*Chapter 1*

# Style and Function: An Introduction

*Michael J. O'Brien and Robert D. Leonard*

PETRUCHIO (to KATHARINA and her tailor, regarding her new gown)

Thy gown? why, ay: come, tailor, let us see't.
O mercy, God! what masquing stuff is here?
What's this? a sleeve? 'tis like a demi-cannon:
What, up and down, carved like an apple-tart?
Here's snip and nip and cut and slish and slash,
Like to a censer in a barber's shop:
Why, what, i' devil's name, tailor, call'st thou this?

Tailor (in response)

You bid me make it orderly and well,
According to the fashion and the time.
—William Shakespeare, *The Taming of the Shrew*

The horse is here to stay, but the automobile is only a novelty—a fad.
—The president of the Michigan Savings Bank,
advising Henry Ford's lawyer not to invest
in the Ford Motor Company in 1903

As the preceding quotes illustrate, styles come and go quite unpredictably, whereas technological developments that are of adaptive significance show no such whimsy, despite reasoned predictions. As the authors and editors of this volume show, we believe this to be the case because different evolutionary

processes are at work in the creation and persistence of stylistic and functional attributes of our artifacts and their associated behaviors.

Recognizing these differences, an explicit distinction between style and function has long been apparent in Americanist archaeology, dating at least to the end of the nineteenth century and the work of personnel connected with the Bureau of American Ethnology. One has but to read, for example, the work of William Henry Holmes (e.g., 1886, 1903) on pottery from the Mississippi River Valley and adjacent regions to gain an appreciation for the fundamental analytical distinction that early prehistorians made between how tools were decorated and how they were used. Holmes well understood how pottery could be used as a tool to investigate both the development of a particular technology and the history of a particular people (Meltzer and Dunnell 1992). Holmes' work has a modern ring to it in terms of how he cautioned about conflating stylistic and functional traits. Although Holmes never used the terms "homology" and "analogy," he well could have. He certainly knew that traits used to establish histories must be homologs (i.e., related—the product of the same intellectual traditions). He also knew that not all traits, no matter how similar they are to each other, are homologous and that similarity may be convergent, or analogous. He also knew that stylistic attributes often clearly measured relatedness and that functional ones may or may not. His message is clear: some kinds of traits are useful for understanding prehistoric function, and others are useful for developing histories of pottery-making peoples. He just didn't know why.

As Lyman (Chapter 5 in this volume) points out, the distinction between style and function was well developed by culture historians writing in the first half of the twentieth century, and unlike their predecessors they often made explicit reference to analogy and homology. A. L. Kroeber (1931:151), for example, pointed out 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." As well, processualists of the 1960s onward (e.g., Binford 1962, 1968; Jelinek 1976; Sackett 1982) showed considerable interest in distinguishing between style and function—a distinction based on the assumption that each refers to different kinds of empirical phenomena and that each is produced by a different process. Culture historians and processualists alike identified various processes that might account for the rise of stylistic and functional traits—diffusion, contact, independent invention, and so on—but these were unconnected to any robust theory that might help explain why one particular process as opposed to any other acted where and when it did to produce either a stylistic or a functional trait (Lyman and O'Brien 1997). Aside from a lack of theory, processualists had an additional burden to bear—a tar baby (sensu Uncle Remus)—a plethora of descriptions, not definitions, of style that resulted in a cacophony. Whether the cacophony was the result of

a lack of theory or worked to preclude theoretical development will be a matter for historians to decide.

Archaeologists interested in applying Darwinian evolutionism to the material record have made the style–function dichotomy an important tenet of their approach, although by no means has there been universal agreement on how stylistic and functional traits are to be recognized or measured—a point made by VanPool (Chapter 7 in this volume) and other evolutionists (Dunnell 1978b, 1980; O'Brien and Holland 1990, 1992, 1995; O'Brien et al. 1994; Teltser 1995). Incorporation of the dichotomy into evolutionary archaeology traces its proximate roots to Dunnell's (1978b) paper "Style and Function: A Fundamental Dichotomy," in which he attempted to create a theoretical focus by tying the concepts of style and function into an evolutionary framework. Dunnell used the term *function* to refer to those forms that directly affect the Darwinian fitness of populations in which they occur and the term *style* to refer to those forms that have no detectable selective values. The difference between the two kinds of traits, then, was definitional, not methodological. This is an important point and one that has been very difficult for many nonevolutionists to grasp. The standard response to hearing these definitional stipulations is, "But style has function." This statement does not recognize that by definition style cannot have function, where function has been defined in terms of those traits that contribute to fitness. If a trait contributes to fitness, it is functional by definition. Whereas we discuss the concept of adaptiveness later, where traits may contribute to fitness and not be under selective control, this is not the problem here. Instead, these critics are confusing their own concepts of style with Dunnell's, by conflating the concepts of function and *purpose*. Stylistic and functional traits have different *purposes*, to be sure, and are the product of different processes—drift and selection, respectively. This dichotomy is the means by which evolutionary theory may be brought to an understanding of the archaeological record. But more about purposes later.

As logical and theoretically pleasing as Dunnell's distinction might have sounded, how did one demonstrate empirically that an archaeological trait, or feature, was functional or stylistic? Further, what exactly did Dunnell (1978b, 1980) mean when he linked style and neutrality? Evolutionary archaeologists since the early 1980s have tended to accept that equation at face value without exploring the epistemological basis for it. This has led to no end of confusion among even those who would refer to themselves as evolutionary archaeologists (e.g., Rindos 1989) and left the approach open to criticism from outside (e.g., Alvard 1998; Bettinger et al. 1996; Boone and Smith 1998).

Our objective in this chapter is to sort through several issues involved in the style–function dichotomy and to place those issues in historical perspective relative to other parts of the evolutionary-archaeology program. We focus primarily on three issues: (1) the equation of style with neutrality, (2) the identification of functional and stylistic (neutral) traits, and (3) the usefulness of the style–function dichotomy in light of what we know about kinds of traits. These are

by no means the only contentious issues involved in clarifying the evolutionary-archaeology program (see Lyman and O'Brien 1998; O'Brien et al. 1998), but many of the others hinge on them (see Cochrane, Chapter 10 in this volume). Neutrality and its antithesis, adaptedness, are concepts that have received considerable attention from evolutionary archaeologists, but it is clear that the amount of attention to date pales in comparison to that afforded those concepts in evolutionary biology. Before turning to those issues, we need to be clearer on what we mean by style and function and how those concepts are related to two other concepts, homology and analogy.

## BASIC DEFINITIONS

Dunnell's (1978b; see also 1978a) definition of function was a departure from traditional use of the word in archaeology—that is, the mental association we make between an object and its use. He defined *function* as "the artificial relationship that obtains between an object at whatever scale conceived and its environment both natural and artificial," explicitly separating it from *use*, defined as "the special case of prehistoric function in which the artificial relationship is motion" (Dunnell 1978a:51). Dunnell made no mention of form in the definitions; hence, there was no attribution of specific forms with specific functions. There might be a correlation between form and function, but under the definition this is an empirical matter, not a theoretical one. If, however, the ascription of function is based on common, everyday experience (in which case we use categories derived from that experience to categorize objects in the archaeological record), then we automatically are making assignments of function based on similarities of form. Doing so denies the possibility of evolution of separate functions.

