A DIALOGUE

EVOLUTIONARY AND INTERPRETIVE ARCHAEOLOGIES

EDITORS

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EVOLUTIONARY AND INTERPRETIVE ARCHAEOLOGIES

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A DIALOGUE

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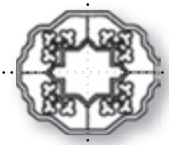

This volume originated in a seminar series held at the Institute of Archaeology, University College London, organised by the editors in the spring term of 2007. In each seminar over 10 weeks, a pair of speakers presented interpretive and evolutionary perspectives on a particular problem or theme, followed by an extended discussion. This was an effective format for seminar debate, repeated in a much shorter time at the Theoretical Archaeology Group annual meeting in York in December of 2007. In the book, we have asked our authors to flesh out their contributions with more case studies and more individual engagement with the contrasting viewpoint. This should enable the chapters to stand alone as contributions to a wider cross-disciplinary dialogue. Our thanks as editors go to the contributors to this volume, to the Institute of Archaeology Publications Committee and committee chairperson Professor Ruth Whitehouse, and to Mitch Allen and Left Coast Press, Inc., for their support of the book. We also thank the research groups (Social and Cultural Dynamics, Complex and Literate Societies, Material Culture and Data Science) and the AHRC CECD at the Institute of Archaeology that made the seminar series possible. Finally, we are grateful to the seminar participants (both as speakers and in the audience) for the critical discussion of ideas.

CHAPTER ONE

Evolutionary and Interpretive Archaeologies: A Dialogue

Andrew Gardner and Ethan E. Cochrane

Divergent Trends in Contemporary Archaeological Theory

One of the few areas of real debate in archaeological theory today is how to classify archaeological theory. Theoretical diversity has become a hallmark of archaeology, and opinions differ as to whether this range can be accommodated within a grander scheme of at least complementary approaches (Hegmon 2003, 2005; Renfrew 1994:10; cf. Renfrew and Bahn 2004:496–501; Tilley 1995) or whether fragmentation should be embraced (Hodder 2001:3–4, 2002, 2003; Moss 2005; VanPool and VanPool 2003a). Either way, the polarised debates over substantive differences in viewpoint that characterised the 1980s seem to have receded (even if these have become somewhat oversimplified as time has passed). Different groups of archaeologists go about their work with limited interaction (Hodder 2001:7; Johnson 1999:182–87). Whether one is in favour of or opposed to the notion of a unified discipline, this cannot be a healthy state of affairs for the intellectual vigour of the field. What is striking is that there has been almost no dialogue between, or even comparison of, two of the most innovative current schools of thought: interpretive and Darwinian archaeologies (notable exceptions including Kristiansen 2004; Mithen 1989; see below). While these are of course internally diverse, each represents the continued unfolding of long-established traditions that have engaged in constructive dialogue before, and surely must again if each is not to become a closed and sterile community.

Why have such divisions become the norm in contemporary archaeological theory? The debates of the 1980s and early 1990s certainly prompted some attempts to synthesise the objectives of processual and postprocessual archaeologies (Renfrew 1994; Schiffer 1988; Trigger 1991, 1998;

VanPool and VanPool 1999; cf. Hodder and Preucel 1996; Yoffee and Sherratt 1993), but the success of these is debateable. As the century has turned, it seems that many have been content to conduct 'business as usual' within their own approaches without seeking to win arguments that are perhaps regarded as unwinnable, or simply as tiresome (Hodder et al. 2008:38). The relationship between theoretical discussion and methods of data handling is also an issue here, with Johnson recently noting that core archaeological concepts remain largely immune to the more dynamic debates (Johnson 2006; cf. Johnson 2004), perhaps making the latter seem superfluous to many. Yet amid the seemingly placid landscape of archaeologists mixing and matching their theoretical viewpoints as they see fit, there lurks the danger that significant theoretical problems are not being worked through. The propensity of archaeological theorists to move overrapidly from one half-baked set of borrowed ideas to the next has been remarked upon frequently (e.g., Bentley and Maschner 2008:5–6; Chippindale 1993:33–35; Hodder 2002:77–78), and without sustained and constructive engagement between different perspectives this process will continue. Each iteration of the cycle leads to further fragmentation but can leave the gaps between approaches, where issues of real import lie, untouched.

It is our contention that nowhere is such engagement needed more than between Darwinian, or evolutionary, and interpretive perspectives. Some attempts have been made to compare and even synthesise elements of these programs (e.g., Kristiansen 2004; Mithen 1989; Shennan 2004; VanPool and VanPool 2003b), but these have tended to begin from a starting point firmly in one or other camp and have focussed on certain issues (particularly agency) at the expense of others. The real debate over the major points of difference, increasingly underway in the broader field of anthropology after a similar period of mutual disdain (e.g., Ingold 2004; Nettle 2009; Schultz 2009; cf. Fearn 2008), has yet to start—and it is hoped that this volume represents such a beginning. An important step in this debate is to move beyond the caricature and misunderstanding that has led to the dismissal of opposing views in the past (e.g., Leonard 2003:145; Shanks and Tilley 1992:53–56), and to seek to clarify where differences are primarily related to the specialised languages being used (cf. Bentley and Maschner 2008:5), and where they are related to fundamental matters of epistemology or of different understandings of the appropriate goals of archaeological research. Our goal is not necessarily agreement, but at the least better-informed disagreement. Furthermore, this kind of engagement is essential not simply to hone the arguments on each side, or to break down false barriers, but also to address a growing contradiction in the public face of archaeology. Both interpretive and evolutionary archaeologists have strong views on the public role of our discipline (e.g., Holtorf 2005:150–60; Shennan 2002:9–14),

and if the former have widened the debate on how, and from whom, archaeological stories should emerge, evolutionary accounts of the long-term development of human behaviour seem to be gaining in media and even political popularity (Newman 2009a, 2009b). The very different kinds of accounts of the past that archaeologists can produce in the public sphere—from extremely general to very particular—highlight some of the contrasts to be worked through.

How might these two schools be defined for the purposes of this volume? Neither is homogenous or uncontested. Up to a point, interpretive archaeology is to postprocessual archaeology what processual archaeology is to the New Archaeology—a maturation of a range of approaches with a broad set of common interests but divergent emphases. The degree of divergence is considerably greater than was the case with processual archaeology (Thomas 2000:1–2), largely because postprocessual archaeology has drawn upon a very wide range of influences—Marxism, feminism, structuralism, poststructuralism and phenomenology foremost amongst them. While there has been some resistance to grouping these diverse archaeologies together, they do have—in common with much cultural anthropology—shared interests in symbolism, meaning, power, identity and closely contextual interpretation, along with a degree of acceptance of relativist or constructivist epistemology (Shanks 2008; Shanks and Hodder 1995; cf. Thomas 2000). Superficially much more focused on the legacy of Darwin, and certainly with a much stronger degree of collaborative research coordination, evolutionary archaeology also has a range of subdivisions and disagreements. These are often classified into three major sets of ideas: Dual inheritance theory employs two distinct transmission (or inheritance) systems, cultural and biological, to explain human variation. Behavioural ecology explains human behavioural variation as a product of our tendency, conscious or not, for adaptive decision making. Evolutionary psychology understands contemporary human behavioural variation as a result of cognitive adaptations that occurred previously in our hominid evolution (Bentley et al. 2008:112–24; Hegmon 2003:214–26; Shennan 2002:15–18). By far, dual inheritance theory and behavioural ecology are the primary frameworks applied by evolutionary archaeologists. One key dimension of variation between these is whether Darwinian principles are considered in terms of the biological reproductive success of humans or rather as accounting for cultural change that is separate from, but interacts with, biological processes. While there are other sources of diversity (Mithen 1989; Schultz 2009), what tends to unite evolutionary approaches is not just Darwinian ideas of variation, transmission and selection, but a commitment to hypothesis testing and theory building relevant to behavioural and archaeological observations.

A Chequered History: The Origins and Development of the Divide

The internal diversity of these two schools of thought perhaps accounts for their dynamism as a product of theoretical debate within their respective boundaries. Crossing these boundaries might be even more productive, especially if engagement can build upon previous points of contact between antecedent approaches and at the same time overcome misunderstandings based upon old stereotypes. In tracing the origins of the relationship between evolutionary and interpretive archaeologies, we need to go back rather further than the obvious processual versus postprocessual debates and explore some connections and contrasts in the nineteenth century. Just as it is important to look into the context of Darwin's thought (Johnson, this volume), so is it salutary to look at the influence that Darwin had on other key thinkers of this period, many of whom read broad applicability into his conceptual framework. Karl Marx, for example, read *On the Origin of Species* in 1860 and likened its ideas to that of class struggle; he sent a copy of the first volume of *Capital* to Darwin in 1873 (and another to Herbert Spencer; Kamenka 1983:xxi, lxxx, xcvii; cf. Patterson 2003:14). Engels's speech at Marx's graveside compared the two thinkers: 'Just as Darwin discovered the law of development of organic nature, so Marx discovered the law of development of human history' (Kamenka 1983:69). The emphasis on the material struggle for survival was a clear point of contact, developed as Marxism progressed and integrated other forms of evolutionary theory (McGuire 2002:26). For others in this period, Darwin's chief influence was opening up a continuity between humans and the natural world and enabling both to be seen as dynamic and interactive rather than static; this was the case, for example, with Pragmatist thinkers like George Herbert Mead and John Dewey (Cohen 2000:85; Sandstrom et al. 2001:217).

Darwin's impact on nineteenth-century philosophy at a broad level was therefore quite profound (Collingwood 1946:129; Delanty 2000:30; Dunnell 1988; Rorty 1999:xx), and while succeeding developments in these and other traditions have hardly applied Darwinian principles to human culture in detail (see Dunbar 2007)—and indeed have criticised some attempts to do so (e.g., Callinicos 2004:xxxvii)—neither have they been afraid to acknowledge this impact. Indeed, Richard Rorty, a contemporary Pragmatist philosopher of some influence in postprocessual thought (Hodder 2003:5; e.g., Webmoor 2007), is clear about Darwin's significance (Rorty 1999:128), including him among the inspirational 'anti-Platonic, antiessentialist, historicizing, naturalizing writers of the last few centuries (people like Hegel, Darwin, Freud, Weber, Dewey and

Foucault)'. Neither interpretive nor evolutionary archaeologists seem to place him in the same company or recognise such connections (e.g., Leonard 2003:146–48; cf. Bintliff 2000:165). Rather than explore the tension between the philosophical implications of Darwinism and any more specific cultural applications, archaeologists have instead become mired in a century-long debate about Social Darwinism and the dangers thereof, which still colours many perceptions today. The first significant wave of evolutionary archaeology in the late nineteenth century was shaped more by Spencer, Morgan and Tylor than Darwin, and supplanted by the more particularist cultural history of the early twentieth century under the influence of Boas in the US and Montelius in Europe (Eriksen and Neilsen 2001:39–41; Leonard 2001:65–66; Trigger 2006:227–30). The second wave of post-WWII evolutionary anthropology, bound up in the New Archaeology, was more genuinely materialist—in a way not dissimilar to classical Marxism (McGuire 2002:89)—and it was primarily to this that postprocessual archaeologists reacted, as well as to the burgeoning manifestations of sociobiology and evolutionary psychology in the 1970s (Shanks and Tilley 1987:137–65; 1992:56). Contemporary Darwinian archaeology as explored in this volume is distinct from both of these earlier phases (Dunnell 1980), but still tends to be tarred with the brushes of determinism, reductionism and ethnocentrism (Leonard 2001:67–68). This is one of the chief obstacles to debate.

From the other side, there are also misconceptions to be overcome concerning interpretive traditions. Though influenced by Darwin, some of the nineteenth-century thinkers held dear by postprocessual archaeologists developed critical views on the problems of behaviourism that foreshadow more recent attacks on evolutionary approaches to culture (De Waal 2002:9–15; Joas 2001:89–90; Patterson 2003:14–15; cf. also Collingwood 1946:115, 129, 211–12, 332). Furthermore, subsequent developments have moved Marxism, for example, away from some of its evolutionary foundations. For the same reasons that postprocessualists rejected elements of evolutionary theory, they rejected classical Marxism's attempt to fit human cultural diversity within universal laws and favoured instead the neo-Marxist and structurationist emphasis on contingent contexts of praxis (e.g., Shanks and Tilley 1987:165–85). These reasons are complex, and they are not all to be ascribed to wrong-headed or lazy intellectual nihilism or misunderstanding of Darwinian theory, as has sometimes been asserted (e.g. Leonard 2001:67–68; cf. Shanks and Tilley 1992:55). Darwinian critics of interpretive archaeology often neglect to acknowledge the range of social theories and attendant analytical tools that are deployed within Marxist, phenomenological or structurationist viewpoints (for example), or to debate the question of whether understanding emergent human social complexity

might require new sets of ideas that deal with this more adequately than does evolutionary theory. Nor are interpretive archaeologists generally extreme relativists; they do openly deploy qualified cultural universals (e.g. Hodder 1985:6, 13; cf. Mithen 1989:485) and certainly do not reject Darwinian accounts of human evolution (creationism tends to be notably absent from the multiple narratives tolerated by postprocessualists, providing an interesting example of the contextual limits of relativism; cf. Geertz 1984; Schultz 2009). To equate postprocessualism with medieval scholasticism (Kohl 1993) is therefore just as hobbling to discussion as accusing evolutionary archaeologists of being Social Darwinists. Many of the differences between the two schools of thought may simply be due to terminological divergences over the last century and a half (Bentley and Maschner 2008:5) and to alternative readings of hallowed texts (McGuire 2002:18), or they may reflect genuine disagreements over understandings of human societies and what archaeology might reveal about these. We will not discover which of these possibilities is most accurate unless debate moves forward informed by the context of intellectual history, but unhindered by outmoded stereotypes.

Carrying Forward the Debate: Themes in This Volume

To move forward we identify a number of key themes that emerge from both the chapters in this volume and related interpretive and evolutionary archaeological literature. These themes represent what we regard as the cornerstone for comparison of interpretive and evolutionary archaeologies and should therefore be kept in mind when reading the chapters in this volume. They are not, however, always explicitly considered by evolutionary and interpretive archaeologists when writing for their colleagues and collaborators, or when attempting to engage archaeologists who adhere to a different framework (e.g., Kristiansen 2004). These themes do not necessarily highlight areas of agreement, but rather areas of, perhaps unnoticed, mutual concern. We are trying here to distil the debate down to its most basic components.

Our first theme is a simple question: what is it that archaeologists study? The answer to this question greatly shapes many characteristics of the evolutionary and interpretive programmes. While for archaeologists of any theoretical stripe, artefacts and other archaeologically relevant physical materials are contemporary phenomena, the focus of study—what archaeologists seek to understand—differs. Although this is an oversimplification (e.g., compare O'Brien and Lyman 2000 and Shennan 2002), evolutionary archaeologists attempt to explain variation in the physical and relative spatial characteristics of artefacts and archaeological features, not the past as such (cf. Binford 1981). In the final

chapter of this volume, Shennan suggests that archaeology should play to its strength, and this is examining the empirical patterns of stability and change in the material record of human existence, and not, by way of contrast, a past lived experience. He argues that evolutionary theory, with concepts such as cultural transmission, lineage and selection, and with a focus upon explaining variation and change, is the most likely framework to produce convincing and rigorous accounts. The concern with explaining variation within a contemporary empirical phenomenon is related to the scientific epistemological standard or scientific method employed by Darwinian evolutionists. Alternative possible explanations are evaluated using generally agreed, and often quantitative, criteria for how well they account for variation in the empirical world (for diverse examples see Allen 1996; Buchanan and Collard 2008; Glatz et al., this volume). In this volume, Colleran and Mace focus on the use of scientific method as a defining feature of evolutionary archaeology and anthropology. They argue that by adopting the philosophical tenets of scientific method, primarily the explicit evaluation of competing hypotheses, interpretive archaeology and anthropology might be more compatible with evolutionary research (cf. Johnson, this volume).

This will, undoubtedly, be undesirable to many, as interpretive archaeologists are more interested in the past per se as experienced and understood by people, both then and now, than the empirical record, though again this is a simplification (e.g., Barrett 2001; Shanks and Tilley 1992:172–240; Thomas 1996:55–64; Hamilton, Sillar, both this volume). This is not to say that interpretive archaeologists are unconcerned with the material record. The material record does shape what is said about the past (e.g., Shanks and Tilley 1989:48–49). Moreover, Johnson in his chapter argues that within interpretive archaeology one can understand the past through the material record in a way that is as empirical and rigorous as the evolutionary programme described by Shennan. Johnson notes that many evolutionary accounts in archaeology are narratives, not much different in terms of 'testability' to interpretive archaeological research. In general, however, it is fair to say that interpretive research recasts, describes and theorises the past, a decidedly non-empirical entity, and therefore we might not expect interpretive archaeological theory to be constrained by empirical sufficiency to the same degree as evolutionary theory, which has been expanded and retooled to apply to the archaeological record (cf. Cochrane 2009). This certainly has engendered, in part, the substantial development of diverse theoretical approaches within interpretive archaeology (Hodder 2003; Thomas 2000). Finally, regardless of one's particular specialization, it should go without saying that both the past and the contemporary archaeological record are legitimate subjects of study.

Undoubtedly related to the issue of what it is that archaeologists study, the different emphasis placed on methods in interpretive and evolutionary archaeologies is a second theme in this volume and related literature. To be clear, we regard theory as the set of explicit assumptions and processes we articulate to supplant our common-sense understanding of the past and the archaeological record. For our purposes here, methods may be differentiated from theory as sets of goal-related procedures for examining phenomena. A short-hand way to think about methods is as the procedures we use to make observations that are explained and understood by theory. Compared to evolutionary archaeology, there is a much smaller body of methodological or 'how-to' literature for interpretive archaeology, although notable exceptions include recent phenomenological literature (e.g., David and Thomas 2009; Hamilton, Whitehouse, both this volume) and discussions of excavation methods (e.g. Chadwick 2003; Hodder 1997; Lucas 2001); the latter, perhaps surprisingly, is not well considered in the methodological literature of evolutionary archaeology. The relatively small role for method in interpretive archaeology may be partly explained as a reaction to processualism, itself largely characterised as a methodological revolution (Meltzer 1979), one focused on scientific method (e.g., Plog 1973), archaeological classification through middle range research (e.g., Binford 1981), the identification of site formation processes (e.g., Schiffer 1987) and abundant methods for generating environmental data (e.g., Butzer 1982). Interpretive archaeologists have often rejected scientific method and have shown little interest in or need of methods associated with middle range research (e.g., Thomas 2004:55–77), preferring to generate observational classes or types from emic categories of ethnography and from documents and personal experience (i.e., phenomenology) (e.g., Hodder 1982a). Processual-associated methods focused on environments and site formation have often provided more useful observations for all archaeologists, including those in the interpretive tradition.

Regardless, we suggest that the relative dearth of explicitly interpretive methodological literature is related to the great diversity of interpretive theory. The many theoretical frameworks used by interpretive archaeologists can comprise radically different central assumptions and foreground quite different explanatory processes, and thus common methods may find little use. For example, Hamilton (this volume) discusses phenomenological methods that use the human senses to experience landscapes in situ. All senses are used, not just vision, when one is in an archaeological landscape to probe how a past person's understanding of a particular place may be related to their bodily experience of it. Hamilton suggests that phenomenologists not abandon perhaps more 'processual' approaches to measuring the landscape (total stations,

GIS and so forth), but that these analyses be deployed subsequent to or alongside phenomenological surveys. In her own work in Italy (Hamilton and Whitehouse 2006), Hamilton has combined phenomenological survey with processual site catchment analysis. In comparison with Hamilton, Sommer's research (this volume), also broadly interpretive, uses a different set of methods, largely unremarked and derived from culture historical and processual examinations of artefact style, to examine ethnogenesis. She argues that the processes leading to the formation of ethnicities, as we understand them in the contemporary world, also likely explain the formation of ethnic groups 7000 years ago in what is now western Germany. While both Hamilton's and Sommer's interpretive research have a common interest in the past individual's experience, their analytical methods are quite different.

In contrast, for evolutionary archaeologists working within a more unified theoretical framework, specific methods have gained widespread use and attention in the literature. Evolutionary archaeological methods include those for classifying artefacts and making observations relevant to evolutionary processes (e.g., Dunnell 1978; O'Brien et al. 2002). In this volume, Cochrane examines methods such as seriation and engineering analyses used to arrange and describe artefacts in terms relevant to cultural transmission and processes such as drift and selection. He compares this with work in memetics that seeks to define cultural transmission units. Other methods in the literature of evolutionary archaeology, for example, lay out the general steps in evolutionary analyses (e.g., Hunt et al. 2001; O'Brien and Lyman 2000) and describe how to generate and explain artefact distributional data (e.g., Lipo et al. 2006; Tehrani, Glatz et al., both this volume). Tehrani, in his chapter here, discusses the use of cladistics, a method for arranging artefact classes into branching trees of cultural relatedness, in evolutionary anthropology and archaeology. He notes that these evolutionary methods have a long history in archaeology and anthropology, stretching back to Pitt-Rivers.

A third theme arising in this volume and prevalent in the wider literature is the generalizing versus particularizing natures of evolutionary and interpretive research, respectively. The appropriateness of archaeology as either a generalizing or particularistic enterprise has been a flashpoint of debate for at least 25 years (e.g., Binford 1962; Hodder 1982b). This either-or characterization has never been particularly accurate (cf. Clarke 1973; Hodder 1985), and indeed we would not expect it to be if what it is that archaeologists study includes both the past and the contemporary archaeological record, understood at a range of scales. Evolutionary theory as used by archaeologists, biologists, behavioural ecologists and others comprises a set of processes used to explain variation across populations (Mayr 1976) and thus, by design, is generalizing to a degree.

Evolutionary explanations usually include a process accounting for the distribution of variants in a group, and even when evolutionary explanations are seemingly targeted at individuals, these explanations only make sense relative to other individuals in a population. For example, Bentley, in this volume, discusses how processes such as drift and selection are applicable to culture. In particular he notes that regardless of whether people consider themselves independent decision makers (or are so-considered by archaeologists) or purposeful copiers of other people, the results of their decisions about what dog breed to own, what to name their baby, how to decorate their pot, or other choices take on recognizable and explicable distributions across populations. Bentley also provocatively suggests that the distribution of different types of archaeological theory can be understood in a similar manner. Evolutionary explanations, like any scientific explanation, may also be considered generalizing because the processes used to generate explanations are mechanistic. A synonym for mechanistic in this case is external; evolutionary explanations refer to processes that are external to the phenomena under study. To take a behavioural example, evolutionists do not necessarily assume that people engage in a behaviour with the intent of maximizing their lifetime geometric mean fitness (cf. Boone and Smith 1998; Lyman and O'Brien 1998). In other words, irrespective of an individual's intent, the distribution of behavioural variants in a population may be explicable via an external or mechanistic process like selection.

The primacy given to population-level descriptions and external processes in evolutionary archaeology contrasts with interpretive archaeological explanations that more often focus on unique or particularistic details of an individual, or a group of individuals, and processes that are internalised within human minds. The concern with the unique contexts of a group of individuals derives from the interpretive principle that the meaning of material culture is actively produced by the makers, users and consumers of material culture, a key plank of early postprocessualism (e.g., Hodder 1985; cf. Johnson 1989; Gardner, this volume). The particularistic quality of much interpretive research comes, in part, from attempts to understand meanings and settings that are unique to an individual or group. Using examples from Peruvian archaeology, Sillar, in this volume, argues that we must understand the motivations and intentions that are unique to people and groups in particular times and places if we are to adequately explain technological change in the archaeological record. This, he argues, reflects the role of human decision making as a primary selection criterion. As suggested above, interpretive archaeological research also involves the use of generalised processes. For example, analyses of embodiment (Whitehouse, this volume) rely upon principles argued to have general applicability, while among theories of agency

and structure (Gardner, this volume), concepts like habitus are treated as relevant in many different contexts. James, in his chapter in this volume, discusses the interpretive archaeological treatment of violence and comments on a series of generalizations that archaeologists in this tradition have used to understand violence and warfare in the past. These include the interpretation of both Iron Age fortified settlements and medieval castles as symbolic manifestations of concepts of community and ideology (that also capture broader-scale social phenomena). Interpretive archaeologists often regard general principles as descriptions of thought processes, desires, intentions or subconscious motivations of individuals or groups, and thus produce explanations or understandings of the past that we might consider internalised. In contrast to some of the mechanistic explanations of evolutionary archaeology, the internalization of interpretive explanations also gives them a particularistic flavour.

The way in which interpretive and evolutionary archaeologists use general principles also influences the understanding of determinism and the extent to which variation in past human behaviour and the results of human behaviour, namely artefacts and features of the archaeological record, can be explained within a deterministic or rule-bound framework. By determinism we mean the concept as it is normally understood by archaeologists (e.g., Hodder and Hutson 2003:7), that is, outcomes are predictable because a particular process is law-like, X causes Y. Determinism in archaeology is linked to processualism (O'Brien et al. 2005), and thus it is no surprise that evolutionary archaeological explanations are also often considered deterministic. Specifically, in evolutionary research the people whose behaviours created the archaeological record are sometimes seen as automata whose lives are forced along particular paths by deterministic processes (Hodder and Hutson 2003:40–41; Shanks and Tilley 1987:143–65; Thomas 1991). The contrary idea of free will and the ability to make choices unconstrained by external forces is often seen to describe interpretive archaeological research, and in particular the individuals in the past that are a focus of this research (see, e.g., Knapp and van Dommelen 2008; Kristiansen 2004:83–85).

Neither of these extremes is, of course, an accurate characterization of archaeological research in either school (Colleran and Mace, Gardner, both this volume). For interpretive archaeologists, it is not 'anything goes', as minimally, all human action is channelled by biological possibilities or by structural constraints. Whitehouse, for example, argues in her chapter in this volume that while human bodies can be understood from an interpretive perspective, that is as cultural 'things', these interpretations are almost always underpinned by biological research, sometimes within the same piece of work (though cf. Fowler 2002; Yates 1993). Interpretive work on human bodies often focuses on the social

and cultural ways that difference is constructed through the body. More prosaic perhaps, but no less true, interpretive archaeological theory provides a set of rules, or deterministic relationships, by which the behaviours, intentions, beliefs and meanings of past lives are reconstructed (Shanks 2008). For interpretive archaeologists there is, however, less concern that 'the correct' past life has been reconstructed, rather that the particular interpretive theory and biological or structural possibilities have been adhered to. Said in a more nuanced way, many interpretive archaeologists would not agree that there is a single 'correct' reconstruction of past life.

The caricature of evolutionary archaeology is similarly strained (e.g., Zeder 2009). Evolutionary archaeologists do not assume that humans in the past (or present) were mindless automata randomly moving through life seeking only to maximise their fitness. More to the point for determinism, evolutionary archaeologists have never suggested there are genes for certain artefact types (cf. Loney 2000), although terms like 'phenotype', referring to the physical expression of inherited information (genetic or cultural), have been sloppily used in the past (Bentley et al. 2008). Moreover, evolutionists do not deny that humans almost always act with intent and consistent with a set of culturally mediated and sanctioned beliefs. However, consistent with their view of evolution as a mechanistic or external explanatory system, evolutionists conduct research by examining behaviour and the results of behaviour to see if observed distributions conform to expectations outlined by evolutionary processes. In this kind of analysis, one could suppose that people were acting 'as if' they had evolutionary processes in mind, but this is unnecessary. This research agenda allows evolutionists to use simple and historically quite useful assumptions, such as those developed in game-theoretic models, to make predictions about the characteristics of the archaeological and behavioural records. One well-used model is the prisoner's dilemma as discussed by Layton (this volume). He notes that to appreciate under what conditions individuals will most likely engage in violence we can examine their possible decisions in terms of a cost-benefit analysis that considers the variable decisions of others, in this case whether to meet the individual's violent challenge with violence or acquiesce. Again, to be clear, research that employs mechanistic explanations such as Layton describes does not assume that people consciously think in game-theoretic terms using categories like 'pay-off', 'defection' and the like (although they might). Importantly for a discussion of determinism, when the predictions of game-theoretic or other evolutionary models such as optimal foraging are not met through observations of the behavioural or archaeological record, such results are also interesting and suggest that a process other than that first assumed may be worth

investigating. For example, Glatz et al., in this volume, examine late Bronze Age ceramics in Anatolia by comparing them to distributions expected by neutral theory as used in evolutionary archaeology. Neutral theory, developed from biological evolutionary theory and archaeology (Dunnell 1978; Neiman 1995; Bentley, this volume) predicts that some artefact types will be stochastically distributed in time and space in a manner that reflects aspects of demography. Glatz et al. find that this is not the case for some pottery phases and are able to suggest alternative processes to explain these distributions. One might argue that the use of simple or deterministic models makes evolutionary theory artificial and somehow unrealistic. All theory, however, is artificial and has different degrees of realism. To wit, few people go about their daily lives thinking of their behaviour in terms of kin selection or how their behaviour might reflect poststructuralist symbolic fluidity. We use theory, evolutionary and interpretive, to take the place of our default sense-making system, our implicit, largely cryptic common sense.

A final theme emerging from the evolutionary and interpretive discussions in this volume, and further afield, is ontological; that is, how do different views on the nature of existence affect the characteristics of evolutionary and interpretive archaeology? Interpretive archaeologists view human culture as something different in kind from the rest of the natural world. The theories used to understand or articulate human action and belief, the human past and present are therefore unique to understanding humans and are not particularly appropriate to other animals or materials. Gardner, in his contribution to this volume, notes that 'agency theory' has been developed over almost three decades in archaeology (and longer in other disciplines) as a means to understand the relationships between acting individuals and societies' institutions. It goes without saying that agency theory is not much used to explain non-human animal behaviour (although see Gosden 2005). Evolutionary archaeologists, on the other hand, view humans as different from other animals, but this difference is one of quality, not kind. The same general principles used to explain behavioural and artefactual variation in non-human animals (e.g., Bonner 1980; Hunt and Gray 2003; Lycett et al. 2009) can be used to explain people as well (cf. Laland and Galef 2009).

Competition or Collaboration? The Future of Archaeological Theory

The emergent themes in evolutionary and interpretive archaeology indicate that many differences in these research programmes are a product of the different objects of archaeological study and different views on the

nature of 'human' in relation to the rest of the world. As there is more than one legitimate answer to each of these questions, is the future of archaeology to be filled with competition between approaches or collaboration in research? Will the outcome of debate between advocates of these perspectives be cross-fertilisation or strengthening of individual standpoints? Will 'survival of the fittest' produce an eventual winner (cf. Bentley and Maschner 2008; Moss 2005; O'Brien et al. 2005) or will the 'dialectical struggle' lead to a new synthesis? We close this joint introduction with some individual points on the purpose and future direction of this important discussion.

AG writes: For me, the great benefit of the debate in our seminar series and in this volume has been the highlighting of taken-for-granted assumptions and modes of working within one tradition. The outcome of the comparison of interpretive and evolutionary perspectives is unlikely to be a unified field, but it should generate better scholarship on all sides. Defending some positions against quite reasonable alternatives, seeing others in new light as they look rather similar to the alternatives, and finding greater clarity in one's views on the goals of the discipline are all very positive experiences. Knowing as much as possible about what the different approaches to the archaeological enterprise are, and why one disagrees with some and agrees with others, is absolutely fundamental to the academic integrity of the individual, and the discipline. With respect to issues I am most concerned with (see further Gardner, this volume), delineating the distinctive features of a coherent approach to the action-structure problem requires not just evaluation of the competing ideas within the interpretive tradition, but close consideration of approaches adopted in the evolutionary paradigm, from methodological individualism to memetics. While I do not find myself persuaded to adopt elements of the latter, the same goes for some interpretive approaches, and I now know more about why this is the case. Above all, though, bringing these two traditions into a comparative perspective is good for debate; some of my most enjoyable academic discussions have come out of the work on this volume. I hope that this is the future that it will contribute to: one of continued, but actively constructed (and convivial), disagreement.

EC writes: I write this closing half a world away (literally and metaphorically) from where my serious thoughts on interpretive and evolutionary archaeologies began. In the course of editing this volume, participating in the original seminar series from which it originates (proposed, in truly collegial spirit, by AG), and through discussions with my interpretive archaeological colleagues, it has become clear to me that archaeology is an enormous, multistranded discipline and that to ensure its continued benefit to both practitioners and public we must support evolutionary, interpretive, and other kinds of research (e.g., classical

archaeology, materials science), teaching and public engagement. The research questions and interests of archaeology, particularly outside of North America, are far too varied to be adequately and satisfyingly understood by any one research tradition. For me, this is interpretive archaeology's greatest contribution—the recognition of multiple constituencies with their different and justifiable expectations for the shape of archaeological knowledge. Like AG, I do not see the discipline unified in the future. Not because of the specific differences between evolutionary, interpretive and other traditions, but particularly because archaeology contains both science-based and non-science programmes with very different epistemological standards. I do not agree with Johnson (this volume) that science is whatever we archaeologists do. However, I agree with him that some evolutionary research is as much a narrative, untestable story as some interpretive archaeology. Maybe herein lies a contribution of the following chapters: in both interpretive and evolutionary programmes there is excellent research and there is poor research, but we can only make this evaluation if we know about each programme's assumptions, methods and goals.

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PART 1

THEORETICAL CONCERNS

CHAPTER TWO

Units of Transmission in Evolutionary Archaeology and the Role of Memetics

Ethan E. Cochrane

Introduction

Archaeology has a long association with evolutionary ideas, dating to the end of the Renaissance and the realization that the past was materially and socially different from the present. By the early eighteenth century, for example, stone tools found by European farmers were no longer explained as magical or mineralogical products, but as tools made by the ancestors of contemporary Europeans (Grayson 1983). Human groups were not static, nor had they devolved from the classical Mediterranean civilizations, but had instead become more socially complex and technologically advanced over time (Trigger 2006). These late Renaissance and Enlightenment ideas suggest that evolution is simply change and progress and do not share exactly the same conceptual foundation as modern Darwinian evolution (Blute 1979; Dunnell 1980), but these ideas do underpin much thinking that is labelled evolutionary in archaeology and anthropology today (e.g., Carneiro 2003; Pluciennik 2005; Trigger 1998).

In contrast, Darwinian evolutionary theory in archaeology has a different intellectual history, discussed below, compared to the progressive sociocultural evolution derived from Morgan (1877), Tylor (1871), White (1959) and Steward (1955). Archaeologists use Darwinian evolutionary theory to explain the archaeological record, implementing

and modifying many concepts first formulated to explain the biological world, but also generating new concepts to explain cultural variation (for overviews, see O'Brien 1996; O'Brien and Lyman 2003; Shennan 2003, 2008b). For evolutionary theory to be applicable to the archaeological record, it must be conceptualised as a material record shaped in part by social learning (Shennan 2002) or, more generally, cultural inheritance, where artefact variants differentially persist through time and space. Archaeologists and anthropologists have understood the record in this way for well over a century (Lyman and O'Brien 2003), and contemporary archaeologists have built upon this and continued to develop an evolutionary archaeological framework by considering how processes such as cultural transmission, selection and innovation explain archaeological variation. A key point of debate for many scholars, both within and outside evolutionary archaeology, is at what scale are evolutionary processes relevant (Aunger and Curtis 2008; Dunnell 1995; Feathers 2006; Lyman and O'Brien 2003; Neff 2001; O'Brien and Lyman 2002a; Pocklington and Best 1997; Ramenofsky and Steffen 1997)? To put the question in a theoretically and methodologically relevant frame, evolutionary archaeologists measure and explain the differential persistence of what: artefact types, attributes of types, individual people, groups or societies? This question is taken up by several authors in this volume. Tehrani suggests that measurement scales may vary according to the analytical techniques used, in his case cladistics. As another example of this, Bentley discusses the processes responsible for the differential distribution of cultural types represented by baby names and dog breeds. Similarly, Gardner notes that the interpretive archaeology focus on agents and agency must also confront similar scale issues to understand the locus of power or activity. Does it reside in groups, individuals or artefacts themselves? Sillar provides an answer for interpretive archaeologists from his work on Inka prehistory. He argues that the intentions of individual people were the creative force driving the Inka state. The scale at which processes operate is plainly a concern for archaeologists, regardless of theoretical stripe.

Some evolutionary archaeologists (e.g., Cullen 2000; Lake 1998) have tackled the question—the differential persistence of what?—with reference to memes, the concept made famous by Dawkins (1976) that refers to ideas or behavioural traits. Memetic theory is built to explain cultural variation (Aunger 2000), including behavioural, linguistic and cognitive aspects, as well as neurological variation in humans and other animals. Memetics, like evolutionary archaeology, postulates that learning, copying and other forms of imitation can be understood as a transmission system where the distribution of variants, ideas or memes is explained by evolutionary processes. Perhaps the most obvious difference between

memetics and other evolutionary frameworks for investigating human variation is that memetic theory focuses on memes themselves, irrespective of the transmission system, be it linguistic, musical, textual, or any other; in the jargon, memetics is substrate neutral. As Aunger (2002:82–83; see also Stanovich 2004:177) argues, the most important difference between memetics and other evolutionary approaches to culture is the claim by memeticists that memes exist, cause their own replication and are the reason for the evolution of culture. Still, like evolutionary archaeology, there is debate within memetics concerning the scale of memes, how to identify them and measure differential persistence (e.g., Aunger 2002; Blackmore 1999, 2001; Gabora 2004).

Most archaeologists who mention memetics do so only in passing or use meme as a synonym for cultural trait, idea and the like (e.g., Boone and Smith 1998; Gosselain 2008; Shennan 2003, and this volume). Does memetics have anything to offer evolutionary archaeology regarding the measurement and explanation of differential persistence? Although a relatively new field, the memetics literature is large, and here I briefly review only a few of the memetics issues with links to evolutionary archaeology. These issues will also be referenced throughout this chapter.

Memetics: Some Basics

Is a meme a physical thing? This is almost a discipline-defining question (e.g., Aunger 2002) in the memetics literature ever since Dawkins (1976) coined the term 'meme' to refer to the non-genetic unit of replicated information. Memes are replicators and according to Dawkins (1983:83) replicators are 'anything in the universe of which copies are made'. He went further and defined active replicators as entities whose characteristics influence their probability of being copied, and passive replicators as entities whose characteristics do not influence copying probability (Dawkins 1983:83; cf. Bentley, this volume). Dawkins (1983:109) also suggested a quite empirically grounded definition of a meme 'as a pattern of synaptic connections', a unit of information in the brain.

Aunger argues that Dawkins's position on the empirical characteristics of memes is imprecise and adds to the definition of replicator (Aunger 2002:72–74), concluding that 'like other replicators, memes are physical things' (Aunger 2002:196). He defines a meme, or neuromeme, as 'a configuration in one node of a neuronal network that is able to induce the replication of its state in other nodes' (Aunger 2002:197). Other memeticists have answered the question, what is a meme? more ambiguously. Blackmore (1999:66; cf. Blackmore 2001), for example, uses meme to refer to non-material things such as ideas, but also physical structures in the brain and observable behaviours. Still others suggest

that the meme is solely non-material or conceptual, but exists at the scale of all the interconnected ideas in a mind, a worldview (Gabora 2004). At the other extreme, meme simply refers to 'the largest units of socially transmitted information that reliably and repeatedly withstand transmission' (Pocklington and Best 1997:81). In brief, the question of memes as material entities or as conceptual units, and at what scale, is still a debated topic in memetics.

Another contentious issue in memetics is transmission. Does memetic transmission work in such a way that evolutionary processes can explain the differential persistence of memes (i.e., replicators)? Some answer this question negatively. Sperber (1996) notes that the process of cultural or memetic transmission does not include faithful replication of memes, cultural representations or other similarly termed units due to inferential processes in human minds. Without relatively faithful replication, selection cannot work (see also Atran 2001). Aunger also argues that replication is a more complicated process than typically assumed. To fidelity and fecundity, Aunger (2002:73–74) adds that for replication to occur, 'the process that generates the copy must obtain the information that makes the copy similar to its source from that same source'. In other words, no outside information should influence replication as this apparently derails the evolutionary process by blending transmission between lineages.

In a similar fashion, Jablonka and Lamb (2006:206–12) maintain that because the replication process in memetic transmission is intimately linked to what is being transmitted, evolutionary processes may not be relevant to the differential persistence of memes. Others disagree. Distin (2004:154–57), for example, states that what is important in replication is the informational content of a meme and that this can be replicated with sufficient fidelity across different media, say when transcribing spoken French directly into written English. Henrich and colleagues (Henrich and Boyd 2002; Henrich et al. 2008) have argued using mathematical models that low-fidelity transmission (and not exact replication), the blending of cultural traits and non-discrete continuous traits do not preclude selection and other evolutionary processes.

A final topic of debate to mention here is the role of interactors (also called vehicles) in memetics. Interactors are entities that house replicators and whose interaction with the environment influences the replication of replicators (Aunger 2002:78–81; Dawkins 1983:114). The standard biological example of the interactor-replicator distinction is an organism and its genes, and it underlines the notion that interactors, such as individual people, do not replicate, but the replicators they carry, genes in this instance, do. In memetics the standard biological example is not so standard, and several issues about interactors are

explored. For example, are there typical interactors in memetics? One interactor candidate relevant here is an artefact. Using a wagon as an example, Dennett (1995:347–48) argues that wagons carry around not only grain and freight, but wagon memes as well and, like other artefacts including pictures, books, tools and buildings, are vehicles for memes. Several other memetic theorists state, however, that artefacts cannot be vehicles or interactors. Distin (2004:79–80), for example, argues that vehicles or interactors must be a product of replicators, and that artefacts are not literally produced by artefact memes, they are produced by people (cf. Aunger 2002:282–85). The concept of interactors or vehicles is also linked to selection because it is an interactor's interaction with the environment (including other interactors) that results in relevant variation for selection (Hull 1988; Lyman and O'Brien 1998:619).

In summary, memetic theorists are concerned with a large range of topics only partially covered here (see also Blute 2010:113–37). I argue that these and many other topics in memetics are not particularly relevant to unit issues and measurement in evolutionary archaeology for two reasons. First, where concepts in memetics might be relevant to explanation of the archaeological record, evolutionary archaeologists have often independently developed such concepts—taking, modifying and adding to the pool of concepts shared by memetics, genetics, population biology, palaeontology and other evolutionary sciences. Second, memetic theory is typically constructed to deal with phenomena quite different from the archaeological record, usually the distribution of ideas and other transmitted variants in living behavioural systems. The archaeological record, however, is not a living behavioural system, it is a record of some of the results of behaviour. This difference affects the kinds of evolutionary concepts we can use to explain it (Cochrane 2009; Lyman and O'Brien 1998).

The next section reviews evolutionary archaeology and provides a backdrop for an example of how units of cultural transmission are conceptualised in this research program. Lipo and colleagues' (1997) work on Late Prehistoric Mississippian ceramics serves as an example of evolutionary archaeology's productive development of transmission units (i.e., memes). This treatment of transmission units is then contrasted with another conceptualization of transmission derived from memetic theory and applied by Lake (1998) to prehistoric European pottery. This section closes by briefly noting the importance of distinguishing theoretical and empirical concepts in evolutionary archaeology.

The remainder of this chapter describes a case study demonstrating how evolutionary archaeology can address some of the questions posed by memeticists. Specifically, Feathers's (2006) analysis of the evolution of

ceramic traditions in the Mississippi River Valley exemplifies how selection explains the differential replication of cultural transmission units.

Evolutionary Archaeology: Some Basics

Evolutionary archaeology combines two research programmes that were previously somewhat separate: one closely tied to Americanist culture history (Lyman et al. 1997) and the other related to population biology and other mathematical models of transmission (Boyd and Richerson 1985; Cavalli-Sforza and Feldman 1981). Behavioural ecology has influenced a third evolutionary research tradition in archaeology (see Bird and O'Connell, 2006; Colleran and Mace, Layton, both this volume), but it is not considered here as behavioural ecologists do not typically consider units of transmission, an analytical assumption termed 'the phenotypic gambit' (Winterhalder and Smith 1992). Regardless of their specific intellectual heritage, all evolutionary archaeologists consider human variation to be a product of separate biological and cultural inheritance systems (O'Brien and Lyman 2002b). Evolutionary archaeologists also recognise temporal and spatial variation in the archaeological record and that information concerning artefact-making behaviours is transmitted from person to person via imitation and other forms of social learning (Eerkens and Lipo 2007; Shennan 2002; Tehrani and Riede 2008). And finally, evolutionary archaeologists explain the differential persistence of variants by processes such as selection at different levels, drift, parallelism and convergence (Bentley et al. 2008). However, the development of different intellectual strands within evolutionary archaeology has led researchers to concentrate on different issues.

In one of the earliest contributions to evolutionary archaeology, Dunnell (1978a) built upon the work of Americanist culture historians who constructed methods for describing the archaeological record so that their descriptions captured variation explicable via cultural transmission, although they rarely used this term (e.g., Ford 1954; Nelson 1916; Philips et al. 1951; Rouse 1939; Spier 1917). More specifically their descriptions, such as pottery styles and fishhook head types, captured variation that was largely neutral with respect to selection. It was these particular culture-historical descriptive methods and the observations they generated that Dunnell linked to the stochastic transmission processes that Gould and his palaeobiology colleagues (1977) offered as an explanation for random clades (Dunnell 2001). Dunnell published his ideas in a somewhat infamous article titled 'Style and function: A fundamental dichotomy' (1978a) where he argued that variation in the archaeological record could be explained as a result of the transmission of variants whose distribution was patterned by selection, what he called

function, or in contrast the transmission of selectively neutral variants whose distribution was a product of stochastic transmission processes, somewhat like the random clades of Gould and colleagues (Cochrane 2001; Bentley, this volume).

Some evolutionary archaeologists have continued the focus on methods for describing the archaeological record in terms amenable to evolutionary theory (e.g., Cochrane 2008; Dunnell 1978b, 1995; Lipo 2001a; Meltzer 1981; O'Brien and Lyman 2002a; Tschauner 1994), and these descriptions are often generated with an eye to macroevolutionary explanations. In some ways, this strand of evolutionary archaeology is conceptually similar to modern palaeobiology (Mesoudi et al. 2006), with a focus on macroevolution and an empirical record that is comprised almost entirely of the phenotypic hard parts of organisms. Evolutionary archaeologists, however, conceptualise artefacts as 'hard parts' whose variation is a product of cultural transmission and preserved in the archaeological record.

Evaluating selection as an explanation for the differential persistence of variability reflected in artefact classes is also a prominent research focus in evolutionary archaeology (e.g., Madsen et al. 1999; Neff 2001; O'Brien and Holland 1990, 1992; Rindos 1985; Rogers and Ehrlich 2008). One way this is done involves comparing artefact classes relative to some performance criteria under controlled conditions to determine if the artefact class hypothesised to be under selection differs in performance from other classes (Feathers 2006; Hoard et al. 1995; O'Brien et al. 1994; Pfeffer 2001; Pierce 2005). Selection is also sometimes assessed through the comparative method (Neff and Larson 1997), where artefactual similarities existing between culturally unrelated populations are considered analogous, thus a product of selection in similar environments. To determine if artefacts are a product of culturally unrelated populations, archaeologists may examine artefact proximity in time and space (e.g., Meltzer 1981) or use phylogenetic analyses (e.g., Tehrani and Collard 2002; Tëmkin and Eldridge 2007; Tehrani, this volume). Finally, selection may be assessed by comparing, in related transmission lineages, rates of change between traits hypothesised to be under selection and those likely to be selectively neutral (e.g., Rogers and Ehrlich 2008).

Selection in the above examples is not necessarily linked to human reproductive fitness. Selection in evolutionary archaeology may refer to the differential replication of artefact classes without a deterministic link to human reproduction (Leonard and Jones 1987) or contrastingly, artefact class replication and human reproduction may be linked in a non-trivial manner (O'Brien and Lyman 2002b; Shennan 2008a). Some scholars have confused selection at the level of artefact classes with so-called cultural selection, or the 'factors that bring about the adoption or

nonadoption of a particular symbol or trait without concern for its effect upon the [biological] phenotype' (Rindos 1985:73). While there are many factors that influence an individual's potential *adoption* of a trait, to evaluate selection as a macroevolutionary explanation for archaeological variation, these factors should be treated separately from mechanisms that influence the *persistence* of traits in populations. To maintain this methodological separation, evolutionary archaeologists might evaluate a selection hypothesis as the differential replication of artefact classes caused by the variable and measurable performance of empirical specimens of a single class in an environment. Such an approach contrasts with that presented by Sillar (this volume), who argues that archaeologists should indeed investigate the individual intentions that lead to trait adoption. Most evolutionary archaeologists, however, question our ability to empirically investigate past intentions.

Mathematical models of transmission and population biology have also influenced evolutionary archaeology (Collard et al. 2008). This approach, known as dual inheritance theory or gene-culture coevolution, was first used by anthropologists, ecologists and population geneticists such as Cavalli-Sforza and Feldman (1981), Boyd and Richerson (1985) and others (e.g., Lumsden and Wilson 1981) to explain patterns of cultural trait transmission. Several aspects of dual inheritance theory led to its initial use by archaeologists as an explanatory framework. First, the definition of culture offered by Boyd and Richerson (1985:33), while using a different terminology, fits with how modern archaeologists understood the generation of artefactual similarities: 'Information capable of affecting individuals' phenotypes [largely behavioural] which they acquire from other conspecifics by teaching or imitation'. Second, the population biology models of trait transmission provided rigorous mathematical models that seemed inherently well suited to generating scientific explanations of the archaeological record as a record of traits (Shennan 2003). And third, the scientific evolutionary foundation of dual inheritance theory (as opposed to sociocultural evolutionism), is perfectly compatible with the ecological decision-making frameworks used by many of the early adopters of dual inheritance theory in archaeology.

Robert Bettinger was an early adopter of dual inheritance theory, writing in 1991 that the theoretical contributions of Boyd and Richerson are 'not well known to most anthropologists [and archaeologists]' (1991:182). Bettinger was one of the first archaeologists to sketch an archaeological explanation using cultural transmission biases articulated by Boyd and Richerson (1985), namely frequency-dependent bias and indirect bias. These biases are conceptually similar to artificial selection (Cochrane 2009) and influence the probability that cultural variants will be transmitted between individuals in a population.

Frequency-dependent bias is a process whereby variant frequencies in a cultural generation are generated by a probability function applied to prior frequencies. In short, individuals often copy the most popular variants. Indirect bias occurs when the frequency of a trait in a cultural generation is deterministically linked to the frequency of other traits. Individuals choose traits based on marker traits of particular individuals. Bettinger suggested these processes might explain the Upper Palaeolithic transition when behaviourally modern humans with art, symbolism and advanced technologies arose relatively suddenly (Bettinger 1991:203–08), but variably (see Powell et al. 2009). Bettinger's sketch was an early example of dual inheritance archaeology and his later research included discussion on the logic of particular transmission biases (Bettinger and Richerson 1996). The empirical application of dual inheritance models has increased in the last decade with archaeologists explaining artefact patterning as result of, for example, frequency-dependent bias during periods of population aggregation (Kohler et al. 2004), indirect bias in the manufacture of projectile points (Bettinger and Eerkens, 1999) and anticonformist transmission in the production of ceramic decoration (Bentley and Shennan 2003).

Units of Cultural Transmission in Evolutionary Archaeology

As mentioned above, some evolutionary archaeologists have tended to focus on classification issues or methods for describing the archaeological record. Similar to evolutionary archaeologists, Americanist culture historians understood variation in the archaeological record to result from transmission processes (Lyman and O'Brien 2003) and developed methods for generating observational units to track transmission. The history of the method and theory used to justify these units is important as it demonstrates that some cultural historians and later evolutionary archaeologists have been defining transmission units, or 'doing memetics', to quote Hull (2000:48), for almost a century. Maurice Bloch (2000) has made a similar point for anthropologists. The first part of this section explores the history of Americanist culture-historical observational units.

Prior to the twentieth century, archaeologists in the Americas did not share common or even distinctly archaeological methods, and many archaeological projects were part of larger general scientific and exploratory expeditions (Dunnell 1986). This began to change with the work of Nels Nelson, Alfred Kroeber, Clark Wissler and Leslie Spier (Lyman et al. 1997). Working at the Tano Ruins of San Cristobal, New Mexico in 1914, Nelson excavated an undisturbed deposit in arbitrary 1 ft. vertical

units and recorded the abundance of different pottery types in these units. At the time this was a relatively novel approach to quantification. In his publication of this work, Nelson (1916:166–67) noted that when counted in vertical provenience units the changing abundance of some of his pottery types approximated unimodal curves (Table 2.1). This was to be expected, he reasoned, as a pottery type comes into vogue, attains maximum popularity, declines in popularity and then goes extinct.

In the Zuni pueblo region, Nelson's mentor Alfred Kroeber (1916) and his colleague Leslie Spier (1917) demonstrated that one could arrange surface assemblages in correct chronological order when pottery in the assemblages was quantified using particular types. These types were mostly defined by surface modifications on pottery such as particular painted designs or glazes. They were able to place the assemblages in the correct temporal sequence by ordering them so that the changing frequencies of types arrayed across assemblages approximated unimodal curves, the pattern Nelson had discovered in his work. Spier (1917:298–99) was able to empirically evaluate parts of his chronological order of surface assemblages by comparing the proportions of pottery types across his order to the proportions of types across stratigraphically superposed ceramic assemblages. He noted that the frequency of pottery types in stratigraphically older assemblages matched the type frequencies for what he surmised were similarly aged assemblages on the present-day surface.

Spier, Nelson and their contemporaries did not develop an explicit rationale for why certain pottery types (or other artefact classes) exhibited regular frequency distributions over time. Kroeber (1919) discussed such regularities with reference to the cyclical nature of change and demonstrated that chronological variation in some aspects of women's clothing, dress widths for example, followed a regular pattern of increasing width, reaching a maximum and then decreasing width. Like Nelson's reason for the unimodal distribution of some pottery types, Kroeber was equally vague about what caused these regularities and could think of no explanatory mechanism, instead referring to the necessity of change in cultural elements (Kroeber 1919:262). Kroeber did, however, observe that utilitarian features will not show these cyclical patterns, thus prefiguring the evolutionary archaeology distinction between selectively neutral variation and variation shaped by selection.

Americanist culture historians in the second decade of the twentieth century were of course not the only scholars who suggested you could create chronological sequences by arranging artefacts according to their similarity. Petrie (1899), for example, did this with Egyptian materials a few decades earlier. What is important here is that Americanist culture historians shifted focus from the dominant strategy of tracking

Table 2.1 Table redrafted from Nelson's (1916) publication with pottery type abundances from a section at the Tano Ruins of San Cristobal, New Mexico. Note the generally unimodal distribution of type I, II and III (and their variants) abundances. See also Lyman et al. (1997: Fig. 3.4).

			Type I, Two and Three Color Painted Ware	Type II, Two Color Glazed Ware			Type III, Three Color Glazed Ware
Thickness of Section	Corrugated Ware	Biscuit Ware	Black-on-White Painted Ware	Red Ware, Black or Brown Glaze	Yellow Ware, Black or Brown Glaze	Gray Ware, Black or Brown Glaze	Gray, Yellow, Pink and Reddish Wares, Combination Glaze-and-Paint Design
1st foot	57	10	2	24	23	34	5
2nd foot	116	17	2	64	90	76	6
3rd foot	27	2	10	68	18	48	3
4th foot	28	4	6	52	20	21	
5th foot	60	15	2	128	55	85	
6th foot	75	21	8	192	53	52	1?
7th foot	53	10	40	91	20	15	
8th foot	56	2	118	45	1	5	
9th foot	93	1?	107	3			
10th foot	84	1?	69				
8 inches	126		103				

the distribution of cultural traits (such as the presence of a particular ceramic vessel handle) to using types to measure frequency variation within those traits in assemblages (Lyman and O'Brien 2003). If types were adequately defined, assemblages could be arranged in time by making type frequencies conform to the expected unimodal distribution. This, in short, describes the frequency seriation method (O'Brien and Lyman 1999).

Culture-historical artefact types used to calculate frequencies for successful seriations measured similarities that are a product of cultural transmission. These artefact types might be called replicators, memes, recipes or something else. In the historical ethnology of the early twentieth century that was developing alongside archaeological culture history in the Americas, the terms 'cultural trait' or 'cultural element' were often used to denote the 'thing' that is passed between people. Similar to debates in memetics summarised above, scholars of the time discussed the variable scale of these units and what might be a minimal functional unit of transmission (Lyman and O'Brien 2003). There is an important difference, however, between the cultural traits and cultural elements of historical ethnology and the types archaeologists used to track transmission over time. Cultural traits were considered by most ethnologists to be emically meaningful, real and empirical units of transmission (a position like that held by some memeticists). The reality and therefore usefulness to the ethnologist was confirmed when multiple ethnographic informants independently agreed that a particular element existed (e.g., Driver 1938; see Lyman and O'Brien 2003). In contrast, the types used by Americanist culture historians to track transmission were generally considered useful if they could generate frequency data conforming to a unimodal distribution. Rouse (1939:19) for example, wrote that 'types and modes [combination of variables that make up a type] are artificial concepts set up by the writer [i.e., archaeologist]'. While most culture historians realised that they were tracking transmission with etic units, there was still hope that these units might also identify units of culture as they would have been conceived by the ancient makers of artefacts. There was some debate about the emic or etic character of artefact types, summarised by Willey and Phillips (1958:13):

> The principal difference of opinion [concerning types] may be crudely stated as opposition between those who believe that types are arbitrarily "designed" by the classifier and those who think that types exist in nature and the classifier "discovers" them. According to the first view, types are simply analytical tools that are to be judged solely on the basis of their usefulness; the second maintains that they have, or should have, behavioural reality in the sense that they would be recognized as norms in the societies that produced the objects being [classified].

That Americanist culture historians questioned the meaning and reality of their units is evidence they did not have a well-developed, explicit theory by which they explained observations of the archaeological record. It is now recognised that the frequency distributions produced by culture historical seriations are explained by evolutionary theory, and that culture historians had constructed units that largely measured the transmission of selectively neutral variation (Teltser 1995). In the absence of selection, it is the stochastic processes inherent in transmission within a finite population that can result in unimodal frequency distributions (Cochrane 2001; Neiman 1995; cf. Bettinger and Richerson 1996).

An Evolutionary Archaeology Perspective on Transmission Units

Evolutionary archaeology is partially built upon the culture historical construction of artefact classes reviewed above, with some archaeologists highlighting the need to define and evaluate the observational units with which we track transmission in the record. Work by Lipo and his colleagues (Lipo 2001a; Lipo and Madsen 2001; Lipo et al. 1997) in the lower Mississippi River Valley exemplifies this focus on classification as an integral part of evolutionary analyses in archaeology. Lipo and his colleagues have examined transmission using simple assumptions and observational classes based on culture-historical types, and their work provides one example of how current evolutionary archaeologists define cultural transmission units.

A goal of Lipo's work has been to determine population structure in the Late Prehistoric period of the Mississippi River Valley. Population structure refers to the subdivisions within a population that influence the probability of transmission between individuals. Change in population structure is a key distinction in the evolution of simple societies, where transmission is largely influenced by distance (discounting age and sex), to complex societies, defined by increased probabilities of transmission within functionally integrated intra-population communities, such as craft specialists (Lipo 2001b). To investigate population structure, Lipo et al. (1997) began by building agent-based simulations of neutral-trait transmission to determine how variation in the density of agents and frequency of transmission across space and over time would be reflected in the empirical distribution of traits in cultural assemblages. Figure 2.1 depicts Lipo and colleagues' simulations where agents may hold one of three cultural trait classes, A–C. There are no spatial restrictions on interaction, and when individuals meet, there is a 50% chance that they will change their cultural trait to that of the other individual. This probability of transmission

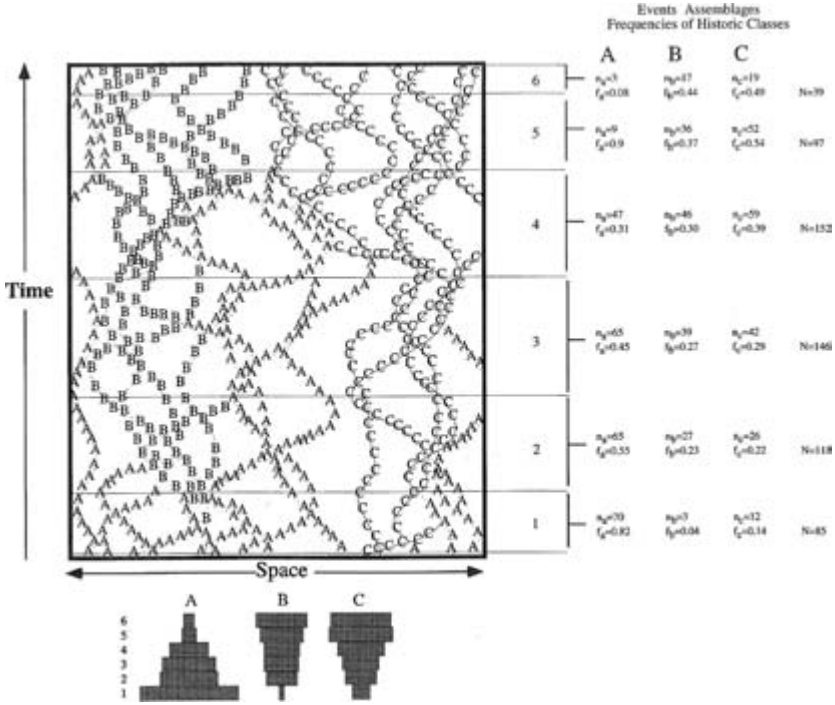

Figure 2.1 Simulation depiction from Lipo (2001a: Fig. 3.1). See text for description. (Image courtesy of C. P. Lipo.)

is set to model, in a simple way, selectively neutral traits. Each 'letter' in the grid represents a discard event used to calculate the frequencies of cultural traits for each time period, 1–6. Note that the time periods, except period 6, are roughly equivalent in length. The frequencies of trait classes per time period at the bottom approximate portions of unimodal distributions. Departures from the unimodal model are expected due to sampling error, and the fact that time periods used to aggregate cultural assemblages are not precisely equal. Figure 2.2 presents a subsequent simulation where the density of individuals across space and their frequency of interaction is varied. Note that only by dividing assemblages to represent the varying densities of agents will class frequencies approximate unimodal curves when arranged in correct chronological order.

Lipo and colleagues' simulation snapshots demonstrate an important point for the argument here: by constructing artefact classes to measure selectively neutral variation—the 50% probability of changing traits in the simulations—we can use seriation to investigate the density of

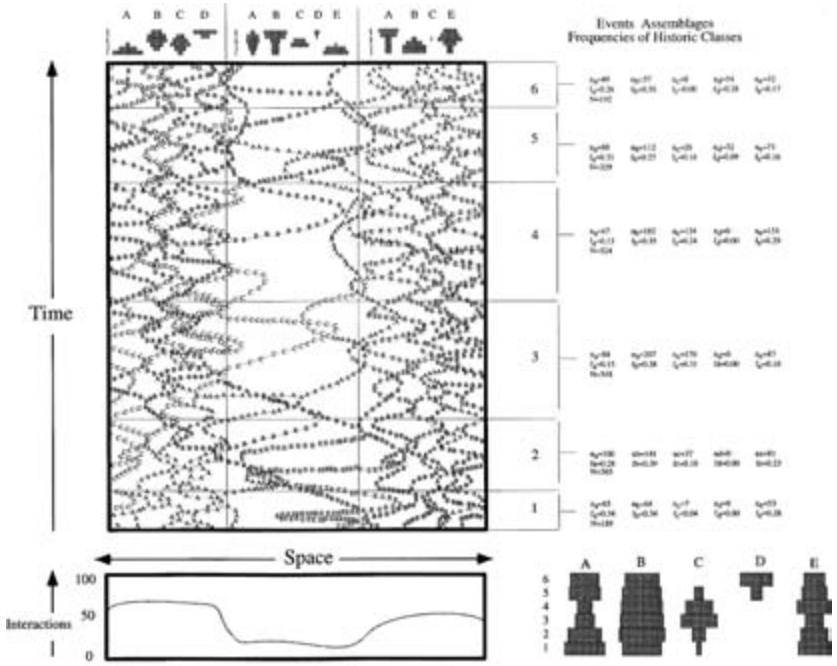

Figure 2.2 Simulation depiction from Lipo (2001a: Fig. 3.3). See text for description. (Image courtesy of C. P. Lipo.)

individuals, the inducements and impediments to transmission likely caused by geographic space, and other aspects of population structure. In this kind of research evolutionary archaeologists are not interested in discovering a unit of transmission, a meme say, for its own sake, but instead are concerned with constructing artefact classes that measure transmission so that population characteristics may be investigated.

With this reconceptualization of seriation, Lipo et al. next analyzed the large ceramic collections made by Phillips et al. (1951) in the Lower Mississippi River Valley. Lipo et al. examined the homogenised surface ceramic assemblages from the Late Mississippian period (AD 1400–1600) using cultural historical types, such as Parkin Punctate and Barton Incised, as these types appeared to largely measure selectively neutral variation. Seriations were iteratively constructed by adding assemblages one at time to build the largest seriation orders possible without departing from unimodal and gradually changing class frequencies. If an assemblage could not be added to a seriation order without creating a departure from a unimodal distribution of class frequencies (within confidence limits), the assemblage was placed in a different seriation

order. Only assemblages that satisfactorily represented sample richness and diversity were used. Six seriation orders were thus created with the spatial location of assemblages in the seriations shown in Figure 2.3. These spatial groups likely depict the boundaries of populations defined by transmission frequencies (evolutionary lineages) influenced only by space and population density, as Lipo and colleagues accounted for time (all homogenised Late Mississippian deposits), formation processes, sample representativeness and the comparison of proportions in constructing unimodal distributions.

To investigate how analytical scale affects the definition of transmission populations, Lipo and colleagues collapsed the original Phillips, Ford and Griffin ceramic types into levels of increasing inclusiveness, regenerated the seriations and plotted the resulting spatial distribution of the seriation groups shown in Figure 2.4. Although, the classes were not collapsed in a rigorously orthogonal manner, decreasing the precision of classes used to track transmission results in a relatively smooth expansion of the spatial positions of the population boundaries, as expected

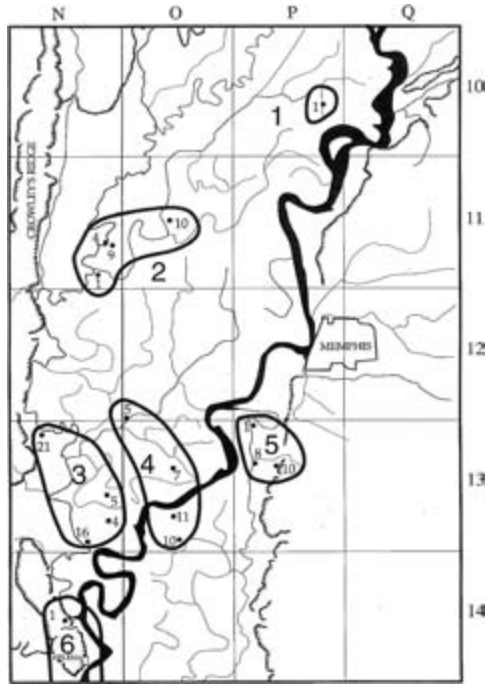

Figure 2.3 Locations of assemblages in the six seriation groups (large numbers) generated by Lipo (2001a: Fig. 4.7). (Image courtesy of C. P. Lipo.)

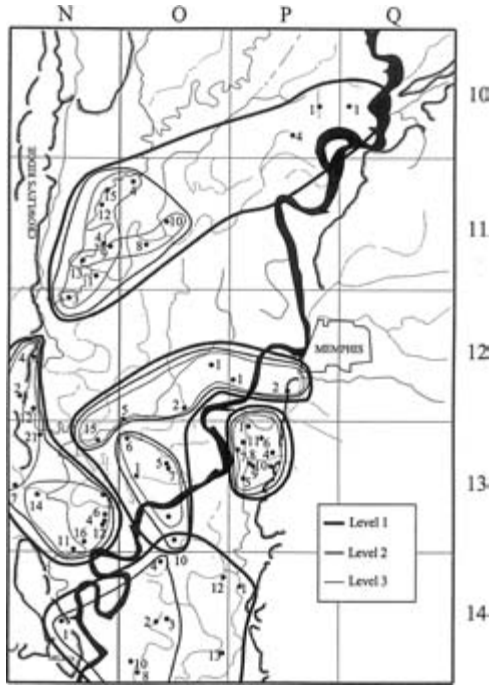

Figure 2.4 Locations of all the Phillips, Ford and Griffin assemblages analyzed by Lipo (2001a: Fig. 4.13). The encircled assemblages indicate seriation groups produced with pottery classes of increasing inclusiveness. See text for discussion. (Image courtesy of C. P. Lipo.)

if the classes track selectively neutral variation. Importantly, it is possible to identify those population boundaries that are defined primarily by decreasing transmission frequencies due to increasing geographic distance, and those boundaries that may represent social or functional impediments to transmission. Identifying the boundaries of past populations is also a goal of some interpretive archaeology research, as demonstrated by Sommer in this volume, albeit employing different methods.

Lipo et al. (1997:327) note that they have simply demonstrated that particular culture historical units measure transmission at various scales and can be used to map the approximate spatial locations of cultural lineages at differing scales of inclusiveness. Their substantive conclusions are important, for if archaeologists are to explain variation in the archaeological record as a product of transmission and processes such as selection, they must be able to define the lineages within which selection may be a relevant explanatory process.

Importantly, the units used to track transmission here bear no necessary link to a unit of cultural information that may be recognised as such by a cultural participant or even an observer in an ethnographic setting. The pottery types used by Lipo and colleagues to track transmission were constructed by culture historians with a simple goal (chronology) and their usefulness evaluated against an empirical expectation (gradual change and unimodality). This treatment of transmission units exemplifies a significant difference between some memetic theorizing that asks, what is and is not a meme? in the empiricist sense of discovering a physical entity that is transmitted (e.g., Aunger 2002; cf. Blackmore 1999; Distin 2004) and much current evolutionary archaeology where units are treated as measurement devices created by the analyst to measure the effects of transmission, not physical packets or things to be discovered that are passed between individuals (e.g., Lipo and Madsen 2001; e.g., O'Brien et al. 2002; Pocklington 2006).

An Archaeological-Memetic Perspective on Transmission Units

Lake's (1998) analysis of European prehistoric pottery provides one example of an archaeological attempt to detect memes or transmission units. Lake states that for material culture variation to be explained through Darwinian processes, cultural transmission must include 'symbolic structure', defined as the transmitting individual's (i.e., the cultural model's) intention (Lake 1998:81). Symbolic structure is the information decoded by the receiver in the generation of behaviour, a process that is distinct from 'taking the model's geometrical perspective and copying the motor-sequence' (cf. Gabora 2004:131–32; Lake 1998:81). Lake (1998:81) labels these two types of transmission 'program-level imitation' and 'impersonation' respectively, following Byrne (1995; cf. Blackmore 1999), and continues that only with program-level imitation can Darwinian processes be used to explain the distribution of behaviours and their material culture results (see also Distin 2004:94). This is so because impersonation involves no encoding of information in symbolic structure and then decoding of the information transmitted and thus is Lamarckian (Lake 1998:85–86).

