

Behavioral Archaeology and the Extended Phenotype

MICHAEL J. O'BRIEN AND THOMAS D. HOLLAND

Americanist archaeology has entertained a long-standing interest in understanding and explaining the behavior of prehistoric peoples—an interest sometimes stated explicitly and at other times only implied. It is difficult to pinpoint when behavior first came under archaeological scrutiny, but certainly by the mid-1970s archaeologists were deeply involved in the search for laws that would explain the human behavior that created the patterns evident in their data.

The clarion call for what became known as behavioral archaeology was made in 1974 in a two-page article in *American Antiquity* that carried the ambitious title “Expanding Archaeology” (Reid, Rathje, and Schiffer 1974). The tone for and principles behind a behavioral archaeology were set the following year with the publication of two articles—“Archaeology as Behavioral Science” (Schiffer 1975a) and “Behavioral Archaeology: Four Strategies” (Reid, Schiffer, and Rathje 1975). Reid, Schiffer, and Rathje (1975:864) defined archaeology as “the study of relationships between human behavior and material culture” and, like Schiffer (1975a), employed the term *behavioral archaeology* to refer to “the study of material objects regardless of time or space in order to describe and explain human behavior.” They also noted that “by virtue of years of research . . . archaeologists now possess an expanding body of theory, method,

and behavioral laws for the study of material objects and human behavior regardless of time or space” (1975:866).

Despite the intervening years, many archaeologists today would still subscribe to the notion that “behavioral archaeology is a synthesis of what archaeologists have done and aspire to do and that the essential inter-relatedness among the strategies has roots deep in the progressive development of the discipline as a whole” (Reid, Schiffer, and Rathje 1975:866). Considerable disagreements exist in the archaeological literature over the most appropriate methods for studying behavior—disagreements that run the gamut from philosophical to methodological—but they belie the fact that behavioral archaeology, by definition, has a central focus on the search for and application of behavioral laws and their corollaries. Despite heated arguments over the proper role of ethnographic analogy in archaeological research, many of the same principles that guided argument from analogy in the 1970s and 1980s still guide behaviorally oriented research today: within certain parameters, examination of contemporary material remains and behavior can guide reconstructions of past behavior. Indeed, it is apparent to us that ethnographic analogy is more central to behavioral archaeology in the mid-1990s than it was a decade earlier. And we should make no mistake about it: dyed-in-

the-wool behaviorists still cling to what we believe is an untenable position—that human behavior can be reconstructed (see Reid, this volume).

Our interest lies not in general patterns of behavior—and certainly not in trying to reconstruct behavior—but in specific behaviors as manifested in functional items in the archaeological record. In simple terms, we are interested in the *outputs* of behaviors. So too, obviously, are those archaeologists who label themselves behaviorists, and although there are some similarities between the two positions, there also are several major differences.

Two specific goals underlie our approach, which we call selection-based, or evolutionary, archaeology (O'Brien and Holland 1990): (1) documenting life histories of individual classes of archaeological objects to determine whether they were adaptations (O'Brien and Holland 1992), and (2) understanding why classes of objects were replaced by other classes. The approach emphasizes change in terms of replacement of one feature with another through the process of selection as opposed to change rendered in terms of a simple transformational process from one state to another (Dunnell 1982, 1988). Our analytical strategy focuses on understanding the engineering design of archaeological objects determined to be adaptations, the underlying premise being that the objects were phenotypic features analytically identical to genetically transmitted, purely somatic features. We discuss these goals in greater detail throughout this chapter.

We believe that common ground exists between evolutionary archaeology and certain behavioral archaeology strategies, specifically the analysis of prehistoric materials from the standpoint of engineering design. We agree with several other contributors to this book (see especially the chapters by Skibo and Schiffer and by Deal and Hagstrum) that technological and functional analyses of how certain objects were used at specific times can conceivably put one on a firmer foundation when inferring the specific behaviors—or *activities*, as Schiffer (1976)

terms them—of the prehistoric manufacturers and users of those objects. Importantly, these inferences are derived directly from experimental evidence viewed against the archaeological context containing the materials being examined. We contend that this kind of research agenda—one based on experimental evidence—will allow us to begin to understand not only the evolutionary trajectories of the humans responsible for the technological products but also the nature of selective regimes.

Our approach, however, is not compatible with all aspects of behavioral archaeology, which itself has widely divergent goals. For example, the common ground to which we refer has nothing to do with approaches grounded in the search for universal laws of behavior. In our view such universalities not only do not exist, they cannot exist. Does this imply that analogs to modern behavioral outputs do not occur in the archaeological record? No. Does it imply that some of the behaviors that create modern outputs were not similar to those that operated in the past? Not necessarily. The term *cultural universals* implies that we somehow have at our disposal the necessary means to infer that the behaviors were or are the same. This feat is difficult, and probably impossible, to accomplish.

Importantly, we should ask, what is at issue here: similar *behaviors* or similar *processes*? Can we say that two behaviors are the same because two groups reduce stone in the same way? We argue that there is a world of difference between the two. We further argue that conflation of behaviors and processes has led to the belief that the archaeological record can be used to construct universal laws of behavior. Again, we view this goal as impossible. We do, however, believe that the archaeological record can be used to examine (not reconstruct) *specific* human behaviors and that Darwinian evolutionary theory offers the best grounding for such an examination.

As Dunnell (1982, 1988, 1989a, 1989b) has pointed out, Darwinian evolution has not enjoyed a favored position in archaeol-

ogy. If archaeologists remain unconvinced that evolutionary theory is entirely appropriate to the study of culture-bearing animals—and it is clear that few archaeologists have tried to understand the evolutionary-archaeological arguments—then any attempt to demonstrate commonalities between a selectionist archaeology and behavioral archaeology is moot. But again, we emphasize the strong belief that there *are* common links between the two—links that have gone unnoticed by both behavioral archaeologists, who in our opinion have missed what is embodied in the concept of *phenotype*, and evolutionary archaeologists, who have taken too dogmatic an approach to a selection-based archaeology.

Our objective in this chapter is to clear up what we view as hurdles not only to uniting select aspects of archaeological analysis of behavior with an evolutionary archaeology but also to incorporating evolutionary archaeology into the discipline as a whole. Several issues we address, although they should have little to do with archaeology, have, unfortunately, been part of our anthropological upbringing and have crept in to color our perspectives on what it is (and is not) to be human. We take special aim at the misguided viewpoint that evolutionary theory is inappropriate to the study of humans, their behaviors, and the by-products of their behaviors.

At least two corollaries of that viewpoint—confusion of trait transmission with the process of evolution and misplaced emphasis on realized fitness as opposed to potential fitness—have created barriers to successful incorporation of an evolutionary perspective in archaeology. Each corollary in its own way has also led to the belief that features in the archaeological record can be removed from their temporal and spatial contexts and used as universal analogs in behavioral studies. But the archaeological record, like the paleontological record, is a record of evolution. As such it is a *historical* record both temporally and spatially specific. Failure to treat the archaeological record as a time-and-space-specific phenom-

enon has led some behavioral archaeologists to believe that if only our reasoning were sharpened and our methods honed—what Wylie (1989b:94) terms the new archaeology's “‘strongly positive’ optimism”—we could understand and explain almost any past behavior (e.g., Binford 1968). Our opinion is that such optimism is unwarranted (but see Wylie, this volume) and that studies linked to ethnographic analogy—one cornerstone of behavioral archaeology—and a search for lawlike generalizations are bound to fail. Alternatively, maybe we should soften our indictment and say that perhaps some such studies are successful but that we have no way of discriminating between successes and failures.

