# Accidents of History: Conceptual Frameworks in Paleoarchaeology

Geoffrey A. Clark

**Abstract** A moment's reflection will show that the various analytical units commonly used by paleolithic archaeologists in western Eurasia (e.g., Aurignacian, Mousterian) are 'accidents of history,' created for the most part by French prehistorians between c. 1880 and c. 1940 in order to solve chronological problems in the years before absolute dating methods had become available. Whether or not it makes sense to continue to use them as anything other than a vague and general *lingua franca* is addressed here, along with the question of what 'transitions' between these units might mean or imply about prehistoric human behavior. Since the units themselves are 'accidents of history,' the transitions between them might not mean anything at all from the behavioral ecology perspective adopted by some American and European workers. The essay compares and contrasts the conceptual frameworks of culture history (CH) and human behavioral ecology (HBE), focusing on archaeological monitors of human adaptation and how these change, or fail to change, at analytical unit boundaries.

**Keywords** Epistemology • Typology • Technology • Middle-upper paleolithic transition • Culture history • Evolutionary ecology • Western Eurasia

G.A. Clark (✉)
School of Human Evolution and Social Change,
Arizona State University, Tempe, AZ, USA

## Some Preliminary Observations

Over the past 15 years, the literature on modern human origins (MHO) has grown to immense proportions. It is evident from even a cursory examination of that literature that there is an enormous diversity of informed opinion about the nature of the archaeological transition, much of it concerned with (1) who made what, how they made it, when they made it and where; (2) how different perceptions of cognitive capacity and chronology influence the interpretation of pattern at the level of analytical units; (3) what processes were involved in transition mechanisms like acculturation, replacement, displacement, hybridization, and genetic swamping; (4) how these mechanisms can be distinguished from one another 'on the ground;' (5) how modernity might be defined (morphologically, cognitively, behaviorally); and (6) what the impetus for the generally accepted modern human exodus from Africa might have been.

All these partly contrastive, partly consilient views are 'fuzzy sets' (Willermet and Hill 1997, 77–88) that differ from one investigator to the next. They turn on vague notions implicit in the conceptual frameworks adopted by those involved in modern human origins research. Despite nominal acknowledgment of the power and generality of evolutionary biology, and the tacit assumption that it constitutes the overarching conceptual framework for *all* MHO research, no one can claim to control all of its aspects or implications. As a consequence, we tend to become consumers of one another's research conclusions, inevitably affected by assumptions about which particular

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construals of pattern in, say, human paleontology appear to square best with our interpretations of the archaeology; which interpretations of the genetic evidence seem most credible given what we know, or think we know, about human paleontology; and so on. While it is clear that any generally accepted explanation of our origins must reconcile patterns in archaeology, human paleontology, and molecular biology, no one so far has been able to do this very successfully, and no easy solution to the problem is in sight.

There is a commonly expressed hope for the appearance of pattern so robust that it will unequivocally support a particular hypothesis, but it is a vain hope in the absence of any consensus about criteria for the definition of modernity. Although we clearly need more data, acquiring data will not, by itself, resolve MHO questions like the nature of the transition, because data are not 'neutral' or intuitively obvious in terms of the meanings we assign to them. They only acquire meaning in the context of a particular conceptual framework (be it archaeological, paleontological, or genetic), and many alternative meanings are possible under the 'big tent' of evolutionary biology. In particular, there are problems with confounding explanations proposed by the advocates of culture history (CH) and those invoked by the adherents of human behavioral ecology (HBE). Although dominant in the United States until the late 1970s (and still very influential in the most common kind of archaeology practiced here—cultural resource management, or CRM), CH is now regarded by many American scholars as a preliminary but necessary step to establish rough approximations of the time/space grids required by HBE. This is especially true of areas where chronometric assays are impossible or difficult to attain, and/or where they are scarce or absent.

I have argued (1) that the basic analytical units used in paleolithic archaeology are a legacy of the CH approach and are 'accidents of history,' created—for the most part—by French prehistorians between c. 1880 and c. 1940 in order to solve chronological problems; (2) that how these units are defined has changed over time; (3) that they are based ultimately on typological systematics; (4) that they have become essentialized or reified to some extent by subsequent workers; and (5) that there is no consensus about what they mean or represent behaviorally (e.g., Clark 1991, 411–440, 2002a, 19–26). I have also tried to show that these claims enjoy considerable empirical support in respect of the most visible of these units—the Aurignacian—taken by many to mark the appearance of modern humans in Europe (e.g., Mellars 2005, 12–27; cf. Clark and Riel-Salvatore 2005, 107–118). In my view, explicit discussion of the nature of the analytical units is (or should be) an important aspect of paleoarchaeological research, since how those units are defined cannot fail to affect perceptions of pattern over the transition interval (here taken to be the 10 millennia bracketing 40 kyr bp).

Remarks like these have sometimes been taken as unwarranted criticisms of European conceptual frameworks and, by implication, the research traditions in which they arose—especially those of the 'founders' of paleolithic archaeology, the French (e.g., Marean and Thompson 2003, 165–167). I wish to make it *crystal clear* that I am *not* criticizing the French, Latin Europeans, Europeans in general, nor, indeed, anyone at all (except perhaps strict empiricists—those who think 'the facts speak for themselves' [Clark 1993, 212, 213]). The French were only doing what all archaeologists do—creating analytical units they thought relevant and appropriate to some problem they were trying to solve (see Sackett [1981, 85–99, 1991, 109–140] for a concise history of the phylogenetic paradigm in French prehistory). It should be kept in mind that paleoarchaeology is not an experimental discipline like physics or chemistry, nor do we have 'natural' analytical units that can be discovered as the life sciences do (Clark 1982, 218–220, 1987, 30–60). We have to create them, and the only way we can do that is in terms of some problem of interest (in the case of the French, how to distinguish different paleolithic assemblages from one another in time and space). But it is more complicated than that. Problems are embedded in problem contexts, problem contexts in research traditions, and research traditions in broader intellectual *milieux* (sometimes called metaphysical paradigms) that differ from one another in respect of implicit biases, preconceptions, and assumptions about their subject matter (here, what the past was 'like') (Clark and Riel-Salvatore 2006, 49, 50). No one

could deny that, if paleolithic archaeology had arisen somewhere other than where it did (e.g., Africa, instead of Europe), the analytical units would have taken on a very different character (see, e.g., the extended critique of Eurocentric bias by McBrearty and Brooks [2000]). From a philosophical point of view, of course, one paradigm is 'as good as another' (i.e., its internal logic is consistent, and its explanations coherent and 'satisfying,' given that logic). But because the assumptions underlying the metaphysic determine the character of its subordinate paradigms (which in turn determine research protocols in any problem context), conflicts often arise in respect of the nature of explanation and what kinds of explanations are regarded as plausible or not. These problems are exacerbated in transition research (in fact, paleoarchaeology generally) because it is of interest and importance to several quite different intellectual traditions. In particular, the contention that prehistory is 'history-like'—an extension of history back into deep time—is problematic because it has far-reaching implications for construals of pattern and what it might mean (Clark 1993, 217–223, 2002a, 20).

Along with some others (e.g., Straus 2003, 2007; Bicho 2002; Zilhão 2001; Zilhão and d'Errico 1999; Karavanic 1995, 2000, 2007; Kuhn 1994; Stiner 1994), it appears to me that—taken in aggregate—the conventional archaeological monitors of human adaptation (e.g., lithic industries; procurement ranges; subsistence data; site layouts, locations, characteristics; etc.) indicate a temporal and spatial mosaic over the transition, everywhere that its archaeological record is fine-grained enough to provide some indication of overarching patterns. I also suggest that, at least in some areas (e.g., northern Spain), the mosaic extends far back in time into the Middle Paleolithic, and up in time through the Upper Paleolithic and Mesolithic (Clark 1989, 589–603). Although of course arguable, this suggests to me that the west Eurasian archaeological record cannot easily be reconciled with *any* construal of an abrupt and complete biological replacement (e.g., Stringer 1992; Stringer and Gamble 1993), nor with a 'wave of advance' colonization model (e.g., Davies 1999, 2001, 195–217; Mellars 2005). Regardless of the position taken on the biological aspects of the transition, the suggestion is a relevant one, since many think transition archaeology is 'hominid specific,' that it 'maps onto' Neanderthals and modern humans respectively, and that the transition interval coincided with the biological replacement of Neanderthals by modern humans. How much empirical support is there for these arguments?

