. He obtained a Bachelor of Arts in prehistoric archaeology from the University of London in 1971, and his PhD from Cambridge University in 1975. He was a lecturer at the University of Leeds from 1974 to 1977, after which he returned to Cambridge where he worked as lecturer, reader and finally professor from 1996 to 1999. He has also been visiting professor at the Van Giffen Institute of Amsterdam, the Sorbonne in Paris, the State University of New York at Binghamton, and the University of California at Berkeley. He began his current position as Dunlevie Family Professor, Chair of the Department of Cultural and Social Anthropology and then Director of the Archaeology Center at Stanford University in 1999. Ian Hodder is the pioneer of post-processual archaeology, which was developed in his own and his students' works during the early 1980s. For the last three decades his reasonings have had a very important impact on the discipline's theoretical discourses and on archaeological interpretations more generally. His fieldwork – where some of his theoretical and methodological principles are put to practice – includes excavations at the famous 9,000 year-old Neolithic site of Çatalhöyük in central Anatolia/Turkey. He is the author of numerous seminal books including *Symbols in Action* (1982a), *Reading the Past* (1986), *The Domestication of Europe* (1990), *The Archaeological Process* (1999) and *The Leopard's Tale* (2006).

The conversation took place as an e-mail dialogue between Ian Hodder and NAR editors Håkan Karlsson and Bjørnar Olsen during spring and autumn 2007. It was completed in March 2008.

**NAR:** Let us start where it all began. The first issue of Norwegian Archaeological Review (NAR) was published in 1968 – which must be very close to your own introduction to archaeology at the Institute of Archaeology at University College London (UCL)? Trying to contextualize this beginning we are interested in how the archaeological world then looked like – as seen through the eyes of a young British student. What was archaeology

to you and your fellow students – (cultural) history, anthropology, a field practice, theoretical engagement? Moreover, in what ways were archaeology and your studies affected by the political issues that stirred up the academic waters at that time?

**IH:** Yes, I went to the Institute of Archaeology (now part of UCL) in 1968. I was involved to some degree in the political

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**Ian Hodder**, Stanford Archaeology Center, Stanford University, CA, USA. E-mail: [ihodder@stanford.edu](mailto:ihodder@stanford.edu)

**Håkan Karlsson**, Department of Archaeology and Ancient History, University of Gothenburg, Gothenburg, Sweden. E-mail: [hakan.karlsson@archaeology.gu.se](mailto:hakan.karlsson@archaeology.gu.se)

**Bjørnar Olsen**, Institute of Archaeology, University of Tromsø, Tromsø, Norway. E-mail: [bjornaro@sv.uit.no](mailto:bjornaro@sv.uit.no)

activism of the time and I went on anti-Vietnam marches in London. As a generation we felt able to ‘change the world’, and despite the naiveté of the claim, I have always felt that working for change was a part of all our responsibilities.

But I did not at the time see much of a link between my leftist political perspective and the idea of doing an archaeology degree. I was 19 when I started in London, and had already by that time done a lot of excavating in Britain, Greece, Crete and Israel. I was excited by archaeology because of the field-work and the social life on excavations and the opportunity to travel. In the 60s in England most excavations were run with amateur and student labour, and I had done my turn as a schoolboy digger at some of the big excavations such as Fishbourne Roman Villa. Many people came into the discipline then as a field practice.

Arriving at the Institute I found myself in the heartland of the Childe approach to prehistory and the Wheeler approach to excavation. The legacies of Wheeler and particularly Childe still dominated the teaching at the Institute while I was there. I was soon deeply enmeshed in the prehistoric culture sequences of Europe. There was no theoretical debate at all. I still remember the mix of shock and excitement when I first saw David Clarke’s book *Analytical Archaeology* (published in 1968) through the window of Dillon’s bookstore by UCL. When I could afford to buy it I read it avidly, and went on to read the Binford & Binford book also published in 1968 called *New Perspectives in Archaeology*. These books seemed to be coming out of another world, and I suppose my attraction to them was because they seemed to provide a way of ‘changing the world’ of archaeology. I remember trying to persuade the faculty at the Institute that we should get the books for the library and have seminars on them – but at the time there was great suspicion of both theory and anthropology, and so I didn’t have much success.

**NAR:** The year 1968 has of course become almost a mantra itself (Paris, Prague) and has come to signify more generally the political radicalization that took place at Western universities in the late 1960s and early 1970s (the Vietnam war, anti-positivism, etc.). As you mention, the very same year also dates some very significant archaeological publications (Clarke 1968, Binford & Binford 1968). In what way were these (and other related ones) conceived not only as ‘new’ archaeology but also as somehow related to the bigger picture and the political radicalization that took place?

**IH:** In my own reading of the Clarke and Binford & Binford books, I did not see any direct link to the radicalism of the time. It was clear that political engagement would have to be separate from the learning of, and



Fig. 1. Ian Hodder in the courtyard in front of the archaeological department in Cambridge in the mid 1980s. Photo: Gwil Owen.

engagement in, archaeology. I accepted as natural that archaeology as culture history or as science was a neutral and distant process, disengaged from the strong sense of injustice that I felt in the world. I attended classes and courses outside the Institute on development topics, and toyed with the idea of shifting to international development studies where the link between scholarship and questions of injustice were clear. But I felt very drawn to archaeology and deep time, and to the whole practice of excavation and making inferences about the past. So I stayed where I was, just accepting that for most people politics and scholarship had to be separate.

You mention anti-positivism. I had no exposure to that at the Institute or in my reading. Before I came to the Institute I had been interested in philosophy but had mainly read positivists like A. J. Ayer. I thought at the time that such philosophies were radical in that they demonstrated that science did not have to depend on entrenched authority structures. Open and transparent testing processes could be put in their place – so anyone could make and test a hypothesis, and science would become democratized. I was also excited by the idea that one could not fruitfully talk about that which one could not observe. So one could not make hypotheses about the other side of the moon until one could observe it. This again seemed liberating and democratizing.

