

Style and Function

Style and Function

Conceptual Issues in Evolutionary Archaeology

Edited by
Teresa D. Hurt and
Gordon F. M. Rakita

Foreword by Robert C. Dunnell



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For my father, Wesley R. Hurt (1917–1997), whose energy and enthusiasm for archaeology and for life will always be an inspiration for me—*TDH*

For my wife, Cally, and my son, Nicholas, who never begrudged my endless retreats to the computer in order to undertake work on this volume—*GFMR*

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Foreword

Robert C. Dunnell

Although my health at the time of the symposium “Style and Function: Twenty Years Later” did not permit my direct participation, the editors kindly invited me to provide a historical sketch of how I came to write “Style and Function: A Fundamental Dichotomy” (Dunnell 1978b; see also Dunnell 1996c). Few people get the luxury of explaining themselves with the benefit of 20 years’ hindsight. Consequently, I welcomed the opportunity not only to try to be clearer where I may have been murky but also to react to some of the misunderstandings that the paper seems to have generated. Hopefully, such an exegesis may alleviate some of the misgivings that archaeologists have about the meeting of evolutionary theory and the archaeological record.

HISTORY

Before looking at the intellectual history, I would be remiss in neglecting the sociological circumstances surrounding the initial publication. In the 1970s there was something of a backlash against the methodologizing of the “new archaeologists” of the 1960s, who had little substantive product to show for their polemic. The cry was to *do* archaeology, and mainline journals, like *American Antiquity*, responded. Many younger, theoretically inclined archaeologists felt that their work was not getting a proper audience. Every bit as irascible as he is today, a youthful Mike Schiffer was a vocal critic of the journal and its editor, Frank Hole. As Schiffer (1978) recounts, Hole (1978) offered to let Schiffer edit one issue of *American Antiquity* to see if he could do any better, a sort of “put up or shut up” scenario. Schiffer asked me to contribute to that effort, and “Style and Function” was that contribution. If Schiffer were not the intellectually

generous person that he is, I am sure he might want to rethink the wisdom of that decision today.

Classification

The accidental intersection of two long-standing interests—classification (e.g., Dunnell 1971a, 1971b, 1986a, 1986b) and seriation (e.g., Dunnell 1970, 1981)—played a major role in my deciding to do “Style and Function.” My major foray into classification, *Systematics in Prehistory* (Dunnell 1971b; see especially pp. ix–x, 7), was an attempt to explore and explain archaeological classification as it was in the 1960s. Why did what “worked” work? Why did what “failed,” fail?

This tack on the creation of units compelled me to think about the theory/classification relation. In retrospect, the lack of impact that *Systematics* had in the 1970s owes as much to the lack of theory in archaeology as it does to anything else. The book is concerned largely with frameworks for understanding how units are generated and the implications of those decisions for archaeological inference and interpretation. It was one thing to point out, even demonstrate, that explicit theory, or something that performs its functions, was *required* to create classifications. But with no theory with which to load those abstract methods, only methodological buffs could wade through the prose to its highly critical and, in many respects, unsatisfying result. Archaeologists in my experience tend to be rather empirical, little given to theoretical persuasion. It is ironic, then, that lack of substantive product of the new archaeology was due to lack of any general theory. Of concern in this context are just three points presented at length in *Systematics*.

First, it is possible to show that classification is an act of the archaeologist, not a discovery in nature. As I developed more fully later (1986a, 1986b), traditional belief in “natural types,” discovery rather than creation (Read 1974; Spaulding 1953), or that one need not be bothered about such matters (e.g., Cowgill 1972) had major deleterious effects on archaeology. Supplying new interpretations of the same old categories (e.g., L. R. Binford 1973; S. Binford 1968; cf. Bordes 1961; Bordes and de Sonneville-Bordes 1970), instead of developing new units appropriate to the ambitious and laudable goals of processualism, doomed the approach. The impact of common sense, our own culture, on creating our observations, our facts, could not be appreciated under those conditions.

Second, classification, the creation of categories, precedes all other activity, whether classification is overt and rationalized or cryptic and *ad hoc*. Quantitative methods for “discovering” categories (e.g., Read 1974; Spaulding 1953; Whallon 1972) can be shown to employ cryptic *ad hoc* categories. To count, to “quantitate,” requires categories, not just phenomena. So-called inductive approaches are thus always illusory; they just employ cryptic principles. Lots of decisions are made in constructing units; they cannot be justified, challenged,

or modified, if they are not acknowledged. This is the role of theory in classification. It provides the explicit, logically coherent basis for deciding how to parse the empirical world into categories that are capable of explanation.

Third, the issue of justification drove home the importance of “problem” for classification. One could justify classificatory decisions only if one had specific objectives. Alas, “problem” was also one of the buzzwords of the new archaeology (e.g., Watson et al. 1971). Here is where I dropped the ball in *Systematics*. I can beg no relief except to point out I was still in my twenties and had only modest clues on how the archaeological world worked. As was *de rigueur* for the new archaeology, I used the *abstract* notion of “problem,” not a *real* problem, to rationalize decisions in my model. My efforts, like those of everyone else, never got very far because archaeology had no theory on which one could draw to create meaningful, explicable units, ask answerable questions, and ensure the integrated nature of the consequent “knowledge.” A new systematics was simply not possible. Consequently, processualism died the death of the *ad hoc* “hypothesis.”

Seriation

No doubt the reader can see where this is leading, but before turning to evolution, I need to connect the other strand, seriation. William J. Mayer-Oakes’ (1955) *Prehistory of the Upper Ohio Valley* was my first contact with professional archaeology. His use and explanation of seriation captured my attention (e.g., Dunnell 1961). Later, working at the University of Kentucky, I encountered Phillips et al.’s (1951) *Archaeological Survey in the Lower Mississippi Alluvial Valley, 1940–1947*. This, in turn, led quickly to the work of James A. Ford (e.g., 1935, 1936, 1938, 1962; Ford and Willey 1949; see also O’Brien and Lyman 1998, 1999) on seriation. Like myself, Ford was also interested in classification and how it interacted with seriation. His “On the Concept of Types: The Type Concept Revisited” (Ford 1954b; see also 1954a) showed that even while the two arenas were related closely in his thinking, he was unable to spell out any details of the articulation (cf. O’Brien and Lyman 1998). Although Ford was more sophisticated than his predecessors (e.g., Kroeber 1916; Nelson 1916; Spier 1917) in his methodological expositions (including such things as sample size, collection units, and classification effects) in the Southwest, he displayed no interest in why seriation worked. Indeed, as Willey (1999) has observed recently, theoretical issues were largely anathema to Ford.

My paper “Seriation Method and Its Evaluation” (1970) was clearly derived from Ford’s methodological efforts. I, however, attempted to clarify the relation between the various approaches to seriation and the warrant for chronological inference. The link to classification was made more directly than Ford: when stylistic types (a.k.a. historical types) (Krieger 1944) were used to describe sets of assemblages the temporal distribution of types was continuous and, when quantified, monotonic. I accordingly identified two algorithms for seriation, one

that worked for presence/absence data, which I termed occurrence seriation, and one that used frequency data, which I termed frequency seriation (see also Cowgill 1972). As Krieger (1944) explained for types and as was true of most culture-historical methods, seriation was founded in generalizations obtained by trial-and-error methods. The crux of the matter for me was: Why did those generalizations obtain? Why is it that stylistic types displayed continuous and monotonic distributions? The standard “explanation” at the time was that “types originate, increase in popularity, and then die out.” This is a not-so-subtle rephrasing of the ordering algorithm it is intended to explain. My own frequency and occurrence “laws” were, unfortunately, simply empirical generalizations as well, reflecting the absence of theory at the time. But this question of why stylistic types have the distributions they do united my interests in classification and seriation and directly stimulated the writing of “Style and Function.”

Evolution

After reading the Gould et al. (1977) paper, “The Shape of Evolution: A Comparison of Real and Random Clades,” the solution to this conundrum hit me. In that paper the authors argue for a random (i.e., nonselective) component in evolution and develop a model that shows that the frequency of random clades is continuous and monotonic, looking just like a Fordian seriation. They were quite mistaken in their own application. They had not demonstrated the existence of a random component in evolution. No one had ever supposed that adaptation occurred at the family level, the level at which they formed their clades. When Gould et al. (1977) added together disparate species, which may well have had detectable adaptive significance individually, the distributions became more and more noisy. The randomness “observed” was an artifact of data aggregation. The key for me, however, was the null model they developed of transmission in time, the familiar “battleship curves.” Their model has since been shown to be overly constraining and inaccurate as a result (e.g., Neiman 1990), but the essential features remain intact. The unimodal distribution, in use for more than 50 years in seriation, turns out to be a null hypothesis for the detection of style. Environmental interaction, selection, pushed frequencies in various directions, but if there was no selection, then there was no direction; the “push” was random. The only pattern that could be present was that induced by the transmission process, and Gould et al. (1977) had shown these patterns were unimodal distributions. Strictly speaking, the distributions were not random but historically constrained variation acted on by a random process (i.e., stochastic). I revisited this argument later (Dunnell 1980, 1982) more clearly and in greater detail.

So the “ah-ha” moment (as Martin Gardner liked to put it in *Natural History* for many years) was that evolutionary theory, Darwinian evolution, explained why seriation worked as it did, why the generalizations I (1970) had deduced were true, even why it contained confounding spatial problems as a dating method. If one took “stylistic” (“historical”) types to be neutral variants (i.e.,

variants whose phenotypic expression did not affect the fitness of the population), then only the process of transmission itself would be registered in distributions. In the case of presence/absence data, distributions would be continuous or, in the case of abundance data, monotonic, as Gould et al. (1977) had shown. This explained, for example, the criteria Krieger (1944) proposed under the rubric of “historical significance.” Transmission requires temporal-spatial contiguity, the essence of Krieger’s historical significance, in a word, homology. Functional or adaptive traits, on the other hand, may converge as a consequence of sharing similar environments and, thus, do not require temporal-spatial contiguity. Of course, functional traits may be transmitted, so the situation is not as simple as implied in Krieger’s work. The important point is, however, that in the absence of theory, Krieger had been able only to insist on or assert the value of his test. He could not justify his choice of criteria, nor could he get beyond his initial insight.

In the last analysis, seriation is a simple deduction from the concept style, defined as neutral variability, for the dimension of time. The occurrence and frequency generalizations turn out to be the direct result of defining style as neutral variation and then examining their distributions in time. With these observations, the problem quickly became a methodological one. How does one *identify* neutral variation in the record? Krieger had already suggested one way—time/space contiguity. Meltzer (1981) showed that, given enough space/time, homologous and analogous similarities could be easily differentiated. My proposal was to use frequency distributions. Stylistic trait frequencies are stochastic (the early seriations, e.g., Spier [1917], even used the term “normal curve” to describe their distributions, technically incorrect but recognizing the role of random processes intuitively, if nothing else). Most stylistic traits are lost by accident as soon as they are born. Most innovations are never transmitted, or at least detectably transmitted. Further, the particular traits that are transmitted are unrelated to fitness effects (i.e., are themselves random). Variation is, however, constrained by previous states, existing variability, so random processes are actually stochastic ones. Those traits that are transmitted assume the unimodal form because, if frequencies are described as percentages, the appearance of additional traits must be at the expense of their predecessors. The larger the frequency of a trait, the greater the probability that it will be transmitted in a stochastic world. Frequencies would move to fixation at 100 percent were it not for mutation, which, by its mere presence, reduces the frequency of the first trait. This too has a random component. The distribution of the maximum frequencies reached by stylistic types should be linear (i.e., a large number of brief type distributions with low maximum value, a few long distributions with very high values, with the bulk of the distributions lying somewhere in the middle).

Functional traits (Dunnell 1978a; see also Dunnell 1996a), on the other hand, being driven by selection, should display an increase in frequency and then a plateau. The slope of the increase can be directly translated as the fitness differential between two traits, slow replacements occurring with slight differences

and steep increases associated with large differences. The trait might be fixed at 100 percent, less the mutation rate, or a polymorphic condition might obtain in which two or more variants coexist at frequencies determined by selection. For example, consider an assemblage of ceramics, the paste of which is a compromise between the requirements of several different uses. Under these conditions, one will find the frequency of the “best” compromise to be driven to 100 percent less the mutation rate. Then consider ceramics in which different pastes are used for cooking, storage, and serving vessels. In this case, frequencies would map on those of the underlying functions. In either case, periods of no change in frequency will characterize functional traits under selection. Only by accident would traits under selection be confused with neutral traits. Because functional traits interact with their environment, the presence of correlations between functional traits and elements of their environments can also be used to identify them. But to show that they are under selection, one has to move beyond correlation to provide a mechanism through which selection can take place. Likewise, an examination of maximum values attained by functional types can serve to identify them. Instead of the high frequency of short-duration, low-value traits and a low frequency of long-duration, high-value traits that characterizes stylistic types, the reverse is true for functional ones. There should be low frequency of short-duration, low-value types and a high frequency of long-duration, high-value types.

What makes the style and function dichotomy fundamental as claimed (Dunnell 1978b)? *Different processes explain the two kinds of traits.* Style is explained by drift. Function is explained by selection. If the two are mixed in one unit, then the counts and/or distributions made using that unit will be explicable by neither. Others had, of course, insisted on the importance of distinguishing style and function or near analogs earlier (e.g., L. R. Binford 1968; Jelinek 1976; Steward 1954), but once again, lacking theory, there was no basis for justifying their position or preferring it over the commonsense “descriptive” type (e.g., Rouse 1960; Steward 1954).

These initial insights linking seriation, evolution, and style have lately been elaborated considerably by Neiman (1990, 1993, 1995a, 1995b), Teltser (1995a), and others (e.g., Lipo et al. 1997), including several of the authors in their chapters in this volume. But as Teltser (1995b) notes, the methodological problems are not inconsiderable, a situation exacerbated by the general lack of familiarity with evolution and scientific explanation in archaeology. Demonstrations of the power of the style–function dichotomy and evolutionary theory in general to explain the archaeological record continue to be needed, a second point well addressed by chapters in this volume.

LESSONS OF HISTORY

That the style–function dichotomy was fundamentally misunderstood by many is clear from a charge often leveled at the formulation, namely, that evolution

did not explain style. The reverse is clearly the case. I came to evolution not by way of selection but by way of drift! Of course, adaptations were explained as well, but then so were lots of other, if untestable, accounts for adaptation (i.e., intention, problem solving, etc.) drawn from our own culture. Historically, style committed me to the power of evolutionary theory. Perhaps this confusion was facilitated by the common conflation of evolution and one of its mechanisms, selection, inasmuch as evolutionary archaeology is often mislabeled “selectionist” archaeology even by people who understand and are committed to the use of evolution (e.g., Jones et al. 1995).

In what turned out to be a moment of exceptionally poor judgment, I chose the term “style” to label neutral variants. Style is what the culture historians and others (e.g., Collins 1927; Deetz 1965; Hill 1985; Kidder 1917; Kroeber 1919; Plog 1980, 1983) had called the kind of variation that made seriation work. There seemed no reason not to give them their due. But style is not just an archaeological concept; it is also an English word. Unfortunately, it seems to have been construed as the latter by many, if not most, readers in spite of the explicit definition of both terms. This misunderstanding has fueled an already huge literature on style that I have analyzed in detail elsewhere (Dunnell 1996b). Apart from the work of Sackett (e.g., 1973, 1977), which flows very much from the cultural-historical tradition, most practitioners assume that “style” exists and then try to discover its meaning (e.g., Conkey and Hastorf 1990 and the references and papers therein). Of course, to discover what an English word means, one consults a dictionary; the meaning of concepts is the product of explicit definition and can be looked up in a discipline’s literature. Confusion has been the product. The nonsense claim, for example, that “style has function” must clearly be using either style or function or both as English words; with the archaeological concepts I *defined*, there is no such confusion possible. Certainly, such an *ad hoc* position assumes that style and function exist as phenomena and not as analyst-constructed categories for perceiving phenomena. Nothing that might qualify as knowledge, either about the archaeological record or about people in general, has been forthcoming from this kind of muddled effort.

Other, more informed commentators, while accepting selection, doubted the existence of neutral traits, even though there is no basis in the theory for such doubt. Nonetheless, until recently, it was a popular view in biological textbooks, where most archaeologists probably learned their biology. In this view all traits were adaptive; for many we just did not know how. The development of biological methods that allowed investigators to look directly at genetic codes, rather than inferring them from phenotypes, has laid to rest all doubts about selective neutrality in scientific circles at least (Kimura 1977, 1983; King and Jukes 1969).

Equally deceiving and just as debilitating is the notion that *things* are either stylistic or functional (i.e., confusing the categories used in observation with the phenomena being observed). In fact, David Rindos once criticized the style–function notion on these grounds in a draft version of a paper I was reading for

him. Things (e.g., a ceramic slip) might be style (not under selection) in one context only to come under selection in another. Indeed, one can only imagine the existence of neutral variability if this is true—style “stores” variability, some of which may become functional at a later date, as Feathers and I showed (Dunnell and Feathers 1991; see also Cochrane, Chapter 10 in this volume). Apparently, Rindos was convinced; his misconstrual never reached print. Furthermore, the scale of selection may change. A slip might be functional, but the color of the slip stylistic, or the slip simply a carrier of color (or texture etc.). Calling something stylistic or functional is not an inference but a hypothesis in need of testing, always.

Aha!, critics will cry. Did you not just admit that style has function? Well, no. Style as a class of phenomena certainly has cost and, therefore, must be under selection; the *amount of style* is certainly not neutral. It must affect fitness. But the cost *differences between styles*, whether it is painted blue or green, assuming color is not under selection, is negligible and, therefore, invisible to selection. Styles are equal cost alternatives without function.

LAST ANALYSIS

Looking back at how the ideas in style and function came to be developed, perhaps the most general conclusion that should be reached is the overarching importance of theory, not the *ad hoc* theoretical statements that occupy much of the literature but an integrated view of reality and how it works. Without a theory we have, more often than not, ended up chasing our tails and howling at the moon. The style and function notions have been latent for a long time in the discipline, but without an explicit theory they could not be linked to our explanatory interests, nor could they be systematically developed. It is this last that occupies the authors of this volume.

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Preface

This volume represents the published results of a symposium organized for the 63rd Annual Meeting of the Society for American Archaeology (SAA) held in Seattle in 1998. That year marked the 20th anniversary of the publication of Robert C. Dunnell's *Style and Function: A Fundamental Dichotomy*. Our objective, however, was not to present a *festschrift* in honor of this work or its author (however laudable that goal certainly would have been). Instead, our intention was to gather together a collection of papers that explore current issues surrounding the concepts introduced by Dunnell. While we did not set out to pay homage to Dunnell's ideas, we are pleased to note that each of the chapters ultimately does so.

The evolutionary archaeology (EA) approach that Dunnell pioneered was built upon a conception of style and function that has often been outside the dominant processual debates over the meaning of these terms. This may be due, in part, to misunderstanding of the EA definition of style as neutral variation. Evolutionary archaeology defines style as variation that is *functionally equivalent*, but not unimportant or without a use within particular social systems. A common rejection of this definition of style is the claim "Too much time and effort go into style for it to be neutral!" (Bettinger et al. 1996:134). Such criticisms miss the mark. Certainly, a ceramic vessel with painted decorations requires more time and effort to produce than one without. Thus, to decorate or not to decorate may be a functional question from an EA standpoint. Moreover, the use of decorations may have a significant impact on reproductive or replicative success in particular social contexts; however, the initial choice of one design element over another does not involve a choice between functionally different variants. In other words, there are no inherent differences in fitness between alternative

styles, though they may come to have important, historically contingent differences in meaning.

A related and equally common misunderstanding of Dunnell's dichotomy revolves around the theoretical nature of the EA definitions of style and function. Recent critiques have observed that traits that are functional at one point (in time or space) can become stylistic at others. This is seen as a violation of a strict style–function distinction and, ultimately, a refutation of Dunnell's definitions (Ames 1996:119; Wills 1994:289). The EA definitions of style and function, however, are ideational or conceptual and not empirical. While they are useful for describing and explaining portions of the empirical world, they are not meant to describe inherent, unchanging characteristics of any trait or object (see Cochrane, Chapter 10 in this volume). As theoretical constructs, their attribution to any particular part of the empirical record is fundamentally a statement regarding the evolutionary forces (drift or selection) thought to be affecting distributions of that class of phenomena through time and space. Thus, no contradiction exists in classifying a trait or object stylistic at one point and functional at another; the change simply reflects a shift in the evolutionary forces at work.

Finally, it has been argued that Dunnell's definition implies that the frequencies of stylistic traits should be essentially random, in the sense of arbitrary or unpatterned (Bettinger et al. 1996:136). As this is clearly not the case in the empirical record, such a definition must be flawed. While it is quite true that Dunnell suggested that stylistic traits could be identified by their "random behavior" (1978:199), he qualified further that "[e]xplanations of stylistic phenomena will be found in stochastic processes" (p. 200). Here, as elsewhere in the EA literature, stochastic implies that current (and future) conditions are probabilistically (not deterministically) related to prior circumstances in the selective environment. In fact, it is just such processes that provide the conceptual background to EA explanations as to why the artifact seriations of the culture historians work as they do, tracking historical change in cultural relatedness rather than adaptation to the environment (Lipo et al. 1997; Lyman et al. 1997; Teltser 1995). Distributions of stylistic traits, therefore, are not arbitrary or meaningless, yet they should behave randomly in relation to the selective environment.

We hope that the chapters in this volume help to highlight and clarify these important distinctions. Nonetheless, our defense of Dunnell's definition of style is not meant to imply that other definitions are unworthy of consideration. Clearly, there exists a multitude of opinions as to the meaning of the term "style" in archaeological research. While not necessarily complementary to an EA framework, they often provide insights that have relevance within broader anthropological research. We believe the same could be said for the EA approach.

Notwithstanding, the rather strict adherence to a programmatic set of definitions (by an admitted minority of researchers) over a period of 20 years may seem rather remarkable (or single-minded) to others who do not share the EA perspective. The reason for such constancy may lie in the EA understanding of

the term “theory.” As Dunnell (1982) noted, theory is composed of definitions regarding how and why the empirical world works as it does and propositions deduced therefrom. A systematically developed body of theoretical propositions should provide the substantive meaning for any and all empirical data. Theoretical perspectives, therefore, must remain internally consistent if they are to provide reproducible explanations for observed phenomena. Any one key concept, by necessity, is interconnected with others. Some tenets are peripheral, with few connections to the rest of the body of theory. Others, though, lie at the center of the web of interconnections. The style–function dichotomy is just such a concept; its place within EA theory is pivotal and paramount. It is no surprise, then, that it remains the introductory concept for later attempts to explicate the perspective (e.g., Leonard and Jones 1987; O’Brien and Holland 1990; Rindos 1986). In this way, it is comparable to core theoretical precepts in other paradigms, such as the dialectic in Marxism or phenotypic plasticity in human evolutionary ecology.

This is not to suggest that the style–function dichotomy has remained unmodified. Significant additions to the concept (or propositions deduced from it) have occurred over the past two decades (Allen 1996; Beck 1998; Hurt et al. 1998; Leonard and Jones 1987; Lipo et al. 1997; Meltzer 1981; Neiman 1995). Chapters in this volume continue this effort, focusing on this fundamental precept of evolutionary archaeology by revisiting the theoretical foundations for Dunnell’s original definitions, exploring additional implications, and proposing and applying new methods for identifying and understanding artifact style and function in the archaeological record.

The editors owe thanks to several people who contributed to the production of this volume. Special thanks go to Robert C. Dunnell for his participation in both the SAA symposium and the edited volume but particularly for his unique insights into evolutionary theory and archaeology, for which we are all indebted. Robert D. Leonard helped us organize and plan the symposium and contributed invaluable advice on transforming the symposium papers into a published work, as did Michael O’Brien. R. Lee Lyman and Michael O’Brien both provided exceptionally thorough and helpful comments on early drafts of all of the chapters. Mary Hurt valiantly proofread the final manuscript. Jose Luis Lanata and Hector Neff also warrant our sincere gratitude for their assistance in seeing this volume through production. All of the authors deserve praise for their ideas and for their diligence during the editing process.

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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

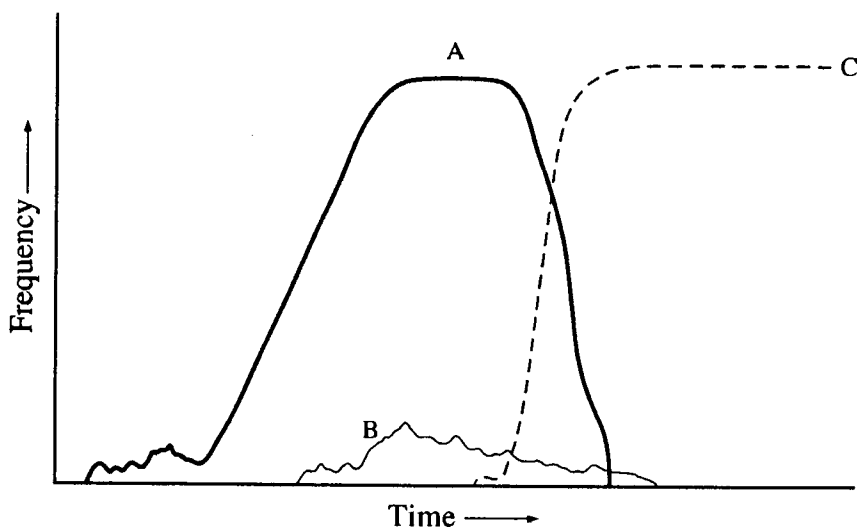
Although Dunnell's (1978b) distinction between style and function was basically a definitional one, he included a cryptic comment regarding how frequencies of variants behave when they are either under or not under selective control: "Traits that have discrete selective values over measurable amounts 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 drift¹

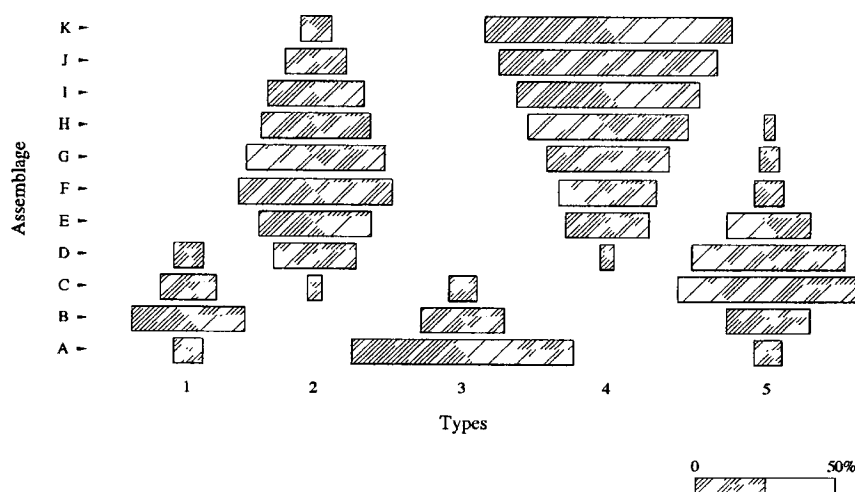


¹Trait A appeared, then drifted along in the population and eventually came under selective control, leading to a rapid increase in expression. Eventually, it became selected against and rapidly disappeared. Trait B never came under selective control but rather drifted through time, eventually disappearing. 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 classes¹



¹Assemblages 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

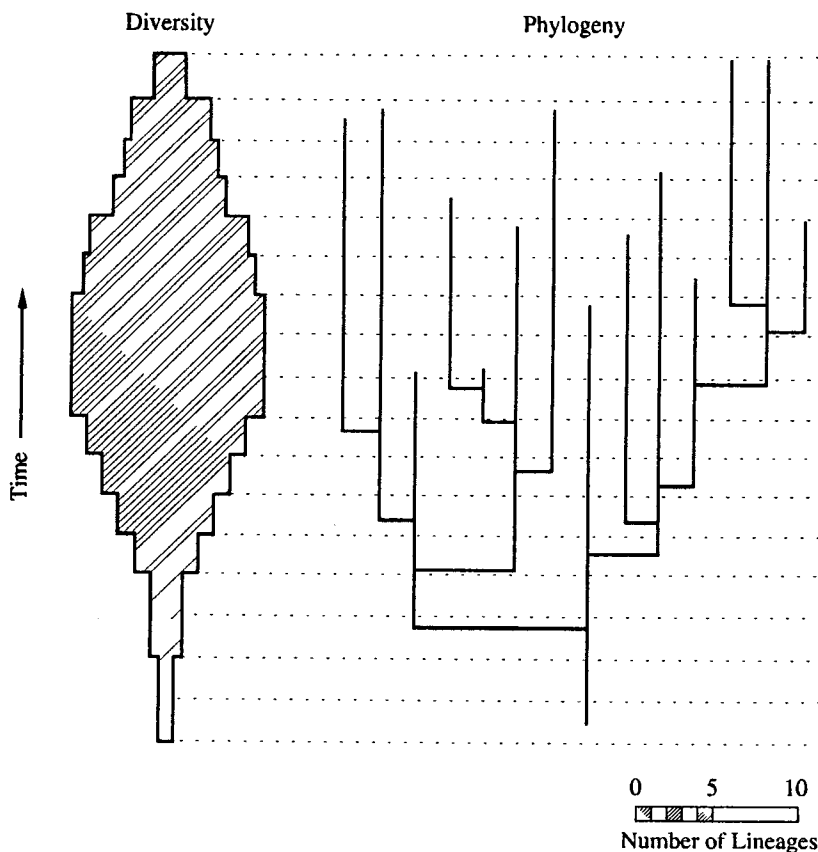
cannot leave it at that because it does not explain the difference between the randomly fluctuating pattern of Figure 1.1 and the battleship curves of Figure 1.2.

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 diagram¹



¹The 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).

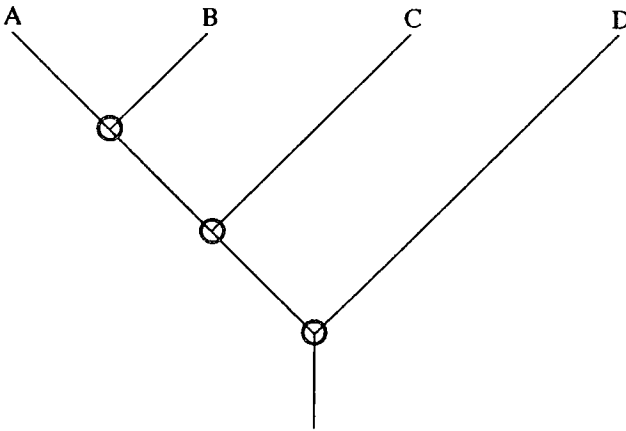
O'Brien and Lyman 1999b, 2000a, 2000b, 2000c). We agree that style itself is neutral and that this has ramifications for how styles are “built” historically and how they are reproduced (Lipo et al. 1997; Lipo and Madsen, Chapter 6 in this volume), but we prefer to move beyond the rhetoric associated 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 taxa¹



¹Three 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.

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Chapter 2

Differential Persistence of What? The Scale of Selection Issue in Evolutionary Archaeology

Hector Neff

INTRODUCTION

From the beginning, the scale of selection issue has figured prominently in the literature of evolutionary archaeology. Twenty years ago, when Dunnell first went public with the idea that selective retention of cultural variation might account for the content and configuration of the archaeological record, he suggested that cultural transmission creates an opportunity for the level of selection to shift up from the individual to the functionally integrated collective of individuals (Dunnell 1996[1978b]:28). Dunnell has mentioned this possibility a number of times since 1978, particularly in regard to the origins of complex societies (e.g., Dunnell 1989). But the opposite possibility—that selection may act at a level *below* the individual human—has not been discussed within American evolutionary archaeology. This omission seems somewhat obtuse considering (1) that there *has* been considerable discussion of how artifacts may exhibit differential fitness (e.g., Neff and Larson 1997; O'Brien and Holland 1990, 1992); (2) that artifacts may exhibit descent with modification regardless of whether individual humans who use them successfully reproduce human bodies or not (Leonard and Jones 1987); and (3) that practical efforts to use evolutionary theory to make sense of the archaeological record invariably focus on artifacts (e.g., Dunnell and Feathers 1991; Neff and Arroyo 1997; O'Brien et al. 1994). Why do we not at least consider this possibility when we think about the scale of selection issue in evolutionary archaeology? As an admittedly heuristic and exploratory exercise, I offer a brief rationale for the latter view in this chapter.

SEX AND CULTURAL TRANSMISSION

In "Style and Function: A Fundamental Dichotomy" (Dunnell 1978a) as well as his 1978 SAA paper (Dunnell 1996[1978b]), Dunnell called attention to the tremendous increase in the pace of change and scale of diversity that became possible with the invention of cultural transmission, and he compared this effect to the "Cambrian explosion" of biological diversity brought about by the advent of sexual reproduction.¹ According to Dunnell (1978a:198), the impact of sex and the impact of cultural transmission are comparable because both shorten adaptive response time and increase the range of responses. But, since adaptive response is *purely* the result of selection (O'Brien and Holland 1990; Neff and Larson 1997), Dunnell's observation begs the question of why the invention of sex and cultural transmission provides an opportunity for selection to act faster and more flexibly.

I suggest that the answer is that both the invention of sexual reproduction and the invention of cultural transmission entail a change in the relationship between replicators on the one hand and, on the other hand, what Hull (1980) calls "interactors" and Dawkins (1976, 1982) calls "vehicles." A replicator is a unit of which copies can be made and whose nature has some effect on the probability that it will be copied (Dawkins 1982:83); the prototypical replicator is a gene. An interactor is the epicenter of the effects that a collection of replicators has upon the world; where genes are the replicators, paraphrasing Dawkins (1982:82), an interactor is a temporary vehicle in which replicators travel about. Evolution occurs as replicators, largely through their effects on interactors (but also through more remote effects on the world [Dawkins 1982]),² achieve different levels of success at making copies of themselves.

For asexual organisms, there is a close parallel between replicators and interactors: the entire genome of an organism is a replicator, and the organism in which the genome resides is an interactor. The organism-lineage is a constantly diverging tree that precisely parallels the genome-lineage. Selection is between organism-lineages, and it yields frequency change in the representation of genome-lineages. If novel characteristics that arise by mutation enhance the ability of the interactor to secure necessary resources, genomes determining those characteristics will increase in frequency as the organism-lineage buds off daughter lineages ever more rapidly.

Things change with the invention of sex. Individual organisms become the vehicles³ for *multiple* replicators, and the replicators can now be identified as individual genes rather than complete genomes. Replicators associate with one another, both temporarily in individual vehicles and more permanently in species gene pools. The species gene pool shows continuity over time and defines the evolutionary lineage, but this is not the level at which selection acts. Instead, selection acts at the level of the interactor (or vehicle).⁴ What happens is that the genes of the species-lineage are reshuffled every generation into a unique, new set of individual genomes, and these temporary constellations of genes

produce varied phenotypic expressions in a new set of interactors. Selection takes place because of fitness differentials among alternative phenotypic expressions (interactors) that have emerged at a particular time through the sex-based sorting of genes and the ensuing developmental processes from zygote through adult.

From the foregoing perspective, the invention of sex by definition shifts the level of selection down. Rather than taking place at the lineage level, as it does in asexual organisms, selection in sexually reproducing organisms is based on the differing fitnesses of interactors (with alternative phenotypic expressions) *within* lineages. In the second case, selection acts much faster and more flexibly, and this is what accounts for the explosion of biological diversity during the Cambrian period that Dunnell (1978a) mentioned in his “Style and Function” paper.

How does the emergence of cultural transmission affect the selection process and, specifically, the level on which selection has its primary effect? As with the emergence of sexual reproduction, the nature of both replicators and interactors changes. The replicators consist of cultural information variably packaged (e.g., as “memes,” “culturgens,” etc.) and transmitted from one individual to another. One can also think of such information packages as making up a “cultural pool,” which shows continuity over time and defines the evolutionary cultural lineage. A key point to which I return in a moment is that, because the operation of cultural transmission is completely independent of genetic transmission, evolutionary cultural lineages are completely independent of genetic lineages.⁵ But what defines the interactor, the phenotypic manifestation of information in the cultural pool? This is the key question, for the differing success of interactors affords the opportunity for selection to shape the pool of cultural variation.

THE ARTIFACT AS INTERACTOR IN EVOLUTIONARY ARCHAEOLOGY

Within evolutionary archaeology, explicit consideration of what constitutes an interactor—that is, the entity that participates in the ecological interactions that give rise to differential persistence (selection) of replicators—has tended to focus on the individual human or the group of humans (e.g., Dunnell 1978a; Lipo and Madsen 1995). Lipo and Madsen (1995) recognize that the criteria by which interactors can be recognized are the same criteria set forth by Lewontin (1970) as necessary for natural selection to take place. That is, entities can be interactors (and natural selection can take place in traits of interest) if (1) the entities vary with respect to the traits; (2) the traits impart different probabilities of survival and reproduction; and (3) there is some correlation between the traits of antecedent and descendant entities. Although Lipo and Madsen (1995) focus on how Lewontin’s (1970) three criteria may apply to supraindividual human organizations, Lewontin’s original emphasis was on the generality of these cri-

teria. Indeed, he goes on to discuss how these properties make natural selection possible at scales ranging from self-replicating molecular variants up to species and ecological communities.

Lewontin's three criteria are general enough to subsume not only supraindividual cultural entities (Dunnell 1978a, 1989; Lipo and Madsen 1995) but also infraindividual cultural entities, such as artifacts. Criterion 1 is unproblematic, since virtually any trait that can be measured on some class of artifact will vary, whether by design or by accident. Likewise, criterion 2 appears to be unproblematic, since some of the ways in which artifacts vary make them more or less efficient at performing certain tasks, more or less attractive to potential users, and so on, and such variation affects the probability that the artifact's design will be reproduced. O'Brien and his colleagues (O'Brien and Holland 1990, 1992; O'Brien et al. 1994) have expended considerable effort to show that engineering considerations can be used to assess the probability that artifact designs will persist (also see Neff and Larson 1997). Criterion 3, applied to artifacts, is no more than a statement that designs can be communicated between individuals, so it too would appear to be unproblematic. In sum, products of human design found in the archaeological record possess all of the properties necessary for natural selection to operate; put another way, artifacts on their own can be viewed as interactors.

But the theory of natural selection is not merely applicable *in principle* to cultural entities below the level of the individual human. Even more important, we can expect selection at the infraindividual level to be a far more potent source of directional culture change than selection at the individual level. Following Lewontin (1970), this expectation is derived from Fisher's Fundamental Theorem of Natural Selection, which holds that the rate of evolution depends on the variation in fitness of the entities undergoing selection. One determinant of variation is heritability of fitness differences. Heritability of cultural traits at the individual or group level is severely attenuated by phenotypic adjustment following transmission (Dunnell 1989). In fact, both the individual and the group can serially adopt alternative, mutually exclusive cultural traits. At the infraindividual (artifact) level, in contrast, plans or designs can be transmitted with almost perfect fidelity, so that descendant artifacts or practices are nearly exact replicas of antecedents.

Lewontin (1970) also points out that, according to Fisher's Fundamental Theorem of Natural Selection, mean fitness is inversely related to generation length, and the rate of evolution therefore is greater for entities with shorter reproductive cycles. A cultural evolutionary process that is tied to the human reproductive cycle (20 or 25 years) will proceed at a snail's pace compared to one that is tied to the reproductive cycles of artifacts, which can be as short as the time it takes to communicate design information between individuals. Or, as Dunnell (1989) noted, "cultural transmission mitigates many of the effects of generations." Rindos (1985:72) appreciated the importance of cycle length in cultural evolution as well: "[T]he system (cultural inheritance) will have a higher mu-

tation rate, a larger number of recombination episodes, and, hence, more potential selection episodes per unit of time.” From this, coupled with the fact that cultural information can be disseminated much more widely than genetic information, Rindos (1985:72) concluded that “part of the additive fitness of the genetic capacity for cultural behavior is the capacity it gives individuals to change behavioral traits rapidly.”

The foregoing expectations derived from evolutionary theory converge with casual reflection about cultural implements we use every day. Both theory and observation suggest that evolution of artifacts (or other human practices determined by cultural inheritance [Rindos 1985]) proceeds much more rapidly than the evolution of cultural or biological traits that are tied to the reproductive cycle of humans. The added insight gained from theory is that acceleration of the rate of evolution is a consequence of the opportunity for selection to operate at a level below the individual human, where it directly evaluates artifacts and cultural practices.

The artifact-as-interactor view can be illustrated with many everyday implements. Take the desktop computer. When many of us began our careers in archaeology, there was no such thing as a desktop computer, whereas now our jobs would seem quite onerous without one. Which of these serial alternatives—computer user or non-computer user—should be considered to characterize the phenotype of an individual archaeologist or some group of archaeologists? Since this culturally inherited trait (desktop computer use) can exist serially in alternative states within the same individual or group, it seems clear that neither the individual human nor the group of humans can be considered the interactor in this case. That role falls instead to the trait itself. The trait is the phenotypic manifestation of a set of coherent cultural instructions, and its relative success or failure determines whether those cultural instructions increase or decrease in frequency in the cultural pool. This process has nothing to do with the success of humans at reproducing human bodies or the 20-to-25-year cycle time needed for them to do so. Non-computer use all but disappeared from the cultural pool in less than a single human generation, and this had nothing whatsoever to do with differential biological reproduction of humans.