This chapter is neither an in-depth review of behavioral archaeology nor an excursion into the epistemological literature on the use and abuse of analogy. For the former, the reader is urged to read the primary literature and to consult other chapters in this volume, especially those by Walker, Skibo, and Nielsen and by Reid. For the latter, we suggest reading Wylie's “The Reaction against Analogy” (1985) and especially her “‘Simple’ Analogy and the Role of Relevance Assumptions: Implications of Archaeological Practice” (1988). We find it difficult to compete with a philosopher's handling of the epistemological issues surrounding analogy and confine ourselves to pointing out what we see as logical fallacies in archaeological attempts to employ analogy.

BEHAVIOR AS AN ANALYTICAL FOCUS

The study of behavior has played an important role in evolutionary biology (e.g., Bonner 1980, 1988) and in evolution-based studies of human behavior (e.g., Betzig, Mulder, and Turke 1988; Chagnon and Irons 1979; Cosmides and Tooby 1987; Pittendrigh 1958; Symons 1979, 1990). Given the importance of behavioral studies and the methodological advances that have been made through them, can't we simply transfer this analytical emphasis to examination of the archaeological record? If humans exhibit

behavior, and that behavior is manifest in the materials that form the archaeological record, can't those materials inform us in general about behavior? What we are really asking is, can't the archaeological record be used to explain past behavior and, at some level, to construct generalized statements about behavior that are universally true?

If we don't care about conflating behavior and process, then the obvious response is that at one level—perhaps in the grand scheme of things a rather trivial level—we *can* study behavior universally. For example, experience has taught us that, to our knowledge, only humans reduce stone to fashion tools. No one would seriously question that a hafted, pointed biface was made by a human, even though we did not see anyone make the object. But what good are such inferences? All we have done is to identify a process—stone reduction. We might be even more specific and say that the process was bipolar flaking, but this tells us nothing about behavior. What archaeologists, behavioral or otherwise, want to know is why prehistoric peoples collected particular raw materials or why they formed tools the way they did. Or, more specifically, they want to know under what conditions people create certain kinds of tools.

It is to answer these types of questions that searches for behavioral laws have been undertaken. But we believe a fundamental problem exists with this approach, namely, the archaeological record contains only the products and by-products of specific behaviors. Those behaviors were timebound and spacebound. A pile of chipping debris reflects in some way behaviors associated with making tools, but it is only the by-product of the behaviors and not the behaviors themselves that we can record directly. Behavioral assessments require inference, and it is there that things can begin to unravel. Where does one draw the line between strong inference and weak inference? On what grounds do we decide that an inference is strong? Archaeology has a long track record of appealing to common sense to explain the archaeological record, but surely we can do better than re-

lying on experience as a sense-making strategy. As Galbraith (1975:243) noted, "When not able to grasp an idea, practical men take refuge in the innate superiority of common sense. Common sense is another term for what has always been believed." Often lost is the fact that what we typically call "common sense" is an evolving rule book that cannot necessarily be applied to the past.

Archaeologists have long realized the dilemma created by appeals to common sense to explain behavior. What has always been needed is a method of verification to ensure that inferences are strong. Ethnoarchaeology assumed the forefront in the battery of approaches open to behavioral archaeologists, becoming a stroll through the "source side" (Watson 1979) to construct Hempelian covering laws (e.g., Watson, LeBlanc, and Redman 1971, 1974) useful for interpreting (explaining) what is found in the archaeological record (Watson's "subject side"). As Wylie (1989c:21) underscored, researchers from the start were deeply divided over how the link between source-side-derived behavior and archaeological remains was to be made, although one point seemed reasonably clear at the time: behavioral knowledge derived from the source side had to be general as opposed to specific in nature; that is, it had to be more than anecdotal.

Thus Schiffer (1978a:232) recommended development of "general statements relating two or more variables without regard to time or place," and Hole (1979:212) advocated searching for "the more timeless essentials." But a certain circularity was seen in those endeavors, leading Gould (1978a, 1978b, 1980a) to suggest that if only we had laws, we could escape the circularity inherent in the use of ethnographic analogy. Gould (1978a:8) urged archaeologists to move from "discovery of 'rules' of behavior as they occur in particular human societies to the possibility of discovering in residue formation 'laws' of behavior that are universal to mankind. . . . Any living society may be studied effectively by ethnoarchaeology, since residue behavior, like language, is universal to man." The laws to which he referred are

“covering laws,” which specify the conditions under which certain human responses will occur.

Wylie (1989c:21) noted that the appeal for covering laws “was originally motivated by the positivist-inspired view that archaeological claims about the past are, properly, the conclusions of explanatory arguments. . . . They require covering laws that establish a secure link between the type of conditions or events to be inferred and their surviving consequents, the archaeological data. On this analysis, the primary task facing a new archaeology is to establish these suppressed linking premises on empirical rather than conventional grounds.” Wylie summarized precisely the predicament in which many of the so-called new archaeologists found themselves, namely, the need somehow to link behaviors to material remains on empirical grounds—to develop laws that would “cover” the range of possible behaviors under various possible conditions.

How did archaeology respond to this call? Not very well, judging from the lack of consensus evident in the literature. Indeed, it soon became difficult to keep up with the players and conflicting views without a score card. Yellen (1977), for example, used his ethnographic data on the !Kung to conclude that “it is unfounded to assume that activities are spatially segregated or arranged by type within a single camp. Most tasks may be carried out in more than one place and in more than one social context; and, conversely, in any single area, one can find the remains of many activities, all jumbled together. Thus behavioral analyses become difficult—if not impossible—to undertake.” Yellen’s reaction was, as Schiffer (1981:905) points out, not to the archaeological study of behavior per se but rather to behavioral universals’ being applied uncritically to the archaeological record—hence Yellen’s (1977:8) quip, “It takes only one pin to prick the balloon.”

Binford (1978a, 1978b) was quick to counter, attempting to use his Nunamiut data to construct middle-range theory that

(he hoped) would bridge the gap between data and explanation. As Wylie (1989c:21) points out, Binford (1981b:27 [see also Binford 1981a]) rejected Schiffer’s middle-range research (e.g., Schiffer 1976) for its failure to distinguish between description of processes and explanation of processes (see Schiffer 1985), and he criticized Yellen for doing little more than providing cautionary tales and rote, mechanistic discussions of !Kung behavior. What Binford was criticizing was an analytical concentration on general facts, which, as Wylie (1989c:21) points out, did little more than replicate what Binford and others saw as the limitations of traditional archaeological research.

Documentation of anecdotal “facts” provided no basis for making inferences beyond the observed. Instead of raising our understanding of causal mechanisms responsible for regularities in the archaeological record—that is, providing an explanation of the regularities—documentation of facts “simply raises the interesting and potentially informative questions of why the observable patterning occurs when and as it does” (Wylie 1989c:21). Binford was correct: ethnoarchaeology had failed to produce little more than case studies of modern behavior—studies often framed in terms of cautionary tales of the perils involved in trying to apply the findings universally. Such studies are interesting and important in their own right, but they do not put us any closer to understanding regularities in human behavior.