Applying this discussion to archaeology, Lake notes that artefacts can be conceived in three ways (cf. Aunger 2002): (1) as replicators or entities representing the symbolic structure—the cultural model's intent—of information transmitted that must be decoded in replication; (2) as interactors, that is the physical entities within which replicators reside; and (3) as a simultaneous representation and expression of transmitted information (Lake 1998:83), as both replicators and interactors. To exemplify how these ideas might be used to explain variation in

the archaeological record, Lake argues that artefacts that represent symbolic structure, information that must be decoded in replication, are themselves symbolic. He notes that we can identify these objects in the archaeological record by considering that their form is arbitrary relative to the information they encode. Lake includes here musical scores, cuneiform and stone inscriptions as typical examples. He does not say that an object such as a stone inscription is a meme per se, but that it is 'an arrangement of matter which retains the initiating structure of the meme' (Lake 1998:83).

For what is probably the most common artefact in the archaeological record, pottery sherds, Lake suggests a problem for evolutionary explanations, as pottery may often be conceptualised as both an interactor and the representation of transmitted information, a replicator, simultaneously. Using the case of Neolithic European pottery, Lake summarises Pétrequin's (1993) work on flat-bottomed beakers found in both the western Swiss Alps and Chalain and Clairvaux in France during the fourth and third millennia BC. The Chalain and Clairvaux populations created flat-bottomed beakers similar to those made in the Alps, but used a different manufacturing process, apparently learning different production steps by examining flat-bottomed beakers that were traded to the alpine villages. Lake argues that the flat-bottomed vessels of Chalain and Clairvaux do not then represent the transmission of symbolic structure, replicators or memes, but rather the transmission of representation and expression simultaneously with no decoding of information. Thus the origins and increasing frequency of flat-bottomed beakers in the Chalain and Clairvaux populations is Lamarckian and not explained by Darwinian processes (Lake 1998:85–86).

There are two problems with this application of memetic theory to the archaeological record. First, the interpretation of Lamarckian transmission is likely unfounded. As Hull (2000) points out, considering any instance of cultural transmission to be Lamarckian arises from drawing inappropriate biological analogies. Lamarckian transmission, by definition, occurs when a biological phenotype is inherited from individual to individual. In cultural transmission, it is information, analogous to the genotype, that is transmitted, so passing on of acquired characteristics (equivalent to phenotypes) does not apply, although the passing on of acquired information does apply. This is not Lamarckian in the problematic sense as humans constantly acquire and transmit information over the course of their lives; there is no instant of transmission as in biological fertilization.

A second problem with the application of memetic theory to the record of flat-bottomed beakers is the conflation of concepts that refer to ideas (ideational units) and concepts that refer to things (empirical

units) (Dunnell 1971; O'Brien and Lyman 2002a). Consider this statement: 'Replicators and interactors function in the evolutionary process to produce lineages. If culture-change conforms to the principles of universal Darwinism, there *must exist cultural manifestations of these entities*' (Lake 1998:79, emphasis mine). Saying replicators and interactors must exist seems to indicate that these are empirical things we can go out and discover—pottery vessels, inscriptions and the like. However, the archaeological record contains no empirical replicators at all. It can be conceptualised as a record of the material results of replicators replicating differentially. Lyman and O'Brien (1998:619) have presented some of the ramifications of this position for archaeologists with reference to palaeontology:

> Paleobiologists do not worry about the reproduction of particular replicators—genes—when they study the evolution of forms of fossil organisms. A bone or tooth is not a replicator; it is part of an organism's phenotype. Whether a tooth represents one or multiple genes is, as yet, unknown, but this does not keep paleobiologists from trying to determine and explain the evolutionary histories of the organisms whose hard parts they study. Similarly artifacts are not replicators; they are what is replicated. Cultural traits conceived in the minds of individuals are the replicators that are transmitted.

Explaining Variation in the Archaeological Record as Differential Replication: Ceramics in Eastern North America

If debates in memetic theory are not particularly useful to evolutionary explanation of the archaeological record, can evolutionary archaeologists still address some of the important questions memeticists, and more generally those interested in evolutionary explanations of human behaviour, have asked? An example from the prehistoric ceramic record of eastern North America demonstrates that they can.

Archaeologists in eastern North America have long noted that tempers in pot sherds demonstrate significant variation in the materials used as well as variation in the preparation of these tempers (e.g., Holmes 1903). Figure 2.5 charts the changing frequency of shell-tempered pot sherds in Lower Mississippi River Valley assemblages and suggests that, as an attribute class or replicator, shell temper in this region is differentially replicated, with a dramatic increase in its replication beginning about AD 700, the date traditionally marking the beginning of the Early Mississippian period. How do we explain this differential replication? For evolutionary archaeologists differential replication may be explained by sorting. As Feathers (2006:101) states, 'Sorting operates

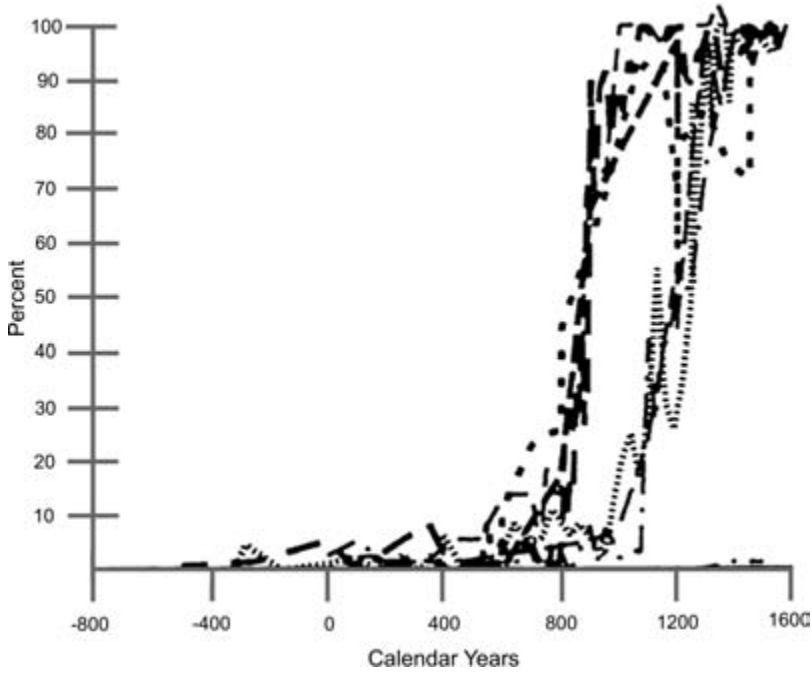

Figure 2.5 Frequency of shell-tempered ceramics in different regions of the Mississippi River Valley represented by different line types. (Redrafted from Feathers 2006: Fig. 1.)

on interactors and at any particular scale of interactor there can be two causes of sorting: those which are a consequence of differential environmental interaction and those which are not'. The first of these, differential environmental interaction, is selection.

To examine sorting, we must be able to define interactors in the archaeological record. For his analyses, Feathers (2006:116–17) makes the case that the ceramic assemblage is the interactor and notes that a set of interactors under examination must be culturally related to each other through transmission, interactors must be functionally independent from other entities (to ensure that we can differentiate pleiotropy or hitchhiking from other forms of sorting), and interactors must have a fast turnover relative to sorting. Therefore, to act as interactors, ceramic assemblages must be defined with these three characteristics in mind. Thus assemblages should correspond to some manufacturing and use unit within which transmission occurs. This could be individuals in a household, or in instances where there is a division of labour, multiple households or settlements. Assemblage boundaries should be expanded

until all parts of an independent manufacturing and use system are included. Typically in evolutionary archaeology the definition of assemblages at particular scales must be treated as hypotheses to be evaluated in the course of analysis. Feathers's research question becomes what qualities do temper classes, the replicators, impart to assemblages, the interactors, so that temper classes are differentially replicated. In particular Feathers is interested in measuring the variable environmental interaction of members of particular temper classes.

In the case of pottery tempers, materials science techniques have been used to assess the performance of different tempers in particular environmental contexts (e.g., Bronitsky 1986; Hoard et al. 1995; O'Brien et al. 1994; Schiffer and Skibo 1987; Sillar and Tite 2000). Our knowledge of the physics and chemistry of ceramic materials indicates that variation in aplastics may affect the workability of wet clay, as well as thermal shock resistance, mechanical toughness, transpiration and other qualities of vessels. By assuming what kinds of characteristics might be under selection, Feathers (1989, 1990, 2006) made ceramic test specimens recreating as closely as possible the materials and technologies of ancient potters and then measured the variable performance of test specimens under specific conditions.

For example, Figure 2.6 displays the variable toughness of ceramic test tiles measured as the speed of crack propagation after the bending strength of the ceramic has been exceeded in a static three-point load test. These graphs show the increasing load applied to a specimen over time. The apex of the graph is the point where the modulus of elasticity is exceeded and the slope to the right of this allows us to compare toughness of the samples as the time until total failure. Sand-tempered test tiles undergo complete failure more quickly than shell-tempered specimens. Additionally, the amount and size of shell temper also has a variable effect on toughness (Feathers 1989). Sand- and shell-tempered ceramics also perform variably in their resistance to crack propagation as measured by work of fracture, the total energy required to extend a crack. Other tempering materials also show performance variations in particular contexts.

Materials science analyses provide a test for the hypothesis that the increase in shell temper replicators is explained by selection for assemblages with relatively strong ceramics. Feathers continues, however, and asks is strength (i.e., ability to resist applied force) the characteristic by which assemblages are being sorted or are there other possibilities? Feathers explores the hypothesis that shell tempering leads to increased workability and that this is the characteristic under selection. Increased workability is advantageous in the context of making vessels of diverse size and shape, qualities of Late Mississippian vessels. Indeed, Feathers

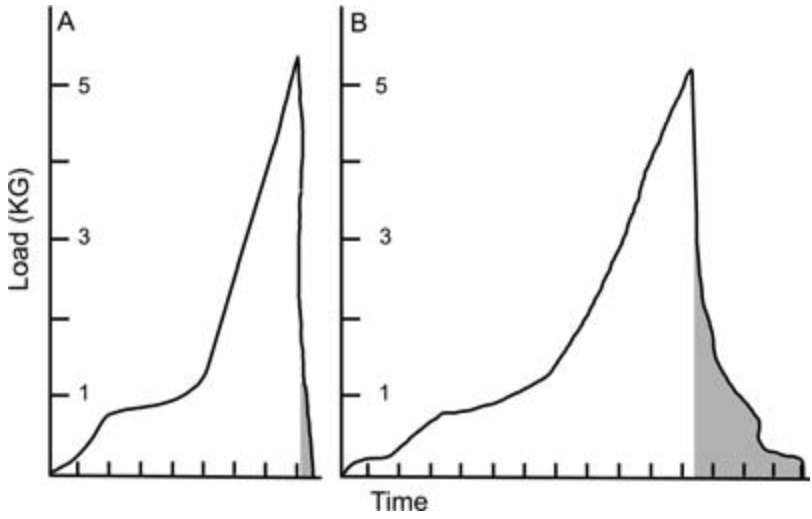

Figure 2.6 Results of a static bend test applied to ceramic test tiles with sand (A) and shell (B) temper. (From Feathers 1989: Fig. 1.)

demonstrates that workability, or yield value and amount of deformation without rupture, is maximised in shell-tempered ceramics, but he also shows that there is no increase in vessel shape diversity that is synchronous with the increased replication of shell temper classes. Feathers (2006:114–15) concludes that workability is not the property under selection that leads to the initial increase in shell temper classes, although increased workability does seem to come under selection later.

Establishing that the increased frequency of shell-tempered sherds is likely explained by differential environmental interaction measured by ceramic strength does not yet tell us why the increased frequency occurred when it did. In other words, can we identify the selective environment that arose approximately AD 700 such that replication was differential? Feathers (2006:118) argues that this new selective environment is associated with changes in pottery firing technology, noting that there is a difficulty in firing ceramics tempered with shell. First, shell temper should be pretreated with heat at about 300–400 °C to transform it from (likely) aragonite to calcite with a plate-like structure that increases strength and also expands volume. Vessel firing technology must also be sophisticated enough to maintain temperatures of 600–800 °C for a set amount of time as this changes the shell temper calcite to calcium oxide and carbon dioxide and thereafter the calcium oxide combines with other materials only after sufficient time has elapsed. If this temperature is not

maintained for sufficient time and the ceramic cools, the calcium oxide absorbs moisture, expands significantly, and can destroy the vessel.

We might expect that changes in firing technology led to a change in selective environment at a time when sand-tempered ceramics still predominated in assemblages, but that resulted in the later performance differences between shell temper and other tempers. Feathers (2006:118–22) assessed the chronology of different firing technologies by comparing sherd characteristics that measure firing environment. Through X-ray fluorescence and infrared spectrometry he examined the presence of particular clay minerals associated with high- or low-temperature firing conditions. Feathers also assessed the atmosphere of firing through Mossbauer spectroscopy and the oxidation state of iron in the ceramics. His results show that firing technology did change just prior to the increase in shell-tempered ceramics. To conclude, Feathers notes that selection for different firing technologies should occur at a larger scale than selection for tempering practices, as tempering practice can differ from vessel to vessel but firing strategies are largely dependent on the availability of fuel that would affect all vessels within a given area. Thus explaining the rise of shell temper across eastern North America may ultimately require larger interactors—more inclusive assemblage definitions—than those used by Feathers.

Discussion and Conclusions

The evolutionary archaeology case studies discussed here represent different applications of evolutionary theory to the explanation of material culture variation in the archaeological record. Lipo and colleagues' (1997) defined units (replicators) to measure transmission following Americanist culture-historical methods so that these units are likely neutral with respect to selection. Differential replication is therefore explained by the stochastic nature of transmission and population structure. The result is that Lipo and colleagues are able to map the spatial locations of cultural transmission lineages, the units within which evolutionary change can be expected to occur. Importantly, they demonstrate that replicators are units defined by the investigator, not discovered in nature (cf. Lake 1998), and these units can be evaluated by comparison with expected empirical distributions derived from evolutionary theory. This reinforces the notion that replicators or memes do not have a single-scale empirical definition, but are units the analyst defines in relation to a particular problem. In a similar fashion, biologists also increasingly recognise that genes are analyst-defined measurement units of varying scale (Portin 1993; Prohaska and Stadler 2008; Stotz and Griffiths 2004). The position that transmission units are analyst defined and etic is opposite to that

often taken by interpretive archaeologists where artefact classes may be considered to identify culturally meaningful or emic categories. Although not explicitly discussed in the interpretive archaeology literature, emic categories or artefact classes would seem necessary if archaeologists want to understand, for example, individual motivations behind the transmission of information (Sillar, this volume) or hope to track the purposeful expression of ethnicity (Sommer, this volume).

While evolutionary archaeologists such as Lipo have investigated the construction of transmission units and the definition of transmission lineages or traditions, Feathers has attempted to identify selection as the mechanism that explains differential replication within transmission lineages. Feathers (1989, 1990, 2006) and others (e.g., O'Brien et al. 1994; Pfeffer 2001; Pierce 1998) define replicators that are hypothesised to contribute to the differential environmental interaction of interactors and thus the explanation of differential replication by selection. Selection in these studies is not synonymous with the differential reproduction of human bodies (Leonard and Jones 1987), as evolutionary archaeologists, like almost all evolutionary scientists, distinguish between genetic and cultural transmission systems (Sober 1992). Also when differential replication in the archaeological record is explained by selection, this does not necessarily implicate past individuals' cultural criteria (Sillar, this volume) or motivations for choosing one variant over another. For evolutionary archaeologists, explaining the adoption of one trait or another from the perspective of someone's past intentions is untestable, as there are no unambiguous and universal expectations for evaluating past intention. Indeed, intention is not a particularly good way to explain outcomes in a wide range of contemporary settings, whether it be understanding the continued failure of governments to close the gap between rich and poor, the success of firms or who will win a game of chess (Ormerod 2005). In contrast, and as exemplified here, evolutionary archaeologists may evaluate the persistence of replicators (artefact classes) using distributional models for selectively neutral traits or the timeless physical measures of variable performance in the historically contingent environments suggested by the archaeological record.

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CHAPTER THREE

Action and Structure in Interpretive Archaeologies

Andrew Gardner

Introduction: The Emergence of the 'Agency Agenda'

The investigation of past agency has been one of the defining features of the interpretive perspective in archaeology, and yet has also generated great debate within this perspective. Indeed, few other concepts exemplify quite so well the ways in which interpretive archaeologies can be both harmonious and fragmentary. This situation creates all sorts of interesting contrasts and connections with other schools of thought, including Darwinian approaches; in this chapter I will endeavour to encompass both the internal debates in interpretive archaeology and some of these wider-ranging comparisons. The road that the concept of agency has travelled in archaeology has been a curious one, and I will argue that along the way something essential has been lost: the twin concept of structure. In this respect the 'agency debates' are illustrative of the ways in which theoretical development often occurs in archaeology, and the picture created is not an entirely pretty one. Nonetheless, these debates have generated significant contributions to a distinctive understanding of past people's lives, and there remains much potential for a more holistic interpretive perspective on the action-structure problematic to develop this understanding further.

The emergence of agency as an explicit theme in archaeology coincides with the advent of postprocessualism in the early 1980s. As with most strands of postprocessual thought, however, this process involved a recapitulation of arguments previously conducted in a range of other disciplines. The relationship between acting individuals—a simple definition of agents—and social institutions or structures has been referred to as the defining problem of sociology (Jenkins 2004:24; Parker 2000:14–15) and has deep roots in philosophy, anthropology and other

fields (see, e.g., Morris 1991; Todorov 2001; cf. Dornan 2002; Gardner 2008; Johnson 1989). Different approaches to this relationship were therefore built into major theoretical movements, such as Marxism and structuralism, that began to have significant influence in Anglo-American archaeology in the late 1970s. Of course, this had also been true of earlier influences, such as functionalism, but it is really only in the ferment of the early 1980s that the problem of agency moved to the foreground as a major issue. This is because it came to stand for one of the main points of departure for postprocessualism: the critique of systems theory and move towards a more 'personalised' past involving active individuals (Hodder 1985; 1991:27–28). Already, however, there were tensions between different elements of what would become 'interpretive archaeology'.

While the inconsistencies between different bodies of theory adopted by postprocessual archaeologists have been noted before (Chippindale 1993; Trigger 2006: 470, 481; see also comments on Shanks and Tilley 1989), this sin is perhaps more forgivable in the context of the early 1980s than is its perpetuation through to the 2000s. Those archaeologists seeking ways of understanding meaning and power in human societies in the 1980s engaged not only with the rich established traditions of structuralism and Marxism but also with much more recent writings in poststructuralist and neo-Marxist thinking. One of the first collections of the postprocessual era, *Symbolic and Structural Archaeology* (Hodder 1982a), builds upon not just the classic work of Lévi-Strauss, for example, but also the then rather recent developments initiated by Bourdieu and Giddens. The 1980s saw archaeologists rapidly engage with a kaleidoscope of social theories, and inevitably contradictions arose. Thus Hodder's early writings within this period tend to refer to knowledgeable, active individuals in the vein of Giddens and, to a lesser extent, Bourdieu (Hodder 1982b, 1985), while Shanks and Tilley turned more to the decentered and rather more culturally determined persons of Foucault and Lacan (Shanks and Tilley 1987:61–78, though cf. Shanks and Tilley 1992:122–29). Rather than these tensions being worked through in subsequent decades, however, they have been exacerbated as the debate has sucked in more and more social theorists without thorough digestion of their work. This continuation of a pace of theoretical turnover established in a revolutionary moment in archaeological thought has contributed to the current fragmentation of the discipline and, at least as far as the theme of agency is concerned, has impoverished the debate (cf. Dobres and Robb 2000; Johnson, this volume). A key argument of this paper is that a more in-depth working through of some of the foundational ideas deployed in these initial debates still has to be accomplished if the promise of the early 1980s is to be fulfilled.

Before surveying the themes that have run through the discussion of agency in interpretive archaeologies, it is worth expanding a little more on the originators of these foundational ideas. The twin titans of Marxism and structuralism have cast a long shadow over discussion of such issues in the whole of the social sciences, and certainly provide much of the context for the seminal work of Anthony Giddens and Pierre Bourdieu. Frequently cited by archaeologists throughout the emergence of the 'agency agenda' (e.g., Barrett 1994; Hodder 1991; Shanks and Tilley 1992; cf. Ortner 1984), books produced by these authors in the late 1970s and early 1980s have underpinned the entire debate in our field (esp. Bourdieu 1977; Giddens 1979, 1984). Both writers sought ways of articulating the reciprocal influence of social structures upon the practices of people and of people upon the formation of structures. Through their respective concepts of habitus, field and capital (Bourdieu) and of structuration (Giddens), they aimed to bridge the gap between micro/subjectivist and macro/objectivist social theories. In doing so they variously critiqued and synthesised the insights of Marx, Saussure and Lévi-Strauss, along with Durkheim, Weber and others (Giddens in particular makes reference to a wide range of phenomenological and interpretive thinkers).

Whatever the merits of these efforts (see below; cf. Jenkins 1992 and Stones 2005 for thorough assessments), this intellectual heritage framed much of the debate in archaeology in the 1980s and early 1990s. Indeed, Giddens's position at Cambridge in this period made him particularly influential on the postprocessual school emerging at the same institution (M. Johnson, personal communication 2009). Other poststructural thinkers were also important, especially Foucault, whose 1970s writings emphasising the historical specificity of contemporary ideas of personhood, sexuality and health chimed with the cultural relativist leanings of much postprocessualism (e.g., Foucault 1970; Martin et al. 1988; cf. Bapty and Yates 1990; McCall 1999), even if they also problematised the idea of the active individual (Baert 1998:125; Elliott 2001:78–102). In the 1990s and 2000s, other voices have been marshalled into the agency debate, particularly those of Alfred Gell (esp. 1998) and Bruno Latour and other actor-network theorists (Latour 1993; Law and Hassard 1999). While coming from different backgrounds—anthropology and science and technology studies respectively—these theorists have in common a dispersal of the locus of agency from human individuals into networks of people and things. This has appealed to archaeologists critical of the supposedly individualistic emphasis of earlier (particularly Giddensian) postprocessual discussion of agency. Such criticism has tended to emerge within the feminist and phenomenological strands of interpretive archaeology (e.g., Gero 2000; Gosden 2005; Meskell 1999; Thomas 2004). Thus reflected,

the diversity of interpretive archaeologies perhaps prohibits a coherent path of progress in the debate over 'agency theory'. However, in reviewing the substance of this debate in more detail, we must consider whether more resolution can be achieved, as Trigger (2006:470) is not entirely off the mark in writing:

> For the most part, postprocessual archaeologists who are concerned with studying social action have functioned as consumers and advocates for a wide variety of conflicting theories advocated by other social scientists. There is little evidence that they have systematically attempted to use archaeological data to evaluate, improve, and integrate these theories.

Internal Diversity: Key Themes and Debates

Some of the major issues at stake in interpretive archaeological discussions of agency have been matters of disagreement since the early 1980s, while others have emerged more recently. Running through these themes, however, has perhaps been one overriding question: how do we locate agency? As we will see, this has applied both at the conceptual (including definitional) and the practical levels. Important as this question might be, attention to it has been such that the debate has been skewed into an obsession with agency at the expense of structure (hence my frequent use of inverted commas when referring to 'agency theory'; in my view, this is something of a misnomer, reinforcing inattention to structural and institutional analysis). This point will be argued at greater length after a survey of the major debates that have occurred.

Foremost among these debates has been that concerned with the issue of individuality. After the straightforward initial plea to restore active individuals to overly systemic or behavioural accounts of the past (e.g., Hodder 1982b:5; 1985:2, 7; cf. Hill and Gunn 1977), the notion of 'the individual' as agent has come under increasing critical scrutiny, latterly being regarded as a largely Modern category of person (e.g., Fowler 2004: 11–22; Thomas 2004:119–48; cf. Hegmon 2003:219–22; Johnson 2006:122–23). Indeed, discussion of whether an agent should properly be thought of as a self-controlled, autonomous individual was already apparent in the mid-1980s, particularly in the more poststructuralist strands of the work of Shanks and Tilley (1987:61–71; see also MacGregor 1994). Others argued that such an individual was also often implicitly gendered as male, and as able-bodied, and this critique is one axis of the important debate over whether postprocessualism has been as open to feminism as is often assumed (Berggren 2000; Engelstad 1991; Gero 2000; cf. Sørensen 2000: 63–67). Following on from this, alternative modes of 'personhood' have been proposed, building

upon aspects of third-wave feminist theory (Brück 2001; Fowler 2000, 2002) and phenomenology (Thomas 2004:119–48), and also drawing upon anthropological studies of ethnographic contexts with apparently 'dividualistic' or relational forms of personhood (esp. Strathern 1988). These have in turn formed part of the impetus for discussions of agency (defined as power or activity) being distributed in networks, collectives and objects, on which more below.

One curious aspect of the critique of decontextualised, individualistic agency in archaeology is that, in practice, there are relatively few cases where this has been deployed. In his discussion of the first generation of postprocessual studies, Johnson (1989) noted that agency in any form was actually rather lacking. Subsequent discussions that have strongly foregrounded the individual have tended to do so either because of specific kinds of evidence (e.g., Flannery 1999; Hodder 2000) or because they are coming from the rather different starting point of methodological individualism (e.g., Bell 1992; Shennan 2004; cf. Knapp and van Dommelen 2008:16–17; Lake 2004). The latter is more frequently associated with scientistic cognitive and Darwinian approaches—providing an interesting point of contact between the traditions under comparison in this volume. The arguments over individuality within the interpretive paradigm are thus more about perceived flaws in different bodies of theory, and about the influence of Modernity over theoretical development, than they are about widespread practice. At this conceptual level, the blame for individualistic theories of agency in archaeology has typically been laid at Anthony Giddens's door (e.g., Bintliff 2004: 174–75; Knapp and van Dommelen 2008:22; MacGregor 1994; cf. Meskell 1999:25, who rather criticises Giddens's actors for being too constrained). This too is curious, being at best a very partial reading of Giddens's work, and it is indicative of the rather selective level of actual engagement in archaeology with this work and the extensive sociological literature building upon it (e.g., Bryant and Jary 2001; Cohen 1989; Stones 2005). Giddens is by no means alone in being misunderstood, but as a result discussion of the relationship between agency and individuals has generated rather more heat than light at either an applied or an abstract level.

Another issue embedded in that of individuality has also been divisive, and this is the matter of intentionality. A common reading of agency has been that it relates to choice, intentional action and creativity (cf. Sillar, this volume). This is implicit in one of Giddens's criteria of agency—the ability of an agent to have always done otherwise—and in much of the philosophical literature on the subject (Giddens 1984:8–14; Giddens and Peirson 1998:78; cf. Barnes 2000; Joas 1996; Macmurray 1957). It has also been an attractive reading in archaeology where identification of choices being made, for example in the production of artefacts, has

seemed to many one way into understanding individual action in the past, albeit a challenging one (e.g., Bell 1992; Morris 2004; Sillar and Tite 2000). However, as some archaeologists have highlighted (Barrett 2001; Dobres 2000), agency in the Giddens/Bourdieu mould is also about the way in which society is reproduced over time by people acting routinely or conventionally, where this reproduction is an unintended consequence of their everyday actions. This actually opens up the kinds of processes and scales of action that can be considered, and several studies now exist of longer-term patterns in social life that balance deliberate and routine practices (e.g., Fewster 2007; Gardner 2007; Joyce 2000; Joyce 2004). In some respects, this consideration of the unintended consequences of action is another point of contact with Darwinian approaches (a point I will return to below; cf. Graves-Brown 1996:168; Shennan 2004:30). However, a further, more recent trend within the interpretive tradition has been to detach agency from human intentionality more completely by dispersing it into networks of people and things (Knappett 2002, 2005; Tilley 2004), even if sometimes the things seem to take on an intentionality of their own (Gosden 2005). As with individuality, then, the relationship of intentionality to agency has become rather cloudy.

A further theme, closely linked to intentionality, has been the identification of agency with different forms of power, leading to attempts to locate actors among either leaders or subversives in past societies. As already indicated, the concept of agency is very much bound up with issues of power—the power to act, or the power to choose—and the influence of Foucault has been strong on interpretive discussions of power as enabling as well as constraining (McCall 1999; Miller and Tilley 1984; cf. Giddens 1984:14–16). In a broad sense this has come through all studies deploying 'agency theory', but some have focussed particularly on a more political dimension of social life and looked either to leaders/ruling elites as actors or to ways in which oppressed groups or classes retain power to negotiate or subvert imposed norms. The former is exemplified by Flannery's discussion of the role of particularly powerful individuals in the development of chiefdoms into states (1999), which employs an extremely narrow definition of agency; on this occasion complaints about actors being portrayed as autonomous supermen would not be far off the mark. Much more coherent and nuanced studies of socially prominent individuals within their contexts can be found in the work of Leone (1984), on a notable American revolutionary, and Johnson (2000), on English aristocrats in the Renaissance, among others. The contrary notion of agency as resistance or subversion is also apparent in a wide range of work, including Shackel's (2000) account of industrialisation in the eastern US, Chapman's (2000) discussion of funerary practice in later prehistoric Hungary, and various examinations

of colonial situations (e.g., Given 2004; several papers in Stein 2005). The way in which both of these apparently opposed trajectories tend to rely on certain kinds of evidence for exceptional practices (or perhaps exceptional evidence for certain kinds of practice) prompts us to consider an issue that underlies all of these discussions of agency: methodology.

However individuality, intentionality and power play into the different definitions of agency that have been used in archaeology (Dobres and Robb 2000:9), the various conceptual problems all have their implications in the practical challenge of 'locating' agency in archaeological data. In many cases, the focus has been on particular types of evidence that seem to give access to individual choices or individual bodies, such as texts or burials (e.g., Hodder 2000; Flannery 1999), and where these are lacking so too, it has been argued, is any evidence for agency (e.g., Arnold 2001; Morris 2004). The alternative view is that evidence for agency is everywhere, because all material culture is a product of human action (Barrett 2000, 2001; Dobres 2000; Dobres and Robb 2005; Johnson 2004). This approach tends to go with a greater appreciation of the interplay of structure and agency, and involves comparison of different patterns across time and space seeking repetition as well as variation (e.g., Gardner 2007; Joyce 2004; Sommer 2001), but it does face the problem that aggregative concepts (e.g., 'assemblage') and methods in archaeology may be obstructive to saying much about agency (Johnson 2004, 2006:123–25, and this volume). As methodological frailty is a charge that has often been levelled at interpretive archaeologies, particularly in regard to 'slippery' concerns such as agency (e.g., Kristiansen 2004:84; Renfrew 1994; Shennan 2002:9–10; Trigger 2006:468–83), this is an important issue. At the same time, the question of whether there needs to be a specific approach for understanding the structure-agency dynamic or whether, like gender, this is a pervasive theme that should always be considered regardless of one's methods is significant. More radically, attention to such issues perhaps demands a wholesale reconceptualisation of the nature of the archaeological record (Barrett 2001) and an emphasis on the involvement of material culture in the unfolding of past social life. In recent years, however, the debate has continued to focus on conceptual issues to do with the definition of agency, inhibiting progress with some of these problems.

One of these issues has been the notion of 'collective agency'. While it is clear that the idea of agents as autonomous individuals is a red herring, given that most discussions of agency in social theory and archaeology work on the basis of some relationship with structure (cf. Gardner 2008), recent attempts to move away from individuals have led to increasing reference to collective actors (e.g., Dobres 2000:133; Dobres and Robb 2005:162; Van Dyke 2008). To some extent, this reflects a

return to a Marxist conception of class consciousness (cf. McGuire 2002:133–34; Trigger 2006:469), and the exercising of power by groups may also offer greater purchase with intractable archaeological data. However, there are some ontological problems with detaching agency from embodied humans and investing it in collectives, not least of which is an obvious intrusion into what must surely be part of the structural domain—the world of institutions and organisations (Fewster 2007; Taylor 2008:307). It is also curious that a move taken partly to get away from the 'Modern' individual should lead to something comparable to the equally Modern world of corporate law where companies can act as legal persons. Having fallen into the trap myself of wanting to find agency in social groupings (Gardner 2004a), it rather seems vital to reconnect agency with embodied humans, in line with Giddens's assertion that 'the only true agents in history are human individuals' (Giddens and Pierson 1998:88; cf. Fewster 2007; Hodder and Hutson 2003:104; Meskell 1999:18–23). This is at least more logically consistent, as will be further argued below.

Raising similar problems of intellectual coherence are recent explorations of object agency (cf. Johnson 2006:125). Deploying a mixture of anthropological work including Alfred Gell's *Art and Agency* (1998) and actor-network theory (Law and Hassard 1999), the initial post-processual emphasis on active material culture has been pushed to its limits in some recent studies (e.g., Gosden 2005; Knappett 2002, 2005; Normark 2004; Olsen 2003; Tilley 2004; Webmoor and Witmore 2008). In this view, objects are not merely mediators of human agency, but form networks with human and other non-human beings through which agency is entirely dispersed. In some of this work there is a sense that the focus is on alternative, 'animist' worldviews and the reasonable claim that these need to be understood in their own terms (Sillar 2004; cf. Hodder and Hutson 2003:101–2; Meskell 2004:3–6). In other accounts, though, there is the clear implication that restricting agency or personhood to people is a Modern perversion and a denial of how powerful things really are (Fowler 2004; Gosden 2005 [*contra* Gosden and Knowles 2001:22–23]; Olsen 2003; Thomas 2000, 2007). Again, there is an irony in this dispersal of agency to things being claimed as an anti-Modern (or antihumanist) movement when it smacks of commodity fetishism (Graves-Brown 1996:177) or Darwinian memetic theory (Cloak 1975; Gosden [2005:198] remarks upon this comparison but fails to draw out the implications). That people depend upon a material world to develop and exert agency is an important insight, albeit hardly a novel one (McCarthy 1984; cf. Fewster 2007; Meskell 2004:50–55; Morphy 2009; Taylor 2008:307). It has been pushed to a breaking point in some of this recent work, however; to the point, indeed, where

humans are once again pawns of systems, as 'people crystallize out in the interstices between objects' in Gosden's words (2005:197; cf. and *contra* Giddens: 'Technology does nothing except as implicated in the actions of human beings' [Giddens and Pierson 1998:82]). With a return to determinism, has the agency debate in interpretive archaeology gone back to square one?

Strengths and Weaknesses of the Interpretive Mode

To answer this question, we need to assess the strengths and weaknesses of interpretive approaches to the action-structure relationship. There certainly have been positive developments. Among the most important of these has been the recognition that people are active in creating meanings and identities through material culture. This notion, one of the central planks of early postprocessualism (e.g., Hodder 1982c:185), has made a significant impact beyond the literature referred to in this paper and is regarded by Trigger (2006:452–55, 483; cf. Renfrew 1994:4) as the major contribution of interpretive archaeology to the field as a whole. Certainly it allows us to explore a fuller range of the power dynamics in any given context and look at material culture as much more than simply a reflection of social norms or a by-product of a system (Hodder 1985:8–9). The accompanying emphasis on how past actors understood the world they were shaping is also essential to analysis of the ways in which structures (and indeed agencies) were perceived and constituted in a specific context (cf. Emirbayer and Mische 1998). In this fashion it is possible to explore constraints upon action without succumbing to the usual universalising determinisms. Indeed, the second positive element of at least the earlier phases of the agency debate to highlight is their resistance to overdetermination. Many of the available models of human life deployed in archaeology have tended towards biological, ecological, cultural or technological determinism, but discussion of agency points in a different direction, where these structuring forces are all relevant, but not overwhelming. That this should be important, of course, is perhaps somewhat debatable, as to some extent this is a political point upon which views will differ (cf. Clark 2000). Nonetheless, there are few issues with more contemporary resonance than the limits of the human ability to act, whether in social (Strauss 2007) or environmental (Weigert 2008) arenas.

More concretely, much of the work informed by theorists like Giddens and Bourdieu has placed great emphasis on situated practice. In terms of method, and in spite of some of the issues noted earlier, there are now numerous comprehensive archaeological studies of people's lives in the past that look for both tradition and transformation

in practices via detailed contextual study of different types of material culture (e.g., Gardner 2007; Joyce 2004; Mizoguchi 2002; Sommer 2001). These approach the kind of 'ethnography' of past lives called for by previous generations of archaeologists (e.g., Taylor 1983 [1948]:170–72), but by unpacking practices and their location in time and space also address issues of power and inequality within very specific contexts. Far from being simply 'human interest stories' (Shennan 2002:10), these accounts have at least the potential to fit into ever-broader contexts of comparison (cf. Hodder 1991:143–46). This is due to another strength of the interpretive perspective: the potential for both understanding and explanation. One aspect of structuration theory that I find particularly appealing is its emphasis on taking account both of what actors think and what social scientists think—linked in Giddens's 'double hermeneutic' (1984:284). Archaeologists have a different relationship to past people than sociologists do to present-day people (Shanks and Tilley 1992:107–8), but the point that we seek to understand actors' lived experience as well as to take a more detached and analytical perspective is an important one if we are to highlight both the diversities and commonalities of human cultures. This point was fully recognised by Hodder in his early postprocessual writing (1985:3, 13), but has tended to fall by the wayside in some of the subsequent debate.

This brings me to some of the weaknesses in the interpretive approach. Many of these have opened up as a result of a lack of deep engagement with some of the theoretical traditions employed, and a seemingly ever-intensifying need to move on to the next idea with indecent haste. Foremost among these weaknesses is simply the imbalance in the debate in the direction of agency. The initial problems with deciding what agency should look like, encompassing the issues of individuality and intentionality in particular, have generated a theoretical obsession with agency at the expense of structure (cf. Harding 2008:161; Hegmon 2003:219; Joyce 2004:8–9). In the haste to ditch supposedly unsatisfactory definitions of agency, the role of structure has been forgotten. Much of the critique of structurationist or interactionist notions of agency has been misplaced because the role of structure in these theoretical frameworks has been neglected (e.g., Bintliff 2004:174–75; Knapp and van Dommelen 2008:22). The fact that the commonly used term for the subject of this paper is 'agency theory' is symptomatic of this situation; since action and structure are always intertwined, this term is misleading. This is much more than a semantic issue, however. The consequence of this discursive tendency in some archaeological accounts has been a flattening of the social world by the conflation of actors and structures. The reduction of social life into

a sort of 'agency soup' leaves us powerless to explore the differential situation of actors within society and the variation between different kinds of institutions. It also seems to belie the intentions of Gell (1998:16–23; cf. Meskell 2004:52; Russell 2007:77–79), if not of Latour (2005:171, though cf. 70–86; see also Cornell and Fahlander 2002; Knappett 2005: 30–33). Despite dubious accusations that structuration theory heads in the same direction (Archer 1995:93–134), the theoretical potential of the work of Giddens, Bourdieu and many other social theorists (especially within the school of symbolic interaction [e.g., Musolf 2003]) to deal with social structures has barely been explored in archaeology.

A further problem, which partly accounts for the disdain shown towards some of these theorists, is the reification of negative stereotypes of Modernity in much archaeological discourse and the consequent manufacturing of spurious 'Others'. This phenomenon is not entirely new, having its origins in the critique of capitalism that was part of the political agenda of early postprocessual writing (Shanks and Tilley 1987; Tilley 1989). Just as this critique could be somewhat crude, so the recent, more wholesale rejection of Modernity (Jorge and Thomas 2007; Thomas 2004; cf. Gardner 2004b) has been rather blunt and has created some unfortunate consequences. Ethnographic models of human life have been preferred to sociological ones in an attempt to escape the influence of the Modern (e.g., Fowler 2004), but such a move fails because, if anything, many sociological models actually undermine our stereotypes of Modernity, such as that of the atomistic individual (e.g., Dewey 1999 [1984]). Where particular points of Modern personhood are claimed to be distinctive in such theories (e.g., Giddens 1991), our task should be to assess whether this is indeed the case, given the broader range of evidence at our disposal (cf. Fewster 2007:109; Meskell 1999:26). This is far from saying that all societies are the same (cf. Hodder and Hutson 2003:104; Johnson 2000), but that we must be very careful about creating too hard a line between 'us' and 'them' (Fowler 2004:20; Gosden 2004:35; Knapp and van Dommelen 2008:17). In a similar vein, a final point of weakness in some recent accounts has been a confusion of empathy and analysis. We should not be stopping at describing people's beliefs about agency, even if we have good evidence for them—and more often than not discussions of non-human agency simply import ethnographies to make their point (e.g., Brück 2001; Fowler 2004; cf. Knapp and van Dommelen 2008:20; Spriggs 2008). This actually destroys sensitivity to context at the same time as inhibiting any degree of social analysis—of understanding the underlying 'principles of human social relation' in Hodder's words (1985:3). This negates our chances of contributing positively to archaeology and

contradicts our assumption that we can critique the present by refusing to believe everything we are told.

Agency's Neglected Twin: Structure

The major point that I wish to develop from this review requires some further elaboration, both in terms of the future direction of the debate within interpretive archaeology and in terms of comparisons with Darwinian theory. There is an urgent need to move away from both the terminology and the obsessions of 'agency theory' (cf. Clark 2000:97) and develop more rounded accounts of structuration and situated practice. This requires a rebalancing of the pendulum swings of the debate over the last 25 years and a deeper engagement with some of the still-vibrant theoretical traditions underpinning this debate. Only with such a move can viable alternatives to Darwinian accounts of cultural change be offered by archaeologists seeking a less reductionist mode of discourse. As Matthew Johnson pointed out in 1989, early uses of 'agency theory' were not active enough. These, perhaps, were a legacy of the emphasis on rather unyielding structures that were an element of Marxist and structuralist strands of thought influential in the late 1970s, and that were clearly important to the case studies he cites (1989:193–95). As I have argued, though, the progress of the idea of agency through the 1990s has been a path of greater diversification and elaboration. Some of the more focussed accounts of individual action have appeared, as well as reactions to them, through which the language of agency has spread to collective and material entities.

In many cases the arguments for these kinds of agency seem to rather be confusions with structure, and this creates a number of problems. Most obviously, the conflation of structure with the 'agency' of groups and objects actually recreates cultural and technological determinism, effectively making the concept of agency useless by locking people back into a passive role (cf. Hodder and Hutson 2003:104). At a deeper level, labelling collectives, networks or material contexts as 'agents' prevents us from using the full battery of conceptual tools for social and institutional analysis that Bourdieu, Giddens and others give us. These include notions of structural conditions and structuring principles (Barrett 2000, 2001), orthodox and heterodox knowledge (Sommer 2001), institutional distanciation across time and space (Gardner 2007; Joyce 2004) and mechanisms of domination and legitimation (Pauketat 2000). Such larger-scale sociological concepts are essential in accounting for the broad patterns of action that archaeological evidence tends to resolve at (cf. Johnson 2004; 2006:123–25; McCall 1999). They also enable us to deal with the long-term processes of social life that may lie beyond the immediate perception of actors, in a similar fashion to aspects of

Darwinian theory (Shennan 2002:9–10) but within a framework that is nonetheless congruent with actors' knowledgeability, there being no drastic separation between micro and macro scales of social theory (Roberts 2006:1–4). We need to be sensitive to the dynamics of power within collectives (Handley and Schadla-Hall 2004) and behind material interventions (Hodder and Hutson 2003:101), masked even though these may be from some actors in a given context, if we are to account for continuities and transformations in practice.

Contextual social and institutional analyses have, however, been lacking in much of the recent work which looks for 'alternative agencies', with the result that more is obscured than revealed. Gosden's search for the desires of objects in early Roman Britain (2005) pays no attention to the various institutionalised dimensions of life in this context and adds nothing new to our understanding of the processes of culture contact underway in this time and place (though cf. Gosden 2004:107–113 for a more useful account set within a more robust analytical framework). By contrast, Creighton's subtly structurationist account of the same period (2006) manages to account for much more of the variation in material culture of the time through attention to the different familial, civic, military and other institutions structuring practice in a range of locations, without losing sight of the actors generating these practices. This kind of more holistic account of the relationships between actors and structures in specific circumstances (see also, e.g., Gilchrist 1994; Johnson 2000) seems to provide a way out of the impasse of the 'agency debate' without abandoning the reasons for the commencement of that debate, nor the essential guiding lights that directed its early stages. There is too much untapped potential in theories of practice, structuration and symbolic interaction for these to be cast aside yet, and there exist rich archaeological resources, in historical contexts in particular, for exploiting this potential and developing understandings of human social life with considerable time depth.

Conclusion: Comparisons Across the Darwinian Divide

In some ways this paper has taken a rather narcissistic line in seeking to open up discussion about problems within the interpretive approach to action and structure. Indeed, such a line is, for some, an intrinsic characteristic of the interpretive approach (Chippindale 1993:35). The goal of this exercise is, however, directed outwards in the sense that the need for a more rigorous application of specific bodies of social theory that I have identified is intended as a response to charges of irresponsibility and purposelessness that have been levelled at postprocessual archaeologies (Shennan 2002:10). That some of the directions in which the 'agency debate' have moved fit this bill seems undeniable, but it does not have to be thus and,

as I have argued at the outset, elements of early postprocessual thought actually laid out a different path. This, then, is perhaps a *reactionary* paper with reference to those early ambitions (cf. Hodder 1982b). Accepting some elements of Darwinian criticism, however, begs the question of how wide the divide between interpretive and Darwinian approaches might be. There are certainly many overlaps and points of contact between these sets of ideas and, in line with some of what I have suggested above, the level of debate in archaeology would be enhanced were these more widely acknowledged, for better or for worse (for recent comparative exercises, see, e.g., Clark 2000; Kristiansen 2004; Trigger 1998). At the extremes, the antihumanistic trend in some recent interpretive archaeologies seems to lead towards cultural or technological determinism and striking parallels with memetic theory, which treats humans as passive carriers for cultural entities (cf. Gosden 2005:194 with Cloak 1975:172). Alternatively, the methodological individualism underpinning many Darwinian models (Kantner 2003; Shennan 2004:25) comes close to the autonomous actor stereotype that has featured prominently in much interpretive discussion. To me, neither of these extremes represent appropriate ways of coming to grips with past or indeed present lifeworlds.

There are other overlaps, though, with the range of social theories I have highlighted as offering the most promise for interpretive archaeologists, and ideas such as memetics are certainly not representative of all Darwinian theory (Cochrane, this volume). A stress on issues such as the unintended consequences of action is common to structuration theory and to Darwinian discussion of 'group selection' (cf. Graves-Brown 1996:173; McCall 1999:17–18; VanPool and VanPool 2003; see also Colleran and Mace, this volume). In the deeper intellectual heritage of these various schools of thought, of course, there are even stronger points of contact between key nineteenth-century thinkers—Darwin's influence on Marx or on G. H. Mead (a major figure in the development of symbolic interactionism, but sorely neglected in archaeology) should not be overlooked (Antonio 2000:116; Joas 1997:35). This might suggest that some of the differences between interpretive and Darwinian approaches are often simply a matter of choices of language and metaphor (cf. Bamforth 2002, 2003; O'Brien et al. 2003). It does seem unnecessary to me to burden social analysis with the terminology of selection, mutation and cost-benefit analysis when there are plenty of other, sociological terms to use that seem more harmonious with the subjects—the human subjects—we are dealing with. To some extent this is a matter of personal choice.

Nonetheless, I do think that there is something rather significant beyond these semantic differences. If postprocessualism has shown anything, it is that the kind of narrative we produce, and the language that we put it in, is of central importance to our goals as archaeologists

working in the present. The use of biological terminology seems to be part of a desire to simplify and reduce the social world to more basic and predictable processes operating at large scales; indeed, this is a deliberate part of the kind of scientific programme evolutionary archaeologists favour (E. Cochrane, personal communication 2009). The irony is that, as with some of the interpretive archaeologies critiqued in this paper, this leaves evolutionary approaches bereft of the tools to tackle the complex structures of the social world, and their intersection with human action. It is the social theory that was championed in the first wave of postprocessual archaeology, as well as some of its hitherto-neglected antecedents, that offers much more substantive means of undertaking the institutional analysis that is required to create genuinely holistic, multi-scalar archaeologies. Such (interpretive) archaeologies will offer more contextually sensitive ways of understanding as well as explaining complex particularities through exploration of all of the dimensions of structuration. The interpretive approach is thus far from being analytically powerless or methodologically flimsy, but certainly has yet to be pushed as far as it can be. In this respect the kind of engagement with Darwinian archaeologies that this volume represents can only come as a welcome challenge.

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CHAPTER FOUR

'Style versus Function' 30 Years On

R. Alexander Bentley

Style and Function and Behavioural Predictions

This volume roughly corresponds to the 30th anniversary year of Robert Dunnell's (1978) 'Style and Function' article in *American Antiquity*. Although I've not agreed with everything Dunnell has written, in recent years I've recognised what a prophetic article this one was. In short, Dunnell (1978) argued for a fundamental difference between cultural phenomena—stylistic versus functional (cf. Binford 1962, 1968; Sackett 1977; Wiessner 1983)—that could be identified by the patterns of change in the frequency of artefacts or behaviours through time. Dunnell (1978, 1980) identified 'style' as cultural elements whose frequencies changed by stochastic, Markovian processes and not as subject to natural selection, evolving the same way as 'neutral traits' in biology. The popularity of a stylistic element varies stochastically through time and space, with no rhyme or reason to its change from one interval to the next. In contrast, a 'functional' element catches on among people until everyone uses it, whereupon its popularity levels off, and it steadily remains at that level of popularity until the next technology comes along to replace it. These useful predictions paved the way for a particular adaptation of evolutionary theory in studies of culture change (Brantingham 2007). Dunnell's 'style versus function' exactly matches the continuum between random copying and selective copying of behaviours (e.g., Bentley 2007).

Dunnell's prediction for functional elements is much the same as the Bass (1969) diffusion principle of modern marketing, as well as cultural evolution models of independent decision makers who weigh the costs and benefits of their options (e.g., Gintis 2007; Henrich and Gil-White 2001; McElreath and Boyd 2007; Winterhalder and Smith 1999; Colleran and Mace, this volume). Such models apply best to technologies and behaviours that seem likely to affect the reproductive success of populations, where one choice is not as good as (or at least not

equivalent to) the other, such as conversion from foraging to farming (e.g., Renfrew 1978), the spread of an adaptively useful technology (e.g., Henrich 2001; Rogers 1962) or even 'costly' artistic expression, when viewed as an effective mating strategy (e.g., Bliege Bird and Smith 2005; Geher and Miller 2007).

At the other end of the spectrum, Dunnell's prediction for stylistic elements is what we now call the neutral model, where there is a large variety of choices that are all essentially equivalent in the sense of utility, such as carpet designs (Tehrani and Collard 2002), pottery decorations (Neiman 1995; Shennan and Wilkinson 2001) and word forms, for example. The reason that Dunnell's 'styles' are now referred to as 'neutral' traits is that they have no inherent value in and of themselves (Binford 1963; Gillespie 1998; Koerper and Stickel 1980). On their own, one neutral trait is as good as any other. The value of a neutral behaviour is strictly a matter of who else has adopted it—one can think of all sorts of 'do as the Romans do' examples such as language accents, dance styles, clothing fashions and so on. These are constantly changing, as Dunnell (1978) described, but at any given time they draw people in to doing likewise. Conceptualised this way, the study of material culture popularity can take advantage of the well-developed neutral model of population genetics (e.g., Gillespie 1998; Ridley 2003) or economics (Brantingham 2007).

Of course, predicting popularity change is the golden goal for advertisers and marketers and anyone else who wants to make a few pennies. Funny then, how after a century of economic theory, marketing studies and punditry, no one has succeeded in predicting fashions (see Gladwell 2006). In fact, a consensus is emerging that what matters in fashion is not *what* the next big thing turns out to be, but *who* adopts it, how they are networked with other people, and how influential they are among those other people (e.g., Gladwell 2000; Watts 2003). Economists who are well aware of this are pointing out that classical consumer choice theory, involving rational agents making independent cost-benefit decisions, does not work for markets where people influence each other's choices (e.g., Beinhocker 2006; Earls 2005; Ormerod 1998, 2005). Even concepts of fairness, something some archaeologists may take to be an innate human universal, have been shown to be culture specific—via doing as the Romans do—through an extensive cross-cultural study involving experimental games (Henrich et al. 2005).

The key difference between style and function, that cultural element frequencies are determined by stochastic processes, or by usefulness and adaptation, was nicely demonstrated by a recent Internet-based experiment on music downloading (Salganik et al. 2006). When subjects were allowed to download music by themselves, presumably based upon

listening and choosing what actually sounded good, the experiment converged upon the same kinds of music being popular each time it was run. In this case, the popularity of music could have been predicted, in theory, by someone with a good ear and knowledge of what people like. The results were quite different, however, when subjects were allowed to view each other's choices. When the opportunity arose to copy one another's choices, download frequencies were stochastic, and the popularity of specific music downloads was unpredictable (Salganik et al. 2006). A similar unpredictability describes how individuals learn to make projectile points, which critically depends on whether the learning is done independently or with the opportunity to view the ongoing choices of other people (Mesoudi and O'Brien 2008).

In general, fashions, or Dunnell's styles, appear to be fundamentally unpredictable, even though they change at a remarkably regular rate (Bentley et al. 2007). This is why the neutral model, a stupidly simple model of random copying among individuals (with occasional innovation), can fit many of the data patterns of stylistic change (e.g., Bentley and Shennan 2003; Hahn and Bentley 2003; Lipo et al. 1997; Neiman 1995; Shennan and Wilkinson 2001). Crucially, it is not proposed that people act randomly, but that the statistics of all their choices, at the *population* level, are comparable to random copying. Against this background 'canvas', more interesting phenomena become visible (e.g., Eerkens and Lipo 2005; Herzog et al. 2004; Glatz et al., this volume). Shennan and Wilkinson (2001), for example, observed that pottery design frequencies fit neutral model predictions for the Early but not the Late Linearbandkeramik (LBK), which in turn suggests either people were receiving new ideas from outside communities or they were becoming more creative. In any case, the neutral (random copying) model raised these new interesting questions about Late LBK society, simply through analysing the frequencies of pottery designs in one location (Collard and Shennan 2000; Shennan and Wilkinson 2001).

In hindsight, what Dunnell (1978) termed a 'dichotomy' may be more useful to consider instead as a spectrum. Given the two extremes—style versus function, or random copying versus selection—often the question is, where do behaviours lie on this spectrum (e.g., Brantingham 2007; Collard et al. 2006)? If behaviours are chosen for some function, they should be predictable at the population level, but if they are randomly copied styles, they should be inherently unpredictable. Using these contrasting models for patterns of change through time we can figure out where a real-world case lies on this spectrum.

One necessary clarification is the difference between random copying and conformity. Do you dance like everyone else in the room because you are conforming, or because everywhere you look you see the same

ideas for your moves? Conformity can be seen as an adaptive decision, requiring a proficient assessment of what others are doing, to intentionally copy the majority and gain acceptance in a group (see Henrich and Henrich 2007). Quite differently, random copying, or the neutral model, is almost like putting one hand over your eyes, pointing at someone randomly, copying their behaviour and repeating this regularly. One process is intelligent, one is dumb, yet the results can be quite similar—many people dancing the same way (like rag dolls filled with Mexican jumping beans, last time I went out), and yet the popular behaviour has no *intrinsic* value other than other people are doing it. In other words, it's not the 'running man' (think MC Hammer, ca. 1990) *in and of itself* that was useful in the 1990s, it was its popularity—if we 'replayed the tape' of cultural evolution, it might have been the 'bus driver' or something totally different.

Subtle patterns may discriminate copying from conformity in popularity data (see Bentley and Shennan 2003; Cochrane 2001), but in any case, copying each other's behaviours is a ubiquitous human tendency, whether it be *selectively* copying under influences of conformity, domination or prestigious individuals (Henrich and Gil-White 2001), or *randomly* copying, where people hardly think about it (Bentley et al. 2004). In fact, the effects of mirror neurons in our brains indicate that we are specially evolved to copy each other (e.g., Mukamel et al. 2010).

In the next section, I discuss how a random copying model (style) appears often to apply more strongly than purposeful selection (function) concerning language use and even academic ideas. The examples I use are not necessarily archaeological, but it should be clear how the modern cases can inform the archaeological ones and vice versa, perhaps leading towards a future synthesis on the evolution of language and popular ideas from the origins of history.

Self-Similar Thinking

Copying applies to the way archaeological theory is generated by scholars, in my opinion (Bentley 2006). By copying, I do not mean plagiarism, but just the fact that ideas of previous publications are adopted in any scholarly publication, as recorded in the references and bibliographies to credit who expounded those ideas before us. How we adopt ideas varies. Some copy references from other papers without reading the articles (see Simkin and Roychowdhury 2003), some refer to prestigious or well-known authors, as I am doing with Dunnell (1978) in this chapter, and some take stock of the latest ideas and conform to them by citing the theory of the month. They are all forms of copying.

'Not I', you may be thinking, and of course all authors create new ideas to some degree; otherwise it *would* be plagiarism (people do, however, notoriously overestimate how original they are). Indeed the neutral model involves a component of innovation to match real-world data patterns. If innovative ideas are particularly good, they may catch on and perhaps even begin a new paradigm (Kuhn 1962), which often acquire labels like 'processualism', 'evolutionary archaeology' and 'structuralist' or 'interpretive' archaeology. One of the illuminating aspects of the neutral model, however, is that even ideas or buzzwords that were never very useful—like the word 'nuanced' (Bentley 2006)—can become highly popular simply through copying (Bentley et al. 2004), with initially obscure innovations ultimately driving the continual change in current popularity (Bentley et al. 2007).

Hence we might look for just two main processes in the evolution of archaeological theory: innovation and copying. The result is a treelike process, with people continually coming up with new ideas, of which a few are copied and most are not. Most academic journal articles, for example, are never cited at all (Bentley and Maschner 2000, 2003; Price 1965; Redner 1998). If those early adopters of the idea are copied again by a majority of authors, the idea may be well on its way to becoming the next big thing. This process is well known in one form or another (e.g., Collard et al. 2006; Hull 2001).

In an insightful chapter called 'Tribal Encounters', O'Brien et al. (2005:235–68) provide a figure very much like Figure 4.1, which shows the treelike nature of this budding off and copying process. In their 'ethnography' of academic publishing, O'Brien et al. (2005) liken the process to an evolutionary tree or phylogeny. I would go a step further and claim that the branching process is actually self-similar, or fractal, based on previous studies using citations data (Bentley and Maschner 2000, 2003). In other words, at any scale you look, you will see the same process. On the large scale, for example, you might see archaeometry splitting off from general archaeology. Zooming in, you then see archaeometry itself splitting into fields of isotopic analysis, trace element analysis, microwear studies, remote sensing and so on. If you then zoom in on isotopic analysis, for example, you would see a focus on carbon and nitrogen isotopes versus strontium or carbon isotopes, and zooming in further you see some individuals committed to measuring those isotopes by one lab method and some by another.

What makes the generation of archaeological theory potentially fractal—that is, truly self-similar as opposed to just treelike—is that the relationships between branch and branched off may be the same at all scales. That is, archaeological theory in general might be five times more prevalent than archaeological science, which is five times larger than isotopic methods,

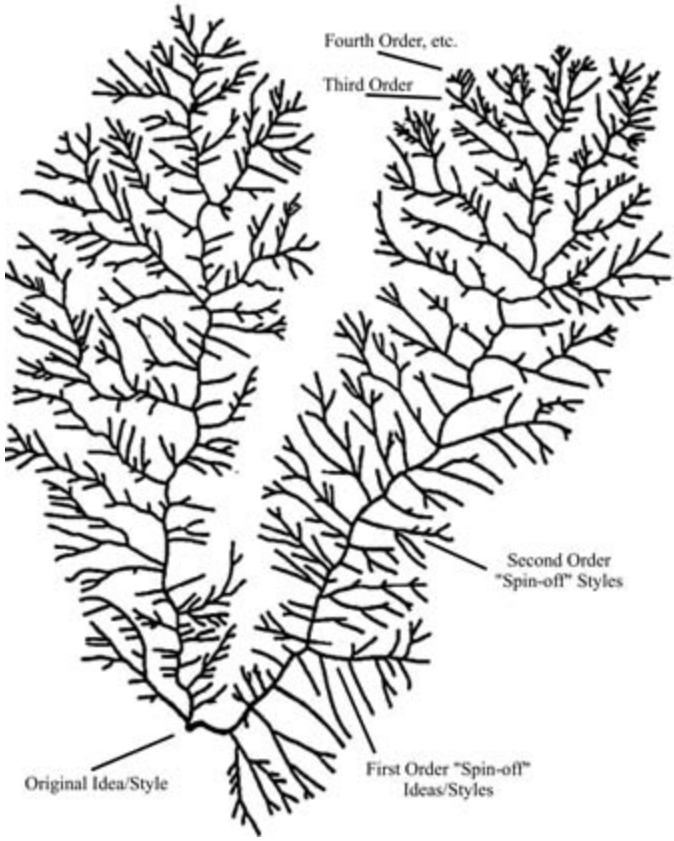

Figure 4.1 Illustration of the spread of an idea as a fractal growth process, as adapted from a real river network depicted by Turcotte (1997: Fig. 8.1).

which is five times larger than carbon isotope applications, which is five times larger than carbon isotope measurements in skeletons, then again for measurements of carbon isotopes in tooth enamel carbonate.

Like a fractal river system with its increasing orders of tributaries (after which Figure 4.1 is adapted), each specialisation occupies a territory that is another order of magnitude smaller, yet I claim the relationship between the branches is the same—they split off, and often begin to compete, no matter how small the academic territory. The branching relationship of interpretive archaeologies to evolutionary archaeologies, for example, is like a smaller-scale version of that between humanities and biological sciences outside archaeology (see Colleran and Mace, this volume), or a larger-scale version of that between structuralist and semiotic approaches within interpretive archaeology.

Thinking about self-similarity, the old clichés about the smaller the field, the bigger the battle just changes to say that the battle is always the same size relative to the territory. This is true of warfare in a wide range of societies (Maschner and Reedy-Maschner 1998; Roberts and Turcotte 1998; see Layton, this volume) and similarly true of academia (O'Brien et al. 2005: Ch. 9). In fact bitter, either-or disputes can rage literally for decades on topics as specific as whether it is better to analyse pot compositions by dissolving them in acid, blasting them with neutrons, or just looking at the grains of sand temper. Consider a recent debate, sparked by a *Science* article (Blomster et al. 2005) on the elemental analysis of Mesoamerican pottery. The study rekindled a battle from ten years before concerning different methods of elemental analysis (Burton and Simon 1996; Neff et al. 1996) and then proceeded like a massive brawl in hockey or basketball—beginning on the main floor (Blomster 2005; Stoltman et al. 2005), and as more and more people piled in on either side (Flannery et al. 2005; Neff et al. 2006a), it spilled into more peripheral areas (journals) where they were allowed to slug it out (Joyce et al. 2006; Neff et al. 2006b; Sharer et al. 2006).

It is clear that, just as we have splitters and lumpers in artefact classification, some archaeologists want us all to get along, while others are spoiling for a fight. There is nothing wrong with this, in fact one learns a tremendous amount about archaeological chemistry from that debate over pottery composition analysis. Similarly, other academic contests have outlined the crucial substantive ambiguities in the discipline and then advanced them, such as Binford (1973) versus Bordes on the nature of cultural assemblages, Schiffer (1985) versus Binford (1981) on the 'Pompeii premise', Gould (1985) versus Binford (1985) on empiricism, and Binford et al. (1988) versus Bunn and Kroll (1986, 1987) on taxonomy and hunting versus scavenging among early hominids. OK, many classic battles have involved Lewis Binford, and isn't that why he is regarded as one of the most influential of American archaeologists? I claim you will find self-similarity here, in that these battles occur at all scales—from grand and sweeping to microspecific—with the same ferocity, clarification of ambiguities and potential to produce a new bifurcation.

Interpretive versus Evolutionary Archaeologies

Perhaps this helps explain how evolutionary and interpretive archaeology came to seem so far apart. I think that these two schools started out quite close to one another about 30 years ago, and then after a bifurcation in response to the conventional theory at the time, each went

on with its branching process, colonising a different academic territory. Like new river tributaries eroding upstream and filling up different upland catchments, the different disciplines (and subdisciplines and sub-subdisciplines within this fractal architecture) became more and more committed to their separation, without *necessarily* involving any conscious intention of the respective practitioners.

Back in the 1980s, Robert Dunnell was the leading figure in his new brand of evolutionary archaeology—emphatic, polarising and relentless. For most today, it is no longer controversial that evolution applies to humans with respect to their environment, as human behavioural ecology has maintained for decades (e.g., Cronk and Gerkey 2007; Jordan 2007; Winterhalder and Smith 1999). Dunnell (1980), however, was writing during a popular time for functionalism, which he saw as a confused mix of notions about culture adaptation, 'progress', cultural 'fitness', competition and natural selection. Cultural fitness, Dunnell (1980:42) argued, could be assessed independently of human reproduction. Later that decade, his students became vociferous proponents of applying evolutionary theory to archaeology, with arguments for scientific rigour reminiscent of the processualism of the 1960s.

At the same time in the early 1980s, figures such as Ian Hodder (1982) and Alison Wylie (1982) were exploring 'epistemological issues raised by a structuralist archaeology', as Wylie (1982) titled one of her articles. Making the case that artefacts have specific symbolic meanings, Wylie (1982) argued that just because structuralist archaeology is less concrete/empirical (than processualism) and deals with unobservable causes, does not mean it is invalid. It just means that the best way to come up with hypotheses is through ethnographic analogy.

As we would expect from two new approaches just beginning their divergence, fundamental similarities between evolutionary archaeology and postprocessual, or more specifically structuralist, archaeology are still apparent in the early 1980s. In fact, if we can consider more specifically Dunnell and Wylie as examples, each expressed the same misgivings about previous theory. Both found environmental determinism to be of little use in explaining past cultural change. Like Dunnell, Wylie (1982) found the hypothetico-deductive method of 1970s archaeology (e.g., Fritz and Plog 1970) to be flawed because it could not successfully test the theory versus the observed archaeological evidence.

In evolutionary theory, one looks for the way in which variation persists through time, and from the standpoint of the cultural traits themselves, the important characteristics are longevity, fecundity and copying fidelity (Dawkins 1976). Similarly under Wylie's (1982) structuralism, the challenge was to prove that meaning determines the relationships between cultural constructs, and then to show that the meaning is

expressed with fidelity and regularity. Also, classification of variation is essential in evolutionary archaeology, such that phylogenies (evolutionary trees) of artefacts or behaviours can be constructed to show change and inheritance through time (Collard et al. 2007; O'Brien et al. 2002). Similarly, the structuralist approach meant that cultural constructs should be ordered in some classification that demonstrates a particular worldview (Wylie 1982). An architectural style, for example, might include a repertoire of geometrical forms and a set of rules for how to put them together (Wylie 1982). For both evolutionary and structuralist archaeology, in my view, the search for meaning underlies artefact classification. In the spirit of this volume, however, many might disagree that the rules and repertoires of a structuralist approach are means to the end of the 'worldview', but simply ends in themselves.

Bifurcations between Evolutionary and Interpretive Archaeologies

Given their mutual dissatisfaction with preexisting theory, it was what the evolutionary and early structuralist schools did with this situation that made the difference and perpetuated a bifurcation. Looking for a fractal process as in Figure 4.1, we might see the seeds of the first bifurcation beginning to be developed.

Dunnell maintained that evolutionary theory was the only scientific way to extract any meaning from the archaeological record. Barely any archaeological theory was safe from his critique. In his view, many of the myriad algorithms, paradigms and theories that had been floated in the history of archaeology lacked any means of determining what was true or false in the past (Dunnell 1992). The mere accumulation of facts, he argued, without a method of falsifying interpretations about them, prevented any archaeological knowledge from building. As more and more plausible, but untestable, assertions piled up to explain the facts, archaeology had reached a state unfit to be called a science (Dunnell 1992).

Wylie (1982), on the other hand, saw the facts themselves as being *biased* by the archaeologist's theory, as structuralists did not see data collection as separate from model formulation. Whereas Dunnell (1980) saw one theory (evolution) and essentially one way to test it (measurement of variation through time), Wylie (1982) saw a much more open-ended question-and-answer process, paving the way for later post-processualists to embrace the state of uncertainty in archaeological knowledge, maintaining its inevitability. Linda Patrick (1985) later contrasted the processualist view of the archaeological record with the textual model motivated by structuralism. The processualist view was essentially that of the 1960s New Archaeology, a physical model

based on palaeontology in its uniformitarian assumption that a record of facts lay in the ground through which all was knowable through universal laws, as defined by the likes of Binford, Schiffer, or Fritz and Plog (1970). While processualists like Binford would not have us study mental phenomena, Patrick (1985) used the textual-structuralist model to treat the archaeological remains as an active record that communicated useable messages to the reader. The translation of these messages was not determined by covering laws, but a grammar by which the material record symbolised past concepts. How material culture relates to society was to depend on the ideological structures and symbolic codes (Hodder 1985).

As any but the most New Age archaeologist would agree, both approaches saw past thoughts—what Wylie (1982) would call 'cognitive structures' and Dunnell would call 'intentions'—as not being directly observable in a prehistoric archaeological record. Wylie (1982), however, argued that this is only a problem if you think only observable data are knowable. But the best theories reach beyond observable data and can be used to predict what will be observed. Hence Wylie (1982) argued that archaeologists can, if they are careful, infer past cognitive structures as expressed in artefacts (cf. Sillar, this volume). In contrast, human intentions for Dunnell (1989:37) were as irrelevant, in an explanatory sense, to prehistoric cultural evolution as 'oak tree intentions' are to 'oak tree evolution'. Later, this issue of determining past mental states would became one of the flash point issues of Evolutionary Archaeology.

Who Wants Reconciliation? Bifurcations upon Bifurcations

In the next phase of the bifurcation process, both evolutionary and interpretive archaeologies became more radical, becoming ever more distinctive from each other and among the subfields within them. As they did so, they each proceeded to erode territory further upland, using the metaphor of the river system in Figure 4.1.

As interpretive (postprocessual) archaeology further evolved along its own 'tributary', Shanks and Tilley (1987) and Thomas (1990) argued that archaeological facts do not speak for themselves, as we can only tell a story using the facts. The relevance of this was determined by what happened afterward. As interpretative archaeology became increasingly literary, the archaeologist was likened to the narrator, with material culture as the text. There is nothing but this text to read, Thomas (1990) argued, from which the narrator must choose the pivotal events.

As Thomas (1990) put it, because material culture is an imperfect language, by interpreting artefacts we are interpreting an interpretation.