The example of the desktop computer can be taken a bit further to underscore the independence of the genetic and cultural pools. How can we account for the remarkable evolution of desktop computers that has taken place since they first appeared on the scene 25 or so years ago? Every couple of months some dramatic innovation in hardware or software seems to appear. We evolutionary archaeologists want to explain this evolutionary history, like other artifact evolutionary histories, as the result of a selection-driven process. What part might success at reproducing human bodies have played in this process? Virtually none: it is the differential persistence of cultural instructions (hardware and software design) that has driven this 25-year-long evolutionary history. Even if one’s choice in computers had a very strong effect on his or her reproductive potential, we could not explain changes on a time scale of weeks and months

by reference to processes that are tied to the human reproductive cycle. Even if all computer users have produced zero offspring, desktop computers will continue to evolve as design instructions persist differentially among the dwindling number of computer users.

But, even if computer users have no offspring, computer use need not dwindle as a proportion of the total population because the reproduction of cultural traits (like computer use) can take place via recruitment from the non-computer-using fraction of the population. Ultimately, of course, any cultural trait with such a severe impact on reproductive success will go extinct, even if it spreads by cultural means throughout the entire human population. The fact that the spread of this hypothetical trait by cultural means to all humans leads to the extinction of humans should not blind us to the fact that, by virtue of its occupation of all potential hosts, the trait must still be considered to have been favored by selection. It is the extinction of the host humans, not its own failure to replicate, that leads to its extinction.

We cannot gloss over the fact that the human gene pool and cultural pool are distinct. To maintain that the same processes lie behind the differential persistence of variation in both pools would be entirely obtuse. On one hand, we can confidently attribute directional changes in gene frequency to a process in which humans with different genotypes achieve different levels of success at reproducing human bodies. Memes, on the other hand, can be replicated without the reproduction of any new human bodies, as the desktop computer example illustrates. To ignore this crucial difference is to raise a formidable barrier to serious theorizing about how evolution shapes the archaeological record.

THE CREE SNOWMOBILE FALLACY

Leonard and Jones (1987) introduced the term “replicative success” to refer to the Darwinian fitness of traits, such as artifacts, as distinct from the reproductive success of individuals. The concept of replicative success takes us part-way to the idea that selection acts at an infraindividual level.⁶ Leonard and Jones (1987:216) themselves apparently reject this view, arguing that “replicative success of a particular trait might or might not affect the reproductive success of the bearer . . . those that do can be considered functional, and those traits with no selective import termed stylistic or neutral.” In other words, while artifacts can exhibit different fitness values, they cannot assume the logical status of individuals with reproductive capabilities. Selection remains at the level of the individual, the replicative success of cultural traits being a mere by-product of the differential reproductive success of individuals. Perceiving ambiguity in such statements, Boone and Smith (1998:S145) have asserted recently that “most of the evolutionary archaeological literature is quite unclear on the mechanism(s) underlying selection.”

Lyman and O’Brien (1998:643) also toy with the idea of infraindividual se-

lection: “[C]ultural replicators are transmitted by social learning and find empirical (phenotypic) expression, of greater or lesser fidelity, in interactors (Graves and Cochrane [1998] use the term “vehicles”) such as Clovis points and shell-tempered pottery.” On its face, this statement would seem entirely consistent with the views advanced earlier, since calling artifacts “interactors” is a clear acknowledgment that selection operates directly on artifacts. Unfortunately, however, in dealing with a specific example of artifact frequency change—Cree snowmobile use—Lyman and O’Brien manifest considerable ambivalence regarding the scale on which selection operates.

Like the adoption of desktop computers discussed earlier, adoption of snowmobiles among the Cree took less than one human generation (Winterhalder 1981). Boone and Smith (1998:S146) point out that it would be ridiculous to attribute such a rapid frequency shift to a selective process driven by differential success of individuals at reproducing human bodies. Lyman and O’Brien (1998: 619) respond by saying that “the ultimate reason snowmobiles replaced snowshoes among the Cree is that those who inherited that trait—regardless of how and to whom it was transmitted—outcompeted those who did not, thereby enhancing both their *reproductive* success and the *replicative* success of snowmobiles.” Later on (p. 643) they add that “we hypothesize that Cree fitness as measured by reproductive success increased [as a result of snowmobile use].” Clearly, their hypothesis is plausible. But this simply has no bearing on the issue raised by Boone and Smith, which is whether the adoption of snowmobiles in less than a human generation can be explained by higher birthrates among snowmobile users. Clearly, it cannot.

The fallacy to which Lyman and O’Brien succumb, which we may call the Cree Snowmobile Fallacy, is the assumption that, if fabrication or use of some artifact enhances human potential to make more human bodies, then the latter effect is a *necessary* part of the evolutionary explanation for the artifact. Few would seriously question the idea that artifacts *can* impact human biological reproductive success; as Lyman and O’Brien correctly point out, this is a plausible hypothesis about the long-term effect of Cree snowmobile use. The fallacy is to claim that potential future effects explain frequency changes that have already taken place. The only necessary component of selection-driven change in artifact frequencies (e.g., snowmobiles among the Cree) is some kind of interference with *cultural* transmission. Enhanced or diminished production of human bodies to serve as targets of cultural transmission events is one possibility, of course, and it may explain some of the artifactual variability in the archaeological record. Certainly it does not help explain adoption of snowmobiles among the Cree or adoption of desktop computers among late-twentieth-century U.S. residents. More generally, the theoretical arguments advanced earlier regarding cultural evolutionary rates at different scales provide little basis for the belief that this is the major or even a very important mechanism in artifactual evolution.

EVOLUTIONARY ECOLOGY VERSUS EVOLUTIONARY ARCHAEOLOGY

The fact that Boone and Smith (1998; Boone 1998) have helped to expose a fallacy to which evolutionary archaeology might be susceptible does not warrant an endorsement of the broader methodological agenda they propose for archaeology. The “evolutionary ecology” approach they favor “explains cultural and behavioral change as forms of phenotypic adaptation to varying social and ecological conditions, using the assumption that natural selection has designed organisms to respond to local conditions in fitness-enhancing ways” (Boone and Smith 1998:S141–S142). But, since our biological ancestors of 100, 1,000, and 10,000 years ago possessed evolved decision-making capabilities similar to ours, how can we explain the vast gulfs that separate the artifactual repertoires of human populations living at these different times? Boone and Smith (1998:S145) propose that “the aggregate consequences of individual phenotypic adaptation can both change environmental conditions . . . and elicit new strategic phenotypic adaptation to these altered conditions.”⁷ But this formulation merely begs the question of what mechanism “elicits” the new adaptation. To leave unspecified the mechanism by which new forms can come into being is to abandon the goal of scientific explanation, for scientific explanation is nothing if it is not mechanistic.

Evolutionary archaeology proposes that new cultural forms can be created mechanistically through a two-step process involving, first, the generation of novel variation and, second, selective retention of the variants. As we have seen, however, tying the second step in this process to reproduction of human bodies is a mistake that drastically curtails the theory’s explanatory power. The artifact-as-interactor view decouples the differential persistence of cultural variation from the reproduction of human bodies. It does so by recognizing that reproduction of artifacts is completely autonomous from the reproduction of human bodies but that artifacts nevertheless exhibit differential fitness values (differential replicative success). Selection at the level of the artifact-as-interactor takes place to the extent that conditions in the world affect the probability of successful transmission of the cultural instructions underlying the manufacture and use of the artifact. Traits of artifacts that increase in frequency as a result of this selective process are the ones we must consider “functional” in Dunnell’s (1978a) terms.

THE EXTENDED PHENOTYPE

The artifact-as-interactor point of view underscores the fact that the human phenotype is determined by information drawn from both the genetic and the cultural pools, via genetic and cultural transmission. Dunnell (e.g., 1989) has been careful to acknowledge this dual inheritance requirement. In some cases, however, the point seems to be overlooked. For instance, commenting on a

passage from Dawkins (1982:198) that deals with extrasomatic constructions of caddis flies and spiders, O'Brien et al. (1994) rhetorically ask, "Unless one wishes to maintain a sacrosanct category for human artifacts, what, logically, is the difference between a mud-dauber's nest and daub from a Mississippian house?" The artifact-as-interactor point of view provides the answer: whereas the mud-dauber's house is determined (presumably) by genetic inheritance, the Mississippian house is determined primarily by cultural inheritance. Selection in the first case must be sought in the conditions that promote differential persistence of mud-dauber genes for house building, whereas selection in the second case must be sought in conditions that promote the differential persistence of cultural instructions about how to build a Mississippian house.

The fundamental importance that O'Brien et al. (1998:495) attach to the conception of artifacts as parts of the human phenotype is well justified. But this insight does not entitle us merely to pay lip service to the fact that cultural inheritance and genetic inheritance follow distinct channels, while we ignore the major implications for evolution of taking these two distinct channels seriously.

CULTURAL VIRUS THEORY

The artifact-as-interactor perspective advocated here is anticipated in many respects by "cultural virus theory" (Cullen 1993, 1996a, 1996b). An axiom recognized in both perspectives (e.g., Cullen 1996b:48) is that, because cultural and genetic characteristics of human phenotypes are reproduced at different times and by different means, cultural genealogies are independent of the genealogies of their biological host bodies.⁸ Artifacts are thus granted the status of individuals, or, in the terms used here, "interactors." Since they do not reproduce (e.g., Brew 1946), they cannot be like organisms, yet, argues Cullen, they can be considered precisely parallel to viral phenomena. Like viral phenomena, cultural phenomena require the services of organisms for their own reproduction. Another way to put this is that artifacts do not, like organisms, contain the design specifications for their own reproduction. As Cullen perceives, the ability for the design to be replicated, not where the design happens to reside, makes artifacts, like viral phenomena, subject to evolutionary processes.

Because genetic inheritance and cultural inheritance are completely independent of one another, the genealogies within which one may expect to observe descent with modification are independent of one another (Cullen 1993). Furthermore, cultural genealogies have no special relationship with human bodies, but instead multiple cultural genealogies may flow through a single body, and the cultural genealogies relevant to an individual human at one phase of his or her life may be completely different from those that are relevant at some other phase.

Cullen uses the insight gained from cultural virus theory to argue for a focus on human consciousness and human agency, even suggesting the existence of

considerable common ground between neo-Darwinian and postmodernist thought (e.g., Cullen 1993:191). At least for archaeology, this would seem to be a huge tactical and strategic mistake. Archaeology has no access to human minds or the memes they contain, except via the hard parts of the human phenotype that are preserved in the archaeological record. In the interest of empirical sufficiency, therefore, the emphasis in our theory building must be on artifacts-as-interactors. This is not to suggest that we should ignore evolutionary psychology or the emerging field of memetics (Rose 1998; Wilkins 1998), any more than paleobiologists should ignore genetics. Rather, it is a caution against imagining that we can directly investigate social learning or the evolved decision-making capabilities of humans. We cannot do this. However, we can develop a body of theory and methods for studying artifacts and other human cultural traits conceived as coevolving interactors within ecological assemblages. I develop this point in a little more detail in the following section.

ADDITIONAL IMPLICATIONS

Conceiving of artifacts as interactors, subject to selection independently of the human bodies in which their corresponding memes are replicated, may promote some new ways of looking at the archaeological record. For one thing, recognizing the dissociation between reproduction of the biological and cultural sides of the human phenotype makes the individual human (body) fade into theoretical and methodological insignificance. Highlighted instead are cultural genealogies and the interactions among artifacts that lead to differential persistence of information within cultural genealogies. One consequence of this shift in emphasis is that human intention is no longer internal to the evolving populations of interactors. To Darwin-dreaders,⁹ lack of concern with intention is reductionistic and/or misanthropic (Rindos 1985:84); but, as Lyman and O'Brien (1998:617–619) point out, contentiousness about the role of intention is high even among those seeking to build evolutionary approaches to the archaeological record. Once the interactors (artifacts) are separated from the entities (humans) that are capable of showing intention, some of the contentiousness may dissipate. Intention and other kinds of human psychological states certainly play a role in generating new variants of cultural replicators (Rindos 1985). These psychological states are givens, just as the fact that DNA molecules undergo base-pair substitutions with a certain probability is a given. But changes in artifact frequencies—the means by which we can observe evolution archaeologically—require a second step, in which ecological interactions take place, and variant forms are selected for or against. As Dennett (1998:S157) says, “There is no conflict between the claim that artifacts . . . are the products of natural selection and the claim that they are (often) the (foreseen) products of intentional human activity.”

Relegating humans and human intent to completely mechanistic roles in the evolutionary process no doubt will be viewed as heresy both by Darwin-dreaders

and by archaeologists who prefer to imagine that our science gives us access to a full record of past human behavior, organization, and conceptual systems. I do not see it that way. What more could we ask for than a theoretical perspective that tells us to focus our attention on that which is concrete and observable in the archaeological record? This perspective takes us a long way toward empirical sufficiency. It avoids the perceived need to “reconstruct” behavior from the artifacts and then to apply our theories to the reconstructed behavior. It tells us to analyze artifacts or other concrete archaeological phenomena in terms of how well their design qualifies them to persist (i.e., in terms of possible variation in fitness values). Fitness differences are explicitly held to reside at the level of the artifact (or other cultural trait), and selection directly affects the information content of the cultural pool.

Of course, evolutionary archaeologists have been recommending for some time that we focus our efforts on the analysis of artifact design (e.g., O’Brien and Holland 1990, 1992; O’Brien et al. 1994). Moreover, a number of convincing empirical studies have appeared in which the fitness of traits, such as artifacts, is assessed completely on its own (e.g., Dunnell and Feathers 1991; O’Brien and Holland 1992; O’Brien et al. 1994). In this light, adopting the artifact-as-interactor perspective may amount to little more than an explicit acknowledgment of the theoretical basis for much existing practice within evolutionary archaeology.

However, I believe that if we explicitly concede that selection and other evolutionary mechanisms work at an infraindividual level where cultural variation is concerned, we will start to look at things differently. We will seek better methods to identify common cultural descent (e.g., Neff 1993; Tschauner 1994), so that we can monitor the effect of selection and drift over time within evolutionary cultural lineages. Coevolutionary relationships between different kinds of artifacts or cultural traits will take on a more important explanatory role, and we will start to view assemblages as ecological communities, within which the various included artifact classes constitute environmental conditions relevant to the selection-driven evolution of other artifact classes. In some cases, we may find that entities identified by our common sense as artifacts-as-interactors are more accurately conceived as parts of interactors conceived at larger scales (Dunnell 1978a, 1989; Lipo and Madsen 1995). We will thus redefine “group selection” to refer not to selection acting on groups of human bodies but to selection acting on fitness differentials among groups or subassemblages of artifacts.

Interestingly, aspects of the artifact-as-interactor perspective are far from novel. As Lyman et al. (1997:19–20) have pointed out recently, Kroeber (1931) looked at cultures in approximately this way almost 70 years ago. Kroeber suggested that cultures should be considered composites of elements of various origins, in much the same way that floras and faunas are temporary aggregates of organisms with distinct evolutionary histories. Kroeber even drew a parallel between cultural elements and species.

Kroeber's view of culture, like the artifact-as-interactor view, is explicitly ecological. Representatives of independently evolving lineages of cultural traits (practices, tools, and other artifacts) are assembled into unique configurations—ecological assemblages—at different places and times. Some lineages of cultural elements can be traced through many distinct ecological assemblages over very long time spans: the keystone arch used by the Romans over 2,000 years ago was later incorporated into medieval cathedrals and still later into public architecture all over the world. But one can also identify key innovations that dramatically increase the energy available and dramatically expand opportunities for the diversification of cultural lineages: the invention of steam power and other innovations of the Industrial Revolution created the opportunity for a dramatic expansion in richness of the cultural biota. Unfortunately, it is also true that the dramatic success enjoyed by various elements of Western European culture has led to the extinction of culture elements of most non-Western people.

The main point I want to emphasize here is that adopting a Darwinian view of the archaeological record does not require us to borrow units from biology. That is basically what we are doing if we insist that individual humans or groups of humans must be the units of selection—the interactors—in the evolution of the cultural sides of our phenotypes. We seem to know instinctively that this will not work; that is why Leonard and Jones introduced the idea of replicative success and why O'Brien and Holland have spent so much time talking about how to use engineering analysis to measure the fitness of artifacts. I suggest, however, that we will make more rapid progress toward a robust evolutionary theory of the archaeological record if we admit that cultural transmission makes each individual human a nexus of multiple cultural genealogies, each shaped independently by selection and other evolutionary processes.

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NOTES

1. "Invention" here refers to the evolutionary innovations of sex at the beginning of the Cambrian period and cultural transmission sometime during the evolutionary history of the human lineage. In both cases the "invention" was not a single engineering achieve-

ment, like the inventions of humans, but rather they resulted from the selection-driven cobbling together of available traits over a long period of time.

2. The qualifier “largely” is necessary here because, in the framework of Dawkins (1982), phenotypic expression of a gene is not necessarily restricted to the form or behavior of the organism in which a gene resides. Instead, “the replicator should be thought of as having *extended* phenotypic effects, consisting of all its effects on the world at large, not just its effects on the individual body in which it happens to be sitting” (Dawkins 1982:4). This “extended phenotype” concept has been used to subsume culturally transmitted traits of humans (O’Brien et al. 1994). However, whereas Dawkins’ original concept is developed with reference to gene selection, O’Brien et al. use the concept to justify including artifacts within the definition of the individual, and the individual remains the target of selection.

3. Groups of individuals can, arguably, constitute vehicles as well (Wilson and Sober 1994). This insight underlies Dunnell’s (1978a; also see Lipo and Madsen 1995) argument that the level of selection may shift up with the advent of complex societies. This qualification does not affect the following argument.

4. Most evolutionary biologists would probably agree with this statement. Dawkins (1982) might take issue with it, arguing that selective retention of some gene takes place because of that gene’s effects on the world, whether those effects are local (through the immediate gene vehicle) or more distant (through the extended phenotype). The term “interactor” (Hull 1980) has a slightly different connotation, encompassing any effect of a gene upon the world, and thus may be preferable here. Obviously, I do not pretend in this chapter to resolve the biological debate over gene selection versus individual selection. The key point here is that sex shifts the level of selection down: individual selectionists would say that it shifts the level down from the organism-lineage to the individual organism; gene selectionists would say that it shifts the level down all the way from the organism-lineage to the individual gene.

5. The idea that cultural traits are transmitted by an inheritance system independent from the inheritance system for genes is so obvious that it surely should generate little controversy. Curiously, however, several individuals who commented on an earlier draft of this chapter were quite strongly opposed to my assertion of complete independence. Coming from evolutionary archaeologists, this opposition is even more curious considering that, as I discuss later on, the idea that cultural traits have their own fitness is not at all novel or controversial in evolutionary archaeology (e.g., Leonard and Jones 1987). I hope to show in the remainder of this chapter that the “radical” (but undeniable) notion that cultural and genetic transmissions are completely independent has clear implications about where we should look for selection when we are concerned with cultural traits.

6. Rindos’ (1985) “second type of cultural selection” (CS₂) might be considered another precursor of the infraindividual selection perspective. According to Rindos (1985: 73), “cultural selection of the first kind” (CS₁) is precisely analogous to natural selection except that feedback is to the cultural inheritance system rather than the genetic inheritance system. CS₂, in contrast, refers to how well a given symbol fits within a given cultural system of symbols.

7. Practitioners of evolutionary ecology do not deny the possibility that selection of culturally inherited traits takes place, but they view its explanatory import as severely limited. As Boone (1998:632) says with reference to the replicative success of Cree snowmobiles, it “explains nothing.”

8. Todd VanPool (personal communication, 1998) has used the appropriate label “Cul-

len's Paradox" to refer to the absurdity of trying to explain differential persistence of culturally transmitted traits by reference to a process (differential reproduction of human bodies) that need have no effect whatsoever on cultural transmission. In light of recent debates, as discussed previously, I prefer to refer to this as the "Cree Snowmobile Fallacy."

9. I borrow the label "Darwin-dreaders" from Dennett (1995). In this context, I am referring to those who oppose any kind of Darwinian perspective on human history.

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Chapter 3

Directionality, Function, and Adaptation in the Archaeological Record

Timothy D. Maxwell

INTRODUCTION

In his 1978 article on the fundamental dichotomy between style and function, Dunnell challenges archaeology to take an explicitly evolutionary approach to understanding the archaeological record. After pointing out that many traits studied by archaeologists have discrete selective values and thus display diverging patterns of frequency change over time, he notes that the distinction between style and function becomes critical. “Style,” he states, “consists of forms having no detectable selective values,” while “function is manifest in those forms that directly affect the Darwinian fitness of the populations in which they occur” (Dunnell 1978:199). The long-term outcome of this distinction has been portrayed graphically (O’Brien and Holland 1990) where a variant under directional selection, a functional trait, increases in frequency at a steadily decelerating rate, while a neutral, or stylistic, trait will drift randomly across generations, either eventually falling to zero or becoming fixed in the population.

Dunnell (1978) provides useful definitions for function and style but does not offer an explicit methodology for detecting the difference. As illustrated by O’Brien and Holland (1990), the diachronic frequency representation of a trait’s history may be helpful, but as they also point out, diverse evolutionary mechanisms may have played a role in creating the observed patterns. Increasing frequencies may implicate the replicative success of a trait (Leonard and Jones 1987) and, by extension, possible adaptation. In other cases, fluctuating frequencies may reflect the neutrality of a trait. However, evaluating the causes behind observed changes in frequency patterns is not always so straightforward. Once evident change in the representational success of a trait is detected, the researcher must consider whether the change is a product of selection or random

walk (O'Brien and Holland 1992). Additionally, traits under study may be linked to other traits undergoing selection or drift, and their frequency representation might simultaneously change in lockstep fashion. The following examples illustrate the possibilities.

DIRECTIONALITY

Selection

If there is directional selection for a particular trait, for example, increased height, then taller organisms have, or tend to have, greater reproductive success than shorter ones, resulting in a pattern of phenotypic change. The possible reasons for the differential success of taller individuals could be multiple, requiring evaluation of possible selective regimes. As a simple example, imagine that in one population taller individuals are better able to detect predators, while in another setting, taller individuals have an increased ability to locate food. In the first case, since taller individuals are able to better escape predation, differential reproduction should result from the increased survivorship of taller individuals, while better nutrition and perhaps an associated increase in fecundity may lead to differential reproduction in the second instance.

Random Walk

A trait can also increase in a population through the process of drift or random walk, and its frequency representation might potentially mimic a pattern of directional selection. In a finite population, particularly a small one, some variants can outreproduce others simply by chance. In other words, by chance alone, a positive correlation between trait value and level of reproductive success can occur. As argued by Brandon (1990), if we envision a population where every member flips a coin prior to reproduction, we can see the probabilities for directional frequency changes that are based on chance events. If the coin comes up heads, the organism reproduces; tails, and the organism dies. In a small population, it is not improbable that a disproportionate number of taller or shorter members would get lucky at the coin toss. Feller (1968) has demonstrated that if the two sides of an unbiased coin are assigned the values of $+1$ and -1 , and the cumulative value of the results of many tosses is calculated, there is a surprisingly large probability that the value of the running sum will remain positive or negative for a long period. Brandon (1990) uses these results as an analogy for trait frequency change where one considers only the possible alternative states of a trait, for example, tall or short, between generations and argues that there is a nontrivial probability of getting a significant run of frequency change in one direction.

Therefore, in a situation where a single trait state is associated with individuals who are "lucky at the coin toss," evolution will occur, but it will not be due to

natural selection. If the pattern continues for many generations, the trait state will eventually go to fixation throughout the population without natural selection ever having played a role. This pattern of change is seen as an inevitable consequence of finite population size. There is some question as to whether trait states as studied in the archaeological record behave in a manner equivalent to alleles, but their alternative states can vary in the way alleles, do (O'Brien and Holland 1992:49–50).

Sorting

A third possible case for a directional increase in height over time is related to the possibility that another trait has undergone selection or drift and created an associated frequency change in the number of taller individuals (see Hurt et al., Chapter 4 in this volume for further discussion of sorted traits). For example, changes in height might occur given the phenotypic plasticity or flexibility of the human species in response to environmental conditions. In the United States over the past 100 years, there has been a secular trend of increase in the average height of the male population (Steegmann 1991). Better childhood nutrition is proffered as an explanation for increased stature (Garn and Clark 1975; Martorell 1989; Steegmann 1985), although such explanation has not gone unchallenged (Henneberg and van den Berg 1990). Although improved techniques in agriculture may have resulted in greater food availability and nutrition, and those techniques may be affected by selection, the pattern of increased height does not result from processes that directly selected for height. The plasticity available in the species led to the consequent change as nutrition improved, leading to the directional patterning in increased height.

As these examples illustrate, if a trait chosen for study shows a frequency increase over time, it may be due directly to selection, be a result of random walk, or be a consequence of selective conditions affecting other traits possessed by the organism with which the trait under study is somehow associated. Therefore, a directional pattern of change in a trait under study cannot be assumed to be strictly the result of selection for a property directly associated with the trait. Given these other possible factors, determining the reasons behind a directional pattern of frequency increase can be a daunting task.

FUNCTION

At one time, biologists argued that the proper approach to determining the evolutionary history of a trait was to first try to explain it as the result of natural selection. If those attempts failed, then the phenomenon could justifiably be explained as a product of chance (Mayr 1983). More recently, biologists have argued that evolutionary investigations should begin with a consideration of drift or chance, since these hypotheses represent proper “null hypotheses” (Beatty 1992:281). Only when the null hypothesis is rejected, can one invoke arguments

for selection. Again, analysis of frequency change can provide clues to the evolutionary mechanisms involved, but given the multiple possibilities for directional change, a consideration of the function of a trait may be necessary. As defined by Dunnell (1978), any trait that increases the Darwinian fitness of its possessor is functional, and the concept of function is central to adaptation. Because adaptations result in increased Darwinian fitness, we expect there to be a tight connection between the adaptation and some function that benefits the possessor. A trait must have a necessary interaction with external conditions for that trait to be functional, and natural selection operates on such functions, leading to adaptation.

Etiological or Proper Function

This brings us to two concepts of function, the first being that of “proper function,” or the etiological reasons that the particular trait was not selected against. The existence and form of an item are explained by its antecedent causes rather than by its goal or purpose. Wright (1973) maintains that valid functional statements must specify the reason that an adaptation appeared within an evolutionary framework. In other words, function must be located in a selective history. If identification of a function lacks this component, it is only a descriptive statement with no explanatory value. This viewpoint is the most widely accepted in the philosophy of biology today (Brandon 1990; Godfrey-Smith 1994; Gould and Vrba 1982; Millikan 1989; Neander 1991; Sober 1984). To ascribe a proper function to an item is to claim that earlier items of the same type had the effect that we now label a proper function and that their having had that effect helps explain the presence of later items of that type (Griffiths 1993:411–412). In other words, etiological approaches look for a “triggering cause” (Dretske 1989). Proper functions differ from other functions in that they can be cited to explain the presence of a functional item. This means that a trait will have a proper function only if it is an adaptation for that function. The trait must have been selected because it performs that function. In Brandon’s (1990: 188) words, a functional trait must increase the “relative adaptedness of [its] possessor.”

The etiological approach has some disadvantages, however. Etiological approaches do not recognize those functions of traits that have not played a role in selection or traits that have been selected for but are now used differently. For example, it has been argued that feathers did not originate as adaptations for flight (Gould and Vrba 1982:7). It is postulated that they originated as insulation and only later were co-opted for flight. Thus, an etiological explanation centers upon the evolutionary origin of feathers in terms of selection for effective thermoregulation and ignores their subsequent function in flight. The distinction between the functional reasons for the origin of traits and the maintenance of those traits has also led to the formulation of new terms, such as exaptation (Gould and Vrba 1982), to explain new uses for traits that originated as adap-

tations to other pressures. For example, Gould and Vrba (1982) would deny that sand digging is a function of turtle flippers, since that use of turtle flippers was not shaped by natural selection. For them, turtle flippers have an ancillary use that was not shaped by selection, so sand digging should not be considered a function of flippers but an exaptation.

Causal Role Function

This brings us to a second approach to the study of function. If we ask about the function of feathers as they exist today, say in tropical or subtropical birds, analyses might turn to more recent reasons for the maintenance of feathers—their function for flight. (For flightless birds, explanations might appeal to phenomena such as sexual selection; e.g., the peacock's feathers function to attract mates.) These are different functions from those for which the trait was selected. Here, an analyst is interested in the mechanical or behavioral operations of the trait and is not limited by the question of origins (Cummins 1975). These analyses are ahistorical in nature and make no distinction between currently functional traits and the evolutionary basis for the function. Of interest are the effects of traits that provide some valuable property and can be discussed without worrying about how they came into being. This approach looks for a “structuring cause” (Dretske 1989), or an explanation of how things work. Neander (1991:181) refers to these as studies of “causal role function,” and they would fall into the category of what Schiffer and Skibo (1987) call “performance characteristics.” Returning to the example of feathers, one can analyze the differential performance of various bird feathers and explain how some feathers function to support sustained gliding, while other feather characteristics allow greater speed. These functional analyses need not address the question of why there was selective pressure for either condition. Perhaps, most importantly, this approach to function allows a single trait to have more than one function, since the effect of feathers on thermoregulation would not necessarily be dismissed.

This form of functional explanation may not distinguish functions from mere dispositions or accidents, however. For example, the study of prehistoric rock mulches in the North American Southwest shows that the mulches conserve soil moisture and raise soil temperature (Maxwell 1995). However, the effect of the mulch on soil temperature appears to result from the dispositional property of rocks to store and release solar radiation, a property likely unrelated to the reasons for the use of a rock mulch. Causal role functions also have the potential to mislead if a study, in a sense, focuses upon the “wrong” slice of time. Consider for a moment the human appendix. Although a remnant of a previous digestive organ that was likely an adaptation for our ancestors, it currently has no definable function. So analysis of its current function would yield no results. That the appendix is still present might be explained by the fact that there is no genetic variation affecting its representation; thus, there is no variation upon

which selection can act (Griffiths 1993:417). However, its original function will not be determined by examining its current function.

In one sense, the two approaches to function do not differ—they both attempt to explicate a mechanistic relationship between a trait and some feature of the environment. This is frequently where archaeologists begin to analyze suspected functional relationships. A danger lies, though, in giving every identified function a role in adaptation, which often happens. To return to the previous example of the rock mulches found in the Southwest, engineering analyses of their performance show that many functions can be ascribed, for example, effective soil water conservation, increased water infiltration, soil temperature increase, and protection of seedlings from wind damage. The totality of these functions has been described as an adaptation to the southwestern environment (Cordell et al. 1984; Lightfoot 1990). It is unlikely, however, that all of these functional effects played a role in the appearance and maintenance of rock mulches (Maxwell 2000). Comparative analyses with other prehistoric southwestern farming techniques in other environments indicate that only the function of soil water conservation played a role in the origin of the mulches and became the basis for adaptation. The other effects of rock mulches provide only accidental or incidental benefits for crop production.

Although the etiological function of a rock mulch may have been identified, the possibility exists that the other identified effects explain the persistence of the rock mulches. Although not identified in the case of rock mulches, adaptations may possess functions that have consequences for continuation of the trait, and those functions may be unrelated to the reasons for the trait's origin. However, clarification of either etiological origins or causal role functions will satisfy Dunnell's (1978) definition of function, that is, those forms that affect Darwinian fitness. This is where Dunnell's definition departs from the two described approaches to function and is perhaps the more useful. His definition will accommodate both perspectives. The determination of the etiological origins of a trait can explain why it is an adaptation, which increases fitness by definition, while the identification of mechanistic interactions between nonetiological functions, or exaptations, and the environment might potentially explain persistence of the trait and how it affects fitness.

Dunnell's (1978) proposed identification of function may be definitional, but the methodology exists to properly place a trait within each definition. Engineering studies of performance characteristics are a starting point; they lead to the identification of the functional effects of a trait or attribute. While engineering analyses can identify the functional effects of traits, though, they cannot identify function in Dunnell's (1978) sense. That is, such studies do not clarify the functional role of the trait in an evolutionary history. Engineering studies are ahistorical in nature, simply isolating and identifying the mechanistic operation of a trait or attribute. Further analysis is required to determine the role of the isolated behavior in an evolutionary context. To illustrate and perhaps overcome some of the difference between Dunnell's definition and common notions

of function, it may be useful to subdivide the concept of function into *functional effect* and *functional role*.

Functional Effect

A functional effect is of an entirely mechanistic nature; it is the physical behavior and consequences of the operation of a trait or attribute. To ascribe a functional effect to a trait is to make a statement about its mechanical behavior but says nothing about the role of the effect in evolutionary change. Many traits have functional effects, but the functional role of a trait affects the fitness of its owner. As discussed in an earlier example, although rock mulches have a functional effect on soil temperature, that effect had no functional role in the origin or maintenance of rock mulches.

Identification of a trait's functional effect does not necessarily determine the role of the effect in the evolutionary history of the possessor of the trait. Performance studies can often ascertain a function for a trait regardless of the environment in which the function operates. Some functions will be timeless and spaceless, invariantly operating in the same fashion even after conditions have changed to the point where they no longer have a role in the life of their possessors. But performance studies do not necessarily clarify the role of the function in the history of the trait's possessor.

Functional Role

In contrast, a functional role is given to an effect that impacts Darwinian fitness and is, therefore, synonymous with Dunnell's definition of function (1978). Functional roles are critical for understanding evolution, since any trait might have multiple functional effects. For example, the human heart has the functional effect of circulating blood, but its beating also has the functional effect of calming infants. It is the first effect, though, that provided a functional role for the heart in evolutionary history. The second effect might be beneficial but probably played no functional role in the selective history of heart development. As found in the study of rock mulches, their functional effect in the conservation of soil moisture gave them a functional role in their appearance and maintenance, not their functional effect on soil temperature.

A statement about functional effect is not synonymous with a statement of causal role function. As with etiological identifications of function, a causal role statement attempts to provide an explanation that singles out a role for the function of a trait in an evolutionary context, even if the explanation is not concerned with origins. Statements of functional effect need not have a similar goal; they simply are identifications of the behavioral capabilities of an item. Studies of causal role function attempt to explain an identified function in specific contexts. For example, what function did kivas serve in prehistoric southwestern societies? While village integration may be a postulated functional effect

for kivas in a causal role function analysis and be deserving of a functional role analysis, the fact that belowground kivas also have the functional effect of retaining heat better than aboveground rooms would likely offer little insight into the functional role of kivas.

Functional role statements identify how an adaptation came to be through analysis of its functional effects in relation to selective pressures. If the effect was not selected against, it may have had a functional role in the evolutionary history of the trait. If so, function as defined by Dunnell (1978) is implicated whether it is an etiological or causal role function.

CONCLUSIONS

In 1978 Dunnell provided a useful start for understanding and identifying function in the archaeological record. Since adaptations have functions, Dunnell also led the way for a better comprehension of the meaning of adaptation. For too long, many archaeologists have assumed that the identification of any beneficial functional effects associated with a trait were evidence for an adaptation. Adaptation, though, is a special and onerous concept that should be used only where it is really necessary (Williams 1966). The concept of adaptation, if we assume that adaptations are the result of natural selection, should not be applied to all perceived beneficial effects without stringent analysis of the interplay between the effect and the evolutionary history of the trait providing the effect. Some functional effects are merely incidental and may not have influenced the evolutionary history of the trait's possessor.

The analysis of causal role function may explain the maintenance of a trait (whose etiological origins may be for identical reasons) or may identify why a previously neutral trait came under selection or why a trait was co-opted for new uses. However, such analysis will not necessarily implicate adaptation. Archaeologists need to distinguish carefully the type of functional statement that they are making. One type will indicate adaptation; the other will not. There is nothing wrong with either approach. We often wish to know why one trait rather than another exists and do not always wish to know how it came to exist. The only caveat is that we cannot easily call the functions of such traits adaptations. Function, unfortunately, will never be a delightful term for us.

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Chapter 4

Explaining the Co-occurrence of Traits in the Archaeological Record: A Further Consideration of Replicative Success

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INTRODUCTION

The realization that the archaeological record is typified by the patterned co-occurrence of artifacts or artifact attributes has formed the basis of archaeological research since its inception. These associations have been formally codified in the cultural-historic types that archaeologists rely on. They have formed the basis of the midwestern taxonomic method and the culture areas developed by the cultural historians (e.g., McKern 1939; Willey and Phillips 1958), the “tool kits” and activity assemblages studied by processual archaeologists (e.g., Binford and Binford 1966; Kent 1984), and the social and class structures interpreted by postprocessual and Marxist scholars (e.g., Bawden 1996). Some of the most important and fundamental debates in archaeology, such as the Ford–Spaulding debate (Ford 1954a, 1954b, 1954c; Spaulding 1953, 1954a, 1954b), the Binford–Bordes debate (Binford and Binford 1966; Bordes and de Sonneville-Bordes 1970; Dibble 1987; Rolland and Dibble 1990), and the Thomas–Flenniken–Bettinger debate (Bettinger et al. 1991; Flenniken 1985; Flenniken and Raymond 1986; Flenniken and Wilke 1989, 1991; Thomas 1981, 1983), have focused on the meaning and explanation of these associations.

In spite of the analytical importance of the association of traits and the debate that has surrounded it, most archaeologists generally treat these co-occurrences largely as a given, not as something to be explained in and of themselves. The reason for this neglect is probably that these associations can simply be taken for granted when approaching some questions. While the researchers may use the associations either to identify their subject matter (e.g., culture areas) or as a means of gaining information about social structures (e.g., social classes or activity areas), the reason that the traits are associated with one another in the

first place is of only incidental importance. For example, the reason that certain designs and sand temper co-occur on a particular prehistoric pottery type may not be important to those who are interested in questions related to temporal and cultural relationships, social structure, or the specific use of individual artifacts.

Such associations have profound evolutionary implications, however, and, therefore, present an interesting and fundamental subject matter for evolutionary archaeologists. In the same way that biologists are interested in explaining why particular genetic traits are correlated, evolutionary archaeologists might be interested in explaining why aboveground dwellings and agricultural production co-occur in a particular area, why a change in grinding technology correlates with a change in ceramic technology, or why shifts in settlement patterns and changes in ceramic decoration appear to be associated in a given region. Thus, instead of being simply a starting point for an evolutionary archaeology, these associations present important subjects of study.

The foundation for such studies has already been developed within the existing evolutionary archaeological framework. The basis for an evolutionary archaeology is the concept of replicative success as presented by Leonard and Jones (1987). Replicative success is defined as the differential persistence of traits, whether they are behavioral or material, through time (Leonard and Jones 1987:214). When we are discussing the co-occurrence of artifacts or attributes of artifacts, we are, in fact, discussing linked replicative success (i.e., the differential persistence of two or more traits that appear to be connected). Other evolutionary archaeologists have also proposed that the concept of sorting may be useful for understanding linked replicative success (Abbott et al. 1996; McGimsey 1995; O'Brien and Holland 1990; Ramenofsky 1995). However, these previous discussions have not explored the full range of sorting processes, nor have they clearly defined sorting.

In this chapter, we suggest that two kinds of processes can lead to linked replicative success in the archaeological record: hierarchical sorting and sorting by hitchhiking. We argue that both of these sorting processes may lead to linked replicative success but that sorting itself is an outcome and should not be identified as a causal mechanism in evolution. We discuss both of these processes in turn and provide archaeological examples illustrating their operation.

LINKED REPLICATIVE SUCCESS THROUGH HIERARCHICAL SORTING

Sorting is a concept that has been used by biologists when discussing evolutionary hierarchies. It has perhaps been most completely developed by Vrba and Eldredge (1984) and Vrba and Gould (1986). Vrba and her associates define sorting as “differential birth and/or death processes among individuals, whether they be genomic constituents, organisms, populations, or species” (Vrba and Eldredge 1984:146). It is “a simple description of differential representation; it

Table 4.1

Hierarchy of biological evolutionary individuals, in descending order, as proposed by Vrba and Eldredge (1984:149)

Evolutionary Individuals
Monophyletic Taxa
Species
Demes
Organisms
Genomic Constituents

contains, in itself, no statement about causes” (Vrba and Gould 1986:217). Therefore, sorting is not an evolutionary mechanism or force but is instead an outcome. While natural selection is one source of sorting, it is definitely not the only one (Vrba and Gould 1986); drift, hitchhiking, and hierarchical effects also can cause sorting.

While sorting is not an evolutionary mechanism as such, it has specific implications for evolution. The majority of Vrba and Eldredge’s (1984) discussion and the thrust of the present analysis focus on what we call *hierarchical sorting*. Hierarchical sorting is important to Vrba and her associates because of their view of punctuated equilibrium (Eldredge and Gould 1972) and their belief that evolutionary processes can occur at a variety of evolutionary scales, including the scales of genomes, organisms, populations of organisms, and species.¹ Additionally, they argue that these “evolutionary individuals” can be organized into a hierarchy, as illustrated in Table 4.1 in which the higher constituents comprise groups of the lower constituents (Vrba and Eldredge 1984:149).

Simplifying their discussion somewhat, Vrba and her associates argue that evolutionary changes at one scale can result in changes at other evolutionary scales. Thus, their position is that evolutionary processes operating on species can affect characteristics of individuals and genomes, that evolutionary processes operating on individual organisms can affect the evolutionary characteristics of genomes and species, and that evolutionary processes operating on genomes can affect the characteristics of individuals and species. However, an asymmetry between the effects of sorting at different levels is present. They suggest that evolutionary processes operating at higher levels will necessarily effect changes at lower levels, but evolutionary processes at lower levels may or may not effect changes at higher levels in the hierarchy (Vrba and Eldredge 1984:166; Vrba and Gould 1986:219). In other words, changes in species caused by species-level selection (i.e., punctuated equilibrium) will necessarily affect individual organisms and gene frequencies, but a mutation in a gene may have no effect on the higher levels (Vrba and Gould 1986:219).

