discussion article

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Genes versus agents. A discussion of the widening theoretical gap in archaeology

Kristian Kristiansen

Abstract

It is argued that archaeological theory during the last decade has lost its theoretical nerve by accepting the development of epistemologically incompatible approaches. Two recent books exemplify the widening theoretical gap in archaeology between different schools of thought, with different interpretative interests. They are Steve Shennan's *Genes, memes and human history. Darwinian archaeology and cultural evolution* (2002), and John Robb and Marcia-Anne Dobres's edited volume *Agency in archaeology* (2000). To overcome this theoretical divide it is necessary to revive the theoretical debate, based upon epistemological principles relevant to archaeology as a historical discipline.

Keywords

agency; culture history; comparative approach; Darwinian archaeology; theoretical debate

During the last decade we have seen a development in archaeological theory from a concern with plurality to an increasing concern with the growing disparity in theoretical thinking (Trigger 1998; 2003a; Schiffer 2000b). Following the often rather fierce debates of the 1980s that positioned postprocessual archaeology, the 1990s saw a need to be reconciled, and a theoretical openness or willingness developed to accept the relevance of different schools of thought. I wish to argue that this development has now reached a critical limit. Theoretical dialogue has been replaced by theoretical closure at the risk of separating archaeological research into mutually incompatible traditions that ignore publications aligned with other theoretical programmes. This has been termed 'redlining' by Michael Schiffer (2000b). If we wish to change this situation we need to reopen the theoretical debate, by relating theoretical positions to epistemological questions relevant to archaeology as a historical discipline.

To get there, however, demands a change of attitude and recognition of some rather simple observations. First, no single theoretical programme covers the whole interpretative range of variation in the archaeological record. Second, theoretical programmes consequently need to become more realistic in their claims to interpretative coverage. Third, theoretical programmes can then more profitably look into the interpretative and explanatory complementarity between themselves. This would allow a reopening of

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theoretical debate based upon a discussion of common themes, such as cognition, where the potential for different theoretical programmes to contribute to it and their limitations could be highlighted (examples in Schiffer 2000b, 5–8). As observed by Steven Mithen, interdisciplinary studies into cognitive evolution have forced both cognitive scientists and archaeologists to challenge some of their basic concepts and interpretations (Mithen 2001, 99–101). In the end there may still be epistemological principles that cannot and should not be reconciled, but they will be linked to different claims of knowledge and their ethical and political consequences in a more clear-cut manner. I do not suggest that theoretical concepts are free-floating commodities to be chosen at will, but I will insist that they bring with them epistemological principles or world views and political histories of use that need to be recognized in the process.

In Part I of this paper I exemplify the widening theoretical gap by discussing two recent, influential books: Steve Shennan's *Genes, memes and human history. Darwinian archaeology and cultural evolution* (2002), and John Robb and Marcia-Anne Dobres's edited volume *Agency in archaeology* (2000). Both agency theory and evolutionary archaeology refer back to biological conditions of modern humans, which they theorize in distinctively different ways. As such they illuminate the problem of incompatibility versus complementarity, and they also exemplify the current trend towards theoretical totalitarianism or salvation, by redlining other approaches than their own.

In Part II of the paper I go beyond agency theory and Darwinian archaeology and identify a number of problems and flaws in current theoretical programmes. They are discussed in three critiques, and I contend that they are responsible for much of the current redlining and lack of dialogue.

In Part III I propose a strategy for initiating a productive theoretical debate. It is based upon the recognition that different theoretical programmes tend to relate to different interpretative realms. This may be considered 'mission impossible' by some, but it is in the nature of scientific and theoretical enquiry that the mission is what happens on the way rather than what is found upon arrival.

Part I

The selfish gene? I take Steve Shennan's book (2002) to exemplify the Darwinian evolutionary approach in archaeology. It is not to be confused with social or cultural evolution, to which it stands in stark opposition. Both these programmes originate in the thinking of the later 19th century, and are based upon a biological and a cultural theoretical programme respectively. Cultural evolution evolved from the works of Spencer, Tylor, Morgan and Marx; evolutionary archaeology is based on the work of Darwin and his followers. While cultural evolution has remained a continuing influence, and saw a revival in archaeology from the 1970s onwards (Feinman and Manzanilla 2000), Darwinian theory evolved only during the last 20 years or so, and not without critique (Leonard 2001, 65ff for references).

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In Shennan's book the widening gap in archaeological theory is apparent from page one. He simply dismisses postprocessual archaeology as a failed attempt by archaeologists to become social anthropologists of the past. It is indeed a striking feature of his book that he has no wish to situate himself or argue his position in relation to contemporary theoretical debates. They are 'redlined'. The same kind of rather high-handed dismissal of research in the social and cultural disciplines, and a strong belief in one's own paradigm (the salvation syndrome), are found among other practitioners of the evolutionary paradigm. Discussing the emergence of complex societies Richerson and Boyd state, 'these are exceedingly complex questions that have defied definitive solution despite much hard work – and much real progress – by social scientists, historians and political philosophers. The development of Darwinian tools encourages a fresh cut at them' (Richerson and Boyd 2001, 201).

In this spirit Steve Shennan's book is an attempt to market Darwinian archaeology as mainstream theory that is able to account for human behaviour in general, rather than a specialized theory for the development of *Homo sapiens*. And it provides an alternative to archaeologists that are not feeling at home in the present postprocessual world. It follows in the wake of a period of 10 to 15 years where sociobiological theory has expanded its domain in archaeology beyond palaeolithic studies.[1] Cultural and biological evolution separated from each other in the later 19th century and developed completely independently for a hundred years (Persson 1999). The two began to merge in the 1980s, most notably reflected from 1985 in Boyd and Richerson's book *Culture and the evolutionary process* (1985). An important step was marked in 1995 with the edited volume *Darwinian archaeologies* (Donald and Maschner 1996) and more recently by Steve Mithen's books (Mithen 2001). Another, yet separate trend, is the use of DNA in archaeological explanation, especially within population and language studies (Renfrew and Boyle 2000). As these approaches are linked to the expansion of new domains of scientific knowledge that already exert a strong impact on archaeological research, they should be critically discussed and theorized from a broad spectrum of approaches.

The question remains, though, whether Darwinian theories of selection and inheritance have something to offer to archaeology beyond a niche for specialized studies in behavioural biology. Shennan needs some convincing case studies to break through the wall of scepticism that prevails in current archaeological theorizing. And he presents a number of cases with some good archaeological evidence that he discusses in interesting ways. I consider the most persuasive parts of the book to be in the chapters dealing with economy, population dynamics and conflict (chapters 5, 6 and 9). The least persuasive are the chapters on culture, tradition and social organization (chapters 3–4 and 7–8).

Two chapters (3 and 4) and 60 pages are used to analyse and discuss cultural traditions and transmissions by applying genetic concepts of learning, selection and mutation. Cultural traditions and transmissions are indeed in need of theoretical and interpretative labour, and have recently been the subject of renewed discussion in social anthropology (Odner 2000;

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Rowlands 2003). This holds in particular for the relation between so-called 'great traditions', such as the Indo-European or Bantu tradition, and 'small traditions', in the form of local adaptations and transformations. Shennan's contribution to the discussion is therefore welcome. It is indeed informed and interesting, especially the part on cultural and social transmission, and often employs good ethnographic examples. But when it comes to explanation he simply strips cultural traditions and transmissions of any social and symbolic context. They rather become behavioural phenomena in the tradition of David Clarke, and are squeezed into a predefined framework of formal variability (O'Brien and Lyman 2003; Shennan 2004a). This is then used to demonstrate that cultural and genetic drift are operating in similar ways. It is based on the assumption that artefacts are components of the human phenotype, and that their replicative success consequently operates according to principles of natural selection – a highly dubious interpretation (Leonard 2001, 71ff). While this may appeal to some due to its simplicity, I found it less rewarding. Such a framework is undeniably part of what may be described as cultural regularities of a non-evolutionary nature. However, most archaeologists would contend that their interpretation demands a set of supplementary theoretical tools, as exemplified by Ian Hodder's book *Symbols in action* (1982). It also demands a new understanding of concepts such as tradition, apprenticeship, innovation and diffusion, all of which lie outside the realm of evolutionary theory at present (Lemonnier 1993, van der Leeuw and Torrence 1989; Sinclair 2000, Figure 13.9; Kristiansen and Larsson 2005, chapters 1 and 2). Therefore this domain of evolutionary archaeology has become the subject of theoretical controversies that are not discussed by Shennan, and which set it apart from other forms, such as evolutionary ecology (Kelly 2000, 64–65).[2]

By contrast, I found the chapters on ecology, population dynamics and economy stimulating. They rather fall within the domain of evolutionary ecology, which has developed along different lines and provided less controversy, as there is a long tradition of theoretical integration between ecological and cultural evolution in archaeology and social anthropology. Detailed interdisciplinary archaeological evidence is now beginning to provide convincing long-term sequences of cyclical trends of expansion and regression in settlement density and economic exploitation, which may be explained in part as a consequence of the operation of evolutionary regularities. Especially impressive is the evidence from Neolithic Switzerland, but similar detailed long-term trends can now be provided for other periods and regions, as demonstrated in chapter 9 on the Linear Band Ceramic sequence. Archaeology, including evolutionary archaeology, can here make important contributions to economic history and general theories on population dynamics. The examples further demonstrate the value of employing quantitative methods, at present disastrously out of fashion.

Throughout the book Shennan does an excellent job of explaining Darwinian concepts and presenting good and often complex case studies in an understandable way. Problems mostly emerge when he tries to synthesize the two. It is thus characteristic that he draws upon the whole theoretical repertoire of interpretation from social anthropology and archaeology when

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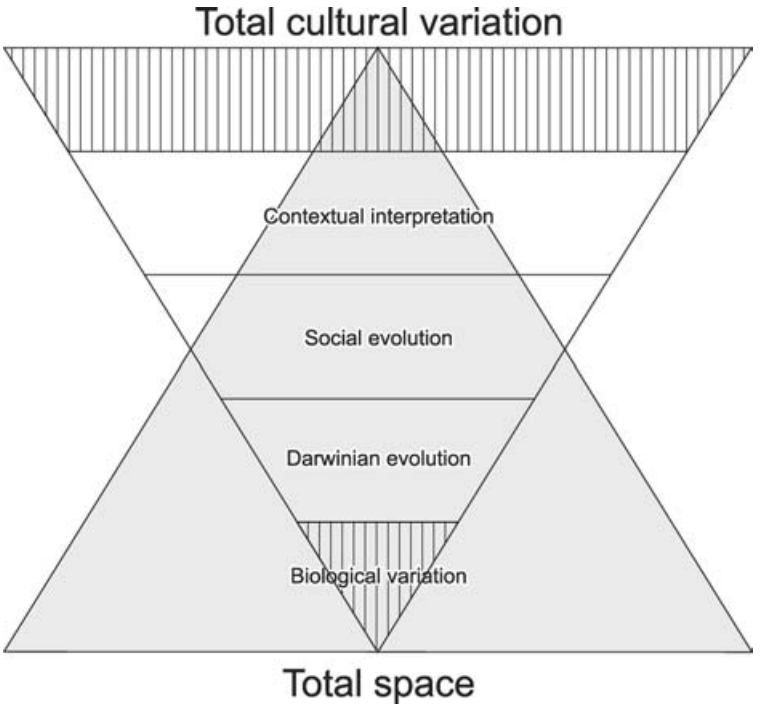


Figure 1 Model of the inverse relationship between theoretical and empirical coverage when one moves from biological evolution to localized contextual studies.

discussing individual cases. He often refers to anthropological case studies to exemplify possible interpretations of the archaeological evidence. He is thus employing a comparative approach. This is in line with a belief in cultural regularities. So far many colleagues, and myself, can follow Steve Shennan. Paradoxically, however, he then translates his interpretations into biological concepts of evolution often without any apparent justification, as most of his cases are ambiguous. He sometimes suggests that other explanations are at play for the variation in the archaeological evidence that does not conform to his evolutionary biological concepts. But this does not make him reflect upon what these other explanations might be. It would indeed have supported his cases for Darwinian evolution if he had taken on board complementary explanations and discussed their interaction. There is of course always a surplus of meaning, as once noted by Shanks and Tilley (1987a, 66). But how much surplus is acceptable? Can there be too much surplus for an explanation to be acceptable? Or are we simply speaking of different levels of interpretation that employ different properties of the evidence?

In Figure 1 I have exemplified and simplified this problematic situation, which of course goes beyond a critique of evolutionary archaeology. It proposes that there exists an inverse relationship between geographical and temporal coverage and the social and cultural variation employed. I suggest that at both the top and the bottom of the diagram interpretations become

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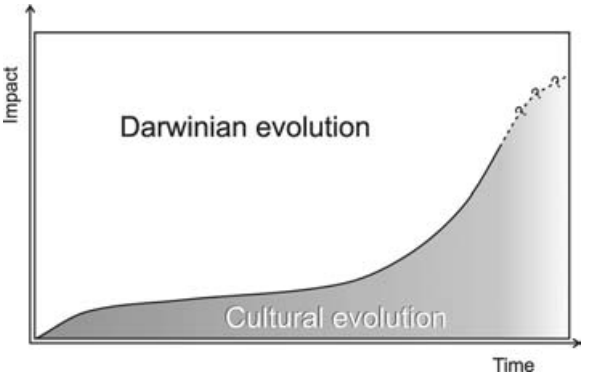


Figure 2 Proposed relationship between biological and cultural forces in human evolution, leaving open for debate their relationship in more recent prehistory.

trivial, by being either too particular or too general. It is in the middle field that the real interpretative and explanatory dynamic is located. This invites a discussion of the theoretical and interpretative relation between different levels and positions. Above all, it suggests that Darwinian concepts of selection, inheritance, costs and benefits, and so on can only be understood with reference to specific cultural and social contexts. If one accepts, as Shennan does, that decisions about costs and benefits are determined by context, one question immediately poses itself: what is left of a Darwinian approach? If selective pressure based on costs and benefits is indeed operating, I would have wished to see a discussion of where these basic evolutionary concepts have an impact, and where not. That might lead to a focus on periods and regions of world historical change where they would be expected to operate, such as the Neolithization process, exemplified by David Rindos's excellent study (1984) and more recent work on the origin of institutions (Foley 2001).

Shennan, however, never takes up the important question of how and where to draw the line between genetic dispositions and culturally and socially determined behaviour (Figure 2). Instead he takes on board concepts from biological evolution to account for complex social and cultural strategies in the Neolithic and the Bronze Age without discussing limits to their explanatory coverage. At worst he is able to reduce complex social processes to simplistic biological notions of male and female behaviour, as in the case of marriage patterns in the Bronze Age (chapter 7). It is assumed that males are primarily interested in mating and women in caring. This is light years away from Mike Rowlands's sophisticated study of kinship strategies in the Bronze Age (1980) or my own attempt to link such strategies to social and economic regularities (Kristiansen 1998b, Figure 217).

