

Style, Function, Transmission:

An Introduction

Anyone interested in applying Darwinian evolutionary theory in archaeology is immediately confronted with a problem: Darwin wrote his theory in biological, not archaeological, terms. Archaeology is not the only natural science to have confronted this problem, as examination of the paleontological literature of the 1940s and early 1950s—the decade immediately following the Modern Synthesis in evolutionary biology—reveals. Paleontologists deal with fossils—hard parts of long-dead organisms—but evolutionary theory is written in terms of living organisms. How can that theory be applied to fossilized organisms—and only portions of organisms at that—when the theory depends so heavily on our ability to deal with such things as species, populations, and the like? It should not be surprising that disagreements existed in paleontology over how to (re)write Darwin's theory in paleontological terms, nor should it be surprising that similar disagreements exist in archaeology. The central problem is to rewrite the theory in appropriate—archaeological—terms.

In the process of working through these disagreements, archaeologists interested in applying Darwinian evolutionism have had to contend with skeptics who view any kind of evolutionary archaeology as simply a metaphor for the study of culture change. Skeptics argue that attempts to apply Darwinian theory to the material remains of hu-

mans are reductionistic (Kehoe 2000; Larson 2000; Maschner 1998; Preucel 1999) if not downright misleading (Mithen 1997, 1998). We have addressed these arguments elsewhere (O'Brien and Lyman 1999c, 2000a, 2000b) and here note simply that the integration of Darwinian evolutionism into archaeology is not reductionistic. It is based in large part on two tenets. First, objects in the archaeological record are hard parts of human phenotypes and thus are the products of evolutionary processes acting on those phenotypes in the same way that somatic features are. Second, the archaeological record is historical in the same way that the fossil record is. Because it is historical, the archaeological record marks the passage of time. Importantly, the objects that comprise the record—flakes of stone, pottery sherds, bone awls—are more than chronological markers in the same way that fossilized organisms are useful for more than keeping track of the passage of time (Lyman and O'Brien 2000a). Artifacts carry the imprint of human manufacture, use, discard, loss, and myriad other activities (Schiffer 1976). They are capsules of variation that were formed at particular instants in the past, and once strung together these capsules present us with a historical recording of how certain human-phenotypic variants replaced others over time. There is nothing reductionistic in any part of this procedure.

Shifts in emphasis within a discipline bring

with them the advent of named approaches, and with respect to the incorporation of evolutionism into archaeology, two approaches—human behavioral ecology (also referred to as evolutionary ecology) and evolutionary archaeology—have come to the forefront, joined at times by behavioral archaeology. Labels have their up sides, but they also tend to create “camps” and, not unexpectedly, camp charters and party lines. These can have a stultifying effect on scientific progress (Wylie 2000). If archaeologists are going to make more than token attempts to explain the archaeological record in Darwinian terms—and in the process make evolutionary studies of interest to the discipline at large—there needs to be a coordinated search for common areas from which to build a useful theoretical and empirical framework. This is the tack Schiffer (1996) took with respect to evolutionary archaeology and behavioral archaeology, and although his effort sparked rebuttal (Broughton and O’Connell 1999; O’Brien et al. 1998), it successfully identified common ground. We took a similar tack with respect to evolutionary archaeology and human behavioral ecology (O’Brien and Lyman 2002a). Consensus building does not mean that all areas of disagreement among evolutionists will disappear (Bettinger and Richerson 1996; Boone and Smith 1998; Broughton and O’Connell 1999; Larson 2000; Lyman and O’Brien 1998, 2001b; Morey 2000; Neff 2000, 2001; O’Brien and Lyman 1999c, 2000b, 2002a; Schiffer 1996, 1999). Rather, it means that all sides must move beyond polemical statements and nonuseful characterizations of one or another of the approaches and instead focus on how to integrate what each group does best. As the approaches mature, and if a synthesis emerges from their unification, lines dividing them will blur.

In our view no issues are more fundamental to evolutionary archaeology than style, function, and transmission. Of these, transmission is perhaps the most important because it is the process that creates lineages, or what in archaeology have long been referred to as *traditions*. Thus a key area of emphasis

in evolutionary archaeology is the reconstruction of artifact lineages—that is, demonstration of heritable continuity as opposed to simply historical (chronological) continuity between archaeological manifestations. Heritable continuity rests on cultural transmission, but that statement in and of itself does not tell us *what* is inherited and *how* it is being inherited. Nor does it answer questions about the processes that sort variation as it is being inherited and what kind of material traces identify the action of one sorting process over another. Can we separate products of selection from products of transmission? Much has been made in evolutionary archaeology of the style-function dichotomy, the former being solely the product of transmission and the latter the product of selection, but what does it mean to say that something is stylistic and that something else is functional? How can we separate the two states conceptually and analytically? There also is the key issue of how functional and stylistic features are transmitted.

The 12 chapters in this book address these fundamental questions and issues. With one exception, David Meltzer’s treatment of style and function in endscrapers that appeared in 1981 (Chapter 6), the papers first appeared between 1991 and 2000. Our intent here is not to provide brief introductions to the papers but to place them in the broader context of evolutionary archaeology, with particular emphasis on style, function, and transmission. Our rationale for providing such a context is derived from the rationale we used in deciding which papers to include. We wanted papers that were short on background and long on substance, particularly analytical methods and results. For example, Charlotte Beck’s (Chapter 7) analysis of projectile points from the Great Basin does not include the word evolution. The take-home message of her chapter is clear: One does not have to laden good empirical work with an extended account of evolutionary archaeology. The only chapter that is not results oriented is Chapter 1, by Robert Bettinger, Robert Boyd, and Peter Richerson, whose take on style differs in several respects from

that of the other authors whose works are included here. We selected it because of the issues they raise, several of which are not addressed in the other chapters.

We offer this introduction as a framework within which the following chapters may be read. Given the limited range of topics covered in those chapters, our remarks are similarly limited. We begin with a brief consideration of two interrelated subjects within the evolutionary archaeology literature—the nature of science and the role of theory within science. We then introduce several critically important concepts within modern evolutionary theory before turning to what we perceive to be key issues in modern evolutionary archaeology. Throughout, references followed by brackets around a number indicate that the paper referred to is included in this volume; the number refers to the chapter number.

SCIENCE AND THEORY

Evolutionary archaeology is not without critics (e.g., Boone and Smith 1998; Preucel 1999). In a recent consideration of the nature of archaeological science in general, philosopher of archaeology Alison Wylie (2000:228) characterized evolutionary archaeology as scientific, dogmatic, and divisive. It is difficult to respond to the first adjective because Wylie fails to define science. What *is* science? We agree with the two criteria held up as hallmarks of science by philosopher of geology Richard Watson (1972, 1991, 1992): Explicitness with respect to reasoning and analytical protocols, and skepticism with respect to any derived conclusion or answer. Wylie (1992a:270) adds that “the hallmark of the traditions of inquiry we commonly call ‘scientific’ is precisely their flexibility (their adaptive responsiveness, as it were) under complex and diverse conditions.” We believe evolutionary archaeology displays all three criteria—explicitness, skepticism, and flexibility. We suspect that everyone would agree that science deals with empirical phenomena. Further, we think that everyone would agree that our understandings of those phenomena must be testable or capable of evaluation;

here resides the skepticism of science. The source of scientific understanding must be theory, and evolutionary archaeologists *are* dogmatic about this. The divisiveness that attends the debate is over the source or kind of theory.

On the one hand, the Americanist-archaeology research tradition centers around the rallying cry that archaeology is anthropology or it is nothing, and thus Americanist archaeologists have long borrowed theories from their sister discipline. Many evolutionary archaeologists, on the other hand, argue that archaeology cannot be anthropology as anthropology is currently constituted. Employment of anthropological theories demands that the archaeological record be reconstituted into something an ethnographer would recognize; hence much effort has been expended on what has variously been termed ethnoarchaeology, middle-range theory, and actualistic studies—research aimed at building linking arguments to substantiate archaeological inferences (e.g., Schiffer 2000; Skibo et al. 1995). Evolutionary archaeologists, however, attempt to rewrite Darwinian evolutionary theory in *archaeological* terms, just as paleobiologists rewrote that theory in paleontological terms (Lyman and O’Brien 2001a). This rewriting also demands actualistic research, some portions of which are used to construct immanent analogies, other portions of which are used to construct configurational analogies (Hunt et al. 2001; Wolverton and Lyman 2000).

A particular theory provides a particular understanding—a way of knowing—about how and why the world works the way it does. As Wylie (1992b:25) notes, “claims about the past are invariably shaped by an encompassing theory... which can also inform the interpretation of archaeological data as evidence for or against those claims.” We would substitute the word “does” for “can also” in the last phrase. Theory provides the guide to what a “correct” answer should look like. It provides structure to research by indicating *which* phenomena are to be examined, *how* they are to be examined, measured, and analyzed, and *what* the analytical results

might mean. Theory suggests hypotheses for testing, and thus deduction plays a major role, although induction is not precluded. The explanatory theory we have chosen—Darwinian evolution—dictates how and why we do archaeological science the way we do.

Writing archaeological theory in Darwinian terms does not mean that anthropological concepts and knowledge are ignored in favor of a culturally or behaviorally sterile physics of artifacts, as some have alleged (e.g., Watson 1986). Rather, it means we seek to avoid many of the perils of inferential reconstructionism and configurational analogies (Wolverton and Lyman 2000). But it also means that we have much left to do with respect to rewriting the theory and generating data appropriate to it (e.g., Hunt et al. 2001; Hurt and Rakita 2001b). It is not a matter of simply subsuming a particular set of reconstructive inferences under an anthropological theory or model, such as some processualists do (Spencer [1997] and references therein). It is no simple matter for the particular reason that *history matters* in Darwinian evolution (Gould 1986)—something downplayed by many anthropological theories. Whereas Boasian historical particularism was inductively directed toward deriving generalities of cultural development, evolutionary archaeology has a decided deductive component because it has a theory that suggests particularistic historical hypotheses for testing. In this respect what evolutionary archaeologists do is similar to what behavioral ecologists do (e.g., Boone and Smith 1998; Kelly 2000).

With respect to rewriting Darwinian theory in archaeological terms, we agree with several of Wylie's (1992a:273) remarks that such a job will require "a nuanced account of how archaeological data—facts of the record—are constituted as evidence, how they are 'laden' with theory such that they can have a *critical* bearing on claims about the cultural past." This is *the* most important message of evolutionary archaeology and has been advocated by its practitioners for over two decades. It explicitly attends the notion shared by processualists and postprocessualists that

"neither data nor evidence are given, stable, or autonomous of theory" (Wylie 1992a:275). Some critics of evolutionary archaeology (e.g., Arnold 1999; Kelly 2000; Spencer 1997) miss the importance of adopting this stance.

Another of Wylie's (1992a:275) points with which we agree is that to identify "archaeological data and their constitution as evidence relevant for understanding the cultural [and, we would add, human behavioral] past depends, inevitably, on some body of linking principles: 'source-side' or background knowledge; middle range 'theory'; mediating interpretive principles." How else could a processualist, postprocessualist, or evolutionary archaeologist distinguish between a flaked-stone artifact and a naturally broken rock? But here the distinction between immanent and configurational processes and properties is critical to the success of evolutionary archaeology (chapters in Hunt et al. 2001) or of any other program of archaeological research. Because the evolution of a lineage is historically contingent, we must use middle-range theory founded in immanence to access the configurational past (Simpson 1963). Otherwise, the past can be no different from the present.

EVOLUTIONARY THEORY AND TRANSMISSION

In Chapter 13 of *On the Origin of Species*, Charles Darwin (1859) outlined a protocol for classifying organisms so as to reflect their phylogenetic affinities. He noted that "We have no written pedigrees; we have to make out community of descent by resemblances of any kind. Therefore we choose those characters which, as far as we can judge, are the least likely to have been modified in relation to the conditions of life to which each species has recently been exposed" (Darwin 1859:425). The degree of importance of particular characters to a classification, the goal of which is to represent evolutionary pedigree, "depends on [the characters'] greater constancy throughout large groups of species; and this constancy depends on such organs

having generally been subjected to less change in the adaptation of the species to their conditions of life" (Darwin 1859:417). Summarizing, Darwin (1859:427) noted that "On my view of characters being of real importance for classification, only in so far as they reveal descent, we can clearly understand why analogical or adaptive characters, although of the utmost importance for the welfare of the being, are almost valueless to the systematist."

Darwin's theory of descent with modification held that one should consider only characters that were neither strongly nor consistently influenced by selection for studying descent. His theory provided a new and logical causal explanation as to why there should be some formal similarities between organs and organisms—what he termed *descent*, today glossed as *transmission*—and why there should be some formal differences between them as well. Many differences were the result of adaptive change; at least some similarities were the result of transmission from a common ancestor: "By unity of type is meant that fundamental agreement in structure, which we see in organic beings of the same class, and which is quite independent of their habits of life. On my theory, unity of type is explained by unity of descent" (Darwin 1859:206). But Darwin also had to contend with modification. Thus, "propinquity of descent—the only known cause of similarity of organic beings—is the bond, hidden as it is by various degrees of modification, which is partially revealed to us by our classification" (Darwin 1859:413–414). Pedigrees are only partially and imperfectly revealed because of modification driven by selection during descent. Thus, Darwin (1859:417) emphasized that one should consider "an aggregate of characters" because "a classification founded on any single character, however [physiologically or adaptively] important that may be, has always failed; for no part of the organisation is universally constant."