The ultimate importance of the concept of hierarchical sorting is the implication that the proximate cause of an evolutionary change at one level may be

changes caused by evolutionary processes operating at different levels in the hierarchy (Vrba and Eldredge 1984; Vrba and Gould 1986). Thus, the cause of phenotypic changes in individual organisms may be the effects of species-level or genic-level evolution. It is beyond the scope of this chapter for us to evaluate the implications of the concept of hierarchical sorting for the model of punctuated equilibrium. Our goal instead is to show that the concept of hierarchical sorting is a useful one for many archaeological questions of an evolutionary nature. We suggest that hierarchical sorting of behavioral and material phenotypic traits is possible and perhaps frequent in archaeological contexts. The effects of evolutionary processes acting on phenotypic variation at a particular scale can result in patterned sorting of material and behavioral traits at different scales. Because our subject matter has shifted from the biological to the archaeological record, though, the hierarchy proposed by Vrba and Eldredge (1984) and illustrated in Table 4.1 is not applicable. Instead of being concerned with the long-term development of species and the accompanying physiological and genetic changes, we are interested in explaining the changes in behavioral patterns within a specific species, *Homo sapiens*.

We suggest that the cultural phenomena that are of interest to evolutionary archaeologists can be organized into hierarchies in a manner similar to the biological hierarchy discussed previously. Like the hierarchy proposed by Vrba and her colleagues, we believe that the hierarchy of cultural phenomena can also be divided into relatively discrete levels differing in their inclusiveness. We suggest that these levels are formed by *nested hierarchies*, as we explain in the following section.

Nested Hierarchies

Evolutionary archaeologists have identified two classes of material and behavioral traits that can be distinguished based on the evolutionary processes that affect them: functional traits and stylistic traits. Functional traits are defined as traits that affect the reproductive success of individuals (Dunnell 1978, 1980) or the replicative success of cultural traits (Leonard and Jones 1987) in a given selective environment. They can be present at a variety of levels. For example, the use of a particular artifact or tool, such as the bow and arrow, or a subsistence strategy, such as maize horticulture, may be functional. However, attributes of a class of artifacts (e.g., temper type or porosity of ceramic vessels) may also be functional.

In contrast, stylistic traits are defined as those attributes that do not affect the fitness of individuals or the replicative success of cultural traits. These traits are not *directly* impacted by the action of natural selection and are, by definition, free to vary irrespective of the operation of selection. The replicative success of stylistic attributes are instead explained by evolutionary mechanisms such as drift and innovation and the characteristics of cultural transmission systems (Neiman 1995).

By definition, all functional and stylistic traits have some degree of replicative success (or lack thereof). The replicative success of some traits may be contingent on their connection with other traits, however. For example, all functional traits have fitness consequences, by definition. The effects of different functional traits are not necessarily equivalent, though. Functional traits can also be present at different scales, and the functionality of a particular trait may be manifested only in the presence of traits at higher scales. To illustrate this point, we consider the factors that affect the weight of projectile points. The weight of arrowheads is an important performance characteristic of bow-and-arrow technology, but the weight of atlatl-dart points is not as important a performance characteristic of atlatl-dart weapon systems (Christenson 1986). This difference in the importance of projectile point weight is caused by the different mechanical and aerodynamic characteristics of the two weapon systems.

Specifically, arrow shafts are light and must flex correctly for the arrow to fly true. The weight of arrowheads has a great impact on an arrow's flex and balance. An arrowhead that is either too heavy or too light will not produce the correct flex in the arrow shaft, will cause the arrow to be unbalanced and will, therefore, cause the arrow to fly erratically (Beck 1998; Christenson 1986; Fennenga 1953). In contrast, the mass of the shafts of atlatl darts is much greater than arrow shafts, and dart points generally do not greatly affect the balance of atlatl darts, except in the case of extremely large and massive points. We expect, therefore, that the weight of arrowheads will be both smaller and less variable than that of atlatl dart points because of the different performance requirements of the two weapon systems (an expectation that has been supported in several analyses of projectile points [e.g., Christenson 1986; Shott 1997]).

We suggest that the weight of projectile points is a functional characteristic because it affects the efficiency of both weapon systems. However, the effect of point weight is different in terms of both its importance and its specific performance requirements for the two weapon systems. The selective forces that affect the weight of the projectile points are contingent on the weapon system that is being used. If natural selection were to begin to favor the use of bows and arrows in a group that had previously used atlatl darts, the weight of their projectile points would change. Thus, the effects of evolutionary processes at one level, the weapon system, would provide the proximate cause for potential changes at another level, the weight of projectile points.

Relationships such as these between the characteristics of projectile points and the general weapon system create a hierarchy in which some behavioral or material attributes are contingent on, or nested within, other more inclusive attributes. These relationships are similar to those presented by Vrba and Eldredge (1984) and Vrba and Gould (1986), in that the different levels in the hierarchy are distinguished by their inclusiveness; higher levels include and integrate those attributes in the lower levels.

We believe that the "scales" created by the asymmetrical relationships of various attributes are not necessarily absolutes, though, because each series of

relationships is unique, based on the behavioral and material traits being examined. For example, the nested hierarchies associated with atlatl darts are different from those associated with bows and arrows, not to mention those associated with ceramics or architectural features. However, we suggest that a general hierarchy will be applicable in most cases (Table 4.2).²

Attributes of artifacts will always be lower in scale than the artifact itself. Thus, while attributes of artifacts such as the wall thickness of ceramic cooking vessels may be subject to evolutionary processes, evolutionary changes at the scale of the entire artifact, such as changes in use and the morphology of ceramic cooking vessels, will necessarily affect the individual attributes of the artifact in some way, even if the result is a reinforcement of the existing attributes. Of course, evolutionary changes at lower scales may cause sorting at a higher level, but, as Vrba and Eldredge (1984:166) observe, this is not necessarily so. For example, evolutionary changes in arrow design will necessarily affect projectile points, but changes in projectile points may or may not produce evolutionary changes in arrow design or other components of the weapon system. Thus, the same asymmetry identified by Vrba and Gould (1986:219) is present here: evolutionary changes at a higher level will necessarily impact lower levels, but changes at lower levels may or may not impact higher levels.

Likewise, individual artifacts are at a lower scale, in general, than groups of functionally integrated artifacts. Using the bow-and-arrow example presented earlier, the entire weapon system is at a higher level than that of arrowheads, because the general weapon system includes the arrowheads, but arrowheads are only one component of the integrated weapon system. Finally, functionally integrated artifacts are lower in scale than functionally integrated groups of artifacts. Thus, behavioral and material patterns such as subsistence strategies, which cause the integration of numerous groups of functionally integrated artifacts such as groundstone assemblages, tools used in planting and harvesting crops, and artifacts used for cooking and consuming foods, are at a higher scale than any of the individual artifact groups.

PREVIOUS ARCHAEOLOGICAL DISCUSSIONS OF SORTING

While sorting has never received a systematic treatment in the archaeological literature, preliminary discussions on the subject have been presented by Abbott et al. (1996) McGimsey (1995), O'Brien and Holland (1990), and Ramenofsky (1995). The present work is largely a continuation of the Abbott et al. (1996) discussion, in the sense that it builds upon the foundation outlined in that work. Regarding McGimsey's dissertation work relating to the concept of sorting, we differ simply in our definition of the term "sorting." We define sorting as the differential persistence of traits, due to linked relationships between traits where at least one trait is affected by either selection or drift. McGimsey discusses sorting as one of three possible causal mechanisms of evolution, along with

Table 4.2

Proposed hierarchy of behavioral and material phenotypic traits as represented in the archaeological record, in descending order of inclusiveness

Phenotypic Constituents
Groups of Functionally Integrated Artifacts
Functionally Integrated Artifacts
Artifacts
Attributes of Artifacts

selection and drift. His goal is to determine whether lamellar flakes of the Illinois Middle Woodland are a functional adaptation, the result of drift, or simply hierarchically sorted traits. We disagree with McGimsey's discussion only in that we would identify sorting as merely an outcome, not an evolutionary force or mechanism. With hierarchical sorting, the causal mechanism is *selection* (or drift) acting at a different level in the hierarchy, not the sorting at other levels that results from this selection. Ramenofsky (1995) has also defined sorting as a causal mechanism, but she has since reversed her position and argued that hierarchical sorting is not a separate mechanism from selection (Ramenofsky 1998).

The Shift from Formal to Expedient Tools in the American Southwest

An example of the sorting of two functional traits is presented by Abbott et al. (1996). While this example deals with functional traits subject to the evolutionary process of natural selection, the basic relationship is the same for stylistic traits affected by evolutionary processes such as drift. Abbott and her colleagues argue that the transition from a biface to a flake-tool technology, which co-occurs with maize agriculture in the North American Southwest, may not be selection for flake-tool technology *per se*. Instead, this technology may be favored because it is linked to the agricultural subsistence strategy, which is the primary trait under selection (Abbott et al. 1996). Previous researchers have argued that a shift from biface to flake production is caused by decreased residential mobility (Parry and Kelly 1987). On the basis of ethnographic analogy, Parry and Kelly reasoned that mobile hunter-gatherers use a formal, biface-based tool technology because of the high costs of transporting stone tool materials. They need an efficient, flexible tool kit from which they can fashion numerous different implements as they cannot afford to carry large amounts of raw materials. In contrast, sedentary farmers do not have to be concerned with transport costs. They have less incentive to maintain a formal technology because they can easily cache large amounts of raw materials from which they can create simple, unmodified flake tools as needed (Parry and Kelly 1987:299).

Abbott and her colleagues agree that the shift to flake-tool technology is indeed related to the change to a sedentary lifestyle, but they argue that sedentism is a proximate-cause, rather than ultimate-cause, explanation of this shift (*sensu* Mayr 1982). They propose that flake technology is a by-product of reduced mobility and that reduced mobility is a product of selection favoring a shift toward intensified maize production (Abbott et al. 1996:39). Therefore, flake technology should be interpreted as a sorted trait that is linked to the adaptation of an intensive agricultural subsistence strategy. They caution, however, that this shift in tool technology probably does not represent an example of “pure sorting” (Abbott et al. 1996:39), where the sorted trait is completely neutral. They note that flake technology likely conferred a reproductive advantage to its users (versus those who have no flake technology) and is indeed an adaptation; however, they argue that there is a significant “adaptive differential” (Abbott et al. 1996:39) between the flake and agricultural technologies, with the shift in subsistence ultimately causing the major fitness consequences favoring these linked technologies.

Fitness Coefficients

The idea of adaptive differential introduced by Abbott et al. could be more formally expressed in terms of fitness coefficients. We use the term “fitness coefficients” to indicate a relative measure indicating the differing effects of particular traits on fitness in a given selective environment.³ Not all functional traits have an equal impact on fitness in a given selective environment. For example, a car with fouled spark plugs may not run very efficiently, but a car without wheels will not go anywhere at all. Similarly, the shape of a projectile point may have a great effect on the efficiency of the weapon system used for killing game. Raw material used in fashioning the point may also have fitness consequences. However, the use of chalcedony rather than chert may have a much smaller impact on killing efficiency than point shape. If one were to haft a large chopper made of chert onto an arrow shaft and an arrow point made of chalcedony onto another arrow shaft, differences in the performance of the two arrows as long-distance hunting weapons would be due more to tool shape rather than material type.

The significance of the concept of fitness coefficients for our discussion of sorting is that if indeed agricultural technology and stone-tool technology were both important functional attributes under selection, then the shift to a flake-based technology would not truly be an example of sorting. However, if the two attributes had greatly differing effects on fitness, and changes in tool technology were simply an inevitable result of changes in the subsistence system due to significant fitness consequences, then we would classify the shift to flake tools as hierarchical sorting. More specifically, this may be an example of what Vrba and Eldredge refer to as “downward causation,” or sorting of variation at lower levels due to selection occurring at higher levels (Vrba and Eldredge 1986:152).

In this case changes in the subsistence system as a whole (a shift to intensive agriculture) effected a change in a particular class of artifacts (stone tools) that were a subset of the subsistence system.

LINKED REPLICATIVE SUCCESS THROUGH HITCHHIKING

The second cause of linked replicative success in the archaeological record is the process of hitchhiking (sometimes called piggybacking). With hitchhiking, two or more traits are associated such that evolutionary mechanisms such as natural selection and drift operating on one of the traits also affect the other trait(s). Just as with the concept of hierarchical sorting, hitchhiking is a means of describing a relationship between traits, not an evolutionary mechanism as such. In the process of sorting due to hitchhiking on traits under selection or drift, the true causal mechanism is either drift or selection, not sorting.

Hierarchical sorting could be considered a special case of hitchhiking. However, we distinguish between hitchhiking and hierarchical sorting in order to bring important conceptual details of each into focus. While hierarchical sorting describes a specific relationship between *scales* of evolution, hitchhiking describes a specific relationship between *traits*, regardless of their scales. The importance of this distinction is illustrated further in the following discussion.

Sober (1984:97–102) has presented an illustrative example that is useful for understanding hitchhiking traits. He begins by suggesting that a number of balls of various colors and sizes are placed within a cylinder with several horizontal layers, each with holes becoming progressively smaller. Balls that are too large to fit through the holes leading to the next level are trapped, while the smaller balls continue down through the cylinder. After the balls have been separated according to their size, it becomes obvious that only green-colored balls are small enough to reach the bottom of the cylinder.

We can conceptualize the cylinder as the selective environment and each horizontal level as time. Through time, the amount of variation is reduced as a product of natural selection; only a portion of the balls possessed the characteristic necessary to continue to the next generation of selection (i.e., are the appropriate size to fit through the holes). However, the color green is a hitchhiking trait associated with the small balls. Within the hypothetical selective environment, then, small balls are being *selected for*, but there is a *selection of* green balls.

Sober's example can easily be transformed into an archaeological framework. For example, in a hypothetical environment, three flaked-stone raw materials are available: a black obsidian, a fine-grained gray chert, and a brown, coarse-grained basalt. All of the raw materials are equally plentiful and equally accessible but provide different performance characteristics. Within the selective environment, the use of fine-grained chert for projectile points is *selected for* because of certain performance characteristics it provides (e.g., sharp, durable

edges). The raw material used to manufacture the projectile points (i.e., the chert) is functional, but the color of the raw material is neutral. However, because the chert is gray, projectile point color will be controlled by the selection for the raw materials used to manufacture projectile points. The color of projectile points will, therefore, be a hitchhiking trait created by the selection for chert as the raw material type.

The preceding example illustrates the importance of the process of hitchhiking within evolutionary archaeology. An archaeologist analyzing the projectile point assemblage would quickly observe that the projectile points are made of the gray chert. However, explaining why the gray chert was used to make the projectile points would require a detailed analysis of the selective environment operating on the individuals producing the points, the range of raw materials available to make the projectile points, and an understanding of the performance characteristics of each raw material (Jones et al. 1995). The challenge facing the evolutionary archaeologist is no different from the challenge facing the evolutionary biologist; they must both identify how the phenotypic characteristic contributes to the fitness of the evolutionary individual (Sober 1993). In our hypothetical example, the sharp, durable edges created by the concoidal fracture of the chert are under selection, not the raw material color. However, the one-to-one correlation between raw material type color and the projectile points may lead to the appearance that the color of the points is under selection. An explanation based on this incorrect premise would necessarily be wrong; therefore, it is of the utmost importance that the evolutionary archaeologist correctly distinguishes between functional attributes and sorted stylistic attributes.

We suggest that two kinds of hitchhiking may occur in archaeological contexts: (1) hitchhiking by historic contingency and (2) hitchhiking by mechanical constraints. Hitchhiking by historical contingency occurs when two or more attributes, regardless of whether they are stylistic or functional, are associated such that when one of the traits is manifested, the other trait(s) is (are) as well, simply due to a chance historical association between the traits. In contrast, hitchhiking due to mechanical constraints occurs when two or more traits are mechanically connected in some way such that evolutionary changes in one trait necessarily will impact some aspect of the manifestation of the other trait(s). We discuss each of these causes of hitchhiking next and present examples of their operation.

Hitchhiking by Historical Contingency

Because evolutionary processes do not direct innovation, they must operate on the variation that is present within the historical context (Dunnell 1980; Leonard and Jones 1987). If two or more traits are connected such that the manifestation of one *by chance* includes the manifestation of the other even if the connection is not mechanically necessary, and an evolutionary process favors one of the traits within the association, the remaining trait(s) will also be fa-

vored. Correlation between genetic characteristics is attributed to pleiotropy and to linkage (Falconer and Mackay 1996:312). The first factor, pleiotropy, is discussed in the section on sorting due to mechanical constraints. The second factor, linkage, is important for conceptualizing sorting due to historical contingency.

Linkage of traits in biological populations is attributed to chance, or random genetic drift. (Falconer and Mackay 1996:16; Hartl and Clark 1989:131, 489). Drift, or sampling error, in small populations may lead to linkage as some genetic characters drift together to high frequency simply due to chance (Hartl and Clark 1989:54, 131, 489). Neiman (1995) has argued that drift may be a significant factor in cultural evolution because it is a product of *effective* population size, or the number of people who actually interact socially with each other on a regular basis. Thus, even though nominal population sizes may be large, drift can still play an important role (Neiman 1995:10).

Two kinds of sorting due to chance historical contingency may occur. First, if a neutral trait is linked to a functional trait by chance, then selection for the functional trait will explain the replicative success of both traits. Second, if two neutral traits are linked by chance, they will appear to drift together. In both cases, the concept of sorting is used to explain only the shared fate of the linked traits, while the evolutionary mechanisms responsible for the differential survival of the traits are selection or drift.

We presented an example of this first kind of sorting in our discussion of the hitchhiking of flaked-stone tool color with tool performance characteristics such as sharpness and durability of edges. Sorting by historical contingency may explain why a trait that is neutral in a particular selective environment, such as raw material color, may appear to be under selection due to linkage to functional traits, such as sharpness or durability. Similarly, traits such as ceramic design style may appear to be under selection when they are simply linked to the introduction of a new temper type or firing technology.

Two or more neutral traits, such as painted pottery design elements, may also be linked by chance. In this case the traits will appear to sort together due to linked drift rather than to selection acting on one of the traits. Such patterning has not been examined as yet, but we can suggest at least one useful application of the concept of linked drift, deriving from the expectations of linkage disequilibrium models used in population genetics.

Linkage disequilibrium is a term used to describe a condition wherein there is a significant, nonrandom association between alleles in the formation of gametes. This means that some alleles at different loci on chromosomes tend to co-occur with each other. In populations where mating is random, linkage disequilibrium should decrease over time as a result of recombination of alleles during the formation of gametes (Falconer and Mackay 1996:17; Hartl and Clark 1989:47–48). Understanding why this should be so is simple: a population without mating barriers results in a larger interacting gene pool, which contributes more variation and more possible recombinations in the formation of gametes. This aspect of linkage disequilibrium makes it a useful concept for archaeolog-

ical application. Just as a larger gene pool contributes more variation, a broader social pool provides more variation and more possible recombinations of information. Therefore, as groups interact, linkage among neutral traits should decrease as ideas are exchanged, and new stylistic elements are learned and mixed into the pool of information. Understanding the process of sorting due to historical contingency therefore may yield new models for interpreting prehistoric regional social interaction.

Hitchhiking by Mechanical Constraints

Unlike sorting by historical contingency, hitchhiking by mechanical constraints occurs when the manifestation of an attribute(s) that is being affected by evolutionary processes directly affects the range of possible variation in other attribute(s). In biology, such a condition is described by the term *pleiotropy*. Pleiotropy is a situation in which a single gene, or cluster of genes, may have multiple phenotypic effects. With pleiotropy, selection for one trait may mechanically affect other traits that are not under selection. Sober provides the example of the correlated relationship between the human jaw and chin. He argues that there “never was selection for having a chin, rather selection for certain other features of jaw structure yielded a chin as an inevitable architectural consequence” (Sober 1984:24). As a result chins may appear to be selected for, when they are merely part of a package of traits linked by “architectural constraints” (Sober 1984:24), and other traits in the package are the target of selection. We suggest that cultural traits may also exhibit a condition analogous to pleiotropy that we call sorting by mechanical constraints. For example, basal width places a constraint on the size of the hafting notches in side-notched projectile points. The notches cannot be deeper than the point is wide, or the point will simply be broken into two parts. If basal width is functional within a hypothetical selective environment, but notch depth is not, notch depth may still be impacted by the action of natural selection affecting basal width. Thus, stylistic traits can be sorted through limitations created by the mechanical constraints of artifacts. (See also VanPool’s discussion of metate traits in Chapter 7 in this volume for an additional example of this type of sorting.)

CONCLUSION

We have argued that sorting is an important concept for archaeological explanation. Of primary importance are the consequences for the assignment of cause in evolutionary explanations. Dunnell (1978) argued against the adaptationist bias of processual archaeology and proposed that a large part of the archaeological record may be composed of nonfunctional variation that cannot properly be explained as adaptations. Explanation of such variation relies on mechanisms such as random cultural drift, rather than selection. Williams (1966) also provides support for this position, based on the principle of parsimony:

“Parsimony demands that an effect be called functional only when *chance* can be ruled out as a possible explanation” (Williams 1966:261, emphasis added; see also Selander 1985:87–88).

If stochastic change in neutral variation can explain the patterning in a succession of design styles, drift is a more parsimonious explanation than is a causal argument that attempts to explain the change as a functional adaptation. We suggest that parsimony also demands a consideration of various kinds of sorting to explain differential replicative success, rather than the assumption that selection or drift is necessarily acting directly on a particular trait at a particular level.

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NOTES

1. The appropriateness of the concept of species-level evolution as stipulated by punctuated equilibrium (Eldredge and Gould 1972) or even evolution at the scale of organisms and genomes is still an open debate in biology (see Dawkins 1982, 1989; Vrba and Eldredge 1984; Wilson 1994). Our discussion of Vrba and her associates is not intended to suggest that we agree that species-level evolution is common or even possible. Instead, we are endeavoring to provide a complete discussion of the concept of sorting as it is used in biology before we discuss its applicability to archaeological research.

2. Terms such as those presented in Table 4.2 are similar to the terms used by Vrba and Eldredge (1984:149) in that they are only general terms that can refer to different types of phenotypic structures based on the situation. One need only examine the ongoing debates in biology surrounding the appropriate definition of terms such as species (Hull 1994; Mishler and Donoghue 1994) or, for that matter, genomic constituents (Dawkins 1982, 1989) to realize that these terms are largely defined in case-specific contexts and, therefore, do not differ greatly from terms such as artifacts or attributes of artifacts.

3. Our use of the term “fitness coefficients” should not be confused with the “selection coefficients” of biologists. A selection coefficient is used to express the relative strength of selection pressure on a particular trait in different selective environments, whereas we use fitness coefficients to express the differing impact on fitness of different functional traits in the same selective environment.

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Chapter 5

Culture Historical and Biological Approaches to Identifying Homologous Traits

R. Lee Lyman

INTRODUCTION

What is archaeological style? Although I think we could all agree that it is not the sort of clothing particular archaeologists wear, recent discussions suggest that there is little else about “style” that modern archaeologists could agree on (e.g., Braun 1995; Carr and Neitzel 1995; Conkey and Hastorf 1990; Dunnell 1995; Hegmon 1992; Hill 1985). Interestingly, there was more agreement 70 to 80 years ago concerning what archaeological style comprised than is evident today. Between about 1910 and 1930, common sense provided the understanding of how and why artifact styles displayed the spatiotemporal distributions they did; it was generally, if implicitly, agreed that styles were types of artifacts that denoted historical—that is, evolutionary or phylogenetic—relations. In short, artifacts that were similar stylistically denoted heritable continuity.

When in 1978 Robert Dunnell published his paper “Style and Function: A Fundamental Dichotomy,” the definition of style he proposed was “quite close to its usage in archaeology, particularly as employed by culture historians” (Dunnell 1978:199). My take on the early-twentieth-century literature (Lyman and O’Brien 1997; Lyman et al. 1997, 1998; O’Brien and Lyman 1998) suggests Dunnell was correct in this assessment. A good example of such is found in the concept of horizon styles (e.g., Kroeber 1944; Willey 1945), which served as index fossils and allowed temporal correlation of spatially separate archaeological manifestations. Used as dating tools, horizon styles were believed to denote rapid diffusion, or transmission, of an attribute style or an artifact style—or at least the idea thereof—over a large area. In short, typologically similar artifacts considered to constitute horizon styles denoted heritable continuity—regardless of the means of transmission but generally denoted with the term “diffusion” if

geographic space was involved or the terms “persistence” or “tradition” if time was involved (e.g., Rouse 1939)—and thus temporal propinquity or what was variously referred to as “cultural relatedness” or “historical continuity.”

Using wording more explicit than that found in the early literature and couching his discussion in evolutionary terms, Dunnell (1978:199) noted that “stylistic similarity is homologous similarity; it is the result of direct cultural transmission once chance similarity in a context of limited possibilities is excluded.” By the last Dunnell meant that the (metaphorical) behavior of style in the archaeological record—the spatiotemporal distributions of various styles—was independent of natural selection and thus was the result purely of transmission. Thus, styles could be used to build chronologies, to measure spatial interaction of populations, and most importantly, to determine and document the phylogenetic histories of artifact lineages. Dunnell’s discussion appears to have had minimal impact on Americanist archaeology given the subsequent literature on style cited earlier, probably because anything remotely related to the culture-history paradigm had been vilified a decade earlier by processual archaeologists (e.g., Binford 1968; Flannery 1967).

Dunnell’s (1978) treatment of the notion “style” was succinct; he did not discuss the historical context within which the culture-historical notion of style developed, how culture historians identified styles, or how biologists were then, or are now, identifying homologous structures or homologs. These are the topics I explore here. In doing so, I take a decidedly narrow view that emanates from what I believe to be the paramount goals of archaeological research—constructing and explaining the evolutionary lineages of cultures as they are represented by artifacts (Lyman and O’Brien 1998; Lyman et al. 1997; O’Brien et al. 1998). This is basically the same goal paleobiologists have had at least since George Gaylord Simpson (1944) published *Tempo and Mode in Evolution*. The founders of the Americanist culture history paradigm—A. V. Kidder, Nels C. Nelson, A. L. Kroeber, Leslie Spier, and others—had this as their goal from the moment they first realized that they could measure time and culture change by close study of variation in artifacts. I conclude with a brief discussion of the method developed by early culture historians and presently available to modern archaeologists who wish to determine the evolutionary lineages of artifacts.

HISTORICAL CONTEXT

In 1953 Gordon Willey (p. 363) stated as an unequivocal methodological axiom of culture history that “typological similarity is an indication of cultural relatedness.” This axiom originated with the use of the comparative method in linguistic studies of the late eighteenth and early nineteenth centuries (Leaf 1979:86–90). As Boas (1904:518) observed, “Owing to the rapid change of language, the historical treatment of the linguistic problem had developed long before the historic aspect of the natural sciences was understood. The genetic relationship of languages was clearly recognized when the genetic relationship

of species was hardly thought of. . . . No other manifestation of the mental life of man can be classified so minutely and definitely as language. In none are the genetic relations more clearly established." Boas was, of course, speaking metaphorically about the "genetic relations of languages," but his point was solid; linguistic similarity was the result of transmission and hereditary continuity.

Such an explanation for similarities among organisms was not used by biologists until the publication of Darwin's *On the Origin of Species* in 1859, when it became axiomatic in that discipline. The published definition of homologous structures often cited today preceded the *Origin* by 16 years. In 1843 Sir Richard Owen defined a homolog as "the same organ in different animals under every variety of form and function" (Panchen 1994:40). Detailed analysis of the early nineteenth-century literature indicates that Owen's conception of homolog originated much earlier and that rather than denote historical or phylogenetic affinity, it was taken to mean representative of a common or archetypal plan (Brady 1985; Padian 1997; Panchen 1994; Rieppel 1994). By archetypal plan, or archetype for short, Owen meant basically an ideal, not necessarily (although generally) primitive, form (see Desmond [1984] for historical details). Owen later distinguished between "homotypy," the "same" organ in various organisms, and "general homology," resemblance of an organism to an archetype (Donoghue 1992:170), thereby explicitly including variation in scale in his concept.

The important thing to note is that the early nineteenth-century biological conception of homolog "did not imply anything more than purely formal relation" (Szarski 1949:124). In short, resemblance—of some unspecified degree—in formal properties such as shape and size denoted homologous structures at whatever scale. Darwin's insight provided a new and logical causal explanation as to why there would be formal similarities between organs and organisms and why the Linnaean taxonomy was hierarchical. Darwin (1859:206) argued that "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." In two short sentences, Darwin clearly distinguished between analogous and homologous characters and provided the first explicitly scientific and theoretical explanation of what had been called homologs among organisms: homologous similarity is historical for the simple reason that it results from hereditary continuity.

My point is that the notion that formal similarity denotes phylogenetic or evolutionary relatedness—not just in the more modern and narrow biological sense of genetic relatedness—has a deep history in the social sciences, deeper than Darwinism and deeper than the genetical theory of heredity. Thus, a form of explanation for formal similarity was available to archaeologists from the get-go. Old World prehistorians such as Sir John Evans (1850) used the notion in his phyletic seriations of British gold coins a decade before Darwin published the *Origin* (Lyman et al. 1997, 1998; O'Brien and Lyman 1999). Flinders Petrie (e.g., 1899) used the notion to erect his set of "sequence dates" for the Egyptian

pottery he phyletically seriated. In the New World, the direct historical approach (Steward 1942) was implicitly founded in the notion. The direct historical approach was used by Cyrus Thomas (1894) to help identify the mound builders. Clark Wissler's (1916) discussion of the "genetic relations" of decorative motifs on the moccasins of North American Indians and A. V. Kidder's (1917) phyletic seriations of southwestern pottery were also based on the notion that formal similarity denoted heritable continuity and thus phylogenetic relations.

Rooted as they were in the comparative method, the notions underpinning Evans', Wissler's, and Kidder's studies and those of other anthropologists and archaeologists were hardly explicit and certainly not well developed. During the nineteenth century and early in the twentieth century, no social scientist bothered to consider in detail the question of why formal similarity *should* denote phylogenetic relations. Boas' (1904) remarks quoted earlier with respect to languages were a rare exception. Boas (1904:522) commented on a "theory of transmission" but was contrasting it with another popular theory of the day—the psychological unity of mankind—as two opposing explanations for cultural similarities. He did not explore the differences and similarities of cultural transmission and genetic transmission explicitly. A. L. Kroeber, Boas' student, highlighted the differences between these two forms of transmission when he equated the term "heredity" solely with genetic transmission—because it was from parent to offspring only—and what we today think of as cultural transmission with "the non-biological principle of tradition"—because the latter could be from parent to offspring, from offspring to parent, or between siblings (Kroeber 1923: 3, 7; see also Kroeber 1917). On the basis of this distinction, Kroeber (1923:8) remarked that "a pure Darwinian anthropology would be largely misapplied biology." Such remarks contributed to the eventual discard of any model of cultural evolution constructed with the biological model of evolution as a blueprint (Lyman and O'Brien 1997).

In the middle of the twentieth century, in the same paper in which he stated the axiom quoted earlier, Willey (1953:368) remarked that "theories of culture change and continuity are fundamental to [Americanist] archeological studies" and that "the treatment of archeological assemblages in any historicogenetic system has a basis in theories of continuity and change." He did not, however, elaborate on what those "theories" were or that the notion of continuity had to rest on heritability and transmission. Albert Spaulding provided a brief but revealing insight to those "theories" in the middle of the twentieth century:

If we view the ultimate task of archeology as the development of the ability to explain the similarity or lack of similarity of any two [archaeological manifestations], the significance of [classifying those manifestations according to their positions in time and space and their resemblance] is easy to state. All [three—time, space, form—] can be related to the proposition that culture change is systematic rather than capricious and to the auxiliary proposition that an important basis for the systematic behavior of culture is *its continuous transmission through the agency of person to person contact*. The

observed degree of similarity between the two [archaeological manifestations] sets the problem to be solved, and time and geographical position are two discrete systems, each of which offers an independent measure of closeness. (Spaulding 1954:14, emphasis added)

Spaulding was merely making explicit what had been implicit in the reasoning of early twentieth-century Americanist archaeologists. Rouse (1939:14), for example, had spoken of the historical processes of diffusion, persistence, origination, extinction, and replacement; the first two implied heritable continuity, but Rouse did not speak of them in these terms. Krieger (1944:272) had spoken of archaeological types as “identifying distinct patterns of behavior or technology which can be acquired by one human being from another, and thus serve as tools for the retracing of cultural developments and interactions.” Spaulding was a bit more explicit than his predecessors about what “similarity” meant. What he referred to as “resemblance” meant “actual physical similarity of tools, utensils, remains of houses, and so on. It means in addition quantitative similarity, a concept which includes not only the form and other physical properties of the artifacts but also the relative popularity of artifact types in the [archaeological materials] being compared” (Spaulding 1954:12). The mid-twentieth-century debate over whether archaeological types were constructs of the archaeologist and thus definitional or were real and empirical and thus discoverable had serious implications for assessing similarity (see O’Brien and Lyman [1998] for discussion of the debate). The former was, and is, as we will see, paramount to assessing whether similar things are analogs or homologs.

Americanist culture historians early in the twentieth century followed their predecessors in the social sciences and seldom referenced Darwin’s theory, though they believed, at least implicitly, that if formal similarity was taken to signify a historical relation between compared items, then heredity or transmission of some sort must be involved. Their belief was the result of using ethnologically documented mechanisms such as diffusion and enculturation to account for typological similarities in the archaeological record. But for want of an explicit and well-developed theory, they tended to put the cart before the horse. Their inferences, well captured by Willey’s (1953) axiom, were often of the form “culture traits are historically—that is, phylogenetically—related because they are similar.” But as Simpson (1961:68–69) noted, biological twins are not related because they are similar; rather, they are similar because they are related. In other words, Willey’s axiom implicitly rested on a notion of hereditary continuity; such was the explanation for typological similarity. The axiom created a tautology that was not testable because the cause of the similarity was lodged in the phenomena requiring explanation. In science, cause is lodged in theory rather than the phenomena in need of explanation. Failure of the culture historians to develop a *theory* of cultural transmission while accepting an often implicit, commonsense understanding of it was the root of the problem.

The failure to build a theory of cultural transmission and heritability was curious, in my view, for Kroeber had, in 1919 (p. 239), suggested that what would become known as styles did not “vary [from each other] in purpose,” and a decade earlier he had suggested that changes in styles constituted merely “passing change of fashion” (Kroeber 1909:5). What Kroeber had done, then, without using modern terminology and without sufficient explication, was to define styles as selectively neutral. The significance of his implicit definition is that it implies that styles diffuse over space and through time merely as a result of transmission rather than for functional or adaptational reasons. Kroeber failed to develop this notion further, however, and did not refer to it in his seminal frequency seriations of Zuni potsherds (Kroeber 1916) or in his later pleas that anthropologists should follow evolutionary biology and pay more attention to the distinction between homologous and analogous structures. The procedure for doing the latter, Kroeber (1931, 1943) argued, was the same for both disciplines. Anthropologists had been grappling with how to distinguish between the two sorts of similarity for decades, and that is probably why they failed to heed Kroeber’s suggestions—he wasn’t saying anything new.

HOW TO RECOGNIZE HOMOLOGS IN ANTHROPOLOGY

Recall the early twentieth-century debates between the so-called diffusionists, including Fritz Graebner (e.g., 1911), W. H. R. Rivers, and G. Elliot Smith (e.g., 1928), and those categorized as historical particularists, including Franz Boas (e.g., 1911), Alexander Goldenweiser (e.g., 1916, 1925), and Robert Lowie (e.g., 1912), the latter group of whom thought independent invention played a major role in cultural change. Within the context of these debates, criteria were developed for distinguishing between cultural phenomena that were formally similar as a result of common ancestry and those that were similar as a result of convergence. Although spelled out in general terms as early as 1916 by Edward Sapir, the criteria were summarized in some detail by Julian Steward in 1929. In short, when a cultural trait was found in two or more localities, the criteria were (1) the “uniqueness” or quality of the cultural trait; (2) the presence of a probable ancestral trait in the same geographic area; (3) the quantity of other shared traits; and (4) the geographic proximity of the localities. Steward (1929) argued that only by close study of each of the particular empirical manifestations representing all of these criteria could one determine whether a cultural trait in an area originated there or elsewhere. The uniqueness criterion was often expressed as a trait’s complexity; the quantity of shared traits and the geographic proximity of compared localities were thought to correspond directly to the probability of contact (and thus transmission and heritable continuity). These three criteria inform the inference of homology. The presence of a probable-ancestor criterion constituted, at least indirectly, the inference desired. Not surprisingly then, rather than indicate how ancestral traits were to be identified, Steward and his contemporaries fell back on the other three criteria to

help determine if an ancestral trait was present. This probably contributed to Kroeber's (1931) lament two years later that anthropologists had failed to borrow biological procedures for distinguishing between homologous and analogous structures. In Kroeber's view, Steward had not told us how to do it.

Kroeber (1931:151) stated that biologists identified homologs as those traits with "specific and structural and not merely superficial" similarity. For a biologist of the early twentieth century, the wings of eagles and those of crows are structurally as well as superficially similar and thus constitute homologs. The wings of eagles and those of bats are superficially, but not structurally, similar and thus constitute analogs. Kroeber was correct, but he failed to point out that, given particularly the pre-Darwinian history of the term, biologists sometimes used rather different definitions of homology (Rieppel 1994). Even biologists writing at about the same time as Kroeber argued about what a homolog was and how to define the concept (e.g., Boyden 1943, 1947; Haas and Simpson 1946; Hubbs 1944; Moment 1945; Szarski 1949); the various meanings of the concept are still discussed at some length within that discipline (e.g., chapters in Hall 1994). Kroeber's (1931) silence on the matter implied that the concept of homology had a single, agreed-upon meaning throughout biology. It did not.

In his second explicit attempt to point out the utility of the concepts of homology and analogy to anthropological research, Kroeber (1943:107) wrote about what he termed "the basic pattern method." For him, "basic patterns are nexuses of culture traits which have assumed a definite and coherent structure, which function successfully, and which acquire major historic weight and persistence" (Kroeber 1943:112). The importance of a "basic pattern" is that "it is determinative of its modifications: it sets the frame within which change can take place; it is one of the factors which jointly produce what happens" (Kroeber 1943:107). Thus, knowing what that basic—one could say *primitive*—pattern was allowed one to separate analogous from homologous structures. Analogous structures would have dissimilar basic patterns—"histories are unlike, the secondary results are like; especially as regards function, use, and behavior" (Kroeber 1943:108)—whereas homologous ones would have similar basic patterns. Kroeber's "basic pattern" sounds like Mayr's (1969:398) definition of an archetype as "a hypothetical ancestral type arrived at by the elimination of specialized [evolutionarily derived] characters."

The biological concept of *Bauplan* embodies a notion similar to Kroeber's "basic pattern." A *Bauplan* is a basic structural design, what "comparative anatomists now call the morphotype" (Mayr 1988:109). This concept appeared "early in the nineteenth century," when it was recognized that "animals cannot be seriated in a smooth, continuous chain from the simplest to the most perfect. . . . Instead, a limited number of discrete types can be recognized, such as vertebrates, insects, and mollusks" (Mayr 1988:405–406). Units not unlike *Baupläne* may be useful as variously inclusive hierarchical units in a taxonomy of cultural units. This is precisely the purpose for which the concept was invented in biology; thus, one can speak of the vertebrate *Bauplan*, the less inclusive

mammalian *Bauplan*, the even less inclusive primate *Bauplan*, and the like (Hall 1996). The critical point to keep in mind, however, is that a *Bauplan* at whatever level of inclusiveness is an *ideational* (theoretical) unit much like an inch or a centimeter; it is a unit of measurement, nothing more. Paleobiologists in particular find such units useful (Foote [1996] and references therein), and there is no reason archaeologists might not find them equally useful *if* they keep in mind that *Baupläne* are ideational, not empirical, units.

The few archaeologists who have picked up on the concept of *Baupläne* (e.g., Rosenberg 1994; Spencer 1997) seem to be using the concept in a fashion similar to that of biologists, that is, as something approximating an archetype such as was proposed by Owen (Striedter and Northcutt 1991:178). However, they also seem to have overlooked the fact that the concept denotes a theoretical rather than an empirical unit; utilized as the latter, no end of problems results (see discussion in Lyman and O'Brien [1998]). Archaeologists have displayed minimal concern thus far for the utility of *Baupläne* in ascertaining phylogenetic history and distinguishing between analogous and homologous structures (e.g., Spencer 1992). Perhaps this is because modern biologists often recognize and distinguish between these two kinds of structures without reference to *Baupläne*.

HOW TO RECOGNIZE HOMOLOGS IN MODERN BIOLOGY

If the concept of homology is to help us identify styles at whatever scale in the archaeological record, we must begin with a definition of it. Which one of the several available should we use? In stating that style "is the result of direct cultural transmission," Dunnell (1978) followed evolutionists (as well as early Americanist culture historians) rather than pre-Darwinian comparative anatomists. Homology in this sense denotes what post-Darwinian biologists interested in evolutionary descent have identified as "special," or "phylogenetic," homology; these comprise formal similarities that are the result of common ancestry and hereditary continuity (e.g., Haas and Simpson 1946; Patterson 1982, 1988; Sluys 1996). With such a definition, a biologist could, after 1859, answer the following questions: Why are the wings of robins and crows similar, yet the two organisms are different species? and, Why are the wings of little brown bats and fruit bats similar, yet bird wings and bat wings are superficially similar but structurally different?