Binford and others were only warming up. Schiffer (1981:905) had no idea in 1981 how prophetic he was: “The Binford-Yellen debates foreshadow much that lies ahead in ethnoarchaeology.” Within four years serious debates ensued between Watson and Gould over the role of analogy in archaeology (Gould and Watson 1982; Watson 1982; see also Gould 1980a, 1980b) and between Gould and Binford over not only the role of empiricism in ethnoarchaeology but also the proper and gentlemanly method of argumentation (e.g., Binford 1985; Gould 1985). The debate between Gould and Watson is more

enlightening from several standpoints, not least of which is a clearer statement of positions. Basically the issue is, as Watson (in Gould and Watson 1982:363) states, whether one can legitimately employ "a procedure that centers on testing to confirm or disconfirm the fit between hypothesized relationships (based ultimately on analogy with living systems) and the empirical reality of the archaeological record."

Watson (1982:445) contends such a position is possible: "I claim that the 'dead' materials of the past are always interpreted explicitly or implicitly on the basis of 'living' materials of the present. . . . We compare forms taken from the archaeological record with present forms, functions, and processes . . . and then make more or less confirmed claims about past functions and processes."

Gould's position, on the other hand, changed radically between 1978 and 1980, though we have never seen this fact pointed out. In his introduction to *Explorations in Ethnoarchaeology*, Gould (1978a:8) urged archaeologists to move beyond a search for rules in particular societies to a search for laws of behavior (see also Gould 1978b). Later, in *Living Archaeology*, he argued that the search for laws should be abandoned (1980a:39): "If we can put aside analogies and laws, with their uniformitarianist assumptions about how humans *ought to behave*, and instead explore methods that will help us find out how they really do behave, perhaps we will develop wider and more satisfying explanations." But even in *Living Archaeology* he sometimes referred to his "general principles" as laws (e.g., 1980a:140). One gets the feeling that Gould was trying to escape the notion of behavioral laws but was unsuccessful. Watson (1982), in her review of *Living Archaeology*, correctly noted that Gould's approach was the same as the one he rejected.

In replying to Binford's (1985) criticism of his approach, Gould (1985:642) presents his agenda for action in clear form: "By applying uniformitarian assumptions [derived from geology, biology, or other natural sciences] we can use empirical science to infer accu-

rately when non-material, cognitive, or ideational factors are affecting outcomes (the argument by anomaly) that differ from our predictions." He is equally clear on his choice of models for the archaeological study of behavior: "As anyone who has read *Living Archaeology* knows, I have found the field of astronomy to be a valuable source of illustrations and models for the empirical science of archaeology. This is no accident, since astronomy, like archaeology, is a science based upon inference. To a large extent, too, astronomy is a historical science, in which the explanation of past events must be based upon uniformitarian principles observed to be operating in the present" (Gould 1985:642). Contrary to Gould's view, however, astronomy—despite containing a historical component—is not a historical science. Paleontology, on the other hand, contains a historical component (certainly the paleontological record is historical in the same way the archaeological record is), but it also is a life science, the fossilized organisms embedded in the record having gone through the same evolutionary processes as those working today on living organisms.

Gould's interest in things biological is evident in several statements in *Living Archaeology*. For example, he contends (1980a:xi) that "human beings are not particles or inanimate entities whose behavior can be explained solely in relation to general laws like those used in the physical sciences." He also notes (1980a:89) that "many principles developed in evolutionary biology and ecology can safely be assumed to have operated uniformly in the past as they do in the present." We certainly second this observation. But then he goes on to ask, "Do we seriously doubt that because people, along with everything else in nature, are subject to the effects of gravity today, they have been subject to these same effects in the same ways at all times and everywhere in the past?" (Gould 1980a:112). Well, most people would not doubt this, but where does such an observation lead us? Why invoke evolutionary biological principles and then talk about gravity? What about selection, which is the

centerpiece of evolutionary theory? Has it, too, had invariant effects on humans? If so, then by now *Homo sapiens* should generally be extinct as a species or individuals should be clones of each other. There really are no other options.

In summarizing Gould's position, Wylie (1985:90) states that according to Gould, "the inference from present to past is thus mediated by well established and closely circumscribed — 'genuine' — uniformitarian principles (ones that have been firmly established in the natural and biological sciences) and it projects onto the past only those invariant regularities that exist in the biologically, physically constrained dimensions of human behavior." But Gould mentions few "invariant regularities" other than gravity, which hardly qualifies as a biological principle. Wylie (1985:90) comments further on Gould's philosophical tack but offers no criticism of it: "It seems that Gould's ambition of achieving a nonanalogical mode of understanding . . . can be achieved in those rare limiting cases where the reconstructed behavior is, by nature, a direct and exclusive consequence of impinging ecological or material conditions. Here, complete explanatory closure is realized; improvements in the background knowledge supporting interpretive inferences raise them to the level of deductive security."

We could not disagree more with Wylie's statement that "complete explanatory closure is realized." What has been explained? How many cases actually exist in which the environment is so restrictive that only a single response can be effected? One could, we suppose, imagine a group of humans wearing nothing but leopard skins and carrying no provisions trying to colonize the Arctic. One could also successfully predict the outcome, without having to rely on an evolutionary framework for the prediction. Unfortunately for the archaeologist, such cases are rare. Vague references to "biological principles" do not put one's research on solid footing, nor do they lead to satisfactory explanatory closure except in the most unusual of circumstances. In the end, we are con-

fronted with a dilemma: "uniformitarian principles" of human behavior do not exist. Rambo's (1991:91) rather blunt assessment of the error committed by cultural evolutionists is relevant to ethnoarchaeology and the search for laws of behavior:

Looking at the observed evolutionary trajectory, they have tended to assume that because a particular sequence occurred, it of necessity had to occur. This has led to concentration on the search for the "causes" of specific cultural evolutionary events (e.g., the origin of agriculture, the origin of the state). But the course followed by cultural evolution, like that of biological evolution, is indeterminate. The outcome we observe is the result of a multiplicity of stochastic events. It happened only because all of these events occurred as they did. It would have happened differently if they had not. The search for "laws" of cultural evolution is thus ultimately futile.

Ironically, Gould's work on the material culture of the Aborigines of the Western Desert of Australia (Gould 1980a) produced many important "engineering" data useful in understanding functional differences among tools. Many of his observations can be used to frame testable propositions that have empirical consequences. In short, we have the kinds of data, expressed in the kinds of units, necessary to begin to examine the effects of evolutionary processes on the Aborigines. This is the logical conclusion to Gould's approach, not the point at which to begin a search for universal principles of behavior. One can empathize with Gould in his empirically driven search for a middle ground somewhere between behavioral principles and behavioral laws, but the truth is that no such middle ground exists.

Wylie (1989c:26), however, believes that although no single proposal for escaping the "interpretive dilemma" in which archaeology has found itself can succeed, when taken together, multiple approaches "comprise a viable strategy for building and evaluating theoretically rich interpretations of archaeological data that do not reduce to arbitrary

speculation." We admire her optimism but do not share it. Covering laws, or rules, or principles do not and cannot exist for the simple reason that behavior changes, and it changes in ways that cannot be predicted; thus no combination of archaeological strategy and appeal to philosophical wisdom can ever construct such laws. How can we expect to construct laws—behavioral or otherwise—if no constants exist?