As Carl Lipo and Mark Madsen (2001) have pointed out, Darwin's theoretical choice

of characters for tracking heritable continuity anticipated the neutral theory of evolution first proposed by population geneticist Motoo Kimura (1968). Subsequent research (e.g., Kimura 1983; King and Jukes 1969) confirmed some aspects of Kimura's notion, although the version of the theory now generally accepted is considerably more complex than the original version (Hey 1999; Krietman 1996; Ohta 1996; Wayne and Simonsen 1998). Nevertheless, Darwin's choice was sound. To determine descent, formal variation in characters manifest as diverse character states must be documented, and those that track heritable continuity and phylogenetic descent must be chosen for analysis over those that track the influence of natural selection. Failure to realize this simple fact results in a problem. The phylogenetic signal provided by characters that are subject to the influence of selection is muted not only by modification within a lineage but by modifications within two unrelated lineages that produce similar results—the well-known processes of parallelism and convergence: "Parallelism is the development of similar characters separately in two or more lineages of common ancestry and on the basis of, or channeled by, characteristics of that ancestry.... Convergence is the development of similar characters separately in two or more lineages without a common ancestry pertinent to the similarity but involving adaptation to similar ecological status" (Simpson 1961:78–79). Parallelism results from the development of similar adaptive designs among closely related phenomena; convergence results from the development of similar adaptive designs among remotely related phenomena (Eldredge 1989b).

How does one make a choice regarding which characters to study if descent is the topic of interest? Biologists and paleobiologists make the choice by first defining two concepts that are part of Darwin's theory of descent with modification. Ernst Mayr (1969: 85) defined these as follows:

Homologous features (or states of features) in two or more organisms are those

that can be traced back to the same feature (or state) in the common ancestor of these organisms.

Analogous features (or states of features) in two or more organisms are those that are similar but cannot be traced back to the same feature (or state) in the common ancestor of these organisms.

In the case of homology, similarity is not part of the definition because homologous structures are by no means necessarily similar (e.g., ear ossicles of mammals and the corresponding jaw bones in lower vertebrates [Crompton and Parker 1978]). Similarity must be referred to in the definition of analogy because non-homologous features that are not similar are not considered analogous.

In biological systematics there are two kinds of homologous characters—those that are primitive (ancestral) and those that are derived. The former are those that have been formally static, and the latter are those that have been modified as a result of an evolutionary process such as natural selection or some vagary of transmission.

The preceding should make it clear that characters that are influenced by natural selection can be and often are transmitted, just as are those characters that are not influenced by selection. Darwin's central point was that if one wishes to trace phylogenetic descent and simultaneously to minimize the influence of selection-driven modification on the phylogenetic signal, then what are today referred to as adaptively neutral characters are the ones to use. A mechanism of transmission is necessary to cause heritable continuity between ancestral and descendant phenomena. If the transmission is done with high fidelity, the descendant phenomena will be clones of their immediate ancestors. But at least two processes decrease the probability of perfect fidelity. In population genetics *drift* refers to the random fluctuation in gene frequencies across generations; thus, it is the frequencies of the transmitted phenomena (in this case, alleles) that change. To be subject to the process of drift, two alternative alleles must have the same fitness—that is, be selectively

neutral relative to one another in the sense that neither confers greater fitness on the bearer. Simulations show that in the absence of innovation, homozygosity will result quickly in small populations and slowly in large populations (Ridley 1993). Fraser Neiman explores this phenomenon in Chapter 2. The other process that influences the degree of fidelity of transmission involves change in the transmitted phenomena themselves. Mutation and crossing over are expressions of this process in genetics. In cultural transmission analogous results occur when a message of less-than-perfect fidelity is sent and received, or a message of perfect fidelity is sent but is misunderstood or incorrectly translated by the receiver. If the message is acted on and has an empirical result in either case, that result is a cultural novelty (LaMotta and Schiffer 2001; Schiffer and Miller 1999a, 1999b). Models of transmission become more complex as the probability of perfect fidelity decreases.

Yet another difficulty in modeling transmission concerns whether the units of transmission are independent of one another. To contend with the possibility that the units of cultural transmission—whatever they might be—are not completely independent, two concepts are necessary. The first concept is *constraint*, which refers to changes or restrictions thereof that are not embedded in the theory of cause (Gould 1989, 2002). The theory is Darwinian evolution, and the causes are sorting mechanisms such as selection and lack of fidelity in transmission, perhaps affected by drift. Sorting mechanisms are external to the phenomena being sorted. Constraint is internal to those phenomena and concerns restrictions on future potential variants, given mechanical or structural limitations, but it also serves as a possible cause of directional change toward one particular variant (and thus builds adaptation) rather than another, or what is termed “channeling” (Gould 2002). The evolutionary history of a neutral trait can be constrained by the evolutionary history of a trait with which it is mechanically linked; the former is replicated and sorted in concert with the replication and

sorting of the latter. Such a neutral trait is said to *hitchhike*—the second necessary concept—with a trait that is selected *for* and thus comprises an instance of selection *of* a (neutral) trait. The neutral hitchhiker can be said to be sorted, where sorting is a “simple description of differential representation” over time that “contains, in itself, no statement about causes” (Vrba and Gould 1986:217). To determine if a character or character state is a hitchhiker demands detailed mechanical and engineering studies (Kornbacher 2001; Pfeffer 2001; Wilhelmsen 2001).

MODERN EVOLUTIONARY ARCHAEOLOGY

Historical reviews of evolutionism in Americanist archaeology (e.g., Dunnell 1980a; O'Brien 1996a, 1996c, 1996d) devote considerable space to the differences between Darwinian biological evolutionism and the cultural evolutionism of Leslie White (1959a, 1959b), but often ignored is the fact that culture historians earlier this century acknowledged the applicability of Darwinian evolutionism to the study of prehistory and made serious attempts to incorporate elements of it into their work (Lyman and O'Brien 1997; Lyman et al. 1997; O'Brien and Lyman 1998, 1999a, 1999b). For example, A. V. Kidder (1932), one of the leading culture historians of the period 1910–1950, believed that the groundwork for a truly evolutionary archaeology did not exist because basic data on cultural variation were unavailable. As Kidder saw it, he did not need a separate theory to explain cultural variation; such a theory—Darwinian evolutionism—was already available. Throughout his career Kidder focused on documenting variation—the singular issue that underlies any evolutionary study—rather than on building a theory of cultural development. He did not see variation in such things as pottery as being fundamentally different from genetically controlled somatic variation. Rather, he saw more of it being produced as a result of the almost limitless imagination of humans and their enormous capacity for effecting change in their social and physical environment.

Some of Kidder's contemporaries, such as Harold Gladwin (1936), argued that because cultural evolution occurred much more rapidly than biological evolution, Darwin's ideas were not applicable to the archaeological record. This viewpoint picked up steam in the 1940s—ironically, at the same time the Modern Synthesis (Huxley 1942) was occurring in evolutionary biology—and reached its zenith with the oft-quoted statement by J. O. Brew (1946:53) that “phylogenetic relationships do not exist between inanimate objects.” During the next several decades, as the result of numerous statements that cultural evolution and biological evolution were necessarily uncoupled phenomena, Darwinian evolutionism was virtually ignored in favor of White's brand of cultural evolutionism, with its emphasis on function and its definition of culture as humankind's extrasomatic means of adaptation. Whitean evolutionism became the cornerstone of processual archaeology in the 1960s (Spencer 1997), and definitions of concepts and units such as band, tribe, and chiefdom were carried over wholesale into processualism (e.g., Binford 1969, 1972a). Processualists occasionally made reference to Darwinian evolutionism, but it wasn't until publication of Robert Dunnell's (1978a) “Style and Function: A Fundamental Dichotomy” that there was an incipient programmatic statement on how to write that particular kind of evolutionism in archaeological terms.

Dunnell's early work (1978a, 1978b, 1980a) was met largely with silence, and although occasional articles (e.g., Leonard and Jones 1987; Meltzer 1981[6]; O'Brien 1987; Rindos 1980, 1985, 1989) and one book (Rindos 1984) appeared throughout the 1980s, it was not until the 1990s that evolutionary archaeology attracted more than modest interest. During that decade the number of articles and books grew exponentially and continues to grow. Some of these focus on method or on rewriting the theory in archaeological terms (Barton and Clark 1997; Hurt and Rakita 2001a; Kornbacher and Madsen 1999; Lyman 2001; Lyman and O'Brien 2000b[12]; Neff 1992, 2000, 2001;

Neff and Larson 1997[11]; O'Brien 1996b, 1996d; O'Brien and Holland 1990, 1992, 1995a, 1995b; O'Brien and Leonard 2001; O'Brien and Lyman 2000b, 2000c, 2000d; Teltser 1995a, 1995b), but many are case studies of how Darwinian evolutionism can be used to explain particular portions of the archaeological record (Allen 1996a[5]; Ames 1996; Aranyosi 1999; Bamforth and Bleed 1997; Barton 1997; Boone 2000; Cochrane 2001, 2002; Dunnell and Feathers 1991 [10]; Dunnell and Greenlee 1999; Feathers 1990b; Graves and Ladefoged 1995; Hamilton 1999; Hughes 1998; Hunt et al. 2001; Hurt and Rakita 2001b; Kornbacher 1999; Ladefoged and Graves 2000; Larson et al. 1996; Leonard 2001; Leonard and Reed 1993, 1996; Lipo 2001; Lipo and Madsen 2001; Lipo et al. 1997[3]; Madsen et al. 1999; Maxwell 1995; McGimsey 1995; Neff et al. 1997; Neiman 1995[2], 1997; O'Brien et al. 1994[9]; Pfeffer 2001; Pool and Britt 2000[8]; Shott 1997a; Sterling 1999; Van-Pool 2001; Vaughan 2001; Wilhelmsen 2001).

Despite these efforts, the state of evolutionary archaeology today is reminiscent of how things were in evolutionary biology in the Synthesis days of the late 1930s and early 1940s, when there was not only methodological disparity but also deep theoretical division among geneticists, neontologists, and paleontologists (Mayr and Provine 1980). An example of the struggle comprising the Modern Synthesis that parallels evolutionary archaeology's history is found in George Gaylord Simpson's efforts to show that paleontology could make significant contributions to the Synthesis rather than being a mere handmaid to it. His landmark *Tempo and Mode in Evolution* (Simpson 1944) provided not only a statement on the applicability of evolutionism to the fossil record but also methods for its implementation. One of paleontology's greatest contributions to evolutionary theory was through its emphasis on paleoecology, which produced the modern field of paleobiology. We believe such a unification will occur in archaeology, given evolutionary archaeology's interest in his-

tory, behavioral ecology's interest in human-environment interaction, and behavioral archaeology's interest in human behavior. But as with the emergence of modern paleobiology, the explanatory theory must be rewritten in appropriate terms. In the case of paleontology, the Modern Synthesis version of evolution had to be rescaled from biological (intergenerational) time to geological (inter-taxonomic) time, and models and their attendant implications had to be cast in *paleontologically visible* terms (Eldredge 1985, 1999). Archaeologists are still grappling with this rewriting (O'Brien and Lyman 2000b), as are those with interests in behavioral ecology (Grayson and Cannon 1999).

Evolutionary archaeology in the broadest sense has more than metaphorical parallels to paleobiology in that both are geared toward providing Darwinian explanations of a material record. Put simply, evolutionary archaeology comprises writing descriptions of the historical patterns of differential character representation and testing theoretically derived hypotheses as to how and why evolutionary processes acted to create those patterns (Jones et al. 1995). Both steps use concepts embedded within Darwinian evolutionism, including lineage, which is a temporal line of change owing its existence to heritability; sorting mechanisms such as natural selection; transmission mechanisms and pathways; and innovation, which is a source of novelties similar to genetic mutation and recombination. Heritable continuity, or fidelity of replication, ensures that we are examining change within a lineage rather than merely a temporal sequence or a case of convergence. Distinguishing between historical and heritable continuity takes on added importance when one carries out comparative studies, which rely on understanding patterns of descent in order to examine the distribution of adaptive (functional) features (e.g., Borgerhoff Mulder et al. 2001; Holden and Mace 1997, 1999; Mace and Pagel 1994; McQuown and Pitt-Rivers 1970; see review in Borgerhoff Mulder 2001). The modern comparative method (see Harvey and Pagel [1991] for its use in biology) is designed to escape

what Francis Galton pointed out in 1889: Comparative studies of adaptation are irrelevant if we cannot rule out the possibility of a common origin of the adaptive features under examination (Naroll 1970). This point is discussed in more detail by Hector Neff and Daniel Larson in Chapter 11.

