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Evolutionary Archaeology

Reconstructing and Explaining Historical Lineages

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In the almost two decades since Dunnell (1980) published "Evolutionary Theory and Archaeology"—an article that Schiffer (1996:646) labeled as the "proximate roots" of evolutionary archaeology—an increasing number of archaeologists have begun to show an interest in using Darwinian theory to examine and explain variation in the things they study. Of course, it could be argued that archaeologists have long been interested in applying evolutionism to the study of change in the material record. If this argument is valid, why single out the last two decades for special treatment? The answer is straightforward: although archaeologists *have* long been interested in evolution, there are significant differences between modern archaeological interest in evolution and that of previous decades. The major difference is that the kind of evolutionism under discussion here—Darwinian evolutionism—represents a signal departure from the kinds of evolutionism in vogue in Americanist archaeology during the late nineteenth century and throughout most of the twentieth century (Lyman and O'Brien 1997, 1998; Lyman et al. 1997; O'Brien and Lyman 2000a).

Darwinian evolutionism traces its roots directly back to the publication of *On the Origin of Species* (Darwin 1859). It is a radical departure from the unilinear, progressive cultural evolutionism of Tylor (1871) and Morgan (1877) that figured so prominently in the writings of prehistorians of the late nineteenth century (O'Brien 1996a, 1996b). Likewise, it bears almost no resemblance to the cultural evolutionism of White (1945, 1949, 1959a, 1959b), Childe (1951a, 1951b), Steward (1955, 1956), and their followers (e.g., Sahlins and Service 1960), which played a comparably important role in the processual archaeology of the 1960s and 1970s. A major difference between these kinds of evolutionism and Darwinian evolutionism, at least as the difference applies in archaeology, is a shift in focus away from the evolution of culture per se and toward the evolution of particular and highly idiosyncratic cultural phenomena. It might be assumed that making such a shift would be easy, but it most decidedly is not. The difficulty stems from a deeply rooted tradition in anthropology, and by extension archaeology, that views culture itself and not cultural phenomena as the appropriate focus of study.

Americanist archaeologists now routinely recognize a brand of archaeology labeled variously as evolutionary or selectionist, recognizing further that its proponents claim a Darwinian intellectual heritage. Several recent articles and reviews (e.g., Boone and Smith 1998; Maschner 1998; Rosenberg 1990, 1994; Spencer 1997), however, have attempted to demonstrate that this claim is empty—that is, that evolutionary archaeology is based on faulty reading of the evolutionary-biology literature. We deny the veracity of such claims (Lyman and O'Brien 1998)

and view them as little more than caricatures written by persons who themselves apparently have not read, or at least not understood, the applicable archaeological and biological literature. These authors reject the kind of evolutionary analysis discussed here on the faulty belief that selection ceased to be an evolutionary mechanism relative to hominid evolution once culture was acquired. This is an indefensible position (Dennett 1995; Lerner 1959; Lyman and O'Brien 1998; Weiner 1994) but one that is widespread both in anthropology and biology (e.g, Angier 1997; Gould 1996; Huxley 1956; Simpson 1949). Other authors (e.g., Schiffer 1996) have argued that the basic tenets of evolutionary archaeology are not so far removed from those underlying approaches such as behavioral archaeology and that the best of these often complementary approaches should work together. We applaud any effort to expand the scope of evolutionary archaeology, although there are epistemological problems involved in some areas of proposed expansion (O'Brien et al. 1998).

Proponents of the evolutionary approach have not been as clear and concise as they should have been in setting forth what it is that makes evolutionary archaeology not only different from other paradigms but, more to the point, what makes it a different *kind* of approach from others in the intellectual marketplace. Darwinian evolutionary theory, especially in its modern (post-Synthesis) expression, is not something that can be picked up through a quick and easy read. If it were, then evolutionary biology would not have witnessed the various intellectual difficulties with which it has grappled over the decades—problems such as the ontological status of species, the nature of adaptation, and so forth. Such conceptual problems are solvable, although resolution has occurred but slowly. If, as evolutionary archaeologists have argued (e.g., Dunnell 1989a; Leonard and Jones 1987; Lyman and O'Brien 1998; O'Brien 1996a, 1996c; O'Brien, ed. 1996; O'Brien and Holland 1990, 1992, 1995a, 1995b; O'Brien and Lyman 2000a, 2000b), artifacts are phenotypic features—a point with which at least the leading behavioral archaeologist (Schiffer 1996) agrees—then it would appear that the archaeological record should inform us about the evolutionary processes that took place in the past. But this can occur only if we are able to sort out such things as homologous from analogous features—a point made by Kroeber (1931) seven decades ago. Such separation can be accomplished, although the process is tedious (Dunnell 1978; Lyman and O'Brien 1999; O'Brien and Holland 1990, 1992; O'Brien et al. 1994; O'Brien and Lyman 2000a, 2000b).

The purpose of this essay is to lay out what the tenets of Darwinian evolutionism are and, perhaps more important, what they are not. We use the discussion to correct several false impressions of evolutionary archaeology and to point out several future directions that the approach might take. It is not our intention to provide a nuts-and-bolts discussion of evolutionary archaeology and the myriad issues that have at one time or another been examined; good bibliographies on these topics can be found in O'Brien (ed. 1996), O'Brien and Lyman (2000a), and Teltser (ed. 1995). In fact, we focus rather narrowly on one key topic that if left unaddressed will undermine serious efforts to apply Darwinian evolutionary theory archaeologically. That topic, which must be at the core of any effort labeled Darwinian, is history—specifically, the development of historical lineages at various scales. As evolutionary archaeology matures, and we are confronted with more and more so-called Darwinian explanations, it will be imperative that we lay out in precise terms why the examination of historical lineages is so critical. Previous failure to do so has made it appear that an ecological approach is commensurate with evolutionary archaeology. Although ecology plays an important role in evolutionary studies, without history the resulting narratives are functional accounts of how things work. They tell us nothing about how certain features changed, nor do they tell us *why* features turned out the way they did. A scientific evolutionism has to answer both functional *and* historical questions.

Archaeology and Science

From the time they take their first course work, archaeologists are constantly reminded that although they study "stuff," that stuff was once part of vibrant cultural systems. They also are reminded, sometimes subtly, other times not so subtly, to keep their sights firmly on the "Indian behind the artifact," as Braidwood (1959:79), echoing Taylor (1948), put it. Despite these reminders, there are those anthropologists, such as Service (1964:364), who would also remind archaeologists of their proper place at the anthropological table: "The day-to-day work of an archaeologist (as an *archaeologist,* not as an archaeologist turned general anthropologist or philosopher) is to dig up the remains of peoples and their cultures, to map, measure, describe, count, and so on, and in his report to make an interpretation of what life was like 'then.'" In other words, although archaeologists should remember that they are digging up the remains of cultures, they should not even begin to think about providing explanations for things being the way they are. Anthropologists, not archaeologists, explain.

