

2 Variation, Selection, and the Archaeological Record

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A science may fall short of perfect clarity in different ways. One is relatively benign. A science may move forward, sideways, and backward as if in a fog that sometimes lifts a little then resettles. . . . But a science enveloped by fog has at least one consolation. A fog does not foster the illusion of clarity; the lack of visibility is patent.

More insidious than the fog is the mirage. Fogs are seen for what they are. Mirages are trickier, engendering the mistaken conviction that things are as they seem.

Sober 1984:1

Introduction

We believe, as do a growing number of archaeologists (e.g., Dunnell 1980, 1982, 1988; Leonard 1989; Leonard and Jones 1987; Rindos 1984), that it is both desirable and possible to achieve the integration of a Darwinian evolutionary paradigm into Americanist archaeology, not simply as a replacement for current views of the archaeological record but rather as a powerful means of incorporating a vast amount of existing knowledge under a unified framework. In a lengthy series of articles, Dunnell (1978a, 1978b, 1980, 1982, 1985, 1986, 1988) laid the groundwork for an evolutionary approach to archaeology, focusing on a materialist strategy that examines change as opposed to transformation. Despite considerable citation of Dunnell's work in one context or another, application of Darwinian evolutionary theory to archaeological problems has been slow to develop, attributable in part to a lack of preadaptation on the part of archaeology to the large body of theory loosely termed

evolutionary. Americanist archaeology is rooted in essentialist thinking, empirical demonstration, and laws of probability (contingency), which makes it difficult for the field to embrace a counter-intuitive analytic framework. A second reason for slow growth is the inevitable series of false starts that accompanies the introduction of a new way of looking at reality. With regard to archaeology, the most time-consuming task is to establish procedures for analyzing the record in such a manner as to derive empirical meaning within the framework of the theory.

Our modest objective here is to outline some of the many considerations that must be addressed in constructing an evolutionary theory useful for examining the archaeological record. Central to our discussion are the concepts of cause and explanation and their linkage to evolutionary mechanisms that effected change in the frequencies of variants now evident in the archaeological record. Key among these, and the one that receives the majority of attention here, is selection. Our goal is to examine the concept of selection—what it is and is not—its role in evolutionary theory, and how a selection-based approach might be used in archaeology.

Evolution in Americanist Anthropology and Archaeology

Evolution, in one form or another, has enjoyed a place in Americanist anthropology, and by extension Americanist archaeology, for over a century. The ebb and flow in popularity of evolution as an explanatory framework for human behavior has, in one sense, been correlated inversely with the rise and fall of other frameworks—evolution was always there when other paradigms failed or grew tiresome. Some anthropological perspectives on evolution have been cast in Darwinian terms, but most have been decidedly non-Darwinian (Kehoe 1982; O'Dowd 1982).

Interest in evolution-based explanations of human behavior on the part of anthropologists is, in one sense, a natural outgrowth of a discipline that, because of its subject matter, has always kept one eye on biology to see where that field was headed. When anthropology became aware of advances made in the 1930s and 1940s toward a new synthesis in biology, its interest was piqued. Refinement of the synthesis during the 1950s and 1960s, especially in the area of

inheritance and genetic transmission, stole the hearts, if not the minds, of anthropologists hungry for explanations of why humans behave as they do (e.g., Tax and Callender 1960). Some anthropologists (e.g., Campbell 1956, 1960, 1965, 1970, 1975) and allied behavioralists (e.g., Alexander 1974, 1975, 1979a, 1979b) saw, correctly, that Darwinian evolutionary theory could include humans comfortably under its umbrella and thereby provide a much-needed link between biology and culture. Unfortunately, anthropology also became rife with a hodgepodge of loose-fitting analogs between biology and culture that did little to advance our knowledge of human behavior. Concurrently, anthropology witnessed the advent of human sociobiology (Wilson 1978; see also Lumsden and Wilson 1981, 1983), which caused many anthropologists to abandon ship and revert to the older, less-controversial position that there is, at least, a biological *basis* for culture, whenever and however it evolved. A growing literature, most of 1980s vintage (e.g., Boyd and Richerson 1985; Cavalli-Sforza and Feldman 1981; Durham 1978; Rindos 1984, 1985, 1986) and subsumable loosely under the term *cultural selectionism*, attests to the fact that Darwinian theory, in one guise or another, is still being used as a framework to explain human behavior.

In Americanist archaeology Darwinian theory has a less well-defined niche. Evolution assumed a place in the new archaeology of the 1960s, but, similar to its status in anthropology in general, it was not Darwinian in nature. Given the agenda of the new archaeology (e.g., Binford 1962) and its intellectual debt to White (e.g., 1949, 1959; see Marks and Staski 1988), it was natural that the search for cultural process—the pillar of the new archaeology—would lead into the anthropological literature and its own peculiar brand of evolution (Kohl 1984). Archaeologists scrambled to incorporate the trappings of cultural evolution into their arsenal, concerning themselves with searching for archaeological correlates of ethnographically known forms of kinship, sociopolitical organization, and so on. Once these were found, archaeologists then attempted to understand how one form transformed itself into another form. Implicit in these exercises was the notion of an evolutionary progression from simple to complex, from less energy capture and use to more energy capture and use (White 1959)—in short, directional evolution. Certain key criteria could be used to place cultural groups along the evolutionary scale, and within tolerance limits, groups could be combined into types and described accordingly (Kohl 1984). Change was seen in

terms of alterations in form. Basic to understanding processes that effected change was the search for discovery of general lawlike principles that covered the ways in which people acted, so that by reference to the laws human behavior could be predicted.

