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Is It Evolution Yet?

A Critique of Evolutionary Archaeology¹

by James L. Boone and
Eric Alden Smith

The application of Darwinian evolutionary theory to archaeology has taken two divergent and rather distinct paths over the past two decades. According to one program, often referred to as evolutionary archaeology, cultural change as seen in the archaeological record can best be explained in terms of the direct action of natural selection and other Darwinian processes on heritable variation in artifacts and behavior. The other approach, referred to as evolutionary or behavioral ecology, explains cultural and behavioral change as forms of phenotypic adaptation to varying social and ecological conditions, using the assumption that natural selection has designed organisms to respond to local conditions in fitness-enhancing ways. We argue that the primary conflict between the two approaches centers on fundamental differences in the way they view the explanatory role of phenotypic variation and more specifically a disagreement over whether behavioral innovation is random with respect to adaptive value (including related issues of current versus future selective advantage and the explanatory role of intentions). These differences lead to contrasts in explanatory scope, empirical application, and theoretical conclusions, which in turn provide the basis for our evaluation of the relative utility of each approach for explaining archaeological phenomena.

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Over the past two decades, a number of programmatic statements advocating the application of Darwinian theory to archaeological phenomena have appeared in the literature. According to one program, cultural change as seen in the archaeological record can best be explained in terms of the direct action of natural selection and other Darwinian processes on heritable variation in artifacts and behavior (Dunnell 1980, 1989; Leonard and Jones 1987; O'Brien and Holland 1990, 1992; O'Brien 1996; Neff 1992, 1993; Teltser 1995*b*). We will refer to this research program as "evolutionary archaeology" and its practitioners as "evolutionary archaeologists" (or, following their own usage, as "selectionists").

Evolutionary archaeologists repeatedly stress that their program represents an epistemological or metaphysical (Dunnell 1980) break with other archaeological and anthropological approaches. Their ambition is clearly one of paradigm replacement rather than supplementation or division of labor: "those who espouse a selectionist approach are in a struggle for the attention of the profession. It is our goal to effect a complete paradigm shift within archaeology, not simply to amuse ourselves with academic debates" (O'Brien and Holland 1995:193-94). In its boldest formulations, evolutionary archaeology dismisses all past explanations of cultural and technological change as vitalistic and unscientific and proposes in their place a system of explanation in which natural selection, drift, and possibly other evolutionary forces explain changes in artifact frequencies without any recourse to human agency, decision making, or behavioral reconstruction.

Contemporary with this development has been the introduction into archaeology and ethnography of a body of theory known as evolutionary or behavioral ecology (e.g., Bettinger 1980, 1991; Hegmon 1989; Kelly 1995; Metcalf and Barlow 1992; Mithen 1989*a*, 1990; O'Connell 1995; Russell 1988; Winterhalder and Smith 1981; Smith and Winterhalder 1992*a*; for a comprehensive overview, see Krebs and Davies 1991 or 1997). Evolutionary ecology explains cultural and behavioral

change as forms of phenotypic adaptation to varying social and ecological conditions, using the assumption that natural selection has designed organisms to respond to local conditions in fitness-enhancing ways. Taking this assumption of adaptive design as a starting point, evolutionary ecologists formulate and test formal models incorporating specific optimization goals, currencies, and constraints.

Judging from the virtual lack of cross-referencing in the literature, these two programs seem to view each other as irrelevant at best and mutually exclusive or even antagonistic at worst. How can it be that two programs that derive from the same overarching theoretical framework—Darwinism—arrive at such different views on how to describe and explain the archaeological record? We argue that the primary conflict between the two approaches centers on fundamental differences in the way they view the role of phenotypic variation, and in particular behavioral variation, in the evolutionary process. From these differences flow a series of consequences in explanatory scope, empirical application, and theoretical conclusions. The aim of this paper is to outline these differences and to evaluate the relative utility of each approach for explaining archaeological phenomena.

Although the relationship between evolutionary archaeology and evolutionary ecology may seem to be a rather arid academic dispute turning on some esoteric points of evolutionary theory, we feel that the implications for future research in archaeology and on culture change generally are quite broad. If the proponents of evolutionary archaeology are correct, a clean sweep of existing paradigms in archaeology and even ethnography is scientifically warranted. Given the powerful, unifying role of Darwinian theory in the life sciences and the fractured state of theory in the social sciences, evolutionary archaeology's claim to the mantle of Darwinism comes at a significant moment in the history of anthropology. For this reason, we have endeavored to use the simplest, least ambiguous language possible in order to make this critique accessible to a broad audience of professionals who may not have expertise in archaeology or evolutionary theory. Of course, there are other critiques of evolutionary archaeology in the recent literature (e.g., Schiffer 1996), but unlike these our critique is rooted in an acceptance of the general Darwinian framework; in other words, we locate the weakness of evolutionary archaeology in its flawed grasp of evolutionary biology rather than in its advocacy of Darwinism *per se*.

We begin with a brief outline of the fundamental logic of natural selection, noting the critical distinction between phenotypes (such as behavioral patterns) and replicators (such as genes). We argue that in many cases the process that evolutionary archaeologists are calling "selection" is not selection at all but phenotypic adaptation to environmental variation; we illustrate this process using two examples from the archaeological and ethnographic records. We then take up a central disagreement between evolutionary archaeology and evolutionary

ecology, the issue of whether behavioral variation is undirected with respect to adaptive value (including the related issues of current versus future selective advantage and the explanatory role of intentions). Finally, we contrast the evolutionary archaeological approach to explaining archaeological change with that of evolutionary ecology, using the origins of plant domestication as a heuristic example. We conclude with a brief assessment of the archaeological promise of each approach.

What Is Evolving? Replicators and Phenotypes

In modern synthetic Darwinian theory, evolutionary change proceeds through the action of natural selection and other forces (e.g., mutation, drift) on genotypic variation and its phenotypic expression. Of these, only selection produces cumulative, directional (nonrandom), and creative evolution. As noted by Lewontin (1970) and many others (e.g., Dunnell 1980:38), in outline natural selection requires only three conditions: variation, inheritance (transmission), and differential fitness. But because of the translation between genotype and phenotype—a process inextricably linked to environmental and developmental factors—and the complexities of inheritance wherever sexual reproduction is present, the simplicity of evolution by natural selection is nested within an extremely complex ontogenetic and populational context. Thus, a somewhat fuller outline of the evolutionary process would be as follows: (1) genetic variation is continually produced by mutation and recombination; (2) this variation interacts with external environmental factors to shape phenotypes; (3) these phenotypes and associated genotypes are differentially successful in surviving and reproducing; (4) offspring inherit (some of) the genes and thus tend to develop the associated phenotypes of their parents; (5) the proliferation of more successful genotypes results in transgenerational increase in phenotypes that are better adapted to local environments.

In adopting the Darwinian framework, evolutionary archaeologists have simply substituted phenotypic variation for genetic, arguing that evolution by natural selection applies to any population of entities characterized by heritable variation and differential replication success of the variants. They further argue that since artifacts are a component of the human phenotype, changes in artifact frequencies through time can be explained by the same principles used in evolutionary biology, that is, the action of selection on phenotypic variation. This position is clearly stated in various passages (e.g., Leonard and Jones 1987:213; O'Brien and Holland 1992:37), including the following: "Artifacts do not 'represent' or 'reflect' something else that is amenable to evolutionary theory; they are a part of the human phenotype. Consequently, artifact frequencies are explicable by the same processes as those in biology"

(Dunnell 1989:45). In this view, the forces or processes that give rise to phenotypic variation are unimportant to the analysis of evolutionary change. All that matters is that variation have some heritable component and that this variation have differential fitness effects such that natural selection can occur. In evolutionary archaeological theory, the production of new phenotypic variation (including novel forms of behavior or artifacts) is seen as conceptually analogous to the process by which new variants arise in the genetic code—undirected mutation and recombination.

Although seemingly straightforward, this approach to evolution by natural selection makes some very problematic assumptions. Foremost among these is the way it handles the genotype-phenotype distinction. To be sure, the heritability requirement does not specify that inheritance be genetic; it could in principle be *cultural* (the standard view in evolutionary archaeology). But selection does require that there be *replicators*—units of heritable variation. As Dawkins (1978; 1982:81–117) and others (e.g., Hull 1980) have pointed out, replicators must have certain causally significant qualities: longevity (they last for many generations), fecundity (they produce copies of themselves), and copy fidelity (they are replicated with near-perfect accuracy). Genes, consisting of DNA, and memes or “culturgens” (symbolically or neurologically encoded information transmitted via social learning) are the two most commonly identified replicators.²

The particulate nature of inheritance—the crucial fact that individuals (phenotypes, or “vehicles,” as Dawkins terms them) do not themselves replicate but are dissolved each generation—has profound implications for evolutionary theory (e.g., Williams 1966, Dawkins 1982, Sterelny and Kitcher 1988). For one, it means that selection will generally design individuals to behave in ways that will lead to maximal representation of replicators in future generations. This in turn focuses analytical attention on the relationships between replicators, vehicles, and phenotypic traits; all these elements play crucial but quite distinctive roles in biological evolution.

How has evolutionary archaeology dealt with this critical issue? According to Dunnell (1980:87), “Perhaps the most fundamental problem in developing evolutionary theory for cultural phenomena is the matter of the unit of transmission. . . . Yet if evolutionary theory is to be applied in archaeology with any rigor at all, this issue has to be addressed in concrete terms.” Despite this pronouncement, the mechanics of inheritance and their critical consequences for the form that evolutionary processes take have been virtually ignored in the evolutionary archaeological literature (with the notable exception of Neiman 1995). Indeed, we are told that the specific mechanisms of inheritance are irrelevant: “knowledge of how inheritance is effected is not neces-

sary nor is a knowledge of the source of variability” (Dunnell 1980:62).

In his recent paper entitled, provocatively enough, “What Is It That Actually Evolves?” Dunnell (1995) discusses the possible evolutionary relationships between individuals, species, assemblages, and societies *but never acknowledges the replicator-phenotype distinction* or the issues raised by nonparental cultural transmission. This omission is striking given the central role that these matters have played in contemporary evolutionary theory in general and cultural evolutionary theory in particular (e.g., Dawkins 1976, Hamilton 1996, Sober 1984, Trivers 1985, and Williams 1966 for biology; Boyd and Richerson 1985, Cavalli-Sforza and Feldman 1981, Dennett 1996, and Durham 1991 for cultural evolution). By making artifacts the evolutionary unit subject to variation and selection, evolutionary archaeologists are either ignoring the replicator-phenotype distinction and all its implications or proposing that artifacts themselves are replicators; either position is highly problematic.

In biology, phenotypes are defined as the observable result of the interaction between genotype and environment (Mayr 1976:10)—essentially all features of an organism except its genes. Obviously, behavior is included in this encompassing definition, even behavior that is culturally transmitted. Evolutionary archaeological theorists have argued that artifacts, being “the equivalents of physical and behavioral traits” (Leonard and Jones 1987:215), are what Dunnell (1989:44) has colorfully termed “the hard parts of the behavioral segment of phenotypes.” Recently, some evolutionary archaeologists have employed Dawkins’s (1978, 1982) notion of the “extended phenotype” as a justification for viewing artifacts as phenotypic traits, arguing that just as biologists “routinely include such things as spider webs and bird nests in their concept of phenotype, we see no reason not to extend in similar fashion the notion of the human phenotype to include such things as projectile points and pottery, or . . . such artifacts as ceremonial architecture” (O’Brien and Holland 1995:181).

But if behavior and its products (artifacts) are phenotypic—a position we agree with—then in order to apply Darwinian analysis to them we must determine what *replicators* are associated with them. More fundamental, Darwinian analysis must examine the ways in which phenotypic traits affect the replication success of their associated replicators. Since evolutionary archaeology has failed to do so, there is no logical foundation for Dunnell’s (1989:45) claim (quoted above) or the following: “Since selection works on the phenotype—the vehicle that carries and protects the germ-line replicators (the genes)—then, with regard to humans, those things they manufacture and use to modify their environment are subject to selection in the same way any somatic feature is” (O’Brien and Holland 1995:181). The problem with this conceptualization is that selection can act on phenotypic variation (e.g., artifact design and frequency) only to the extent that it is heritable—that is, correlated with replicators transmitted from parent

2. Other nominees for replicator status include immunological antigens (Burnet 1959, Ada and Nossal 1987) and neural circuits in Edelman’s (1987) “neural Darwinism” theory of learning.

to offspring (or, in the case of cultural replicators, from model to recipient). The evolutionary archaeological program assumes this correlation without further examination and often without even articulating the assumption. Furthermore, given that evolutionary archaeology is positing *cultural* inheritance, its failure to pay attention to the effect of cultural transmission pathways (e.g., parental versus nonparental, generational versus peer-to-peer, one-to-many versus many-to-one) as well as secondary forces such as evolved preferences is puzzling. Though its proponents often cite the theoretical literature on cultural evolution (e.g., Boyd and Richerson 1985, Cavalli-Sforza and Feldman 1981, Durham 1991), they do not discuss the conclusion of this theory that cultural transmission might sometimes produce evolutionary trajectories that differ radically from those governing traits linked to Mendelian inheritance.

Evolutionary ecology takes a different and conceptually more complex but realistic view of phenotypic variation. It holds that organisms (including humans) have been designed by selection to make extensive adaptive adjustments of their phenotypes. A nonbehavioral example of this is the tanning response found in all but the darkest-skinned or albino people. Tanning is clearly phenotypic variation, even when it involves historical, intergenerational change (e.g., a population that has gone from working in the fields to working in factories and exhibits a diachronic shift in the frequency or intensity of tanning). In the behavioral realm, this process of adaptive phenotypic variation involves the interaction between genetically or culturally evolved cognitive mechanisms and variable environmental conditions. Under this view, natural selection's primary role lies in accounting for these cognitive mechanisms—that is, why they evolved and why they work the way they do—and not in culling behavioral variation. Correspondingly, environment plays a causal role in eliciting phenotypic variation, not just a selective one after the fact. In colloquial terms, the evolutionary ecological position is nothing more than a claim that organisms have problem-solving abilities at various levels (physiological, morphological, behavioral) and scales (short-term, developmental, lifelong).

A phenotype's tendency or capacity to respond differentially to varying environmental conditions is called phenotypic *plasticity* (or lability). The phenotypic plasticity of a given replicator (e.g., genotype) over a range of environmental conditions is termed its *reaction norm* (Lewontin 1974:404; Stearns 1992:61–65). In this view—standard within biology in general, including evolutionary ecology—phenotypic variation that results from the interaction of genotype with environment does not itself constitute evolutionary change, though an organism's capacity for adaptive phenotypic plasticity is an evolved trait shaped by natural selection. If the phenotype's reaction norm is very broad, the potential for synchronic variation or diachronic change in the phenotype without any evolutionary change per se (i.e., without changes in the frequency of heritable

traits or heritable variances of traits) is correspondingly great.

Behavior is typically the most labile component of an organism's phenotype. As contemporary evolutionary biologists see it, the evolutionary *raison d'être* of behavior is to allow organisms greater flexibility in responding to variable environmental challenges in ways that enhance survival and reproduction (Dawkins 1976: chap. 4; Pulliam and Dunford 1980:chaps. 1–3). In other words, behavioral plasticity allows organisms to adapt to changes in environmental conditions more rapidly than they could through the process of selection acting on genetic variation. Thus, even though this plasticity exacts fitness costs (in terms of development, metabolic maintenance, and potential malfunction), in particular niches it more than repays these costs and hence has evolved by natural selection.

Evolutionary ecology generally analyzes phenotypic variation in terms of "adaptive strategies"—that is, as a series of fitness-enhancing behavioral responses to different environmental states (assuming that these states have been recurrent within the evolutionary history of the organism's lineage and that the responses fall within its norm of reaction). This form of phenotypic response is thus construed to be based on a set of evolved "decision rules" (Krebs 1978)—genetically evolved cognitive mechanisms that guide development, learning, problem solving, and stimulus response. Hence, in this view behavioral variation itself is not the direct product of natural selection. Rather, selection enters the explanation only indirectly, as the process that designed the behaving organism (or in fact its ancestors) to respond facultatively and adaptively to particular environmental conditions.

Most writers of both approaches seem to agree on the theoretical importance of cultural transmission to an evolutionary understanding of historical change. For example, without it evolutionary archaeology could make no claim that phenotypic variation is heritable, one of the three essential requirements of the theory of evolution by natural selection. At the same time, the two approaches diverge in their treatment of phenotypic traits. Evolutionary archaeology treats these (especially artifacts) as both vehicles (phenotypic) and replicators (directly subject to natural selection). In contrast, evolutionary ecology expects the transmission of cultural variants to be heavily influenced by previously evolved cognitive biases or decision rules. If the latter view is correct, frequency changes in these variants over time may be caused by factors other than concurrent natural selection.