Lest we be misinterpreted, note that we are *not* saying that *no* relation ever existed between behaviors and materials, since obviously archaeological materials were produced by humans exhibiting behaviors. We are saying that no *deterministic* (universal) relations ever existed. No amount of shifting back and forth between source-side and subject-side research can ever hope to escape the dilemma of trying to determine when and under what conditions certain behaviors will occur, or of using, in a retrodictive sense, universal laws (or principles) to ascertain why certain behaviors did occur.

What ever led us to suppose that a deterministic relation did exist? The answer to this fundamental question is found in the very ontological position—essentialism—implicitly held sacred by archaeology. And it is this position that evolutionary archaeology attempts to replace. Archaeologists' preoccupation with the philosophy of science may have made them more aware of how philosophers think, but the emphasis was on the wrong kind of science.

ESSENTIALISM AND MATERIALISM

Dunnell, in several articles (e.g., 1980, 1982, 1985, 1989a, 1989b), has continually emphasized the two major ontological positions available to science, noting archaeology's preoccupation with essentialism as opposed to materialism. We extended this argument (O'Brien and Holland 1990), but we feel compelled to make it again here. Contrary to comments we have heard, the distinction between the two positions relative to archaeology is anything but a red herring. Archaeology, or any other field of inquiry, cannot hold positions in both camps. Either one

holds to the tenets of materialism or one holds to those of essentialism; one cannot do both. Indeed, part of archaeology's interpretive dilemma stems from attempts to hold to both, though when the chips are down essentialism ends up carrying the day. Thus behavior becomes an inherent—essential—characteristic of humans, and regardless of space or time, this essential characteristic can be called upon to serve as an explanatory device. All that remains to be accomplished is to specify some boundary conditions under which certain behaviors will automatically become activated. This premise, we contend, underlies the search for laws of behavior.

Essentialism, as coined by Popper (1950), is an ontological position that views reality as a unified system of bounded phenomena—things—that can be defined regardless of time and space. Any statement made about relations between and among units within a set of entities must, by definition, be universally true, since the entities themselves are timeless and spaceless. Change is viewed strictly in terms of conditionally reversible transformations; the essentialist perspective focuses on the replacement of one form by another or on the transformation of one form into another (Hull 1965). Criteria for group cohesion—that is, criteria for inclusion in one set (kind) or another—are based on shared characteristics; thus the phenomenological world is composed of a series of discrete entities, variation among which is viewed as explanatory but variation within which is viewed as noise. Dunnell (1988:16) notes that "in short, *kinds are empirical*. This view of the nature of reality spawns a particular suite of methodologies which have as their principal objective the segregation of observed variation into significant and non-significant kinds in order to extract the essential (hence essentialism) nature of kinds from observed variation, usually by the pursuit of central tendencies. Kinds are explained. Variation is rendered as difference" (emphasis in original). Mayr (1963, 1976; see also Sober 1980, 1984) coined the term *typological thinking* to refer to the essentialist perspective in biology (e.g., Kitts 1984;

Kitts and Kitts 1979; Schwartz 1981), which views species as “natural kinds.”

In contrast, the materialist, or populational, perspective (Ghiselin 1974, 1981; Mayr 1976, 1977, 1987; see also Dobzhansky 1951) holds that “only individual phenomena have reality and that every endeavor to infer from them an essence is a process of abstraction” (Mayr 1976:12). Thus reality is not viewed as a unified system; “things” do not really exist but are in a state of flux—always becoming something else. In other words, “relations between phenomena are not timeless, nor can universal statements be made about the relations, because no static set of phenomena exists. Space and time are kept separate, and relations between phenomena are space- and timebound. Under this view, ‘kind’ is not empirical, though at any given moment in time and space we can create kinds based on observation” (O’Brien and Holland 1990:37). If the latter statement were not true, we would doom the study of evolution to the metaphysical realm (Dunnell 1988, 1992). In short, under a materialist viewpoint kinds do not exist in any empirical sense but rather are consequences of observation. So long as analytical boundaries are held constant, variation between and among objects is rendered as change.

Where does cause fit into essentialist and materialist perspectives? Under the former, cause is viewed as a process stemming from inherent properties of the “things” being examined. Analytical interest shifts automatically to questions of “how” because we already know “why” certain things take place. In the study of humans we can invoke the concept of culture to explain the “why.” The empirical side of the inferential equation—the data—is used to examine or, usually, to bolster the nonempirical side of the equation. In Watson’s (1979) terms, the researcher uses data from the source side to examine propositions based on inferences from subject-side phenomena. Anthropologists and archaeologists alike would argue that what they seek are answers to “why-type” questions, but we contend that what they seek are *not* questions of ultimate causation. The ultimate

cause, in these instances, is already known: it is human intention or, alternatively, some vaguely defined “adaptive process” (O’Brien and Holland 1992).

On the other hand, materialism views cause in terms of the mechanics involved—mechanics that are external to the objects under examination. Materialism ties explanation directly to the outside mechanics that are at work on inherent variation. Thus, inferences concerning the nature of change can be made only after we have identified and measured variation. By definition, explanations of change are, under the materialist perspective, historical in nature (Dunnell 1982: 9). We reiterate that cause is external to the things being examined.

Anthropology in general and archaeology in specific have long traditions steeped in essentialism. A variety of “things”—objects, cultural-temporal units, and sociopolitical units—are lumped or split on the basis of perceived similarities. Once this operation is completed, change is documented by observing how one form is transformed into another. In a very real sense, the archaeological record is viewed as a precursor to the ethnographic present. Despite the considerable literature that has grown up around the question of how ethnoarchaeology can best serve the needs of archaeological inference making, we are left with the inescapable fact that generalizations weaken considerably when pushed back in time because the individual phylogenetic histories to which all organisms, here humans, are tied become obscured, if not lost (O’Brien and Holland 1990:39). No amount of posturing and no number of appeals to the philosophy of science will change the fact that modern, ethnographically studied groups are our phylogenetic *cousins*, not our temporally displaced *ancestors*.

Archaeologists today, like the Bureau of American Ethnology archaeologists of a century ago, have been slow to realize this dilemma because of a strict adherence to an essentialist framework. For example, Gould (1985:643) states: “My earlier argument by anomaly should be viewed as nothing more

than an empirically-satisfying way of moving from one kind of archaeological reality to another, just as, in astronomy, perturbations occurring in the observed behavior of phenomena in space occurring in relation to accepted laws of mass and motion lead one to infer the measurable effects of other, less well understood but equally discoverable, phenomena that may operate according to different principles." Thus it was entirely consistent under such a framework for archaeologists, when confronted with the self-imposed need for laws, or principles, to turn to empiricism for the answers: "Inasmuch as the philosophy of science developed around the 'hard' sciences and archaeologists did not conceive themselves as inventing a new kind of science suited to their historical questions, the main thrust of archaeological effort was to reorganise archaeology so that it was compatible with physics-based models. . . . The role of laws would seem to have come into prominence because, as universally true, atemporal statements, they overcome the problem of temporal access by making time irrelevant" (Dunnell 1989a:6). The veracity of Dunnell's comment is evidenced by Gould's (1980a:41-42) asking, "Wouldn't we be better off to state general *propositions* about human behavior which posit relationships that are invariable in time and space?"