## The West Eurasian Mousterian and the Transitional Industries

Let's take a look at variability *within* the Mousterian of western Eurasia, as recently summarized by Howell (1999) (Table 1). Note, first, the ever-increasing number of spatially, temporally, and/or compositionally variable kinds of west Eurasian Mousterian industries. Recognized primarily on technological and typological grounds, the 20 Mousterian facies shown in Table 1 represent a quantum increase in variability over the half-dozen or so facies recognized as recently as the early 1990s. Ignoring inevitable problems with sampling error (largely a function of the amount of work done in a particular area), the facies appear to vary among themselves according to (1) aspects of raw material (availability, package size, quality, modal production sequences, procurement range); (2) average amount of reduction and (3) utilization of particular artifact classes; (4) functional constraints related to forager mobility; and (5) the nature, (6) size, (7) duration, (8) integrity, and (9) intensity of site use or occupation. Taken together, they document a complex mosaic of adaptations that, in aggregate, persists for c. 200,000 years, overlapping extensively with both the Lower and Upper Paleolithic over the entire geographical expanse of western Eurasia. When combined with the many transitional industries now recognized in the same area (Table 2), it is possible that Mousterian formal variation, site characteristics, and faunal inventories rival (perhaps even exceed) those of the early Upper Paleolithic.

## The Culture History Approach

Since its inception in the latter half of the 19th century, the European approach to paleoarchaeology has been dominated by a blend of natural and

**Table 1** Mousterian spatial-temporal variants (after Howell 1999, 218–226)

- *Charentian with 2 subtypes (pan-European)*
- *Typical complex (pan-European)*
- *Levantine Mousterian with 3 subtypes (Levant)*
- Typical-Crvena Stijena type (Balkans)
- Vasconian (northeast Spain)
- *Denticulate Mousterian (pan-European)*
- Mousterian of Acheulean Tradition, 2 subtypes (western Europe)
- *Mousterian-Châtelperronian (southwest Europe)*
- Cambresian (northwest Europe)
- *Pontinian (Latium)*
- *Mousterian-Karstein type (central Europe)*
- Mousterian-Tata type (Hungary)
- Mousterian-Staroselje type (Crimea)
- Mousterian-Tsutskhvatskaya type (Crimea, western Caucasus)
- Mousterian-Kudara type (western Caucasus, Georgia)
- *Zagros Mousterian (Zagros Mountains)*
- *Micoquian with 6 subtypes (central Europe)*
- Acheulo-Yabrudian (Levant)

Facies with hominid fossils (all Neanderthals except Levantine Mousterian) are *italicized*.

**Table 2** Transitional industries and sites (*) with claimed transitional levels

- Châtelperronian (southwest France, northern Spain)
- Szeletian (central Europe, especially Hungary)
- Uluzzian (south-central Italy)
- Olschewian (Croatia)
- Bachokirian (Bulgaria)
- Bohunician (central Europe)
- Aurignaco-Mousterian (Italy)
- Late Mousterian (north-central Italy)
- Uluzzo-Aurignacian (Italy)
- Zagros Aurignacian (Zagros Mountains)
- Jerzmanovician (Poland)
- Bryndyzian (Poland)
- Ahmarian (southern Levant)
- Altmuhlian (Austria)
- Lincombian (southern England, Brittany)
- Streletskayan (Crimea)
- Emiran (Levant)
- Boker Tachtit (Israel)*
- Tor Sadaf (west-central Jordan)*
- Warwasi (northwest Iran)*
- Umm el-Tlel (Syria)*

geological science, heavily reliant upon a typological systematics that emphasizes retouched stone tools (Sackett 1981, 85–99, 1988, 413–426). The cultural transition is, therefore, usually demarcated by changes in the retouched tool components of archaeological assemblages. The rationale and justification for doing this is seldom made explicit, but lurking just beneath the surface is the tacit assumption that the stone tools represent the remains of quasi-historical, stylistic microtraditions, transmitted from one generation to the next through the medium of culture. Since retouch modes and edge configurations are equated with social learning, it is assumed that the time/space distributions of 'diagnostic' stone tools are, to a degree, 'history-like'—congruent with the

boundaries of identity-conscious social units loosely analogous to the tribes, peoples and nations of history. This kind of reasoning is then extended to modes in the overall forms and frequencies of the artifacts themselves. Problems with the enormous spatial extent and temporal persistence of such hypothetical social units have often been overlooked (although cf. Bar-Yosef [1991, 371–395]).

## Views of the Middle/Upper Paleolithic Transition

The transition is like a greased pig—it is very elusive, slippery, hard to grasp in its entirety, and prone to generate misunderstanding ( . . . if understanding the motives of a pig is regarded as a reasonable thing to do). In my view, this is because of three interrelated aspects of *all* paleoarchaeological research: (1) no universal means of communication, (2) the ambiguity of research questions or hypotheses, and (3) an absence of an overarching conceptual framework. Paleoarchaeologists lack a metalanguage (e.g., mathematics in the physical sciences) that defines concepts and terms precisely and uses them consistently according to the parameters of a fully-axiomatized, explicit conceptual framework based on grounded theory accepted by consensus.[1] This makes it exceedingly difficult to formulate research questions precisely enough to generate test implications from them. Put another way, anthropology has no metaphysical paradigm against which the products of its 'normal science' can be measured. Although a scientific research protocol is a regulatory ideal (something to strive for—we all think we are 'doing science'), and methodological standards are very important to the 'science-like' aspirations of the discipline, many paleoarchaeological questions are rather open-ended ones, little constrained by the parameters of any recognizable paradigm. In contrast, questions in physical science are classifiable by their boundary conditions. I am indebted to James Eighmey (personal communication 1997) for the tongue-in-cheek observation that archaeological questions can be treated analogically as if they were 'gaseous,' 'liquid,' or 'solid,' according to the constraints imposed upon them by the conceptual frameworks within which they are formulated. 'Gaseous' questions are unconstrained by any discernible framework (e.g., origins of religion—essentially unbounded, expands like a gas to fill the conceptual container at equal density). 'Liquid' questions (probably the most common kind) are weakly constrained by boundary conditions (e.g., evolutionary origins of religion—bounded on one axis [it presupposes a naturalistic approach], but expands to fill the container on all other axes). 'Solid' questions are uncommon (e.g., neurophysiological and sociobiological origins of religion—bounded on most axes, with little room for expansion). He remarks that, even if they acknowledge its existence, it is exceedingly difficult for paleoanthropologists to arrive at a consensus on what shape the conceptual container should be! History, of course, is another matter altogether, which is why culture history is so problematic in 'deep time.'