Selection or Phenotypic Adaptation? Two Examples from the Archaeological Record

Evolutionary ecologists and evolutionary archaeologists seem to agree that "natural selection is the primary ex-

planatory mechanism in scientific evolution" (Dunnell 1980:49). Selection operates as a mechanism or process of evolutionary change in a population when some variable heritable trait has correspondingly variable effects on the fitness of the individuals that inherit it. For example, one major component of fitness is the number of offspring that survive to reproduce; parents that produce more surviving offspring relative to others in the same population are said to have higher reproductive success. A heritable trait that causes its bearers to have higher reproductive success than others in the population will increase in frequency within the population over time.

Evolutionary archaeologists have tended to consider all directional phenotypic change through time as the result of natural selection acting directly on cultural variation (Dunnell 1978). Yet most of the evolutionary archaeological literature is quite unclear on the mechanism(s) underlying selection. Some (e.g., O'Brien and Holland 1995:190–91; Ramenofsky 1995:135–39) suggest that selection works via reproductive differences among the individuals who utilize certain variable artifacts to interact with the environment. Others (e.g., Leonard and Jones 1987:214; Jones, Leonard, and Abbott 1995:28–29; Teltser 1995a:5–6) argue that it is replicative success of phenotypic traits (behavior or artifacts) that matters, whether or not this is tied to reproductive success. Sometimes this ambiguity concerning the mechanism(s) of selection is directly indicated, as in Jones et al.'s (p. 26) reference to "functional traits, and the processes that influence their differential survival, collectively termed selection." Most selectionists simply avoid stating a position on the issue or do not recognize it as an issue.³

In contrast, evolutionary ecology argues that selection acting on heritable variation is but one of several processes by which changes in the frequency of phenotypic variants through time occurs. As we have suggested, one of the most important of these processes is individual phenotypic variation in response to environmental variation such as exogenous changes in prey abundance, climate change, and the like—in other words, nongenetic adaptation to local (and locally variable) conditions. Evolutionary ecology also proposes that the aggregate consequences of individual phenotypic adaptation can both change environmental conditions—as through increases in population density, resource depletion, habitat modification, or mate availability—and elicit new strategic phenotypic adaptation to these altered conditions. Thus, quite complex and directional changes in phenotypic variation over historical time are expected to result from the mecha-

nisms of phenotypic adaptation privileged in evolutionary ecology.

Behavioral variation of the type just discussed will produce corresponding variation in the kinds and frequencies of artifacts and ecofacts associated with these behaviors, including those which become part of the archaeological record. Hence, the resulting variation and directional change that we observe archaeologically cannot be assumed to have resulted from natural selection acting on culturally transmitted variation; it could instead be the result of facultative behavioral strategies that are themselves the product of earlier evolutionary processes extending back thousands or millions of years. Thus, if we wish to explain variation in the archaeological record (or any other manifestation of human behavior), we have at least two alternatives: we can attribute this variation to the action of natural selection on adaptively random cultural variation (the evolutionary archaeological program), or we can attribute it to adaptive phenotypic variation (the evolutionary ecological program).

In sum, analyzing synchronic variation or diachronic change requires that we consider two distinct causal processes, one evolutionary and the other phenotypic. Evolutionary ecologists tend to focus on strategic phenotypic response and assume that the trait under study has been designed by natural selection to have sufficient phenotypic plasticity to track environmental variation optimally (i.e., in fitness-maximizing ways). Hence, they do not equate phenotypic variation with evolutionary change; instead, they attribute it to evolved capacities for adaptive variation (behavioral, physiological, etc.). The evolutionary archaeological paradigm, in contrast, minimizes the role of phenotypic adaptation via decision making and ascribes adaptive change to the action of natural selection on culturally inherited phenotypic variation. Neither view is likely to be 100% correct, but we argue that the evolutionary ecological approach is likely to explain a much larger proportion of the phenotypic change preserved in the archaeological and ethnographic records. We will now examine this issue and illustrate these principles with reference to two cases of historical change.

THE EMERGENCE OF BROAD-SPECTRUM FORAGING IN THE ARCHAIC

Our first example concerns changes in prey choice that are revealed in the archaeological record of the North American Archaic. This period is generally characterized by a major shift in hunting emphasis from large-bodied game to smaller prey and in many cases a greater emphasis on gathering and processing of wild plant foods (Bayham 1979). This trend has been closely documented in Central California by Broughton (1994); a parallel trend occurred in many regions of the Old World during the Mesolithic. In the language of foraging theory, the trend can be characterized in terms of an increase in diet breadth (Broughton 1994:501), wherein

3. Recently, some evolutionary archaeologists (Jones, Leonard, and Abbott 1995:28; Ramenofsky 1995) have suggested that nonrandom or directional frequency change of traits can also be due to "sorting" (differential persistence of evolved lineages [Vrba and Gould 1986]) or some other type of correlation with traits undergoing selection. To date, however, no publication has explored the archaeological implications of this proposed mechanism.

“change in emphasis” is defined as a process by which human foragers broaden the range of prey taken by progressively adding lower-ranked (i.e., less efficiently harvested) prey types to a previously narrower diet of higher-ranked prey types (Smith 1983a, Kaplan and Hill 1992).

Why would humans expand their prey choice to include lower-ranked prey types? Broughton (1994, 1995) uses optimal foraging theory to generate a number of hypotheses about patterns of prehistoric prey choice in Central California, including the central one that a decline in the abundance of more profitable (higher-ranked) large-bodied prey, such as artiodactyls (deer and elk) and sea otters, led to an expansion of the optimal (and hence observed) diet to include more lower-ranked prey such as shellfish, small game, and fish. Broughton suggests that the per-capita decline in high-ranked prey was due to long-term increase in human population density, but the optimal-prey-choice model would predict the same broadening of the diet in the case of anthropogenic resource depletion. Climatic change may also have been responsible for this decline in high-ranked prey in some regions.

Whatever the cause, foraging theory predicts that reduced encounter rates for higher-ranked resources will eventually shift the optimal (efficiency-maximizing) diet breadth to include lower-ranked (higher-cost) resources (Charnov 1976, Stephens and Krebs 1986). This is because the increased search time resulting from declining encounter rates for high-ranked prey reduces the overall return rate for specializing on such prey relative to the returns that can be obtained from expanding the diet to include lower-ranked but more frequently encountered prey types.⁴ Thus, evolutionary ecology explains the trend toward broad-spectrum foraging in the Archaic in terms of the long-term aggregate consequences of changing individual decisions of Archaic foragers in response to declining availability of large-bodied animals. From this perspective, adaptive phenotypic flexibility (decision making) is entirely sufficient to explain the trend in question, and no appeal to selection acting on cultural variation is necessary. Natural selection is required only to explain why Archaic foragers (in common with human and nonhuman foragers everywhere) have evolved the cognitive capabilities to make adaptive economic decisions.

SNOWMOBILES IN THE SUBARCTIC

The above example of adaptive change centered largely around changes in resource choice. Of course, the Archaic and Mesolithic periods are also characterized by the appearance and spread of a wide range of tools, im-

plements, and facilities for capturing, processing, and storing the animal and plant foods that were added to the diet. Since the techniques for making and using such implements must have spread through some process of cultural transmission—that is, we do not imagine that each Archaic forager reinvented, say, the side-notched point or seed-beater basketry as needed—one might argue that here is where evolutionary archaeology’s selectionist paradigm should come into full play. But again, we must keep in mind that natural selection acting on culturally transmitted variation is not necessarily the only or even the most important process responsible for the spread of innovations and corresponding artifact frequency changes. From the evolutionary ecological perspective, adaptive phenotypic plasticity (decision making and selective imitation) is a more plausible alternative.

To illustrate this point we shift our attention to a more recent example of technological change in foraging strategies that allows us to examine the actual process of change in more detail. In an article on Native American artifact replacement by European goods, Ramenofsky (n.d.) posits that the rapid increase in European horses in the 16th century and the Cree use of snowmobiles (Winterhalder 1980, 1981) are “clear examples of *variants that increase due to selection*” (n.d.: 7, emphasis added). With respect to the Cree use of snowmobiles, we are in a good position to examine this claim more closely.

At the time of his field study (which took place in 1975), Winterhalder found that snowmobiles had come into general use among the Boreal Forest Cree, with considerable effect on their foraging strategies. If Ramenofsky’s claim that this process is due to selection rather than decision making is correct, we must imagine the following course of events to have occurred: First, some Cree foragers adopted, for whatever reasons, the use of snowmobiles in hunting, while others continued to walk to hunting sites on snowshoes. The snowmobile users then experienced higher fitness in the form of larger numbers of surviving offspring, perhaps because of greater foraging returns that could be used to feed more offspring or because of higher return rates that freed time to engage in other fitness-increasing activities, such as mating, child care, and wage earning. Snowmobile use was then transmitted culturally to the offspring of snowmobile adopters. Since hunting resources are ultimately limited, the resulting increase in snowmobile use eventually led to replacement of snowshoe hunters by snowmobile users.

Clearly, the process just outlined would be quite slow, requiring many generations to result in the replacement of snowshoe hunting by snowmobiles, the number of generations depending upon the fitness differential between snowmobile users and traditional hunters. Yet Winterhalder (1981:88) reports that the Cree adopted snowmobiles over the space of less than one human generation. Hence, it seems clear to us that the rapid increase of snowmobile use, *contra* Ramenof-

4. There are significant complications involved in applying this optimal-prey-choice logic to central-place foragers utilizing multiple patches; these issues are reviewed in general terms by Stephens and Krebs (1986), Smith (1991), and Kaplan and Hill (1992) and discussed in detail for the prehistoric California case by Broughton (1995).

sky, cannot be due to the effect of natural selection acting on variation in locomotion techniques among the Cree.

Two objections to our argument might be raised at this point. First, given the short time involved, one might question whether Ramenofsky really means that natural selection is the process or mechanism responsible for the increase in the frequency of snowmobile use. Our interpretation that she does is bolstered by her recent discussion of the spread of the horse among historic Plains Indians (1995:138–39, emphasis added):

Within 100 years of its introduction, the horse had diffused as far east as Texas, north into Canada, and south into Mexico (Ewers 1955). This rapidity suggests that the horse was a functional trait that greatly increased the fitness of individuals within populations. The strength of the horse out-competed humans and dogs as a means of transport; the speed of the horse gave it a unique advantage in hunting. *Consequently, individuals who owned horses reproduced in greater numbers than others.*

Clearly Ramenofsky sees the advantages of horses as leading to their spread through natural selection (of cultural variation) rather than through adaptive decision making, and since she has recently (n.d.) linked this case with the Cree shift from snowshoes to snowmobiles and termed both the result of “selection,” we think our interpretation of what she means by “selection” is correct.

Second, one might question whether “selection” on cultural variation must act through differences in biological reproduction. As discussed in detail by cultural evolution theorists (e.g., Cavalli-Sforza and Feldman 1981, Boyd and Richerson 1985), since cultural inheritance is not limited to parent-offspring transmission, the replication rate of memes need not be constrained by the generation length of culture bearers. Thus, some memes may spread “horizontally” (e.g., between peers) or even “obliquely” (from elders to various sets of non-descendant juniors) in a rapid fashion more akin to epidemics than to genetic inheritance. However, if (as in the example discussed here) the postulated evolutionary mechanism is natural selection, then differential transmission requires heritable variation in individual survival and/or individual reproductive success, and therefore generation length becomes an important rate-limiting constraint. The alternative that “snowmobile memes” were transmitted more effectively than “snowshoe memes” to non-descendant Cree (as well as offspring), while plausible, is not natural selection; more significant, it requires precisely the kind of adaptive decision making that evolutionary archaeology is dedicated to eliminating from archaeological explanation.

How, then, can we characterize this process of change within the framework of evolutionary theory? We would argue that increased snowmobile use is the result of the fact that Cree hunters, like all humans, inherit

evolved cognitive capabilities that allow them to perceive the relative efficiency of different means (e.g., snowmobiles versus snowshoes) for acquiring resources and to make decisions regarding adoption of new technology or patterns of behavior according to which will produce the highest net gains.⁵

We argue that adopting such an explanatory strategy allows for a much richer analysis of change as well. For example, Winterhalder argued that the Cree adopted snowmobiles because doing so increased foraging efficiency by reducing the amount of time it took to travel from settlements to hunting areas; in optimal foraging terms, snowmobiles decrease prey search time. Using the optimal foraging framework allowed Winterhalder to make various predictions regarding changes in the variety and range of prey taken upon encounter with the adoption of snowmobiles. Specifically, an increase in search efficiency is predicted to result in a constriction of the diet (1981:89); Cree hunters using motorized transport should concentrate on more profitable prey such as moose and ignore lower-ranking prey with lower return rates such as hare. As it turned out, Winterhalder was able to confirm that Cree diet was broader prior to the adoption of snowmobiles and outboards. We note that without the evolutionary ecological postulate that evolved cognitive abilities allow foragers to weigh the economic costs and benefits of various tactics and strategies and to choose the tactic or strategy that gives the highest energy return under the circumstances, the intimate strategic relationship between foraging technology and diet breadth would remain theoretically opaque.

DISCRIMINATING PHENOTYPIC FROM EVOLUTIONARY CHANGE

It might be argued that, if natural selection and adaptive decision making have the same outcome (e.g., snowmobile adoption) and enhance fitness in either case, it doesn't matter whether we adopt the evolutionary archaeological or the evolutionary ecological explanation. Now, it is true that adaptive decision making over a short time scale may produce results (including fitness effects) that are equivalent to the effect of natural selection acting over longer time scales. This is largely because capabilities for phenotypic adaptation (including adaptive decision making) are themselves the product of past natural selection. However, this does not mean that the two processes of adaptive change should be

5. Just how detailed and fine-tuned these cognitive mechanisms are and how they are shaped by inheritance or learning are empirical matters that evolutionary ecologists are continually investigating in humans and nonhumans. For example, how closely can people or other organisms discriminate different mean rates of energy gain, given variance in these rates over time? How extensively are other components of fitness (e.g., mortality risk) traded off against resource capture rate? What are the relative roles of genetic and cultural inheritance in shaping these decision rules? We think it premature to take strong positions on these issues.

conflated or that we can assume they will *always* produce the same outcomes. The reasons for this have some rather far-reaching implications for how we should view variation, selection, and evolutionary change.

First, natural selection results in trait frequency changes through time by favoring some variants and culling out others; the basis of this culling process is the differential success of replicators, generally via differential survival and fertility of organisms that exhibit the variant traits and transmit the variant replicators undergoing selection. This means that the rapidity with which selection can act is significantly constrained by the generation span of the organism in question. Second, the strength of selection depends on (1) the amount of variation already existing in the population and (2) the degree of differential fitness that the variant traits confer on the individuals carrying them—mathematical features of natural selection enshrined in every textbook on the subject.⁶ In contrast, the rapidity with which phenotypic response to changing environmental conditions occurs is dependent not on the amount of variation that already exists in a population but on the rapidity with which environmental change is occurring. Nor is the rapidity with which innovations can spread through learning or cultural transmission necessarily dependent on the amount of preexisting phenotypic variance. This is because innovations or variants can increase in frequency in a population not just through a culling process acting upon existing variation but because they in some way satisfy evolved preferences or decision rules better than do existing variants (Boyd and Richerson 1985:175). In other words, although traits adopted through social learning may well affect fitness, their increase or decrease in frequency through time is not necessarily through the mechanism of differential reproduction. We believe this point is critical for understanding phenomena such as the adoption of horses or snowmobiles.

Is Behavioral Variation Analogous to Mutation?

THE CONCEPT OF UNDIRECTED VARIATION

A central tenet of evolutionary archaeology is the idea that behavioral variation and innovation are undirected or independent with respect to selection. As Dunnell (1980:62) succinctly puts it,

Selection determines which [behaviors] will be transmitted, not which will occur. Behavioral variability, no less than mutation in strictly biological settings, does not direct evolution. Selection acting on variation does. If inclusive fitness or any other evolutionary concept would allow us to predict the appear-

ance of individual behaviors, the same notions should allow us to predict mutations, a patently absurd notion.

This statement illustrates the reasoning by which this approach rejects any explanatory paradigm which includes decision making or adaptive response. It is ultimately based on the following logic:

1. Darwinian evolution designs adaptations through the action of natural selection acting upon heritable variation.
2. Through cultural transmission, phenotypic variation (including behavior and artifacts) becomes heritable.
3. Undirected variation is an essential aspect of genetic evolution.
4. Therefore, it is essential to cultural evolution as well.