Using Darwin's phrase "descent with modification" does not mean we perceive cultural evolution—literally, the production of artifact lineages in archaeology—to be only anagenetic and gradual (*contra* Larson 2000).¹ It has long been recognized (Gladwin 1936; Kroeber 1931; Steward 1944) that cultural lineages not only diverge but converge and intermingle, and thus cultural evolution comprises instances of anagenesis (single, non-diverging line of evolution), cladogenesis (branching evolution producing multiple lines), and reticulation (creation of a dendritic pattern of descent). Further, despite claims to the contrary (Larson 2000), archaeologists clearly recognize that the tempo of change can vary significantly both within and between lineages (O'Brien and Lyman 1999a, 1999c, 2000b). The historical aspect of Darwinian evolutionary theory is meant to address how and more particularly why a particular form came to exist.

Evolution comprises change in the composition of a population over time, and in archaeology the population comprises artifacts, which are viewed as phenotypic features. Robert Leonard and Tom Jones (1987: 215) were the first to make this explicit, noting that "as a quality of the phenotype, artifacts are the equivalents of physical and behavioral traits." Dunnell (1989a:44) underscored this tenet, stating that artifacts "are the hard parts of the behavioral segment of phenotypes." Extension of the human phenotype to include ceramic vessels, projectile points, and the like is based on the notion that artifacts are material expressions of behavior, which is undeniably phenotypic. This extension has been viewed by some (Kehoe 2000; Larson 2000; Loney 2001; Maschner 1998) as problematic, but this view is not held widely outside anthropology. Biologists routinely view things such as

spiders' webs and beavers' dams as phenotypic characters (Dawkins 1990; Turner 2000; von Frisch and von Frisch 1974), and some have explicitly stated that "a culturally acquired behavior becomes part of the overall phenotype" (Cavalli-Sforza and Feldman 1981:14). Artifacts are undeniably temporally enduring manifestations of behavior. Archaeology's unique claim within the natural sciences is therefore its access to past phenotypic characters. Ethnographers, historians, and others who study humans are limited to living humans or written records; only archaeologists have access to the entire time span of culture, however one chooses to define it.

The significance of this statement is found in a parallel with paleobiology. Modern biologists who undertake cladistic analyses might protest that the fossil record is unnecessary to determine the phylogenetic history of organisms, but this position is losing ground as paleobiologists more frequently use the fossil record to help test cladistically based hypotheses of phylogeny (Clyde and Fisher 1997; Donoghue et al. 1989; Fisher 1994; Forey 1992; Fox et al. 1999; Hitchin and Benton 1997; Huelsenbeck and Rannala 1997; Norrell and Novacek 1992; Novacek 1992; Smith 1994, 2000; Wagner 1995). The important point is that historical questions are the most obvious ones archaeologists can ask, although admittedly this is hardly a strong warrant for asking them. But if the issue is evolution, then historical questions *must* be asked. This in no way implies that historical questions are the *only* ones that must be posed, but they are the ones that address why certain manifestations occupy particular positions in time and space (Bettinger and Richerson 1996). Here we are assuming a fundamental difference between historical concerns and chronological concerns. The latter seek merely to arrange phenomena in a temporal sequence. The former require not only chronological coordinates for phenomena but also seek temporally ordered cause-effect relations between phenomena. This is in no way meant to imply that all historical questions concern evolutionary mechanisms;

clearly, however, all evolutionary questions are historical in the sense indicated.

Any evolutionary investigation is a two-step process. First, lineages are constructed, here cultural (or more correctly, artifact) lineages, and second, explanations are made for the lineages being the way they are (O'Hara 1988). Both steps are theoretically informed. With Darwinian evolutionism as a guide, the first step comprises the documentation of descent with modification, and the second step involves the identification of the mechanisms that caused the changes or periods of stability within a lineage or series of related lineages. Accomplishment of the first step requires tight chronological control and documentation of heritability between the archaeological manifestations comprising the lineage(s) (O'Brien and Lyman 2000b, 2002a). Accomplishment of the second step requires that hypothesized mechanisms, such as natural selection, that result in sorting be tested during periods of change and that the hypothesized absence of sorting (presence of stabilizing mechanisms) be tested during periods of stasis. The analytical challenge is to determine which is applicable in any given situation. This challenge demands the study of immanent properties and processes and the construction of laws concerning them (Gould 1986; Simpson 1963, 1970), as well as the construction of a set of units for measuring and describing a lineage's fossil record—that is, for writing a historical chronicle. Explaining *why* a lineage is configured the way it is demands that the uniqueness of historical contingencies be considered. These points have been made time and again by biologists (Burian 1988; Lewontin 1974; O'Hara 1988; Szalay and Bock 1991) as well as by archaeologists (Dunnell 1980a, 1992a; Lyman and O'Brien 1998; O'Brien and Holland 1990, 1992; Wolverton and Lyman 2000).

Most archaeologists with an interest in evolution probably would agree that evolution comprises "any net directional change or any cumulative change in the characteristics of... populations over many generations—in other words, descent with modification" (Endler 1986:5). What is not explicit in this

definition is the requirement of heritability effected by transmission and the resultant replication of ancestral forms by descendant forms. *This* is what makes evolution distinct from mere change: Fidelity of replication resulting from transmission results in descendant phenomena resembling to greater or lesser degrees their ancestors. There are three minimum requirements for evolution to occur: (1) variation exists among individuals (at some scale), (2) variant characters are inherited (requiring transmission), and (3) variants are replicated at varying frequencies. The concepts of *replicators* and *interactors* are useful for exploring these requirements (O'Brien and Lyman 2002a).

Replicators and Interactors

The three minimum requirements for the occurrence of evolution noted in the preceding paragraph are affected in biology by genes, which serve as the units of transmission and inheritance and also as the source of variation among phenotypes. Simplifying, selection for a particular portion of the range of phenotypic variation and against other portions results in Darwinian evolution of the gene pool, mediated in part by adaptive plasticity (see below). Various researchers have noted parallels between the processes and patterns of biological evolution and those of cultural evolution and have sought a unit of cultural heritability that creates variation upon which selection can act. Several names have been applied to such a unit, including *mnemotype* (Blum 1963), *culturgen* (Lumsden and Wilson 1981), and *meme* (Dawkins 1976, 1982). The first two were poorly formulated in terms of scale—their formal definitions aligned the denoted concepts more closely with genotype than with gene—and have not seen wide use. The third, *meme*, has become the term of choice because its definition closely aligns in scale with a unit of cultural inheritance (e.g., Blackmore 1999; Wimsatt 1999).

Some investigators (e.g., Cavalli-Sforza and Feldman 1981) have suggested that a *meme*—abbreviated from the Greek root *mimeme*, for imitation—is not a discontinu-

ous unit, where a gene *is* discontinuous. In particular, the scale of a gene (relative to an organism) is known whereas the scale of a meme (relative to a culture) is not. The anthropological concept of cultural trait—defined by Clark Wissler (1923:50) as a “unit of culture”—used during the first half of the twentieth century did not help resolve the scale issue because cultural traits could be of virtually any scale. The issue was further conflated by the fact that cultural traits could be either meme-like (concepts in the minds of people) or they could be empirical (artifacts or behaviors). Critics of the meme concept argue that the analytical difficulty resides in the unclear scale of a unit of cultural transmission relative to a cultural trait (as an empirical character of a culture) of analytical interest, their reasoning apparently being that no such scale problem attends the biological unit of transmission—gene. But we note that a phenotypic trait of an organism can be polygenic (influenced by multiple genes) and that pleiotropy also occurs (a single gene influences multiple traits). Therefore, there is no necessary one-to-one correspondence between a gene and a somatic trait. There has in fact been a “long-standing debate in genetics about how large or small a fragment of a genome ought to count as the replicating unit (that is, as something that is copied and which can be treated separately). [For various reasons] individual genes cannot be considered as replicators, because they do not behave as separable units during reproduction” (Lloyd 1992:337). An early, clear statement of the issues and an attempt at resolution is found in Lewontin (1970), but the discussion continues (e.g., Lloyd 1992; Sterelny et al. 1996). Our point is simple. In biological evolution the units of heredity and transmission are known, but biologists still grapple with the problem of which gene(s) or allele(s) controls which character(s) or character state(s) of an organism. To demand that a one-to-one correspondence be found between a particular unit of cultural transmission and a particular cultural feature is to demand more knowledge of cultural heredity than of biological heredity.

Some commentators have protested that it is unclear what, exactly, a meme *is*, which limits its analytical value (Aunger 1999). Again, this seems to us to be an insignificant problem. Darwin and subsequent evolutionists went far in explaining biological phenomena with an equally vague notion of the units of biological transmission (genes) prior to the Synthesis of the late 1930s and early 1940s. This is not to say that we think the search for a workable and empirically verifiable concept such as a meme should be halted, but neither do we think we should stop all efforts to explain the archaeological record in evolutionary terms until such a discovery is made. We need a label for a concept like meme—“a unit of information residing in a [human] brain” (Dawkins 1982:109)—and at present we prefer the term *replicator* for that unit of cultural transmission because it implies nothing about the scale of the unit. The related term *interactor* concerns the somatic or phenotypic traits or units directly influenced by natural selection. To help clarify the discussion in the following, when necessary we use the term replicator followed by meme or gene in parentheses to distinguish the kind of replicator—cultural or biological—under consideration.

Irrespective of whether the units are biological (genes, somatic traits) or cultural (memes, artifacts, behaviors), the concept of replicator denotes units that are more or less equivalent to genes, and the concept of interactor denotes units more or less equivalent to the phenotype (Hull 1980, 1981, 1988a, 1988b; see also Lloyd 1992; Sterelny et al. 1996). It cannot be overemphasized that replicators and interactors are *concepts* rather than things, and it is important to be clear as to what they comprise conceptually. A replicator is an entity that passes on its structure directly through replication, and an interactor is an entity that directly interacts as a whole with its environment in such a manner that replication is differential as a result of selection (Hull 1980). This is an important point because differential replication (sorting) can also be the result of the vagaries of transmission (the replicators available for

transmission, the replicators actually transmitted, how often a replicator is transmitted, and so on) rather than the result of natural selection—a process in which the differential extinction and proliferation of interactors result in the differential perpetuation of the replicators that produced them (Hull 1980).

Natural selection results in sorting, but so do the vagaries of transmission. One analytical challenge squarely faced by archaeology is to determine when sorting results from transmission mediated by natural selection and when it results merely from the vagaries of transmission (Beck 1995a). This challenge is addressed by particular analytical units constructed to measure each kind of sorting (Lyman 2001; Meltzer 1981[6]). Making the matter more difficult is the fact that the fidelity of replication can vary, which begs the question of how far one can bend the fidelity requirement and still have replication (Heyes and Plotkin 1989)—a matter of scale that must be dealt with analytically on a case-by-case basis (Hull 1981; Lewontin 1970) and that depends on the analytical units used. Replicators and interactors within any given section of any given lineage—whether of organisms or of artifacts—can be of the same or different scales.

Selection works directly on interactors and only indirectly on replicators. Evolution is not equated precisely with changes in the frequency of classes of replicators—evolutionary processes can result in stasis—but such changes typically accompany evolution. Evolution can be described as a change—not to preclude stasis—in adaptation and diversity (Mayr 1991), which is manifest as a change in relative frequencies of classes of (typically phenotypic) phenomena. Behavioral ecologists would probably agree with this discussion, but they would argue that much of the adaptational change we see with respect to human cultures is the result of phenotypic plasticity (Boone 1998; Boone and Smith 1998)—that is, a change in interactors (cultural traits) involving no change in replicators (memes). We have no quarrel with that caveat, but we also note that such change is

better labeled “adaptive plasticity” because the change is in response to interaction with the environment (Brandon 1985).

The Scale of Change

One of the major difficulties many archaeologists and anthropologists have with using Darwinism to write and explain a cultural lineage’s history resides in a set of interrelated arguments that were well expressed recently by Neff (2001). These can be summarized as follows: (1) the operation of cultural transmission is independent of genetic transmission; (2) differential replication of cultural characters has nothing to do with differential biological reproduction of humans; (3) genetic inheritance and cultural inheritance are completely independent of one another; and (4) cultural evolution is faster than biological evolution. These statements are correct in the sense of specifying empirical generalizations derived from the ethnographic record of numerous cultures, including our own—such as the fact that changes in computer technology are more rapid than biological change in humans (Neff 2001)—but the fact that such things are possible does not mean that they are inevitable (O’Brien and Lyman 2000d).

Cultural transmission is largely independent of biological transmission (e.g., Boyd and Richerson 1985; Cavalli-Sforza and Feldman 1981), but this does not mean that cultural transmission will *never* be correlated with or causally related to the degree of genetic relatedness between a transmitter and a receiver. Considerable cultural transmission occurs between close genetic kin, less occurs between remote genetic kin, and even less occurs between nongenetic acquaintances in groups whose social organization is founded on kinship rather than on economic status. This is an empirical matter. Nonetheless, just because the two transmission mechanisms are independent does not mean they will not occasionally be correlated and even causally related. The same reasoning applies to the second argument listed above: Just because biological reproduction and artifact replication are independent does not mean they will

never be correlated or causally related. This also is an empirical matter requiring further testing on a case-by-case basis.