Archaeologists in the 1960s began to chafe under such strictures, preferring to think that maybe Service and anthropologists with similar views were referring to archaeologists of a day gone by—the culture historians who had labored to put things in proper time-space sequences but who, it was thought, had showed little interest in understanding the processes that shaped culture and hence the archaeological record. The crop of archaeologists that came along in the 1960s *was* interested in such processes, and its members were bound and determined to make archaeology an equal partner in the study of culture. To do so required that archaeology be made more anthropological. There was an alternative—to make archaeology a humanity—but given that Americanist archaeologists had long seen themselves as scientists (Lyman and O'Brien 1997; Lyman et al. 1997; Willey and Sabloff 1993), this option had little appeal.

But what was the best approach to take in attempting to make the archaeology of the 1960s more scientific than its predecessor had been—in fact, to make it so scientific that anthropologists such as Service would have to sit up and take notice? The answer was obvious: Turn the archaeological study of culture into a science, just as White (1949) had said could be done for anthropology in general. And because culture had evolved—White (1959a, 1959b) and his intellectual forebears such as Spencer, Tylor, and Morgan had demonstrated as much—evolutionism would naturally come along as part of the scientific package. The wedding between anthropology and archaeology that was supposed to create a scientific approach to understanding the archaeological record had actually been anticipated decades earlier (Dunnell 1982; Lyman and O'Brien 1997; Lyman et al. 1997; O'Brien 1996a, 1996b, 1996c), but the formal union did not take place until the early 1960s, when Binford (1962) published "Archaeology as Anthropology," in which he made an eloquent case for uniting the two fields into a common program. He followed that article up three years later with "Archaeological Systematics and the Study of Culture Process" (Binford 1965). At that point processual archaeology was born—an offspring of White's "culturology"—literally, the science of culture (White 1949).

The union between the two disciplines that took place in the 1960s, although hailed in some quarters as *the* replacement for the stale, old paradigm of culture history that had gripped Americanist archaeology since early in the century, actually vindicated numerous culture historians who had long assumed they were reconstructing past cultures (Dunnell 1982) and at the same time trying to understand something about the processes that had created those cultures. To be sure, some culture historians were little more than "trait chasers," but many others understood what culture is and how to study its manifestations archaeologically. Ford, for example, the chief architect of culture history in the southeastern United States (O'Brien and Lyman 1998), kept culture clearly in focus throughout his career. In one of his earliest ar-

ticles Ford (1935:8) stated that culture "is in reality a set of ideas as to how things should be done and made." He also pointed out, using wording typical of the period, that culture "is in a continuous state of evolutionary change since it is constantly influenced both by inventions from within and the introduction of new ideas from without the group" (Ford 1935:8). And what about Binford's call to make archaeology anthropological? This was nothing new; numerous culture historians had been calling for this for decades. Phillips (1955:246–247), for example, one of Ford's contemporaries, was the author of one of the more memorable lines in Americanist archaeology: "New World archaeology is anthropology or it is nothing"—a quote with which Binford (1962) had no quarrel. Phillips might have penned the quote, but he was speaking for many of his generation when he wrote it. Of course, no one ever asked *why* archaeology should be anything other than archaeology.

Despite warnings raised by a few ethnographers during the early processualist days—one of whom (Harris 1968:360) even went so far as to encourage archaeologists to "shrive yourselves of the notion that the units which you seek to reconstruct must match the units in social organization which contemporary ethnographers have attempted to tell you exist"—archaeologists plunged headlong into the science of culture, employing anthropological concepts and units to understand such things as prehistoric marriage rules and the kinds of descent systems that were in use in the past (e.g., Deetz 1965, 1968, 1970; Hill 1966, 1970; Longacre 1964, 1966, 1968). These exercises, although interesting, began to lose some of their appeal when chinks began to appear in the new archaeological armor. Archaeology thus found itself in a peculiar situation. The discipline, by aligning itself with ethnology, had wanted so desperately to become scientific, but the early studies were found to be flawed on both methodological and conceptual grounds (e.g., Allen and Richardson 1971; Dumond 1977; Friedrich 1970; Stanislawski 1972). For one thing, the exercises were based on what reviewers found to be naive assumptions about the social structures that underlay such things as information transmission among female potters. Some of this criticism could be passed off as carping about details, but most processualists realized that they were going to have to work harder to provide solid, scientifically valid answers to questions that were being posed about the past. What about all the cultural regularities that ethnologists such as White said were out there waiting to be discovered? How could *they* be found? At issue, of course, was one big, underlying question: How did one go about making anthropological archaeology scientific?

The answer was provided yet again by Binford, who suggested that archaeologists should look to the philosophy of science for answers—a tack that he claimed (Binford 1972:17) he had earlier been instructed to take by White. As a result processualists began reading the works of philosophers Hempel and Nagel, among others, and almost immediately the archaeological literature was replete with references to such phenomena as the hypothetico-deductive approach, the deductive-nomological model of explanation, and bridging arguments (see Fritz and Plog 1970). The most influential book dealing with science and archaeology written during the early processual period was Watson, LeBlanc, and Redman's (1971; see also Watson et al. 1984) *Explanation in Archeology: An Explicitly Scientific Approach*. Watson and her coauthors were later joined in their quest to make archaeology scientific by several philosophers (e.g., Salmon 1975, 1982; Salmon and Salmon 1979), but it was Watson and her coauthors who first laid out a detailed blueprint for how to make archaeology an equal partner in the scientific enterprise. They argued strongly that, first, archaeology could and should be a science and, second, that the end product of any scientific endeavor had to be explanation. The means to achieving that end lay in strict adherence to the scientific method: "Archeologists should begin with clearly stated problems and then formulate

testable hypothetical solutions. The degree of confirmation of conclusions should be exhibited by describing fully the field and laboratory data and the reasoning used to support these conclusions. This is what we mean by an explicitly scientific archeological method" (Watson et al. 1984:129).

Watson, LeBlanc, and Redman were clear on how they defined science, acquiring many of their views on science from Hempel's (1965) *Aspects of Scientific Explanation, and Other Essays in the Philosophy of Science:*

> [S]cience *is* based on the working assumption or belief by scientists that past and present regularities *are* pertinent to future events and that under similar circumstances similar phenomena will behave in the future as they have in the past and do in the present. This practical assumption of the regularity or conformity of nature is the necessary foundation for all scientific work. Scientific descriptions, explanations, and predictions all utilize lawlike generalizations hypothesized on the presumption that natural phenomena are orderly....
>
> The ultimate goal of any science is construction of an axiomatized theory such that observed regularities can be derived from a few basic laws as premises. Such theories are used to explain past events and to predict future ones. Good theories lead to prediction of previously unsuspected regularities. Logical and mathematical axiomatic systems are essential as models of scientific theories, but no empirical science has yet been completely axiomatized. As Hempel indicates, it may ultimately turn out for any science, or for all sciences, that the goal is actually unattainable. (Watson et al. 1984:5–6, 14)

The vision of science that Hempel espoused was a reintroduction to the philosophy of nineteenth-century empiricism, although the term usually applied to his view is *logical positivism.* It is difficult to argue with either Hempel's notion of science and how it operates or with Watson, LeBlanc, and Redman's restatement of Hempel's views. Science *has* to be based on the working assumption that past and present regularities are pertinent to future events and that under similar circumstances similar phenomena will behave in the future as they have in the past and do in the present. Without this assumption science would be little more than a chaotic enterprise best left to soothsayers and the like. But this by no means suggests that there is only one way that science views nature; what was lost on the processualists was that science is not some monolithic ontological and epistemological entity that prescribes one particular formula for understanding the natural world. There are at least two vastly different ways the natural world can be viewed, both of which are applicable to the study of why organisms change. Both are based on understanding regularities in the natural world, but one deals with regularities both in process and product, and the other views regularity only in terms of process. Under the latter, products cannot be "regular," because they are, in terms of shape and composition, contingent on how processes affected earlier products in the ancestral line. The differences between the two are explored in more detail below.