Simply put, Hempelian science is totally unsuited to the study of organisms because of its inability to offer insights into the ultimate cause of change—unless one subscribes to the belief that species are "natural kinds," each of which has an essence, and that change from kind to kind is transformational. Biologists have long recognized that although life histories of organisms are in one sense *structured* by empirical laws—chemical and physical laws, for example—they are not *governed* by empirical laws. The one law that does apply to organic evolution is the law of contingency: what happens at any point in an organism's life or within a lineage's evolution is contingent on but not determined by what happened at previous points. Archaeologists, however, have unwittingly adopted a philosophy of science that

deals exclusively with the physical-chemical side of the world, not the life-history side. This adoption has, as we pointed out above, been made in the face of statements to the contrary.

Importantly, this issue should not be confused with the issue of whether the sense-making scheme used in archaeology should be science or historicism (Bamforth and Spaulding 1982). The issue is, rather, what type of science should be employed. Methods of inquiry spawned from the liaison with physical laws has led archaeology deeper and deeper into a dilemma from which it has yet to extract itself, though, like an unlucky gambler, it keeps investing more time and resources in an attempt to recoup its original stake. Our opinion is that an evolutionary archaeology—an archaeology centered on the notion of selection but which also clearly identifies the roles played by other evolutionary mechanisms such as mutation and drift (Dunnell 1978; O'Brien and Holland 1990, 1992)—offers an opportunity to recover, but to do so will require not only a complete change in perspective but also a reexamination of behavior as a topic of archaeological concern. In our opinion, time would be better spent examining specific behaviors as well as the *effects* of behaviors on humans, as derived through analysis of material remains, rather than focusing on a search for behavioral laws.

With regard specifically to humans, the other way Hempelian science might work is if one would subscribe to the strange notion that somehow humans and their behaviors have escaped the effects of selection. If such is the case, then a physics-based, essentialist model of reality becomes more appropriate as an approach to understanding human behavior. The essentialist perspective would work here because, like atoms and other phenomena of interest to chemists and physicists, humans would behave the same regardless of time. For example, carbon atoms have an essence and hence no developmental history. They behaved yesterday as they behave today and will behave tomorrow. If four hydrogen atoms are linked to a carbon atom,

methane is formed, regardless of when or where the reaction takes place. This statement was as true a hundred years ago as it will be a hundred years from now. Regardless of our level of understanding of atomic-level phenomena, and regardless of whether we can actually see the arrangement of atoms, we know the statement holds. It must, lest the foundations of chemistry fail.

This is an example of the timeless-spaceless element of empirical science. If humans *have* become more atomlike, because we now are immune to mutation, selection, and drift, then all we have to do is search for boundary conditions in order to establish lawlike propositions regarding behaviors. After all, some chemical reactions are sensitive to energy inputs (e.g., photosynthesis); likewise, under the essentialist view of humans-as-kinds there should exist basic environmental parameters that condition human responses. If we can isolate the parameters, then we can predict behavioral outcomes. Unfortunately, at least from the archaeologist's standpoint, humans do not behave like atoms and cannot be treated in an essentialist manner. The next question becomes, can evolutionary theory accommodate humans within its framework, or do we have to construct a separate theory?

EVOLUTIONARY THEORY AND ITS APPLICABILITY TO HUMANS

Several corollaries stem directly from a reliance by archaeologists on essentialism as a perspective—corollaries that very much influence how we examine human behavior. Perhaps the most significant corollary is the inapplicability of evolutionary theory to the study of humans. This perspective, in essence, views humans as entities that somehow have been removed from the evolutionary process because of the intervention of culture. In other words, natural selection and other processes of biological evolution brought hominids to a certain point, at which time the evolutionary processes relinquished control to culture. The anthropological literature is replete with arguments over the nonapplicability of evolutionary

theory to humans—a view that is based on confusion, misinformation, misreadings, and above all on poor applications of the theory by proponents of a Darwinian-evolutionary approach. The arguments, to one degree or another, revolve around a basic tenet and its corollary: cultural evolution is not analogous to genetical evolution because the modes of trait transmission are different. Therefore, any analogy between biological evolution and cultural evolution is contrived.

Few anthropologists argue that the *propensity* for culture (however defined) is not genetic; rather, they argue that once hominids developed the capacity for culture, this newly emergent property took on a life of its own and became uncoupled from genetic control. We agree with Bonner (1980:19) that “it is only by making a clear distinction between genetical and cultural change that we shall ever be able to understand the causes and the mechanisms of change in any organism capable of both cultural and genetical evolution,” but in doing so we have to be careful not to throw the proverbial baby out with the gene pool. When attempting to understand humans and their behaviors, we do not have to throw genes and culture into a blender, but neither do we have to take the position that evolutionary theory is inappropriate to the study of humans. Such an error is made when one confuses biological *mechanisms* with evolutionary *principles*. The point is that biological mechanisms such as genetic heritability are *not* the same as evolution by natural selection. All genetic heritability does is ensure that the variation created in one generation can be transmitted to succeeding generations. This is not to downgrade the role played by heritability, but only to link it with its proper function.

Several studies have carefully separated the mechanisms of evolution from evolutionary processes and have attempted to determine how genetic and cultural traits could coevolve, each through *separate* mechanisms. Boyd and Richerson (e.g., 1985), for example, propose a “dual-inheritance” theory of the human evolutionary process to “emphasize that the potentially novel

effects of culture result from the fact that the determinants of behavior are assumed to be transmitted via two structurally different inheritance systems" (Boyd and Richerson 1985:2). Cavalli-Sforza and colleagues (e.g., Cavalli-Sforza and Feldman 1981; Cavalli-Sforza et al. 1982; Feldman and Cavalli-Sforza 1976), on the other hand, examine changes in the phenotype brought about by learning skills, which in turn would affect the selection of specific genotypes. Indeed, Cavalli-Sforza and Feldman (1981) coined the term *cultural selection* to refer specifically to models of biased cultural transmission as opposed to biological transmission. Unfortunately, the term confuses the issue because it creates an unnecessary distinction. Transmission mechanisms vary and should be kept distinct for some kinds of analysis, but this separation belies the fact that selection is blind to the source of variation. Once heritable variation, of whatever form, has been generated, by whatever mechanism, it is fair game for selection. We need not divide selection into kinds (e.g., natural selection, sexual selection, and cultural selection) in order to understand either the process or its outcomes.

SOCIOBIOLOGY AND FITNESS MAXIMIZATION

A significant portion of the anthropological backlash against the application of evolutionary theory to the study of humans is actually a backlash against sociobiology, a catch-all term for various studies putatively demonstrating a genetic basis for specific behaviors. No doubt human sociobiology has fallen short of the mark, much of it consisting of contrived applications of "Darwinian" principles to human behavior. Most such studies deserve the criticism levied at them, for reasons Rambo (1991:61) mentions: "The continued resort to reductionist explanations by sociobiologists suggests that they do not recognize the incredible diversity that characterizes culture. Biology can possibly explain the pan-human capacity to par-

ticipate in cultural systems. It can also suggest why certain universal patterns of belief and behavior characterize our species. But biology does not provide explanations for the development of cultural differences—the origin of cultural 'species'—that is the central question in anthropology." We assume Rambo uses the term "biology" here as shorthand for biological *mechanisms*.