In Steve Shennan's new Darwinian world we are left with a reduced past. Social and cultural concepts able to account for both the complexity and some of the regularities in the material record are replaced with biological concepts that can at best characterize and eventually explain some portion of that variation. Complexity is reduced to biological behaviour with a

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social and cultural outcome. This is not in itself uninteresting, but demands supplementary discussions of possible parallel and interacting factors at work. However, in the individual case studies on population dynamics and economy, Shennan successfully demonstrates the relevance of quantitative methods in revealing long-term changes and cycles, and this takes archaeological explanation and interpretation beyond postprocessual and contextual studies. This is an arena for renewed theorizing where archaeology and evolutionary ecology can make a significant contribution to population theory and economic history at a general level. The question remains, however, whether Darwinian archaeology provides the only or the most relevant theoretical framework for doing just that.

Darwinian archaeology undoubtedly has important contributions to offer, but its proper field of enquiry and relevance is still in need of clarification. It should further be recognized that it embodies rather different strands of theoretical thinking and methodological applications, as I have emphasized. There is indeed a theoretical and intellectual gulf between the rather simplistic and much-criticized school of Dunnel and O'Brien (Bamforth 2002; 2003; Kehoe 2000; O'Brien, Lyman and Leonard 2003), and the more sophisticated and convincing applications to account for major long-term social and economic regularities (Runciman 2001). Shennan's book is a bold and well-written attempt, but fails to convince because he wishes to employ biological evolution universally rather than define those perhaps more restricted areas where it has something to offer. A more optimistic conclusion would be to say that it exemplifies both areas of productive application and areas where there are still problems to be solved.

The selfish agent? Agency and agents (whatever their constitution; even selfishness can appear meaningful in the right context) have been with archaeology for nearly 20 years now. They originated in the works of Giddens and Bourdieu (Dobres and Robb 2000) and were taken on board by the front runners of the postprocessual paradigm to distance it from previous functionalist and evolutionary, so-called deterministic, paradigms. As noted by Dobres and Robb, today 'agency' has become a taken-for-granted buzzword signalling theoretical correctness. But what kind of politics is hidden behind the rhetoric? Is it merely a guise for an excessive liberal, narcissistic self-preoccupation or is it freeing prehistoric people trapped in functionalist determinism? Is it at all possible to define these concepts archaeologically with a minimum of interpretative rigour, or have we in sheer idealism crossed the border to a speculative archaeology? Lately agency and agents have been accompanied by a search for the formation of the self, the body and its encultured experiences. This further stresses the need for a critical discussion of what constitutes agency theoretically and empirically.

Marcia-Anne Dobres and John Robb are less dogmatic and more sceptical regarding the role of agency in archaeological theory than one might expect. And they invite others to present the case studies. Most of these are interesting, but could just as well have appeared in any other edited book except those which deal with agency from a more theoretical perspective. Ian Hodder takes us on an interpretative journey from Ice Man to Çatalhöyük. He suggests

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these cases present a window to the past, which demands a different rhetoric linked to small-scale contexts. He never touches upon some of the basic questions about the limits to agency under different social and historical conditions, or the potential for agency in others. He is rather interested in the potential for reviving individual life stories, which is something other than agency. Hodder rightly points out, however, that 'insufficient attention has been given to the role of small-scale events and processes within the long term' (Hodder 2000, 22). Both Wobst and Barrett deal with the tension between structured routines and traditions and agency (Wobst 2000; Barrett 2000), a central historical problematic, while Joan Gero adds a much-needed feminist perspective to agency studies (Gero 2000). There is a certain conceptual sloganism prevailing in several of the articles. Concepts as much as content signal your theoretical affiliation. Within seven pages Barrett uses the words 'agency' 60 times, 'action' 38 times, 'agent' 28 times and 'event' 18 times (but see Barrett 2001 for a more rewarding contribution). After this experience in condensed hyper-theory, I started thinking about the potentially productive outcome of archaeologists agreeing not to use their most beloved theoretical concepts for one whole year.

Interestingly enough, evolutionary archaeology has placed agency on the theoretical agenda, as individuals are thought to be responsible for the creation of new selective traits leading towards better fitness and cultural or social survival (Clark 2000). It shows that agency is an open concept that can be employed in different theoretical contexts for different interpretative goals. Does it make it too slippery to be useful? Or is this an example of productive bridge-building?

Dobres and Robb present a thorough description of agency in Western philosophy and its application in archaeology in an introduction that is subtitled 'paradigm or platitude?'. These doubts, of whether agency is a theory of its own or rather an ideological interest of our own time that needs to be historically and socially contextualized to gain meaning, are further underlined in the concluding chapters. Here Elizabeth Brumfiel and Henrietta Moore wisely and critically advise us. Brumfiel notes, 'Agent-centred analyses seem as vulnerable as other modes of archaeological interpretation to the projection of our own values onto past societies. And agency theorists have simply not responded to this critique when it has been levelled at their work' (Brumfiel 2000, 254). Moore concludes that terms such as agency are domain-terms: 'We use them as a kind of disciplinary shorthand, to indicate an area of human life, capacity, and relations we are referring to' (Moore 2000, 262). Such concept-metaphors have the role to create spaces, to open things up for analysis, and to maintain ambiguity, according to Moore.

What can we conclude from this? Moore's concept-metaphors – or door-openers – suggest that agency needs to be historically contextualized to establish meaning and direction in archaeological interpretation. Thus it is subordinate to theoretical and interpretative concepts specific to such historical contexts. This is exemplified in Alfred Gell's book *Art and agency* (1998). He defines agency as being social and relational. That includes also material objects and art, which are ascribed agency once they are immersed into social relationships. It is demonstrated by case studies on religious

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idols and style. Animation, divinity and power can thus be ascribed to specific objects that have undergone special rituals and/or are decorated in a certain way. The observed practice of interchangeability of agency and power between humans, animals and objects in ethnohistorical contexts can be meaningfully understood by applying a particular, contextualized definition of agency. In this way Gell turns an abstract Western concept of agency into a useful theoretical tool in a specific interpretative context.

There is consequently no need for a general theory of agency, as it emerges from specific historical contexts when properly theorized. Matthew Johnson puts it this way: 'I suggest . . . an historicity of agency' (Johnson 2000, 214). In much the same vein Julian Thomas has recently criticized an autonomous perception of the individual lurking behind much discussion about agents and agency in archaeology (Thomas 2000, 148–50; 2002).

It might be argued that the preoccupation with agency, knowledgeable agents and embodied experience rather bears witness to a narcissistic self-centredness of the present. Apparently we cannot imagine or accept other conditions in the past than those prevailing in our own lives in the present. On the other hand, a truly contextualized study of agency in the past would certainly be enriching and might hold one or two surprises as to the nature of agency in premodern societies. We may not find the comfort or the confirmation we expect, but that would probably demand not only richer archaeological materials but also textual and iconographic evidence (for an illuminating example, see Meskell and Joyce 2003).

I thus come to the same conclusion for agency as for evolutionary archaeology: that it would profit from more realistic assumptions as to its interpretative coverage. Interest should rather focus on providing a realistic assessment of archaeological limitations and potentials in relation to specific interpretative problems. The truly historical contribution from past histories lies in our ability to understand how past cosmologies shaped perceptions and behaviours in ways we can only grasp by understanding the underlying differences from the present. To achieve that, I prefer to abandon much of the present conceptual repertoire linked to agency. My taste is rather for being conceptually and historically explicit, talking about experience, interest, motivation, selfishness, sacrifice, power, manipulation, exploitation, opposition and resistance, to name a few key concepts. And such an understanding demands comparative culture-historical and ethnographic studies. They may not be able to cover all prehistoric conditions, but they certainly represent a first step.

Filling or widening the theoretical gap in archaeology? The two books exemplify the borrowing of theoretical concepts and perspectives from other disciplines as well as the subsequent formation of apparently opposed interpretative strategies and interests. As expressed by Dobres and Robb, 'We also need to acknowledge the hard-learned lesson of history: that archaeology has been colonized by too many theoretical empires originating in disciplines with standpoints and agendas very different from our own' (Dobres and Robb 2000, 14).

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Is it a symptom of the poverty of archaeological theory that an attempt has not even been made to discuss other positions than one's own? Or have we been enriched by a selection of new approaches, creating a plurality of perspectives? Does a 'postcolonial, multivocal past' employing the current political correctness infuse the cultural disciplines with moral capital? Perhaps part of this diversity is inherent in modern archaeological practice, and should rather be recognized as an existential part of the craft of archaeology (Shanks and McGuire 1996). Archaeology has diversified in the process of being integrated in the expanding cultural production of modern society, from academic research through heritage and museums to ethnic revivals and the search for origins. But, beyond that, competing theoretical agendas still remain, now hidden away in interpretative themes. Hodder's recent edited volume *Archaeological theory today* (2001) displays the disparate directions of archaeological theory very well (but also the absence of new theoretical ideas). It apparently does not disturb the editor to state that despite 'the enormous gaps and disagreements about fundamentals, and despite the evidence that archaeological theorists are trapped in separate non-communicating discourses, there is at least some indication of moves forward' (Hodder 2001, 11). That is optimism based on rather modest expectations. This statement would, by anyone other than a hopeful editor, be considered an alarming signal. Where, then, are the moves forward to be located?

John Clark recently concluded a comparative interpretative analysis of the use of agency in Darwinian and postprocessual archaeology by stating that 'both perspectives have strengths and weaknesses and could be substantially improved by recourse to more realistic assumptions about the nature of the operation of society and individual agents' (Clark 2000, 110). While this has not been accomplished in theoretical debate yet, interpretative practice has gradually changed direction and become more user-friendly.

A number of thematic books have explored select aspects of a contextual approach in more mainstream reinterpretations of well-known archaeological evidence (Bradley 1993; Gosden 1994; Tilley 1994; 1999), or applied it convincingly to a larger body of evidence (Barrett 1994; Earle 1996; Thomas 1996; Sørensen 2000). Richard Bradley's 'postprocessual-lite' version has been especially successful in communicating its interpretative potential to the non-converted (Bradley 2002). Likewise, processual archaeology has updated itself in the light of these new developments in works by Renfrew (1982; Renfrew and Scarre 1998), Bintliff (1991) and Knapp (1992). The tradition of the grand narrative has been developed in works by Hodder (1990), Sherratt (1997) and Kristiansen (1998b). However, most of these works are in early or later prehistory. Quite different discussions go on once we approach Classical archaeology and the Iron Age, where written sources become increasingly important (Andren 1998; Morris 2001; 2004; Hedeager 2001; Moreland 2001). Does this suggest another divide in theoretical thinking between early and late prehistory, between prehistoric and historic archaeology? The Bronze Age comes in between, belonging rather in protohistory (Bietti Sestieri 1996; Kristiansen and Larsson 2005). Compared to this mass of books in the processual, postprocessual or contextual tradition, Darwinian archaeology occupies only a small niche.

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There is much to suggest from this admittedly cursory review that right now different theoretical and interpretative perspectives live parallel lives without much dialogue, but paradoxically they share the same interpretative interests in cognition, landscape, memory, agency and so on. Is this a first step towards a new theoretical discussion about their interaction rather than their opposition? Dividing lines not only separate processual from postprocessual perspectives but also run between those who favour a local perspective and those who favour a wider regional or global perspective. Or between those who derive their theoretical inspiration from philosophy or psychology rather than from evolutionary biology and those who believe it should rather be derived from comparative culture-history and social anthropology. In addition, new journals are mobilized to support the various directions. There are journals of world system theory, social archaeology, material culture, anthropological archaeology, archaeological science, public archaeology and more.

Theoretical and methodological variety should be considered a disciplinary strength, and archaeology derives much of its identity from a position between science and the humanities, between knowledge and interest, between history and heritage, between management and research (Kristiansen 1996; Shanks 2004). Where are we heading right now in this diverging world of archaeological practice? The dominance of reinterpretations of traditional archaeological sequences, and the flow of new textbooks and readers suggest that we are in a consolidating phase, where once-new theoretical positions are being grounded in reinterpretations for students to read and take on board. This is a traditional strategy for taking possession of interpretative authority. Gradually it will foster a critical reaction we have yet to see. To promote such a change I propose that some problems in current theoretical programmes need to be addressed (or redefined, as some are familiar) and their consequences discussed. I further contend that they are responsible for much of the present interpretative closure and consequent redlining in theoretical debate.

Part II

First criticism: the lack of anthropological and culture-historical theorizing

In recent years we have seen a theoretical and interpretative development that has favoured local variation at the expense of regional or inter-regional regularity. It went hand in hand with a critique of general interpretative frameworks and comparative ethnographic works that were considered to be rooted in a modern perception of the world, or even to be immoral (Gosden 1999, 9). This rather high-handed dismissal of ethnohistory and comparative culture history went hand in hand with a sympathetic quest for understanding the otherness of the past and an emphasis on prehistoric people as knowledgeable agents, who were able to act and negotiate their own destiny. It tended, however, to promote an idealistic Western concept of autonomous individuals only governed by some divine force, called Being, borrowed from Heidegger. Paradoxically, modern sociology and philosophy of the most diverse kinds were mobilized to support this civilization critique of previous, universalistic interpretations of the past. There is indeed quite a

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theoretical and ideological distance from Foucault and Bourdieu to Gadamer and Heidegger, but Julian Thomas employed them all, as did others (Thomas 1996, Part I; see also Gosden 1994; Karlsson 1998). More recently, however, Thomas has criticized the concept of autonomous agents (Thomas 2002) and presented a more historically informed view on archaeological interpretation (Thomas 2004b).

The declared objective of the critique that we should be more open to the otherness or the unfamiliarity of the past is, of course, well taken. It was exemplified in Michael Shanks's book *Experiencing the past* (1992) and in a new phenomenological approach to interpretation that stresses human and bodily experience (Tilley 1994). Having abandoned comparative ethnohistory and anthropology, the limitation inherent in this de-culturized and individualized version of phenomenology was that the interpretative bodies were mostly Western, middle-class males (Gero 2000; Thomas 2004b, 24). It found supplementary inspiration in the outcome of thousands of excavations from commercial archaeology that revealed many new aspects of the prehistoric past in need of reinterpretation. In that I follow Thomas and Barrett in their critique of traditional interpretations that squeezed the evidence into conventional boxes (Thomas 1996; Barrett 1994), and Hodder in his quest for a more reflexive excavation practice (most recently Hodder 1999).[3]

However, philosophies and theories are not free-floating commodities to be employed at will. They bring with them ideological and political agendas, and the employment of Heidegger and agency theory is linked to a modern discussion about the relation between society and the individual, about how one constitutes human perception, human intentionality and forces of change (Dobres and Robb 2000). In the end it comes down to an old schism between materialism and idealism, between Marx and Hegel, dressed up in more advanced philosophical concepts.