There are still a few philosophers around who view science strictly in Hempelian terms, but by the middle of the 1970s it was becoming clear that the model was dying. There were attempts to keep it alive—for example by linking it to Nagel's (1961) *bridging-law* concept—but these also eventually died out except among archaeologists, who began making bridges between the archaeological present and the archaeological past through such things as ethnographic analogy and ethnoarchaeology (see Fritz 1972). In other words, archaeologists were using the present as an analog of the past—an approach that had been of concern to some culture historians, who saw the dangers in conflating homologous and analogous structures. For example, Kroeber (1931:151) remarked, "The fundamentally different evidential value of homologous and analogous similarities for determination of historical relationship, that is, genuine systematic or genetic relationship, has long been an axiom in biological science.

The distinction has been much less clearly made in anthropology, and rarely explicitly, but holds with equal force." Despite such early warnings, processualists *had* to resort to analogy; how else were they going to discover the laws that Hempel said were there—the very laws that, once discovered, led to the formulation of "axiomatized theory" and thus ultimately to explanation?

Now archaeology could access the past through the present. All that had to be done was to discover patterning in archaeological data sets and interpret the patterning in terms of modern or ethnographic analogs (O'Brien 1996a). Or, conversely, ethnographically observed behavior could serve as a guide to what to look for in the archaeological record. If one found enough corollaries between the past and the ethnographic present, then perhaps laws could be written to account for the similarity in pattern. Any slight deviations between or among patterns could be explained away in terms of slightly different "boundary conditions," to use Hempel's phrase, that had impinged on the creators of the past and present signatures. The end result of this exercise was scientific explanation—defined as interpretation through the discovery of laws. This is the reason that Watson (1986:452) directly equated archaeological interpretation with "describing and explaining the real past."

There are, however, several archaeologists who do not agree with this conflation of interpretation and explanation nor with the belief that the Hempelian view of science by itself can be applied to the study of organisms, including humans. No one has ever denied that essentialist laws—those governing chemical and physical phenomena—do not apply to organisms, but at the level that concerns archaeologists—behavior (why we do what we do) and the products of those behaviors (tools and the like)—they cannot play a deterministic role. They might play a probabilistic role, but at this point it is unclear what this means. How are we to assess the validity of any probabilistic claim?

Deterministic laws may work well for explaining why physical things such as elements and molecules act the way they do, but they do not work well for explaining how and why organisms came to be the way they are. A nitrogen atom, for example, is always a nitrogen atom, regardless of where it is in time or space, and there are deterministic laws that govern how nitrogen atoms interact with other atoms. Similarly, burning H_2 in O_2 always produces water. We know that it does today, just as we can bet that it will a million years from now. The safety of the bet resides in our knowing what the laws are that govern the behaviors of atoms *and* in our understanding the various chemical-physical mechanisms that carry out the dictates of the laws. Those kinds of laws apply to invariant properties of *inanimate* objects, but they do not work on such things as the behavior of organisms (O'Brien and Holland 1990, 1995b).

Lest anyone think that this portrayal of processual archaeology as a law-based discipline is a straw man, note, for example, how Fritz and Plog (1970:405), in their widely cited article, "The Nature of Archaeological Explanation," defined law: "A statement of relation between two or more variables which is true *for all times and places*" (emphasis added). Could such a statement be any more definite in terms of what the processualists were after? One might, as has been suggested elsewhere (O'Brien 1996a), skirt the issue and make a semantic distinction between "universal facts" and "laws"—Binford (1972:18) claimed that White once noted that "Julian Steward doesn't know the difference between a universal fact and a law"—but this obscures the real issue, which is the purported existence of invariant laws that govern human behavior. If there are such laws, then the essentialist notion of science is quite adequate for archaeology. If there are not, then where do we look for explanation? We might begin by taking a closer look at Darwinian evolutionary theory.

DARWINIAN EVOLUTIONISM

As Eldredge (1995:10) points out, evolutionary biologists are fond of tracing their intellectual roots directly back to Darwin, especially to *On the Origin of Species*. Evolu-

tionary archaeologists claim a similar, though broken, intellectual lineage. It is interesting, however, that most Darwinists rarely define what it is that makes their work Darwinian. This is certainly as true for archaeologists as it is for biologists. The words *evolution, selection, adaptation,* and *drift* appear regularly in evolution-based studies, but in delving into both the biological as well as the archaeological literature, one soon gets the feeling that there is considerable diversity of opinion not only on what the terms themselves mean (Feder 1997; see Keller and Lloyd's [1992] attempt to ease the problem in evolutionary biology) but on what falls under and what falls outside the Darwinian umbrella. Darwin did not and could not have written a treatise that would withstand a century of biological progress and remain unchanged. Critics might argue that the Modern Synthesis of the late 1930s and early 1940s undermined some of Darwin's central tenets, but this is nonsense. Darwin produced a way of looking at the natural world that was radically different from what came before it, and the basic tenets of his theory remain unaltered. Hence, we might expect, at the very least, considerable agreement on what is and is not Darwinian, but recent discussions in archaeology (e.g., Boone and Smith 1998; Lyman and O'Brien 1998; Maschner 1998; O'Brien and Lyman 1998; O'Brien et al. 1998) lead one to suspect this is not the case.

So what *is* Darwinian evolution? What does any scientific endeavor have to entail to qualify as being Darwinian? Following Mayr (1991) we can identify five components, or theories, to Darwin's overarching theory. First, evolution does indeed occur; second, every group of organisms that now exists, once existed, or will ever exist sprang from a common ancestor, and all ultimately go back in time to a single origin of life; third, species multiply by splitting into separate species; fourth, evolution takes place through gradual change within populations as opposed to through transformation; and fifth, heritable variation is constantly being produced in every generation, and those individuals who have particularly well-adapted sets of characters will tend on average to give rise to the next generation. In the simplest of terms Darwinian evolutionism is a framework for explaining change as the differential persistence of variation (Campbell 1970; Dunnell 1980; Lewontin 1970, 1977). In classical Darwinian terms differential persistence of variation was conditioned by environmental effects—the relentless selector of variants based on their fitness (adaptedness). We now know and have known since the third decade of the twentieth century that there is room for chance—drift—in the evolutionary process.