The independence of biological and cultural evolution must be treated as a null hypothesis (O'Brien and Lyman 2002a). Although the hypothesis can be tested in an ethnographic setting, a significant problem remains: We want to explain the archaeological record, not the ethnographic record. Simpson (1944) grappled with the problem of genetic change being invisible to the paleontologist. The result of his efforts was the distinction between what today are termed microevolution and macroevolution, the former concerning what a biologist can perceive among living organisms—genetic change—and the latter concerning what a paleontologist can perceive between species and higher taxa, or large-scale phenotypic change. We seriously doubt that archaeologists will ever be able to perceive change in cultural replicators (memes) the way an ethnographer might. But archaeologists *can* see change in artifacts of various scales. Granting that artifacts are likely “polygenic,” or more properly, poly-replicator—what Neff (1993) refers to as the result of “recipes” and what we define as a set of ingredients and a set of rules for putting them together to produce an artifact—archaeology’s hand typically is forced to study macroevolutionary phenomena (O'Brien and Lyman 2002a). This comprises the issue of scale mentioned above. One advantage that cultural transmission has is that transmission is freed from more or less fixed units, meaning that transmitters and receivers both can analyze recipes into constituent parts (replicators) (Dunnell 1992b). This conclusion has significant implications for what Neff (2001) terms the “Cree Snowmobile fallacy.”

Following Ann Ramenofsky (1998), we (Lyman and O'Brien 1998) hypothesized that Cree fitness measured as *reproductive success* increased as a result of increased use of snowmobiles. Either that, or the increased use was a result of drift. Several individuals, including Neff (2000, 2001), argued that the hypothe-

sized change in reproductive success was unreasonable because snowmobile use reached fixation in less than a human generation. Further, it reflected human choice, not the action of natural selection (Boone 1998). To help place our take on these criticisms in perspective, we note that the adoption of snowmobiles by Skolt Lapps of northeastern Finland took place in less than a decade and resulted in major economic, political, and social change in less than 15 years. Importantly, the ethnographer who documented this phenomenon (Pelto 1973) wondered about the long-term results it would have on these people and their culture. Four points are worth making.

First, phenomena such as the rate of snowmobile adoption by individuals, changes in the relative socioeconomic status of individuals, and changes in political structure through a series of steps all comprise changes in multiple cultural replicators. These would be archaeologically invisible because each such microevolutionary change occurs in a matter of weeks or months.

Second, because humans can intentionally make choices about how to behave in particular situations based on learned possibilities (Boyd and Richerson 1985), they are considered by some to be adaptively plastic and thus not subject to the forces of natural selection (Boone and Smith 1998). Numerous nonhuman taxa, however, are ecophenotypically plastic, but this does not completely shelter them from natural selection and in fact sometimes presents selection with variation on which to work (Agrawal 2001; Robinson and Dukas 1999; West-Eberhard 1989). Further, the conclusion that adaptive plasticity shields humans—or any organism—from natural selection is an empirical generalization founded on temporally limited observations of microevolutionary processes. Finally, to suggest humans can dodge natural selection by making choices regarding behaviors is to suggest that artificial selection and natural selection are separate and distinct. Modern biology, however, takes the former to be a special case of the latter (Hull 1988a).

Merely because people can eliminate some possible behaviors from consideration based on knowledge of possible outcomes of acting out those behaviors in particular situations does not mean they will always choose a behavior that is immune to natural selection (Stein and Lipton 1989). Modern evidence that they will not is provided by the so-called greenhouse effect, the long-term macroevolutionary effects of which on the human genotype and human culture will be determined only after the passage of more human generations.

Third, archaeologists regularly consider the long-term effects of cultural (human behavioral) change on human reproductive success, largely because of the lack of intra-generational temporal resolution afforded by archaeological chronometers (Jones 1998; Leonard 2001). Such an admission does not deny the importance of intent and decisions regarding change over time in human behaviors. Rather, it underscores that as archaeologists we must shift the level of cause from those that are microevolutionary, proximate, and archaeologically invisible to those such as natural selection that are macroevolutionary, ultimate (Mayr 1961), and archaeologically visible. That we believe that this necessity will not only result in various disputes but also eventually produce a workable solution in archaeology is predicated on the success of punctuated equilibrium in paleobiology (Eldredge 1989a, 1999; Gould 2002), despite the continued existence of a chasm between (genetic) microevolution and (inter-taxonomic, paleontological) macroevolution (Bell 2000). The precise analog of this chasm drives the contentious issue of human adaptive plasticity (Boone and Smith 1998; Leonard 2001; Lyman and O'Brien 1998)—something readily observable at the microevolutionary scale of the ethnographic record but invisible at the macroevolutionary scale of the archaeological record.

Fourth, the microevolution-macroevolution-scale distinction does not represent an insurmountable hurdle to evolutionary archaeology, because archaeologists can di-

rectly monitor the *replicative success* (Leonard and Jones 1987) of cultural characters—a form of differential persistence of variants that can be completely independent of biological reproduction. The replicative success of a particular cultural character may or may not affect the reproductive success of the bearer (Leonard 2001), which means that characters may be replicated irrespective of whether they confer a selective advantage or disadvantage on their bearers—an aspect of cultural characters to which we return in our discussion of style and function. Evolution involves the transmission and replication of replicators. There are no agreed-on units of cultural transmission, but we can define them theoretically as the largest units of socially transmitted information that reliably and repeatedly withstand transmission (Pocklington and Best 1997). The frequency of such units of information is a product of transmission, selection, and innovation (Cochrane 2001). As archaeologists, we want to measure the effect of transmission on variability, and culture-historical types are entirely reasonable as proxies for the units of cultural transmission (Lipo and Madsen 2001; Lipo et al. 1997[3]). The replicative success of these units is what evolutionary archaeologists monitor and look to explain (Leonard 2001; O'Brien and Lyman 1999c, 2000b, 2000c).

ISSUES IN MODERN EVOLUTIONARY ARCHAEOLOGY

Evolutionary archaeology has dealt with numerous issues in its attempt to rewrite Darwinian evolutionism in terms that are applicable to the archaeological record. The four points we made with respect to snowmobile adoption and microevolutionary and macroevolutionary scales comprise an incomplete sketch of how Darwinian evolutionary theory can be rewritten in archaeological terms. We chose three additional topics for extended discussion in the remainder of this paper: units and their construction, heritable continuity and lineage construction, and transmission. The topics are purposefully

broad categories that allow us to explore several related issues—style, function, selection, and drift—that cross cut discussions in the chapters in this book.

Unit Construction

Evolutionary archaeologists have debated the merits of various units that have been proposed to track phenotypic change through time (Beck 1995b[7]; Dunnell 1995; O'Brien and Lyman 2000b, 2002b; Teltser 1995b)—a perhaps not unexpected occurrence given the centrality of unit construction in Americanist archaeology from the start. Despite the overwhelming array of typologies and procedures for constructing them that can be found in the Americanist archaeological literature of the twentieth century, the categories fall into several general kinds, the descriptors of which tell us something about the kind of analytical work the individual units are supposed to accomplish. For example, Julian Steward (1954) distinguished among what he termed “morphological,” “historical-index,” and “functional” types. The first comprised units that were elementary and descriptive; the second were markers used to identify chronological and spatial differences; and the third were based on use. These three kinds of units remain the mainstays of Americanist archaeology.

Regardless of the kinds of categories that exist and of the immediate purposes to which units are put, archaeologists, like other scientists, categorize things for one reason: to identify variation. As important as identifying variation is, there must be a theory that explains the variation. *Systematics*, or a logical means of creating a set of units for a specified purpose, provides the necessary linkage between theory and the empirical world. Archaeological categorization schemes tend to be idiosyncratic, with each investigator either creating his or her own system or making substantial modifications to an existing one. Despite the occasional adoption of standardized units, rarely is it asked whether the units are appropriate for the analytical purposes to which they are being put.

Because it concerns heritability and continuity, Darwinian evolutionism is a way of thinking about populations of phenomena—groups that at once differ among themselves while simultaneously comprising individuals that are not clones of each other. Further, Darwinian evolutionism views relations among phenomena as being time- and space-bound, and as such it contrasts sharply with the ontology of the physical sciences, in which the essential properties of an object dictate whether it is placed in one group or another and variation among objects in a group is viewed as little more than annoying distraction (Lewontin 1974). Darwinian evolutionism requires units that exactly and precisely allow differences in time and space to be measured. The issue of the appropriateness of particular kinds of units is addressed in detail by Melinda Allen (Chapter 5), Charlotte Beck (Chapter 7), and Robert Bettinger and Jelmer Eerkens (Chapter 4).

Archaeologists with an interest in evolutionism have consistently made the distinction between two kinds of units—empirical (real) units and theoretical (measurement) units—the latter defined as units that have explanatory significance because of and only because of their theoretical relevance. Much of the empirical research that has been done in evolutionary archaeology has by-passed traditional archaeological units and employed a particular kind of theoretical unit, the *class*, which is a measurement unit that specifies the necessary and sufficient conditions that must be displayed by specimens in order for them to be identified as members of that class (Dunnell 1971). The important point here is not that there is no role for traditional units such as named artifact types, because some of them are excellent at what they are supposed to do, such as tracking the passage of time (e.g., Beck 1998; Thomas 1981, 1983b; Thomas and Bierwirth 1983). In some cases traditional types can be used in studies of cultural transmission, as demonstrated empirically in Chapter 3 by Carl Lipo, Mark Madsen, Robert Dunnell, and Tim Hunt; in Chapter 4 by Robert Bettinger and

Jelmer Eerkens; and in Chapter 12 by Lee Lyman and Michael O'Brien. But even the most useful types are not multipurpose units, nor are classes. Rather, they are useful for *specific* analytical purposes.

Whether or not archaeological units are "discoverable"—an issue that in the early 1950s became one of the most hotly debated topics in Americanist archaeology (Lyman et al. 1997; O'Brien and Lyman 1998)—is less important than the issue of whether or not the units used perform their intended analytical purpose. We believe that units, particularly classes, built intentionally on the basis of theoretical considerations would more often fulfill their intended analytical role than units believed to be recognizable or discovered in a pile of specimens. This is why evolutionary archaeologists have argued that whether archaeological units measure the properties of interest or not must be tested (Lyman and O'Brien 2002; O'Brien and Lyman 2000b, 2002b). Such testing also allows one to contend with the fact that variation is more or less continuous whereas classes comprise discontinuous units. Testing should be capable of indicating whether the discontinuous units (classes) of, say, 1–30 degrees, 31–60 degrees, and 61–90 degrees for analyzing the edge angles of stone tools measure that continuous character in analytically useful ways, or if units of 10 degrees each are better.

Heritable Continuity and Lineage Construction

If two things are similar but also somewhat different in form and also different in age, do they indicate that change has taken place? For example, if we chronologically align a sample of projectile points, does this ordering represent change? From a Darwinian viewpoint change is represented *only* if the temporally aligned things are phylogenetically related, in which case the similarity of form and difference in age signifies inheritance and thus continuity—an ancestor-descendant lineage. If we cannot establish heritability—establish that two things are related by ancestry—we cannot be sure that we are not

dealing merely with a chronological relationship—that object Y simply follows object X in time. It is establishing a hereditarily based link between the two that is important in an evolutionary study. Biologists and paleobiologists do this by separating out analogous characters from homologous characters—not always an easy task (Fisher 1994; Smith 1994; Szalay and Bock 1991)—and then subdividing homologous characters into shared ancestral and shared derived characters. Only the latter are used to construct hypotheses about specific phylogenetic histories.

Style and Function. Anthropologists have long been interested in the problem of how to distinguish between analogs and homologs—an interest shared by culture historians of the mid-twentieth century and processual archaeologists of the 1960s on (e.g., Binford 1962, 1968; Jelinek 1976; Sackett 1982). A. L. Kroeber (1931:151) was the first anthropologist to draw explicitly from Darwinism when he noted that 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." Culture historians and processualists identified various processes—diffusion, contact, independent invention—that might account for the rise of different kinds of traits, but these were connected only loosely to a theory that might help explain *why* one particular process as opposed to another acted where and when it did to produce one kind of trait as opposed to another kind (Lyman and O'Brien 1997).

Archaeologists with an interest in evolutionism have also underscored the importance of different kinds of traits, but the discussions that have grown out of this interest (e.g., Dunnell 1978a; O'Brien and Holland 1990; Rindos 1989) have not always been clear and thus have led to criticism (Alvard 1998; Boone and Smith 1998; Ortman 2001). For example, Bettinger et al. (1996[1]:134) claim that "Too much time and effort go into

style for it to be neutral!" We suspect that they mean individual *styles* as opposed to *style*. We take the latter to be a theoretical construct and the former as empirical phenomena. In the following we begin by exploring the source of the confusion and the ontology of style in an effort to reveal epistemological solutions to distinguishing style and function in the archaeological record.

The roots of the problem are traceable to Dunnell's (1978a) "Style and Function: A Fundamental Dichotomy," in which he attempted to tie the concepts of adaptively neutral characters (traits) and fitness-influencing characters into an evolutionary framework with the terms *style* and *function*, respectively. He defined functional characters, at whatever scale, as those that interact with their natural and artificial environment and influence fitness (see Dunnell 1978b). Styles, or stylistic characters, are those that do not have detectable fitness values. The latter definition is in large part the source of the criticism: Because styles minimally have a production cost (if not also maintenance costs), they must not really be adaptively neutral. But Dunnell (1978a), in fact, clearly stated that all characters have a cost in terms of energy, space, and matter and are thus an unavoidable part of the whole selective picture—a point reiterated a few years later by Meltzer (1981[6]). Dunnell also noted that (1) a reservoir of selectively neutral variability—*some* of which, through changing conditions, may ultimately acquire adaptive value—has clear selective value, and (2) analytically this can be treated as a problem of scale—meaning that specific adaptively neutral forms are functionally equivalent manifestations rendered as attributes of larger entities that can be accounted for in terms of natural selection. These observations were repeated in the seminal substantive study to employ the dichotomy in an effort to sort artifacts into stylistic and functional categories (Meltzer 1981[6]), but all such remarks have largely been overlooked by proponents and critics of the dichotomy alike. One result has been a recent book devoted to explicating the style-function dichotomy (Hurt and Rakita 2001b).