Traits, or features, that confer Darwinian fitness on an organism—Dunnell's functional traits—may arise among different breeding populations as independently generated features—analogs—or as products of a common developmental history—homologs. Analogs are ahistorical in that they arise not from common phylogenetic backgrounds of the organisms under consideration but rather as similar solutions to similar problems. The term "solution," in keeping with the definition of analogs as features similar in function but different in structure and origin, implies no particular form. In English we equate the term "axe" with chopping, which is one, but certainly not the only, solution to felling a tree. Axes can be used for a variety of other purposes, but by equating axe with chopping, we eliminate those purposes from consideration. This is important with respect to the identification of homologs versus analogs, as similar forms (e.g., axes) may be homologs but put to different uses, or analogs independently developed. Both cases are important to recognize.

In biology homologs are differentiated from analogs on the basis of structural differences and developmental histories, but it is clear from even a cursory glance at the biological literature that there is no easy solution to the problem

of deciding which features are analogous and which are homologous (see Lyman, Chapter 5 in this volume, for additional discussion). Mistakes in assignment are made all the time. Part of the problem in distinguishing homologs from analogs is a result of confusing form and structure. Homologs, defined correctly, are features similar in structure because of a common origin; homomorphs, however, are features similar in form but different in structure. Thus, homomorphy is a superficial resemblance resulting strictly from convergence and not from common origin. Archaeology's confusion of form and structure has led on numerous occasions to certain features being labeled as homologs and thus to the construction of routes of diffusion, when in reality two forms were similar because of convergence—the derivation of a common solution to an adaptive problem. In other words, the use of pottery by people all over the world does not mean that pottery was "invented" once and then diffused in a series of transoceanic contacts over the globe.

Do not be misled by the use of the term "function" in defining what an analog is. As we have noted, functional traits can be homologs just as readily as they can be analogs. The key to whether a feature is homologous or analogous is strictly a matter of its history. If the feature occurs in two organisms, and it occurs in the common ancestor of those two organisms, then it is homologous regardless of whether it is functional or stylistic. Conversely, only functional traits can be analogs. Why? Because we assume that styles are so complex that the probability of duplication by chance is astronomically low (Gould 1986). Therefore, if we find two ceramic vessels containing identical decoration, we assume that they are from the same tradition, or line of cultural heredity, unless we have evidence to the contrary. That is, our hypothesis is that they are homologous. Only rarely, if ever, would two independent groups of people arrive at exactly the same way of decorating their vessels. There is no reason to suspect that we will *never* find such an example, especially with simple geometric paints or surface treatments, but the more parsimonious explanation of such a phenomenon is that the vessels share a common developmental history, if there is reasonable geographic proximity.

Evolutionists 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. If this were the case, then changes in, for example, the hafting elements of projectile points, which we not only assume *a priori* to be functional but which can also be demonstrated empirically to be functional, would be useless as a basis for measuring the flow of time. This decidedly is not the case, however, as many studies have shown (e.g., Beck 1998; O'Brien and Lyman 1999b, 2000a; Thomas and Bierwirth 1983; Wilhelmsen 1997). We are unsure as to why evolutionists have focused on style to show time, although it may be because of our reticence to concatenate stylistic and functional traits within the same classification, thereby risking the chance of conflating historical relatedness and convergence as well. We may want to reconsider this, however, as it is quite possible that our types with both stylistic and functional attributes that

seriate especially well do so because both the stylistic and functional elements involved are homologs. This may well explain the success of many cultural-historical types that include both stylistic and functional traits and certainly points out a mistake made by many evolutionists over the last dozen or so years.

What exactly is the nature of this error? Evolutionary archaeologists (ourselves included) frequently present the familiar Lewontin (1974:8) quote: "We cannot go out and describe the world in any old way we please and then sit back and demand that an explanatory and predictive theory be built on that description." This is, of course, correct. A similar position was taken by James Ford in his wonderful debate with Albert Spaulding regarding the reality of archaeological types (Ford 1954a, 1954b, 1954c; Spaulding 1953, 1954a, 1954b). To put this debate in its simplest form, Spaulding argued that artifact types were discovered, while Ford in his responses articulated the view that types are not "real" but instead are imposed on the data to suit the purposes of the investigator. This is a classic archaeological debate, as relevant today as it was nearly half a century ago.

Evolutionary archaeologists agree with Ford, and it comes as no surprise that they agree with Lewontin as well. Yet, this apparent symmetry breaks down when we consider what Spaulding might think of Lewontin's statement. While we cannot, of course, speak for him, we find there to be nothing inconsistent with his articulated perspective and Lewontin's. In fact, we believe that Spaulding would be in complete agreement, as he wanted types that are meaningful (i.e., real), as does Lewontin.

So, why the inconsistency? We propose it is the result of the work of a few evolutionary archaeologists who ritualistically cite Lewontin and then proceed to violate his axiom regarding unit creation. The violation comes when theoretical units are created without a clear problem definition and without any consideration of meaningful empirical content. This is often practically accomplished by building paradigmatic classifications of any variation that can be described at whatever scale deemed "appropriate" for the case at hand. Often, the exercise begins by examining someone else's types and then arguing that because types are not "real," the other investigator's types mean nothing—and they then prove it by shifting scales downward, thereby demonstrating that the original types obscure variation. Unfortunately, this is often where the exercise stops—with someone else's work demolished and a paradigmatic classification all dressed up with nowhere to go.

This problem besets several contributions in the Ramenofsky and Steffen (1998) edited volume (with two notable exceptions, by Neff and Beck) and is unfortunately programized in Ramenofsky and Steffen's introductory paper. If, as VanPool and Hurt et. al (Chapters 7 and 4, respectively, in this volume) show, not all artifacts and traits of artifacts are equal in terms of fitness contributions, why should they be equal in our paradigmatic classifications? Let us put it this way—we can argue rightfully that classifications and types are not real and are constructed for our purposes, but natural selection does not act on

our abstract categories; it operates on real-world phenomena. Those who do not take their theoretical units to empirical ones with evolutionary meaning are the ones to whom Lewontin is talking.

Here, Dunnell's theoretical and empirical units are confounded. The theoretical units are the only place to start, but we must then examine them to see if they refer to phenomena that were, indeed, the fodder of selection. This is why the chapters in this volume are important, as they make this connection. Others have as well—Dunnell, Feathers, Braun, and Neff come to mind—but this is at the scale of attributes of artifacts—primarily with ceramics. Beck (1998) and the following chapters are important in a new way because they take it to the artifacts themselves.

Neff makes this point, albeit somewhat differently, whenever he writes of grouping procedures (especially Neff 1993), and perhaps it is time to start listening to him. That is, we must be prepared to utilize any procedure—paradigmatic, intuitive, or automatic, as Neff puts it—to build units of demonstrable evolutionary importance. For example, any theoretical paradigmatic classification of projectile points that keeps arrowheads and dart points in the same classification is not going to identify units that selection operated upon. It can be explicit, systematic, paradigmatic, ideational, or for our own purposes and yet can be absolutely worthless for any and all purposes.