If the perpetual production of heritable variation—of whatever sort—is central to Darwinian evolutionism, then we are left with the inescapable conclusion that that particular evolutionism is a body of theory and method built around the subject of *change.* The subject of Darwinian evolutionism *is* change, not simply difference and similarity—a point with which biologists have long been familiar but that was first pointed out in archaeology by Dunnell (1980) less than two decades ago. In fact, Dunnell's (1980) discussion of Darwinian change is one of the best encapsulations that has yet been produced in either biology or archaeology, although his telegraphic style tends to obscure the full impact of his message:

> The continuity implied in the terms change and persistence bespeaks a fundamental assumption: the phenomena being examined are historically and empirically related to one another (Alland 1973:3). It is also critically important to note that evolution views change as a *selective,* and not as a *transformational,* process. Variability is conceived as discrete. Change is accomplished by alteration of the frequency of discrete variants rather than alterations in the form of a particular variant. This characteristic places rather severe constraints on the application of evolutionary theory, although perhaps not as severe as it may appear on first reading. (Dunnell 1980:38)

Change *has* to be the central point of any study professing to be Darwinian. And as

Mayr (1991:107) points out, it has to be a particular kind of change: "[D]uring and after the evolutionary synthesis [of the late 1930s and early 1940s] the term 'Darwinism' unanimously meant adaptive evolutionary change under the influence of natural selection, and variational instead of transformational evolution. . . . Any other use of the term Darwinism by a modern author is bound to be misleading."

Philosophers of science (e.g., Hull 1983, 1985) have a difficult time trying to encircle Darwinian evolutionism—that is, defining what it is that unites modern followers of Darwin's theories. One could argue, as Mayr (1991:104) has, that "unless a person is still an adherent of creationism and believes in the literal truth of every word in the Bible, every modern thinker—any modern person who has a worldview—is in the last analysis a Darwinian." He goes on to point out that the "rejection of special creation, the inclusion of man into the realm of the living world (the elimination of the special position of man versus the animals), and various other beliefs of every enlightened modern person are ultimately based on the consequences of the theories contained in the *Origin of Species*." Although evolutionary archaeologists would agree in principle with Mayr, they also would find it rather ironic that many evolutionary biologists, who obviously profess allegiance to central Darwinian tenets, would be excluded under this definition because they specifically have *not* eliminated the special position of humans relative to other animals.

Mayr, of course, is referring to creationism when he refers to the "special position of man," but it is true nonetheless that numerous Darwinian scholars (e.g., Gould 1996) believe that culture and all the things that go with it exempt humans from natural selection. Even Mayr himself slams the door shut on the role of selection vis-à-vis humans, stating recently, "I do not feel there's any natural selection in any positive sense going on right now" (Angier 1997:10). Based on the context of Mayr's remarks, he presumably does not reject that selection once operated on humans; rather, he believes that because the species now covers the globe, there is no opportunity for isolation—a mechanism he has long championed as being a major cause of speciation (e.g., Mayr 1959). Interestingly, Mayr goes on to note, "Theoretically, we could have cultural evolution and develop higher and better concepts" (Angier 1997: 10). Here Mayr sounds just a bit like White. In such a view the human phenotype is perceived as being so plastic that it can adapt to just about anything that nature throws at it. Evolutionism with respect to humans, then, becomes little more than attempting to understand how humans adjust to their environment, usually through reference to deviations from optimal responses. In other words, it becomes a hunt for adaptations. This is the heart and soul of so-called Darwinian ecological anthropology (e.g., Boone and Smith 1998; Smith 1991; Smith and Winterhalder 1992; Winterhalder and Smith 1981)—a field that tends to focus on the functional aspects of humans and to bypass the historical reasons for those functions (Lyman and O'Brien 1998; Vayda 1995a, 1995b).

The one element left out of these studies—something that *cannot* be left out of any study purporting to be Darwinian—is *history*. Darwinian evolutionary theory is a theory about how and why particular organisms look as they do and behave as they do at particular times and in particular places. That, by definition, makes Darwinian evolutionism historical. As Gould (1986) put it, in Darwinian evolutionism, "history matters." Selection, drift, gene flow, and all the other evolutionary processes are important factors in Darwinian explanations, but without history they are simply processes and mechanisms with little to do and certainly nothing to produce. Darwin was a historian of nature, always keeping one question in front of him: Why do things become something else? His whole theory of the multiplication of species, as with his theory of common descent, was a theory about history: All groups of organisms are descended from a common ancestor, and through time groups of them gradually split off to become new species. What is descent with modification through

natural selection—the embodiment of Darwin's various theories—if it is not a historical statement? History implies the passage of time, and time is what is missing from many recent attempts to employ Darwinian evolutionism in anthropology and archaeology (O'Brien and Lyman 1999b). It is that missing element that is the focus of much of the subsequent discussion.

DARWINIAN EVOLUTIONISM IN ARCHAEOLOGY

One of the problems with applying Darwinian evolutionism archaeologically is that Darwin did not have the archaeological record in mind when he formulated his theory—that is, it was not written in archaeological terms (Dunnell 1995). But having said that, we cannot, as Rindos (1989:5) once pointed out, blame Darwin for not doing our work for us. It is up to us to devise and systematically use methods and units that allow us to incorporate archaeological materials into evolutionism. Modest efforts have been made in this direction since the publication of Dunnell's (1980) "Evolutionary Theory and Archaeology," but progress has been slow. This should have been predictable to anyone who, after reading Dunnell's article, looked ahead to what the future held for an evolutionary archaeology. The discipline was fragmented into any number of "isms," with new paradigms springing up annually—an event welcomed with open arms by some archaeologists (e.g., Schiffer 1988) but certainly not by all.

The term *evolution,* which saw considerable use during the heyday of processualism as a result of the influence of White, Sahlins, and Service through Binford, was rarely used in the 1980s except in a large and almost metaphorical sense. There were a few exceptions—Rindos's (1984) *The Origins of Agriculture: An Evolutionary Perspective* and Leonard and Jones's (1987) "Elements of an Inclusive Evolutionary Model for Archaeology," for example—but they were few and far between. But again, to anyone sizing up the situation in 1980, this should have been predictable. It is not easy to displace Whitean thinking, especially when what is replacing it is a theory about how and why organisms change over time, becoming in the process entirely new kinds of organisms. How, one might ask, does such a theory help us archaeologically, where the material record is inorganic? Stones and pots do not evolve—Brew (1946) pointed that out decades ago, effectively squelching any further attempts on the part of culture historians to incorporate biological evolutionary language into their analysis (Lyman and O'Brien 1997). Given such a perspective, which was discipline wide by 1980, the future should have looked grim indeed for any prospects of incorporating Darwinian evolutionism into archaeology.