It is an excellent overview of the subject, with many of the chapters in it building on the groundwork laid in the chapters included here.

In specifying the dichotomy, Dunnell was denoting theoretically derived concepts that accounted for observations (Cochrane 2001; Dunnell 2001; Lipo and Madsen 2001). The concepts themselves are multifaceted, and that is why it is important to note that the style-function dichotomy is definitional, not methodological (O'Brien and Leonard 2001). Style and function are theoretical constructs, not empirical ones (Hurt and Rakita 2001a). The concept of style is derived from the neutral theory of genetic evolution, where it is clear that the fitness or susceptibility to selection of many replicators is not absolute but rather is relative to other replicators, including but not limited to their own alleles. Thus, *styles* (plural) are equal-cost alternatives (Dunnell 2001), and *style* (singular) denotes those variants that are functional equivalents. The U.S. Mint is now producing state-commemorative quarter dollars and will eventually produce 50 styles of them (or more, if territories eventually are included). Each will be the functional equivalent of the other at the scale of discrete coin, but each style will vary at the scale of the design motif (character state) signifying the state a particular coin commemorates.

Lest one think style is restricted to artifacts, we note Richard Lewontin's (1978) discussion of the one-horned Indian rhinoceros and the two-horned African rhino. He suggested the best evidence indicates that "horns are an adaptation for protection against predators, but it is not true that one horn is specifically adaptive under Indian conditions as opposed to two horns on the African plains. Beginning with two somewhat different developmental systems, the two species responded to the same selective forces in slightly different ways" (Lewontin 1978:228). Here we have a classic matter of scale: The character of "horns" is functional; the character states—one horn or two—are stylistic. Of course, not all variants have functional equivalents, else natural selection

would have no role in evolution. But the message is clear: Do not confuse having a horn or not having a horn (a character) with the number of horns (the character state). Note particularly the scale change. We suspect that often such a scale change is where style and function can be distinguished in empirical cases, as it is with state-commemorative quarter dollars.

We can use a biological analog, in extremely simplified form and with no attention paid to intervening agents, to illustrate the distinction between how stylistic and functional characters behave over time. A functional character (or character state) that is not being selected *against* will begin at some arbitrary point above zero and increase in frequency at a steadily decelerating rate toward some optimal value. This and only this gives selection its apparent directional component. Selection against the character results in selection against replicators (genes) of the character and reverses the trend and decreases the frequency. Two possible outcomes exist: Either the character eventually disappears or, if different expressions of the character confer equivalent fitness (although not necessarily equal under all environmental conditions), then the result can be a balanced polymorphism. Conversely, a character (or character state) not under selection can drift through a population from generation to generation, its frequency fluctuating randomly—sometimes in one direction for a few generations, then in another, and so on, as shown by Fraser Neiman in Chapter 2. Given infinite time, one of two outcomes will occur: Either the character will reach a frequency of zero and thus be eliminated from the population, or it will reach a value of one and become fixed in the population.

Our statement that it is the increase in frequency of a character that gives selection its apparent directional component does not mean that selection is the only evolutionary mechanism that can produce directionality—a criticism that has been aimed at evolutionary archaeology's emphasis on selection as the most important evolutionary process (Bettinger et al. 1996[1]; Boone and Smith

1998). Evolutionary archaeologists *have* considered directional change resulting from processes other than selection (Abbott et al. 1996; Dunnell 1978a, 1980a; Lipo and Madssen 2001; Lipo et al. 1997[3]; Neff 1992; O'Brien and Holland 1990, 1992), as have behavioral ecologists (Bettinger and Eerkens 1997, 1999[4]; Bettinger and Richerson 1996; Bettinger et al. 1996[1]). What has often been overlooked is Dunnell's (1978a: 199) remark that if "the traits in question do not have large positive or negative selective values, and the environmental constraints that bring about selection are not fixed but vary randomly around gradually changing means, we should expect that a fair proportion of the total trait assemblage that makes up the description of an organism would behave on the whole in a rather random fashion even though no individual transmission could be said to be neutral." Dunnell's point is well taken, although his choice of the word "random" was an unfortunate one. What he should have said—and he points this out later in the same article (Dunnell 1978a:200)—is that neutral traits behave *stochastically*. It is the adaptively neutral characters—the *styles*—that behave stochastically, not *style*.

It is important to emphasize that there are different scales at which style can be analyzed, as demonstrated in Chapter 5 by Melinda Allen and in Chapter 6 by David Meltzer. The characteristic random, zig-zag pattern of a neutral character (one not under selective control) characterizes the life history of a *single* neutral character (or character state). Shifting the scale of analysis upward, *styles* are complex units composed of many individual stylistic characters (or character states). Styles come in, they become frequent, and they die out and are replaced by other styles. This behavior makes styles useful for constructing chronologies—a fact well known in Americanist archaeology since the early twentieth century (Lyman et al. 1997; O'Brien and Lyman 1999a). But the battleship-shaped curves that result from this behavior tell us nothing about shifts in frequency of *individual* character states or about mosaic evolution. The difference be-

tween the random, zig-zag pattern and the battleship-shaped curves is attributable in part to the Markovian nature of style, but of equal importance is the fact that, again, styles are constructed of smaller parts. Thus there is a shift in scale from simple to complex as one moves from an examination of the components to the overall style (Meltzer 1981[6]; O'Brien and Leonard 2001; Teltser 1995b). There also is a shift in scale from what can be conceived of as a microevolutionary, single-generational temporal unit to a macroevolutionary, multigenerational unit (Neiman 1995[2]; Teltser 1995b). The individual components might exhibit zig-zag patterns through time, but at more inclusive scales, where the components are lumped and time averaged, the pattern becomes the familiar battleship shape (Lyman and Harpole 2002).

The distribution of styles, because they are adaptively neutral and thus do not affect fitness, should theoretically be different from the distribution of functional forms over time and space. Styles measure interaction, transmission, and inheritance, whereas functional forms *sometimes* measure transmission as mediated by natural selection and at other times measure adaptational change alone (Beck 1995a). The latter may display discontinuous, or multimodal, frequency distributions over time as a result of convergence or of fluctuations in selective environments. This brings us to an important point—the equation of style with homology and the equation of function with analogy. No other single issue has done more to confuse the style-function dichotomy in the archaeological literature than these seemingly straightforward equations.

In the case of style and homology, an equation is justified, but in the case of function and analogy it is not. Functional characters (traits) can be either analogous or homologous, which is why we noted above that functional forms *sometimes* measure transmission as mediated by natural selection and at other times measure adaptational change alone. Homologous characters result from common ancestry. If we are sure that the

characters with which we are dealing are homologous, then the phenomena exhibiting those characters are by definition related back through a common ancestor. In archaeology, the objects are related because of cultural transmission of various kinds (Boyd and Richerson 1985). But this tells us nothing about whether the homologous characters are functional or stylistic. O'Brien and Leonard (2001:5) state, "Do not be misled by the use of the term 'function' in defining what an analog is. . . . The key to whether a feature is homologous or analogous is strictly a matter of its history." A better way of putting it would be, "Although analogous characters can *always* be assumed to be functional, the reverse is not always true. Functional characters can be either homologous *or* analogous."

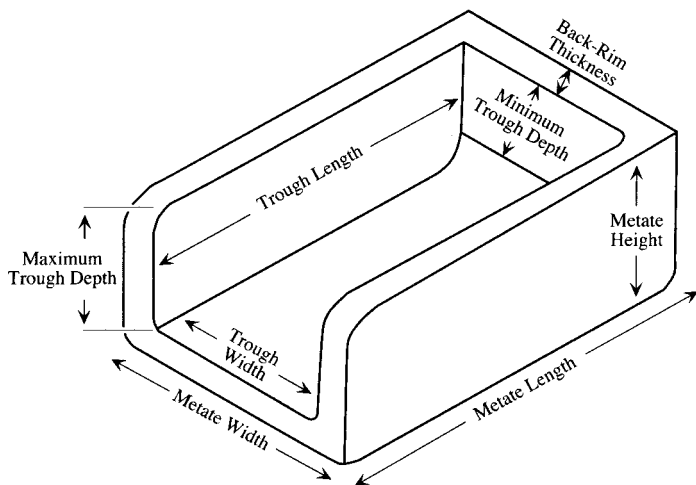
In archaeology we assume that such things as decorations on pots are so complex that the probability of duplication by chance is small (Gould 1986). If we find, say, two ceramic vessels containing identical decoration, we conclude that they are from the same tradition, or line of cultural heredity. They are homologous. We would normally not suspect that two completely independent groups of people arrived at *exactly* the same way of decorating their vessels, given the myriad possibilities available. There is no reason to suspect that we will *never* find such an example, but the more parsimonious explanation of such a phenomenon is that the vessels share a common developmental history and are from the same tradition. Again, scale is an important consideration. Take for example the argument that began in the late 1950s when Ecuadorian archaeologist Emilio Estrada and two American colleagues, Betty Meggers and Clifford Evans (Estrada 1961; Estrada and Evans 1963; Estrada and Meggers 1961; Estrada et al. 1962; Evans et al. 1959; Meggers et al. 1965; see also Ford 1969), raised the possibility of transoceanic contact between Japan and coastal Ecuador sometime around 3000 B.C. The basis for their claim lay in similarities between some of the pottery they were excavating on the coast of Ecuador and pottery they had seen in collections of amateurs and local museums on

Kyushu, the southernmost island in the Japanese chain. Bolstering their claim was the apparent contemporaneity of the Japanese and Ecuadorian pottery as determined through radiocarbon dating. If the pottery on the Ecuadorian coast *was* derived from Japan, how did it get there? The investigators had an answer for this: Japanese fishermen were blown off course, and Pacific currents carried them to the Ecuadorian coast. It was there that they taught local fishermen the art of pottery making. At the scale of individual design elements or small sets of elements, one can see the similarities. However, at the larger scale of complete design, the resemblances quickly fade. This is why we and others have continually emphasized the importance of differentiating among various scales of analysis when discussing style and styles.

Three final points need to be made. First, evolutionary archaeologists have emphasized the usefulness of stylistic traits for chronological purposes, making it appear as if those traits are the *only* kind that have such use, but this again masks the real issue: Is a character or set of characters homolo-

gous or analogous? If only stylistic characters or sets of characters (styles) could be used, then changes in, say, the hafting elements of projectile points, which not only we assume a priori to be functional but which can also be demonstrated empirically to be functional (Hughes 1998), would be useless as a basis for measuring the passage of time. However, this decidedly is not the case, as countless studies have shown (e.g., Beck 1995a, 1995b[7], 1998; Thomas and Bierwirth 1983; Wilhelmsen 2001). Second, we note that further obfuscating the analytical distinction between style and function is the fact that some characters and character states result from constraint—either their historical antecedents affect future possibilities or mechanical requirements necessitate particular characters or character states, resulting in channeling or directional change without selection as the mechanism (Gould 1989, 2002). Third, Darwin's theory is not written to account for characters of use-wear displayed by many artifacts. The net result is that not all seemingly functional characters are the same (Box 1).

Variable	Corrected Coefficient of Variation
Trough Length	9.77
Trough Width	11.34
Total Metate Length	11.39
Total Metate Width	11.90
Total Metate Height	16.74
Back-Rim Thickness	23.35
Maximum Trough Depth	34.70
Minimum Trough Depth	35.85



BOX 1. NOT ALL FUNCTIONAL CHARACTERS ARE CREATED EQUAL: METATES FROM PAQUIMÉ

Style and function are dichotomous conceptual categories, but they carry empirical manifestations. This means that at a particular time-space position a character (an empirical unit) is either functional—meaning that it is under selective control—or it is stylistic—meaning that it is not under selective control. In analytical terms, the distinction is not as clear as it might seem

because functional characters do not necessarily have equal impacts on the fitness of an individual. One measure of character importance is the fitness coefficient: The lower the coefficient, the less importance played by a character. But how can this coefficient be estimated? One means is through the use of corrected coefficients of variation (Sokal and Rohlf 1981), which take into account differences in the absolute size of the variables being measured and the tendency for variation in small samples to be underestimated. Because the amount of variation in a character is likely to reflect the strength of the selective forces operating on that character, corrected CVs can be used as proxies of fitness coefficients: The lower the corrected CV—meaning less variation is present, and thus stronger selection—the higher the fitness coefficient.