So I think you can see from all this that for young people at the time, archaeology in Britain was not a theoretically or politically engaged milieu. People went into archaeology as part of the increasing public interest in excavation and discovery, itself linked to the expansion of what was then called ‘rescue archaeology’ in England. There was no theoretical discussion in London, and little elsewhere. The dominant paradigm was still culture history and environmental or ecological approaches (soil and pollen analysis, etc.). Even Childe’s Marxism was downplayed. At the theoretical level in the Institute the

only real question was whether ‘cultures’ represented ‘peoples’. And yet there were these washes of something else lapping up against the Institute’s doors – strange voices and alien ideas from Cambridge and the United States to which many younger people felt immediately drawn.

**NAR:** It is interesting to note that you did view positivism as a radical philosophy that had the possibility to make science more democratic. In many ways this is of course a valid statement, and well-known neo-positivists philosophers have claimed their approach as a necessary means to free science from the prejudice of tradition and authority and even as a means for human emancipation. In the light of this, is it possible to conceive your own ‘positivistic-Clarkean’ approach to archaeology during the 1970s (and the new archaeology more generally) as somehow politically motivated, i.e. a striving for a democratization of science? If so, was the fact that you left this position in favour of more humanistic philosophical reasoning also anchored in a realization of its social and political limitations, i.e. that it did not lead to any democratization? Or was it rather due to a realization of its epistemological limitations?

**IH:** I had come to accept that while an explicitly positivist archaeology could be democratizing, its frame of open neutrality meant distancing oneself from the political realm. It seemed that one had to accept oneself split in two, half scientist and half social being.

But this did not mean that archaeology had no social role. From the late 18th century at least, archaeologists had distanced themselves from antiquarians by claiming science. This urge towards a scientific profile was closely tied to the emergence of a public and a state archaeology. If archaeology was to be cared for by the nation state, it was increasingly important that it be accountable – and hence the search, in the form of



someone like Pitt-Rivers, for rigorous and repeatable procedures. The public presentation of the past in museums also increasingly became professionalized through the 19th century. So right from its inception, archaeology as a science had been tied to the notion that archaeology had a public role. In this sense the New Archaeologists were just stating what archaeologists had always claimed, even if they chose a more extreme, positivist perspective, and even if they saw themselves radicalized by the social movements in the 1960s.

So at UCL in the late 1960s I absorbed the idea that archaeologists had a professional and paternalistic duty to follow repeatable and accountable procedures (dig well) for the larger public good. But the increasingly esoteric aspects of archaeological science seemed to take one farther away from that goal. As I became engaged in spatial analytical techniques in archaeology at Cambridge as a PhD student in 1971–1974, I seemed to be increasingly drawn away from public interest. The split inside oneself seemed to get bigger. And of course, in the wider world, science was itself coming more into scrutiny. The moral, ethical and social implications of scientific research were coming more into the forefront, especially in areas in which I was most interested, such as development and the environment. Too many development projects based on ‘scientific research’ seemed hopelessly flawed. I spent much time too reading human geography where there was already a reaction setting in to the ‘New Geography’

I remember talking to Chris Tilley at a TAG meeting in the 1980s, and suddenly feeling that, as post-processual archaeology took hold in Britain, the split inside us seemed to start to be addressed. So it wasn’t until post-processual archaeology that I recognized that it was possible to do good science at the same time as being critically engaged in society. That recognition developed gradually as post-processual archaeology emerged. To put it the other way round, I am sure that one

of the main attractions to non-positivist approaches was the frustration of the internal split, and the desire not to put one’s social self in abeyance when doing archaeology.

Perhaps the whole history of archaeology since the late 18th century can be written in terms of two opposing trends. On the one hand, there was increasing democratization. I have talked about the 19th and early 20th century paternalism and professionalization that was associated with museums, state archaeology services and the expansion of archaeology in universities. In the mid 20th century this process was taken further in the New Archaeology, closely allied to another major expansion of archaeology in universities and in rescue and cultural resource management. In the late 20th century an expansion of public engagement accompanied the globalization of archaeology and the emergence of indigenous and post-colonial voices.

On the other hand, this democratizing process was closely tied to an increasingly specialized archaeological discourse. The methods and theories of archaeology have been increasingly elaborated. So archaeology as a science became increasingly divorced from the public realm. Indeed, the two opposing trends were intimately connected, the greater public interest allowing the increased emergence of a specialized and controlled discourse. Post-processual archaeology is in some senses simply another step in this process of dual democratization/specialization. It attempts to be multivocal and participatory, promoting collaborative research with stakeholder communities, while at the same time adopting highly abstract and esoteric concepts and language that require a very specialized training. Its claim to make a radical break from this long trajectory of an increasing opposition between democratization and specialization must lie in its ability to conduct applied research that is both critically aware and practically engaged.

**NAR:** Your replies add some shade to the negative and politically reactionary image

that was narrated of positivism and the new archaeology by post-processual archaeologists during the 1980s. Little credit was then assigned to the processualists for making social relevance part of the archaeological agenda. How do you view this today when the gunpowder smoke – at least partly – has drifted away?

**IH:** I hope it is clear that I do think that the New Archaeologists should be given credit for having influenced archaeology in a direction where the social relevance of archaeology became part of the agenda and where theoretical/philosophical discussions played a more central role. In my view, earlier archaeologists such as Collingwood and Childe were very successful in making links between philosophy, theory and archaeological practice. And I have made my point here that earlier generations of archaeologists saw themselves as having clear public roles. But New Archaeologists pushed the democratization/specialization dialectic to such an extreme that the internal tensions became intolerable to a new generation of young archaeologists in Britain in the 1970s and 1980s. The split had become too large, too extreme. Some resolution was needed. Post-processual archaeology pushed the tensions still further while at the same time professing to resolve them.