For this reason, Thomas (1990) argued that an analogy must be specified in terms of its model of history, whether it be essentially the same as today (middle range theory, universalism) or distinctly different from our own experience, with no constants (e.g., are mortality and sexuality just transient notions of modernity?), or some reconciliation of the two. This kind of inquiry led interpretive archaeologists to draw from hermeneutics, the study of the relationship between the interpreter and meaningful material. Shanks and Hodder (1995) viewed classification more sceptically than had Wylie (1982), as subjective, artificially discrete and dependent upon the method of interpretation.

Other structuralist concepts were made more radical as well. Taking Wylie's epistemology a step further, Shanks and Hodder (1995) argued that knowledge is *inherently* uncertain and underdetermined, which represents the (external) influence of poststructuralism. Since infinite possible measurements can be made of the archaeological record to support plausible interpretations, Shanks and Hodder (1995) advocated a free-association approach in looking for meaningful connections in the record, rather than just measuring and comparing what scientific method dictates. They had effectively gone to the opposite extreme from the evolutionary archaeological goal of archaeology as a science; archaeology is narrative, and the creativity and subjectivity of the interpreter has to be acknowledged (Shanks and Hodder 1995). Even identifying a 'posthole' in the ground was an uncertain, creative act.

Such radical departures from the 'normal' scientific thinking was predictably critiqued as relativist, and the ensuing debate was sometimes interesting but often led to a rather mundane talking past one another. Kelly (1992), for example, argued that archaeology is not easy, that something is knowable from the archaeological record and that objective knowledge of the past is better than creative interpretations, because objective science brought us penicillin and satellites to Jupiter. This kind of critique was based upon a misconception, as Shanks (2008:133) recently clarified:

> It is not difficult to find a caricature of post-processual archaeology, in textbooks, among excavators on a field project, anywhere archaeology is a matter of debate rather than simply a source of information about the past. The caricature takes the form of an archaeology rooted in an abstract body of difficult (and probably irrelevant) theory which, in opposition to processual archaeology, celebrates historical particularity and the individual and lacks a methodology that can deliver any kind of secure knowledge. The proponents of post-processual archaeology, in this caricature, are often seen as overly politically motivated, as much interested in contemporary cultural politics as in developing knowledge of past societies. I call this a caricature because a careful reading of the primary literature shows that it makes little sense.

Shanks is right about postprocessualism's critics, but it also seems that postprocessual archaeologists have actively sought to differentiate themselves, make a clean break and hasten the bifurcation. It was easy to pay empty lip service to reconciliation, but only on the uncontroversial matters. Kelly (1992), writing more or less as a processualist, was happy to do archaeology that was relevant to modern society, present more to the public and teach debate over cultural process rather than uncontested culture history. Who could argue with these things? They are pragmatic and fairly self-evident.

Theory, on the other hand, is much less negotiable. When Patrick (1985) suggested the physical (i.e., processual) and textual model for the archaeological record could be reconciled, she did not mean it, since her article overtly promoted the textual model over the physical model. The general idea of theoretical reconciliation (e.g., Hegmon 2003) seems to be fundamentally objectionable, causing people to choose sides (Hegmon 2005; Moss 2005). I think the reason for this is that Figure 4.1, and its process of bifurcation, is a natural model for how human ideas evolve, especially when people are competing for intellectual prestige. Here my view, very similar to O'Brien et al. (2005: Ch. 9), contrasts with those in this volume (e.g., Mace and Colleran, Tehrani) who argue that interpretive ideas will be absorbed into the Darwinian explanatory framework. I am not saying they *ought* not be reconciled, for that would be great, just that they *shall* not.

Bifurcations within Evolutionary Archaeologies

As modelled in Figure 4.1, after evolutionary and postprocessual archaeologies split from their processual forebears (with processualists holding together to this day as lumpers), they continued the same self-similar story of bifurcations within each respective smaller category (some might point out that evolutionary and postprocessual archaeologists have occasionally revived culture history [e.g., Hodder 1982; Lyman et al. 1997], and therefore the process is cyclical. I do not agree at all, because the revised version is never the same as the original). I will focus in on evolutionary archaeology, the branch with which I am more familiar. Dunnell (e.g., 1980) fervently argued for archaeologists' use of evolutionary theory, strictly on the terms he defined, which caused a further bifurcating with other evolutionary theorists.

In this sense, it is not so remarkable that Dunnell's brand of evolutionary archaeology alienated even other early proponents of evolutionary theory in archaeology. Particularly controversial was his insistence that human intentionality is unimportant to cultural evolution. Another was his treatment of artefacts as part of the human phenotype. His

arguments radically countered conventional views of human cultural transmission.

In lumping human artefacts into the category of the human phenotype (e.g., pottery as analogous to bird's nests and spiderwebs), Dunnell provocatively argued that mechanisms of inheritance need not be known in explaining cultural evolution. If a given cultural trait is heritable to a measurable degree, and if it also affects the fitness of the person using it, then that trait was subject to natural selection—full stop (Dunnell 1980:49). Although he advocated the application of biological evolution to cultural phenomena, Dunnell (1989:39–40) did not, as is often assumed, require that genetic transmission be mapped on to all parts of the human phenotype, because 'a large fraction of the human phenotype, including most behaviour, is demonstrably the product of a different system of transmission: cultural transmission'.

Under Dunnell's influence, evolutionary archaeologists were arguing that to explain prehistoric artefacts as being the way individuals *intended* to make them could not explain anything at all on the evolutionary time scale. The argument of evolutionary archaeology was more to say 'forget what people's intentions were, let's focus on tracking artefact variability through time and across space', which could then be subject to evolutionary analysis. In other words, 'intentions' could not themselves be an explanation for behavioural change, because intentions are part of the very behaviour that is subject to evolutionary natural selection (cf. Sillar, this volume).

These ideas were just as vigorously critiqued by human behavioural ecologists (e.g., Boone and Smith 1998), concerning such issues as phenotypic plasticity, the difference between genotype and phenotype and the units of cultural versus genetic transmission. As the debate expanded into graduate seminars and conferences of the late 1990s, many mistook it for a semantic misunderstanding over 'intentionality'. The published debate, however, made explicit the question of temporal scale of evolutionary analysis—on a microevolutionary, essentially ethnographic time scale, intentions are quite relevant (Boone and Smith 1998), whereas on a macroevolutionary, much longer time scale, speaking of 'intentions' only conflates the tempo and mode of evolution (Lyman and O'Brien 1998, 2001).

This debate occurred at the same time that dual inheritance theory (e.g., Boyd and Richerson 1985) was gaining more interest, focussing on the ways individuals learned behaviours from their parents and unrelated contemporaries. The debate was intense enough that many human behavioural ecologists might have called themselves evolutionary archaeologists were it not for 'Dunnell-vision', which some critics described as genetic determinism. In the process, the label 'evolutionary

archaeology' (EA), as general as it was, came to refer specifically to the brand advocated by Dunnell and his students.

In many ways, the debate over intentionality came to resurrect the nineteenth-century contrast between Darwinian and Lamarckian evolution. Critiques of EA stressed that it was not warranted to rule out Lamarckian evolution (where individual experience can be passed on to subsequent generations) in cultural transmission just because Gregor Mendel (1822–1884) discovered it was not the case for biological evolution. This was never the EA argument, however; as Dunnell (e.g., 1989:89) himself often repeated, evolution is just the differential persistence of variation through time, through transmission and selection. With the dust settling now, both sides clearly see nothing in the definition of evolution that forbids human knowledge from being passed on to subsequent generations (e.g., Gabora 2007; Lake 1998; cf. Cochrane, this volume).

What Has Been Gained?

As interpretive archaeologists in this volume highlight their achievements (and see Gardner 2007; Shanks 2007), I will focus on how this continual process of copying and innovation, growth and division has developed evolutionary archaeology. Despite—or perhaps even because of—the controversies over Dunnell's brand of culture evolution in archaeology, his single-minded effort to bring evolution into archaeological theory motivated a great deal of debate, research and interest in cultural evolutionary studies. As with Lewis Binford (on a smaller scale perhaps), many found urgency in Dunnell's cause, particularly his former students (e.g., Cochrane 2001; Hurt and Rakita 2001; Madsen et al. 1999; O'Brien and Lyman 1996), while many saw his theory as seriously misguided (Boone and Smith 1998). During the 1980s or 1990s, only a few sought any common ground between these extremes (Maschner 1996), but this slowly began to change (e.g., Shennan 2002), and evolutionary archaeologists found increasing value in the history of archaeological thought in which Dunnell had been so interested (yet critical of) early on (Dunnell 1986; Lyman and O'Brien 2003, 2004; Lyman et al. 1997; O'Brien and Lyman 1998; O'Brien et al. 2005). Now a better understanding of cultural evolution makes use of the full range of previously disparate efforts (e.g., Cochrane 2009; Eerkens and Lipo 2005; Mesoudi and O'Brien 2008; O'Brien et al. 2002).

Now that the dust has largely settled from archaeology's version of the 'evolutionary wars' (however, see Zeder 2009), it is easier to pick out some of the real gems in Dunnell's early theory. For example, Dunnell (1980:51) put into evolutionary terms the connection between complex

societies and the amount of information that can be transmitted across generations. Above the threshold of information that one individual can pass on, he argued, the scale of selection would shift from the individual to the group or society, such that complex society becomes the functional interaction of specialised groups. This is very similar to ideas of 'extrasomatic storage' of information (Renfrew and Scarre 1998) and such ideas as the degree to which certain prehistoric transitions reflect demographic change (e.g., Henrich 2004; Shennan 2000). How much of the Upper Palaeolithic 'revolution' in cave art is due to increases in population in western Europe (Powell et al. 2009)? In combining this with Dunnell's (1978) style versus function idea, pottery designs can be treated as the neutral styles, while numbers of longhouses are used to estimate population size, which then provides an elegant means of assessing the degree of cultural exchange with exogenous communities not even excavated (e.g., Bentley and Shennan 2003; Shennan and Wilkinson 2001).

Other issues have changed in the last 30 years. After decades of post-processual critique, materiality theory and the rapid rise and fall of dot-com equities, YouTube videos, MySpace personalities and throwaway books, ideas of random copying and drift are almost unavoidable. In fact, it is becoming increasingly common to make analogies between modern practice and prehistoric material culture, with a new interest in branding studies being one example (Wengrow 2008). Studies of modern cultural change allow a higher level of detail that can lead to related insights about prehistoric cultural change through time.

The relationship between evolutionary theory and other disciplines has also changed. Seeing archaeology as a historical science, rather than a lawlike one like physics (fashionable in the 1970s and returning to fashion today), Dunnell emphatically advocated evolution as the *only* theory to explain variation and cultural change in a causal way. Only evolution could provide the means of testing the truth of interpretations. Alternatives such as the laws of physics, which are constant in time and space, were not appropriate for archaeological processes, where every datum is unique. Since the mid-1990s, however, physics has changed and started explicitly applying analyses of dynamic, historical processes of change—such as network evolution, complex adaptive systems, information cascades, sudden state changes and extreme events—towards models of social change. In the last decade, the science of interacting particles (or network nodes) has provided significant insights into modelling collective interactions in social systems, from Internet communities to pedestrian and vehicle traffic, economic markets and even prehistoric human migrations (e.g., Ackland et al. 2007; Albert and Barabási 2002; Barabási 2005; Farkas et al. 2002; Helbing et al. 1997; Newman et al. 2006).

Nevertheless, Dunnell's (1992) critique is still relevant. Variation is the focus of archaeological study; Dunnell argued that change, not the 'noise' as it is for physics, is persistent through time and space, such that predictive laws do not exist. Such 'social atom' models (Buchanan 2007) are crucially dependent on the assumed rules of interaction, and the direct analogy between people and particles (or network nodes) often strays too far from reality (Reide and Bentley 2008). Simple diffusion models for human migrations (e.g., Ackland et al. 2007), for example, still do not account for the attractive tendencies of humans to congregate in settlements, or to make long voyages of exploration, as we know is a regular occurrence among any mobile groups (Brantingham 2006).

The best approach, then as now, is evolution, and the tools that come with over 100 years of studying change among entities that pass on their similarities to others through time. As Daniel Dennett (1995) argued in *Darwin's Dangerous Idea*, evolution applies to almost any process of change, rather than the just biology of non-human organisms. Since archaeologists measure empirical variation (frequencies of discrete elements), Dunnell (1980) saw change in terms of selection of discrete elements rather than gradual transformation of forms. The unit of transmission continues therefore to be a key concern for prehistoric cultural evolution (e.g., Dunnell 1995; Lyman and O'Brien 2003; O'Brien and Lyman 2002; see Cochrane, this volume). In going further, many have gone back to Dawkins's (1976) meme concept to postulate that *culture itself* evolves within its environment of human minds (Aunger 2000; Lake 1997, 1998; Mesoudi et al. 2006; Shennan 2002) regardless of the difficulty of agreeing on definitions of culture or finding units to quantify it, which are really just our problems as scientists to overcome.

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CHAPTER FIVE

Intentionality Matters: Creativity and Human Agency in the Construction of the Inka State

Bill Sillar

Creativity and Human Agency

Evolutionary and Interpretive Archaeologies

Evolutionary archaeologists study the past primarily through a consideration of the variation of specific traits (such as artefact types), quantifying their survival and decline over time. The temporal pattern of these traits is then examined focusing primarily on Darwin's concept of descent with modification. The primary modification to biological evolution has been the recognition of 'dual inheritance' within cultural transmission, where some traits may be copied (or abandoned) due to contemporary cultural influences (e.g., siblings, neighbours or work partners). Within interpretive archaeology much of the focus has been placed on agency and ideology, drawing on a wide range of materials and theories derived primarily from anthropology and sociology to explain the context that gave rise to specific sites or artefacts. One of the most significant features of this has been to highlight the 'active' role that material culture plays in communicating meaning and the reproduction of social relations. Both approaches have much to learn from each other. Evolutionary archaeology provides a caution against the idea that all change was intended or directed. Interpretive archaeology stresses that in order to understand what people did in the past, we need to consider the specific social context of the people involved and what motivated their actions.

One distinction between these approaches is the degree to which people's individual decision making is considered to have a fundamental influence over change. For instance, Shennan (2002:9) critiques our 'desire to see people in the past as the active knowledgeable agents', or

seeing 'change as the outcome of the conscious choices of individuals'. As Bailey (1981) observed, part of these theoretical differences relates to the 'time scale' of our explanations. Some aspects of the archaeological record, and some methodologies and theories, are better placed to study the immediacy of when objects were made and used. For instance, studying the *chaîne opératoire* and the technological choices that informed an artefact's production highlight the agency of thinking individuals (Dobres 2000; Schlanger 1994). But seriation and cladistics are better approaches for measuring the gradual change in the form and frequency of artefacts over time (Bentley et al. 2007; Lycet 2008). In trying to explain the origins of pottery in specific cultures and environments, we may try to understand the specific form of the first pots, how they were made, what they were used for and their context of use in display or competitive feasting (Barnett and Hoopes 1995). By contrast, when looking at the long-term implications of pottery production and use it is more likely that the analyst would emphasise the adaptive benefits of storing, transporting and preparing food, the emergence of craft specialization or the environmental impact of these activities (Arnold 1985).

An Argument for Agency

Human agency primarily rests in our ability to both imagine and enact different actions while continually reevaluating the efficacy of these actions within changing situations (Dobres and Robb 2000; Emirbayer and Mische 1998; Gardner 2008). However, people's individual agencies are framed within the cultural structures that surround and inform them (Giddens 1979, 1984). There can be no complete break from the past precisely because the knowledge and material conditions that we utilise to enact change are informed by our previous experience and cultural practice (as stressed by the evolutionary approach). Yet one of the greatest strengths of agency theory is to consider why participants within their specific social context would have undertaken the activities under study.

A fundamental expression of human creativity is our ability to combine tools, techniques and materials in novel ways to achieve specific goals over short time periods (van der Leeuw and Torrence 1989). Artefacts can be considered as an externalised aspect of human adaptation, the 'extended phenotype' (Dawkins 1982) of the human animal 'in the same way beaver dams and bird nests are parts of phenotypes' (O'Brien and Holland 1992:37). However, this does not capture the immediacy with which people respond to current conditions and the creative way they work together and combine a series of previously disparate raw materials, tools, techniques and knowledge to achieve specific directed goals.

Equally, technologies, designs and social structures are only used while they are effective in relation to the immediate needs of the people directing the activity, but if these cease to fill social needs and aspirations they will be abandoned. It is precisely our agency that makes the speed and direction of changes within human society and material culture very different to the time it takes animals to develop new behaviours (such as beaver dams and bird nests) or for breeding populations to respond to environmental pressures.

Shennan (2002:9) states that it is naïve to believe that our agency is distinct from other animals or that it plays a significant role in directing longer-term change, claiming that the reasoning behind human behaviour is only of importance to the extent that it encourages, or inhibits, transmission of a behaviour and that we can never control the long-term consequences of our cumulative actions. Theorists of structuration and agency also consider human knowledge to be incomplete and imperfect: 'It is hard to exaggerate the importance of the unintended consequences of intentional conduct' (Giddens 1984:11–12). For Giddens, like Bourdieu (1977), the end result of almost all individual agency is to reproduce and perpetuate social and material structures. Nonetheless, humans are knowledgeable agents who choose what to do within their understanding of their situation and the limitations of their material and social context. This makes the processes of innovation, emulation and enculturation within human societies different from adaptation and transmission within animal populations because people's interpretation of their situation and their intentions are the major 'selection criterion', allowing major changes to take place in less than a single human generation. Neff acknowledges that 'the decision-making context is the selective context' and that 'decisions have to accumulate in order to drive evolutionary change. This is how proximate (technological choices) can accrue into evolutionary history' (Neff 2001:280; cf. Cochrane, this volume).

Sociality: The Cultural Significance of Commitment

An essential aspect of human agency is the social relations through which people develop their self-perception and their understanding of how to communicate and engage in cooperative behaviour. If we are to identify what makes human agency different from that of other animals, we need to consider how this self-conscious cognitive ability developed. Mead (1934, 1938) argued that human self-consciousness, like that of other animals, emerges out of the dynamic process of our interaction with physical objects, including our own and other people's physical bodies. The significant difference with human 'self-perception' is our capacity

to conceptualise the perspective of another person; this is what Mead considered to be humans' advanced capacity for *sociality*. This allowed people to develop an ability to empathise with other human beings by using our imagination to 'take the attitude' of another individual or the 'generalized other' of a social group (Mead 1932:87), and it is this that is fundamental to our ability to develop more complex social strategies that extend beyond immediate kin.

The common cultural assumption that people *do* have agency and that we *are* responsible for our actions is central to the rules of acceptable behaviour that structure social interaction. Assuming that human volition has consequences, and that our actions play a major role in causing change, is fundamental to the social expectation that individuals can, and should, take responsibility for their actions. It is precisely because we consider ourselves to be active knowledgeable agents that societies can construct rules and etiquettes of acceptable social behaviour. This is central to all social structures from family relations and delayed reciprocity to taxation and legal codes. This is the feature of human agency that extends our cultural commitment beyond the family or kin group to other individuals, institutions, places and ideologies. It is these individual commitments, and our accountability to wider social groups, that makes it difficult to separate individual agency from the corporate agency of the larger group. Society is structured by each individual's commitment to other individuals, social groups, institutions, activities, places and beings (Sillar 2004). Investigating the development of state societies, or gender relations, requires a consideration of changing social ideals and practices regarding individual and group responsibility. In the second part of this essay, I will consider the relationship between individual agency and wider social structures in the development of the Inka state and argue that one of the central features of Inka state development was precisely the cultural rules of social responsibility.

Purposeful Action and Proximate Cause

A potential problem within evolutionary archaeology is that, through focusing on the long-term rhythm of artefact origin, persistence and demise, the specific way in which people made and used the artefacts and the immediate decision making by artisans and artefact users is underplayed. Unless we consider what individual objects were used for, and the purpose of specific actions, we ignore the proximate cause of much change. Most archaeologists do make some assumptions about the purpose of people's actions and decisions in the past. For example, there has been much debate over the purpose of changing pottery temper and how it affected the function of cooking pots made with that temper

(Feathers 2006; see Cochrane, this volume). All this debate is based on defining the pottery's 'performance characteristics', which require assumptions about how the pot was used (Schiffer and Skibo 1997; Tite et al. 2001). The process of innovation can only be understood from the framework of the tradition of which it is a part; thus, in explaining change we need to 'think forward' and consider the immediate purpose and intent of the people enacting this change (van der Leeuw 1991).

The dynamics of scale are an important part of the social context within which people act. For instance in larger social groups the actions of one or two individuals may have significant intended or unintended consequences as they are picked up and responded to by other individuals, developing into crowd behaviour. There has been some stress within evolutionary archaeology on the concept of 'conformist bias', where emulating others is presented as a low-cost and effective strategy for people to position themselves in society (Bentley and Shennan 2003). While models of this behaviour look effective when studying the frequency and distribution of one or two traits, they underemphasise the agency that is involved in exactly which traits each individual chooses to emulate and the fact that each individual selects a wide range of traits in their daily life, such that the complete combination used by any one individual may be unique. As dramatised on countless TV crime shows, while one trait may be shared by millions of people, as the investigators discover a few more traits the number of individuals that combine all of these becomes narrower and narrower. It is precisely the individual's relationship to their surrounding social structure that is expressed in this creative accumulation of traits (or artefacts) and the social connections that they represent. However, while this model of individual selection of traits fits particularly well with modern consumerism, there are different factors at play when we are considering how social structures can either facilitate the essential interdependence that emerges through the process of craft specialisation, or the coordination of large numbers of people in construction projects, armies or state bureaucracies. The nature of the social connections can have a major effect on the commitment and energy that individuals will be prepared, or persuaded, to give to collective goals. The social organisation can also provide the context within which knowledge is reproduced and skills are transferred to others through simple observation or more structured apprenticeship. In the second part of this paper, I will discuss the social context of technological and organisational developments in Inka craft production and building construction. These examples will allow us to consider how individual skills can be reorganised and coordinated through larger-scale social structures and institutions to achieve specific aims that are beyond the capacity of any single individual.

The Materiality of Agency

Humans are *bricoleurs*, with a capacity to creatively combine tools, techniques and materials in novel ways to achieve specific goals over short time periods. This fits the common model of 'cultural evolution' as a 'tree' of ingrown branches—highlighting how cultural traits not only diverge (like animal species) but may also be recombined in novel 'hybridised' combinations. This model could represent the creative process through which people combine cultural traits to address their current needs. While this may be analogous to biological processes of hybridisation (rather than divergent evolution), the purposeful combination of many disparate components, as well as the ability to assess the efficacy of these actions, requires a consideration of directed human agency.

One of the primary principles of evolutionary archaeology is the concept of dual inheritance, where 'cultural transmission' describes the many ways in which people learn behaviour. However, this is usually used to describe the cultural mechanisms for taught behaviour (the cultural equivalent of descent with modification), and does not sufficiently problematise the creative way in which people observe the material world and develop innovative responses to it: 'Thus if I learn to make a stone tool by experimenting with striking pieces of stone together, what I learn does not count as cultural information' (Shennan 2002:63). Yet, the ability to copy the appearance of an artefact without having seen the precise actions used to make it is a fundamental human ability, reliant on cultural knowledge of materials and techniques as well as the skill to closely observe and interpret material objects. Artefacts are themselves part of the process of cultural transmission as they encode information, but what information is extracted depends on the *interpretation* of the agent who perceives and engages with these artefacts. This takes place in many different ways. For instance, some artefacts are specifically designed for 'symbolic storage': books materialise the spoken word and insignia can express institution and rank. The facility we have to invest an object with information content allows cultural knowledge to be transmitted and interpreted without the immediacy of observing the person who originally made the object (cf. Lake 1998; Cochrane, this volume). But this reading of symbolic meanings derives from a more fundamental ability that people have to evaluate the form and detail of an artefact and assess its construction, purpose or significance in relation to the observers concerns.

While great attention has been placed on how humans make tools, insufficient emphasis has been placed on the cognitive and creative abilities that allow us to use objects made by others. This is a much more unique feature of the human animal—we can acquire an entirely 'new'

artefact and think through how to use it by drawing on our memory of similar artefacts and envisaging the consequences of different actions before we try any of them out in practice. Our ability to turn things around in our hands and our minds allows us to draw on our memories and try out new juxtapositions, combinations and metaphorical transfers prior to exploring these further through material practice. This process is fairly immediate when we are dealing with artefacts that are familiar to us, so that we can walk into a room for the first time and know how to use the mug, chair and pen that are in it. But ancient or foreign artefacts may also engage our attention, and these require greater deliberation if we are to try to interpret and reproduce them for our own society (well-known examples of this process include the European race to imitate Chinese porcelain or the influence of ancient Athenian and Roman buildings on eighteenth- and nineteenth-century European architecture).

In the next section of this paper I will discuss how the social purposes of Inka stonework, pottery and architecture need to be considered if we are to understand why the Inka state invested energy in coordinating human labour for their production and distribution. The symbolic content of this material culture, which expressed identity, status and more abstract religious meanings, was central to why these objects were produced and played a vital role in creating the Inka state as a visible presence in the Andean world. The copying of ancient stoneworking technologies, the hybridisation of pottery techniques and designs, and the complex combination of materials and skills in building construction, which are discussed below, all demonstrate how directed aims provided the impetus for changes in material culture that are difficult to investigate using solely the principles of Darwinian evolution.

Agency, Structure and Innovation in the Inka State

The Development of the Inka State

The Inka developed amongst a dozen small ethnic groups settled around Cuzco from ca. AD 1000 to 1400. These small chiefdoms used cooperative labour to extend their intensive valley agriculture, drawing in resources from neighbours (Bauer 2004; Covey 2006; D'Altroy 2002). Even at the height of the Inka empire, Cuzco was never a large metropolis, and the Inka relied strongly on integrating other ethnic groups as soldiers, labourers and state officials. Their ability to coordinate labour enabled them to draw upon personnel to provide a military force that first defended the nascent Inka state and then became a tool for imperial expansion. The Inka used marriage alliances, bargaining and military force to start to expand beyond the Cuzco heartland sometime between

1300 and 1400 (Bauer 2004). By the time of the Spanish conquest in AD 1533, the Inka state included some 80 or more ethnic groups with a combined population of around ten million in a 4000 km long stretch of western South America (D'Altroy 2002). The Inka then sought to incorporate the populations of these new territories within their labour exchange practices and to intensify production in a patchwork of agricultural, herding, mining and craft production centres. The Inka used the production of food and drink to provide the reciprocity that justified labour extraction, craft products to express and solidify ethnicity and social rank, and construction work to create administrative facilities.

Labour Tax: A Hierarchy of Social Commitments

Changes in access to, and control over, material resources and human labour are central to most sociopolitical transformations, and cultural norms about social responsibilities are at the heart of these 'economic' relations. The Inka economy developed from practices of structured reciprocity similar to that within a household or kin group, where elite households were able to draw on a larger labour pool as long as they justified this through the provision of food, drink and security. Kolata (1983) has suggested that the whole Inka empire could even be considered as a single household-based economy, which relied on the supply of labour and redistribution of state-owned resources. This is partly why each Inka emperor tried to expand the state boundaries or intensify state-level production in order to acquire the resources needed to provide the food, drink and gifts of cloth that were essential to maintaining respect and justifying requests for labour and military allegiance.

The Inka economy relied on a 'taxation' of labour, where a polity or ethnic group was expected to provide a rotation of workers to plough Inka state fields, fight military campaigns and engage in construction projects (Murra 1980, 1982). The Inka promoted the leaders of the ethnic groups they assimilated as dependent clients responsible for facilitating Inka access to labour. In principle, subject populations of able-bodied adult males paid taxes in periods of a few months' corvée labour (*mit'a*) to work in agriculture, construction, mining, portage or military activities, but continued to have access to the fields, flocks and resources of their home territories (D'Altroy 2002; Murra 1980, 1982). The logic of this system required the Inka to provide corvée workers with the raw materials and tools they needed to carry out work for the state. Thus, some key resources, including land, animals and minerals, were taken into state ownership in order to provide the resources that the *mit'a* transformed through their labour into goods and infrastructure, which then belonged to the Inka. The provision of food, drink and festivities

as reciprocal remuneration to the workers required the construction of extensive structures devoted to large-scale food preparation and storage (Morris and Thompson 1985), and the effects of this reciprocity have been identified at the household level (D'Altroy and Hastorf 2001).

There must have been a relatively wide sharing of the ideology surrounding the state labour tax to facilitate these changing relations of production and consumption. This begs the question of what the socio-economic and status benefits of participating in state-sponsored production were for the 'subject' populations. Perhaps the most important feature of this was that the Inka managed to incorporate state labour tax within the economies of the ethnic chiefdoms. Many ethnic leaders probably subjugated themselves because their status and positions were then secured as they became essential intermediaries for the Inka state. Workers were given clothing, food and drink, and there may well have been some social value in participating in the public works and state rituals, developing new skills and experiences that added to individual status. Working for the Inka state was, though, organised through the ethnic group and was an expression of loyalty to the ethnic lord, thus working for the state ultimately justified access to the land, resources and reciprocal labour of home communities. Although the Inka state collapsed immediately after the Spanish conquest, the economies of many ethnic groups survived until the Spanish market system took over (Murra 1975). The Inka state brought about a decline in petty interethnic warfare and, by making ethnicity the primary organisation toll of the empire, solidified and entrenched the ethnic economies and promoted chiefly authority.

In the process of subjugation, the Inka altered previous gender relations (Silverblatt 1987), mechanisms of reciprocity and redistribution (LeVine 1987; Morris 1993), household and ethnic economies (D'Altroy and Hastorf 2001; Murra 1980) and craft production practices (Costin 1998). There is historical and archaeological evidence for a gradual drive to greater and greater state control over labour, with 'increasingly large numbers of people devoting increasingly larger parts of their time to state activities' (Morris 1993:184). During the period of the final full-Inka ruler, Huayna Capac, there seems to have been further emphasis on *mitmakuna* (colonists working for their ethnic group and the Inka state, including at enormous state farms such as Cochabamba and Abancay, as well as craft production centres), *yanakuna* (lifelong servants who worked directly for the Inka rather than through an ethnic group) and *aqllakuna* (women removed from their families and working within state institutions). Each of these contributed to a greater level of production controlled directly by the state (D'Altroy 2002; Murra 1980). Thus Inka reorganisation of production allowed them to restructure much of

Andean society even as they continued to use Andean social norms. Inka technologies for stoneworking, pottery making, weaving, mining and metal working all used relatively simple, small-scale tools that could be used at a domestic level. But, it was the scale of organised labour that allowed the Inka to make large-scale investments such as canals, terracing, roads, bridges and building complexes as well as agricultural, craft and military work across the empire.

In the following sections I wish to explore how the organisation of some of these activities demonstrates unique aspects of human agency to work to directed goals. Yet, this system of labour exchange and uneven reciprocity running from the household, through the ethnic group to the state was all built upon social commitments. It is precisely because the Inka and their subjects considered themselves to have some level of individual and group responsibility that they were able to construct rules and etiquettes of social obligation that extended into a complex web of social relations, facilitating new levels of production and construction. The assumption that individuals are knowledgeable agents who can take responsibility for their actions, and the consequences of their actions, allows us to stretch our commitments beyond family relations to include delayed reciprocity and state taxation. This is a central feature of human agency, without which it would be impossible to structure complex societies.

Hybrid Crafts

As an example of state craft production we can consider textiles, which were an essential tool for the Inka, as great quantities of cloth were made for and redistributed by the state to mark the ethnicity, status and activity of those to whom was given (Costin 1993, 1996, 1998). Most women within the empire were required to spin and weave some lower-quality (*chusi* and *'awasqa*) cloth at home from wool that was supplied from state stockpiles. The Inka then used this high volume of lower-quality cloth to kit out the men fighting in the Inka army or working on state construction projects (Costin 1993, 1998). Fine-quality (*qompi*) cloth used for higher state officials, noble lords and sacrifice was woven by the *aqllakuna* (chosen women), who were selected by the Inka from around the age of 10 to enter lifetime service within state institutions (*aqllawasi*) where they became skilled weavers and also prepared food and beer as well as performing ritual roles. Fine *qompi* cloth was also made by specialist weaving families (*qompikamayoc*), who remained members of their original ethnic group but were resettled as *mitmakuna* into craft production centres and given sufficient land to support themselves.

At some of these production centres potters from two or more ethnic groups were resettled and required to produce a quantity of pottery vessels

used to provide the food and drink to work parties and at state-sponsored festivals and rituals (Bray 2003; D'Altroy et al. 1998). Comparison to pre-Inka pottery-making techniques and styles in these areas suggests that local potters were being used as well as *mitmakuna* from more distant areas, and that the potters made a variety of pottery forms and decoration. Some of these were 'Inka', while other vessels at these centres were made in local and hybrid styles (Costin 2008). For instance, one hundred pottery-making families were moved under the authority of Inka Huayna Capac to a new craft production centre at Milliraya (near Lake Titicaca) to work alongside some one thousand weavers; these potters produced both Inka- and local-style pottery (Spurling 1992). Similar pottery enclaves were set up at Wayakunta, near Cajamarca (Espinoza Soriano 1970), Potrero-Chaquiago in northwest Argentina (Lorandi 1984), Cañocillo in the Jequetepeque Valley (Donnan 1997), and in the Leche Valley (Hayashida 1999:347). The close relationship between pottery manufacture and textile production at several of these sites is significant because Inka pottery decoration shared features with textile designs, and it is likely that the spatial proximity of their production facilitated this sharing of design elements. In a study of the style of costal pottery made under Inka rule, Costin (2008) suggests that the Inka supported the inclusion of local stylistic elements within state-sponsored pottery and textile production (e.g., Chimu iconographic elements on the uniquely Inka form of the *aryballo*). Incorporating aspects of local styles helped to naturalise the imposition of Inka imperial order and maintain the identity of the ethnic groups that were the principal tool of social organisation within the state.

These artisan centres must have promoted a transfer of knowledge of techniques and designs. The resettlement of families involved in artisan work from different ethnic origins and with different craft skills promoted a new dynamic in learning networks. This was further enhanced as the state provided greatly improved access to a wide range of raw materials and tools. An important aspect of this was that the artisans only undertook some of their production for the Inka state, as they remained productive members of their own ethnic group and participated in the ethnic economy of these client chiefdoms. Understanding the development of these new 'state' artefact styles requires a consideration of large-scale economic structures and evaluating how individual objects functioned within people's practical activities and social strategies. It is not possible to identify the individuals who made 'Inka-style' textiles or pottery, but these artefacts were created by people integrating previous knowledge and skills with new materials, techniques and designs to create hybrid artefacts that played a vital role in materialising the Inka empire.

Stonework

The Inka are famous for the form and landscaping of their architecture, the precision and style of their stone masonry and their use of hard andesite rock. The Rumiqolqa quarry is known to have been the preferred source of andesite for prestige Inka construction projects (Protzen 1985). Stone quarried from Rumiqolqa was transported 35 km upslope to Cuzco, but this is nothing in comparison to Ogburn's (2004) identification of more than 450 finely worked andesite blocks (weighing 200–700 kg) that have been sourced to the Rumiqolqa formation found at Saraguro, Ecuador, some 1,600 km away! This breathtaking example of long-distance human haulage highlights the significance that the Inka attached to the andesite of Rumiqolqa. It is clear that this stonework was considered emblematic of prestige buildings by the Inka. There are examples of fine Inka stonework that had been hacked apart at Tomebamba, the newly established Inka capital that was largely destroyed during the internal Inka 'civil war' between Huascar and Atahuallpa immediately before the Spanish conquest (Hyslop 1993:346). This suggests that the stonework was considered a powerful symbol of the Inka.

This stonework was achieved by simple tools and a large amount of well-organised labour, pounding the rocks with hammerstones to produce the characteristic pockmarked surface and bevelled edges of the Inka style (Protzen 1985). At the construction site, the preparation of a wall required the top of the stones already in place to be individually prepared to receive the precise shape of the stone being placed above, producing a close, secure fit without cement (Protzen 1982). Prior to these Inka constructions, there was very little dressed masonry in the Cuzco region. Earlier sites such as Pikillacta and Choqepukio are characterised primarily by making use of the flatter edges of natural or split stone, with minimal dressing for corner stones, but nothing approaching the detailed preparation of Inka stonework. Inka stonework appears as a significant innovation around AD 1400. Given this lack of a local antecedent, some credence may be given to historical accounts that suggest that the Inka took their inspiration for fine masonry from the ancient site of Tiahuanaco, some 500 km from Cuzco in the Lake Titicaca Basin, even though Tiahuanaco's high-quality stonework had ceased to be produced some 500 years earlier. It was not the behaviour of active artisans, but the materiality of ancient artefacts within the revered ancient site of Tiahuanaco that provided the inspiration:

> Pachacutic saw the magnificent buildings at Tiaguanaco, and the stonework of these structures amazed him because he had never seen that type of buildings before; and he commanded that his men should carefully observe and take note of that building method, because he wanted the

> construction projects in Cuzco to be of the same type of workmanship. (Cobo 1988 [1653]:141, Bk. 12, Ch. XIII)

> I have heard Indians state that the Inkas made the great buildings of Cuzco in the form they had seen the rampart or wall one can see in this village [of Tiaguanaco]. (Cieza de León, *La Crónica del Perú,* 1553, 301, Pt. 1, Ch. CV, cited in Protzen and Nair 1997:146)

Protzen and Nair (1997) made a careful comparison of Tiahuanaco and Inka stone masonry and emphasise that there are major technical differences in the preparation and laying of the stonework: 'Whatever [the Inka] saw when they first came upon Tiahuanaco, and whatever they borrowed from there, if anything, they thoroughly reinterpreted and made their own' (Protzen and Nair 1997:166). Like European attempts to imitate Chinese porcelain, this did not result in a faithful copying of the original, but in the start of a unique local tradition. It was the material object of Tiahuanaco's stone walls that acted as a medium to transfer traits across time and space, but this was only possible due to individuals observing and interpreting these walls. While this stoneworking is a relatively simple technology, the amount of stone dressing and fitting needed to achieve a short section of Inka wall is not the sort of application of labour expenditure that would be undertaken by a small-scale domestic unit. It is precisely in the context of increasing social complexity, with people who have reasons to seek prestige architecture and the social ability to coordinate labour, that this technology could be revived. While many technologies are developed and transmitted through small-scale kin systems that nurture skills from an early age, there are some technologies, such as this fine masonry, that can only be developed within much larger social networks, and this will affect how the techniques are conceived and transmitted. Thus Inka stonework provides an excellent example of two distinct aspects of human agency: (1) the ability to interpret a 'foreign' artefact and then, like the *bricoleur*, to creatively combine available tools, techniques and materials to achieve a similar effect, and (2) the ability to integrate a large work force to achieve an innovative directed project.

Constructing the State: Monumental Architecture

The Inka state was integrated through a network of roads that linked state facilities such as lodging house and relay stations (*tambos*), administrative centres, ritual and pilgrimage sites, storage structures (*collcas*) and agricultural, mineral and craft production centres (D'Altroy 1992, 2002; Hyslop 1984). This road system linked the various ethnic groups, facilitating the collection of tribute, the movement of labour

and military personnel, and the processions of Inka nobles inspecting their subjects and performing rituals. Inka administrative and ritual centres utilised a number of repeated structures such as storage (*collcas*), raised platforms (*ushnus*), large halls (*kallanka)*, plazas and *acllawasis*, using similar aspects of site plan, building forms and architectural style across the empire (Astuhuaman 2008; Gasparini and Margolies 1980; Hyslop 1990). The familiarity of these structures would have provided a context for people from around the empire to carry out actions and perform rituals that reproduced the Inka state in far-flung parts of the imperial territory (Acuto 2005; Hyslop 1990). Yet, no two sites are the same, and what is also striking is the individuality of each location, such as the 'temple of Viracocha' at Cacha (the modern village of Raqchi, Department of Cuzco).

Cacha was the mythical origin place of the Canas, where the creator god, Viracocha, threatened the population with a volcano and then saved them by halting its eruption. Here a 'large building' was built that Betanzos (1996 [1557]:175, Ch. XLV) states Inka Huayna Capac commanded to be erected near the volcano and dedicated to the cult of Viracocha. The monumental complex that survives in Raqchi includes a massive structure, which is probably the 'large building' Betanzos described and Garcilaso (1989 [1612]:290, Ch. XXII) referred to as a 'temple'. The land on which the Inka structures were built was substantially altered through the construction of high-quality terracing, which served to accentuate the visibility and dramatic view of the new temple. The most prominent part of this 'large building' is the central wall surviving to a height of 12 m, constructed using high-quality Inka fitted stonework at its base and completed using large rectangular adobe bricks (Figure 5.1). This wall formed the central partition, and roof support, for a rectangular building measuring 92 m by 25.25 m. The stone bases of 11 pillars are set down on either side of the central wall, with one pillar surviving to a height of approximately 8 m. The architectural analysis of Gasparini and Margolies (1980:248, Fig. 234) suggests that these pillars served to support the beams of a sloping roof, and this ties in very well with Betanzos's description. All of this technology, as well as the architectural form, were quite foreign to the Canas and must have required skilled workers who came from, or were trained in, the new building techniques being developed in Cuzco. The adobe bricks are also a 'new technology' as these are made by placing mud over *ichu* grass and rolling it like a Swiss roll before shaping it into rectangular blocks of different sizes, a technique that can also be seen in a few of the adobes used in Inka Cuzco. The roofing of this structure must have been a major undertaking as there are no native trees in the Andean highlands that could provide lengths of

Figure 5.1 Photograph of the surviving central wall of the large Inka building, the 'Temple of Viracocha', at Raqchi. The small tile roof is a modern conservation measure, but the stone base, the 12 m high mud brick wall and the footings for the columns are original. (Photograph by Bill Sillar.)

wood able to span the 6 m between the central wall and the columns and again from the columns to the outer wall; the sloping beams of the roof would have been even longer (cf. Betanzos 1996 [1557]:175, Ch. XLV). The timber for this roof was probably imported from the lowland *montaña* region, and in the absence of wheeled transport these would have been carried by people from around 75 km away. The roofing of this building must have been a massive task; working around 12 m above ground level to cover some 2500 m^2 (presumably with grass thatch).

It is not just the form, materials and techniques of this building, but also the sheer scale of the undertaking that demonstrate that it must have been built by a substantial labour force that the Inka enticed, persuaded or forced to work here. The use of 'new technologies' and the conception of this as a single building scheme all proclaim this as a major innovative project, yet it is using materials, techniques and design ideas featuring in other Inka buildings. This building could not have been constructed without an overall plan, or the coordination of a large labour force with a range of distinctive specialist skills. It therefore provides a strong example of the human ability to achieve specific short-term aims that are beyond the capacity of any single individual. Garcilaso de la Vega (1989 [1612]:290, Ch. XXII) describes how people processed inside the temple, and it is clear that the temple

was deliberately designed to express aspects of Inka cosmology and to provide a specific space for appropriate ritual behaviour (Sillar 2002). Thus, although this building is unmistakably Inka, it is a unique structure built for a unique place. It can only be explained by considering how the Inka interpreted the history and meaning of this specific place within their cosmology and the design and implementation of a carefully planned project that required the coordination of a range of skilled workers and materials.

Conclusions

The purpose of this paper has been to highlight the importance of agency and interpretation as sources of innovation and directed change that need to be considered in relation to cultural transmission. I have highlighted how processes of change in material culture and human societies differ from the wider processes of adaptation and transmission within animal or plant populations. In doing so, I identified five key features that diverge from normal models of descent with modification in Darwinian evolutionary archaeology.

First, and foremost, is the role of human agency. People have specific social aims and intentions that are major 'selection criterion' in directing change (cf. Cochrane, this volume). I have not focused on the role of 'great men' in directing change (although individuals such as Huayna Capac may have played decisive roles). Rather I have tried to illustrate how human agency can be identified within specific technical acts and coordinated building projects. Although agency is driven by individual aims and intentions, it is primarily expressed through people working together to enact wider social goals and in the reciprocal exchange of goods and services that act cumulatively to create and transform social structures, such as the Inka state. This is quite distinct from evolutionary models that investigate the emergence, spread and decline of patterns of traits, but focus less on why people would choose to make, use or abandon the artefacts. Innovations are always conceived of, disseminated or rejected by individuals who have social commitments, cultural ideals and prejudices.

Secondly, humans creatively combine tools, techniques, materials and designs in novel ways to achieve specific goals. This *bricoleur*-like capacity is a supreme example of human agency. Although it may not be possible to identify the individuals who made specific hybrid objects, these artefacts could only be created by individuals using their previous knowledge and skills to incorporate new influences such as new materials, techniques and designs. Evolutionary archaeology recognises the human ability to combine 'traits', but this needs to be understood

as a creative social strategy directed by short-term aims and objectives (cf. Tehrani, this volume).

Thirdly, people interpret the physical form and symbolic content of material culture. Material culture plays an exceptional role in inspiring human ingenuity, partly because the mere act of picking up an artefact engages our cognitive abilities to interpret what it is made from, how it was made and how it could be used, as well as what the designs may mean. We should not be so arrogant as to assume that these questions are the preserve of trained archaeologists. I have encountered at least as much curiosity about these issues amongst the farmers and artisans I've worked with in Peru and Bolivia as I do amongst my archaeology students in London. I used the example of the Inka imitation of Tiahuanaco stonework to show how material culture can provide inspiration, rather than artisans copying behaviour itself, but this is just an extreme example of a daily curiosity and appreciation of the artefacts around us and our ability to use objects made by other people. Thus, through human interpretation, objects can play an active role in transmitting and transforming human behaviour and social structures. This ambiguous role of artefacts, as objects with information content that only becomes active when people choose to interpret them as meaningful, has been a major theme within interpretive archaeology, but the implications of this for cultural transmission have yet to be addressed by evolutionary archaeology.

Fourthly, it is precisely because people considered themselves to be self-aware agents, with some level of individual and group responsibility, that they could construct rules and etiquettes of social obligations. It is through the resulting network of social relations and agreements to work cooperatively together that humans have become such a distinctive force on the planet, with the ability to develop and enact material projects that could not be achieved at the individual or family level. Thus changes in technology, design or the use of goods need to be considered in relation to equally important changes in the moral economy that defines what can, and cannot, be exchanged by whom. Changes in the control over material resources, human labour and their distribution are at the core of social rules about morality and responsibility. Changes in society need to be considered as changes in social ideals regarding individual and group responsibility.

Fifthly, humans can organise groups of people to take on a range of specialist roles and to undertake coordinated projects, achieving rapid directed change. In some ways this is the cumulative effect of all the points raised above and is a vital aspect of the dynamic between individual agency and the social structures through which labour can be coordinated. I used examples of Inka stonework and the construction

of monumental architecture to illustrate this point. The scale of these activities and the physical demands of the material technologies required a significant amount of labour to be organised and the development of a range of specialist skills. It was precisely these acts of production and consumption that made the Inka empire a reality through the experience of participation and the resulting visibility of the infrastructure that was constructed.

Evolutionary principles could be used to model and analyse the processes of cultural development and material change discussed above, but they need to include intentionality, creativity and social agency as distinctive aspects of humanity. The Darwinian focus on descent with modification is justified, precisely because the very knowledge that we utilise to enact change is itself informed by our previous experience and cultural knowledge. We are partly conditioned by the materials, the techniques and the understanding that we inherit, but we are always able to combine these elements in novel ways, and evolutionary concepts of 'dual inheritance' need to stretch to accommodate the varied mechanisms of this creativity.

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PART 2

CONTEXTS OF STUDY

CHAPTER SIX

Interpretive Archaeologies, Violence and Evolutionary Approaches

Simon James

Introduction

My brief in this chapter is to look at how archaeologists working within frameworks labelled postprocessual or interpretive have dealt with human violence and the ways in which they may have engaged with evolutionary archaeologies in relation to these matters. Here, at the outset, let me offer first-order approximations of answers to these questions: they don't, and they haven't. However, seeking to understand these conclusions may tell us something interesting about the community of interpretive archaeologists. I will seek to explain why there has been widespread 'silence on violence', examine the limited but increasing number of interesting exceptions and explore why engagement with evolutionary approaches remains virtually non-existent, rarely extending beyond more or less ritualistic denunciations of 'sociobiological determinism'.

I focus primarily on what the French call 'Anglo-Saxon' scholarship, i.e., principally that conducted in the UK and North America, the main loci of postprocessual thought (related 'traditions' in areas like Scandinavia tend not to use such labels for self-description; e.g., Otto et al. 2006). This underlines the problem of defining the boundaries of interpretive or postprocessual archaeological discourse; I am advised

that some whose work seems to me to represent this school would reject the label.

On Violence, and Related Matters

I want to start with some basic observations about violence and closely related issues. Why this is necessary will, I hope, become apparent from what follows. A degree of physical violence in various forms is encountered in just about all documented human societies. Many have invested great technological skill and aesthetic talent in creating dedicated material culture—weapons—to help some inflict injury or death on others and also physical defences—armour, fortifications—against such assaults. Some have created groups and places specialising in infliction of violence (e.g., torturers, places of execution). Patterns of violent behaviour are also observed among some, but apparently not all, related primate species (e.g., 'peaceful' Bonobos [Thorpe 2005:4; see also Nystrom 2005; Layton, this volume]). Consequently, even if we reject any innatist assumption of some universal human 'drive to violence', if it is axiomatic that archaeology must study the human past holistically, it follows that when dealing with material remains of past societies, we should at least be alert to any evidence for violent actions within and between social groups, and associated cultural customs and practices. If we find them, we must seek to understand their nature, extent and context, and to interpret their roles, meanings and representations in society. Equally, we should consider carefully whether lack of evidence for violent practices represents a truly peaceable reality (equally worthy of investigation, as apparently rare) or is only apparent (see below).

Before proceeding, we should pause to define violence and related terms and concepts: conflict and war. These terms are often treated as though they are effectively synonymous. At least, little effort may be made to distinguish between them. As will be explained, I regard this as a central confusion hampering discourse. The *Oxford English Dictionary* (OED) offers the following definitions:

> violence, *n.*
>
> 1. a. The exercise of physical force so as to inflict injury on, or cause damage to, persons or property; action or conduct characterized by this; treatment or usage tending to cause bodily injury or forcibly interfering with personal freedom.
>
> b. In the phr. *to do violence to*, *unto* (or with indirect object): To inflict harm or injury upon; to outrage or violate. Also *to make violence*.
>
> 2. a. With *a* and pl. An instance or case of violent, injurious, or severe treatment; a violent act or proceeding.

conflict, *n.*

1. a. An encounter with arms; a fight, battle.

b. *esp.* A prolonged struggle.

c. (without *article* or *pl.*) Fighting, contending with arms, martial strife.

Sillitoe offers the following definition of war (thanks to Bob Layton for this usefully brief definition):

> A relationship of mutual hostility between two groups where both try by armed force to secure some gain at the other's expense. (Sillitoe 1978:252)

The English word 'violence' can refer to non-physical actions, e.g., verbal assault inflicting emotional pain and humiliation. It can include physical acts against inanimate objects, hence the phrase 'interpersonal violence' is often used to distinguish injury inflicted on the body. Violent acts of all kinds may be impulsive (physical lashing out as an emotion-driven response, often as instantaneous reflex, triggered by perceived hurt to body, sense of self or property) or instrumental (inflicted as a deliberate strategy calculated to achieve some aim, at the expense of harm to others).

Violence—hereafter generally referring to physical, interpersonal violence—is the most comprehensive term among those listed above, encompassing all manifestations from a slap to global war; although not immediately apparent from the OED definition, (physical) conflict usually connotes clashes between groups, who may or may not be armed; war is armed conflict.

It is crucial to note that much interpersonal violence—in many, perhaps virtually all, known societies—had and has nothing to do with war on any definition. This was even true of so overtly militarised a society as imperial Rome (my own field of research), in which much physical mayhem—domestic beatings of free women and children, private abuse or killing of slaves, armed robbery and endemic banditry, juridical savagery, political violence and 'recreational' bloodshed in the gladiatorial arena—was seen as distinct from war (although its incidence was doubtless related to the contemporary propensity for warfare). I use 'violence' as the all-embracing term here, then, with 'conflict' and 'war' as distinct subsets covering particular forms of collective violence. (Boundaries between these categories are of course often blurred and subjective, and terms prone to manipulation, e.g., the 'troubles' in Northern Ireland were a 'war' to the Provisional IRA, a 'conflict' to the British army.)

It is equally important to observe that the English word 'violence' has a strongly pejorative undertone, taken over from its Latin root *vis*. Roman society regarded the *controlled* application of *socially sanctioned* physical force to others as fundamentally necessary to the maintenance of the social order and, in contexts such as foreign war, even beneficial to society (at least, so long as it was winning [James, forthcoming a]). The term *vis* connoted socially unsanctioned, unreasonable or uncontrolled force, which endangered the lives and security of the community.

Violence, then, is a pejorative term for a morally problematic topic, especially for academics, among whom it is commonly anathematised and sublimated (they reserve their aggression for reviewing each others' work). Consequently, this aspect has a unique place, unlike that of any other covered in this volume. In the Anglophone West, violence is something inflicted by bad people or collectivities: brutal spouses, criminals, terrorists, 'rogue states'. 'We', on the other hand, eschew physical action entirely, or at most use minimum, proportionate force (*sic*) to resist robbers, intruders or aggressors (involving infliction of harm that is not defined as violence). In dealing with questions of past violence, then, such cultural tendencies and apparent contradictions (if not hypocrisies) would seem especially to demand the kind of critical self-awareness which postprocessual archaeologists advocate.

Interpretive Archaeologists and the Question of Past Violence

I opened by saying that, as a first rough approximation, interpretive archaeologists simply have not dealt with issues of violence, conflict and war. Taken as a whole, the 25 or so years of the history of interpretive archaeology have indeed generally been noteworthy for the *lack* of attention scholars working in this tradition have given to these matters. This was especially true through the 1980s and down to the mid-1990s, since when there have been some important, although still sporadic and limited, exceptions.

It is notoriously difficult to prove a negative. However, a review of some key postprocessual texts and readers indicates the absence of matters of violence from mainstream interpretive discourse (if one can speak of such a thing). So, for example, in their influential *Re-Constructing Archaeology: Theory and Practice*, Shanks and Tilley discuss power but there are no index references to conflict, competition, force, violence or war(fare) (Shanks and Tilley 1992). Similar lacunae are to be found in Tilley's *Interpretative Archaeology* (Tilley 1993) and Thomas's *Interpretive Archaeology: A Reader* (Thomas 2000). Whitley's *Reader in Archaeological Theory: Post-Processual and Cognitive Approaches*

(Whitley 1998) does not list violence or conflict in the index, although there are several references to warfare. However, these largely refer to quotations from processualists being attacked, or to recent colonialist aggression as an example of the 'badness' of the modern world. It is not discussed as a phenomenon of the past, worthy of study in its own right. *Domination and Resistance* (Miller et al. 1989), of which Tilley was an editor, does have extensive index entries referring to power and military force, but none to conflict, violence or war. The 1995 edition index lists violence, but the references primarily relate to 'symbolic violence'.

It is interesting that Shanks's application of his postprocessual approach to a particular cultural context and body of archaeological material, *Art and the Greek City State: An Interpretive Archaeology* (Shanks 1999), does refer to violence, especially hoplite warfare. However, this aspect is here neither critically discussed nor problematised. Rather, its treatment seems to be taken over pretty much unchanged from traditional historiography on Classical Greece: violence is primarily equated with war, which was simply a fact of life, a given among *polis* societies. Other individual writers have engaged with issues of violence, especially since the mid-1990s. In the context of recent archaeologies of colonialism, Carmen Schrire's *Digging Through Darkness* (1995) includes graphic treatments of violence clearly influenced by postcolonial thought, in a work that seems to me equally influenced by postprocessualism (although I am advised she may not accept the latter label herself). However, this describes violence, and evokes emotion at its horror, rather than analyses it. Another example is Paul Treherne's paper 'The Warrior's Beauty' (1995). This valuable piece offers a critique of conceptions of ideology, advocates the notion of 'lifestyle' through the example of European Bronze Age 'warriors' and attempts to revivify the idea of 'the [*sic*] ancient warrior élite' in later prehistoric and early historic Europe. Violence is here a key part of the conceptual framework, but is little discussed per se.

Keeley's Challenge, and Interpretive Responses

The later 1990s marked a watershed in archaeological studies of violence, especially in response to Keeley's polemical (and processualist) *War Before Civilization* (1996). This claimed a widespread 'pacification of the past' among contemporary archaeologists, denying or ignoring the extent of violence in prehistoric societies. His thesis has been attacked, for example by Thorpe, who argued that Keeley may have been right for the prehistoric Americas, but was not for Bronze Age and Iron Age Europe (Thorpe 2005:1). However, regarding the processualist and postprocessualist Anglophone archaeology which was his focus,

I argue that, even if for rhetorical effect Keeley overgeneralised from too few examples and probably exaggerated likely rates of violence (Chapman 1999; Parker Pearson 2005:25), he was indeed substantially correct about the 'pacification of the past'. Long before I encountered his book, my own independent impression was that in, for example, British Iron Age scholarship, dominated by researchers following post-processual agendas, consideration of violence—or social conflict of any kind—had been airbrushed from understandings of the period. Even more surprisingly, the same is true for most archaeological discourse on the Roman Empire. These two cases are discussed below. Parker Pearson also reports the alleged impossibility of getting funding for archaeological studies of violence in Sweden due to academic anathematisation of the subject (Parker Pearson 2005:19).

Whatever its true merits or shortcomings, Keeley's book had considerable impact, partly because attempts to prove him wrong led to new work, and because it catalysed renewed interest in warfare (if not in wider violence) already in the air, driven, in Parker Pearson's view, by the accumulation of new archaeological evidence (Parker Pearson 2005:21). This is seen in a number of books, especially conference proceedings on studies of violence and conflict, i.e., Carman's *Material Harm: Archaeological Studies of War and Violence* (1997a), Carman and Harding's *Ancient Warfare* (1999) and Parker Pearson and Thorpe's *Warfare, Violence and Slavery in Prehistory* (2005). These volumes have strong postprocessual strands, although many of the contributions still belong to processualist or other traditions.

Nevertheless, attention to violence has remained very uneven and somewhat ghettoised across period- and region-based subdisciplines in Anglophone archaeology. So, for example, during the later twentieth century, interpretations of British Iron Age hillforts changed from representing them as dens of cutthroat Celtic chieftains to processualist Central Places, and then in postprocessualist treatments to expressions of social relations and ideology. They became physical manifestations representing the symbolic construction of community, for example, through communal acts of building, perhaps shaped by cosmology, in matters such as elaborated eastern gates. The focus shifted almost entirely to ideology and symbolism, and martial functions were hardly discussed any more, even though symbolic and practical military factors are absolutely not mutually exclusive—indeed, they may be mutually reinforcing (Armit 2007; James 2007). Likewise, insular Iron Age 'Celtic art'—La Tène-style metalwork, largely comprising weaponry and equestrian equipment, i.e., artefacts designed for interpersonal violence and related display—was also virtually ignored for two decades, although this is at last changing (Garrow et al. 2008).

The 'demilitarization' of hillforts and neglect of La Tène artefacts were central manifestations of a general abandonment of the discussion of violence in British Iron Age archaeology, to which Sharples's paper on warfare in Iron Age Wessex was an exception proving the rule (Sharples 1991), as it evoked little response. Indeed, the draft national research agenda for Iron Age archaeology in England circulated in 2000—sponsored by English Heritage, but steered largely by postprocessual archaeologists—made no mention of violence, conflict or warfare at all (Armit et al. 2000), although after I criticised this during the consultation phase, reference to these matters was included in the published version (Haselgrove et al. 2001).

British-based Roman archaeology offers a similar picture. Since the 1980s, as interpretive approaches have revolutionised the subject, overt discussion of violence has remained curiously muted even in accounts of this, a militaristic, expansionist empire. Currently the leading paradigm for development of the Roman provinces (e.g., Creighton 2005; Millett 1990; Terrenato 1998a, 1998b, 2001, 2005) emphasises elite negotiation and integration, although there are dissenting voices (e.g., James 2001; Mattingly 2006; Webster 2001). This is partly because 'mainstream' Roman archaeology concentrates on the civil provinces and leaves the archaeology of armies and frontiers to a virtually separate community of military scholars still largely working in a traditional German culture-historical style (James 2002). Yet even these discuss everything pertaining to the Roman military except, in rare instances, what pregunpowder muscle-powered weapons actually did to bodies (on which see James, forthcoming b). Ancient historians dealing with the Roman military are much less squeamish about martial and other violence, if rarely informed by anything resembling interpretive theory (e.g., Lendon 1997), but any archaeological examination of the Roman military from postprocessual perspectives (e.g., Gardner 2007) remains exceptional.

Treating a much later period, Matthew Johnson's innovative *Behind the Castle Gate* (2002) critiques traditional studies of castle architecture conducted in martial functionalist terms, emphasising that such structures were also about ideological display and symbolism, factors that, especially in later examples, sometimes came to outweigh considerations of military practicality. Johnson is careful to avoid going to the opposite extreme, falling into the trap that, for a while, ensnared study of Iron Age hillforts (above)—interpreting castles in terms of their symbolic functions to the exclusion of their martial nature. However, despite his careful wordings, traditionalist critics have accused him of exactly this. In my view, Johnson left himself vulnerable to such 'resistant readings' because, while he does discuss military factors and, briefly, sieges, he does not really deal directly

with the actual violence that sometimes took place at castles and that underpinned contemporary elite value systems as a whole (chivalric masculinity, knighthood). If the objective was to show the falsity of the martial/symbolic distinction in interpreting castles, would it not have more effective in disarming (*sic*) traditionalist criticism also to address the intertwining of ideology, symbols and symbolic behaviour with fighting tactics and martial practicalities seen during sieges, and in wider contemporary warfare, both 'chivalric' *and* often ferocious? In discussions about this, Matthew has conceded that examining such aspects would have strengthened his arguments.

More generally with regard to interpretive treatments, he also agreed that

> You could argue that such is the stress in p[ost]p[rocessual] arch[aeology] on ideology/symbolism that weapons etc. are routinely interpreted without reference to (what may be) their primary function. (M. Johnson, personal communication 2008)

Thus there is a danger—often, on the evidence cited here, the reality—that violence effectively vanishes from discourse, or is perceived to do so.

If silence has characterised prevailing postprocessual treatment of violence, there have been substantial signs of change over the last decade or so, especially evident in the work of postprocessual prehistorians. Treherne's paper on the 'warrior's beauty' (1995) is valuable precisely because it places warfare in the context of society, not a separate domain 'somehow outside and largely independent of the social' (Parker Pearson 2005:25–26). Parker Pearson himself has built on Treherne's work, directly dealing with how weapons worked and why, interpreting development and use of the rapier in Bronze Age Europe, in the context of emergent honour codes (Parker Pearson 2005:27–29). In a similar vein, Melanie Giles has produced a fascinating paper examining the reality, social context and symbolic meaning of weapons and combat in Iron Age Ireland and Britain, especially East Yorkshire (Giles 2008).

Nevertheless, despite signs of growing awareness in some quarters and important examples of new work, overall there is still no developed postprocessualist discourse on violence, and such matters are still not on the radar of mainstream interpretive archaeology. I hope I will be proved wrong, but I predict that general neglect of this important aspect of human life will continue in postprocessual (and, indeed, other) archaeologies. This is because of the processes that I believe 'silenced violence' in the first place. For is this silence inadvertent oversight, subconscious avoidance or wilful evasion? Is postprocessual archaeology as a whole 'in denial' about human violence?

Why the Silence on Violence?

To some extent, the explanation for neglect of violence as a factor in the human past lies in the nature of surviving archaeological data, regardless of the theoretical orientation of investigators. Where violence is known from historical evidence to have been occurring on a substantial scale (as in the Greco-Roman world), direct physical evidence of violent practices—even large-scale warfare—can be surprisingly elusive. Few cultural contexts produce many (or any) in-your-face discoveries like the Crow Creek massacre deposit (Zimmerman 1997). Even for the Second Punic War, Rome's struggle with Hannibal (218–201 BC), probably the greatest war in Mediterranean antiquity in which hundreds of thousands died, we have as yet no scientifically studied mass graves of victims from its multiple major battles and many lesser engagements, and hardly any weapons. So, in part, neglect of violence may be a simple case of out of sight, out of mind, and of forgetting that absence of evidence is not evidence of absence.

Even where evidence for violent practices does survive, we often fail to recognise it because we are not looking for it. For example, it has become apparent that the extent of wound trauma on Iron Age British skeletons has often been significantly underreported (Boyleston 2000; Knüsel 2005:357, 375). Further, and less justifiably, even where evidence of violent practices, direct or implicit, *has* been recognised, there has also been a persistent tendency to explain it away (e.g., British hillforts) or simply to pass over it in silence (e.g., neglect of La Tène weapons). Here myopia shades into active avoidance, or denial.

A tendency to 'pacify the past' is actually neither especially new, nor by any means confined to interpretive archaeologists. To be sure, culture-historical traditions often interpreted evidence of cultural change in the past in terms of war and invasions, emphasising martial remains like Iron Age hillforts and 'Celtic' weapons, Roman frontier systems and 'Dark Age warrior burials'. However, many researchers working in such traditions did (and do) not do so, preferring, for example, to focus on the ostensibly peaceful 'real' achievements of earlier peoples, in art, architecture or urbanisation.

It is also unsurprising that many processualists have been disinclined to deal much with matters of violence. Notably in the context of prehistory, many processual archaeologists were seeking to 'put clear blue water' between their new conceptions of early societies and established views. Hence, for example, although Cunliffe remained happy to interpret the British Iron Age in terms of the warrior Celtic chiefdoms that dominated the previous culture-historical paradigm (e.g., Cunliffe 1988), most other processualist archaeologists were anxious to abandon them.

Their new understandings of the British Iron Age often emphasised instead the skills and productivity of the farmers managing the densely settled and intensively worked agrarian landscapes being revealed by aerial photography, field survey and excavation (e.g., Reynolds 1979). And, especially in a prevailing intellectual climate that regarded warfare and other forms of violent conflict as temporary aberrations punctuating peaceful normality (see below), processualist focus on the systems through which society functioned automatically gave no priority to matters seen as spanners in the works. Thus the baby of violence (and social conflict in general) was thrown out with the bathwater of culture history.

However, given the emphasis many postprocessualists have placed on power and ideology in their discourse on past societies, one might have expected that they would extend this interest to matters such as violent conflict—an obvious connection explored in regard to state power by a figure who has had a great influence on British postprocessualist archaeologists in particular, the sociologist Anthony Giddens (1985). Yet, in the event, it seems that they tacitly continued the processualist reaction against the naïve, sometimes uncomfortably bloodthirsty revelling in violence of earlier scholarship by ignoring the matter. We may ask, why?

In the case of British Iron Age research, this could be (and has been) justified by reference to the fact that Continental academic discourse on the European Iron Age, and popular historical narratives of European prehistory, are still firmly wedded to notions of ancient warrior Celts, and thus there remained, and remains, a real need to establish clear blue water between old and new conceptions of the Iron Age. However, such arguments are inadequate to explain the silent burial of violence in wider, general interpretive discourse. In part, I suggest that interpretive archaeologists have simply been influenced by the wider, Western liberal cultural context they share with processualists.

At least in the West, during the later twentieth century the levels of domestic, criminal or juridical violence experienced by most people declined strongly, as did military participation. Compared with their grandparents' lives, violence was less and less likely to form part of people's personal experience (especially among groups likely to become academics), so violence was less likely to be on people's radar when considering social interactions. At least until the destruction of the World Trade Center in 2001, most people's general perception of violence had become something largely confined to ritualised and simulated forms ('contact sports', movies) and news reports from less fortunate parts of the world.

Although the causal relationship is not entirely clear, the actual decline in violence has run in parallel in the West with a cultural rejection, general but especially among the liberal intelligentsia, of physical violence in

any form, whether military, juridical or domestic (Sheehan's *Monopoly of Violence: Why Europeans Hate Going to War* [2008] was not available to me while writing this). Most would agree that rejecting beating of children and spouses, abolishing capital punishment and condemning militarism represents a general advance in human values. This went hand in hand with a general, if rarely explicitly articulated view that violence represents aberration from the norm, an unpleasant epiphenomenon. However, treating violence and warfare as aberrant and discussing them in terms of pathology, figuratively or literally (e.g., in medical and public health literature [Krug et al. 2002]), raises potential problems for the study of the human condition. It inadvertently creates powerful disincentives to discussing violence at all. Discourse on such matters has become uncomfortable and, I suggest, stands now in the place that discussion of sex is supposed to have had in Victorian England. This makes it risky to study in fields like archaeology, since investigators are likely to encounter suspicions about their true motives, and even open hostility (Parker Pearson 2005:19).

Such an unusual situation of attached moral opprobrium makes it difficult for us to discuss these matters freely. I believe that it represents a simple continuation of wider twentieth-century Western intellectual rejection of violence, which makes it dangerous to explore other human cultural perspectives that may have valued violent behaviours (such as warrior codes [Treherne 1995]) without the researcher risking suspicion of harbouring, if not advocating, such views and so be seen as morally compromised. There are obvious robust justifications for such study ('does the student of slavery develop a yearning for its return? Is the historian of Hitler necessarily a Nazi?' [Parker Pearson 2005:19]). However, this does not necessarily dispel the cloud of suspicion.

Violence remains a cultural taboo in the current climate of wider society, in archaeology as a whole and in the wider interpretive archaeology we are considering. I believe that this is why, as I observed above, postprocessualists and indeed archaeologists in general still have not developed a fully mature discourse on violence in the past. It is also why I think that development of such discourse is likely to be halting at best. Telling evidence that the subject remains to be properly theorised is, in my view, provided by continuing conceptual confusion over fundamental terms and their meanings. It is noteworthy that recent works by, or at least initiated by, postprocessualists generally emphasise war first, and violence second. There is nothing necessarily wrong with this, of course, if the reasons are explained. However, it is striking how, in practice, there is a tendency to treat the two words as virtual synonyms and in discussion to elide 'warfare and violence' to 'warfare (and violence)', and then just 'warfare' (e.g., Thorpe 2005 and Parker Pearson

2005 for such tendencies). This privileges violence between groups and diverts attention from violence within groups, which may be at least as important. Why is this happening? Are archaeologists still unconsciously trying to place a *cordon sanitaire* around violence so that, even when they do deal with it, they corral it into 'warfare' where it can safely be treated separately, as peripheral to and aberrant from 'normal' human life? Discussion in the University College London seminar on which this chapter is based also drew my attention to a very different possibility with the same outcome. Some (including me) admit to a horrid fascination with warfare, as epitomising human violence par excellence in an archetypal form. Discussing it may give the same kind of frisson encountered in watching war films or horror movies—but which would not be generated (at least in most people) by a film or discussion on domestic violence. If repulsive to the intellect, war can seem emotionally exciting and positively attractive if (self-) represented as violence ('force') justified by a noble cause (on the deep association of war with positive and quasi-religious emotions, see Ehrenreich 1997). Similarly, warlike computer games like *Rome: Total War* attract less opprobrium than do those about criminal violence or murder like *Grand Theft Auto*.