Mechanical constraints of performance will also affect the amount of variation within a character and can complicate an ordering of functional characters in terms of their selective importance. Experimental studies and performance analyses, however, should help identify those characters tied to mechanical constraints and should also help identify characters that are more important to the use of an artifact than others are (O'Brien et al. 1994[9]; Pool and Britt 2000[8]; VanPool 2001; VanPool and Leonard 2002). The proposed ranking based on performance and experimental studies can then be compared to the ranking of the attributes based on their corrected CVs. If the rankings are roughly identical, then the corrected CVs probably reflect differences in the selective importance of the characters.

Several evolutionary studies have used corrected CVs in precisely this manner, including David Vaughan's (2001) analysis of Acheulean handaxes and Todd VanPool's (2001; VanPool and Leonard 2002) analysis of basalt metates from Paquimé, a large Postclassic site in Chihuahua, Mexico. VanPool examined nine characters, eight of which (excluding weight) are illustrated in the diagram and listed in ascending rank order according to their corrected CVs. Length, width, height, trough length, and trough width all have roughly the same corrected CVs, which are small in comparison to those of the other characters. Back-rim thickness is more variable, but much less so than maximum trough depth and minimum trough depth.

These findings indicate that large trough metates and two-hand manos are the most efficient means of grinding large amounts of corn and other organics in a limited time. That this form of metate is present at Paquimé suggests that these metates were designed to process corn or other organics quickly; the morphology of the Paquimé metates should, therefore, maximize the area of the grinding surface. Because the length and width of the trough directly control the area of the grinding surface of a metate, trough length and trough width should be functional characters, a finding that is consistent with their relatively low corrected CVs. Total length, total width, and height are probably mechanically constrained by requirements of trough length and trough width and also by transport limitations of the maximum size of the basalt block from which the metate is manufactured. They are thus functional characters with low corrected CVs, but they are not as constrained as trough length and trough width.

Back-rim thickness, which exhibits greater variation than the previously mentioned characters but less variation than the remaining characters, is likely a functional character with an extremely low fitness coefficient relative to other functional characters. Back-rim thickness is constrained by the interplay between length and width of the grinding surfaces and the limits on the size of the metate blanks caused by weight; it is a sorted character controlled by the action of natural selection operating on these other functional characters. Thus natural selection is not operating on it directly. Maximum trough depth and minimum trough depth are a result of the amount of use and are extremely variable as a result.

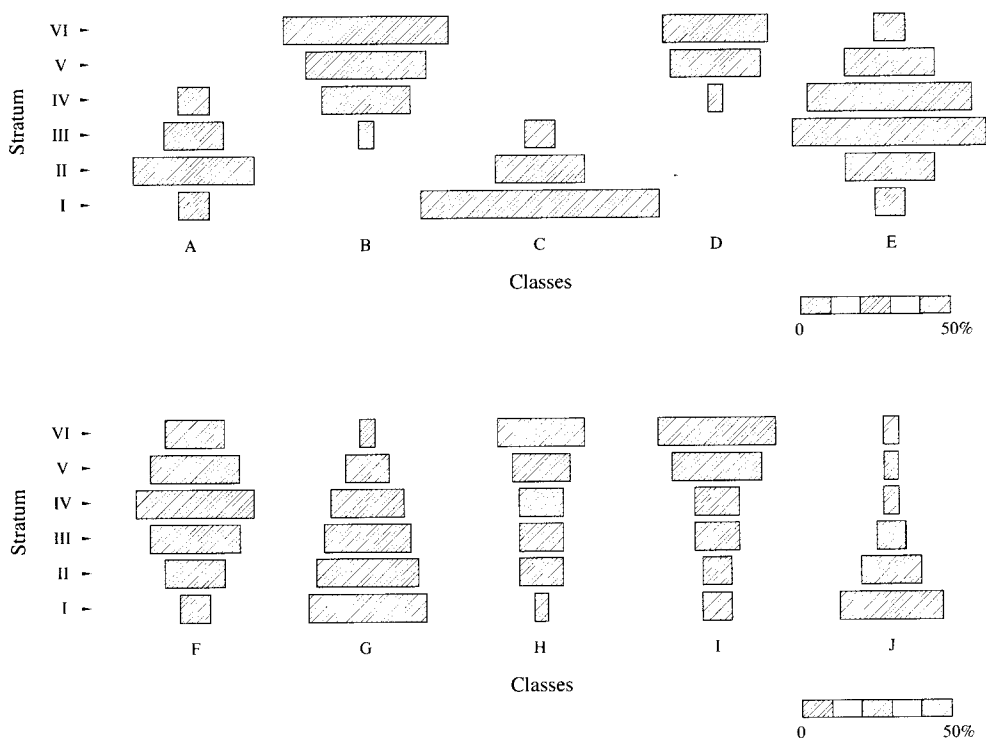


Figure 1. Examples of centered-bar graphs produced by percentage stratigraphy. There are six assemblages, one from each stratum (I–VI), in both the upper and lower examples. In this fictional example, change is faster and turnover more rapid among classes in the upper graph than among classes in the lower graph.

Life-History Graphs. Several kinds of life-history graphs are useful for reconstructing and studying phylogenetic histories of artifact lineages. These include seriation graphs (Lipo 2001; Lipo et al. 1997[3]; Lyman 2001; Neiman 1995[2]; O'Brien and Lyman 1999a, 2000a, 2000b, 2000c, 2000d; Teltser 1995b), cladograms (Harmon et al. 2000; O'Brien et al. 2001), and clade-diversity diagrams (Lyman and O'Brien 2000b[12]). Archaeological interest in life-history graphs is not a new phenomenon. Americanist archaeology has a nearly century-long history of graphing cultural change (Lyman et al. 1998), and its roots can be traced back to nineteenth-century Europe. Such graphs have been produced by means of several methods (Lyman and O'Brien 2000b[12]; Lyman et al. 1998), the two most common being stratigraphic observation (Fig. 1) and seriation (Fig. 2). The use of seriation—a procedure for order-

ing phenomena based on their formal similarity (Rowe 1961)—in archaeology rests on the axiom that formal similarity is an indication of relatedness (Willey 1953). The underpinning and implicit theoretical notion is that formal stylistic similarity denotes a close phylogenetic relationship in a sense analogous to that in biological evolution (Lyman 2001)—artifacts are stylistically similar as a result of cultural transmission. Whether the two phenomena *are* in actuality linked in an ancestor-descendant relationship remains to be demonstrated empirically.

There are several kinds of seriation (Rowe 1961), each of which employs a distinct kind of unit (O'Brien and Lyman 1999a). One technique is phyletic seriation, which has its proximal roots in the biological notion of anagenesis. Archaeologists who perform phyletic seriations talk about one kind of artifact developing or evolving into another or going

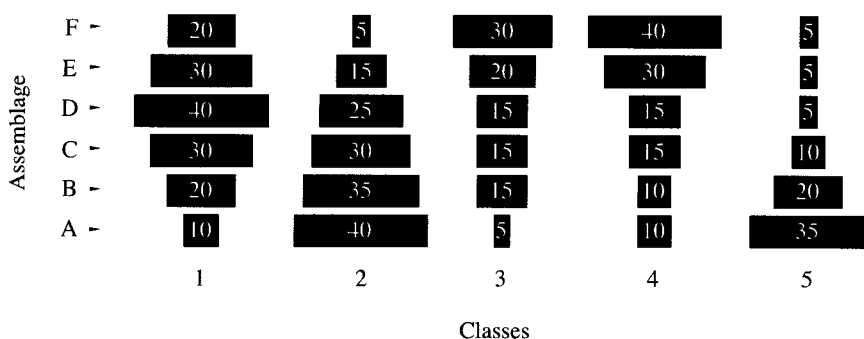
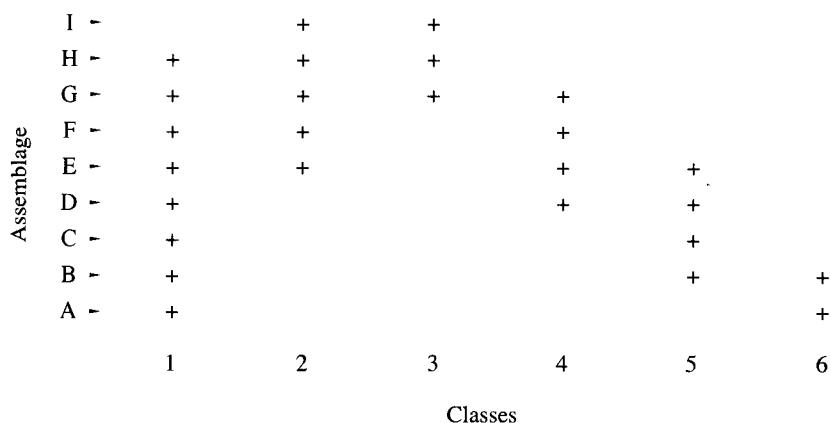


Figure 2. Two kinds of seriation: top, hypothetical occurrence seriation of nine assemblages using six artifact classes; bottom, hypothetical frequency seriation of six assemblages using five artifact classes (assemblages are ordered on the basis of artifact-class percentages, with bars summing to 100 percent for each assemblage). Occurrence seriation assumes a class, or historical unit, will have a single, continuous distribution over time, whereas frequency seriation assumes a class will have a single, continuous distribution over time and also that the relative frequency of each class will fluctuate gradually and unimodally over time. Deviations from these expected patterns can be the result of sampling error, but they might also indicate that the units are not historical classes. Only relative chronological ordering can be achieved through frequency seriation; further, time can run in either direction through the ordered assemblages.

extinct. Phyletic seriation sorts empirical units, or specimens, to reflect a character gradient (O'Brien and Lyman 1999a). Although the temporal implications of such a gradient can be tested with independent chronological data such as stratigraphy, the phylogenetic significance of the gradient is not so easily tested (Koch 1986).

Occurrence seriation and frequency seriation are distinct from phyletic seriation because they measure similarity in a decidedly

different manner. Occurrence seriation and frequency seriation begin with theoretical units, each of which has a temporal distribution displayed by the empirical specimens it contains. Each theoretical unit is explicitly defined at the start of analysis, and specimens in collections are identified as a member of one unit or another based on the definitive characters of the units. Both occurrence seriation and frequency seriation measure the similarity of assemblages of artifacts on the

basis of shared, or “overlapping” (Ford 1938a; Kidder 1924), theoretical units. Occurrence seriation, shown at the top in Figure 2, assumes a historical unit will have a single, continuous distribution over time; frequency seriation, shown at the bottom in Figure 2, assumes a historical unit will have a single, continuous distribution over time and also that the relative frequency of each unit will fluctuate gradually and unimodally over time. As with phyletic seriation, the chronological significance of an occurrence seriation or a frequency seriation can be tested with independent temporal data.

Whether one uses occurrence seriation or frequency seriation to order assemblages, ideally each unit will display a more or less unique temporal distribution, yet each will overlap at least partially with at least one other unit. The overlap of units allows us to infer that temporal continuity is being measured. It is important because it rests on the notion that there is a direct phylogenetic connection between the assemblages that possess overlapping (shared) units (Lyman and O’Brien 2000b[12]). The empirical members of each theoretical unit are by definition identical and thus share the same set of replicators—they are “genetically” related—and thus they monitor transmission, as shown by Fraser Neiman in Chapter 2 and by Carl Lipo et al. in Chapter 3.

Overlapping serves as *the* test implication of occurrence seriation as a measure of heritable continuity; it is the *first* test implication of frequency seriation as a measure of heritable continuity. That the frequency-distribution curves of frequency seriation are expected to fluctuate unimodally rests on the theoretical axiom that transmission unmediated by selection will be stochastic—a replicator will be transmitted and replicated or it will not (Raup and Gould 1974; Raup et al. 1973). That such transmission will produce approximately unimodal curves is based on theory (Teltser 1995b; von Vaupel Klein 1994) and has been demonstrated time and again by computer simulation (Lipo et al. 1997[3]; Neiman 1995[2]; Raup and Gould 1974; Raup et al. 1973). Thus, production of

the familiar battleship-shaped curves of relative abundances of types serves as the *second* test implication of frequency seriation as a measure of heritable continuity.

Unlike phyletic seriation, occurrence seriation and frequency seriation entail test implications for their phylogenetic significance (hypothesized homologous similarities) (Dunnell 1970; Lipo et al. 1997[3]; Lyman 2001; Neiman 1995[2]; O’Brien et al. 2000; Teltser 1995b). Types that are homologous will display particular distributions over time and space; types that are not homologous will display different distributions. The analytical focus of frequency seriation is on changes in relative frequencies of specimens representing each of multiple stylistic variants. One can build and test a hypothesized artifact lineage with the frequency-seriation technique and then monitor change in diversity of either stylistic variants or, after reclassifying the artifacts in functional terms, functional variants. The requisite first step, however, is in building and testing the lineage, and that requires standard frequency seriation of historical types—what Alex Krieger (1944:272) referred to as types that have “demonstrable historical meaning”—to ensure that heritable continuity is being monitored rather than merely a chronological sequence of variation.