While we accept 1–3, we hold that 4 is a non sequitur.

The principle of undirected variation is certainly critical to the theory of natural selection, for reasons stated clearly in the following passage (Rindos 1989a:39):

From the Darwinian perspective, undirected variation is important for its role in fueling the engine of evolutionary change by generating new forms which may then be subject to selection. Indeed . . . without a true concept of undirected variation, natural selection is not only unnecessary but is actually impossible. If variation is less than undirected, then natural selection cannot be seen as a *creative* force in evolution. . . . Only if we see variation as being produced randomly with respect to selective pressures may we claim that the directionality that may be observed in evolution over time is the result of natural selection.

In the genetical theory of evolution by natural selection, the ultimate source of variation is mutation of the genetic code. This mutation process (along with other sources of genetic variation, such as recombination) is generally recognized to be undirected or random. What exactly does “undirected” or “random” mean here? It means that the “chance that a specific mutation will occur is not affected by how useful that mutation would be” (Futuyma 1986:76), where “useful” refers to effects on the organism’s survival and reproduction (i.e., fitness value). For example, the probability that the mutation that gives rise to the sickle cell trait will occur is completely unaffected by the current prevalence of malaria. Once the variant exists, the prevalence of malaria acts as a selective factor determining its frequency in the population’s gene pool. The production of mutations is “undirected” with respect to current selective advantage, and hence the production of novel variation in genetical evolution is entirely independent of the current selective advantage of new variants.

The central issue at hand, then, is whether the generation of behavioral variation is independent of selective pressures (i.e., uncorrelated with adaptive benefit) and

6. This relationship between the strength of selection and the amount of heritable variation also applies to cultural transmission systems (see Boyd and Richerson 1985:chap. 6).

hence conceptually analogous to mutation. We argue that it is not. While past selection does not determine when, where, how, and why a particular mutation will occur, it does determine to a large extent when, where, how, and why an organism will express a particular behavior (or other phenotypic state) in response to current (or even projected) environmental conditions. Although mutations (with possible rare exceptions) are never statistically directed in fitness-enhancing directions, many species have evolved capabilities for phenotypic modification that are indeed directed towards fitness enhancement. In the decision-rule paradigm of evolutionary ecology, these take the form “under condition α do x , but under condition β do y ” (where x confers greater fitness benefit than y under α but less than y under β).

Of course, the directedness of behavioral innovation is a matter of degree. We expect that behavioral innovation will sometimes be random with respect to fitness gain, particularly in novel ecological and social settings; the various problem-solving cognitive mechanisms (rational choice, scenario construction, etc.) are certainly not omniscient. Even in these cases, however, we expect that genetically evolved learning mechanisms (e.g., operant conditioning) will reshape behavior in fitness-enhancing directions within a relatively short time (i.e., less than the lifetime of an individual organism); after all, this fitness payoff is why selection designed these mechanisms in the first place.⁷ Obviously, if we are correct about the generally adaptive nature of behavioral innovation, this will often short-circuit the chance for natural selection to alter the frequencies of such behavior.

THE FUTURE IS NOW

In arguing that behavioral or cultural innovations are “undirected” with respect to selective pressures, some evolutionary archaeologists have in fact expanded the original concept of undirected or adaptively random variation to apply to both current and *future* selective conditions (i.e., conditions that do not yet exist). For example, Jones, Leonard, and Abbott (1995:18) state that “innovations arise independently of the processes of selection. While the production of variants is to a degree constrained by preceding states of the system, the nature of that variation is not determined by the future course of the system.” By “future course of the system” Jones et al. seem to mean future selective pressures, for in the next few sentences they illustrate their point by arguing that many technologies have become “far more successful in contexts unrelated to ones for which they were intended originally” (p. 18) and that “neither individuals nor the systems they operated in could foresee the long string of events leading to” the evolution of agriculture. The summary statement of this paper is

“Cultural systems provide a wide array of variation generating mechanisms, including rational decision making, but none of these guide evolution *over the long term*. That is accomplished by forces of selection and drift” (p. 29, emphasis added). In a similar vein, Dunnell (1996a:xi) states: “We see ourselves as solving problems and therefore rebel at the notion that the generation of variation is random with respect to selective conditions. Yet there is absolutely no evidence that a Lamarckian engine is at work in our evolution, since we have no access to future selective conditions.” While adding the (questionable) label “Lamarckian,” the thrust of this statement is the same as that of Jones et al.: human innovation is adaptively random because it does not anticipate *future* selective conditions.⁸ Clearly, focusing on the “unguided” nature of cultural variation over “the long term” is a radical expansion of the original axiom of the genetic theory of evolution that mutation is adaptively random with respect to *current* selective conditions.

While we certainly agree that neither behavioral variants nor mutations can be determined or caused by future selection or anything else that has not yet occurred—cause must precede effect in any coherent causal account—this is not a logical criticism of adaptationist or intentionalist accounts of cultural innovation. The evolutionary archaeological argument follows superficially from the fact that the genetical theory of evolution by natural selection involves a two-step process: “1) the production of undirected variation and 2) the sorting of these traits by means of differential success of the variant forms over time” (Rindos 1989b:8). We have argued above that what is true of genetic variation is not necessarily true of cultural or behavioral variation, but in any case the argument we are exploring here is distinct in that it extends the axiom of undirected variation to refer to *future* selective pressures.

Why would this extension be made? Although this is never fully spelled out, our best inference from the evolutionary archaeological literature and conversations with several key proponents is that it serves to protect their belief that behavioral innovation is analogous to (undirected) mutation against attack on the grounds that humans obviously do engage in directed phenotypic (behavioral, technological, and cultural) innovation. Thus, the counterargument goes, even when such innovations are consciously directed towards solving current adaptive problems, since no one can foresee future environmental states or other changes in selective pressures *in the long run*, behavioral variation is effectively undirected.

Our interpretation is supported by Neff’s (1992:146) claim that “to direct evolution through innovation, humans would have to solve future problems and exploit future opportunities, and would have to anticipate the impact of particular solutions on conditions in the more

7. The literature on behavioral innovation and learning is of course vast; for useful reviews of the relation between innovation and adaptive outcomes, see Boyd and Richerson (1985), Marler and Terrace (1984), and Plotkin (1994).

8. We term the “Lamarckian” label questionable because Lamarckian evolution posits that variation is responsive to *present* selective forces, not future ones.

long-term future." This argument strikes us as faulty in that it confuses the claim (of evolutionary ecology, for example) that phenotypic innovation is often effectively directed at solving existing adaptive problems and opportunities with the much stronger claim that it is successful in anticipating changed adaptive conditions in the future. In doing so, this argument holds phenotypic adaptation to a higher standard than natural selection itself; to see this one can simply reword the statement by substituting "adaptation" for "innovation" and "natural selection" for "humans."⁹

Having argued that innovation would have to be omniscient in order to be a significant adaptive force, many evolutionary archaeologists conclude that most or all directional change in human history must be due to selection. A particularly strong version of this view is that "all change is the result of selection acting upon the undirected variant cultural forms existing at earlier points in time" (Rindos 1989a:28). This logic is exemplified in some evolutionary archaeological discussions of domestication, where the admission that people may engage in incipient domestication in order to increase their food supply or reduce risk is countered by arguing that in the long run this innovation will lead to resource specialization, population growth, and hence increased resource variability and nutritional risk. Such unintended long-term consequences are then used to blunt the adaptive relevance of decisions and behavioral innovations.

A striking example of this kind of argument occurs in Rindos's (1989a:33, emphasis added) discussion of the effect of maize production in the prehistoric American Southeast:

The most obvious way to deal with the interacting factors of increasing population, increasing potential yield, and increased variance in that yield would be an attempt to buffer the system by increased association and trade within and between regions. Then, if a crop is bad in one locality, maize could be imported from other localities during the crisis period. This is a type of activity that *requires no foresight*, merely a response to a specific condition of immediate reduced food availability. Furthermore, *over time such arrangements could grow and have consequences that were totally unforeseeable* at the moment that the exchange systems were initially established.

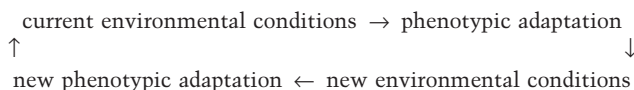
9. Neff (1992:146) in fact states the analogy to be that "DNA molecules know nothing of future evolutionary needs or opportunities. The same observation applies to humans who introduce innovations to solve perceived problems or exploit perceived opportunities." But the analogy is faulty on two grounds. First, DNA molecules can never "know" anything, and neither can natural selection, whereas humans *do* have the cognitive capability to model future states of the world and the impact of those states on their survival or other fitness correlates with some degree of accuracy. Second, as we are arguing here, the axiom of undirected variation in Darwinian theory applies to the *present* adaptive value of variation, not any future adaptive value.

Although we are skeptical that trade systems (particularly the ritualized forms that occur between sovereign polities like the Southeastern chiefdoms) "require no foresight," we have no objection to the rest of Rindos's statement. But we do not see how these factors weaken explanations based (in part) on adaptive decisions. While we grant that adaptive change often has many unforeseen consequences, we wonder why this is any more effective in emasculating the causal role of cultural innovation or behavioral adaptation than the same argument applied to natural selection. Surely "over time" the selective pressures favoring any trait (such as domestication) will change as ecological and demographic factors alter. In the same way, if behavioral responses to current adaptive problems ultimately alter the adaptive landscape (e.g., through population growth), new responses to the changed conditions can be expected to arise. Only if the rate of environmental change exceeds the capability for phenotypic adaptation or the changes are too subtle to be detected must we assume that innovation is nonadaptive.

A related critique of the evolutionary archaeological position concerns its failure to recognize that humans have highly developed and evolutionarily specialized cognitive mechanisms for projecting past experience into the future and formulating behaviors that "anticipate" future environmental contingencies (Tooby and DeVore 1987; Byrne and Whiten 1988). This does not mean that our explanations of such decision-making behaviors locate the *cause* of the behavior in its actual future results, nor does it mean that strategic or decision-making models place their explanatory emphasis on "intended effects." According to evolutionary ecology, the causes of a behavioral strategy are to be located in the interaction between an organism's evolved and learned cognitive and problem-solving capabilities and its current environmental conditions. Hence, cause precedes behavioral effect, and a deterministic, evolutionary theory of behavioral variation is possible.

According to evolutionary archaeology, because people cannot foresee *future* selective conditions or unintended consequences of their actions, natural selection ultimately determines cultural evolution, regardless of the short-term strategic adjustments people may consciously or unconsciously make to *present* conditions. Thus, since organisms cannot foresee the changed selective conditions of the future, their phenotypic responses are adaptively impotent and serve only as grist for selection's mill; all explanatory weight is carried by selection. Clearly this approach and evolutionary ecology have rather different views of the relation between ecological and evolutionary time scales. Since evolutionary ecology assumes that selection has designed organisms to be able to solve most adaptive problems, hypotheses guided by this assumption predict that people (and many other organisms) will be quite capable of responding to changed selective conditions with new adaptive strategies. Granted, a solution to one adaptive problem may lead to the emergence of new problems,

but this is just as true in the realm of natural selection as in that of behavioral problem solving. In the evolutionary ecological view, the process is a recursive one whose causal structure can be diagrammed thus:



There is no finality or teleology to this view, just as there is none if one substitutes “evolutionary adaptation via natural selection” for “phenotypic adaptation.”

Of course, all behavioral strategies played out in the present have some effect on future environmental states. For example, in the case of diet-breadth expansion discussed above, the successful foraging strategies of early Archaic hunters may have caused a reduction in the abundance of large terrestrial mammals, to which later Archaic foragers had to adjust. Such effects, commonly termed “unintended consequences” (i.e., effects other than those sought by the decision-making organism), may constitute “unselected consequences” as well—that is, effects that alter the selective pressures impinging on the behavioral strategies being analyzed. Explaining unintended or unselected consequences, it seems to us, is primarily a historical problem, not an evolutionary one. By this we mean that not all change with observable material consequences (such as might show up in the archaeological record) is *evolutionary* change—descent with modification caused by evolutionary mechanisms such as natural selection or drift.

We suspect that much of the reason some evolutionary archaeological theorists emphasize “future” selective conditions as an ultimate cause of change is their desire to make evolutionary theory a theory of unintended consequences—that is, a science of history (see Dunnell 1982). Rindos (1989a:38–39) provides one clear statement of this view:

Viewing variation as undirected brings about a change in the way in which we set about attempting to explain cultural evolution. Here, the spread of behaviour throughout a society, or . . . throughout the species, is the result of the fitness induced by that behaviour. . . . Rather than seeing change as a consequence of the adoption of a particular form of behaviour, emphasis is placed upon the historical *consequences* of a particular variant form of behaviour for the humans exhibiting that behaviour.

Our main objection to this is simple. Unintended and unselected consequences take the form of environmental effects as well as genetic and phenotypic ones. Some environmental effects are caused by human agency, while others, such as exogenous climate change, are not. With respect to phenotypic changes that result, the distinction is not important, since humans must adjust to these new conditions either way. What is important is that the environment itself is not “heritable” (a repli-

cator) in any Darwinian sense.¹⁰ Hence, environmental change, whether exogenous or anthropogenic, is not an evolutionary process. We argue that human history can certainly be explained, at least in part, *in terms of* evolutionary processes, but evolutionary change and historical change are not the same thing.

INTENTIONS AND CAUSES

A third way in which the “selectionism” of evolutionary archaeology and the “adaptationism” of evolutionary ecology may seem to diverge concerns the causal role of intentionality. Whereas evolutionary ecological theory and analysis often refer to “decisions” and “strategies” and “goals,” a key programmatic element of evolutionary archaeology is denial of the explanatory relevance of goals or intentions for evolutionary analysis. For example, O’Brien and Holland (1990:44) contend that

in one sense we can speak, rather trivially, of intent being a proximate cause of something, but of what analytical value is such a statement? Proximate causes, in any scientific framework, are functional causes, i.e. how things work. To invoke intent as an explanation robs valid functional questions of their interesting parts and replaces them with vitalistic, directional components.

Similarly, Dunnell (1989:37) writes, “If human intentions cause human history and diversity, then do we suppose that squirrel history and diversity, or oak tree history and diversity, or star history and diversity are the consequences of squirrel intentions, or oak tree intentions, or star intentions? Generally not. These phenomena are understood without recourse to vitalism.”

We expect most behavioral ecologists to be in general agreement with both of these statements. Let us be careful, however, not to throw the baby out with the bathwater. Behavioral ecologists do not view behavioral variants, “strategies,” or “decisions” as isomorphic with “intentions.” It is quite possible to talk about an oak species’s strategy for seed dispersal without assuming that trees have actual “intentions” in dispersing seeds. In general, evolutionary ecology employs such strategic language as a convenient shorthand, analyzing adaptive design such as seed dispersal *as if* a plant had dispersal intentions without assuming that it in fact does. In the case of organisms that do seem to have intentions (such as humans), these are viewed as proximate causes in the manner suggested by O’Brien and Holland. Ultimately, such intentions are explained as

10. In the case of coevolution between two or more populations (as in predator-prey systems) or social interactions with fitness consequences within a population (as in mating systems or intraspecific resource competition), the relevant aspect of the environment for any player does contain replicators and hence is subject to evolutionary change. For that reason, evolutionary ecologists have adopted special techniques such as evolutionarily-stable-strategy theory (e.g., Maynard Smith 1982) for analyzing such dynamics.

products of past (genetic or cultural) evolution; as Daly and Wilson aptly put it, "natural selection has no goals, but it is the reason why organisms do" (1991:219). But—and here is the nub of the dispute between the two approaches—to the extent that intentions contribute to phenotypic adaptation, they produce adaptive changes without concurrent selection.

Many evolutionary archaeological theorists seem to recognize only two possibilities: either intentions explain everything (the alleged conventional wisdom which they criticize) or they play only the minor supporting role of generating undirected variation. Thus, we are told that "for whatever reason, anthropologists are incapable of shrugging free of intention as the ultimate explanatory device (see Ramenofsky 1995). Intention, however, explains nothing but how variation might be generated" (O'Brien and Holland 1995:180). Having reduced intentionality to a black box that generates variation, O'Brien and Holland go on to undercut even this role: "This is not to say that intentions explain the generation of variation, only that, like a host of other agents, they can spawn variation."