**NAR:** You mentioned earlier it was not until post-processual archaeology that you recognized that it was possible to do good science at the same time as being critically engaged in society. However, isn't it true that this compatibility to a large extent was facilitated by a quite radical change in the conception of 'good science'? A change that not only involved the rejection of most positivist scientific pillars (truth, objectivity, testing, etc.), but even entailed a widespread suspicion towards the very concept of science itself (echoing the attitude of highly influential hermeneutic and poststructuralist thinking). Thus, if good scholarship and socio-political commitment now became compatible to what

extent was this facilitated by new – and maybe quite liberal – criteria of the former?

**IH:** I do not believe that good scholarship or good method, or particular forms of science or positivism are by themselves liberal or illiberal. It is not a simple question of certain approaches being more productive of the public good or individual rights. It's always a question of how those methods, epistemologies and philosophies are put into practice. From the 1980s, and also before, it came to be accepted that archaeologists should be striving for not just good science but social justice. While I agreed with this I also think many post-processual archaeologists went too far and sacrificed the notion of good science. I do not think one can evaluate good science outside the context of social justice, but I do think we have to act as professionals in such a way as to interpret responsibly – both in relation to society and to the data. This balance was at times lost in the 1980s and unnecessarily acute reactions at times resulted. But I also think that the early post-processual archaeology work could be and has been criticized for being restricted in scope (Engelstad 1991) and insufficiently concerned with critique of its own assumptions. It needed to be 'grounded', and that link to real-world issues came about through feminist and indigenous archaeologies, and from the great expansion of the public face of archaeology.

**NAR:** Let us move back a little again: During the late 1970s there were several 'movements' that were challenging the theoretical hegemony of the new archaeology and thus prepared the ground for a post-processual archaeology. Your own ethno-archaeological work is of course among them. At the same time there was the Marxist inspired work by British scholars such as Michael Rowlands, in Scandinavia there were similar Marxist approaches as well as Jarl Nordbladh's structuralist interpretations of rock art, and in the US new and innovative work (partly Marxist, partly structuralist)

were carried out in historical archaeology by scholars such as Mark Leone and James Deetz. To what extent did you at that time know about and see yourself as kindred with these approaches and that you had, so to say, a 'common goal' (as allied in the revolution soon to come)? At what time did it become evident that what was happening could not be seen as just another addition to a 'new archaeology', but was in fact radically challenging the very foundation of it?

**IH:** If you look back at the 1982 volume *Symbolic and Structural Archaeology*, as I have been asked to do recently (Hodder 2007a), it is striking how non-confrontational it is and how desperately concerned to find predecessors and ancestors. It does this to an almost ludicrous degree! In *Reading the Past* (1986) too I was very keen to show that structuralist archaeology already existed, and to point to the continuities with the people you mention. We invited Deetz, Leone, Conkey, Rowlands, etc. to participate in conferences and seminars in the 1980s.

But at the same time, these alliances were often uncomfortable. Again, if you look at *Symbolic and Structural Archaeology* you can see a real difference between those from the States embedded in structuralism, whereas our work in Cambridge, derived from Bourdieu and Giddens, was all about the critique of structuralism and Marxism. People were held together by the excitement of doing something radical and different from processual archaeology, but there were immediately tensions.

**NAR:** When post-processual archaeology emerged in the early 1980s, however, it was very much canonized as a British phenomenon and instantly associated with Cambridge, yourself and your students. A number of books and papers emerged, with titles and a style of writing that emblematically announced a new era. New forums emerged as well, such as TAG. Rereading the publications from 1982, 1983 and 1984 one easily senses the excitement and enthusiasm that characterized the early years

of post-processual archaeology. Another observation is that early post-processual archaeology seems to have been very united in its opposition against the new archaeology, getting much of its identity precisely through this opposition and negation. Later this has changed. In your reply above you mentioned how you recognized that the split inside post-processual archaeology started to be addressed. Can you say a bit more how you experienced these early years and the subsequent fragmentation and new rivalry? Was it another 'loss of innocence'?

**IH:** TAG was actually started by processual archaeologists such as Colin Renfrew in Sheffield and Southampton. But it quickly came to be dominated by the processual–post-processual debate. Yes, there was a lot of excitement and enthusiasm as well as bitter reactions and angry exchanges. It is often said that post-processual archaeology was united in its opposition to the new archaeology. But even that is not entirely true. For example, several of our colleagues in the States espoused a positivistic approach at the time (Leone), or were quickly concerned to find a middle way (e.g. Wylie). In general, in fact, archaeologists in the States have seen less of an opposition between processual and post-processual archaeology. It is not assumed there that one will come to dominate the other, whereas in Britain and Europe such assumptions are routinely made.

But you asked what I felt about the splits that gradually emerged in British post-processual archaeology. For many I felt that they were a natural and healthy debate. They showed that post-processual archaeology could move forward in a productive way. At times I felt that there was too much 'throwing the baby out with the bathwater', but often I welcomed the 'splits' as productive growths in a debate. The latter is what I felt about the shift from texts and meanings to phenomenology and being. It is also what I felt about the shift from individuals to agency. It is very much what I felt about the rise of a feminist



archaeology and the Marxist critique of someone like McGuire. The part of the internal post-processual debate that I found most frustrating was the adherence by some (including myself at one moment) to a very radical and self-destructive version of a constructivist position. This harmed the debate, and for a very long time limited the impact of post-processual archaeology.