Importantly, evolutionarily useful units are likely to be nonrandom associations of traits, as the chapters here predict. Yet, does this not sound like Spaulding, when we already know that Ford was right? Yes and no. Despite the flaws with Spaulding's procedure (e.g., there is no reason to presume that nonsignificant associations are not subject to evolutionary processes), there is no *a priori* reason to think that with sufficient attention to the theoretical, his methodology (or others like it) will not yield units that are of evolutionary significance if evaluated as such. Does it really matter where our units of demonstrable evolutionary utility come from? If so, stick *only* to the ideational, systematic, equally weighted, and paradigmatic, and only by pure chance will evolutionary explanations ever be constructed.

Our point here is simple. Ford was correct regarding *theoretical* units. They are not "real." They are the products of the mind of the investigator. Spaulding was correct regarding *empirical* units. They must be real—that is, have evolutionary significance. Lewontin speaks to both issues.

Now that classification is rethought, we need to explain briefly how functional and stylistic traits get replicated. Although that topic is beyond the scope of a detailed discussion here (see Boone and Smith 1998; Lyman and O'Brien 1998 and accompanying comments), suffice it to say that the traits, regardless of whether they are functional or stylistic, get replicated by the same processes. That's not the important point here; what is important is whether or not selection operates on the trait and, from an empirical standpoint, how we identify a trait as being functional or stylistic.

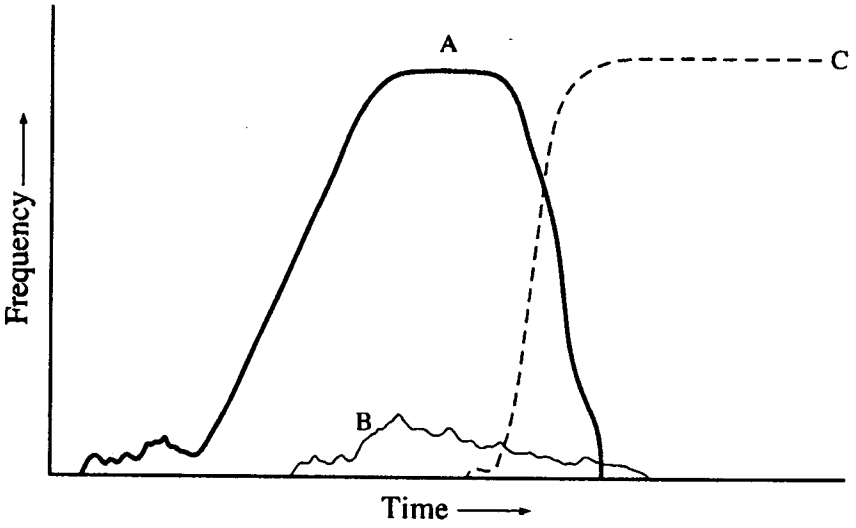
## STYLE AND NEUTRALITY

Although Dunnell's (1978b) distinction between style and function was basically a definitional one, he included a cryptic comment regarding how frequencies of variants behave when they are either under or not under selective control: "Traits that have discrete selective values over measurable amounts of time should be accountable by natural selection and a set of external conditions. Traits identified as adaptively neutral will display a very different kind of behavior because their frequencies in a population are not directly accountable in terms of selection and external contingencies. Their behavior should be more adequately accommodated by stochastic processes" (Dunnell 1978b:199). What exactly does this mean? It means simply that variants under selective control behave differently—that is, they have different distributions in time and space—than do those that are not under selective control.

O'Brien and Holland (1990) 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 traits behave over time (see Vaughan, Chapter 8 in this volume for a discussion of the relationship of variation to stylistic and functional traits). O'Brien and Holland expect a trait—more likely a particular state that a trait is in, similar to one of several allelic expressions of a gene—that is being selected for to begin at some arbitrary point above zero and to increase in frequency at a steadily decelerating rate toward some optimal value (Figure 1.1). This, and only this, gives selection its apparent directional component. Selection against the trait—in reality, selection against bearers of the trait—reverses the trend and sends the curve downward. Two possible outcomes exist: either the trait eventually disappears from the genotype, or, if different expressions of the trait confer equivalent fitness (although not necessarily equal under all environmental conditions) to some of the possible bearers, then the result can be a balanced polymorphism. Conversely, a trait not under selection can drift through a breeding population from generation to generation, its frequency fluctuating randomly—sometimes in one direction for a few generations, then in another, and so on, as demonstrated by Neiman (1995). Given infinite time, one of two outcomes will occur: either the trait 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 (see Figure 1.1).

Notice that in the preceding paragraph we said that the increase in frequency of a trait gives selection its apparent directional component. This does not mean that selection is the only evolutionary mechanism that can produce directionality—a criticism that has on occasion been levied against evolutionary archaeology. For example, Boone and Smith (1998:S145) claim that evolutionists have "tended to consider all directional phenotypic change through time as the result of natural selection acting directly on cultural variation." This is untrue; as Maxwell points out in Chapter 3 in this volume, evolutionary archaeologists have always considered directional change resulting from processes other than

**Figure 1.1**
**Hypothetical changes in frequency of traits under selection versus traits under drift**[1]

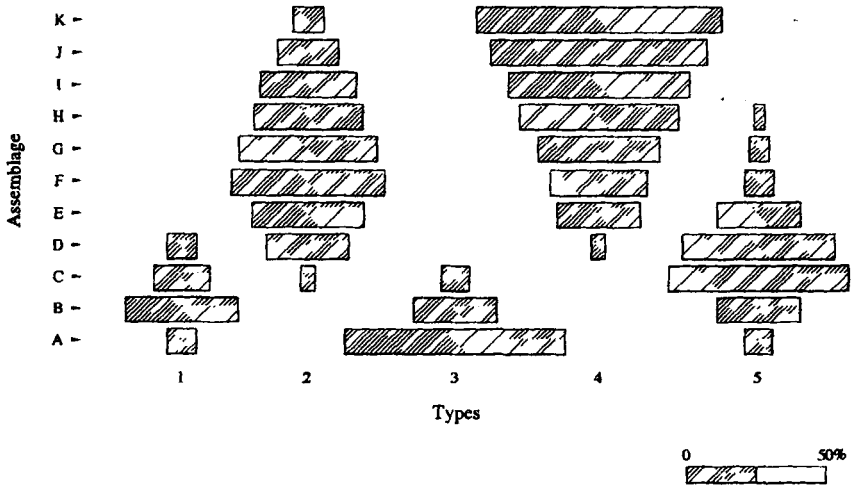



[1]Trait A appeared, then drifted along in the population and eventually came under selective control, leading to a rapid increase in expression. Eventually, it became selected against and rapidly disappeared. Trait B never came under selective control but rather drifted through time, eventually disappearing. Trait C was also selected for, but much more quickly than trait A was. Also, its rise to fixation within the population (the point at which the curve levels off) was more rapid than the rise of trait A, signified by the steeper curve for trait C (from O'Brien and Lyman 2000b).