We have no disagreement with the view that culture is not controlled genetically. Indeed, to us it makes no difference even if it were. What does make a difference is that evolutionary principles are being confused with biological mechanisms. Biological mechanisms, such as genetic inheritance, are not the same as evolutionary processes, though even Darwin, who was ignorant of Mendel's breakthroughs, realized the importance of particulate inheritance. The important point is that particulate inheritance—what we have come to know as genetical inheritance—is only one of several means by which things get transmitted. Why should we be surprised that Sahlin's (1976a), for example, found a lack of correspondence between kinship rules among several human societies and expectations derived from Hamilton's (1964) argument for inclusive fitness? Hamilton formulated a mathematical expression that modeled organisms' actions *if and only if* they maximized their inclusive fitness. He never said all organisms maximized their inclusive fitness, and he certainly never stated that humans do. Why should Betzig (1988:6; see also Betzig 1989) be chagrined to discover that modern cultures do not conform to predictions of "a Darwinian view of human behavior"? Darwin never formulated a comprehensive view of human behavior, despite a long-standing interest in the subject (Barrett, Weinshank, and Gottleber 1987; Gruber 1974). Rather, he concentrated on showing that behaviors in humans had their roots in phylogenetic ancestors—an interest that became the focus of *The Expression of the Emotions in Man and the Animals* (Darwin 1872).

There simply is nothing inherent in evolu-

tionary theory that states that organisms *must always act in accordance with some maximizing strategy*. As Darwin himself figured out, no such thing as a perfectly adapted organism exists. Adaptedness (fitness) has to be examined in terms of *relative* adaptedness (O'Brien and Holland 1990, 1992, 1995). As we like to quote, "selection cannot produce perfection, for in the competition for reproductive success among members of a population, it is sufficient to be superior and not at all necessary to be perfect" (Mayr 1982:589). What we need to remember is that "individuals do not consciously strive to maximize anything; they behave *as if* maximizing something. . . . Individuals may strive for something, but it will be a morsel of food, an attractive female, or a desirable territory, not inclusive fitness" (Dawkins 1990:188-89).

This simple, elegant statement should eliminate once and for all a narrowly defined and completely illogical emphasis on realized fitness, which, as Burian (1983:299) points out, is not what Darwin had in mind: "Darwin almost certainly meant the phrase 'survival of the fittest' to stand for the *tendency* of organisms that are better engineered to be reproductively successful" (emphasis added). In other words, "If *a* is better adapted than *b* in environment *E*, then (probably) *a* will have greater reproductive success than *b* in *E*" (Brandon 1990:11). We term this success *expected fitness*. Note the use of the word "probably." Darwinian evolution does not say that organisms—human or otherwise—will maximize their fitness. They may *act* as if they are maximizing their fitness, but all the theory says is that some organisms will do better—in reality, their gene lines will do better—than other organisms and they will do so because of superior (not perfect) design. And that is *all* the theory says. Williams (1966:159) sums up the position neatly: "Measuring reproductive success focuses attention on the rather trivial problem of the degree to which an organism actually achieves reproductive survival. The central biological problem is not survival as such,

but *design for survival*" (emphasis added).

Nowhere in *On the Origin of Species* (Darwin 1859) is there mention of humans acting to maximize their fitness (inclusive or otherwise). Enough counterexamples to such a weird interpretation now exist to fill several volumes. No one, to our knowledge, has claimed that behaviors behind such things as pot making were conscious or unconscious attempts on the part of prehistoric potters to maximize their fitness. Similarly, no one has claimed that better-engineered pots (better in terms of several quantifiable dimensions [e.g., Schiffer and Skibo 1987]) necessarily led to an increase in realized fitness. What has been claimed (e.g., Dunnell and Feathers 1991; O'Brien and Holland 1990, 1992; O'Brien et al. 1994) is that it is possible archaeologically to demonstrate that users of superior pot-making technologies were *potentially* more fit than those using other technologies. Thus, some pot-making behaviors may, in both an expected- and a realized-fitness sense, confer differential fitness on the users.

But this type of engineering-design analysis is only the first step. It remains to be demonstrated whether and, importantly, how the technologies affected fitness. In other words, detailed engineering studies of technologies allow us to detect changes and to determine whether certain technological products came under selective control—that is, became adaptations—but this hardly informs us about why the products changed or whether the changes actually caused humans to reproduce differentially. As we discuss elsewhere (O'Brien and Holland 1992; O'Brien et al. 1994), the next step, once adaptations have been identified, is to link engineering data to other aspects of the archaeological record over which there exists tight temporal control. For example, data from Woodland-period sites in the central Mississippi River valley indicate that ceramic-technological change, in concert with other technological and social changes, had significant impact on the reproductive success of human groups residing in the area (O'Brien and Holland

1992; see Buikstra, Koningsberg, and Bullington 1986; Holland 1989; O'Brien 1987).

INTENTION AND RATIONAL BEHAVIOR

Sociobiology has been attacked on the valid grounds of reductionism, but unfortunately the waters have been muddied by other, ill-founded criticisms as well. Boyd and Richerson (1985:132) note that for many anthropologists sociobiology fails as a theory of human behavior because it does not afford a role to rational behavior: "Even Marshall Sahlins (1976[a]) and Marvin Harris (1979), who concur on little else, agree that it is through the invention of cultural variants and the choice among existing ones that genetic imperatives are transcended. In our terminology, these authors are arguing that human behavior cannot be predicted by sociobiological theory because the forces of guided variation and biased transmission dominate cultural evolution." What a strange argument for Harris and especially for Sahlins to make! Sahlins castigates sociobiology for its claims of universal behavior based on tenets of Darwinian evolution, but who among anthropologists is a bigger supporter of cultural regularities in the form of "universal kinds" of sociopolitical organization?

As Mayr (1973:388) notes, behavior is "perhaps the strongest selection pressure operating in the animal kingdom." He also argues (1982:612) that "many if not most acquisitions of new structures in the course of evolution can be ascribed to selection forces exerted by newly acquired behaviors (Mayr 1960). Behavior, thus, plays an important role as the pacemaker of evolutionary change. Most adaptive radiations were apparently caused by behavioral shifts." Mayr's statements are important because they emphasize that behavior is an *agent* of change rather than a recipient of change. No one seriously interested in applying selection theory archaeologically has ever argued that behavior, including intention, is not an extremely important source of variation. Humans are thinkers and doers, and their be-

haviors spawn an almost infinite array of variation, resulting in part from differences in perception and intention. But analytical importance is placed on variation, any form of which is ripe for selection, and not on intent. As Steward (1956:72) stated, "a specific invention is not explained by saying that man is creative." Such an "explanation" is nothing more than an appeal to simple directionality. Unless one subscribes to the notion that culture, however defined, is purely Lamarckian (see Rindos 1989), such a view is untenable. To invoke intent as an explanation of anything answers interesting questions—how and why things got to be the way they are—with trivial responses.