Frequency seriation entails three procedural requirements (Cowgill 1972; Dunnell 1970; Ford 1962; Lipo 2001; Lipo et al. 1997[3]; Phillips et al. 1951; Rouse 1967; Rowe 1961; Teltser 1995b). First, the assemblages of artifacts to be seriated must be of similar duration. Meeting this requirement ensures that the placement of particular assemblages in an ordering is the result of their age and not of their duration. The second requirement is that all assemblages to be ordered must come from the same local area. Meeting this requirement, although an analytically complex procedure (Dunnell 1981; Lipo et al. 1997[3]), attends the fact that transmission has both a temporal and a spatial component (Teltser 1995b) and attempts to control for the latter in an effort to measure only the former. This requirement is particularly important because it contends with

the fact that cultural evolution can be reticulate. Identifying “local areas” is thus an empirical matter (Lipo 2001; O’Brien et al. 2000) and must be done on a case-by-case basis if artifact lineages are to be identified accurately. Perhaps the clearest example of how to meet this requirement is Lipo et al.’s work in the central Mississippi River valley (Chapter 3), which makes use of sherds collected by Philip Phillips, James Ford, and James Griffin (1951) in the 1940s.

The third requirement is less a requirement than a theoretically phrased explanation for the temporal behavior of styles. It holds that the assemblages to be ordered in a frequency seriation must all belong to the same cultural tradition, which can be defined as (1) a temporal continuity represented by persistent configurations in single technologies or in other systems of related forms (Wiley and Phillips 1958) or (2) a socially transmitted form unit or series of systematically related form units that persists over time (Thompson 1956). Therefore, if one meets the third requirement of the seriation method, then heritable continuity is assured, and phylogenetic affinities between the seriated assemblages are guaranteed. The third requirement means that the seriated assemblages must be “genetically” related (Dunnell 1970; Ford 1938a; Kidder 1916), where “genetically” is used as a metaphor for cultural transmission. The use of theoretical units, or classes, satisfies this requirement in that similarity is measured not as empirical units that *resemble* one another to greater or lesser degrees, such as in a character gradient represented by a phyletic seriation, but as *changes* either in the presence/absence of theoretical units variously held in common by distinct assemblages or in the frequencies of those variously shared theoretical units. Thus the definition of evolution as changes in variants—rendered either as presence/absence or as relative frequencies—over time is explicitly incorporated into occurrence and frequency seriation.

Like seriation, cladistics seeks to arrange phenomena in time, and also like seriation its results comprise a hypothesis of particular

phylogenetic relations among phenomena. We view cladistics, and the phylogenies it produces, not as end products but as solid starting points from which to begin to answer some of archaeology’s historical, and therefore evolutionary, questions. Archaeologists have recently begun to explore the use of cladistics in creating phylogenetic histories of artifacts (Harmon et al. 2000; Leonard 2001; O’Brien et al. 2001, 2002). The logical basis for extending cladistics into archaeology is the same as it is in biology: Artifacts are complex systems, comprising any number of replicators. The kinds of changes that occur over generations of tool production are constrained, meaning that new structures and functions often arise through modification of existing structures and functions as opposed to arising *de novo*. That is, recombination of memes into new configurations rather than innovation is more common. The history of these changes, including additions, losses, and transformations, is recorded in the similarities and differences in the complex characteristics of related objects—that is, in objects that have common ancestors (Brown and Lomolino 1998).

Placing taxa in ancestral-descendant sequence is done by creating a rooted phylogenetic tree. Homologous characters (traits) are used to place the taxa in putative proper position, but only certain homologous characters are used. These are *shared derived* characters as opposed to *shared ancestral* characters. The difference between these two kinds of characters is illustrated in Figure 3, which shows three rooted trees that are tracking the evolution of projectile-point lineages that stem from a common ancestor. For simplicity we are tracking only a single character, fluting, for which two character states are possible, fluted and unfluted. Over time, Ancestor A, which is unfluted, gives rise to two lines, one of which, like its ancestor, is unfluted and the other of which is fluted (Fig. 3a). Thus the character state “fluted” in Taxon 2 is derived from the ancestral character state, “unfluted.” In Figure 3b, Ancestor B (old Taxon 2) gives rise to two new taxa, 3 and 4, each of which carries the derived character

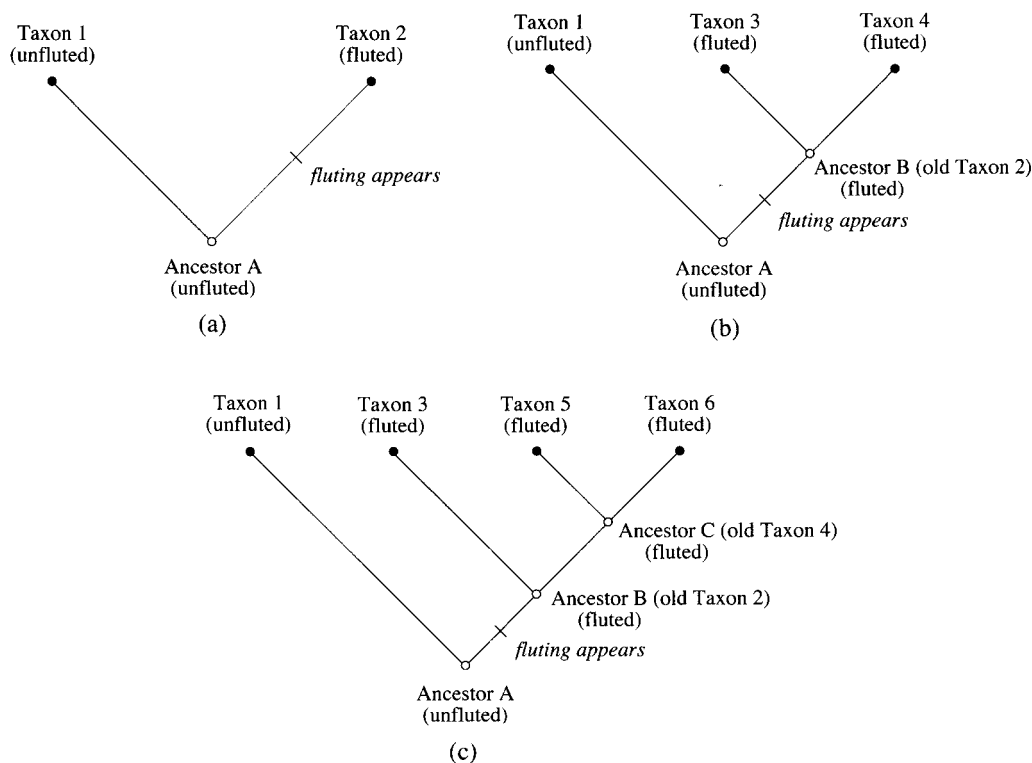


Figure 3. Phylogenetic trees showing the evolution of projectile-point taxa. In (a), fluting appears during the evolution of Taxon 2 out of its ancestral taxon. Its appearance in Taxon 2 is as an apomorphy. In (b), Taxon 2 has produced two taxa, 3 and 4, both of which contain fluted specimens. The presence of fluting in those sister taxa and in their common ancestor makes it a synapomorphy. In (c), one of the taxa that appeared in the previous generation gives rise to two new taxa, 5 and 6, both of which contain fluted specimens. If we focus attention only on those two new taxa, fluting is now a symplesiomorphy because it is shared by more taxa than just sister taxa 5 and 6 and their immediate common ancestor. But if we include Taxon 3 in our focus, fluting is a synapomorphy because, following the definition, it occurs only in sister taxa and in their immediate common ancestor.

state, “fluted.” At this point “fluted” becomes a shared derived character, meaning it is shared between taxa 3 and 4. In Figure 3c, in which two descendant taxa have been added, fluting is now an ancestral character *relative* to taxa 5 and 6. In other words, if we were given taxa 3, 5, and 6, and we looked only at a single character—fluting—we could not be sure which taxon was older than either of the other two. That’s because fluting appeared too early in the ancestral line to have the kind of discriminatory power we need.

We expect several objections to the use of cladistics in archaeology (O’Brien and Lyman 2002a; O’Brien et al. 2001, 2002). First, it might be argued that artifacts do not

breed—akin to Brew’s (1946) statement quoted earlier. This statement is true; tools (interactors) do not breed, but neither do the teeth and bones (interactors) studied by paleobiologists. Tool makers, however, *do* breed, and they *do* pass on information (replicators) to other tool makers. Transmission creates *tool traditions*, which are collections of *tool lineages* (O’Brien et al. 2001, 2002). Second, it might be argued that although there are tool traditions, they are almost impossible to discover archaeologically because of the rapidity with which cultural evolution produces variation. As we noted earlier, the tempo of cultural evolution is much faster than that of biological evolution, but this

hardly means that we cannot see change and track its manifestations. Third, it might be argued that the mode of cultural evolution is different than the mode of biological evolution—reticulate and branching, respectively—and that diffusion and its attendant processes swamp all traces of phylogenetic history, thus reducing the cultural landscape to little more than a blur of hybrid forms. This line of reasoning is not new (e.g., Kroeber 1948; Steward 1944) but has been resurrected in a modern form that seeks to completely preclude cultural phylogenetic studies (e.g., Dewar 1995; Moore 1994a, 1994b; Terrell 1988, 2001; Terrell et al. 1997; Terrell and Stewart 1996; Welsch and Terrell 1994; Welsch et al. 1992).

We agree that some cultural evolution is reticulate, but we do not view this as being epistemologically problematic. For one thing, biological evolution often involves reticulation (Arnold 1997; Doolittle 1999; Endler 1998), especially in the plant kingdom (Levin 2002), but this has not precluded phylogenetic analysis (Nelson 1983; Wagner 1983). More importantly, the term hybridization has been used by critics of phylogenetic analyses (e.g., Moore 1994a, 1994b) to denote any instance of horizontal transmission and thus to signify reticulate evolution. *Hybridization* is the process that produces one or more *hybrids*. Biological definitions such as “hybridization involves successful matings in nature between individuals from two populations... which are distinguishable on the basis of one or more heritable characters” (Arnold 1997:4) give a general sense of the process and the result. We say “general” because several key terms in this definition are unclear. Typically, interbreeding members are conceived of as members of separate species—witness Mayr’s (1969:405) definition of hybridization as “the crossing of individuals belonging to two unlike natural populations, principally species.” This definition follows the biological-species concept popularized by Mayr (1942) and Theodosius Dobzhansky (1937)—species are reproductively isolated populations—and makes it sound as if hybridization is not only rare but

ontologically messy because the analytical purpose of the biological species *unit* is to study speciation and divergence. Despite its generality, the definition of hybridization given above (Arnold 1997) is useful because it, like the similar one of “interbreeding of individuals from what are believed to be genetically distinct populations, regardless of the taxonomic status of such populations” (Rhymer and Simberloff 1996:84), avoids restricting the process to interspecific mating (Endler 1998), thereby underscoring the issue of the scale of units: Are the pertinent units discrete objects, attributes of objects, or aggregates of discrete objects?

Principles of unit scale apply regardless of whether one is studying the phylogenetic history of fossils, cultural groups (Mace and Pagel 1994), or languages (Platnick and Cameron 1977). Goodenough (1997:178) makes an excellent point with respect to language: “Contact between Japan and the United States has resulted in considerable borrowing in language and culture by Japan and some reverse borrowing by the United States, but their languages and cultures retain their respectively distinct phylogenetic identities.” Linguists do not guess as to whether two or more languages share a phylogenetic history. Innumerable case studies have provided the basis for deciding which linguistic characters might be derived—bound morphemes and vocabulary, for example—and which might be analogous—syntax, for example (Nichols 1996). Thus the comparative method groups languages not on the basis of all shared similarities but according to the distribution of shared *innovations* relative to a reconstructed protolanguage ancestral to the whole family (Ross 1997). This is nothing more than separating shared derived characters from ancestral characters and using the former to construct a phylogeny. The same principle applies in archaeology.

Once an artifact lineage has been constructed on the basis of frequency seriation or cladistics, the next task is to explain why that lineage has the appearance that it does. New artifact forms may replace old ones if the former have higher selective values than

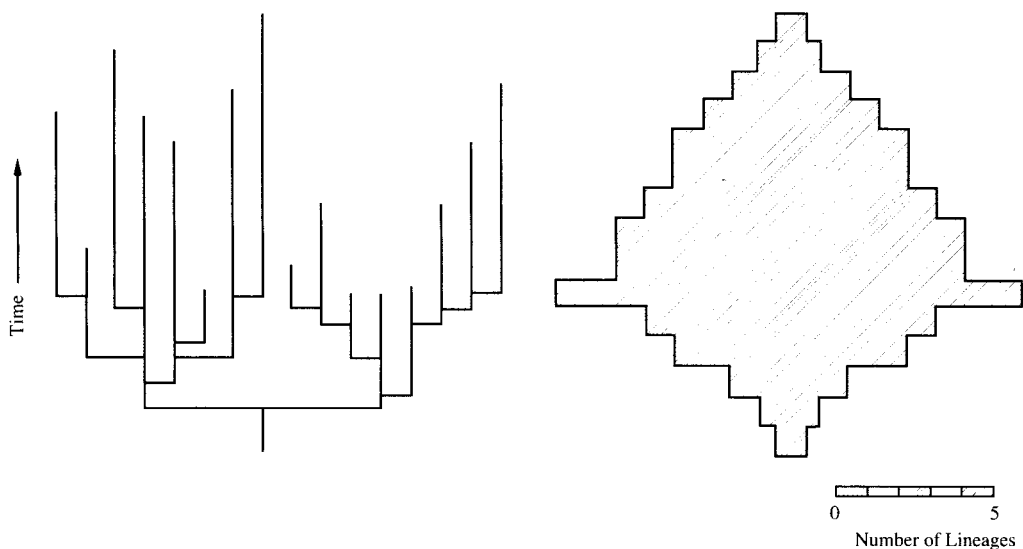


Figure 4. A model for producing a clade-diversity diagram. The phylogenetic history of taxa—lineages and related sets of lineages—is shown on the left, and the resulting clade-diversity diagram is shown on the right. Note that one important aspect of understanding phylogenetic history of related taxa is calculating the starting and ending points of taxa. Once this is done, the data can be summarized as at the right, which shows the waxing and waning of the number of taxa (after Raup et al. 1973).

the latter, or new forms may simply be added to a particular lineage. Paleobiologists have for some time constructed graphs showing the frequency of lower-level taxa within a particular higher-level taxon in order to display the history of life. Many, but not all, of these graphs produced in the last several decades comprise what are referred to as clade-diversity diagrams, which display the fluctuating absolute frequency of taxonomic richness within monophyletic groups over time. We explore this issue in greater detail in Chapter 12, but because clade-diversity diagrams are new to archaeology, we provide a brief introduction to them here.