However, the past two decades have seen modest growth in literature on the subject. Modern efforts to sketch out an evolutionary archaeology now include dozens of articles and papers,[1] as well as several books and dissertations,[2] that either implicitly or explicitly hinge on extending the phenotype to include things found in the archaeological record. These attempts not only have been based on a better understanding of the complexities of evolution than the level of understanding demonstrated by culture historians, but most proponents also understand the need to avoid charges of reductionism—the distillation of theoretical principles from one field for use in another—which was the death knell of attempts made by cultural historians to incorporate evolutionism into archaeology.

Evolutionary archaeologists might be pleased with the renewed attention Darwinian theory is currently receiving in the discipline, but they also realize that there are two related measures of success. The first has to do with how faithfully evolutionary theory is being applied and the second with how widely it is being applied as an explanatory framework. With regard to the first point, it is difficult to call evolutionary archaeology successful if what is being produced in its name does not follow logically from Darwinian evolutionary theory. Critics will immediately point out, however, that regardless of whether we are talking about biology or archaeology, Darwinian evolutionism itself is

not some integrated, compact package of theory and approach but a much larger, umbrella-like set of theor*ies* and approach*es*. There is truth in that statement. Fortunately, one does not have to interpret Darwin's writings to understand how his reasoning revolutionized the ways we view nature. *On the Origin of Species* is not Holy Scripture; hence, we do not have to search for deep meaning or worry about putting its words in the context of the time. Darwin's explanation of descent with modification works just as well today as it did in the 1860s.

Does this mean that Darwin knew of and identified all the mechanisms of evolution? No. If he had, there would have been no need for the Modern Synthesis in biology that occurred in the late 1930s and early 1940s and wedded the views of the paleontologists with those of the geneticists and neontologists. In other words, Darwin's original theory has been refined and extended as we learn more about the natural world. But significantly, Darwin understood that variation was present in nature; he understood the rudiments of transmission and inheritance (like produces like); and he understood, in the quaint parlance of his day, how selection worked. Together those three things created the only engine that Darwinian evolution needed. Other components of the engine were identified later, but they did not change its basic design. Could it legitimately be argued that so much extension has been made to the original theory that Darwin himself would not recognize modern evolutionism? No. He *would* recognize it for the simple fact that his three basic components—variation, inheritance, and selection—still form the centerpiece of the theory.

The key point is that when archaeologists profess to do Darwinian archaeology, they should ask themselves, is it really Darwinian, or is it something else (Lyman and O'Brien 1998)? One has to be careful here, lest what is being said sound condescending and self-aggrandizing. No one has ever suggested that Darwinian theory is a cure-all for archaeological problems. In fact, it is not and cannot be a panacea because it was not written in archaeological terms. Neither is it a cure-all for the myriad problems that confront evolutionary biologists, and thus they make use of essentialist approaches—say, of the physical-chemical kind—in their everyday work. It is, however, *the* solution to archaeology's *evolutionary* problems (O'Brien et al. 1998).

With regard to the second point above, of what use is theory, regardless of how well it explains something, if no one bothers to employ it in real-world situations because either it is not understood or it appears that the theory is being applied in contrived situations? It should come as no surprise that archaeologists, as with scientists in general, have large stakes in the paradigms under which they operate, and hence they are not easily convinced that the manner in which the discipline has traditionally approached the material record is based on the wrong model of science. What about the theory being used to explain the nature of the record? Could it be inappropriate as well? Or, worse yet, is it possible that the "theory" really is not theory? Some archaeologists are so strongly wedded to one paradigm or another that they will never be convinced that Darwinian evolutionism has anything to offer. Then there are those who might be willing to listen to the argument but who are unwilling to accept the view that things in the material record are phenotypic in the same way that somatic features are.

However, there are numerous archaeologists who probably *can* be convinced that evolutionism is the most powerful explanatory tool available for some phenomena. Acceptance, however, will hinge directly on a basic understanding of what evolutionary archaeology is and what it decidedly is not. We are going to have to be clear about such issues as what is meant by Darwinian evolution, what it is that evolves, how change is measured, and so on. As noted previously, even among those with an interest in evolutionary archaeology, there appears to be some confusion over basic premises as well as method. Part of the confusion, we suspect, resides in lack of familiarity on the part of archaeologists with the biological literature, part of it rests on the fact that classical evolutionary

theory does not begin to include all the terms needed to address archaeological phenomena (Dunnell 1995; Teltser 1995b, 1995c), and part of it is the result of a lack of clarity on the part of evolutionary archaeologists.

This brings us to the crucial point of this essay: the role of history in evolution. This is the missing component in some of what today is called evolutionism, and if evolutionary archaeology is going to have an impact on the discipline at large, it is going to have to contend with this issue. The evolutionary-archaeology literature has been relatively clear on the centrality of history in any Darwinian-based enterprise, but apparently the message has not been made as forcefully as it otherwise might have been. Evolutionary mechanisms such as selection and drift have been afforded front-row status, as have topics such as adaptation, but discussions have taken for granted that the reconstruction and explanation of historical lineages is the long-term goal of evolutionary archaeology, just as it is for paleobiology. Perhaps we should no longer take this issue for granted and be more specific about the role of historical explanation in evolutionary archaeology.

HISTORY MATTERS

Evolution constitutes change in the composition of a population over time. In archaeology the population comprises objects, which are viewed as phenotypic features, and "it is the differential representation of variation at all scales among artifacts for which [evolutionary archaeology] seeks explanations" (Jones et al. 1995:28). Time is treated as a continuous, as opposed to a discontinuous, variable, and "change is conceived in terms of frequency changes in analytically discrete variants rather than [in terms of] the transformation of a variant" (Teltser 1995c:53). Such changes *could* be the result of natural selection—shifts in adaptational state—or they *could* be the result of drift (O'Brien and Holland 1992) or other mechanisms. The analytical challenge is to determine which is applicable in any given situation (Dunnell 1978; O'Brien and Holland 1990)—that is, to construct and explain artifact lineages (O'Brien and Lyman 1999a, 2000b). Such a proposition demands the study of immanent properties and processes and the construction of laws concerning them (Simpson 1963, 1970; see also O'Brien et al. 1998)—an endeavor to which behavioral archaeologists have made numerous contributions (e.g., Schiffer and Skibo 1987, 1997; Schiffer et al. 1994; Skibo et al. 1989; Vaz Pinto et al. 1987)—as well as the construction of a set of units for measuring and describing a lineage's fossil record (Lyman and O'Brien 1999)—that is, for writing a historical (materialist) chronicle and narrative. *Explaining* the lineage involves the writing of an evolutionary narrative and demands that the uniqueness of historical contingencies and configurations be considered (e.g., Beatty 1995; Simpson 1963, 1970). These points have been made repeatedly by evolutionary biologists (e.g., Lewontin 1974; O'Hara 1988; Szalay and Bock 1991), as well as by evolutionary archaeologists (e.g., Dunnell 1980; Lyman et al. 1997; Lyman and O'Brien 1998; O'Brien and Holland 1992; O'Brien and Lyman 2000a), but perhaps not strongly enough.