Darwinian evolutionary theory provides the explanation for special homology; the explanation is found in the definition of the concept. But the definition does not tell us how biologists identify instances of special homology. Not surprisingly, then, there is disagreement on procedure within biology, though it is generally agreed that the results should be testable (e.g., Brady 1985; McKittrick 1994). What is perhaps surprising, if one recalls Willey's axiom, is that operationalizing the concept in biology does not rest solely on formal similarity. "[H]omologous structures may be extensively similar or very dissimilar. Therefore similarity is to be considered something quite apart from considera-

tions of homology. . . . Similarity does indeed suggest homology, but dissimilarity is also abundantly evident in homology. The concept of homology cannot logically be tied into any definitions of similarity or dissimilarity” (Smith 1967: 101). This does not mean that mere morphological or formal similarity is not important. Such “similarity is the factor that compels us to *postulate* homology” (Cracraft 1981:25). Thus, as Colin Patterson (1988:604) indicates, “morphologists would agree with [the] point [that morphological similarity] can be factual, whereas homology ‘must usually remain an hypothesis.’ ”

If a form of Willey’s axiom is not the means by which biologists identify homologous structures, then what is? In biology, discussion continues with respect to what exactly homologous structures comprise, what they signify, and how they are to be recognized (see, e.g., the three papers by Sattler [1984], Stevens [1984], and Tomlinson [1984] or the more recent discussions of Minelli and Peruffo [1991], Rieppel [1992], and Roth [1991]). Rather than sort through this extensive literature, I take as paleobiology’s and archaeology’s shared goal the recognition and identification of special homologs, that is, those that denote common ancestry, and I summarize how paleobiologists have attained that goal.

The paleobiological literature suggests there are two basic approaches to identifying instances of special homology, each founded in a unique epistemology. Criteria typically used by those following a cladistic approach are (1) positional, or what is referred to as topological, identity; (2) typological, not necessarily functional, identity; (3) ontogenetic identity, which seems to be the least frequently used criterion; and (4) congruence—that is, multiple traits should display the same ancestral-descendant relations between organisms, especially traits that are independent. Although some biologists have outlined potential flaws with these criteria (e.g., Striedter and Northcutt 1991), many believe that they allow the inference of homology. Because the identifications are inferences, biologists agree that their validity must be tested in various ways, such as by using each criterion as a test of every other criterion (Brower and Schawaroch 1996; Forey 1990).

Noncladistically inclined biologists, or those usually referred to as evolutionary taxonomists, prefer to identify characters as homologous “if they can be traced back to the same state in the common ancestor” (Mayr 1969:85; see also Bock [1977] and Simpson [1975]). Evolutionary taxonomists point out that the positional and typological criteria used by cladists are insufficient because “homologous structures are by no means necessarily similar” in terms of these attributes, such as in the case of mammalian ear ossicles and the jaw bones of lower vertebrates (Mayr 1969:85). Evolutionary taxonomists argue that one must be able to trace through transitional specimens the shift in position and form of, for example, a bone from serving as a part of the chewing apparatus, to serving as a part of the hearing apparatus (see Crompton and Parker [1978] for a lucid account of these homologous structures).

Both cladists and evolutionary taxonomists pay attention to the number of characters that might denote homologous similarity, believing many are better

than few (the test of congruence [Rieppel 1994]). Both find that the “kind, extent, and amount of similarity provide the primary data of historical science” (Gould 1986:66). Otherwise, they differ. On one hand, cladists tend to ignore the spatiotemporal distributions of character states because they fear that such extrinsic attributes may be misleading given the vagaries of the formation and preservation of the fossil record. Instead, they focus on the intrinsic attributes or characters and document the state that they are in within particular organisms. As Stephen J. Gould (1986:66) has remarked, “Cladistics is the science of ordering by genealogical connection, *and nothing else.*” Its difficulty, Gould (1986:68) continued, is that it has often been applied to “inappropriate data,” by which I believe Gould means nonhomologous attributes. Evolutionary taxonomists, on the other hand, pay close attention to the extrinsic attributes of spatiotemporal distributions of characters in addition to the intrinsic formal attributes of those characters—the character states themselves—in their attempts to identify homologous structures (e.g., Smith 1994; Szalay and Bock 1991). Interestingly, a similar procedure was outlined by Irving Rouse about 40 years ago, although he was not as explicit as one might hope.

DISCUSSION

Echoing Spaulding’s (1954) remarks quoted earlier, but in a direct response to Philip Phillips and Gordon Willey’s (1953) discussion of Americanist culture historical method, Rouse (1955) pointed out that there were three steps to determining the historical relatedness of archaeological units. First, determine the extent or degree of their similarity or resemblance. Second, determine their degree of proximity in time and space; contiguity in both denotes the potential for contact or interaction and thus the potential for transmission and heritable continuity—in short, an evolutionary or phylogenetic relation. To determine if contact had taken place required the third step, which comprised the distinction between analogous and homologous similarities. From this third step, one could determine the phylogenetic history of the units. But Rouse, like Steward before him, was not explicit about how the third step was to be accomplished, nor did he identify the properties or variables by which similarity or resemblance was to be determined. Spaulding (1954), as we have seen, mentioned Rouse’s first two steps, but not the third. Within the discipline generally, the first and third steps of Rouse’s procedure were accomplished with Willey’s (1953) axiom that “typological similarity is an indication of cultural relatedness,” which, as one should correctly guess by now, did little to resolve the problem because it typically was operationalized as mere formal or typological similarity.

Although acknowledging that the spatiotemporal propinquity of compared phenomena was important, in their reply to Rouse, Willey and Phillips (1958: 31) directed attention away from the more critical and fundamental issue of developing a theory of cultural transmission by arguing that the desired inferences of “cultural relatedness” demanded interpretive concepts that were “cul-

turally determined.” They suggested one must identify *traditions*, *horizons*, and *horizon styles* because these notions were founded in ethnological reality and denoted “some form of historical contact” *rather than* “phylogeny” (Willey and Phillips 1958:30), the latter denoting to them a genetic connection abhorred since the 1940s, when the schemes of Harold Colton (e.g., 1939) and Harold Gladwin (e.g., 1936) had fallen from favor (see Lyman and O’Brien [1997] for additional historical details). The contradiction internal to Willey and Phillips’ position escaped comment. How could cultural transmission presumed to result from “historical contact” and thus transmission and heritable continuity fail to be a process that produced a particular phylogeny? The contradiction escaped notice no doubt because culture historians saw themselves as studying the history of *cultures* manifest as the evolution of artifacts, which, after all, did not interbreed (Brew 1946). In other words, Kroeber’s (1917, 1923) earlier distinction of (genetic) heredity and (cultural) tradition as two separate mechanisms producing evolutionary continuity was resurrected by Willey and Phillips in slightly modified form, but with little theoretical development beyond what had been employed by Evans, Petrie, Wissler, and others since the middle of the nineteenth century.

Interesting parallels are found between Willey and Phillips’ protocol and modern cladistics and between Rouse’s protocol and that of modern evolutionary taxonomists. Both emphasize some of the critical variables at the expense of others. On one hand, within biology, a growing awareness among cladists that space and time are important has resulted in what is called stratocladistics (e.g., Fisher 1994; Smith 1994); this involves the use of temporal data such as stratification to help construct a cladogram of evolutionary divergence rather than constructing a cladogram based solely on intrinsic formal attributes (e.g., Benton and Hitchin 1997). Evolutionary taxonomists are studying character states more closely and in terms of concepts developed by cladists rather than merely examining the spatiotemporal distribution of formal characters (e.g., Jernvall et al. 1996; Szalay 1994). The majority of archaeologists, on the other hand, seem to be stuck largely with Willey’s 45-year-old axiom and Steward’s 70-year-old rules. Few archaeologists seem to be aware that a viable alternative has existed virtually since the birth of the culture-history paradigm 80 years ago.

AN OLD SOLUTION TO AN OLD PROBLEM

Frequency seriation provides a way to monitor phylogenetic history because it is a method that tests hypothesized historical, specifically phylogenetic or heritability dependent, relations among artifacts (Dunnell 1970; Teltser 1995). Successful frequency seriations—ones that produce continuous and unimodal frequency distributions of types—depend on four conditions: that the seriated assemblages (1) be from the same local area, (2) comprise historical types, (3) be of similar duration, and (4) be from the same cultural tradition. The first condition is Steward’s geographic-proximity criterion, though in frequency ser-

iation it is meant to ensure that time alone is being measured by formal variation in artifacts. The second criterion ensures that time, rather than function or technology, is being measured. The third criterion ensures that a unimodal frequency distribution for each particular type is possible and that the position of a particular assemblage in the arrangement is a result of its age rather than of its duration. The fourth criterion is, of course, what one hopes is demonstrated—that the types are, metaphorically, genetically related or the result of transmission and heritable continuity.

The notion underpinning frequency seriation is (1) that a historical type will occupy a single chunk of the temporal continuum and (2) that its frequency distribution will be unimodal through that chunk of time. This notion of a historical type, in turn, rests on the theoretical axiom of hereditary continuity (e.g., Neff 1993; Neiman 1995; Tschauner 1994). That homologous similarity is demonstrated in successful frequency seriations—ones that result in all seriated types displaying unimodal frequency distributions—is so for two reasons. The first relates back to the issue of the nature of types: are they real, or are they arbitrary constructs, what I would term ideational units or classes? Frequency seriation demands that they be the latter so that their distributions can be determined (Dunnell 1970). Further, because frequency seriation uses classes typically termed “styles,” typological identity is ensured by its being imposed on the artifacts rather than being derived from them. Each artifact identified as a member of a particular class is related phyletically to every other specimen within that class, given their properties in common—Spaulding’s (1954) notion that typological resemblance is the result of transmission—and their spatiotemporal propinquity (e.g., Phillips et al. 1951; Rouse 1955; Steward 1929). Elsewhere this is referred to as the *type/species sense* of hereditary continuity (Lyman and O’Brien 2000; O’Brien and Lyman 1999).

The second reason that successful frequency seriations denote transmission, heritable continuity, and homologous similarity is equally simple to grasp conceptually. A successful frequency seriation results in *multiple* associated historical types of a particular category of artifact—ceramics or projectile points, for example—displaying unimodal frequency distributions. This is the congruence test of phylogeneticists and cladists and is also Steward’s multiple-trait criterion. More importantly, with respect to the congruence test, the multiple classes that are seriated are assumed to be related phylogenetically, given the requirement of seriation (Dunnell 1970; Lipo et al. 1997) that all seriated collections derive from a single cultural tradition, which, by definition, reflects transmission, persistence, and hereditary continuity (Phillips and Willey 1953; Willey 1945; Willey and Phillips 1958). Elsewhere (Lyman and O’Brien 2000; O’Brien and Lyman 1999) this is referred to as the *tradition/lineage sense* of hereditary continuity to signify the potential for a diversity of units, at whatever scale (e.g., Neff 1992), within a tradition or lineage. Here, the phylogenetic implications of the hierarchical structure of the Linnaean taxonomy in biology could be transferred to a similar hierarchical alignment of artifacts. Thus, metaphorically, “pot-

tery” might be aligned with a biological family, “types” of pottery with biological genera, and “varieties” of pottery with biological species, or the like. Classes of pottery can be seriated, as they constitute a pottery tradition or monophyletic group, and projectile points constitute a different, independent tradition or monophyletic group. The two “families” of artifacts evolve independently of one another, and each therefore can serve as a test of the ordering produced by the other (Dunnell 1970).

The notion of cultural tradition in the sense of Kroeber and of Willey and Phillips as a line of hereditary continuity had been expressed early on by Nels Nelson. He reasoned as early as late 1913 or early 1914 that stratigraphically superposed artifacts showed “nothing but time relations” (Nelson 1916:163). This was fine, and Nelson is often remembered by historians for his contribution in this regard (Lyman et al. 1997). But what is more important is that he also wanted multiple types found stratigraphically mixed together, “one gradually replacing the other . . . because [such] accounted for the otherwise unknown time that separated the merely superposed occurrences of types and from the point of view of the merely physical relationships of contiguity, connected them” (Nelson 1916:163). In short, the connection was evidence of heritable continuity and what would later be known as a cultural tradition. Two decades later, James Ford (1938:11) recognized exactly this when he noted that the act of ordering via seriation must include “overlapping.” By this he meant that type A may fall within periods 1, 2, and 3, type B within periods 2, 3, 4, and 5, and type C within periods 4, 5, and 6. The *overlapping* temporal occurrences of the types—A and B in 2 and 3, B and C in 4 and 5—connect the sets of materials being seriated and indicate heritable continuity between collections (see Dunnell [1970] and Lipo et al. [1997] for extended discussions).

CONCLUSION

It is perhaps not surprising that frequency seriation and, to a lesser extent, what has been termed phyletic seriation (Lyman et al. 1997, 1998) should reflect hereditary continuity. Hereditary continuity is precisely the notion—if poorly developed theoretically and seldom stated explicitly—upon which these analytical techniques were built in the late nineteenth and early twentieth centuries (O’Brien and Lyman 1999). But the discipline at large seems to have forgotten this. For example, just when discussions over the importance of heritable continuity were reaching a peak in intensity, Clement Meighan (1959:203) indicated that “overlapping of similar [classes] in different finds” was critical to frequency seriation, but he did not say why. Two things seem to have happened that exacerbated disciplinary forgetfulness regarding hereditary continuity while simultaneously retaining seriation merely for dating purposes.

Stratigraphic excavation, followed quickly by dendrochronology and a bit later by radiocarbon dating, obviated any need to perfect the theoretical underpinnings of the seriation method. Stratigraphic excavation meant relative time

was readily visible when an excavated site was stratified (see Rowe [1961] for an interesting take on this), and dendrochronology and particularly radiocarbon dating meant absolute dating was possible. Seriation was rendered an unnecessary implement in the archaeologist's kit of dating tools. The second reason that archaeologists forgot the fundamental (if implicit) theoretical underpinnings of seriation is that they discarded any notion that a Darwinian-like model of evolutionary phylogeny was applicable to cultural phenomena in the 1940s (Lyman and O'Brien 1997). This exacerbated the discipline's failure to develop theory appropriate to the seriation method. Instead, as I have shown, various archaeologists suggested how to distinguish between analogs and homologs, but their suggestions were incomplete and, if followed, did not provide testable results because they were founded in empiricism and common sense rather than theory. Seriation, though it derives from an as yet incompletely developed theory, incorporates those suggestions and provides an analytical means to test hypothesized homologs.

As philosopher Marc Ereshefsky (1992:90) points out, the distinctive aspect of Darwinian evolutionary theory resides in the fact that it requires transmission—heredity—and this in turn “requires the different generations of a population to be connected by reproductive ties, and such ties require those generations to be spatio-temporally connected.” Control of the formal, spatial, and temporal dimensions is necessary to the production of historical chronicles constituting cultural phylogenies. To ensure that any produced chronicle is a phylogeny and not simply a temporal sequence demands that homologous similarities be identified and used to demonstrate hereditary continuity. Dunnell's explicit definition of archaeological style as comprising homologous similarity was a necessary step to building an archaeological theory founded in Darwinism. Americanist archaeologists working prior to 1960 identified some of the critical analytical steps to identifying homologous similarity. It is time to return to these ancient writings, to supplement them with what modern paleobiologists have discerned, to use the seriation method to test results, and to get on with the business of writing and explaining cultural history.

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Chapter 6

Neutrality, “Style,” and Drift: Building Methods for Studying Cultural Transmission in the Archaeological Record

Carl Lipo and Mark Madsen

INTRODUCTION

In recent years, a growing number of social scientists have argued that to understand human behavior within a scientific framework will require the application of evolutionary theory. Within archaeology, Robert C. Dunnell and others have led the charge to design an explicitly evolutionary approach to the past (e.g., Barton and Clark 1997; Broughton and O’Connell 1999; Dunnell 1978, 1980, 1989, 1992, 1995; Leonard and Jones 1987; Neff 1992; Neiman 1995; O’Brien and Holland 1990; Rindos 1984; Teltser 1995). From an evolutionary perspective, the principal mechanism explaining differential persistence of transmitted variation is natural selection. For selection to be operative, not only must variants be transmitted, but at least some of the variants must interact with the environment and do so differentially (i.e., result in differences in fitness). Such variants are commonly referred to as functional or “adaptive,” though the latter term is fraught with undesirable connotations in the human context. For decades, however, biologists have realized that not all variation results in differential interaction with the environment or, in other words, that some variation is selectively neutral (Crow and Kimura 1970; Kimura 1977, 1983; King and Jukes 1969). That does not mean, as some writers have supposed, that such variation cannot be explained by evolution, only that it is not explained by selection. Such variation is explained by transmission processes alone or in combination with sampling error (e.g., Gulick 1872, 1905; Wright 1931, 1932, 1940, 1948, 1949), processes that are Markovian in nature (Dunnell 1978, 1981; Gould et al. 1977). From the beginning of archaeological efforts to incorporate evolutionary theory into practice, both neutral and adaptively significant variation have been important to theory-building efforts.

Despite recognition of the basic concepts needed, extension of evolutionary theory to archaeological phenomena required two additional steps. The first, beginning in the 1950s, was the recognition that an organism's phenotype was not bounded by its skin (Bonner 1980; Griffin 1984; Walker 1983). These researchers argued that behavior, as much as bones, constitutes the phenotype. The second step was the recognition that culture constituted a second mechanism for trait transmission in addition to genetics. This step was facilitated by the first step inasmuch as most morphological traits are transmitted genetically while many behavioral traits are transmitted culturally, even in animals (Bonner 1980; Alejandro Lynch 1996; Payne 1996).

The changes required in archaeological theory and practice to implement an evolutionary approach are substantial; few tasks are harder than the development of a descriptive language with which to describe and measure historical change in the quantitative terms required by evolution. There are three general requirements for evolution by natural selection to occur, following Lewontin (1974). First, individuals must vary. This variation must then confer differing probabilities of survival and/or replication (i.e., differences in fitness). Finally, some of this variation must be transmitted to peers or offspring with better than random fidelity (i.e., heritability). Each of these requirements necessitates measurement of *relative* differences that are observable only as changes in the frequency of variants. The tabulation of frequencies must be conducted within some bounded set of individuals in order to be meaningful. The last requirement highlights the fact that evolutionary explanations require the definition of *populations*. In addition, evolutionary explanations require descriptions of variation within groups that represent *interacting* populations. This requirement is necessary for two reasons. First, interaction contributes to the fitness of individuals. From the perspective of the individual, the selective environment is composed not only of the physical surroundings but also of other individuals and their behavior (Wright 1932). Second, interaction generates heritability of traits.

Regardless of whether or not the subject of a study is an adaptive feature of the human phenotype, the use of evolutionary theory in archaeology demands that we trace the connections of inheritance and transmission. Those connections will be best studied using attributes of the phenotype that satisfy two of the three requirements Lewontin outlined—variation and heritability—but not the third. Those aspects of behavior or artifacts that do not contribute to differential fitness will, therefore, reflect variation in transmission and heritability, as well as a component of random chance. Thus, we expect that analysis of patterns of interaction and inheritance can be done by isolating and studying those attributes of the record believed to be selectively neutral. As in other aspects of an evolutionary approach to archaeology, Dunnell (1978, 1980) first made the link between selective neutrality and the archaeological concept needed to operationalize it: the concept of style.

While style may seem a strange concept to situate in an evolutionary context at first glance, artifact classes based on a notion of style have played a pivotal

role in archaeology and thus require explanation. Artifact styles were the cornerstone upon which culture-historical chronologies were built and served to define the spatial and temporal groupings we still use today. Later, early “new archaeologists” used stylistic classes in an attempt to examine social organization (e.g., Deetz 1965; Hill 1968; Longacre 1964, 1970). While these uses of style may appear difficult to reconcile, Dunnell (1978) pointed out that if one considered stylistic attributes to be selectively neutral, variation in their representation over time and space would reflect transmission and inheritance alone, rather than the interaction of people with their physical environment. Over regional scales, patterning in stylistic classes would reflect the continuity and position of cultural lineages on a landscape; local-scale patterning in stylistic classes would likely reflect community organization.

Dunnell’s notion of style as selectively neutral variation provides the guiding concept necessary to build a science of cultural transmission for archaeology to use with past populations. The concept alone is not sufficient, however, to allow us to actually study and map transmission in the record. Methods grounded in evolutionary concepts while still realistic for archaeological samples are required. In this chapter, we review the connections between the archaeological concepts of style, neutrality as it is used in evolutionary theory, and allied notions such as drift. We then review methods available for assessing neutrality in real data sets and demonstrate their use with data from the Lower Mississippi River Valley.

STYLE IN ARCHAEOLOGY

The first formal use of the term “style” in reference to the choice of terminology for describing attributes comes from Holmes (1886). He occasionally employs the notion of style in a way that suggests that he considered the term to designate decoration in contrast to functional properties (e.g., shape) of artifacts. The concept was developed further in the work of early culture historians such as Kidder (1915), Kroeber (1916a, 1916b), Nelson (1919), and Spier (1917). In the Southwest in particular, archaeologists connected the concept of style with chronological change observed through stratigraphy. These workers demonstrated that if artifacts were described with what Kidder (1915:453) termed “stylistic” classes, then the frequency of these classes displayed a characteristic “monotonic” distribution, and this property could be used to date otherwise undatable assemblages. Culture history was founded on the basis of this method, later termed “seriation.”

Seriations were generally agreed upon to be built from stylistic attributes; there was, however, no effort to explain why stylistic classes exhibited monotonic distributions. In the view of culture historians, style was considered to be anything that worked in the construction of seriations, and evaluation procedures were trial and error (e.g., Krieger 1944). Debates involving the culture-historical type or stylistic classification focused on ontological issues (e.g., Ford 1954a,

1954b; Spaulding 1953, 1954). Theoretical matters, such as the choice of criteria for classification, were ignored altogether. The relationship between the properties identified as style and the distributional characteristics of stylistic classes thus remained obscure, and the stylistic or historical type remained intuitive.

The advent of the new archaeology brought little clarity to the explanation of style. Instead, the situation became considerably worse. Archaeologists such as Longacre (1964, 1970), Hill (1966, 1968), and Deetz (1965) used the same notion of style as did the culture historians (e.g., Ford 1935, 1936; Kidder 1915; Kidder and Kidder 1917; Nelson 1919), though they made a point of claiming to do otherwise. To make matters worse, they used the same attributes (e.g., Spaulding 1954) as their predecessors. Rather than construct new classifications that would describe the record in a manner appropriate to the mechanisms that they wished to use in explanations, they simply claimed that the classes already in use could be used. Built on this premise, the product of the new archaeology was primarily a series of new interpretations. However, since there were no changes in the relationship between attributes and mechanisms, the new interpretations were generated without warrant (e.g., Plog 1980; Phillips 1970).

The situation has steadily deteriorated through time to a point at which style has a variety of meanings, and the choice is left to the individual researcher. In their survey of archaeological concepts of style, for example, Conkey and Hastorf (1990:1) report: "What we think of as style is pervasive in human society, no matter how we define it and style is involved in all archaeological analysis, whether it is covertly or overtly discussed. It is style that creates and defines types, culture types, and even types of evolutionary trajectories." Sackett (1990: 33) goes so far as to praise the degree of ambiguity inherent in the current use of the term "style." Ironically, despite or perhaps *because* of this confusion, it is generally agreed that "the study of style and its place in research and interpretation in archaeology is central and determining" (Conkey and Hastorf 1990: 1).

STYLE AND SELECTIVE NEUTRALITY

In 1978 Dunnell introduced the first theoretical basis with which the concept of style could be understood and the behavior of what are considered to be stylistic traits could be explained. Using a framework grounded in the principles of Darwinian evolution, Dunnell (1978:199) defined style as "those forms that do not have detectable selective values." Stylistic traits, therefore, are best explained as *neutral* traits, and stylistic similarity can be treated as not having been impacted by natural selection but rather the direct result of cultural transmission. Dunnell's explanation finally put the concept of style within the realm of science and gave archaeologists a direction for building methods that are useful for exploring how and why particular cultural attributes have particular distributions in time and space. In this way, we are able to explain the success

of the culture historians and to develop new tools in the study of the archaeological record (Lyman et al. 1997).

One of the primary characteristics that mark stylistic traits is *equivalence*, as one of the primary attributes of neutrality is functional redundancy. Meltzer (1981:314) suggests that “in many instances, the choice between certain kinds of design elements on ceramics is not a functional consideration, but rather is historically determined and selectively ‘neutral,’ because there is no inherent advantage between one element and the next. The actual presence of the design, however, has a selective value because that particular design serves to mark a certain individual or group boundary (or whatever other function it may serve).” O’Brien and Holland (1990) suggest that engineering studies are a way of distinguishing stylistic from functional traits (e.g., Gould et al. 1977). Engineering studies involve the analysis of performance between different alternatives. The smaller the degree of performance differences for any particular function, the more likely the traits will have neutral distributions.

The explanation of style as neutral traits has a relatively profound impact on the potential of evolutionary theory to account for human behavior and points to the necessity of building an archaeology based on Darwinian theory. Dunnell (1978:168) not only argued that must we explain the “fixation of a particular form as a consequence of the increased Darwinian fitness that its presence confers on its transmitters” but also recognized that “not all elements can be assigned unambiguous positive or negative selective values.” As in evolutionary biology, elements that confer relatively equivalent fitness contributions to their bearer within a given environment are defined as neutral traits. The recognition that some, if not many, traits behave as if they are adaptively neutral is an essential component of the kind of evolutionary archaeology envisioned by Dunnell and others. Dunnell suggested further that neutral traits might even be more important to archaeology than they have been to genetics, on the premise that “if anything, cultural transmission should act to increase the capacity for . . . neutral traits” (Dunnell 1978:198).

NEUTRALITY AND DRIFT

A related concept in any discussion of neutrality is *drift*. Drift occurs as a result of change in frequencies of traits that is caused by sampling error (Wright 1931). Neutral traits in small populations are particularly susceptible to drift. Dunnell (1980) first linked drift among neutral traits and the properties of “style” when he noted that stylistic elements are characterized by their “unimodal” behavior in time and space and that in evolution, changes due to drift have a Markovian structure imparted by inheritance. These two observations led Dunnell (1980) to argue that cultural phenomena noted as style display Markovian temporal structures indicative of drift and a lack of selection. This relationship makes methods such as seriation, which rely on Markovian distributions to function, central for building explanations of historical phenomena.

For the most part, interest by archaeologists in neutrality has focused less on neutrality *per se* and more on the Markovian properties that neutral traits exhibit in time and space. In particular, Dunnell (1978, 1980) and others (e.g., Lyman et al. 1997; O'Brien 1996b; Teltser 1995) have argued that culture historians succeeded in building chronologies through the use of stylistic (i.e., neutral) traits that were independent of external conditions. Consequently, it was possible to define purely historical, nonrepetitive classes with frequencies that could be ordered chronologically.

Thus, although there has been significant discussion about the relationship between style, neutrality, and drift since Dunnell's original argument in 1978 (e.g., Meltzer 1981; Neiman 1995; O'Brien and Holland 1990; Teltser 1995), no specific arguments have been made to link together style, the conditions under which drift occurs, and the neutral theory. For the most part, stylistic traits are assumed neutral based on their "driftlike" distributions. But since drift occurs due to sampling error and, therefore, in populations of neutral and nonneutral traits, the relation between drift and neutrality is not a simple one. Style (*sensu* Dunnell 1978) denotes archaeological classes that are manifestations of the theoretical concept of neutral traits. Driftlike characteristics in trait distributions, however, do not necessarily mean that these traits are neutral; drift does not equal neutrality. While drift is a process that depends on sampling error due to small effective population size, neutral traits may be common in large populations, where drift can have little effect. Clearly, simply stating that the concept of neutrality is capable of explaining stylistic traits is insufficient for understanding the conditions under which particular sets of attributes will or will not have unimodal distributions. Fortunately, biology has a long and rich tradition in studying neutral traits from which archaeologists can draw. Thus, to expand our understanding of neutrality and its relationship to style, we turn now to the origins of the notion of neutrality and examine its current use in biological research.

DEVELOPING METHODS FOR EXAMINING NEUTRALITY: NEUTRAL THEORY AND CULTURE

The notion that there is heritable variability not related to individual adaptation dates back to Darwin. He argued (1859:374), for example, that "on the 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." In addition, Darwin (1859:366–367) claims that "in formerly discussing certain morphological characters which are not functionally important, we have seen that they are often of the highest service in classification." Despite this recognition, however, the notion that some traits have no adaptive value remained controversial among biologists who held that natural selection remained supreme in driving variability among organisms. For

example, a paper by Gulick in 1872, arguing that natural selection could not account for the local differentiation of Acatinellid snails in the Hawaiian Islands, stirred up tremendous controversy among biologists such as Wallace, who rejected the notion that differentiation among species could be produced by any but the action of natural selection (Wallace 1889:148, 150). The debate over the role of natural selection in producing all observed patterns of variability in organisms was not quickly resolved and continued over the next 60 years (Provine 1986:220).

The existence of nonadaptive traits and their effects in evolution were quantified and codified with the development of the “neutral theory” in the 1950s and early 1960s, even though many scientists thought that the genetic mechanisms of natural selection were mostly solved and that neo-Darwinism was at its peak (Ohta 1996:673). In the late 1960s protein sequence data were beginning to be available for comparative studies. Motoo Kimura, a Japanese population geneticist and staunch neo-Darwinist, was working to combine population genetics with molecular data. In 1967 he compared the amino acid sequences of hemoglobin and cytochrome *c* of several mammalian species. By extrapolating the number of sequences to the total genome, he was surprised to find that the number of mutant substitutions was too large to be tolerated by the theory of natural selection as held at the time (Ohta 1996). Based on this discrepancy, he proposed the neutral mutation-random drift theory. This theory states that most mutant substitutions at the molecular level in evolution are caused by random genetic drift rather than natural selection. Kimura (1968) published his findings in *Nature*. Further evidence was published shortly after Kimura by J. L. King and T. H. Jukes (1969), who considered more of the biochemical evidence than Kimura. Together, these papers caused quite a controversy among evolutionary biologists (Ohta 1996).

Since Kimura’s development of the neutral theory, there have been numerous modifications that proved necessary in its application to biochemical evolution. In the 1970s biochemists such as Ohta (1973) proposed that the “nearly neutral theory,” in which mutations are very slightly deleterious, better accounts for the data on protein evolution and polymorphisms that were being collected at the time (Ohta 1996). In the original neutral theory, a new mutation was assumed to be either selected or neutral. In the new theory, on the other hand, the focus of research was on the interaction between random genetic drift and selection. The theory, as a consequence, became quite complicated. In the 1980s comparison of DNA sequences became possible, and it was noted that great chunks of DNA such as synonymous sites or pseudogenes exhibited rapid evolution (e.g., Kimura 1983:159).

Consequently, the neutral theory fell back into favor (Ohta 1996). More recently, in the 1990s, new DNA sequence data have shown that patterns of substitutions at selectively important sites differ from patterns of substitution at unimportant sites. The unimportant sites, it seems, are more influenced by neutral theory, whereas important sites are influenced by natural selection (Gillespie

1991). The differences in substitutions, therefore, provide an opportunity to detect selection. Many scientists have thus begun to argue that the strictly neutral theory is not entirely satisfactory (Gillespie 1991; Kreitman 1996; Ohta 1996).

The nearly neutral theory may be summarized as follows: random genetic drift and selection both influence the behavior of very weakly selected mutations, with drift predominating in small populations, and selection in large populations (Gillespie 1991; Ohta 1996). Most new mutations are deleterious, and most mutations with very small effects are likely to be slightly deleterious. Such mutations are selected against in large populations but behave as if neutral in small populations.

Arguments against the strictly neutral theory do not mean much for the practical usefulness of the neutral theory in building evolutionary explanations of variability. As Mayr (1963:212–221) has pointed out, there is no compelling theoretical reason that neutral traits cannot exist or that, within our ability to detect them, alternative forms of traits could not have equivalent costs and benefits to an organism (Dunnell 1980). Although neutral theory has been challenged, the theory remains at the core of modern evolutionary biology. The neutral theory proposes the following: random genetic drift and selection both influence the frequencies of mutations whose fitness differentials are very small; in small populations sampling drift predominates. Neutral theory addresses relative differences in fitness between alternative traits, not absolute fitness, because the degree to which traits are “equivalent” depends both on the engineering relationship between the traits themselves and on the strength of sampling effects, given population size. In small populations, small fitness differences, for example, may be effectively neutral due to sampling effects, where the same difference would be significant in a larger population. Thus, far from being a model about the way all traits work, neutral theory is more commonly used today as a means for generating null models, against which we can detect the action of drift, selection, and other evolutionary mechanisms (Wayne and Simonsen 1998). That is, despite uncertainties about details, the neutral model with explicit empirical expectations provides a useful conceptual framework for thinking about variation and evolution and a potential “barometer for detecting selection acting” on the variability of parts of the phenotype (Kreitman 1996:682). From an empirical point of view, incorporating neutrality into methodology and explanations is critical to the success of evolutionary programs for geneticists, biologists, and archaeologists alike. For archaeologists the challenge is to build methods for evaluating hypotheses of neutrality in the context of cultural transmission.

Neutrality and Cultural Transmission

The notion of neutrality was constructed to account for patterns of variability seen in genetics, specifically, amino acid sequences. It is necessary to ask, therefore, whether variability in the distribution of cultural traits can be modeled

using the neutral model. As a theoretical concept, the neutrality concept is true *by definition*, and determining whether it applies in any particular case is an empirical question. As noted earlier, there is no *a priori* reason that the neutral model will not hold for cultural transmission (Dunnell 1978, 1980; Teltser 1995).

There may be, however, substantial reason that any particular genetic model may not be appropriate empirically for studying cultural phenomena. In general, genetic transmission operates between parents and offspring and is characterized as “vertical” transmission (Cavalli-Sforza and Feldman 1981). Unlike genetic transmission, cultural transmission is free to operate in other directions. It is common, therefore, for researchers to divide transmission into modes: vertical, horizontal, and oblique (e.g., Boyd and Richerson 1985; Cavalli-Sforza and Feldman 1981; Sober 1991). Horizontal transmission applies to transmission within members of the same generation; similarly, oblique transmission describes transmission from an individual to a member of the different generation (Cavalli-Sforza and Feldman 1981) who is not a direct descendant.

In each of these scenarios, cultural transmission is conceived of as distinct but analogous to genetic transmission in which discrete packets of information are passed between individuals in meaningfully defined generation groups. It is not clear, however, if a genetic model is appropriate in the case of cultural transmission. Introducing generations in transmission assumes that transmission events are discrete and, consequently, that generations can be identified. While the units and events of transmission are prominent in genetic transmission, the same is not true for cultural transmission. Cultural transmission is continuous through both time and space; there are no easy temporal and spatial packages of transmission.

In order to proceed further in our discussion of the neutral model, we must ensure that the measurement units used in our neutral model are applicable to the cultural case. The first question to be asked is: What is the unit of transmission? In genetics, the answer is well developed. Genetic transmission occurs in a packet of information that is spatially and temporally discrete. Transmission of genetic material occurs in the process of nucleotide substitution in DNA and of amino acid substitution in proteins. Cultural transmission, however, has no such discernible units.

Consequently, there is little agreement about the appropriate unit of analysis for cultural transmission. On the one hand, Cavalli-Sforza and Feldman (1981) take an epidemiological approach and study the distribution of cultural traits across populations due to processes in which traits passed either from parents to children (vertical transmission) or from child to child or adult to adult (horizontal transmission). They describe cultural characters as “second order organisms” but define them very loosely. Alternatively, Boyd and Richerson (1985) and Durham (1991) offer studies of transmission that make use of particulate models while simultaneously arguing that particles are not a necessary part of their theory. Their theories are presented from the point of view of individuals

that emphasize how cultural traits affect individual fitness and biological reproduction. Finally, Dawkins (1976) coined the term *meme* to refer to any cultural trait capable of being transmitted. In this “memetic” approach to the study of cultural transmission, analytic focus remains on the packet of information (memes) that is replicated through human communication and within human brains. The memetic perspective has seen substantial support in the past several years, occasionally as merely a metaphor but increasingly as a coherent body of related concepts (e.g., Best 1997; Best and Pocklington 1999; Blackmore 1999; Gabora 1997; Aaron Lynch 1996; Alejandro Lynch 1996; Alejandro Lynch and Baker 1993, 1994; Alejandro Lynch et al. 1989; Marsden 1998; Payne 1996; Payne et al. 1988; Pocklington and Best 1997).

Although there are no agreed-upon empirical units of cultural transmission like genes, it is possible to build units that are theoretically equivalent to genes. Williams (1966) defined the gene as the unit that segregates and recombines with appreciable frequency. Pocklington and Best (1997:81) define cultural transmission units as “the largest units of socially transmitted information that reliably and repeatedly withstand transmission.” Importantly, cultural transmission units are measurements of the *effect* of transmission on variability. In this way, culture-historical types, as conceived of by archaeologists, are entirely compatible with this definition of the unit of cultural transmission.

Because units of transmission are conceptual and not “directly” observable, it is necessary to devise means by which the effects of transmission can be identified. As well understood by the early cultural historians (e.g., Colton and Hargrave 1937; Ford 1936, 1938:262; Ford and Griffin 1938:3; Kidder 1915, 1917; Kroeber 1916a:44, 1916b; Nelson 1919; Spier 1917) and discussed by Phillips et al. (1951:220), the task of measuring cultural transmission is accomplished through the use of historical types (Dunnell 1970, 1986; Lyman et al. 1997). Historical types are conceptual units composed of combinations of traits. Observations are made along a large number of explicitly defined dimensions. The intersection of unique combinations of attributes creates classes, some of which are physically manifest in a given situation, and many of which are not (Dunnell 1970). Those combinations of attributes that are of significance for studying heritability will display contiguous spatial and temporal distributions. Of these combinations, some will display distributions consistent with a history of active selection, while others will display distributions consistent with a history of neutrality and drift. Both kinds of “classes,” however, are the products of continuous transmission and inheritance. We can, however, study the spatial and temporal distributions of classes separately, in an effort to determine the processes that are responsible for their histories. Indeed, most methods available to us for identifying the action of selection or its converse, neutrality, rely on the distributional characteristics of classes in space or time. The methods we introduce in the following sections are also distributional in character.

METHODS FOR EXAMINING NEUTRALITY AND STYLE

One important aspect of the investigation of the neutrality of cultural traits is the recognition that neutrality is a concept designed to account for the distribution of phenomena and not a phenomenological property of an organism or artifact. Consequently, types, memes, attributes, or traits are not, by themselves, neutral or nonneutral. Rather, any particular type or set of attributes can have neutral or nonneutral distributions at various times and places (see Cochrane, Chapter 10 in this volume, for further discussion). Attribute distributions change through time and can shift from distributions consistent with neutrality to ones that are characterized by mechanisms involved in selection. Methods are needed, therefore, to evaluate whether particular attributes and their attendant empirical distributions meet the expectations of the neutral model. We need *null models* that can be used to evaluate existing empirical distributions. Using the general theory of neutrality, it is possible to derive two null models for neutrality, one based on functional equivalence and one based on stochastic change within closed arrays caused by drift and mutation.

Equivalency Methods

The theory of neutral mutation predicts that the variability in a population is a balance between the origin of new forms due to mutation (and transmission from external populations) and the extinction of old forms due to drift (Alejandro Lynch and Baker 1993). In this case, there are forms that exist in a population that are functionally equivalent. Using Kimura and Crow's (1964) infinite alleles model in which memes belong to a single locus with infinite alleles, a standard model of neutral mutations can be summarized as follows. There is a population of N memes with an infinite series of population types with no selective differences among them. Successive generations are formed by a process in which the N genes or memes are obtained by repeated sampling with replacement from the N genes or memes of the present generation. Each time a gene or meme is sampled, there is a probability, v , that a unique mutation occurs or that a new form enters the population by immigration. At equilibrium, the diversity of forms in a population (s_e) is related to mutational and migrational input and to extinction by random drift by Equation 6.1:

$$s_e \approx 2Nv + 1 \quad (6.1)$$

where N is the population size, and v represents the combined effects of mutation and migration rates.

Ewens (1972) has shown that the total number of distinct alleles or memes s in a sample and the sample size n are sufficient to provide an expected distribution of meme frequencies. This expected distribution is used as a null hy-

pothesis for evaluating the distribution of attributes for neutrality. From the expected and observed distributions, a maximum likelihood method can be used to determine if the observed sample corresponds with the expected values of the model. If the distribution of memes is not strongly impacted by selection, it is expected that one or another combination should not be strongly favored. In the limiting case, memes will, therefore, be evenly distributed in the population. In a process similar to resampling that uses the original distribution as a means for generating the expected distribution, Ewens' method (1972) calculates the likelihood that the original distribution was generated from an even distribution of the same composition of variant combinations. The observed distribution of memes is calculated as a meme identity (I) statistic. Watterson (1978) showed that I is a good test statistic for examining the expected and observed distributions. I can be calculated using Equation 6.2 as follows: if there are s different memes in a population of size N , and if the frequency of the k th meme is p_k , then we can define *meme identity* as:

$$I = \sum_{k=1}^s p_k^2 \quad (6.2)$$

The expected value of I can then be obtained by using Equation 6.3:

$$E(I) = 1/(\theta + 1) \quad (6.3)$$

where

$$\theta = 2N_e v \quad (6.4)$$

and is derived from Ewens' (1972) maximum likelihood method. The expected value of I , the *meme identity*, can be obtained by simulation using a fast algorithm developed by Stewart (1977).

Ewens' method, therefore, provides a means of deriving a null distribution for neutral traits, assuming that only the processes of mutation and sampling drive variability in trait frequencies. Using resampling on an empirically observed distribution of traits, the test generates an expected distribution and iteratively calculates the likelihood that the original assemblage was derived from a multinomial distribution of the same composition. Deviations from the predicted distribution indicate that factors other than immigration, mutation, and the elimination of types due to random extinction shape frequencies of memes. These factors potentially include selection but also sampling and errors in measurement.