selection (see also Dunnell, Foreword in this volume; Hurt et al. 1998). As Lyman and O'Brien (1998:621) note, critics have failed to grasp the significance of the evolutionist conception of style, which clearly incorporates the biological notion of drift (Abbott et al. 1996; Dunnell 1978b, 1980; Lipo et al. 1997; Lipo and Madsen, Chapter 6 in this volume; Neiman 1995; O'Brien and Holland 1990, 1992). Contrary to some assertions (e.g., Boone and Smith 1998), Dunnell (1978b) did not argue that any sustained directional change in artifact-type frequency is a sign of selection at work. Rather, he stated that there are two mechanisms for the apparent directionality of change, one of which comprises selection, and the other transmission. The apparent direction of evolutionary change is just that—it is apparent and is explicitly not part of evolutionary-archaeological theory or of evolutionary theory in general. It is not part of either theory because it explains nothing; rather, it is "an observation about the record of change" (Dunnell 1980:42) that itself requires explanation—a fact long recognized by paleobiologists (e.g., Gould et al. 1977, 1987; Raup 1977; Raup and Gould 1974).

Returning to the discussion of how traits under selection and those not under

**Figure 1.2**
**Hypothetical frequency seriation of 11 artifact assemblages using five artifact classes**[1]




[1]Assemblages are ordered on the basis of artifact-class percentages, with bars summing to 100 percent for each assemblage. Only relative chronological ordering can be achieved through frequency seriation; further, time can run in either direction through the ordered assemblages.

selection behave, we now have a methodological issue as opposed to simply a definitional one. In short, we have an empirical basis for separating functional and stylistic (adaptively neutral) traits. We stated that a trait that was being selected for would begin at some point above zero and increase in frequency at a steadily decelerating rate toward some optimal value. Conversely, a trait not under selection drifts from generation to generation, its frequency fluctuating randomly. After an infinite amount of time, either the trait will reach a frequency of zero and thus be eliminated, or it will become fixed. But as we know, styles, at least in the way we usually think of them, do not behave this way. Styles come in, they become popular, and then 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 late nineteenth century (Lyman et al. 1997). In theory, stylistic traits on ceramic vessels act no differently than do other stochastically propelled traits, but one might logically ask: How do we get from the randomly fluctuating pattern shown in Figure 1.1 to the neatly defined battleship curves of a seriation shown in Figure 1.2—a transition that Dunnell (1978b), O'Brien and Holland (1990, 1992), and others have said was possible? Life histories appear orderly, even those randomly generated (e.g., Gould et al. 1977, 1987). The question becomes: Do battleship curves—life histories—actually reflect a random distribution? The answer is yes, but we

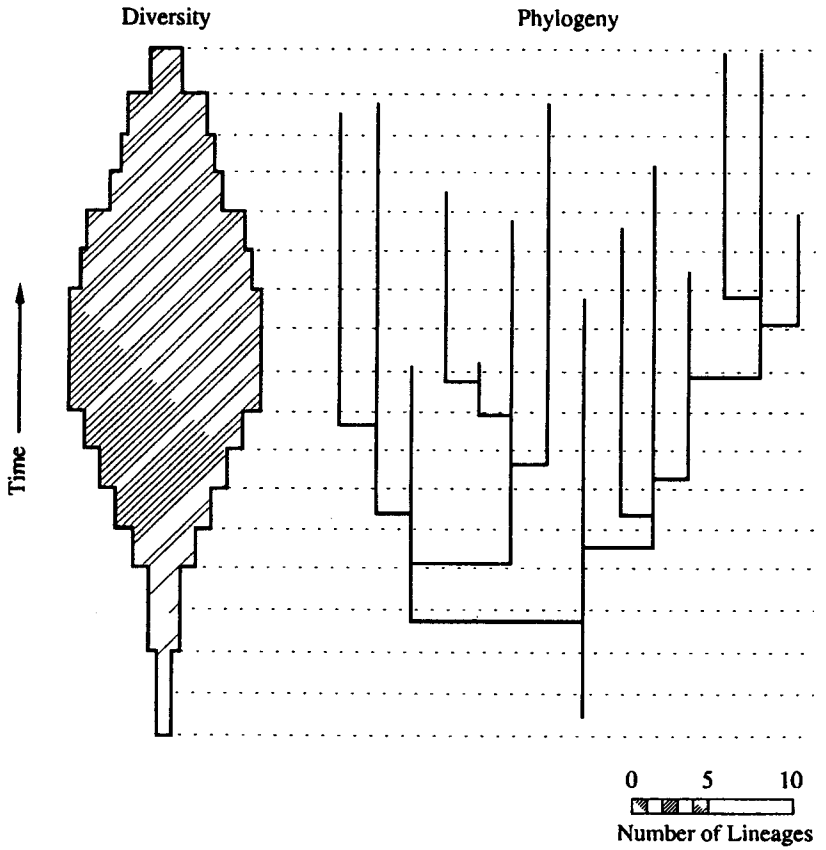
cannot leave it at that because it does not explain the difference between the randomly fluctuating pattern of Figure 1.1 and the battleship curves of Figure 1.2.

Of critical importance is the scale at which style is analyzed. The characteristic random, zigzag pattern results from a single trait state that drifts along; conversely, battleship curves illustrate life histories of complex units composed of many trait states. We call these complex combinations styles. Thus, battleship curves tell us nothing about shifts in frequency of individual states in which an individual trait might reside. The difference between the random pattern and the curves seen in seriation diagrams 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. The individual components might exhibit zigzag patterns through time, but at the more complex scale, where the components are lumped, the pattern becomes the familiar battleship shape.

Recently, we have come to the conclusion that some of what O'Brien and Holland (1990) said in their paper "Variation, Selection, and the Archaeological Record" was incorrect or at best glossed over an important issue. Nor did they help the issue much in their later paper "The Role of Adaptation in Archaeological Explanation" (O'Brien and Holland 1992). They noted that, "Battleship curves, in one sense, are equivalents of biological clades. The shape of most archaeological clades, which have their widest points at midsection, is identical to the shape of random biological clades at idealized equilibrium" (O'Brien and Holland 1990:54). They drew this conclusion in part from Dunnell's (1978b) abbreviated discussion of style and in part from Gould and Raup's work with simulating biological clades (Gould et al. 1977; Raup and Gould 1974; Raup et al. 1973). Based on recent work (Leonard 1999; Lyman and O'Brien 1999a, 1999b, 2000; Lyman et al. 1998; O'Brien and Lyman 1999a, 2000a, 2000b, 2000c), however, we realize the equivalence of life-history curves and random-clade diagrams to be ill conceived. So-called clade-diversity diagrams, on the one hand, display fluctuations in taxonomic richness over time (Figure 1.3). Each horizontal bar comprises the absolute frequency of classes—of whatever taxonomic level—per time interval. The battleship-shaped graphs of frequency seriation, on the other hand, display the relative, or proportional, frequency of individual specimens per class, or taxon, per time interval (Figure 1.2). Further, each clade comprises a monophyletic group—that is, a group encompassing all taxa that share a common ancestor as well as the common ancestor (Figure 1.4).