Boone and Smith (1998:20) state that "evolutionary change and historical change are not the same thing." By implication more than explication they maintain that (a) much of culture history consists of a chronicle of "unintended and unselected consequences" (p. 20); (b) phenotypic adaptation can "produce adaptive changes without concurrent selection," and such constitutes history but not evolution (p. 21); and (c) "phenotypic adaptation in response to environmental conditions... *is* change" (p. 22). In other words, evolutionary change, on the one hand, is historical and encompasses only change that is driven by selection; history, on the other hand, encompasses nonselection-driven change within a plastic phenotype over time. From Boone and Smith's discussion it appears that the only *evolution*—selection-driven change—that has occurred is the creation of a plastic phenotype—human culture—that can shift states as its human carriers/replicators deem necessary (Lyman and O'Brien 1998).

The only role for Darwin's natural selec-

tion in such a scenario is that of a past producer of phenotypes or cultures that were adaptively plastic. Although there probably are reasons to believe there is some truth in this, it appears in some respects to be more a presumption regarding the archaeological record and modern cultures than it is axiomatic. Certainly it has not been tested archaeologically. More important, how could it be? How can the plasticity of that part of the human phenotype we term *culture* be measured archaeologically? No one has answered this question satisfactorily, and hence a critical question is left begging. *When* did particular cultures, if we choose to use that typological construct, become plastic, and *why* did they become plastic in the particular times and places that they did? This leads directly to the relevance of history in any inquiry categorized as "Darwinian," be it biological, anthropological, or archaeological.

Few if any evolutionary biologists would argue that history is a noncritical component of what they do (see the chapters in Nitecki and Nitecki 1992). The plethora of statements to this effect, especially with respect to identifying particular character states as "adaptations," continues to grow (e.g., Baum and Larson 1991; Brandon 1990; Burian 1992; Coddington 1988, 1990; Gould 1986; Gould and Vrba 1982; Leroi et al. 1994; Nitecki and Nitecki 1992; O'Hara 1988; Sober 1984a; Taylor 1987; West-Eberhard 1992). There is a less explicit recognition of history in anthropology (e.g., Boyd and Richerson 1992b; Mace and Pagel 1994; Vayda 1995a, 1995b) and archaeology (e.g., O'Brien and Holland 1992), with one commentator (Terrell 1999) going so far as to deny the essential role of selection in producing adaptations. Biologists would be surprised to learn this. Boyd and Richerson (1992b:179–180) acknowledge the importance of history, noting that "Darwinian theory is both scientific and historical. The history of any evolving lineage or culture is a sequence of unique, contingent events." They grapple with the question of what makes change historical, as well as with the issue of how to make historical explanation scientific. Evolutionary archaeologists should agree with Boyd and Richerson's (1992b:201) conclusion that in "the biological and social domains, 'science' without 'history' leaves many interesting phenomena unexplained, while 'history' without 'science' cannot produce an explanatory account of the past, only a listing of disconnected facts." Further, both anthropologists and archaeologists should examine the exchange between White (1938, 1945) and Kroeber (1946) in this regard (see Lyman and O'Brien [1997] for a discussion of this exchange).

Archaeology's claims to unique status within the human sciences are its recognition of how deep and wide human dependence on artifacts is and its access to those portions of past phenotypes. Ethnographers, ethnologists, sociologists, and others who study humans are limited to living human organisms or written records. Only archaeologists have access to the entire time span of culture, however it is defined. Wissler, for one, *could* have argued that the archaeological record was unnecessary to his use of the culture-area and age-area notions to write history (see the review in Kroeber 1931), but instead he employed that record to confirm directly his ideas about culture history (e.g., Wissler 1919). The significance of this observation is found in an instructive parallel in paleobiology. Modern biologists who undertake cladistic analyses might protest that the fossil record is unnecessary for determining the phylogenetic history of organisms, but this position is losing ground as paleobiologists have come to use the fossil record more frequently to help test cladistic hypotheses (e.g., Clyde and Fisher 1997; Donoghue et al. 1989; Fisher 1994; Forey 1992; Hitchin and Benton 1997; Huelsenbeck and Rannala 1997; Norell and Novacek 1992; Novacek 1992; Smith 1994; Szalay and Bock 1991; Wagner 1995).

The important point here is that historical questions are the most obvious ones archaeologists can ask. This, of course, does not mean that such questions *have* to be asked. However, archaeologists should ask historical questions not only because they have

access to data that provide a direct test of historical hypotheses (whether founded in cladistical analyses [e.g., Mace and Pagel 1994], Wissler's age-area notion, or some other model) but also because answers to historical questions are critical to gaining a complete understanding of cultural manifestations occupying particular time-space positions. To demonstrate why this is so we must be clear on what we mean by both history and explanation.

From an evolutionary perspective, to "*explain* means to identify a mechanism that causes evolution and to demonstrate the consequences of its operation" (Bell 1997:1). Importantly, the causes precede the consequence or effect of the working of the mechanisms. Two important mechanisms are selection and drift (transmission), both of which are historical mechanisms. They operate constantly, at some times more strongly or more rapidly than at others, creating the varying tempo of evolutionary change (e.g., Gould et al. 1987). So what is history other than the passage of time? O'Hara (1988:144), following philosophers of history, provides a useful discussion:

> [G]enerally speaking a *chronicle* is a description of a series of events, arranged in chronological order but *not* accompanied by any causal statements, explanations, or interpretations. A chronicle says simply that *A* happened, and then *B* happened, and then *C* happened. A *history,* in contrast to a chronicle, contains statements about causal connections, explanations, or interpretations. It does not say simply that *A* happened before *B* and that *B* happened before *C*, but rather that *B* happened *because* of *A*, and *C* happened *because* of *B*. . . . *Phylogeny* is the *evolutionary chronicle:* the branched sequence of character change in organisms through time. . . . [H]istory, as distinct from chronicle, contains a class of statements called *narrative sentences,* and narrative sentences, which are essential to historical writing, will never appear in [chronicles]. A narrative sentence describes *an event,* taking place at a particular time, with reference to *another event* taking place at a *later* time. . . . Just as narrative sentences distinguish history from chronicle, *evolutionary narrative sentences* distinguish evolutionary history from evolutionary chronicle.

Two critical points can be inferred from O'Hara's discussion: (1) false or inaccurate chronicle cannot produce accurate history, and (2) narrative sentences provide the explanations of why chronicles are the way they are. Archaeologists such as Kidder, Rouse, and Ford, who were categorized as culture historians and criticized by processual archaeologists of the 1960s and 1970s, recognized these distinctions decades ago (Lyman and O'Brien 1997; Lyman et al. 1997; O'Brien and Lyman 1998, 2000a). However, they could not escape the same problem that plagues evolutionary biology today—one identified by O'Hara (1988) when he distinguished between the explanation of *states* and the explanation of *events* of change. The former emerges from what others have termed essentialist thinking; the latter constitutes materialist thinking (Hull 1965; Mayr 1959; Sober 1980) and distinguishes Darwinian evolution as not only a *different* theory of change but a different *kind* of theory (e.g., Lewontin 1974). It demands, in short, a different ontological perspective. Culture historians tended not to recognize the existence of this perspective and attempted to explain the difference in culture states (types) in anthropological terms, not realizing that a more rigorous approach existed—one that would have entailed explaining events of change in Darwin's materialist terms. Their struggles led ultimately to the fall from favor, first, of the utility of any Darwinian sort of evolution as a model for theory building (Lyman and O'Brien 1997) and, second, of virtually the entire culture-history paradigm (Lyman et al. 1997).