The critique has also overlooked the fact that much of the otherness one was looking for in the premodern world – such as different conceptions of time, what constitutes humans and their perception of the world – was already to be found in social anthropology (summarized in Ingold 2000). This was demonstrated by Gosden in a historically contextualized return to social anthropology (Gosden 1999). However, a comparative ethnohistory and ethno-archaeology was generally abandoned in the 1990s in favour of a belief in the interpretative, hermeneutic dialogue between the archaeologist and the material evidence as the primary road to knowledge about the past. The theoretical guide originated with sociological thinkers and philosophers. This strategy privileged the archaeologists as the great interpreter with the (unintended?) consequence of leaving too much room for speculation due to the lack of comparative culture-historical correctives.

The first question to be asked is whether philosophical concepts and modern civilization critique constitute relevant interpretative parameters for understanding an archaeological past. Is this more useful than ethnohistorical evidence of the variety of ways humans have organized themselves and perceived the world throughout history? Not to speak of the practical experiences of a real otherness provided by ethno-archaeology. And do the two strategies need to be opposed?

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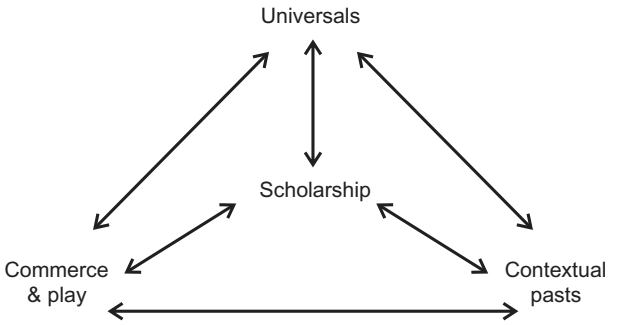


Figure 3 Model of the relationship between basic epistemological concepts and archaeological scholarship (modified from Hodder 1999, Figure 11.3).

The second question is whether we can at all avoid employing universalistic concepts. Ian Hodder discusses this dilemma in his recent book *The archaeological process*, where he recognizes that general claims and universals are unavoidable (Figure 3). In many situations of political oppression they provide the basis for critique and resistance (Hodder 1999, 205–6). In other situations, universals can be misused to promote interpretative hegemony. Once again, universals and generalization are context-dependent.

Second criticism: the lack of historicity – or why are agents more popular than genes right now? What I am missing in current debates is a critical concern with historicity. How do past and present theoretical and philosophical discourses relate to historical conditions? What we have seen in some postprocessual works is rather a simplistic and distorted critique of modernity, with the purpose of creating an arbitrary opposition that promotes postmodernity and the end of history.

Modernity is not as homogeneous a phenomenon as often described. In *Cultural identity and global process*, Jonathan Friedman demonstrates that postmodernity and globalization are part of civilizational cycles that have been with us since the Enlightenment period or even earlier (Friedman 1994, Figure 2.4). Others have described such shifts more narrowly as related to critiques of the Enlightment project (Gray 1995). I have termed these cyclical shifts 'Rationalism' and 'Romanticism' (Kristiansen 1998a). Theoretical discourses in the social and cultural sciences cannot be separated from these global cycles (Kristiansen 1998b, Figure 14). Thus postmodernity is historically an inseparable part of modernity. Postprocessual archaeology is likewise part of a culture-historical 'Romantic' revival with antecedents in the earlier 20th century (Harbsmeier and Larsen 1989). Local history, places, memory, race and identity were central concepts during this period. Physical anthropology was employed in archaeology to demonstrate the history of races, which went hand in hand with race philosophies and politics to eliminate unwanted elements. This happened not only in Germany, but also in more subtle ways throughout most of the Western world.

It may not be totally accidental that in the culture-historical revival of postmodernism and postprocessual archaeology interpretations are now

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promoted that link genetic variation with language (Renfrew and Boyle 2000). Renfrew should be criticized – not on political or ideological grounds for opening this Pandora's box to the world of Kossina – but on scholarly grounds for employing a simplistic theoretical notion of correspondence that does not consider properly those many other social and economic forces and conditions at work. The result is a simplified and historically distorted picture of genes and language that may serve to reinforce racist and nationalistic interpretations from people with such interests.

It is likewise inescapable that much of the philosophical inspiration in recent postprocessual thinking originates in the earlier 20th century. It marked a period after the First World War that had lost faith in the rationality of the Enlightenment and its conception of science. There has been some discussion of Heidegger's political sympathies in the 1930s (Thomas 1996, Part I), but they were a natural, practical outcome of his idealist philosophy. Not only Heidegger, but also much of the phenomenological theory on self and the body spring from the same period and ideological context. The period from the early 20th century until the end of the Second World War represented a return to idealistic and romantic perceptions in philosophy, culture history and sociology that went hand in hand with a new ideological and political climate. The philosopher Heidegger and the archaeologist Kossina relegated their understanding of the conditions of existence from the material to the immaterial world. Heidegger's Being and Kossinna's theories on culture and race (unified in the folk-spirit) are founded upon the same Romantic belief that forces beyond social and economic explanation govern not only existence but also history. Rooted in an unexplainable notion of a natural (superior) force, such concepts could be manipulated politically when linked biologically to a Master Race. Likewise it is often overlooked that the same period saw the development of a cultural aesthetic of the natural body that had great popular appeal as it was employed in a reorganization and mobilization of youth in new forms of sport and gymnastics that spread throughout Europe. The natural, well-trained body exhibited in new daring costumes and new social forms of group gymnastics under strong leadership became a popular idiom. It was employed with great ideological and propagandistic skill in Germany, where it represented a break with old constraining traditions. It therefore became popular among the youth, and at the same time it became a mobilizing, ideological force that could later be exploited successfully for war in the military.

Are there no historical or ideological implications to be recognized in the renewal in the 1990s of writers and philosophical and theoretical concepts from the 1930s? Without proposing that conditions in the 1920s and 1930s correspond to the present, I would at least expect some critical reflection upon the relationship. While history does not repeat itself, some of its components are reused.

The key question is, what consequences should be drawn from this for a critical and reflexive archaeological practice, if ideological conditions of the present relate to theoretical trends in the social and human sciences in a general way (Figure 4)?

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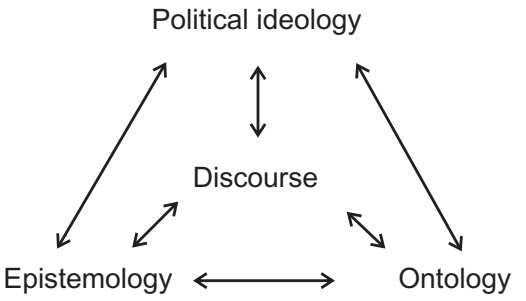


Figure 4 Model of the dynamic relationship between intellectual/ideological forces and theoretical discourse/paradigm.

Third criticism: the confusion of political and theoretical critique and of nature and culture Much of the critique delivered by postprocessual archaeology against processual and Darwinian archaeology has confused ideology and theory. I suggest this has been caused by lack of grounded historicity, which has left the field open for simplistic political critiques in the guise of theoretical flaws. An understanding of the historical conditions for the rise and the use of certain types of theoretical discourse does not imply that we can draw any easy conclusions as to the legitimacy of archaeological interpretative practice. Most theoretical and interpretative frameworks in the social and historical sciences have been subject to ideological simplification and subsequent political perversion. That was true of evolutionary Marxism of the later 19th century, as well as the culture history of the earlier 20th century. Both discourses have inspired theoretical revivals in the form of neo-evolutionism or processual archaeology and neo-culture history in the form of postprocessual archaeology.

The postprocessual critique of social evolutionary theory is a good example of the confusion of ideological and theoretical principles and practices. In Shanks and Tilley's book *Social theory and archaeology*, a heavy critique was launched on social evolution for being teleological and deterministic (Shanks and Tilley 1987b, chapter 6), which is echoed in much later postprocessual work. It is announced that 'any notion of social evolution is theoretically flawed and embodies unwarranted ethnocentric evaluations' (Shanks and Tilley 1987b, 138). We later learn in seven paragraphs of all the flaws, including totalizing holism, universality, directionality, determinism and reductionism. It makes up an impressive catalogue of concepts to be avoided by the politically correct postprocessual student. Rowlands and Kristiansen (1998) have at some length spelled out part of the theoretical misrepresentation inherent in this critique and in other postprocessual critique, and so have others (Yoffee and Sherratt 1993). As already stated, similar accusations can be made of all culture-historical paradigms (Kristiansen 1999). Political and ideological misuse cannot define the usefulness of theoretical approaches; the only protection against that is scholarly excellence and critical awareness. This includes a careful investigation and evaluation

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of basic theoretical principles and their use. Let us therefore discuss in some detail the two concepts of intentionality and directionality, as they figure prominently in both agency theory and Darwinian theory.

Directionality is not thought to be at work in natural evolution. Adaptation, selection and fitness operate no matter the direction (but see discussion in Graves-Brown 1996). Whether group or individual selection is more important is debated. To the contrary, I propose that human culture is based upon intentionality and directionality, with the latter following logically from the former. If one accepts agency and intentionality as a genetic constitution of modern humans, their operation is a constant ingredient in all social and cultural traditions and change. Multiple intentional choices add up to long-term effects that consequently can only be considered directional, even when beyond the experience of individuals.

However, the unintended long-term effects of multiple short-term intentions create a changed framework for new intentional decisions. Such decisions are, of course, always taken under various social, cultural and cosmological constraints, represented by institutions (Runciman 2001; Earle 2001; Kristiansen 2004a). They are responsible for the directionality, as it were, in the short term, being shaped and constrained by history and tradition. Or, as stated by Sassaman, 'normative structures are long-term derivatives of agency' (Sassaman 2000, 149). In the long term they also establish the boundaries for directional change, as they determine the range of evolutionary options.

Tradition is constituted by cultural and cosmological rules of conduct in which social and economic practices are dialectically inscribed. This dialectical relationship between social practice and cultural tradition defines the available space for agency. However, if – or rather when – ecological and economic thresholds are crossed on a major scale, a historical situation emerges where the room for agency and multiple experiments expands, eventually leading to a social transformation (Figure 5).

From this it follows that processual and postprocessual theory share a theoretical foundation of human intentionality and directionality that is context-dependent. It is the different choices of contexts and interpretative interests (economy versus culture) that constitute the main difference between processsual and postprocessual archaeologies. This will become evident when more theoretical labour is invested in understanding the role of traditions and institutions in prehistory.

Furthermore, if one accepts that agency and intentionality imply directionality in human history and social evolution, one cannot transmit Darwinian concepts of selection and inheritance to culture without basic modifications, as Darwinian selection rests on a different theoretical foundation (discussions with different positions in Rindos 1985; 1986; and VanPool and VanPool 2003a). The dual inheritance theory does not, in my opinion, represent that modification. It states that biological and cultural evolution is based on different principles without stipulating the theoretical consequences. What would they be? An indication is perhaps given in a recent article by Richerson and Boyd (2001). In the first part they remain within their scholarly domain and present an interesting hypothesis called 'the tribal social

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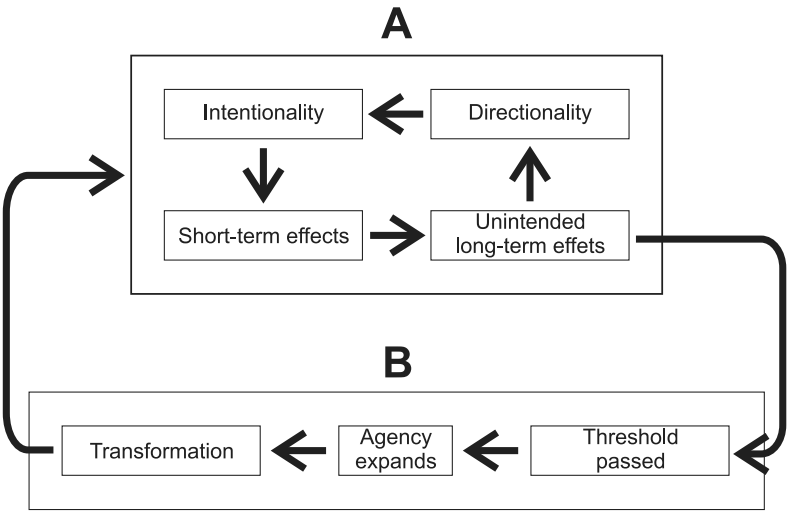


Figure 5 Theoretical model of the dynamic relationship between long-term tradition (A), short-term transformation (B) and their internal articulation.

instinct hypothesis', whose origin and possible social significance they discuss. Once they move on in the second part to explain the emergence of social complexity they merely repeat the most basic concepts from archaeological and anthropological textbooks. This is a far cry from their introductory statement, that Darwinian tools encourage a fresh cut on the big historical changes after the Ice Age. On the contrary, evolutionary theory has apparently nothing to offer on the subject of the emergence of social complexity that is not already present in existing social theory.

Most archaeologists accept that Darwinian archaeology is a relevant theoretical framework for understanding the evolution of modern humans, and contributions in this field are rarely questioned. It is the problematic general application to cultural evolution that is resented and causes problems. They can be solved by carving out an evolutionary paradigm of more modest ambition that supplements the dominant theoretical paradigms in the social and culture-historical disciplines. They were, after all, developed specifically for that purpose over more than a hundred years.

Postprocessual archaeology has similarly to come to terms with a more modest ambition as a supplementary interpretative discourse to processual archaeology and, vice versa, processual archaeology must accept a similar role in relation to postprocessual archaeology. From this, a new integrated culture-historical archaeology should develop that integrates basic components of previous paradigms (Figure 6), which I shall briefly discuss in conclusion.

Conclusions Let me summarize my critiques so far.

- By abandoning comparative culture history and ethnohistory, post-processual theory has lost a historical corrective to archaeological interpretation. It has privileged the interpretative role of the archaeologists

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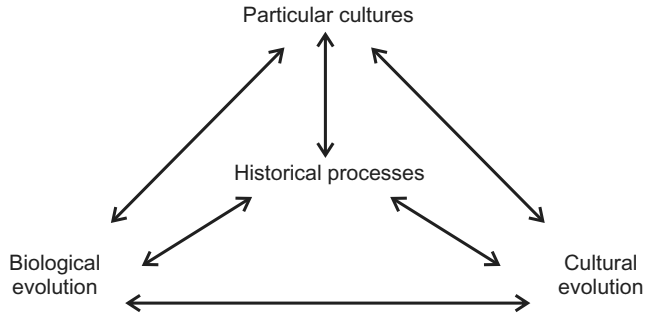


Figure 6 Model of some basic interpretative frameworks and their relationship with historical processes of change, indicating their complementary roles.

with the danger of developing interpretations rooted in a Romantic modern perception of the past.