You interestingly talk of another ‘loss of innocence’. For me that came from impulses from outside. Much of the early post-processual debate had scholarly and intellectual roots, but it was clear from the start that an intellectually closed elitism could easily emerge from, for example, post-structuralist approaches. This was particularly pointed out by those who felt disempowered within and around the debate. So feminist, marginal and indigenous voices came to be allied with but also to confront post-processual positions. For me this was the ‘loss of innocence’, and post-processual archaeology has in my view been successful in so far as it has been able to become engaged in real-world issues and contribute to social justice while at the same time being rigorously engaged in scientific practice. My own ‘loss of innocence’ has been worked through in what I call ‘reflexive archaeology’ and in my work at Çatalhöyük.

Another way of talking of the ‘loss of innocence’ is to say that involved the realization that it was not enough simply to follow an intellectual direction, however coherent and logical it might seem. I found post-structuralism very attractive intellectually and I found the arguments of a radical constructivist position difficult to deny. But clearly it was all too innocent to follow these intellectual trends divorced from the context of their production. The internal split that I described above between science and social justice could not be resolved by post-processual archaeology until this innocent stage had been passed.

**NAR:** We seem to agree that early post-processual archaeology was held together by

the excitement of doing something radical and different from processual archaeology. Still you also notice some immediate tensions between its US and UK advocates due to different philosophical and theoretical orientation. In Britain the influences came mainly from Bourdieu and Giddens while in the US structuralism and Marxism (although important in UK as well) seem to have been more influential. This seems to suggest that early British post-processual archaeology actually was quite theoretically united. Can you recall how and when you ‘discovered’ the works of Bourdieu and Giddens and explore a bit the role they played in early post-processual archaeology?

**IH:** Yes I do think that early post-processual archaeology in Britain was fairly united around the work of Bourdieu and Giddens. The influence of these writers in Cambridge was very clear and early. Bourdieu was intensively read very soon after it came out in English in 1977. His work was brought to Cambridge archaeology in the late 1970s at least partly by archaeologists who had been trained as anthropologists in Durham (Henrietta Moore and Sheena Crawford). Giddens was down the road in Sociology and in Kings College in Cambridge and several of the archaeologists at Cambridge at that time knew him personally and he came and talked to the fledgling post-processual group. I think it was Mike Parker Pearson who knew him best and brought his work to us most clearly. Bourdieu and Giddens provided a strong theoretical basis for post-processual archaeology in Britain throughout the early 1980s and their work was taken up by others such as Julian Thomas and John Barrett.

**NAR:** Returning to the Atlantic disparity, you mentioned that the opposition between processual and post-processual archaeology in the US was less articulated than in Europe. Why do you think the climate in the discussions differed?



Fig. 2. *Ian Hodder signing a panel at an exhibit about Çatalhöyük in Istanbul in 2006. Photo: The Çatalhöyük project.*

**IH:** It is not easy to explain why the processual/post-processual archaeology debate took such a different course in the US. Partly, of course, there was the very fact that it was a British initiative! But an important factor was undoubtedly that American archaeology had split itself off from the interpretivist developments in American cultural anthropology. American archaeology was thus cut off from the types of writers and influences that might have produced an interpretive turn in American archaeology. American archaeology stayed lodged in an older evolutionary and positivist paradigm. Also extremely important was that the New Archaeology had been hard fought. It was seen as a battle that many had invested in. There was a great expansion of archaeology in US universities in the 1960s and 1970s.

Thus very many people had started their careers very heavily committed to something they felt very strongly to be right. To then have to face a wave of criticism from young interpretivists from Europe was difficult – many people were affronted, dismissive, angry – even seething. It has taken a very long time for post-processual archaeology to have much purchase in the States, and it has mainly come from a younger generation less committed to the Binfordian revolution. It remains the case that post-processual archaeology is seen as alongside other approaches in the States.

**NAR:** Your own work seems to have taken another direction from the late 1980s onwards (becoming more empirically based, your involvement in a large scale excavation project/Çatalhöyük), which may be seen as a reaction towards (and possible frustration over) the fragmentation and theoretical wrestling within post-processual archaeology. What are your own comments to such an interpretation? An interesting aspect of ‘The Domestication of Europe’ is that you wrote a ‘grand narrative’ (and proposed a kind of grand theory or master model) at a time when such approaches were doomed in anthropology (pace Clifford, Marcus, Geertz, etc.). Was this an intentional opposition against the numerous (and often narcissist) textual approaches of the ‘writing culture’ time?

**IH:** Despite what I said above, I do not think that I became more empirically based. I did run a large scale excavation right through the 1980s (from 1981 to 1987 together with Chris Evans at Haddenham in the Cambridgeshire Fens – see Evans & Hodder 2006a, b). I have always been committed to being a field archaeologist. My focus on Çatalhöyük since 1993 was a reaction to the debates in post-processual archaeology in that I came to feel that not enough impact had been made on method. It wasn’t enough or adequate to do theoretically aware social archaeology but still use the same old methods that had been

produced by empiricists like Wheeler and positivists like Binford. I wanted to try and develop reflexive methods, and at the same time show that one could write narratives about the past that were richly embedded in contextual information. I wanted to do a socially active archaeology that was at the same time good science. Whether I have managed to do that is for others to judge.

As regards issues of ‘grand narrative’, I do feel that archaeologists have a duty to contribute to public debates about the ‘origin’ of ‘settled life’ or ‘civilization’ even though I will always remain a die-hard contextualist. I tried in both *The Domestication of Europe* (1990) and *The Leopard’s Tale* (2006), unsuccessfully, to deal with this contradiction. But I remain fascinated by the issues, by the links between James Deetz’s ‘small things forgotten’ and the large scale processes to which archaeologists have a unique access. Most of my present work tries to find a (to me) satisfactory approach to this issue.