Violence, Postprocessualists and Darwinian Approaches

When consideration of violence by interpretive archaeologists remains so limited in general, I find it unsurprising to have encountered no direct engagement with evolutionary archaeological approaches to the issue of violence in particular. This may be because, insofar as they have dealt with violence, Darwinian archaeologists apparently tend to treat it in quite abstract terms, as just another social strategy. It is not clear that they have been any more direct, or less squeamish, in dealing with these matters than other schools. However, that is for others to discuss (see Layton, this volume). Nevertheless, where such fields are deemed to impinge on the turf of archaeology, and especially with regard to human violence present and past, there has been some postprocessual reaction to wider Darwinian-based scholarship, i.e., evolutionary anthropology, evolutionary psychology and/or 'sociobiology' (work in any area of the life sciences sometimes, apparently, being lumped together under the last label).

Most interpretive discourse simply ignores these matters entirely. Treherne, in his 'warrior's beauty' paper (above), a tour de force in critiquing notions of ideology, does attack that style of scholarship that 'utterly denies the organic existence of the body' (Treherne 1995:119) and very nearly deals with warriors' corporeal violence itself (1995:127–29). Yet, on the grounds that 'bodily expression, activity and representation

are socially learned and hence culturally variable', he dismisses any consideration of 'mere biology' as a factor in understanding what people in general or warriors in particular are, think or do with a contemptuous wave of the hand (1995:117).

In the introduction to his valuable edited volume *Material Harm: Archaeological Studies of War and Violence*, John Carman, a self-described interpretive archaeologist, does not go much further (Carman 1997b). He denounces 'innate aggressionists' (unidentified, but presumably he had 'Darwinians' such as sociobiologists and evolutionary psychologists at least partly in mind) as wrong and dangerous (Carman 1997b:3–5). However, he does not actually cite any examples of recent academic research or writing taking such a position. Rather, he simply refers the reader to sections of Ashley Montagu's then already over twenty-year-old book *The Nature of Human Aggression*, which was attacking even older arguments (Montagu 1976).

More recently, Parker Pearson has written that 'violence and aggression are socially, culturally and contextually situated rather than being an inescapable condition of human nature' (Parker Pearson 2005:22), while, in seeking to explain warfare, 'what is lacking or denied in functionalist or sociobiological approaches are concepts of intentionality, decision-making and human agency in any terms other than material or biological imperatives (Parker Pearson 2005:23). The validity of this charge, however, is here hard to assess, because it comprises assertions backed up with no evidence or references.

Failure to cite specific examples of work in the fields they attack makes me wonder if interpretive archaeologists are actually reading what 'biologists' (i.e., anyone applying biological ideas, and especially evolutionary theory, to the understanding of the human past and present) really have to say at all. These passages fit with less restrained comments I have encountered in discussions at conferences and elsewhere, which (unsurprisingly) suggest few postprocessualists have much detailed knowledge of evolutionary theory or of genetics. At the most general level, any prospect of engagement between interpretive archaeologists and biologists in the broadest sense faces a gulf of mutual ignorance and incomprehension, which often spills into contempt. Fundamental differences in styles of discourse do not help. Hence many of those trained in the life sciences tend to consider much writing in the arts and social sciences, especially in traditions such as interpretive archaeology, to be full of relativistic, postcolonialist, postmodernist, post-everything-else self-indulgent drivel, often veering into total meaninglessness. The posturing vacuity of some scholarship in this vein was notoriously exposed by the physicist Alan Sokal who, in 1996, managed to dupe the cultural studies journal *Social Text* into publishing a paper full of postmodern

phraseology and quotations but actually quite meaningless (Sokal 1996). However, there is grave danger in presenting extreme examples of 'Their' views and implying that they typify the entire community, thereby stereotyping or caricaturing an entire outgroup (as 'Enemy'). This is a practice indulged in with relish by some Darwinians, notably Stephen Pinker at his sneering worst in his attacks, in various passages of his (otherwise exhilarating) *Blank Slate*, on the style of recent mainstream scholarship in the humanities and social sciences, in which he would certainly include postprocessual archaeology (Pinker 2002). Such hostility is reciprocated by some archaeologists in what looks to be the epitome of C. P. Snow's famous 'two cultures' division, first adumbrated in the late 1950s (Snow 1993). To many archaeologists the scientific terminology and ostensibly detached mode of expression found in much 'biological' writing can seem not just incomprehensible, alien and uninteresting, but lacking in emotional engagement, cold, dreary, reductionist (a term with its own negative overtones) and mechanistic. Worst of all, it looks determinist.

There is a widely held perception among archaeologists that evolutionary approaches simply equate to insistence on an innate biological drive to violence (see discussion in Layton, this volume)—that is, an emetic cocktail of violence and determinism. Anything evoking the D-word triggers knee-jerk responses among postprocessual (and many other) archaeologists, who emphasise the virtually infinite variability of human cultures and, especially recently, the importance of human agency—even if, as Andrew Gardner has observed (personal communication 2008), some interpretive writing itself appears *culturally* determinist (e.g., Gosden on humans and artefacts; see Gardner, this volume). Response to this perception of genetic determinism tends to be fierce, especially because of its supposed potential political implications. Any suggestion of an 'innate drive to violence' would give comfort and ammunition to militarists and fascists. Are biologists naïve in ignoring the dangers or advocating such ideas because they are congenial to their presumed to be right-wing personal politics—or might they even have their own sinister political agenda?

There seems to be a further perception that 'Their' minds are closed. I recently asked one postprocessualist archaeologist for her view of 'Darwinian' archaeologists and she said she believed the problem was that they have 'already made up their minds what the answer is'—a charge that seems to me just as applicable to many interpretive archaeologists, who appear equally to have decided what the answer is *not*.

We see in these reactions not simply hostility to what 'biologists' and their fellow-travellers in particular are arguing (or supposedly arguing), but also deep suspicion about their motivations. 'Darwinians' are different, dreary, determinist, therefore dodgy and probably dangerous.

However, since violence is shown (or rather, asserted) *not* to be an inevitable or inescapable part of the human condition, it can be treated as a purely cultural matter (actually, a non sequitur). Consequently, '(socio)biological' ideas are to be rejected wholesale.

In my experience, then, most postprocessual (and other) archaeologists regard 'biological theories' in general with incomprehension and lack of interest, and simply ignore them. Those moved to respond to them, especially with regard to issues of human violence, seem not to offer justified conclusions based on evidence, or on real engagement with 'evolution-informed' literature. There probably are indeed myopic determinists in the wider 'Darwinian camp'. Perhaps 'Darwinians' largely are right wingers and some even crypto-fascists; E. O. Wilson, the 'father of sociobiology', has certainly been accused of such things, but accusation is not evidence (and much that was thrown at Wilson comprised wilful misreading, or outright doctored quotes, from the work of someone who is actually a 'lifelong liberal Democrat' [Pinker 2002:108–15]). Like some of their antagonists, interpretive archaeologists moved to comment on 'Darwinian' work rarely proceed beyond caricature, denunciation or what looks like visceral prejudice to substantive critique. On either side, to engage in such tactics is not scholarship, but mere sectarian rhetoric.

A rare and important exception to the apparently general failure to engage with Darwinian literature is provided by Nick Thorpe who, in a survey of approaches to the origins of 'warfare and violence', devotes space to biologically informed approaches (Thorpe 2005:3–5). He considers several relevant strands, focusing on evolutionary psychology. Starting with Tooby and Cosmides's notion of the recent/modern human mind being the result primarily of Pleistocene environments and evolutionary pressures (citing Cosmides et al. 1992 and Pinker 1997:42), he proceeds to 'the three main competing theories of warfare situated within evolutionary psychology—territorial, reproductive and status competition—[which] should all be susceptible to analysis from the archaeological evidence of early prehistory' (Thorpe 2005:3). He offers critique under each heading. He points to evidence seemingly contradicting E. O. Wilson's claims for a universal human instinct to partition others into friends and aliens and to be territorial (citing Wilson 1978:119), counterciting work apparently showing 'no correlation between ethnocentrism and xenophobia' (citing Cashdan 2001; Thorpe 2005:3–4). He also more convincingly attacks supposed parallels between 'territorial wars' among chimpanzees on one side, and human 'gang culture' and warfare such as that of the endlessly cited Yanomamö on the other. Likewise he highlights studies undermining claims for supposed reproductive advantage offered by human warfare (Thorpe 2005:4–5).

Thorpe also questions notions that warfare is 'the inevitable outcome of violent competition between young males striving for status and prestige … channelling their natural aggression outside the group' (citing Maschner and Reedy-Maschner 1998); conceding that some evidence actually supports the idea, he cites examples that ostensibly do not (Thorpe 2005:5). He further rightly focuses on gender and violence and assumptions that it is wholly or almost wholly a 'boy thing' (Thorpe 2005:5). Highlighting the significance of the need to put warriors into an altered mental state, often drug induced, to face something so literally dreadful as combat and killing, he suggests that 'any biological urging to kill ha[s] to be heavily reinforced by cultural methods (Thorpe 2005:5). This looks to be true—although this would seem not to allow for the observation that humans can experience strongly conflicting biologically rooted emotions, such as fear and revulsion, at the same time as anger and aggression, and that here these are often overcome by biochemical means effected through rites and practices—i.e., an interaction of biology and culture.

Much of the remainder of Thorpe's paper is devoted to a survey of archaeological evidence for 'conflict', 'violence', but above all 'warfare' during the Palaeolithic and Mesolithic (Thorpe 2005:6–11). He concludes that, while there are cultural contexts that do show elevated levels of violence and indeed massacres, overall the record suggests great variations in the nature and incidence of violence over prehistoric time and space (Thorpe 2005:11). This may well be true, but study of the archaeology of the Iron Age and Roman eras has led me to conclude that sparseness of archaeological evidence for violence in any given cultural context is at least as likely to be due to non-deposition or non-survival of material traces as actual absence of mayhem (James 2007, forthcoming c). Yet even if we take Thorpe's evidence for great variability at face value, is the premise of his conclusion (a widely held assumption) actually valid? He asserts that 'the biological theories (unless they admit a considerable degree of cultural influence) *imply a constant level of violence* not supported by the archaeological evidence, which demonstrates significant variations in evidence for conflict from virtually none to apparent massacres' (Thorpe 2005:11, emphasis mine). Because evidence for violence is (apparently) highly variable in the archaeological record, levels were *not* constant: ergo, 'biological theories' are discredited. Perhaps, yet it is not clear to me that even the rawest 'sociobiological' interpretation of human behaviour *necessarily* need imply a *constant* level of violence in all regions at all times. In recognising the complexity of higher primate behaviour, and of the multiplicity of factors at work in large social groups in varied and variable environments, it seems plausible to me that it might actually predict considerable variability in

outcomes—including violent ones—of human interaction. Fixed-rate mayhem in all circumstances may only be a naïve, simplistic assumption about the outcomes of 'biological theories' made by outsiders. To my knowledge, it remains to be actually demonstrated that it is an inevitable prediction of 'biological theories'.

Thorpe, then, offers thoughtful and challenging critique, yet his arguments give this reader, at least, the impression that he was already sceptical that genetics or evolutionary heritage have any demonstrable or useful role to play in explaining violence (or 'warfare', into which he tends to elide it), and so sought arguments and cases which seemed not to fit with such explanations. This looks like 'engagement' more in the military than the academic sense. Perhaps significantly, towards the end of his critique Thorpe quotes Malinowski's argument that 'human beings fight not because they are biologically impelled but because they are culturally induced (Thorpe 2005:5, quoting Malinowski 1941:23). This implies a familiar 'either-or' stance on 'biology' and 'culture', implicitly ruling out an alternative that, in my view, better fits the cases we encounter in the human past and present: human beings fight (and/or cooperate) because they are biologically enabled *and* because they are culturally induced.

Such rare extended skirmishes aside, my mental image of interpretive archaeologists and 'Darwinians' as a whole, especially with regard to ideas on human violence, fittingly remains a military one: of two camps, each keeping to the comfort zone of their own familiar ramparts, sometimes glaring at each other across windswept intervening no-man's-land. However, I do believe that this no-man's-land could yet become fruitful common ground, for those who summon the courage to risk venturing onto it (and the risks they run may be more to their backs than to the front, for their temerity in breaking ranks).

One or two postprocessual writers do seem to leave the door open to admitting a role for genetics and the evolutionary heritage in understanding human violence. In the introduction to his *Material Harm*, although in practice he seemingly proceeds to interpret violence solely in cultural terms, Carman proclaims that he 'starts from the premise that all human beings have a genetically programmed *capability* for violence', and continues, 'In rejecting a *purely* biological explanation for aggression, the focus is shifted towards the social and cultural dimension of violent acts' (Carman 1997b:3, emphases mine). In principle, then, he concedes a *partly* biological explanation. Similarly, Thorpe qualifies his own critique: 'The biological theories *(unless they admit a considerable degree of cultural influence)* imply a constant level of violence' (Thorpe 2005:11, emphasis mine). It seems to me that this qualification is fundamental; and indeed at least some 'biologists' are quite aware of the importance of cultural variability, and are far from being the crude genetic determinists

many postprocessual archaeologists seem to assume. Stephen Pinker, for example, a leading developmental psychologist and (in the broadest sense) evolutionary biologist, has cited his own conscious decision not to have children as an example of personal and Western-cultural overriding of any supposed deterministic drive to transmit his own genes ('if my genes don't like it, they can go jump in the lake' [Pinker 1997:52]). And indeed his magnificent book, *How the Mind Works*, presents detailed evidence and interpretation explaining what humans do as inextricable products of *both* genetically framed structuring of body and mind *and* cultural factors with great variability (Pinker 1997).

Rather than dealing with 'irresistible drives', it seems that research findings in genetics, evolutionary studies and the sciences of mind suggest we are dealing with innate *capacities* and *tendencies* that may or may not be exhibited according to local cultural traditions and to specific circumstances, and that vary between individuals, partly according to genetic factors. With particular regard to human propensity to inflict violence—and equally, to avoid it, and/or to engage in peaceable collaboration—it is my own impression that while this is strongly influenced by socialisation, personal cultural experience, and circumstance, genetic make-up also plays a major role. I was powerfully (and literally) struck by my infant son's capacity when upset or angry to kick, slap, punch, pinch, scratch or bite, even before he could walk, when he had neither experienced nor yet witnessed any such behaviours. At five years old he continued sometimes to lash out when frustrated, furious or overexcited in rough-and-tumble play—but only at home, I think because he sensed that here it was contextually 'safe' to let rip. At school and at friends' houses, he never initiated attacks on other children or even retaliated to physical assaults by others; rather, teachers commented on his marked empathy for other children. To my eyes, his violence was clearly an innate *capacity*, which he quickly learned to control, to explore and to deploy selectively, according to cultural rules and social context.

To work on the a priori assumption that 'biology' or 'nature' are completely irrelevant to how humans think and behave towards each other, that 'culture' alone explains everything, is preposterous. It is poor scholarship not even to investigate whether there might be something in the notion that both 'nature' and 'culture' may be at work. To denounce 'Darwinian' approaches unheard, out of ignorance, prejudice and fear, is something worse. It is high time to start seriously considering what they *really* have to say.

Recently Pinker, in a typically bold, brilliant and challenging essay, has opened up a potentially fruitful area of common ground that archaeologists can and should help explore (Pinker 2007). He has

drawn on the work of a variety of historians, archaeologists and other social scientists to highlight a bold, counterintuitive and startling claim about the history of human violence. Notwithstanding the minority holding onto notions of a past Golden Age or Noble Savagery, it is commonly held that human societies are, at least usually, somewhat violent, sometimes very violent, and always have been; particularly with world wars still in living memory, few believe in human progress with regard to these matters over the *longue durée*. Yet, even taking into account all the mayhem we know about through time—apparently from the Palaeolithic (*pace* Thorpe), more certainly since the start of historical records, perhaps irrefutably for the last millennium and even including the carnage of the twentieth century—the evidence we possess suggests *overall* rates of human violence and killing really have declined enormously and continue to do so. Further, Pinker argues, this downward trend is so marked, so fast and so global that it cannot be explained with reference to genetics or evolution, primarily or perhaps at all: there must be a huge cultural component. However, what this may comprise, acting on vast scales of time and space, remains obscure. He highlights some candidate explanations, but leaves the question hanging. Here Pinker has created an opening for archaeological engagement in interdisciplinary discussion and investigation of this astonishing apparent long-term phenomenon. Its overall reality obviously needs further critical examination, and even if it is validated, the many major blood-soaked deviations from this alleged trend also remain enormously important, representing variability equally in need of deeper interpretation.

In my view, interpretive archaeologists can only benefit from serious engagement with those working in the sciences of genetics, evolution and the human mind, and from the investigation with an open mind of what these disciplines may have to contribute to issues of common interest—not least, the human propensity for violence. This is something I have barely started on yet myself, but what I have encountered so far seems to me too interesting, and far too important, to ignore.

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Notes

Armit et al. 2007 was not available to me at the time of writing.

In a paper on postprocessual archaeologies, especially one critical of them, I should state my own theoretical orientation. I discovered the excitement of biology and evolution about the same time I developed a passion for archaeology, and at university only just opted for the latter over zoology. Trained in a robustly empirical (i.e., theory-hostile) tradition of archaeology a decade before interpretive approaches really developed, I always maintained a great interest in, and sympathy for, the scientific approach. Subsequently, influenced by Stephen Jay Gould in particular, I became fascinated with the ways in which personalities, genealogies of scholarship and cultural contexts have influenced scientific and wider scholarly research, not least in constraining which data were identified, which were treated as significant and how they were interpreted ('soft social constructivism'). When, especially under the influence of Matthew Johnson, I decided to try to engage with formal archaeological theory, the ideas I had gleaned from biologists seemed to resonate strongly with the reflexive nature of emergent post-processual archaeology and its emphasis on the ways in which data are perceived and understood through a veil of theoretical assumptions. I found the new archaeological approaches very exciting, if too often more concerned with sounding profound than with clear communication. However, recently it has not been clear to me where interpretive archaeologies are going.

At the same time, my own life experiences made me interested in the psychology of how and why we come to see the past as we do, why individuals end up in particular theoretical schools, and why these groupings seem to be disinclined, even unable, to talk to each other (James 1993). In recent years, it has seemed to me that the newly emergent 'sciences of the mind', especially evolutionary psychology, are offering some remarkable potential insights, not least into these issues of academic tribalism. However, although a major advance, these will doubtless prove no more a panacea for understanding either ourselves or our ancestors than other approaches, and evolutionary psychology in particular seems destined to struggle with accusations of writing teleological 'just-so stories' about how we got to be as we are.

The roots of my particular interest in human violence and especially warfare will be explained in the preface of a forthcoming book, (James, forthcoming a).

To summarise, I take an eclectic approach to matters of theory, drawing not least on the two schools examined in this volume. I leave others to judge whether this is traditional, robust English pragmatism or because I am too dim to understand either approach properly, and so can't make up my mind.

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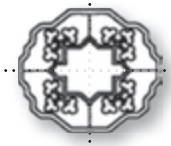

CHAPTER SEVEN

Violence and Conflict: Warfare, Biology and Culture

Robert Layton

Introduction: Interpretation and Explanation

All human societies construct worlds of cultural meaning, and all human behaviour encounters external constraints to which it must adjust. The German sociologist Weber distinguished between two methods in social science: understanding and explanation (Weber 1947:79–80). Explanation depends on detecting statistical regularities in people's behaviour that are accounted for in terms of general laws. Understanding is based on meaningful interaction with others to discover the culturally specific ways in which they make sense of the world. An interpretive approach to anthropology and archaeology seeks, in part, to understand people's motives and the meaning they attribute to their own and others' behaviour. To the extent that cultures are self-referential (e.g., that the meaning of a word or concept is defined in terms of other words within the language), interpretation depends on achieving a degree of 'intersubjectivity' with those whose activities we interpret (Layton 1997). Alfred Schutz (1972 [1932]) argued that awareness and meaning are obtained by 'reflecting' back, or casting a retrospective glance upon lived experience. Schutz used the term 'intersubjectivity' to describe the condition in which we experience the world as something whose significance we share with others because we have lived with them for long enough. To the extent that our previous experiences differ, we can never fully achieve intersubjective understanding. Human cultures are highly variable and different cultural worlds can exist within the same ecological space, as a comparison of central Australian hunter-gatherer cultures with those of the Kalahari demonstrates.

A Darwinian anthropology and archaeology, on the other hand, asks whether human behaviour is truly as indeterminate as interpretative

theorists suggest. As Meltzer (1990:186) asks, are Shanks and Tilley (1987:154–55) correct to argue that 'most social and material practices have no demonstrable physical survival value for human populations'? If correlations *can* be shown between particular patterns of behaviour and their ecological context, then external constraints upon the variety of possible cultural worlds can be detected and past behaviour can to some extent be explained in terms of general principles, without recourse to the meanings actors attributed to their behaviour. There is, therefore, potential for interpretive and evolutionary perspectives to coexist peacefully within archaeology, where interpretive archaeology reconstructs the specific ways in which people belonging to past cultural traditions made sense of the world around them, while Darwinian archaeology identifies the environmental constraints to which behaviour had to adapt if those cultural traditions were to persist (Layton 1992). James makes similar points in his even-handed companion piece in this volume.

There is also scope for conflict between humanistic and scientific archaeologists. Those who favour the interpretive approach, taking their cue from Derrida (1976), can point out that science often relies on explanatory models that are derived from the particular way in which the scientists' culture makes sense of the world. With the collapse of European colonialism, native peoples argued that anthropology and archaeology had been used to justify colonial conquest, with theories and interpretations imposed on the weak by the powerful. Shanks and Tilley (1987) pointed out that we inevitably make sense of the past from our perspective in the present. The people whose remains we excavate are powerless to challenge our interpretations because they are dead. A striking example is that of the Aboriginal Tasmanians. In the nineteenth century, Tasmanians were widely thought by Euro-Australians to be related to Neanderthals. The last of the 'full-blood' Tasmanians were well aware of the value that had been attributed to their bodies as curios or scientific specimens, but efforts to protect them failed. William Lanney's head was stolen, his hands and feet taken to the Royal Society of Tasmania's rooms and a tobacco pouch made from part of his skin. The Royal Society subsequently obtained the entire skeleton of Truganini and placed it on public display until 1947 (Ryan 1981:216–20; see also Richardson 1989).

This chapter critically assesses Darwinian explanations of human warfare, where both the usefulness, and the excesses, of explanation in the social sciences can be highlighted. Hobbes's claim that the original human condition was one of a war of every man against every other man is a classic example of theory based on presupposition rather than empirical research (Hobbes 1970 [1651]:65). Hobbes's only effort to provide empirical support for his theory was the throw-away remark, 'For the

savage people in many places in America have no government at all; and live at this day in that brutish manner' (Hobbes 1970 [1651]:65). It is surprising how often Hobbes is still evoked, when he had so little evidence to support his claim. Kaplan, in 'The Coming Anarchy', portrays the consequence of social disorder in Africa as '*nature unchecked*' (Kaplan 1994:54, italics in the original), while Chagnon (see below) gives a Hobbsian account of Yanomamö warfare. Hobbes's personal experience was of the English Civil War, and his 'original condition' was nothing more than an imagined counterpoint not unlike the 'environment of evolutionary adaptedness' of some recent writers, such as Cosmides, Tooby and Barkow (1992) and Pinker (1997:42). Hobbes's continued popularity is due more to his epitomising a deep thread in European cultural thinking than to empirical accuracy.

Evolutionary theorists can, on the other hand, be equally critical of social theorists' claim that culture has exempted modern humans from the constraints of natural selection. Giddens claims that people's behaviour is grounded in a need for 'ontological security' rather than adaptation to the environment (Giddens 1984:86, 228). He confuses the Darwinian theory of adaptation with nineteenth-century progressive evolutionism, arguing that to be plausible a theory of evolution must identify a single mechanism of change and a universal sequence of types of social organisation. As Dunnell (1988) shows, social scientists have repeatedly failed to appreciate that Darwinian evolution is not progressive. Darwinian fitness is not defined in absolute terms, but in relation to the organism's specific local environment. Progressive evolution is driven by the internal dynamics of a social system. Marx gave the most detailed and persuasive account of progressive social evolution in his study of the growth of capitalism, but even Marx recognised that there may be more than one pathway in progressive evolution (see Marx 1964). There is no overall direction in Darwinian evolution. Darwin argued that random variations between individuals in a population have different consequences for survival in a particular environment; what is adaptive in one environment will be maladaptive in others.

Darwinian theory has, however, been refined through the concept of coevolution, which tends to shift the explanation for evolutionary change away from the individual organism towards the interaction of organisms in an ecological system through parasitism, symbiosis and competition between predator and prey. This provides a useful analogy with social evolution. Coevolution recognises that species interact and affect each others' fitness. In van Valen's (1973) 'Red Queen hypothesis', cheetahs preying on gazelles favour the survival and reproduction of the fastest gazelles, but this in turn creates selective pressure on successive generations of cheetahs, favouring the fastest hunters. Both species must run

faster to 'stay still'. Cavalli-Sforza (1971) proposed that culture could offer an alternative source of adaptations to genetic evolution, thus enabling the coevolution of genes and culture, an argument that has since been taken up by many writers (for recent examples see Enquist and Ghirlanda 2007 and Henrich and Boyd 1998). The most famous example of human-animal coevolution is seen in adaptive responses to dairy farming (see Holden and Mace 1997). Adult hunter-gatherers cannot, generally, digest lactose. They do not need to, because milk is not part of their diet. The introduction of dairy production from domestic cattle modified the human environment. Some farming populations responded by undergoing natural selection, through which the minority of individuals who had a genetic capacity for lactose digestion in adulthood had greater reproductive success than those who had not inherited that capacity. Other cultures discovered ways of breaking down lactose by using bacteria to create yogurt and cheese, to overcome the genetic deficiency. Clearly Neolithic societies lacked detailed knowledge of bacteria and cultural adaptation almost certainly proceeds by a mixture of random and intentional variations in behaviour (Cassidy and Mullin 2007; Mesoudi 2008). Even when cultural variation is random, the capacity for culture may be adaptive. Since new cultural traits can be adopted during people's lifetime and are not only transmitted from parent to child, culture may allow adaptive behaviours to occur more rapidly than can be achieved through genetic change. Not all cultural traits are adaptive, as Dawkins (1976) has pointed out, but some genes are also maladaptive.

Applying Interpretive and Explanatory Methods in Archaeology

Archaeology is situated between geology and sociology. Are the methods of either sociology or geology applicable to archaeology? Hermeneutics (interpretative sociology) has its origin in biblical scholarship (Eco 1990:11–22). Faced with the possibility of unlimited allegorical interpretation of biblical passages, scholars such as Augustine and Aquinas sought to restrict interpretation to only those senses that could have been intended by the authors (a constraint that Shanks and Tilley brush aside). Interpretative archaeology has a similar problem. There appears to be a meaningful pattern in the distribution of material remains, but we cannot be certain what meaning was intended. The term 'abduction', originally used by Peirce and revived by both Eco (1990:59) and Gell (1998:14), is useful here. It characterises the logical procedure a person can adopt when they *think* they have detected a meaningful pattern in events and act upon that supposition (Eco 1990:59). One of Eco's examples, taken from Peirce, is the following: 'I once landed at a seaport in a Turkish province, and met a man on horseback, surrounded by four horsemen holding a canopy

over his head. As the governor of the province was the only personage I could think of who would be so greatly honoured, I inferred that it was he. This was an hypothesis' (Eco 1976:131, citing Peirce 1931–1958). Peirce could presumably have interviewed Turks and tested his understanding by behaving in ways that seemed appropriate (bowing before the man shaded by the canopy, perhaps). The problem for archaeology is that we cannot live among the people with whom we seek to achieve intersubjective understanding. Like biblical scholars, we must ask what interpretations are authorised by the 'text' we examine. Richards's study of the Neolithic landscape on the Orkney Islands of Scotland is one of the most convincing, since the similarities and differences between the tomb of Maes Howe and the nearby contemporary houses are precise, and the narrow entrance to the tomb faces the sunrise at the winter solstice (Richards 1996:202). Most prehistoric landscapes are more ambiguous than Maes Howe.

Giles (2008) combines explanation and interpretation effectively in her analysis of the social role of weapons in late Iron Age Yorkshire (see also James, this volume). On one hand, the paper presents explanatory generalisations, based on anthropological evidence, about the importance of violence in agropastoral populations. This argument can be supported by evidence that, in Africa, pastoral nomads rely on men to defend livestock and generally practice patrilineal descent, whereas horticulturalists, where much productive work is done by women, frequently practice matrilineal descent (Holden and Mace 2003; cf. Goldschmidt 1979 on pastoral societies more generally). However, quantitative measures of the injuries apparent on skeletons from late Iron Age cemeteries in Yorkshire show that only 4% display evidence of wounds that might have been sustained during violent events. Acknowledging that not all fatal injuries would leave a trace, Giles nonetheless infers that weapons were therefore designed to deter by display as much as to kill. Moving to interpretation of how that goal might have been achieved, Giles recognises (2008:68) that 'martial weapons "worked" on people because these individuals were predisposed to interpreting the world in a particular way'. Drawing on Gell's concept of the technology of enchantment, she suggests that the complexity of Celtic decorative designs, seen during display but not combat, dazzled the viewer's eye. The culturally specific association of the colour blue with women's ornament and red with martial equipment lends support to an interpretive association of redness with anger and violence.

However, both Richards and Giles resort to supposedly cross-cultural cognitive structures. Richards's interpretation relies on structural equations such as life is to death as above ground is to underground, Giles to the cross-cultural association of the colour red with violent combat. If we accept Derrida's postmodern logic, that is the point at which an

exotic culture 'is shaped and reoriented by the glance of the foreigner' who imposes familiar categories on the unfamiliar (Derrida 1976:113). Interpretive archaeology makes us aware that past human behaviour was rich in meaning. Having opened this new door onto the past, however, interpretive archaeologists can merely stand on the threshold where familiar cultures remain accessible to a backward glance.

While interpretation seeks to particularise, explanation seeks to generalise. The long time span available to archaeologists in which to study changes in technology and social behaviour, and the huge range of natural environments in which humans have lived, offers opportunities to test adaptive hypotheses that are denied by the brief time span and limited range of societies studied in sociology. They also offer archaeologists the opportunity to study changes that proceeded at too slow a rate to be perceptible to human agents (Dunnell 1988). However, while cases such as the evolution of dairy farming demonstrate that genetic and cultural evolution can occur over a few thousand years, some theorists (particularly those adhering to the school of evolutionary psychology founded by Cosmides and Tooby [2004; Cosmides et al. 1992]) tend to argue that all significant human genetic evolution occurred during the much longer period our ancestors were hunter-gatherers living in the so-called 'environment of human adaptation'. This legendary environment (was it savannah, or semidesert, or tropical coast?) facilitates a number of just-so stories. Miller (2000), for example, has suggested that serial monogamy and female choice in Pleistocene human populations provided selection pressure for creative displays by human males analogous to those of bowerbirds, giving rise to art and dance. Miller commits the fallacy, common among nineteenth-century social scientists, of explaining the known (contemporary art) in terms of an unknown (the imagined role of art in early human evolution). Wrangham and Peterson (1996:63) draw a parallel between human and chimpanzee behaviour to argue that warfare is genetically determined, that 'chimpanzee-like violence preceded and paved the way for human war, making modern humans the dazed survivors of a continuous, 5-million-year habit of lethal aggression'. These universalising explanations for specific behaviours are essentially untestable. To be persuasive, an adaptive explanation must be able to correlate variation in behaviour with demonstrable variation in the environment.

Evolutionary Hypotheses for Human Violence and Conflict

Simon James's paper in this volume distinguishes between war and the broader category of human violence, noting that recent archaeologists of both evolutionary and interpretive schools have avoided

discussing violence. In the remainder of this paper I restrict discussion to evolutionary hypotheses for the incidence of warfare in human societies. Sillitoe (1978:252) defined war as 'a relationship of mutual hostility between two groups where both try by armed force to secure some gain at the other's expense' (see also James, this volume). The 'naturalness' of warfare has been debated at least since the European Enlightenment. Hobbes's claim that the original human condition was one of a war of every man against every other man was challenged within 40 years: Locke countered that humans are naturally social, writing, 'Men living together according to reason, without a common Superior on Earth with Authority to judge between them, is *properly the state of nature*' (Locke 1960 [1689]:280, italics in the original). This view was developed further in the following century by Ferguson: 'Mankind are to be taken in groupes [*sic*], as they have always subsisted' (Ferguson 1995 [1767]:10). A solitary wild man caught in the woods, Ferguson argued, is no more representative of humanity's original state than an eye that had never seen anything. The most certain aspect of the environment of evolutionary adaptedness is that early humans were social animals.

An influential literature has nonetheless developed over the past 20 years claiming that violence is not only inherent in human nature, but that it is adaptive. During the 1960s, writers such as Ardrey (1967) and Lorenz (1966) popularised the idea that warfare was linked to 'instinctive' defence of territories. Chagnon (1988) later claimed that among the Yanomamö of South America, killing other men and capturing their women enhances male reproductive success, and suggested that the Yanomamö represented the typical condition of small-scale human society. Chagnon evokes a Hobbesian image of the Yanomamö when he describes a Yanomamö man who was sent by missionaries to the territorial capital: 'There he discovered police and laws. He excitedly told me he had visited [the territorial governor] and urged him to make law and order available to his people' (1988:990). Although Chagnon (1988:989) expressed the view that 'similar tribes while still independent of the nation state' would show similar levels of violence, he stopped short of claiming the Yanomamö represent the original human condition. Wrangham and Peterson (1996:64) went further in their claim that 'no human society provides a better opportunity for comparison [with chimpanzees] than the Yanomamö because they have been so remarkably protected from modern political influences'.

The Argument for Natural Selection of Warfare during Hominid Evolution

The evidence from chimpanzee behaviour is not as clear-cut as Wrangham and Peterson assert. The first observations of intergroup

violence among chimpanzees were made by Jane Goodall (1986) at Gombe and Nishida et al. (1985) at Mahale. These are, in fact, the only two plausible cases of a chimpanzee troop taking over territory belonging to a neighbouring troop. The events at Gombe took place after the supply of bananas, used to habituate the chimpanzees to the presence of Goodall's research team, had been drastically reduced. The Gombe chimpanzee community then split into two groups and became polarised within a range they had previously apparently shared. Over a period of two years the males of the larger group killed at least some of those in the smaller group and took over their females and territory. While chimpanzee territories in the Mahale Mountains are generally exclusive, groups 'M' and 'N' showed, for a time, some overlap of ranges. The area where this overlap occurred was later occupied exclusively by group 'M'. There is circumstantial evidence for raiding, but no direct evidence that one group of males systematically wiped out another in order to gain access to females and hence increase their reproductive success. There is substantially more evidence of chimpanzee males killing members of neighbouring communities during border patrols. A more recent study at Gombe (Wilson et al. 2004) concludes that such killings may be perpetrated to reduce competition for food or the number of rival males, but not to capture females.

A Broader Perspective on Violence between Human Groups

Is warfare in small-scale societies always as ruthless as Chagnon suggests? In 1932 the Australian anthropologist Stanner witnessed a 'large-scale fight' between two Aboriginal groups. Despite the 'anger, challenge and derision' on both sides, there was also control. Only light duelling spears were in use. Towards sunset, the battle ceased 'and some of the antagonists began to fraternise'. Several weeks later, Stanner attended an initiation ceremony. Both sides to the dispute were present. Even though they were 'at violent enmity . The bad feeling had been suppressed, after the aboriginal fashion, for a necessary tribal affair' (Stanner 1960).

Aureli et al. (2002) provide a modern parallel to the arguments of Locke and Ferguson. They propose that species as dependent on social life as chimpanzees and humans should evolve mechanisms for resolving disputes:

> For gregarious animals, conflict of interest, while unavoidable, may compromise the benefits of group living or neighbourliness, *especially when it escalates into aggression* . Behavioural mechanisms that mitigate conflicts, prevent aggressive escalation and resolve disputes should therefore be strongly selected in animal living in stable social organisations . Similar costs are likely in territorial species that have stable relations with neighbours (325, emphasis mine).

They cite, as one example, the observation that male chimpanzees engage in reconciliation more frequently than females. Moreover, the frequency of border patrols among chimpanzee communities varies. Patrols are much less frequent at Budongo than at Gombe (Sean O'Hara, personal communication 2009). Intergroup aggression among chimpanzees may therefore be stimulated by pressure on resources induced by loss of land to encroaching human farmers, which is more of a threat at Gombe, suggesting that the frequency of intergroup aggression among chimpanzees is a response to specific ecological conditions. Nettle (2009; cf. Cosmides and Tooby 1992:209) distinguishes between evoked and transmitted culture. 'Transmitted culture' refers to learned traditions such as carpet weaving techniques, whereas evoked traits are traits that, while having a genetic basis, are only expressed (evoked) in specific environmental conditions. Nettle argues that there is abundant scope for humans to have developed the kind of environmentally contingent behaviours that are underpinned by evoked cultural adaptations. He cites research showing that in populations prone to experience food restrictions, people seek reproductive partners with an ample body size, whereas in affluent Western populations, people prefer mates with relatively thin bodies. The principle of evoked culture offers a means to overcome the deterministic character of some arguments for a genetic basis in human behaviour. Both humans and chimpanzees have the capacity for cooperation and for conflict, and it this dual capacity that allows us to construct hypotheses about the specific conditions that may evoke violent conflict over resources.

When Might Warfare Be Adaptive?

Earlier claims that humans are genetically predisposed to wage war therefore need careful scrutiny to establish

1. the specific ecological circumstances in which violence appeared to contribute to reproductive success;
2. archaeological evidence for the conditions leading to the onset of violent conflict in human populations.

I propose to follow Locke and Ferguson's lead in moving beyond a Hobbesian approach. Locke and Ferguson argued that people enter into social relations out of self-interest. Ferguson had the ethnographic evidence of Lafitau's study of the Iroquois to support his conclusion that, before the state assumed responsibility for upholding the law, people owed their safety to 'the warm attachment of their friends, and to the exercise of every talent which could render them respected, feared or beloved' (1995 [1767]:211). The problem, then, is to explain the

conditions under which people repudiate social relationships. Keeley sketches a picture of warfare as endemic in human society at least since the origin of cultivation and perhaps earlier. Even Keeley (1996:127–28, 139–40), however, concedes that the incidence of war varies in time and place.

Evolutionary game theory (Maynard Smith 1982) has provided a very productive way of explaining why individuals may change their strategy according to circumstances. Intentionality is not necessarily implicated; the bottom line is the relative impact a choice between competing strategies has on organisms' reproductive success. Two models from the theory of games are particularly helpful: the prisoner's dilemma and the zero-sum game. The prisoner's dilemma uses the model of two suspects who have been arrested and are being interrogated in different rooms. Each is told that, if they alone implicate the other in the crime, they will be rewarded. If both confess, however, both will receive a moderate sentence, since their confession helped the police solve the crime. Finally, if one refuses to confess (i.e., refuses to 'defect'), even though the other has done so, his sentence will be heavier. If the other prisoner is suspected of having confessed, it will therefore be better to confess oneself (Trivers 1985:389–90). At first sight, the most rational plan seems to be to defect rather than trust the other prisoner to remain silent. Mutual defection is, however, more costly than cooperating with the other prisoner to ensure both remain silent. If neither confesses, both must be released without charge. Since they are secluded from one another in the cells, anticipation of the other prisoner's response must be based on prior knowledge. If the game is played once, the best strategy will be to defect, because the risk of betrayal is too great. To rely on cooperation, the prisoners must have already interacted with each other in ways that test their loyalty to one another. Axelrod (1984) found the simplest strategy, when such a dilemma is faced repeatedly, is to start by anticipating the other will cooperate (not confess) and then, on subsequent occasions, do what the other player did in their previous move. In this way other players who cooperate are rewarded, but those who defect are punished. However, Axelrod also found that if an end to mutual dependence can be foreseen, partners in reciprocal exchange will succumb to the temptation to defect from the relationship.

The prisoner's dilemma model can be used to explain intergroup violence among the Yanomamö. The Yanomamö are horticulturalists who subsist on a combination of hunting and plantain cultivation. They live in politically independent villages and rely on alliances built through marriage exchange with neighbouring villages to lessen the risk of attack. Helbling proposed that the specific context of Yanomamö social behaviour promotes violence. He argued that they are trapped in a form

of the prisoner's dilemma that discourages the development of reciprocal altruism. Each village must convey the impression that they are 'tough guys' rather than trusting 'suckers'. Further, if their partners in an exchange relationship betray them, the effect of military defeat would be so devastating that it would be too late to punish the partners by not reciprocating in the next round of the 'game', as many of those who were betrayed will be dead (Helbling 1999:108–9). This creates a social environment that favours aggressive individuals. Alliances will only be sustained if both sides anticipate a long-term benefit, an outcome that is difficult to rely upon under such circumstances (Helbling 1999:111). Not only is this scenario particularly acute among politically uncentralised horticulturalists such as the Yanomamö (compare warfare in Highland New Guinea), the Yanomamö do not live in a state of primeval isolation from the wider world. They had interacted with outsiders since the eighteenth century as victims of slave raiders, enemies of settlers and subjects of missionary endeavours. More recently they have suffered attacks from gold miners. Competition for resources obtained from outsiders such as guns and machetes increases violence. The Yanomamö do not represent the original human condition.

In another example, the disintegration of Yugoslavia after the Yugoslav Communist Party was disbanded in January 1990, illustrates the endgame in the prisoner's dilemma. As McAdam et al. (2001:251) wrote of Russia under Gorbachev's reforms, 'Time horizons contracted rapidly. On the large scale and the small, people could no longer count on payoffs from long-term investment in the existing system; they reoriented to short-term gains and exit strategies.' In Yugoslavia, 'conflicts over various issues in shifting localities were symbolically manipulated to polarise public opinion along the lines of resurgent ethnic identities' (Denich 1994:369). Serbs recalled the wartime atrocities of the Ustashe, pointing to the fact that Tudjman, the Croatian leader, had revived the chequerboard Croatian flag last flown by the World War II Fascist Ustashe. Croatians countered by recalling massacres and forced relocations of Croats perpetuated by the wartime Serb Chetniks, and the killing of tens of thousands of anti-Communist refugees turned back at the Austrian border by the British army (Denich 1994:379; Tanner 1997:160). Denich quotes a Croatian Serb, 'So long as Yugoslavia's federal structure was emphasised, we didn't raise questions about national [Serb or Croatian] consciousness and national institutions. We considered Yugoslavia to be our state But now that there are fewer and fewer Yugoslavs and more and more Croats, Slovenians, Serbs, Albanians and so on, we realised that we Serbs in Croatia need to return to our own national identity' (Denich 1994:377). Like prisoners caught in the 'dilemma', they switched to mutual defection. Jansen analyses the published accounts of

three women (two Croat, one Serb) of the dissolution of the Yugoslavian state. The novelist Dubravka Ugrešic wrote, 'suddenly everything had to change: address books, the language and our names, our identity . Everything changed with astonishing speed into old garbage' (quoted in Jansen 1998:95). People who had not discarded their Yugoslav identities became known as 'Yugozombies'.

An archaeological case potentially amenable to this explanation is warfare among the Pueblos of the Southwest United States. Tainter (1988:183–86) provides a detailed and ingenious analysis of the collapse of the Pueblo society centred on Chaco Canyon, hypothesising that peripheral communities withdrew from the political system after AD 1100 as the benefits they gained from long-distance trade declined. Tainter argues that long-distance trade between lowlands and uplands benefited both regions but, as population increased and the distance between settlements declined, the benefits of intercommunity exchange were lost. Keeley (1996:104) argues that violent deaths among the Pueblos increased during the same period. McGuire (2002) looks critically at the evidence for Pueblo violence and its use by other archaeologists to support Hobbesian arguments. McGuire's conclusion is consistent with the argument in the present paper. He proposes the question asked should not be, were ancient Pueblo people violent or not? but, how are warfare, social change, cannibalism, adaptation and religion related (McGuire 2002:127)? 'People are not by nature either peaceful or warlike; some conditions lead to war, others do not' (McGuire 2002:141).

The second useful model derived from game theory is the zero-sum game. In a non-zero-sum game, players can increase their winnings through cooperation. The prisoner's dilemma was devised to show how a zero-sum game could be transformed into a non-zero-sum game (Nasar 1998:118–19). In a zero-sum game the winnings are fixed, and players are therefore in competition to see who can gain the largest share. Low-latitude hunter-gatherer bands in the Kalahari, Australia and Malaysia generally allow neighbouring bands to share resources in their territory during times of abundance (Endicott 1988; Layton 2005; Lee 1979; Peterson 1975). This is a non-zero-sum game. Peterson and Long argue that even in the rich tropical woodland of Arnhem Land, an Aboriginal band of 40 occupying a territory of 400 km^2 would have had to defend a boundary of 70 km, equivalent to 2 km/man. Boundary defence is therefore not practised anywhere in Australia (Peterson and Long 1986:29). Moreover, 'Aborigines, and most other hunter-gatherers, live in environments subject to great fluctuations in the weather and in the abundance of game and plant resources' (Peterson and Long 1986:143). When water fails at one water hole during a drought, people can join relatives or exchange partners at other water holes. Rain falls unevenly

in the deserts of Australia and southern Africa, and after rain everyone converges on the fortunate area to exploit its plant foods (see Layton 1986:26, 34–35 and Myers 1982:183 on the Western Desert). In the Kalahari, drought occurs two out of five years and is severe in one year out of four, but rainfall can vary by a factor of ten over a few miles (Lee 1979:352). Mutual insurance against local drought was one of the main reasons for maintaining interband links among the G/wi (Silberbauer 1981:459).

Dyson-Hudson and Smith (1978) draw on studies of animal behaviour to suggest that hunter-gatherer bands would only defend their territories when resources were sufficiently dense and predictable to justify the cost. Once this is the case, the benefits of interband cooperation disappear. Field (2004) appeals to Dyson-Hudson and Smith's predictive model to explain the emergence of defended sites on Fiji. The Northwest Coast of North America provides the best ethnographic example of such a process. In the ethnographic past, trespassers from other bands found on lineage land were killed (Boas 1966:35–36; Drucker 1965:47; Hunn 1982:33–34). The Northwest Coast of North America has, however, been inhabited since 9000 BC (Maschner 1997). During the long period between 9000 and 3500 BC, when the shoreline fluctuated as the postglacial sea level rose, groups were small and mobile. The first evidence for conflict on the Northwest Coast occurs by 3000 BC. It coincides with the earliest shell middens, suggesting more stable settlements, and is seen primarily in non-lethal skeletal injuries. Maschner cautions that violent conflict may have occurred earlier, without generating archaeological evidence. From AD 20–500, however, the onset of warfare is evident in the construction of defensive sites, the amalgamation of what may have been single lineage communities into large villages and population decline. The recurve bow was introduced to the region at that time. Thus, even if a tendency to violence existed among earlier Northwest Coast populations, warfare between coastal settlements is a product of specific ecological and social conditions discouraging reciprocal altruism between villages. As Field (2004) argues, ecological change that results in resource patches becoming denser and more predictable in their distribution, and therefore more readily appropriated or defended, transforms the non-zero-sum game played by low-latitude hunter-gatherers who benefit from access to each others' territories in times of local shortage into a zero-sum game, where resources are finite and worth fighting for.

A similar study, based on data from the small Polynesian island of Rapa (Kennett et al. 2006) found that the island was colonised around AD 1200, but that people first resorted to fortified hilltop settlements nearly two to three hundred years later, with eight of the island's

ten fortifications dating to between AD 1650 and 1825, more than 400 years after colonisation. Kennett et al. consider two possible causes: resource depletion caused at least partly by human modification of the environment, and population growth.

Conclusion

The interpretive approach alerts us to the many ways in which attempts to explain the incidence of warfare from an evolutionary perspective may be undermined by cultural bias. Our theories may derive from personal bias or from political motives (see discussion in James, this volume). Bald claims that we have a genetic disposition for war are not rigorously testable, since they do not specify the conditions under which the hypothesis would not apply. Archaeological evidence is ambiguous (Edgeworth 2003; James, this volume). There will always be a risk of pushing analysis beyond the point where openmindedness and material evidence allow us to discriminate between alternative explanations. Were fatal injuries accidental or deliberate? Were weapons made for display or for practical use against other humans? Was cannibalism practiced on the bodies of loved relatives or despised enemies? I have argued that, providing we accept the limitations of available evidence, the uniformitarian principles on which evolutionary explanations are based help archaeology throw useful light on the dynamics of conflict and cooperation in human societies by broadening the study of conditions that provoke warfare and hence, perhaps, gaining a better appreciation of how to ameliorate the risk of future wars.

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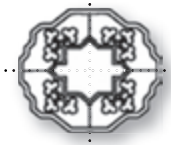

CHAPTER EIGHT

Tribes, Peoples, Ethnicity: Archaeology and Changing 'We Groups'

Ulrike Sommer

The Definition of the Term 'Ethnicity'

The word 'ethnicity' has been used in several contexts, both as a seemingly more neutral description for what used to be called 'tribes', 'peoples' and 'races' and for minority groups inside bigger political structures, normally nation-states (Eriksen 1993). In the this chapter, I will look at some of the definitions of ethnicity, followed by a discussion of the study of past ethnicities by archaeological means. A case study of a change of material culture in the early/middle Neolithic of Germany will illustrate the possible links between material culture, within-group conflict and the formation of a new ethnic groups (ethnogenesis) approached from an interpretive perspective.

Definitions of ethnicity are numerous and contested. Basically, two types of definition can be differentiated. An essentialist definition describes what an ethnic group *is*, whereas a constructivist definition tries to describe what an ethnic group *does*. Most scholars agree that common descent, a name, a shared history and a common culture, language, religion and system of values form important parts in a definition of ethnicity. These factors can either be taken as real or as generally believed in by the members of the group.

Sokolovskii and Tishkov (1996) have classified the different approaches to ethnicity as primordialist, instrumentalist and constructivist. According to these authors, primordialist theories of ethnicity assume the objective reality of 'some real, tangible foundation to ethnic identification' (Sokolovskii and Tishkov 1996:191), which can be either biological (common descent and an instinctive altruism toward biologically related persons) or cultural (an enculturation from early childhood that is rarely, if ever, overcome).

Under the instrumentalist paradigm, existing cultural practices, forms and values are mobilised as a political instrument in the pursuit of a specific agenda. The concept also allows for latent or silent ethnicity, in the case of groups who do not use common cultural practices as the basis for *displaying* group membership and pursuing common aims or claims. So far, an instrumentalist definition of ethnicity does not seem so very different from a constructivist one. In principle, the formation of new ethnic identities in the Roman Empire (Roymans 2004) or by early medieval peoples around aristocratic warrior groups and their followers (*Gefolgschaft*), described by Wenskus (1961), could also be termed instrumentalist ethnicity. The difference to a constructivist approach lies more in the ideological loading of the term 'ethnicity' than in its definition. Many 'instrumentalist' authors see ethnicity, or indeed any form of tribal organisation, as a developmental stage to be overcome on the way to a modern nation, either by assimilation to the majority culture ('melting pot' ideology) or by the development of tribes or tribal coalitions into 'proper' postcolonial nations in what was called the Third World. 'Ethnicity was treated as a remnant of the pre-industrial social order' (Sokolovskii and Tishkov 1996:xx) and tribalism or 're-tribalisation' (Hughey 1998) as a threat to peaceful global progress. Under this premise, ethnicity takes the guise of manipulation by conniving and implicitly reactionary elites, or the regression into an imagined previous state by alienated economic migrants. Ethnicity is implicitly treated as an ideology, or rather, a form of false consciousness used to manipulate a basically mute and passive majority. On the other hand, the concept of silent ethnicity implies the objective existence of definitive and unequivocal criteria for (potential) membership in an as yet non-existent ethnic group and thus has links to an essentialist definition.

I personally do not find the term 'instrumentalist' very helpful, as any ethnic identity has an instrumental aspect. The boundary to constructivist definitions is diffuse at best. It also implies an element of conscious manipulation as opposed to 'naturally grown' identity groups with a long history that introduces essentialism through the backdoor and attributes ethnicity only to the Other (Bhabha 1994:359–67). A constructivist approach emphasises the situational, contextual and manipulated nature of social and ethnic identities: 'ethnic identity is socially constructed and subjectively perceived' (Hall 1989:19). This approach also emphasises the political uses of ethnicity, 'such as the ability to structure inter-group relations and to serve as a basis for political mobilization and social stratification' (Sokolovskii and Tishkov 1996:192). Ethnicity is seen as an 'imagined community' based on the belief in common descent, shared history, the creation of a common system of values and a common culture. This can be accompanied by the adoption of a common language and religion.

Ethnicity is a 'continuing ascription' (Sokolovskii and Tishkov 1996), determined by the current circumstances of the group and the position of the individual in question. It is thus highly contextual and situational. Even if the group itself can persist over long periods of time, neither the composition of the membership nor the criteria adopted to select and describe members nor the signs adopted to signal this membership need to remain constant (Barth 1969a); indeed, they are highly unlikely to do so.

Sokolovskii and Tishkov (1996:193) place the postmodern approach, which is seen to emphasise the fluid and 'mercurial nature of ethnicity' in a fourth category. Postmodern concepts of ethnicity stress the concepts of hybridity, creolisation, the free choice of identities and the existence of situational identities, but start from a constructivist framework. In the essay 'Locations of Culture', Bhabha (1994:2) discusses how the 'in-between-spaces' between formerly fixed categories like class, race and gender 'provide the terrain for elaborating strategies of selfhood—singular or communal—that initiate new signs of identity and innovative sites of collaboration, and contestation in the act of defining the idea of the society itself.' This is a specific trait of the 'present', however one prefers to label it, and possibly something quite new and unique. Bhabha claims that 'strategies of [presumably individual] empowerment' (1994:2) can be formulated in the interstices of competing claims of the national and diverse 'ethnic' groups on the individual that may be antagonistic and incommensurable. When Bhabha talks of 'performance of cultural engagement' and the 'presentation of difference' (1994:3), this is not about the inherited and unchanged values and traits inherited from previous generations, but about the creation of difference (and similarity) to fit the present individual or political situation. Stuart Hall (1992:302) has also argued that globalisation and postmodernism have destroyed traditional localised cultures and created detached, disembedded and infinitesimally fractured identities. Jürgen Habermas (1979:110–16) has pointed out that personal identity (ego-identity) only comes into existence with the advent of modernity. Previously, identity was mainly defined by descent, residence and inherited group affiliation (tribe, guild, caste, church, corporation). In modern society, individual choice of vocational identity supports changing ego-identities (Habermas 1979:110).

It is thus questionable if postmodern concepts of identity can be transferred to prehistory. On the other hand, there have been a number of ethnographic and ethnoarchaeological studies looking at the importance of ethnic affiliation, the possibility of changing it (Barth 1969a; Haaland 1969; Hodder 1982a) and the way ethnic identity is created and adapted (cf. Cohen 1969). They have emphasized that ethnic units can be stable and quite permeable at the same time, and that the strength of ethnic affiliation is determined by the specific historical circumstances of each

group. In some periods, ethnicity might indeed have been determined at birth and remained unchanged for the rest of a person's life; at other times, individuals had to change their individual status or ethnic ascription radically, sometimes more than once. Slavery and deportation in ancient empires, colonisation, slavery and forced expulsion in the eighteenth and nineteenth centuries and labour migration in the nineteenth and twentieth centuries are only some examples. Ethnicity is also dependent on sociopolitical context—on the social position, gender and class membership of a person (Hall 1999). It can be argued that medieval peasants, for example, probably defined their ethnicity quite differently than the aristocracy (Smith 2008).

In archaeology, the most influential publication about ethnicity has undoubtedly been Frederik Barth's volume *Ethnic Groups and Boundaries* (Barth 1969b). Barth defines ethnic identity as a feature of social organization (Barth 1994:13). He focuses on the processes of boundary maintenance rather than on any 'content' within such boundaries, as ethnicity is seen as situational, that is, always defined (and indeed maintained) in contrast to an Other. In fact, only the delineation and maintenance of ethnic boundaries enables different groups to live together. Behaviour that is unacceptable inside a group can be tolerated if attributed to an outsider. Group membership is defined by a common set of norms and behaviour, but this can vary rather widely once the ethnic boundary has been established (cf. Layton, this volume).

The self-definition of most ethnic groups seems to imply a time depth covering several generations, up to a mythical ancestor beyond the memory of living generations. This creates the expectation that ethnic groups are stable over the long term, if not eternal. As the antiquity of a group tends to define their status, this may be misleading. Some modern nations and tribal groups have formed in sudden, revolutionary acts or around a charismatic leader. Many authors assume that the formation of the early medieval *gentes* of Europe was a relatively rapid process (Garipzanov et al. 2008; Geary 2002; Pohl and Reimitz 1998; Wenskus 1961), but that the resulting groups soon acquired a deeper history in their origin myths (Graus 1989; Prehm 1996; Sommer 2009). If the rejection of a biological definition of ethnicity is taken seriously, there is no reason why ethnogenesis—that is, the formation of an ethnic group—could not be as sudden as the formation of a modern nation.[1]

Construct and Reality

Nowadays, most scholars would agree that ethnic groups are constructs. But even if the history and (homogeneous) composition of a group are entirely fictional, the moment members of this group decide to act

together for common aims (or pretend the existence of common aims) and accept a degree of personal risk (from loss of status to loss of property to the loss of their lives), this group does begin to exist and will continue to exist as long as people act as members of this specific group (see Sillar, this volume, for a related analysis of group intention). Ethnicity is maintained by daily practice (Lucy 2005:97), and thus an imagined origin can be translated into social reality very quickly.

The main difference between an ethnic group and a political party, a religious movement or a rebellious subaltern group is the claim of common biological descent. But one type of group can easily transform into the other, and often the boundaries are blurred from the beginning. To complicate matters, in many languages, including English, kinship terms are used to describe political formations and associations (cf. Vansina 1985), which does not necessarily imply a belief in actual descent. Genealogies are adapted to the actual political situation as a matter of fact. Family relations are not exclusively biological either. Adoption in both ancient Greece and Rome meant a complete change of allegiances, including tribal affiliation and ancestor worship, as does marriage in some agnatic societies.

Stuart Hall has used Derrida's concept of *différance* to describe modern detached and disembedded identities. For Derrida, the meaning of any signifier can never be fully understood, as it depends on an 'infinite semiosis', a constant comparison to other signifiers whose meaning is also not fully fixed (Descombes 1981:172–80; Mishra 2006:61–65). Both in the semiosis of language and in the definition of personal identity, the process is deferred, infinitesimal and thus endless. The very self is fractured, as it depends in its definition on the Other. This 'impossibility of identity' (Hall 1987:117) may be philosophically insightful, but it is not very useful politically—'the politics of infinite dispersal is not politics at all' (Hall 1987:117). Hall (1990) thus called for a strategic cut, a (temporary) acceptance of a more fixed (but only partial) identity that allowed political alliances and political action (see also Spivak 1987:205–11). Ethnic identity may be created by such a cut. This is perhaps a more useful way of looking at ethnic identity than to label it as imaginary. Ethnicity uses a set of overlapping traits of individual actors. These neither describe individual identity nor the group identity completely, nor are they in any way stable. They have been chosen for strategic reasons, in the pursuit of a political goal, but they are rarely invented anew. As the sociopolitical context and aims of the individuals forming the group change or fade, so do the traits, and so does the concrete location of the cut (see Bentley, this volume, for an evolutionary argument explaining why some traits may come to characterise a group).

This leads to the question of the necessity of ethnicity. Is ethnic identity a trait every human being has, and always has had? Or is it something that is only adopted in specific historical conditions and that can disappear again? Abner Cohen (1969:4) differentiates between ethnic categories, defined as shared culture, and ethnic groups that share selected cultural traits and are politically active *as a group*. Mike Rowlands (personal communication, 2007) has described ethnicity as politically mobilised culture, which also emphasises its historical specificity. This question is vital to bear in mind as we consider the discussion of ethnicity in archaeology.

Ethnicity and Archaeology

Most authors will agree on the importance of self-ascription to an ethnic group, the recognition of this ascription by outsiders and the connection of this ascription with some kind of social costs, be it the obligation to pay taxes, to take up arms in a conflict or to follow a set of common rules. For prehistorians, the core question is how far this self-ascribed ethnicity is accessible without written records (cf. Hall 1997; McInerney 1999). Barth categorically states that 'the critical focus of investigation … becomes the ethnic boundary that defines the group, not the cultural stuff that it encloses' (1969a:16). This does not sound too hopeful for archaeology, a subject that defines itself as the study of material culture surviving from the past and traces of past behaviour (Veit et al. 2003). But Barth uses a rather narrow definition of material culture that focuses on artefacts and ignores structures and patterns, i.e., the distribution of finds (Sommer 2003). Postmodern theories emphasise that artefacts and the body are intrinsic components of any form of practice (Moebius 2008:61).

According to Cohen (1969:202), ethnic groups need distinctiveness, communication, decision making, authority, ideology and discipline in order to survive. Ethnicity is also reflected in daily practice (customs and traditions [Barth 1969a; Bourdieu 1977; cf. Schütz and Luckmann 2003]). Group membership is often signalled by items of material culture, the most common of which are dress (Wobst 1977) and personal ornaments (Hodder 1982a: Ch. 5; see also Tehrani, this volume). Certain emblemata can be used to signal group membership in specific situations, a flag being the most striking example. 'Normal' items of material culture can also become loaded with special significance. Group membership can be inscribed on the body—ephemerally by paint or permanently by tribal tattoos, scarification, tooth mutilation and the brands of slaves and convicted criminals. Alternatively, these marks can also indicate religious affiliation (cf. Elm 1996), social status

and ownership. Signals also include gestures (the 'Masonic handshake') and moral values. The latter normally involves behaviour associated with specific items of material culture or architecture, for example, *purdah* and the layout of women's quarters.

Daily practice will be reflected in artefacts, but especially in features, most notably the remains of architecture, the layout of settlements and the patterning of artefacts. Refuse disposal, for example, can be a distinct ethnic marker (Hodder 1978; 1982a: Ch. 8; 1982b:61–65; South 1978a, 1978b, 1979). Traditionally, similar material culture was equated with ethnic groups (Jones 1997). This came under increasing criticism with the advent of the New Archaeology. David Clarke (1968) threw doubt on the monothetic nature of archaeological cultures, while Lewis Binford, reacting to the Boasian tradition in American archaeology, proposed a purely functional definition of culture as 'man's extrasonic means of adaptation' (Binford 1972; cf. White 1949). Material culture was seen as passively 'reflecting' the environment, the economy, the nature of tasks performed in a specific site (Binford 1973, 1978) and the intensity of communication (Plog 1978). While Ian Hodder, in criticising the functional view of culture and laying the groundwork for an interpretive perspective, argued for a holistic and integrated view of present-day and prehistoric cultures (culture as 'meaningfully constituted' [Hodder 1982a:186]), Sîan Jones (1997, 2007, 2008) rejects the concept of 'archaeological culture' altogether. She sees archaeological cultures as a modern narrative device, based in a nationalist and potentially racist framework: 'from an archaeological perspective, it cannot be assumed that there is any fixed relationship between particular material types and particular identities. Rather than neat, coherent cultural entities, the resulting pattern is more likely to be a complex web of overlapping styles of material culture relating to changeable expression of ethnicity in different social contexts' (Jones 2007:327).

A number of ethnoarchaeological studies have investigated the relationship between ethnic identity and material culture (e.g., Hodder 1982a; Larick 1986; Wiessner 1983, 1984, 1997). One important result is that different items of material culture commonly relate to different aspects of group identity. The big problem for archaeology is thus to differentiate the markers for ethnicity from those for other group affiliations. A second problem has to do with different temporal scales. Ethnic affiliation can persist even when most or all of the traits marking it have been abandoned (Cohen 1969:192; Song 2003:9). At the other extreme, 'stuff', a particular way of clothing or shaping and decorating pottery, will probably be retained after its ideological meaning has been lost—'all culture tends to survive' (Gluckman 1958:63, after Cohen 1969). Practice leads to habitus (Bourdieu 1977; Gardner 2007); customs

and patterns of movement may be retained long after they cease to be consciously used to signal difference. Different habits, once established, will maintain boundaries, even if they are not actively infused with meaning. Unfortunately, archaeologists will be hard put to differentiate between ethnic categories and ethnic groups (see above). Attention to the lifespan and the range of variation in particular items of material culture may be helpful here (cf. Sommer 2001).

I thus understand ethnicity as a part of the political sphere. Ethnicity is both an ideology and a practice, and both are linked to specific historical circumstances. Superstructure and practice, expressed as habitus and as material culture, will probably develop at different speeds and lag behind the actual political conflict, thus producing assemblages of material culture more long lived than the political mobilisation that caused them in the first place. Marx's dictum that 'the tradition of all dead generations weighs like a nightmare on the brains of the living' (Marx 1960 [1852]:xx) may well have been true for some epochs of prehistory as well. Under the interpretive paradigm, the main interest is in the meaning of material culture and its use in marking boundaries and expressing group membership or conflict. This is mainly done by mapping the distribution of artefacts and comparing the speed and extent of change in different artefact categories. Darwinian archaeology also addresses these questions in a very formalised and deliberately reductionist framework. Questions like the rate of change in material culture (Bentley and Shennan 2003; Collard and Shennan 2000) and the nature of change—inherited tradition versus borrowing from other groups (Collard and Tehrani 2005; Tehrani and Collard 2002)—are of interest to both research traditions. Unfortunately, finding a common language for interpretive and Darwinian archaeologists seems to be a major problem. The reductionist framework of Darwinian archaeology can also lead to assumptions that may seem unduly simplistic to scholars working under the interpretative paradigm (cf. McElreath et al. 2003; Colleran and Mace, this volume). An extended case study will be of use here in elucidating the main variables considered in the interpretive paradigm.

Case Study: The Hinkelstein Group

The following case study looks at a small culture group in western Germany (Figure 8.1) that split off from the late Linearbandkeramic (LBK) tradition of central Europe ca. 4900 BC, and examines how this split can be explained in terms of changing ethnicity viewed from the interpretive perspective as discussed above. As the title of this book indicates, there is no unified interpretive framework, nor should there

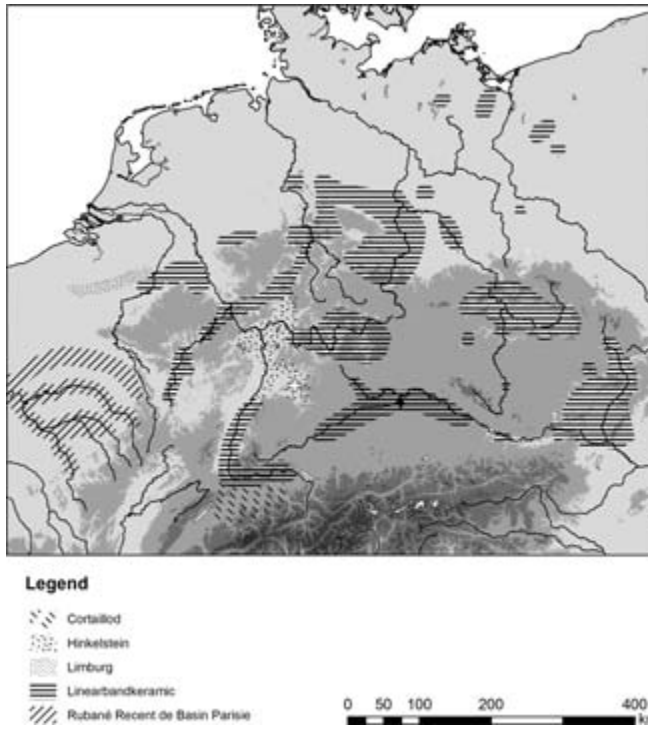

Figure 8.1 Distribution of Middle Neolithic cultures in central Europe. (Map produced by Susanne Geck.)

be one. In the case study I will apply an eclectic mixture of mainly Marxist and structural approaches to the analysis of a small and short-term 'archaeological culture' to understand how material culture (in the broadest sense) can be related to political processes. The presentation of the archaeological material is of necessity extremely abbreviated, and the complex concept of ideology probably insufficiently linked to the main theme of this chapter—ethnicity. Hopefully, some of the connections will become clear in the course of the discussion.

The Hinkelstein (HS) type of the early Middle Neolithic (Dammers 2001, 2003, 2008; Eisenhauer 2002; Spatz 1999) was produced during a rather brief period of time, for 100–150 years, maximally 200 years (Figure 8.2; Eisenhauer 1999). It was selected for a case study because—in spite of serious gaps in knowledge—the area is well studied, precluding any migrations from uninvestigated areas, and small enough for comprehensive coverage. Thus, traditional explanations for culture change—migration and culture contact—can be excluded with some confidence. It will be argued that the formation of Hinkelstein culture

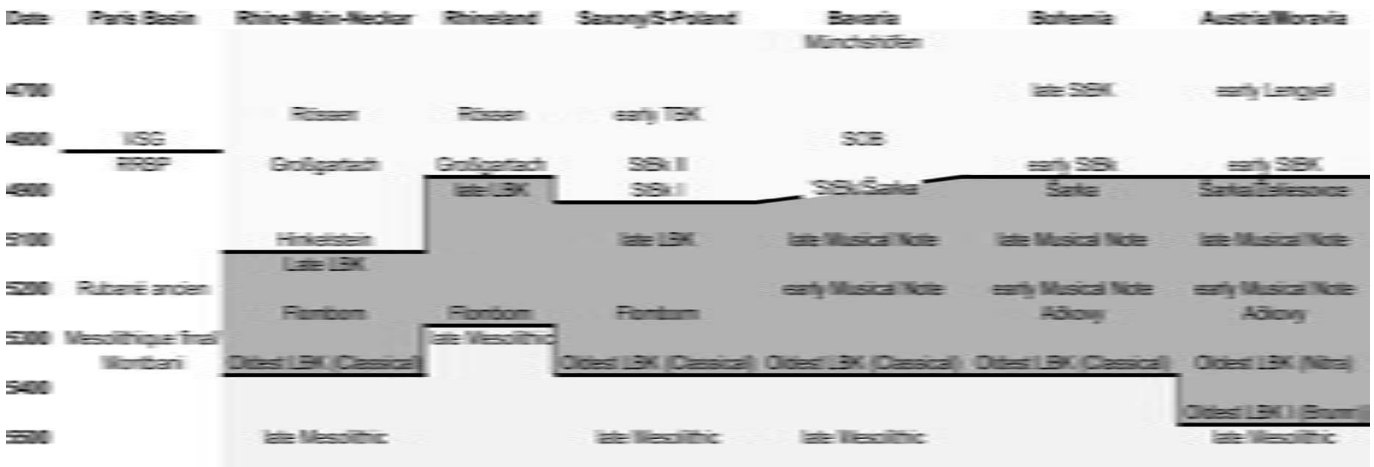

Figure 8.2 Chronology of the Early and Middle Neolithic in central Europe.

is a case of ethnogenesis caused by the adoption of a new ideology or religion, basically a political process that was proclaimed in a conscious, highly visible change of selected elements of material culture.