- By abandoning cultural and historical concepts of evolution, based upon social properties of human behaviour, intentionality and directionality, evolutionary archaeologists have developed a theoretical framework that is epistemologically flawed and leads to a misrepresentation of the past, if not applied with due respect to its relevance.
- By abandoning epistemological historicity, processual and postprocessual archaeologists succeeded in presenting their approach as novel, when it was in reality embedded in historical and ideological traditions. This has led to a dangerous confusion of theoretical and political critique.

In the final part I shall discuss some possible strategies for developing a climate for productive theoretical debate. It implies among other things recognition that different theoretical programmes often relate to different interpretative realms.

Part III

Evolution and agency, macro- and micro-histories As the reader has already understood I feel at home neither with Darwinian archaeology nor with agency. Both represent fundamental properties of human behaviour that may serve as a catalyst for discussing current interpretative practices, but neither of them qualifies as a general theoretical framework for understanding the past. They both refer back to biological universals that fail to grasp the full historical and social complexity of the past. However, they also refer to dominant trends in current thinking, and therefore need to be taken seriously. Biological evolution has much in common with early New Archaeology and its preoccupation with establishing scientific laws, raising archaeology to the status of science. I suspect that the adaptation of biological evolution in archaeology reflects a similar wish to join the natural sciences and avoid the ambiguity of historical particularism. This has been spurred by the revolution in genetics, the results of which will also have enormous consequences for archaeology. But does that imply a subordination of archaeology to a biological paradigm? Likewise the definition of prehistoric people as

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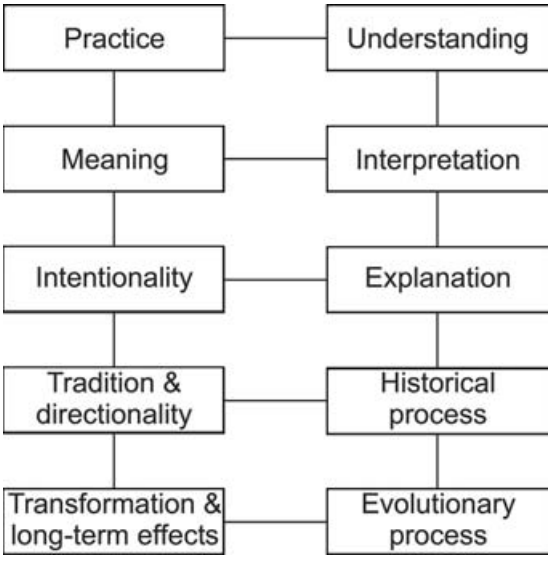


Figure 7 Contextualized ladder of inference in archaeological interpretation and explanation.

knowledgeable agents puts a universal human capacity of social action and intentionality at the forefront of interpretation, which tends to ignore or downplay the shaping or even constraining role of historical conditions and traditions. Also, in this field archaeology is assimilating current debates and concerns in sociology, psychology and related disciplines, where agency can be studied under quite different conditions than in archaeology.

It seems to me that a preoccupation with Darwinian evolution and agency tends to miss what is the real strength of archaeology – the explanation of historical processes and an understanding of how people act and interact through material culture. This dynamic between the individual and collective in the shaping of tradition, between local and global historical processes, is perhaps the most striking contribution of archaeology compared to other cultural disciplines. Rather than looking for theoretical inspiration in biology or psychology we should perhaps return to those disciplines with which archaeology shares basic interests – social anthropology and history. Here is a rich comparative field of evidence that may enrich archaeological interpretations in the middle field between genes and agents. And archaeology may likewise be able to enrich social anthropology and history in the process by providing a unique understanding of the long term and the materiality of social relations and institutions (DeMarrais, Castillo and Earle 1996; Tilley 1999; Earle 2001; 2002; Kristiansen 2001a; Renfrew 2001; Fletcher 2004; Fahlander and Oestigard 2004).

By accepting that processual and postprocessual archaeologies occupy different, but complementary, interpretative rooms, we may finally be able to replace Christopher Hawkes's 50-year-old ladder of inference with one that does not discriminate between different properties of archaeological evidence as its main criteria (Hawkes 1954). In Figure 7 I propose instead to

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distinguish between different contextual levels of interpretation. It suggests that hermeneutic understanding and processual explanation belong together, but occupy different contextual realms on the ladder of inference (which is not a ladder, but rather a feedback loop). At the level of historical and evolutionary processes the complexity of the evidence defies traditional epistemological rules of inference and rather demands a type of informed, argued narrative.

Is it too much to expect an intellectual and theoretical capacity, which can simultaneously interpret the practice and meaning of lived experience and explain its ideological role in social reproduction? Do we really need to perceive of these two realms of interpretation as opposed to each other? In *Reading the past* Ian Hodder explored this contrast in a critique of Marxist archaeology (Hodder 1986, chapter 4). So did many later postprocessual students. However, in his most recent book, *The archaeological process* (1999), Hodder has opend up for a more integrated approach to interpretation. Such integration has also been forcefully argued by Bruce Trigger (2003a).

Rather than abandon evolution and agency, I propose we should look for ways in which they contribute to each other. In doing so I take for granted that archaeologists possess the intellectual capacity and theoretical flexibility to move back and forth between different levels and types of interpretation and explanation, to explore the dynamic relationship between local and global. I do not take for granted, however, that everyone has the will or the interest to do so, if not motivated by good examples. And I fully acknowledge that there exist irreconcilable theoretical positions linked to different interests and philosophical stands.

Door-openers to empty interpretative rooms – and some proposals of how to fill them Let me present some simple observations for further reflection. It is undeniable that social evolution has taken place, including some major historical transformations that affected large regions or world systems. Historical regularities (social, demographic, economic and so on) are undoubtedly at work in the long term. It is also undeniable that this framework does not explain much local and regional variation so that at the micro level of interpretation much can be learned about the ways people of the past reacted to the world around them and created changes from generation to generation. However, it is likewise undeniable that such variation was part of shared traditions in time and space, which in turn were part of culture-historical regularities of a non-evolutionary nature. It is also undeniable that modern perceptions of humans, animals and cosmos are unable to account for such concepts in, let us say, the Neolithic or the Bronze Age, because modern and premodern cosmologies are fundamentally different. Consequently, it is undeniable that the prehistoric past cannot be approached from the present without a guide. It does of course remain a matter of debate whether that guide is ethno-archaeology and contextualized ethnohistory (Gosden 1999; Helms 1988; 1993), philosophical perceptions of the world (Thomas 1996; Karlsson 1998), comparative culture history and history (Andren 1998; Earle 1996; 2001; Morris 2000; Trigger 2003b), or perhaps some mix of all.

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Following on from this I propose that particular local practices can only be understood against an established background of shared traditions in time and space. To get there we need to unwrap contextual studies from their localized interpretative constraints. No society is an island. Furthermore, meaning can only be established by employing comparative culture-historical and ethnohistorical evidence. We cannot dream up new interpretations we have no concepts for. No divine, interpretative Being can replace the theoretical labour needed to formulate interpretative concepts. It may help us to open a door to new perceptions of the past, but once the door is open, we realize the room is empty. We therefore need to contextualize, theoretically and historically, our most commonly employed interpretative concepts each time we start a research project. We need to fill the room with interpretative furniture, as has indeed been done in several of the works mentioned. But more often such concepts as agency, memory or power are applied in a rather unreflected way, as if their meaning is already understood or will be revealed solely through archaeological interpretation. But each historical epoch and context, whether local or global, demands theoretical redefinition and conceptual adjustments to account for their historical and cosmological traditions and particularities. There is no universal Self or Being.

Based on this insight, Julian Thomas recently concluded a discussion on archaeological interpretation with a rather defeatist statement: 'What remains unresolved is how we can appreciate the *diversity* of ways in which any past world must have been understood, with only our own location to speak from' (Thomas 2004b, 34, original emphasis). Besides being a universal problem of all historical research, this statement also exemplifies the limitations of a philosophically inspired postprocessual interpretative archaeology. My answer is that the best we can do is to employ systematically a comparative, contextualized ethnohistory and culture history, one that takes us beyond analogy and opens up a different and broader historical understanding. This I should consider an interpretative dictum. It demands a renewed discussion of the theoretical and interpretative role of history, social anthropology and ethnohistory in archaeology. Today their roles in archaeological interpretation are often contradictory: dismissed in theory, but employed in interpretative practice. However, a new interdisciplinary orientation is slowly beginning to emerge in both theory and interpretative practice (Gosden 2004; Kirch and Green 2001; Knapp 1992; Rowlands 2003; Yoffee 2004; Kristiansen and Larsson 2005, chapter 2).

Here, then, is the challenge, as I see it: to develop interpretative perspectives which explore the dynamic articulation between micro- and macro-histories (Cornell and Fahlander 2002), between generational history and long-term history in a Braudelian sense, between different world views or cosmologies and their organizing role in the past. It demands a new comparative and holistic perception of archaeological and culture-historical knowledge, one that is truly interdisciplinary. It may even demand new ways of organizing both research and teaching. I contend we are in the process of constructing a new culture history, unified by material culture studies in all their variety and contexts, from natural science to oral tradition and written texts. It spans archaeological and historical contexts from history, ethnohistory,

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anthropology and prehistory. Some have called this post-disciplinary science (Fahlander and Oestigaard 2004). I am not proposing a unifying theoretical framework, but rather a unifying approach to culture-historical studies of material culture. This will bring about new theoretical concerns to be discussed and developed.

Can we bring about a different kind of grand historical narrative, one that traces the formation of different forms of self-perception, of cosmology, of nature and of animals (to name some select examples)? And can we try to bring this into a dialogue with concrete case studies that help us to understand the ways such world views were interpreted and practised locally, and how they articulated with social and economic strategies? We may then finally be able to resolve the historical oppositions between events and structure, between agency and evolution, between processual and postprocessual approaches.

Acknowledgement

This paper originated from a presentation and discussion at the Cambridge theoretical archaeology seminar during my stay at the McDonald Institute in the autumn of 2003. I wish to thank the participants, mostly Ph.D. students and young postdoctoral researchers, for an inspiring discussion. Several people read, and commented on, a draft version in the spring of 2004 during my stay at Stanford University. I wish to thank Bill Rathje, Chris Witmore, Timothy Earle and Alice Kehoe for inspiring comments. Finally, three anonymous reviewers made several useful comments, some of which I employed.

Notes

[1] This is also apparent in the formation of new sections in several archaeology departments especially in the US and the UK for archaeological/biological studies. In UCL Steve Shennan is professor of a research department in evolutionary archaeology with many Ph.D. students.

[2] All societies are truly innovating in terms of creating and re-creating a distinctive material culture of their own defined by social and symbolic meaning. It may be considered a universal feature of modern humans. Variation therefore is repetitive rather than evolutionary. Evolutionary archaeology proposes to characterize formal typological variability according to a set of predefined concepts (phylogenies) that is separate from historical and social variation (O'Brien and Lyman 2003). This is by many considered a fundamental theoretical mistake. Critics contend that material culture varies according to several social and economic parameters that cannot be subsumed under a single theoretical or methodological framework detached from historical interpretation, as once demonstrated by Steve Shennan (1978; also Kristiansen 1998b, Figure 25). The application of phylogeny to material culture has recently been critically debated and resisted (Bamforth 2002; 2003; Kehoe 2000; response from O'Brien, Lyman and Leonard 2003). Bamforth's statement as to its theoretical flaws remains conclusive: 'The assertion that the selectionists have identified the operation of natural selection in the archaeological record fails to meet the most basic standards of archaeological reasoning' (Bamforth 2003, 581). This polemic may be taken to exemplify incompatible epistemological positions. If one believes in O'Brien's application of natural selection to culture then the theoretical approach with less replicative success should vanish over time.

[3] This contextual and reflexive archaeology, stripped of comparative culture-historical evidence (which of course is unconsciously employed as all theoretical concepts are

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generalized abstractions), has indeed much in common with the development of modern excavation techniques of the 1940s and 1950s. It shares with it the belief that the past can be approached from a multiplicity of perspectives through its material evidence, which demanded therefore the most accurate and systematic documentation. Likewise, a reflexive archaeology demands highly elaborate empiricist documentation strategies. The main difference between them, besides the rhetoric, is perhaps that in the 1950s there was no Internet to facilitate a reflexive dialogue from all corners of the world. And of course the archaeologists of that day were employing a positivist terminology of objective principles of documentation which today archaeologists have abandoned. But by and large they share the same objectives and documentation strategies.

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'Mission impossible!' Comment on Kristian Kristiansen, 'Genes versus agents. A discussion of the widening theoretical gap in archaeology'

Ulrich Veit

'Theoretical archaeology' as it exists today is – as we all know – primarily a product of anglophone archaeology. It developed as a challenge to traditional archaeological thinking in the 1960s under the label 'New Archaeology' and it flourished in the late 1970s and 1980s with the productive confrontation of 'processual' and 'postprocessual' approaches. In these 'wonderful years' theoretical archaeology gained reputation and influence far beyond its early centres. All over Europe and beyond mainly younger archaeologists found it useful and fascinating to join these debates (Hodder 1991).

The discussions on the epistemological potential of archaeology, which started at that time, continue until today, but the enthusiasm once associated with them seems to have declined. Some archaeologists even feel that theoretical archaeology during the last years has run into a crisis. Is this analysis correct and, if so, what are the reasons for this development? What went wrong? Is there a way back to those happy times? These are some of the questions to which Kristian Kristiansen is trying to find answers in his article.

The starting point of his discussion is the realization that there exists a widening gap within theoretical archaeology between epistemologically incompatible approaches such as Darwinism and agency theory. In this 'current trend towards theoretical totalitarianism or salvation' (p. 78) Kristiansen sees a major obstacle to the future development of this field of archaeological enquiry. It is therefore of vital importance to him that a climate for a more productive debate within theoretical archaeology be established.

The argumentative structure of Kristiansen's paper is very clear. The description and analysis of the current situation of theoretical archaeology on the basis of two recent publications (Part I) is followed by the formulation of a list of severe criticisms (Part II). The article is completed by the outline of a strategy for initiating a new productive theoretical debate – a real 'mission impossible' as the author admits (Part III).