These somewhat daunting obstacles to communication aside, changes in the character of retouched stone tools over the European transition have been interpreted in five or six (at least partly) contrastive ways. (1) Some workers see the transition as a largely in situ phenomenon everywhere, with clear evidence of lithic continuity between late Middle and early Upper Paleolithic (EUP) assemblages (e.g., Cabrera et al. 2001, 505–532; Clark 2002b, 50–67; Wolpoff et al. 2004, 527–546; Straus 2007, 11–18). A variant of this interpretation is the 'assimilation model' proposed by Fred Smith and colleagues on the basis of the fossil evidence (e.g., 1989, 35–68, 2005, 7–19; Churchill and Smith 2000, 61–115). It postulates that anatomically modern humans emerged first in Africa, and radiated from there to Eurasia, but that 'more than incidental' genetic exchange occurred between the expanding modern and the indigenous archaic populations (Smith et al. 2005, 15). The AM is gaining adherents, partly because it relies quite heavily on evidence for continuity in the archaeology. (2) Others argue that certain EUP industries are 'adaptive responses' by Neanderthals to the arrival of modern humans producing Aurignacian industries. Whatever that might mean, it implies that Neanderthals modified existing Mousterian technologies *because of* contact with moderns to produce assemblages with mixed Middle and Upper Paleolithic

characteristics (e.g., Allsworth-Jones 1986; Valoch 1990, 115–124; Djindjian et al. 2003, 29–48; see d'Errico et al. [1998] for a critical review of acculturation). A third point of view (3) is that no such intermediate industries exist and, when contemporaneous late Middle and early Upper Paleolithic assemblages are present in the same site or region, the EUP (especially the Aurignacian) must therefore be intrusive (e.g., Adams 2007, 91–110; Hublin 1995, 931–937; Kozlowski 2000, 77–107). This scenario implies that the authorship of LMP and EUP industries is known with certainty and can be generalized; and that, in some parts of Europe, archaic and modern groups coexisted for millennia but did not interact with one another to any significant extent. There are many variants of this model, which is perhaps the most popular view of the relationship between the late Mousterian, Micoquian, Uluzzian, etc., of Neanderthal authorship, and the 'real' EUP (= Aurignacian), made by modern humans. Sometimes called 'the indigenist model' (Harrold and Otte 2001, 5), a fourth perspective (4) is that typologically discrete Châtelperronian and Aurignacian industries are 'hominid-specific,' and that Neanderthals making Châtelperronian artifacts underwent a separate and earlier Middle-Upper Paleolithic transition, independent of but fully equivalent to that involving moderns and the Aurignacian (e.g., Zilhão 2001; Zilhão and d'Errico 1999, 1–68). Finally, (5) some have remarked on the 20 or so transitional industries now known from eastern and central Europe (e.g., Howell 1999). Of unknown authorship, these industries exhibit assemblage characteristics typical of neither the Middle nor the Upper Paleolithic as defined in the west (e.g., Svoboda 2005, 69–76). In some respects the opposite of the indigenist model, this scenario uncouples assemblage types from hominid types (except—usually—in respect of the Aurignacian), and interposes a separate 'transition interval' between the Middle and the Upper Paleolithic, occupied by industries that are neither Middle nor Upper Paleolithic. (For expanded discussions of transition scenarios, see Camps [2006], for Iberia; papers in Brantingham et al. [2004], for central and eastern Europe and Asia; Olszewski [2001], for the Zagros Aurignacian; and Hovers and Kuhn [2006] and Riel-Salvatore and Clark [2007], for western Eurasia.)

## Patterns Generated by Typological Systematics

Leaving aside preconceptions about authorship which tend to influence the meaning assigned to pattern, and restricting the discussion to the retouched tool components themselves, it is pretty clear that there is much more continuity across the transition than has generally been recognized. It could be the case that the different perspectives just summarized are inextricably bound up with the classifications used to compare Middle and Upper Paleolithic retouched stone tool inventories. As has often been remarked, quite distinct and largely incompatible typological systems are used to characterize these assemblages (e.g., Bisson 2000, 1–48). This also affects construals of pattern over the transition and what pattern might mean in behavioral terms. Let's take a look now at some of the patterns supposedly characteristic of the Upper Paleolithic.

### *Upper Paleolithic Stone Artifact Diagnostics*

First, there is the issue of imposed form and standardized shape, both associated with Paul Mellars (e.g., 1989, 1994, 2000), both supposedly more evident in even the earliest Upper Paleolithic assemblages than they are in the Middle Paleolithic. Many workers have noted that, despite assertions to the contrary, UP typological variation by no means consistently displays a high degree of formal standardization, nor do the types themselves segregate neatly and unambiguously (e.g., Monnier 2006, 57–84). In fact, as Sackett has remarked (1988, 418), 'the amount of intergradation between types is sometimes so great as to frustrate even the most experienced typologist,' which suggests that the types (and perhaps even the type groups) might represent no more than modal points along a continuum of morphological variation.

A second point is that *all* paleolithic tools (not just Mousterian ones) were heavily subjected to modification over their use-lives by continual use, breakage, subsequent rejuvenation, and/or intentional reworking. This means that a continuum of

formal transformation is likely the rule, rather than the exception; that there might not be much design specificity in either the Middle *or* the Upper Paleolithic; and that Dibble's arguments about formal convergence in Mousterian sidescrapers (e.g., 1995, 299–368) could apply with equal cogency to many UP tool types, including most of the *fossiles directeurs* (Sackett 1988, 419).

Finally, of the 92 types recognized by the most commonly used UP typology (i.e., de Sonneville-Bordes and Perrot 1953, 1954, 1955), most sites actually contain relatively few of them, suggesting that what are perceived to be discrete types might, more often than not, simply represent successive stages in the modification of a single generalized tool and/or minor alterations in form primarily determined by variations in blank morphology (essentially the same argument first proposed by Dibble [1984, 1987] for Mousterian sidescrapers). The implication is that many (perhaps most) Upper Paleolithic retouched tool inventories are not more complex than their Middle Paleolithic counterparts, nor do they conform to more rigorous design specifications, nor are they more functionally specific—considerations that all but erase the supposed cognitive differences between the hominids that made them (Monnier 2006; Clark 2002b).

Rather than taking their adequacy for granted, we need to directly confront the very real possibility that the existing systematics might not be up to the task of answering many questions deemed important in paleolithic research. I suggest that we do not even know what the conventional archaeological analytical units are, or mean, or represent, behaviorally. It is a facile assumption of those who have faith in the adequacy of typological systematics that we are discovering, via retouched stone artifact typology, something very like the remains of identity-conscious social units analogous to the tribes, peoples, and nations of history. To those who come to MHO research from an historical perspective (often the case in Europe, perhaps not so common in the United States), paleolithic archaeology is essentially culture history (or paleoethnography) projected back into the Pleistocene, and patterns are typically explained post hoc by invoking processes like those operating in historical or ethnographic contexts. The whole CH approach is predicated on (1) the existence of tool making 'traditions' manifest in artifact form that are detectable over hundreds of thousands (even millions) of square kilometers; (2) the idea that such 'traditions' persisted unchanged and intact over tens (or, in the case of the Lower Paleolithic, hundreds) of millennia; and (3) the conviction that they are detectable at points in space separated by thousands of kilometers and tens of thousands of years of time (e.g., Goren et al. 2000; Hou et al. 2000).

I have argued at length that this culture history paradigm, while internally consistent in respect of its logic of inference, cannot be reconciled with the human behavioral ecology perspective adopted by many American workers, and (1) that most of the paleolithic 'index fossil' tool types are ubiquitous (or nearly so), at least in western Eurasia, and carry little temporal and probably no social information whatsoever; (2) that there is only a minimal and generalized learned behavioral component to chipped stone artifact form, constrained as it is by rock mechanics; (3) that there are no universal correlations between particular kinds of hominids and particular kinds of lithic assemblages; (4) that there is much formal convergence in the (few) processes by which humans chip stone; (5) that formal convergence is conditioned by contextual factors—technology, raw material quality, size, distribution in the landscape, etc.—especially as affected by mobility; and (6) that it almost certainly overrides any hypothetical 'cultural' component. In other words, I believe it is possible to explain many (perhaps most) pattern similarities in paleolithic archaeological assemblages without recourse to typology-based tool-making traditions. I make three points specifically in regard to typological systematics (Clark 2002b):

## Problems with Typological Systematics

First, there are logical problems with a significant cultural 'signal' in the form of (most) paleolithic artifacts. For one thing, the time-space distributions of prehistorian-defined analytical units *exceed by orders of magnitude* the time-space distributions of any real or imaginable social entity that might have produced and transmitted them. Unless one resorts

to essentialism (e.g., there is an ineffable 'Aurignacianness' manifest in the appearance of, say, Dufour bladelets), there is simply no behavioral or cultural mechanism whereby a hypothetical tool-making tradition could have been transmitted over thousands of years and millions of square kilometers. Thus, something other than historical connectivity must account for pattern similarities.