**NAR:** The phrase ‘more empirically based’ in our last question was a bit misleading. What we meant to say was rather that your research became more focused on larger cultural-historical issues (on ‘the big picture’), such as the ones you mention in your reply (‘the origins of’). Still, you didn’t really answer the question that such grand narratives and theories (for good or bad) at that time were questioned and quite out of fashion in much social and human science research. Authors like Hayden White and Michel Foucault had for long argued against a history based on reason, origin and continuity. White even claimed that we needed a history that will educate us to discontinuity, since ‘disruption and chaos are our lot’ (1978:50). Similar concerns were voiced in anthropology regarding the possibility of writing culture, stressing ambiguity, fragmentation and inconsistency. Thus, when you wrote a grand narrative like ‘The Domestication of Europe’ (stressing

continuity and a certain logic to history) this was clearly against the current. What we were curious about was if this was a conscious reaction towards the popular fragmentary perspectives or if you consider such narratives and grand concerns as an inescapable part of doing archaeology (as one may interpret your last reply)?

**IH:** While I was very influenced by Foucault and White and by the writing culture critique in anthropology, it seemed to me that archaeologists have to accept that they are not historians or literary critics or anthropologists. Archaeologists have a rather peculiar and special perspective – the long term. Looking at the long term, like it or not, there IS a big picture. I remember talking to Rob Foley in Cambridge. He of course takes a strong evolutionary position, but we always enjoyed talking across the intellectual gulf between us. And I had to admit he was right in one respect. If you take the long-term archaeological view, it is difficult to deny there have been clear general trends. The one he kept pushing me on was that domestication emerged in many parts of the world not until the Holocene. It then emerged in many places at about the same time. As much as one might try to deconstruct the word ‘domestication’, it is hard to deny that there remains something on a large scale to explain.

In the 30,000 years between 40,000 and 10,000 BCE there are no New Yorks, no wheels, no harmoniums! But now there are lots of large cities, lots of wheeled vehicles, and lots of musical instruments. There is a clear trend. There is more than disruption and chaos, like it or not. I will continue to be suspicious of many of the evolutionary arguments used to explain these large-scale processes because they so often end up being reductionist. So, as I said before, what I seek for is some approach that will allow contingent and haphazard historical processes while at the same time allowing for large-scale narrative accounts and the big picture.

**NAR:** In your response concerning ‘good science’ you say that you do not think that good science can exist outside the context of



social justice, and that you think there is a professional responsibility both in relation to the society and to the data. At the same time you also state that many post-processualists went too far when they lost the balance and sacrificed the notion of good science. Does this mean that you consider some 'constructivist' post-processual positions as bad science? If so, it should be interesting to hear briefly about who these post-processualists were/are and what constructivist positions (in more detail) you do consider as 'bad science'.

**IH:** I am not sure what you mean by constructivist – I don't think many people take an extreme view that the past is only a construct of the present. I certainly don't. I take a more dialectical view which I would still describe as critical hermeneutics. But putting that issue of definition aside, yes, absolutely, I do think there is bad, really bad post-processual archaeology, as there is really bad processual and culture historical archaeology. You are not going to be successful in getting me to name individual cases! I have outlined at some length (in my 1999 book *The Archaeological Process*) how archaeologists produce accounts that are 'good science' in that they 'fit' the data, are coherent within existing knowledge, are methodologically rigorous, and so on, but clearly I accept that evaluation of fit and coherence are also embedded within a social context. I take the view that the past and the present (like the future and the present, and the future and the past) are built in relation to each other.

**NAR:** There is another trajectory concerning your position towards constructivism that can be worth touching upon. In a number of texts from the 1980s, not at least in *Reading the Past*, you advocate a standpoint where the interpretation and the meaning that we ascribe to the past and its material culture are dependent upon the present context. However, at the same time you stress that there is a meaning inherent in the past and its material culture that exists independent of the interpreter and that can be distilled with

certain methods. This in-between standpoint, that is fully understandable in relation to your view of scientific responsibility mentioned above, has been criticized as being both an impossible epistemological standpoint and as not being radical enough. What do you say about this critique?

**IH:** I have always argued against notions of radical independence. Archaeologists can explore different lines of evidence certainly, but these lines are in my experience never fully independent. And I do not think there is a meaning 'inherent in the past' – I see a process of interaction between past and present. Is it an impossible epistemological standpoint? *The Archaeological Process* where I lay my views out has many naïve components, but since then the work at Çatalhöyük has I think shown that the approach is possible. Many authors outside the discipline have described some version of a critical hermeneutics. So no, I don't see the position as impossible. Is it radical enough? I do not think that being radical for its own sake has much to recommend it. I am more interested in having an impact, making a difference, doing something that works and is sustainable in the real world.

**NAR:** In your response you imply that your field work at Çatalhöyük is just a sort of logical continuance of the field work you did carry out during the 1980s at Haddenham since you have always been committed to be a field archaeologist. You also state that at Çatalhöyük you did see the possibility to put post-processual archaeology into practice trying to develop post-processual method. However, critics may argue that this goal has not reached since traditional methods have dominated the excavations. Do you feel that Çatalhöyük has been a post-processual methodological testing ground, and if so in what ways?

**IH:** I am not sure what people expected to occur at Çatalhöyük. I have sometimes felt that people expected something really bizarre

and flakey to happen – certainly that we would not try and do good science. I suppose this expectation is part of the demonizing that happened in the polarization between processual and post-processual archaeology. But my aim was in fact to try to do better science, as much as that might sound full of hubris! I also intended to act professionally in relation to the site and the Turks and future generations. As I said earlier, whether we have succeeded in any of this is for others to judge. My claim would be that we have been successful, at least to some degree. Much of what we do does not differ from what would take place on any site. And if you want, you can focus on that and ignore the rest. But there is also an additional layer of documentation that we are producing – where we document the documentation (for a description of the reflexive methods see Hodder 2000 and 2005). This added layer occurs in diaries and videos. These allow critique and re-evaluation of what we have done in the field. There is also a lot of dialogue and interaction – multivocality. This occurs in the ‘priority tours’ around the trenches, in Goddess seminars at the site, in the seminars (funded by Templeton) where anthropologists, theologians and philosophers are brought to the site so they too can contribute ‘at the trowel’s edge’. The main publication of the site archaeology (Hodder 2007b) is a blending of different forms of text (often written by the excavation staff in the field) and different forms of image (by different types of illustrators and artists). The words of people from the local village are present in the volume that discusses some general themes (Hodder 2006). The site guard, Sadrettin Dural, has written his own book about the site and the project (2007).