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As Kristiansen has long been critically observing and commenting on developments within anglophone theoretical archaeology it comes as no surprise that the arguments presented here are not entirely new. On the contrary, most of them are familiar from Kristiansen's numerous earlier publications. On the whole the present article offers a commentary on the latest developments within this field of research and Kristiansen refers to nearly all important protagonists in the theoretical debates of the last decades. The bibliography of his article consequently reads like a *Who's who* in theoretical archaeology.

This demonstrates Kristiansen's deep involvement in these discussions, to which he has made many substantial contributions over the years. This also explains the large number of internal references to recent discussions, which make the article difficult to understand for those who are less familiar with these debates. His paper is therefore clearly not intended to convince those archaeologists who have still preserved a critical distance towards the project of a theoretical archaeology as conceived within anglophone archaeology. Instead, Kristiansen adds a new statement to the long-lasting internal dialogue within the relatively small group of theoretical archaeologists.

The first part of the paper is almost entirely made up of a review of two recent books, which both exemplify a specific direction of recent theoretical archaeology: Darwinian archaeology (Shennan 2002) and agency theory (Dobres and Robb 2000). It does not seem appropriate to offer here a review of these reviews. The conclusions drawn by Kristiansen in this part of his article may better be commented on by the respective authors. I will instead concentrate on the ideas put forward by Kristiansen in his discussion of these reviews, primarily in Parts II and III of his article.

In general, I agree with many of Kristiansen's observations and comments on the actual situation of theoretical archaeology. This may primarily be attributed to the fact that we both feel at home neither with Darwinian archaeology nor with agency theory. Furthermore, like Kristiansen, I am convinced that archaeology shares some basic interests with social anthropology and history, which both offer a rich field of comparative evidence. And finally I agree with him that ethno-archaeological studies may help us to understand the archaeological record better.

From this point of view, it seems indeed strange to see that many recent approaches, instead of arguing from a broad comparative perspective, favour a 'hermeneutic' methodology, which leaves more room than necessary for speculation. The archaeologist-as-scientist of the 1990s has obviously been replaced by the archaeologist-as-author (see, for example, Hodder 1990). That means that the main task for the archaeologist has become telling stories about the past, which have relevance to the present. Whether they are 'true' or not in this context seems to be of secondary importance (see Veit 2003a).

So far the common ground which I share with Kristiansen. My problems with his discussion start when he ultimately argues for the use of universals in archaeology (p. 89). The discussions of the last decades have in my opinion clearly shown that reference to human universals is only of very limited

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value in archaeology. The search for human universals does not solve our central problems. Such a functionalist approach is not able in particular to explain the great variety of cultural traditions, which are documented in the archaeological record. We therefore need to look for theoretical approaches that can explain this variety (Veit *et al.* 2003).

This situation in archaeology can usefully be compared to that in anthropology, where the same problems have been discussed for much longer. Clifford Geertz (1973, 33–54) has long ago shown that the search for human universals does not lead to concrete results but only to loose associations. What is more, a generalizing approach in anthropology misunderstands the nature of culture. 'Culture' in conceptualizations of this kind is seen as a simple addition to the biological, psychological and social properties of people – and not as the central feature of human existence, which it really is. For Geertz, there is in fact

> no such thing as a human nature independent of culture. Men without culture would not be the clever savage of Golding's *Lord of the Flies* thrown back upon the cruel wisdom of their animal instincts; nor would they be the nature's noblemen of Enlightenment primitivism or even, as classical anthropological theory would imply, intrinsically talented apes who had somehow failed to find themselves. They would be unworkable monstrosities with very few useful instincts, fewer recognizable sentiments, and no intellect: mental basket cases.

We are, as Geertz summarizes, 'incomplete or unfinished animals who complete or finish ourselves through culture – and not through culture in general but through highly particular forms of it' (Geertz 1973, 49).

These circumstances lead Geertz to reject the so-called '"stratigraphic" conception of the relations between biological, psychological, social, and cultural factors in human life' (1973, 37). In this view, people are a composite of several 'levels' or 'layers', each superimposed upon those beneath it:

> As one analyzes man, one peels off layer after layer, each such layer being complete and irreducible in itself, revealing another, quite different sort of layer underneath. Strip off the motley forms of culture and one finds the structural and functional regularities of social organization. Peel off these in turn and one finds the underlying psychological factors – 'basic needs' or what-have-you – that support and make them possible. Peel off psychological factors and one is left with the biological foundations – anatomical, physiological, neurological – of the whole edifice of human life (Geertz 1973, 37).

Ironically, it is exactly this kind of model which presents people as 'a hierarchically stratified animal, a sort of evolutionary deposit' (Geertz 1973, 38), which is now promoted again by Kristiansen as a means to integrate the diverse branches of recent theoretical archaeology (see his Figure 1). According to him the different theoretical programmes are not incompatible, but relate to different interpretative realms: 'Processual and postprocessual

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archaeologies occupy different, but complementary, interpretative rooms' (p. 95). So hermeneutic understanding and processual explanation in a certain sense belong together, even if Kristiansen acknowledges at one point 'that there exist irreconcilable theoretical positions linked to different interests and philosophical stands' (p. 96).

This situation is represented by a new 'ladder of inference' (Figure 7), which could and should replace the famous 'ladder of inference' with which Christopher Hawkes (1954) exactly 50 years ago tried to discriminate between different properties of the archaeological evidence according to the respective field of archaeological interest. I do not agree with Hawkes because his concept underlies exactly the sort of reductive culture concept that was criticized above (for details see Veit 2003b). And the same criticisms apply to Kristansen's updated version, in which culture is subordinate to other spheres of human existence. It is relegated to a surface phenomenon, to be analysed by those interested in it (e.g. postprocessual archaeologists). But at the same time the main structural matters are to be found in the social realm of human existence as dealt with by Kristiansen and others. So what Kristiansen presents to us as an integrated approach to archaeological theory in reality is not more than a lean version of processualism. This also becomes clear from the terminology he prefers, with key concepts like 'intentionality', 'directionality' and 'generalization'.

This criticism should not be read as a total rejection of the kind of approach Kristiansen is looking for. With regard to the large number of case studies available to this kind of research today, we can easily assess the epistemological value – as well as the limitations – of such an approach. At the same time, this may well also be one of the reasons why a younger generation of archaeologists have now turned their backs on these old problems of the 1970s in search of new challenges and new paradigms. This seems entirely legitimate to me, even if these new approaches cannot be subsumed under older paradigms, and even if they cannot be easily transformed into methodological rules to deal with archaeological evidence.

What might such an alternative look like? Some of the publications from younger archaeologists cited by Kristiansen give hints to its direction. For me, at the moment, the greatest challenge to the kind of archaeology that Kristiansen's generation stands for comes from those historical studies which are based on the ideas of Michel Foucault (see especially Veyne 1992; 2003). They outline a wholly new approach to the past which is at the same time historistic and materialistic. Adaptations of those ideas to the field of archaeology are still rare (e.g. Rössler 2004) but this may well change in the near future.

What does this mean for the future of a theoretical archaeology in general? Do we really need a reunification of archaeological theory under one roof? In my opinion, such a reunification is not possible without misrepresenting at least one of the recent dominant epistemological traditions. But such a reunification is at the same time not necessary, at least as long as our primary aim is not to defend old cognitive structures, together with the institutional and political structures associated with them. From this point of view, Kristiansen's message appears to be much more political than theoretical and

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it is therefore somewhat ironic that Kristiansen accuses recent postprocessual archaeologies of a 'dangerous confusion of theoretical and political critique' (p. 94).

So far, then, my general objections towards the new concept of an integrated theoretical approach to archaeology as proposed by Kristansen. One point of disagreement perhaps deserves further mention. It relates to Kristiansen's understanding of the German philosophical and archaeological tradition. To me, it seems very strange to see the archaeologist Gustaf Kossinna (1858–1931) and the philosopher Martin Heidegger (1889–1976) grouped together in order to illustrate a specific historical stage of philosophical and ideological reasoning (p. 90), because apart from a certain affinity of some of their ideas to those of National Socialist propaganda (e.g. the concept of a master race),[1] the epistemological concepts underlying their writings do not have very much in common. While the work of Kossinna remained largely based on 19th-century positivism emphasizing data collection and methodology ('settlement-archaeological method') (Grünert 2002), positivism as well as science and methodology are rejected out of hand in Heidegger's ontological philosophy. In Habermas's words, 'wholesale devaluation befalls scientific thinking and methodically pursued research, because they move within modernity's understanding of Being prescribed by the philosophy of the subject' (1987, 136; 1985, 163).

This case clearly shows that the correlation between theoretical and philosophical discourses and the specific historical conditions under which these discourses developed is perhaps not as close as Kristiansen seems to believe. Things are usually much more complicated and the history of archaeological thought cannot be fully explained by reference to cyclical shifts between the two extremes of 'Rationalism' and 'Romanticism' (p. 89), as many other factors must also be taken into consideration (for details see Veit 1995; Veit in press).

Note

[1] It should also be remembered that Kossinna died in 1931, two years before National Socialist rule was established in Germany.

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Agency. A personal view *John Robb*

Introduction

It is always intriguing, and occasionally horrifying, to see what one has written refracted through another person's eyes. This is all the more so in academic writing, in which people often become identified with (or even fashion themselves into icons of) a given idea, and the act of refraction adds a political or positional hue. Meditating on this fact gives us a starting point for a brief exemplary consideration of agency.

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A few clarifications

As a preliminary to discussion, it is necessary to clarify a few points in Kristiansen's article 'Genes and agents' which substantially misconstrue Dobres's and my work (Dobres and Robb 2000) and agency theory more generally.

On 'theoretical agendas' Kristiansen refers to agency theory as a coherent school or movement with a theoretical agenda, and is even surprised to find Dobres and myself 'less dogmatic and more sceptical' (p. 83) than he expected! Even a casual reading of the introduction to *Agency in archaeology* reveals that the book does not present a single theoretical agenda – Table 1.1 presents twelve quite distinct definitions of the concept – and it intentionally includes as wide a variety of approaches as possible, including some with which each of us personally disagrees. I present some of my own views on agency below, but emphatically not with the understanding that this represents the field as a whole. This generally reflects the state of the field, in which agency tends to be used as one useful concept among many by a wide range of theorists rather than serving as a banner for a unified position.

On seeing cultural difference To me, Kristiansen's most serious assertion is that agency theory denies us the ability to see cultural difference because what we will see in the past is merely reflections of our own present. In fact, this is an epistemological danger with all archaeological interpretation. One of the principal attractions of agency theory is precisely that, by explicitly focusing upon the historical particularity of agents and their social construction, we can think clearly about what kinds of people there were in the past and avoid simply projecting our own self-image onto antiquity.

On 'selfish agents' In fact, it is difficult to understand exactly what Kristiansen means by the 'agency theory' he is criticizing. In some places, agency derives from 'biological universals' – this is presented as a well-accepted credo, though no serious agency theorist makes this argument; rather, it seems Kristiansen's own view. In other places, agency means post-structuralism *à la* Bourdieu and Giddens, with individuals pursuing their goals as the unintended consequences cascade through history. In other places, agency is about intentionality, though this is in fact something which many post-structural theorists are at pains to disavow (McCall 1999). In yet other places, agency theory is equated with a phenomenological interpretation of Heidegger as a Romantic quest for meaning. The point is that no one theory, right or wrong, can do all of these things and in many ways they are incompatible with each other; conflating them into a generalized 'agency theory' constructs the duck-billed platypus of the theoretical world: incoherently designed and all target.

What is revealing is how Kristiansen actually uses agency theory to build his argument. His critique refers to faceless theorists: it is what Kristiansen expects agency theory to be. In contrast, when he cites in detail people who actually engage with the concept – Gero, Sassaman, Clark, Johnson, Hodder, Brumfiel, Moore, Gell and Gosden, among others – he uses them with enthusiasm to argue for 'a truly contextualized study of agency in the

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past' (p. 85). In other words, the 'agency theory' of the 'selfish agent' seems born out of rhetorical symmetry with the 'selfish gene' as a stimulus for the author's meditations, and hence may come across to readers as basically a straw man. In practice it is Kristiansen's new brand of theory, but for its stripe of historical materialism it actually closely resembles much current work on agency in both processual (Hegmon 2003) and postprocessual (Gardner 2003) traditions.

Agency: a personal view

Agency is very simple and very complicated at the same time. As natives, we understand an immense amount about it without thinking; we express in seconds, in a gesture or a few words, something which it takes studious aliens years to explicate.

The best starting point for a brief explication is the fact of action. Action is often considered intentional: people act to pursue goals. Furthermore, intentional action in archaeological theory is often considered instrumental: people act to pursue success, power or prestige (Earle 1997; Hayden 1995; Price and Feinman 1995). This is arguable, but even if it is so we must recognize a social sleight of hand where this ultimate, generalized goal is transformed into, and often hijacked by, a proximate goal relevant to a given context. The point is that we necessarily act within what Barrett (1988) has called fields of action. Archaeologists have generally short-circuited fields of action (which are particular, numerous and nit-picky to study) in blasting a road from agency to action, but this has been a mistake. All action begins with a definition of what is being done, and this in turn calls into play resources of meaning: how the action is understood, intended, prescribed, limited, chosen and experienced. Action is situated: I can choose freely a course of strategic action, but not from infinite options and not without both external constraints and my own internalized sense of why I want what I want and the correct way to go about it.

Let us take an example – writing an article for an academic journal. This is certainly an intentional activity, and most academics recognize that it can have a certain strategic, game-like aspect; we may perhaps stretch to construe it as an act of intentional manoeuvre in pursuit of an abstract 'power' or 'prestige', but this is neither necessary nor sufficient to understand it. The most obvious point is that, while academic writing might gain one a modest increment in a salary review or tenure a year or two sooner, as a long-term strategy of aggrandizement it is a bit hopeless compared (say) to spending comparable effort and years of education practising law, medicine, business or computing. Hence if agency is about strategic pragmatism, academics are flops at it! For another thing, such reduction does not explain how we formulate our intentions, for instance why we take pains to write something we actually think is true, why we care about making ourselves or our subject understood, or how we balance an intention to write with other intentions such as teaching well, fulfilling family obligations or keeping our gardens in order. Moreover, academic writing as a field of action channels and restricts our actions in ways we do not normally even think about. For example, when Kristiansen and I take exception to each other's comments, we reach for a keyboard rather

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than a gun or a custard pie, we think and write within a particular register of academic discourse which moderates difference with civility and detachment, and we each understand what the other means by their actions within the standards of this field of action. Finally, action carries obligations and long-term commitments too: learning, thinking, dialogue. In other fields of action, these are negotiated through apprenticeship, bridewealth, reciprocal trade obligations, a mortgage. In academics, our discussion here is presumed to be among selected, enfranchised equals who have achieved this status through a complex process of self-formation (internalizing a highly specialized discourse of little interest to anyone else), specific events such as thesis defences, conference papers, and publications, and social relationships within which learning and using such a discourse is possible. Being a free agent is entangling.