The distinctive Hinkelstein pottery is based on LBK prototypes, but some new shapes, such as pointed-rim bowls, pedestalled bowls and conical bowls, were introduced (Meier-Arendt 1975; Zapotocká 1972). The shape of other pots shows subtle changes (e.g., a dropping waistline). Compared with the LBK, the range of decorative motifs is severely reduced. LBK pots are typically decorated with broad bands in both curvilinear and rectangular patterns forming spirals and meander derivates, with a plethora of secondary motifs filling up the space left by the main ornaments. Hinkelstein ornaments, in contrast, consist almost exclusively of patterns derived from triangles or trapezoids with straight or bent sides, filled with parallel hachures. They can have open tops or sides (tree motifs). Combinations of triangles form winglike conjoined trapezes and running zigzag patterns or the so-called toad crosses. The decoration was executed in single incisions, multiple incisions or dragged incisions.

More fundamentally, perhaps, the grammar of the pottery decoration was changed. While the 'classic' LBK, except in the extreme west, used a threefold repetition of motifs, normally accompanied by three lugs, HS pots are consistently partitioned into four decorative zones. At the time of this change in pottery style, burial position was altered from predominantly crouched burials on the left side to a consistently stretched position flat on the back. The bodies are consistently oriented SE–NW, in contrast to a much more flexible orientation in the LBK (Bulla 1998; Häusler 1994, 2001). Grave goods are, on average, more numerous and presumably more valuable than before. The graves contain pottery, ground stone items like adzes (often with shaft holes), axes and querns, shell ornaments and pierced animal teeth. Large amounts of meat are used as grave goods, another innovation (Spatz 2002:283). In the cemetery of Trebur, many skeletons were covered with parts of a single animal, either the hindquarters or complete half animals. Women are predominantly associated with sheep, men with pigs (Spatz 1999; Spatz and von den Driesch 2001).

There was a complete change in the supply of lithic raw material. Previously, the preferred raw material for most of the northwestern LBK had come from the Rijkholt area (near Maastricht in Dutch Limburg), with some Baltic flint on the northern rim and Hainault and French flint on the western periphery (Zimmermann 1995). Now, Rijkholt flint was dropped almost completely in favour of Jurassic banded flint from the Kelheim area in Bavaria (Abensburg-Arnhofen, see Figure 8.3 for location of sites). In the LBK, spondylus shell was used for bracelets, beads and belt buckles. Today, the shell is found in the Black Sea and the

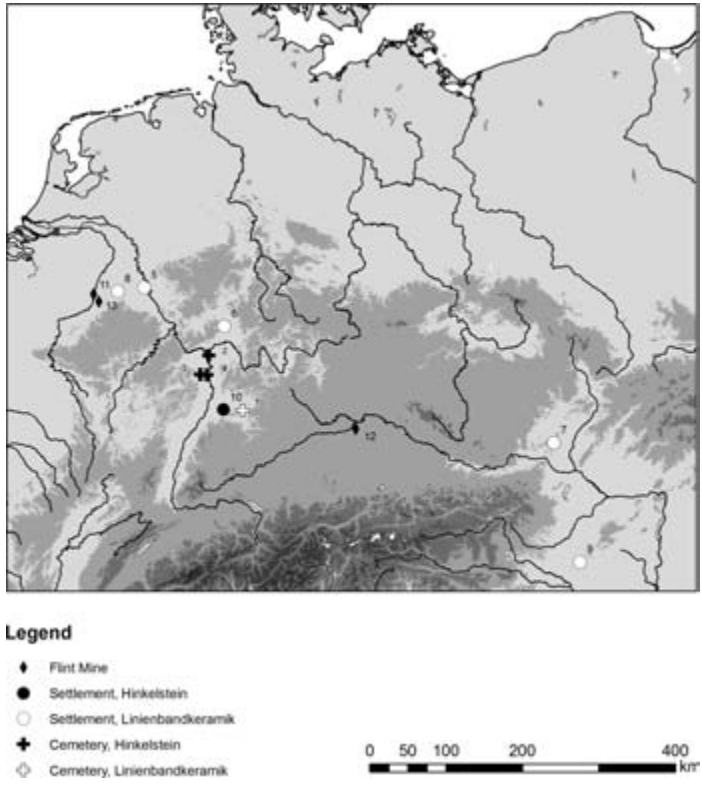

Figure 8.3 Location of key sites mentioned in the text: 1. Talheim; 2. Trebur; 3. Monsheim; 4. Worms; 5. Köln Lindenthal; 6. Rödgen; 7. Schletz; 8. Merzbach Valley; 9. Esztergályhorváti; 10. Kraichtal-Gochsheim; 11. Rullen; 12. Abensberg-Arnhofen; 13. Rijkholt. (Map produced by Susanne Geck.)

Mediterranean (Niszery 1995; Spondylus [blog] 2009). The origin of the LBK shells is disputed, but almost certainly lies outside the distribution of the LBK, indicating long-distance trade. Shell artefacts reached sites as far away as eastern France (Jeunesse 1997) and are assumed to be high-status items.

While the use of spondylus as evidenced by grave goods peaks in the earlier LBK, it continues into the later phases in diminished quantities. There are also imitations of spondylus artefacts in bone. In the Hinkelstein culture, spondylus disappears almost completely; instead, large numbers of pierced animal teeth are found in the graves, presumably as dress ornaments. The upper eyeteeth of stags occur in large numbers in some graves (230 in grave 63 in Trebur [Spatz 2002:282]) and must represent a high value or at least impressive hunting prowess. They

are mainly associated with female burials. Imitations in bone also occur. Like the sweet water shells and fossil shells of local origin common in HS graves, animal teeth occur but rarely in LBK burials and thus indicate a clear break with previous traditions.

Hinkelstein houses are poorly known, as the main distribution area is prime agricultural land and subject to severe erosion by ploughing. As the deep borrow pits accompanying the houses start to disappear in the course of the late LBK (Coudart 1998; Fritsch 1998), houses in general become less visible through time. The few possible Hinkelstein houses are similar to later Middle Neolithic houses, with rectangular, trapezoid or curved walls (Biel 1994; Strien 1986) and a greatly reduced number of interior posts. The long walls sometimes continue beyond the short walls and form a forecourt (Renner 1998: Fig. 2). The roofs of LBK longhouses are supported by numerous deeply founded interior posts (Coudart 1998; von Brandt 1988), while Middle Neolithic roofs (Großgartach and Rössen) rest on the walls (Dohrn-Ihmig 1983; Luley 1992). The interior of the latter are more or less free of roof supports and divided up into individual rooms. The new static principle would have necessitated a totally new way of building houses. The trapezoid- and ship-shaped houses had a sloping roofline, a highly visible difference from the straight roofs of LBK buildings.

As far as the settlement pattern data, mainly based on surface finds, can be relied on, there is no settlement continuity from the late LBK to Hinkelstein. Hinkelstein settlements were either built in new locations or on sites only used in the older phases of the LBK. As in the LBK, soils on loessic substrates were preferred (Sielmann 1971), but low-lying riverine locations seem to be more common in the Hinkelstein culture. The production of anthropomorphic and theriomorphic figurines, common in the LBK except in the extreme west, stops. If we follow Lüning's (2005, 2007) interpretation of these small clay statuettes as ancestors, this would indicate a fundamental change of cult.

The Reasons for Change

Hinkelstein is only found in a very limited part of the LBK settlement area. In other regions, the late LBK continued, and small amounts of Hinkelstein pottery have been found in late LBK settlements like Köln Lindenthal (Buttler and Haberey 1936) or Rödgen (Meier-Arendt 1966). In the eastern and southeastern parts of the LBK distribution, a more or less contemporaneous change to stoked pottery (Stichbandkeramik, StBK) and Southeast Bavarian Middle Neolithic (SOB) took place. These are, in many ways, comparable to the Hinkelstein development: a reduction in the motifs used in pottery decoration, a change of burial rites

and the cessation of figurine production can be observed. The changes in architecture are also comparable, with StBK houses being generally boat-shaped. This indicates that the change that led to the creation of Hinkelstein was not a completely isolated phenomenon. StBK pots in Hinkelstein cemeteries and settlements prove some contact between the groups.

A number of reasons have been advanced for the rise of Hinkelstein, which can be grouped under three broad headings (Table 8.1). In my opinion, none of these reasons is very convincing. In the following, I will examine each in turn and assess the archaeological evidence.

Social Unrest

There is some evidence of violence in the late LBK. The 34 people buried in a shallow pit at Talheim had predominantly died from head wounds inflicted with shoe-last adzes (Wahl and König 1987). A pit at the Hungarian site of Esztergályhorvaty contained 25–30 victims of violence (Makkay 2000), while the ditches of the earthwork at Schletz, Lower Austria, not only yielded about 200 human skeletons, but the uneven

Table 8.1 Possible explanations for the rise of Hinkelstein culture from the Late Linearbandkeramic tradition of central Europe approximately 4900 BC.

Reason	Source
Social unrest and conflict	
Increased social unrest and warfare	Meier-Arendt 1975:156; Spatz 1991:26
Breakdown of trade networks	Zimmermann 1995
Breakdown of communication networks because of a growing population and the resulting economic independence of smaller areas	Lüning 1982
Environmental change	
Degradation of the environment caused by overexploitation	Lindig 2002:199
Climate deterioration	Meier-Arendt 1975:156; cf. Schmidt, Gruhle and Rück 2004
Outside influences	
Influence of StbK or immigration of potters from this area	Zápotocká 1970:19; 1972; Kaufmann 1976:103
Influence of the French late LBK (RRBP)	Jeunesse 1998/1999, 2008; Strien 1993
Influence of a Mesolithic substrate	Buttler 1938:27

distribution of the sexes provides strong evidence for the abduction of young women (Teschler-Nicola et al. 1996). Still, three sites are not enough to demonstrate widespread violence (*pace* Wild et al. 2004), especially as burnt houses are notably absent and no change in the settlement pattern (towards more aggregated or fortified sites) can be observed.

In the late LBK there are stronger regional differences compared to the uniformity of the earliest phase (Cladders 2001). However, while differentiating between a new culture group and subgroups of a culture is admittedly subjective in the extreme, and the terms themselves are not at all well defined (Lüning 1972; Müller-Karpe 1973; 1974), most scholars would agree that the differences between what has been labelled the early Middle Neolithic (HS, StBK, SOB) and the late LBK are greater than those between late LBK groups (with the possible exception of the RRBP).

The idea that differences in material culture are the result of decreasing communication has been labelled naïve (Barth 1969a:9). In the case of the late LBK, it would certainly need refining, as there is evidence for continuing long-distance trade in flint and raw material for axes. The deposits in the earthwork at Herxheim (Zeeb-Lanz 2009) are evidence for long-distance movement of flint and pottery in the late LBK. The complete reorientation of the flint supply at the beginning of HS can be the effect, as well as the cause, of change. In addition, the grey Rijkholt flint loses popularity in favour of the yellowish Rullen flint from the same geological source in the contemporaneous late LBK of the Rhineland (Hohmeyer 1997). Isotope analysis may be able to elucidate the extent of personal mobility, which should be in some way related to the intensity of communication, but at the moment the results are confusing (cf. Nehlich et al. 2009). The analysis of the pottery from the Merzbach valley (Frirdich 1994; Bentley and Shennan 2003) would indicate that newly founded settlements were less conservative stylistically than older, bigger ones and that the range of variability in pottery increased towards the end of the LBK. This would argue for social inhibitors of stylistic change that gradually weakened.

In any case, HS did not seamlessly develop by the increasing stylistic differentiation of a specific late LBK group, as its distribution covers several different late LBK local groups (see Figure 8.3), and the stylistic elements used show a clear break with previous traditions and a reduction of both the number of techniques and motifs used.

Environmental Change

There is no clear evidence for a worsening climate (Strien and Gronenborn 2005) nor for environmental degradation (Bogaard 2004:167–68; Rösch 1998).

Culture Contact

Buttler's Mesolithic substrate theory has been quite popular. Trapezoid arrowheads, as well as an increased importance of the hunt as evidenced by the teeth of wild animals and arrowshaft straighteners in graves, have been cited as supporting arguments. Unfortunately, there is no evidence of any Mesolithic population in the area after ca. 6000 BC (Gronenborn 1992; Terberger 2003:70–71; cf. Kind 1997). Kraichtal-Gochsheim, the sole HS settlement where bones have been analysed, yielded only 6.8% wild animals (Boessneck 1982), well within the LBK limits (Arbogast et al. 2001). Isotope analysis of the bones (Dürrwächter et al. 2003) may provide more information regarding the geographic origin of the Kraichtal-Gochsheim population. Contact with neighbouring groups to the east (Kaufmann 1976; Meier-Arendt 1975; Zapotocká 1970, 1972) and west is attested (Jeunesse 1997, 1998/1999, 2008), but does not in itself explain the change in material culture.

Hinkelstein as a Religious Group?

In 1979, Eduard Sangmeister mused that the producers of HS pottery might well have been 'a religious sect, which developed a new type of pottery for a new burial rite, incorporating various traditions and new developments (StBK)' (Sangmeister 1979:36). His view did not find much support at the time. More recently, Spatz (2002, 2003) took up Sangmeister's idea. He interpreted the toad crosses and tree motifs on the pottery as stylised worshippers and signs of a new cult. A religious sect is commonly defined as a group that splits from a larger religious community to emphasise different aspects of their common belief. It presupposes some kind of organised and codified religion. Thus, a new religious movement formed in a similar cultural background might be a better description.

Luckmann (1967) defined religion as a specific social universe, a socially objectified system of meaning that relates to the everyday world and a world that is seen as transcending this everyday world: 'The objectification of the symbolic universe as a … system of meanings requires that the experiences which are used in the construction of such a system of meaning carry meaning themselves. The meaningfulness of subjective experiences is in itself the result of social processes' (Luckmann 1967:81, my translation). In non-industrial societies, there is normally no distinction between the private sphere and the religious sphere. Ideologies—that is, systems of interpretation constructed and transmitted socially in constant interaction—provide a reservoir of prefabricated solutions for problems and systems of interpretation and form the normative frame of individual behaviour. Types, schemes of interpretation and patterns of behaviour that are part of an ideology form a hierarchy of meanings

(ibid.:94; cf. Berger and Luckmann 1967). Thus, 'daily life habits' (Luckmann 1967:95)—and, the archaeologist would add, their material correlates—are an integral part of a larger ideational system.

Decorating pottery with triangles need not be a religious act as such, but it is part of a system of relevance that also includes the transcendental sphere. This differentiation does not only take place on the level of what Wiessner (1983) termed emblemic style—pottery decoration and shapes—but also on the more fundamental level of Sackett's isochrestic style (1982, 1990), anchored in motor habits, as is demonstrated by the change in the patterns of symmetry in the pottery decoration and the changed structure of Middle Neolithic houses. Seen in this way, the disassociation from LBK patterns and the decorative system can indeed be interpreted as a conscious expression of ideological differences. A schism or a sect would want to express its opposition to a traditional and extremely stable system of meaning, and also act out this opposition in daily behaviour. The signals are clearly addressed to the LBK, as they take up the common cultural vocabulary and change it in a very visible and presumably provocative way (cf. Hodder 1982a: Ch. 4; Hodder 1991).

Different ranges of visibility (Wobst 1977) are involved in this process. Burial patterns, very visible to the archaeologist, are not necessarily so to the outsider. Even if non-group members were admitted to the rituals, deaths do not occur often enough to make burial customs a very effective signal. But burial, like any other *rite de passage*, has a strong impact on social cohesion (Gramsch 2004, 2010). Social ties have been torn by the death of a member, and the identity and cohesion of the group has to be recreated by joint action, the conspicuous enactment of shared customs and the display of symbolically loaded material culture (Dalton 1996; Hayden 2009).

Pottery was probably used mainly inside the Hinkelstein houses, though there might have been some movement of containers or even potters into the LBK area. It would thus have been of low visibility outside the specific residence or consumption group, but would form a strong daily reminder of group identity as long as it retained its message of difference and—probably—purity. The houses, in contrast, were highly visible. Even before entering the village, a stranger could clearly identify the allegiance of its inhabitants. The interior, lacking the forestlike profusion of posts typical of a LBK house but rather with fixed and distinctive interior divisions, signalled a different way of living together.

Even if a religious schism is interpreted as an indication of social unrest and fission, it does not explain the reasons for these phenomena. If external factors like climate change, soil exhaustion and incursions of strangers can be excluded, the reason for social change must be sought in internal contradictions. While the term religious schism can be used

to describe the change in ideology, the changes in material culture merit the designation of Hinkelstein as an ethnic group, striving to express the strongest possible distance to the LBK.

The Social Structure of LBK and HS

So what kind of social contradictions can be expected in the LBK? Lüning characterised it as a tribal society, the authority resting with tribal elders, ideologically underpinned with ancestor worship (Lüning 2007), without offering any supporting arguments (see Gronenborn 2009 for the context of the German discussion). In a processual framework, Piet van de Velde (1986, 1990, 1993) has interpreted the LBK as a Big Man society, Annick Coudart (1991) as a segmentary society. Segmentary societies are organised along descent lines, and the fissuring of a 'younger' lineage is the normal way of resolving conflicts, a thesis that finds support in the work of Frirdich (1994) on the settlement patterns in the Merzbach valley. Land is normally held in common, and the means of production tend to be simple, human labour being the main or only source of energy. In recent societies, the division of labour is by gender and age; family and lineage elders often hold a position of authority (Fried 1975; Sahlins 1968). The means of production are held in common, are the private property of each cultivator, or are owned by the elders (Terray 1972:127).

The internal contradictions and antagonisms in segmentary societies have been discussed by a number of authors—for example, by Meillassoux (1964, 1976), Godelier (1973) and Terray (1972)—in a Marxist/structural Marxist framework, mainly based on case studies from west Africa. They have identified potential planes of cleavage between the genders and between different age groups, especially between elders and younger unmarried men. In the following, I will try to apply some of their concepts to the late LBK and Hinkelstein. Control of the means of production in Early Neolithic Europe will have been difficult. Potential agricultural land was abundant, agricultural tools so simple that they could be produced by everyone (cf. Weiner 1992). While ground stone and flint tools were often made of raw materials obtained in long-distance exchange, local substitutes were available in most areas, even if they were not popular (cf. Sommer 2006).

In many recent segmentary societies, the power of the elders rests on their control not over the means of production, but over the means of reproduction. In societies with several spheres of exchange (Dalton 1969; Polanyi et al. 1957), it is mainly the elders who control the supply of goods used as bridewealth (e.g., Bohannan 1955; Goody 1973; Meillassoux 1964). They use it to arrange multiple marriages of their

own, but also to supply younger men of their lineage with bridewealth. Marriage often puts the younger men in long-term debt to the elders. They have to work their fields or herd their cattle to pay them off, thus increasing the wealth and power of the former.

Payment for persons, like bridewealth and *wergild*, often consists of items only exchanged in a restricted sphere as part of social rather than purely economic relations (Dalton 1966). They are only exchanged for other goods from the same sphere, and these exchanges take place at a very limited number of occasions (Dalton 1969:78). Such items are labelled prestige goods and the corresponding economy a prestige-goods economy. The identification of goods from the prestige sphere of circulation in prehistoric contexts is problematic. By their very nature, they should be in constant circulation (Wagner 1978:69); prestige and political power is not achieved by hoarding items, but by giving them away. Ethnographic information about burial rites is sparse as a rule, but the use of prestige items as grave goods seems to be quite rare. Archaeologically, bones and containers may indicate the consumption of the goods in question, not their burial with the deceased (cf. Dietler 1990; Metzler et al. 1991). In contrast, most of the potential prestige goods of the central European Early Neolithic come from graves. It can be argued that burial, like hoarding (Halstead and O'Shea 1982), is a way of preventing an inflation of prestige goods, but there are obvious differences to the ethnographic studies in sub-Saharan Africa and Oceania used as analogies. The term 'prestige good' is also used for artefacts that demonstrate the status of an individual (Plourde 2008). The burial of such individual insignia with their owner would make sense; unfortunately, the co-mingling of both groups of items seems to have prevented any in-depth discussion of the problem as such.

In the European Early Neolithic, non-utilitarian items traded over long distances, such as spondylus shells (Müller 1995; Müller et al. 1996; see Trubitt 2003; Dalton 1996 for the use of shells as prestige goods), and exotic raw materials with locally available substitutes, such as amphibolite and pthanite axes, are likely candidates for prestige-sphere goods. For the LBK, the grave goods in cemeteries would indicate a relative equality of the genders and a generally low emphasis on gender differences (cf. the data in Bulla 1998). Nor does age seem to have caused fundamental differences in the number and nature of grave goods, although older individuals tend to have slightly more 'valuable' grave gifts (Bulla 1998). It has to be kept in mind, though, that the percentage of the population to be inhumed in formal cemeteries is unknown (van de Velde 1993; see also Haidle and Orschiedt 2001; Krause 2000; Trautmann and Wahl 2005; Veit 1996).

The archaeological record contains no obvious evidence for bridewealth, polygyny or a political domination of elder males for the LBK, but such evidence is notoriously difficult to come by archaeologically. The data from Talheim point to a patrilocal society (Eisenhauer 2003; Price et al. 2006), but, as ethnographic evidence shows, female captives can be integrated into a matrilinear society as well (cf. the example of the Yao in Barth 1969b). If bridewealth is taken as payment for future children, it does not have to be restricted to patrilocal societies either, though brideservice seems to be more common here (Lancaster 1976). The sudden changeover to a new system of prestige goods made from locally available materials like sweet water and fossil shells and the teeth of wild animals, as well as the meat of domestic animals, points to a shift of power corresponding to the beginning of HS.

Drawing on the discussion of internal contradictions and antagonisms in segmentary societies outlined above, this change in potential prestige items can be interpreted as a rebellion against those who previously controlled the means of reproduction. The opposition is expressed in all areas of daily life, but draws on the previously used material culture. The selection of a new range of prestige goods more readily available locally was accompanied by the formation of a new trading network for flint. This rebellion seems to have originated in a small core area in Rheinhessen (Spatz 2002:283; Zápotocká 1972), but it spread quite fast (Eisenhauer 2002; Spatz 1999). The choice of deer teeth emphasises prowess in hunting, a prerogative of young persons. The burial of these items, either with the hunter/huntress or their spouse, prevented intergenerational accumulation. As the diet of the LBK (Döhle 1994) contains an abnormally low percentage of wild animals compared with other Neolithic cultures of central Europe (Arbogast 2005; Arbogast et al. 2001), this emphasis on hunting may have been another conscious decision used to emphasise the contrast to the previous regime, which may have imposed sanctions upon any utilisation of the wild.

Conclusion

The Hinkelstein culture can thus be interpreted as the formation of a new ethnic group, arising in conscious opposition to the LBK but recruited exclusively from populations that once belonged to the ideological sphere of the LBK. Or at least, that is one of the stories that *can* be told (Tilley 1993) about the changes at the end of the early Neolithic (cf. Glatz et al., this volume).

Certain assumptions, for example, the inferred cultural continuities and discontinuities within the LBK and between LBK and HS, should be amenable to evolutionary analyses, in particular cladistics (see Tehrani,

this volume); other assumptions, such as the reasons for the change, remain open to speculation and depend on further research into the social organisation of the Early and Middle Neolithic.

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Note

1. The use of the term 'ethnogenesis' to describe material culture change resulting from intergroup borrowing, as opposed to phylogenesis or branching (Collard and Shennan 2000; Collard et al. 2006; Moore 1994, 2001; Tehrani and Collard 2002), is misleading and should be avoided in favour of the more unequivocal terms of 'blending' or 'cultural diffusion'.

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CHAPTER NINE

Cultural Selection, Drift and Ceramic Diversity at Boğazköy-Hattusa

Claudia Glatz, Anne Kandler and James Steele

Introduction

The relationships between political organisation, its evolution and specialist craft production can be highly variable, and the causal mechanisms are still debated among interpretive archaeologists, processualists and evolutionary archaeologists (e.g., Brumfiel and Earle 1987; Clark and Parry 1990; Costin 1991; DeMarrais et al. 1996; Henrich and Boyd 2008; Patterson 2005; Rice 1981; Shepard 1956, 1963). In early state societies, luxuries intended for the social or political arena (e.g., Sinopoli 1988), as well as more mundane commodities such as plain utilitarian pottery, were subject to various mechanisms of centralised political control. Third-millennium BC cuneiform evidence, for instance, documents a meticulously overseen ceramic industry in southern Mesopotamia of the Ur III period (Potts 1997). Inspired by these rare, as well as historically and culturally contingent, glimpses of a centralised production organisation, archaeological evidence is also often interpreted to this effect (e.g., Sillar, this volume; Sterling 2001; see Blackman et al. 1993 for an exception). Middle Assyrian imperial control over production has been inferred from the presence, in state-related find contexts, of a restricted repertoire of plain utilitarian pottery, which is standardised in appearance and assumed to be the result of mass production (Pfälzner 1995).

Political interference with production, in theory, may manifest itself in a variety of ways depending on underlying elite agendas. These may range from the gaining of sociopolitical power through conspicuous consumption and politically inspired strategies for imposing cultural conformity, to the more economically motivated need to secure the supply of government institutions with utilitarian products. As Gardner points out in this volume,

examination of sociopolitical power plays a prominent role in agency studies of interpretive archaeology (see also Sillar, this volume). The actual process of production may be controlled through the ownership of production locations or specialists (or both) in a classical scenario of attached specialisation (Costin 1991); through the exclusive ownership of, or access to, raw materials and their supply (in the case of pottery this is probably less relevant); and through consumer demand, in cases where state institutions are the prime consumers of the output of free or independent workshops. Different strategies of control—and from an interpretive archaeology perspective, resistance (Gardner, this volume)—may be manifested in different archaeological attributes. For ceramic vessels such as the bowls that are the focus of this chapter, we expect different strategies of control to manifest themselves in the following archaeological patterns: The gaining of sociopolitical capital through conspicuous consumption may be manifested in a visually distinctive subset of vessels whose attributes attest to high skill and labour investment. Function and context of use of such prestige items should be associated with arenas of competitive social display. A politically motivated strategy of cultural homogenisation—which may originate in and encapsulate economic or administrative considerations—may be manifested in standardised formal (and possibly also decorative) repertoires such as the imperial service of the Inka empire (e.g., Costin and Hagstrum 1995:627). State sponsorship of pottery production with strong clustering of metric attributes (e.g., rim diameter and vessel volume) may point towards standardised measurement units for the storage, distribution or production of rations. This has been suggested, for instance, for the bevelled rim bowls of 4th millennium BC Mesopotamia (Chazan and Lehner 1990; Millard 1988; Nissen 1988). In contrast, economically motivated state sponsorship of pottery production, in the absence of such metric or other requirements of cultural homogeneity, should display levels of standardisation similar to those expected from independent specialist producers engaged in the production of a repertoire in which the limits of variation are determined by what are culturally acceptable or recognisable formal and functional types and size categories.

In this chapter, we examine the ceramic tradition of the penultimate and final Late Bronze Age (LBA) occupation phases in the Upper City of the Hittite capital, Boğazköy-Hattusa (Figure 9.1). The Late Bronze Age (1600–1200/1180 BC) ceramic tradition of north-central Anatolia (NCA) tends to be described as plain, standardised and mass produced (Genz 2005; Gunter 2006:357–61; Henrickson 1994, 2002; Jean 2006:328; Schoop 2006:216), while its developmental sequence shows reductions in vessel form diversity against a background of strong continuity in major types (Fischer 1963; Neve 1984; Schoop 2003). NCA pottery in the LBA, like that of the majority of contemporary societies

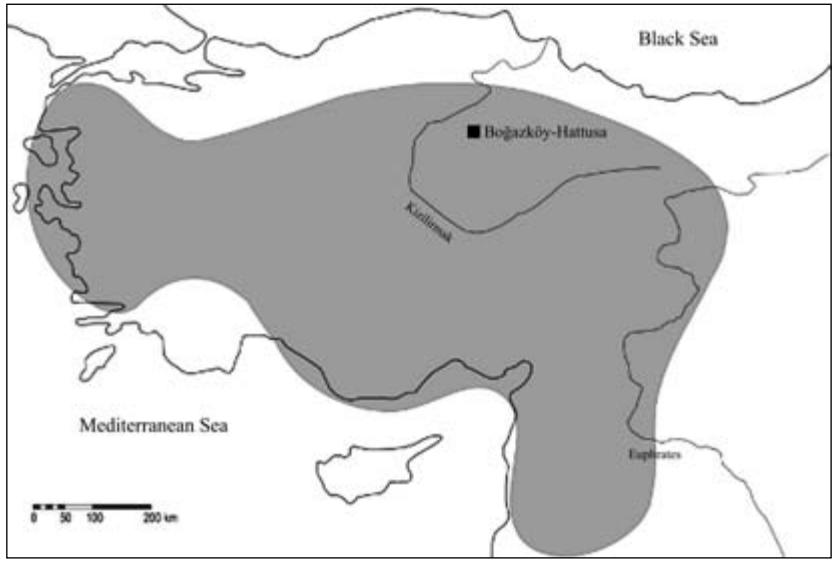

Figure 9.1 Map of site location and approximate extent of the Hittite empire.

in the Near East and Egypt, is predominantly wheel made, monochrome with a buffish paste, and with very little investment in surface treatment. Decorative elements are almost entirely absent. The LBA ceramic tradition is firmly rooted in the preceding Middle Bronze Age, with the majority of vessel shapes being produced for around 600 years (ca. 1800 to 1200 BC). Few shapes appear or fall entirely out of use over the course of its history, and the stability and apparent widespread spatial distribution of the repertoire has led some scholars to advocate a tight relationship between NCA pottery, its apparent spread to peripheral regions and the Hittite state apparatus (Müller-Karpe 2002:257), either through centrally overseen production and empire-wide production standards (Gates 2001, 2006) or, more generally, through the presence of administrative and military personnel and their ceramic requirements (Müller 2003, 2005; Postgate 2005, 2007). Müller-Karpe, for instance, states that 'the creation of an *Einheitsstaat* with a strong state dependency of large parts of the economy is not only evident in the homogeneity of the pottery, but is likely to have also been a catalyst for the standardization of the repertoire and a tendency of formal simplification' (2002:257, our translation). To date, however, neither the degree of standardisation of NCA pottery, nor the modes of its production, nor the question of why imperial ventures should be interested in controlling the production of utilitarian pottery have been explored empirically in any satisfying way.

We present two alternative models of the evolution of NCA pottery repertoires, with the differences reflecting alternative possible mechanisms of elite or state control of pottery production. If LBA pottery production in the Upper City of Boğazköy-Hattusa, which at least in the final phase was clearly physically associated with central institutions, was subject to a politically motivated strategy of cultural homogenisation, then we would expect to find low rates of innovation by individual potters and a stable repertoire composition. We would also expect to find more conformism in the choices exercised by consumers of pottery, leading to a superabundance of sherds from the most common forms. Alternatively, if pottery production was subject to economically motivated state sponsorship of production in the absence of strictly enforced standards of production—driven by the purely economic considerations of securing the supply of daily commodities—then the assemblage should display relative fluidity in repertoire composition over time, except in those characteristics that are functionally relevant to the economic agenda.

Interpretive archaeologists would likely offer a number of possible theoretical frameworks to understand variation in the pottery attributes analysed here. We find, however, that when using evolutionary theory, the evaluation of particular possible answers can draw upon a wide range of empirically tested models. To determine which scenario fits the evidence from the final two phases of LBA pottery production from the Upper City, we need to consider a null model of the expected pattern of production and consumption of ceramic variants subject only to 'drift', in the absence of either functionally biased selection or cultural conformism. There have been numerous recent applications of neutral models to archaeological ceramic assemblages (e.g., Eerkens and Lipo 2005; Kohler et al. 2004; Lipo 2001; Neiman 1995; Shennan and Wilkinson 2001; see Bentley, Cochrane, both this volume, for reviews). The neutral model of cultural diversity in the absence of selection is one in which new traits appear by a process akin to random mutation, and existing traits are randomly chosen to be copied so that their frequencies change only because of chance sampling effects. Mathematically this is modelled as a multiplicative stochastic process with introduction of new variants at random intervals (such processes are usefully reviewed by Mitzenmacher 2003). A particularly well-studied case of such a process is the so-called 'neutral theory' in genetics, which explains the evolution of adaptively neutral allele frequencies through drift (Kimura 1983). The theory was developed to explain the enormous variability in genetic systems, which seemed far greater than would be expected if all genes had adaptive significance and were subject to natural selection. Importantly, the neutral model of cultural diversity does not maintain that individuals make choices randomly, but that the population-level effects of individual

choices may take on random or stochastic distributions (Bentley, this volume).

In archaeology, a stochastic model of change in trait frequencies is usually associated with choices among traits that have no associated variation in payoff (Dunnell 1978; cf. Brantingham 2007). Neiman's (1995) work on cultural transmission of selectively neutral stylistic elements in Woodland ceramics introduced neutral theory from genetics and explored evidence for changes in the scale of interaction and in the number of potters whose vessels were being circulated in a particular study region. More recent archaeological applications of cultural transmission theory have concentrated on the influence of social norms on the rates of transmission of particular cultural variants (Eerkens and Lipo 2005; Kohler et al. 2004; Shennan and Wilkinson 2001).

Figure 9.2 shows typical output from a simulation of evolving ceramic assemblage diversity under drift. In this case, we start with a uniform distribution of 10 variants, each making up 10% of the assemblage, and then propagate them over 100 copying cycles using the random copying rule, with new variants introduced with a likelihood $\mu = 0.01$. Under drift, most variants dwindle towards extinction while a few others increase in abundance. We will not present a more detailed analysis of the neutral model here, since this has already been done for archaeologists at greater length by Neiman (1995) and by Shennan and Wilkinson (2001). Archaeological use of this model has focused on two issues: the inference of changes over time in the size of the population reproducing the variants and innovation rate; and the inference of non-neutrality (i.e., the existence of other processes other than drift influencing variant frequencies) at a given location and time step. Where there is no departure from neutrality, variation in the size of the population reproducing the variants and innovation rate, or both, has been inferred from the frequency distribution of variants. Neiman (1995) found a trend across time for an increase in diversity in inventories of Woodland ceramic stylistic attributes, which he interpreted as reflecting an increase in the scale of the social network and in the number of pottery producers. Departure from neutrality in the composition of archaeological ceramic assemblages has been asserted by Shennan and Wilkinson (2001; see also Kohler et al. 2004) using the magnitude and sign of the difference between a diversity index empirically measured from a given assemblage and an index estimated under the neutrality assumption. In effect, such studies are estimating excess evenness or concentration in a frequency distribution, which might be observed as excess convexity or concavity in a log-log plot (see also Mesoudi and Lycett 2009). Where empirical diversity in pottery stylistic traits exceeds the level expected under drift, an anticonformist bias has been inferred, that is, novel variants are being

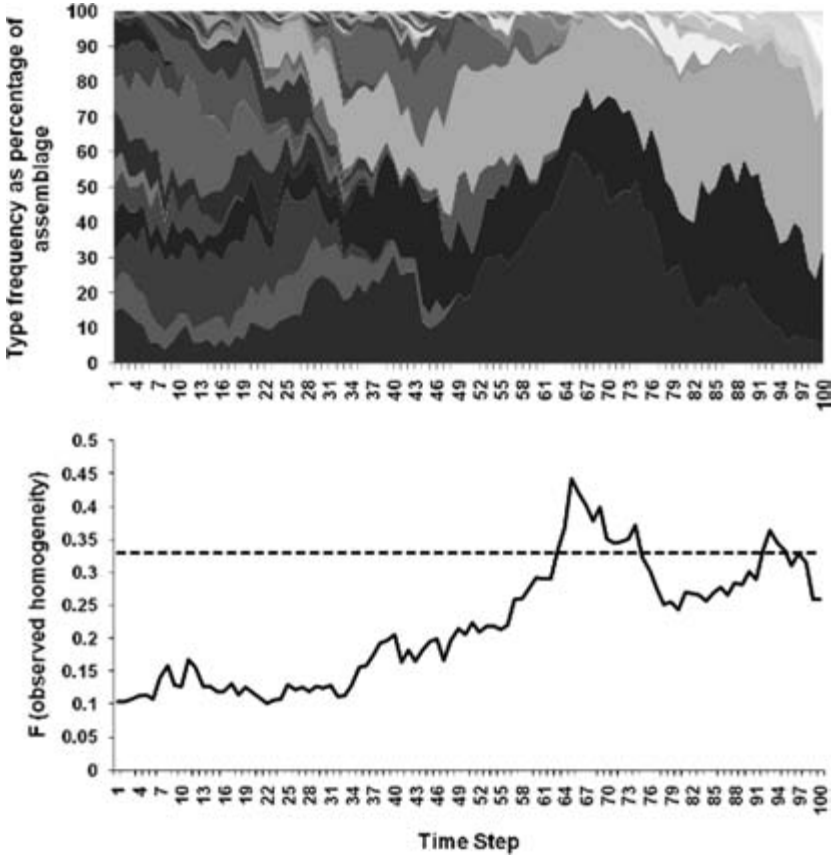

Figure 9.2 A sample simulation of the effects of drift on assemblage diversity. The initial condition is 10 types each making up 10% of the assemblage, which then evolves under random copying with a constant population size of N*e* = 100 independent copying events in each cycle, and a mutation rate μ = 0.01. The upper graph shows the changing proportions of the assemblage made up by each type (types are vertically stacked in order of appearance over the time course of the simulation). The theoretically expected equilibrium homogeneity level *F* is 0.33, shown as the dotted line in the lower graph, which also shows the empirical homogeneity level evolving towards that mutation-drift equilibrium over the time course of the simulation. (Redrafted from Steele et al. 2010: Fig. 1.)

transmitted more than expected. Where the reverse is the case, a conformist bias has been inferred; in other words, novelty is being erased more quickly than expected.

Here we examine a database of ceramic bowl types from two successive phases of occupation of the Upper City at Boğazköy-Hattusa using

the neutral model and our expectations about state control of ceramic production. Politically motivated strategies of cultural homogenisation will be evidenced through low rates of innovation or conformist bias. If pottery production was subject to economically motivated state sponsorship of production to secure the supply of daily commodities, then the assemblage should display higher rates of innovation. We ask the following questions:

1. In a given phase, do the assemblage characteristics indicate a role for factors other than random drift in pottery choices?
2. When comparing assemblages from successive phases, do changes in the frequencies of individual variants reflect functional non-equivalence and imply that variants were being selectively reproduced?
3. When comparing two phases, do the assemblage characteristics indicate any change in either the size of the population of potters or their rate of innovation?

Analytical Background and Data

Boğazköy-Hattusa, capital of the Hittite empire and the largest Bronze Age settlement in Turkey, lies around 150 km east of the modern Turkish capital of Ankara on the central Anatolian plateau (see Figure 9.1). The approximately 180 ha site occupies a rocky terrain and slopes around 300 m from south to north (Figure 9.3). Occupation in the lower part (Unterstadt) of the Hittite capital stretches back to the 3rd millennium BC. During the Late Bronze Age, a large temple, storage facilities and living quarters dominated the Lower City. The Upper City (Oberstadt) is an approximately 100 ha southerly addition to the older part of the town and the palatial area on Büyükkale. The area was first enclosed by a 3.4 km long fortification wall in the second part of the LBA and includes 30 monumental structures or temples, numerous smaller buildings, a pottery production area in the final occupation phase, water reservoirs and a monumental glacis for defensive as well as representational purposes.

The majority of the excavated ceramic material from the temple quarter in the central Upper City at Boğazköy-Hattusa derives from the final two LBA occupation phases (Oberstadt 3 and Oberstadt 2, referred to here as O. St. 3 and O. St. 2). These are distinguished architecturally by a phase of abandonment at the end of O. St. 3 and a partial stratigraphic overlap of later O. St. 2 structures (Table 9.1). Houses and monumental buildings belonging to O. St. 3 appear to have been abandoned and left to decay; some were levelled and new functions assigned to the freed-up spaces. House 4, for instance, appears to have been abandoned and the ground levelled to make way for two phases of pottery

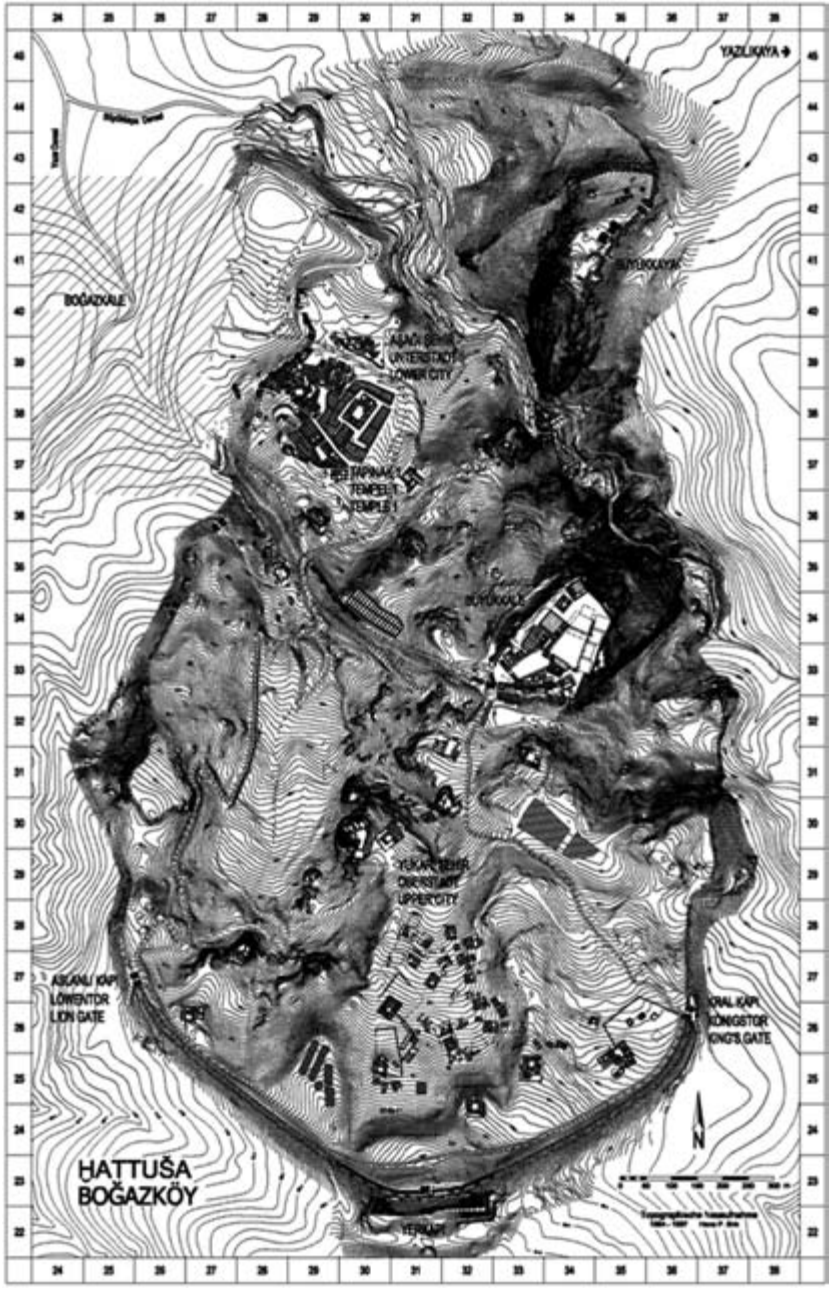

Figure 9.3 Plan of Boğazköy-Hattusa. (Image courtesy of J. Seeher.)

Table 9.1 Contexts associated with the Boğazköy-Hattusa Upper City phases from two research projects.

	Boğazköy-Hattusa Upper City Phases				
Reference	**O. St. 4**	**O. St. 3**		**O. St. 2**	
Müller-Karpe (1988)	House 2	Houses 1, 2 (older basement fill), 3, 4, 5, 6, 7		Kilns 1, 2, 3, 4, 5	
	Temples II, III, IV	Temples VI, VII	Abandonment	Temple VI (later) – destroyed by fire	Destruction by fire
Parzinger & Sanz (1992)		Houses 16, 20, 21, 22		Houses 24–30, 31–37, above Temple 24	
		Temples 9, 10, 18, 19, 21, fill Temple 15		Temples 7, 8, 12, 20, 26	
				Kilns 5, 6, 8, 9, 27	

kilns (Müller-Karpe 1988:5). The Upper City material in O. St. 2 is the only unequivocal case of pottery production that was at least physically attached to monumental structures. At the end of the Late Bronze Age the temples and other structures were abandoned and some were destroyed by fire (Seeher 2001).

The entire Upper City complex was initially dated by archaeologists to the last decades of the city's LBA existence in the late thirteenth century BC (Müller-Karpe 1988:161; Parzinger and Sanz 1992:72–73). The original, textually driven chronology of Neve (1992, 1999) would allow as little as 50 years for the two main occupation phases. More recent archaeological work and a reassessment of the chronological span of tablet and glyptic archives, however, suggest a much longer occupation of the Upper City from at least the fifteenth century BC onwards (Mielke et al. 2006).

The Late Bronze Age ceramic tradition is firmly rooted in the preceding Middle Bronze Age, with the majority of vessel shapes being produced for around 600 years (ca. 1800 to 1200 BC). Few shapes appear or fall entirely out of use over the course of its history, and consequently

trends within that period have been recognisable only from frequency seriation of stratified assemblages (Schoop 2003, 2006). At the time of writing, this frequency seriation has been defined quantitatively only for the second part of the LBA based on analysis of the two latest LBA occupation phases—O. St. 3 and O. St. 2, identified by independent archaeological criteria—of the Upper City at Boğazköy-Hattusa (Müller-Karpe 1988; Parzinger and Sanz 1992; also Schoop 2003, 2006). In this paper, we concentrate on the published ceramic material from the Upper City for several reasons. These include a number of practical considerations such as careful excavation and full presentation of data, as well as large sample sizes, which are required for the kind of mathematical analyses conducted here. Beyond these practical considerations, Boğazköy-Hattusa's role as the imperial capital makes it at least very likely that imperial policies originated and were also implemented there. In order to establish a broader perspective on the relationship between ceramic production and the Hittite state, however, future research is necessary to broaden the chronological as well as social-geographical breadth of this study.

With regard to the Upper City pottery assemblages, Müller-Karpe (1988:161–62) and Parzinger and Sanz (1992:47) have pointed out a number of changes that we will analyse in more detail below. These include changes in the predominant formal characteristics between O. St. 3 and O. St. 2 such as increasingly thickened rims. They also noted a replacement of popular vessel forms by shapes that had previously been less common. For instance, bowls with inverted rims appear to have been replaced by bowls with everted rims, while the role of plates with stepped rim profiles seems to have been taken over by shallow bowls or platters with simple rounded rims. Müller-Karpe and Parzinger and Sanz also observed a general shift from a more even distribution of plain coarse and finer wares in the first of the two occupation phases, also with the occurrence of red-brown and white slipped vessels, to a much more uniform utilisation of mostly coarse plain wares across the entire Upper City ceramic assemblage. O. St. 2 is conventionally seen as the final phase of decline in the NCA ceramic tradition at the very end of the LBA, both in terms of formal diversity and of the use of finer wares and attention to surface treatment.

Müller-Karpe (1988) and Parzinger and Sanz (1992) recorded and examined the pottery from two adjacent excavation areas, which were excavated from 1978 to 1980 and 1982 to 1987. The assessment of type frequencies for chronological purposes was the aim of both studies and their results, in terms of the relative frequency distributions of functional vessel categories and their formal variants, are largely comparable for the two phases. The use of two different typological schemes,

however, hampers a more rigorous comparison as it complicates the amalgamation of the two data sets. In the case of Müller-Karpe (1988), the ceramic material from phase O. St. 3 derives mostly from houses and temples, while the later phase is represented almost exclusively by pottery from five kiln structures (see Table 9.1). The samples from the two phases, therefore, present two different contexts, one of pottery consumption, the other of production. This is not the case for the material presented by Parzinger and Sanz (1992), where pottery from the later phase was found in and around kilns as well as in consumption contexts (see Table 9.1). For the purpose of this paper, therefore, we use the Parzinger and Sanz (1992) data set, since it provides greater typological resolution as well as greater consistency in the contexts represented in the two phases.

In the absence of decorative motifs, the null hypothesis of selectively neutral transmission will be tested using frequencies of discrete artefact types. To avoid variation in vessel frequencies that is clearly functionally contingent, we will examine formal variation in rim shape within a single functional category (bowls). Like most preexisting typologies in the archaeological literature, the Parzinger and Sanz NCA pottery typology is based on a hierarchical taxonomy that is extensionally derived, that is, extracted from a historically contingent group of things, although types are defined in some descriptive detail. Evolutionary archaeologists have pointed out that hierarchical taxonomic classifications privilege some dimensions of variation over others, often without justification. These scholars have advocated paradigmatic classification, which treats all dimensions of variation equally (Neff 1993; O'Brien and Lyman 2002; see Sterling 2001 for a study of Egyptian Meidum bowls based on a paradigmatic classification). However, in this case the very detailed variant categorisation defined by Parzinger and Sanz is likely to capture even rare variants in rim shape, while their database also includes records of variation in bowl diameter and in fabric and surface coating within each of these rim shape–based groups and variants.

The Parzinger and Sanz (1992) data set includes a total of 6,927 contextually secure rim fragments. Parzinger and Sanz (1992) identified 17 (A–P, Q for bases) functional vessel categories (jars, cooking pots, bowls, etc.). These 17 vessel categories are further subdivided into a total of 60 formal groups and 244 subgroups. The catalogue lists find context, vessel type and formal classification, ware type, number of rim pieces as well as estimated rim diameters. Bowl fragments are numerically the most prominent vessel category in the Upper City assemblage. Parzinger and Sanz (1992) identified nine primary formal bowl groups (Figure 9.4): I1 (bowls with simple rounded rims), I2 (bowls with simple thickened rim),

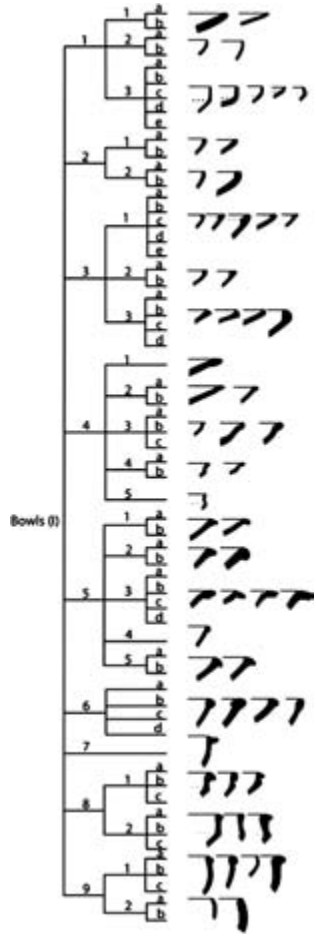

Figure 9.4 Upper City ceramic bowl repertoire (modified from Parzinger and Sanz 1992). This hierarchical taxonomy shows nine bowl types (I1–I9) with multiple type variants (e.g., type I9.2b at bottom of the taxonomy). (From Steele et al. 2010: Fig. 2.)

I3 (bowls with inverted rim), I4 (bowls with sinuous profiles and everted rims), I5, I6, I7 (bowls with everted rims), I8 (carinated bowls with everted rims) and I9 (bowls with inverted walls). Each of these nine groups is further subdivided into one to five formal subgroups, and up to five variants within each of the subgroups, yielding a total of 61 different bowl forms whose frequencies vary across the O. St. 3 and O. St. 2 phases (Figure 9.5). Relatively large sample sizes (O. St. 3: 1,393 and O. St. 2: 2,061) as well as the wide range of formal types and variants make the bowl assemblages a promising starting point for testing the neutral model.

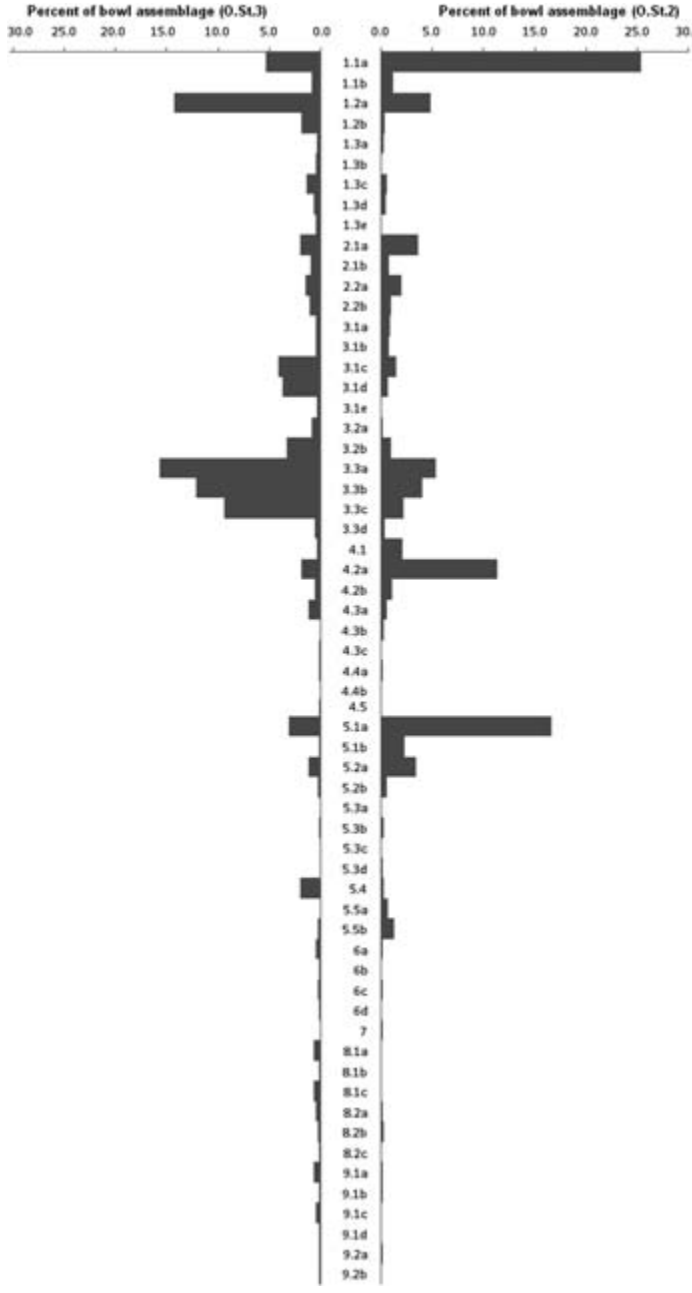

Figure 9.5 Percentages of each bowl type in the Upper City rim sherd assemblages from O. St. 3 and O. St. 2. (From Steele et al. 2010: Fig. 3.)

The Late Bronze Age wheel-made pottery from the Upper City is also classified according to four overall ware categories (A, C, D, E; Tables 9.2 and 9.3). Variation in colour, temper, firing atmosphere and hardness are expressed in subcategories. For this study, we used only the four overall ware distinctions. Ware A consists of red-brown, medium to coarse mineral-tempered clay (0.5–3 mm inclusions) with little or no surface treatment beyond smoothing and occasional washes. Ware C combines a finer fabric with grain sizes rarely exceeding 0.5 mm and a fabric colour ranging from beige to red with red-brown, polished slips. Ware D consists of fine as well as coarser fabric types (0.2–3 mm inclusions) with beige to white slips of varying thickness. Ware E designates a fine beige to red fabric (0.1–1 mm inclusions) with smoothed or plain surfaces.

Due to postdepositional processes such as erosion, as well as Iron Age and later reoccupation of the Upper City, the Late Bronze Age ceramic material derives almost exclusively from rooms in up to two-storey-high half-basements that were built against the natural slope. The vast majority of the Upper City data comes from basement room fills that include sherd and architectural remains from collapsed upper basements and ground floor levels (Parzinger and Sanz 1992:3–14, 74, 89–90; Table 9.3).

Results

The frequency distributions of types and variants do not depart significantly from those expected under drift in either phase, so we need to look at specific attributes to detect if changes in attribute frequencies reflect functional non-equivalence and imply that variants were being selectively reproduced (for details of this analysis, see Steele et al. 2010). Bowl shapes that were produced more often in plain coarse ware in the first phase tended to become more popular in the second phase. A linear regression analysis with stepwise variable selection (Table 9.4) shows that the abundance rank of bowl variants in the later phase was predicted not only by their rank in the earlier phase (as would be expected under drift), but also by the proportion of each variant that had been made using the plain coarse ware in that earlier phase. This is inconsistent with a neutral model and indicates that by the later phase, popularity of bowl types was associated with functionally significant characteristics that had become subject to selection. Ware frequencies are also summarised for each phase in Tables 9.3 and 9.4.

The most common wares in O. St. 3 are the finer plain ware, which makes up 34.5% of all bowl rim sherds, and the red slip–coated ware, which makes up 32.6% of bowl rim sherds. In the later phase, these

Table 9.2 Frequencies of rim sherds of the main bowl groups in each of the four wares, by phase.

	Ware A (plain coarse)	Ware C (red slip)	Ware D (white slip)	Ware E (plain fine)	Total
O. St. 3 Phase					
Type I-1 (bowls with simple rounded rims)	80	111	28	141	360
Type I-2 (bowls with simple thickened rims)	22	12	8	36	78
Type I-3 (bowls with inverted rims)	171	276	53	214	714
Type I-4 (bowls with everted rims)	22	19	5	19	65
Type I-5 (bowls with everted rims)	41	16	5	37	99
Type I-6 (bowls with everted rims)	7	2	1	6	16
Type I-7 (bowls with everted rims)	-	-	-	-	-
Type I-8 (carinated bowls with everted rims)	5	11	2	17	35
Type I-9 (bowls with inverted walls)	6	7	2	11	26
TOTAL	354 25.4%	454 32.6%	104 7.5%	481 34.5%	1393
O. St. 2 Phase					
Type I-1 (bowls with simple rounded rims)	590	32	3	61	686
Type I-2 (bowls with simple thickened rims)	94	5	-	52	151
Type I-3 (bowls with inverted rims)	240	35	4	68	347
Type I-4 (bowls with everted rims)	300	8	1	15	324
Type I-5 (bowls with everted rims)	501	6	1	11	519
Type I-6 (bowls with everted rims)	5	-	-	3	8
Type I-7 (bowls with everted rims)	2	1	-	1	4
Type I-8 (carinated bowls with everted rims)	4	-	-	7	11
Type I-9 (bowls with inverting walls)	11	-	-	-	11
TOTAL	1747 84.8%	87 4.2%	9 0.4%	218 10.6%	2061

Table 9.3 Frequencies of rim sherds of different ware types found in the 23 contexts reported by Parzinger and Sanz (1992) (see Table 9.1).

O. St. 3 Phase	Ware A (plain coarse)	Ware C (red slip)	Ware D (white slip)	Ware E (plain fine)	Total
House 16	2	5	4	5	16
House 20	16	16	4	12	48
To House 20	133	229	47	146	555
House 21	24	62	5	49	140
House 22	6	2	0	1	9
Subtotal (houses)	181	314	60	213	768
	23.6%	40.9%	7.8%	27.7%	
Temple 9	69	48	13	152	282
Temple 10	31	16	5	53	105
Temple 15	25	27	3	15	70
Temple 18	8	7	1	6	22
Temple 19	33	38	22	38	131
Temple 21	7	4	0	4	15
Subtotal (temples)	173	140	44	268	625
	27.7%	22.4%	7%	42.88%	
Total (O. St. 3)	354	454	104	481	1393
	25.4%	32.6%	7.5%	34.5%	
O. St. 2 Phase					
House T 24	47	0	0	7	54
Houses 24–30	116	2	1	13	132
Houses 31–37	36	0	0	5	41
Sub-total (houses)	199	2	1	25	227
	87.7%	0.9%	0.4%	11%	
Temple 7	766	29	1	97	893
Temple 8	289	26	1	16	332
Temple 12	156	7	2	36	201
Temple 20	55	20	3	15	93
Temple 26	68	3	1	10	82
Sub-total (temples)	1334	85	8	174	1601
	83.3%	5.3%	0.5%	10.9%	
Kiln 5	118	0	0	11	129
Kiln 8	15	0	0	1	16
Kiln 9	53	0	0	4	57
Kiln 27	28	0	0	3	31
Sub-total (kilns)	214	0	0	19	233
	91.8%	0.0%	0.0%	8.2%	
Total (O. St. 2)	1747	87	9	218	2061
	84.7%	4.2%	0.4%	10.6%	

percentages decline to 10.6% and 4.2% respectively. There was selective reproduction of large, coarse plain ware vessels in the later phase (O. St. 2). Bowls with larger diameters in the earlier phase (O. St. 3) tended also to be made more often in the coarse plain fabric. Figure 9.6

Table 9.4 Linear regression analysis (with stepwise variable selection) of the effects of bowl variant characteristics in the earlier phase (O. St. 3) on abundance in the later phase (O. St. 2). Abundance of bowl variants in O. St. 2 is predicted by their rank in the previous phase (O. St. 3) and also by the percentage of the variant that was made in coarse plain ware in that earlier phase. There is no separate effect of vessel diameter or of percentage of variants made in the other ware types in the earlier phase.

Dependent Variable	Predictor	B Coefficient (Unstandardised)	t	Sig.	Pearson's *r*
Rank of variant in O. St. 2	Constant	16.9 ± 3.4	5.0	< .001	0.76
	Rank of variant in O. St. 3	0.61 ± 0.1	6.1	< .001	
	Percentage of variant made in plain coarse ware (A) in O. St. 3	–28.2 ± 6.8	–4.1	< .001	
Variant as % of total O. St. 2 assemblage	Constant	–0.01 ± 0.01	–0.6	0.56	0.47
	Variant as % of total O. St. 3 assemblage	0.49 ± 0.16	3.0	< 0.01	
	Percentage of variant made in plain coarse ware (A) in O. St. 3	0.05 ± 0.03	2.2	0.03	

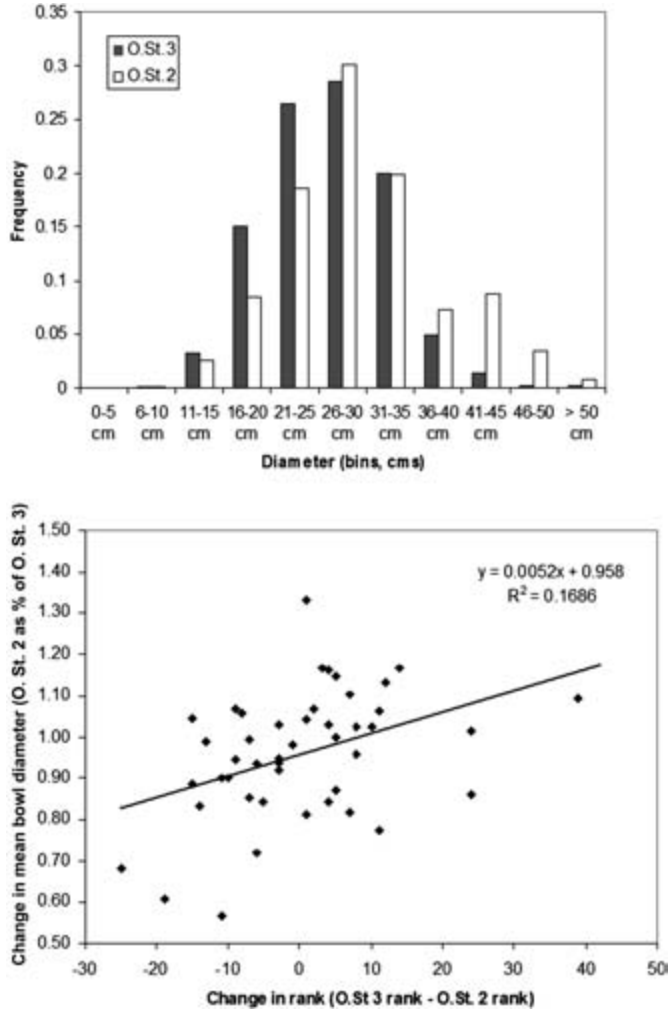

Figure 9.6 The upper histogram shows proportional frequencies of bowls of all types by diameter and phase. Mean bowl diameter in O. St. 3 = 26.5 cm, in O. St. 2 = 29.7 cm. The lower plot shows the correlation between changing abundance ranking and change in average diameter of bowl variants represented in both phases (O. St. 3 and O. St. 2). Pearson's $r = 0.40$, $p = 0.006$. (Redrafted from Steele et al. 2010: Fig. 6.)

shows a trend not apparent from the variant frequencies themselves, which is for the bowl variants that increase in relative frequency (higher rank) in O. St. 2 also to be of increased mean diameter in that later phase while retaining the rim form that is the basis of their typological

classification. The most likely reason for this is that bowls produced in the coarse fabric in O. St. 3 tended also to be among the larger examples of that variant, so that when we find in O. St. 2 a larger proportion of that variant made in coarse fabric the mean diameter is also greater.

For both the main bowl groups and the variants, the diversity statistics calculated under the assumption of the neutral model (the power law exponent [Bentley et al. 2004] and the index θ_E [Neiman 1995]) suggest that there was a decrease in the number of potters and/or in the rate of innovation in the later phase (details in Steele et al. 2010). Estimates of annual pottery consumption seem to indicate that in O. St. 2 fewer potters may have been required to satisfy the demand of the state institutions in the Upper City. The values in Table 9.5 are estimates of the aggregate floor areas of temples and other buildings in the two final occupation phases of the Upper City (Neve 1999, 2001) and estimates of annual ceramic consumption based on the ca. 12,000 vessels reconstructed by Whitelaw (2001) for the Late Bronze Age palace at Pylos, whose ground plan measures ca. 7,000 m^2. Although Aegean palatial societies operated on a different spatial scale than the Hittite and other Near Eastern states and empires, state-related functions of storage and large-scale consumption at the palace of Pylos and in the temple quarter of the Upper City should allow us to draw general parallels with regard to ceramic consumption and labour time in Late Bronze Age palatial societies. Drawing on ceramic production rates from ethnographic studies and allowing for a less competitive non-market environment, Whitelaw (2001:65) proposes a relatively conservative (Hruby 2006) average production rate of 27 vessels per full-time potter per day and a 300-day working year for the potters at Pylos. This would result in an annual output of around 8,100 vessels per potter per year, falling closer to the output of ethnographically observed low-rate producers

Table 9.5 Estimates of annual pottery consumption and potters in the Upper City of Boğazköy-Hattusa (based on Whitelaw 2001).

	O. St. 3 (total)	O. St. 2 (total)	Pylos Palace
Approx. area (m^2)*	26,757	17,259	7,000
Population	-	-	-
Consumption (p/a)	45,869	29,587	12,000
Potters (full-time)**	6	4	2
Potters (part-time)**	12	8	4

*Average floor areas for temples and houses in each phase were added for buildings with only partial ground plans (includes all contemporary buildings, not only those included in the ceramic database used in this analysis) (following Neve 1999, 2001)

** Estimated following Whitelaw (2001): 8,100 vessels per potter/year (27 vessels per potter/day for potters supplying the Late Bronze Age palace at Pylos)

(ca. 6,000 vessels per year per potter) than high-output producers (ca. 14,000 to 15,000 vessels per year per potter) (Roux 2003:769–70).

In the two final occupation phases of the Upper City at Boğazköy-Hattusa, we find that overall architectural space decreases from ca. 27,000 m^2 in O. St. 3 to 17,000 m^2 in O. St. 2 (calculated from Neve 1999, 2001). While some bias from differential exposure of the two levels cannot be excluded, there does appear to be a trend towards a reduction in the number of buildings and spaces being given over to craft production. According to our estimates, which are based on the above square meterage and a comparison with Whitelaw's estimates of annual consumption for the Pylos palace, annual consumption in the buildings of O. St. 3 amounted to at least 46,000 vessels per annum and 30,000 vessels in O. St. 2. To cater for this demand in pottery by the state institutions of O. St. 3, six full-time or 12 part-time potters would be required. The demands of O. St. 2 could be met by four full-time or eight part-time potters.

In both Upper City levels, the number of full- or part-time specialists necessary to produce what seems to be an enormous number of vessels is very small. Circumstantial corroboration for low numbers of ceramic specialists in contemporary Aegean and Near Eastern settings include a study of fingerprints from the Pylos palace (Hruby 2006), which indicate that the entire corpus of fine ware found in the final destruction layer may have been produced by a single potter. Near Eastern textual sources also seem to point in this direction. Potters are usually referred to as one or two individuals in ration lists or lists of professionals (e.g., Heltzer 1982: KTU 4.609, KTU 4.367, KTU 4.339, KTU 4.46; Jakob 2003:473–75; Mayer 1978: Ration list 14,593 (R 76) and CT 51,3,10; Müller-Karpe 1988:150–60).

Discussion

We have proposed that if LBA pottery production at Boğazköy-Hattusa was subject to centrally enforced standards of pottery production as part of a political strategy of cultural homogenisation, then we would expect to find low rates of innovation by individual potters and a stable repertoire composition. We would also expect to find more conformism in the choices exercised by consumers of pottery, leading to a superabundance of sherds from the most common forms. Alternatively, if pottery production was subject to economically motivated state sponsorship in the absence of such centrally enforced norms—driven by the purely economic considerations of securing the supply of daily commodities—then the assemblage should display relative fluidity in terms of repertoire composition over time (the repertoire should be allowed to 'drift'),

except—crucially—in those characteristics that are functionally relevant to the economic agenda. To evaluate the evidence, we also used the neutral model to define a null hypothesis of the diversity expected from chance sampling effects.

In this study we found evidence of considerable repertoire fluidity over time, consistent with the economically motivated state sponsorship of pottery production. However, we found evidence for cultural selection in the O. St. 2 assemblage in favour of large, simple-rim or everted-rim bowls of coarse fabric. Finer red-slipped ceramics decline throughout the Late Bronze Age and seemingly edge close to extinction in O. St. 2. We also found an indirect indication of a possible reduction in the number of potters in that later phase.

These results suggest that while there was selection at work, it was not driven by a conformist bias, that is, where individuals preferentially copy the most common variants. If that were the case then we would expect to see a superabundance of the most common types in both phases, and in the later phase, an increase in abundance of types that were already the most frequent. This is not the case. Rather, if we follow Müller-Karpe's (1988) functional interpretation, the changes between the two phases suggest an increased importance in food consumption/preparation for firm over liquid or semi-liquid food, a preference for larger vessels and an increased preference for coarse and potentially less labour-intensive ware types. The long-term trend of decreasing numbers of elaborate slipped and polished vessels may be related to the decreasing role of high-investment pottery in social contests. Late Bronze Age Near Eastern and East Mediterranean value systems, at least at the top of the social hierarchy, appear to have moved on to other materials such as precious metals, stone or ivory for social and political negotiation. Knappett (2001) has argued that plain, hastily produced vessels at the Late Bronze Age palace at Pylos were not themselves the objects of conspicuous consumption but that this pottery served as furnishings in public feasts and festivals, another form of social contest. If at all, the Late Bronze Age north-central Anatolian pottery examined in this study should fall into the latter category. In the case of shallow bowls with simple rounded rims (type I1.1), which become a dominant type in the O. St. 2 repertoire, we may be seeing a functional amalgamation or changeover from previously popular plates with stepped rims (Müller-Karpe 1988:127), for which frequent traces of secondary burning indicate a function in food preparation (Schoop 2003:173).