Certainly we still use single context recording and we take a lot of samples and try to be as rigorous as we can in the trenches and in the labs, using as many of the latest scientific techniques as we can. But why would you expect us not to do this? I wanted to show that a reflexive archaeology can do good archaeology in two ways. First the project takes being

socially responsible seriously. So we, like most projects today, are concerned about collaboration and engagement. Increasingly the site involves and trains Turkish archaeologists, and there are now three Turkish excavation teams working in the project. We have large and sustained educational programs and are planning further outreach and public engagement. I have tried to bring as much public involvement as close as I can to ‘the trowel’s edge’. Second, we try to collect more data in that we document the documentation so that others can rework what we have done, re-evaluate our assumptions, see what we took for granted.

It hasn’t all been smooth and many problems remain. We have published some of the tensions that emerged (Hodder 2000). But these tensions suggest that, whatever the critics say, we must be doing something different. One example is that many of the professional archaeologists who excavate at Çatalhöyük were initially taken aback at all the intrusion into their work by other specialists and voices. They were suspicious and found the process difficult, slow and cumbersome. More demands were being made in terms of dialogue, interpretation (always needing to be ready to express what your half-baked thoughts were on video!), and work (producing diaries as well as unit sheets and so on). I think the most difficult problems we continue to face are between the different regional traditions – with Turkish and British and American and German archaeologists tending to be reluctant to accept other ways of excavating. We seem very entrenched in our routines which become obviously, objectively ‘right’. But we have recently made some progress in producing hybrid documentation systems and I am hopeful that we will be able to move further in these directions.

**NAR:** Developing a new and reflexive method became a major concern for you during the 1990s. Method was of course a key issue in the new archaeology and is it possible to see your emerging concern with this (albeit different) as a modifying turn in



the processual–post-processual debate? In some works from the mid 1990s onwards you have voiced opinions which may be seen as a plea for a ‘middle ground’ (based on the ‘the best’ from the two positions). Is this a correct observation? If so, to what extent is this a response to what may be seen as more extreme tendencies within post-processual archaeology (e.g. those who ‘went too far and sacrificed the notion of good science’)?

**IH:** This move towards reflexivity came about because there seemed to be a logical inconsistency in pursuing an interpretivist approach that was based on a critique of positivism and objectivism while at the same time using the same methods that had been developed by positivist and objectivist field archaeologists. There had to be more than just theory. So the move towards a reflexive archaeology was not a search for middle ground. Nor was it a response to what I thought were examples of poor post-processual archaeology. It was an attempt to find methods that were not premised on positivist and objectivist stances. For example, archaeologists in Britain had come to use the single context recording system, and in the USA other forms of highly codified recording systems had been developed. These grew out of the demands of contract archaeology but they also responded to the notion that a standardized and universal objective method would produce reliable data. It was not acceptable for an interpretivist position just to accept data produced in this way. It was not enough just to focus on theory and interpretation while turning a blind eye to the ways in which the data were being constructed. A critical and reflexive stance seemed necessary.

**NAR:** Let us return more explicitly to archaeological theory. Its relevance has recently been questioned and there are even some suggestions of its death. The infra-structural development within the profession, where a growing majority of archaeologists work outside academia, have

led some colleagues to argue that archaeological theory is largely irrelevant to the topics and challenges faced by most archaeologists. Others have voiced the claim that archaeological theory anyway is little more than a dogmatic burden that we should liberate ourselves from, and hailed pragmatic and eclectic approaches as a far better alternative. How do you react to these positions? In what way do you think the role of archaeological theory is changing?

**IH:** I do not see clear evidence for a death of archaeological theory, or even a slowing down. Versions of TAG seem to be sprouting all over the world; there is even a TAG starting in the USA (New York University 2008) and the students at Stanford are organizing a USA version of TRAC (Theoretical Roman Archaeology Conference) called CRAC (Critical Roman Archaeology Conference). There seems an insatiable demand for more theory books and more new editions of old theory books. There are many very exciting new theoretical agendas – the body, materiality, memory, scale, temporality, new takes on evolution, an impact from human behavioural ecology, new approaches to heritage, new collaborative forms of research and so on.

We will always need and use theory – theories about method (ways of doing, analysing and interpreting), and theories about how societies work and change. We may be more or less explicit about the use of theory, and there may be more or less controversy surrounding it. But theory will always be there. And of course, from my point of view, being explicit is important so that we can be reflexive and socially critical.

I do not agree that archaeologists outside academia always see theory as irrelevant. There are exciting new approaches in Swedish contract and field archaeology, and I know from the work of Chris Evans in the contract Cambridge Archaeological Unit, that it is possible for a very strong and creative theoretical thread to be a central

part of contract archaeology. The problem is more that in some contract contexts, the system has been allowed to become so very heavily competitive and money-driven, that theory becomes a luxury – or is seen to be.

But I do agree that the heavy theoretical debate of the 1980s and 1990s did leave a lot of people cold, and perhaps rightly. Theory in itself was shown to be rather abstract and distant – most people seemed to be able to carry on without it. And I do think that theory divorced from society and substance quickly becomes self-indulgent. Because of the great diversity of theory in archaeology many people have felt that you can just pick and choose. Theory in all these ways seems less central. And that may be right. As I said earlier, good archaeology depends on many factors, social, methodological, as well as theoretical.