This method has been profitably used in studies of cultural transmission outside archaeology. In an analysis of birdsongs, for example, investigators count the numbers of syllable variants that constitute each birdsong within a population. These combinations are treated as memes. Lang (1995), for ex-

ample, demonstrated that within-sample variability was primarily neutral for Illinois and German populations of Eurasian tree sparrows. Furthermore, the distribution of birdsong attributes resulted from immigration and mutation, and subsequent elimination of memes was due to random “extinction.”

Markovian Distributions

Among archaeologists, the existence of a Markovian distribution of traits through time has been used as an indication of their neutrality (Dunnell 1980; Neiman 1995; Teltser 1995). With only a few exceptions (e.g., Gould et al. 1977), this aspect of neutrality is unexplored by other evolutionary disciplines. As archaeologists, we are uniquely positioned to exploit this feature of neutral traits in the study of cultural transmission.

Markovian distributions can be generated in any finite population. In a culturally interacting, finite-sized population, frequencies of traits are dependent on the composition of the trait population from which those traits were derived. Over time and in an environment in which selection has little to no effect, the frequency of any particular trait is determined by the previous generation. Free to vary yet contingent on the past, at any point in time frequencies have an equal probability of increasing and decreasing. Such distributions are said to be Markovian because the frequency of any trait is dependent on its abundance in the immediately preceding population configuration (Dunnell 1978; Neiman 1990; Teltser 1995:60).

Using a general model of cultural transmission in which individuals learn from each other, Neiman’s (1990, 1995) and our own simulations (Lipo et al. 1997) show that stochastic processes can produce “monotonic” distributions through time. Primarily, this distribution is a function of measuring change within a population of stochastically changing variants of sufficient number using a closed array of frequencies. As Neiman (1990) noted, although erratic changes are a function of Markovian change, sample size greatly impacts the “smoothness” of the monotonic distribution. This sample size is a function of both collection methods as well as variability in local behavioral processes that produced the populations sampled.

Although Markovian distributions have often been used as an indication of the neutrality of traits, there has been remarkably little statistical work to evaluate this model against archaeological samples. Rather than make claims about neutrality, it should be possible to determine the degree to which particular sets of observations meet the expectations of the neutral model. To meet this need, we have developed a statistical test that we have termed the “random walk” test, to evaluate how closely an empirical distribution matches a neutral null model. The neutral model, considering the effect of Markovian change in closed-array frequencies, states that while there may be change within the frequencies of classes, this change should not be strongly biased in one direction or another. Our test, based on a random walk analysis, examines whether the sum of dif-

ferences in frequencies between pairs of assemblages, given a particular order, is statistically different from zero.

Because the value of the differences between frequencies of a type between assemblages is a product of sample size, we followed a standard bootstrapping routine (e.g., Efron and Tibshirani 1993). This procedure consisted of generating 1,000 pairs of bootstrapped assemblages of the original sample size using the original frequency values as a null distribution. We then calculated the sum of differences between the frequencies of types in pairs of bootstrapped assemblages. This procedure allowed us to produce a normal distribution of difference values for each pair of assemblages from which a mean and standard deviation were calculated. Using the 95 percent confidence intervals, we then evaluated whether the mean of the summed differences between pairs of assemblages was statistically distinguishable from zero. For those types with distributions across assemblages that have summed differences that are statistically indistinguishable from zero, we cannot reject the hypothesis that these types meet the expectations of the neutral model.

A Hypothetical Example

As an example of the application of this method, we generated a set of sample assemblages with types whose frequencies changed both monotonically and directionally across the assemblages (Table 6.1). We then conducted the random walk test for these sets of assemblages. The results are shown in Table 6.2. The results of the random walk test demonstrate how it is possible to test a null hypothesis of stochastic distributions for a series of frequencies. In the test, we evaluated at a 95 percent confidence level whether we were able to distinguish the sum of differences between pairs of assemblages for percentages of a single type from a total value of zero. Inability to falsify the null hypothesis suggests that the distribution of frequencies meets the expectations of the neutral model. Falsifying the null hypothesis, on the other hand, indicates that either sample size or sampling is affecting class frequencies or that the type is not composed of attributes that are compatible with the neutral model. This latter result might lead one to evaluate whether the attributes being measured are under selection.

One consideration in the use of the random walk test for evaluating the neutrality hypothesis is the requirement it places on the number of assemblages used. Increasing the number of assemblages helps one evaluate whether types produce descriptions of material that meet the expectations of the neutral model or not. The larger the number of assemblages, the smaller the error term, since the error term is a function of the number of assemblages used to compare the sum of differences against zero, and the less likely it will be to reject the null hypothesis.

Table 6.1**Example set of 10 hypothetical assemblages with five types¹**

Assemblage	Type 1	Type 2	Type 3	Type 4	Type 5
A	10	40	0	5	45
B	20	30	0	15	35
C	25	20	0	25	30
D	30	5	10	30	25
E	30	0	25	35	10
F	20	0	30	40	10
G	10	0	45	40	5
H	5	0	40	50	5
I	5	0	35	55	5

¹The numbers for these assemblages were created to form assemblages that sum to 100 across the five types. The distribution of Type 1 was set to have a monotonic distribution; the remainders have directional changes across the assemblages.

STYLE AND NEUTRALITY IN THE ARCHAEOLOGICAL RECORD: AN EXAMPLE FROM THE MISSISSIPPI RIVER VALLEY

The work of Phillips, Ford, and Griffin (1951, hereafter PFG) provides an excellent example for demonstrating how these methods are useful for evaluating the relationship between “stylistic” attributes and neutrality in the archaeological record. Their studies of Mississippi River Valley ceramic assemblages firmly established the utility of stylistic attributes for studying chronological relationships of archaeological assemblages (Dunnell 1985; Lyman et al. 1997; O’Brien 1996a; O’Brien and Dunnell 1998). Between 1940 and 1948, PFG made systematic ceramic collections across the Mississippi River Valley, mapping late prehistoric sites, making surface collections of pottery, and conducting test excavations. During the course of their survey, PFG (1951) collected 346,099 sherds from 383 localities from across the valley. Each of the sherds was tabulated according to culture-historical types, built on a mix of stylistic and functional attributes that passed the test of historical significance.¹ Overall, the types were useful for building chronologies. With these tabulated data, James Ford constructed five seriations for the study area in order to minimize the effects of geography that were readily apparent in the latest time horizons (PFG

Table 6.2

Results of the random walk test conducted for sample assemblages in Table 6.1

Type	Mean Difference between Assemblages	Standard Deviation	Sample Size (Sherds)	Number of Assemblages with Type	C.I. Lower 95%	C.I. Upper 95%	Includes Zero? ¹
1	0.008	0.090	155	9	-0.052	0.068	YES
2	0.115	0.054	95	4	0.061	0.169	NO
3	-0.053	0.092	185	6	-0.127	0.022	YES
4	-0.072	0.063	295	9	-0.113	-0.030	NO
5	0.046	0.059	170	9	0.007	0.086	NO

¹“Yes” and “No” values refer to whether the mean differences between assemblages for the type fall within the 95 percent confidence intervals. Only Types 1 and 3 in this example meet this criterion.

1951:224). These five seriations, built on “stylistic” types, form the basis of the chronology of the region and are the foundations of spatial units known as phases.

Recently, we performed a series of seriation analyses using the PFG data to examine how these collections could be used to study community structure in past populations (Lipo et al. 1997). Our results demonstrated that the composition of assemblages varies through both time and space and is consistent with a model of transmission in which populations are spatially structured. Like PFG’s chronological conclusions, however, our findings were predicated on being able to equate style with neutrality. Like most researchers, we assumed that the culture-historical types created by PFG of the lower Mississippi River are examples of neutral archaeological descriptions since they were successful in measuring chronologies. However, other than qualitative arguments, neutrality has been assumed, not demonstrated.

The methods presented here provide an excellent means for solving this conundrum; we can now statistically evaluate the neutrality hypothesis by determining whether the frequencies of culture-historical types used in the PFG study match the expectations of the neutral model. As discussed here, Ewens’ method provides one means of testing the statistical properties of the PFG types by comparing these frequencies with the distribution expected in a neutral, infinite alleles model (Alejandro Lynch and Baker 1993). In this method, a meme identity value is calculated using the frequency of each type in each sample, and the expected meme identity is derived using Equation 3.3. In order to generate error terms that can be used to evaluate whether the expected and the observed identities were statistically the same, confidence intervals of diversity estimates were calculated using a resampling procedure that generated 1,000 replicates (Wu 1986). From this distribution, the 2.5th and the 97.5th percentiles were used as the 95 percent confidence limits. We made the calculations for this statistic using a Turbo Pascal program written by A. Lynch (Lang 1995).

Table 6.3 presents the relationship between the observed number of types with the calculated expected number of types for PFG assemblages in the Memphis and St. Francis regions. In PFG assemblages, the observed distribution of classes consistently fell within the expected interval derived from a stochastic model of trait frequencies, suggesting that the PFG ceramic classes do measure neutral variation. The error terms, however, are relatively large, suggesting that within-sample variability may be due to immigration, mutation, and drift but that there may be other factors driving variability in types besides those that are predicted explicitly by the neutral model. These factors potentially include selection, sample size, and the *ad hoc* nature of the construction of the PFG types.

The previously described random walk test also provides a means for examining if drift is driving changes in frequencies that would be particularly strong if the populations were relatively small and the types neutral. After ordering the assemblages from north to south, we examined assemblage differences to determine if the sum of the differences between the assemblages was

Table 6.3

The calculated values of the observed and expected meme identity and the 95 percent confidence intervals for each assemblage in St. Francis and Memphis area assemblages

Site Name	Observed Meme Identity	Expected Meme Identity	95% CI
Carson Lake	0.288	0.4148	0.177, 0.698
Parkin	0.505	0.5345	0.253, 0.842
Turnbow	0.494	0.4649	0.228, 0.782
Williamson	0.503	0.5236	0.19, 0.86
Fortune	0.58	0.6365	0.285, 0.876
Neeley's Ferry	0.499	0.5316	0.263, 0.867
Vernon Paul	0.549	0.6207	0.271, 0.956
Barton Ranch	0.489	0.5236	0.261, 0.862
Cummins	0.652	0.5099	0.266, 0.889
Rose Mound	0.477	0.4523	0.224, 0.773
Big Eddy	0.334	0.5236	0.219, 0.898
Pouncey	0.481	0.476	0.24, 0.781
Cramor Place	0.477	0.4715	0.239, 0.784
Mound Place	0.388	0.3559	0.166, 0.602
Shelby Place	0.274	0.2427	0.125, 0.449
Grant Place	0.411	0.6211	0.301, 0.896
Nickel	0.48	0.4975	0.242, 0.823

Starkley	0.293	0.4829	0.245, 0.831
Castile	0.502	0.3982	0.19, 0.617
Kent Place	0.273	0.4998	0.236, 0.864
Davis	0.375	0.5811	0.275, 0.874
Clay Hill	0.326	0.5522	0.259, 0.892
Hollywood	0.289	0.4346	0.23, 0.813
Commerce	0.304	0.4149	0.184, 0.789
Belle Meade	0.366	0.4543	0.223, 0.787
Beck	0.305	0.4038	0.217, 0.684
Walls	0.48	0.332	0.16, 0.647
Irby	0.186	0.3423	0.173, 0.572
Woodlyn	0.479	0.3545	0.169, 0.589
L. Cormorant	0.262	0.3545	0.163, 0.608
Moore	0.339	0.5744	0.262, 0.836
West	0.496	0.4671	0.239, 0.823
Owens	0.34	0.3689	0.158, 0.695
Dundee	0.67	0.5848	0.271, 0.9
Montgomery	0.297	0.4329	0.197, 0.774
Oliver	0.373	0.4131	0.189, 0.717

statistically distinguishable from zero at an $\alpha = 0.05$ level of significance. Hypotheses tested here are:

H_0 = Classes describe the assemblages in neutral terms with respect to geography. In this case, the summed mean differences between the frequencies of types in pairs of assemblages would not be distinguishable from zero.

H_1 = Classes do not describe the assemblages in neutral terms with respect to geography. The summed mean differences between the frequencies of types in pairs of assemblages are distinguishable from zero.

The results of our random walk analyses are shown in Table 6.4. Of the culture-historical types used in the PFG study, only Owen's Punctate does not have a distribution across the assemblages that meets the expectations of the neutral model. There are two potential reasons that this may be so. First, Owen's Punctate appears only in a few very late assemblages and may not have a distribution that is large enough to evaluate for neutrality. Second, it was used by PFG as a "tentative" type because it was identified in only small, fragmentary sherds. PFG admit "it is to be doubted whether we are actually dealing with a 'type' or merely a collection of sherds which share similar *observable* characteristics" (PFG 1951:136, italics in original). Consequently, the distribution of Owen's Punctate may be entirely random or mark the distribution of a variety of functional criteria that produce sherds with the size and characteristics as those identified as Owen's Punctate. The former seems likely in view of PFG's discussion. The general conclusion of these analyses is that except for Owen's Punctate in the late prehistoric assemblages of the Mississippi River Valley, the PFG types behave as if they are neutral.

CONCLUSIONS

The archaeological literature on evolutionary theory has long recognized the importance of neutrality and has linked neutrality to the concept of "style" and stylistic classes. Dunnell and others also recognized that methods were needed for identifying and studying the distribution of stylistic classes in the record. Principal among these methods has been seriation, borrowed from culture-historical practice and extended by Dunnell (1970, 1981) and others (Lipo et al. 1997) for this purpose. At the same time, the need for additional distributional tests has been explored by Neiman (1995), whose discussion of the Ewens algorithm prefigures our own. In further developing the Ewens test and introducing the random walk test, we believe that the tool kit for studying cultural transmission in the archaeological record is becoming rich enough to deal with real-world complexity.

Building a set of scientific methods for studying cultural transmission, whether in archaeology or among living peoples, is a critical step on the road to explaining human behavior in an evolutionary context. Darwinian evolution

Table 6.4

Analysis of neutrality on culture historical types and Memphis and St. Francis assemblages

Type	Mean Difference between Assemblages	Std. Dev.	Sample Size (Sherds)	Number of Assemblages	C.I. Lower 95%	C.I. Upper 95%	C.I. Includes Zero?
Parkin Punctated	0.00727	0.2785	5210	36	-0.086	0.100	YES
Barton Incised	0.01066	0.2102	2623	35	-0.060	0.082	YES
Ranch Incised	0.00148	0.0458	159	23	-0.018	0.021	YES
Vernon Paul Appliqué	-0.00043	0.0182	13	8	-0.013	0.012	YES
Fortune Noded	-0.00007	0.0215	74	13	-0.012	0.0119	YES
Manly Punctated (T)	-0.00069	0.0119	13	2	-0.018	0.016	YES
Kent Incised	0.00097	0.0711	225	31	-0.025	0.027	YES
Rhodes Incised	0.00024	0.0347	52	14	-0.018	0.019	YES
Walls Engraved	0.00140	0.0479	93	16	-0.022	0.025	YES
Hull Engraved	-0.00035	0.0371	32	10	-0.024	0.023	YES
Mound Place Incised (T)	0.00039	0.0117	9	7	-0.008	0.009	YES
Old Town Red	0.00826	0.2058	901	35	-0.061	0.078	YES
Carson Red-on-Buff	0.00264	0.0183	12	1	-0.034	0.039	YES
Owens Punctated	0.02246	0.0152	13	5	0.0089	0.036	NO

is, even more than the theory of natural selection, fundamentally an exploration of the consequences of strong inheritance. Evolution is descent with modification; natural selection is but one mechanism for modification within a lineage of individuals. Because of its historical position within evolutionary theory, however, selection has attracted most of the effort of archaeologists seeking to use evolutionary theory for explanation. While selection is important to explanation, we believe that the foundation of an evolutionary archaeology is a firm understanding of descent and inheritance in cultural populations, along with scientific methods for dealing with the interaction between sampling, formation processes, and the distributional patterns that reflect transmission. Only with such methods will we be able to gain a clear picture of cultural lineages within which selection may act.

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NOTE

1. See Krieger (1944). Strictly speaking, PFG types are not quite this consistent; Ford's types approach this condition.

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Chapter 7

Style, Function, and Variation: Identifying the Evolutionary Importance of Traits in the Archaeological Record

Todd L. VanPool

INTRODUCTION

Undeniably, an individual's behavior, including the use of particular objects of material culture, affects his or her reproductive success. This realization underlies *all* archaeological approaches that treat culture as an adaptation. It also provides the foundation of the direct application of Darwinian evolutionary theory to the study of human culture (Dunnell 1980; O'Brien and Holland 1990). Specifically, when variation in behavior and artifacts results in different reproductive rates (i.e., differences in evolutionary fitness), it will necessarily be impacted by natural selection. However, not all variation affects the reproductive potential of individuals. Some behavioral and artifactual traits may simply not affect reproduction, and a series of alternative traits may result in the same fitness levels within a particular environment (O'Brien and Holland 1990).

Additionally, some attributes may be selectively neutral at the scale of human reproduction but may affect the replicative success of behaviors and their associated artifacts (Leonard and Jones 1987). These attributes will still be acted upon by natural selection, albeit the selection will be operating at a different, lower level (the level of replicative success) than that of human reproduction (Leonard and Jones 1987; Neff 1992, Chapter 2 in this volume).

A fundamental dichotomy is, therefore, created between traits that are affected by natural selection and those that are not (Dunnell 1978; Leonard and Jones 1987). This dichotomy has been formalized using the concepts of function and style (*sensu* Dunnell 1978). Within an evolutionary archaeological approach, *functional attributes* are defined as traits that are affected by natural selection, while stylistic traits are defined as traits that are selectively neutral (Dunnell 1978; Leonard and Jones 1987; O'Brien and Holland 1990).

Functional traits can be present at a variety of artifactual and behavioral levels (Beck 1998; Neff 1992). For example, subsistence strategies (e.g., maize horticulture), the presence of a particular artifact or tool (e.g., the bow and arrow), or characteristics of an artifact (e.g., artifact length or width) may all be functional. Regardless of the scale, though, the evolutionary process of natural selection will affect the frequency of functional traits. In contrast, stylistic traits vary irrespective of natural selection, and their frequency will be the result of evolutionary processes such as drift and sorting (Abbott et al. 1996; Beck 1998; Hurt et al., Chapter 4 in this volume; Lipo and Madsen, Chapter 6 in this volume; O'Brien and Holland 1990).

It is clear, then, that *the* primary methodological hurdle in applying a Darwinian evolutionary framework to the archaeological record is differentiating between stylistic and functional traits (Allen 1996; Dunnell 1978; Leonard and Jones 1987). Until evolutionary archaeologists are able to identify which evolutionary process(es) is affecting the frequency of a particular phenotypic attribute under study, evolutionary theory cannot be rigorously applied. Unfortunately, they have not yet succeeded in developing a general methodology that will consistently solve this substantial methodological challenge. Instead, they often follow archaeologists using other theoretical perspectives and use intuitively derived demarcations (e.g., Blankholm 1990; Hegmon 1992; Hill 1985; Sackett 1982, 1986; Sampson 1988; Wiessner 1983). This leads to *a priori* divisions—such as that the decorations on the exterior of ceramic pots are stylistic, while the temper of the pots is functional—that are based on unexamined assumptions concerning the importance of various behavioral and artifactual traits. As a result, the distinctions between stylistic and functional traits change from researcher to researcher, and the basis for the division between style and function, is at best an educated guess based on the gut feelings and experience of the archaeologist or the shape of trait frequency distributions through time (Beck 1998; Braun 1987; O'Brien and Holland 1990:52), and at worst an unnecessary impediment that systematically prevents an accurate evolutionary understanding of the archaeological record.

The goal of this discussion is to present a method that will allow archaeologists to distinguish between functional and stylistic traits. This method builds on the foundation presented in O'Brien et al. (1994) and will provide archaeologists with a means of examining a suite of attributes of artifacts, determining which are likely stylistic and which are functional, and then evaluating their groupings.

This chapter is organized in three sections. In the first portion, several theoretical issues that are directly related to differentiating stylistic and functional traits are discussed. Specifically, I address the different evolutionary patterns that natural selection can produce, introduce the concept of fitness coefficients, and briefly discuss the effects of drift operating on stylistic traits. I also argue, along with others (e.g., Beck 1998; Maxwell 1995; O'Brien et al. 1994), that

functional traits are necessarily important performance characteristics affecting the usefulness of selectively important artifacts.

The second portion of the chapter presents a model describing how the amount of variation in the attributes of artifacts such as projectile points, ceramics, and groundstone can be used to distinguish functional and stylistic attributes. Finally, I apply the outlined methodology to a collection of metates from Paquimé, Chihuahua, Mexico. I quantify the variation in several attributes of these artifacts, thereby developing expectations concerning which attributes are functional and which are stylistic. The results are then compared to performance studies of metates in order to evaluate the effectiveness of the proposed methodology.

VARIATION AND EVOLUTIONARY PROCESSES

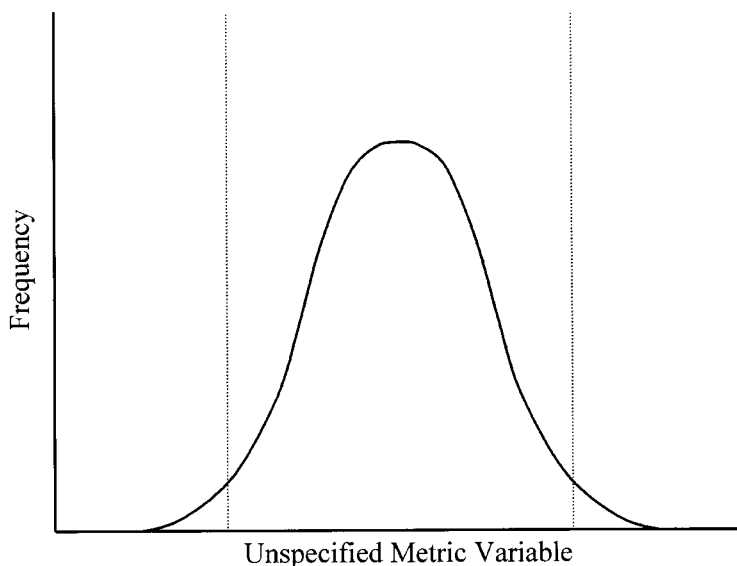
Ultimately, differentiating between functional and stylistic attributes is an empirical issue, although it clearly includes a theoretical component. A behavioral or artifactual trait may be functional or stylistic based on the specific selective environment. It is, therefore, impossible to develop an absolute list of attributes of artifacts or behaviors that are *always* functional or *always* stylistic (see Cochran, Chapter 10 in this volume).

Functional Traits

While there is no absolute or universal distinction between traits that are functional and traits that are stylistic, functional traits of artifacts such as projectile points or ceramics must impact the ability of individuals using these artifacts to accomplish selectively important tasks (including social tasks). The reason for this has been outlined by O'Brien et al. (1994) and rests on the premise that functional attributes by definition affect the fitness of individuals or the replicative success of artifacts within a given environment. In order to do so, a functional trait must impact the interaction of the individual with the environment in some way. Functional traits are, therefore, important performance characteristics (Schiffer and Skibo 1987:599, 1997), by definition (O'Brien et al. 1994).

The Operation of Selection. Although natural selection operates on all functional traits, it may not have the same effect in every case. Three general patterns can be produced by natural selection (O'Brien and Holland 1992:40–41; Jones et al. 1995:27). The first and perhaps most widely recognized is stabilizing selection. It is created when natural selection works to bind the amount of variation in a population around a mean. Using Figure 7.1 as a heuristic illustration of the variation in a functional trait of a population, stabilizing selection will cause the variants in the tails of the distribution to drop out (i.e., the tails of the distribution are selected against), producing a distribution similar to that in Fig-

Figure 7.1
Hypothetical distribution of a functional trait



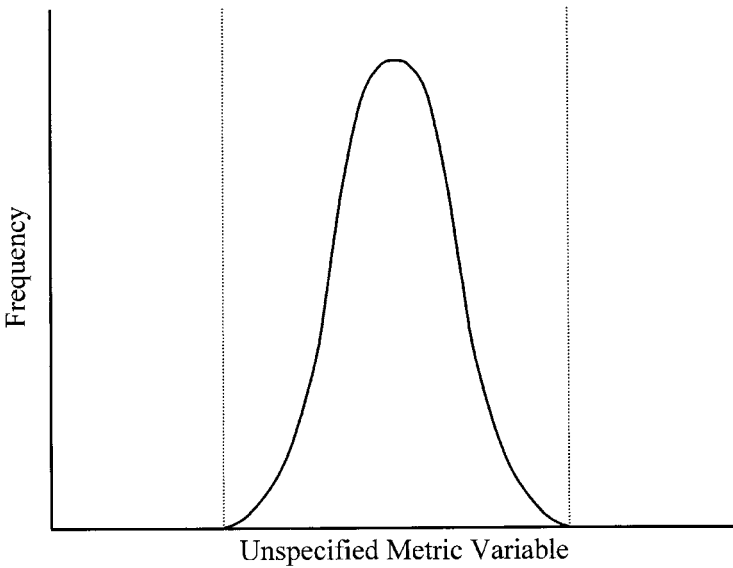
ure 7.2, in which the amount of variation in the population decreases through time.

The second pattern is directional selection, which is created when one tail of the range of variation in a functional trait is favored over variants toward the middle or alternate tail. Again using Figure 7.1 as a heuristic illustration, directional selection will produce a distribution similar to that in Figure 7.3.

Finally, natural selection can be disruptive, thereby favoring variates in both tails of a distribution and selecting against variates toward the center. Using Figure 7.1 as a starting point again, disruptive selection will produce a pattern similar to that in Figure 7.4.

Each of these types of selection will produce different empirical patterns in the archaeological record (Beck 1998). However, they are identical in that natural selection limits the *amount* of variation in functional attributes. This trend ultimately results from the fact that functional attributes are performance characteristics impacting the usefulness of an artifact for performing specific tasks (see Dawkins 1996:38–47 and Sober 1993:36–38 for similar points from a biological point of view).

For example, Skibo (1992) discusses the performance requirements of cooking vessels among the Kalinga of the Philippine Islands. He observes that cooking pots must have a low porosity. Otherwise, water from the inside of the vessel will escape to the surface of the vessel, turn to steam, and thereby cool the vessel's surface. If a cooking vessel is too porous, the contents of the vessel

Figure 7.2**Changes in a hypothetical distribution of a functional trait resulting from stabilizing selection**

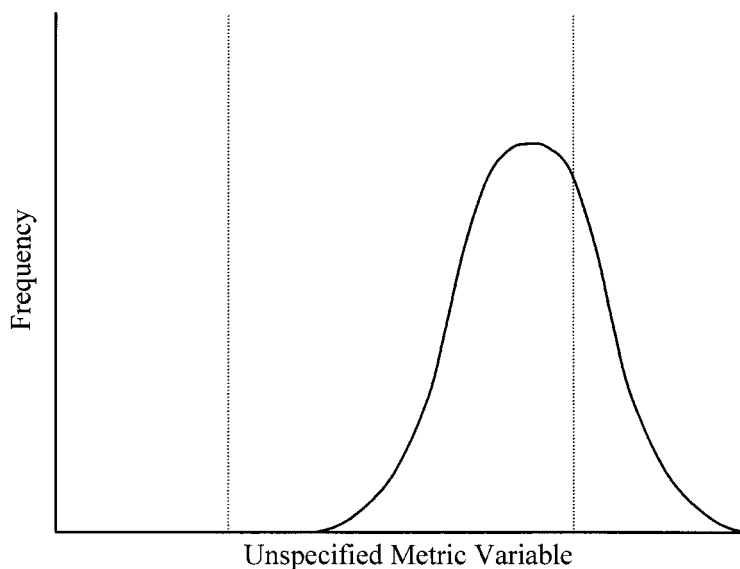
will never reach the boiling point regardless of the size of the fire or the length of time the pot is heated (Skibo 1992). Therefore, porosity is an important performance characteristic for cooking pots.

If natural selection within a given environment favors the use of ceramics for boiling food, then it will act to limit the amount of variation in cooking vessel porosity. As a result, those manufacturing techniques that consistently produce vessels with the required characteristics will be favored over techniques that do not. Natural selection will, therefore, act to constrain the variation in porosity (i.e., place limits on the range of variation in the performance characteristic).

Ultimately, the selective environment results in a characteristic being an important performance characteristic, and the dynamic nature of selective forces suggests that functional attributes can become stylistic, and stylistic attributes can become functional as the environment changes (Dunnell and Feathers 1991: 34). For example, some characteristics of a top may be stylistic when it is a child's toy. However, when it becomes a gyroscope in a missile, the same characteristics may become functional. Thus, we would expect the amount of variation in many attributes of tops to be considerable when compared to the amount of variation in gyroscopes in a particular type of missile. Variation within a single attribute of a particular technology may, therefore, fluctuate between being limited and great as changes in selective forces shift over time and space (Beck 1998).

Figure 7.3

Changes in a hypothetical distribution of a functional trait resulting from directional selection



Observing that natural selection will limit the amount of variation in functional traits does not necessitate that all functional traits will be equally variable. I suggest that two factors in particular can affect the amount of variation allowed in a functional trait: differences in mechanical performance requirements and differences in fitness coefficients. I discuss each of these factors in turn.

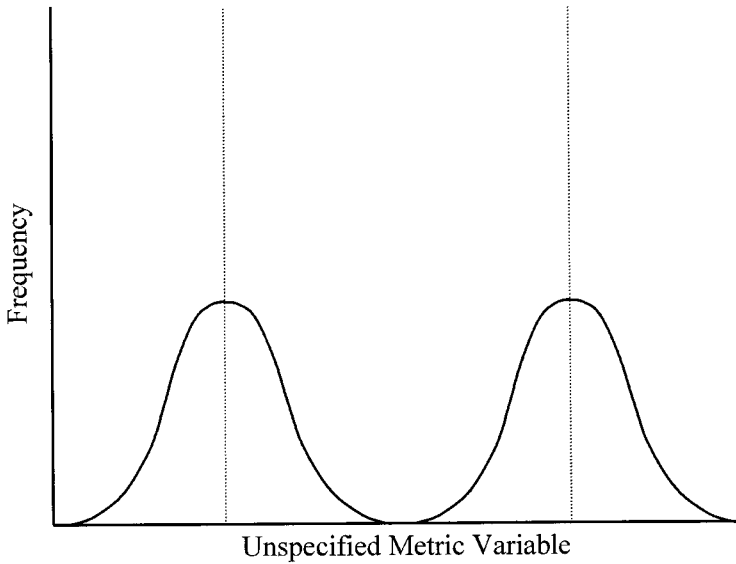
Mechanical Constraints. As illustrated earlier, mechanical requirements related to the use of artifacts necessitate that artifacts have certain characteristics in order for them to be useful for certain tasks (Schiffer and Skibo 1987, 1997). However, the variation allowed by the mechanical requirements of performance may be variable, depending on the artifact type and use.

For example, in a hypothetical environment the wall thickness of ceramic cooking vessels is a functional characteristic, because it affects the rate of heat transfer to the contents of the vessels, the vessels' resistance to thermal and mechanical shock, and the maximum temperature that the vessels' contents can reach (Braun 1987; O'Brien et al. 1994). The wall thickness of ceramic storage vessels may also be a functional characteristic, because it affects the vessels' durability, the probability that a pot will break during firing, and other similar factors. Natural selection will therefore act to limit the amount of variation in the wall thickness of both cooking and storage pots.

The limits on the amount of variation in wall thickness created by natural selection may not be equal, however. Natural selection may cause the wall thick-

Figure 7.4

Changes in a hypothetical distribution of a functional trait resulting from disruptive selection



ness of the cooking pots to be more tightly constrained when compared to the storage pots, as dictated by the vessels' use in a given environment. Minor variation in the thickness of the walls of cooking pots may greatly alter the specific thermodynamic characteristics of a pot, thereby causing the variation to be tightly constrained. In contrast, a great deal more variation may be allowed in the wall thickness of storage pots, because the only requirements of the walls is that they not break during firing and when handled normally. The wall thickness of storage pots can thus vary from relatively thin walls to fairly thick walls, while the wall thickness of the cooking pots is more tightly constrained, in spite of the fact that wall thickness is a functional characteristic in both cases.

Fitness Coefficients. The second factor that can result in differences in the amount of variation in functional traits is differences in fitness coefficients or, rather, difference in the strength of the selective pressures affecting functional attributes. The concept of fitness coefficients rests on the realization that the selective forces acting on functional characteristics are not necessarily equally strong. Some functional traits may be paramount to reproductive or replicative success, while other characteristics are much less important, even though they are functional. The relative differences in the strength of the selective pressures acting on different attributes can be represented using the concept of fitness coefficients. While fitness coefficients are an ideational unit that cannot be precisely measured, they can be used as a relative measure indicating the impact of certain traits on the fitness of individuals relative to other traits.¹

For example, the artifacts used by prehistoric Inuit in the Arctic region included boots, various forms of temporary houses, fishhooks, boats, whale and seal harpoons, whale oil lanterns, paraphernalia used in conjunction with domesticated dogs, and a wide variety of clothing made from the fur of a number of different animal species (Harp 1978). Each of these artifacts undoubtedly contributed to the survival of the individual Inuit and is, therefore, likely a functional trait. However, while an effective whaling harpoon weapon system is undoubtedly a functional trait for prehistoric groups in the Arctic, it is certainly not as important to the survival and reproduction of an individual as is clothing appropriate for the harsh weather. A poorly designed harpoon might force individuals to focus on other food resources and perhaps lead to limited starvation and population redistribution over the long term. In contrast, poorly designed clothing will lead to hypothermia and death fairly rapidly. Clothing, therefore, likely has a higher fitness coefficient than whaling harpoons, meaning that the impact of clothing on the fitness of the individual is greater than is the impact of whaling harpoons.

By definition, the frequency of attributes with greater coefficients of fitness will be more heavily impacted by natural selection than will the frequency of attributes with lower fitness coefficients. Biologists refer to this phenomenon as the strength of selection and use it to explain differences in the rate of change in certain phenotypic attributes. Further refining the concept of the strength of selection using fitness coefficients will help archaeologists specify the relationship between various functional traits (see also Hurt et al., Chapter 4 in this volume; and VanPool and Leonard, 2000).

Stylistic Traits

The evolutionary mechanisms that operate on stylistic attributes, drift and sorting/hitchhiking, have been discussed in detail elsewhere (Hurt et al., Chapter 4 in this volume; Lipo and Madsen, Chapter 6 in this volume; Neff 1992; Neiman 1995) and are not addressed further here. In general, stylistic traits should show more variation over time and across space when compared to functional attributes of the same technology, because they are not constrained by natural selection. For example, while the porosity of ceramic cooking vessels may be functional in some environments, the porosity in decorative pots sold to tourists may be stylistic. Natural selection will create limits on the variation in the porosity of cooking vessels but will not do so for vessels produced for tourists. As a result, the porosity of ceramics produced for tourists will generally range more greatly than will the porosity of ceramic cooking vessels, for the simple reason that differences in clay composition and surface treatment among potters will cause a great amount of variation when compared to the standardization in the manufacture of cooking vessels that will be created by natural selection.

A MODEL FOR DIFFERENTIATING STYLISTIC AND FUNCTIONAL ATTRIBUTES

The first step in identifying stylistic and functional attributes is to quantify the amount of variation in an assemblage. Attributes of a given technology that demonstrate little variation are likely to be functional, while attributes with more variation are likely to be stylistic, all other variables being equal. Two types of traits are present on any artifact: metric attributes (traits that change by degree such as artifact length and width) and qualitative attributes (traits that change in form such as ceramic temper type or lithic raw material type). Variation in each type of trait must be measured differently.

The variation in functional qualitative attributes will be limited to only a few (or one) of the possible alternatives. For example, if temper type is an important functional characteristic, then the temper types used should be limited by natural selection to only a small portion of the possible temper types. In contrast, stylistic qualitative attributes should be free to vary (e.g., if the decoration on the outside of a pot is truly stylistic, then the pot can be decorated with chevrons, triangles, squares, hatching, anthropomorphic figures, or any other design elements).

The variation of metric functional traits is constrained between two extremes: being too large and being too small (see also Beck 1998). If a functional characteristic falls into either extreme, its continued use will be selected against. For example, as the basal width of a projectile point increases past a certain size, the point will become increasingly difficult to haft securely. Likewise, if the basal width becomes too small for a point to be hafted, the point will not be usable (as a projectile point).

Evolutionary processes such as drift affect stylistic traits. The distributions produced by drift are random with respect to natural selection (O'Brien and Holland 1990), and the amount of variation in metric stylistic attributes can take any value, including a bounded distribution such as that indicative of functional traits. However, because stylistic metric traits can take any value within the physical constraints of the artifact, they will probably vary within a much larger range relative to functional traits, especially through time. More importantly, though, metric stylistic traits should not be important performance characteristics.

While the discussion of the model has been implicitly based on a pattern of stabilizing selection thus far, it is also applicable to cases of directional and disruptive selection. However, several issues must be considered. First, as illustrated previously in Figure 7.3, the amount of variation in functional traits that are subject to directional selection is constrained at any one time and/or place but may appear to be great through time or space, because the upward or downward movement of the distribution will result in a broad range of values. It is therefore necessary to maintain secure temporal control of the artifacts being analyzed, so that such differences can be detected (see Braun 1987).

Likewise, the amount of variation in functional traits subject to disruptive selection may be easily confused with the variation expected in stylistic traits. With the selection for diversity, there will be a period when the distribution of variates will be extremely variable as the frequency of variates in the tails increase and the frequency of variates in the center of the distribution decrease. Through time and/or space, then, the amount of variation in the distribution of variates will increase, to a point. After a certain point, though, the amount of variation in both tails will become more constrained, and two (or perhaps more) distributions with limited variation will be created. The time required for the differentiation of the two distributions will depend on the strength of the selective forces operating on the distribution (i.e., the fitness coefficient of the trait). However, by carefully controlling for time and space, the distinctive pattern expected in traits subject to selection for diversity can be identified.

Quantifying Variation

In order to differentiate functional and stylistic traits on the basis of differences in trait variability, a framework must be developed that will allow variation to be measured in a quantitatively meaningful way. Differences in the amount of variation in qualitative attributes within assemblages can be identified by quantifying the number of alternative qualitative attributes present within an assemblage and using nonparametric statistical procedures such as the chi-square analysis. Such approaches are especially ideal when using paradigmatic classifications (Dunnell 1971, 1995).

A natural choice for quantifying the variation in metric traits is the corrected coefficient of variation (Sokal and Rohlf 1981:58–60). The corrected coefficient of variation (corrected CV) is an expression of the standard deviation as a percentage of the mean of the parent distribution. It controls for the absolute size of the variables being measured while also accounting for the tendency of variation to be underestimated in samples of small sizes. Use of the corrected CV allows the variation present within vastly different distributions and variables to be compared directly. Changes in corrected CVs for the same attribute between classes should reflect the shift of an attribute from a functional to a stylistic role (or vice versa) or changes in mechanical constraints caused by changing technology at a higher scale (e.g., mechanical constraints of projectile points may change with the shift from atlatl darts to arrows).

Once the variation in the traits of a group of artifacts is quantified, the traits can be ranked in ascending order by their corrected CVs. This ordering will provide the starting point for differentiating stylistic and functional traits and may also provide clues to the fitness coefficients of the traits.

Identifying Function and Style. The identification of stylistic and functional metric traits begins by ordering the attributes from the smallest to the largest corrected CV and comparing the resulting ranking to performance studies and experimental analyses to determine if the attributes with low CVs are, in fact,

important performance characteristics, and if attributes with high CVs (relative to the other CVs) are not. It is entirely possible, although by no means necessary, that the corrected CVs will produce two intuitively recognizable groups, attributes with low corrected CVs (likely functional traits) and attributes with high corrected CVs (likely stylistic traits). However, it is possible that a more continuous distribution of corrected CVs that cannot be as easily divided into two groups will be produced. In both cases, performance and experimental studies will hold the key to differentiating functional and stylistic attributes.

Because of the relationship between functionality and performance characteristics, the attributes with lower corrected CVs will correspond to traits that are important performance characteristics, in most cases. As one progresses toward the attributes with larger corrected CVs, the attributes should cease to be important performance characteristics. Ideally, one should be able to draw a line dividing the ranked traits into two groups: those that are important performance characteristics and those that are not. These groups should correspond to functional and stylistic traits, respectively.

Unfortunately, ordering qualitative attributes by the amount of variation they possess is not as straightforward as ordering variation in metric traits. As mentioned previously, nonparametric statistical tests such as the chi-square test and the subsequent analysis of residuals allow the direct comparison of variation between some qualitative traits. However, measures of richness and evenness may also provide a useful avenue in comparing the variation within different qualitative attributes of an artifact class. Regardless of which approach is used, those attributes that demonstrate little variation are still expected to be functional, while attributes with more variation are likely to be stylistic. Once again, the expectations based on their variation can be evaluated using performance studies.

When attributes that are known to be important performance characteristics are not less variable than traits that are not known to be performance characteristics, one of two possibilities is likely present. First, drift or sorting may have caused the distribution of a stylistic trait to be tightly bounded. If this is the case, an examination of the mechanical and archaeological relationships between the various attributes will help to identify (1) the presence of sorting and/or hitchhiking, as Hurt et al. (Chapter 4 in this volume) discuss, or (2) the factors that decreased variation as a result of drift as outlined by Neiman (1995).

Second, it is possible that attributes with lower CVs that are not known to be performance characteristics are in fact important to the use of the artifacts and are, therefore, functional traits. This is especially likely for traits that are important for social use and information exchange, traits that may be under selection at the scale of replicative success. Additional examination of the archaeological context of the artifacts may help identify the specific importance of the attribute. Ultimately, this may be one of the most powerful features of the proposed method; it will help identify those attributes whose selective importance is unknown and provide a means of studying changes within them.