In the present study the traits analysed were formal typological ones, some of which potentially affect bowl function (for example, the presence or absence of inverted rims). Analysing traits in a neutral model when their functional equivalence is not proven is a familiar strategy in

ecology. A neutral model has been applied in this field as a null model of species abundance (for example, to explain the abundance ranking of tropical forest tree species at a given location), and within ecological studies the presumption of functional equivalence among species is highly debatable (e.g., Hubbell 2005). Nonetheless the neutral model has been hard to falsify, and its application has led to the recognition that species abundance distributions may reflect multiplicative random processes as well as adaptive variation in niche characteristics. Furthermore, strong tests of departure from neutrality (and thus of the presence of selection) have been shown to involve rather more than simply testing the 'goodness of fit' of empirical data to a theoretical frequency distribution. In this paper we have used such a strong test, and by showing a correlation between vessel characteristics and their abundance ranking in the later phase (O. St. 2) we have identified clear evidence of departure from neutrality (the neutral model would predict functional vessel equivalence, and therefore the absence of any such correlation). There is considerable literature on the association of vessel performance in, for instance, cooking tasks or the retention of liquids with fabric coarseness, types of inclusions, wall thickness and surface treatment (Rice 1987). At LBA Boğazköy-Hattusa, selection seems primarily to have been on the ware types and perhaps secondarily on the bowl dimensions, and there seems also to have been selection against the inverted rim forms; not all the lower-order rim variants, however, need necessarily have been functionally significant in their own right. Some of them may have increased in frequency through 'hitchhiking', where a variant increases in frequency because it is functionally tied to another variant whose distribution is a result of selection.

Our results do not indicate that ceramic bowl diversity in either of the two Upper City occupation phases shows the tendencies we would expect from an environment in which pottery production and consumption are centrally regulated to promote social conformity to specific standards or norms. The potters' selective reproduction of bowl types did not lead to a superabundance of one or just a very few bowl forms, selected on the basis of prior abundance rather than functional efficacy. The occupation of O. St. 3 falls into the Hittite imperial phase proper, in which we might expect Hittite administration to have had a good grip on production, if they were indeed inclined to do so. Our results, however, do not point in this direction; the frequency distribution of bowl forms in O. St. 3 does not depart from the expected levels of diversity under drift. O. St. 2 is the last phase of LBA occupation at the Hittite capital, during which abandoned temples and houses from the previous period are replaced by new structures and a series of kilns and other craft production areas are installed in the central temple quarter. Our results

indicate that assemblage composition in O. St. 2 is selectively biased in favour of large everted- or simple-rim bowls in coarse plain ware. This implies a level of cultural selectivity inconsistent with the random multiplicative process of the neutral model, and is consistent with state sponsorship of pottery production driven by the purely economic considerations of securing the supply of daily commodities.

Historically, although currently somewhat afloat in terms of absolute chronology (Schoop 2003:171), O. St. 2 spans the final phase of Hittite political downturn and possibly also the final abandonment of central institutions and its aftermath (Seeher 2001). We have, thus, to ask what circumstances brought about the centralisation—at least physically—of craft production in this phase. Security from external foes, such as the Kaska (e.g., Glatz and Matthews 2005; Zimansky 2007), may have been an important consideration in moving potentially hazardous production facilities within the confines of a settlement. With this move, the number of producers catering for the Upper City temple quarter may have been reduced, leading to a reduction in repertoire diversity and functional amalgamation. Reasons for the observed surge in coarse plain wares may be partly functional in nature. The most popular bowl type in O. St. 2 seems related to cooking and baking activities in which a coarser fabric is advantageous. Demands on a reduced number of potters may also play into a decline in ware diversity and result in products with signs of hasty production and a decline in technical quality (Schoop 2009). It is difficult to pin down the motivations for this selective behaviour, as the functions of these bowls are not known beyond their likely use in the preparation and/or consumption of firm foods. Here there is, of course, an area of overlap with interpretive archaeologists who seek to understand the intentions of people in the past (e.g., Sillar, this volume).

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CHAPTER TEN

Cultural and Biological Approaches to the Body in Archaeology: Can They Be Reconciled?

Ruth D. Whitehouse

> 7 April 1852.
> Went to the Zoo.
> I said to Him —
> Something about that Chimpanzee over there reminds me of you.
> (Carol Ann Duffy, "Mrs. Darwin," from *The World's Wife*)

These witty lines by Britain's new Poet Laureate are an entry point into the dichotomous ways in which the body is studied in archaeology and other social sciences such as anthropology and sociology. On the one hand, it refers to Darwin's primary idea of the evolution of species by natural selection, a fundamentally biological understanding of bodies, while on the other it describes one of the ways in which humans relate to each other, using culturally created language to interpret embodied experience.

In fact human bodies can clearly be understood as both biological and cultural; what is critical is how the relationship between biology and culture is conceived. Recent work has been highly polarised: in evolutionary biological analyses, the biological body is primary, with culture regarded, explicitly or implicitly, as the means through which humans act out their biological destiny (culture as 'extra-somatic means of adaptation'[1]), while in postprocessual or interpretive analyses culture is regarded as a more or less autonomous sphere with its own logic and dynamics (culture as 'webs of significance'[2]), with biology no more than a constraint on the range of possible human behaviour. In interpretive studies, biological attributes and processes, although frequently referenced in terms of the ways they are culturally interpreted, remain untheorised within the framework of present day scientific understanding. The polarisation of the literature means that these underlying positions

are rarely justified explicitly or even discussed, but are largely taken for granted and, since scholars within each group write only for each other, there is little incentive to examine, challenge or modify them. This chapter summarises recent work on the interpretive side of the divide, emphasising the diversity of perspectives present and their potential for further development. It then considers whether there is scope for any kind of reconciliation between the two broad approaches and concludes that while the divide is too great to be easily bridged, both sides would benefit from giving serious consideration to the work of the other.

The Body in Archaeology

Archaeologists have always studied bodies. This may seem a statement of the obvious, but it is worth stating since it makes archaeology something of an exception in the social sciences. Chris Shilling, in his classic text *The Body and Social Theory* (2003 [1993]: Ch. 2), describes how in sociology the body had been historically (before the 1980s) something of an 'absent presence'—implicitly there in many types of research, but rarely the central focus of study. The same could probably also be said of social anthropology. This was not the case in archaeology, probably because human bodies have always constituted a major component of the archaeological record, too prominent to ignore. Of course the bodies traditionally studied by archaeologists are very different from those studied by sociologists or sociocultural anthropologists. Rather than fleshy, living, performing bodies, archaeological bodies are long dead, usually appearing as skeletons, whether complete, disarticulated, fragmentary or burnt. Even in the cases where flesh and skin survive, these are distorted, altered by deliberate or accidental processes of mummification. Nonetheless, because of the importance of the human body in the archaeological record, it has always formed a specific focus of study. Traditionally studies of archaeological bodies have been of scientific type, often subsumed under the label of 'osteoarchaeology' and concerned mainly with characterising populations rather than individuals in terms of factors such as life expectancy, diet and disease. In this respect body studies in archaeology have been closer to those of biological anthropology than to sociocultural anthropology or sociology. However, in spite of their concentration on biological aspects, these archaeological studies, unlike those of evolutionary biology, have not in the main been associated with explicit theory about the dominant role of biology in human development, nor indeed with explicit theory of any kind.

It was not until the 1990s, with the delay characteristic of the adoption of theoretical approaches from other disciplines into archaeology, that approaches to the body derived from the social sciences were taken

up. In an article published in 1996 under the title 'The Somatization of Archaeology', Lynn Meskell characterised what she described as a burgeoning field in archaeology, while also criticising it for what she perceived as its concentration on issues of power, derived from Foucault, at the expense of embodiment and agency (Meskell 1996). In the years since that article was published embodiment has also become a major focus of study, and there is now a wide range of different approaches to the body in archaeology, all of which could broadly be described as 'interpretive'. Important books on various aspects of this field include Hamilakis et al. (2002), Joyce (2008), Meskell and Joyce (2003), Rautman (2000) and Sofaer (2006). An indication of the state of body studies now can be seen in the ambitious research project entitled 'Changing Beliefs of the Human Body', funded by the Leverhulme Trust and based in the Universities of Cambridge and Leicester (www.arch.cam.ac.uk/lrp/intro.html) between 2005–2009. This project, directed by Dušan Borić and John Robb, has addressed the question of how humans change what they believe about the human body. It involved archaeologists, historians and anthropologists and took the form of five different studies ranging in time periods from early prehistory to the present day. The outstanding point here is that the project was not concerned primarily with the biology of human bodies, but with the beliefs that people hold about those bodies, including, but only as one of many different viewpoints, current scientific understandings. This concentration on variable and changing attitudes to the body, rather than on a relatively stable and unchanging biology, characterises interpretive approaches to the body in archaeology.

In this chapter, I shall attempt to describe and exemplify these approaches and then go on to discuss the relationship between studies of this type and ongoing research of 'scientific' type on archaeological bodies.

Interpretive studies of the body in archaeology can be characterised in a number of different ways. For instance, two main approaches can be recognised, one of which concentrates on 'the body as symbol', treating the body as a cultural text, a system of meanings, separate from individual lived bodies. The second category, 'embodiment', is concerned with the embodied experiences of individual lives. Archaeological studies exploiting the idea of the body as symbol appeared earlier than those concerned with embodiment. Studies of this kind owe their greatest anthropological debt to the work of Mary Douglas (1966, 1970), who equated the physical body with the social body, so that, for instance, body boundaries and body orifices can be taken to signify social boundaries and passages. They tend to be associated with studies of social organisation and of power distribution within society. By contrast, embodiment studies tend to concentrate on individual lives

and issues of agency. However, both these categories embrace a range of different approaches and can also at times overlap. Here I shall use a different basis for discussion: a traditional archaeological approach that depends on the nature of the evidence being studied. I shall recognise three main categories: (1) archaeologically surviving human remains; (2) representations of human bodies in two- or three-dimensional form; and (3) phenomenological studies starting from the embodied experience of the modern researcher. Of course, this categorisation no more provides clear-cut divisions than the body as symbol/embodiment system, and there are many studies that draw on more than one category of evidence. However, I have found it the easier system to use and shall do so here. It is worth emphasising that my aim is not to provide any kind of complete synthesis but simply to exemplify the range of approaches to the body found in interpretive archaeology.

Human Remains

One early example (in terms of interpretive approaches) is Shanks and Tilley's (1982) study of Neolithic mortuary practices, which clearly represents an example of the 'body as symbol' approach. They looked at a small number of long mounds in Sweden and southern England, used for disarticulated collective burials, in which the skeletal material had been recorded with sufficient care that the precise location of each bone was known. They analysed the distribution of the bones in the different chambers or zones of the mounds in terms of categories of age and gender, and also in terms of body divisions: right/left, upper/lower, trunk/limbs. They demonstrated that the distribution of the different bones was not random, and they interpreted the patterns found in terms of a number of structuring principles, which are assumed to be fundamental principles for the Neolithic societies being studied, in life as well as death. The first principle was the assertion of the collective and the denial of the individual, indicated by the process of dismantling of individual bodies and the regrouping of bones in different configurations. A second principle was the expression of boundedness, equated with the exclusiveness and solidarity of the local social group; this was indicated by the practice of storing combinations of bones in specific, bounded areas. A third principle was an emphasis on basic body symmetries, since disarticulated bones were regrouped on this basis. Yet another principle was the importance of distinctions based on age, since bones of immature individuals were separated from those of adults. In some, but not all, of the tombs, gender distinctions were also recognised. Characteristically, both for the time it was written and for studies in 'the body as symbol' tradition, the article addressed the issue of power distribution in society. The authors

argue that Neolithic society in northern Europe was in fact hierarchically organised; therefore the assertion of the collective demonstrated in the burial practices is interpreted as a mode of masking the reality of the power exercised by an elite over the rest of society.

My second example takes a very different approach. Robb's (2002) study focuses on what he calls 'osteobiography', a term he borrows from Saul and Saul (1989) to refer to the interpretation of human skeletal remains to throw light on a range of life conditions and events. This approach can be directed at understanding the lives of particular individuals, but Robb is more interested in illustrating the composite lives of a community or wider population—that of the Italian Neolithic in the case study in question. He emphasises this point with his use of another term, 'biographical narrative', which he defines as a 'cultural idea of what a human life should be'. The biographical narrative does not relate purely to biology, but is at least in part about the 'socialisation of biological change' (Robb 2002:155). Biographical narratives are well documented ethnographically: they generally involve the division of the human lifespan into a number of socially recognised stages (often different for males and females) that are related, at least in a general way, to the biological processes of growth, maturation, degeneration and death. They involve at least four stages—child, adult, senior adult and (after death) ancestor—and sometimes many more. Transitions between the different culturally recognised stages are usually marked by rites of passage, at the end of which the individual has taken on a different social persona, with different roles, relations and obligations to others. Robb emphasises that, however well defined the cultural ideal, progress through the stages of life for any individual is not an inevitable or passive process: for one thing, people vary in their ability to live up to the ideal. Also there may be alternative trajectories available, perhaps with positions of prestige available to those with special skills or abilities.

Whereas anthropologists study living people in their progress through the stages of life, archaeologists must turn to what survives in the archaeological record, and here human skeletal remains offer the most direct evidence—hence 'osteobiography'. In his 2002 study, Robb presents both a general account of Italian Neolithic people as revealed by their skeletons and a detailed analysis of one individual, labelled Catignano I, from a site in the Abruzzi. Catignano I was an adult female, who had suffered at least seven episodes of growth interruption in her childhood (as shown by enamel hypoplasia defects in her teeth). As an adult she had had both upper second premolars deliberately removed, possibly when she achieved adult status (deliberate tooth removal has been recognised in a significant percentage of female skeletons of the Italian Neolithic, though the teeth extracted were usually canines or incisors). As an adult

the Catignano I woman suffered from various conditions apparent in the skeleton, including osteoarthritis and dental disease. Most seriously, she suffered from osteitis of the pubic symphysis, which may have been caused by pelvic infection following pregnancy. She had also suffered a severe cranial trauma, probably caused by a direct blunt-force blow, which she had survived. She also survived two surgical trepanations in the same area of her skull, possibly aimed at alleviating symptoms caused by the cranial injury. She died, of unknown causes, between the ages of 40 and 50, as one of a probably small number of senior and experienced females in her community. Robb's concentration on this one individual might seem to belie his professed interest in collective cultural biography; however, throughout his description of the Catignano I skeleton, he refers both to comparative skeletal material and to other archaeological evidence to discuss how typical or exceptional the features of this particular life had been. In this way the individual is skilfully placed in the broader cultural context.

My third example relating to skeletal remains comes from Sofaer's 2006 book, *The Body as Material Culture: A Theoretical Osteoarchaeology*. Sofaer's book represents an explicit attempt to reconcile traditional osteoarchaeology, which studies skeletons in a framework that implicitly considers human bodies to be relatively unchanging and universal, with more recent studies in interpretive archaeology that consider human bodies to be socially constructed, varying contextually and historically, but that rarely deal with human remains themselves. Robb's study, just discussed, represents an exception to this rule and has something in common with Sofaer's approach. However, the studies have different emphases: while Robb focuses on the relationship between the biological changes that occur during a human lifespan and cultural ideas of what a human life should be, Sofaer aims to study the body as material culture, in the same kind of way that artefacts are studied. In this framework the body, including the skeleton, is regarded as produced by social practice, including diet, exercise, lifestyle, risk taking and disciplinary regimes. One of the aspects she is particularly interested in is the interaction between the body and objects—the artefacts that archaeologists commonly study.

Her case study relates to the sixteenth- through nineteenth-century site of Ensay in the Outer Hebrides (Sofaer 2006:106–12, 140–41; see also Sofaer Deverenski 2000). The crofting community at Ensay had a strictly gendered division of labour documented ethnographically and historically. Sofaer's study of the human remains from the Ensay cemetery demonstrated significant differences between the skeletons of men and women, which she relates to the division of labour, particularly the load bearing undertaken by women. Women carried heavy loads of peats

and seaweed in baskets called creels, which were supported by a woven strap across the breastbone and around the shoulders, with the weight resting on a pad just above the pelvis. This led to a characteristic posture, modifying the normal S-shaped curvature of the spine and altering the way weight was transferred down the spine. As a result Ensay women were less affected than men by changes to the thoracic vertebrae, but were strongly affected by osteophytosis in the lumbar vertebrae. Sofaer interprets these changes in terms of interplay between natural and activity-induced stresses on the spine and specifically with the use of the creel for load bearing. Sofaer clearly recognises, although she does not quite say it explicitly, that the skeleton is not produced in the same way as the artefacts more commonly labelled 'material culture', since in the case of the skeleton, human action works in a much less direct way and the role of biology is considerable, both in terms of the original 'material' worked on and of the natural processes (such as ageing or responses to environmental conditions such as a damp climate) interacting with those of cultural activity. Nonetheless she shows that studying the body as material culture allows a range of new insights and understandings that traditional osteoarchaeology simply bypasses.

Representations

Representations of the human body have always been studied in archaeology, but before the era of interpretive archaeology such studies were often aesthetically based and drew mainly on the approaches of traditional art history. There were other traditions of study too: the structuralist interpretations of the French school, applied especially to Palaeolithic cave paintings (e.g., Leroi-Gourhan 1968) and the Mother Goddess interpretations of female figurines, a long-standing tradition that had all but been abandoned by the 1980s, when it was revived by Marija Gimbutas (1982, 1989, 1991) and gained great support among one part of the women's movement, though not, it must be said, among archaeologists, feminist or otherwise. Processual archaeologists mostly ignored the entire field of 'art' since it seemed to have little to contribute to the economic and technological concerns that dominated their studies. With the development of interpretive archaeology, a series of critiques of earlier approaches has appeared as well as a new range of studies. Many of these have emerged in the context of the field now labelled 'gender archaeology', much of which is explicitly inspired by feminist theorising. This field is too large to summarise here, but a comprehensive introduction is provided by Nelson (2006). Here I have selected three case studies to illustrate different approaches to the body based on the study of representations.

My first example, also the earliest in date, is not usually classified as part of gender archaeology, although it does focus on issues of gender within a discussion about developing an archaeology of the body. It is Tim Yates's 1993 study of Bronze Age rock art in Scandinavia, which draws heavily on Continental theory and fits into what is sometimes called 'the literary turn' in theory and into studies of 'archaeology as text' that were popular among interpretive archaeologists at that time. In his examination of the carvings of Bohuslän on the west coast of Sweden, he looks at the human figures, which have traditionally been divided into those with a penis shown, equated with men, and those without, assumed to represent women. Yates challenges the straightforward association of the penis with maleness and its absence with femaleness, which he considers a culturally specific Western notion. Instead, he argues that in Bronze Age Scandinavian society, masculine identity had to be guaranteed by signs applied to the surface of the body (the penis, and also weapons, on the human figures; and on the deer representations, antlers). Therefore he argues that the figures lacking penises and weapons are not necessarily female: they could be male children or adolescent boys who had not yet been assigned cultural masculinity.

Among the studies of representations of the human body that fall within the 'gender archaeology' framework and are grounded in feminist theory, one significant emphasis has been a challenge to the rigid two sex/two gender classification characteristic of our own society. One example is Naomi Hamilton's (2000) study of figurines. This article offers a critique of previous studies of the prehistoric anthropomorphic figurines of the Near East and Europe, which includes the Mother Goddess literature mentioned above but also other work, including Peter Ucko's famous study of 1968, which dismissed the Mother Goddess theory and discussed many other possible uses and interpretations of the figurines. Hamilton outlines a series of problems in the ways in which figurines have been identified as 'female' or 'male' by various authors. Ucko, for instance, recognised only penises and breasts as indicators of sex, dismissing apparent pubic triangles as leg muscles or waist or hip lines. She points out that using different criteria for identifying the gender of a figurine obviously changes the relative numbers of each gender present, which in turn may affect the interpretation of the figurines as a whole. Whatever the criteria used for identification, it is clear that there are a number of ambiguously sexed or dual-sexed figurines (a few) and of sexless figurines (many). Hamilton suggests that the first category may refer to dual-sexed individuals or gender-crossers, as are well known from the ethnographic literature. Perhaps more interesting is her interpretation of the sexless figurines. Most authors of figurine studies go to some lengths to assign these figurines to one gender or the other of the binary

system, using a range of arguments to support their decisions. Hamilton, however, suggests that they may indicate that, contrary to our expectations, sex was not a major structuring factor in the societies in question.

Another trend in recent studies of human representations in archaeology has been to challenge the idea that they are either symbols of general qualities such as 'fertility', or straightforward reflections of the roles and activities of people in past societies. Instead they are interpreted as being actively used in cultural practices concerned with the negotiation and performance of identity. One of the studies that has taken this approach furthest is Meskell and Joyce's (2003) ambitious comparative study of ancient Mayan and Egyptian personhood. Meskell and Joyce draw on a wide range of sources, both textual and material, in their examination of embodied experience in these two cultures; iconography, especially representations of humans and deities, is one of the most important of these sources. They look at various aspects of embodiment and deal not only with lived experience, but also with the experience of death and life beyond death. To take one example, in Chapter 6 ('Phallic Culture') they look at different aspects of male sexuality revealed in the archaeological record.

In relation to New Kingdom Egypt, Lynn Meskell discuss not only textual sources, but also model phalli dedicated at shrines to the goddess Hathor and imagery in satirical papyri, such as the Turin Papyrus. The phalli offered to Hathor are interpreted as relating to general fertility, both agricultural and human, and possibly as referring to cures for both impotence and childlessness. The Turin Papyrus contains one section depicting parodic scenes of sexual intercourse between short, aged men with exaggeratedly large penises and young, highly sexualised women in a range of acrobatic poses. These representations contradict the formal Egyptian artistic canons and represent a specific erotic genre, interpreted as being 'crafted by a literate man for other men's viewing pleasure' (Meskell and Joyce 2003:115).

In relation to the Maya, Rosemary Joyce points out that actual scenes of sexual activity are rare, but believes that sexuality can nonetheless be identified in human figures portrayed on stone monuments and on pottery vessels. Specifically she identifies a focus on the young, active male body and suggests that this was the object of the gaze of both older men and adult women. This leads on to a discussion of whether same-sex relationships between men occurred in Classic Maya society, a view rejected in traditional scholarship as incompatible with an emphasis on heterosexual sex and the production of children, which is well documented at least among the Postclassic Maya and related groups. Joyce, however, argues that the two views are only incompatible within the 'heteronormative' worldview of our own society and that it is quite

possible that desire for beautiful young male bodies by both men and women was normal in Maya society.

Phenomenology

My third type of body study in archaeology takes as its starting point the body of the researcher, exploring archaeological sites and landscapes through the experiences of his or her senses. Hamilton (this volume) provides both the theoretical background to phenomenology and places it within the broader context of landscape archaeology. Here I shall simply describe a few examples, to provide a 'feel' for the nature of this kind of work.

My first example comes from the book that introduced phenomenology to archaeology, Chris Tilley's *A Phenomenology of Landscape* (1994), and is the account of Tilley's walk along the Dorset Cursus, an almost 10 km long, linear Neolithic ritual monument, bounded by banks and ditches, in southwest England (Tilley 1994:172–96). Walking along the Cursus from northeast to southwest, Tilley describes how it runs up and down slopes, crosses water and other natural topographic features, incorporates preexisting long barrows and relates to others outside. During the journey, the banks of the Cursus (estimated as originally up to 1.7 m high) at times would have obscured all views outside, while at other times barrows and prominent natural features would have come into and out of view. Tilley, along with others, interprets the Cursus as a ritual space and suggests that it was used for initiation ceremonies involving liminal states and rituals of reversal. The following quotation refers to the stretch of the Cursus that crosses Gussage Cow Down (Tilley 1994:198):

> Novices are taken out of the mundane everyday world and into the enclosed and bounded space of the Cursus. They move along, going downslope, shut out from the world in the direction of the dying sun, stumble down the concealed ancient river cliff, and then cross wet land to reach the Gussage barrow in the centre of the Cursus. They experience it moving out of sight and changes in the Cursus direction. When they eventually approach the barrow they move round its higher SE end with great ceremony, and see for the first time the barrow outside the Cursus to the south, with its open unenclosed lower end facing them, and are instructed with tales of the ancestors and supernatural beings. Ahead on Thickthorn Down two other barrows are visible, but outside the Cursus. They move down the slope, cross water and continue up-slope to Thickthorn Down, where the barrows are concealed and the massive barrow-like terminus blocks the view. Climbing out of the Cursus, they return to the everyday world with the ditches around the Thickthorn barrows in the 'right' place at the lower end.

I include this lengthy quotation to illustrate how Tilley weaves sensory experience (based on his own bodily journey along the Cursus) into his

interpretation of the past use of the monument and shows how appropriate the sensations described would have been for initiation ceremonies.

I adopt a related approach in my examination of two Mediterranean caves that were used for ritual purposes (Whitehouse 2001), although by choosing two caves that are geographically separate (one in southern Italy and one in Menorca) and were used at different periods (the first Neolithic to Copper Age, the second Bronze Age), I was aiming at a level of generalisation that would probably be anathema to Tilley. I describe the experience of entering and moving through the caves, with their alternating low narrow passages and larger chambers, and the contrasts it affords with the light, space and unselfconscious movements of life outside. The experience involves disorientation, restriction of movement, vision diminished in the darkness (illuminated at best by flickering lamplight) and enhancement of other senses, assaulted by unfamiliar sounds, smells and sensations on the skin, all contributing to probable feelings of pain and fear. I interpret these caves as being used for rites of passage, including initiation rites (like the Dorset Cursus, though quite different in form), and I argue that the difficult physical journey through the caves provides a material metaphor for the symbolic journey of the rite of passage. In elaborating this interpretation I follow Pierre Bourdieu (1977:89–94), who tells us that bodies take metaphors seriously. For example, terms like 'upper' and 'lower' are applied metaphorically to different strata in hierarchically organised societies, and such relative statuses are learned by individuals through habitual bodily practices such as climbing onto podia or thrones (for the upper class) or bowing or curtseying (for the lower). In the case of the Mediterranean caves, I argue that the participants in the rites would have learned through the difficult and painful experiences of their bodies of the importance and transformative nature of their journey.

Much phenomenological work in archaeology, like the two examples just described, has been concerned with ritual sites and practices, but more recently attention has also been directed to the bodily experiences of everyday life and its taskscapes. One project by Sue Hamilton and myself, related to southeast Italy in later prehistory, is described in Chapter 12 (Hamilton, this volume), and I will not discuss it further here (see also Hamilton and Whitehouse 2006).

Discussion

These examples of body-based studies by no means represent the full range of such work, but I hope that they give an impression of the breadth and variety found. Because of this variety, it is quite difficult to characterise the work as a whole, but the studies do have some features

in common, features typical of interpretive archaeology in general. They are all concerned with culturally specific contexts and not with the broader 'human story'. They also tend to emphasise description—'thick description' of the kind that Clifford Geertz has taught us to value (cf. Colleran and Mace, this volume)—rather than explanation. When explanation is offered, it is about features of the society in question and not about human behaviour or human evolution in general. All this is in line with postmodern scholarship in general and its rejection of grand themes and metanarratives. The emphasis in much of this work is on the *differences* between human bodies, regarded as socially and culturally constructed, rather than assuming a generalised and presumed universal human body. A partial exception to this rule is provided by the phenomenological work, which implies some degree of uniformitarianism of the human body in its assumption that the embodied experience of the present-day researcher is relevant to the interpretation of past experience. Another feature of interpretive archaeological work on the body is perhaps rather more surprising, and it is an omission rather than a positive trait. In spite of the new emphasis on the body, remarkably little attention is given to one particular bodily function: reproduction. Even the new approaches to osteoarchaeology, exemplified here by Robb (2002) and Sofaer (2006), while they do mention childbirth, do so almost in passing, and they pay far more attention to other processes, such as the effects of maturation and ageing, or the effects of habitual labour and the interaction with specific tools or equipment. Moreover, as I have discussed elsewhere (Whitehouse 2007:34–36), recent gender archaeology has also largely avoided consideration of reproduction, an omission I attribute to the inheritance from second-wave feminism of a deep-seated determination never to characterise women as wives and mothers. Even in one of the most recent books to join the gender archaeology literature, Rosemary Joyce's *Ancient Bodies, Ancient Lives: Sex, Gender and Archaeology* (2008)—a book that as a whole I admire greatly—women are described as 'Goddesses, matriarchs, and manly-hearted women' (Chapter 2), 'Amazons, queens, and sequestered women' (Chapter 3), 'Sensuous figures, celibates, and sex-workers' (Chapter 4), but nowhere are they described as 'mothers', despite the somewhat reluctant admission that many women of the past *were* mothers and were often valued in that role in their respective societies.

So, what is all this interpretive work on the body to be compared and contrasted with? The main contender is not actually evolutionary archaeology, which in its recently revitalised form has been concerned mainly with the evolution of culture rather than the species (see Shennan 2002, 2008 for a useful summary and Shennan 2009 for recent examples of such studies). Interpretive archaeologists do have issues with such

studies, addressed elsewhere in this volume, but they are not directly relevant to the present discussion, which is focussed on the body. The most obvious disciplines to consider by way of contrast to interpretive studies of the body are evolutionary biology and evolutionary psychology (for the 'ultra-Darwinian' versions see, for example, Dawkins 1976, 1982; Dennett 1996; Pinker 1994, 1997; for less fundamentalist versions see, for example, Dover 2000; Gould 1981, 1989, 1996; for critiques of evolutionary psychology from a range of disciplines see Rose and Rose 2001). We might also add those studies within evolutionary archaeology that are concerned with the cognitive development of early humans (e.g., De Beaune et al. 2009; Mithen 1990, 1996, 2005; Stout 2008).

These branches of study in many ways represent the opposite of the interpretive studies discussed in this chapter. They are by definition about the 'big story' of human evolution; they offer universal explanations about human behaviour, past and present, based on the notion of a universal, unchanging human body; above all they regard reproduction as central to the story. Indeed the need to reproduce, whether on the part of genes, cells or complex organisms such as human beings, is considered the main driver of evolution, with natural selection ensuring 'the survival of the fittest'. In the more extreme versions of this type of theorising (e.g., Cosmides and Tooby 2004), all aspects of human behaviour are explained in terms of a universal human nature, established by evolutionary processes and finding its final form no later than the late Pleistocene, since which era insufficient time has passed for any subsequent change. There is no scope here for considering differences between bodies (except what are taken as primary differences between men and women, whose need to reproduce takes different forms and leads to different behaviour patterns), nor for culture or society as domains with any type of autonomy. So the two types of study do seem to be poles apart, diametrically opposed.

It is now time to return to the question of this chapter's title: can these two approaches to the body be reconciled? It is perhaps easier to answer a secondary question: are they at all likely to be reconciled in the near future? The answer to this second question is no, at least not in archaeology, where the two types of study seem set on resolutely parallel courses, with their respective practitioners writing only for themselves and finding no reason to defend, or even set out explicitly, the fundamental principles underlying their work—although the present volume provides a rare and encouraging exception. But *could* they be reconciled, should the will to achieve this appear from somewhere? The answer ought to be in the positive, since patently bodies are both biological and cultural, however the relationship between the two is conceived. Perhaps it is primarily a matter of scale, the level of generalisation aimed at, and the

kinds of question being researched (see Colleran and Mace, this volume, who pertinently refer to the differences between proximate and ultimate causes). Even those most committed to a culturally constructed understanding of the human body would probably admit that there is a level of generalisation that allows the description of *Homo sapiens sapiens* as a biological species and that it should be possible to document the evolution of that species, as of any other.

At this level of generalisation, evolutionary theory can clearly offer explanations for human behaviour. What it cannot do is offer explanations for the minutiae of human history. For instance, evolutionary theory might be able to offer explanations of the human tendency to violence and explain in Darwinian terms the contexts in which warfare occurs (see Layton, this volume). What it cannot do is explain the causes of the First World War (or any other). For this one needs to understand specific social organisations, cultural traits and historical circumstances, which requires the kind of interpretive skills developed by historians. Another example would relate to the understanding of emotion, which has begun to be addressed by interpretive archaeologists in recent years (e.g., Tarlow 1999). Evolutionary theory certainly has things to say about emotions (indeed Darwin himself devoted a whole book to the subject, *The Expression of the Emotions in Man and Animals*, 1998 [1872]). It establishes that emotions are inherited and offers possible explanations of their survival advantages. What it cannot do is assess their contribution to culturally specific social practices or unique historical events. Still less can it tell us what it is like to experience these emotions in particular contexts, although evolutionists may believe that these are universal experiences that we already know as part of our membership in the species, ignoring any role that society, belief systems or contingent circumstances may make to the experience. Again, any kind of re-creation of the role of emotion in the past requires imaginative interpretation—outside the scope of evolutionary theory. A good example is provided by Tarlow's (2002) examination of changes in burial practices in nineteenth-century Britain, which saw an increasing preoccupation with the presentation of a beautiful corpse. Tarlow interprets this in terms of the body as the locus of identity and efforts made by the bereaved to extend their emotional relationships with the deceased beyond death. These examples could be multiplied many times over. In the end it all depends on what we are interested in. Mary Midgley (2001:71) puts it very well, and I make no apology for quoting this passage at length:

> These various ways of thinking are like a set of complementary tools on a workbench or a set of remedies to be used for different diseases. *Their variety is the variety of our needs*. The forms of thought that we need for understanding difficult social dilemmas are distinct from those that we

> need for chemistry and those again from historical thinking, because the questions that we must ask in these areas are of different kinds, though of course all these forms and all these questions are related in the context of life as a whole.

So, on this basis, we might argue that what we need is not actually reconciliation between two different approaches, but rather respect by each group of practitioners for the work of the other.

I would like to end on this conciliatory note, but I feel that I cannot finish without addressing one of the main foci of difference between the two approaches, which relates to methodology. It is sometimes claimed that evolutionary approaches are superior to interpretive ones because of their reliance on the scientific method, based on the production of testable hypotheses, whereas interpretive approaches are characterised by cultural and epistemological relativism, with interpretations that have no way of being validated beyond their ability to persuade. In their chapter in this volume, Colleran and Mace discuss this issue and argue powerfully for bridging the divide between the two viewpoints by the adoption of a shared methodology, which can then allow a correspondingly shared means of evaluating claims to validity. Such a methodology, in Colleran and Mace's view, should be 'scientific', defined as involving definitional clarity and the generation of hypotheses that can be tested. In some ways it is difficult to quarrel with this argument. All academic work should meet standards of rigour, such as internal consistency, logic of argument and appropriate use of evidence, but the issue of testability is contentious, since 'testing' means different things in different contexts. No hypotheses about the past can be tested in the sense that we can test propositions about the present material world (e.g., 'metals expand on heating'), that is by repetition of experiments, preferably in laboratory conditions. Nor can we use the sort of tests that we apply in contemporary medical or sociological research, where strict experimentation is impossible on ethical grounds, but we can nonetheless carry out repeated studies controlling particular variables. We cannot test propositions about the past in this way because the circumstances that produced the phenomena we wish to explain have gone and can only be reconstructed in part and with alternative explanations always possible. The kind of testing we can apply to hypotheses about the past, as Colleran and Mace make clear, is in terms of goodness of fit to the evidence. Interpretive archaeologists should have no problem with this kind of testing, which is part of normal academic practice. However, it is important to emphasise that because a hypothesis generates particular expectations, which are then supported by an examination of the evidence, this in no way 'proves' the hypothesis, since the same pattern of evidence could be explained by other hypotheses (possibly many other hypotheses). Thus

the explanatory aspect of evolutionary theorising, which is based on the *assumption* that evolutionary principles offer the best approach to understanding human behaviour, is no less a matter of interpretation than the various explanations offered by interpretive scholars.

The methodological differences between evolutionary and interpretive approaches are in fact directly related to the kinds of explanations being sought. If we are interested in what Colleran and Mace call 'the deepest possible drivers of culture' (this volume, p. 299), based on universal aspects of human nature, then the emphasis on quantitative studies and testing of data collected on a cross-cultural basis make good sense. If we are interested in human behaviour in specific cultural and historical circumstances, then thick description and the detailed examination of the unique evidence relating to those circumstances is appropriate. It really isn't a question of one approach being better than the other. This would be equivalent to saying that, for instance, physics is better than history and, if pursued to its logical conclusion, would exclude large sections of academe from acceptance as valid scholarship. Let us accept that in archaeology, as in the wider academic world, different approaches can cast light on different aspects of past societies, and then everyone can get on with the bits that interest them. If we can find the tolerance and interest to read the works of those adopting different approaches from our own, so much the better. Since archaeology aims to address past societies in their entirety, collectively we need all the methods and all the ways of thinking we can find.

Notes

1. This definition was originally formulated by Leslie White (1959:8) and subsequently adopted by Lewis Binford.
2. I take this from Clifford Geertz (1973:5). The precise quotation is, 'Believing, with Max Weber, that man is an animal suspended in webs of significance he himself has spun, I take culture to be those webs, and the analysis of it to be therefore not an experimental science in search of law but an interpretive one in search of meaning'.

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CHAPTER ELEVEN

Missing Links: Cultures, Species and the Cladistic Reconstruction of Prehistory

Jamshid J. Tehrani

Introduction: Artefacts as Species

In a famous passage in *The Descent of Man*, Charles Darwin speculated that his ideas about variation arising through descent with modification may apply as well to languages as species: 'The formation of different languages and of distinct species and the proofs that both have been developed through a gradual process are curiously parallel' (Darwin 2005 [1871]:676). Darwin proposed that languages could be classified into hierarchical taxonomic groupings similar to biological families, genera, and other taxa based on their genealogical relationships. This idea was taken up—or possibly even anticipated by—August Schleicher (1869), the founder of modern historical linguistics. Schleicher hypothesised that relationships among the Indo-European languages could be directly modelled on the kind of tree diagrams used by Darwin to depict the phylogeny of biological species (Figure 11.1). Thus, he suggested that they were all derived from a single common ancestral language that gradually differentiated into separate branches like 'Romance' or 'Germanic'. Many contemporaries of Darwin and Schleicher believed that the analogy between organisms and languages could be extended to other cultural domains. One of the most important of these figures was Augustus Henry Pitt-Rivers (Pitt-Rivers 1875, 1906), whose ethnographic collections were conceived with the express intention of demonstrating how the principles of evolution are borne out in tools, weapons and craft objects. As he explained, 'human ideas, as represented by the various products of human industry, are capable of classification into genera, species and varieties, in the same manner as the products of the vegetable and animal kingdoms, and in their development from the homogeneous to the heterogeneous they obey the same laws' (Pitt-Rivers 1875:307).

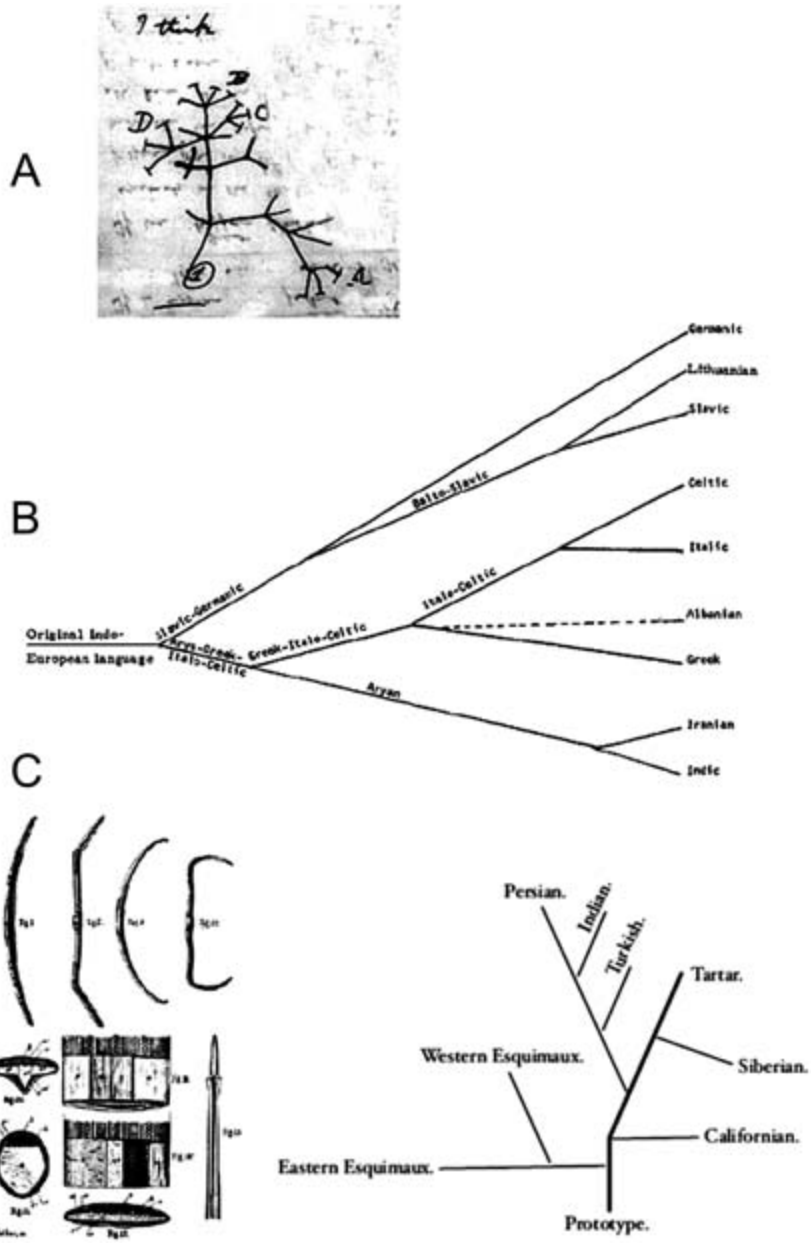

Figure 11.1 Branching lineages drawn by Darwin (1837) for species (a), Schleicher (1869) for Indo-European languages (b) and Balfour (1889) for cross-bows (c).

Pitt-Rivers's elucidation of these laws blended Darwinian ideas of 'descent with modification' with a number of now-discredited notions of the evolutionary process. Thus, he mistakenly characterises the history of life as 'a succession of gradually improving species' (1875:307) and the history of culture as a singular, predetermined course leading from 'primitive' hunter-gatherers to technologically advanced, socially complex 'civilisations'. Despite these flaws, many of Pitt-Rivers's ideas resonate loudly with modern approaches to cultural transmission in Darwinian archaeology (e.g., Eerkens and Lipo 2007; O'Brien 2008). Thus, he describes how artefacts are generally copied from other artefacts, just as biological individuals are copies of other individuals.[1] He also noted that copies may differ slightly from their model due to small errors or improvements in design and technique (see also Sillar, this volume). He compared these gradual changes in artefact form from generation to generation to the accumulation of small modifications that drive the evolution of species.

As with species and languages, Pitt-Rivers believed that it was possible to trace the development of artefacts that were widely distributed throughout the globe to their original 'root form'. For example, he argued that similarities among crossbows made in different societies across Europe and Asia suggested that these traditions were all derived from a single proto-crossbow. His theory was tested by Henry Balfour (1889), who literally dissected the crossbow collections with the methodical rigour that we might expect of someone who had first been trained in comparative anatomy. Balfour's efforts produced the first phylogeny of a material culture tradition that was explicitly based on the branching 'family tree' models employed by biologists and historical linguists (see Figure 11.1). Balfour's phylogeny supported Pitt-Rivers's notion that the geographical distributions of different bow types could be explained through descent with modification from a single original form. Balfour proposed a Central Asian origin for the bow, which was then adopted and successively modified by populations who adopted it as it spread north to the Arctic regions and then west into Siberia and across the Bering Strait into America, west to Persia and Europe, and south to the Indian subcontinent.

In addition to these similarities, Pitt-Rivers observed that reconstructing lineages of cultural 'descent with modification' presents the same kind of methodological challenges confronted by palaeontologists (Pitt-Rivers 1875:309). Foremost among these is the fact that the archaeological record and fossil record are equally patchy in their coverage of the prehistory of artefacts and organisms respectively. As a result, there is often little physical evidence for the ancestral taxa that are assumed to have existed in the past. Instead, the characteristics of

these 'missing links' can only be inferred from the traits exhibited by their (presumed) descendents. This brings us to another problem that archaeologists and biologists share, which is how to distinguish true 'family resemblances' that are the result of transmission from similarities that evolved independently. For example, similar morphological adaptations can be observed in species that almost certainly evolved subsequent to their last common ancestor (e.g., wings in bats and birds), just as similar technologies can arise in completely different cultural contexts (e.g., writing in the Middle East, China and Mesoamerica, or pyramids in ancient Egypt and Mexico). Another confounding factor is the transmission of traits across separate lineages, resulting in similarities among taxa that are only distantly related to one another. This can occur in many biological species, particularly in plants and microbes, and is likely to be even more common in cultural evolution, where trade and other forms of exchange can potentially lead to the widespread borrowing and blending of cultural traits (e.g., Moore 1994; Terrell 1988).

In the past, researchers like Balfour in anthropology or Schleicher in linguistics had to rely on their own (often highly subjective) judgements about sources of similarity and degrees of relatedness. However, in the last 50 years or so, biologists have developed a powerful set of tools to address similarity and relatedness in a more scientific fashion. Recently, a growing number of linguists, archaeologists and anthropologists have begun to explore ways of applying these tools, the modern methods of phylogenetic analysis, to reconstruct cultural prehistory. In discussing phylogenetic methods, I will focus on the technique that is most widely used in archaeology—cladistics.

Cladistic Analysis

Cladistic analysis focuses on variation in the constituent parts, or 'characters', of a group of taxa. In biological species, characters may comprise DNA sequences or morphological traits. In languages, characters are usually based on lists of words, such as core vocabulary items. In the case of material culture, the criteria for defining taxa and characters depends on whether the specific research question concerns relations between the objects themselves or the peoples that produce them. Thus, characters may consist of elements of the design or structure of technological classes, such as different types of arrowheads (O'Brien et al. 2001, 2002), musical instruments (Temkin and Eldredge 2007) or other artefacts. Or they may code for stylistic traits associated with the cultural assemblages of different populations (e.g., Jordan and Shennan 2003; Tehrani and Collard 2002, 2009a, 2009b). In either case, it is

worth emphasising that the lack of naturally bounded units in material culture does not undermine the applicability of phylogenetic methods. As O'Brien et al. (2001:134) have pointed out:

> Whether a tooth represents one or multiple genes—replicators—is as yet unknown, but this does not hinder the efforts of palaeobiologists to determine and explain the evolutionary histories of the organisms whose phenotypic hard parts they study Cultural traits conceived as ideas held in the mind of individuals are the replicators that are transmitted If there is phenotypic change, and if over time enough variation is generated, cladistical analysis might indeed be able to detect the phylogenetic signal.

Cladistic analysis reconstructs relationships among taxa or classes by distinguishing characters that are evolutionarily novel (also termed 'apomorphic' or 'derived'), from those that were present in the last common ancestor of all the taxa under study, which are labelled 'ancestral' or 'plesiomorphic'. The presence of a derived trait in two or more taxa provides evidence that they are descended from a common ancestor of more recent origin than the ancestors they share with the other taxa under analysis. In this way, cladistics enables us to infer the existence and qualities of ancestors even when there is no physical evidence for them in the prehistoric record. The myriad techniques and terminology of cladistic analysis have recently been expounded for archaeologists and anthropologists by O'Brien and Lyman (2003).

There are several methods to identify which traits are derived and which are ancestral, the most popular of which is outgroup analysis (e.g., Arnold 1981). An outgroup is defined as a taxon that shares a common ancestor with the taxa under analysis (the ingroup), but is of more distant origin than the ancestor that the analyzed taxa share with each other. Since the outgroup does not share an exclusive common ancestor with any individual member of the ingroup, it follows that when a character occurs in two states among the study group, but only one of the states is found in the outgroup taxon, the former is considered the derived state and the latter the ancestral state. Once the direction of change has been established for each character, the next step in a cladistic analysis is to construct a branching diagram that connects taxa according to their relative derived status. This diagram is known as a character cladogram. An example of a character cladogram is shown in Figure 11.2, which indicates how variations in a type of carpet ornament that is typical of the weavings of tribal Turkmen, called a *gul*, can be described in cladistic terms: the shape of the ornament is similar in all the taxa, but there are several differences in the interior design. In the outgroup taxon and the Tekke taxon, we can see what appear to be darts or birds protruding from the heart of the *gul*. In the three remaining

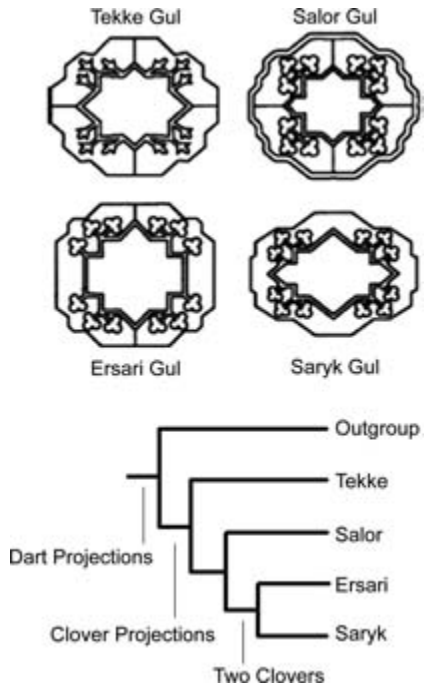

Figure 11.2 Character cladogram for a Turkmen rug ornament known as *gul*, which varies from tribe to tribe. Among the Tekke the ornament features dartlike projections in the interior. In the other three groups shown here, the projection takes the form of a clover with one stem (Salor) or two (Saryk).

taxa, these take a different form—that of clovers. Since the dart/bird form is found in the outgroup and the clover form is found only in the ingroup, we can infer that the clover evolved subsequent to the last common ancestor shared by the ingroup. In other words, the presence of the clover design provides evidence that the Salor, Saryk and Ersari share a common ancestor that is not shared with the Tekke. Studying the *gul* of these three taxa more closely, we can see that it is possible to make further distinctions. Thus, in the case of the Salor and Ersari, the clover is divided into two stems, whereas in the Saryk case the clovers have only one stem. Again, this suggests that the clover design has evolved in two forms. If we assume that the Salor and Ersari form is derived with respect to the Saryk form, then this would imply that they share a common ancestor that is not shared with the Saryk (although it should be noted that it is equally possible that the Saryk form is derived, in which case we cannot be sure that the Salor and Ersari are more closely related to one another).

Ideally, the distribution of the character states among the taxa will be such that all the possible character cladograms are congruent with one another. Normally, however, a number of the character cladograms will suggest relationships that are incompatible because, as noted earlier, common descent is not the only source of similarity among taxa. How can we sort true family resemblances (known in phylogenetic terms as homologies) from similarities resulting from other processes such as independent evolution and borrowings (homoplasies)? The cladistic approach deals with this problem by generating an ensemble or consensus cladogram that is consistent with the largest number of characters and therefore requires the smallest number of evolutionary changes to account for the distribution of character states among the taxa. This approach is based on the principle of parsimony, the methodological injunction that explanations should never be made more complicated than necessary (Sober 1988). Thus, Figure 11.3 shows an ensemble cladogram derived from five characters (1–5) exhibited in four ingroup taxa (A–D). The characters are binary and exhibit two potential states, present (0) or absent (1). For all the characters, the ancestral state is absent and the derived state is present. Characters 1 and 2 are present

	Character 1	Character 2	Character 3	Character 4	Character 5
Taxon A	0	0	0	0	0
Taxon B	1	1	0	0	1
Taxon C	1	1	1	1	1
Taxon D	1	1	1	1	0
Taxon X	0	0	0	0	0

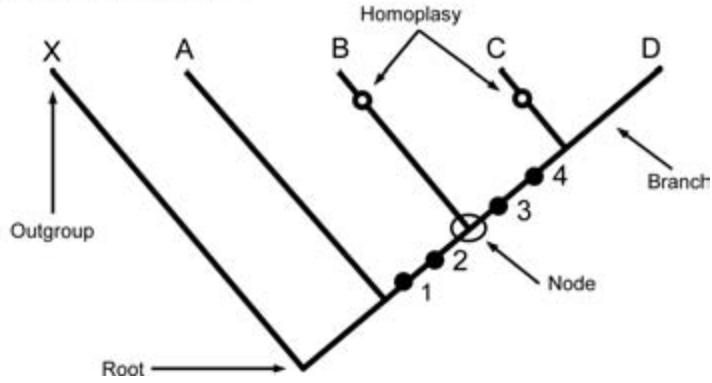

Figure 11.3 Example of a character matrix and the cladogram derived from it using parsimony.

in taxa B, C and D but absent in A. Characters 3 and 4 are absent in taxa A and B and are present in C and D. Character 5 is present in B and C and absent in A and D. The character cladograms for 3 and 4 are compatible with the character cladogram for 1 and 2. The cladogram for character 5 is also compatible with the cladogram for characters 1 and 2, but contradicts the cladogram for characters 3 and 4. Thus, while all the characters agree that taxa B, C and D share a common ancestor that is not shared with taxon A, they provide conflicting evidence as to whether taxon C is more closely related to taxon B or taxon D. Parsimony dictates that we should favour the latter hypothesis because it can account for a greater number of similarities among the taxa than the alternative, requiring only one additional evolutionary change (in character 5) as opposed to two (in characters 3 and 4).

As Lyman and O'Brien (2006) have pointed out, cladistics has important advantages over previous techniques for tracking continuities in artefact forms, such as seriation analysis. While there are several variants of seriation analysis, a general limitation of this approach is that it assumes taxa are related in a linear fashion (Lyman and O'Brien 2006). Cladistics, on the other hand, explicitly incorporates the possibility that taxa may comprise multiple parallel lineages. The latter arise when a tradition splits into two or more daughter traditions that in turn give rise to their own descendents. Artefacts that are distributed over wide areas, and which are likely to have been spread by diffusion or migration rather than the expansion of a single continuous population, are especially likely to follow these more complicated patterns of inheritance. For that reason, cladistics is increasingly used to reconstruct material culture lineages, although some researchers continue to use both methods in tandem (e.g., O'Brien et al. 2001, 2002; O'Brien and Lyman 2003).

Despite its advantages, cladistics has one major limitation—it is not able to easily represent transmission across lineages. Several researchers (e.g., Hurles et al. 2003; Temkin and Eldredge 2007) have therefore advocated alternative methods such as split-decomposition and neighbour-net analysis (Figure 11.4). These methods represent phylogenetic relationships as networks, rather than trees, which enables them to explicitly show multiple (and conflicting) character histories (cf. Cochrane and Lipo 2010). Although these techniques provide a useful way of visualising patterns in a dataset, networks involving many taxa can get very messy and are difficult to interpret. This is compounded by the lack of consistent or principled methods for discriminating true blending from the other forms of homoplasy discussed above. As a result, we cannot be sure whether resemblances that conflict with descent relationships are the result of trade, exchange and other processes, or independent inventions (Tehrani and Collard 2009a).

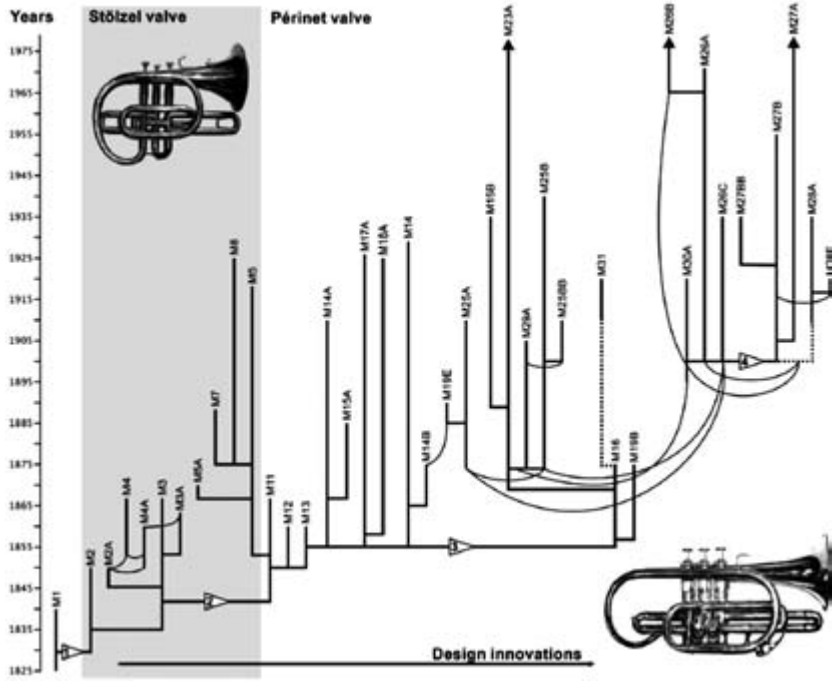

Figure 11.4 Reticulated phylogeny of cornets, showing major innovations (numbered triangles), splits, and borrowings (represented by the curved threads connecting branches) in the instruments' lineage. (Reproduced from Temkin and Eldredge 2007:149, Fig. 2.)

Another way of dealing with multiple character histories is through Bayesian phylogenetic analysis (e.g., Lewis 2001). This approach addresses conflicts within a data set by generating a sample of possible trees, rather than a single, most parsimonious tree. Trees are sampled in proportion to their probability, which is estimated in relation to prior knowledge about the evolution of the characters under study, such as different rates of gain and loss. Bayesian techniques are becoming increasingly popular in biology and are also making headway in studies of language evolution (e.g., Gray and Atkinson 2003; Gray et al. 2009). They have yet to be applied to material culture data because at present too little is known about which models can be applied to the evolution of stylistic and technical characters. Since those derived from molecular studies are unlikely to be appropriate, we will probably need to develop a new set of models that are specifically designed to capture the characteristics of cultural change. Debate about cladistic

methods continues in both biology and cultural evolutionary studies (e.g., Cochrane and Lipo 2010; Gray et al. 2007; Kelchner and Thomas 2007; Rieppel and Kearney 2007); in the meantime, parsimony-based cladistic analysis provides us with a principled and robust method for reconstructing descent relationships among artefacts that has been shown to be successful in a number of case studies. It is to these that we now turn.

Applications of Cladistics to Material Culture

Cladistics has been used to investigate multiple issues in the evolution of material culture. Several studies have used cladistically derived trees as a way of testing long-standing hypotheses concerning the origins and spread of technologies, including prehistoric stone tools (Buchanan and Collard 2007, 2008; Lycett 2007, 2009; O'Brien et al. 2001), ancient scripts (Skelton 2008), pottery (Cochrane 2008; Harmon et al. 2006), textiles (Tehrani and Collard 2009b) and musical instruments (Temkin 2004; Temkin and Eldredge 2007). For example, Lycett's cladistic analyses of Levallois cores found evidence to support the suggestion first put forward by Movius in the 1960s that these mode 3 technologies probably evolved from earlier mode 2 Acheulian handaxes in Africa (Lycett 2007). However, Lycett subsequently showed that—contrary to widespread belief—this shift did not occur via the so-called transitional (or 'proto-Levallois') cores found at Victoria West in South Africa, which do not appear to be ancestral to later Levallois cores (Lycett 2009).

Other studies have used cladistic reconstructions of material culture lineages to track the movements of populations that are associated with them. Buchanan and Collard (2007) analysed early Palaeoindian stone tools (Clovis points) to investigate the peopling of the Americas. They derived a phylogeny of tools sampled from archaeological sites across the continent and compared it to several colonisation scenarios that have been proposed in the literature. Their tool phylogeny was most consistent with the hypothesis that the Americas were colonised via the ice-free corridor that existed in the Northwest Coast 12,000 years ago. While Buchanan and Collard's study examined the dispersals of entire populations, cladistics can also be used to investigate the movements of individuals between populations. A recent paper by Cochrane (2008) analysed pottery produced in prehistoric Vanuatu and Fiji as a means of estimating migrations between the two archipelagos. Migration events were inferred from clades indicating that a ceramic type produced in one location was descended from a type produced in another location, rather than from a local ancestor. Although it is possible that not all these instances involved migrations of actual people (as opposed to their ideas

and products), some were supported by independent evidence provided by skeletal studies.

A third group of studies has used cladistics to investigate patterns of cultural transmission among populations (e.g., Collard et al. 2006; Collard and Tehrani 2005; Jordan and Mace 2006; Jordan and Shennan 2003; Shennan and Collard 2005; Tehrani and Collard 2002, 2009a, 2009b). Specifically, these studies have tested whether similarities and differences among the cultural assemblages associated with different groups can be best explained by descent with modification from common ancestral populations ('phylogenesis'), or borrowing and blending among neighbours ('ethnogenesis'). The relative contributions of these two processes can be estimated by examining how well the distribution of character states fits the most parsimonious tree(s) obtained from the assemblages. Under phylogenesis we would expect the history of most traits to be highly correlated with one another, resulting in a good fit with the most parsimonious tree(s). If, on the other hand, populations frequently borrow and blend cultural traits, this would result in more complex and conflicting distributions of similarities and differences and a correspondingly high number of homoplasies in the data, for example, as cultural similarities shared by populations after they diverge from a common ancestor. Further assessments regarding the roles of these processes can be made by comparing the most parsimonious tree(s) to other data on the descent histories of populations, such as their linguistic affinities (e.g., Jordan and Shennan 2003).

One of these studies was carried out by myself and Mark Collard on Turkmen weavings from Central Asia (Collard and Tehrani 2005; Tehrani and Collard 2002). We analysed weavings produced during two distinct periods of Turkmen history. The first period was prior to the military conquest of the Turkmen by Imperial Russian forces in the 1880s, during which time the Turkmen practiced a nomadic-pastoralist lifestyle. The second period was after the Russian colonisation, which led to the forced settlement of the Turkmen and the increasing commercialisation of craft production. Weavings made during this period can be identified by the use of synthetic dyes as opposed to ones extracted from local plants and insects. Cladistic analyses of rug ornaments such as the *gul* design described earlier (see Figure 11.2) suggested that phylogenesis dominated the evolution of Turkmen rug traditions in both periods. However, the number of homoplasies increased with the inclusion of weavings made in the later period of Turkmen history, suggesting that borrowing among groups may have increased. This is compatible with historical evidence that previous barriers to intertribal cooperation, in particular warfare, were removed following the conquest of the Turkmen, while the growing commercialisation of craft production may have encouraged weavers to adopt designs from the

most successful groups (Tehrani and Collard 2002). This illustrates that while cladistic analysis may have originally been developed to solve problems in evolutionary biology, it can provide rich insights into social history too.

Artefacts as Species Revisited

The adoption of cladistic approaches in archaeology can be seen as the belated fulfilment of Pitt-Rivers's ambition to incorporate 'the science of the arts' within the same evolutionary framework as biology and linguistics. The failure of this vision to materialise earlier can be attributed to the critique of nineteenth-century anthropology developed by Boas, Kroeber and others in the early twentieth century. They argued that the laws of cultural inheritance are fundamentally different from the laws of biological inheritance: whereas physical traits can only be transmitted 'vertically' from parents to their offspring, cultural traits can be borrowed 'horizontally' from any number of sources. Moreover, while members of other species are not usually able to interbreed with one another, there are no inherent constraints on communication among humans belonging to different social groups. Thus, Boas proposed that

> animal forms develop in divergent directions, and an intermingling of species that have once become distinct is negligible in the whole developmental history. It is otherwise in the domain of culture. Human thoughts, institutions, activities may spread from one social unit to another. As soon as two groups come into close contact their cultural traits will be disseminated from one to the other. (1940:251)

This contrast was famously depicted by Kroeber in his diagram (Figure 11.5) showing how the branches on the tree of life grow and then split, whereas those on the tree of culture are tangled together and often merge.

The rejection of the idea that cultural history followed natural laws was a crucial development in anthropological and archaeological theory. It led many researchers to abandon comparative analyses of relationships among artefacts sourced from different societies and periods, to focus instead on how they are produced, used and exchanged in their immediate local contexts. However, the continued suspicion of biologically inspired approaches to artefact analysis (e.g., Moore 1994; Terrell 1988) has proved to be a major hindrance to dialogue between interpretive and evolutionary archaeologists. This is unfortunate, since it lacks a firm empirical basis. As we have seen, researchers have successfully used cladistics to reconstruct coherent lineages of cultural descent with modification for a wide range of artefact assemblages.

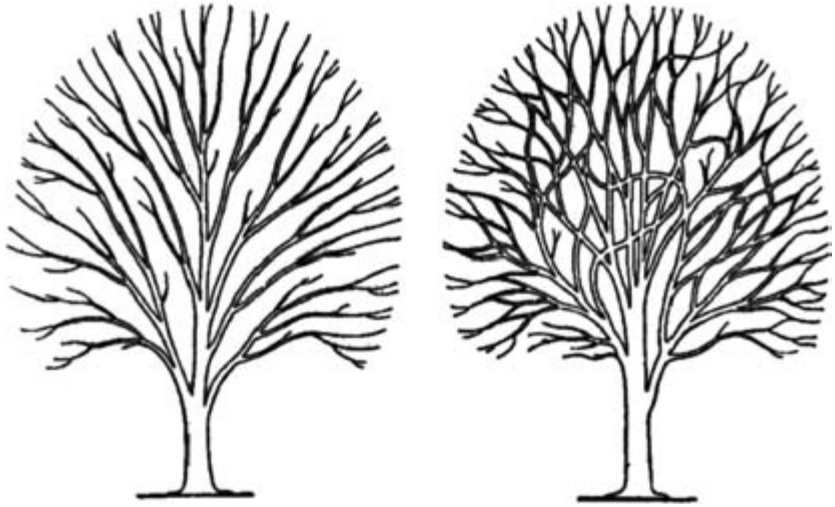

Figure 11.5 Kroeber's 'The tree of life and the tree of the knowledge of good and evil—that is, of human culture' (Kroeber 1948).

The longevity of these traditions is consistent with ethnographic data on traditional craft apprenticeships, which ensure that skills are usually transmitted with a high degree of fidelity from one generation to the next (e.g., Tehrani and Riede 2008). In many small-scale societies, the transmission of these skills is mainly 'vertical' from parents to offspring (Shennan and Steele 1999). However, even when individuals do acquire traits through 'horizontal' processes, this does not automatically lead to widespread borrowing and blending among traditions. In fact, a recent study by Mark Collard and myself on the transmission of weaving knowledge in southwestern Iran (Tehrani and Collard 2009) showed that the phylogenetic signal of traits that are learned and passed on among peers can be just as strong as that of vertically transmitted traits. This is because it is easier to access knowledge from members of the same community than from members of different communities. Whereas horizontal transmission among members of the same group is facilitated by the members' physical proximity, common language and shared cultural norms, communication among members of different groups is often impeded by the existence of ecological boundaries, language barriers, endogamy and xenophobic prejudices (e.g., Barth 1969; Durham 1990, 1992; Gil-White 2001; McElreath et al. 2003). Consequently, despite the clear differences between cultural transmission and genetic transmission at the individual level, cultural evolution at the level of the group often appears to be remarkably similar to the evolution of species diversity (Collard et al. 2006).

Nevertheless, it is clear that the relative inputs of ancestral versus neighbouring societies varies in different regions and periods, as illustrated by the Turkmen case discussed above (Tehrani and Collard 2002). The roles played by specific social institutions, cultural norms and preferences in determining this variation have so far received limited attention by Darwinian archaeologists. Further elaboration of their importance will require greater integration of phylogenetic and social-historical and interpretive perspectives. Related to this issue is the question of why some aspects of artefacts seem to be preserved so faithfully when others are apparently discarded or borrowed much more readily. To give an example, Jordan and Shennan (2003) report that while indigenous Californian basketry traditions evolved primarily through ethnogenesis, some traits (i.e., twining techniques) appear to have been mainly inherited from ancestral populations. In the absence of any obvious intrinsic factors that might explain these different transmission patterns, we are entitled to ask, why were weavers less accepting of other tribes' twining patterns when they were otherwise so open to foreign influences? Again, further investigation of these kinds of questions must include more detailed contextual analysis of craft production in its broadest sense—including material, sociological and symbolic dimensions (see Sillar, this volume). Another issue for future research concerns how different traditions relate to one another. Many artefacts have complementary functional or reciprocal relationships with one another through their integration into specific technological and symbolic systems. As a result, the evolution of one type of artefact may be constrained by another, as Ortman (2000) demonstrated in his elegant analysis of skeuomorphisms in Mesa Verde pots made in the Great Pueblo period, which are clearly derived from contemporaneous textile designs. Similarly detailed qualitative analysis has the potential to greatly enrich phylogenetic approaches to the coevolution of artefacts (e.g., Jordan and Mace 2006; Riede 2009) and vice versa. Interpretive and Darwinian archaeologists both have much to gain from working together on these topics, which provide an immediate context for discussing the missing links between their respective approaches.

Note

1. One may object that whereas genetic mutations are random, cultural change is often guided by intentionality. However, as Mesoudi, Whiten and Laland have pointed out, we should not overestimate the importance of intentionality as a proximal source of variation in culture since 'ultimately it matters less to the Darwinian process how variation arises than that variation exists and is exposed to selection' (Mesoudi et al. 2004:4). In any case, the distinction would be lost on Pitt-Rivers, who was unaware of

the mechanisms of genetic inheritance and largely subscribed to Lamarck's theory that organisms could pass on their acquired characteristics to the next generation.

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CHAPTER TWELVE

The Ambiguity of Landscape: Discussing Points of Relatedness in Concepts and Methods

Sue Hamilton

Introduction: A Slippery Concept

> I knew my boundaries—the garden was 'mine', the hedges were 'ours'—'we' cut them, the fields were our taken right to go into—they touched our hedges and the resident cows chewed them. By contrast the deserted railway line was a vegetated tangle of unknown ownership beyond the fields and was more problematic—'was it safe? I could not be seen'. Beyond was the forest and sandstone ridge, a vista that belonged to me from my daily angle of vision sitting on the back door step, but was a place only once visited. To this day I do not know the actual size of this viewshed, the extent and quality of the tenant farmer's land or the scale of the estate that it was situated in, or the most energy-efficient path to the nearest hamlet, and I often wonder if it would have made any difference if I had been an adult. What I am clear about is that what I understood and now describe as a home territory is a landscape where land-nature-culture and space coalesced into a heterogeneous whole that was coterminous with the context of observation and experience. (S. Hamilton, personal memories of a childhood landscape)

At its broadest, this chapter is about making sense of human actions at a terrain-based scale. That landscape archaeology is a subdiscipline of archaeology is self-validated by the existence of Master's programmes and Chairs in landscape archaeology in UK universities, yet what a landscape *is* is a slippery concept. On the one hand archaeological literature has sacrificed acres of trees in analysing the parameters and contents of landscape archaeology, while on the other its practitioners recurrently use the word 'landscape' with an abandonment that apparently requires no explanation and perhaps has now reached its peak of application in

archaeology. The multiplicity of work and breadth of application that takes place within the domain of landscape studies proffers a plurality of methods, explanations and interpretations that have been variously crafted to the purposes of the archaeological constituencies that embrace it. In contemporary archaeology 'landscape' has become a 'terrain in which highly evolved empirical methodologies confront conceptual approaches' (David and Thomas 2008:25), in which a rift is evoked between interpretive archaeologies of landscape and the strongly quantitative- and predictive-oriented approaches of processual and evolutionary archaeologies. In this chapter I aim to explore the nature of these supposed differences in major traditions of archaeology—variously drawing upon examples from the UK, Continental Europe and the Pacific. In doing so I arrive at making a particular case for the key role of a phenomenology of landscape that goes beyond the parameters of phenomenology's now traditional deployment in archaeology.