Thus, despite superficial similarity in the graphs generated by each analytical method, clade-diversity diagrams and frequency-seriation graphs display decidedly different kinds of information. We find the information contained in seriation graphs, clade-diversity diagrams, and cladograms to be significant to evolutionary archaeology from the standpoint of reconstructing phylogenetic histories of artifact lineages (Leonard 1999; Lyman and O'Brien 1999b, 2000;

**Figure 1.3**
**A model for producing a clade-diversity diagram**[1]



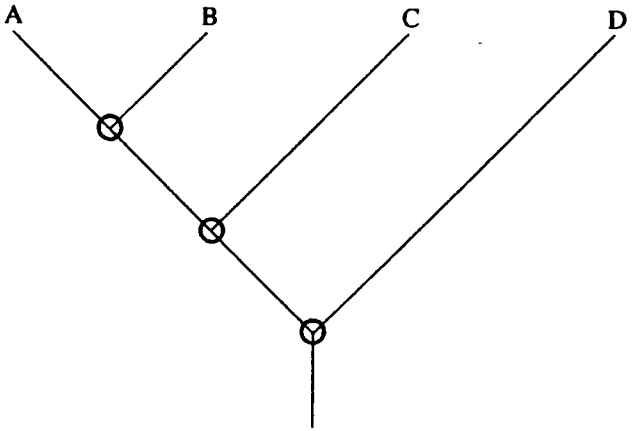

[1]The clade-diversity diagram is shown on the left, and the phylogenetic history of taxa used to produce the diagram is shown on the right. The clade-diversity diagram shows the waxing and waning of the number of classes through time (after Raup et al. 1973).

O'Brien and Lyman 1999b, 2000a, 2000b, 2000c). We agree that style itself is neutral and that this has ramifications for how styles are "built" historically and how they are reproduced (Lipo et al. 1997; Lipo and Madsen, Chapter 6 in this volume), but we prefer to move beyond the rhetoric associated with the concept of neutrality and actually get some analytical work done—that is, to begin to construct phylogenetic histories of artifacts.

## ADAPTEDNESS, ADAPTATIONS, AND NEUTRALITY

As important as the distinction between style and function is, it overlooks an important issue that has received little treatment heretofore in the evolutionary-

**Figure 1.4**
**A hypothetical cladogram showing the phylogenetic relation among four classes, or taxa[1]**




[1]Three clades are illustrated: (1) Taxa A, B, and their common ancestor (circled), (2) Taxa A–C and their common ancestor, and (3) Taxa A–D and their common ancestor.

archaeology literature. That issue is, What do we do with features that increase the adaptedness, or fitness, of the possessor(s) but that are not products of selection? To bring the problem into focus and in an attempt to avoid some of the problems associated with the dichotomous terms "style" and "function," O'Brien and Holland (1992) created three categories of traits: (1) traits that are under selective control and that increase adaptedness; (2) traits that are not under selective control and that increase adaptedness; and (3) traits that are not under selective control and that do not increase adaptedness. A fourth category—traits that are under selective control but that do not affect adaptedness—is an impossibility. Other categories have been created to accommodate things such as tagalong, or hitchhiking, traits, but we bypass discussion of them here as Hurt et al. (Chapter 4 in this volume) provide a more detailed discussion that need not be repeated.

Traits in Category 1 are adaptations, which, following the definition provided by Gould and Vrba (1982), are traits that not only increase the fitness of the possessor but have come under selective control. Under Dunnell's (1978b) definition, traits in both Categories 1 and 2 qualify as functional traits, although it is clear from the contexts in which Dunnell used the term "function" that he actually was referring to traits in Category 1—that is, those that both contribute to adaptedness and are products of selection. Dunnell's use of "style" refers to traits in Category 3—traits (in reality, states of traits) that do not contribute to adaptedness and therefore are neutral.

But cannot style contribute to adaptedness, whether or not stylistic traits come

under selective control? In one sense it can, and it is for this reason that confusion exists over Dunnell's linkage of style and neutrality. Style, as Dunnell (1978b) used the term, is neutral only to the extent that, at a given time, any particular stylistic trait is as fit as any other stylistic feature. The critical point here is use of the word "trait" and the confusion it creates. For this reason we prefer to use the term "trait state" instead of trait. Importantly, there may be several or many alternative states (attribute states of a specific dimension [see Dunnell 1971; O'Brien and Lyman 2000a]) in which a trait can reside, with each state conferring equivalent, or in some cases nonequivalent, adaptedness to the possessor. Thus, as O'Brien and Holland (1992) point out, it is important to separate the concept of style—an ill-defined complex of traits and trait states—from the phenomenon of "stylistic elements."

Lewontin (1978) used the rhinoceros as an example to examine neutrality and alternative states. Rhinoceroses presumably developed horns as a means of defense (not that we are saying horns evolved for the purpose of defense). Indian rhinoceroses developed a single horn, and African rhinoceroses two. Does that mean that the latter are better adapted for defense than are the former? Probably not, at least not that we can determine. Simply put, two once-related populations found similar solutions to a common problem. The important point is that there appears to be no increased adaptedness that hinges on the number of horns a rhinoceros has. The question of why some rhinoceroses have one or two horns is entirely different from the question of why rhinoceroses have horns at all. The former question deals with lineage development only, while the latter addresses adaptation. In short, the presence of horns is an adaptation; the number of horns appears to be neutral.

An example of more archaeological relevance is the practice of incising circles, chevrons, or birds into the moist exteriors of unfired pots. First, is it important to decorate pots at all (the presence of decoration being a trait)? Second, is it important to use circles instead of raptorial birds or squares (the individual designs being states of traits)? It would make little sense to call a circle an adaptation, but it might make sense to call vessel decoration an adaptation within a given setting. We could construct a number of scenarios where loosely knit social groups distributed across a landscape use decorative displays for social *purposes*—either for integration or for information exchange (e.g., Braun and Plog 1982; Wobst 1977). By participating in the social-identification system of which the marked pots are a feature, a person might increase his or her adaptedness. For example, food can be shared in time of need, new mates can be found, and so on. By not participating in the system, a person could be affecting his or her adaptedness relative to other individuals in the region. Importantly, these *purposes* cannot be confused with either function or use, as defined earlier.

This raises a related point. We might suggest that despite the wide range of decorative variants possible in the world, there are some that the groups using the pots find unacceptable. Or more probable, there are variants that make no

sense to the users. Thus, there is an acceptable range of decorative variants available. As long as makers and/or users remain within the range, which we might expect could and would change over time, their adaptedness, at least relative to this one dimension, is not affected adversely. However, pot makers and/or users who consistently defy the limits of acceptability certainly could have their adaptedness affected. Again, what is important from the standpoint of adaptedness is not that the pots specifically have circles or squares on their exteriors—or that the pots are decorated at all—but that if the pots are decorated, the makers and users know which elements are acceptable and act accordingly. As noted before, here styles serve a *purpose*, distinct from the earlier definitions of use and function.