What such pronouncements overlook is the critical difference between positing intentions as the *root* cause of some phenomenon and positing them (or their functional equivalent, such as the decision rules of evolutionary ecology or the cognitive algorithms of evolutionary psychology) as *intermediate links* within a causal pathway. Evolutionary archaeology rejects the first position, and because it fails even to recognize the second it considers the matter concluded. In contrast, while sharing its rejection of intentions as root causes, we hold that evolutionary explanations of human history and behavioral change generally need to include intentions or their equivalent in the causal pathway because these often provide the link between natural selection and behavioral regularities. That is, past genetic (and perhaps cultural) evolution has shaped the human psyche to be very effective at solving adaptive problems, and one important element of the psyche is what we commonly label "intentions" or "goals" or "preferences."

We have pointed to both commonalities and differences between evolutionary ecology and evolutionary archaeology. In contrast with evolutionary archaeology, evolutionary ecology posits that humans have remarkable capabilities to adapt their phenotypes to their environments through learning and rational calculation. The two approaches agree that we have the evolved capacity for the cultural transmission of the phenotypes so acquired to the next generation. Taken together, this adaptive dynamic resembles a Lamarckian process more than a strictly Darwinian one (Boyd and Richerson 1985, Gould 1979). Does this mean that we are suggesting that evolution is "directed" by human strategic responses to the environment, as Dunnell (quoted above) implies? Not at all. As we have argued above, phenotypic adaptation in response to environmental conditions does not cause change, it *is* change.

Begging the Meaningless Question: How Do We Explain Change?

What can the evolutionary archaeological paradigm gain by viewing behavior as strategic problem solving? We would answer: a great deal of explanatory power that it currently lacks. Rindos (1984) eloquently charts the evolutionary effects of plant domestication but when considering why humans adopted domesticates in the first place concludes that this is a "question without meaning" (p. 141). In the genetical theory of evolution by natural selection, it may well be meaningless to ask why, say, the sickle cell trait arose in the first place, because the mutation which produced it presumably occurred independently of its benefit to its heterozygous carrier (i.e., mutation is random with respect to adaptive value). Selection determines only the degree to which it would spread or persist in populations with a high incidence of malaria. It is, however, not meaningless to ask why, for example, a forager faced with a series of options might decide to invest more time or energy in the propagation of plant foods. The distinction turns once again on the issue of undirected variation. Given what we know about mutations, the prediction that hemoglobin mutations are more likely to occur in areas where malaria is endemic is sure to be incorrect, but the prediction that foraging populations characterized by sedentary settlement and resource intensification are more likely to innovate by adopting cultigens is plausible.

Other applications of evolutionary archaeology exhibit the same pattern of question-begging exemplified by Rindos. Variation is noted—people were planting a variety of crops, people were specializing or generalizing (Leonard 1989), people were living in larger and smaller settlements, some people were organizing themselves into collective labor forces (Leonard and Reid 1993)—but in each case the behavioral variation just happens to occur. The only opportunity for explanation that remains is to make up plausible post hoc stories about why a given variant was selected for or against. This procedure seems just as susceptible to the charges of circularity and storytelling that are leveled at many "adaptationist" explanations. Furthermore, the post hoc nature of the selectionist explanation (i.e., the evolutionary archaeological account of why the trait was selected for or against) virtually guarantees that it cannot be tested: it ultimately takes the form "Such-and-such was selected for because it was adaptive, and this is why."

The prevalence of selectionist "just-so" stories in the evolutionary archaeological literature has recently been recognized by at least some of its proponents, and we cannot improve upon the following self-critique (O'Brien and Holland 1995:188):

These "just-so" stories are neat little explanations tethered loosely to evolutionary principles by the unguarded use of the word "selection." Measurable

variation too often becomes *prima facie* evidence for a groundless cause-and-effect relation between an artifact and some nebulous concept of fitness and adaptation. The terms “selection,” “selective forces,” and “selective agents” become a ready means of understanding patterns that emerge from our analysis of data for which we have no other ready explanation.

In contrast, the evolutionary ecological research strategy is hypothetico-deductive, using explanatory models to develop testable predictions and then looking for evidence that bears on these predictions (Smith and Winterhalder 1992*b*). *Why* were people planting things rather than continuing to rely on wild foods? *Why* did they do it where they did, when they did? *Why* were they generalizing or specializing? *Why* did large settlements occur in some places and not others? *Why* did people organize themselves into large labor forces in some places and not others? The logical structure of evolutionary ecology seems to us to be uniquely suited to answering these kinds of questions. Once we accept that behavioral innovation is not adaptively random, optimization models can be used to produce hypotheses regarding which environmental factors are eliciting the variation. And since these hypotheses typically incorporate specific ideas about the currency, constraints, and relevant environmental variables, they can be tested empirically.¹¹ Structuring the problem in this manner allows one to suggest an answer to the “meaningless” question posed by Rindos.

Rindos (1980, 1984, 1989*a*) has been the primary evolutionary archaeological contributor to the literature on domestication. As he portrays it, domestication will occur whenever certain ecological circumstances apply. Specifically, when a foraging population modifies the environment so as to (unintentionally) enhance the dispersal, survival, or pollination of a plant population, this modification will increase the food supply of the foragers and hence favor their behavioral patterns via natural selection (of cultural variation). Domestication ensues when plant varieties that are genetically more susceptible to human propagation and harvest are favored through human-mediated natural selection. But, as Rindos notes, given the symbiotic and coevolutionary nature of this scenario, we could just as well say that plants domesticate humans as the opposite.

In outline, Rindos’s analysis consists of four tenets: (1) domestication is fundamentally a *coevolutionary* process that alters the traits of both domesticators and

domesticates; (2) the process is *unintentional* and results from natural selection acting on adaptively random variation in human behavior (culturally inherited) and domesticate morphology/physiology (genetically inherited); (3) the result is generally *symbiotic* (enhancing the fitness of both parties) though not necessarily stable; (4) domestication eventually leads to large increases in human population density. Of these, only 2 is either controversial or truly novel; it is also the element that places Rindos’s account squarely within the evolutionary archaeological framework. In any case, Rindos is at pains to differentiate his scenario from more conventional ones in which humans intentionally favor more productive plant varieties or engage in selective breeding. As he puts it (1989*a*:34), “cultural processes such as innovation or discovery are processes that *permit*, but do not directly cause, cultural change. In the case [of domestication], the true reason for cultural change may be detected only in the social and demographic consequences of agriculturally induced changes in environment and behaviour.” For purposes of this paper, then, it is critical to consider Rindos’s claim—so characteristic of evolutionary archaeology—that a “true” or valid explanation of domestication cannot assign a causal role to behavioral innovation or problem solving.

While a number of writers have discussed domestication from an evolutionary ecological perspective (e.g., Layton, Foley, and Williams 1991, Hawkes and O’Connell 1992, Kaplan and Hill 1992, Redding 1988), the fullest account is by Winterhalder and Goland (1993 and esp. 1997). As they put it, the “microecological” perspective of optimal foraging theory can be used to explain how low-ranked plant resources could have entered the diet of hunter-gatherers, initiating the coevolutionary relationships which created domesticates (Winterhalder and Goland 1997:32).

The Winterhalder-Goland analysis relies primarily on the prey-choice model discussed above. In this model, prey (including protodomesticates) are ranked by their postencounter profitability, which is independent of their abundance or encounter rate.¹² Four categories of protodomesticates are thus possible: profitable/abundant, profitable/scarce, unprofitable/abundant, and unprofitable/scarce. While Winterhalder and Goland discuss scenarios involving each of these four possible starting points, for brevity we will note only one, the case of a protodomesticate that is unprofitable (low-ranked) but very abundant. As Winterhalder and Goland note, the prey-choice model predicts that such a resource will be ignored as long as higher-ranked prey are

11. In optimization theory, “currency” refers to the variable that is maximized by the optimal solution. In evolutionary ecology, currencies, such as resource capture rate or predator detection rate, are usually proxies for fitness, chosen because they are more readily measurable than fitness itself. (For general discussions of optimality modeling from this standpoint, see Krebs and Davies 1991, Maynard Smith 1978, Parker and Maynard Smith 1990, and Stephens and Krebs 1986; for applications to human behavior, see Smith 1987, Smith and Winterhalder 1992*b*, and various authors in Smith and Winterhalder 1992*a*.)

12. “Profitability” here means expected net return (e.g., in calories) per unit handling time, where “handling” is defined as pursuit, capture, and processing (i.e., any actions required to consume a resource once it is encountered). Profitability is independent of encounter rate whenever items are handled singly or in any other way that does not create an economy of scale. For a detailed discussion of these points, see Stephens and Krebs (1986:chaps. 2 and 3) or Smith (1991:204–9).

sufficiently abundant, but if such prey decline in abundance (because of localized depletion, climate or habitat change, human population growth, or any other reason), the protodomesticated will be incorporated into the diet. Since in this case the protodomesticated is very abundant, the initial result will be a major infusion of food energy into the population and a broadening of the diet (as all higher-ranked resources will continue to be taken whenever encountered). The longer-term result will be massive ecological change, as human population growth fueled by the protodomesticated produces depletion of many wild resources; there will also be narrowing of the diet as the protodomesticated becomes ever more profitable and abundant by undergoing the coevolutionary modifications proposed by Rindos.

In their conclusions, Winterhalder and Goland discuss other approaches, including that of Rindos, and in fact show how an evolutionary ecological analysis can be articulated with the evolutionary archaeological emphasis on unintended long-term consequences. Foraging theory can be used to illuminate the role of various ecological circumstances in eliciting phenotypic responses (behavioral innovations) that enhance forager fitness and have unintended but profound consequences for human population ecology. In providing the middle-range theory needed to connect selective pressures with behavioral responses, "foraging theory can supply hypotheses on questions the Rindos model neglects: What circumstances led humans to select certain species for exploitation? What are the economic and population processes that accompany growing dependence on domesticated and cultivation?" (Winterhalder and Goland 1997:127). This case suggests that the two approaches can be complementary rather than competitive, but this complementarity requires an acknowledgment that decisions and actions are fundamental to human (phenotypic) adaptation, not simply generators of adaptively random variation on which natural selection will then act.¹³

What We See and What We Know: Can Archaeologists Study Behavior?

In addition to the theoretical issues discussed above, there is a more immediate and empirically based issue that leads evolutionary archaeologists to reject a focus on the analysis of behavior in archaeology. This is the view that archaeology should focus on changing artifact frequencies through time because these are the only phenomena that are empirically observable in the ar-

13. Signs of a move towards rapprochement with (or co-optation of?) evolutionary ecology can be seen in recent suggestions that optimization analysis can play a valid role in evolutionary analysis. For example, Jones, Leonard, and Abbott (1995:27) refer to engineering criteria and adaptive optimization as "powerful tools in understanding the selective processes acting through ecologic relations that govern technologic change" (see also Graves and Ladefoged 1995; Maxwell 1995; O'Brien and Holland 1995:190). Tellingly, though, only Graves and Ladefoged cite any of the abundant evolutionary ecological literature on optimization analysis.

chaeological record. Since behavior is not empirically observable in the archaeological record, the argument goes, we cannot study it there (e.g., Dunnell 1980:88; 1989:43). Taken at face value, this sounds like a form of radical empiricism, the tenet that science can only consider directly observable phenomena.¹⁴ Radical empiricism was once prevalent in a variety of sciences but has been widely abandoned. Indeed, such a restriction would eliminate much of nuclear physics, return psychology to 1950s-era behaviorism, cripple historical geology, and strip evolutionary paleontology to its (fossilized) bones.¹⁵

It is characteristic of such radical empiricism that it is more readily advanced as a doctrine than adhered to in practice; archaeology is no exception. Perusal of the evolutionary archaeological literature reveals that virtually the only cases in which changing artifact frequencies per se actually provide the empirical focus are studies of stylistic variation (Neiman 1995)—which evolutionary archaeologists have typically seen as resulting from drift, not selection. In any case, evolutionary archaeologists are clearly interested in larger issues as well. These include the origins of domestication, population aggregation and dispersal, collective labor, and the origins of complex societies.

All of these topics involve whole suites of variable behaviors with which artifacts are only indirectly associated. We do not empirically observe domestication being carried out in the archaeological record; rather, we observe artifacts and ecofacts that we *infer* to be associated with domestication behavior. We do not observe people aggregating or dispersing in the archaeological record, nor do we observe small independent social groups coalescing into larger sociopolitical units. The archaeological record does not reveal humans exchanging goods or engaging in mobility or sedentism. What we see are archaeological correlates—material entailments—of these various behaviors. Dunnell's call for archaeologists to abandon behavioral reconstruction (1989:45) has become a clarion call in evolutionary archaeology; yet as soon as we utter words like "settlement pattern" we have already engaged in behavioral reconstruction. Hence, Dunnell's advice strikes us as almost impossible to follow in practice. In our view, the question of whether or not we can empirically study or

14. This radical empiricism seems to have played a role in narrowing the way some selectionists view evolution, as seen in their definition of evolution as consisting of *the differential persistence of variation* (e.g., Dunnell 1980:38; Jones, Leonard, and Abbott 1995:14; Ramenofsky 1995:135; Teltser 1995a:4, 5). While such differential persistence is certainly fundamental to evolution, the explanatory power of Darwinian theory comes from its success in accounting for the adaptive design of phenotypes and the creation of new designs. After all, Darwin made adaptive design and divergence his central object of explanation and entitled his magnum opus *The Origin of Species* (not *The Differential Persistence of Variants*).

15. For a recent critical review of philosophical issues concerning "observables" in scientific explanation, explicating how and why unobservable phenomena are necessary and proper elements of scientific investigation, see Kitcher (1993).

infer behavior in the archaeological record is entirely one of degree and hence of considerably less metaphysical importance than some evolutionary archaeological theorists (e.g., Dunnell 1989:43) would have us believe.

There is another strand to the rejection of behavioral reconstruction that turns on matters of explanatory logic rather than empirical sufficiency. This concerns the idea that behavior has no lawfulness other than that externally imposed by selection. While we share evolutionary archaeology's skepticism concerning the inductive epistemological strategy of "behavioral archaeology" (*sensu* Schiffer 1976), we share the latter's view that behavioral reconstruction of *some sort* is essential to an understanding of large parts of the archaeological record. In particular, we reject the view that the absence of behavior per se from the archaeological record makes behavior an inappropriate object of archaeological explanation. We also reject the non sequitur that since behavior varies we cannot use it to explain the past, as in the following passage: "There is no deterministic relation between the behavioral terms of reconstruction and the debris of the archaeological record. Such a relation would have to be founded in laws, and behavioral laws, as just noted, cannot exist because behavior changes. Without a deterministic relation between the two, behavioral explanations are untestable in the archaeological record" (Dunnell 1992:216). That behavior "changes" does not make behavioral explanations "untestable" any more than changes in selective forces make selectionist explanations untestable. Indeed, from the evolutionary ecological perspective (as well as most of behavioral biology and social science), the regularities ("deterministic relation") between behavior and the archaeological record in fact *derive* from behavioral change that responds to variation in social and natural environments.

Furthermore, one does not have to adopt a "law and order" (Flannery 1973) view of behavior in order to posit regularities between behavior and the archaeological record. In fact, the evolutionary archaeological framework only makes sense if there are regularities between environmental factors and archaeological change, regularities which work via selection, drift, and other evolutionary mechanisms. Evolutionary ecology adds the assumption that regularities also are instantiated via phenotypic adaptation, including behavioral and technological responses. It also posits that many behavioral regularities (decision rules) are predictable because these have been designed by past natural selection.

As we argued above with respect to "intentions," one does not have to view behavioral factors as root causes of historical change to consider them important parts of historical (including archaeological) explanation. While we recognize that "because the archaeological record does not provide any direct observational access to human behavior, the methods used in an evolutionary archaeology will look very different than, for example, an evolutionary ethnography" (Teltser 1995a:3), we insist that evolutionary explanations of the archaeological record must implicitly or explicitly trace causality

through behavior. For one thing, geological or climatic factors shaping taphonomic processes cannot themselves evolve in any Darwinian sense. Teltser's suggestion that evolutionary archaeology must develop new theory to deal with behavior because evolutionary biology "does not include the necessary terms to address behavioral phenomena" (1995a:3) would be true only if one ignored the explosion of theory and data developed in evolutionary behavioral ecology over the past three decades (e.g., Alcock 1993, Krebs and Davies 1997). Unfortunately, evolutionary archaeology seems generally to have done just that. As a result, it is just as vulnerable to the charge of parochialism and misinterpretation of Darwinism as it has shown midcentury cultural evolutionism to be (Dunnell 1980, Leonard and Jones 1987).