The implication is obvious: you can only write and publish as an academic within a long-term project of the self – being an academic. Indeed, the link between action and self-formation is so obvious in this case that it forms part of an explicit discourse called 'education'. And we reconstitute this field of discourse in practice: when we review a journal article or a grant application, correct a doctoral student's writing or examine their thesis, ignore or engage with a presentation at a conference, evaluate job applicants according to their publication records, or for that matter conduct our own activities in the internalized anticipation of someone else doing the same work as ourselves. Finally, our actions, each individually intended, justified and executed, accumulate to create statistical relations independent of, and often contrary to, what we intend or how we understand ourselves – for instance, the mastery of academic discourse via university education as a tool of social exclusion.

Hence action has intended effects, but it also reproduces fields of action and is continual self-formation, the creation of a capability for a specific kind of agency. It follows that we cannot speak meaningfully of agency as an abstract, raw or pure noun, but only of specific, contextualized or situated agencies. It further follows that these are multiple – the same person may experience the contextual agency of an academic, a potter, a parent, a politician – and none is exhaustive. Though each step in this argument is logically compelling, at this point the landscape may begin to look strange. For example, our point of departure is the common presumption that agency involves individual action, but the units of social action quickly become blurred, with individuals constituted by and participating in multiple agencies. Then there is the question of material agency. I do not see much point in rather theological disputes about whether material things can 'have agency'. But it seems inescapable that the computer makes my writing possible, gives the final product a specified material form, organization, usability and transactability, and habituates me. As Ingold argues, the tool and the hand become inseparable (Ingold 2000); not only has writing longhand without the continuous ability to reformulate text become difficult for me, but when I write longhand now, I find myself thinking every so often that I should 'save' the handwritten document. An equally strong argument can be made for the collective agency. Virtually any social action is possible through coalitions of agencies whose intentions may be quite different. To return to our example,

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as an academic writer I want to formulate and communicate a particular argument. But my writing is enabled and in fact required by university employment; the institution simply wants entries on a research inventory and does not care whether they deal with agency or aardvarks. The journal's project is to publish a sufficiency of articles of appropriate quality and topic; unlike the university, the editor will care deeply about these things, while lacking the author's commitment to the particular argument. The printer is fulfilling a contract, the journal secretary is drawing a paycheck, and so on; we can even continue on to discuss the agency of the readers without whom the process is incomplete. Moreover, much of the agency is delegated; both the university and the journal will appropriate specific aspects of the writing, as part of their own projects, in return for enabling it, and hence in a sense each agent is acting both for themselves and on behalf of others of whose project the action forms part. Such delegation is generally a parameter of the situation understood by all, an aspect of its constitution as a field of action.

To conclude, this is a personal view of agency, not merely because it is what I think myself, but because agency is personal, it is about acting as a certain kind of person to do a specific kind of action in a specific situation. Although individuals act intentionally, agency is not necessarily about individuals and quite often an individual's identifiable intentionality plays a minor part in social action and its consequences. In effect, intentionality refers to the 'agency of why' (from the actor's point of view, not from an explanatory point of view); the reproduction of social relations in fields of action gives us the 'agency of how'. And the latter often defines the former and shapes its results. It is for this reason that we should resist the temptation to view intentional, motivated action as the spark that drives social life and social change.

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Culture, society and evolutionary theory *Stephen Shennan*

Kristiansen's paper represents a typically stimulating and thoughtful response to some of the recent theoretical trends in archaeology, and it is equally characteristic of his now long-established 'public intellectual' role in European archaeology that he should want to bring his concerns into the arena of open debate and to explore common ground between different perspectives. I find myself in agreement with many of his conclusions, of course without accepting his view that they are largely incompatible with the Darwinian view that he criticizes. In what follows I want to respond to some of his criticisms and address what I see as some of his misconceptions about the nature of evolutionary approaches and their implications for understanding the human past.

A good place to begin is with his title and its opposition between genes and agents, which in my view should be rejected (Shennan 2004b). People

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are evolutionary agents whose actions and agency are based in part on propensities derived from their genetic heritage, shaped by millions of years of natural selection of their ancestors, and by cultural traditions that they learn from other members of their community; they respond to the situations in which they find themselves in the light of those propensities and traditions. The 'choices' they make, conscious and unconscious, are not determined (cf. Dennett 2003), but that does not mean that they are not to some degree predictable.

In the case of humans and, it is increasingly clear, many other animals as well (Avital and Jablonka 2000; Danchin *et al*. 2004) success in survival, reproduction and parenting depends not just on individual capacities and individual learning from experience (which depends on other capacities, like sensitivity to environmental cues), but also on socially learned traditions, passed down through generations. To the extent that specific propensities that lead to greater success in the aggregate outcomes of survival, reproduction and parenting have a genetic basis, they will tend to spread through the population. In addition, however, natural selection can also act on cultural traditions and their bearers, where those traditions represent 'adaptations', socially learned ways of doing things that have an effect on people's survival, reproduction and parenting success. To the extent that they are passed on through the generations and enhance success, these too will spread. Cereal agriculture, for example, seems to have represented a successful adaptation in this sense in many parts of the Old World, and the adoption of the sweet potato in others (see e.g. Wiessner and Tumu 1998 for New Guinea); both of these new adaptations led to population expansions. Equally, however, such practices, and indeed many others, can spread as a result of social learning between unrelated individuals who see advantages in them (use their agency) and who may then enhance their own survival and reproductive success as a result, but this by no means exhausts the list of processes that affect the success or otherwise of socially learned traditions, which in some circumstances may spread despite consequences which are maladaptive from the reproductive success perspective (see below).

In the light of these introductory considerations concerning the nature of evolutionary agents and the factors affecting them we can turn to Kristiansen's argument. A key aspect of this is that Darwinian archaeology is over-ambitious and imperialistic and (like postprocessualism, in his view) should, in effect, know its place. This lies largely in the analysis of such subjects as subsistence patterns and population, and perhaps especially in early periods. 'Most archaeologists', he says, see it as a 'relevant theoretical framework for understanding the evolution of modern humans . . . It is the problematic general application to cultural evolution that is resented' (p. 93).

I do not have a problem with different people following different approaches, although I hope that their rejection of evolutionary ones is not based on the kind of misconceptions seen in places in Kristiansen's paper. However, his seems to me a curious argument because what it implies is that there is a point in the history of human evolution when people float free of nature and become purely cultural beings, a kind of secular version of the acquisition of the soul. Even though he acknowledges elsewhere that

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evolutionary ecological ideas are generally relevant to the understanding of subsistence and population, the idea that they should be restricted to that domain is odd. As a processual materialist Kristiansen would be the first to argue that ways of getting a living are intimately linked to forms of social organization, which in turn connects to aspects of ideology and religion. Indeed, he has on many occasions analysed the relations between these things in a stimulating and productive way (e.g. Kristiansen 1984). If these links do exist and processual materialists are allowed to pursue their implications, it seems inappropriate to veto archaeological evolutionary ecologists from doing the same thing from their own perspective. No approaches know the limits of their range of application until the boundaries have been pushed, and evolutionary approaches to the study of human behaviour are still extremely novel. In fact, there have recently been very stimulating examinations of the nature of religion from the perspective of evolutionary psychology (e.g. Boyer 2001; Atran 2002), while evolutionary game theory is revolutionizing understanding of the generation and maintenance of social cohesion (e.g. Skyrms 1996). To what extent these ideas can be employed in archaeology remains to be seen, but that is not a basis for insisting on being an angel with a fiery sword who says, 'Stop!' This topic is beyond the realm of evolutionary understanding. Or even for saying, as the British Broadcasting Corporation has to do in relation to politics, 50% of your space has been allocated to Darwinian ideas, in the interests of balance the remainder of your paper should present the postprocessual (or any other) view.

When we turn to Kristiansen's treatment of evolutionary approaches to cultural transmission, especially his note 2, I think there are some confusions,[1] as well as criticisms of specific evolutionary views that I do not myself share. Like postprocessualism and Anglicanism, evolutionary approaches to understanding human action represent a broad church that accommodates many different, sometimes mutually incompatible, views. This was very clearly seen in the debate between the evolutionary ecologists Boone and Smith (1998) and the so-called evolutionary (*sensu stricto*), or selectionist, archaeologists Lyman and O'Brien (1998). This selectionist view, as originally defined by Dunnell (1978), does indeed insist, or at least did insist, that artefacts are part of the human phenotype, that their prevalence or otherwise through time depends either on their impact on human survival and reproductive success (i.e. natural selection) or on processes of random drift, and that human intentions are irrelevant to the process. This view is rejected by most evolutionary thinkers, including myself, and is the position (confusedly) addressed in Kristiansen's note 2.[2]

The contrasting dual inheritance view (Boyd and Richerson 1985; cf. Cavalli-Sforza and Feldman 1981) is that cultural traditions can usefully be considered from a Darwinian viewpoint because they embody the processes required for evolution in a Darwinian sense to take place: cultural traditions are inherited through social learning, innovation generates novelty, and various processes act on existing and novel variation, affecting what is passed on into the future. Some may wish to see that as a metaphor. That is fine by me. The role of analogy and metaphors in scientific reasoning is well established (e.g. Wylie 1985). In my view, however, cultural and genetic evolution are

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better seen as different instantiations of a more general Darwinian process, defined in terms of the generation, inheritance and winnowing of variation (there are other instantiations, e.g. the immune system or the process of brain development (Edelman 1992)). The main point, though, is to be specific about the mechanisms relevant to particular versions. This requirement has been met directly by authors such as Boyd and Richerson and their colleagues, through their characterization and detailed mathematical analysis of processes such as conformist transmission (where people tend to adopt the most common version of a practice in their vicinity – when in Rome do as the Romans) and their implications (e.g. Boyd and Richerson 1985, 204–40; Henrich and Boyd 1998; 2001). The fact that we acquire our cultural traditions from a variety of different people and can modify them more than once a generation means that cultural patterns often do not coincide with genetic ones, that they change more quickly (presumably at least part of the reason why culture became important in the first place) and that sometimes they can be propagated despite having reproductively maladaptive consequences (see Boyd and Richerson 1985, 178–88). Kristiansen is right when he states that an evolutionary approach to such concepts as tradition, innovation and diffusion requires a new look at them, but this has been actively under way for 20 years and is producing new insights not just into the implications of conformism and the possibility of maladaptation but also into the innovation diffusion process (Henrich 2001) and many others (see now Richerson and Boyd, in press).

One specific issue that has attracted the attention of those studying culture from an evolutionary perspective is whether there exist core cultural traditions that persist through time affecting many different aspects of life, or whether specific cultures are better seen as being made up of 'multiple packages' with different histories relating to different cultural practices. This is, in fact, the diachronic version of the issues I addressed in their spatial dimension in a paper (Shennan 1978) to which Kristiansen refers positively. There is a range of factors that may lead to cultural elements being passed on through time together – from functional interdependencies through their all being learned from the same people to their being linked together as part of a common symbolic universe. In fact, contrary to Kristiansen's claim that an evolutionary approach ignores symbolism, I described Ortman's (2000) work on textiles as a metaphor for pottery as precisely an example of symbolic considerations arising from prehistoric Pueblo cosmology leading to certain decorative possibilities being considered illegitimate because they did not fit in with a set of core beliefs (Shennan 2002).

One useful tool in addressing these issues is the application of phylogenetic methods originating in biology to the identification of the relationships between cultural traditions. Contrary to Kristiansen's claim in his note 2, this does not embody any presumptions about the mechanisms that produce the patterns; still less does it assume the operation of natural selection on culture. Tree-building phylogenetic methods start from the same assumption as historical linguistics: that entities evolve by a process of splitting, arising from the fact that, in the case of languages, once speech communities split, innovations occurring in one community will not occur in the other, and

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that as the length of time since the split increases the number of independent innovations which distinguish the two communities will grow (cf. e.g. Ross 1997). The methods also provide measures of the strength of the 'tree signal' in the data being analysed; they do not automatically lead to the conclusion that the history of the evolution of a particular cultural tradition has been based on tree-like splitting. Tehrani and Collard's (2002) analysis of Iranian carpet-making traditions concluded that they did seem to have evolved largely through a process of splitting rather than blending between the traditions of different groups, but the fact that the tree signal was stronger in the earlier period than the later one was historically informative about the effect of Russian military intervention on the evolution of these traditions. In fact, these phylogenetic methods, which are themselves evolving (see e.g. Mace, Holden and Shennan, in press), provide means of addressing long-standing culture-historical issues like the cores-versus-packages question described above; if different sets of cultural attributes have different histories they will be characterized by incompatible trees, quite possibly with different degrees of 'treeness'.

Following through Kristiansen's argument, we come now to the issue he raises concerning the appropriate level of detail for the development of productive interpretations and explanations of archaeological phenomena, and to his question: what is left of a Darwinian approach if one accepts, as I do, that costs and benefits are determined by context. The important point to make here is that this is the case throughout evolutionary biology. The theory of evolution by natural selection provides a framework for analysis, including a set of expectations that one brings to the analysis of particular case studies, whether these concern bird feeding behaviour, the infanticide carried out by male lions when they first take over a pride, or the decisions made by foragers on whether or not to start cultivating maize (Barlow 2002); specific costs, benefits and constraints must be identified and their implications thought through in the light of a very well-founded set of theoretical principles. The application will be productive if it directs us to previously unconsidered aspects of the situation in question and not only establishes whether our initial expectations are met or not but also explains why. Unlike the evolutionary approach, which has an explicit theory of human behaviour, the other frameworks discussed by Kristiansen all make assumptions about the way humans behave in certain circumstances, but they are rarely if ever explicit about those assumptions.

In this light it is interesting to examine Kristiansen's dismissal of the theory of sexual selection as a framework for explaining variation in male and female actions and interests. Whatever the merits or otherwise of my own efforts in this direction, it is certainly the case that there is a body of detailed empirical work that points to the productiveness of this perspective, such as Boone's (1986) analysis of the marriage decisions of the 15th-century Portugese nobility or Voland's (1995) examination of the same issues among 18th-century Frisian farmers. However, the example I wish to look at in some detail is the work of Holden and colleagues on the evolution of patriliny and its relation to the spread of cattle-keeping in Bantu Africa (Holden and Mace 2003; Holden, Sear and Mace 2003).