It is not profound to note that there are different scales at which features in the archaeological record can be examined, one of which is the regional scale. For example, without a perspective on the recurrence of cooking-vessel designs across broad regions of the midwestern United States, our picture of the life histories of ceramic vessels would be heavily biased. We could be left wondering whether there was some reason that a particular local group decorated its pots for a while and then abandoned decoration. It is not too much off the point to note that lack of attention to detail at the regional level seriously impeded our understanding of post–A.D. 300 developments in the Midwest (O'Brien and Holland 1992). For years received wisdom among archaeologists was that the "Hopewell Interaction Sphere," characterized at many sites by nicely decorated vessels and the occurrence of exotic materials, came to a sudden halt as a result of groups becoming more isolated in their behavior and the concurrent lack of benefit from participating in the sphere. Braun (1977, 1985), however, demonstrated conclusively that instead of becoming more isolated, at least in terms of ceramic similarities, post–A.D. 300 groups actually showed heightened homogeneity. The misconception was a result of analytical interest that for decades had focused solely on decoration instead of on manufacture *and* decoration.

Part of the confusion over style and neutrality undoubtedly stems from the fact that, as we have pointed out before (e.g., O'Brien and Holland 1990, 1992; O'Brien et al. 1998), the source of selection is tied to human intent. Anthropologists argue that humans select ceramic styles, methods of hafting projectile points, and a myriad of other things on the basis of culturally influenced choice. Thus, the argument runs, style cannot be selectively neutral. As O'Brien and Holland (1992) point out, this dilemma is nothing more than the result of the same word having more than one meaning. Selection as an evolutionary process has little to do with cultural selection as applied colloquially. What is meant in the latter sense is simply "choosing" one thing over another. Humans indeed are selective agents, but only when they affect the adaptedness either of themselves or of other organisms. For example, animal breeders are active selective agents. Likewise, the seemingly capricious, but in reality patterned, choice by collectors of butterflies of one color or another is as potent an agent of selection as is the choice by any bird. In both cases the butterflies meet unhappy endings,

and the gene pool of which they were a part is adjusted accordingly (O'Brien and Holland 1990). But this is a far cry from choosing one design element over another for vessel decoration. Clearly, the more important analytical problem is understanding the pool of acceptable variation for given points in time and determining how remaining in the pool versus straying outside it affects adaptedness.

The concept of intent is much more than definitionally problematic, as it leads to explanatory problems as well. It is increasingly common to explain human outcomes in terms of the intentions of the agents involved. Unfortunately, this leads to a vitalistic explanation of little merit. While we have said it countless times before, it seems necessary to say it again here—there is a significant discrepancy between intentions and outcomes. Every prehistoric farmer who ever put hoe or digging stick to earth intended success. Many failed. To explain the success of the successful in terms of their intentions is absurd. They were successful not because of their intentions but because of the particular variant they generated, the vagaries of chance, and the operation of natural selection. We can think of no better example of the potential and real failures of behaviors that are the result of such intentions than comes to mind with the current debate regarding global warming. Billions of dollars are being spent to try to deal with this pressing global problem, regardless of whether or not our globe is truly warming. As archaeologists, however, we recognize a significant problem here. While global warming is seen as a major environmental threat (especially if you have property on Miami Beach), we recognize that we are in an interglacial period and that it is perhaps in all our best interests to encourage as much warming as possible! In other words, natural selection will act on the variation we generate, and the outcome is uncertain despite our best predictions, whether it be with respect to global warming or investing in the Ford Motor Company in 1903.

What about Category 2 traits—those that may affect fitness but are not under selective control? A biological example of such a trait would be a mutation, and the corresponding nonbiological feature would be an invention, discovery, or similar product of a moment in time. Not all such products of the moment affect adaptedness, especially those that arise and go unnoticed. Others very well could affect adaptedness, and many of them will go on to become adaptations. For example, the wheel was used as a toy for 2,000 years in many societies before it was put to practical use. O'Brien and Holland (1992) provide a more detailed example of a human who picks up an animal hide, punches a hole in it, and puts it on, thereby potentially increasing his or her adaptedness relative to others in his or her group. At that point the hide is functional, but it is not an adaptation; it is merely a "mutation" relative to one human's phenotype. A number of sequences could follow. If, after a few generations, the person's offspring were living longer and producing more children than were their conspecifics, then the wearing of skins would become an adaptation. Or, if after a few weeks or months, other members in the group noticed that the skin wearer appeared to

be more comfortable than they were, then they might start wearing skins. At that point the wearing of skins might be considered an adaptation. But suppose the skin wearer died the day after he or she started wearing skins, without telling anyone else how warm he or she felt? Then we have another example of a mutation that remained a novelty.

In this single example we potentially have the makings of all three categories of traits. The wearing of skins, we could predict, affects adaptedness, regardless of the time we make the examination—that is, when the feature is a mutation or when it is an adaptation. Thus, the trait falls in Category 2. If it is acted on by selection, then it moves to Category 1. But does the kind of skin matter? Is bearskin, for example, superior to wolf skin, or does each confer an equivalent advantage to the wearer? In other words, are the relative fitnesses equivalent? Notice that the level of examination has shifted here from the trait itself—skin or no skin—to the attributes of the trait, similar to the shift seen in our example of pot decoration. Detailed engineering studies of different kinds of skins found in our imaginary archaeological record would have to be conducted before this question could be answered.

As can be seen from this extended discussion, evolutionary archaeologists begin with no assumptions regarding whether or not a particular technology or attribute of a technology is stylistic or functional, an adaptation or neutral with respect to selection, or contributes to adaptiveness and is not under selection. In the short run, this puts us at a bit of a disadvantage. Many processual archaeologists and evolutionary ecologists assume, *a priori*, that all traits are adaptations. Evolutionary ecologists are explicitly clear in assuming that technologies are the product of natural selection. Hurt et al. (Chapter 4 in this volume) consider an alternative position, that it may be best to assume neutrality and demonstrate adaptation if the case can be made. In general, it may be best to assume neither and struggle to make the best argument we can for each, which is no easy task.

## CONCLUSION

Critics of evolutionary archaeology (e.g., Boone and Smith 1998; Schiffer 1996; Spencer 1997) often make it sound as if evolutionists focus all their attention on selection as opposed to acknowledging that other evolutionary mechanisms exist. Part of this criticism is attributable to evolutionists, ourselves included, who have emphasized the role of selection as the strongest evolutionary mechanism, but none of us have ever claimed that it is the only mechanism. No one has ever even implied that style can be ignored in an evolutionary framework. Neutrality does not translate into "unimportant," if one defines evolution as "any net directional change or any cumulative change in the characteristics of organisms or populations over many generations—in other words, descent with modification. It explicitly includes the origin as well as the spread of alleles, variants, trait values, or character states. Evolution may occur as a

result of natural selection, genetic drift, or both" (Endler 1986:5). We like this definition precisely because it pinpoints both selection and drift as important evolutionary processes. We emphasize that "genetic" drift is not the only kind of drift at work among evolving populations.