Ironically, the proposal to operationalize evolutionary analysis strictly in terms of archaeologically observed phenomena brings its own intractable problems to the evolutionary archaeological program. This is particularly clear in the way selectionists have attempted to operationalize the concept of selection in the analysis of archaeological change. Since selection is inarguably a dynamic process while the archaeological record is essentially static (Binford 1983:19–20), we cannot actually observe selection occurring in the record any more than we can observe behaviors. In a very influential article on patterns of artifact frequency change, Dunnell (1978) attempted to solve this problem by arguing that any sustained directional change in the frequency of an artifact type is a sign of selection at work.¹⁶ But what might be true for genetic evolution and on palaeontological time scales seems to us far more problematic in an archaeological or historical context. The well-documented secular trends of increased stature and earlier age at menarche (Eveleth and Tanner 1990, Wood 1994), for instance, while quite directional and sustained (having continued for centuries in some populations) and of considerable magnitude (e.g., up to 30% reduction in menarcheal age in some populations), clearly result not from selection or any other form of evolutionary change but from phenotypic programs that respond to varying nutritional input with varying growth and maturation rates. A more archaeological example of the same process is the case of change in the character of the osteological remains in some stratified sequence, the early occupants of a site or region having very robust skeletal structure while later occupants are more gracile. Barring migration, this could be due to evolution in robusticity (e.g., due to declining selection pressure for channeling energy to bone growth or even to drift in a small population) or to phenotypic adjustments (maturing bones subject to less stress develop less robustly). The former change is evolutionary, while the latter is not. Or skeletal remains might show a diachronic increase in signs of nutritional stress (e.g., Har-

16. Dunnell's argument has become a fundamental axiom in the evolutionary archaeological literature; see, for example, Jones, Leonard, and Abbott (1995), Neiman (1995), O'Brien and Holland (1990, 1992, 1995), among others.

ris lines); this again is phenotypic rather than evolutionary change. By positing natural selection as the only source of sustained directional change, evolutionary archaeological theorists have become prisoners of their limited explanatory framework.

Conclusion

Virtually all animals and plants have evolved at least some capacity to adjust phenotypically to varying environmental conditions. Therefore many environmental changes will produce a change in the observed mean phenotype of a population *without any selection or evolutionary change*. Put another way, variable phenotypic adjustment by individuals in a population to changing environmental conditions does not just constitute "variation" as raw material for selection; it constitutes *a form of nonrandom, directional adaptive change in and of itself*. In humans, the capacity for problem solving and for adapting phenotypically to a wide range of environmental conditions is highly developed (and a product of our evolutionary history). The material entailments of these processes produce an observable archaeological record of adaptive change. We have argued that explanation of this kind of adaptive change requires a specific kind of evolutionary logic. A central goal of this paper has been to outline this logic, show how it differs from the evolutionary archaeological use of the concept of selection, and suggest that adopting it can produce more powerful explanations of adaptive change in the archaeological record.

Evolutionary archaeologists do not generally deny that behavior may involve problem solving or that behavioral variation may occur as much by design as by chance (e.g., Rindos 1989b:13–15; O'Brien and Holland 1990:44–45). They do, however, consistently deny that the problem-solving nature of behavior has any role to play in the *explanation* of phenotypic change over time. Again, this seemingly contradictory view stems from their insistence on adopting a strict genetic analogy, with the two-step (unguided variation, selection) process of evolutionary change it entails. In contrast, evolutionary ecologists argue that behavioral variation (including innovation) is at least partially guided by perceived costs and benefits linked to environmental variation.

We expect that all parties can agree that variation in artifact frequencies and spatial patterning through time and across space must ultimately be produced by behavioral variation (in conjunction with taphonomic and other nonbehavioral factors). Yet directional and adaptive behavioral change need not be directly the result of evolutionary processes such as natural selection or drift. Instead, it may be due in large part to facultative phenotypic response to varying environmental conditions. However, even in this case natural selection ultimately helps to explain behavioral variation, since the capacity to respond behaviorally and, indeed, very often adaptively is an evolved capacity. This is the fundamental tenet of evolutionary ecology.

Natural selection (and other evolutionary forces) may shape behavioral variation in another way, through a distinct process of cultural evolution. The degree of independence of cultural evolution from genetic evolution is as yet unresolved (e.g., Boyd and Richerson 1985, Durham 1991, Flinn 1997, Rindos 1985). Whether or not one accepts a distinct and autonomous role for cultural evolution, there is no need to banish behavior and decision making from the explanatory framework of evolutionary archaeology.

While the theory of natural selection provides a powerful explanatory mechanism, it becomes even more powerful when linked with other concepts, including some from the social sciences. Indeed, the rich developments in contemporary evolutionary theory (e.g., Maynard Smith 1982, Krebs and Davies 1997) would not have been possible without extensive borrowing from decision theory and economics. Those wishing to analyze human society, behavior, and technological change using evolutionary theory would be far better off emulating this judicious borrowing and adaptation of social science concepts (cf. various authors in Smith and Winterhalder 1992a) than heeding evolutionary archaeology's dismissal of any elements of social science and decision theory as metaphysically tainted.

For archaeology, one of the major implications of the distinction between evolutionary ecology and the evolutionary archaeology program critiqued here concerns the relation between archaeological evidence—temporally and spatially varying occurrences of artifacts, ecofacts, features, and sites—and processes of evolutionary change. The evolutionary archaeologists see temporal variation in the frequencies of these archaeological entities as evolutionary change per se. This view is exemplified in Dunnell's (1978) argument that directional changes in artifact frequencies signal selection at work while stochastic changes reflect the evolutionary process of drift. It appears that one of the main attractions of this line of reasoning is the extremely (and in our view overly) simple and direct link that is implied between archaeological data and the powerful, well-established (in biology) theory of evolution by natural selection.

In contrast, we hold that while the application of evolutionary theory to archaeology can lead to important gains in explanatory power, it offers no quick fixes. As a subset of modern evolutionary theory, evolutionary ecology is a rich source of ideas, but it is a theory about behavior, not about the archaeological record per se. Hence, we foresee a continuing, active program of research and analysis dealing with the relationship between past behavior and the formation of the archaeological record in the tradition of Binford's (1992) "middle-range theory," Schiffer's (1976, 1987) "behavioral archaeology," and ethnoarchaeology as defined by O'Connell (1995). What has been conspicuously lacking from this research is a powerful and coherent theory of behavior that can underlie and unify middle-range theory. We agree with O'Connell (1995) that evolutionary ecology could remedy this need.

Evolutionary archaeology's claim that archaeologists

can explain the archaeological record by applying natural selection theory directly to observable features of this record is based on several misconstruals of evolutionary biology and cultural evolution. As discussed above, attempts to apply evolutionary explanations directly to artifact frequencies or other observable aspects of the archaeological record typically ignore the phenotype-replicator relation and the distinct mechanisms and complexities pertaining to cultural transmission. The evolutionary archaeological research program appears to be driven more by an empiricist metaphysic—now widely abandoned as unworkable and unnecessary in other sciences—than by the logical entailments of Darwinian evolution. Virtually ignored by the proponents of this approach, the evolutionary ecological framework is increasingly being employed to explain phenotypic variation in both ethnographic and archaeological contexts. We feel confident in concluding that not only is it possible to study behavior and ecological adaptation in the archaeological record but indeed we must do so if we expect to make evolutionary sense of it.

Comments

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This essay brings into sharp relief a ubiquitous confusion that has dogged discussions of cultural evolution, one deriving, I suspect, from a subtle misreading of Darwin's original use of artificial selection (deliberate animal breeding) and "unconscious" selection (the unwitting promotion of favored offspring of domesticated animals) as bridges to his concept of natural selection. While it is true that Darwin wished to contrast the utter lack of foresight or intention in natural selection with the deliberate goal seeking of the artificial selectors in order to show how the natural process could in principle proceed without any mentality at all, he did not thereby establish (as many seem to have supposed) that deliberate, goal-directed, intentional selection is not a subvariety of natural selection. The short legs of dachshunds and the huge udders of Holsteins are just as much products of natural selection as the wings of the eagle; they simply evolved in an environment that included a particularly well-focused selective pressure consisting of human agents. These phenotypes fall under the same laws of transmission genetics, the same replicator dynamics, as any others—as special and extreme cases in which the default "randomness" or noisiness of selective pressure has been greatly reduced.

Applied to cultural evolution, the implication is this: There is no conflict between the claim that artifacts (including abstract artifacts—memes) are the products of natural selection and the claim that they are (often) the (foreseen) products of intentional human activity. I

have no direct acquaintance with the works of evolutionary archaeology discussed by the authors, but assuming that their account is fair it seems that the evolutionary archaeological theorists think that the only way to be hardheaded and scientific about the Darwinian evolution of culture is to deny all intention, all rationality, on the part of human culture makers. They opt for "selection rather than decision making." That is simply a mistake, for the same reason it would be a mistake to say that the fancy plumage of prize pigeons is the result of decision making rather than selection. But Boone and Smith seem to fall into the same trap. For instance, they are surely right that the adoption of snowmobiles by the Cree cannot be accounted for in terms of the differential biological replication of the snowmobile users, but they misread the more interesting meme's-eye view (Dawkins, 1976, Dennett 1995). They say: "The alternative that 'snowmobile memes' were transmitted more effectively than 'snowshoe memes' to nondescendant Cree (as well as offspring), while plausible, *is not natural selection* [emphasis added]; more significant, it requires precisely the kind of adaptive decision making that evolutionary archaeology is dedicated to eliminating from archaeological explanation." On the contrary, from a meme's-eye perspective in which the snowmobile meme is seen as the replicator with *its own* fitness, just like the fitness of the domesticated horses that spread so quickly among the Native Americans after their introduction, then cultural evolution can be seen to be due to "adaptive decision making" while also being a variety of natural selection.

Some memes are like domesticated animals; they are prized for their benefits, and their replication is closely fostered and relatively well understood by their human owners. Some memes are more like rats; they thrive in the human environment in spite of being positively selected against—ineffectually—by their unwilling hosts. And some are more like bacteria or viruses, commanding aspects of human behavior (provoking sneezing, for instance) in their "efforts" to propagate from host to host. There is artificial selection of "good" memes—such as the memes of arithmetic and writing, which are carefully taught to each new generation. And there is unconscious selection of memes of all sorts—such as the subtle mutations in pronunciation that spread through linguistic groups, presumably with some efficiency advantage but perhaps just hitchhiking on some quirk of human preference. And there is unconscious selection of memes that are positively a menace but prey on flaws in the human decision-making apparatus, as provided for in the genome and enhanced and adjusted by other cultural innovations—such as the abducted-by-aliens meme, which makes perfect sense when its own fitness as a cultural replicator is considered.

The antagonism between the evolutionary archaeological and evolutionary ecological camps is perhaps then due to an overshooting by both sides: the former sees the prospect of an evolutionary account of artifacts and ideas that treats human beings as "mere" vectors,

and the latter sees the prospect of providing evolutionary accounts of the adaptive strategies made possible by the plasticity of the human phenotypes, and neither side sees how the two perspectives can be put together—but they can be. The genetic evolution of basic behavioral capacities and dispositions and preferences creates highly versatile human phenotypes whose norms of reaction are immense and *largely* fitness-enhancing. But these developments bring into existence a medium of cultural transmission that engenders a new genre of replicators, and while some of these are favored by and in turn enhance the adaptive strategies already laid down, others exploit them “for their own benefit.” Evolutionary archaeology should pursue all these phenomena together. Both sides win; both sides have a contribution to make.

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Boone and Smith provide a focused and convincing critique of Dunnell’s school of evolutionary archaeology and as an alternative offer evolutionary ecology, a set of behavioral explanations derived from optimal foraging theory. I am sympathetic to their theoretical positions. While evolutionary archaeology is to be applauded for its attention to the archaeological record per se, the school is naive to assume that one can understand archaeology without understanding the human behavior, society, and culture that produced it. Boone and Smith instead focus on human behavior and the evolutionary roots of human cognitive abilities. They sit on the interfaces between evolutionary biology, cognitive science, and economics. These interfaces offer extraordinary opportunities for an integrated theory of human behavior, but as with all reductive theories, the limitations are profound. What is missing is an understanding of the emergent properties of larger systems and social institutions.

In their review of evolutionary theories in archaeology and anthropology, Boone and Smith, somewhat surprisingly, ignore processual archaeology. Processual archaeology has a long history that derives from Steward’s (1955; Harris 1977, Johnson and Earle 1987) theory of multilineal cultural evolution (for example, Binford 1972, Blanton et al. 1996, Earle and Preucel 1987, Flannery 1972, Wright 1984). These theorists focus on the organization of human society and on models that explain the development of social institutions. Evolutionary or behavioral ecology, in contrast, focuses attention on individual activities that are not extensively institutionalized. Much of this work has dealt with foraging behavior and movement (see, for example, Kelly 1995), but a profound difference observed in human life experiences cross-culturally is in the scale of institutional elaboration: family-level, local groups, chiefdoms, states, and world systems (Johnson and Earle 1987). To

be sure, the individual agent, sculpted by both culture and evolutionary selection, is important in all institutional contexts, but the properties of the institutions—for example, the male supremacy complex in local groups (Harris 1977) and the competitive political economy in chiefdoms (Earle 1997)—dominate (or channel) individual behavior. The revolution of macroeconomics, as a further example, recognized that the operation of economic systems cannot be reduced to individual rationality; labor, capital, and monetary flows have properties that must be modeled and understood.

Neither evolutionary archaeology nor evolutionary ecology considers how institutions are built up, operate, and determine human behavior, on the one hand, and the artifactual record, on the other. Although group selection has been shown to be theoretically inadequate and functionalist assumptions are in disfavor, institutional dynamics are evidently critical to the operation of human society and must be understood to explain long-term evolutionary processes. Anthropology’s concern with institutions is of long standing, because the physical reality of clans, lineages, chiefdoms, churches, businesses, bureaucracies, and the like, determines much of what happens in human lives.

Selection is central to the evolutionary theories considered in this article. But what determines the genetic fitness of a human? Certainly much has to do with personal decisions about subsistence, mating, display, competition, and the rest. Behavioral ecology offers intriguing explanations of these. In complex society, however, a person’s reproductive success and cultural impact are more likely an outcome of institutional affiliation and context. Whether one is a king or a pauper, a member of a large clan or of a small one, an urban craftsman or a rural farmer, Muslim or Hindu fundamentally determines the nature of one’s life and activities in particular societies. Choices are heavily constrained in ways that are very basic to complex human societies. These are problems of how individuals associate and how they identify and control each other’s activities. The evolution of human society involves the formation of institutions and the maintenance of power relationships within and between them (see, for example, DeMarrais, Castillo, and Earle 1996, Earle 1997).

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This paper raises several significant issues with regard to the ongoing challenge of building an evolutionary archaeology. In the effort to create a polemic, however,

it does little more than caricature a continuing, indeed, nascent, discussion—conflating or simplifying many critical points and driving a wedge between complementary aspects of ecology and evolution in Darwinian theory. As a consequence, some will seize upon this paper as a warrant for abandoning the effort to bring historical science to anthropology. We think that this would be unfortunate.

Boone and Smith argue that the problem with evolutionary archaeology lies in its “flawed grasp of evolutionary biology, not in its advocacy of Darwinism per se.” They offer instead what they believe is “a conceptually more complex but realistic view of phenotypic variation.” Central to their argument is the familiar, decidedly non-Darwinian claim that “organisms (including humans) have been designed by selection to make extensive adaptive adjustments to their phenotypes. . . . Under this view, natural selection’s primary role lies in accounting for these cognitive mechanisms.”

This claim is empirical, not theoretical. The argument is that many organisms including humans have developed behavioral systems so efficient that they act optimally and adaptively with no effective role for natural selection in their operation and persistence. Such a position is at home in anthropology, where teleological causation has dominated the non-Darwinian paradigm of cultural evolution (e.g., Harris 1979; see Dunnell 1980, Richerson 1977, Smith 1983). Despite its significance, Boone and Smith do not attempt to substantiate their empirical claim of human adaptiveness independent of selection but merely assume its truth as axiomatic.

But such a claim is not widely shared by evolutionary ecologists. Stephens and Krebs (1986), for example, point out that foraging theories are formal models of optimal behaviors given specific assumptions, specific environments, and biological constraints. Among the reasons for testing formal models they list “to ask how good organisms are at doing their jobs,” “to ask what animals are designed to do,” and “to analyze behavioral mechanisms” (p. 183). Theoretical models in evolutionary ecology are measurement tools; they are heuristic—a point made often and by many (e.g., Bettinger 1986; Krebs and Davies 1987; Maynard Smith 1978; Smith 1987:205). In short, the use of notions of optimality, decision making, adaptiveness, etc., derived from field biology or microeconomics does not eliminate the causal role of natural selection or other evolutionary processes in explaining genetic or cultural change.