Identifying the Relationships between Functional Traits. Style and function are dichotomous categories such that a trait is either one or the other. While this is true in an absolute sense (either a trait is or is not affected by natural selection), the real-world application of the distinction is not so clear. The reason is that functional traits do not necessarily have an equal impact on the fitness of an individual; that is, they can have different fitness coefficients. In reality, the fitness coefficient of an attribute can range from very high to zero (attributes that have a fitness coefficient of zero are stylistic traits). As the fitness coefficient of an attribute decreases toward zero, the importance of the attribute to the survival of the individual decreases, and the impact of natural selection on the range of variation of the attribute is lessened.

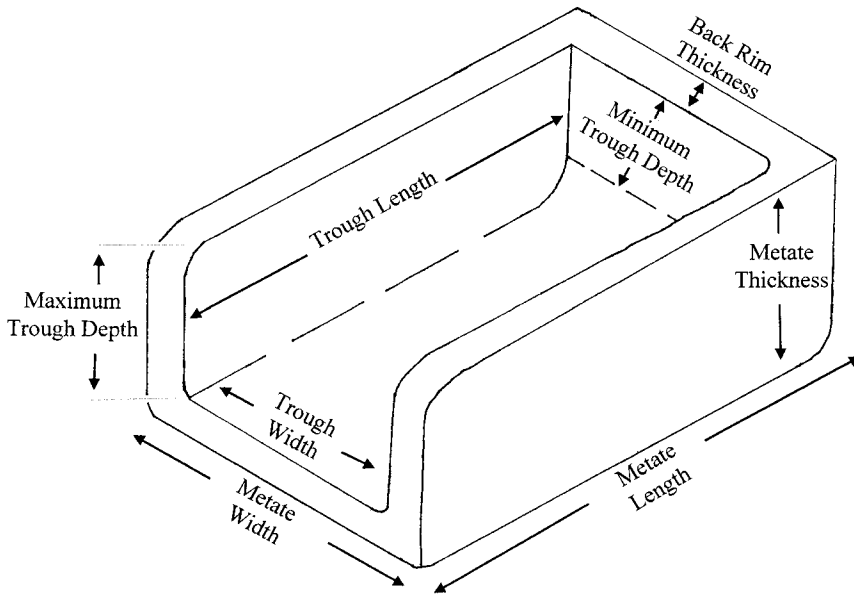
The ranking of the attributes of an artifact assemblage in ascending order by their corrected CVs can be called the proposed ranking of fitness coefficients. This is because the amount of variation is likely to reflect the strength of the selective forces operating on the attribute (see also Vaughan, Chapter 8 in this volume). Of course, the mechanical constraints of performance will also affect the amount of variation within an attribute and can complicate this ordering of functional traits by their selective importance. However, experimental studies and performance analyses will help identify those attributes that are allowed to vary more greatly as a result of mechanical constraints and will also help distinguish those traits that are more important to the use of an artifact than others.

The proposed ranking based on the 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 do, in fact, reflect differences in the selective importance of the attributes (i.e., do reflect the relative fitness coefficients of the attributes). Thus, the proposed methodology not only has the potential of allowing functional and stylistic traits to be differentiated, but also has the potential of allowing archaeologists to identify the relative influence of selective forces on traits and thereby developing expectations regarding the selective causes and rates of change in functional attributes.

DIFFERENTIATING STYLE AND FUNCTION IN METATES FROM PAQUIMÉ, CHIHUAHUA, MEXICO

The utility of the proposed method will be demonstrated through the analysis of a portion of the metates recovered from Paquimé, a large site in the Casas Grandes region of Chihuahua, Mexico. The application is divided into two sections. I begin by quantifying the amount of variation in metric attributes of the artifacts and then rank the attributes in ascending order by their corrected CVs. I then compare the rankings to performance studies of the artifact types and differentiate those attributes that are likely to be functional from those that are likely to be stylistic.

The analysis uses a portion of the data collected by Charles C. Di Peso during his investigations at Paquimé and reported in Di Peso et al. (1974:162–171).

Figure 7.5**Type 1A metates from Paquimé, Chihuahua, Mexico**

The metate collection from Paquimé comprises 238 metates and metate fragments divided into five types with numerous subtypes. However, roughly half of the assemblage (102 specimens) is composed of metates from a single subtype (Type 1A metates), while the remaining subtypes have relatively few members. Given the need for a statistically viable sample, this analysis, therefore, focuses on the Type 1A metates, which are extensively shaped trough metates with square corners and one closed end (Figure 7.5).

Because natural selection can potentially affect any aspect of an artifact class' morphology, differentiating functional and stylistic traits necessitates data that allow the variation in the general artifact morphology to be measured. Read (1982:73–74) observes, “To completely characterize a shape means to be able to recreate the shape using only those measurements [taken].”

Fortunately, Di Peso et al.'s (1974:164–171) data are therefore ideally suited for this study. They recorded 11 metric variables (Table 7.1 and Figure 7.5) and one qualitative attribute, which re-create each artifact's shape. Table 7.1 lists the summary statistics for the metric variables, and Table 7.2 provides a rank ordering of the variables by their corrected CVs.

As Table 7.2 indicates, total length, total width, total height, trough length, and trough width all have roughly the same corrected CVs, which are small in comparison to those of the other variables. Back rim thickness, trough width, and trough length are more variable, but much less variable than maximum

Table 7.1

Summary statistics of metric variables measured on metates from Paquimé, Chihuahua, Mexico by Di Peso et al. (1974)

Variable	No.	Avg.	Std.	CV	CV*
Total Length	99	46.34	5.26	11.36	11.39
Total Width	97	37.52	4.45	11.87	11.90
Total Thickness	99	19.97	3.34	16.70	16.74
Trough Length	92	41.49	4.04	9.74	9.77
Trough Width	95	29.14	3.29	11.31	11.34
Back Rim	100	5.03	1.17	23.29	23.35
Minimum Depth	95	7.43	2.66	35.76	35.85
Maximum Depth	92	13.12	4.54	34.61	34.70
Weight	80	48853.75	18208.48	37.27	37.39

trough depth, minimum trough depth, and weight. Based on the model presented earlier, total metate length, total metate width, total metate height, trough length, and trough width appear to be functional characteristics. The model further suggests that the remaining variables either are functional traits with a much lower fitness coefficient than the previously mentioned traits or are stylistic traits. Additionally, all of the Type 1A metates are composed of a macroscopically identical vitreous basalt, suggesting that the raw material of the metates may also be functional (because of the very low richness of material type used). As discussed earlier, the plausibility of these conclusions can be evaluated by comparing the rank ordering to performance studies of similarly shaped metates.

Function, Style, and the Performance Characteristics of Trough Metates

Metates are a single component of a technology designed to reduce corn and other organic and inorganic materials into smaller-sized particles, such as flour, through grinding (Adams 1993:332). Previous research has identified several important performance characteristics of metates (Adams 1993; Hard et al.1996; Horsfall 1987; Lancaster 1986; Nelson and Lippmeier 1993; Wright 1993). Primary among these are the size of the grinding surface, raw material, and the user's ability to rejuvenate the grinding surface for extended use.

Hard et al. (1996), Mauldin (1990, 1993), and Lancaster (1986) all document a relationship between the level of corn dependency of prehistoric groups and metate grinding surface area. According to their research, the size of the grinding surface impacts the amount of energy and time required to grind corn and other materials (Hard 1990:137–141; Hard et al. 1996:256). As the size of the grinding surface increases, the amount of energy required to grind for a period of time

Table 7.2

Rank ordering of metric variables of the Paquimé metates by their corrected coefficients of variation

Variable	CV*
Trough Length	9.77
Trough Width	11.34
Total Metate Length	11.39
Total Metate Width	11.90
Total Metate Thickness	16.74
Back Rim Thickness	23.35
Maximum Trough Depth	34.70
Minimum Trough Depth	35.85
Weight	37.39

increases, but the amount of time required to grind a given amount of material decreases (Adam 1993: 333–334).

Based on these findings, large trough metates, such as those found at Paquimé, and two-hand manos are the most efficient means of grinding large amounts of corn and other organics in a limited time (Mauldin 1990). The fact that this form of metate is present at Paquimé suggests that these metates were designed to process corn or other organics quickly, a finding that is consistent with the archaeological context of the metates (Di Peso et al.1974:162). 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 must be functional characteristics, a finding that is consistent with their extremely low corrected CVs.

Variation in raw material is absent, as the Paquimé metates are all composed of a macroscopically identical vesicular basalt. Several aspects of raw material affect the performance of metates (Hayden 1987:14–17; Horsfall 1987:340–347). Coarser raw materials will allow faster grinding but will not produce as fine-grained a product as will a smoother surface (Horsfall 1987:341; Nelson and Lippmeier 1993). Raw material will also affect the durability of the metates, thereby affecting both the labor costs of manufacture and the length of the use-life of the metates (Horsfall 1987:340; Nelson and Lippmeier 1993:294). Because of the labor investment in metate use and production, these characteristics are probably important performance characteristics. This conclusion is further strengthened by the fact that other sources of raw material potentially suitable for metates were not used.

During the summer of 1997, a survey project sponsored by the University of New Mexico and the Instituto Nacional de Antropología e Historia attempted to find possible sources for this basalt. Several possible sources were identified.

However, other sources of vesicular basalt macroscopically distinct from that used for the metates were much closer to Paquimé, suggesting that either the texture or the durability of the raw material is an important performance characteristic and, therefore, a functional characteristic.

The ability to rejuvenate the grinding surface directly relates to both the frequency of manufacture and the transportation costs of metates (Horsfall 1987: 341–342) and thereby impacts the labor investment and energy requirements of groundstone production. Nelson and Lippmeier (1993) have found that metates used at “permanent” settlements tend to be designed for extended use/rejuvenation when compared to metates used at “temporary” sites. As a result, they found that the morphology of metates designed for reuse differed significantly from metates designed for limited use. In particular, the metates designed for reuse are better shaped and “thicker, heavier forms” (Nelson and Lippmeier 1993:297).

The Paquimé metates are clearly designed for reuse. Di Peso et al. (1974: 163) observe that 48 of the metates show evidence of surface rejuvenation in the form of a series of grooves worn into the metate walls, representing different grinding surfaces. Additionally, the unused or slightly used metates in the sample demonstrate only a very shallow trough, indicating that the grinding surfaces were initially shallow and then deepened extensively through use. Given the thickness of the metate block (average thickness of the metates is 19.97 cm), the potential for reuse must be an important performance characteristic of the Paquimé metates. As a result, the total height of the metate should be a functional characteristic, a finding that is again in agreement with the limited variation observed in the Paquimé assemblage.

In spite of the fact that it has the greatest corrected CV of all of the variables considered here, weight is also likely a functional trait. While the high CV would normally suggest that the weight of the metates is a stylistic trait, it is possibly an important performance characteristic during one stage of the metates’ use-life: transportation to Paquimé. The specific source(s) of the raw material used in the construction of the Paquimé metates has not been securely located, but, to my knowledge, no macroscopically similar basalts are present in the area directly surrounding Paquimé. The metates or metate blanks must have been transported to the site over a considerable distance (probably greater than 15 km). The transportation of the large basalt blocks would have required a substantial investment in energy (average weight of the *used* metates is 48.9 kg). As a result, I argue that the weight of the metates is an important performance characteristic, at least during transportation, and is therefore likely a functional attribute. The amount of variation in metate weight was, therefore, probably initially limited. Because the total length, width, and thickness of the metate blocks control the weight of the metates, these attributes are also important performance characteristics, which is again consistent with their relatively low corrected CVs.

Once the metate or metate blank had been transported to Paquimé, however,

weight probably ceased to be an important performance characteristic and became stylistic. Di Peso et al.'s (1974) analysis indicates that the metates were often recovered at the location of their use, suggesting that the metates were rarely moved once the prehistoric inhabitants of Paquimé began to use them. As a result, weight was no longer a performance characteristic and no longer affected the usefulness of the metates. The variation that was once constrained by natural selection was free to increase irrespective of natural selection. Because a change in any of the other variables also caused the weight of the metates to change, the variation in weight quickly increased as the variation in stylistic variables associated with the metate's use increased. This additive variation resulted in the large amount of variation illustrated on Table 7.2. In contrast, the total width and total length of the metates were not affected by the metates' use and therefore remained relatively standardized.

The morphology of the Paquimé metates is largely a result of a compromise, then, between two selective forces operating somewhat in opposition to one another: the selection for metate blocks suitable for a large grinding surface and multiple surface rejuvenations and the selection for light metates/metate blanks that could be easily transported. As a result, the metates/metate blanks were constrained to basalt blocks that were thick, long, and wide so that troughs appropriate for two-hand *manos* could be formed, and grinding surfaces could be rejuvenated, but not overly large so that metate weight could be held at a minimum.

Back rim thickness, which exhibits greater variation than the previously mentioned variables (except for weight) but less variation than the remaining variables, either is a functional trait with an extremely low fitness coefficient relative to the other functional attributes or is stylistic. The metate performance studies cited earlier do not mention it as an important performance characteristic; however, it may be functional because a back rim that is too large will likely interfere with the efficient use of the metate. Ultimately, though, whether it is a stylistic or a functional trait with a low fitness coefficient may be moot. Because back rim thickness is constrained by the interplay between the length and width of the grinding surfaces and the limits on the size of the metate blanks caused by weight, it is, therefore, a sorted trait controlled by the action of natural selection operating on these other functional attributes (Hurt et al., Chapter 4 in this volume). 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. These attributes are stylistic, and their variation is confined by only the total thickness of the metates.

Summary of Analysis

I argue that the single qualitative attribute and six of the nine metric variables (raw material, grinding trough length, grinding trough width, total length, total width, total height, and weight) considered are affected by natural selection.

With the exception of weight, these conclusions are supported by both the limited variation predicted by the model outlined earlier (i.e., low CVs) and metate performance studies. I suggest further that weight is an example of the transformation of an attribute from being functional to being stylistic within its use-life.

Back rim width appears to be a sorted trait controlled by the interaction between the trough characteristics and the ultimate size of the metate. As a result, natural selection does not operate directly on back rim width but still constrains the amount of variation within the attribute through the process of sorting. Finally, maximum trough depth and minimum trough depth appear to be stylistic traits that are simply a product of the amount of use a metate has received.

CONCLUSIONS

The differentiation of stylistic and functional traits is the most pressing methodological problem facing evolutionary archaeology today. Until this fundamental problem is solved, Darwinian theory cannot be applied effectively to the archaeological record for the simple reason that it will remain impossible for archaeologists to determine which evolutionary processes are affecting a particular trait. The present discussion outlines one general solution to this problem.

The core of the proposed method is the premise that functional traits, regardless of their scale or specific manifestation, must affect an individual's interaction with the environment (including its social components). Functional traits must therefore be performance characteristics, by definition. In contrast, stylistic traits will not significantly impact an individual's interaction with the environment and will, therefore, not be performance characteristics.

Because functional traits are performance characteristics, the amount of variation in them will be limited. Only a portion of the possible range of variation will lead to results that are favored by natural selection. Thus, a large portion of the potential variation possible within a trait will be selected against, thereby creating a limited amount of variation within a functional trait.

In contrast, stylistic traits will be free to vary irrespective of natural selection. They can thus vary over a much greater range, at least potentially, than functional traits. While stylistic traits may show limited variation at any one time and/or place because of evolutionary processes such as drift and sorting, over time and space they should be much more variable than functional traits.

Using the proposed method, then, we can evaluate predictions based on previous knowledge of the selective environment concerning whether a specific trait is likely to be functional. We can also examine a suite of traits, determine which traits are functional, and then use this to understand further the selective environment and the changes it effected through time.

NOTE

1. Fitness coefficients should not be confused with the biological concept of selection coefficients, which involve comparing the strength of selection on the *same* trait in *different* environments. Fitness coefficients are used to compare the strength of selection on *different* traits in the *same* environment.

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Chapter 8

A Million Years of Style and Function: Regional and Temporal Variation in Acheulean Handaxes

C. David Vaughan

INTRODUCTION

The goal of this study is to investigate the extent of temporal and geographic variation in Acheulean handaxes, using an evolutionary model to understand the sources of any observed variation and change. Its significance derives from the widespread belief that the Acheulean represents a unique cultural period in human history characterized by evolutionary stasis rather than variation and change (Wynn and Tierson 1990). Acheulean stone-tool technology in general and Acheulean handaxes in particular are commonly perceived as having remained “remarkably” uniform in shape across roughly three continents and a span of approximately 1.5 million years (Klein 1989:213; Phillipson 1994:34; Tattersall et al. 1988).

This view persists despite other evidence that hominid populations during this time were growing and expanding into new geographical areas, adapting to varied environmental conditions, and developing many abilities and characteristics that earlier hominids lacked (Phillipson 1994:34). Under these circumstances the absence of variation and change in artifacts during the Acheulean would present a dramatic contrast to the rest of human history. Moreover, the causes of such homogeneity would be critically important to our understanding of human biological, cultural, and cognitive evolution (Wynn and Tierson 1990: 73). The presence of variation and change, on the other hand, would radically undermine traditional thinking about the Acheulean and open new avenues for explaining these changes within numerous theoretical frameworks, including Darwinian evolution.

There are a number of reasons for the impression of homogeneity among Acheulean handaxes. One reason for the Acheulean’s “markedly monotonous

flavor” is the lack of any clear correlation between their various shapes and the geographic regions where they have been found (Wynn and Tierson 1990:73). Another reason is that the grouping of handaxes into types for the construction of chronologies, or untested assumptions about the relevant variables, has prevented *a priori* the opportunity to know the true extent of temporal variation (Ramenofsky and Steffen 1998). As Wynn and Tierson (1990) point out, however, the simplest explanation for the persistence of the accepted view may be the fact that no studies of temporal variation in handaxes have been undertaken on an interregional scale (see, e.g., Isaac 1977; Roe 1968 for regional studies). Wynn and Tierson’s (1990) comparison of late Acheulean handaxes from Europe, East Africa, India, and the Near East was the first to examine interregional variation. However, these authors did not attempt to measure temporal variation across regions. Thus, the principal reason for the persistence of the accepted view of the Acheulean may be simply an artifact of the history of research on the Acheulean.

There are numerous potential sources of variation in Acheulean stone tool morphology. Among them are differences in raw material, activities, manufacturing techniques, cultural traditions, postdepositional factors, human morphology, sexual division of labor, drift (in the evolutionary sense), and selection. However, because evolutionary theory provides a coherent structure for answering questions about why artifacts vary and change across time and space, I use the evolutionary concepts of drift and selection to generate a model for quantifying and understanding the ultimate causes of any observed variation and change. I then apply this model to a sample of 251 Early, Middle, and Late Acheulean handaxes from Asia, Africa, and Europe. The results suggest that the relevant question does not concern the causes of uniformity in Acheulean handaxes. The important question is: What are the sources of their spatiotemporal variation and change? Having demonstrated greater variation in handaxes than has been previously recognized, I show that an evolutionary model can increase our understanding of the sources of this variation.

CONCEPTS, UNIT STRUCTURE, AND METHODS

Concepts

Evolutionary mechanisms operate on phenotypic variation, and artifacts are the “hard part” of human phenotypes (Dunnell 1989:45). The evolutionary mechanisms that operate on those attributes of artifacts that affect individual fitness are different, however, from those that operate on the attributes that do not affect fitness. Thus, evolutionary theory cannot be reliably used to elucidate the evolutionary processes underlying variation in artifacts until the attributes of artifacts that affect individual fitness and those that do not are explicitly differentiated (Allen 1996).

Functional attributes are defined as those attributes of an artifact that affect

individual fitness within a given selective environment (Dunnell 1978). “Fitness” is a measure of the extent of an individual’s reproductive success (Dawkins 1982:183). An attribute is thus considered “functional” if the benefits to individual reproduction outweigh its costs. Moreover, attributes should be *selected for* (and therefore appear and be maintained in the archaeological record) when the benefits of these attributes exceed their costs.

In contrast, *stylistic* attributes are those attributes that do not affect individual fitness (Dunnell 1978). An attribute is thus “stylistic” if there is no additional cost to individual fitness (reproduction) in possessing some alternative attribute. Thus, selection does not control the appearance or persistence of stylistic attributes. Instead, stochastic processes, the character of the transmission system, and the size of the population control stylistic attributes (Neiman 1995).

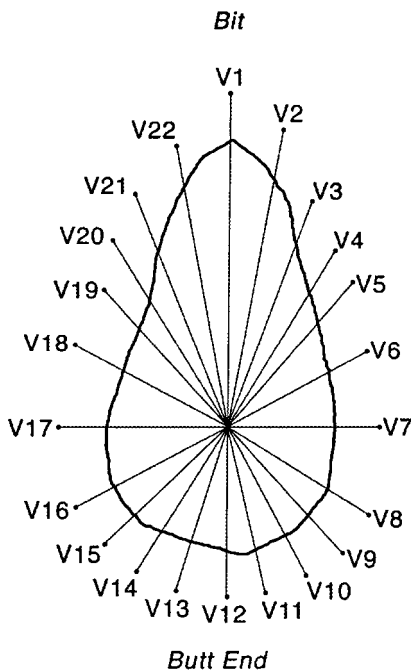
Unit Structure and Methods

An evolutionary explanation of temporal and geographic variation in Acheulean handaxes requires not only a methodology that recognizes variation as meaningful but also units of measurement that are constructed with the goal of measuring variation (Ramenofsky and Steffen 1998). This goal demands that units be conceived of as abstractions that begin with definition rather than with the data-sorting process (Ramenofsky and Steffen 1998). By creating conceptual measurement units that are not defined by reference to a previously determined spatiotemporal group of artifacts (e.g., handaxes) the measurement scales retain a constant value across time and space. This allows both variation and temporal change in empirical configurations to be described by the measurement tool (Dunnell 1986).

Wynn and Tierson’s (1990) polar coordinate technique uses a system of 22 rays emanating at various angles from the midpoint of the long axis to measure the plan shape of handaxes (Figure 8.1). This technique makes fewer assumptions about the variables relevant to measuring the morphology of handaxes than previous length, breadth, or index methods. Therefore, with certain modifications I adopted this measurement technique for this study. The resulting unit structure has two parts.

First, I defined the unit structure for measuring variation in Acheulean handaxes as the “distance in tenths of a centimeter from the centerpoint to the periphery” for each of the 22 “rays” of Wynn and Tierson’s (1990) polar coordinate system. While designed to work within the context of this study, this unit structure is conceptual, with no relationship to any specific group of handaxes. Consequently, it can be applied to all handaxes as a unit structure for measuring variation. It also has a degree of precision because the measurements are quantitative and numerical. In sum, this unit structure provides a fixed gauge against which individual variation or change in the empirical configuration of the various members of the sample can be measured.

One of the disadvantages of the polar coordinate technique, however, arises

Figure 8.1**Polar coordinate system for handaxes (adapted from Wynn and Tierson 1990)**

from arbitrariness in “centering” the artifact. Wynn and Tierson define the center as the “midpoint of the long axis” (Wynn and Tierson 1990:75). However, it is unclear how they defined the long axis. I assume they defined it as the longest *possible* vertical axis from the bit to the butt, but this is uncertain because they mention it only when discussing the measurement of Acheulean “cleavers” (Wynn and Tierson 1990:74). Uncertainty about midpoint location affects each measurement and can lead to the failure to measure the point of maximum breadth (Wynn and Tierson 1990:75). Since the width of a handaxe might be an important variable in handaxe form, I decided that it should be measured in each case.

To resolve these problems, I defined a “centerpoint” as the intersection of the longest vertical axis running from bit to butt and the longest horizontal axis running from side to side. This resulted in a consistent means of positioning the origin of the polar coordinate system, and the maximum breadth of each handaxe was always measured.

Handaxes are often thought to be bilaterally symmetrical. If so, measuring the distance from the centerpoint to identical points on the opposite edges would be unnecessary. However, not all handaxes are perfectly symmetrical, particularly early ones. Moreover, I used all 22 measurements because I could find no

mention of any convention for displaying the same “face” of each specimen when drawing or photographing handaxes for publication. When the publisher provided multiple views of a specimen, however, I always chose the image placed on the left hand side of the display.

The second part of my unit structure is an equation for the meaningful quantification of variation. For this purpose, I chose the corrected coefficient of variation (Sokal and Rohlf 1981:58–60). The corrected coefficient of variation (CCV) expresses the standard deviation as a percentage of the mean. Therefore, it corrects for differences in the absolute size of the variables being measured and accounts for the tendency of variation to be underestimated with small sample sizes. Thus, use of the CCV allows direct comparisons of the variation within distributions with differing means. It is computed using equation 8.1.¹

$$CCV = (1 + 1/4n) \times (s/\bar{Y} \times 100) \quad (8.1)$$

Use of the CCV will allow the identification of functional and stylistic traits of Acheulean handaxes by quantifying the amount of variation in the attributes of shape (variable length) within and across geographic (i.e., regional) and temporal groups. By comparing the CCVs within regional and temporal groups, those attributes likely to be functional can be distinguished from those that are stylistic. Comparisons of the behavior of these attributes across regions and times can then be used to inform the nature of the processes affecting variation and change.

AN EVOLUTIONARY MODEL

My model focuses on the dimensional attributes of individual handaxes, but the handaxes are divided into groups from particular times and regions. Thus, the dimensional attributes of individual handaxes are the units of analysis, but the attributes of handaxes across regional and temporal assemblages are the scale of analysis (Steffen et al. 1998). Therefore, this model addresses the effect of various sources of variation on the attributes of individual handaxes but draws conclusions and identifies patterns that should be recognizable at the scale of regional or temporal assemblage.

The model begins with the style–function dichotomy. This dichotomy is theoretical, mutually exclusive, and exhaustive in principle (Allen 1996). Moreover, it leads to specific expectations about how these two classes of attributes will behave through time and across space (Allen 1996:98). Because stylistic and functional traits are controlled by different evolutionary mechanisms, the model posits that selection and drift will produce different spatiotemporal patterning of the observed variation in these traits (Beck 1998; VanPool, Chapter 7 this volume). A summary of this model is presented in Table 8.1.

Table 8.1
Summary of model

Source of Variation	Expectations
Drift/Cultural transmission	Assuming recurring changes in Acheulean population structure across regions and time, stylistic attributes should display more variation than functional attributes at any given time or place. Spatiotemporal differences and changes in the amount of variation in stylistic attributes will reflect changes in the frequency of interaction, or the rate of innovation, associated with regional and interregional population dynamics.
Selection	Selection will limit the range of variation in functional attributes to those that fit within a more bounded range than expected for stylistic attributes. This bounded variation will crosscut both geography and time. Spatiotemporal differences and changes in the amount of variation are due to differences among and between geographic regions (and times) in selective environment, activities, patterns of use, or functionally related attributes.

The Temporal and Spatial Dynamics of Selection

While any particular attribute may be functional or stylistic depending on the selective environment, functional attributes within any particular selective environment will confer reproductive benefits that can be evaluated in terms of performance characteristics (Beck 1998; O'Brien et al. 1994; Maxwell 1995). Performance characteristics are attributes that an artifact must possess in order for it to fulfill its functions in a specific activity (Schiffer and Skibo 1987:599). Over the long term, evolutionary theory suggests that individuals using artifacts with performance characteristics within a range of variation that promotes both the fulfillment of the artifact's functions and a net benefit to the reproduction of that individual should come under selection. Individuals using artifacts whose performance characteristics are beyond that range should incur costs that exceed the benefits and, therefore, should be selected against. All else being equal, over the long term variation in performance characteristics of an artifact should be limited by selection to those that fit within the limited range that confers a net benefit within a given environmental context.

The spatiotemporal distribution of functional traits arises from absolute differences in fitness between one variable and another (Beck 1998). To persist archaeologically, the trait must enhance, or at least not lessen, the fitness of an individual in a new population. Because functional traits are adaptive, they should also crosscut time and space (Allen 1996:98; Dunnell 1978). In addition, spatiotemporal differences in the range of variation in functional traits should

correlate with differences in particular environments, patterns of use, activities, and other functional complexes (Allen 1996).

Thus, my model predicts that the range of variation in the functional traits of handaxes should be limited by selection to those that fit within the limited range that confers a net benefit with some given environmental context. Moreover, this relatively bounded variation should crosscut both geography and time. While remaining relatively bounded across these dimensions, however, spatio-temporal differences and changes in the amount of variation in functional attributes are explained by differences among and between geographic regions (and times) in selective environment, activities, patterns of use, or functionally related traits.

The Temporal and Spatial Dynamics of Drift and Transmission

The stylistic attributes of Acheulean handaxes should be more variable over the long term than functional traits (within the selective constraints on the artifact as a whole). Because stylistic attributes have no detectable effect on fitness, their frequency is not explicable by selection. Instead, their frequency over time is controlled by random forces such as drift. However, transmission also controls the spatiotemporal distribution of stylistic attributes (Beck 1998; Dunnell 1978; Jones et al. 1995; Neiman 1995; O'Brien and Holland 1990). While drift decreases the amount of variation, population growth results in increased innovation and interaction, and thus increased variation (Neiman 1995). Therefore, repeated changes in regional population dynamics during the Acheulean should cause repeated changes in the innovation and interaction rates. This, in turn, might result in either an increase or decrease in the amount of variation in stylistic attributes. In either event, drift would then act to reduce this variation, probably over relatively short time spans (Neiman 1995). Viewed over numerous cycles, the stylistic attributes of Acheulean handaxes should display a greater range of variation across time and space than handaxe functional attributes at any given time or place. In addition, changes in the amount of variation in stylistic attributes across space (Asia, Africa, and Europe) should reflect changes in the frequency of interaction or the rate of innovation associated with regional and interregional population dynamics.

SAMPLING AND PROCEDURES

Lacking access to a large sample of handaxes, I drew my sample from scaled drawings or photographs of handaxes obtainable from published site reports. To address the question of regional and temporal variation, I randomly chose a large, well-dated sample of pictures of handaxes from each of the temporal divisions and regions of the Old World. I ultimately measured a total of 251 artifacts from Europe, Asia, and Africa, the dates of which spanned the Upper, Middle, and Lower Acheulean periods.

Temporal control over the handaxes is probably the most serious problem with the sample. Handaxes in this sample were dated by various methods and thus with varying degrees of precision. Nevertheless, I relied on the authors' temporal assignments, while recognizing that dating problems could substantially under- or overestimate the ages of individual specimens or groups of specimens.

To obtain the desired measurements, I transferred Wynn and Tierson's polar coordinate template to a sheet of clear plastic. I then overlaid this sheet on the photograph or drawing of each handaxe, placing the centerpoint at the intersection of the widest and longest dimensions of the handaxe. I next recorded the distance in tenths of a centimeter along each of the rays, measuring from the centerpoint to the termination of the ray at the edge of the handaxe. I then standardized each measurement by multiplying it by some factor based on the scale of the drawing or photograph. The CCVs were calculated on these standardized measurements.

RESULTS

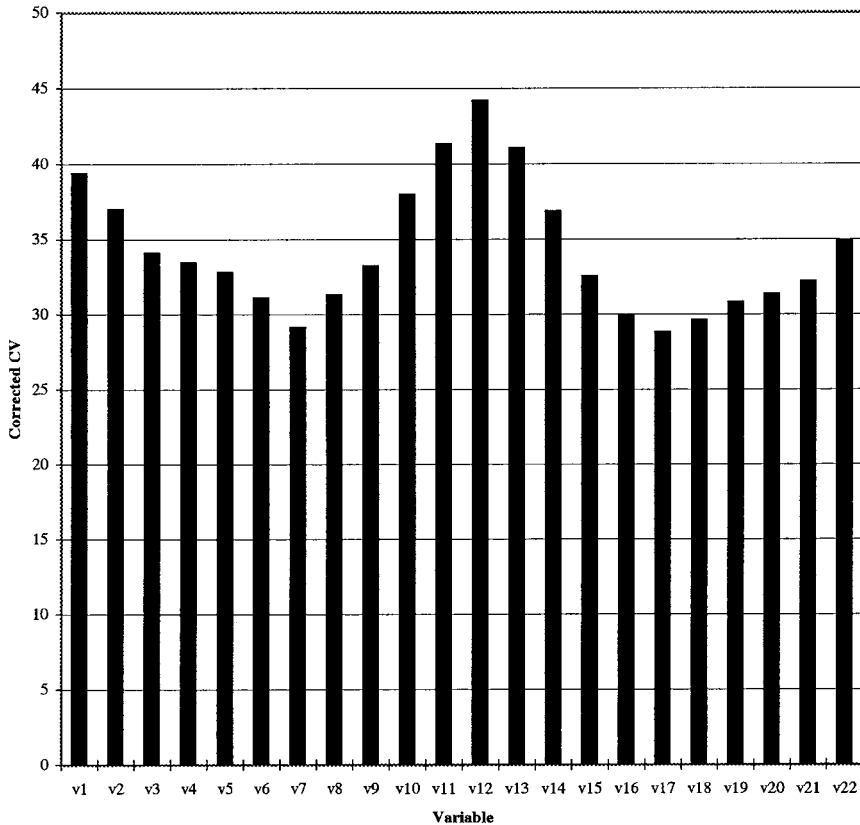
Figure 8.2 shows the values of the CCVs on each of the 22 dimensional variables of all 251 handaxes. The highest value—and thus the greatest amount of variation among the 22 dimensions—is the distance from the centerpoint to the middle of the butt (variable 12). Variables 11 and 13, which define the shape of the butt, have the next highest values. The CCVs of these variables suggest relatively greater variability in the morphology of the butt than in other attributes, for example, width. Specifically, they suggest variation in the butt ranging from rounded to flat.

The next highest value corresponds to variable 1, the distance from the centerpoint to the bit. The CCVs on variables 2 and 22 are also relatively large and suggest that the morphology of the bit varies substantially from pointed to more round.

In contrast, the smallest CCVs are those measuring variation in maximum width (variables 7 and 17). They indicate that the width of handaxes is considerably less variable than any of the other variables describing handaxe morphology. The dimensions that describe the morphology of the two sides or edges (variables 6, 8, 16, and 18) are also less variable than the other dimensions. This indicates an incremental curving of the sides away from the points defining maximum width, toward both the bit and butt.

In terms of handaxe form, when Figure 8.2 is viewed as a whole, it reveals a gradual, steplike reduction in the value of the CCVs as measurement proceeds from either the bit to the maximum width or from the midpoint of the butt to the maximum width. This pattern may reflect a certain degree of uniformity in the shape of handaxes, which can be described as an almondlike shape.

Figure 8.3 compares the CCVs on the sample sorted by region. It is immediately apparent that the overall steplike reduction in variation persists as meas-

Figure 8.2**Corrected coefficients of variation on 22 variables of total handaxe sample**

urement proceeds around the bit or butt toward the edges, regardless of geographic region. Again, this pattern appears to reflect the previously recognized uniformity in the overall morphology of handaxes across regions. That is, there is no apparent correlation between particular handaxe shapes and geographic regions (Wynn and Tierson 1990).

Upon closer examination, however, differences in the nature and amount of variation become apparent. For example, the amount of variation in the dimensions defining the shape of the butt (variables 11, 12, and 13) of Asian handaxes is greater than that of European ones, which is, in turn, greater than that of African handaxes. In other words, the morphology of the butt of Asian handaxes tends to vary considerably more than that of handaxes from the other geographic regions. Asian handaxes also display greater variation in dimensions defining the morphology of the bit (variables 1, 2, and 22) than either European or African handaxes, but European and African handaxes are quite similar in this regard.

Figure 8.3
Corrected coefficients of variation on 22 variables of African (top), Asian (middle), and European (bottom) handaxes

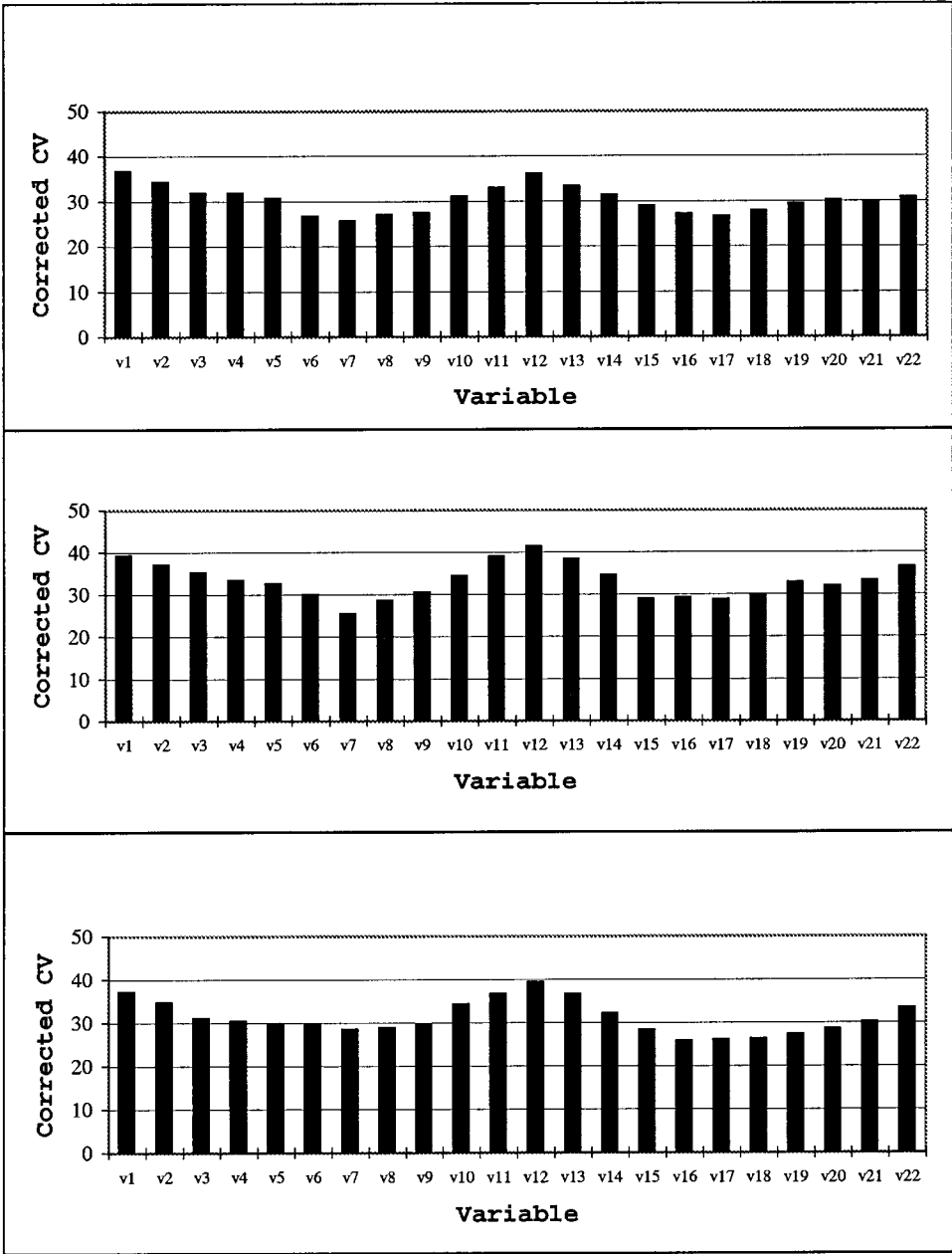


Figure 8.4 sorts the corrected coefficients of variation on African handaxes by temporal period. Again, the overall pattern persists, but differences between the Early, Middle, and Late Acheulean become evident. For example, the amount of variation in all dimensions increases from the Early to Middle Acheulean but then decreases again in the Late Acheulean. Notably, the amount of variation in the dimensions defining the morphologies of the butt and the lateral edges are noticeably greater in the Middle Acheulean than in either the Early or Late Acheulean. In other words, the morphology of the butt, the maximum width and the extent to which handaxes became more or less constricted at points away from maximum width are notably more variable during the African Middle Acheulean than at any other time. These results can be understood as indicating the presence of more different handaxe shapes during the Middle Acheulean. During this time, handaxe morphology ranges from round to pointed. During the Early and Later Acheulean in Africa, however, handaxe morphology appears to have been substantially less variable.

Figure 8.5 and Table 8.2 present the results necessary to attain the goals of this research, that is, to compare temporal variation in and across each of the three regions. Figure 8.5 presents the results graphically, while Table 8.2 presents them numerically; both demonstrate that there are regional differences in the amount of variation at any given period as well as differences in the direction of change across time.

The amount of variation in European handaxes increases from the Early Acheulean through the Late Acheulean. In contrast, the amount of variation in the shape of Asian handaxes decreases substantially from the Early to Middle Acheulean, followed by extreme variation during the Late Acheulean. African handaxes, however, show the opposite trend. Variation increases from the Early to Middle Acheulean but decreases in the Late Acheulean.

These graphs also reveal differences in the amount of variation among geographic regions during the same temporal period. For example, the relatively small amount of overall variation during the Middle Acheulean of Asia contrasts with much greater variation in the Middle Acheulean of both Europe and Africa. Moreover, the amount of variation in Late Acheulean handaxes from Asia is much greater than that in Late Acheulean handaxes from either Europe or Africa.

EVALUATING UNITS OF MEASUREMENT

Further analyses of these results generally show that differences between the CCVs do not reflect differences in handaxe size. In addition, the temporal differences and change in the CCVs are statistically significant (Tables 8.3 and 8.4).