Our opinion parallels Rindos's (1984:4): "Man may indeed select, but he cannot direct the variation from which he must select." This statement, of course, can be read incorrectly to mean that humans have no choice in matters, being merely organisms that are stuck with a series of limited, preselected options. What we mean is that humans are one agent of change, but the way they go about effecting change is not unlike the manner in which natural selection operates. Indeed, the *manner* of selection is unimportant at this point. Consider the selection of one species of butterfly over another because of its bright color: "The seemingly capricious (but patterned) choice by butterfly collectors of a moth of one color over another is as potent an agent of selection as is the choice by any biologically driven bird. In both cases the moth met a less-than-happy end, and the composition of the gene pool to which the moth contributed is adjusted accordingly" (O'Brien and Holland 1990:57). The important point is that variation exists—variation generated by whatever means—and that selection, of any kind, acts on that variation. And that is all that matters.

EXTENDING THE PHENOTYPE

The last issue we examine is the way in which the phenotype has been conceptualized in anthropology. Our position is that it has been defined too narrowly, almost to the

point where it is viewed as a complete one-to-one expression of the genotype. Or, where it has been expanded, this expansion includes elements of behavior but little else. It is this restricted notion of phenotype that perhaps has been archaeology's biggest stumbling block relative to incorporating a selectionist perspective. Ironically, anthropologists, probably because of lack of grounding in biology, tend to regard the phenotype as composed of strictly biological features. Some might include general behavioral characteristics within the realm of the phenotype, but such an attribution is based solely on the belief that humans can intentionally modify their behavior as an automatic adjustment in the face of physical or social environmental change. In other words, they automatically adjust (adapt) to new situations posed by the social and physical environment.

Recall our earlier discussion of essential characteristics versus properties. Given that humans are not timeless and spaceless entities and that behaviors change dramatically through time in the face of environmental stimuli, how could behavior be thought of as being an essentialistic property? If behavior is nonessentialistic, then the products and by-products of human behavior likewise are nonessentialistic. Importantly, both behaviors and products of behavior are phenotypic properties, neither of which—and this is especially important to recognize for the products produced by behaviors—is simply reflective of or related to past phenotypes (Dunnell 1988:23). We state elsewhere (O'Brien and Holland 1992:37) that objects in the archaeological record "are parts of phenotypes in the same way beaver dams and bird nests are parts of phenotypes." As such, the objects potentially can contribute useful information regarding adaptedness and adaptation (see O'Brien and Holland 1992)—information that is just as significant as that gained from an analysis of purely biological features.

We see no a priori reason why human skeletal remains, for example, should be slotted into an analytical framework wholly at

odds with the one applied to grave goods interred with a body. The human body throughout its phylogenetic history has been shaped by selection and thus is an adaptive response (though attribute states of certain components probably are neutral). Pots also are adaptive responses (O'Brien and Holland 1990, 1992; O'Brien et al. 1994). The difference between the body and associated grave goods lies solely in the degree to which the phenotypic expressions—the body and pots—are encoded within the genotype. Are we so naive as to assume there are genes for making pots? Of course not, though conceptually our argument would not change even if there were. Recall that earlier we made the point that one needs to keep separate the individual mechanisms of transmission—biological versus cultural—from the process of evolution. Chromosomes carry instructions for making a human body, a portion of which, the brain, has been under continuous selective pressure for tens of millions of years. Brains impart to humans the behavioral ability to create objects, some of which serve to further the protection of the vehicle carrying the hard-wired recipe for replicating itself and some of which do not. Regardless, the objects are phenotypic expressions. If they serve to protect the vehicle and because of this role have come under selective control, then they should be considered adaptations (O'Brien and Holland 1992).

We believe strongly that many of the misconceptions that have arisen over this position have resulted from the mistaken belief that for this to be true then somehow the actual behaviors that create objects such as pots must necessarily be genetically encoded. This is nonsense. If we ignore for a moment the genetic component of behavior and focus exclusively on the postembryological role of behavior, perhaps we can clear up the matter. Phenotypic behavioral responses are conditioned in part by genes and in part by environment. For the sake of this discussion it does not matter how much each contributes. No one would argue against the notion that individual behaviors, acting in concert with physical and social dimensions of the envi-

ronment, sculpt and remodel the human body during postembryological development. In like manner, objects created by humans change constantly as a result of behavioral changes. The point is simply this: we do not need to invoke the genotype exclusively when talking about phenotypic characteristics, nor do we need to invoke genetic transmission when we talk about phenotypic change. Behavior has everything to do with both the somatic parts of the phenotype and the "material" aspects of the phenotype. Unfortunately, phenotypic behavior is not fossilized in the archaeological record.

We admit that at one time (O'Brien and Holland 1990) we were ambivalent over whether one needed to consider objects themselves as parts of human phenotypes or whether one could simply subscribe to the notion that the behaviors and intentions that created the objects were parts of phenotypes (see Boyd and Richerson 1985:36; Geertz 1973:143-46). Unlike the road to hell, the evolutionary pathway is not paved with good intentions. Humans, or any other organisms for that matter, are products of evolutionary processes, the most important of which probably is selection. Selection cannot act on the intangible; intent must be transformed into some physical reality before evolution can take place. Similarly, behavior must have a physical consequence if it is to enter the selective process. Birds, for example, did not evolve because of egg-laying behavior but rather because they produced a physical consequence—an egg—that could either break too soon, resulting in the death of the embryo, or break when the hatchling was viable in the outside world. Consider the plight of the bald eagle and other birds in the 1950s and 1960s. DDT in the food chain resulted in fragile eggs that all too often broke before hatching. The birds faced extinction not because of a change in their egg-laying behavior but because of selection against thin-walled eggs.

Therefore, to clear up any misunderstanding and to bring our thinking in line with the views of many contemporary biologists, we re-

cently (O'Brien and Holland 1992) emphasized that both the objects *and* the behaviors are phenotypic. We believed at the time that this was a noncontroversial view, but we have since been proved wrong. Judging from conversations and comments from reviewers and readers, the issue of what is and is not phenotypic is anything but settled among archaeologists. Those whom we might call "behavioral archaeologists" have few problems with the attribution of behavior as phenotypic; rather, the controversy centers on calling products and by-products of behavior phenotypic.

But why *not* label the products of behavior phenotypic? Such a proposal is common in biology (e.g., Bonner 1980, 1988; Dawkins 1990), but it has rarely been applied to humans. Reasons for this lack of application are unclear, though we suspect they are tied to the beliefs that phenotypes are controlled genetically and that humans are not subject to selection because our ability to reason—human intention—has severed the link. Therefore, material remains, though viewed casually as adaptations, are really intentional products constructed solely to adjust humans *in a directed sense* to the environmental stresses they face.

These views present insurmountable barriers to acceptance of evolutionary theory in archaeology. So long as humans are viewed as being above much of nature's reach, and so long as human intentions are substituted for theory (Dunnell 1982), we cannot escape the interpretive dilemma mentioned earlier. And yet every reason exists to bypass emphasis on intention and to focus on phenotypic change in human organisms. There is something intrinsic in the archaeological record—material remains—that gives us access to the historical pathways taken by human individuals and groups. By logically extending our commonly held notion of phenotype, we gain access to those pathways. In other words, our goal becomes to explain why certain functional features (adaptations) occurred when and where they did. And if we accept the notion that selection works on variation, then the appropriate procedure is

first to document that variation—importantly, phenotypic variation upon which selection can act—exists.