One problem is that it is difficult to argue now that archaeology has a coherent theory. As I say, it all seems a matter of choice and personal preference. Perhaps a solution is something more unified and consensual.

**NAR:** In a ‘turn of the century’ paper dedicated to archaeological theory (Hodder 2002), you provided us with a very positive and optimistic view. You talked about an undoubted diversity and vigour in theoretical debate and that archaeologists ‘are more than ever aware of the theoretical underpinnings of all data recovery, description and sequencing, and ... they are more than ever aware of the diversity of theoretical approaches being explored’ (Hodder 2002:77). However, to what extent do you think archaeologists have been successful in developing theoretical frameworks of their own, theories that can be claimed as ‘archaeological’ and not just applied or borrowed approaches? Looking at archaeology’s relation to other disciplines it seems as if we are very concerned with our neighbours and their intellectual capital, while few sociologists, philosophers and social anthropologists pays much attention to what

archaeologists do – and even less to our theoretical and analytical reasoning. What do you think causes this asymmetry? Do you think there exists any archaeological theory today that is of relevance to other disciplines and social theory more generally?

**IH:** In this and the last question you are showing yourselves to be rather Eeyore-like! I take a much more up-beat view than you do. Maybe that is because I now work in an anthropological context in which I see lots of people drawn to archaeology and to its theories. Those archaeologists working on materiality, the body, constructions of history, agency, the long-term and so on are definitely being read outside the discipline. New approaches to and theories about heritage are producing lively interdisciplinary debate. Cognitive and evolutionary anthropologists and psychologists are drawn to archaeology as are linguists and artists. Just as one small example, over the past two years we have brought the same group of eminent anthropologists, theologians and philosophers to spend five days at Çatalhöyük (funded by the Templeton Foundation), learning about what we do, helping us interpret ‘at the trowel’s edge’.



Fig. 3. Ian Hodder at Çatalhöyük in 2006. Photo: The Çatalhöyük project.

They have been fascinated and engaged, in the theories as much as the data. They have gone away and written their own papers using the site and our interpretations. These seminars will continue.

I can think of many field projects in which archaeologists and members of other disciplines work closely together, at all levels, including theoretically. But I do not think that archaeologists have on the whole been very good at writing for those in other disciplines, engaging them, bringing them in. To other disciplines archaeology often seems highly specialized and inward-looking. Archaeologists in the USA have increasingly separated themselves off from the main theoretical currents in cultural anthropology, have gone less and less to the anthropology meetings and more and more to their own separate archaeology meetings.

There are many archaeological perspectives and theories, and little that is agreed and consensual at the theoretical level. When editing or writing a book about archaeological theory today one is forced to do little more than celebrate the diversity. This cannot help those looking in from the outside. It may be the case over the next decade that, as the controversies die down, and as people pick and choose, the discipline will be able to rediscover some core of theory to which many would subscribe. We do not seem to be quite there yet, but I sense that it may not be that far off.

**NAR:** If we should point to one area where archaeologists have had some impact it is (of course) within material culture studies. The highly influential environment at UCL comes first to mind. However, despite being to a large extent recruited from archaeology this group is situated in an anthropological department (which in Britain and Europe has much more significance than in the US) – and most of them prefer to address themselves as anthropologists. Do you think this is accidental? Would they have had the same impact if they worked in an archaeological

department or under the banner of archaeology? In other words, is there a ‘stigma’ to archaeology that makes it less fashionable as a social science – and that it is easier to be listened to if you talk about material culture, time, etc., from a more generally recognized and ‘secure’ social science position (as a sociologist, anthropologist or philosopher)?

**IH:** As I answered above, I do not see a stigma – in the USA I would say there is often an envy towards archaeology (better funded, more sure of what it is doing, on a rising tide). The UCL group is a great example of a productive two-way dialogue in which archaeologists have played a very key role. I am not sure whether it matters how individuals in the group self-identify. What is important is that there has been a productive dialogue between archaeological and social anthropological approaches. Archaeology may have been more able to make an impact there because it reached out – through mechanisms such as the *Journal of Material Culture*. It is precisely this reaching out that I said above was a key component if archaeology is to have a wider theoretical relevance. This UCL example shows that if people talk to, write for, engage each other, two-way flows are possible. By being in an anthropology department archaeologists may have been more able to achieve that.

However, my own view of the UCL group is that it has not been archaeological enough. While this is not the place to get into an extended discussion of their work, most members of the group most of the time do not look at objects in an archaeological way. They rarely get to objects themselves. They are more interested in the social and other relations surrounding and embedded in objects. I think this is a weakness because, as a result, the object’s dependence on people is left out of the analysis. The physical and chemical materiality, and the hard physical processes which archaeologists are so good



at discussing, are rarely brought into the discussion. There is the asymmetry.

**NAR:** We are close to three decades since what came to be known as post-processual archaeology emerged and whatever one may think of the outcome it is hard to deny that it produced very different archaeologies at least in some parts of the world. It triggered a number of new theoretical positions and has fuelled much debate. Looking back at the development that has taken place since the early 1980s, what in your opinion are the most significant (positive) achievements? What wishes did not come through? And looking into the archaeological crystal ball, what do you think will be the most crucial issues for the discipline during the coming decades?

**IH:** For me by far the most important shift has been the widespread recognition of the close relationship between archaeology and society, though as I described above I do not think this was radically new. But I think it was taken in new directions, both as a result of internal and external factors. There has been a shift towards ethics, rights, heritage. At the interpretive level, the notion that material culture is active has been part of a growing debate about materiality, meaning, practice. There is wider discussion of the body, personhood, temporalities, memories, memorialization and so on. All this seems to me to be positive.