Boone and Smith have confused empirical observations (and heuristic assumptions) about the behavior of humans with the theory used to explain that behavior. They assume things that we might seek to understand in scientific terms and thus confuse description with explanation. Such confusion is, unfortunately, common to the mainstream empiricism of the social sciences (Willer and Willer 1973). At another scale, they conflate the explanatory structure of ecology and evolution, imagining that differential persistence in the long term can simply be conceived in spacelike (functional) terms

in contrast to the timelike (historical) frames of evolution (Dunnell 1982; see also Elster 1983). As a consequence, they overlook the critical explanatory significance of variation. Their central claim therefore represents neither the position of evolutionary ecologists nor the “Darwinian” approach in general.

Boone and Smith rightly point out that archaeologists need to begin taking formal models of cultural transmission seriously (e.g., Boyd and Richerson 1985) as part of a general mathematization of our reasoning, data description, and testing. Formal models will be necessary to test hypotheses of culturally transmitted replicators and the phenotypes they create, a difficult task whether one is referring to genetic or cultural information-to-phenotype linkages. Nevertheless, most advocates of an evolutionary archaeology understand the need for a clearly articulated theory of cultural transmission if evolution in phenotypes is to be studied rigorously. The mechanics and quantitative properties of transmission cannot be studied in the fossil record; instead we must combine formal modeling and experimental study with living animals to work out the details. What falls to archaeology today is the task of deriving expectations from theories of transmission and selection that can be rewritten in a form measurable in a fossil record of behavior and morphology. Whether the models involved are of selection or neutrality, evolutionary archaeologists have begun the difficult task of bringing individual-based models into concordance with the time-averaged pooled archaeological record of patterned behavior and multiple phenotypes (e.g., Lipo et al. n.d.). However, unlike Boone and Smith, we believe that the strictures of science require the subject matter to be the record itself, not the untestable behavioral reconstructions imagined from ethnographic analogies. In this regard, we oppose the uncritical use of behavioral reconstruction (e.g., following Schiffer [e.g., 1976, 1996] and his students) as an empirically sufficient strategy for archaeological research.

In our view, and we suspect the view of the majority of evolutionary ecologists and evolutionary archaeologists, it is the anthropological baggage of cultural evolution and the reification of theory from tool to empirical reality that reveal Boone and Smith’s central claim to be misguided.

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Boone and Smith make a persuasive argument that phenotypic adaptation, not natural selection or drift, has been the primary force guiding human cultural evolution, perhaps since the rise of anatomically modern humans. Their case builds from several observations about humans, among which are unprecedented behavioral plasticity, the capacity for facultative response to opportunity and constraint, and the rapidity with which these evaluations may be transmitted by cultural

means. They deny that natural selection is directly involved in most instances of adaptively significant cultural change because the pace of cultural change is too rapid to be monitored by biological reproduction. In their view, the technologies and organizational strategies of humans evolve, to be sure, but this is not because external conditions favor some variants over others through differential fitness but because of a more direct process, adaptation.

This view asks selectionists to reconsider the mechanism behind change, abandoning a view that technology and behavior are directly under the influence of selective pressures for a view more akin to artificial selection. The external environment to which humans adapt culturally represents opportunities and constraints in the literal, not the metaphorical sense, since humans can identify them and ruminate on the best course of action to enhance success. In the chain of causal statements explaining change, then, the environment is more distant and decision making more proximate to the events under scrutiny. If natural selection plays any role, it is in engineering decision rules or cognitive algorithms, which Boone and Smith claim guide adaptive responses. These decision rules, which are readily transmitted culturally, are products of selection and perhaps evolved deep in human history. We do not learn from this discussion, however, whether these rules show phenotypic variability or are mutable—in short, whether natural selection might still be acting on them. In fact, we do not learn of the empirical status of these rules at all, that is, whether they are traits that natural selection really can work on. For a science professing a materialist outlook, this reliance on what in other theoretical guises amounts to norms or elementary structures seems inappropriate; one wonders if competition between individuals or among corporate groups or serious failures to implement rules correctly have evolutionary consequences.

According to Boone and Smith, phenotypic adaptability is the most parsimonious explanation of rapid adoption or abandonment of behavioral or technological variants. How else, after all, can we explain the rate and scale of adoption of much modern material culture of apparently little selective value (unless one subscribes to the fitness value of waste)? In this view, we no longer maintain the key distinction between mechanisms of transmission and of proximate cause, on the one hand, and those relating to the persistence of variants across many generations, on the other. Drift and selection disappear; the steady application of decision rules, generation by generation, will yield the same directional or static patterns of behavioral variant representation. But how can it be shown that this is a result of adaptation rather than selection when even the effects of single biological traits on reproductive fitness are so difficult to measure?

Given the difficulty of showing that phenotypic adaptability rather than natural selection is the operative mechanism in most cultural change, do the examples support Boone and Smith's claims? Certainly the

adoption of the snowmobile by Cree hunters was rapid—within a generation. This does seem to support a mechanism other than natural selection for the success of this technology. But can we claim from the example that there have been or will be no consequences for biological fitness or, to move into a less certain realm, for cultural reproduction, let us say in the fitness of decision-rule variants? To evaluate these questions, along with a longer time series we would need a less inclusive scale, for instance, comparing corporate groups with differential access to snowmobile technology or groups whose foraging territories differ in resource quality.

In the end, I am intrigued by Boone and Smith's notions of decision rules and have further appreciation for the cascading effects of cultural transmission at that scale of analysis. They have striking consequences for what might be phrased as cultural founder effects. These mechanisms are not, however, as clearly responsible as natural selection and drift for longer-term patterns that archaeologists regularly consider.

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Boone and Smith have provided a valuable comparison and needed critique of the two dominant uses of Darwinian theory in archaeology. They label these approaches evolutionary archaeology and evolutionary ecology. As an example of how they differ, Boone and Smith examine how Winterhalder's (1981) work on the adoption of snowmobiles by the Cree in the mid-1970s is explained by each. They suggest that Ramenofsky's interpretation that natural selection is directly at work in the adoption of snowmobiles fails to consider the decision-making plasticity of Cree foragers. This plasticity is continually stressed throughout their discussion.

I suggest, however, that the entire Cree example is flawed, regardless of which interpretation one accepts. Neither accounts for all the costs that would enter into the decision making of a typical Cree forager ca. 1975. Although Winterhalder (1981:71) discounted the cost of the purchase of the snowmobile and the fuel to operate it, the costs are not insignificant; hard currency is limited but necessary to obtain the snowmobile and fuel in the community of Muskrat Dam Lake, where Winterhalder conducted his research.

If we view a snowmobile as a material good that passes through many phases during its useful life, we can divide a snowmobile's life-cycle cost into at least three phases: (1) initial purchase cost, (2) maintenance, and (3) eventual replacement. Although adopting snowmobiles increased foraging efficiency among the Cree, thus permitting higher return rates, Winterhalder and, subsequently, Boone and Smith have assumed that all Cree hunters could embrace this method for higher for-

aging efficiency. However, for some Cree hunters living in Muskrat Dam Lake in 1975, the limited hard currency available to them coupled with an assessment of the overall life-cycle costs of a snowmobile may have led them to forgo a snowmobile and continue to use snowshoes in hunting instead. Thus, the decision to use "new" technology may have been economic and not based on a way to increase foraging efficiency. The worst-case scenario would be if a "successful" traditional Cree hunter could not afford a snowmobile; he would be forced to forgo a method of hunting that would further increase his foraging efficiency.

In not considering or discussing how hard currency affects decision making, Boone and Smith miss the opportunity to demonstrate how an evolutionary ecological approach can incorporate the impact of Western economic systems on our understanding of traditional subsistence economies through such models as optimal foraging and diet breadth. In fact, Smith has considered sub-Arctic forager participation in a mixed economy in his study among the Inujjamiut (Smith 1991:357–97). Using this study instead of the Cree case would have strengthened Boone and Smith's argument that evolutionary ecology can be used in both archaeological and ethnographic settings. The Inujjamiut case study would also have highlighted the weakness of evolutionary archaeological explanations in general and Rame-nofsky's in particular by showing that they are too quick to embrace the direct action of natural selection as an explanation of human behavior. Additionally, Ramenofsky's explanation ignores the significant influences and methodological difficulties that forager participation in a mixed economy has for explanations incorporating natural selection. Smith (1991:368–70) at least attempts to identify and then confront these difficulties.

I presume that if we were to introduce mobile global-positioning-system equipment and mobile satellite telephones into Cree society of 1997, even if all available hunters had equal access to snowmobiles, we could again cite economic reasons and not evolutionary archaeological or evolutionary ecological explanations for further increasing the efficiency of these foragers. We should consider how "new" technology confers value on very accurate locational information about the distribution and density of mobile and sessile resources. We could then ask under what circumstances this locational information would be defended (see Dyson-Hudson and Smith 1978).

Elsewhere, Boone and Smith discuss how evolutionary ecology can incorporate intention into its explanations. I find it ironic that Richard Dawkins, often cited by evolutionary archaeologists, discusses intention when he compares (1996:16) the differences between evolutionary processes and designoids. In the case of the potter wasp and the mason bee, "the wasp and the bee didn't consciously or deliberately design their pots. . . . [They have] no concept of a pot as a work of art, or as a container" (Dawkins 1996:16). The wasp and the bee are "working" to provide protection for the next genera-

tion through the construction of containers that look to us like pots.

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I strongly agree with Boone and Smith's critique of evolutionary archaeology. Although human populations are at some level affected by the same selective processes as affect all organic life, humans are different because they have a "response" time that is less than a generation, acquired traits are "passed down" from parents, peers, and/or others, and innovation is not simply a function of random mutation and thus "undirected." But selection (beyond selection that occurred in the distant past to create the plastic behavioral capacity of humans) may have occurred and still be occurring. My problem with evolutionary archaeology is that it tends to mask, obscure or gloss over past human behavior to the point where the paradigm is no longer useful as a *learning strategy*. I agree that evolutionary ecology provides a better learning strategy (and has far more empirical cases to its credit); I don't see evolutionary archaeology providing a method for determining the truthfulness of its assertions. The high ratio of theoretical to empirical treatises is telling.

It is still unclear to me whether evolutionary archaeology attributes directional change to the replicative success of the trait or to the direct increased reproductive fitness of its bearers. Both seem to be potentially at work. If a man comes up with a new projectile point or hunting tactic that cuts the cost of harvesting game and increases his return rate, it is reasonable to assume that his neighbor will copy that behavior *unless* something limits access to the technology or start-up costs (e.g., knowledge, manufacturing skills) are too high or the proposed change would reduce the user's ability to meet another cultural goal that the neighbor perceives as more important (and presumably has or had adaptive benefits). Perhaps, then, selection in the strict sense (of the bearers of a trait) gains strength as a force over phenotypic adaptation in cultural change along some gradient of increasing cost of adopting a new tactic or technology.

For example, some argue that about 1,000 years ago speakers of Numic languages migrated into the Great Basin of western North America and replaced (or subsumed) the existing "non-Numa" population. Bettinger (e.g., Bettinger and Baumhoff 1982) argues that the non-Numa were nomadic and took high-return-rate resources such as large game; the Numa were less nomadic and had a broader diet, including small seeds and pinyon nuts, utilizing some different technologies (e.g., seed beaters) and tactics (e.g., green-cone-pinyon procurement). Bettinger argues that the Numa outcompeted the non-Numa, who migrated, became extinct, or intermarried and lost their language. A common re-

sponse to this scenario is: Why didn't the non-Numa adopt the Numa's behavior?

One reason could be that the Numa's foraging tactics carried a cost that was perceived as too high by non-Numa. Among many foragers, sharing meat from large game is a primary way men acquire prestige and possibly more mating opportunities and/or resources for their offspring; among others, men devote time directly to giving resources and attention to their offspring (Kelly 1995). If the Numa came from an environment in which the latter tactic conferred greater fitness than the former, then they would have imported this trait into the Basin. Non-Numa men might have been reluctant to give up hunting even if in the long term that choice made them less competitive, as they might have seen only the perceived loss in status and benefits that would result from forgoing an opportunity to hunt in order to collect small game or to care for children.

Likewise, women's foraging is affected by whether children tag along and whether they collect some of their own food (see Hawkes 1996). Without children, women can collect at higher return rates. The amount of foraging that children do is linked to how easy it is to get food and how dangerous foraging is for children (this would have been the same for the Numa and non-Numa) and also by the perceived availability of caretakers. In some foraging societies, children have multiple caregivers; in others the mother is the primary caregiver. What if Numa women entered from an environment in which a notion of child care prevailed (presumably because it bestowed greater adaptive advantage) that resulted in their leaving children at camp and this allowed them to forage more efficiently than non-Numa women and, presumably, raise more offspring to adulthood?

These factors may have resulted in a higher growth rate for the Numa, resulting in their "takeover" of the Great Basin. This scenario may or may not be correct, but the more general points are that human cultural behavior may be under selection à la evolutionary archaeology in only a limited range of cases; seeing technology as part of the phenotype and not also as a reflection of behavior (the tactics) that is part of the phenotype could be misleading (i.e., behavioral reconstruction is somewhat necessary); the frequencies of behaviors in a population, as evolutionary ecology argues, are largely the outcome of individuals' making decisions among often conflicting goals within natural and social environments that set different payoffs to different behaviors.

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Let us consider a hypothetical society of farmers trying to make a living in an arid environment. Rainfall is unpredictable and may be highly localized. The uplands tend to receive more moisture, but the growing season

is shorter here than in the lowlands. Pests are a constant threat, yet major impacts tend to be localized. Technology includes a variety of chipped stone tools, digging sticks, and hoes. Farmers may plant early and/or late in the spring to avoid killing spring and fall frosts and may plant in single fields or many. While most farmers take advantage of simple technology that increases water runoff onto fields, others dry-farm. Males are the farmers, and land is held by women. Cultural transmission occurs obliquely when a boy's mother's brother teaches him how to farm. Transmission occurs horizontally when farmers share knowledge. How to be a good farmer is encoded in religious teachings, and ritual objects serve as mnemonic devices that also provide instruction in acceptable farming practices. Farmers who generate successful harvests are much admired, and their consultation and advice are appreciated. Hero myths are constructed around a few. If necessary, weave into this picture the vicissitudes of meaningful human life, however you see them.

If I am understanding Boone and Smith correctly, in evolutionary ecological terms the behavior of this hypothetical society has been designed by natural selection to make extensive adaptive adjustments or exhibit phenotypic plasticity. The extent of these behaviors over a range of environmental conditions is called the reaction norm. Decision rules—here learned behavior—guide what people do, not natural selection. Explanations are often constructed in terms of efficiency and costs and benefits, among other factors that might influence decision rules. Here human intent plays a role, as individuals may indeed direct the course of their own existence, at least in part. Boone and Smith assert that this perspective yields valuable understandings of human behavior. I agree. I particularly value it for explaining the evolved behavior of contemporary and historic peoples within a synchronic framework.

Now let us push this society through time for a few thousand years or so, letting them build an archaeological record that represents aspects of the reaction norm and decision rules for times t_1 , t_2 , t_3 . Given the environment specified above and human ingenuity at generating variation (or attempts at problem solving, if you prefer), few readers would be surprised if the archaeological record of our group exhibited changing reaction norms as well as decision rules. The varied strategies and tactics of production should exhibit differential success in response to changing environmental conditions. Some behaviors and technologies may be dropped from the repertoire completely as new ones are generated. From time t_1 through t_3 our hypothetical society has constantly been in the process of becoming something else, with associated new reaction norms and decision rules. Importantly, and as evolutionary biologists have taught us, it is never possible for our hypothetical society to reattain a previous state, given the vagaries of history that influence evolution.

To the evolutionary archaeologist this is human evolution. Evolutionary archaeologists seek to account for change in human behavior primarily in terms of the

evolutionary processes of natural selection, sorting, and drift. Intent and individual or group efforts to direct the course of evolution are irrelevant at this scale, as the generation of variation, however knowledgeable and directed, is independent of natural selection. All of our problem-solving farmers intended success as they employed their technologies and thus indirectly influenced the course of evolution.

However, not all succeeded, and through time, the operation of the evolutionary processes of natural selection, sorting, and drift likely would have continually and irrevocably changed the society. To the evolutionary archaeologist, explanation of these changes is achieved in part by isolating the relative importance of these mechanisms through time in evolutionary context. Evolutionary archaeologists assert that this perspective yields valuable understandings of the evolution of human behavior. I agree.

To understand why evolutionary archaeologists adopt the metaphysical position they do, interested readers might examine the literature discussing the differences between timelike and spacelike frameworks, as illustrated above. They may also want to examine the extensive biological literature that discusses the different, yet interdependent, metaphysics of evolution and ecology.