For another, we have no guarantees that the basic analytical units themselves are discrete in time and space, are 'the same thing' whenever and wherever they are found. In fact, it is highly likely that they are not. The Aurignacian as defined in France and in the Levant is the quintessential illustration of this problem. Apart from the occasional appearance of carinated tools in a few Levantine Aurignacian levels (notably at K'sar Akil in Lebanon [Marks 1993]), and a small number of split-based bone points from the Israeli sites of Hayonim and Kebara (Bar-Yosef 2000), the only similarity between the French and the Levantine Aurignacian is the name itself, imported from France by several generations of Levantine scholars trained in the francophone tradition. Whatever the Aurignacian is, it is manifestly not a 'culture' or a 'tradition.' The same can be said of all the other prehistorian-defined analytical units used to impose order on Upper Pleistocene archaeological sites in time and space. There is, of course, a range of informed opinion as to how far back in time 'cultures' and 'traditions' might be identified empirically and whether or not it is reasonable to expect that traces of them would be found in collections of stone artifacts (e.g., Bar-Yosef 1991; Close 1977; Goring-Morris et al. 1996).

Finally, there is the question of resolution and its consequences for identifying a tradition 'on the ground.' Most workers would acknowledge that no known paleolithic site sequence, or series of site sequences, is anywhere near fine-grained enough to allow us to identify the remains of the hypothetical social units that would have been the bearers of these lithic 'traditions' (i.e., even in the best-dated sites, assemblage resolution and integrity are far too low, and traditions too fleeting in time, to be recognized). Moreover, the generally-acknowledged fluidity of forager territorial boundaries would, in short order, have impossibly confounded any stylistic patterns that might have been manifest in stone tool form in the archaeological context. So, even if there were a 'cultural' component to the form of paleolithic stone artifacts, we could not possibly detect it. It is not enough to claim, as some have done (e.g., Hou et al. 2000), that we cannot yet model 'paleoculture' adequately. In fact, we already have a relatively sophisticated model for paleoculture in the HBE approach described below. The culture history paradigm, on the other hand, is simply not up to this task. By invoking identity-conscious 'migrants' whose peregrinations are supposedly manifest in timeless, changeless tool-making traditions (e.g., Locht and Révillion 2002, 146–160), process in the remote past is treated as if it were analogous to process in recent historical contexts. While this is a perfectly reasonable thing to do from the perspective of many CH advocates *on both sides of the Atlantic,* it does not make much sense from an HBE perspective.

In sum, (1) the absence of an overarching conceptual framework specific to 'paleoarchaeology;' (2) the tendency to view paleolithic archaeology as 'history-like,' replete with processes and analytical units analogous to the tribes, nations, and peoples of history; and (3) the scarcity in university curricula of what might be called an explicit concern with the logic of inference (i.e., epistemology) are the principle factors that contribute to conflicting interpretations of pattern, both in paleolithic archaeology in general, and in 'transition archaeology' in particular. Because of the European tendency to train paleolithic archaeologists in history and natural science, it could be argued that CH approaches are more common there than they are in the US, where prehistory is considered an aspect of anthropology and is typically taught in anthropology departments. American anthropological archaeology is well-known for an emphasis on (some might say obsession with) epistemology—how we know what we think we know about the remote human past. As noted above, CH dominated American archaeology from the 1920s through the 1960s, and it was precisely because of its perceived deficiencies (e.g., purely inductive research protocols, too much post hoc accommodation, no deductive component manifest in hypotheses, no test implications, etc.) that method and theory courses became widespread there during the 1970s.

Two reviewers of this manuscript took exception to these contentions (or at least the categorical expression of them). However, in much of Continental Europe, at least, there appears to be little explicit concern with the logic of inference in university curricula dedicated to the training of prehistorians. Such courses are more common in the UK and perhaps in the Netherlands (e.g., Corbey and Roebroeks 2001), possibly because of more widespread use of English there. As noted above, in the US, CH perspectives are no longer common in academic research, although they tend to be much more prevalent in CRM.

## Culture History, Transition Archaeology, and Paleoarchaeology—Problem Areas

In my opinion, there are four general problem areas that afflict transition archaeology specifically and paleoarchaeology in general. Each is more or less directly linked to the adoption of the CH approach and the implicit assumptions that underlie it. Two important ones are (1) essentialism and (2) reductionism (more accurately, the intricate tangle of essentialism and reductionism that arises from adoption of a CH perspective). A third problem is (3) over-reliance upon post hoc accommodative argument, and the failure to build a deductive component manifest in test implications into explanation candidates. Finally, and as mentioned previously, (4) the absence of a conceptual framework of sufficient scope and generality to deal with process questions related to adaptation stands in the way of more compelling explanations for pattern similarities and differences.

### *Essentialism*

In archaeology, essentialism is most often linked to typology because typological systematics plays such an important role in the definition of stone age analytical units (so important, in fact, that typology can sometimes 'trump' patterns defined on the basis of more objective criteria like radiocarbon dates). Essentialism is a philosophical standpoint that originated in classical antiquity based on the concept of essence, and founded on the idea that metaphysical essences really exist in nature and are intuitively accessible, resident in the mind. Essentialism is often juxtaposed with realism, the philosophical doctrine that universals exist outside the human mind.

The history of classification in the CH approach can account for the importance of essentialism in its research protocols. Paleolithic archaeology on the Continent developed at about the same time as the archaeology of ancient foragers in the US (i.e., those dating to the Paleoindian, Archaic periods). On both continents, it originated in the kind of natural history that dominated much of 19th century European and American intellectual life. Until Darwin, classification in the life sciences consisted of the systematic arrangement of organisms into groups or categories according to established criteria. Linnaean species were held to be the immutable products of divine creation, and the process of classification simply involved the assignment of the proper species identification to each individual organism. A type specimen was used to define the species and served as the unique standard of comparison for identifying and categorizing other specimens.

With the realization, in the first half of the 20th century, that populations of individuals, rather than individuals themselves, are the units of classification, the concept of variation somehow had to be accommodated in biological systematics. After the 1930s, classification became a descriptive preliminary to life scientists, who began to look for explanations in genetics, ecology, and development, using principles derived ultimately from the work of Darwin, Wallace, and Mendel. Unfortunately, *many paleolithic archaeologists never made this crucial conceptual transition*. Archaeological sequences in 'key' caves and rockshelters were, and in many cases still are, seen through a typological filter as analogous to geological and paleontological type sections with time-sensitive index fossils and sequences transferred more or less directly from the earth and life sciences to the study of human culture history. Well-known examples include Mugharet et-Tabun in Israel, K'sar Akil in Lebanon, El Castillo in Spain, and Combe Grenal and Laugerie Haute in France.

## Reductionism

Tangled up with essentialism is a rather naïve kind of reductionism that tends to normalize or minimize variation in the perception of pattern in paleolithic archaeology by emphasizing the kinds and frequencies of retouched stone tools. Like essentialism, reductionism has a long history. It is based on a coherent philosophical position that sees modern science as materialist, and the heir to 19th-century mechanical materialism—the basis for the development of industrial capitalism. In *Lifelines—Biology Beyond Determinism,* British biologist Steven Rose (1998) recognizes three kinds of reductionism: (1) methodological, (2) theoretical, and (3) philosophical reductionism (Rose 1998, 21–43). Methodological reductionism is fundamental to all science. Some might even say it is a cognitive necessity for all sentient organisms in order to cope with the bombardment of perception. Sometimes called parsimony, theory reduction aims for a maximally satisfying description of some aspect of the experiential world, while simultaneously minimizing the number of laws and variables. While methodological reductionism is universal in science and theory reduction desirable and attainable to some extent in the life and social sciences, philosophical reductionism is deeply problematic in all science.