With respect to neutrality, we need to remember that style is neutral only to the extent that, at the time of origin, any particular stylistic feature is as "fit" as any other stylistic feature. Hartl (1988:172) points out a common misconception in evolutionary biology over the meaning of neutrality, whereby "only genes that are unimportant can undergo neutral mutations. The fallacy here stems from failing to understand that neutral mutations are assumed to be equivalent in function, not lacking in function." What we see as the persistence and spread of stylistic traits may speak more about the fitness of the trait in terms of itself—what Leonard and Jones (1987) refer to as replicative success—than about the success of the possessor(s) of the trait. There is, however, no *a priori* reason to think that functional traits—those under selective control—do not affect the fitness of the possessors. Importantly, traits that are stylistic under one environmental regime may take on functional roles in a different environment. With respect to an aircraft, gray paint may be stylistic in peacetime, while serving as camouflage during combat.

We still have a long way to go in making Darwinian evolutionism compatible with the examination of change in the archaeological record. Happily, as the chapters in this book demonstrate, the last several years have seen evolutionary archaeology move beyond the fits and starts that any new way of looking at something entails, but we are still far short of demonstrating to the average archaeologist that Darwinian evolutionism is a superior product to any number of alternatives readily available in the marketplace. We need more applied case studies that build on and extend those already available, especially of the kind that are geared toward the detailed unraveling of complex histories of artifacts as disparate as Acheulean hand axes (Vaughan, Chapter 8 in this volume) and Polynesian fishhooks (Pfeffer, Chapter 9 in this volume). This is the only means by which to separate analogs from homologs—a need that, as we noted earlier, was voiced almost 70 years ago by Kroeber (1931:151). Despite the insight he displayed, Kroeber, for want of a theory, never developed the method. We might do better, but to do so requires that we (1) understand the difference between functional and stylistic traits, (2) know how to recognize them, (3) understand that style and function do not translate into homology and analogy, and (4) recognize that adaptations are a special class of evolutionary unit. Being clear on these matters will help us go a long way toward clearing up the confusion that has existed in evolutionary archaeology ever since the publication of Dunnell's original article on style and function in 1978.

## REFERENCES

Abbott, A. L., R. D. Leonard, and G. T. Jones
1996 Explaining the Change from Biface to Flake Technology: A Selectionist Ap-

plication. In *Darwinian Archaeologies*, edited by H. D. G. Maschner, pp. 33–42. Plenum Press, New York.

Alvard, M. S.
1998 Comment on "The Goals of Evolutionary Archaeology: History and Explanation" by R. L. Lyman and M. J. O'Brien. *Current Anthropology* 39:630–631.

Beck, C.
1998 Projectile Point Types as Valid Chronological Units. In *Unit Issues in Archaeology: Measuring Time, Space, and Material*, edited by A. F. Ramenofsky and A. Steffen, pp. 21–40. University of Utah Press, Salt Lake City.

Bettinger, R. L., R. Boyd, and P. J. Richerson
1996 Style, Function, and Cultural Evolutionary Processes. In *Darwinian Archaeologies*, edited by H. D. G. Maschner, pp. 133–164. Plenum Press, New York.

Binford, L. R.
1962 Archaeology as Anthropology. *American Antiquity* 28:217–225.
1968 Archeological Perspectives. In *New Perspectives in Archeology*, edited by S. R. Binford and L. R. Binford, pp. 5–32. Aldine, New York.

Boone, J. L. and E. A. Smith
1998 Is It Evolution Yet? A Critique of "Evolutionary Archaeology." *Current Anthropology* 39(Supplement):S141–S173.

Braun, D. P.
1977 *Middle Woodland–Early Woodland Social Change in the Prehistoric Central Midwestern U.S.* Ph.D. Dissertation, Department of Anthropology, University of Michigan, Ann Arbor.
1985 Ceramic Decorative Diversity and Illinois Woodland Regional Integration. In *Decoding Prehistoric Ceramics*, edited by B. A. Nelson, pp. 128–153. Southern Illinois University Press, Carbondale.

Braun, D. P. and S. Plog
1982 Evolution of "Tribal" Social Networks: Theory and Prehistoric North American Evidence. *American Antiquity* 47:504–525.

Dunnell, R. C.
1971 *Systematics in Prehistory*. Free Press, New York.
1978a Archaeological Potential of Anthropological and Scientific Models of Function. In *Archaeological Essays in Honor of Irving B. Rouse*, edited by R. C. Dunnell and E. S. Hall, pp. 41–73. Mouton, The Hague.
1978b Style and Function: A Fundamental Dichotomy. *American Antiquity* 43:192–202.
1980 Evolutionary Theory and Archaeology. In *Advances in Archaeological Method and Theory*, Vol. 3, edited by M. B. Schiffer, pp. 35–99. Academic Press, New York.

Endler, J. A.
1986 *Natural Selection in the Wild*. Princeton University Press, Princeton, N.J.

Ford, J. A.
1954a Comment on A. C. Spaulding's "Statistical Technique for the Discovery of Artifact Types." *American Antiquity* 19:390–391.
1954b On the Concept of Types: The Type Concept Revisited. *American Anthropologist* 56:42–53.
1954c Spaulding's Review of Ford. *American Anthropologist* 56:109–112.

Gould, S. J.
1986 Evolution and the Triumph of Homology, or Why History Matters. *American Scientist* 74:60–69.
Gould, S. J., N. L. Gilinsky, and R. Z. German
1987 Asymmetry of Lineages and the Direction of Evolutionary Time. *Science* 236: 1437–1441.
Gould, S. J., D. M. Raup, J. J. Sepkoski, T. J. M. Schopf, and D. S. Simberloff
1977 The Shape of Evolution: A Comparison of Real and Random Clades. *Paleobiology* 3:23–40.
Gould, S. J. and E. S. Vrba
1982 Exaptation—A Missing Term in the Science of Form. *Paleobiology* 8:4–15.
Hartl, D. L.
1988 *A Primer of Population Genetics*. Sinauer, Sunderland, Mass.
Holmes, W. H.
1886 Ancient Pottery of the Mississippi Valley. *Bureau of Ethnology, Annual Report* 4:361–436.
1903 Aboriginal Pottery of the Eastern United States. *Bureau of American Ethnology, Annual Report* 20:1–201.
Hurt, T. D., R. D. Leonard, and T. L. VanPool
1998 Comment on "The Goals of Evolutionary Archaeology: History and Explanation" by R. L. Lyman and M. J. O'Brien. *Current Anthrolpology* 39:635–636.
Jelinek, A. J.
1976 Form, Function, and Style in Lithic Analysis. In *Cultural Change and Continuity: Essays in Honor of James Bennett Griffin*, edited by C. E. Cleland, pp. 19–34. Academic Press, New York.
Kroeber, A. L.
1931 Historical Reconstruction of Culture Growths and Organic Evolution. *American Anthropologist* 33:149–156.
Leonard, R. D.
1999 *Discussion on Evolutionary Archaeology*. Paper Presented at the 64th Annual Meeting of the Society for American Archaeology, Chicago.
Leonard, R. D. and G. T. Jones
1987 Elements of an Inclusive Evolutionary Model for Archaeology. *Journal of Anthropological Archaeology* 6:199–219.
Lewontin, R. C.
1974 *The Genetic Basis of Evolutionary Change*. Columbia University Press, New York.
1978 Adaptation. *Scientific American* 239:156–169.
Lipo, C. P., M. E. Madsen, T. Hunt, and R. C. Dunnell
1997 Population Structure, Cultural Transmission and Frequency Seriation. *Journal of Anthropological Archaeology* 16:301–333.
Lyman, R. L. and M. J. O'Brien
1997 The Concept of Evolution in Early Twentieth-Century Americanist Archeology. In *Rediscovering Darwin: Evolutionary Theory and Archeological Explanation*, edited by C. M. Barton and G. A. Clark, pp. 21–48. Archeological Papers of the American Anthropological Association, No. 7. American Anthropological Association, Washington, D.C.
1998 The Goals of Evolutionary Archaeology: History and Explanation. *Current Anthropology* 39:615–652.