The second fall from favor opened the door for what came to be known variously as the "new," or "processual," archaeology of the 1960s and 1970s. The processualists were reacting to the tendency of culture historians to invoke simplistic "causes" such as invention, diffusion, and migration to explain vari-

ation in the archaeological record. As a replacement for what they viewed as the nonscientific nature of culture history, the processualists called for adoption of Hempel's (1942; see also Hempel 1965), covering-law model of science, just as evolutionary biology had in part done a few decades earlier (O'Hara 1988). This was, as noted earlier, an inappropriate model to adopt. Its adoption meant that *states,* as opposed to *events of change,* were to be explained. Although the difference between the two was recognized in evolutionary biology (Hull 1965; Mayr 1959; Sober 1980), this sticky ontological dilemma still plagues that field of inquiry, as evidenced by the continuing debate over the ontological status of species (e.g., Cracraft 1983, 1987, 1989; Davis 1996; Ereshefsky 1989; Ghiselin 1974a, 1974b, 1981, 1987; Hull 1976; Kitcher 1984a, 1984b; Mayr 1987, 1993; Mishler and Brandon 1987; Mishler and Donoghue 1982; O'Hara 1993; Schwartz 1981; Sober 1984a, 1984b; Wilson 1996). What has the dilemma meant for archaeology?

In the four-plus decades since Phillips (1955:246–247) stated emphatically that archaeology must be anthropological, the discipline has spent an inordinate amount of energy trying to effect that transformation. Along with this effort have come no fewer than three problem areas: (1) a reincarnation of Spencer's, Tylor's, and Morgan's stage (O'Hara's [1988] *state*) model of cultural evolution (Carneiro 1973; Steward 1956; White 1959a, 1959b), (2) White's (1959b:8) definition of culture as humankind's extrasomatic means of adaptation (Binford 1965: 205), and (3) Hempel's covering-law model (e.g., Binford 1968b; Fritz and Plog 1970; Watson et al. 1971). Given archaeology's desire to be anthropological, the first and third trouble areas reinforced the essentialist ontology that ethnographic states were to be explained, and the first and second dictated the kinds of explanation to be generated. The law-based character of the latter resulted in attention shifting from homologous—that is, historical—similarity both to analogous similarity and to the familiar functionalism, resulting in the production of details concerning how a state works—in short, functional accounts. Such a perspective is extremely important, as we have stated repeatedly (Lyman and O'Brien 1998; O'Brien et al. 1998), but by itself it can tell us nothing about *why* a particular state came to work the way it did, save to say that it was inevitable. In other words, history does not matter; in particular, lip service may be paid to historical contingency, but it is not critical to the building of satisfying, or what Schiffer (1995a:34) referred to as "richly contextualized and audience friendly," narratives (see O'Brien et al. 1998; Rambo 1991).

Perhaps an example will clarify the significance of the preceding discussion. Borrowing from O'Hara's (1988) discussion, we might ask why flamingos are pink. This is a question of character state. Thus one could reply that flamingos are pink for the purpose of mate recognition. But as O'Hara (1988:150) points out, this is purely a "functional, rational, or purposive" answer. There is nothing inherently wrong with such an answer, and such research can be a fruitful source of hypotheses (e.g., Mitchell and Valone 1990), but an evolutionist would emphasize the historical basis for flamingos being pink and thus would rethink and perhaps even rephrase the question from one concerning a state of being to one concerning continuous change. To do so requires that we first establish that flamingos have not always been pink so that we could then say they became pink for a certain (historical) reason. We might note that some existing flamingos are more, or less, pink than others. We could then search for reasons for the variation. Recalling that evolutionary change requires heritable variation, we might wonder if perhaps lighter-colored birds have different ancestors than darker ones do. Variation in today's flamingos might, or might not, have an obvious purpose or function, but it surely has an evolutionary history. As Mayr (1961) once noted, to ascribe a function to the bird on his porch migrating south every autumn is one thing; to know the historical reasons for its doing so is something else entirely.

We in no way are suggesting that every study of function has to pay homage to the historical pathways that led to the production of a certain tool, for example, but the joining of function and history becomes critical when the topic is adaptation. The underlying assumption of most anthropological studies of adaptation is that natural selection has shaped the observable plastic phenotype we call culture. Leaving aside the important question of when such a supposedly plastic phenotype first appeared in the hominid lineage (or if it appeared before that particular evolutionary branch began), there is value in studies that seek to measure fitness, or degree of adaptedness, among cultures. For example, there should be no argument with Bettinger and Richerson's (1996:224) statement that knowing the functional reason for why a dog pants—to regulate body temperature—is important, but is it true that one need "not question that this panting is the result of a long evolutionary history" and that such a history "is beside the point directly at hand"? To ignore history misses the critical point that biologists (particularly paleobiologists) have come to appreciate: To be considered an adaptation, a trait must have a history demonstrating it was shaped by selection—a point elaborated at length in archaeology by O'Brien and Holland (1992). As Gould (1995) put it, we need not bother studying the fossil record—either paleontological or archaeological—if we can see all evolutionary processes and products in a petrie dish or among a group of college students.

Evolutionary archaeologists would not deny, and in fact would emphatically endorse, the notion that showing that a particular phenotypic trait has a positive fitness value is critical. Behavioral archaeologists would agree with this point (Schiffer 1996). In archaeology this requires the measurement of mechanical properties of artifacts (Schiffer and Skibo's [1987] *performance characteristics*) in a manner similar to the way one determines that a dog regulates its body temperature by panting (O'Brien and Holland 1995a; O'Brien et al. 1994). Does a particular kind of pottery work better within the particular time-space position it occupies than some other kind of pottery does? If so, why? In other words, how does that particular *state* of pottery work in that context? Evolutionists (e.g., O'Brien et al. 1994) have joined forces with behaviorists (e.g., Skibo et al. 1989) in beginning to understand the nature of such historical contexts. These are, however, only the first questions that must be answered. Additionally, what is the selective environment in which that state occurred, and what were the selective environments that led to its appearance? In other words, what was the *history* that led to that kind of pottery's selection? These are questions about the history of change in pottery, and they are what makes evolutionary archaeology evolutionary. Answering the questions regarding pottery state requires the use of immanent properties and processes; answering the questions regarding pottery *change* requires the use of configurational properties and processes.