Criticisms of reductionism come from many quarters (Rose 1998, 73–96). New Age philosophers argue, for example, that the human experience is uniquely multivalent and richly textured, and that reductionism drains the life out of that richness and texture. Feminist philosophers of science contend that reductionism typifies the masculine, cognitive, objectifying approach to the world taken by modern science, and that it fails to respect the validity of personal, subjective experience. Some ecologists criticize reductionism because it appears to deny the interconnectedness of the living world. Reductionism is not a unified concept, however, and there are many construals of it, depending upon context and standpoint (Rose 1998, 1–20).

How is reductionism manifest in transition archaeology? Although conceptually quite distinct, reductionism and essentialism converge on typological systematics because typology is privileged in much paleolithic research, especially on the Continent. When there is no overarching conceptual framework, when theory building is largely implicit, and when pattern in the remote past is treated as analogous to, and explicable by, pattern in recent history, it does not make much sense to talk about philosophical or theory reduction in any formal way, although the latter figures implicitly in any effort to explain observed patterns. As in science generally, most reduction takes place at the methodological level, as different workers relentlessly 'pattern search,' emphasizing different suites of variables differentially. This is evident in the use of trait lists to identify behavioral and morphological modernity (thankfully, this is going out of style—see Clark and Riel-Salvatore [2005]; Mellars [2006]; and papers in Bar-Yosef and Zilhão [2006] on problems with the definition of the Aurignacian), but remains problematic in paleoanthropology because there is no consensus about just what 'modernity' is (a philosophical question), either in the past or the present, nor how it might be detected archaeologically.

So, (1) if there is no consensus definition of 'modernity,' (2) if the appearance of 'modern' behavior (however defined) is thought to coincide with the transition interval in any particular region, (3) if 'modern' behavior is considered a 'package' with at least some empirical referents, and (4) if there is little or no explicit concern with the logic of inference, then how can we expect to arrive at a consensus about anything? Modern human origins research becomes a thing of shreds and patches, without any boundaries or rules that might constrain choice in interpretations of pattern. We cannot even come to an agreement that pattern exists, let alone whether it is 'significant' or not, what it might mean, or whether it bears any formal relationship to an hypothesis we are trying to test (Fig. 1). In essence, *theory becomes method.* In the francophone tradition, this can be traced back to the influence of André Leroi-Gourhan (1964, 1965), who emphasized a functionalist approach predicated on the conviction that concepts and theories were worthless in the absence of concrete applications demonstrating their utility. Pattern searching came first; explanation of pattern came later and was largely intuitive and inductive. Because of a general mistrust of epistemology, the origins of concepts and theories were thought to be irrelevant, and their logical coherency

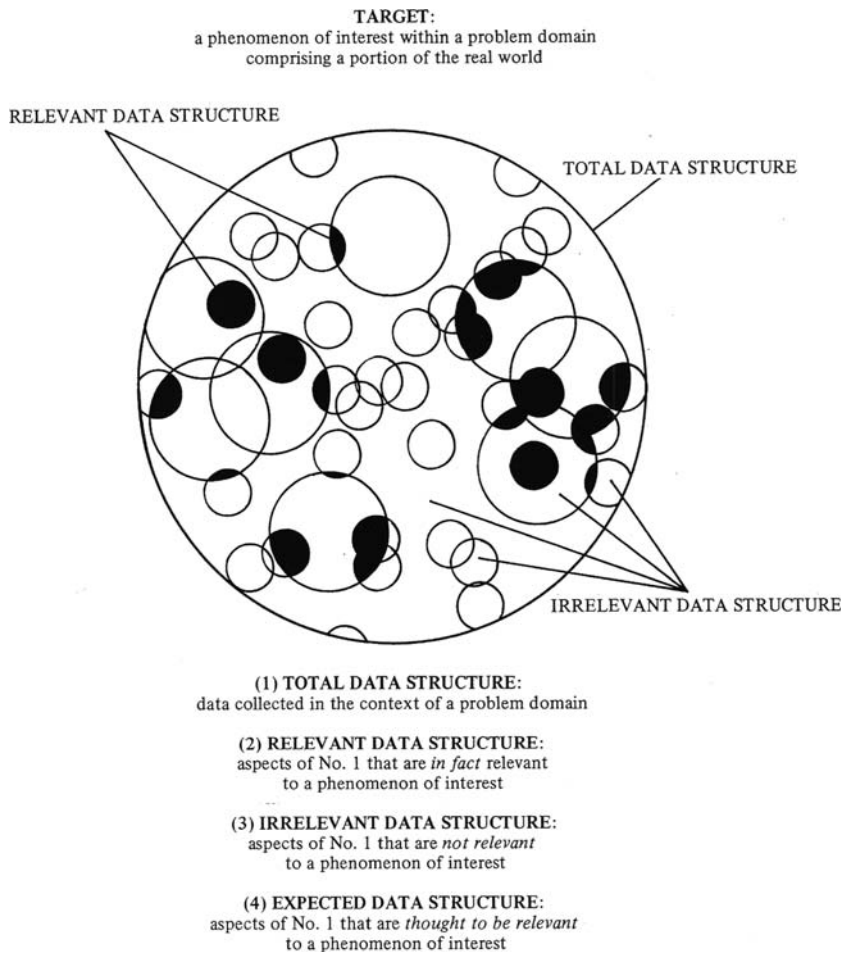


**Fig. 1** A schematic representation of Carr's (1985) categories of information about the real world. The target is (2), relevant data structure. Expected data structure (not shown) may correspond poorly, well, or not at all to relevant data structure

unimportant (Audouze 1999, 167–175; Coudart 1999a, 161–167, 1999b, 653–664).

## *Post Hoc Accommodation*

Many research designs in paleoarchaeology today are basically unconstrained or weakly constrained 'pattern search' approaches using variables selected more by convention or for convenience than for any diagnostic utility in choosing among test implications generated by null and alternative hypotheses. These pattern searches are what Binford has called post hoc *accommodative arguments*—explanations developed after data have already been collected and analyzed to explain patterns detected in them (1981, 31, 82, 83). There is a certain circularity to post hoc accommodation, and its research protocols tend to be wholly inductive. In consequence, it is only as convincing as the ingenuity of the investigator allows it to be. It can always be questioned by anyone inclined to reject the variables identified as 'significant to measure' or to disagree with how those variables are defined and measured.

Post hoc accommodative argument sets the agenda for future research; it does not constitute a genuine test of an hypothesis. It is a weak form of inference because the research designs that incorporate it typically lack a deductive component that plays off pattern in unrelated data sets (here genetics, human paleontology) against those in the primary area of inquiry (here archaeology). Paleoarchaeology has tended to rely on methods borrowed from other fields that developed in the absence of general theory as a series of conventions

for assigning meaning to pattern. Human paleontologists are perhaps better off in this regard because they can invoke neo-Darwinism as an overarching paradigm. However, in both fields, these conventions exhibit a 'fad-like' quality in that they change in concert with changes in highly-visible, somewhat intangible, commonly recurring research contexts (e.g., modern human origins research [Clark 1999, 2029–2032]). A typical inductive research scenario involves a pattern search that, if at all competent, cannot fail to produce correlations among the variables examined. The question then becomes how to assign meaning to the patterns thus isolated. One's imagination is typically engaged to identify the conditions that, if they actually occurred, would account for the observed pattern. Most paleoanthropologists are sufficiently creative to be able to come up with a more-or-less plausible set of circumstances that could account for the observed 'facts.' However, it is important to keep in mind that the degree of fit between the imagined conditions and the observed properties of the data set does not constitute a test of the accuracy of that reconstructed series of events. What usually happens is that warranting arguments are marshaled to support the plausibility of the proposed explanation—to show that it is not unreasonable to suppose that it might have occurred the way the investigator suggests that it did. Plausibility is frequently supported by an 'argument from elimination' that assumes all potential causes of the pattern can be identified, enumerated (and ideally, ranked, or assigned a probability), and that all but one can be eliminated as the (proximate) cause of the phenomenon in question. However, the assertion that all possible causes were not in fact identified is sufficient to undermine the credibility of the argument (Binford 1981, 82–86). The case rests on the plausibility of the warranting arguments invoked in support of the explanation (or in some deplorable cases, by recourse to 'authority').