1999a *Classification, Systematics, Typology*: The *Critical Step in Evolutionary Archaeology*. Paper Presented at the 64th Annual Meeting of the Society for American Archaeology, Chicago.

1999b *Graphing Change and Testing Lineages with Seriation*. Unpublished manuscript in possession of the authors.

2000 Measuring and Explaining Change in Artifact Variation with Clade-Diversity Diagrams. *Journal of Anthropological Archaeology* 19:39–74.

Lyman, R. L., M. J. O'Brien, and R. C. Dunnell

1997 *The Rise and Fall of Culture History*. Plenum Press, New York.

Lyman, R. L., S. Wolverton, and M. J. O'Brien

1998 Seriation, Superposition, and Interdigitation: A History of Americanist Graphic Depictions of Culture Change. *American Antiquity* 63:239–261.

Meltzer, D. J. and R. C. Dunnell

1992 Introduction. In *The Archaeology of William Henry Holmes*, edited by D. J. Meltzer and R. C. Dunnell, pp. vi–l. Smithsonian Institution Press, Washington, D.C.

Neff, H.

1993 Theory, Sampling, and Analytical Techniques in the Archaeological Study of Prehistoric Ceramics. *American Antiquity* 58:23–44.

1999 *Populations*. Paper Presented at the 64th Annual Meeting of the Society for American Archaeology, Chicago.

Neiman, F. D.

1995 Stylistic Variation in Evolutionary Perspective: Inferences from Decorative Diversity and Interassemblage Distance in Illinois Woodland Ceramic Assemblages. *American Antiquity* 60:7–36.

O'Brien, M. and T. D. Holland

1990 Variation, Selection, and the Archaeological Record. In *Archaeological Method and Theory*, Vol. 2, edited by M. B. Schiffer, pp. 31–79. University of Arizona Press, Tucson.

1992 The Role of Adaptation in Archaeological Explanation. *American Antiquity* 57: 36–59.

1995 The Nature and Premise of a Selection-Based Archaeology. In *Evolutionary Archaeology: Methodological Issues*, edited by P. A. Teltser, pp. 175–200. University of Arizona Press, Tucson.

O'Brien, M., T. D. Holland, R. J. Hoard, and G. L. Fox

1994 Evolutionary Implications of Design and Performance Characteristics of Prehistoric Pottery. *Journal of Archaeological Method and Theory* 1:259–304.

O'Brien, M. J. and R. L. Lyman

1999a *Methodological Challenge of an Evolutionary Archaeology*. Paper Presented at the 64th Annual Meeting of the Society for American Archaeology, Chicago.

1999b *Seriation, Stratigraphy, and Index Fossils: The Backbone of Archaeological Dating*. Kluwer Academic/Plenum Press, New York.

2000a *Applying Evolutionary Archaeology: A Systematic Approach*. Kluwer Academic/Plenum Press, New York.

2000b Darwinian Evolutionism Is Applicable to Historical Archaeology. *International Journal of Historical Archaeology* 4:71–112.

2000c Evolutionary Archaeology: Reconstructing and Explaining Historical Lineages.

In *Social Theory in Archaeology*, edited by M. B. Schiffer, pp. 126–142. University of Utah Press, Salt Lake City.

O'Brien, M. J., R. L. Lyman, and R. D. Leonard
1998 Basic Incompatibilities between Evolutionary and Behavioral Archaeology. *American Antiquity* 63:485–498.

Ramenofsky, A. F. and A. Steffen (editors)
1998 *Unit Issues in Archaeology: Measuring Time, Space, and Material.* University of Utah Press, Salt Lake City.

Raup, D. M.
1977 Stochastic Models of Evolutionary Paleontology. In *Patterns of Evolution as Illustrated by the Fossil Record*, edited by A. Hallam, pp. 59–78. Elsevier Scientific, Amsterdam.

Raup, D. M. and S. J. Gould
1974 Stochastic Simulation and the Evolution of Morphology—Towards a Nomothetic Paleontology. *Systematic Zoology* 23:305–322.

Raup, D. M., S. J. Gould, T. J. M. Schopf, and D. S. Simberloff
1973 Stochastic Models of Phylogeny and the Evolution of Diversity. *Journal of Geology* 81:525–542.

Rindos, D.
1989 Undirected Variation and the Darwinian Explanation of Cultural Change. In *Archaeological Method and Theory*, Vol. 1, edited by M. B. Schiffer, pp. 1–45. University of Arizona Press, Tucson.

Sackett, J. R.
1982 Approaches to Style in Lithic Archaeology. *Journal of Anthropological Archaeology* 1:59–112.

Schiffer, M. B.
1996 Some Relationships between Behavioral and Evolutionary Archaeologies. *American Antiquity* 61:643–662.

Spaulding, A. C.
1953 Statistical Techniques for the Discovery of Artifact Types. *American Antiquity* 18:305–313.
1954a Reply (to Ford). *American Anthropologist* 56:112–114.
1954b Reply to Ford. *American Antiquity* 19:391–393.

Spencer, C. S.
1997 Evolutionary Approaches in Archaeology. *Journal of Archaeological Research* 5:209–264.

Teltser, P. A.
1995 Culture History, Evolutionary Theory, and Frequency Seriation. In *Evolutionary Archaeology: Methodological Issues*, edited by P. A. Teltser, pp. 51–68. University of Arizona Press, Tucson.

Thomas, D. H. and S. L. Bierwirth
1983 Material Culture of Gatecliff Shelter: Projectile Points. In *The Archaeology of Monitor Valley 2: Gatecliff Shelter*, edited by D. H. Thomas, pp. 177–211. Anthropological Papers, No. 59. American Museum of Natural History, New York.

Wilhelmsen, K. H.
1997 *Events of Manufacture Ordered by Attribute-Scale Occurrence Seriation.* Paper

Presented at the 62nd Annual Meeting of the Society for American Archaeology, Nashville.

Wobst, H. M.

1977 Stylistic Behavior and Information Exchange. *University of Michigan, Museum of Anthropology, Anthropological Papers* 61:317–342.