Landscape Archaeology: From Picturesque to Empirical

'Landscape archaeology' is a hackneyed, anodyne category, and yet at the same time it has spawned a huge range of approaches and perspectives on emplaced human action. The ubiquity of its use as a term has resulted in it encompassing almost any study that considers the pattern of human activity on the earth's surface at a topographic scale usually greater than the location of a single site. There are differing national traditions and trajectories of landscape archaeology. Landscape archaeology in Britain has its genesis in W. G. Hoskins's seminal *The Making of the English Landscape* (1955), and subsequently in the work of Michael Aston and Trevor Rowley (1974) and in Michael Aston's (1978) *Interpreting the Landscape*, with their application of a landscape-based methodology to the study of medieval boundaries and field systems. These works expounded the use of field observation and aerial photographs to 'read' the palimpsest of earthworks comprising the land surface, isolate patterns and diachronic sequences, and facilitate an understanding of the historical evolution of the English landscape. Hoskins's work offered a vision of an England with its spiritual roots in the countryside where the preindustrial character and modification of the landscape was identity affirming. In this vein his writing fed upon a nineteenth-century preindustrial Romanticism, strikingly present in the works of the English poet William Wordsworth, that had its origins in the eighteenth century and Gilpin's *Observations on Several Parts of England*, with its analysis, description and illustration of 'picturesque beauty' (Gilpin 1808, published some 15 years after it was first written). Something of the flavour and essences of these studies and evocations of landscape remain

in or have been resurrected by current UK landscape studies. Indeed, it is one of the strengths of an ongoing identification and field mapping of upstanding archaeological features and monuments on a regional scale. It is present in the thick description and metaphor-laden writing and illustration of the qualitative aspects of these and their associated landscape forms and context that postprocessual and phenomenological studies of landscapes espouse, and it is echoed in the ongoing debates about the interpretive representation of landscapes using mapping, line drawings and photographs, and in some instances installation art and landscape performance art (Tilley et al. 2000).

From the mid-1970s, landscape studies focused on the landscape as a provider of resources for human subsistence needs, and period- and place-specific patterns of settlement and land use were isolated. In particular the 'New Geography' of the 1960s and 1970s provided models and mapping methodologies for characterising and quantifying the economic potential and scale of the territory surrounding a settlement in terms of site catchments and the application of geometric tessellation techniques such as Thiessen polygons (Haggett 1965). Alongside this, it incorporated the use of the predictive modelling of fall-off patterns of artefact densities from source areas and production centres using techniques such as trend surface analysis (Hodder and Orton 1978). Collectively, these approaches isolated and generated new understandings of the nonrandom spacings of sites and artefact densities on a regional and geographically extensive scale, such as the distribution of hillforts in central southern Britain (Cunliffe 1971), and distinctive fall-off patterns from source areas such as Neolithic stone axes (Cummins 1980) and Baltic amber in Late Neolithic and Bronze Age Europe (Shennan 1982). Such work inevitably had explanatory outcomes relating to the subsistence strategies and/or associated socioeconomic structures. A specific fieldwork outcome of the idea that sites have mapable subsistence territories was Claudio Vita-Finzi and Eric Higgs's (1970) development of the technique of site catchment analysis. The catchments were ascertained by walking out in each of the cardinal directions from the centre of a site for the exploitable distance that was viable in a day's walking (up to approximately 5 km for a farming settlement). Site catchment studies were subsequently carried out on a regional scale, for example, in the field on the numerous Neolithic ditched enclosures of the Tavoliere Plain of southern Italy (Jarman and Webley 1975) and, using a combination of map work and field observation, for the Neolithic causewayed enclosures of southern Britain (Barker and Webley 1978; Drewett et al. 1988). While the economic viability of sites was the primary concern of site catchment analysis, its emphasis on emplaced investigations of a site's surroundings prefaced a vein of postprocessual landscape archaeology

in not only connecting sites to places but in involving researchers in physically walking through the landscapes of their focus sites.

Within a decade these modelling methods had become less fashionable, but there was a growth in the number and scale of regional field archaeological programmes involving detailed mapping of archaeological landscapes by traditional survey techniques and walk-over survey. In Britain many of these encompassed upland landscapes, beyond present-day settlement, which have large tracts of upstanding archaeology including megalithic monuments and stone-walled field systems, enclosures and houses—notably Dartmoor, Bodmin Moor and the Derbyshire Peaks. At the same time, the first explicit regional archaeological projects were undertaken in Mediterranean Europe involving large-team systematic field surveys, surface collection of artefacts and excavations, the first of which was Ward Perkin's 1950s–1970s South Etruria survey of surface evidence (Potter 1979). Subsequently, extensive field surveys were undertaken in south-central and northern Greece and on Crete, in which land surfaces were 'combed' in transects for evidence of surface finds, settlements and land-use patterns (Barker 1996). This tradition of large-scale surface survey has continued and increased in recent decades in landscape archaeology with the use of Global Positioning Systems (GPS), and total station survey techniques. In Britain the advent of total station survey allowed the Royal Commission on the Historical Monuments of England (RCHME) to initiate programmes to remap major categories of field monuments such as Iron Age hillforts and Neolithic causewayed enclosures in increased topographic detail. Concurrently, some of these surveys have employed phenomenologically oriented, emplaced interpretation—notably those of Oswald and McOmish as RCHME field investigators (e.g., Oswald and McOmish 1995). The latter works highlight the interpretive nature of all forms of field-based mapping, mapping choices being reliant on 'seeing' features and considerations of what is topographically important. In the case of total station survey, mapping requires the choice of discrete points from which to take the mapping readings. In the context of excavations, a related debate has arisen over the use of total stations for on-site planning of excavated features and how this separates the planning from the excavator's process of interpretation. In modern contract archaeology these tasks are separated in terms of personnel and work sequence (Edgeworth 2003). This is a product of systems of organisation rather than an intrinsic incompatibility of digitally mapping sites and interpreting sites through aspects of bodily presence.

From interpretive perspectives, however, Western traditions of cartographic mapping, and New Archaeology's use of spatial models to characterise the placement of sites in landscape space, are mostly

seen as the objectification of land/landscape. This objectification has a long historic pedigree back to the written record of the Domesday Book, Saxon charters and the early medieval mapping of land to clarify and allot ownership and administrative units. From the twentieth century, access to aerial photographs, and the modern development of digital mapping using earth observation satellites positioned many thousands of kilometres above the earth, have greatly magnified this sense of landscapes being appropriated by outsiders and being placed in a detached dimension of analysis that is the antithesis of how most individuals think of the landscapes that they inhabit. This outsider's perspective is variously characterised as a 'bird's-eye view', the viewpoint of an extraterrestrial spying device, 'God's-eye view' or Thomas Nagel's (1989) 'the view from nowhere' (Thomas 1993:21–25).

On the Cusp: Modelling Landscape Patterns and Trends in the Late Twentieth/Early Twenty-First Century

Since its emergence in the 1950s, GIS has facilitated the development of predictive models concerning the location of sites and the physical parameters that most correlate with these locations. GIS uses continuous distance and elevation data provided by automated mapping systems and displays variables of a different nature on separate maps or combined on maps as overlays. This results in the production of thematic landscape maps, which many have argued present a further level of 'cold and distant vision' and a detachment in understanding from the reality of the worlds in which communities and people lived because they rely on rationalised perspectives that were never available in their entirety to premodern communities. At the same time, critics of GIS frequently suggest this to be implicit environmental determinism due to the use of information from preexisting empirical mapping traditions. However, as Llobera states, this data has no inherent deterministic properties—rather it is a specific incorporation of environmental information, and there is nothing *contra* incorporating the results of other ways of studying landscape into GIS analysis, including person-centred perspectives of space (Llobera 1996). Mark Gillings, particularly, is beginning to consider the range of sensory influences affecting interpersonal and intercommunity spatial relations and their possible accommodation in GIS mapping (Gillings 2007a, 2007b). This has resonances with the phenomenologically oriented sensory fieldwork of Hamilton and Whitehouse (2006) discussed below, and vice versa. As Wheatley (2004) points out, GIS has no hidden agenda and is dependent upon the intellectual milieu of its application.

The computer has in addition provided the tools for dealing with immense databases and identifying multivariate patterning and relationships at the spatial, temporal and organizational level of groups in the landscape. This was particularly prevalent in the processual archaeology of Britain in the 1970s and still remains present in American traditions of landscape study (e.g., Cochrane 2002). These quantitative data methods produce generalised patterns—often geographically discrete clusters of shared attributes or monument types—sometimes interpreted as indicative of dominant groups and their territories (McCoy 1979). These offer a top-down interpretation of the past rather than place-specific interpretations.

The large-scale data processing provided by computing is likewise central to evolutionary archaeology, which has risen to prominence over the past 10 years. Evolutionary archaeology's contribution to landscape studies centres on the explanation of larger-scale material cultural and behavioural trends in space. It takes its inspiration from the structures, concepts and methods used in evolutionary biology. In particular, it uses phylogenetic techniques (branching diagrams) to explore the evolutionary relationships of populations and their quantitative impact on chains of social information transmission and the processes and characteristics of decision making. The cultural 'learning process' associated with exploration, uptake and adaption to new landscapes during the initial colonisation process and beyond is thus modelled in terms of the social transmission of information, the ability to use information from previously inhabited landscapes, and adaptation to the preexisting architectures of human presences in a landscape (Rockman 2003; Rockman and Steele 2003). In this vein evolutionary archaeology can deal with geographically and temporarily extensive processes such as those associated with the spread of agriculture in Europe (Collard et al. 2008). In a landscape context, evolutionary archaeology thus seeks to furnish us with unsentimental lessons of 'how and why we lived where we did' in terms of measurable gains and chains of knowledge perpetuation (Rockman and Steele 2003: back cover). In eschewing considerations of culturally specific beliefs and practices in landscape use, it forfeits potentially productive opportunities for interface with interpretive archaeology and that field's finer-grained consideration, rather than a general prediction, of why we do what we do.

Other recent studies that employ more eclectic means (traditional field survey, the utilisation of information from sites and monuments records, and collation of data from multiple preexisting field surveys) to explore past landscapes through some considerable time depth and differing timescales are inspired by the historians of the French *Annales* school. Landscape studies in this tradition simultaneously consider the

long-term socioeconomic processes underlying specific patterns (the *longue durée*) and also the underlying shorter-term, fluctuating patterns. At the same time an interpretive perspective is recognised in the interplay of long-term processes with the concept of *mentalités*—the attitudes and ideational understandings of the past. An excellent example of this is Fokke Gerritsen's study of the Bronze and Iron Age communities of the Meuse-Demer-Scheldt region of the Netherlands and their relationship with landscape, and the long-term impact on landscape organisation (Gerritsen 2003). This work draws together many different threads of investigation and can be seen as uniting both processual and interpretive approaches, particularly drawing upon Ingold's (2000) perspective of 'dwelling', but focusing on communities rather than individuals (Thomas 2008:302). I would see this as a means of embracing economic and socioideological factors within a single study tradition that provides a nuanced understanding of the development, use and meaning of past landscapes.

Interpretive Landscapes

Postprocessual archaeology's strongly oppositional stance to empirical methodologies of landscape archaeology has led it to characterise processual archaeology as treating spaces as passive media and reducing human behaviour to generalised normative elements. As for evolutionary archaeology, postprocessual archaeology has quite simply not engaged with it, due to a lack of sympathy with its predictive methodologies and what it would see as a wholly pragmatic consideration of landscape. Empirical and pragmatic approaches to landscape are aligned with a Western perspective that has prevailed since the seventeenth century, that land ownership (tenure) is an economic rather than a philosophical concern. What such perspectives sidestep is the idea of landscape agency with humans involved in making decisions that are positively aligned with actively and creatively interpreting the specific world around them. Human landscapes are experienced in these ways in the present and in numerous anthropological examples, so why ignore it in the past? (Rainbird 2008:268). Since the late 1980s, preeminently in British archaeological traditions, there has been a major engagement with person-centred interpretations of landscape. This form of peopling the landscape has spawned numerous conceptual variants such as 'contested landscapes' (Bender and Winer 2001; Jarman 1993), 'gendered landscapes' (Edholm 1993), and 'sacred geographies'. The latter in particular draws upon the Australasian Dreaming cosmologies that formalise land and seascapes into 'spiritscapes' and imbue natural features with the visual manifestations of ancestors and with supernatural

properties (McNiven 2003). The intricacy of Australasian indigenous traditions was acknowledged as a key inspiration for Tilley's (1994) *Phenomenology of Landscape*, whose applied focus is on the Neolithic monumental ritual landscapes of Wales and south-central Britain. The myths and ethnographies that lie apart from modern Western cultural traditions provide good reason for suggesting that archaeologists should study unaltered landscape features systematically. Oceanic ethnographies and the rich traditions of Polynesian cognitive culture, notably those of Hawai'i, have at their essence a kinship between human and non-human forms, with every natural phenomenon and form of life being an embodiment of a particular god (Bacchilega 2006, see esp. Chapter 2).

A seminal development in recent years in UK and European studies is the uptake of the idea that natural places, often striking features of the landscape such as rock outcrops, distinctive trees and the like, have specific meanings (Bradley 2000; Tilley et al. 2000). As Bradley states in his discussion of the landscape traditions of the Saami and neighbouring groups of hunter-gatherers of northern Scandinavia, their sacrifice sites are components of a mythical landscape and are situated in liminal geographical positions at the edges of different mythical worlds quite separate from those encountered in field archaeology (Bradley 2000:13). The extension of archaeological landscape studies to a deeper cultural consideration of natural places and landscape features requires not only that natural features are acknowledged as potentially ideologically meaningful but that they are considered as legitimate targets for archaeological survey and excavation (see below). A further development of these approaches is the framing of landscape not solely as the provider of the raw materials of survival but as a nexus of elemental substances that mediate cultural existence. There has been a focus in British Bronze Age archaeology, for instance, upon elucidating the traditionally conceived elemental constructs of earth, fire and water as metaphorically associated with places and actions and life cycles (Brück 2001; Owoc 2002). In a landscape context, these approaches have perhaps been most extensively developed for water (Strang 2008). For example, European Bronze Age metalwork is both metaphorically and physically returned to liquid in its tradition of being deposited in rivers and lakes (Stevens 2008). Water can be conceived of as the substance of life-changing journeys, as with Polynesian voyaging (Richards 2008), and 'abnormal' water, such as stalagmites and stalactites in southern Italian cult caves, was a focus for rituals (Whitehouse 1992). Human engagements with vital and metaphorical landscape substances are registered as creating intense and transformative sensory experiences. Thus, a practical landscape exploration of these phenomena is validated.

The role of phenomenology in investigating sensory experience has possibly attracted the most extreme hostile reaction of all variants of postprocessual archaeology—yet to me phenomenological approaches have great potential to create productive bridges between empirical and interpretive stances. In particular, I see this in the use of phenomenology as a method of field enquiry. This is the theme I will now consider.

Field Phenomenology: Scramblings on Foot?

> Poets make the best topographers. (Hoskins 1973 [1955]:17)

I here make a case for the cohabitation of phenomenology and sensory interpretive perspectives in landscape projects that involve various scientific and empirical forms of surface and below-surface fieldwork. Recognising phenomenology and aspects of sensory culture as central to fieldwork requires experimentation with, and development of, new forms of cross-skill articulation.

In archaeology, field phenomenology is essentially a method of enquiry. The human body is its research tool and enquiry is undertaken through 'my' or 'your' personal actuality of 'being in the world', or more pragmatically, the researcher being in the field. Work should be commenced without prior assumption; the objective, according to Tilley, is to provide rich description of experiences of the sensed, carnal body of the fieldworker in a way that allows others to understand the nuances and complexity of the landscape and the journeys through the landscape undertaken by the researcher(s). Practitioners of phenomenology are often accused of not making their methods clear, although this is a gross misrepresentation (Cummings 2004; Hamilton and Whitehouse 2006; Tilley 2004, 2008). The product of embodied enquiry is expressed by Tilley as being reliant on detailed penning of experiences on paper/notepad and is contrasted with the evils of using highly technical landscape recording and investigating equipment (unspecified but by implication total stations, geophysical prospecting equipment, compasses and the like), which mediates and dulls experience, or the abstracted outside perspective of statistics and computation (Bender et al. 2007:51; Tilley 2008:271). In this oppositional vein, in which Tilley delights, the need for methodological integrity is insinuated into phenomenology, potentially making combination with these other forms of landscape investigation conceptually impossible. In actuality this is not really the case, since phenomenological survey has been used within the frame of larger field projects and inevitably draws in varying measures upon already mapped and identified locales (e.g., The Stonehenge Riverside Project, http://www.shef.ac.uk/archaeology/research/stonehenge; The Bodmin Moor Stone Worlds

Project, Bender et al. 2007). Fleming, who is every bit as oppositional as Tilley, has savagely characterised phenomenology and its interpretive outcomes as 'dreaming too far', stating the inadvisability of ditching 'the heuristic, argument-grounded strengths of conventional landscape archaeology' (Fleming 2006:267). However, it is an oversimplification to understand the two positions as wholly separated. For instance, we must recognise that many of the traditional perspectives of British landscape archaeology are based on simple phenomenological experiences such as the visual dominance of topographically false-crested prehistoric long mounds and Bronze Age round barrows or the apparent placement of specific monuments to be intervisible. The perceived need for encounter through 'laborious scramblings on foot' of traditional field landscape study (Hoskins 1973 [1955]:15) is mirrored in the central tenet of phenomenological methodology as being deep familiarisation with landscape: 'To be a good phenomenologist is to try to develop an intimacy of contact with the landscape akin to that between lovers' (Tilley 2008:275).

Phenomenology's integrity of field practice is not hermetic, in spite of insinuations otherwise. It does not in actuality ignore the evidence of the environmental sciences or the fundamental question of what period-specific landscapes looked like—or at least look like in pollen diagrams (Cummings and Whittle 2003; Tilley 1994:83; *contra* Chapman and Geary 2000). What is more at issue is how we familiarise ourselves with landscape in the field, and how we sequence our enquiry and conjoin it with interpretation. Phenomenology involves coming to an understanding of meaning through primary in situ experience of the sensory multiplicity of landscape, our embodied reaction to both cultural and natural features, and the juxtapositions between the two. It relates to the way in which our bodies understand the scale and interrelationships of the full sensory ranges of landscapes. It is not necessarily antisocial. It can involve working alone or perhaps with a discussant or scribe. It can also involve working in groups, which, in my experience, more directly leads to an exploration of aged and gendered perspectives and also the development of recording formats and methods that allow for a pooling of information between separate workers. All phenomenology is fixed in space and variously generates, both in the field and in publication, forms of description and notation that allow for subsequent assessment by others of perceived patterns and interpretations. This often includes diagrams of viewpoints, representations of journeys, tables and *pro forma* records. Whether phenomenology in the prosaic guise of *pro forma* records and tabulation remains phenomenology in a 'Tilleyesque' poetic sense (Tilley 2008) may be open for discussion, but still at its core there remains a focus on

understanding landscape through embodied experience and through human physical competences.

Within the context of wider landscape projects, phenomenology ideally comes before targeted searching, mapping and excavation, so that its interpretive perspectives productively inform and widen the agenda of the subsequent work. This makes the product different from traditional field archaeology, which either tests preformed hypotheses or has interpretation being separated temporally and subsequent to the mechanics of mapping, excavation and recording. On the Bodmin Moor Stone Worlds Project, the consequence of having phenomenology as a primary method of enquiry was the excavation of a prominent natural feature (an upright, lozenge-shaped stone that we named the 'shrine stone') that had been influential to our sensory understanding of the Bronze Age landscape and archaeology of the Moor (Bender et al. 2007). Our excavation of the 'shrine stone' was unable to demonstrate—due to the absence of any finds of Bronze Age artefacts—that Bronze Age communities actually used this stone. Instead, we revealed distinct geomorphological patterns of stones resulting from their downhill movement, upending and sorting. Elsewhere in our excavations we recognised similar prominent upright stones and associated stone patterning as having been repeatedly selected and—in our phenomenological understanding of the Moor—used as a purposeful appropriation of the symbolic power of the stones for a key architectural element of the Bronze Age houses, namely, the prominent 'backstone' in the centre back of the house wall, opposite the entrance. Here phenomenology was not a stand-alone methodology but part of interdisciplinary consideration of the hill and its stones, involving a collaboration between excavation, phenomenology, traditional surveying and geomorphological expertise (Hamilton et al. 2008).

To ignore phenomenology and interpretive stances would be to believe that landscapes are *only* understood by their inhabitants in terms of economic pragmatics—and this seems improbable—or that there are no possibilities of exploring prehistoric past beliefs and worldviews, or that the fact that we have bodies and senses has no impact on how we function in the world. While phenomenology provides an ego-centred worldview, its method involves both static and perambulatory engagements with space and landscape. It thereby has the ability to reveal and separate centred understandings of the world, which see the world from the viewpoint of a centre landscape point, home or ritual monument, from 'decentred cosmologies', where cultural landscapes are based on understanding through movement and travel (Kristiansen and Larsson 2005:359). It this respect it has the potential to isolate the breadth of scales of landscape patterning that are equally associated with more conventional large-scale field and mapping projects.

All discussions of phenomenology note a Western emphasis on simple vision and recognise that field phenomenology should involve other sensory considerations such as physical effort, colour, the reflective qualities of textures and surfaces, sound, smell and sensory deprivation to describe and explore meaning through experience. Ingold (2000:265) puns on the 'groundlessness' of modern Western sensory precepts, but it is possible to better educate ourselves on the diverse qualities of sensory experiences. The sonic qualities of landscape have in particular been more recently investigated—including experimentation with the sonic properties of rocks in the Bronze Age landscapes of the Preseli Mountains in Wales and experimentation with the acoustics of Orkney's Neolithic chambered tombs (see Watson 2006). In redressing this emphasis on vision, phenomenology can also work within the realms of experimental archaeology. The Tavoliere-Gargano Later Prehistory Project (southern Italy) has, for instance, experimented with the ranges over which awareness of aged and gendered human sound, colour, textures and surface finishes (of various skins, fabrics and metals) and smells (dung, meat, etc.) would have impacted on humans in various archaeological locales in southern Italy (Hamilton and Whitehouse 2006). Our aim in this is to better educate ourselves about the practical and ideological sensory issues that past communities potentially would have drawn upon in their understandings of daily life. In this we have not worked in opposition to traditional and more empirical investigative field methods, but have experimented with existing traditions of understanding space and home territory; thus we have experimented with collecting the traditional information of site catchment analysis—soil type, location of major topographic changes—alongside phenomenological narratives of journeys through territories. Here interpretation lies in negotiating juxtaposed types of knowledge, for instance the implications of prime agricultural land that is secret in terms of vision and sound but proximate to a settlement. Likewise, communities may be in viable sound and visual distance of each other but separated by treacherous ravines and crags. This full sensory understanding is key to how we assess the relatedness of people, communities, places and landscape.

To date, there has been a greater orientation of phenomenological landscape work on recognised ritual landscapes with obvious monumental structures than on Ingold's (2000) 'taskscapes'—the places and contexts of daily activities—which the Tavoliere-Gargano work discussed above centres on. This imbalance needs redressing if the full range of field phenomenology as a methodology is to be exploited and realised interpretively. Sacred and secular are not necessarily separate but are interwoven; there are times and circumstances when the sacred comes to the fore, while at other times functional activities are more prominent (Bender et al. 1997).

Affective Presentation: Meshing Disciplines

> Our pictures will express the plastic equivalents of sounds noises and smells
>
> *From the point of view of form*: sounds, noises and smells can be concave or convex, triangular, elliptical, oblong etc.
>
> *From the point of view of colour*: sounds, noises and smells can be yellow, red, green, indigo, sky blue and violet. (Carlo Carra, *The Painting of Sounds, Noises and Smells,* 1913 [quoted in Pierre 1969:105])

Today, project videos and websites provide extensive means of portraying our sensory engagement with, and understanding of, archaeological landscapes using sound, movement, and colour in addition to text. The Landscape and Perception Project (http://www.landscape-perception.com/) conducted under the auspices of the Royal College of Art, which aims to reconsider the audiovisual perception of the prehistoric landscapes of Preseli, Avebury in Wiltshire and Stonehenge, powerfully brings the communicative strengths of archaeoacoustics and visual mapping to our attention. I was particularly 'struck' by the sonic qualities of field rocks when struck as gongs. But even with the restrictions of the printed page, it seems to me that a sensory archaeology of landscape should not only read differently (as advocated by Tilley) but also *look* different. Images succinctly alert us to what is conceptually missing as well as what is present in the focus of a study, such as the lack of topographic information from site catchment diagrams, or the way that GIS viewsheds give only an indication of what can be seen, rather than what things and actions can be understood. Are the intervisibility diagrams of a phenomenological landscape study any more or less evocative and understandable than the Thiessen polygon diagrams of hillfort studies? The point here is that both interpretive studies of landscape and more empirical studies equally need to experiment with re-presentation. An interest in doing this is more evident in interpretive archaeology (Hamilton and Whitehouse 2006; Tilley et al. 2000). Representation is potentially an arena in which different facets of landscape archaeology can feed into common and multiple representations of past landscapes on the 'page'.

Dangerous Liaisons: Landscape Dialogues

> I once heard someone say in a dusty bar somewhere in West Texas, 'there ain't nothin' in the middle of the road 'cept white lines and dead armadillos'. (Johnson 1999:187)

I have here reflected upon the well-rehearsed arguments and explanatory points that frame the formulation of different archaeologies of landscapes. It can be argued that that my characterisation of phenomenological versus processual/evolutionary approaches to past landscapes has been extreme, or does not represent, for example, modern evolutionary approaches (see Postscript). My aim, however, has been to deal with the perceptions of the protagonists while at the same time seeking avenues for a co-multiplicity of approaches. There are more resources—ideological, technological, sociological and economic—for inhabiting the landscape than we are prone to recognise and encompass in any single approach. Landscapes are polysemic, so why create a polarity between interpretive approaches and processual/evolutionary approaches? Barber's (2003) analysis of Maori fishing practices, for example, highlights how explanations are not mutually exclusive; traditional cultural prohibitions ensured that the seas were harvested safely in accordance with ritual rules, and at the same time these resulted in fishing strategies that ensured sustainable catches, which in turn reinforced the efficacy of ritually proscribed behaviour. Matthew Johnson's admonition that taking the middle ground in intellectual polarity should be banned because it becomes a stand-in for critique of one's own theoretical position is too simple (Johnson 1999:187). My concern here is certainly not to get run over in the middle of the road but rather to recognise the productively complementary value of different methods of enquiry in providing a holistic assessment of landscape use and understanding. Within this perspective, I see the incorporation of phenomenological perspectives as vital.

Given the long-standing trend in landscape studies for the production of collected works, the huge variety of landscape archaeology is easily accessed outside this article, but in considering and acknowledging this variety it becomes clear that what is missing are approaches that deal with landscape's hypercomplexity. Landscape research needs to step away from the polemics of competing approaches. It needs to tackle landscape holistically, as a network of relationships and complex interactions where the actions of prehistoric people were not necessarily exclusive in their intent or incompatible with multiple understandings of place. Perhaps here complexity theory has something to offer (cf. Bentley and Maschner 2008). A contemporary, anthropological reframing of complexity theory rejects simplistic, linear explanations of cause and effect and instead frames understanding at the scale of the totality of interactions with a strong emphasis on 'the qualitative analysis of complex systems of meaning with notions of pattern, metaphor and analogy' (Mosko 2005:36). It also recognises that apparently simple patterns can emerge out of innumerable chaotic and complex

interactions (Mosko 2005:35). In this there lies a space for interaction between qualitative and quantitative approaches. In archaeology, a starting point to doing this is in the co-working of practitioners of different paradigms and participation in reflexive dialogues of fieldwork and data analysis. Yet the real problem may be that this involves conflicting kinds of present-day people rather than innately irreconcilable facets of past-inhabited landscapes. A key irony is that in fieldwork there is much potential common ground on how to become familiar with a landscape and the varying scales at which this work should take place.

Postscript

Evolutionary archaeology is often implicitly described from a postprocessual or phenomenological polemical stance as assuming that humans in their inhabitation of landscapes only think 'adaptively' or with economic concerns in the forefront of their minds. It can be asserted that this characterises no modern, mainstream evolutionary approach (Lyman and O'Brien 1998). 'Rather, since we do not know how people attribute meaning to their world we develop models *as if* evolution/adaptation was important. Importantly, it is just as interesting when these models fit observations of the archaeological record, as when they don't When predictive models of site location (based on subsistence, economics, defence, visibility, etc.) do not accurately characterise site locations we might ask—why not?' (Cochrane, editor's comment on this chapter). The secondary 'why not' is wholly important. A postprocessual or phenomenological approach begins rather than ends with the 'why not?', and considers that while people do things that may have economic pragmatics as a bottom line, it is not overt or central in their understanding and construction of the meaning of the landscapes that they inhabit. To consider and methodologically encompass these perspectives in tandem rather than sequentially is the challenge.

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PART 3

FUTURE DIRECTIONS

CHAPTER THIRTEEN

Contrasts and Conflicts in Anthropology and Archaeology: The Evolutionary/ Interpretive Dichotomy in Human Behavioural Research

Heidi Colleran and Ruth Mace

Evolutionary theory continues to transform how we understand ourselves and our place in the world, providing a powerful, general framework within which to understand the evolution of human behavioural diversity. Within and beyond biology, appreciation of evolution as a guiding principle has allowed previously disparate disciplines to communicate and integrate (Dunbar and Barrett 2007). Evolutionists have called for the social sciences to become more Darwinian (e.g., Boyd and Richerson 1985, 2005; Cartmill 1994; Gintis 2004; Henrich et al. 2008; Lieberson and Lynn 2003; Mesoudi 2007; Mesoudi et al. 2006; Sear et al 2007; Shennan 2002), yet a profound mistrust of the evolutionary approach to human behaviour is still pervasive in many social science disciplines (Blute 1997; Boone and Smith 1998; Bryant 2004; Hodgson 2002; Ingold 2000; Marks 1995, 2004; Rose 1999; Steel 2004). A sometimes bitter divide has emerged between those who consider themselves on one or the other side of an intellectual boundary that supposedly separates 'interpretive' and evolutionary approaches to behaviour. Disagreements abound over the usefulness and validity of scientific approaches in anthropology and archaeology, over the methods that may be justifiably used to examine human behaviour, and over the

use and understanding of evolutionary theory and terminology (Aberle 1987; Carrithers et al. 1990; Dunnell 1982; Trigger 1998). Unhelpful caricatures of the work of each 'side' contribute to a general feeling that the differences are irreconcilable (see Fearn 2008 for recent mudslinging in anthropology; Kristiansen 2004 for discussion in archaeology). Many evolutionists are enthusiastic about subdisciplinary communication in the behavioural sciences (we include ourselves), but reconciliation under Darwinian theory may not be realistic, since there are many in the social sciences who do not wish to study evolutionary approaches to human behaviour. Moreover, disagreements over the validity of disparate research areas will not be resolved through enforcing a shared theory. However, integration may be possible through shared commitments to basic 'scientific' methodological approaches that enable communication and critical evaluation. This will in turn depend on a commitment to evidence-based research.

Rather than a formal set of strictures, the scientific method is a philosophical outlook on the means to evaluate theory-driven research, which allows competing theories to be critically appraised and evaluated using shared criteria. Though the actual methods used may vary greatly in their theoretical, statistical or mathematical sophistication, the scientific method is ultimately no more than a research disposition that values the forming and testing of competing hypotheses against available evidence. The Darwinian conceptual framework, in turn, is also a philosophical system, emphasising continuity and cumulative causal explanation. There is nothing in this system that downplays or ignores human intentionality or purposefulness (Hodgson 2004). Yet a major roadblock in the debates between interpretive and evolutionary research is the fact that many social scientists do not, or cannot, distinguish a bad piece of science, on scientific grounds, from a good piece of science, leading to an overreliance on verbal philosophical or political arguments for criticism. Such arguments are of course potentially useful, but they can stifle and contort debate about the content and validity of the research. They may be laden with assumptions that are unquestioned, probably because communication across the interpretive-evolutionary divide is limited. Yet if constructive communication and disciplinary development is our aim, we must begin by recognising our differences clearly and respectfully. Maintaining a polarised debate between cartoons of evolutionists as insensitive, Machiavellian powermongers (Collins 2009) and interpretivists as unrigorous, idiosyncratic relativists is pointless.

In some areas of anthropology, it is tacitly assumed that evolutionary theory is dangerous; that its scientific methods and reductionism lead inevitably to positivism, scientism and racism, and that epistemological relativism is the only way to avoid these consequences (D'Andrade

2000; Roscoe 1995; Segerstråle 1992). These ideas are to some extent justified reactions to the negative implications of twentieth-century misapprehensions of a 'progressive' sociocultural evolution (Blute 1979; Dunnell 1988) and the use of genetics to justify racial and population-level discrimination. They also reveal important differences in how each subdiscipline understands theory and human experience. However, continued concern with the imputed political underpinnings of distortions of Darwinism (Ingold 2000, 2007; Rose 1978, 1999) means that criticism of current evolutionary research frequently conflates political consequences with the fundamental tools of scientific research. No doubt, there are cases where naïve evolutionary logic is used to justify inane and insensitive conclusions about human behaviour (e.g., Kanazawa 2006, 2007), and these are justifiably criticised by evolutionists and others alike, on both theoretical and scientific grounds (Dickins et al. 2007; Gelman 2007; Lawson et al. 2008; Wilkinson and Pickett 2007). However, the vast majority of contemporary evolutionary research in anthropology and archaeology is based on the principles of variation, transmission and selection, with no value judgements attached, and a focused debate about evolutionary theory's relevance in providing a framework for understanding human behaviour should be based on scientific rather than political justification. Similarly, provocative conclusions are better refuted by examinations of their evidentiary basis than by objections to their political implications.

Postmodern theory 'calls into question' the claims of any generalising, totalising explanation of human behaviour. However, in their extreme forms, ideas about the impossibility of objectivity (Geertz 1973; Marcus and Fischer 1986) and the socially constructed, political nature of science (Feyerabend 1975; Kuhn 1962; Latour 1985) undermine attempts at explaining human behaviour at all. These ideas can lead to assumptions that scientific knowledge is not epistemically special or particularly trustworthy, but instead the result of a social, historical and politically driven Western ideology (e.g., Lewontin 1993; Sahlins 1977; for discussion, see Collins 2009; Fricke 2003; Sidky 2003; Yoshida 2007). On the other hand, disdain for scientific method and unified theoretical grounding leads to an understanding of anthropology as 'an empirical ragbag of hot topics', cobbled together (however artfully) by academics who claim that their approach takes 'real' people more seriously (Bloch 2008; Hart 2004:4). Concerns about genocentric and ethnocentric accounts of behaviour are genuine and important for maintaining integrity in human behavioural research. However, uncritical cultural and epistemological relativism is not necessarily the appropriate response (Bloch 2005; Carneiro 2004; D'Andrade 2000; Murdock 1971). Dichotomies between nature and nurture, biology and culture, and indeed between

interpretive and evolutionary research itself do not help to promote or clarify dialogue. More dangerous to communication, however, are the subtle sleights of hand that transform science into scientism (e.g., Marks 1995), objectivity into oppression (e.g., Abu-Lughod 1991), reductionism into biological determinism (e.g., Rose 1999) and generalisation into dehumanisation (e.g., Urla 1993).

Though there may be some who take this view, we do not believe that social anthropologists and interpretive archaeologists are all unchecked relativists who deny the usefulness of science. In spite of differences, we are all empirical researchers working with the same raw material. Indeed, many interpretivists are involved in formulating hypotheses, making general claims and comparing individuals, groups and culture. Evolutionary biology necessarily underlies behavioural science simply because of the fact that we are a species that has evolved (Gintis 2004). Predicating our understanding of human behaviour on evolutionary considerations is therefore justifiable. Though many social scientists would agree with the first proposition, many would not agree that the second follows from it. This is because the social realm, as conceived of by Durkheim, is considered a reality sui generis, and requires distinctive, indigenous principles of understanding (Bryant 2004; Durkheim 1979). Scientific methodology holds the prospect of clarifying to what extent this is the case, since it is a hypothesis that can be tested. We believe, however, as do others (e.g., Kitcher 1987; Wilson 2005), that resistance to evolutionary theory often hangs on persistently flawed or limited understandings of evolutionary theory (e.g., Loney 2000; for discussion, see Henrich et al. 2008) and its conflation with sociopolitical implications. We believe that appreciating the importance of evolution in explaining current human behaviour can promote dialogue and stimulate interesting research between those on either side of the self-imposed divide, leading to a plurality of complementary approaches at different levels of analysis that can be evaluated using the same criteria.

We do not prescribe a universal research agenda based on evolutionary theory. The aim of evolutionary anthropology and archaeology is not to colonise interpretive territory and impose some biological, reductionist hegemony. Rather, it attempts to combine an ethnographic understanding of situated individuals with the scientific method of generating and testing hypotheses, as a means to evaluate their explanatory power (for an overview in archaeology, see Shennan 2008; for anthropology, see Winterhalder and Smith 2000). The insights of both interpretive and evolutionary approaches can be meaningfully incorporated into a broader aim of understanding human behaviour as long as we do not assume that one is incompatible with the other. Anthropology and archaeology are extremely diverse, and there are many kinds of science

and many levels at which it can be carried out. There is only one scientific methodology that can allow these different approaches to communicate: the explicit testing of hypotheses about behaviour that can be critically evaluated using an accepted metalanguage. This requires a commitment, at least to begin with, to some form of shared definitional clarity, however imperfect, so that interpretations can be understood beyond the particular study, and to structured data collection in addition to other information that can allow analysis of and comparison across studies. If divergent models of individual behaviour can become more equivalent through being translatable, they will not have to be mutually invisible. This way, paradigms in anthropology and archaeology can be substantively enriched by the scientific content of others (Gintis 2004).

Our aim in this chapter is to promote dialogue by clarifying a number of important misunderstandings about contemporary evolutionary anthropology (also known as human behavioural ecology, or HBE), explaining some of its central theoretical and methodological commitments. The chapter will focus on two general areas of discontent with evolutionary research in anthropology and archaeology: (1) objections to evolutionary research in principle, based on perceived implications of evolutionary theory, and (2) objections to the methodology of evolutionary research. Our fundamental points are first that the principles of evolution (cumulative causality based on variation, transmission and selection) can be applied to most, if not all, complex systems, including human intentionality; and second that the truth or falsity of such a guiding framework for social science should be established through testing competing hypotheses against available evidence, rather than exclusively philosophical or political arguments. Using empirical examples we will show how research from other disciplines (including ones that don't share the same theory) can be incorporated into a broader view of human behaviour, once they share a common logic of explanation and evaluation.

Objections to Evolutionary Research in Principle

Don't We All Make General Statements about Behaviour?

Evolutionary research assumes that general theories about human behaviour are possible, but is often derided as proposing unsophisticated generalisations (Bryant 2004; Ingold 2000; Kitcher 1987; Marks 1995; Rose 1978). This criticism reveals a number of issues. First, it is not clear how general claims are avoidable, and anthropologists and archaeologists working outside the evolutionary framework do not refrain from making general claims about behaviour (structuralism, Marxism, structuration

are all such general theories). Interpretive models, expressed in terms of personal meaning and motivational patterns, are not themselves independent of a view of humans (Fricke 2003). Even those who do deny the possibility of general theories at all (e.g., Abu-Lughod 1991) assert that humans are autonomous self-determining agents, with intentions and motivations that are not amenable to measurement; itself the product of general theories (Bourdieu 1977; Giddens 1984). Relativism itself cannot begin without a general starting point (O'Grady 2002).

What is objected to, then, is surely the perceived 'totalising' or 'essentialising' nature of evolutionary statements rather than the fact that they are general. This perception misunderstands the focus of much evolutionary research, which aims to understand variation in human behaviour. There is a tendency to assume that evolutionists see human nature as fixed and universal rather than variable, and that variability is accidental rather than essential (by contrast, modern anthropologists often seem to evolutionists to be more concerned with irregularity and local specificity than regularity in behaviour). Widespread use of statistics and mathematics (e.g., Glatz et al., this volume) would seem to support this intuition, since many social scientists see the application of such methods as removing individual-level variation (more will be said on why this is not the case below).

Evolutionary anthropology does not see human nature as fixed or universal. On the contrary, it is predicated on variation. It assumes that people, through their behaviour, respond flexibly to the conditions in which they find themselves. Natural selection is assumed to have produced individuals who have the capacity to respond adaptively to changes in their social and physical environments. So the human behavioural phenotype—the result of the developmental interaction between an individual's genetic heritage and its social and natural environment—is plastic. We should therefore understand natural selection as acting on individual 'norms of reaction' rather than on individual behaviours themselves. Behaviour is thus expected to vary probabilistically according to the constraints of particular environmental conditions.

To take an example from 'parental investment' theory, individuals are expected to vary 'investments' of their time and energy (and other resources) in having children, depending on the context. An important aspect of the context involves the opportunity costs of raising and providing for children (such as the ever-increasing level of education necessary to ensure academic and work-related success in developed economies) and the demand for children as economic contributors (more common in agricultural and traditional populations). The fact that our resources (time, money, etc.) are necessarily limited may lead to trade-offs between the quantity of offspring produced and their 'quality', i.e., the amount

of investment they receive (Becker and Lewis 1973; Hagen et al. 2006; Mace 1998, 1999). In contemporary postindustrial contexts, parental investment is potentially unlimited since there appears to be no end to the ways we can enrich our children's lives. Thus, a form of 'runaway' parental investment in decreasing numbers of children may follow (Mace 2007). In contrast, despite widespread intervention programmes, the slow fertility declines in areas of the developing world (particularly sub-Saharan Africa) may be due in part to a lack of incentives for challenging traditional reproductive preferences that emphasise high fertility (for example, jobs for educated individuals) (Mace et al. 2006; Mace and Colleran 2009). Certainly, long-held reproductive preferences appear to be rapidly surmountable when faced with economic constraints, as in the Ethiopian capital, Addis Ababa (Gurmu and Mace 2008; Mace 2008). HBE sees reproduction partly as the outcome of investment strategies, so understanding how people in different contexts negotiate these trade-offs may help to explain the variation we see in fertility levels, both within socioeconomic strata in countries in the developed world (Lawson and Mace 2008, 2009), and across populations in general (Bongaarts 2003; Bryant 2007; Kaplan et al. 2002).

Genetic Determinism

If variation in human behaviour is to be understood in terms of an evolved phenotypic plasticity and therefore largely in terms of social and environmental influences on this plasticity, what of the persistent assumption that evolutionary research is oriented primarily towards genetic determinism (e.g., Ingold 2007; Rose 1999)? Genetic determinism is often taken to imply that if our behaviour has evolved and is at least partly genetically heritable, then we are necessarily impelled to behave in certain ways. The 'gene's-eye' view (Dawkins 1976, 1983) in anthropology and archaeology should therefore constitute a fundamental threat to our idea of ourselves as moral beings, since if we lose the capacity to make choices then morality and responsibility are impossible (Radcliffe-Richards 2000). There are two issues here. One is related to definitional clarity—the understanding of the term 'determinism' as a global thesis of cause and effect that implies total predictability and allows no intermediate influences. The second issue is the idea that genetic determinism has different, more negative implications for free will than other forms of determinism, such as cultural determinism.

Firstly, critiques of genetic determinism often misunderstand the 'gene for' concept. This is not a literal dictate but a conceptual shorthand. Genetic transmission is vastly more complex than most people realise, possibly as complex and difficult to model in detail as cultural

transmission. Yet the use of the 'gene' as a heuristic device has not hampered our ability to predict and understand macroevolutionary genetic processes. In any case, genetic differences do not correspond, one to one, to behavioural differences. Even the strongest genetic determinism does not require that behavioural traits are literally fixed by genes, but that genes constrain the expression of behavioural outcomes within narrow norms of reaction. Thus, the idea that genes by themselves fix socially significant behaviour is one that no informed scientist holds (Rosoff and Rosenberg 2006). 'Genetic determinism' is therefore not really a strict form of determinism at all, in the sense of fixing behaviour immutably. While our DNA is fixed, the pathways from genetic information to behavioural outcomes are influenced both before and after birth by developmental and environmental (including social) processes. Indeed, the field of epigenetics explores how non-genetic factors may influence the differential expression of genes themselves (Jablonka et al. 2005). Moreover, the field of gene-culture evolution, pioneered by Richerson and Boyd and colleagues, examines how behaviour can be preserved and passed on via entirely non-genetic means through imitation, learning and teaching (Boyd and Richerson 1985; Richerson and Boyd 2005).

Second, unless we believe in spontaneous, uncaused intentionality, we are all determinists in the general sense of the word, and intentionality, complexity and their emergent properties should therefore be subject to causal investigation. Yet a pervasive assumption in anthropology and archaeology is that somehow environmental or cultural determinants of behaviour are less 'determining' than genetic ones (Dawkins 1983). Put another way, culture, rather than biology, more extensively determines humans (Bloch 2005).

The distinction is misleading, since both are important for answering most questions comprehensively. If we are all determinists of some form, though, the particular determinants we think influence behaviour have no bearing on the issue of human morality, responsibility or free will. We may not be able to change our DNA or even how our genes get expressed, and we may inherit socially significant predispositions (alcoholism, for example), but we can certainly change our behaviour. If we are to reject the evolutionary paradigm on the basis that its causal conjectures deny us responsibility for our actions, then those who privilege environmental or cultural determinism instead must demonstrate how their position is not similarly troubled. At a philosophical level, this stance additionally assumes an unproblematic and uncontested definition of responsibility. Whether we can be ultimately responsible for our actions depends not on what level of Darwinism is true, but on how we understand responsibility. If we accept that there is an underlying

order in the world, whether we can access it or not, then, given the overwhelming evidence, Darwinism is an accurate depiction of the processes that structure a good deal of it. And unless we assume that freedom means being able to go back and change the causal paths that were in place before we were born, Darwinism allows for all forms of responsibility in the ordinary sense of the word (Radcliffe-Richards 2000). The truth of Darwinism does not imply a loss of freedom or the capacity to act morally or responsibly. Evolutionary theory commits you to the idea that behaviour is reliably influenced by genetic, cultural and environmental factors. Nowhere is it implied that evolutionary accounts of behaviour justify the abandonment of morality.

In any case, objectionable social consequences (e.g., inequality, discrimination) come about as equally through environmental and cultural determinants as through genetic ones. As one evolutionist put it, it's not as if the world became unequal only after Darwin's theory arrived (Wilson 2005). To the extent that we are determined (genetically or otherwise), how we decide to run our societies is a completely separate issue. In fact, rather than leading straight to discrimination and oppression, learning about any elements of our behaviour that may be under relatively greater genetic control can inform how we structure our societies to react to and accommodate them. To take a very simple example, consider the possibility of a genetic basis for homosexuality. If 'genes for' homosexuality could be identified, perhaps screening for and discrimination against embryos carrying those genes would become widespread? We have already shown how the 'gene for' concept renders this a simplistic argument from the outset. Yet even if clear genetic determinants could be found, this evidence would remain completely distinct from the ethics of how we react to it. (Incidentally, the strongest determinant yet found for homosexuality is the number of older biological brothers a man has, clearly not a genetic effect [Bogaert 2006]).

Critics might counter that our value systems themselves may be under some genetic control and might be very hard to change, thus limiting our ability to appropriately react to ethical or moral issues. Yet even if our values themselves were genetically determined, the social context in which they are expressed does not have to be (de Melo-Martín 2003). Genetic determinism can therefore actually show us ways in which we need to address our values and social systems (Pinker 2002). It does not permit us to abnegate them.

Reductionism

Opponents of genetic determinism usually also object to reductionism (Rosoff and Rosenberg 2006), because reductionism is often assumed

to support and sometimes equate with genetic determinism (Segerstråle 1992). The idea of any simplifying explanation of human behaviour may be anathema to interpretivists, whose analysis focuses on the details of human social life (however, see Whitehouse, this volume). Again there are two issues here. First, reductionism can take different forms, and it is not always clear to which form critics are objecting. At least two can be identified. Ontological reductionism implies that complex wholes are simply the sum of their parts. Methodological reductionism implies that complex wholes can be explained and understood in terms of their parts. Evolutionary anthropology often employs a methodological, but never an ontological, reductionism. The two are not the same, and conflating the former with the latter has strong implications for how such research is viewed (Rosoff and Rosenberg 2006; Segerstråle 1992). The second, related issue concerns the fact that reductionism is a methodological heuristic for a particular level of evolutionary analysis, by no means the only one important for evolutionary research.

Evolutionary biologists have defined four logically independent yet equally valid levels of analysis and explanation that can help answer the question 'why' an organism behaves in a certain way (Tinbergen 1963). First, we can ask why an individual behaves in a certain way in terms of what are known as 'proximate causes'. This refers to an individual's immediate motivations and intentions, and the social, psychological and biochemical mechanisms that mediate them. Second, we can ask how behaviour is influenced by developmental or ontogenetic causes; how an individual's development, as well as their genetic heritage, may have led them to behave in a particular way. Developmental processes do not necessarily lead to particular behaviours, but influence the range of behaviours we display within particular norms of reaction. Third, we can ask about the evolutionary history of behaviour, when it arose and why it followed a particular pattern. This would be a phylogenetic explanation. Finally, we can ask how and why behaviour influenced an individual's ability to survive and reproduce; this is an ultimate explanation about the evolutionary function of behaviour. Thus, ultimate explanations address why the behaviour was selected for rather than how it developed. This involves questions about how we, as a species, may have adapted to survive, and to what extent such adaptations underlie the variation in behavioural characteristics we see today (see Cochrane, this volume, for an example of artefact-centred ultimate explanation).

Evolutionary anthropologists and archaeologists often focus on the ultimate level of explanation, asking what selective advantage may have caused one behavioural variant to be selected at the expense of others. Explanations of the same behaviour from different explanatory levels are not mutually exclusive and do not pose any problems for such

analysis; indeed they are preferable. Mutually consistent answers from more than one level of analysis enrich our understanding of behaviour (Barrett et al. 2002). Tinbergen's distinction between proximate and ultimate levels of explanation was originally developed to help clarify the study of animal behaviour (a field of which he was a founder) rather than explicitly for understanding human behaviour. However, the heuristic categorisation works well in evolutionary anthropology and archaeology too. The general classification of proximate and ultimate explanations provides an excellent way of distinguishing the kinds of processes researchers are interested in. They help to clarify that certain concepts and terminology may be more relevant in some studies than others (for example, subjective understandings of experience in proximate-level research) and can thus eliminate confusion about what aspect of behaviour is under study. Varied evolutionary approaches to behaviour seek explanations at all of these levels.

For example, cultural variants may be subject to selection and transmission processes (Boyd and Richerson 1985), independent of or co-evolving with genes, and it is possible to generate proximate and ultimate explanations of changes in cultural norms (for example, the features of a cultural variant that might lead to its spread, the learning and teaching mechanisms that we use to pass on or obtain information, and the biological success of the organism that adopts a cultural variant). These ideas are being tested and refined using a wide range of methods at different levels of evolutionary analysis, in addition to those used in interpretive anthropology and archaeology. These include statistical models of anthropological data (e.g., Borgerhoff Mulder 2009; Mace and Colleran 2009); phylogenetic models of coevolution between resource acquisition and cultural practices (Fortunato et al. 2006; Holden and Mace 2003) and of language and kinship patterns (Gray and Jordan 2000; Jordan et al. 2009); computer simulations of social learning conditioned on archaeological and genetic data (Powell et al. 2009); experimental psychology (Mesoudi 2008); economic games (e.g., Fehr and Gachter 2002); role-play experiments (Newson et al. 2007); and primate (Whiten 2000) and other animal social learning research (Day et al. 2001; Kendal et al. 2009; Laland and Williams 1998). The thread uniting these studies is not a reduction of culture to biology, nor a focus on the same level of analysis, or even the same specific methodology, but the joint commitments to a causal explanation of behaviour and the use of tools to test their hypotheses that can go beyond subjective interpretation. Yet this in no way denigrates the value of a subjective understanding of behaviour. Rather, it points to how a truly integrative approach provides a means for including research that focuses on a different level of analysis. The multilevel approach of evolutionary research allows an incorporation of evidence from a wide variety of

research paradigms far beyond the remit of biology and genetics and more importantly, beyond the confines of evolutionary research itself. Fruitful interdisciplinary collaborations continue to enrich the theoretical and evidentiary basis of evolutionary theory as a result. For example, research into the evolution of human cooperation incorporates economic game theory and experimental research methods (e.g., Axelrod and Hamilton 1981; Bolton and Zwick 1995; Boyd et al. 2003; Fehr 2002; Henrich 2000); cultural evolutionary theory draws on social psychology, sociology and demographic research (Boyd and Richerson 1985; Henrich and Boyd 1998; Henrich et al. 2008; Newson et al. 2005; Richerson and Boyd 2005); and primate and other animal research as well as palaeoanthropology are used to inform life history theory (e.g., Bogin and Smith 1996; Hill and Kaplan 1999; Kaplan et al. 2000; Peccei 2001) and social network analysis (e.g., Dunbar 2008; Hill and Dunbar 2003; Roberts et al. 2009). Certainly, the specific research aims of evolutionary and interpretive approaches may be different, but they can be understood as asking different 'whys' and really do not have to be mutually exclusive. We can study the twin processes of diversification and constraint in the context of different kinds of developmental history.

A Spectrum Rather Than a Dichotomy

A multilevel understanding of behavioural diversity should clarify the fact that there is a broad spectrum of research focus in anthropology and archaeology, rather than a sharp division between evolutionists and interpretivists. However, false and unhelpful dichotomies persist in separating evolutionary and interpretive inquiry, such as that between nature and nurture, between culture and biology and between objectivity and subjectivity. These are predicated on untenable distinctions in both theory and practice that serve only to distort and occlude the common ground between them. The legacy of nineteenth-century German idealist philosophy has been to dichotomise knowledge into opposed and competing camps. Thus the nomothetic, positivist, pragmatic *naturwissenschaften* (natural sciences) are based on the search for general laws, require verification through replication, employ mathematical and statistical analysis and value predictive validity. These are contrasted with the idiographic, spiritual-hermeneutic *geisteswissenschaften* (sciences of the spirit), which seek understanding of individual events, ideas and values that may never recur in the same way and can therefore be neither predictable nor replicable (Shankman 1984). Geertz (1973:xx) took this division further in asserting that interpretive knowledge was superior to and would eventually replace the reductionist approach of positivist science, in a 'refiguration of social thought'.

However, as we have argued, the acceptance of one does not necessarily threaten the validity of the other. In fact, they can and should be highly complementary. By continuing to assign separate and distinct epistemic responsibilities to different subdisciplines, the study of interdependent, relational social life is degraded (Bryant 2004). Moreover, the interpretive-evolutionary dichotomy itself engenders theoretical and methodological distance between the subfields, obscuring the insights of one from the other. Any synthetic approach to the behavioural sciences must seek to transcend, rather than uphold, these traditional dichotomies.

Our interest in cultural, social and biological diversity should be matched by a greater tolerance for intellectual diversity. However, privileging the interpreter or ethnographer as the authority on a piece of research, rather than making methods and assumptions transparent and open to replication, criticism and refutation, engenders validation systems that may sometimes be overly influenced by status than on relations to any evidence (Roscoe 1995). It leaves too much room for speculation and too little for comparative correctives (Kristiansen 2004). A systematised approach to comparing and validating competing research claims allows each study to be questioned on the merits of its evidentiary and explanatory basis as well as on its argumentative cogency.

Objections to the Methods of Evolutionary Research

Apart from the theory itself, objections to evolutionary analysis usually involve discontent with the way evolutionists go about examining human behaviour, and the language they use to describe it. Concerns about the legitimacy and usefulness of evolutionary methods relate to at least three areas: (1) the widespread use of statistics and mathematical modelling, (2) the heuristic use of terminology to cover a range of culturally specific contextual information, and (3) the implications that a generalised terminology has for cultural sensitivity in the field. We will briefly address each of these in turn.

Do Statistics Dehumanise?

Suspicions about the use of statistics in human research often surround worries of 'mismeasurement' (Segerstråle 1992), the removal of important variation (Thompson and Roper 1980), and the potential for dehumanisation as a result (Urla 1993). In particular, the idea that the behaviour of a group (e.g., the concept of averages) can reflect the propensities of the individuals in that group is considered a crude form of reductionism (Kitcher 1987), one not compatible with an understanding of individual variation and subjective experience (Asad 2002). There are

three issues here. The first concerns how evolutionists are assumed to understand the relationship between statistics and reality. The second relates to conceptual confusions about what exactly statistics do to data. The third concerns the imputation of political implications to statistical outputs.

First, statistical constructs are not themselves natural attributes of an individual or population, and no informed scientist would maintain otherwise. Evolutionists do not think that once you can calculate it, a relationship is necessarily real. Most peer-reviewed evolutionary research in human behaviour contains tentative and conservative speculations about results, as well as clear explanations of the limitations of the study and suggestions for improvement. Calculations are not usually considered to represent 'real' causality. However, statistical relationships are expected to be good proxies for hypothesised relationships because they are specified by extremely narrowly defined constraints, and are usually considered informative only when they are highly unlikely to have occurred by chance (usually with a less than 5% possibility). Unfamiliarity with the stringency expected of statistical analysis, as well as its flexibility in accounting for a vast array of factors, can lead to assumptions that causality of any sort cannot be established using the heuristics of correlational analysis. Thus the relevance of ever-refined statistical methods assumed to bear no relation to reality, and with it the value of a common methodological benchmark, is often unclear to interpretivists.

Second, the idea of holding something constant in statistical analysis (like cultural or other variables) is sometimes equated with eliminating its importance. It can therefore be seen as removing individual-level variation, or at least relegating it to a surface phenomenon, when for many it is the very thing we are interested in. The importance of *ceteris paribus* (other things being equal) clauses in evolutionary anthropology could therefore be seen as philosophically suspect. The objection is that individual factors vary so much (and perhaps not even systematically) that highly complex systems may not be reducible to *ceteris paribus* assumptions. Holding factors constant may therefore not reflect reality.

Aside from the fact that statistical analysis allows you to examine just how much individual factors actually vary, these objections reveal some problematic expectations about the methods and the kinds of results obtained in such analysis. They are also based on the idea that these methods may conceal unjustifiable conceptual 'blank spots'. Controlling for variables that we know to be important in the real world is valuable because it allows other variables to be examined in isolation. For example, we know that education is an important complement of fertility decline and an influence on the likelihood of having heard of or used modern contraception (e.g., Behrman et al. 2002; Martin

1995; Stephenson et al. 2007). Thus an analysis of some proximate determinant's additional effect on reproductive behaviour (say, religious cosmologies prohibiting the use of contraceptives or fear about a particular method's effects) is likely to be more informative when we hold the education level of individuals constant, i.e., when we look at the differences in those proximate factors between people of the same education level. This helps us not only to distinguish between alternative explanations, but to assess what proportion of the variation in a measurable aspect of a particular behaviour can be attributed to different elements of the social, cultural and biological environment. Statistical methods do not provide the whole answer, but point to general influences that may be important, and often counterintuitive. We must, of course, be on our guard for circularity and conceptual blank spots. Old evidence may appear spurious in the light of new evidence through methodological refinements, ethnographic evidence and more sophisticated or culturally sensitive study design. Yet old evidence, even if it is spurious, is useful, since it points to areas for improvement in both theory and method. Science proceeds through such incremental steps. The commitment to refining methods and theory 'on the job' is one of the cornerstones of scientific research, which means that incomplete theories can be tested as an ongoing means to develop them.

A third concern related to the statistical analysis of group-level behaviour is that statistical constructs, by subsuming individuals under averages and by controlling their variation, can themselves engender stereotyping and discrimination (e.g., Urla 1993). Thus individual testing is more justifiable than group testing. We have already argued that the results of research are separate from the ethics of dealing with any political implications they may carry. As long as statistical results can be scrutinised for conceptual inaccuracies and circularities, then these worries can be allayed. This is precisely the motivation behind systematic analysis and public scrutiny via peer review in evolutionary (and all scientific) research. If there is nothing else to be gleaned from this, it is the fact that more social scientists, in diverse disciplines, need to learn more about statistical methods so that this scrutiny can be possible. By ignoring statistical analysis, some interpretive researchers actively disempower themselves, leaving them a set of tools for approaching and critiquing science that relies mostly on verbal, political and philosophical arguments, when in fact bad science can usually be more easily dismissed on scientific grounds.

Statistical concepts have been used in anthropology for well over a century (Driver 1953), and early quantitative treatments of human behaviour have been employed by influential anthropologists within and beyond the evolutionary cadre (e.g., Boas 1894; Murdock 1949, 1957;

Tylor 1889). In addition to a plethora of multivariate data analysis techniques, social scientists have been busy developing ever more refined methods for exploring sociocultural systems. These are helping to account for random variation and structure in the data (e.g., linear mixed models, multilevel models; see Glatz et al., this volume), the stochasticity inherent in dynamic interactions (e.g., stochastic-dynamic models), the evolutionary constraints that influence diversity (e.g., phylogenetics and cladistics), and indeterminacy in behaviour (e.g., computer simulation and agent-based models).

On the basis of existing research, the answer to the question of whether statistical or mathematical models are of any use for human behavioural research is surely a resounding yes. That statistical analysis should be used in conjunction with ethnographic research when possible is a recognised aim, and many anthropologists acknowledge that quantitative data can support sensitive accounts of behaviour (Thompson and Roper 1980). Further, that quantitative data can reflect approximately 'real' relationships that reflect the theoretical expectations of a given level of analysis can be seen in research that brings the two together. For example, Mace (1998) compared optimal behaviours derived from a stochastic dynamic model of behaviour predicated on evolutionary considerations with structured ethnographic data about reproductive behaviour from the Gabbra of Kenya. She was able to establish that, in this population, wealth inheritance and fertility strategies may have coevolved to differentially maximise the number of grandchildren born under different ecological conditions. The evolutionary logic behind this is that when parental investment in children has to be high (for example when the costs of marriage and setting up home are high), then smaller families may have higher fitness over the long term. When reproductive success is conditional on resources, concentrating these resources into small numbers of children may be more advantageous than diluting them among large numbers of children. As a result, average wealth in the population may actually be driven higher.

Similarly, the use of mathematical modelling combined with other ethnographically based quantitative research on the evolution of menopause has pointed to gaps in both theory and methods that would otherwise be extremely difficult to identify (Shanley et al. 2007). Mathematically, these methods help to test assumptions made in evolutionary arguments that would otherwise be impossible, for example relating to long-term fitness effects of current reproductive behaviour (e.g., Hill and Kern Reeve 2004)

Whether these models have any explanatory power derives not from their grounding in an evolutionary understanding of behaviour. Rather, it comes from their use of clear methodology to support their

propositions that can be scrutinised and criticised. Systematic analysis of explicit assumptions and description of results in a language that is understandable by others only enhances the reliability of ethnographic observation across studies. It does not degrade it. Thus statistical and mathematical modelling are immensely useful for determining if the predictions derived from theoretical commitments are likely to conform to real behaviour.

Can Sophisticated Terminology and Definitional Clarity Coexist in Ethnography?

Perhaps the most palpable conflict between evolutionists and interpretivists centres on the use and meaning of terminology and definitions, and the type of reasoning they may represent. Operational definitions (e.g., strategies, parental investment) are seen by some sociocultural anthropologists as useless oversimplifications (Chibnik 2005; Ingold 2000; Kitcher 1987; Rose 1999) that cloud important implications for ethnographic understanding. The importance of unambiguous definitional clarity in evolutionary anthropology seems to run counter to the 'thick' accounts of human behaviour usually associated with detailed interpretive ethnography. Some social anthropologists have even claimed that evolutionary anthropologists as a result do not conduct proper fieldwork, being somehow less capable of cultural sensitivity in the field than social anthropologists, and less conscious of their own cultural presuppositions (Ingold 2000).

We have already examined how misapprehension of the heuristic use of terminology can lead to assumptions of oversimplification. In much the same way that the 'gene for' concept is used as shorthand for a complex genetic process, the term 'strategy' is used as shorthand for generalising about complex patterns of behaviour. This does not imply that evolutionary anthropologists don't believe in culturally dependent contextual meanings. It similarly does not imply that evolutionary anthropologists assume that human intentionality is strategic in any conscious way. Rather, it is a commitment to a shared terminology for understanding behaviour at a specific level of analysis (the ultimate or functional level) in the interest of a causal explanation for why that type of behaviour evolved. Nebulous subjective understandings are a major part of human experience, but as explained earlier, the level of analysis that many evolutionists are interested in means that they need to operate at a certain level of abstraction. Terminology may change as new evidence appears and theoretical refinements are required. However, the commitment to an objectified terminology is what makes evolutionary approaches comparable, and some decontextualisation is necessary in

order for terminology to be generalisable. The benefit of this endeavour is that different studies can then be mutually intelligible, and thus critically evaluated in terms of their common theoretical framework.

Nonetheless, the explicit appreciation of the cultural influences on behaviour is what helps to generate hypotheses and culturally sensitive research designs. Ethnographically based knowledge helps evolutionary anthropologists to critically question the results of quantitative research when their findings violate their ethnographic understanding; this in turn helps the redevelopment and refinement of quantitative methodology (Tehrani, this volume). Data inspires theories and quantitative models as much as theory inspires data collection (Bentley, James, both this volume). However, the dominant concern in the interpretive programme is how we are to understand culturally specific meanings of terms, and how these meanings change relative to cultural, personal or other subjective experiences. We agree that these are important drivers of variation in behaviour. However, the use of interpretive ethnographic methods alone does not ensure that a particular piece of research is culturally informative or meaningful on any level other than an individual case study (Miller 1997). Data collection that is open to scrutiny and validation by individuals other than the particular ethnographer will make it possible to compare and critically assess the validity of the interpretation. Malinowski himself appears to have regretted not collecting more structured data: 'Were I able to embark once more on field-work, I would certainly take much greater care to measure, weigh and count everything that can be legitimately weighed and counted' (Malinowski 1935:159).

Conclusions

Disparities in the focus on terminological sophistication reveal a crucial difference between how interpretive and evolutionary researchers use terminology to arrive at their research aims. Evolutionary and other scientific approaches do not typically require analysis of the nuances of terminology because they are concerned with understanding how precisely defined and theoretically derived concepts or models help to reveal or 'disclose' a reality that may underlie human intentionality, or at least, emergent properties of it that can be matched to explanatory processes. Interpretivists, on the other hand, are not necessarily concerned with removing layers but with understanding them as they are, believing that local practices and the layers of context are themselves what constitutes the most important or interesting reality. As a result, there is a fundamental difference between a desire to explain more or less objective phenomena on the part of evolutionists, and a desire to understand subjective phenomena on the part of interpretivists. This

difference is, we believe, the pivot around which most disagreements over the use of evolutionary theory in anthropology and archaeology revolve. Evolutionary and interpretive programmes operate at essentially different levels of analysis, with different explanatory aims. However, we believe that methods for assessing the validity of competing theoretical claims can be commonly held.

The stratigraphic conceptions used in evolutionary analysis might lead some to think that culture is relegated to a surface phenomenon by evolutionists. However, in giving primacy to interpreted subjective experience and a disdain for explicitly testing the word of particular authors, interpretivists, more than anyone, seem concerned with surface phenomena. Rather than assuming it is irrelevant, evolutionary analysis is for those interested in understanding the deepest possible drivers of culture. Evolutionary anthropologists understand subjective experience as only part of human behaviour, and while implicitly including it, do not share the belief that it is always the most salient or only possible level of behavioural explanation. Both types of research can be integrated into a broad and varied empirical and theoretical framework if we appreciate that they are asking different kinds of questions, and require different kinds of answers, but only if we agree to a shared means of evaluating their claims to validity.

Ultimately, whether the scientific method is the right approach is not a political, cultural or historical question; it is an epistemological one. Evolutionary anthropologists are well aware of the notoriously problematic nature of knowledge and the philosophical questions relating to our ability to access an external reality (assuming that one exists!). Moreover, they appreciate that we access our knowledge via our senses and perceptions (Sidky 2003). This does not preclude the attempt to understand human behaviour in a systematic and scientific way. No matter what language you use to describe it, science works. And no matter how many scientific revolutions, knowledge based on ever-refined measurement is cumulative. New scientific knowledge is built upon old scientific knowledge, a characteristic not so easily used to describe the products of interpretivist research.

But in order to do any science at all, anthropologists and archaeologists must begin formulating and testing hypotheses derived from their theories, engendering an evidence-based approach to theory development and methodological refinement. This way, competing hypotheses can be given an equal hearing. Contemporary scientists do not claim that they are disinterested observers, uninfluenced by society and ideology. Certainly, there are many interconnections between social influences and the reason, evidence and data that science prides itself on. Sociopolitical forces are as present in the critiques of science as they are in science itself. This does

not easily translate into an indictment of science, however, since it has a methodology that allows flaws to be brought to light and criticised, itself an evolutionary process (Hull 1988; O'Brien et al. 2005). Anthropologists and archaeologists should not dodge the fact that there are cross-culturally recurrent patterns of human behaviour that may be explainable using structured, causal, scientific analysis. Moreover, they should acknowledge that the touting of occasional counterexamples does not negate these general trends (Bloch 2008), nor do they lead directly into a refutation of evolutionary theory. Research pragmatics, rather than conceptual pigeonholing, should be our unifying banner. Following Harris (1995) then, we believe that the research aim for any science-oriented anthropologist is not to debate whether objective social science is possible, but to find ways to judge whether one partially constructed truth is better than another.

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CHAPTER FOURTEEN

A Visit to Down House: Some Interpretive Comments on Evolutionary Archaeology

Matthew H. Johnson

Introduction

Until the last few years, interpretive and Darwinian, selectionist, or evolutionary archaeologies have not had a troubled or adversarial relationship; rather, they have had very little relationship at all. Their visions of archaeology appear to be poles apart; the style and tone of the writings of either school are, it would seem at first appearance, like chalk and cheese. If the last 20 years have seen a growing diversity and fragmentation of archaeological theory, then one artefact of that fragmentation has been a lack of dialogue between different fragments. Scholars engage in 'redlining' of large areas of archaeological theory (Kristiansen 2004). Where the 1980s saw furious arguments at conferences, the first decade of the twenty-first century is more likely to see polished papers in parallel sessions delivered to the already converted.