It must be acknowledged that there is no simple solution to this dilemma (Binford proposes more emphasis on 'middle range theory'—actualistic studies that allow us to use arguments from elimination with greater sophistication). To be fair to transition archaeologists (and to the discipline), post hoc accommodation is an aspect of all scientific research that is not purely and classically 'experimental' (whence the scorn heaped on the life and social sciences by the physical sciences). It is possible to deduce hypotheses from general theory in highly experimental fields like physics, where there is a large body of grounded theory, where theory is fully axiomatized, where argument is sustained mathematically, and where laboratory conditions are tightly controlled. None of these conditions applies to paleoanthropology.

In the absence of a strong deductive component manifest in hypothesis formulation, one can strive for what has been called consilience—the interlocking or coherence of causal explanations across multiple problem domains (Mayr 1982; Bernstein 1983; Wilson 1998). However, for consilience to work, there must be consensus about basic definitions, terms, and concepts. In my opinion, there is very little consilience in paleoarchaeology, and almost no concern with the logic of inference underlying its knowledge claims. That said, little is to be gained by ignoring these epistemological issues. If we continue to do that, we will continue to fail to confront the fundamental ambiguity of pattern in both the archaeological and paleontological records. We will fail to develop a basis for making strong inferences about the past (Clark and Lindly 1989, 661–663; Clark 2000, 851–853).

## Absence of a Conceptual Framework

Clearly, the absence of a unifying conceptual framework specific to paleoarchaeology has impeded progress in arriving at a satisfactory solution to the question of our origins (in general), and the nature of the Middle-Upper Paleolithic transition (in particular). For such a conceptual framework to be viable, it must at least be consistent with the core tenets of evolutionary biology (Sober 1991, 17–38), yet flexible enough to allow for investigation of the wide range of problems associated with 'evolution and adaptive design in ecological context' (Winterhalder and Smith 1992, 4). There are a number of potential candidates (e.g., behavioral ecology, reproductive ecology, evolutionary psychology, dual inheritance theory, evolutionary genetics, community ecology, animal ethology, decision theory, etc.), all of them concerned in one way or another

with the behavior of social mammals (usually primates). Sometimes lumped under the rubric of human behavioral ecology (HBE), these approaches were recently outlined, compared, and contrasted with one another by Bruce Winterhalder and Eric Aldan Smith (2000, 51–72). Although these approaches are sometimes regarded as bounded (especially by their adherents), Winterhalder and Smith (2000, 53) point out that they have increasingly come to be viewed as largely complementary fuzzy sets, overlapping with one another in problem foci, the sources of hypotheses, and some other aspects of research design (Tables 3, 4).

## Human Behavioral Ecology

HBE arose out of the larger field of evolutionary ecology during the mid-1970s because of growing dissatisfaction with hunter-gatherer decision-making models (especially those concerned with resource acquisition), until then drawn largely from ethnographic accounts. Given the epistemological questions with, and limitations of, CH approaches applied to 'deep time,' it is difficult to continue to defend the position that paleoarchaeology is 'just another kind of culture history.' Over the past 15 years, there has been some recognition of this, and of the largely unrealized potential of HBE to serve as a conceptual framework for all kinds of prehistoric archaeology, from that of early hominids to that of the very recent time frames with which New World workers are concerned (Clark 2003, 51–68). These efforts, embodied now in more than a dozen books published since 1995, seek to demonstrate at the levels of 'high' and 'middle range' theory, and at the level of case studies and applications, the conviction that HBE constitutes the most promising conceptual framework within which to understand human biological and cultural evolution, 'writ large' or 'small.'

**Table 3** Evolutionary anthropology—an adaptationist perspective: major approaches compared *

| Aspect compared | Behavioral ecology | Evolutionary psychology | Dual Inheritance theory and evolutionary archaeology |
|---|---|---|---|
| **Focuses on** | Extant forager behavioral strategies | Environment of evolutionary adaptedness (EEA) | Culturally inherited variation |
| **Approach studies** | Humans | Humans, other higher primates | Humans |
| **Temporal scale** | Short-term (phenotypic) | Long-term (genetic) | Medium-term (cultural) |
| **Emphasis on** | Forager socioecology | Brain evolution (cognitive neuroscience, genetics) | Information transmission in a social context |
| **Source of variation** | Social learning (esp. as it affects subsistence, reproduction) | Cognition (esp. as it affects mating strategies, social organization) | Social learning and its material consequences |
| **Direction of transmission** | Mainly horizontal, oblique | Vertical (usually) | Horizontal, vertical, oblique |
| **Expected current adaptedness** | Highest | Lowest | Intermediate |
| **Source of hypotheses** | Formal models derived from animal ecology, ethology | Informal inferences derived from extant higher primate behavior | Formal inferences derived from social geography, demography |
| **Hypotheses tested by** | Quantified ethnographic observation, statistics | Cross-genera surveys (some laboratory analysis) | Statistical methods (usually), some use of formal models |
| **Research protocols** | Observational | Observational, experimental | Observational |
| **Primary subfields are** | Ethnography, social anthropology, oral history | Primatology, biological anthropology, linguistics | Archaeology, cultural anthropology |

* Modified from Smith (2000), Winterhalder and Smith (2000), O'Brien and Lyman (2000), Steele and Shennan (1996), Barton and Clark (1997).

**Table 4** Some contemporary scholars active in human behavioral ecology, evolutionary psychology and dual inheritance theory/evolutionary archaeology*

| Behavioral ecology | Evolutionary psychology | Dual inheritance theory/evolutionary archaeology |
|---|---|---|
| M. Alvard | L. Aiello | R. Bettinger |
| D. Bird | S. Baron-Cohen | P. Bleed |
| R. L. Bliege-Bird | L. Betzig | R. Boyd |
| N. G. Blurton-Jones | D. Buss | R. C. Dunnell |
| M. Borgerhof-Mulder | L. Cosmides | J. A. J. Gowlett |
| K. Hawkes | F. de Waal | T. Holland |
| K. Hill | R. Dunbar | R. Leonard |
| A. M. Hurtado | H. Fisher | R. L. Lyman |
| H. Kaplan | R. Foley | H. Neff |
| R. Layton | K. Gibson | M. O'Brien |
| J. O'Connell | S. (Blaffer) Hrdy | P. Richardson |
| E. A. Smith | S. Mithen | S. Shennan |
| P. Wiessner | J. Plavcan | J. Steele |
| B. Winterhalder | M. Potts | |
| | M. Small | |
| | J. Tooby | |
| | C. van Schaik | |
| | R. Wrangham | |

* The tripartite division is a 'fuzzy set;' many listed do research in more than one approach, and the approaches themselves overlap with one another.