Statistically significant differences in the mean lengths of the variables across temporal groups from a particular geographic region might suggest significant differences across time in the average *size* of handaxes from that geographic region. We might conclude from this that differences among the CCVs reflect

Figure 8.4
Corrected coefficients of variation on 22 variables of Early (top), Middle (middle), and Late (bottom) handaxes from Africa

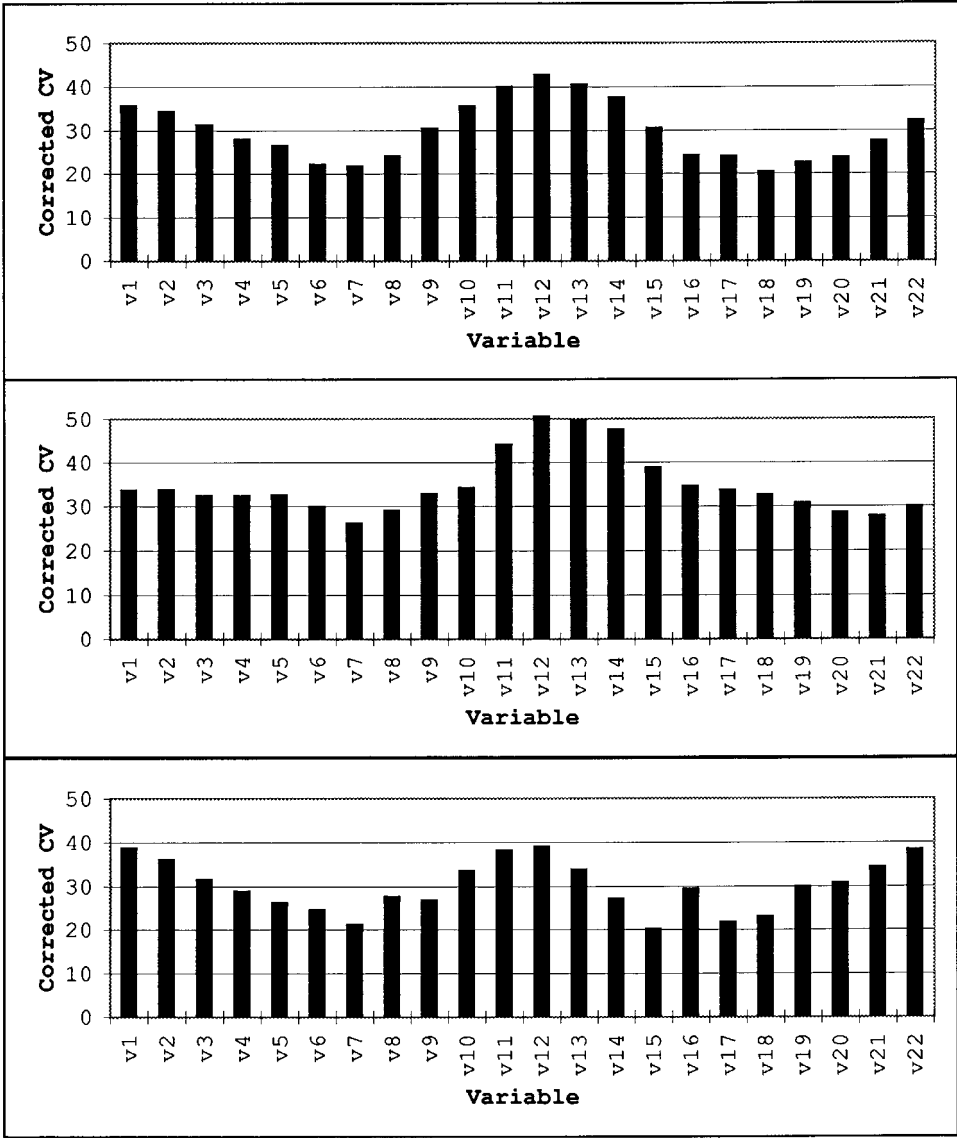


Figure 8.5
Comparison of corrected coefficients of variation on 22 variables of Early, Middle, and Late Acheulean handaxes from Europe (top), Asia (middle), and Africa (bottom)

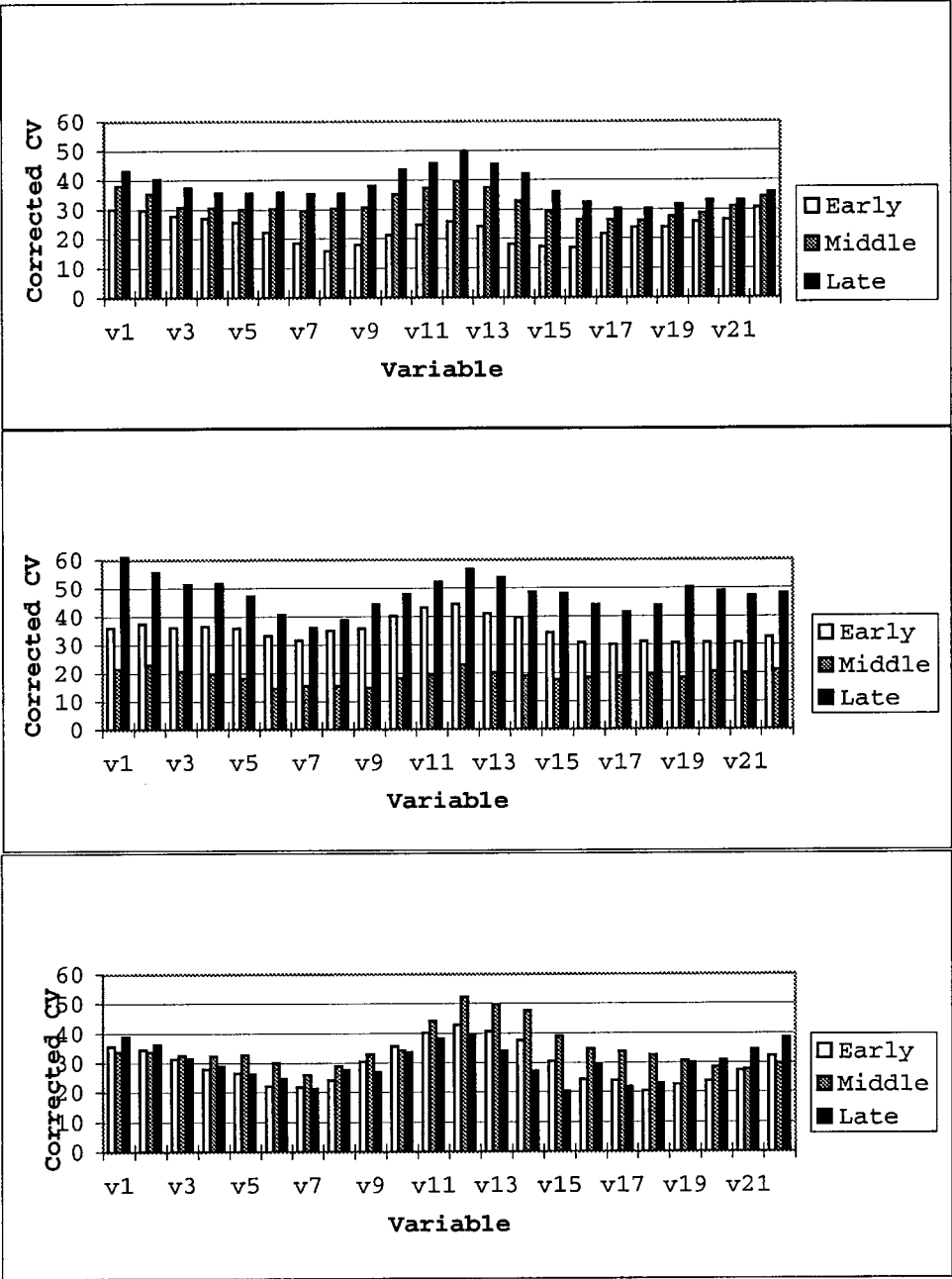


Table 8.2

Corrected coefficients of variation on 22 variables of Early, Middle, and Late Acheulean handaxes from Europe, Asia, and Africa

[illegible]

Europe	v14	v15	v16	v17	v18	v19	v20	v21	v22			
Early	18.13	17.26	16.88	21.63	23.81	23.78	25.8	26.39	30.53			
Middle	32.79	29.2	26.46	26.41	26.15	27.47	28.64	31	34.31			
Late	42.05	35.97	32.42	30.18	30.26	31.7	32.98	33.05	35.78			
Asia	v14	v15	v16	v17	v18	v19	v20	v21	v22			
Early	39.7	34.24	30.76	29.95	31.09	30.56	30.77	30.8	32.68			
Middle	19.14	17.63	18.4	18.94	19.25	18.19	20.49	19.92	21.15			
Late	48.56	48.01	44.06	41.43	43.77	50.24	48.95	47.14	48			
Africa	v14	v15	v16	v17	v18	v19	v20	v21	v22			
Early	37.58	30.49	24.28	23.98	20.4	22.62	23.82	27.49	32.28			
Middle	47.59	38.88	34.56	33.71	32.51	30.74	28.59	27.72	29.91			
Late	27.24	20.16	29.46	21.69	23.04	29.93	30.88	34.47	38.4			

Table 8.3
Results of Bartlett's test¹

Region	Variable	Var @ t1	Var @ t2	Var @t3	n1	n2	n3	v	Pooled Var	Bobs	*
Region 1	v1	5.718746	8.653756	11.93238	27	86	28	1.013116	8.742253	7.956922	
Region 1	v4	1.591396	2.041926	2.660529	27	86	28	1.013116	2.078075	3.996129	*
Region 1	v7	0.429231	1.130156	1.497725	27	86	28	1.013116	1.070013	22.78088	
Region 1	v10	0.773276	1.89583	2.984061	27	86	28	1.013116	1.897249	25.39482	
Region 1	v12	1.482336	2.689926	4.769669	27	86	28	1.013116	2.869315	20.31749	
Region 1	v15	0.449516	1.24022	1.947196	27	86	28	1.013116	1.229568	29.58913	
Region 1	v17	0.61584	0.902068	1.076336	27	86	28	1.013116	0.882237	4.745663	*
Region 1	v20	1.618558	1.761822	2.18078	27	86	28	1.013116	1.8168	1.558561	*
Region 2	v1	9.5961	6.06166	40.16255	25	23	11	1.026696	13.66586	34.96777	
Region 2	v4	3.945	1.766008	6.626909	25	23	11	1.026696	3.56788	15.40657	
Region 2	v7	1.756433	0.598972	1.998	25	23	11	1.026696	1.344853	16.66083	
Region 2	v10	4.948233	1.258142	5.843636	25	23	11	1.026696	3.622734	25.4612	
Region 2	v12	7.84	2.638972	9.110727	25	23	11	1.026696	6.023655	17.25866	
Region 2	v15	3.07	0.938933	5.51	25	23	11	1.026696	2.668509	27.73719	
Region 2	v17	1.5909	0.811739	3.072182	25	23	11	1.026696	1.549316	15.11671	
Region 2	v20	2.634767	1.7617	6.56727	25	23	11	1.026696	2.994009	15.43298	
Region 3	v1	10.97082	10.65218	6.954842	20	11	20	1.0303	9.314779	2.459686	*
Region 3	v4	2.107658	5.502909	1.591474	20	11	20	1.0303	2.610679	13.44294	
Region 3	v7	0.806211	9.136182	0.561158	20	11	20	1.0303	2.444621	80.14433	
Region 3	v10	2.155658	2.154727	1.856842	20	11	20	1.0303	2.037183	0.281922	*
Region 3	v12	3.358	3.862182	2.689763	20	11	20	1.0303	3.198527	1.076614	*
Region 3	v15	1.739579	.534545	0.598184	20	11	20	1.0303	1.453395	17.63901	
Region 3	v17	1.020289	2.757636	0.562105	20	11	20	1.0303	1.200872	20.57834	
Region 3	v20	1.689342	4.592727	1.731868	20	11	20	1.0303	2.311047	10.20992	

¹* = accept Ho: Var t1 = Var t2 = Var t3 (alpha = 0.05: Xsq.crit = 5.99)

Table 8.4
Summary of results of one-way analyses of variance¹

Europe	<i>P</i> -value	<i>P</i> -value	Africa	<i>P</i> -value	<i>P</i> -value	Asia	<i>P</i> -value	<i>P</i> -value
Variable	ANOVA	KW	Variable	ANOVA*	KW	Variable	ANOVA	KW
v1	.76	.47	v1	.05/.06	.001	v1	.04	.04
v4	.84	.55	v4	.03/.03	.005	v4	.10	.14
v7	.89	.66	v7	.02/.02	.004	v7	.07	.12
v10	.57	.21	v10	.25/.24	.07	v10	.91	.74
v12	.26	.11	v12	.18/.17	.06	v12	.97	.81
v15	.85	.69	v15	.62/.57	.30	v15	.37	.35
v17	.70	.77	v17	.37/.38	.17	v17	.05	.09
v20	.28	.28	v20	.05/.05	.02	v20	.02	.02

¹An asterisk indicates the second *p*-value is with observations 165 and 168 omitted.

differences in handaxe size rather than differences in the amount of variation. On the other hand, no statistically significant differences in the mean population lengths of the variables across the temporal groups would suggest that the mean size of handaxes from that geographic region can be considered the same. This would further suggest that observed differences in the CCVs across temporal groups reflect differences in the amount of variation within these temporal groups, rather than differences in mean handaxe size.

Significant inequalities in the variances among temporal groups of handaxes would support the conclusion that the observed differences in the CCVs are significantly different. While not a “direct” statistical comparison of differences in the CCVs, a formal test of the differences in variances is nonetheless useful once potential concerns about differences in size are eliminated (Sokal and Rohlf 1981:59). In short, a direct statistical comparison of variances can substitute for a direct comparison of the CCVs.

Preliminary analysis of these data suggested that the assumption of a normal distribution that underlies the standard one-way analysis of variance (ANOVA) test might not be satisfied in all cases. However, the graphical summaries (boxplots, stem and leaf displays, and normal probability plots) generally showed that the distributions of measurements within temporal groups were essentially normal. Side-by-side boxplots and interquartile range comparisons showed that population variances across temporal groups were also generally equal, with only a few exceptions. Thus, to compare the typical values for each variable (i.e., to investigate differences in handaxe size), I chose both the standard ANOVA test and the nonparametric Kruskal-Wallis (KW) test. I compared the results of the ANOVA to those from the KW test in order to evaluate the conclusions of each. I chose Bartlett’s method as an appropriate technique for direct comparison of variances.

I selected eight variables (1, 4, 7, 10, 12, 15, 17, and 20) for purposes of these analyses. Analyses of all 22 variables would generate an unmanageable amount of data, and these eight variables represent measurement points around the entire handaxe. I grouped the measurements of these sample variables by geographic region and time and made comparisons across times within each geographic region. Table 8.3 shows the number of handaxes contained in each group in the columns labeled “n1,” “n2,” and “n3.”

To determine whether the CCVs were measuring differences in handaxe size rather than differences in the amount of variance, I constructed the following null hypothesis:

Parameters:

Alpha level = .05

μ_1 = mean population length of variable v_1 at time t_1 in region r_1 .

μ_2 = mean population length of variable v_1 at time t_2 in region r_1 .

μ_3 = mean population length of variable v_1 at time t_3 in region r_1 .

$H_0: \mu_1 = \mu_2 = \mu_3$

H_a : not H_0 .

I used this same hypothesis structure to test differences in the mean lengths of each variable ($v1$, $v4$, $v10$, $v12$, $v15$, $v17$, $v20$) across the Early, Middle, and Late Acheulean ($t1$, $t2$, and $t3$ respectively), in each of the three geographic regions of Europe ($r1$), Africa ($r2$), and Asia ($r3$).

Table 8.4 summarizes the results of the one-way ANOVA. In general, the P -values are extremely high, indicating that the null hypothesis of no differences in population mean length cannot be rejected. The exceptions are variables 1, 4, 7, and 20 in Africa and variables 1, 17, and 20 in Asia. The omission of observations 165 and 168 in Africa (which appeared as outliers on the graphical summaries) increases the P -value on variable 1 to .06. This suggests that these outliers were affecting the mean and that the null hypothesis cannot be rejected when they are eliminated from the analysis. Thus, the null hypothesis of no differences in the population mean lengths of these variables was rejected in only 6 of the 24 cases tested.

The Kruskal-Wallis (KW) test is a nonparametric method for testing the hypothesis of equal population medians against the alternative that not all population median lengths of the measured variables are equal. Because it is essentially an ANOVA on ranked data, it eliminates the influence of extreme variants. It is also relatively insensitive to unequal variances. Thus, it is an ideal method for evaluating the results of the standard ANOVA test discussed earlier.

Table 8.4 shows the results of the KW test along with the results of the standard ANOVA test. They strongly support the results of the ANOVA test. The null hypothesis of equal population median lengths for each variable across the Early, Middle, and Late Acheulean in Europe, Asia, and Africa cannot be rejected in all but six of the same seven instances where it was rejected by the standard ANOVA procedure. The exception is variable 17 in Asia, where the P -value of the KW test is .09. In this single instance, the KW analysis results in not rejecting the null hypothesis, while the standard ANOVA test suggests differences in the population mean lengths of variable 17 in Asia. Again, the null hypothesis was rejected in only 6 of the 24 cases examined.

The hypothesis structure for applying Bartlett's method is similar to that used in the ANOVA and KW tests. The hypothesis structure is:

Alpha level = .05

Ho: variance $s1$ = variance $s2$ = variance $s3$

Ha: variance $s1$ does not equal variance $s2$ does not equal variance $s3$,

where variance $s1$ equals the variance in the group of length variables $v1$ at $t1$ (Early Acheulean) in $r1$ (Europe); $s2$ equals the variance in the group of variables $v1$ at $t2$ (Middle Acheulean) in $r1$; and $s3$ equals the variance in $v1$ at $t3$ (Late Acheulean) in $r1$. Again, this same hypothesis structure was used to test differences in the variances of each temporal group of variables ($v1$, $v4$, $v10$, $v12$, $v15$, $v17$, $v20$) across the three time periods ($t1$, $t2$, $t3$) in each of the three geographic regions ($r1$, $r2$, $r3$).

Large values of the Bartlett's statistic ("*Bobs*") suggest that population variances are unequal. For a size .05 test, the null hypothesis is rejected when *Bobs* $\geq$ the upper .05 percentile for the chi-squared probability distribution. For two groups ($k-1$) and $\alpha = .05$, the upper tail value for the chi-squared distribution is 5.99.

Table 8.3 shows the results of the Bartlett's test. In general, the null hypothesis of equal variances should be rejected. The six exceptions (i.e., where *Bobs* $\leq$ 5.99) are shown by an asterisk in the last column of the table. These correspond to variables 4, 15, and 17 in *r1* (Europe) and variables 1, 10, and 12 in *r3* (Asia). Interestingly, only 6 of the 24 cases show statistically significant equal variances when groups of variables are compared across time in a particular geographic region.

In summary, in 75 percent of the cases analyzed, the standard ANOVA and KW one-way ANOVA detected no statistically significant differences in the mean population lengths of the variables selected. Thus, I conclude that the CCVs calculated on these variables generally did not measure differences in the size or magnitude of the variants related to handaxe size. As they should, the CCVs measured the amount of variation present within the temporal groupings of length variables. Also, in 75 percent of the cases analyzed, Bartlett's test showed unequal variances in these groups across time within regions. This indicates statistically significant spatiotemporal differences in the CCVs calculated on these length variables.

DISCUSSION AND CONCLUSIONS

Contrary to the generally accepted view of persistent homogeneity across time and space, there exist considerable spatiotemporal differences and changes in the amount of variation in Acheulean handaxes. Thus, the relevant question is *not* the causes of uniformity in these handaxes but rather how to account for variation and change.

As measured by the CCVs, these analyses suggest that the functional attributes of handaxes are their maximum width and the relationship between the lateral edges and the width. Functional attributes should have less variation than stylistic attributes, and the amount of variation in the variables associated with these attributes is *always* less than that of any other attributes, regardless of time or place. One explanation for this pattern is that handaxe width (and the morphology of the lateral edges) had an impact on the fitness of Acheulean individuals manufacturing and using these handaxes. Selective pressures over the long term favored individuals using handaxes within the performance-limited range of variation described by the relatively smaller CCVs for these attributes.

While remaining less variable than other traits at all times and places, the amount of variation in these functional attributes nonetheless differs between geographic regions and changes across time within these regions. These spatiotemporal changes and differences appear to reflect difference and change in

selective environment, activities, patterns of use, or in the functionally related traits of these handaxes.

Other handaxe attributes, such as the length and shape of the bit and butt, appear to behave as predicted for stylistic attributes. When compared to variability in the attributes of width, there is relatively greater variation in these attributes across all times and geographic regions. Moreover, different patterns of increasing and decreasing variation through time are apparent in each of the three regions sampled. These results suggest interesting demographic changes across these regions during the Acheulean cultural period.

The continuous increase in the amount of variation in European handaxe morphology through the Early to Late Acheulean suggests either a broadly continuous migrational flow of groups into the region throughout the Acheulean or perhaps increased interaction among existing populations. In contrast, the dramatic decrease in the amount of morphological variation in Asian handaxe bits and/or butts during the Middle Acheulean suggests the possibility of large-scale Asian emigration. During the Late Acheulean in Asia, however, large-scale return of populations into the area or reestablishment of associations among existing populations is indicated.

The data from Africa suggest a third pattern of changes in population structure. The apparent increasing variation in the butts and bits from the Early to Middle Acheulean suggests either population movement back into Africa during the Middle Acheulean or increased interaction among African populations, at least relative to the Early Acheulean. The Late Acheulean, however, sees a return to population or interaction frequencies similar to those during the Early Acheulean.

Finally, the gradual, steplike reduction in the value of the CCVs as measurements proceed from bit to the maximum width (or from the midpoint of the butt to the maximum width) suggests a linkage in the sorting of all dimensions at the scale of the whole artifact. It is possible that there is sorting on handaxe width. The nature of this linkage is beyond the scope of this chapter, but may be related to the mechanical constraints of handaxe technology (see Hurt et al., Chapter 4 in this volume).

In conclusion, I suggest that there is considerably more spatiotemporal variation in Acheulean handaxes than has been previously recognized. This variation indicates that the Acheulean was not a period of evolutionary stasis but rather another cultural stage characterized by variation and change. This view places the Acheulean more in line with the rest of human history, and at the very least opens new avenues for understanding human cultural, biological, and cognitive evolution during this remarkable period in our long evolutionary history.

NOTE

1. In this formula, n represents the number of handaxes within a given group, $\bar{Y}$ represents the mean of all measurements of a given variable of that group (e.g., vl), and s is the standard deviation of that group of measurements.

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Chapter 9

Implications of New Studies of Hawaiian Fishhook Variability for Our Understanding of Polynesian Settlement History

Michael T. Pfeffer

INTRODUCTION

Much of our understanding of East Polynesian prehistory has been based on analyses of artifact similarities (Bellwood 1979; Kirch 1984, 1985, 1986). Fishhook similarities in particular have been widely used by researchers trying to reconstruct East Polynesian settlement history (Burrows 1938; Davidson 1971; Emory et al. 1959; Sinoto 1962, 1966, 1967, 1968, 1991). However, researchers have also used fishhook similarities and differences to explore how populations have become adapted to varying environments (Crain 1966; Johannes 1981; Kirch 1980, 1982; Nordhoff 1930; Reinman 1967, 1970). The disparity between these two approaches hints at a widespread, but often ignored, problem. In short, researchers have simply assumed that certain fishhook attribute similarities are diagnostic of cultural affinities and chronological change, while others are indicative of adaptive response.

In the former case, researchers assume that these similarities are *homologous* (or due to cultural descent), while in the latter, they recognize that these similarities may be *analogously* related (or due to convergent evolution) (Lyman et al. 1997; Sober 1984; see also Lyman, Chapter 5 in this volume). Unfortunately, few researchers have attempted to evaluate empirically, or theoretically justify the validity of, either assumption (but see Allen 1996; Crain 1966). This calls into question conclusions concerning our understanding of East Polynesian population interaction, chronological change, and adaptive response and points to the need for a systematic and scientific reassessment of Polynesian fishhooks.

The need for a new appraisal of fishhook variability was amply demonstrated by Allen's (1992) study of Southern Cook Island fishhooks and with her call for a systematic, scientific reanalysis of Polynesian fishhook variability (Allen

1996). Allen's research has identified a range of potential problems with previous efforts to identify stylistic and functional variability and has established a scientific protocol to guide future fishhook research. Drawing on her analyses, several researchers have begun the process of implementing her research program and have focused on Hawaiian fishhooks as an appropriate case study (Moniz-Nakamura et al. 1995; Pfeffer 1998, 1999, 2000).

Moniz-Nakamura and colleagues' research has highlighted theoretical and methodological problems with past efforts to understand the evolutionary processes governing the spatial and temporal distribution of fishhooks in Hawaiian prehistory (e.g., Emory et al. 1959; Sinoto 1968). Pfeffer (1998, 2000) has expanded on Allen's (1996) research and has proposed a more detailed program to systematically analyze fishhook variability. Preliminary findings suggest that these analyses will significantly alter our understanding of Hawaiian fishhook variability and may have profound implications for our understanding of East Polynesian settlement history (Pfeffer 1999).

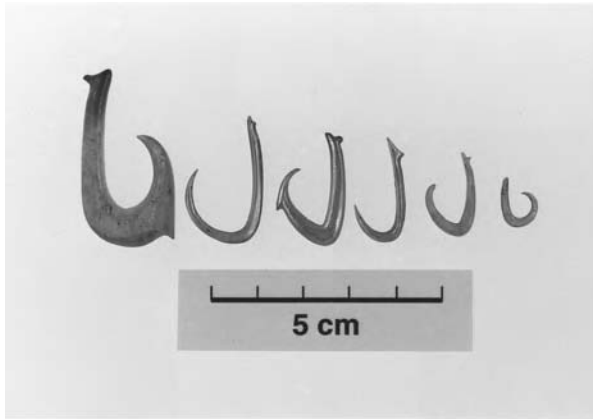
One aspect of Hawaiian fishhook variability that has intrigued researchers has been the apparent preference for certain raw materials in fishhook manufacture (e.g., Emory et al., 1959; Goto 1986; Kirch 1980:116–117, 1985). In particular, researchers have been interested in understanding the high frequency of pearl oyster shell fishhooks at some Hawaiian localities. Researchers have also been intrigued by the development of the composite, or “two-piece” fishhook in Hawai'i and have proposed that development of this technology is tied to material limitations inherent in mammal bone (Kirch 1985:200–201). To date, however, almost no empirical research has been done to evaluate these claims (but see Goto 1986). This is unfortunate because understanding why certain raw materials were chosen over others is basic to any scientific study of fishhook variability.

To begin tackling the issue of raw material use, we must first compare observed variability in fishhook raw materials to measures of raw material natural abundance. Using this information, we can ask several basic questions. First, can we account for observed variability in all fishhook raw materials because of raw material abundance alone? Second, do patterns of raw material abundance account for observed raw material variability within different fishhook classes? If raw material abundance cannot account for observed patterns of variability, then how do we explain the apparent preference of one raw material over another?

Answering these questions will establish a framework within which we can begin to isolate and evaluate variability in fishhook raw material properties, form, cost, and performance in ancient Hawai'i (Pfeffer 2000). These variables will provide the necessary measures to identify the transmission processes responsible for the distribution of a given trait through time and across space. In other words, these measures will enable researchers to identify traits under selection and those traits whose distributions are governed by stochastic processes within a given selective environment (Brandon 1990; Dunnell 1978). This, in

Figure 9.1

Pearl shell and mammal bone one-piece fishhooks from several Hawaiian assemblages



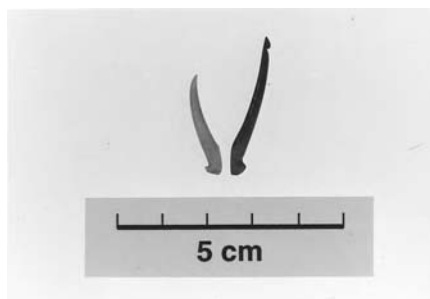
turn, will enable researchers to begin identifying those fishhook attributes that are useful as homologous markers and those that are likely to be analogously related.

FISHHOOKS

Fishhooks, while relatively simple, are more complex than has generally been appreciated by archaeologists (Pfeffer 2000). Typically, researchers have divided fishhooks into two broad classes: simple or “one-piece” fishhooks; and composite, or “two-piece” fishhooks (Figures 9.1 and 9.2). In addition, researchers also commonly divide composite fishhooks into those used for angling and those used for trolling (Goto 1986). One- and two-piece fishhooks are also commonly separated into “J-shaped” or “jabbing” hooks and “circle-shaped” or “rotating” hooks, respectively (e.g., Crain 1966; Johannes 1981; Reinman 1967). The latter description refers to variability in a given fishhook’s overall shape, although certain functional assumptions have also been incorporated into the rotating/jabbing dichotomy. Here, analyses are restricted to an examination of one- and two-piece fishhook raw material variability.

Fishhook Use

Fishhooks accomplish the task of procuring prey from an aquatic medium through the articulation of three related, but distinct, tasks, *prey-attraction*, *prey-retention*, and *prey-retrieval* (see also Crain 1966; Johannes 1981; Pfeffer 1998, 2000; Reinman 1967, 1970). Prey-attraction requires that a fishhook or fishing lure entice prey to attack the fishhook. This is typically accomplished through

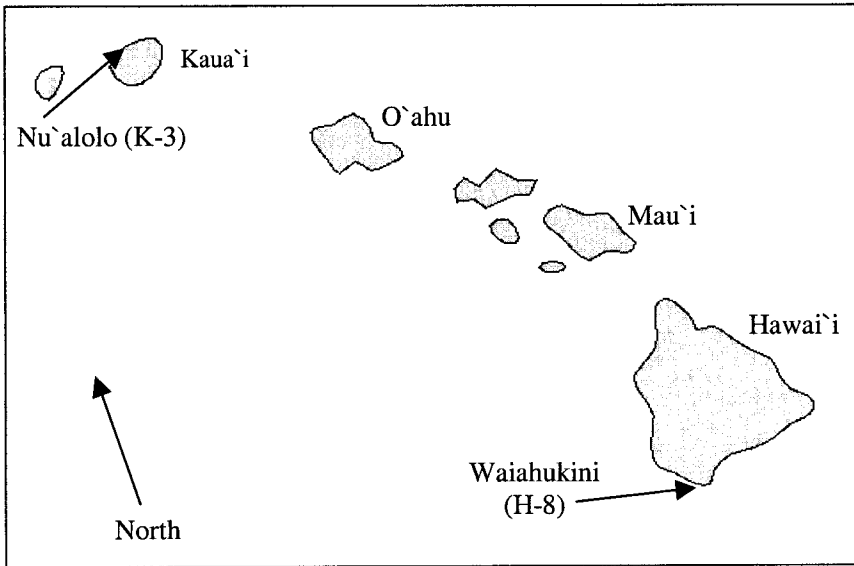
Figure 9.2**Two-piece fishhook point and shank limbs from Hawai'i**

the addition of color, a bit of food to act as “bait,” and/or the addition of design elements such as tassels. Prey-retention requires that the prey, once hooked, will remain on the fishhook until the angler can secure it. Prey-retention is primarily a function of hook geometry. Different hook designs require varying degrees of angler or prey interaction to maintain contact with the prey. Prey-retrieval requires that the fishhook and related angling apparatus be strong enough to withstand the stresses and strains imposed when an angler attempts to secure prey from an aquatic medium. Prey-retrieval is primarily a function of the interaction of raw material physical properties and hook design. Each attribute relating to these three tasks must integrate with attributes relating to the other two tasks for a given fishhook to work properly. In other words, an attribute that enhances one task should not have a severely adverse impact upon another.

Research Program

To begin distinguishing stylistic, functional, and pleiotropic or sorted (Hurt et al., Chapter 4 in this volume; Sober 1984) attributes, we must first be able to explain variation in attribute form and raw material properties and how each attribute or combination of attributes interacts with a given set of external conditions (Dunnell 1978; Meltzer 1981; Pfeffer, 2000). In other words, detailed and systematic experiments are required to evaluate fully how different fishhook attributes and attribute states impact a fishhook’s ability to attract, retain, and retrieve prey.

Mechanical studies are needed to identify potential performance differences in fishhook design, size, and raw material properties and to understand how these factors contribute to a fishhook’s potential failure through use (Anderson 1995; Cotterell and Kamminga 1990; Gere and Timoshenko 1997; O’Brien and Holland 1990). Raw material studies are needed to identify variability in a given raw material’s structure and ability to withstand the application of stress and strain along different dimensions. Acquisition and manufacturing cost analyses

Figure 9.3**Map of Hawaiian Islands showing K-3 and H-8 localities**

are needed to identify variability in the amount of energy needed to obtain fishhook raw materials and fashion them into usable hooks. Taken together, these analyses enable researchers to understand how fishhooks operate, how well they perform, and why they fail or break under specified conditions.

ARCHAEOLOGICAL ASSEMBLAGES

With an understanding of the kinds of analyses needed to evaluate fishhook variability, we can briefly consider two Hawaiian localities from which major fishhook assemblages were recovered (Emory et al. 1969; Soehren and Kikuchi 1965; Figure 9.3). The two assemblages are geographically separated and lie adjacent to very different marine habitats but overlap significantly in time of occupation and duration of use. Each assemblage contains a large number of fishhooks and related gear and was excavated under relatively controlled conditions during the late 1950s and 1960s. In addition, both assemblages have been used in attempts to reconstruct Hawaiian and Polynesian prehistory. These factors, among others, make these two assemblages ideal for asking questions about the evolutionary history of Hawaiian fishhook variability.

Nu'alolo (K-3)

Kaua'i, the oldest of the main Hawaiian Islands, has the best-developed near-shore reefs and reef slopes in Hawai'i (Goto 1986; Kirch 1985:30). The Nu'alolo

valley is located along the abruptly sloping northwest shore of the island and is adjacent to shallow near-shore reef and reef slope conditions. Abundant reef species, including wrasses (*Labridae*), parrot fish (*Scaridae*), goat fish (*Mullidae*), and surgeon fish (*Acanthuridae*), inhabit the reefs and slopes, as do predatory jacks (*Carangidae*) and snappers (*Lutjanidae*) (Goto 1986; Kirch 1985; Randall 1985).

A large habitation area was first excavated near the shoreline at Nu`alolo in 1958, and excavations continued through 1964 (Kirch 1985; Soehren and Kikuchi 1965). These excavations were known as “Nu`alolo” or “Nu`alolo Kai” and were given the site numbers “K-3” and “K-5,” although they are often lumped together as “K-3” (“kai” meaning sea in Hawaiian [Pukui and Elbert 1973:107] and referring to the locality’s proximity to the shore). Additional excavations were carried out in 1990 by several University of Hawai`i personnel, although the results of these recent excavations have not been published. Recent efforts to redate the Nu`alolo material have identified three discrete chronological units, which span a period between A.D. 1450 and A.D. 1750 (Moniz-Nakamura et al. 1995). For the present analyses, I have examined 699 whole fishhooks, fishhook fragments, fishhook blanks, hook-manufacturing debitage, and related faunal remains (such as cut mammal bone and pearl shell fragments) from the Nu`alolo assemblage.

Waiahukini (H-8)

Hawai`i Island is the youngest and largest of the main Hawaiian Islands and is located at the southern tip of the archipelago (Goto 1986; Kirch 1985; Macdonald and Abbott 1970; Morgan 1996). There is almost no near-shore reef development along Hawai`i Island’s volcanically active south shore, and near-shore waters slope quickly into benthic and pelagic depths. Shallow-water fish are abundant, but pelagic and benthic species such as tuna (*Scombridae*), mahimahi (*Coryphaenidae*), and marlin (*Istiophoridae*) occur within meters of the shore as well.

Located near the southern tip of the island (South Point), the H-8 locality is a lava tube shelter adjacent to Waiahukini Village. A number of important archaeological localities were excavated in this area, beginning late in 1953 and continuing through 1959 (Emory et al. 1959, 1969; Kelly 1969). The H-8 locality was first excavated in 1954 by Emory, Bonk, and Sinoto, although digging was suspended after one field season and did not begin again until 1956 (Emory et al. 1969:2–3). In 1956 Bonk continued excavating at the shelter with University of Hawai`i-Hilo students and excavated until 1958, when excavations were finally suspended.

Comprising four discrete stratigraphic units, the H-8 materials date to between A.D. 950 and A.D. 1800 (Emory et al. 1968:41; Emory and Sinoto 1969; Kirch 1985). According to Emory and Sinoto (1969:10), a total of 1,339 fishhooks and fishhook blanks was recovered from H-8. In addition, they recovered some

6,809 fishhook manufacturing tools and an unknown quantity of midden, marine faunal remains, and related debris. Goto (1986:198, 254), examined 490 one-piece and 373 two-piece fishhooks ($n = 863$) from the Waiahukini shelter for his dissertation analysis. Here, I have examined 1,600 fishhooks, fishhook fragments, fishhook blanks, hook-manufacturing debitage, and related faunal remains from H-8.

FISHHOOK RAW MATERIALS

Fishhooks from these two assemblages, combined with materials recovered from roughly 30 other localities, have been used to identify commonly used Hawaiian fishhook raw materials (Emory et al. 1959; Goto 1986). Using these assemblages, researchers have claimed that pigs (*Sus scrofa*), humans (*Homo sapiens*), dogs (*Canis familiaris*), and pearl oysters (*Pinctada margaritifera*) were the most commonly used species in fishhook manufacture (Emory et al. 1959; Goto 1986). Pearl shell is easily identified, as are rarely used materials such as bird bone, turtle shell, and fish bone (due to differences in their form and physical properties). Unfortunately, less success has been made in differentiating other types of mammal bone from one another, although marginal success has been claimed for identifying human-bone fishhooks (Goto 1986: 196–197). However, recent research by Greenlee and her colleagues (1998), suggests that microscopic examination of structural differences may enable researchers to identify unambiguously artifacts made from each of these species. Their research is continuing to evaluate the utility of this method.

Mammal Bone

Pigs, dogs, and humans were and are common to all of the major islands (with the possible exception of Lana`i, where pigs may have been unknown prehistorically). All three species contain bones of sufficient size, thickness, and density to create fishhooks. A variety of sea mammals, including whales, dolphins, and porpoises (commonly ascribed to *Kogia breviceps*, *Stenella longirostris*, or *Delphinus delphis*), and the Hawaiian monk seal (*Monachus schauinslandi*) may have also been used to make fishhooks, although use of these materials seems to have been exceedingly rare (Goto 1986). Other Hawaiian mammals include the Polynesian rat (*Rattus exulans*) and the Hawaiian hoary bat (*Lasiurus cinereus semotus*), but both are very small and do not appear to have been widely utilized as fishhook raw materials.

Based on this evidence, it appears that pigs, dogs, and humans were the only mammals whose bones were of appropriate size, physical structure, and abundance to provide the necessary raw materials for fishhook manufacture. Therefore, it is likely that these three mammals were the primary source for the vast majority of mammal-bone fishhooks recovered from Hawaiian archaeological contexts. Unfortunately, due to the limited nature of our present ability to

differentiate bone artifacts made from these three species, we must be content to label all nonsea mammal and nonbird bone fishhooks as “mammal bone.”

Pearl Oyster Shell

The use of pearl oysters as a source of material for various artifacts (including fishhooks) is well known (e.g., Kirch 1985). Two kinds of pearl oyster occur in Hawaiian waters. *Pinctada radiata* is a small oyster that rarely measures over 5 cm in length (Walther 1997). *Pinctada margaritifera*, on the other hand, is much larger, reaching well over 20 cm in length and 15 cm in width. Both species live amid the reef, along the reef slope, and in lagoons. However, the only area in Hawai`i known to have contained beds of either species is Pearl Harbor on O`ahu Island, and these beds consisted almost entirely of the smaller *Pinctada radiata*.

Large *Pinctada margaritifera* beds were almost certainly never found in Hawai`i. Individual oysters occur with some frequency on the reefs, reef slopes, and lagoons surrounding O`ahu and Kaua`i (Walther 1997). However, the species seems to have been rare on the islands of Ni`ihau, Mau`i, Moloka`i, Kaho`olawe, Lana`i, and Hawai`i. There was a large population of *P. margaritifera* at Pearl and Hermes reefs in the northwestern Hawaiian Islands until Western pearl hunters decimated them near the turn of the twentieth century (Walther 1997). It is possible that Hawaiians made voyages to these reefs to harvest pearl shell, but there is no evidence to support or refute this hypothesis. Hawaiian fishermen did, however, trade goods for pearl-shell fishing lures during the historic era and refer to prehistoric voyages to Tahiti to trade for pearl oyster fishhooks and other artifacts (Kahalelio 1902).

According to both Walther (1997) and Goto (1986:196–201), the relative abundance of pearl oysters decreases significantly from northwest to southeast in the main Hawaiian Islands. Both authors found that pearl oysters and pearl-shell artifacts are more abundant on Kaua`i and O`ahu Islands but rare on the rest of the island chain. This seems to be due to differences in abundance of the oyster’s preferred habitat of lagoons, reef passes, and areas along well-developed reef slopes. As the two oldest islands, both Kaua`i and O`ahu have lagoons and abundant reef slopes and well-developed coastal reef fringes along portions of each island. The rest of the islands, however, have less-developed reef flats and lagoons, although extensive reef flats occur along the southern shore of Moloka`i and along the north and east shores of Lana`i. There is also some reef development on Mau`i, but almost none on Kaho`olawe and Hawai`i Islands.

Summary

It appears that medium-sized mammal bone (pig, dog, and human) was locally available on all of the main Hawaiian Islands, with the possible exception of

Lana`i, where pigs may have been absent or uncommon in prehistoric times. Pearl oysters, on the other hand, were relatively abundant on Kaua`i and O`ahu Islands, increasingly rare on Mau`i, Moloka`i, Lana`i, and Kaho`olawe, and very rare on Hawai`i Island. In addition, pearl oysters do not appear to have been as common as pigs, dogs, or humans on any of the main Hawaiian Islands.