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I am in complete agreement with the views expressed by Boone and Smith: evolutionary archaeology is a doomed enterprise based on flawed theoretical premises that can make no significant contribution to our explanation of the variability in the archaeological record and our understanding of past behaviour. The reasons for this are sufficiently well described in their article that I have no reason to repeat or expand upon them (as I have done elsewhere [Mithen 1997]). How cultural selectionism/evolutionary archaeology can have been taken so seriously for so many years and how so many new publications adopting this approach continue to appear in the literature is quite beyond me. Similarly, I am repeatedly surprised that prestigious academics whose other work I highly respect seem to take seriously the notion of “memes” as a cultural equivalent of genes (e.g., Dennett 1996): the notion of “memes” is simply fallacious, the vital differences between “memes” and genes having been exposed by Lake (1996). Therefore, while I applaud this article by Boone and Smith, I am also saddened and irritated that such an article is necessary at all.

My strong feelings on this issue no doubt arise from the fact that I have written at length in support of the evolutionary ecological approach in archaeology. In my 1990 book I put forward the argument that natural selection is relevant in our discipline only with regard to how it has shaped the human mind/brain—our means

of making decisions. We can safely assume that it has provided us with a means for adaptive decision making, and as archaeologists we can exploit this assumption for developing models of past behaviour. But such models need to be based at the level of individual behaviour, and to operationalise them we need to take into account the formation processes of the archaeological record (Mithen 1993). One solution as to how we can move from the short-term decisions of past individuals to the long-term, aggregate patterns of the archaeological record is by using computer simulation as a methodological tool.

I must, however, raise three issues regarding this “adaptive decision-making” apparatus that we carry within our minds/brains. Perhaps the most important is whether we have a single, general-purpose decision-making/learning device or multiple devices each having been selected to solve a different adaptive problem in our evolutionary past. The weight of the evidence from psychology (e.g., Hirschfeld and Gelman 1994) and human evolution (Mithen 1996) suggests strongly that it is the latter. This has very considerable consequences for how an evolutionary ecological approach to human behaviour can be developed, especially when dealing with behaviour for which specialised adaptive decision-making devices are likely to be absent.

Because the pace of culture change has been so rapid during the past 50,000 years—during which we assume that the decision-making devices of the mind/brain have not evolved—we may be inherently unsuited to making adaptive decisions in relation to a whole range of problems found in the modern world (e.g., should we create genetically engineered foods? should we use new medical technologies to keep extremely premature babies alive?). Moreover, when faced with these problems for which we do not have an evolved decision-making device, we may apply one suited to a different type of problem, with highly maladaptive results. In this regard, adopting an evolutionary ecological approach in which emphasis is placed on evolved cognitive mechanisms does not constitute an adaptationist programme.

A second but related issue is the narrow range of behaviours to which an evolutionary ecological approach is currently applied. It is fine for tackling the “food and sex stuff” about human behaviour—these are precisely the problems for which adaptive decision-making cognitive devices are likely to have evolved. Thus archaeologists adopting an evolutionary ecological approach have had some success at understanding the food choices made by past people and technological choices when these directly impinge on food acquisition and, in anthropological contexts, when tackling issues such as mate choice. But at present this Darwinian approach appears to have little to say about aspects of human behaviour which do not have such direct bearing on reproductive success, such as which art style to choose or which religious entities to believe in. Unless archaeologists who adopt an evolutionary ecological approach begin tackling these issues rather than just focusing on subsistence, their approach will never become widely

employed within the discipline: free rein will be given to theoretically flawed approaches (e.g., evolutionary archaeology) which seek legitimation by aligning themselves with a Darwinian paradigm, and evolutionary ecology will remain the preserve of hunter-gatherer specialists alone.

There is certainly potential for developing a Darwinian archaeology which can address issues with a less direct bearing on reproductive success than foraging and mate choice (i.e., food and sex). But to do so we must delve deeper into the evolutionary history of the mind/brain and go beyond making vague generalisations about evolved cognitive mechanisms for decision making, as I myself have attempted to do (Mithen 1996). As archaeologists we need to build stronger links with evolutionary psychologists (who also need to work more closely with archaeologists to achieve their own aims) so that we can develop more sophisticated models of evolved decision-making devices and, more generally, of how our minds work. An example of the work to which we can perhaps aspire is Pascal Boyer's (1994) study of the transmission of religious ideas. In this he shows how an understanding of evolved mental mechanisms can constrain the likelihood with which different religious ideas can survive the rigours of cultural transmission.

A third issue I wish to raise is that of multiple goals. When an optimal-foraging/decision-making model is developed, one normally assumes a single goal, such as minimising risk or maximising the rate of energy intake (or the "meliorising" equivalents [Mithen 1989b]). The application of the model evaluates the likelihood of achieving this goal, together with whether one has correctly identified the constraints under which decision making takes place. The problem of multiple goals is present when dealing with any animal species, but it becomes particularly acute when dealing with humans. Subsistence behaviour, for instance, is so thoroughly embedded in social and ideological behaviour that decisions about which foodstuffs to exploit are made in light of many competing goals—to satisfy one's nutritional needs, to secure food to share, to gain high-prestige food, to conform to ideological beliefs about appropriate foods to eat, etc. This interaction of a multiplicity of goals has been made most clear in relation to projectile points, the design of which among modern humans is clearly made in light of a wide range of social, economic, and ideological factors (e.g., Wiessner 1993). I believe that this problem of the complex interbedding of any activity in multiple domains of behaviour is a particular feature of modern humans (Mithen 1996) and consequently creates problems in the development of optimal-foraging/decision-making models not faced by ecologists dealing with other animal species. Indeed, the foraging models currently applied to modern hunter-gatherers as described by Boone and Smith, which are largely unmodified from those used for other animals, are probably more appropriate for premodern human behaviour prior to the emergence

of this interbedding of behaviour or what I have termed "cognitive fluidity." Of course, a problem we face is that their development and application become more difficult as we go farther back into human evolution because of the increasingly coarse grain of the archaeological record.

In summary, I agree wholeheartedly with Boone and Smith's critique of evolutionary archaeology and support their espousal of evolutionary ecology in archaeology. But at present, this remains as narrow in the types of human behaviour that it tackles as it did two decades ago, when exactly the same generalisations about evolved decision-making cognitive devices were being made. I want to see much greater attention paid to the nature of evolved mechanisms in the mind and a more explicit recognition that a Darwinian archaeology does not constitute an adaptationist programme. I believe we need greater willingness to tackle nonsubsistence-related issues in archaeology from this perspective and to acknowledge that models developed for other foraging species may not be as easily applicable to modern humans as has been previously argued. These comments are not meant to detract from the value of Boone and Smith's paper, and I certainly want to see a much greater application in archaeology of foraging models in which explicit reference is made to individual decision making of precisely the type they describe. I simply think that the evolutionary ecological approach they support is capable of making a far greater impact on our understanding of human behaviour in the past and present than is currently the case.

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"Is It Evolution Yet?" is a timely and important contribution to the developing theory of evolution in anthropology. For too long evolutionary archaeology and evolutionary ecology have worked in isolation. I am hopeful that this article signals a change in which mutual benefits outweigh costs. Although there is much in it that is worthy of in-depth discussion, I restrict my comments to Boone and Smith's criticisms of my work on native artifact replacement.

The basis for Boone and Smith's criticism is one sentence in which I link the spread of snowmobiles among the Cree to the success of the horse in North America. The spread of the horse occurred over approximately 100 years, that of snowmobiles in less than a generation. As Boone and Smith state, the Cree example is too brief in evolutionary time to consider Darwinian selection as the mechanism of change, but selection was not the point. I linked the two introduced technologies to show that adoption is ongoing; at some later time snowmobiles may become grist for the evolutionary mill.

Their criticism is, however, out of context, because

my paper in press is not about Cree or snowmobiles; a continuation of ideas published in 1995, it is a *preliminary* effort to build an evolutionary archaeological description of why European artifacts replace native artifacts, with a “replacement” defined as “adoption and persistence.” Dunnell’s concept of function (as fitness costs) frames the discussion, and I consider how European and Native American raw materials could affect individual fitness differentially, resulting in replacement.

My approach contrasts with the culture-contact literature on artifact change and with Winterhalder’s research with the Cree (1977, 1980, 1981). Acculturationists describe and explain artifact change. Although their descriptions are rich in detail, their explanations are problematic. They lodge explanation in the adoption process or the superiority of European products and technologies, both incomplete in evolutionary terms. Because evolution is a two-stage process, adoption cannot explain persistence. If we assume that artifacts contribute to fitness, survival of populations is partially a consequence of artifacts. The survival of human populations on both sides of the Atlantic suggests that, though different, artifacts were perfectly adequate, fitness-enhancing solutions. Why, then, should European technologies and artifacts replace native materials and products? I still think that this is an important and largely unanswered question.

Although an evolutionist, Winterhalder is not directly concerned with snowmobiles in his Cree work. His interest is Cree prey choices and changing diet breadth. Snowmobiles are a new artifact adopted because they reduce search and pursuit times—that is, they increase efficiency. In contrast to statements by Boone and Smith, Winterhalder does not test hypotheses on changing diet breadth. Nor does he analyze the costs (fuel, labor etc.) of adoption (see Smith 1991 for analyses of some costs among the Inujjuamiut). Simply, Winterhalder assumes benefits of the new technology without evaluating costs. Although more satisfying than portrayals of artifact change by acculturationists, Winterhalder’s account still misses the mark because he embeds snowmobile adoption in foraging theory. Boone and Smith consider this an acceptable explanation for snowmobile adoption, but I am less convinced, especially in light of the sweep of artifact replacements in Cree hunting equipment—rifles, canvas canoes followed by outboards, and wire traps (Rogers 1954; Winterhalder 1977, 1981).

The snowmobile case highlights significant differences between the two evolutionary approaches. They are girded by different concepts and assumptions, and because we do not know which, if any, of these are correct, I believe that serious intellectual discussion must begin with them. Boone and Smith, for instance, adopt a narrow view of Dawkins’s evolutionary units, the replicator and the vehicle (Dawkins 1978, 1982, 1989). Artifacts cannot be the focus of investigation because artifacts are not replicators. Genes are replicators and, over

evolutionary time, have built cognitive structures in which individuals make optimal decisions. Focusing on behavior facilitates examining whether or not decisions maximize fitness.

My conception of evolution is strongly influenced by Hull’s units, the replicator and the interactor (Hull 1980, 1981, 1988, 1994). His definitions free replicators and interactors of a particular taxonomic level. In addition, depending on circumstances, the same entity can function as both replicator and interactor. Using these units to measure and track change has significant ramifications for evolutionary description and explanation. First, artifacts can be the focus of investigation, making our task the determination of whether and in what ways alternative traits differ in fitness. Slight differences in costs can have consequences over evolutionary time. Secondly, evolutionary pathways are quirky and unpredictable. This very quirkiness raises fundamental questions about evolved cognition and optimality. Because we are historical documents, it is possible that historical cognitive structures and circumstances sometimes conflict, resulting in decisions that are less than optimal and may reduce fitness. In this case, we need to rethink whether universal optimality is a fruitful tool for measuring evolutionary change.

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This article raises a number of important issues, some of which we discuss elsewhere (Bettinger, Boyd, and Richerson 1996). There are two points we want to stress here, one theoretical and the other empirical.

The theoretical point is that cultural transmission makes decisions that individuals make an evolutionary force much like natural selection. One outcome of the coupling of deliberate invention and strategic adoption of innovations to transmission by social learning is to make even very weak, marginal decision making an agent of rapid change at the population level in the long run. Indeed, the main hypothesis to emerge from the theorizing of Boyd and Richerson (1985) was that the great adaptive advantage of the cultural system in a highly variable environment such as the Pleistocene stems from the substantial increase in rate of evolution possible when weak, low-cost but statistically adaptive decision rules supplement natural selection. Very crudely, what we call the costly-information hypothesis holds that decision-making forces and natural selection acting on cultural variation ought to be equally important. We say “crudely” because it is impossible to predict the mixture quantitatively without knowing much more than we do about the costs and benefits of setting

up decision-making and culture-transmitting psychologies. One way of portraying the debate between evolutionary archaeology and evolutionary ecology is to say that they have different intuitions about the relative importance of decision making and natural selection in causing cultural evolution. The costly-information hypothesis suggests that this is an entirely reasonable debate. The extremes—all selection or all decision making—are equally unlikely, yet the debates in all fields of the social sciences tend to portray rational-actor theory and culture-historical explanations (of which the evolutionary archaeological account is a particular theoretically motivated version) as competing rather than ultimately complementary.

The empirical point is obvious: we need to estimate the strength of decision-making and selective effects in a broad sample of cases before we can make any generalizations. Unsophisticated wet-finger-to-the-wind empiricism suggests that the costly-information hypothesis is plausible. Consider stock archaeological patterns: The origins of agriculture, the rise of states, and other major features of cultural evolution are events with time scales of millennia, yet over millennia fantastic changes in and diversifications of cultural adaptations occur. Cultural evolution is too slow to be explained entirely by individual strategizing but much faster than unaided organic evolution, consistent with a mixture of selective and decision-making forces. Archaeologists could make this argument much more quantitative. Paleontologists have measured the rates of organic evolution in terms of a unit known as the “darwin,” where 1 darwin is a change by the factor e , the base of the natural logarithms, in a character per million years (see discussion in Ridley 1993: chap. 19). A fair number of such rates have been measured and can be used as a yardstick to see if rates of cultural evolution do really generally exceed those of organic evolution. For example, since the end of the Pleistocene human societies have increased in size from averages of perhaps 1,000 to an average of perhaps 1,000,000 in the past 10,000 years. This gives a rate of change of ca. 700 darwins, compared with a range of 0.11–32.0 darwins in a sample of 46 morphological characters from post-Pleistocene mammals (Gingerich 1983). Human societies are clearly evolving very rapidly, and the evolutionary force of decision making is a candidate for explaining why. Some hints might come out of a large sample of evolutionary rates. The theory suggests that in domains where judgments are easy, individual decisions should be strong forces and evolution correspondingly rapid. The Cree’s adoption of snowmobiles is an example. In general, we might expect that in many domains of technology diffusion, easy-to-try-out, easy-to-acquire innovations will spread rapidly. Contrariwise, social organizational innovations are hard to observe (just how do polygynists manage the complexities of multiwife households? monogamous ones try most of us) and hard to try out (the range of marriage institutions one can experiment with in one lifetime is strictly limited).

It also should be possible to measure the relative im-

portance of selection and decision-making forces directly in living societies. As far as we are aware, there is only one set of data that comes close to doing this. Roof and McKinney (1987:chap. 5) present data derived from questionnaires estimating the effects of birthrates (selection) and switching (decisions) on the net growth rates of groups of churches in the United States. There are striking effects of selection. The biggest discrepancy is between black and conservative white Protestants and those with no affiliation. People with no affiliation have only a little more than half as many children as the two types of conservative Protestants. Conservatives also have about a 30% birthrate advantage over liberal Protestants. Decision making also has a big effect, especially on loss rates from liberal and moderate Protestant churches. Historically, liberal churches, perhaps because they were socially more prestigious, attracted considerable net switching from moderate churches. Among younger people, this flow has slowed to a balance of switching in and out, while the net loss to nonaffiliation is about 9% of the number born to liberal churches. Among under-age-45 people, conservative white churches have a slight net gain (5%) over other churches and a slight net loss (3%) to nonaffiliation. Thus, conservative Protestant churches are growing because their birthrate is well above replacement and because they mostly hang onto these kids. The liberal churches are well below replacement fecundity, suffer significant net losses to nonaffiliation, and have lost their attractiveness as vehicles of status mobility. One study is a grossly insufficient test of the costly-information hypothesis, but it exemplifies the pattern it predicts; decision making and selection are both important in this case. There is a nice irony in the discovery that the success of conservative Protestants stems in large part from natural selection.

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Boone and Smith’s critique of evolutionary archaeology centers on its failure to accommodate the empirical consequences of cultural transmission. Evolutionary archaeologists maintain that natural selection acts on heritable (genetic or cultural) phenotypic (physiological or behavioral) variation contributing to an individual’s fitness. This position runs into problems. Natural selection is tied to reproductive success, and because it is confined to parent-offspring relationships it is constrained by the rate at which biological organisms reproduce. Cultural transmission, however, is not confined to parent-offspring relationships, it does not necessarily imply reproduction, and behavioral change within populations can occur more rapidly than biological generations reproduce. The consequences of this position are significant but unnecessary. Evolutionary ecologists maintain that natural selection accounts for phenotypic plasticity—in this case cognitive abilities

enabling organisms to vary their behavior in response to environmental change. Thus, whereas evolutionary archaeology identifies directional changes in trait frequencies as the result of natural selection, evolutionary ecology might attribute the same changes to adaptive adjustments (phenotypic variation).