What is graphed in a clade-diversity diagram is the number of taxa within a higher taxon—the number of orders within a class, the number of families within an order, and so on. A monophyletic group, or clade, comprises one or more lineages having a common ancestor. New lineages arise only by branching or diversification, and each clade contains only and all those taxa deriving from a common ancestor plus the common ancestor. Because a clade-diversity diagram shows

change in the number of extant lineages over time, it comprises a history of the origination and extinction of taxa within a clade, as shown on the left in Figure 4.

Clade-diversity diagrams reveal various evolutionary events that took place and the potential causes of those events. The same kinds of insights can be gained in archaeology precisely because it, too, often is faced with apparent evolutionary trends, although in artifactual rather than organismic variation (Lyman and O'Brien 2000b[12]). A clade-diversity diagram can be easily generated from a frequency-seriation graph. One has but to tally how many classes occur in each temporal interval and then generate the appropriate graph of centered and stacked bars, as shown on the right in Figure 4. A clade-diversity diagram rests on hypothesized phylogenetic relations among the units included in the graph, whereas a frequency-seriation graph tests hypothesized phylogenetic relationships among the included units. Clade-diversity diagrams display fluctuations in taxonomic richness over time; each horizontal bar comprises the absolute frequency of

classes—of whatever taxonomic level—per time interval. The battleship-shaped graphs of frequency seriation display the relative frequency of individual specimens per class per time interval. Several early papers in evolutionary archaeology (Dunnell 1980a; O'Brien and Holland 1990) clumsily stated this difference, making it sound as if the two were more or less equivalent. Notably, some paleobiologists (e.g., Gould et al. 1977) also have confused randomly generated biological clades with archaeological frequency seriations.

Once an artifact lineage has been constructed, artifacts can be reclassified into functional units. Changes in richness and/or heterogeneity of functional classes within an artifact clade over time would, we suspect, reveal much regarding adaptive history (Lyman and O'Brien 2000b[12]; O'Brien and Lyman 2000a). Such evolutionary trends are at the macroevolutionary scale—here, within a clade—and are explicable in macroevolutionary terms (McShea 1994). Similarly, studies of variation in functional characters such as are described in Box 1, if arrayed against time, may reveal much about the history of particular tool traditions as extrasomatic means of adaptation (Gould 1988).

Clade-diversity analyses may grant insight into adaptive plasticity (Lyman and O'Brien 2000b[12]). It might be reasonable to conclude that we are dealing with plasticity were a set of functional classes to change from, say, manifestation A to manifestation B and then back to manifestation A, in concert with changes in the selective environment. Such a conclusion might involve demonstrating that changes in prey population took place coincident with functional changes in artifact assemblages. As we point out elsewhere (O'Brien and Lyman 2002a), archaeologists employing foraging theory as a part of behavioral ecology have shown such changes in prey populations (Broughton 1994, 1999; Broughton and Grayson 1993; Butler 2000; Cannon 2000; Stiner et al. 1999, 2000), but correlations with changes in artifacts have rarely accompanied these studies (see Bettinger 1991c). A caution must be noted, how-

ever, because even with apparent stability in the composition of artifact assemblages rendered in traditional functional terms (e.g., knives, scrapers, and projectile points), how artifacts were used may have changed (Grayson and Cannon 1999), with no apparent change in functional classes. In such cases, classification according to manifestations of use-wear—a different way of measuring function—might (or might not) reveal variation coincident with changes in prey.

Transmission

A key component of Darwinian evolutionism is heritability. A population of organisms can exhibit considerable variation, both genetically and phenotypically, but if there is no means of passing on replicators to succeeding generations, there can be no evolution. Modes of transmission and how those modes bias what is being transmitted play a significant role in both evolutionary biology and archaeology, although for the most part archaeologists have made little effort to model cultural transmission and instead have merely noted that transmission results in such archaeological phenomena as traditions and horizons (Lyman 2001). The situation is different in anthropology, where various individuals have attempted to model cultural transmission. Luigi Cavalli-Sforza and Marcus Feldman (1981:54), for example, noted that whereas biological transmission is only “vertical,” or from parent to offspring, cultural transmission can also be “horizontal”—“between any two (usually [genetically] unrelated) individuals...of the same generation”—and “oblique”—transmission between unrelated individuals of different generations. Interestingly, they suggested cultural transmission “must have been primarily vertical for much of human evolution” (Cavalli-Sforza and Feldman 1981:55), a suggestion with which we agree (Lyman and O'Brien 2001b). Nevertheless, a significant amount of cultural transmission was not vertical, and Boyd and Richerson (1985) have presented detailed models of various cultural-transmission pathways.

We believe cultural transmission has a

direct bearing on the fitness of culture-bearing organisms, as do Cavalli-Sforza and Feldman (1981), although evolutionary archaeology itself has reached no consensus on this point (Lyman and O'Brien 2001b; Neff 2000). There appears to be more consensus among evolutionary ecologists, two of whom, Bettinger and Eerkens (1999:239[4]), sum up the situation in a manner that precisely parallels our reasoning:

It seems clear to us that cultural transmission must affect Darwinian fitness—how could it be otherwise? And Darwinian fitness must also bear on cultural transmission. Again, how could that not be true? At minimum, humans must have the biological, hence, genetically transmitted, ability for the cultural transmission of behaviors that certainly affect Darwinian fitness. It is obvious, at the same time, that cultural transmission differs in fundamental ways from any form of genetic transmission.... Again, this is what we would expect.... [A]s with sexual reproduction, the human use of cultural transmission is simply the exploiting of an evolutionary opportunity. To deny that would imply that the culturally mediated evolutionary success of anatomically modern humans is merely serendipitous happenstance.

Obviously, transmission is integral to the style-function dichotomy, but it also transcends that issue in that the replicators of all characters, stylistic as well as functional, can be transmitted in a cultural system. Thus in terms strictly of transmission, the kind of character is unimportant. What *is* important is whether a particular character gets transmitted at all and the form in which it is transmitted. Is the character that shows up in the recipient a faithful copy—a clone—of the character in the transmitter, or is it different in form or structure? If the latter, is the variation so small that it makes no difference in terms of the recipient's fitness? In other words, are the two states of the character functionally and adaptively equivalent? Of more importance, what effect might that

variation, even if slight, have on much later generations? Slight variations, when coupled with succeeding slight variations, can have cascading effects on organisms far removed generationally from the time when a particular variant first appeared (O'Brien and Lyman 2002a). This issue is addressed head on by Christopher Pool and Georgia Mudd Britt in Chapter 8, by Michael O'Brien, Thomas Holland, Robert Hoard, and Gregory Fox in Chapter 9, and by Robert Dunnell and James Feathers in Chapter 10.

From a methodological standpoint, recognition of the importance of transmission underlies all approaches used to reconstruct cultural lineages, including seriation and cladistics. Thus transmission should be of more than minimal interest to archaeologists. The charge has been leveled (Schiffer 1996), however, that although evolutionary archaeologists pay lip service to transmission, they have been slow to carry out relevant research. They have also been criticized (Boone and Smith 1998) for failing to pay attention to cultural-transmission pathways as well as to secondary forces such as evolved preferences. There is truth to those charges, but part of the criticism might be a result of misreading what evolutionary archaeologists have said. Although they have stressed that selection is blind to the source of variation (O'Brien and Holland 1990, 1992), no one has claimed that transmission represents a relatively unimportant topic of investigation. If Ernst Mayr (1973) is correct that behavior is perhaps the strongest selection pressure operating in the animal kingdom, then we need to take it all that more seriously when the animals are humans, in large part because cultural transmission is a form of behavior.

Neither evolutionary archaeologists nor behavioral ecologists "discovered" the importance of transmission as a research topic. Americanist archaeologists have long had interest in cultural transmission, which is nothing more than a new term for diffusion in various guises. Hector Neff (1992, 1993), an evolutionary archaeologist, could be mistaken for almost any culture historian of the

mid-twentieth century when he states that it is transmission that allows a community or localized group of communities to produce a style of pottery so distinctive as to be easily distinguished from the pottery produced by other centers. Similarly, culture historians Phillips et al. (1951:62) noted that vigorous centers of ceramic production “impress their ideas on less enterprising neighbors,” “popularity centers will be seen for different styles,” and “between these centers, styles vary and trend toward those of other centers in rough proportion to the distances involved, subject of course to ethnic distributions and geographic factors.” Phillips and his two colleagues were joining a long list of Americanists who saw style as a means of identifying prehistoric social groups. This tradition continues unabated, being well expressed in the work of processualists (Binford 1989; Jelinek 1976; Sackett 1982, 1990; Wobst 1977), behavioralists (LaMotta and Schiffer 2001; Schiffer and Skibo 1997; Wiessner 1983, 1984, 1997), evolutionists (Barton 1997; Bettinger and Eerkens 1997, 1999[4]; Lipo et al. 1997[3]; Neiman 1995[2]), and others who are difficult to characterize (Braun 1991, 1995; Braun and Plog 1982; S. Plog 1983, 1990; Shott 1997a).

Despite advances made by archaeologists in understanding the transmission process, perhaps the most significant advances from an evolutionary standpoint have been those by nonarchaeologists, including Cavalli-Sforza and Feldman (1981), William Durham (1976, 1978, 1982, 1990, 1991), and especially Robert Boyd and Peter Richerson (1982, 1983, 1985, 1987; Bettinger et al. 1996[1]; Richerson and Boyd 1992). This work often is referred to as *dual-inheritance theory*, and although there are significant differences in terms of how various authors view the transmission process (Winterhalder and Smith 2000), there are enough similarities that they can be viewed here as complementary. In dual-inheritance theory, genes and culture provide separate, though linked, systems of inheritance, variation, and change. The spread of cultural information is affected

by several processes such as the strengths of the transmitters and receivers, decision making, and natural selection. Several studies by behavioral ecologists have applied Boyd and Richerson’s models of cultural transmission to the archaeological record, one of the more intriguing being that of Robert Bettinger and Jelmer Eerkens (Chapter 4), who examined differences in projectile-point morphology in two locales in the Great Basin. Their study hopefully will spawn similar kinds of investigation.

CONCLUDING REMARKS

We believe the issues raised here are worth considering if we ever hope to place archaeology on a scientific footing, meaning one that emphasizes explanation instead of simply interpretation. We stress that what has become known as evolutionary archaeology and as human behavioral ecology are not unified bodies of principles and methods, but to be taken seriously they must become just that (O’Brien and Lyman 2002a). We do not view it as unreasonable that there always will be debate over how to rewrite Darwinian evolutionary theory in archaeological terms, but we *do* view it as unreasonable to assume that there are *multiple* Darwinian archaeologies—as a recent book title suggests (Maschner 1996)—any more than there are multiple Darwinian biologies. Is there room for theoretical and methodological challenges? Most decidedly so, just as there are in evolutionary biology and paleobiology six decades after the Modern Synthesis (Gould 2002). Despite some of the hype and rhetoric that has accompanied these theoretical and methodological challenges, especially in paleobiology, they are not challenges to the veracity of Darwin’s evolutionary mechanisms.

The only things important for Darwinian evolution are variation, however it is generated, and transmission, however it is realized. Differential reproductive success among organisms will result in large part from selection, drift, and mentalist processes. In a similar vein, replicative success among variant artifact forms will result from the same

processes. Thus we cannot agree with Stephen J. Gould's (1996, 1997) urging that the phenomenon of cultural evolution be labeled cultural development rather than "evolution" because the latter carries too many biological connotations. Selection and transmission, when incorporated into an explanatory theory, provide what culture historians were looking for and what archaeologists today are still looking for: the tools to begin explaining cultural lineages—that is, to answering why and how those lineages came to look like they do. Robert Brandon (1990:

183) acknowledged that when using Darwin's theory we may never know when we have truly answered "how actually" questions, although he also stated that "no one can fairly describe [a 'how possibly' explanation] as merely an imaginative bit of story telling." The chapters in this book go a long way toward pointing out how we can begin answering such questions.

Note

1. The Daniel Larson referenced here and immediately below is not the same one whose chapter appears in this volume.