RAW MATERIAL ABUNDANCE

Given these patterns of abundance, the frequency of both mammal-bone and pearl-shell fishhooks should be predictable for different archaeological localities if raw material abundance alone is the primary reason for choosing one raw material over another. More specifically, the frequency of fishhooks made of common materials should be greater than the frequency of those made of more rare materials. To test this hypothesis, I examined the available habitat for each raw material category to obtain a rough measure of natural abundance and compared these data to observed fishhook frequencies. The following assumptions were made:

1. Pigs, dogs, and humans all live in areas of human environmental alteration.
2. Pearl oysters live in and are exploitable in any waters that are 20 meters or less in depth.
3. Raw material species are spread evenly across each habitat range.

No attempt was made to modify assumption 3, although available evidence suggests that pearl oysters are not distributed as evenly across their habitat as pigs, dogs, or humans are in their respective habitats. Nor did I attempt to identify microhabitats for any of the species examined.

Given these assumptions, several maps in *The Atlas of Hawaii* (3rd edition) were examined to identify areas of human alteration and potential pearl oyster habitat on Kaua`i and Hawai`i Islands (Juvik and Juvik 1998:122–123). All areas listed as unaltered by humans were ignored, and measurements were made only on modified terrestrial habitat and the marine zone within 20 meters of shore.

Using a grid superimposed over each map, an estimate of the amount of available habitat area on Kaua`i and Hawai`i Islands was calculated for each raw material species. For Kaua`i, a rough estimate indicates that 34 percent of the available raw material habitat is suitable for pearl oysters, while 66 percent is suitable for pigs, dogs, and humans. Conversely, on Hawai`i Island, only 16 percent of the available habitat is suitable for pearl oysters, as compared to 84 percent for pigs, dogs, and humans. It should be noted that these measures are only rough approximations, but they provide a relative measure for comparing habitat area among islands.

Table 9.1

Chi-square analysis of K-3 observed to Kaua`i Island expected values for fishhook raw materials

Locality	Pearl Shell	Mammal Bone	Total	Chi-square	<i>P</i> -value
K-3	452	163	615	61.63	≤ .001
Kaua`i Island	34	66	100		
Total	486	229	715		

ANALYSIS OF FISHHOOK RAW MATERIAL ABUNDANCE AND PREFERENTIAL USE

Using this information, we can compare the expected raw material abundance against observed fishhook raw materials at both of the archaeological localities. To obtain a statistically valid measure of this comparison, several one-sample chi-square analyses were performed on each assemblage. One-sample chi-square analysis is used when an empirical sample is compared to a theoretical population to assess how well the theoretical population can account for observed variability (Shennan 1988:65–70). Here, the theoretical population is derived from the expected values for each raw material habitat area, while the empirical measures are obtained from my analysis of each assemblage.

Kaua`i Island

A total of 452 pearl-shell and 163 mammal-bone ($n = 605$) artifacts was examined from the K-3 assemblage and compared to the expected frequencies of 34 percent pearl shell to 66 percent mammal bone. The null hypothesis (H_0) states that there is no significant difference between the observed frequency of pearl-shell and mammal-bone artifacts recovered from K-3 and the expected frequency based on natural abundance. In other words, if natural abundance alone is the primary criterion for raw material selection in fishhook manufacture, then we would not expect to see a statistically significant value in the chi-square analysis. The test produces a chi-square value of 61.63, which is statistically significant at or above the .001 level (Table 9.1). Therefore, we can reject the null hypothesis.

Pearl-shell artifacts are significantly overrepresented in the assemblage, while mammal-bone artifacts are underrepresented. Based on this analysis, I argue that raw material abundance alone cannot account for the observed distribution of fishhook raw materials at K-3 and that pearl-shell is being selected preferentially over mammal bone as a fishhook raw material. This suggests that some factor other than natural abundance is responsible for the high frequency of pearl-shell artifacts at K-3.

Table 9.2**Chi-square analysis of H-8 observed to Hawai'i Island expected fishhook raw materials**

Locality	Pearl Shell	Mammal Bone	Total	Chi-square	P-value at .05
H-8	250	1294	1544	0.0025	3.84
Hawai'i Island	16	84	100		
Total	266	1378	1644		

Table 9.3**Chi-square analysis of observed raw material frequencies of K-3 one-piece fishhooks to Kaua'i Island expected raw material frequencies**

Locality	Pearl Shell	Mammal Bone	Total	Chi-square	P-value
K-3	254	125	379	35.98	≤ .001
Kaua'i Island	34	66	100		
Total	288	191	479		

Hawai'i Island

The results obtained from K-3 were then compared to those obtained for the H-8 assemblage on Hawai'i Island. A total of 250 pearl-shell and 1,294 mammal-bone fishhooks, fishhook fragments, and related artifacts was examined from the H-8 assemblage ($n = 1,544$), and compared to the expected frequencies of 16 percent pearl shell to 84 percent mammal bone. Again, the null hypothesis (H_0) states that there is no significant difference between the observed and expected frequency of pearl shell and mammal bone. One-sample chi-square analysis produces a chi-square value of .0025, which is not significant at the .05 level (Table 9.2). Therefore, we cannot reject the null hypothesis. In fact, closer examination reveals that the observed and expected frequencies match remarkably well. This suggests that raw material abundance can account for the observed variability in fishhook raw materials at H-8.

Analysis of One-Piece Fishhook Raw Materials

Given the patterns observed for all fishhook-related raw materials from K-3 and H-8, each assemblage was broken down into two broad fishhook classes, one-piece and two-piece fishhooks. A preliminary analysis of the K-3 assemblage ($n = 379$) identified 254 pearl-shell and 125 mammal-bone one-piece fishhooks. These values were compared to expected frequencies of 34 percent pearl shell to 66 percent mammal bone for Kaua'i Island, and a one-sample chi-square analysis was performed (Table 9.3). The analysis produced a very high

Table 9.4

Chi-square analysis of raw material frequencies of H-8 one-piece fishhooks to Hawai'i Island expected raw material frequencies

Locality	Pearl Shell	Mammal Bone	Total	Chi-square	P-value at .05
H-8	107	406	513	1.23	3.84
Hawai'i Island	16	84	100		
Total	123	490	613		

chi-square value (35.98) that is significant at or above the .001 level. As with the analysis of all raw materials together, one-piece pearl-shell fishhooks are greatly overrepresented, while one-piece mammal-bone fishhooks occur with less frequency than would be expected. This suggests strongly that there is a preference for pearl shell over mammal bone in the manufacture of one-piece fishhooks.

A similar test was performed on the one-piece fishhooks recovered from the H-8 assemblage (Table 9.4). Preliminary findings have identified 107 pearl-shell and 406 mammal-bone, one-piece fishhooks ($n = 513$) from H-8. These values were compared to expected frequencies of 16 percent pearl shell to 84 percent mammal bone. A one-sample chi-square analysis resulted in a very low chi-square value (1.23), which is not significant at the .05 level. As with the overall analysis of H-8 raw materials, we cannot reject the null hypothesis of no significant difference between natural abundance and raw material preference for one-piece fishhooks at H-8.

Analysis of Two-Piece Fishhook Raw Materials from K-3 and H-8

One-sample chi-square analyses were performed on all identifiable two-piece fishhook segments (shank and point limbs) from the K-3 and H-8 assemblages. A total of 9 pearl-shell and 48 mammal-bone ($n = 57$) two-piece fishhooks was identified from the K-3 assemblage and compared to the expected frequencies of 34 percent pearl shell to 66 percent mammal bone for Kaua'i Island. It should be noted that this sample is small and, therefore, may be subject to greater error. However, each sample exceeds the minimum recommended requirements for chi-square analysis (Shennan 1988). The analysis suggests that there is a significant relationship (chi-square = 6.05; significance = .025) and that we can reject the null hypothesis (Table 9.5). Closer examination reveals that pearl-shell, two-piece fishhooks are underrepresented in the sample, while mammal-bone, two-piece fishhooks are overrepresented. This stands in contrast to the results obtained for the analyses of all fishhook raw materials and for one-piece fishhooks at K-3. It appears that mammal bone is being preferentially selected over pearl shell for two-piece fishhooks at K-3.

Table 9.5

Chi-square analysis of observed frequency of K-3 two-piece fishhooks to Kaua'i Island expected raw material frequencies

Locality	Pearl Shell	Mammal Bone	Total	Chi-square	P-value
K-3	9	48	57	6.054	≤ .025
Kaua'i Island	34	66	100		
Total	43	114	157		

Table 9.6

Chi-square analysis of observed frequency of H-8 two-piece fishhooks to Hawai'i Island expected raw material frequencies

Locality	Pearl Shell	Mammal Bone	Total	Chi-square	P-value
H-8	5	418	423	46.08	≤ .001
Hawai'i Island	16	84	100		
Total	21	502	523		

A similar pattern is observed for the two-piece fishhook assemblage from H-8 (Table 9.6). Five pearl-shell and 413 mammal-bone, two-piece fishhooks ($n = 423$) were identified in the H-8 assemblage and were compared to the expected frequencies of 16 percent pearl shell to 84 percent mammal bone in a one-sample chi-square analysis. The analysis produced a chi-square value of 46.07, which is significant at or above the .001 level. As with the previous analysis, the low number of pearl-shell, two-piece fishhooks is of concern, but the observed value does meet minimum recommended values for chi-square analysis. Like the K-3 analysis, the results obtained from H-8 suggest that mammal bone is preferentially selected over pearl shell for the manufacture of two-piece fishhooks.

DISCUSSION

These analyses show that significant variability exists between the raw materials used for different fishhook classes at each locality. Overall, it appears that pearl shell is being preferentially sought out over mammal bone for the manufacture of one-piece fishhooks but not for two-piece fishhooks at K-3. This contrasts with the evidence presented for H-8 on Hawai'i Island. Here, it appears that we cannot distinguish any preference in raw material choice for one-piece fishhooks, although mammal bone does appear to be the material of choice for two-piece fishhooks.

Clearly, the relationship between fishhook raw material abundance, fishhook

design variability, and raw material preference is more complex than has generally been realized. Furthermore, some factor other than abundance is driving the distribution and frequency of pearl-shell, one-piece fishhooks on Kaua`i and perhaps Hawai`i (if refined estimates of habitat range change for pearl oyster). Unfortunately, pending further analyses, the reasons for the observed variability in fishhook raw material use remain unclear.

CONCLUSIONS

Fishhook attributes have been used to identify cultural continuities in time and space and have played an important role in culture-historical reconstruction in Oceania. This use, however, presumes that the attributes employed are stylistic (i.e., neutral) in the absence of analyses that demonstrate the nature of their transmission. My analysis suggests that there may be additional functional considerations involved in raw material choice (beyond abundance) for different fishhook types, which might be revealed in further studies of fishhook performance characteristics.

Preliminary findings from two Hawaiian localities show that we simply cannot continue to make the kinds of broad assumptions of comparability in form, ancestry, or function that have characterized previous fishhook analyses. Given these problems, a systematic reassessment is both warranted and critically needed to take an initial step in explaining fishhook variability in ancient Hawai`i and throughout Polynesia.

To fully implement these considerations, evolutionary theory provides the causal-historical framework needed to explain why different hook attributes occur where and when they do in prehistory. Engineering provides the methods needed to evaluate cost and performance differences in fishhook design and use. Integrating mechanical experiments with analysis of fishhooks, manufacturing debris, and other archaeological remains provides the distributional context needed to analyze variability in each fishhook attribute through time, across space, and in relation to associated environmental variables. Together, such analyses may explain why certain fishhook raw materials and other attributes occur when and where they do in prehistory. In addition, they may finally enable researchers to isolate which fishhook attributes are functional and which are stylistic for a given historical context, allowing researchers to isolate homologous traits that may be used to trace cultural descent—thus fulfilling a longtime goal in Polynesian archaeology.

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Chapter 10

Style, Function, and Systematic Empiricism: The Conflation of Process and Pattern

Ethan E. Cochran

INTRODUCTION

The theoretical distinction between style and function (Dunnell 1978b) is axiomatic in evolutionary archaeology. This dichotomy, later elaborated upon by others (e.g., Leonard and Jones 1987; O'Brien and Holland 1992), has remained an effective theoretical tool in evolutionary archaeology for over 20 years because it defines the processes responsible for patterned variation in the archaeological record. Cultural transmission and innovation are the processes that lead to stochastic variation analyzed as style (Neiman 1995); natural selection is the process that leads to directional variation analyzed as function.¹

There is an important and often overlooked by-product of conceptualizing the style–function dichotomy in terms of transmission, innovation, and selection processes. If evolutionary style and function refer to these universal or immanent processes (see Lyman and O'Brien 1998), then style and function cannot be made into *a priori* descriptive adjectives of empirical phenomena. In other words, things are not inherently stylistic or functional; only variation resulting from a process can be explained as stylistic or functional. As explanatory labels, stylistic and functional may be applicable to different artifact classes depending on time, place, and analytical scale (see Dunnell 1995; see also Hurt et al., Chapter 4 in this volume; Maxwell, Chapter 3 in this volume; Neff, Chapter 2 in this volume, for discussion and applications). For example, in a particular environmental and cultural setting the relative frequencies of several temper classes (e.g., grog, shell, and sand) across a ceramic sequence may be a product of the differential fitness of these tempers. Natural selection may be an explanation, and the temper classes labeled functional. However, in another setting and with another ceramic sequence, frequencies of the same temper classes may

not exhibit variation indicating fitness differences. Transmission may, therefore, be an explanation for the class frequencies in this setting, and the temper classes labeled stylistic.

Much more detailed explanations of artifactual variation in terms of the style–function dichotomy are exemplified in studies of ceramic temper (e.g., Dunnell and Feathers 1991; O’Brien et al. 1994) and a growing body of other successful archaeological applications (e.g., Allen 1996; Barton 1997; Dunnell 1978a; Graves and Cachola-Abad 1996; Lipo et al. 1997; Neiman 1995; Meltzer 1981; Pierce 1998), including those by authors in this volume. Nevertheless, there is continued questioning of the usefulness of this dichotomy (e.g., Bettinger et al. 1996; Schiffer and Skibo 1997). Debate over theoretical concepts in a developing science such as archaeology is desirable, and in this spirit I argue that there is a fundamental flaw in many of the attempts (e.g., Ames 1996; Sackett 1990) to dismantle the style–function dichotomy developed by Dunnell and other evolutionary archaeologists.

This flaw is the conflation of conceptual categories (style and function as universal processes) with empirical categories (style and function as the observed characteristics of particular artifacts). The conflation often goes undetected and is a component of the methodological approach Willer and Willer (1973) call systematic empiricism. A recent example should help clarify what is meant by the systematic empiricist conflation of conceptual categories (theory) and empirical categories (observation).²

Systematic Empiricism: A Recent Example

In an introductory section of his work on Pacific Northwest Coast art, Ames (1996) conflates style as an empirical observation with the processes that evolutionary archaeologists reference with the term “style.” Ames defines a style as “attributes of artifacts . . . shared among a group of artifacts produced by a common production system” (1996:119, quoting Davis 1990:19). It is important to notice that a style here is an empirical category defined through observation and follows the common English definition. Style is essentially a way of doing something. Applying his definition, Ames suggests that a hypothetical set of pottery decorations is a particular style and that there may be a number of causal processes, including natural selection, shaping this style’s history.

According to Ames, groups of similar pottery decorations are *a priori* the empirical category style, and any explanation for changes in pottery decoration *must*, therefore, refer to style. With such an empirical tautology pottery decoration is forever and everywhere style, and changes in the frequency of pottery decorations may result from natural selection or several other processes. Because natural selection and other processes can affect style as pottery decorations, Ames concludes that evolutionary archaeology’s concept of style linked to selective neutrality is defective.

In his discussion Ames has conflated a theoretical category, style referring to

universal processes, with an empirical category, style referring to qualities of specific phenomena. The category style in evolutionary archaeology does not join selective neutrality (theory) with style as any set of distinguishing attributes (observation). Ames' critique of Dunnell's (1978b) and O'Brien and Holland's (1992) concept of style is based on the premise that these authors were referring to style as an empirical category, style as we understand it in English, as a set of (often aesthetic) shared attributes.

In this chapter I explore further how the systematic empiricist conflation of theory and observation distorts Dunnell's original definitions of style and function in evolutionary archaeology. In the preceding paragraphs, the style–function dichotomy and systematic empiricism have been simplified to demonstrate briefly the effects of this distortion. In the next section I argue that the systematic empiricist treatment of the style–function dichotomy is related to the confusion of natural language and scientific concepts when reading Dunnell's original (1978b) formulation. I also provide an example of systematic empiricism in the development of modern genetics. In the following section, I demonstrate the explanatory potential of treating style and function as processes or conceptual categories. Using a brief example from the archaeology and classification of monumental architecture in the Pacific, I illustrate the theoretical character of style and function. Finally, the architectural analysis leads to a consideration of how both the roles of reproductive and replicative success (Leonard and Jones 1987) and the predictions of transmission rules (Boyd and Richerson 1985) are related to style and function.

Systematic Empiricism and the Style–Function Dichotomy

In evolutionary archaeology style is a conceptual category referring to the processes that explain the distribution of artifacts. To say that a particular distribution is stylistic is to say that the variants are historically related through transmission processes predominantly unmediated by selection. Initial tests of this explanation involve, minimally, comparing observed and expected stochastic distributions with confidence intervals (e.g., Lipo et al. 1997; Neiman 1995). Expected distributions are described by patterns of drift and innovation (Neiman 1995).

To state that a set of artifacts is functionally distributed suggests that the particular distribution is the outcome when artifacts vary in relative fitness.³ Initial tests of this explanation involve analyses of the variable performance of artifacts in specific contexts, an avenue of research followed by both evolutionary archaeologists and others (e.g., Bronitsky and Hamer 1986; Feathers 1989; Maxwell 1995; O'Brien et al. 1994, 1998; Pierce 1998; Rye 1976; Schiffer et al. 1994; Schiffer and Skibo 1997; Young and Stone 1990). Because of evolutionary convergence, functional distributions may not necessarily reflect cultural transmission within a single population. Producing testable explanations that demonstrate the variable influence of transmission only, transmission working

with selection, and selection only on the structure of cultural lineages may, in fact, define the practice of evolutionary archaeology (Lyman and O'Brien 1998).

Most evolutionary archaeologists treat style and function as conceptual categories that refer to the universal processes of innovation, transmission, and natural selection. Unfortunately, in the evolutionary archaeology literature the use of style and function as conceptual categories is not always clear. Archaeologists, both evolutionary and others, consistently quote Dunnell's original definition and reinforce a strongly empirical conception of style. Dunnell wrote that "style denotes those *forms* that do not have detectable selective values" (Dunnell 1978b:199, emphasis added). Stopping here, it is easy to interpret style as an empirical category of objects if form denotes observed attributes. However, analytical problems arise when style is treated as an empirical category: how are attributes unambiguously identified as stylistic, or how can an attribute be stylistic at one time and functional at another? Analytical problems such as these may have led many archaeologists to eschew Dunnell's perceived empirical treatment of style. Indeed, the analytical problems associated with any empirical treatment of style are partly the cause of the various "style debates" in archaeology (e.g., Binford 1986; Dietler and Herbich 1998; Sackett 1985, 1986; Wiesner 1983, 1985). Complicating matters further, archaeologists undoubtedly found little use for Dunnell's discussion of function because it was not quite synonymous with the English-language definition of function as "purpose" or "use" (but see Meltzer 1981).

It is clear that any practical use of the category style must describe and potentially explain *distributions*, not objects. For example, after he defines style, Dunnell suggests that to employ the style-function distinction, "a profitable direction may lie in identifying stylistic elements by their random behavior" (Dunnell 1978b:199). The phrase "stylistic elements" may connote for some an empirical character to style, but of course an element or attribute of a single artifact cannot exhibit a random behavior. Randomness necessitates multiple observations in time and/or space. Therefore, distributions comprised of multiple observations (on one or many objects) can be described as random.

The meanings of style and function proposed by Dunnell are not equivalent to the English-language definitions of these terms. The meaning of any conceptual category in science is not established by fiat, but rather by the usefulness of that category in producing testable explanations of phenomena. Thus, the difference between evolutionary style and function and other archaeological meanings for these categories is not mere definition. The difference is in the application of these categories to archaeological explanation. Evolutionary style was successfully, albeit implicitly, applied by culture historians to produce the time-space systematics that are still useful today (Dunnell 1978b:199; Lipo et al. 1997; Lyman et al. 1997). Over the last two decades, archaeologists have continued to produce viable explanations of phenomena using the concepts of evolutionary style and function.

Conceptual and Empirical Categories: A Genetic Example

Several archaeologists (e.g., Neiman 1995; Lipo and Madsen, Chapter 6 in this volume; Teltser 1995) have noted that the evolutionary notion of style, as implicitly adopted by culture historians, is related to ideas about the selective value of genes, particularly to neutral gene theory developed by geneticists beginning in the 1960s (e.g., King and Jukes 1969; Kimura 1979, 1983). The history of research on the selective value of genes emphasizes gene neutrality as a conceptual and not an empirical category. In turn, this history underscores the relationship between evolutionary style and selectively neutral variation.

In the 1920s Chetverikov, a Russian geneticist, suggested that phenotypes are not determined simply by the one-to-one expression of gene to phenotypic character but that phenotypes depend on the interactions between genes. In other words, phenotypes depend on the “genotypic milieu” and the environment (Chetverikov 1926). Western population geneticists, however, studied the effects of genes as independent entities with inherent selective values. These geneticists took theoretical selective values and gave them empirical status. With this approach, population geneticists could explain the behavior of individual gene systems, but their models were not appropriate for empirical applications beyond Mendelian genetics. Mayr (1959:2) called this approach “bean-bag” genetics and later contrasted it with the “unity of the genotype” (Mayr 1975), his phrase highlighting the relative nature of gene interactions in producing the phenotype.

The selective value of an individual allele at a gene locus is not a fixed empirical attribute. The selective value of an individual allele depends on its genotypic milieu or the aggregate relationships of many genes and the environment. In the same vein, an individual artifact cannot be stylistic or functional in an evolutionary sense. Style and function are conceptual categories that refer to the processes that produce variation in the archaeological record. Style as a conceptual category is often confused with the systematic empiricist notion that a particular style is an empirical thing. Only carefully constructed arguments, analyses, and descriptions of empirical phenomena will convey the conceptual nature of the style–function dichotomy.

CLASSIFICATION AND THE STYLE–FUNCTION DICHOTOMY IN EVOLUTIONARY ARCHAEOLOGY

Sackett (1982:78) noted that “since classification so often serves as the idiom of thought for the working archaeologist . . . no argument about style and function is really completed until it has been translated in the language of systematics.” Thus, if we use the style–function dichotomy to produce potential explanations of the empirical world, it is critically important to count phenomena with classes explicable in terms of natural selection, innovation, and transmission. To relate these processes to groups of phenomena, classification could proceed in a random fashion, combining attribute variables without purpose to

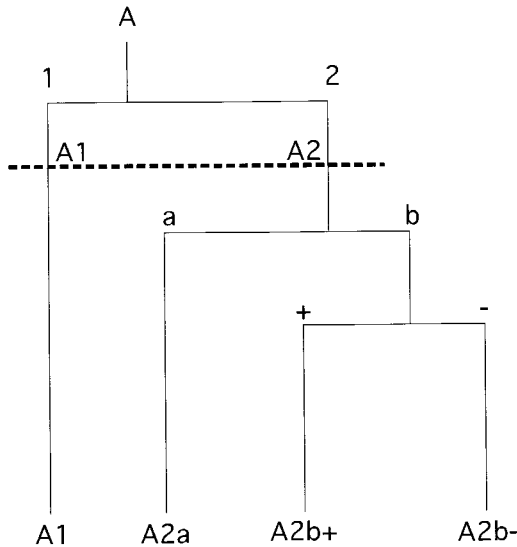
form the larger classes of analysis. Such an approach is obviously inefficient. Even if the classes produced patterned variation, we still have the formidable task of describing a plausible and testable mechanism that created the variation.⁴ At least two interrelated methods of classifying phenomena in evolutionary terms provide a way out of this dilemma. First, by understanding the theoretical relationships between the performance characteristics of artifacts, artifact raw materials, and the environments within which artifacts interact, we can construct arguments relating artifact classes and attributes of artifact classes to the processes of natural selection and transmission (Maxwell 1995; O'Brien et al. 1994; Pfeffer, Chapter 9 in this volume). Second, by taking a theoretically informed trial-and-error approach (Teltser 1995), we can construct paradigmatic classifications and examine frequency distributions of phenomena at various levels of classificatory precision.

In this chapter I concentrate on the second approach. Two characteristics of paradigmatic classification make it a valuable method for constructing empirical units within an evolutionary framework: (1) paradigmatic classifications are ideal for describing variation, and (2) the classes constructed are easily decomposed or refined to produce related classes of differing precision.

The first characteristic of paradigms, the ease with which they handle variation, is important because variation is a necessary component of all evolutionary change. Variation is produced through errors in transmission and innovation, and variation is a requirement for natural selection. Leonard and Jones (1987: 207) point out that "other means of classification might [serve] equally well, but few other classificatory or typological structures are so competent at describing variation" as paradigms. Paradigms are ideal for describing variation because they can produce unlimited classes. These classes are all related, as they are composed of the same intersecting dimensions, each with a potentially infinite number of modes (Dunnell 1971). Unlimited classes are not, of course, anyone's analytical dream. The point is that paradigmatic classifications provide unlimited potential for recognizing variation.

The dimensional structure of paradigmatic classifications underwrites their second important characteristic for analyses of style and function. Paradigmatic classes may be made more precise or inclusive in a systematic way such that the relationships between all classes in the paradigm are similarly changed. As Lipo et al. (1997) point out, this feature is important when examining the effect of transmission at different scales. Imagine, however, analyzing the differing effects of transmission between individuals and transmission between groups composed of those individuals (i.e., differences in the scale of transmission) with a taxonomy composed of unequally weighted attributes (Figure 10.1). Frequencies based on the classes defined at the bottom of the taxonomy do not have consistently scaled relationships to frequencies based on the classes defined at another level in the taxonomy (indicated by the dashed line in Figure 10.1). Any argument for differences in the scale of transmission and selection is logically derailed by such a taxonomy.

Figure 10.1
Taxonomy comprising nondimensional classes¹



¹Class criteria are noted at the taxonomic branches. Class definitions are at the bottom of the taxonomy. Dashed line shows class definitions at a more inclusive level in the taxonomy.

An example will help to demonstrate the relationship between classification and style and function when style and function refer to processes of selection, innovation, and transmission. While these processes are universal, the distribution of empirical variants can be influenced by deposition, taphonomy, scale differences in evolutionary processes (Neff, Chapter 2 in this volume), sorting (Hurt et al., Chapter 4 in this volume), and the contingent nature of history. As the chapters in this volume demonstrate, the style–function dichotomy is conceptually clean but empirically messy.

Style and Function: A South Pacific Example

I use an explanatory sketch from ongoing research on the archaeology of the Society Islands in the South Pacific to exemplify the points made so far. Initial settlement of the Society Islands may have begun ca. A.D. 600–800 (Spriggs and Anderson 1993), but evidence for an earlier human presence could be missing due to geomorphological processes and massive sampling problems (Kirch 1986). The inhabitants of the Society Islands (Tahiti, Bora Bora, and others) built *marae*, or rectilinear rock temples comprising paved courtyards, sometimes elevated, and also sometimes augmented with stone altars of variable elaboration. Stone uprights were also sometimes placed on *marae*. Other stone struc-

tures were built throughout the Society Islands, including domestic structures of different shapes and structures described ethnohistorically as archery platforms.

According to ethnohistorical sources (e.g., Henry 1928:123–147), many different kinds of *marae* were built: huge national *marae*, *marae* used by local chiefs and priests, craft-guild *marae* (e.g., canoe-builders' *marae*), and small family *marae*, to name a few. This suggests that a varied cross-section of the Society Islands population built *marae*. The construction of *marae* may have begun around A.D. 1200 and increased over time (Wallin 1993:65–70). Apparently, however, the majority of *marae* construction ceased before Western contact. Unfortunately, archaeological dates for *marae* are far too few to make anything more than these general statements about *marae* chronology (Cochrane 1998).

With a simple paradigmatic classification and a minimum number of dimensions defining construction material, shape in plan view, subsurface internal features, and internal architectural features, a classification of Society Islands stone architecture can be created. For example, using Table 10.1 and taking the modes “basalt,” “rectilinear,” “absence,” and “altar” for the four dimensions, an architectural class is created that would likely identify some set of *marae*. If the mode in dimension I is changed from “basalt” to “basalt and coral,” a different architectural class that identifies another set of *marae* is created. If additional research supports the notion that both of these architectural classes identify *marae*, they can be combined into a *marae* superclass. Actually, comparison of the descriptions (compiled by Wallin 1993) of individual *marae* in the Society Islands with the paradigm (see Table 10.1) indicates that 18 of the 144 possible architectural classes in the paradigm constitute a *marae* superclass. The 18 architectural classes that make up this *marae* superclass are listed in Table 10.2. Almost all of the remaining 126 architectural classes in the paradigm will be easily recognized as either domestic architecture or archery platforms.⁵

After classifying Society Islands architecture, a possible temporal distribution of structures identified with the *marae* superclass is represented by the curve on the left side of Figure 10.2. This distribution represents the frequency of the *marae*-building phenotypes in the Society Islands over time. This is potentially a functional distribution explained by transmission of variants whose frequencies are a product of selection (i.e., adaptation). Testing this explanation, however, requires several arguments and additional analyses. First, the population of *marae*-building phenotypes must belong to a single transmission lineage; otherwise, convergent evolution could be an explanation. Second, detailed arguments about the relative fitness of *marae*-building and non-*marae*-building phenotypes are necessary. These arguments should discuss appropriate measurable variables and the potential for evolutionary sorting of traits (e.g., Graves and Ladefoged 1995; Neiman 1997; see also Madsen et al. 1999). Lastly, the fitness of the *marae*-building phenotype at variable times is probably related to other traits (e.g., subsistence, degree of sedentism, or level of social complexity). More complex models of these trait relationships may increase the dynamic

Table 10.1
A paradigmatic classification of architecture¹

DIMENSION I: Construction Material	DIMENSION II: Plan-view Shape	DIMENSION III: Subsurface Internal Features	DIMENSION IV: Internal Surface Features
a. Basalt	a. Rectilinear	a. Hearth	a. Altar
b. Coral	b. Round-ended	b. Shaft	b. Uprights
c. Basalt and Coral	c. Y-shaped	c. Hearth and Shaft	c. Altar and Uprights
		d. Absence	d. Absence

¹Column heads identify the dimensions. Each row describes the various modes for each dimension.

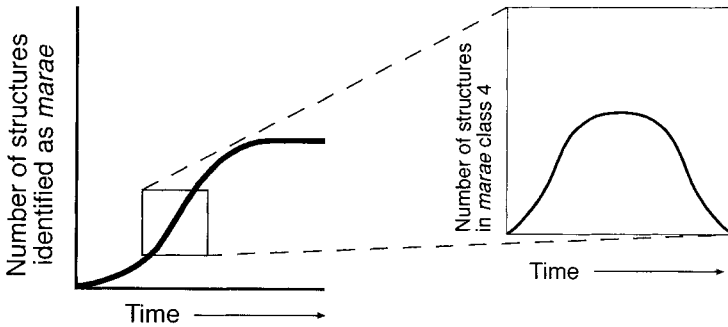
Table 10.2
Classes in the *marae* superclass¹

Marae Class	Dimension I	Dimension II	Dimension III	Dimension IV
1	a	a	d	a
2	a	a	d	b
3	a	a	d	c
4	b	a	d	a
5	b	a	d	b
6	b	a	d	c
7	c	a	d	a
8	c	a	d	b
9	c	a	d	c
10	a	a	b	a
11	a	a	b	b
12	a	a	b	c
13	b	a	b	a
14	b	a	b	b
15	b	a	b	c
16	c	a	b	a
17	c	a	b	b
18	c	a	b	c

¹Each row is a *marae* class (1–18) included in the *marae* superclass. *Marae* class definitions are indicated by the letters in each row that correspond to particular modes of a dimension (modes listed in Table 10.1). Dimensions are indicated at the column heads.

Figure 10.2

Comparison of the functional distribution of the *marae* superclass (left) versus the stylistic distribution of *marae* Class 4 (right)



sufficiency of explanations for the frequency of *marae*-building phenotypes (see Holland 1989 for a similar argument related to changes in human fertility).

While the distribution to the left in Figure 10.2 may be functional, consider examining a single class of *marae* within the *marae* superclass. Here, the precision of the analysis is increased by using less inclusive classes. *Marae* exhibit different raw materials (e.g., coral, basalt) and internal architectural elements (e.g., altars, uprights). *Marae* class 4 (see Table 10.2), for example, contains rectilinear coral structures with altars. We could identify *marae* class 4 structures from the set of *marae* enclosed by the box on the left side of Figure 10.2 and, by plotting the frequency of *marae* class 4 structures against time, a very different distribution may result. This is shown on the right side of Figure 10.2, where coral *marae* with altars display a potentially stylistic distribution. Were it to satisfy appropriate tests, this distribution could be explained by transmission and innovation with little influence from natural selection.

To summarize, after using one classification, artifact frequencies conform to expectations of functional distributions and may be explained by the associated processes of selection and transmission. In another classification a subset of the same artifacts forms a distribution where frequencies conform to the expectations of style and may be explained by the processes of transmission and innovation. A particular piece of monumental architecture, just like any other single artifact or artifact attribute, is neither universally stylistic nor functional.

DISCUSSION

The main point of the preceding arguments and examples is certainly not new to many evolutionary archaeologists. The systematic empiricist conflation of conceptual categories and empirical categories as well as the topics of classification and grouping and explanation and generalization have been discussed repeatedly in the literature (e.g., Dunnell 1982, 1992a, 1992b; Lyman and

O'Brien 1998; O'Brien et al. 1998). Here, I have tried to demonstrate how this conflation may have misled many opponents of the style–function dichotomy. If more archaeologists explore the explanatory power of the style–function dichotomy in terms of selection, transmission, and innovation, perhaps the development of archaeological evolutionary theory will accelerate.

One area where theory development seems to be progressing is in the role of interactors and replicators in evolutionary explanations (Boone and Smith 1998; Neff, Chapter 2 in this volume; Lyman and O'Brien 1998; O'Brien and Holland 1992). Following Hull (1988) and Dawkins (1976), replicators are those entities that reproduce with some degree of fidelity (more specific labels proposed include culturgen, memes, and genes), while interactors are the entities that interact with the environment and carry one or more replicators. The actions of interactors make replication differential⁶ (cf. Neff, Chapter 2 in this volume). If ideas or memes about how to make artifacts are replicators, then replicator frequency is related to interactor frequency when interactors are humans or human groups.

Leonard and Jones (1987) discuss this relationship and link the replication of artifacts and the reproduction of people to the processes embodied in the style–function dichotomy. They suggest that stylistic distributions are driven disproportionately by the replicative success of artifacts. Here a replicator, for example, the idea of building *marae* in a certain fashion, reproduces predominantly as a result of cultural transmission and confers no measurable reproductive benefit to the interactors that carry it. On the other hand, functional distributions are driven disproportionately by the “reproductive success of the bearer [i.e., interactor]” (Leonard and Jones 1987:214). In this instance a replicator may produce a phenotype with a selective advantage. The frequency of this replicator and phenotype is then a product of natural selection within a lineage or the convergence of separate lineages. In sum, “each trait has a . . . replicative fitness that may or may not affect the Darwinian fitness of its bearer” (Leonard and Jones 1987:214) depending on the natural and cultural environment.

Artifact frequencies, therefore, are sometimes influenced by the additional fitness that artifacts confer to people. When the number of people increases, more interactors are available to carry around the replicators and produce the artifacts we measure. Thus, whenever interactors are humans or human groups, human reproductive success may have an effect on artifact frequencies. This means that human reproductive success may also influence the frequency of artifacts that confer no additional fitness to the humans that carry them, resulting in the hierarchical sorting (Hurt et al., Chapter 4 in this volume; Vrba and Eldredge 1984; Vrba and Gould 1986) of neutral traits with the fitness of interactors.

Another area of active theory development is the debate over the role of cultural transmission rules in shaping trait distributions. Some argue that the style–function dichotomy fails to consider transmission rules, including various forms of bias, and guided variation (Bettinger et al. 1996; see also Boone and

Smith 1998; Dunnell 1992a:214; Richerson and Boyd 1992; Richerson et al. 1998). Various forms of bias (e.g., frequency-dependent and indirect) and guided variation reduce overall variation in the pool of cultural replicators that can be transmitted (Bettinger 1991); with insufficient innovation drift is the result. Bettinger et al. (1996:148) conclude that “there are no simple qualitative rules to distinguish these drift-induced patterns from those produced by simple adaptive processes like selection.”

True, there are no simple rules, but there are several ways to begin distinguishing the different processes that produce similar patterns. For example, if a putative functional distribution is best characterized by drift, then we would expect no potential selective differences between the high-frequency variant of a trait and the other variants of the trait. As I described earlier, performance analyses of ceramics and other artifacts are well suited to this kind of question. Distinguishing the proposed functional *marae* distribution above (see Figure 10.2, left) from a pattern of drift is more difficult but would involve demonstrating the greater potential fitness of *marae*-building phenotypes from non-builders. If *marae* building is a “wasteful phenotype” in a variable environment (see Madsen et al. 1999) or is a form of competitive advertising (see Graves and Ladefoged 1995; Neiman 1997), then there may be good reason to expect a functional distribution.

There may be other scenarios where it is difficult at first glance to determine what processes are creating particular distributions. Sequential functional distributions describing changes in a particular technology may mimic sequential stylistic distributions if the rates of change were similar (Bettinger et al. 1996: 148). Again, we would expect the high-frequency technological variants of each functional distribution to exhibit potential selective differences (measured through performance analyses or other means). This would not be the case for sequential stylistic distributions.

Finally, putative functional distributions may result from guided variation and/or direct bias operating in consciously adapting (i.e., problem-solving) populations. In both cases naive individuals learn of traits through observing others (transmission) and exhibit traits that they feel are most beneficial to themselves (Bettinger 1991:186–190). Beneficial traits increase in frequency as in a functional distribution. There is no reason to suggest, however, that natural selection does not shape the *distribution* of phenotypes produced by problem-solving individuals (Jones 1998). Furthermore, we can test an explanation based on natural selection (in ways outlined earlier), but it is unclear how one tests the explanation that a distribution is a result of people’s behaving adaptively. That explanation often appears foregone.

CONCLUSION

The distinction between conceptual and empirical categories is vital to the style–function dichotomy. With this distinction we avoid the systematic empir-

icist conflation of theoretical principles with empirical observations, which has led some to reject evolutionary definitions of style and function. Dunnell's (1978b) introduction of a single, specific meaning to a word that previously had many ill-defined meanings (see Conkey and Hastorf 1990:1) redefined in explicit terms the implicit way in which style was understood by the culture historians (Teltser 1995). When style and function are recast in evolutionary terms, they encompass the processes of transmission, innovation, and selection. These are universal processes that apply to any self-replicating system, genetic or cultural, where variants differ in fitness (Pocklington and Best 1997:79).

In an effort to demonstrate the application of the style–function dichotomy, I have presented an example of how a paradigmatic classification might be applied to the *marae* of the Society Islands. This example led to a consideration of the processes encompassed by the style–function dichotomy and how these processes might shape distributions of *marae*. Importantly, this example demonstrated that individual artifacts or features are not stylistic or functional; only distributions are stylistic or functional.

The *marae* example highlighted the role of style and function in two current debates in evolutionary archaeology: the replicator–interactor distinction and the role of transmission rules in shaping empirical distributions. Replicators are those units of information whose frequency is a product of transmission, selection, and innovation. The number of interactors that carry particular replicators may also be a product of natural selection as interactors with less fit phenotypes are removed from the population. Along with replicators and interactors, transmission rules can be better understood in terms of the processes embodied by style and function. Transmission, selection, and innovation can account for all the distributions linked to transmission rules, but with the added bonus of implicating tests of the putative explanations.

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NOTES

1. In this chapter I do not discuss different kinds of selection such as diversifying and stabilizing selection (but see VanPool, Chapter 7 in this volume).
2. The contrast between conceptual and empirical categories is not new to archaeology. See Dunnell (1971), Krieger (1944), Rouse (1939), Ford (1954b).
3. The simple characterization of fitness used in this chapter ignores several problems

associated with the term. The problem most germane here involves the different fitnesses measured by human reproduction and artifact replication, respectively. Additionally, methods to measure human reproductive fitness are also variable.

4. This is the same dilemma at the heart of the Ford–Spaulding debate (Ford 1954a, 1954b; Spaulding 1953, 1954) and subsequent problems identified with statistical grouping (Dunnell 1986) and quantified essentialism (O’Brien and Holland 1995).

5. I am not arguing that the efficacy of this classification is a function of its ability to identify emic groups, or classes recognized by other archaeologists in the Society Islands. However, the ability to generate groups that likely have some connection to different sets of behaviors (e.g., eating, sleeping, protection from the elements, in contrast to some form of ritual and/or larger group activity) does suggest that such a classification may be parceling out variation important to evolutionary processes.

6. Replicators may lie about without human interactors (in a book, let’s say) but require a human interactor at some stage to get on with the business of replicating. Developments in artificial intelligence, however, may some day lead to “cultural change” associated with interactors that have little to do with humans (Dennett 1995).

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