While I agree with the central conclusions of Boone and Smith's critique, they raise many issues requiring clarification and refinement from both sides. The two fields have quite different points of departure—evolutionary ecology from an ethnographic perspective, evolutionary archaeology from an archaeological one. These differences incorporate long-term concerns over different methodological issues. The question what is or can be observed goes far beyond the role of behavioral reconstruction to a concern with identifying appropriate units of measurement, the perspective of time-scale, and more mundane issues such as chronological resolution. Consequently, what Boone and Smith might regard as a flawed grasp of evolutionary biology may reflect a more sophisticated methodological appreciation than they recognize.

Their example of the snowmobile and the horse (e.g., Ramenofsky 1995; Winterhalder 1980, 1981) provides a useful illustration for many points. In the Plains example, the perspective of a broader time-scale with which archaeologists generally operate reveals an unprecedented population, suggesting more than phenotypic adjustment. For example, horses and related technology effectively raised the carrying capacity of the Plains environment, providing the means to support higher population densities than were previously possible. While the source of people included refugee populations, the viability and persistence of higher population densities should reflect something about reproductive fitness. Boone and Smith's basic argument—that *adoption* of the horses can be best understood in terms of adaptive decision-making rules—remains cogent, however, and the difference underscores one of their critical points. By refusing to recognize the relevance of any mechanisms to effect phenotypic adjustments (e.g., decision-making rules), evolutionary archaeologists are at a loss to account for the mechanisms underlying selection. As Boone and Smith argue, in the absence of the "postulate that evolved cognitive abilities allow foragers to weigh the economic costs and benefits . . . and to choose [which one] gives the highest energy return . . . the intimate strategic relationship between foraging technology and diet breadth would remain theoretically opaque."

Criticism of the evolutionary archaeological position on undirected variation and human intention is more a matter of what constitutes the "innovation" than a fundamental theoretical disagreement. Evolutionary archaeologists identify the European introduction of horses and associated technology to the North American continent as the "innovation." In this context, it is unlikely that the Europeans' intention was to provide Native Americans with the means to remain viable populations. If I understand them correctly, evolution-

ary ecologists would identify the innovation as the incorporation of horses and associated technology into a new behavioral context. In this case their criticism would be justified. The evolutionary archaeologists' position does not imply a theory of unintended consequences, but given what they identify as the innovation it often turns out that way. More to the point, however, their position on human intention and innovation is better explained as a reaction to the privileged position and causal role these mechanisms have been given by more firmly entrenched models of cultural evolution. Boone and Smith seem to recognize this issue without fully appreciating its significance.

Nevertheless, while attempting to create a distance between Darwinian and cultural explanations, evolutionary archaeologists may have become victims of their own rhetoric and, as Boone and Smith suggest, "thrown the baby out with the bath water." The tendency to trivialize functional explanations and privilege evolutionary ones is pervasive in the evolutionary archaeological literature and undermines its proponents' ability to provide a clear accounting for the mechanisms underlying selection. While attempting to expunge behavior in response to the inductive epistemological strategy they have adopted they have somehow forgotten to include that theoretical principles must be written in behavioral terms, that archaeologists must invent the methodology (Binford 1964) to apply them, and that behavioral inferences are explanations (Neiman 1990).

In sum, evolutionary archaeologists face significant limitations in their failure to accommodate the empirical consequences of cultural transmission. To address these issues may require shedding some hazardous rhetoric currently associated with the program rather than necessarily retreating from any basic theoretical premises. Failure to do so will deny them access to a theory of behavior consistent with Darwinian principles and some much-needed methodological equipment.

Reply

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The diversity of reactions to our article mirrors the controversy and lack of agreement that characterize the evolutionary study of human behavior and culture. This suggests that the sample of commentators is a representative one, but it of course does not facilitate a compact reply. We have grouped our responses under five headings: the definition of natural selection, the explanatory logic of phenotypic adaptation, the relative causal efficacy of each of these forces in shaping human behavior and its products, the effects of temporal scale and historicity, and the problem of explaining complex social processes and institutions.

What is *natural selection*? Dennett wants the concept of natural selection to cover all cases where hereditary variation is transmitted differentially through time. Thus, people choosing snowmobiles over snowshoes or cows with higher milk yield are engaging in "a subvariety of natural selection" called "intentional selection." Our primary purpose in distinguishing phenotypic adaptation (including forms involving intentionality) from natural selection (including forms involving cultural inheritance) is to highlight the very different dynamics and causal processes involved in these two forms of change. Insisting that we lump all these things under the rubric of "natural selection" is a semantic game, since after all we must ultimately distinguish them from each other in order to understand how they work. And, frankly, we think that the conceptual distinctions established in Boyd and Richerson (1985: esp. 174 ff.) between natural selection (of genetic or cultural variation) and such factors as guided variation or biased transmission are more useful than the lumping advocated by Dennett.

Is phenotypic adaptation descriptive or explanatory? The comment by Hunt, Madsen, and Lipo indicates that some evolutionary archaeologists are still unable or unwilling to understand the logic of behavioral ecology and phenotypic adaptation. Hunt et al. fault us for making a claim—that natural selection has designed organisms to be able to assess environmental conditions and vary their behavior adaptively, rather than behaving randomly and letting selection sort things out—that in their view is "empirical, not theoretical," and in need of substantiation. We have already suggested that the empirical evidence for this claim is overwhelming and referred readers to the literature in behavioral ecology. Hunt et al. attempt to refute us on our own grounds by quoting from a key work in behavioral ecology (Stephens and Krebs 1986), but, curiously, the phrases they quote are precisely about how natural selection has designed organisms to engage in phenotypic adaptation.

We feel that Hunt et al. are the ones confused about the difference between (empirical) description and (theoretical) explanation. The explanatory models of behavioral ecology (e.g., the prey-choice model, the polygyny-threshold model) are middle-level theoretical constructs, deductively linked to more fundamental theories of neo-Darwinism (as discussed in any textbook on evolutionary biology or animal behavior). These models are used to generate testable hypotheses, which are then evaluated with empirical evidence; the results either (provisionally) support the models or lead to model revision or abandonment. The extent to which (a) natural selection has designed structures of phenotypic adaptation, (b) organisms employ these structures in the way predicted by behavioral ecology models, and (c) such phenotypic adaptation is similar to but faster than natural selection is subject to empirical evaluation. But the logical devices used to generate these empirical claims—the middle-level models and deductive links to neo-Darwinism noted above—are theoretical structures. Rather than engaging in imperious state-

ments about what is "non-Darwinian," "confused empiricism," "historical science," "untestable behavioral reconstructions," "reification," and so on—what we might call confusing slogans with theory—Hunt et al. should have pointed to some empirical evidence supporting their faith that natural selection directly shapes the archaeological record. Their failure to do so is not a good sign for the future of unreconstructed evolutionary archaeology.

Can phenotypic adaptation short-circuit natural selection? Evolution by natural selection is a process that shapes diversity when there are (a) replicators with (b) high copying fidelity and (c) differential replication success as a result of (d) interaction with some aspect(s) of the environment. We agree that cultural replicators (memes) sometimes meet all these criteria and hence that culture is sometimes subject to evolution by natural selection. But the central thrust of our article was that, contrary to evolutionary archaeological dogma, natural selection is not the only or necessarily the primary mechanism of cultural or behavioral change over time.

Leonard agrees in part but still wants natural selection (as well as drift and "sorting") to rule when change is considered over longer periods—"a few thousand years or so"—and when replicators "exhibit differential success in response to changing environmental conditions." Under these conditions, he argues, "intent and individual or group efforts to direct the course of evolution are irrelevant at this scale, as the generation of variation, however knowledgeable and directed, is independent of natural selection." But, as we argued at length, this gambit has little logical force. First, phenotypic adaptation is capable of responding to "changing environmental conditions" and indeed can be expected to do so more rapidly than natural selection does. Thus, if the pace of environmental change which Leonard invokes is not too rapid to confound evolution by natural selection, it cannot be too rapid to confound phenotypic adaptation. If successful (if people's estimates, decision rules, and phenotypic programs do indeed produce adaptive change), then phenotypic adaptation will *remove* the selective differentials that are essential to the action of natural selection; as we phrased it earlier, natural selection will be short-circuited. This is not an anti-Darwinian position but rather a phenomenon recognized by virtually all evolutionists who study behavior.

Second, the requirement that variation be generated independently of selection applies to replicators; but in a Darwinian world it cannot sensibly apply to phenotypic variants. Neither natural selection nor phenotypes can anticipate the future in any thorough way. But phenotypes (organisms) can respond to many environmental changes in adaptive ways, without waiting for genetic or cultural change; indeed, they are *designed* (by a history of natural selection) to do so. We of course recognize that people are not always adaptively successful in their strategizing, either because they do not have sufficient information to know what choices (e.g., plant early, plant late) will result in the best phenotypic pay-

off (e.g., harvest yield) or because they are faithfully executing genetic or cultural instructions that have been fixed by past genetic and cultural evolution but are not adaptive to novel environmental conditions (points made by Ramenofsky and Mithen). In these latter situations, natural selection and the other evolutionary forces may indeed prove decisive; but this outcome cannot be decided a priori or dogmatically as the evolutionary archaeological literature has been all too ready to do. Rather, it must be studied empirically and theoretically with great care before we can conclude much about the relative strength of the two mechanisms of adaptation.

The spread of snowmobiles among Cree and other northern peoples and of horses among Indians of the Great Plains are cases in point. While Ramenofsky agrees that the one-generation spread of snowmobiles is too rapid to be explained by natural selection, she apparently still defends her suggestion that the spread of domesticated horses among the Plains Indians “over approximately 100 years” is due to “Darwinian selection.” But 100 years is also far too rapid for natural selection to account for the spread of a trait amongst diverse Indian nations over a vast area, as elementary calculations will reveal, so we do not see the point of distinguishing snowmobiles and horses on these grounds. True, adoption of horses, by greatly reducing search and transport costs for buffalo hunting (as well as giving decisive military advantages), increased the Plains carrying capacity and “reflect[s] something about reproductive fitness,” as Teltser puts it. But attention to the ethnohistoric and linguistic record (sources of evidence typically absent from archaeological contexts) indicates that most of the Plains equestrians were derived from non-Plains peoples who immigrated to the Plains when the horse opened up a new niche. While certainly there was some expansion of equestrians relative to nonequestrians (e.g., village horticulturalists like the Mandan and Pawnee) via differential reproduction, which suggests that natural selection could be part of the explanation for the spread of the horse, we believe the evidence indicates that it was primarily due to decision making guided by evolved preferences (for increased food, military security, and status).

As Kodack notes, although Winterhalder did not test hypotheses about the reasons for Cree adoption of snowmobiles, some literature does exist that suggests that Cree and others made this choice on the basis of the kinds of efficiency considerations that are central to the logic of foraging theory (as reviewed in Smith 1991: chap. 9). If correct, this is a good illustration of the general point made in our article that phenotypic adaptation may often act to short-circuit natural selection; dog teams (or snowshoes) and snowmobiles competed in phenotypic rather than evolutionary time, and by the time the competition was over (in less than a generation) there was no artifact variation (let alone *heritable* variation in the correct Darwinian sense) left to be “grist for the evolutionary mill.”

What happens to ecology in evolutionary time? Sev-

eral of the commentators (Leonard, Jones, Hunt et al.) seem to imply that the evolutionary ecological approach is somehow more appropriate to synchronic or very short-term change. For example, Leonard suggests that evolutionary ecological explanations are fine for “contemporary and historic peoples in a synchronic framework” but a diachronic perspective requires a shift to a “timelike” framework. In a similar vein, Hunt et al. appear to question our “imagining that differential persistence in the long term can simply be conceived in spacelike (functional) terms.” Why can’t it be? As long as the selective conditions that gave rise to a set of decision rules governing phenotypic response are in place, the same range of adaptive strategies will continue to be played out indefinitely. Phenotypic adaptation occurs continuously. Natural selection on variation occurs continuously. Time scale is not the issue here. To see why, try to imagine a theoretically derivable rule that would specify the exact point in a time continuum when phenotypic adaptation “stops” and evolution “starts” (hence the title of our article). The real difference in the two approaches is the way in which units and variables are defined and employed in theory building.

The comments of Leonard and of Hunt et al. both imply that the explanatory structures of evolutionary ecology and evolutionary archaeology differ to the point where they are hardly comparable. Specifically, they adopt Dunnell’s position that evolutionary theory employs a timelike theory while ecology is spacelike in its structure. According to Dunnell’s (1982: 9–10) original formulation of this distinction, predictive statements are possible when theories are constructed in “time-space free” units and variables. Evolutionary theory, he argues, is time-like because the morphological and behavioral configuration of a particular organism at a given point in time is, in the last analysis, the product of its specific evolutionary history—a history full of contingencies and quirks that are unpredictable. Hence, evolutionary trajectories are unpredictable—as Dunnell has recently put it, “cause [in evolutionary change] is historically contingent, thus explanation must be historical as well” (Dunnell 1996b:5). To put the problem in more concrete terms, one cannot look at a population of Devonian amphibians and predict that porcupines or parrots will evolve in the Tertiary. A myriad of historical contingencies (for example, global climate change, tectonic plate movements) will have intervened to produce the specific evolutionary trajectories that result in the life-forms we observe today. Certainly we do not deny the necessity of taking historical contingency into account when producing narrative descriptions of historical change, evolutionary or otherwise. We question, however, the wisdom of restricting evolutionary explanation to this specific historical level of generality; this in effect makes phylogenetic reconstruction the only goal of evolutionary analysis.

As we hope our article has made clear, evolutionary ecology defines units and variables in more general “time-space-free” terms that allow predictive state-

ments to be made about the form that behavioral and morphological strategies will take under specified environmental conditions. For example, in foraging theory, the relevant units include predator and prey type; relevant variables include search and handling time and food value. These units are time- and space-free in that they apply generally to any species, time, or place. This kind of theory building is not limited to "ecology." Sexual selection theory, first developed by Darwin (1859 and esp. 1871) and since greatly elaborated (e.g., Andersson 1994, Ryan 1997), is constructed in an analogous fashion. The units are males and females; variables include mate preferences, competitive ability, and phenotypic quality. Thus, if we want to explain why peacocks have evolved elaborate tails for sexual display while bullfrogs use croaks, we must deal in phylogenetic analysis and historical contingency. But this has not prevented behavioral ecologists from developing generalized predictive theories about the form and frequency of sexually selected displays (see Neiman [1990:42–46, 58–60] for further discussion of this general issue and a critique of the position that evolutionary theory requires a different kind of metaphysics from other "hard" sciences).

Can evolutionary ecology deal with complex social phenomena? Both Earle and Mithen fault evolutionary ecology for failing to deal with complex social institutions and processes. This is a significant charge, both because of the obvious importance of such phenomena in human history and prehistory and because evolutionary archaeologists sometimes claim to have uncovered a scalar phenomenon of group selection in complex societies (e.g., Dunnell 1980). It is certainly true that to date evolutionary ecology's primary contributions to analyzing human behavior have focused on individual-level behavior and relatively simple socioeconomic systems. This is partly because of evolutionary ecology's commitment to methodological individualism—the notion that properties of groups (social institutions, populations, societies, etc.) are products of the actions of its individual members (Elster 1983; Smith and Winterhalder 1992b:39)—and partly because in any new field of study it makes sense to start with simple and tractable problems. But there is in fact a growing body of literature in human behavioral ecology that deals with social relationships, interdependencies, conflicts of interest, multiple goals and currencies (e.g., nutrition, status, and mating opportunities), and even "emergent properties" of social systems (e.g., Boone 1986, 1998; Dickemann 1979; Neiman 1997; Hawkes 1996; Hill and Hurtado 1995; Rodseth et al. 1991; Winterhalder 1990; reviews in Boone 1992, Borgerhoff Mulder 1992, Rogers 1992, Smith 1992). Readers should not mistake the examples we cited in our article, which were chosen for heuristic simplicity, for the present (Mithen) or possible (Earle) scope of human behavioral ecology.

Earle is surprised that we "ignore processual archaeology" and its attempts to "explain the development of social institutions." Our article, however, was clearly

advertised as a critique of evolutionary archaeology of the "selectionist" variety from the theoretical standpoint of evolutionary ecology. An evaluation of processual archaeology (or cultural ecology) from this standpoint requires a separate treatment (see O'Connell 1995 for some work along those lines) and would reveal antagonistic as well as complementary or convergent relations between processualism and evolutionary ecology.

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