Since its appearance in the mid-1980s, HBE has expanded to encompass multiple domains, some of them clearly applicable to paleoarchaeology (e.g., optimal foraging theory, resource transfers, dietary diversification and intensification, mating strategies, male/male competition, male/female division of labor—sexual selection in general), others less so (e.g., origins and consequences of agropastoral economies, conservation biology, demographic transitions, origins of social inequality). HBE advocates argue that natural and sexual selection, and other Darwinian mechanisms and processes, act on human behavior and more or less directly influence the material products of that behavior. HBE is often highly quantified. It adopts a hypothetico-deductive research protocol that derives testable hypotheses from mathematical models originating in a neo-Darwinian conceptual framework. Although the constituent paradigms of HBE overlap extensively with one another in terms of concepts, methods, and problem domains, all share a focus on adaptation, are explicitly reductionist, and are firmly anchored in post-synthesis evolutionary biology. In aggregate, they address research domains that, one might think, would be central to the concerns of a genuinely interdisciplinary, integrative paleoarchaeology (e.g., primate life history, demography, maturation, mating strategies, reproductive ecology, resource transfers, division of labor—indeed, all aspects of hominid sociality). HBE assumes that holistic approaches are inadequate to model complex socioecological phenomena, and that essential features of an adaptive problem must be captured and isolated first in order to understand them.[2] Despite this significant contrast with the particularism evident in much sociocultural anthropology, forager ethnographies play an important role in HBE, and there is some methodological overlap (see Winterhalder and Smith [2000, 52–54] for expanded discussion of HBE, comparisons with evolutionary psychology and dual inheritance theory). By focusing on the requirements of HBE at the theoretical and methodological levels, the approach goes some considerable distance toward creating a novel, coherent framework for explaining all kinds of variation in the archaeological record.

What is so striking about the literature of this research tradition (in addition to its 'newness'—most of it postdates 1985) is how extraordinarily fruitful it has been in terms of testability, predictive adequacy, internal coherence, external consistency,

simplicity, and unifying power—epistemic qualities that should be highly prized by any archaeology that aspires to be scientific (Winterhalder and Smith 2000, 65–67). But the extent to which many paleolithic archaeologists are even aware of this literature is arguable.

## The Extended Phenotype

Basic to the case for adopting HBE as the conceptual framework for paleoarchaeology is the notion of the generalized or extended phenotype—the idea that the material remains of human behavior are as much aspects of the human phenotype as are the observable features of human biology (Dawkins 1990; O'Brien and Holland 1995, 175–200). Quantitatively, the archeological record is immensely richer than that of human paleontology, and in its later manifestations, effectively holds biological macroevolution constant. Thus, it could be argued that paleoarchaeology constitutes a better basis for building hypotheses about human behavioral evolution than does the exceptionally 'coarse-grained' time-space grid of the human fossil record (even though the latter is relatively uncomplicated by 'culture'). Most of those involved in transition research will acknowledge that, regardless of the hominid involved, there is a large component of learned behavior that acts to generate phenotypic variety, manifest in the material remains Neanderthals and modern humans manufactured, modified, lost, and discarded. Evolutionary archaeologists would maintain that selection operates on the behaviors that produced this mountain of clutter, and culture—construed here as learned behavior—simply constitutes part of the human phenotype, just as it is part of the phenotype of chimpanzees and bonobos (Clark 1997, 311).

## An Adaptationist Perspective

Consistent with modern evolutionary biology, an HBE perspective also entails adoption of an adaptationist view of human social behavior, conceptualized as systemic in nature. There are many definitions of adaptation (Mithen 1993, 393–398). One that is both widely used and consistent with HBE is 'any structure, physiological process, or behavioral pattern that makes an organism more fit to survive and reproduce' (Wilson 1975, 577). It could be argued that an important goal for archaeologists involved in transition research (in fact, paleoarchaeology in general) is to develop an approach to the study of the paleolithic that emphasizes changing adaptive systems. Those archaeological research traditions in which CH has a prominent place tend to overemphasize the characteristics of retouched stone tools, as if these were somehow meaningful in their own right, or to treat subsistence, paleoenvironmental, and site contextual information as if these were data categories independent of the lithics. Some have suggested that the tendency to compartmentalize aspects of the research gets in the way of the more unified approach demanded by HBE (e.g., Binford and Sabloff 1982).

## Adaptation: A Local and a Regional Problem

For all hunter-gatherers, adaptation is both a local and a regional problem, depending on the resolution of the temporal scale that is the target of inquiry (e.g., daily, seasonal, annual range, change at the generational scale, over evolutionary time, etc.). It can be defined biologically (in terms of inclusive fitness) or, in the present context, behaviorally, by identifying particular behavioral solutions from a range of possible solutions that would allow human foraging groups to persist over time. Studies cast in a broadly ecological systems framework seek to understand the evolutionary significance of different kinds of human behavior *without making the assumption that all such behavior is necessarily adaptive* (i.e., some [probably most] behaviors are adaptively neutral, some maladaptive, some beneficial in particular places and moments in time). More important, adaptation has specific empirical referents that can be monitored using archaeological data and that can potentially inform us about the nature of change or process (i.e., whether change is directional, continuous, or not; whether change is

occurring at similar or different rates; whether patterns of change are correlated with one another across different suites of variables). Analyses guided by an adaptationist perspective should be able both to identify correlated sets of variables that are changing in tandem with one another, and to isolate those that are static or exhibit a different pattern of change. Paleoclimatic fluctuations are controlled by the palynological, sedimentological, and geomorphological studies that are so fundamental to the European natural science research traditions. Time, however, is a reference variable against which to measure change attributed to other causes. Whenever possible, time is controlled by absolute dating methods and, in default of samples suited to such techniques, by dated paleoclimatic information—*never* by the supposedly time-sensitive characteristics of the retouched tool components themselves (see also Clark and Barton 1997, 309–319).

An essential aspect of HBE, the adaptationist program demands both a regional perspective and a multivariate approach to the assessment of systemic change. What this means in respect of transition research is a balanced approach that examines (1) lithic typology and technology on both sides of the transition; (2) the characteristics of raw material acquisition (source, size, quality, distribution) and transfers and how they affect lithic reduction strategies under a variety of mobility models (e.g., Kuhn 1995); (3) any evidence for organic technologies; (4) taphonomic and subsistence aspects of the archaeofaunal record; (5) site characteristics (numbers, size, artifact and faunal densities, diversity); and (6) settlement patterns in relation to paleotopography and resource distributions. As much of this pattern searching as possible should be quantified to avoid or minimize the essentialism inherent in an overemphasis on typology.

## Discussion

In my opinion, it is difficult to justify continuing to search for unambiguous lithic markers of our basic analytical units, as though they were designs painted on pottery vessels. From an HBE standpoint, such a search is meaningless. Regardless of who made the late Mousterian, transitional, and EUP industries, *a temporal and spatial mosaic of different human adaptive systems* appears to be documented empirically—one that long precedes and long postdates the Middle-Upper Paleolithic transition. This, I submit, is exactly what we would expect to find, given that adaptation is—always and everywhere—historically contingent and context specific. Arguments to the contrary invoked by the CH school imply that traces of identity-conscious social units can be wrung from empirical patterns in the paleoarchaeological record, and that these patterns, manifest in lithic typology and technology, are transmitted over time and space by traditions (i.e., social learning). The CH approach has been used successfully for several generations with regard to ceramic decoration in the recent prehistory of the American southwest, where humans are unequivocally 'modern,' temporal resolution is measured in decades, and where a rich ethnographic record allows us to monitor social learning in 'living' societies not very different from their pre-contact antecedents. Whether it is justifiable or warranted to treat paleolithic stone artifacts in a similar fashion, as culture historians would maintain, is, in my view, problematic (Clark 1989, 1993, 1994, 2005).