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The first point to which attention should be drawn is that the co-occurrence of patriliny and cattle-keeping in a number of different Bantu groups could simply be a result of the fact that these groups are descended from a common ancestor that was patrilineal and kept cattle. In this case the connection would be, in effect, a historical accident; there would be no particular reason to attach any special significance to it. To establish whether there is some significant causal relationship between the two it is necessary to exclude this possibility, and the phylogenetic methods referred to above provide the basis for doing this; once an evolutionary tree has been constructed, in this case of the relationships between Bantu languages (Holden 2002), statistical methods can be used to investigate whether there is a 'real' relationship between the two phenomena. This is the comparative method as used in biology and it has provided a new and more rigorous basis for cross-cultural comparison in the social sciences (Harvey and Pagel 1991; Mace and Pagel 1994). In the present case it was possible to show that there were several independent co-evolutions of the practice of patriliny and cattle-keeping, and to support the hypothesis that acquiring cattle led previously matrilineal Bantu language-speaking groups to change to patrilineal or mixed descent (Holden and Mace 2003).

The explanation for this lies in the reproductive benefits associated with patrilineal descent of wealth in the context of cattle-keeping, especially in societies that are polygynous rather than monogamous, where the reproductive benefits to sons outweigh paternity uncertainty (cf. Morgan 1877, cited in Holden, Sear and Mace 2003, for an early identification of the importance of this relationship). If the benefit of wealth to sons divided by the benefit of wealth to daughters is greater than the inverse of paternity certainty then it is adaptive to pass wealth to sons (Holden and Mace 2003, 2429–30). In the case of societies where cattle are used for bridewealth, large herds may allow men to obtain several wives and thus increase their probable reproductive success. Holden, Sear and Mace's (2003) study of the effects of wealth on the fertility of men and women among the patrilineal camel-herding Gabbra of northern Kenya and the matrilineal horticultural Chewa of Malawi found that among the Gabbra the benefit of wealth in animals to sons was nearly three times higher than that to daughters, so that it would take a very low paternity certainty (less than 0.36) to justify female wealth inheritance, while among the Chewa the benefit of wealth in land to sons was only just higher than it was to daughters, so that it only needs a paternity certainty lower than 0.94 to favour daughter-biased investment. I would suggest that issues of this sort are very relevant to understanding Bronze Age Europe and that it would be extremely interesting to look at the kinship strategies discussed by Rowlands (1980), to which Kristiansen refers, in this light.

At this point it is necessary to clear up a misconception on Kristiansen's part when he says that directionality is not thought to be at work in (non-human) biological evolution, and contrasts this with 'multiple intentional choices [that] add up to long-term effects that consequently can only be considered directional, even when beyond the experience of individuals' (p. 92). But, of course, the same is true of multiple unconscious 'choices', or 'choices' based on animal intentionality, whose success or otherwise results ultimately from

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natural selection. One example is the process known as 'niche construction' (Laland, Odling-Smee and Feldman 2000), the alteration of the environment (in the broadest sense) by animal and plant activity that changes the selection conditions for future generations. The early Earth would have been poisonous for oxygen-breathing animals. It has become a viable environment for them as a result of millions of years of prior evolution that have changed the initial environment out of all recognition. This is not the only type of directionality that has characterized non-human biological evolution, as Maynard-Smith and Szathmary's (1995) account of what they call 'the major transitions' clearly demonstrates; the appearance of multicellular animals, for example, was a qualitative novelty which meant that the history of life on Earth was never the same again. This does not imply that there is any teleology in biological evolution any more than there is in social evolution, nor does it take away the fact that day-to-day life is relatively directionless (cf. the long-standing distinction between general and specific evolution used to contrast the positions of Leslie White and Julian Steward).

In conclusion, I would endorse Kristiansen's challenge for the future of archaeology – to explore the dynamic relation between micro- and macro-histories and construct a new culture history – but I do not have any confidence that we will find the tools to do so in a relatively atheoretical discipline like history or in social anthropology; surely the latter, if any, is a discipline that has lost its way (cf. Gamble 2004). The point about the evolutionary approach is not that it is completely novel; on the contrary, it provides a theoretical justification for a lot of the things that archaeologists have always done, which make sense from an evolutionary point of view. If this was all it had to offer then archaeologists could continue as they always have done, secure in the knowledge that they have got things right, albeit now founded on an explicit theory of human behaviour. In my view, however, whether it is the new insights into culture history obtained by applying phylogenetic methods (e.g. Tehrani and Collard 2002), the new understanding of human social institutions that is arising from evolutionary game theory (e.g. Bowles 2004), or the implications of looking at pastoralism in the light of sexual selection theory, discussed above, there is more than enough productive insight already to justify pursuing evolutionary approaches to archaeology in whatever direction they may lead, not irresponsibly but without accepting any limitations.

Acknowledgements

I would like to thank Mark Collard for helpful comments on an earlier draft.

Notes

[1] This is also true of his note 1. Virtually all American departments where anthropological (i.e. non-Classical) archaeology is practised are departments of anthropology in which all the different fields of anthropology are represented, including biological anthropology and archaeology. This has always been the case and the situation has not changed. Similarly, for many years now many British archaeology departments have included a variety of scientific specialists, including specialists in various aspects of biological anthropology. A

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five-year grant from the UK Arts and Humanities Research Board enabled the Institute of Archaeology and the Department of Anthropology at UCL and the Department of Archaeology at Southampton to set up the AHRB Centre for the Evolutionary Analysis of Cultural Behaviour in 2001, directed by myself, with a number of Ph.D. students and post-doctoral researchers. Its work is ongoing (see www.ucl.ac.uk/ceacb/). There are no comparable centres in the United States, although there are of course concentrations of people with evolutionary interests in some anthropology departments. The recently founded Leverhulme Centre for Human Evolutionary Studies in Cambridge is based in a biological anthropology department and its work tends to be more orientated towards the earlier periods of prehistory where Kristiansen thinks evolutionary approaches should stay.

[2] Although it is irresistible to point out that Kristiansen's ironically meant statement at the end of his note 2, 'If one believes in O'Brien's application of natural selection to culture then the theoretical approach with less replicative success should vanish over time' corresponds to Thomas Kuhn's (1962) view of how scientific paradigm shifts occur (see Hull 1988 for a detailed account of an evolutionary approach to the history of science).

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Discourse and dialogue. Some observations and further reflections

Kristian Kristiansen

Is it possible to enter a dialogue across discursive boundaries? Some comments suggest that agents engaged in defining the discourse, or some section of it, tend to guard the gates of their discourse against hostile intrusions (critique). They therefore do not engage in dialogue. The comments by John Robb provide an example of the power of discursive constraints, while Stephen Shennan is willing to open the gates enough for a good discussion. Ulrich Veit provides a critical overview that situates my critique within a wider postprocessual/postmodern framework.

General background to the article

Let me briefly situate my article and its aims as a prologue to discussion.

The article 'Genes versus agents' is an attempt to raise a discussion about a number of current theoretical problems in archaeology as seen through the eyes of an increasingly senior practitioner. I do not stand outside the discourse, of course, but my theoretical stance has moved towards a more historical approach in recent years, as will become apparent in a forthcoming book (Kristiansen and Larsson 2005). The research leading up to this work has confronted me with the role and meaning of establishing 'heroic fame' through individual deeds (travels, combat and so on) in the Bronze Age. I found very little I could use in the current discourse on agency, but I found a lot I could use from ethnohistory and ancient history. Thus in the end I had to construct a new theoretical framework in collaboration with my colleague Thomas Larsson to account for the kind of historical processes operating in the Bronze Age at a more concrete historical level. In short, we had to situate

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and define our interpretative concepts within a historical context, in a dialogue with the more general concepts from which they originated. This experience has also made it clear that historical regularities are just that – historical. They cannot be constructed through some ahistorical process of structural comparison of isolated time/space slices of evidence – sometimes called cross-cultural analysis – without violating fundamental theoretical principles of historical explanation, which demands a larger time/space perspective. Hence my scepticism with regard to certain types of evolutionary theory. Thus the reintegration of social anthropology with archaeology and history demands a theoretical and interpretative reformulation of their historical relationship (Gosden 1999; Rowlands 2004).

This experience has made it clear to me that many problems and flaws in current archaeological thinking stem from theoretical and philosophical borrowing without adequate adjustments. Theoretical discussions tend to stay at a general level, whereas archaeological interpretations often are highly localized and contextualized. This theoretical and interpretative divide was most clearly exemplified in Julian Thomas's classic book *Time, culture and identity* (1996). A theoretical bridge of interpretative concepts relevant to the archaeological contexts was simply missing – as in most other theoretical applications to empirical case studies. Consequently archaeological theories often lack direction and realism as to their interpretative coverage. It has left the interpretative field too open to speculation and manipulation for my taste. The many different approaches to agency and evolution notwithstanding, I consider this a valid critique. What I am missing in much recent postprocessual works are clear theoretical and methodological guidelines defining interpretative relevance and coverage within the historical context being studied. With some illuminating exceptions, such as Bradley's and Tilley's more recent works (Bradley 2002; Tilley 1999) or Hodder's application of the concepts of 'domus' and 'agrios' (Hodder 1990), too little theoretical labour is invested in developing contextualized interpretative concepts. I have therefore advocated a more historically informed approach to theory-building, just as I have proposed a new 'ladder of inference' that defines present theoretical approaches by the interpretative realms they occupy. By this I hoped to invite a discussion of a more holistic approach to theory-building, a new kind of theoretical hybridity to replace the prevailing pluralism with its lack of theoretical integration. This scope of the article was not understood or commented upon by John Robb.

To be challenged or not – that is the question

John Robb does not want to be challenged and therefore he abstains from debate. His comments follow the boringly predictable formula of being misrepresented. Hence nothing or very little can be attributed a positive value worth discussion. Instead the misguided reviewer of his book, and the readers, are taught what agency means to John Robb in academic discourse. His statement, that 'we cannot speak meaningfully of agency as an abstract, raw or pure noun, but only of specific, contextualized or situated agencies' (p. 106) is nearly similar to my own conclusion. His personal view, however, reveals his reliance on Giddens rather than Bourdieu or Gell. As such he

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remains within the discourse of modernity. My own preference is rather with Pierre Bourdieu's practice theory than with Anthony Giddens's agency and structuration theory. And the reason is simple: Bourdieu situates his theoretical reflections on actors in concrete case studies of traditional society. Actors are here understood by how they are situated within fields of forces or social actions (symbolic, economic and cultural capital), which makes his concepts both more historical and generally more comprehensible, although his writing is sometimes incomprehensible (Bourdieu 1977; 1990). Giddens, on the contrary, writes in an easily digestible manner, and is therefore more popular among archaeologists (especially Giddens 1984). My problem with Giddens is that I find he theorizes trivial routines of agents in the postmodern society without ever getting beyond it. There is a genuine lack of empirical tension, he remains abstract and implicitly modern, he is studying modernity and how it emerged (summarized in Giddens 1990). There is therefore no point in archaeology projecting Giddens back into the past, since you cannot see what was different and how it contrasts with the present. His theoretical project is moreover driven by an ideological agenda to add liberalism to labour politics. In this respect, he is not unlike Althusser, who was driven by a communist vision of the totalitarian role of theory after the revolution and who was unmasked early on by E.P. Thompson, before Althusser later unmasked himself (Thompson 1978). For these reasons, I have always preferred Maurice Godelier, Jonathan Friedman and Mike Rowlands among the structural Marxists, as their theoretical work has always been grounded in and counterbalanced by empirical studies.

In their closed theoretical laboratories, Althusser and Giddens were able to construct a new theoretical order that did not have to contend with empirical resistance. Their theoretical constructs were fed by the political/ideological agenda of their day rather than by empirical studies. It is from these examples that my scepticism against employing purely theoretical thinkers originates, because they are too much part of a modern ideological or political project. There is of course nothing wrong with a political agenda, but the study of the past should provide independent cases from which such theoretical agendas can be critically discussed. Gordon Childe provides an exemplary role model here (Rowlands 1996). Agency without a grounded theoretical and empirical anchor in past conditions is subject to the same kind of modern manipulation.

The critical points I raised about agency, however, remain without an answer (as do the questions I posed). My point is that no matter how many approaches there are to agency, they share the problem that the concept remains too general to be useful without additional theoretical labour. Agency, like all general theoretical concepts, needs to enter into a relationship with other concepts to gain meaning and direction, and such meanings are context-dependent. It is in the lack of contextual theorizing that most studies fail. As I cannot think of any good archaeological examples that develop and define agency in a relational, contextualized way (but see Normark 2004), I had to employ an example from anthropology. In my argument, Gell's book is not primarily about giving agency to things but about how to establish a proper theoretical and interpretative framework that links material culture and human agency in a specific historical context.

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Thus Gell's book provides an interesting theoretical example of how to conceptualize materiality, an approach taken up recently by Knappett and Normark (Knappett 2002; Normark 2004). Gell takes Bourdieu's argument further by saying that persons and things share the same fields – i.e. artefacts and nature have (secondary) agency and can be included in these fields of force. Gell calls this abduction and I think this has a very relevant application to archaeological/material culture studies. But apparently this has no appeal to John Robb, whose example is little more than rational choice theory where a competitive self is objectified and only constrained by what others might do to you. Instead he takes offence by my presumptuous use of the book review as a straw man for developing another theoretical agenda, so the book does not receive a qualified in-depth discussion. However, I never pretended to perform an in-depth discussion about the various approaches to agency, as Dobres and Robb had already done that in their introductory article. In that sense the book reviews can be said to act as a straw man for a wider discussion, which is clearly stated as the intention. What I hoped to achieve was simply to point to a number of theoretical problems and interpretative flaws in the ways archaeologists have employed the concept. John Robb chooses the easy way out by pretending that the diversity of approaches to agency is prohibitive to such a discussion. I contend that a general understanding of agency as presented by John Robb in his comment may serve as an undergraduate lecture on the subject, but has no interpretative relevance when confronted with archaeological material.

In 2000 John Robb, together with Marcia-Anne Dobres, wrote an introduction called 'Agency in archaeology' to a book with the same title. A lucid and informative article, where they state that the 'goal of this book is to create a dialogue among archaeologists interested in agency, archaeologists critiquing it, and archaeologists for whom the jury is still out' (Dobres and Robb 2000, 4). Memory can be sometimes astonishingly short.

Stephen Shennan, on the contrary, accepts the challenge and provides a good platform for debate that clarifies a number of the questions and critiques I raised (some of which comforted me), and provides a new perspective on others (some of which persuaded me). Shennan elegantly develops a rhetorical counterpart of my critique that paints me as a dogmatic controlling commissar who thinks that evolutionary theory should know its place, and stop expanding its domain. But what I really propose is that it should explore its new domains and argue better for them than Shennan does in the book, by providing a discussion of how they interact with other interpretative domains. After all, he should be in the best of positions to do that, as he is one of the rather few archaeologists who has changed his basic theoretical framework, from one of soft structural Marxism (at least we once shared quite a number of interpretations on later European prehistory) to one of Darwinian evolution. And while I am aware of the institutional role of biological anthropology, what is new is its expansion into later prehistory, and the potentials and tensions it provides.