Until invited to write this chapter, I had had little contact with the current literature on Darwinian, or evolutionary, archaeology. Selection of areas of expertise is always necessary, particularly given the necessarily open-ended and interdisciplinary nature of theory ('to admit the importance of theory is to make an open-ended commitment, to leave yourself in a position where there are always things you don't know' [Culler 1997:16]), but the contrast can be drawn with the very rich and productive intellectual space that connects interpretive archaeologies with cultural evolutionary theory, that is, the theoretical tradition stemming from the social thought of Spencer rather than the biological thinking of Darwin, which characterises and classifies social life in chiefdoms and states. In this latter field, there has been an extensive and often productive dialogue with postprocessual thinking, as seen for example in discussions of power, materiality, heterarchy and related concepts, and

which often relates themes such as gender and structuration theory to questions of social process and state formation (cf. Beck et al. 2007; various chapters in Brumfiel and Fox 1994; Crumley and Levy 1995; DeMarrais et al. 2004; Hegmon 2003). In a vulgarised conception of theory, traditions of cultural evolution stand in greater proximity to interpretive ideas than does Darwinian evolution. If this volume achieves one thing, it may be to show this perceived lack of proximity between Darwinian and interpretive approaches to be at best an oversimplification and at worst misleading.

In this chapter, I want to outline a few elements of the goals and intellectual aims of archaeology as a discipline. My perspective is broadly an interpretive one, though I want to draw out some common elements and themes, and make some critical remarks about interpretive and other approaches along the way. In particular, I want to grapple with archaeology's aspirations. Archaeologists claim to be able to understand and account for the past in an empirically and theoretically rigorous way, to reach out beyond the material traces of the past to past cultural life. While interpretive archaeology has been glossed by many of its detractors as abandoning these claims in the face of a disabling plurality of views of the past, I have never accepted such an analysis. In this paper, I want to reassert these claims, and more broadly to reassert the roles and responsibilities of archaeology as a serious social science.

I will develop this position by responding particularly to positions and statements found in Stephen Shennan's book *Genes, Memes and Human History* (Shennan 2002), and to Shennan's chapter in this book. Central to the position I want to develop will be a revisiting of the nineteenth century, both in terms of the intellectual genesis of modern archaeology and in terms of the social and historical context of the thought of Charles Darwin.

The Entangled Bank

Shennan throws down a challenge at the end of his book which I would like to take up:

> On the last page of *On The Origin of Species* Darwin famously asked his reader 'to contemplate an entangled bank, with birds singing in the bushes, with various insects flitting about, and to reflect that these elaborately constructed forms, so different from each other and dependent on each other in so complex a manner, have all been produced by laws acting around us'. It is time for archaeologists to stop being beguiled by the complexities of their own entangled banks and to start producing accounts of human history that make use of the principles Darwin established. (Shennan 2002:271)

This is a powerful plea with which I have much sympathy. However, the target of Shennan's comment is in my view misplaced. Throwing one's hands up at the complexity of the bank is not a position easily identified with interpretive archaeologies; when interpretive traditions protest against what they see as excessive reductionism, the alternative proposed is never simply to allow oneself to be beguiled by complexity. Rather, being beguiled by complexity is a position of mindless particularism, of the accumulation of data devoid of an explicit theoretical framework. Such an inductive particularism can only be intellectually credible if allied to the expectation that a future generation will put all the pieces together, or to a naïve empiricism in which no theoretical labour is needed as the data will simply speak for themselves. Such an explicit position ran out of intellectual steam a long time ago, but is still regrettably common in much of traditional archaeology and continues to drive much archaeological enquiry at an implicit and untheorised level (I discuss this continuing empiricism in Johnson 2007 and 2010).

The interpretive response is actually very different. It is (at least for this author) to reframe the challenge—in this case, to ask questions about Darwin's and Shennan's very powerful image of engaging with the historicity of the entangled bank. I am going to argue in what follows that *a central terrain of engagement between interpretive and Darwinist archaeologies is the nineteenth-century historical context of Darwin's ideas*. When understood in their context, these ideas are actually central to an understanding of their application to archaeology now and in the future, though not necessarily for the reasons Darwinian archaeologists themselves choose to put forward.

The interpretive response starts by decentering the claims to a 'scientific' superiority made by Darwinian archaeologists. Before the 'discovery' of genetics, evolutionary theory had a quite tenuous claim to completeness and rigour. There was a central element to its arguments that remained quite unverifiable—it could not grasp the central question of the source of variation in the natural world. The missing element was genetics. The genetic causes of variation in the natural world were not known to Darwin, and would not be integrated into evolutionary theory until the early twentieth-century rediscovery of Mendel's work on genetics. It can be argued that his theory was incomplete. The literary and cultural critic Gillian Beer (2000:xviii) makes the point that nineteenth-century Darwinism was 'poised on the edge of metaphor' because the genetic basis of variation was not known.

Darwin's use of the image of the bank, then, was rhetorical, allusive and literary and must be understood in those terms. Indeed, it is commonplace to assert that Darwin cannot be understood without understanding the literary construction of his books, and there is an extensive

discussion on the relationship between nineteenth-century evolution and literature (for example Beer 1996a, 1996b, 2000). In possibly his most well-known passage from *The Descent of Man*, Darwin famously uses literary devices, including a portrayal of early humans as devils or imps, to make his point:

> The various facts have been given in the previous chapters. The early progenitors of man were no doubt covered with hair, both sexes having beards; their ears were pointed and capable of movement; and their bodies were provided with a tail Our progenitors, no doubt, were arboreal in their habits, frequenting some warm, forest-clad land. The males were provided with great canine teeth, which served them as formidable weapons. (Wilson 2006:894)

Observation of a central role for rhetoric and allusion in Darwin's work, and of missing elements, does not make it somehow less 'scientific'. After all, the history of science is replete with famous narratives featuring politics, passion, and personal rivalry (the discovery of the structure of DNA; Galileo's decision to write in Italian rather than Latin to appeal to a wider audience; Friedrich Kekulé's 'daydream' in which he 'discovered' the structure of benzene [Wilcox and Greenbaum 1965]). What it does do is to decenter the notion that there is a single way to do science, and that there is a straightforward and fundamental distinction to be made between the methods of 'Science' and those of non-science (cf. Colleran and Mace, this volume) After all, Darwinian archaeologists themselves have debated the notion of Darwinian explanation as metaphor (Bamforth 2002, 2003; O'Brien and Leonard 2003). If metaphor is an unacknowledged but nevertheless central element to Darwinian archaeology, then Darwinian archaeology remains a valid project, but it must give up the high ground: Darwinian claims to producing materially better explanations than their fluffy humanist counterparts must be severely eroded. Such a point is familiar, even uncontroversial, in many circles; however, I am going to go on to argue that one of the least attractive aspects of modern Darwinian archaeology is its (rhetorical) assertion of just such a 'scientific' high ground and its (rhetorical) denigration of others as standing outside this ground.

So the way is open to the asking the classic question of the interpretive turn, the question that directs us to the intellectual, cultural and political framing of any given set of ideas—in other words, to context: why did Darwin choose the image of the entangled bank?

Darwin's image is historically particular. You get entangled banks in the temperate surroundings of the English countryside not, arguably, in the plains, deserts and rain forests of the world. Recent scholarship has emphasised how the material for Darwin's insights came from the

local, English landscape just as much as his better-known voyage in the *Beagle* (Boulter 2008). There is no obvious physical correlate for his bank, unfortunately (such was the impact of Darwin's image that other writers imagined themselves to be looking at just such a bank in other parts of the country, for example, Williams-Freeman's pioneering work on field archaeology in Hampshire [Williams-Freeman 1915:90]). I will take it as one of the banks to be found in the countryside on the borders of Kent and Sussex around Down House, Darwin's home. When one visits Down House or reads much of the biographical literature around the Darwin legend, Darwin's walks around this landscape, and in particular his pacing up and down his 'thinking path' at the bottom of the property, are referred to repeatedly in what is a standard trope of the history of science in which the intellectual journey of the lonely genius is told in terms of toil and enlightenment (for example, the classic introduction to Darwin [Miller and Van Loon 1982] and the English Heritage guidebook to Down House [Morris et al. 1998]).

As such, the entangled bank may well have been a product of enclosure, a banked hedge thrown up in the eighteenth century. Banks of this kind were part of a historical process of commodification and privatisation of the countryside. In much of England, they represented a sudden break with the rural practices of the past, cutting across previous community-based patterns of farming the land through large open fields and commons within which ordinary people held a cluster of rights and mutual obligations. Hedging and ditching, then, marked and enforced private ownership of the land. For this reason, the creation of the hedges and banks of enclosure was a contemporary topic of engagement with Romantics, Marxists and others (Johnson 1996, 2007:11–14).

Perhaps, even more suggestively, the entangled bank was one of those which separated Darwin's home from the neighbouring Lubbock family estate. John Lubbock is routinely cited as one of the great 'founding fathers' of prehistoric archaeology; his work brought together an appreciation of time depth and the principles of evolution from Lyell and Darwin with the record of prehistoric stone tools and other implements and their parallels with ethnographic examples. Lubbock's neighbourly relationship with Darwin and the relationship between Darwin's work and Lubbock's seminal *Prehistoric Times* are well known (Trigger 2006:171–76), though modern Darwinian archaeologists explicitly disavow Lubbock's governing model of progressive cultural evolution through stages.

What interests me about the neighbouring Lubbock estate is that it is laid out according to the principles of Romanticism. It comprises grassland interspersed with clumps of trees, laid out across the rolling downland. Such landscapes were artificially laid out, but strove to

appear natural. Further, such English landscape gardens were held by the culture of the time to be emblematic of what were considered to be distinctively English values of political freedom, the contrast being drawn with French and Italian formality and geometrical pattern being the product of political tyranny and, of course, what was perceived as the pernicious and foreign influence of Catholicism. For many, under the influence of the eighteenth-century antiquarian Stukeley, such 'natural' landscapes were also a return to the appearance of the prehistoric or (what was held to be) Druidic landscape before the arrival of the Romans (Johnson 1996:149–51; Williamson 1995).

To understand the bank in the manner of interpretive archaeology, then, is not to throw up one's hands in despair at its complexity and abandon any attempt at explanation, but rather to engage with human history. Attempts by Darwinists to engage with the history of enclosure and of landscape change in the postmedieval period do exist, but have been faltering. There is a useful literature on the formation of the medieval common fields as a response to risk, and conversely on the dissolution of the open fields and their replacement with enclosed fields as being due to the growth of a market economy. The growth of markets, it is suggested, meant that individual farmers could always buy their food in lean years (McCloskey 1979; see also Dahlman 1980 and Winterhalder 1990 for a comparative view). There has also been a suggestion from human behavioural ecologists that the apparently 'natural' layout of such landscapes, with grassland interspersed with clumps of trees, appeals to the hardwiring of the human brain in that it repeats the spatial ordering of the African savannah (Heerwagen and Orians 1993). Neither of these attempts convince fully, particularly in the latter case.

The Goals of Archaeology: Fundamentalism/Plurality

Is this exploration of the entangled bank anything more than an intellectual parlour game? I suggest that it is directly pertinent to an exploration of the goal or goals of archaeology. One such goal, possibly the central goal, has to be to account for past human behaviour in a rigorous and scientific manner. Many Darwinist archaeologists make the claim that it is not just another narrative to be placed alongside the plurality of so much of modern archaeology, a plurality which many Darwinist and more broadly processual writers see as potentially disabling in that it opens the door, in their view, to an unbridled relativism. I have argued above that such a claim needs to be decentered. Further, I think that this claim of scientific rigour, and conversely the imputation of a lack of scientific rigour to much of interpretive and indeed culture-historical archaeology,

is paradoxical in nature. Scientific rigour is one of the key attractions of much Darwinist thinking and also one of its key weaknesses.

Scientific rigour, or more accurately the perception of science as a single, fundamentally 'correct' way of doing things in which other ways of thinking are possible but cannot be placed within the domain of 'Science', is loaded ideologically. Depending on who is speaking, rigour turns into dogmatism, method turns into inflexibility, principle turns into fundamentalism. Your thought is inflexible and dogmatic, whereas mine is rigorous; conversely, your thought is fluffy, whereas mine is flexible and eclectic. Rigour in itself, then, is not a necessarily or automatically desirable attribute; it is one of a linked set of rhetorical terms of praise or denigration whose worth depends on the observer's viewpoint.

More fundamentally, 'scientific rigour' is a zero-sum game. The more 'rigour' the archaeologist has, the less she or he can be argued to engage eclectically with the varied experience of the world, and the less he or she engages constructively with what archaeologists actually do. The most rigorous Darwinist programmes are those of Robert Dunnell and his students, often writing from North America, who pair a commitment to a Darwinist archaeology with a commitment to Science with a capital S (for example Barton and Clark 1997; O'Brien 1996). In such views, Darwinism becomes a fundamentalist and sectarian programme that, by its own affirmation, fails to engage with the discipline as a whole (for example in its rejection of traditional culture-historical concepts such as culture, phase and type, concepts that for better or worse continue to structure a large part of traditional field and analytical enquiry; O'Brien and Lyman 2002 discuss this issue). Shennan's *Genes, Memes and Human History* is in implicit agreement with much of my assessment of the Dunnell school, since there is no reference to Dunnell's writings in his book (apart from a single 1979 article; see Preucel 1999 for an effective critique of selectionist archaeology).

The alternative, which Shennan follows and which is more characteristic of British writing on evolutionary archaeology, is to pursue a much looser and more eclectic mix of Darwinist ideas, seen also for example in the varied work of Stephen Mithen (2005), Clive Gamble (2007) and indeed a great many of the 'evolutionary' contributors to this volume. As a result, Shennan's suggestions and arguments are more engaged and more convincing—more productive of dialogue.

I fear, however, that Shennan, like all archaeologists (including myself) trying to work in the difficult space between scientific fundamentalism and a disabling plurality, is trying to have his cake and eat it too. Shennan holds on to a commitment to science, most explicitly in his rejection of empathy, prehistoric ethnography and (it is implied) agency theory in the first few polemical paragraphs of his Introduction (2002:9–10) and

in the opening part of his paper in this volume. Shennan also asserts that we should play to archaeologists' strengths—that the archaeological record is best suited to explorations of long-term stability and change, and conversely, unsuited to what he sees as 'prehistoric ethnography'. As a scientist, my response is that I have no a priori expectations of what I will find in the archaeological record. We might indeed find patterns that suggest very long-term stability or trajectories of change, for example, as Shennan has convincingly explored for the Palaeolithic (Shennan 2001), or we might find evidence of sudden, qualitative transformation, as any appreciation of the Darwinist concept of punctuated equilibrium would affirm. It is difficult to stand in the middle of the ruins of the Forum in Rome or in front of the north façade of Bodiam Castle, and assert on an a priori basis that an exploration of the values, meanings and lived experience of such monuments, and an understanding of agency, is somehow a less valid—or a less intellectually rigorous—requirement than what are often rather tortured attempts to fit such phenomena into a framework derived from Darwinian evolution. Whatever Shennan's rhetoric in the first and last few pages of the book, the mix of ideas presented in *Genes, Memes and Human History* comes very close to abandoning any claim to scientific fundamentalism: it comes closer, then, to being just another narrative. Kristian Kristiansen says something similar when he attempts to locate both Darwinist ideas on the one hand and agency theory on the other not as all-encompassing explanations but rather as suited to particular phases or elements of the archaeological record (Kristiansen 2004).

More fundamentally, what can be called 'the eclectic turn' opens Darwinist archaeology up to the same issue that confronted Darwin and his followers in the period before the discovery of genetics. Shennan discusses the issue that there is no obvious or reliable counterpart in human culture to the idea of the gene. He explicitly proposes the idea of the 'meme' as its counterpart (2002:7–8 and succeeding pages). This idea is attractive, but it is unclear how it can attain more than the status of an extended metaphor, however fertile or persuasive such a metaphor might be (see Cochrane, this volume). Indeed, Shennan gets tired of the metaphor very quickly; it makes no appearance after page 64 of his book.

Such lines of argument are not ones that many interpretive archaeologists will have an a priori or baseline objection to: it leads archaeologists to some interesting explanations of particular episodes or phases of the prehistoric past. But insofar as they rest at their core on a metaphorical conception of reproduction through memes rather than genes, models of this kind implicitly retreat from any attempt or pretension to a single method or scientific 'high ground'. An archaeological audience, then, is quite entitled to be sceptical when Darwinist arguments become excessively complex or counterintuitive. Not only do such arguments

abandon the scientific high ground, they appear to move away from the economy and simplicity that is such a hallmark of the best of Darwinist thought (see Layton, this volume, for a similar argument). For example, are Bronze Age leg chains about women's choice in reproductive success (Shennan 2002:204)? No they are not, they are about male domination. Is handaxe symmetry about male competition and display (Shennan 2002:197–98)? No, it is about a recursive relationship between agency and artefact. Both these Darwinist arguments push the evidence to an interpretive distance, and propose a complexity of archaeological explanation that would make the most ardent postprocessualist blush (see also the very complex language and equations to be found in Bentley and Shennan 2003, which equals the difficulty of the most 'obscurantist' postprocessual writing, or the interpretive gaps found in Kohn and Mithen 1999, where the argument rests on a set of interesting but unproven assertions about what early hominid behaviour might have been). They remind us of a much older theoretical decline and fall, that of Marxism, from the beauty, economy and structure of the classical Marxist theory of history, fashioned again in the later nineteenth century, to the more complex and obscure later Western Marxist traditions.

A metaphorical conception of Darwinist explanation means, then, that there is no fundamental, baseline distinction to be made between the core nature of Darwinist arguments and those of interpretive archaeologies. A good example in this regard is Fraser Neiman's account of 'the lost world of Monticello' (Neiman 2008). Shennan is correct to single this article out for praise as an important and compelling argument (Shennan, this volume). Neiman's discussion of the economic and social strategies of free and enslaved peoples in the seventeenth- and eighteenth-century Chesapeake and the way these strategies are manifested in the archaeological record is intelligent, insightful, and reflects a deep understanding that comes from a lifetime working with the material. The trouble is, it's not particularly Darwinist. A rigorous and even brutal editor could strip out from the paper elements of jargon, references to evolutionary game theory, rephrase 'fitness interests' as simply 'interests' and 'demic spread' as simply 'spread' and insist that 'emphasising the causal connection between artefacts and their meaning' (Neiman 2008:164) does not necessarily equate to an evolutionary approach, any more or less than it equates to, say, a Marxist approach. Though it does not reference it, Neiman's detailed and perceptive discussion of Monticello shares much in common with Upton's discussion of different axes of status in Monticello I and II and the derivation of these axes from local and vernacular traditions, with an emphasis on symbolic and cultural divisions (Upton 1998:20–38). Upton's account is also centred on the tensions between master and enslaved servants, and comes

from an intellectual position within architectural, folklife and historical studies very similar to that of interpretive archaeology.

The point here is not that Neiman is incorrect or somehow insincere in his claim to be inspired by Darwinist thinking in his interpretations. It is, rather, that there is no *necessary* contradiction between a more eclectic approach to Darwinian arguments and elements of interpretive archaeology. Neiman, like Shennan, makes strong rhetorical claims about the insufficiency of interpretive approaches to historical archaeology, but his objections seem to be centred on what he sees as an undue fragmentation of different interpretive theories and a lack of engagement with the archaeological record in the work of some scholars. These points are arguable and I have some sympathy with the implicit position taken, but they do not add up to a sustained demonstration of the superiority of a Darwinian historical archaeology.

I am arguing here that we have returned to the entangled bank—that the more eclectic versions of Darwinism actually represent a return to an intellectual space last occupied by late nineteenth-century Darwinism before the development of genetics. Shennan himself comments that the book *Genes, Memes and Human History* 'is a very 19th-century one in the issues it addresses. If that is so, it is because the 19th-century pioneers of anthropology asked most of the right questions' (2002:262; he very sensibly avoids implying that they found anything like the right answers). For the nineteenth century, as Shennan implies, the role of anthropology, and implicitly archaeology, was to be reductionist: that is, to reduce the study of human behaviour to a single evolutionary scheme that could be expressed simply in terms of a few fundamental drivers and variables—the set of processes that produce the entangled bank. I do not mean to be pejorative or dismissive here; 'reductionism' has become, like 'empiricism', 'positivism' or indeed 'relativism', a routine term of abuse that often serves as a poor substitute for the hard work of critique. Any attempt to explain the world involves a degree of reductionism, and conversely any accommodation of 'theory' to 'practice', however defined in the human sciences, involves an amelioration of that reductionism.

In many respects, though not in all, nineteenth-century Darwinism can be represented as a reaction to the concerns of the eighteenth-century Romanticism that produced Lubbock's landscaped park. Romanticism, particularly in its more vulgarised and popular forms, proceeded from assertions about what human beings were, fundamentally, and concerned itself with questions of human essence and Man's relationship to Nature. Discussion of these questions of ontology and essence, of what humans essentially are, their essence and relationship to the world, often bordered and continues to border on the metaphysical. In its vulgar forms, Romanticism habitually spoke about the world in emotive and mystical

terms, and in particular about the importance of the individual and of emotional empathy between human beings (Wordsworth's famous maxim was, 'What is a Poet? He is a man speaking to men'). As such, its project is largely recognisable to a modern archaeological audience, because the concerns of Romanticism are picked up today in interpretive archaeology and in much phenomenological writing. When Shennan condemns recent writing on phenomenology as 'attempting to empathise with people's lived experience' (2002:271), he is implicitly levelling the charge that interpretive archaeology is following a Romantic agenda (this argument, which I feel has some validity, is developed more fully in Johnson 2006 and 2007; see also Hamilton, this volume).

What I am suggesting here is that much of the gap between interpretive and evolutionary approaches apparent in discussions of the goals of archaeology today stems in part from a replaying of much earlier debates and concerns at the genesis of modern archaeological enquiry in the late eighteenth and nineteenth centuries. This is not to imply that archaeological theory is going around in circles or that debate is useless; it implies rather that many of the questions we are concerned with in archaeology remain enduring ones, and these enduring concerns are about the very widest questions of the discipline's role now and in the future. Such an argument shares some intellectual ground with Kristiansen (2008, particularly Figure 2).

These questions are enduring because they address the same enduring problems. Nineteenth-century archaeology and anthropology sprang from the twin roots of Romanticism and Darwinist science as two opposed but related responses to the world around them. European men and women saw the massive changes and transformations in the world around them, the industrial and agrarian revolution, the development of colonialism and Empire; their intellectual reaction was either to attempt to grasp the authentic roots of a culture and an unmediated Nature they felt was being left behind (the Romantic response) or to order, systematise and legitimate the study of human transformation into an evolutionary system and ladder. I am suggesting that archaeology remains, then, in the grip of these fundamentally opposed but also strongly related questions. However, such an observation raises a further issue: is archaeology fundamentally about the cultural and political concerns of the present or about the past?

The Goals of Archaeology: Past/Present

One of the points Shennan makes strongly is that archaeology has lost sight of its role in explaining and accounting for the past, and I think he has a point. For me, the central goal of archaeology is very simple: to

find out about the past through the study of material remains that are here with us, in the present. Further, I am interested in process. I want to know what factors shaped human history. I want to know why human social development took the path that it did.

Such a stance is not one that is common to all strands of interpretive archaeology, however defined. A series of famous statements by Shanks and Tilley veer close to the line that archaeology is about critique in the present rather than the past—though in fairness, they never quite cross this line and elsewhere deny such a dualism should be drawn in any case:

> 6.5 Archaeology is nothing if it is not critique.
>
> 6.5.1 We do not argue for truths about the past but argue through the medium of the past to detach the power of the truth from the present social order. (Shanks and Tilley 1987:213; see also 1987:189)

Julian Thomas also shies away from such strong formulations (2000:6–7), while for the Lampeter Archaeology Group (1997), no one ever claimed to judge arguments on exclusively political grounds.

There are three points to be made here. First, there is no necessary epistemological conflict between focus on the past or on the present. Lewis Binford (1987) was the first to rightly insist that we do not study the past, we study the present, and the position that the past is constructed by archaeologists independently of the present is held by only a very few hard-liners—again, it is characteristic of a mindless atheoretical particularism rather than by anyone engaged with theoretical debate on either side. In a sense, the choice to stress one or the other, the past or the present, is a rhetorical one, an act of positioning made by different people and groups for different reasons—which should prompt the observer, just as we have done with Darwin's entangled bank, to ask about the framing of the question, to enquire why this person or group chooses to position themselves in such a way. Contrast these two positions and ask why the speakers choose to foreground certain concerns:

> Our core strengths are our diversity and international representativeness, our dedication to redressing inequality and our commitment to innovation, critique, experiment, and excellence. An appreciation of the richness obtainable from global diversity and a willingness to face the challenges of engaging in social justice issues are integral to WAC. (Smith 2003)

> The main aim of archaeology is to obtain valid knowledge about the past
> The growth of 'cultural heritage' studies, important though they are, should [not] be allowed to displace an archaeology concerned with finding out about the past. (Shennan 2002:9)

Second, there is an issue of plurality, or to put it another way, a hierarchy of knowledge. Whatever my view, or Shennan's view, there remain other positions, and there remain different views on the extent to which archaeologists could or should attempt to marginalise, 'redline' or shut these other positions down. This concern is seen most explicitly with issues relating to indigenous archaeology or with theoretical voices outside the English-speaking world. The pretty routine position taken by interpretive archaeology is to endorse plurality, but I do want to raise a concern with this: an unthinking plurality or 'tolerance' can lead, in practice, to a refusal to take other positions seriously and a slide into an archaeology that refuses to challenge or critique. This point was made by Shanks and Tilley in critique of Hodder back in the 1980s and surfaces indirectly in Marxist attacks on postmodernism's endorsement of plurality; it is a position hinted at by Maria Berglund (2002).

The limits to plurality can be seen in the 'debate' over intelligent design. Both Darwinists and interpretive theorists have a problem with intelligent design. Steven Jones has famously given up attempting to debate with creationists, a failure he attributed to their intellectual obstinacy, but that to the outside observer suggests that something deeper is going on here. Darwinists have nothing to say to its proponents. 'Postmodern relativism' is often accused of creating an interpretive space within which intelligent design can be given 'equal status' (for example, Gross and Levitt 1997). But it is difficult to point to a single 'postmodernist' who explicitly endorses any kind of intellectual credence being given to such arguments (though not, admittedly, impossible: the social constructivist Steve Fuller has intervened in these debates [Fuller 2008]). Conversely, however, interpretive responses fail to convince; the criteria by which indigenous groups are encouraged in a plurality of views and yet religious fundamentalists are denied analogous rights remain quite unclear, except on the grounds of judgments made in the political present (see the discussion in Layton 2004). Proponents of intelligent design themselves use a rhetoric that draws from both sides—it is laden with scientific jargon, yet makes powerful appeals to 'teach the debate'; it has been suggested by cultural anthropologists that it is precisely this political flexibility that accounts in part for the greater success of creationism in North America (Coleman and Carlin 2004).

Third, there is an issue of history, or, more loosely, a consciousness that archaeology is never produced in a cultural or political vacuum. Again, we return to the late nineteenth-century context of Darwinist thought and its association with racist and other politically repugnant standpoints, for example, in the work of Pitt-Rivers. Most would agree that moral condemnation of such forms of politics is no substitute for serious and empirically informed analysis of statements made about the

past. Equally, however, most would I think reject the other end of this spectrum, the empiricist fallacy—that is, that accounts of the past can be evaluated in a vacuum, completely independently of present cultural and political conditions.

What this means, I think, in terms of the goals of archaeology, is that the question of whether archaeology is 'fundamentally' about present or past is a philosophical red herring. This does not mean that it is unimportant. The rhetorical strength with which Shennan and others question recent stress on heritage, when compared with the rhetorical strength with which activists in the WAC foreground political issues, is telling us something very important about the nature of intellectual life today. I'll return to this point in the Conclusion.

The Goals of Archaeology: Human Beings

A second element of the nineteenth-century divergence is in its understanding of human beings. One of the most frequent clichés in archaeology is that its goal is to understand human beings, not artefacts (though it has to be said that such claims invariably come on the last page of books otherwise utterly devoid of human interest or understanding). Arguably, such a goal was central to New Archaeology, most obviously in the citation of Willey and Phillips's dictum that 'archaeology is anthropology or it is nothing', referred to in Binford's classic article 'Archaeology as Anthropology' (1962:217; Flannery's [1967:120] selection of the ecosystem behind both the 'Indian' and the artefact gives a rather different emphasis). It is also found in many versions of interpretive archaeologies, most obviously forms of agency theory; and it is a position I am passionately committed to. However, it is not central to all forms, and one might point to a tension between an agent-centred approach derived from the work of Giddens and the antihumanism present in strands of Derrida's and Foucault's work. (Foucault was not, in my view, an unqualified antihumanist; he traced the different ways the human subject was constituted within different discursive formations, but never denied the existence of a human subject). A common tactic is to deny the human/object dualism by asserting, in different ways, that objects have agency—a position that usually traces its intellectual ancestry back to the work of Alfred Gell (for example, Sillar 2004) and/or actor-network theory (Latour 2005; see also Gardner, this volume).

I think that there is a potential convergence here between certain forms of postprocessualism and evolutionary archaeology. The decentering of the human subject that is so central to poststructuralist and postmodern thought is most powerfully demonstrated not by Derridean critique but rather by the concrete history of human evolution. The

archaeology of early hominid species carries the powerful message that there is no essential core to being human; it is the emergence and development of a combination of a number of different traits, with a process of Darwinian selection acting partially or totally upon this development (Gamble 2007). And if that process is a partly contingent one, then it could be argued that there is no necessary essence to humanity—a strikingly postmodern and anti-essentialist conclusion that has been reached by a Darwinist argument. I'll return to this question in my Conclusion.

Genes, Memes and Human History approaches the question of human beings in a more direct way: in parts, it advocates a return to culture history and artefact typology, most obviously in its visual language. Several diagrams show the development and evolution of artefacts almost as if the pots have developed little legs and are running around (as well as little genitalia and are reproducing genetically/memetically modified versions of themselves). His intellectual project, then, ends up being profoundly opposed to early New Archaeology in this sense, whereas interpretive archaeologies are the direct descendants of New Archaeology (also of early New Archaeology's confidence to say things about past lifeworlds, which Shennan rejects as an attempt to do prehistoric ethnographies).

Now of course, culture history always paid lip service to human beings—'the Indian behind the artefact'. But in practice, it tends not to do so. For Mortimer Wheeler, ex-Director of the Institute of Archaeology, 'dead archaeology is the driest dust that blows' (1954:13). But Wheeler's work avoided dead archaeology not by reformulating culture history but, I would argue, because his genius transcended the theoretical limitations of the intellectual framework.

Conclusion: The Discipline's Role in the Present and in the Future

Surely, if the discipline has any role now and in the future, it is to engage with concrete issues at points where different dualities meet: present and past, culture/politics and scientific knowledge, human culture and natural environment.

Whatever one's theoretical views, archaeology holds a very privileged position in the humanities and sciences. It enjoys an intellectual and cultural space where instead of asserting a priori notions about what makes us human, they can be subjected to empirical investigation. What is interesting in this context is the rather hoary old point that archaeological theory of whatever stripe has relatively little impact on practice, and more broadly, little impact on the public perception and role of archaeology in contemporary debates. This view is debatable, but insofar as it holds any validity we can point towards a striking common ground

between interpretive and Darwinist approaches. Both make extremely strong claims for their intellectual importance, yet neither have had the wider impact that such claims, if true, might warrant.

Clifford Geertz wrote many years ago that the debate over relativism in anthropology was an exchange of worries rather than a meaningful engagement of views (Geertz 1984). What I have tried to show in this paper is that the debate over Darwinist and interpretive ideas has very deep roots of at least a century and a half. What I am proposing for the future, then, is that the time is surely overdue to move away from an exchange of worries to a substantive exploration of past humanity. Perhaps, beneath the discursive consciousness of the archaeological community, this is already happening; perhaps this is why, since the post-processual polemics of the 1980s, there has been no further (claimed) revolutionary change in archaeological thought. If so, what we have seen is not intellectual stagnation, but the developing maturity of the discipline.

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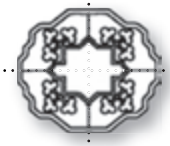

CHAPTER FIFTEEN

An Evolutionary Perspective on the Goals of Archaeology

Stephen Shennan

Introduction

There are many different goals of archaeology but insofar as 'archaeology stakes its claim to be a responsible and intellectually rigorous discipline on its ability to produce convincing accounts of the past' (Johnson 2006), some goals are likely to lead to more convincing accounts than others. As I have argued elsewhere (Shennan 2002), much recent archaeology in the postprocessual tradition has effectively set itself the goal of producing 'tabloid human-interest stories' about past people's supposed lived experience, placing archaeologists in the role of ethnographers of a lost 'ethnographic present', struggling hopelessly to overcome the problems posed by the fact that the people they would like to talk to about their perceptions of landscape and other topics are long dead and most of the residues of their lives long decayed (see, e.g., Fleming 2006 for a detailed critique of much recent landscape archaeology in this vein; see also Brück 2005 for a critique of these approaches from within the broad postprocessual tradition). Goals such as this, at least for that vastly greater temporal extent of the archaeological record that precedes written documents, are unlikely to produce convincing and intellectually rigorous accounts.

This is more likely to be achieved by setting goals that play to archaeologists' strengths. To identify and explain patterns of stability and change in the material record of human existence (especially over the long term) seems to me to be such a goal, and it is one that many would endorse. It leaves open the possibility of a wide range of views about the best approaches to achieving it, but all of them involve theoretical commitments of one kind or another that provide the foundations for the approach taken. Here I want to argue that the best theoretical

frameworks are those that are most productive, in the sense of generating open-ended programmes of empirical archaeological research that produce convincing and intellectually rigorous results, and specifically that modern cultural evolutionary theory meets this requirement better than most of the available alternatives.

By theory here I mean something more specific than what is often meant in archaeology: a set of well-founded principles that provide a basis for explaining patterns of variation in the world. In the case of the biological world, the edifice of Darwinian evolutionary theory that has been built up over the last 150 years provides the principles and has produced a variety of remarkably productive research programmes at all levels, from the microscale genetic to macroscale palaeontological history covering millions of years. The development of the idea that explaining patterns of stability and change in the material record of human existence can be encompassed within the same framework is much more recent, although it has early precursors. It depends on recognition of the fact that, like life on earth in general, human culture changes through a process of 'descent with modification'. Biological evolution is not a metaphor for cultural evolution. Both are instances of evolutionary processes based on inheritance, mutation/innovation, selection and drift, though the specific mechanisms that operate in the different domains often differ (cf. Beinhocker 2006; Dennett 1995). However, the fact that the intensive development of evolutionary research in general, and evolutionary theory in particular, has a much longer history in biology means that it has been a sensible strategy for cultural evolutionary theorists to start with ideas and methods from the former and explore the ways in which they need to be modified to accurately represent cultural processes (Boyd and Richerson 1985; Cavalli-Sforza and Feldman 1981).

In the case of culture, the inheritance mechanism is social learning: people learn ways to think and act from others. Of course, the routes through which culture is inherited are much more diverse than those for genes (Cavalli-Sforza and Feldman 1981), and different routes have different consequences for the patterning of cultural change through time. Variation in what is inherited is generated by innovations. These innovations may be unintended copying errors, but they can also be intentional changes, perhaps arising from trial-and-error experimentation or 'deductive tinkering' (Beinhocker 2006:249–52), which lead an individual to stop performing a task the way she or he had previously learned and to start doing it differently, or even to do something different altogether. Whether this novelty will be widely adopted depends on a range of selection and bias mechanisms, many of which have no equivalent in genetic evolution but whose existence and importance have formed the subject of major developments in cultural evolution theory over the last 30 years

(again, the foundations are Boyd and Richerson 1985; Cavalli-Sforza and Feldman 1981; Richerson and Boyd 2005 [for a less maths-intensive treatment]). It is important to spell out these mechanisms.

Natural selection in the narrowest sense affects humans as it does members of all other species, and its implications are the focus, in different ways, of evolutionary psychology and human behavioural ecology (for a broad overview, see Dunbar and Barrett 2006). However, natural selection can also act on cultural attributes, in the sense that those individuals who inherit or acquire certain cultural attributes may have a greater probability of surviving and/or reproducing than those who do not (Dunnell 1978); as a result, those cultural attributes will become increasingly prevalent. For example, it is clear that, in many parts of the world, adopting an agricultural rather than a hunting-and-gathering way of life led to greater reproductive success for those who adopted it in comparison to those who did not; as a result, the cultural traits that characterise agriculture spread and, in some cases, subsequently influenced genetic evolution (for example, the ability to digest lactose [e.g., Burger et al. 2007]). An analogous process of cultural selection can also operate if individuals with certain cultural traits are more likely, by virtue of those traits, to be taken as models for imitation than others, and these individuals in turn become successful models as a result. The traits concerned will become more prevalent even if they have no bearing on reproductive success whatsoever and, indeed, even if they are deleterious to it, because if a trait is passed on in a manner other than by parents to children, there is no reason for its success to depend on the reproductive success of the individuals concerned. For example, if celibate priests are more likely to be influential teachers than other adults and if, as a result of what they teach, their pupils are more likely to be celibate priests and teachers, then the values they teach will increase in frequency in the population.

However, in addition to these selection mechanisms, a number of 'bias' processes can affect what is transmitted; these bias processes are factors that affect what and whom people try to copy when they are learning from others. Thus, 'results bias' refers to the situation in which people look at what other people do (for example, the crops they plant), compare the results with what they are doing themselves, and then change what they do because the other way of doing things seems to be more effective; social norms or interaction strategies may also become more or less prevalent on the basis of the returns they give. 'Content biases' are affected by features of transmissible phenomena that make them intrinsically more or less memorable for reasons relating to the structure of the mind or the strong reactions they provoke; examples may include fairy tales or so-called urban myths. 'Context biases' are aspects of the context of learning that affect what is transmitted; thus,

something may be copied simply because the person initially doing it is prestigious ('prestige bias') or because it is what most people do locally ('conformist bias'). In the latter two cases, whether a particular cultural attribute or practice becomes more prevalent in a population has nothing to do with its intrinsic properties but only with the context of learning.

There is also a cultural equivalent of genetic drift. In other words, the frequencies of particular cultural attributes can change for essentially chance reasons not involving any preference for a particular attribute. Whom or what you copy may simply be a random choice dependent on whom or what you meet.

Finally, it is important to note the process of 'niche construction' (Odling-Smee et al. 2003), which refers to the situation in which the outcome of an evolutionary process at one time period changes the selection pressures operating in a following one. Thus, the spread of the adult human ability to digest lactose in some populations arose as a result of new selection pressures created by the domestication of animals and the consequent drinking of milk, i.e., a new humanly created cultural niche, leading to a process of gene-culture coevolution (Durham 1991).

The 'Meme's-Eye View'

As just described, all of these processes focus on the people involved in them. This is obviously an extremely important perspective, but it is not the only one. It is also important to look at things from what Dawkins (1976) called 'the meme's-eye view'—the perspective of the cultural attributes themselves. It has often been suggested that the definition of heritable cultural attributes is arbitrary and problematical (e.g., Weiss and Hayashida 2002, cited in O'Brien et al. 2010), and implies the existence of cultural 'particles'. However, as a number of authors have shown, even continuous traits (i.e., physical measurements) can provide a perfectly satisfactory basis for the operation of Darwinian processes (see, e.g., Eerkens and Lipo 2005; Henrich and Boyd 2002; Henrich et al. 2008). O'Brien et al. (2010) provide an extensive and important discussion of units of transmission and offer a useful shorthand definition: 'cultural traits are units of transmission that permit diffusion and create traditions—patterned ways of doing things that exist in identifiable form over extended periods of time' (see also Cochrane, this volume).

The 'meme', or, less snappily, the 'cultural trait' perspective matters for several reasons, not least because by and large, these culturally transmitted features are the data that archaeologists and anthropologists have available. The question then becomes, to what extent is it possible to identify the action of the various cultural evolutionary processes outlined above on the basis of distributions of variation in the present, or at various points

in the past, a possibility that archaeological data allow? In this field, as in so many others, theoretical modelling has far outstripped empirical investigation. Even demonstrating that a pattern of contemporary variation, as in the case of present-day ethnographic data, or a pattern of continuity through time, as in the case of archaeological data, results from the operation of an inheritance process as opposed to independent invention in similar conditions is not necessarily straightforward (see below).

The difference in perspective may be illustrated by a recent paper on the evolution of Polynesian canoes (Rogers and Ehrlich 2008) in which the authors identify the process acting on the functional canoe traits as stabilising natural selection. This is true from the perspective of the traits themselves, in that particular traits survive and are copied preferentially as a result of their greater functional effectiveness—something that could in principle be tested experimentally. However, viewed from the perspective of the human agents, the results presented do not distinguish between natural selection operating on the agents via their cultural traits, and thus on the future frequency of those traits, and results bias, as defined above. In other words, the process could have operated as a result of the makers and users of ineffective canoes drowning more frequently, thus leading to the demise of those designs, whereas groups with better-designed canoes, perhaps different communities, survived and colonised new islands; alternatively, it could have worked through people observing the performance of different canoe designs and preferentially copying those they perceived as more effective. The latter would potentially be far faster, and the implied timescale difference could provide a basis for distinguishing between the two processes.

The Cultural Evolutionary Research Programme

The framework outlined above leads to an interdisciplinary research programme made up of three different strands whose results feed back into one another. First, is the need to characterise the evolutionary processes that produce variation in human cultures, societies and economies in space and time. As noted above, this characterisation is far less developed than in evolutionary biology, and the outline of processes given above is very generalised. Making progress here involves, for example, carrying out psychological experiments to identify the specific factors affecting the cultural transmission process and the extent to which it leads to successful outcomes (e.g., Mesoudi and O'Brien 2008; McElreath et al. 2005) or ethnoarchaeological studies of patterns of social learning and their consequences with respect to different aspects of material culture (e.g., Gosselain 2000 [despite his antievolutionary stance]; Roux 2007; Tehrani, this volume). Of course, processual archaeology is very familiar

with this sort of approach since it corresponds closely to Binford's (e.g., 1981) middle range theory.

The second strand involves identifying the consequences of the repeated operation of those processes in different conditions by means of modelling (see, e.g., McElreath and Boyd 2007). This is of central importance because the consequences of the repeated operation of specific processes cannot simply be intuited or derived from thinking through the consequences of verbal descriptions (see, e.g., Glatz et al., this volume). Because much of the cultural evolutionary literature is in this mathematical mode, it is unfortunately rather inaccessible to many archaeologists, but important specifically archaeological work in this domain is represented, for example, by modelling the consequences of drift processes on change in the frequencies of discrete cultural traits (Neiman 1995) and change in the values of continuous ones (Eerkens and Lipo 2005).

The final strand is more conventionally archaeological and involves using an understanding of the processes and their consequences to explain patterns of stability and change at particular times and places in a number of interrelated domains. One such domain concerns the histories of culturally transmitted practices and norms, and represents what may be called 'neo-culture history'. The identification of culture-historical patterns in the various parts of the world has been one of archaeology's greatest achievements, but traditional culture history had very weak descriptive methods and explanatory mechanisms at its disposal (Lyman et al. 1997). The outline of cultural evolutionary processes given above provides the basis for recognising that different factors affect the differential inheritance and thus prevalence of different cultural practices, or even the same practices in different regions (e.g., Bettinger and Eerkens 1999), and that the cultural patterns prevalent in specific regions at specific times can be the result of the linking together of different cultural packages with different histories (Boyd et al. 1997). The tools of evolutionary classification provide a basis for disentangling these different histories, because they provide methods for tracing patterns of descent with modification. Developed in the context of defining relationships between species in such a way as to reflect their evolutionary descent, evolutionary classification procedures have been applied to the reconstruction of language trees; to tracing the relationship between manuscripts copied from one another, on the basis of similarities and differences between them in the copying errors they show; and to various cultural attributes (see, e.g., Gray et al. 2007; Lipo et al. 2006; Mace et al. 2005).

A second domain concerns the history of human populations. Perhaps paradoxically, given what has been said above about the complexity of cultural evolutionary processes, the perspective also gives new life to that explanatory mainstay of traditional culture history—the idea that cultural

change is associated with population change, the best-known recent example of this argument being the Renfrew-Bellwood farming and language dispersal hypothesis (see, e.g., Diamond and Bellwood 2003). On the one hand, this stems from the recognition that human populations, like those of any other living creature, are subject to natural selection; they expand when new reproductive opportunities arise, are subject to density-dependent checks but can overshoot local carrying capacities and then decline, or be negatively affected by adverse environmental conditions or competition from other populations. On the other hand, it has become apparent that some cultural attributes are strongly subject to vertical parent-child inheritance, or at least within-community inheritance (Tehrani, this volume), as a result of such processes as conformist bias, so that there really may be an association between specific cultural attributes and specific populations, as traditional archaeologists claimed, even if such attributes do not have a specific ethnic signalling function. In this case, such attributes will simply hitchhike as the cultural baggage that happens to be associated with a particular expanding, stable or declining population and will share its fate. Analyses of ancient DNA are beginning to provide independent evidence of such culture-population links (e.g., Linderholm 2008). However, even if cultural attributes are neutral and change simply as a result of drift (see Bentley, this volume), the fact that innovation and drift are dependent on the size of populations and the extent of their interaction means that demographic history remains central to any evolutionary perspective (Shennan 2000).

The third set of histories is concerned with social institutions and is, in a sense, the familiar agenda of social evolution. However, it is viewed from a different perspective, in particular that of the 'New Institutional Economics' (e.g., North 1981, 1990) and evolutionary game theory (see, e.g., Bowles 2004: Ch. 11; Skyrms 1996; and the extensive literature on altruism, e.g., Fehr and Gächter 2002), which examines the payoffs of different competing interaction strategies. At its core are social agents—individuals with norms, dispositions, knowledge and resources—who make decisions in their own interests in the light of constraints and opportunities, who sometimes innovate and at other times follow existing practices. Those social and economic strategies that produce beneficial outcomes for the agents will spread through the members of the groups concerned, and may themselves be replaced if circumstances change. Moreover, when outcomes are aggregated they can have consequences unintended by any individual social actor, including the emergence of qualitatively new forms of social and economic patterns. There are three reasons why these local actions produce broad-scale social evolutionary patterns: First, successful patterns of action spread because they are perceived to be successful by others in similar situations. Second,

selection on social, cultural and economic strategies will often result in similar outcomes in the face of similar situations. Third, processes of self-organisation operate in social interactions, leading to convergence on various kinds of 'attractors'; thus, for example, in Turchin's warfare model (Turchin 2003, cited in Kohler et al. 2009), where the prevalence of warfare is dependent on population size and the latter in turn is affected by the incidence of warfare, high levels of warfare will decrease population density, which eventually results in a diminution of warfare, leading again to higher rates of population growth. If there is no change in the local carrying capacity, then over time warfare prevalence and population density will fluctuate but will eventually converge on a stable equilibrium. This equilibrium represents an 'attractor' to which the system will converge (Kohler et al. 2009; cf. also Sperber 1996 for an example where the idea of an 'attractor' is used to explain the stability of the key elements of the Little Red Riding Hood fairy tale, despite all the variation in detail).

Accordingly, in order to understand specific large-scale transformations, we do not need to indulge in broad abstractions of the type prevalent in 1960s and 1970s neoevolution (see Leonard and Jones 1987 for a critique), but to carry out theoretically informed analyses of particular situations.

Last, there are histories of constructed niches. As indicated above, coevolutionary interactions in general can be seen as niche construction processes; thus, the adoption of agriculture with domestic animals created a new environment that favoured the spread of the genetic mutation that permitted the consumption of liquid milk into adulthood in populations of early farmers (Burger et al. 2007). In an archaeological context the altered physical environments produced by human action are the most obvious constructed niches (see, e.g., Shennan 2006; Smith 2007) and would have changed the selection processes across the whole range of human activities, from the optimal subsistence strategy to be pursued, to the best of various competing social strategies to pursue in terms of their payoffs, to the prevalent form of prestige goods.

If the question is asked at this point, what difference does it make to the goals of archaeology if you take an evolutionary approach of the kind described? the answer in some respects is not much, in the sense that archaeologists have always tried to write the sort of histories just described. However, the integrated cultural evolution framework proposed here provides us with a well-founded set of principles and microprocesses for understanding the histories, and it offers new analytical methodologies for studying patterns and processes. Moreover, the approach plays to archaeology's strengths in that it is the only discipline to provide long-term empirical records of cultural transmission and, in contrast to the largely normative descriptions of social

and cultural anthropology, routinely collects data on intrapopulation variation, the prerequisite for evolutionary processes of whatever kind to act.

Addressing Evolutionary Goals: Some Brief Examples

Identifying the Operation of Cultural Inheritance in the Past?

It cannot simply be assumed that all patterns of continuity and discontinuity in the past relate to the operation of cultural transmission processes; this was the mistaken assumption of culture history. When we are looking at changing relative frequencies of decorative motifs on pottery, we can be reasonably confident that this is the case because of what we know about the processes by which pottery making is learned and innovations are adopted; in effect, we have an ethnoarchaeological warrant at least to make this a starting assumption. If, on the other hand, we look at the changing proportions of wild to domestic animals found in prehistoric settlements through time, we cannot make the same assumption. Thus, in the case of later Neolithic Switzerland (Figure 15.1), it appears that the fluctuations in the proportion of wild animals are related to changing climatic patterns, such that poorer climatic conditions are associated with an increased emphasis on hunting, because of adverse effects on domestic staples (Huester-Plogmann et al. 1999). Similarly, it is likely that the changing relative proportions of fast and slow small game over the course of the Natufian at Hayonim cave (Figure 15.2) are not due to arbitrary changes in cultural preferences but stem from shifting prey choices, as a result either of 'results bias' modifying previous acquired choices or individual trial-and-error learning, both based on the evolutionary principles of optimal foraging, inasmuch as humans, like other animals, are sensitive to the costs and benefits of resource choice decisions. Thus, over a long period, predation pressure on slow small game increased, leading to a widening diet breadth and the inclusion of lower-ranked fast small game in the diet. However, the effect of the Younger Dryas cold climatic interval was to reduce human populations in the region. The result was that slow small game populations were able to recover and, given their better return rates, once again formed an important part of the diet (Stiner and Munro 2002).

Phylogenetic Analysis of the Plant Assemblages Associated with the Spread of Farming from the Near East to Europe

The adoption of an evolutionary approach makes possible the introduction of analytical methods from evolutionary biology that can provide a new perspective on long-standing archaeological questions. Phylogenetic

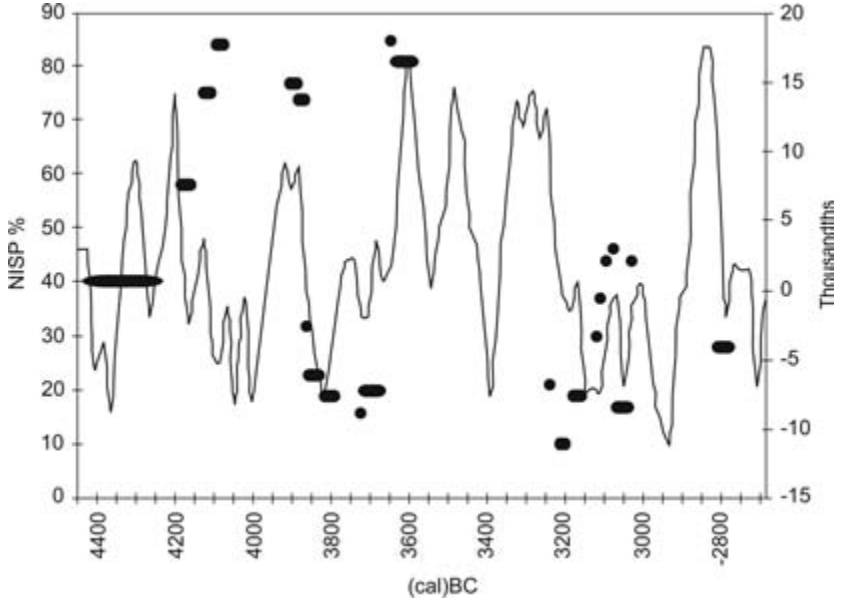

Figure 15.1 Fluctuations in the proportion of wild animals through time in faunal assemblages from the Swiss Neolithic (NISP %, filled circles and lozenges), against a climatic indicator, the delta ^{14}C value (in thousandths); higher values indicate cooler, wetter conditions. (From Huester-Plogmann et al. 1999.)

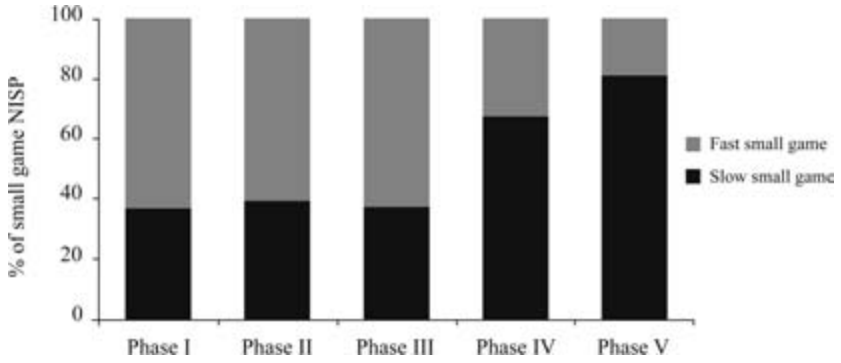

Figure 15.2 Relative abundance of small game types during five phases of Natufian occupation at Hayonim Cave, ordered from oldest to most recent. The Early Natufian is represented by phases I–III, Late Natufian by phases IV and V. Slow small game include tortoises only; fast small game are birds and small mammals. (From Stiner and Munro 2002:202, Fig. 9.)

techniques originating in biology reconstruct relationships among a set of taxa (e.g., species, genera, families), on the basis that the taxa are linked by a specific version of 'descent with modification' involving successive branching. New taxa emerge through the splitting of existing ones and the new taxa are reproductively isolated from one another. When new character states, for example the ability to breathe on land among amphibians, appear in a particular taxon they will be inherited only by descendants of that taxon and no other. It is the pattern of distribution of the resulting shared derived character states that provides the basis for reconstructing the tree of relationships.

In the case of the origin and spread into Europe of crop production systems based on domestic cereals and pulses, i.e., the 'founder crops' of Neolithic agriculture, there are several reasons to think that a branching model of descent with modification is an appropriate way of conceptualising the relationships among the different regional packages of crops and associated weeds found at the earliest farming sites. Most obviously, the spread of domesticated crops involved the transmission of the crops themselves, genetic descendants of the ancestral species of the Near East. Second, the cereal and pulse founder crops that spread into Europe had essentially a single regional origin. Third, the spread of agricultural systems based on those crops, whether or not it involved demic as opposed to cultural diffusion, was a dispersal/expansion process. Expansion processes are inherently likely to produce branching patterns of change as successively modified sets of features spread from one place to another. Finally, ethnographic work on traditional agricultural systems indicates that farming practices are usually both relatively conservative and transmitted vertically between generations (e.g., Netting 1993). It is thus highly probable that Early Neolithic farming practices were characterised by the same features (Bogaard 2004).

The crop assemblage data from 250 early Neolithic sites were aggregated into a number of regional groups (Figure 15.3); the resulting evolutionary tree is shown in Figure 15.4 (see Coward et al. 2008). By and large the branching pattern corresponds to a combination of geographical proximity and ecological-climatic similarity, so that archaeobotanical assemblages from areas closer to or similar to the Near Eastern source are less derived—have undergone less evolutionary change—than those from further away. However, the method also revealed interesting anomalies that had not previously been appreciated. Cyprus is remarkably derived for a region that is so close to the founder areas; that is to say, its archaeobotanical assemblages have undergone many changes compared with those of the nearby regions that were ancestral to them. This must say something about the nature of the processes acting during the dispersal. Similarly, another anomaly is represented by Bulgaria, the first

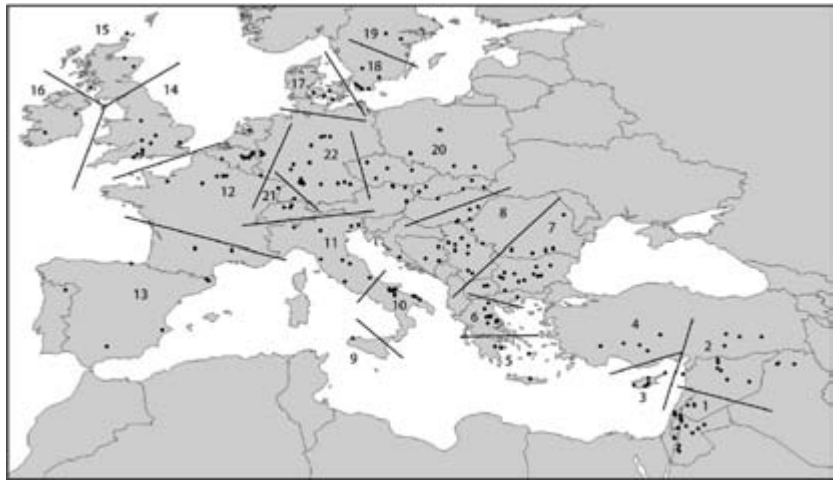

Figure 15.3 Map of the distribution of the archaeobotany assemblage sample sites, showing the boundaries of the regions used in the analysis. (From Coward et al. 2008:44, Fig. 1.)

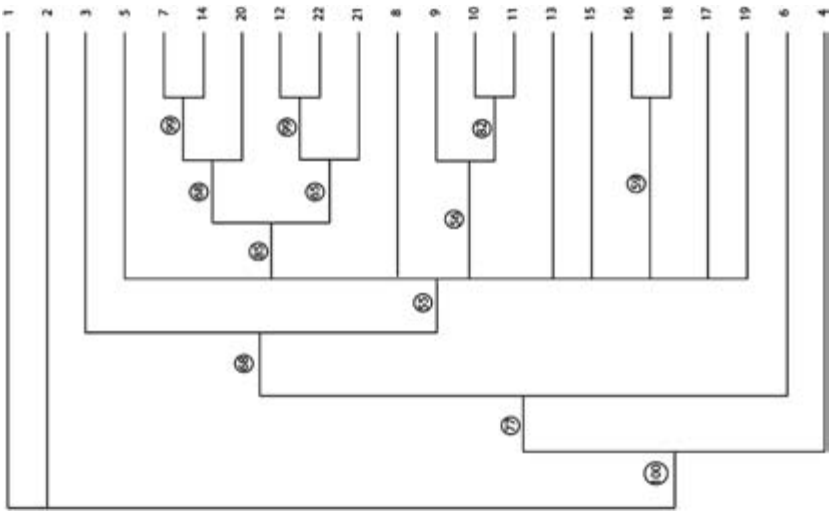

Figure 15.4 Evolutionary tree of early Neolithic archaeobotanical assemblages from southwest Asia and Europe, showing the pattern and extent of successive changes from the ancestral assemblages of southwest Asia as farming spread. The entities in the tree are the regions shown in Figure 15.3 and the figures in circles refer to the percentage probability of the branch of the tree with which they are associated. (From Coward et al. 2008:47, Fig. 4.)

stop on the route of a continental spread of cereal agriculture via central and northwest Anatolia, which turns out to be the most derived region in Europe in the sense that it has undergone more changes from the ancestral state than any other. Given its location it should be one of the least derived and might be expected to be similar to central Anatolia (region 4). Moreover, the highly derived Bulgarian plant spectrum cannot be considered ancestral to the assemblages of region 8, the former Yugoslavia, which look much more like descendants of the Greek/east Mediterranean line as well as possessing the relatively unchanged features that make them the plausible ancestors to the central European LBK complex that would be expected on other grounds. In short, taking an evolutionary perspective on the spread of farming and using appropriate tools enables us to produce a history with interesting novel elements that can be related to other archaeological patterns (Coward et al. 2008).

Modelling Change in Cultural Lineages: Style and Cultural Drift

In general terms, the most prevalent archaeological definition of 'style' would see it as referring to different 'ways of doing', and there may be many different reasons for 'ways of doing' different things to stay the same, or to change. In the case of artefact decorative styles, a great deal of attention has focussed on evaluating the role of drift processes. Following the logic of genetic drift, in cultural drift change through time is the result of chance processes arising from the random copying of cultural attributes in finite populations, with some possibility of innovation. It is very unlikely that any individual act of copying, for example, of a ceramic decorative motif, will be random (see Johnson, this volume), but if everyone has their own reasons for copying one person rather than another, the result will be that there are no directional forces affecting what or who is copied (see Bentley, this volume). As noted above, the key achievement here was Neiman's (1995) demonstration of the way the mathematics of the neutral theory of evolution could be used to generate quantitative expectations of what a distribution of artefact frequencies should look like if drift is the only factor affecting it. In effect, the methods provide the basis for a null hypothesis. If a particular distribution fails to depart from neutrality, there is no reason to postulate anything other than drift as the process producing it (Bentley et al. 2004). If there is a departure, then something further needs to be invoked to account for it (Shennan and Wilkinson 2001). It is important to note that drift as a process can exist only in the context of an evolutionary model that includes transmission; without a process of copying or inheritance it is meaningless.

A recent study by Shennan and Bentley (2008) took the neutral model as a null hypothesis in explaining the changing frequencies of pottery-decoration variants among the early farming communities of southwestern Germany, using the data published by Strien (2000). The analysis was carried out at two levels: the region as a whole and the individual site level, but only the latter results are described here.

The results gave strong evidence that the patterning in the changing decoration variant frequencies was mainly a result of drift, in that the amount of diversity within each site within each phase correlated strongly with that predicted by the neutral model (Figure 15.5). The changes through time in site ceramic-decoration diversity predicted by the neutral model and those actually observed also showed very similar patterns (Figure 15.6).

The results of our analysis confirm that the approach pioneered by Neiman (1995) provides substantive information about the factors affecting stylistic change in the pottery of the earliest farming communities in central Europe (cf. Sommer, this volume). The patterns are largely a result of drift, which itself is affected by only two parameters: innovation rate and effective population size. To suggest that only two parameters affect the chronological distribution of stylistic variation may seem

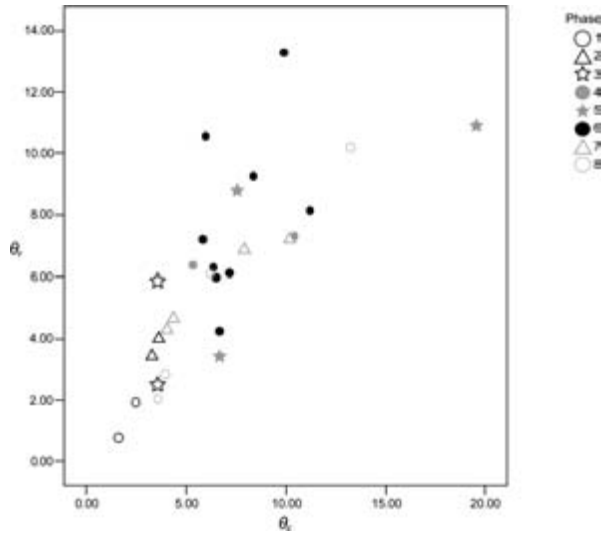

Figure 15.5 The actual diversity values (θ_F) of early Neolithic ceramic assemblages from southwestern Germany in terms of the relative frequency of different decorative types present at each site in each phase, plotted against the neutral diversity values (θ_E) predicted if random copying was the only process affecting the frequencies.

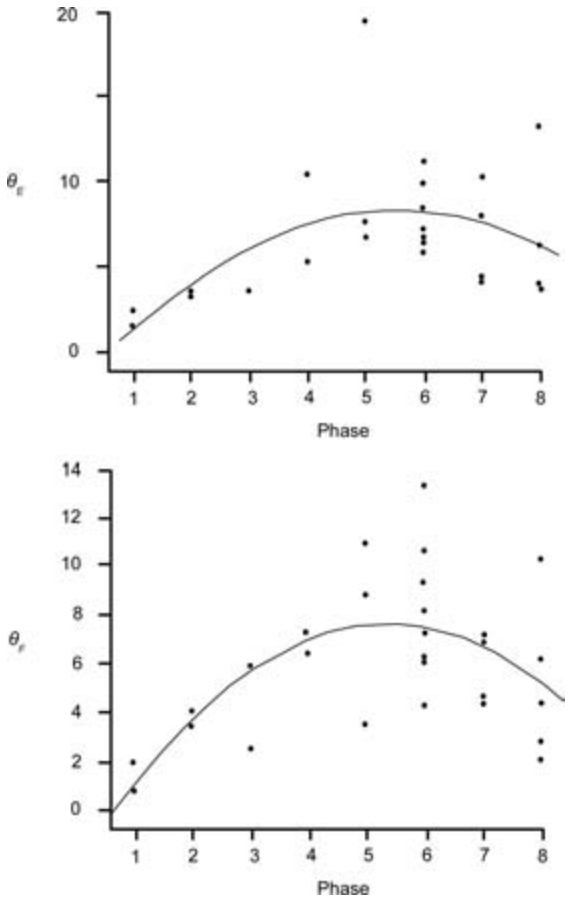

Figure 15.6 The neutral diversity values for each site plotted against the sequence of phases (top); the actual diversity values for each site plotted against the sequence of phases (bottom). (Redrafted from Shennan and Bentley 2008:172, Figs. 14.4 and 14.5.)

surprising to many, and it represents in itself a powerful simplification. It also provides us with a basis for making well-founded hypotheses about long-term patterns in the interaction and demography of the first farming societies of southwestern Germany, which can now be subject to further testing.

However, it is important to point out that what emerges very clearly from all the work with drift models is that there cannot be a radical separation between function and style, or between the operation of selection and biasing forces and drift. There is a continuous spectrum from pure drift to very strong selection or bias, just as certain activities

depend very strongly on transmission and others are most strongly conditioned by variation in the environment facilitated by behavioural plasticity.

Evolutionary Approaches in Historical Archaeology

The relevance of the approaches just described to the achievement of archaeological goals is not restricted to prehistory; they are just as relevant to historical archaeology. To the extent that they have seen more application in the former than the latter, this is much more to do with differences in the disciplinary training of the practitioners and the traditionally narrative descriptive approaches of the historians that historical archaeologists generally seek to emulate. In fact, recent historical periods are likely to offer rich data for evolutionary as for most other archaeological approaches. Patterns of relationship between different vernacular architecture traditions, for example, are likely to be illuminated by the application of evolutionary phylogenetics and other methods of analysis. Niche construction theory provides a framework for understanding the evolution of historical landscapes, including the relative importance of the roles of copying processes and cost-benefit considerations over time. However, this is work for the future. It is appropriate to conclude these illustrative case studies by summarising a recent fully developed study that has shown in considerable depth the potential of evolutionary method and theory for illuminating issues in historical archaeology.

Neiman's paper 'The Lost World of Monticello' (2008) shows in detail, across a series of spatial scales from macro to micro, how evolutionary theory can provide a basis for explaining changes through time in a range of social and economic spheres in colonial eastern North America between the seventeenth and nineteenth centuries. The particular aspect of the paper selected for this illustration is Neiman's evolutionary game theory–based model to account for the widespread occurrence of subfloor pits in the large structures characteristic of eighteenth-century slave housing in the Chesapeake region. The fact that they are not found in other regions of North America and the Caribbean where slave sites occur, nor in contemporary West Africa, suggests, Neiman proposes, that the reason for their occurrence must have something to do with specific conditions in this particular region.

His proposal is that the pits were used by slaves as places for the safekeeping of their food rations and other belongings in a situation where they could not choose the other individuals with whom they lived, were unrelated to them, and had no knowledge of their previous history, in particular whether or not they could be trusted. Placing belongings in

pits covered by boards that would have to be removed to get at their contents would have both hidden them from view and made it more likely that anyone who did try to steal would be caught in the act. In such situations knowledge of people's accumulating reputations would provide a basis for conditional cooperation with other individuals, based on their reputational standing:

> Subfloor pits would increase the extent and accuracy of the knowledge of past strategic choices that are necessary for selection to favor conditionally cooperative strategies within groups. In other words, the game-theoretic models suggest that subfloor pits were the "hard parts" of strategies invented by Chesapeake slaves to bootstrap residential group morality in the absence of choice over residence group membership. (Neiman 2008:177–78)

The validity of this proposal is strengthened by the fact that subfloor pits disappear from slave housing in the region at the end of the eighteenth century, a time when the units of slave housing become smaller, probably associated with an increased opportunity for slaves to choose whom they lived with and to live in family-based groups. In the Caribbean and South Carolina, where subfloor pits do not occur, slave houses were always smaller and the settlements themselves larger, with the result that there was a greater potential for choosing whom you lived with and not having to include nonpreferred partners.

The changed situation in the Chesapeake just described implies an improvement in the conditions under which slaves lived. Neiman relates this to other evidence, on the one hand of increased kinship density among slaves, most of whom were born locally by the end of the eighteenth century, and on the other to the fact that a changing economy altered the agricultural and other tasks that the slaves undertook, requiring more skilful work that could be less easily monitored; the result was a shift to some degree away from negative sanctions and towards positive rewards.

The summary here only covers one element of a much broader and richer paper, but the implications are clear. Evolutionary ideas provide a framework for generating causal accounts based on theoretically justified mechanisms, for example, game theoretical analyses of the dynamics of trust, of key aspects of stability and change in eighteenth-century colonial North America just as they do in the European early Neolithic, and indeed in the globalised world of the twenty-first century (see, e.g., Beinhocker 2006).

> Emphasizing the causal connection between artifacts and their meaning [significance] both motivates systematic study of variation in the archaeological record and offers helpful clues about what the meanings might be. That in turn makes it much more likely that archaeology can produce knowledge about the past that cannot be gained from documents. (Neiman 2008:164)

Conclusion

A cultural evolution approach to archaeology based on 'descent with modification' offers a consistent and coherent way of characterising population-level variation in the material remains of past societies and postulating the forces acting on it. Evolutionary theory can be used to develop appropriate models for cultural evolution that provide mechanisms to explain the patterns we observe in the past. Many of the aims, methods and approaches archaeologists have always used are consistent with the Darwinian perspective on cultural evolution, but there is much to be gained by adopting it explicitly and following through its implications. It is in this sense that Boyd and Richerson (2000) argue that Darwinian ideas are not the 'universal acid' corroding all previous conceptual frameworks suggested by Dennett (1995), but are more like a 'better mousetrap' for doing things that anthropologists have always wanted and tried to do. The fact that archaeology represents only one aspect of the interdisciplinary study of cultural evolution is a strength because the different elements of the evolutionary programme inform one another. Moreover, work carried out over the last 20 years, and especially over the last 10, has demonstrated that the approach is generating a productive research programme that addresses the goal of identifying and explaining patterns of stability and change in the material record of human existence, and leads to the accumulation of archaeological knowledge by producing convincing and intellectually rigorous accounts of the past.

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