Bettinger and Richerson (1996:226) state that "given time's ravages, few archaeologists will ever be privileged to participate in constructing a 'how actually' explanation." They appear to make two points. First, they are stating that the historical chronicles and narratives (in the sense of O'Hara 1988) that evolutionary archaeologists construct are merely plausible stories. This, of course, is true, but it also is true that the accounts are theoretically informed and thus are *testable*. Second, Bettinger and Richerson are arguing that the real story will never be known. Apparently, what they are doing in distinguishing "how possibly" from "how actually" explanations is suggesting that they find little satisfaction with the former, characterized by O'Hara (1988:149) as statements regarding "how a change *may* have taken place," and would much prefer the latter, or how a change "*did* take place" (O'Hara 1988:150). Bettinger and Richerson's point is that the latter is impossible to attain. Paleobiologists would not disagree, but neither would they throw up their hands in despair.

We make two important points relative to Bettinger and Richerson's (1996) discussion. First, in making the distinction between

"how possibly" and "how actually" explanations, they reference Brandon (1990) but take his discussion out of context. Brandon's (1990:176–184) point was that "how possibly" explanations are quite valuable and in many cases can only be epistemologically distinguished from explanations of the "how actually" sort. When a "how possibly" explanation both accounts for numerous observations and provides an empirically and logically coherent explanation, it attains the status of a "how actually" explanation, yet it remains testable in light of new evidence. Brandon acknowledges that we may never know when we have truly attained the latter, although he also states that "no one can fairly describe [such a 'how possibly' explanation] as merely an imaginative bit of story telling" (Brandon 1990:183).

The second point relative to Bettinger and Richerson's discussion is the nature of their claim that the archaeological record is a poorly preserved reflection of evolution. Excusing archaeologists from rigorous scientific standards because of the formational history of the archaeological record harks back to the decades-old lament over the imperfection of that record (e.g., Hawkes 1954; MacWhite 1956), although even in those days some individuals perceived this as a not-insurmountable problem (e.g., Bennett 1943, 1946). In the context of the last two or three decades these laments are founded largely on the belief that the archaeological record is a relatively static phenomenon and must be reconstituted, with great analytical care, into dynamic human behavior if our questions are to be answered (e.g., Boone and Smith 1998; Schiffer 1996). Paleobiologists have come far in their dealings with the quality of the fossil record since the explicit placement of taphonomic research under the umbrella of biological evolution (e.g., Benton and Storrs 1994). Studies of formational processes of the archaeological record have made similar strides (e.g., Schiffer 1972b, 1983, 1985, 1987), although it has been suggested (Lyman and O'Brien 1998) that perhaps it is time for archaeologists to shift gears and bring themselves in line with paleobiological taphonomy and to stop complaining that the record is a poor reflection of anything.

CONCLUDING REMARKS

With Dunnell's work in the late 1970s archaeologists began to explore the inclusion of archaeological phenomena under a Darwinian umbrella, and in retrospect a fairly solid foundation has been laid. Attention has been paid not only to ontological and epistemological issues but also to purely methodological ones. Incorporation of archaeological materials into a Darwinian framework rests on the key tenet that those items were parts of previous phenotypes—what Dunnell (1989a: 44) referred to as the "hard parts of the behavioral segment of phenotypes." Without this tenet the application of evolutionary theory to the understanding of how things found in the archaeological record came to be the way they are makes absolutely no sense. Further, without the belief that evolutionary processes, including selection, continue to work on humans, it also is senseless to apply evolutionary theory archaeologically. Evolutionary ecologists, at least some of them (e.g., Boone and Smith 1998), do not believe that selection still works on humans—a view shared by a number of eminent evolutionary biologists—Gould (1996) and Mayr (1982; see also Angier 1997), for example—and so for them evolutionary archaeology is an empty exercise.

Evolutionary archaeologists are going to have to face the fact that not all their colleagues are going to be persuaded that the approach has something to offer. Focusing even more narrowly, fewer colleagues yet will be persuaded that, given the configurational nature of the archaeological record, Darwinian evolutionism offers the *best* hope of explaining it in scientific terms. No one likes to be preached to, and unfortunately much of the previous evolutionary archaeological literature has tended to ignore some of the rather careful work of those outside the confines of the evolutionary paradigm. For evolutionary archaeology to have the intended impact on the discipline, proponents have to begin paying attention to such things

as behavioral studies. As we have pointed out here and elsewhere (e.g., O'Brien and Holland 1995b), studies of specific prehistoric activities (Schiffer 1976)—the heart of the behavioral paradigm—are essential components of evolutionary archaeology. In addition, evolutionary archaeologists are going to have to be clear about such things as what it is that evolves and the units that they use in structuring the detailed kinds of analysis called for in an evolutionary study (O'Brien and Lyman 2000a). But the most critical point to be stressed in future evolutionary studies is that evolution is based in history; in fact, at base level evolutionism is the unraveling of complex historical lineages—a point that behavioral archaeologists have long realized (Schiffer 1976). The complexity can only be dealt with by sorting out analogous from homologous features, which itself is a historical concern.

Someday soon, there hopefully will be no reason to continue the struggle of showing the applicability of Darwinian evolutionism to the archaeological record, and we can get on with developing and using appropriate methods to sort out artifact-lineage histories. These methods, in our opinion, will come from paleobiology (Lyman and O'Brien 1998; O'Brien and Lyman 1999a, 2000a, 2000b; O'Brien et al. 1998)—a point not emphasized previously in much of the extant evolutionary archaeological literature. As with the paleontological record the archaeological record is a recording mainly of evolutionary successes, but it is an exceedingly rich one that creates an excellent laboratory for examining the ebb and flow of various evolutionary processes across the millennia. If some people choose to believe that humans are immune from the process of selection and have been since the advent of culture, so be it. But for those who do not hold to such a belief, evolutionism offers a solid basis for developing scientifically sound explanations for archaeologically visible change. But such an approach cannot ignore its most important component—history.

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Notes

1. See, for example, Braun (1990); Dunnell (1978, 1980, 1982, 1985, 1988, 1989a, 1989b, 1992a, 1992b, 1994, 1995, 1996); Feathers (1989, 1990a); Graves et al. (1990); Jones et al. (1995); Leonard (1989); Leonard and Jones (1987); Leonard and Reed (1993, 1996); Lipo et al. (1997); Lyman and O'Brien (1997, 1998); McCutcheon (1995); Madsen and Lipo (1993); Neff (1992, 1993, 1997); Neiman (1995); O'Brien and Holland (1990, 1992, 1995a, 1995b); O'Brien and Lyman (2000a, 2000b); O'Brien et al. (1994, 1998); Reed and Maxwell (1990); Rindos (1989); Schiffer (1996); and Teltser (1995b, 1995c).
2. See, for example, Bettinger (1991); Feathers (1990b); Ladefoged (1993); Lyman et al. (1997); Maschner (1996); Neiman (1990); O'Brien (ed. 1996); Teltser (1988); and Teltser (ed. 1995).