On agency, Shennan contends that human intentionality and agency are guided, at least in part, by rational choice linked to our genetic heritage. I agree, at least in part, with this observation, and like Shennan I find

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explanatory power to be located in a relational, contextualized field of human, social interaction. However, waters divide when it comes to the role between genes, agency and human propensity. Shennan states, 'To the extent that specific propensities that lead to greater success . . . have a genetic basis, they will tend to spread through the population' (p. 108). Two dubious propositions are linked together here, namely that certain propensities have a genetic basis, and that it is this genetic basis rather than tradition and social interaction which is responsible for its success. But later Shennan states, 'Equally, however, such practices, and indeed many others, can spread as a result of of social learning' (p. 108). While I have sympathy with Shennan's openness to a series of related and unrelated explanatory factors, I find his argument of a genetic disposition and transmission of successful behaviour in some contradiction with his otherwise critical approach to artefacts and human phenotypes.

In his comments Shennan now makes an interesting and sophisticated attempt to provide examples of the explanatory role of evolutionary theory. He argues for the usefulness of the phylogenetic approach to describe cultural traditions. To begin with we share an interest in core cultural traditions versus multiple cultural packages or practices. The problem begins when diachronic variation in material culture is analysed and explained by reference to phylogenetic theory. Although I accept that it does not embody any presumptions about the mechanisms that produce the patterns (but why, then, employ the concept?), I am still not convinced that this is a theoretically valid procedure. The principle of splitting, for once, implies a principle of continuity, otherwise there would be no tree. My first critical concern is with the inherent tendency to look for splits and continuities in the archaeological material that may not always be there. My second critical concern is that the method separates analytical and explanatory or interpretative procedures. As a formal analogy I can recognize its heuristic value, as a historical explanation I cannot. It is true that typology embodies a certain regularity of gradual change, often referred to as genetic. This, however, was always a rhetorical analogy – a metaphor. I discussed this problematic with some Bronze Age examples many years ago and demonstrated that variations in typological change – innovation versus tradition – could always be linked to social and economic conditions (Kristiansen 1985).

Shennan bridges his argument with reference to historical linguistics. Although this may be a more suitable analogy, it implies a whole new discussion about the relationship between language and culture. But it also opens up a more productive, comparative approach based on the principles of historical linguistics (Kirch and Green 2001). In Eurasia all Indo-European languages can be demonstrated to share a common history and origin (although debated), and they retained continuity over millennia (Lehman 1992; 1993). Both internal change and splitting occurred. Religion and comparative mythology share with language a history of *longue durée* in several regions in the world, including splitting and local developments over time. Indo-European religion provides a case in point (Puhvel 1987).

Archaeological cultures during the same long time period, however, do not share these characteristics, except in certain specific historical situations where migrations or trade and travels transmitted a cultural package to

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wider regions, from where it developed local trajectories. In Europe one can mention the Linear Bandkeramik and the spread of agriculture, or the so-called Kurgan, Corded Ware and Battle-Axe cultures of the 3rd millennium B.C. But even here the evidence does not conform to a single, linear model (Gkiasta *et al.* 2003; Thomas 2003). Material culture does not normally exhibit millennia-long traditions with tree-like splits. To me the evidence suggests that different rhythms and dynamics characterize different realms of social and cultural change – from language, physical type (DNA) over religion to social institutions and material culture. The latter is probably the least suited to a study of phylogenetic change. Quite evidently we are in need of an integrated study of the long-term dynamics of all of these historical processes. How are we to explain the contrasting processes of convergence and divergence between language, religion, institutions and material culture in later European prehistory and early history?

Patrick Kirch and Roger Green have recently carried out a comparative 'model study' for ancestral Polynesia (Kirch and Green 2001). They have applied a phylogenetic model to integrate all the various strands of evidence from linguistics and physical type to religion, social institutions and material culture. They were able to persuasively demonstrate a phylogenetic history of ancient Polynesia. Precisely because Polynesia consists of a series of discrete units derived from a common ancestor, I suspect it represents the only clear-cut example in the world where the evidence marches in step with a phylogenetic model.

In European prehistory, long-lived social traditions of one thousand years may occur, but so may distinct discontinuities when they are terminated. Within such traditions certain elements may remain stable while others change, but in no obvious tree-like manner. In accordance with this Shennan states, 'if different sets of cultural attributes have different histories they will be characterized by incompatible trees, quite possibly with different degrees of "treeness"' (p. 111). I consider such variations to be historically constituted, and linked to the reproduction of social and economic institutions and their accompanying traditions.

The question now becomes one of whether it is theoretically justified to apply both a social-economic and a phylogenetic explanation to the same phenomena – can both be true at the same time, while referring to different principles of explanation and regularity? Does phylogenetic theory allow the principle to be at work in certain cases and not in others, as would often be the case in archaeology? I remain sceptical of a narrow use of the phylogenetic model to analyse typological variations in material culture. In my historical understanding of material culture genetic trees have no explanatory power. There has to be congruence between form and content, and phylogenetic theory does not fulfil those criteria when applied strictly to the study of material culture. However, I can see the relevance of a general, comparative phylogenetic model as in the case of ancient Polynesia, and such an approach might also prove productive in other regions, even if the results would be more complex and contradictory.

Shennan's other example takes up the question of the relationship between kinship systems and reproductive success. This is a more clear-cut case, because we agree on the relevant aspects relating kinship to wealth and

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reproduction. So the question becomes one of explanatory priority. Is reproductive fitness the cause or the effect, or is power the cause or the effect? That men control cattle as bridewealth leads to control over marriage and women in order to access production of children. But is access to women to reproduce children the primary reason for power? Ostensibly yes, as large numbers of children were being produced by a limited number of titled men and others were not allowed to marry or have children. However, cattle had strong symbolic and cosmological value as well as economic, which was demonstrated when a massive slaughter of cattle was carried out as part of revolts against the Europeans when they arrived. It was, in part at least, a symbolic revolt, that was thought to destroy the British ability to rule. So it can be argued that power over people is based on power over cosmology, elite exchanges and ritual power – that this forms the field of forces that shaped people's actions. Which once again takes us back to a historically situated, relational and intersubjective discussion of agency. My preference is for the latter form of explanation, but I cannot deny that reproductive fitness can be said to be an outcome of the process. However, such a biological perspective remains too narrow to fill my interpretative horizon, even though I will not deny its relevance.

Divided we stand? Practice and discourse

Ulrich Veit directs his critique right at the central issue – any proposition for a more integrated theoretical dialogue is subject to a certain perspective – and deems the venture an impossible one. My proposal is characterized as having a processual leaning. While I recognize this inherent constraint – and readily accept my discursive origins (although a few postprocessual leanings are also discernible) – I do believe that an analysis of interpretative domains is central to identifying areas of converging and diverging theoretical interests. It is true that both processual and postprocessual approaches claim total interpretative coverage, but empirical analysis – I suggest – will demonstrate that this is not the case. Hence I believe we need to develop research analysis as a helping tool in theoretical discussions, as it provides us with some empirical flesh and blood (some examples in Kristiansen 2001b). Postprocessualists talk about economy, yes, but in distinctively different ways than processual archaeologists, where it figures more prominently and with a different explanatory emphasis. So the issue is not to develop a new integrated archaeological theory, but rather to develop a discussion about interpretative strategies and boundaries. It is part of an ongoing process of breaching the many dividing forces of the discipline. In addition, an empirically better-founded characterization of the various approaches will raise the overall level of theoretical debate and allow different positions to be spelled out with greater clarity.

Academic and organizational divisions in archaeology are linked to its expanding role in society and are here to stay. Among the earliest recognized and scrutinized was that between rescue archaeology or cultural resource management (CRM) and academic archaeology. Colin Renfrew pointed to the problems of an emerging sector of CRM in the United States in a classic article in 1983 entitled 'Divided we stand'. Most recently Björnar Olsen

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reused the title, and added a question mark, when discussing theoretical developments in Scandinavian archaeology (Olsen 2002). In the article he explores the narrative plots of Scandinavian archaeology, following Hayden White (1987). Such plots are derived from the deeper structures of discourse. Whether one prefer Bourdieu's analysis of the academic environment in *Homo academicus* (1984), Thomas Kuhn's work on research paradigms (1962) or Gadamer's work on hermeneutics (1965), they all demonstrate the socially embedded nature of research environments and their interpretative practice. And they consequently all raise the question – with different emphases – how do we cross over from one conceptual circle of understanding and practice to another, whether in the present (between paradigms), or the past (between present and past)? The answers given by Kuhn and Bourdieu are that the power of discourse and the paradigm is very strong, and that it prevents one in reality from siding with more than one at the same time.

These widely shared academic experiences have a number of implications, and they grant the sociological practice of academic environments a leading role. Any attempt to create integration and break down existing barriers has to start with a formulation of a strategy of practice. The swift rise to dominance of a theoretical archaeology in Great Britain and its continued development may in no small measure be due to the integrating role of the annual TAG (Theoretical Archaeology Group) conferences taking place since 1979. Offshoots of this strategy soon emerged in Scandinavia (Nordic TAG since 1985) and in Germany. At the European level the formation of the EAA (European Association of Archaeologists) and the *European journal of archaeology* is likewise an attempt to break down national and political research barriers, as well as barriers between heritage management and academic archaeology. Bringing people together from different environments has the power to create a new practice, which in turn may result in new theoretical agendas.

If one accepts the above, it implies that humanistic discourses at a general level are grounded in present-day interests. It is therefore questions rather than answers that matter in the first place. They tend to shift the emphasis from the nature of truth to the nature of the world and how to understand it. In short, it grants pre-understanding a leading role. Pre-understanding therefore becomes a focus of theoretical and philosophical debate. Theoretical frameworks for posing or answering questions are therefore dependent on the dominant 'ideological climate', as it were, in the world. So far this has been a Western domain rooted in antiquity, revived during the Renaissance and reformulated during the Enlightenment period. Only recently has this humanistic project been questioned in archaeology and criticized by postcolonial, antihumanistic theories for being a hegemonic eurocentric Western project (Gosden 2001; Thomas 2004a).

One's position as a human being, humanist and researcher within the ideological and ontological cycles of modernity will therefore have a significant impact upon one's choice of theoretical framework:

1 You are born and educated into a discourse, rather than choosing it. It is part of the maturing process one must go through as a social and academic being, and the two cannot be separated.

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2 Having passed this maturity process and attained a place of one's own within the discourse, it takes a highly developed social and academic consciousness to change one's position. In archaeology, I can think of Gordon Childe, who began his career in the culture-historical discourse of Kossina, and ended it in the social evolutionary discourse that rose to dominance after the Second World War. Ian Hodder started as a processual archaeologist in the David Clarke geographical tradition and changed into a postprocessualist. Colin Renfrew seems to have taken the opposite route, beginning with social evolution and ending with language and genes, although he did not change his processual framework.
3 Politically and economically backward regions, or academic disciplines without political or ideological importance, may stick to an earlier discourse while discourses are changing in the centres of political and economic development. This explains the notion of 'being out of time', and is logically consistent with the proposition of the dominance of political ideologies for humanistic discourse. Examples would be the Soviet Union and part of communist Europe before 1990, and archaeology in Germany and France until the 1990s.
4 Principles of what constitutes knowledge differ between the Rationalist and Romantic discourses. Rationalism is linked to positivism and its analytical rules of conduct, while Romanticism is linked to hermeneutics and their emphasis on understanding. In its postmodern version it also criticizes the humanistic project of Western Enlightenment.

I consider these observations to be nearly self-evident and they can easily be confirmed by empirical observation (Bourdieu 1984; 1996). From this I draw two conclusions:

1 To overcome the constraints of academic discourse and its pessimistic perspective on knowledge one has to develop strong theoretical foundations for the production of historical and archaeological knowledge that are closely linked to the nature of their empirical data. This is what Hayden White did in his work on the historical narrative as a specific form of representation of historical data, which also included a critique of the philosophical undermining of the humanistic project (White 1987; discussion in Last 1995). As a philosophical critique it was forcefully forwarded by Habermas at the same time (1987). In order to unwrap the historical disciplines from total embrace in discursive political ideologies one therefore has to recognize this condition as well as to develop a specific theoretical framework for the production of historical and archaeological knowledge. It implies a theoretical distinction between the production of knowledge as an interpretative or analytical procedure and as a social or ideological practice. In line with this, we have seen a series of books that take up this challenge. Ian Hodder's *The archaeological process* (1999) attempts to develop a reflexive approach embedded in the routines of practice – excavation, documentation, science and so on – and their transformation into historical knowledge (see also Criado Boado 2001; Jones 2002).

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2 We should take the separation between politics and academia much more seriously than it has been in recent times. Political control is creeping inside universities under the banner of management control and the need to conform to the needs of society in a more direct manner. However, academic freedom remains a principal, universal Western value that is at the root of Western progress. The fact that academic life and research interests are linked to interests in the present only sharpens the need to maintain a free and critical academic environment. A contextualized definition of universals cannot and should not be avoided. However, my comment on universals is not so much linked to a theory of human universals as to ethics, which is becoming a growing issue in archaeological practice around the world. Because of the politicized nature of archaeological practice in many regions, I have increasingly come to the realization that we need to distinguish more carefully between our roles as researchers and as activists. Without reference to certain principles of ethics we have no weapons against political misuse as there exists no non-ideological way of distinguishing between good and bad use of the past. I have developed these issues in a recent article, which is just one among many contributions to an increasing field of discussion (Kristiansen 2004b; also Zimmerman, Vitelli and Hollowell-Zimmer 2003; Karlsson 2004).

From pluralism to hybridity

So were does all this take us? For my own part, the comments have made it clear that no text can be read without a pre-understanding. I have perhaps failed in providing enough background about my own changing or expanding theoretical position and its role for the critical concerns raised in the article. My main concern has been to direct attention towards the need for a more rounded theoretical discourse that covers all types of human activity and history. I am not proposing one overarching theoretical framework; on the contrary, I am suggesting many overlapping approaches, whose interpretative potential is tried out in multiple hybrid ways – in short a movement from a diverging theoretical pluralism to an engaging, converging theoretical hybridity.

Finally, I wish to thank the commentators for their contributions to a debate which, I believe, is only beginning to take shape. Perhaps I can excuse myself for being unclear by my divided interests, being torn apart between the theoretical framework of structural Marxism and world system theory on the one hand and, on the other, my quest for understanding specific historical processes as they unfolded within such a world view. For me it is part of a continuous endeavour to reach the unreachable; that is, to be able to influence history through insight into it and communication of that insight – a historical existentialism.

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