Leaving aside the important issue of what they might mean, the commonly invoked criteria for modern behavior (e.g., Mellars 2006, 167–182) show *no correlation whatsoever* with the appearance of morphological moderns anywhere, including their alleged homeland, Africa (McBrearty and Brooks 2000). Some of the criteria originated long before the appearance of Neanderthals, became elaborated in Neanderthal contexts, and were either lost or became still more marked features of the human condition during and after the Upper Paleolithic. Preconceptions about the authorship of the transitional industries, and typological myopia, have caused some to overlook the ecosystemic contexts in which Upper Pleistocene hominids, as social animals, evolved. Modern humans are not, of course, the 'end product' of that evolution, and are only unique in the sense that any species is unique—by virtue of possessing a unique evolutionary history. The point is that we can no longer afford to approach the problem of the transition in the purely inductive, piecemeal, atheoretical fashion that has

been the practice of the CH approach for more than a century.

## A New World Example—Clovis Origins

Contrasts between CH and HBE are not confined to the paleolithic, nor to the Middle-Upper Paleolithic transition. They are also manifest in different interpretations of models for the initial human colonization of the Americas. In a recent essay 'deconstructing' the North Atlantic Model of Clovis origins (Stanford and Bradley 2002; Bradley and Stanford 2004), I suggested that, although we can plausibly explain *some* pattern continuities (i.e., those free to vary independently from functional constraints) by invoking social learning (i.e., traditions) in contexts like recent southwestern prehistory, to do so in 'deep time' is likely to be difficult (if not impossible) because of the factors noted above: (1) the low resolution of the Pleistocene archaeological record does not allow us to identify identity-conscious social units; (2) identity-consciousness is, always and everywhere, a 'fuzzy set' with permeable boundaries (Owen 1965, 675–690); (3) ethnohistoric traditions have limited 'life-spans,' much shorter than those implied by CH advocates for their paleolithic counterparts; (4) paleolithic traditions have an enormous geographical extent, exceeding that of any real or imaginable identity-conscious social unit that might have transmitted them; and (5) even if we could detect the material residues of lithic traditions in 'deep time,' the mobility characteristic of all foragers would, in short order, have impossibly confounded any pattern that might have allowed us to identify them (Clark 2004, 103–112).

Bruce Bradley (2006, 212–217), who is widely known for his lithic expertise, reviewed the book in which this paper appeared (Barton et al. 2004). In regard to traditions, he makes a distinction between those he calls 'situational determinists' (e.g., Straus, Meltzer, Goebel, Clark) and those he calls 'independent inventionists' (e.g., Stanford, Bradley, many Old World prehistorians on both sides of the Atlantic), arguing that the former overemphasize independent invention and formal convergence, and deny a significant role to social learning, whereas the latter—while acknowledging the importance of formal convergence—also take social learning into account. Both perspectives seek to explain pattern in lithic technology, but emphasize different causal factors differently. Bradley takes issue with the situational determinists for oversimplification:

> . . . if the contexts are similar, the technologies will inevitably be similar. [pg. 216]

and for trying to explain pattern at too gross a scale:

> The challenge for (both) the situational determinists and the independent inventionists is to demonstrate their conclusions with *detailed* technological and situational analyses. [pg. 216, emphasis in original]

Although this is a perfectly reasonable suggestion, it encounters difficulties because of the physical properties of the cryptocrystalline rocks usually selected for knapping. Chipped stone is not a '-plastic' medium like metal or clay, nor is it as malleable as ground stone or bone worked by cutting, grinding, and polishing. This 'convergence of form' is characteristic of *all* lithic reduction, regardless of where it occurs in space and time. Separate species or not, it is clearly important to ask whether we can detect significant behavioral differences encoded in the material remains attributed to Neanderthals and moderns. The resolution and sophistication of our analytical methods *are* important, and we should continue to strive to improve them. That said, I simply do not believe (for the reasons just noted) that there is likely to be much of a social transmission 'signature' in the form of most chipped stone artifacts, or that more refined analysis is likely to be fruitful if the medium involved (cryptocrystalline rocks) is relatively intractable to stylistic imprint, and the time-space resolution so coarse-grained as to preclude the identification of the makers of the stylistic microtraditions implied by the CH conceptual framework.

## Concluding Remarks

It has been my intention here to compare and contrast the logic of inference that underlies the research conclusions of two intellectual traditions—that of culture history (CH) and that of human

behavioral ecology (HBE). I am not claiming that one is better than the other. I am claiming that the implicit biases, premises, preconceptions, and assumptions each one of us brings to the geographical areas and problem domains in which we work can have a significant effect on how we explain things. I submit that these nebulous, but no less real entities structure archaeological research in complex and subtle ways, and offer broadly defined conventions by which we attempt to give meaning to pattern. I also think that paradigmatic biases exhibit a fuzzy but modal character, manifest geographically and temporally, that is essentially the product of the scholarly traditions in which workers have received their training, combined with the compromises they must make in order to come to grips with archaeological evidence in actual, 'real-world' situations. There are differences of opinion as to whether or not it is possible to identify the parameters of national or regional research traditions, whether all bias is idiosyncratic, and even whether such things as research traditions exist (Knüsel 1992, 981–986). If they do exist, differences amongst them should be most apparent at the level of the metaphysic—the overarching conceptual framework that governs the entire research enterprise—since there is clearly much overlap in lower-order sociological and methodological paradigms (Masterman 1970, 59–90).

Readers should keep in mind that paleolithic archaeology is, for the most part, a nonexperimental field that is poorly developed conceptually and in which epistemological concerns are shared by only a small number of practitioners (let alone accorded any importance). That tends to leave 'high theory' (explanation) as something to be built 'from the bottom up.' Although the now-venerable and wholly commendable concern with middle-range theory (e.g., Binford 1981) has led to important new insights about the natural and cultural processes that combine to create pattern in an archaeological record, there are no guarantees that anything will cohere at higher levels of abstraction. If there is any coherence, it will come from a shared metaphysic that is essentially the product of a research tradition. For an increasing number of American paleoarchaeologists originally trained in anthropology, the metaphysic is that of human behavioral ecology. For many European scholars trained in ancient history and natural science, the metaphysic is that of history.

## Notes

1. I am indebted to two anonymous reviewers for pointing out that there is more explicit concern with inferential logic in the European research traditions than I had originally given them credit for. Since the mid-1980s (and largely due to criticisms of typology), much of this literature is concerned with technological systematics (especially, although by no means confined to, *chaînes opératoires* [which by their very nature focus on technology and raw material transfers]). There is a large body of literature devoted to technological systematics, and to cite even a sample of it here would make an overlong paper even longer. Some of the more important workers are Boëda (2005), Geneste (1990), Geneste et al. (1997), Laville and Marambat (1993), and Meignen (1988). Then there are a series of books and papers that focus on explanatory frameworks at the highest level, e.g., Leroi-Gourhan (1964, 1965), Gardin (1980), Gallay (1989), Stoczkowski (1994), Coudart (1999a, 1999b), Cleuziou et al. (1991), Audouze (1999), Bicho (2002), Rigaud (1997), Delagnes and Meignen (2006), Vega (1993), papers in Scarre and Stoddart (1999), and an Ucko-edited volume (1995), to name just a few. Except for the works of André Leroi-Gourhan, however, much of this literature has had relatively little impact on paleolithic archaeology. Whether or not the shift in emphasis from typology to technology has had an impact on explanation is more difficult to ascertain. As mentioned previously, a case could be made that many European explanations for pattern (especially those in the Franco-Cantabrian 'heartland') are largely uninformed by theory, are 'history-like,' and are based on artifact-making traditions that persist for, in some cases, tens of millennia. I would submit that the contention that they are technocomplexes begs the question of transmission as much as the contention that they map onto identity-conscious social units of some kind. A good example is the conclusion

that the (presumably Neanderthal) makers of the Mousterian at Bettencourt exemplify a tool-making tradition that persisted in the Somme valley for c. 40,000 years (Locht and Révillion 2002, 167; cf. Clark 2005).

2. The idea that the undifferentiated cultural system cannot be analyzed and understood holistically was first articulated by Binford more than 45 years ago (1962, 217–225).

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