Archaeologists, like paleontologists, have access to a historical record of exactly when and where that variation occurred. But archaeological data rarely are cast in terms of units that can be explained by evolutionary theory (Dunnell 1980, 1988; Leonard and Jones 1987; O'Brien and Holland 1990, 1992). Before gains can be made, new data will have to be generated in terms of units that highlight rather than suppress variation. This in turn, as Dunnell (1987) suggests, will necessitate a change in the character of research designs and fieldwork. But at an even more fundamental level, the broadly held (at least among anthropologists and archaeologists) notion of phenotype will have to change.

No one has ever found a gene or series of genes that control how a beaver builds a dam or how a spider spins a web. But never having found such genes does not destroy the logical proposal that such activities are controlled genetically, given what we know about behavioral genetics. If one accepts the notion that individual organisms act as vehicles for replicative units—genes—then the vehicles to a certain degree must do their job of protecting (and propagating) the germ-line replicators. Otherwise, the germ line dies and that's the end of the story. Nature has shaped an almost infinite number of vehicles, some of which are better than others in a relative sense (one organism's superior vehicular design allows it to outpropagate that of another), and some of which are more or less equal in terms of getting the job accomplished.

Our point is that if certain genes control the formation of "bodily" portions of the phenotype, and those portions protect the germ-line replicators, then the genes that control nest-building behavior likewise can be considered as producing protection for the germ line. The logic is identical. Dawkins (1990:198) makes the same argument:

The house of a caddis is strictly not a part of its cellular body, but it does fit snugly

round the body. If the body is regarded as a gene vehicle, or survival machine, it is easy to see the stone house as a kind of extra protective wall, in a functional sense the outer portion of the vehicle. It just happens to be made of stone rather than chitin. Now consider a spider sitting at the centre of her web. If she is regarded as a gene vehicle, her web is not a part of that vehicle in quite the same obvious sense as a caddis house, since when she turns round the web does not turn with her. But the distinction is clearly a frivolous one. In a very real sense her web is a temporary functional extension of her body, a huge extension of the effective catchment area of her predatory organs.

We extend this argument by considering the remains of a mud-dauber's nest and a fragment of daub from a Mississippian period wall-trench house—both recovered from identical archaeological contexts. Modern biologists would have no difficulty in dealing with the dauber's nest within the framework of the extended phenotype. Yet many archaeologists will feel compelled to introduce some interposing behavioral analog to explain the human-constructed house remains. If, as Dawkins (1990) and others argue, the step from a genetic basis for morphological development to a genetic basis for behavior is conceptually negligible (and we agree it is), then the step from behavior to extended phenotype—here mud-daubers' nests and Mississippian houses or caddis stone houses and spider webs—also is negligible.

CONCLUSION

Archaeology's strength lies in the material world. It is a historical-material science predicated on tangible remains of the past. All we possess as archaeologists, and all we can ever hope to recover from the past, are objects. The difference between a behaviorist and an evolutionist perspective is not one of simply splitting semantic hairs over whether it is behavior or the products and by-products of specific behaviors that are of analytical interest. Past and continuing emphasis on

some gestalt vision of human "behavior" is not advancing the discipline. This does not mean that we cannot intelligently infer specific actions, or *behaviors*, from analysis of the objects, but it should be remembered that the reliability of inferences decreases rapidly as the behaviors are distanced from the physical world. Inferring butchering activities from analysis of cut marks on animal bone and examination of use wear on stone tools, for example, will always be on firmer footing than more removed inferences regarding kinship systems or political structure. This is the difference between strong and weak inference and is a topic worthy of considerably more attention than we can provide here. Our conservative view is that for the present we should confine inferences to those based on use-wear analysis, replicative studies, and engineering studies. Technological and functional inferences, which, it so happens, go hand in hand with an evolutionary approach (and, not coincidentally, with a behavioral-archaeological focus), should become the central focus of archaeological inquiry.

An evolutionary perspective encompasses the notion of an extended phenotype, whereby both behaviors and the products and by-products of behavior—archaeological objects—are viewed as phenotypic. Extending the human phenotype does not require abject surrender to the realm of sociobiology, nor does advocacy of an evolutionary approach imply that analytical interest is focused on maximized fitness. And, finally, evolutionary archaeology is not some thinly disguised cover for biological reductionism. Rather, the notion of the extended phenotype explicitly recognizes that humans, above all other types of organisms, maintain a vast reservoir of adaptations that are not controlled by purely genetic means. Like organic adaptations, features in the archaeological record will be best explained when we accept that humans are neither immune to nor exempt from selection and other evolutionary processes, despite our best intentions. The continued exclusion of modern humans from the process of evolution is a

position expected from dogmatic creationists, not from enlightened scientists.

Where does this discussion leave us relative to behavioral archaeology as it is espoused in its myriad forms? The chapters in this volume demonstrate that *behavioral archaeology* is not a unidimensional, monolithic term that can comfortably be applied anymore. Indeed, in her chapter, Wylie applauds behavioral archaeology for expanding its scope and moving beyond many of the "doctrinaire restrictions" that normally accompany "positivist" science. Further, Wylie believes that evolutionary archaeology is the true heir of the positivist approach from which behavioral archaeology escaped. As positivists (if that is what Wylie wishes to identify us as), we view her comments with a certain amount of irony, since it is the scientific and logical aspects of behavioral archaeology that we find so appealing.

We liked formation-process studies 20 years ago (and still do), and we applaud the introduction of replicative experiments into the study of such things as pottery manufacture. Our opinion is that much of the work carried out by self-proclaimed behavioral archaeologists is exactly the same kind of analysis any selectionist would undertake. Philosophers would recognize this if they focused more on data and less on epistemology. Even a casual reading of the chapter in this volume by Skibo and Schiffer suggests that they are as interested in the performance characteristics of clay cooking containers as we are. Indeed, the work of Schiffer and his students and colleagues (e.g., Schiffer 1990; Schiffer and Skibo 1987; Schiffer et al. 1994; Skibo 1992; Skibo, Schiffer, and Reid 1989; Vaz Pinto et al. 1987; Young and Stone 1990) has produced many of the ideas we now routinely incorporate into our discussions of how selection works on pottery-producing groups. The point of departure between our approach and, say, that of Skibo and Schiffer rests in their attempts to link the technological and functional aspects of ceramic vessels to things such as the gender of the producers of the ceramic vessels, whereas we would use those aspects to begin to exam-

ine the fitness of the producers. There is still a wide gulf between behavioral archaeology and evolutionary archaeology—and there always will be—but we have been pleasantly surprised by the overlap in the kinds of data produced by proponents of the respective positions.

ACKNOWLEDGMENTS

We thank James Skibo, Axel Nielsen, and William Walker for inviting us to participate

in the Society for American Archaeology symposium where the paper on which this chapter is based was presented. We obviously employ a different approach to examining the archaeological data, and their willingness to allow us to discuss our views in this book is greatly appreciated. We also benefited greatly from comments provided by Mike Schiffer, Alison Wylie, and Lee Lyman, all of which forced us to rethink several issues raised in the original paper.