As I suggested earlier, I see the main challenge as being how to convert the critical stance into one that has practical effects. Too much archaeology, driven by market concerns, has become routinized and pared down to the point of being less than a profession and less than a discipline. Too much heritage is driven solely by numbers and income. What can a theoretically aware archaeology do in the face of these pressures? For me that is the challenge of the coming decade. The rate of the destruction of the past increases dramatically. How can we respond to this? Theoretical engagement (about ethics, sustainable development,

global heritage) and critically aware responses seem in ever greater need. It is not enough to become repetitive functionaries and practitioners. It is not enough to engage in internal specializations and theoretical niceties. The challenge is how to infuse our greater theoretical sophistication into a discipline with greater social engagement.

**NAR:** During the last decades the number of publications, books, journals, etc., devoted to archaeology and archaeological theory has increased tremendously. At the same time information technology has exploded, making information and discussions more open and fleeting than before. New wikis, blogs and discussion lists are constantly being created. This development can be seen as having both positive and negative effects. On one hand the field of archaeological theory becomes harder to control and thus the discussions can develop more openly and democratically without (much) editorial or peer review censorship. On the other hand the field becomes extremely fragmented and its discussions harder to grasp and scrutinize. How do you view these developments and their impacts on archaeology and archaeological theory? A somewhat peculiar outcome is that despite the declared future death of journals in their printed form new printed journals are constantly being launched. How do you view the future of a journal such as *NAR* within the framework of the developments outlined above?

**IH:** As noted earlier, I do think there will be attempts to create some convergent consensus in the field regarding theory, but I do not expect this to decrease the debates and the dispersal in the many new media that have and will emerge. I disagree that the new technologies bring openness and democracy – they do so only amongst a Western elite. Many parts of the world and many social groups in the West do not feel that wikis and blogs do much to help their marginality and exclusion. I have not seen that wikis and blogs do much to help the rights of

disadvantaged groups such as native Americans or diasporic groups excluded from national pasts. The successful community outreach and engagement programs that I have seen have not been helped much by these new media – which in my view mainly favour interaction amongst the well-situated. Within that narrow world I do welcome the divergencies and fragmentations brought by the new technologies. There have always been synthetic and antithetical tendencies amongst intellectuals; now they just happen instantaneously.

In the elite academic world, it does seem to be the case that the new media get added on to older forms rather than replacing them. Indeed, as you say, monographs and journals seem to thrive. We seem a long way from saying that tenure and promotion cases can do without peer-reviewed journals. Because the lives and incomes of academics depend on them, books, monographs, journals will continue into the immediate future at least. The economic and social relations of academic production mean that, even more than before, ‘quality control’ has to be protected in the face of a proliferation of media outlets and diverse voices. Because NAR has managed to project itself into an international system of circulating academic prestige, I do not see it as having an uncertain future .... yet!

**NAR:** Let us round off this conversation with some questions relating more directly to your own career. You have become increasingly associated with Çatalhöyük. As is often the case in archaeology, the identity of a site fuses with that of an archaeologist. What are your reflections on this kind of ‘distributed’ identity?

**IH:** Archaeologists have long had the notion that they ‘own’ a site which they dig. The excavator often gets identified with the site and his or her career is often based on that ‘ownership’, on the rights to publication and images, on an exclusive relationship. The identification between excavation director and site is also deeply embedded in the way

that the discipline works. The Turks insist that there should be clear and limited directorial responsibility. Permits are given, as in many countries, to an individual or small group of individuals.

The identification between site and director, the insidious idea of ‘ownership’, undermines the attempts at wider partnerships. Any special relationship between me and the site can inhibit moving towards the idea of shared pasts and stakeholder involvement. I have a professional duty towards the site, but this relationship should not be construed as exclusive. I dislike the ways in which the discipline gives rights to the past to individuals and individual institutions. I am hopeful that we can move towards a collaborative, participatory, shared relationship with the past. There are lots of wonderful projects that lead the way in this type of work. But the co-identification seems thoroughly unhelpful.

**NAR:** Finally, what are the main differences between the Ian Hodder of 2007 and the one writing and acting back in the early 1980s? Does your current position in an affluent US elite university and as the director of a large, well-sponsored project undermine some of the critical stance that you sought to take as a younger academic?

**IH:** Thank you for all your questions. I have found them stimulating and thought-provoking.

You ask whether responsibilities and duties limit one’s ability to be critical. Post-processual archaeology, at least at Cambridge, emerged in the 1980s in a very elite university. David Clarke, Chris Tilley, Mike Shanks and I all came from the oldest college in Cambridge, and one of its smallest and most elite. It is in the nature of elite academies in Britain that they tolerate diversity of intellectual direction – they have sufficient privilege to embrace and make a virtue of a certain radicalism. As my own responsibilities have increased, however, it is true that I have found myself more keen to search for



solutions, integrations, compatibilities than I was in the early 1980s. In an earlier response I said that I thought the discipline might be ready to define some core theoretical position. I am keen to move in that direction myself – more now than I was then.

I still feel that a strong critical role is possible. In this interview I have mentioned some of the problems that need critical debate. There are all the problems of contract archaeology, of commercialized heritage, of universalizing (Western) valuations of heritage, of the loss of livelihood through looting and destruction. There are all the social dangers of those that would reduce human behaviour to economic costs and benefits. There are all the social exclusions brought by the supposedly inclusive new media.

Perhaps I am blind to the critique of what we are doing at Çatalhöyük. But one of the main reasons for trying to run an open project, with data and diaries available on the web, is the hope that the critique will still be made, at least amongst an intellectual community. We have been holding debates and open fora at the site with the local community, as well as with Goddess groups. I hope to be open to criticism in these discussions and I intend that the criticism will be responded to.

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