Clearly, archaeology had as its goal to become a science, but the type of science to which it aspired, i.e., physical science, does not and cannot deal with living organisms. One property that physical sciences possess is the ability to predict. Physical sciences such as chemistry and physics also are more deterministic and hence less probabilistic than are sciences that deal with organisms. To use a mundane example, a carbon atom is like any other carbon atom, all else being equal. When all else is not equal, chemistry has developed a theory to include discrepancies. We can predict with considerable assurance that carbon atoms today behave the same way carbon atoms behaved a million years ago and the same way they will act ten million years from now. In this sense the laws of chemistry, as with most physical laws, are timeless; they are not tied to history. But animals carry historical baggage with them; in essence they are products of their histories. The historical (evolutionary) paths taken by organisms can be followed to some extent, and present archaeological theory allows us to reconstruct bits of the paths taken by human groups throughout history. But in reality our operations produce little more than static views of developmental histories, one stacked on top of another and separated by thin time lines of various length (Dunnell 1982, 1988; Plog 1974).

The new archaeology had as its goal to use those snapshots to create a moving picture of human prehistory. The thinking was that if we could be more rigorous in our procedures, and if we could observe archaeological sites in the making, then perhaps we would be in a much better position to identify laws that govern human behavior. Concurrent with this emphasis came wholesale borrowing of concepts and terms from other disciplines, especially ecology and occasionally evolutionary biology. Borrowing often was done uncritically, to the point that archaeology was plagued with logical weaknesses, ill-constructed analogs, and trivial homologs (Schiffer 1981). Most forays into the biological literature were little more than "search and seizure missions" (Hardesty 1980:161) whose express purpose was to carry back as much booty as possible. Results of such forays have tended to be concoctions of ingredients—one part biol-

ogy and two parts anthropology added to an archaeological base. Whatever the formula is, it was not created in Darwin's kitchen.

In the following pages we illustrate some of the problems that archaeology has faced in its attempts to develop an evolutionary perspective, how it has gone astray both in its fundamental assumptions and approach, and how it might overcome the problems. Lest our position be misinterpreted, we are not suggesting that it is possible to effect a wholesale transfer of biological principles to the study of variation as seen in the archaeological record. This makes no sense. Darwin was interested in heritable variation, as are archaeologists, but Darwin's variation was evident generationally and included, as we term it now, the genotype. Variation as seen in the archaeological record does not necessarily pass through the phenotype-genotype-phenotype process. This in no sense precludes a focus on selection from an archaeological perspective if we accept the premise that things viewed in the archaeological record were part of past human phenotypes. Or, as one might argue, the *behaviors* that created the objects were parts of human phenotypes. At this point we see little to be gained by arguing strenuously for or against one of these two positions (but see Boyd and Richerson 1985:36; Geertz 1973:143–46).

We are especially concerned that our discussion not be seen as a reductionist argument that focuses solely on human reproductive success. Darwinian evolution has as one of its central tenets the selection against certain variants that arise biologically (oftentimes referred to as selection *for* favorable variants). Selection operates on humans as it does on all other organisms, and the presence of certain features may give some humans increased fitness relative to other humans. And, as with other organisms, the presence of certain features among humans may confer no increase in fitness; i.e., the features are neutral relative to reproductive success. But to compound the analytical problem, when we deal with humans we must confront the phenomena of *social* reproduction and *social* success, which, though perhaps *linked* to biological reproduction and reproductive success, are not the same. Thus we again emphasize that one cannot create a simple analog between biology and human behavior.

Several caveats must be registered prior to our discussion. First, space constraints preclude anything more than a brief examination of selection theory and its role in Darwinian evolution. Excellent references to the subject exist, including *The Nature of Selection:*

Evolutionary Theory in Philosophical Focus (Sober 1984) and *Adaptation and Natural Selection* (Williams 1966). We find it impossible to compete with biologists and philosophers of biology on the subject of selection, and we urge casual and critical readers alike to explore the subject in more detail. Second, what we propose here is in no way a blueprint for an "inclusive evolutionary theory" for archaeology (Leonard and Jones 1987) but rather an outline of things we should consider in drawing up the blueprint. As a starting point, we might ask, Why adopt a Darwinian evolutionary perspective for archaeology? The field, judging by some measures, appears to have prospered without it. If such an approach is, in some respects, counterintuitive, is it worth making necessary wholesale changes in the way we view the archaeological record? This depends on the questions we want answered and, more important, on how we plan to go about answering them. We suggest that archaeology has confused these points, often asking evolutionary questions but using the wrong view of reality to answer them.

Views of Reality

There are as many views of reality, often termed ontological positions, as there are viewers, all revolving around how one perceives both the nature of being and the process of becoming. In biology and the philosophy of biology a long-standing debate has centered around the nature of species. Are species, as some have argued (e.g., Kitts 1984; Kitts and Kitts 1979; Schwartz 1981), "natural kinds," or classes of objects, that have an essence? Or are species best thought of as individuals, each containing parts that are reproductively compatible with other parts of the individual (e.g., Ghiselin 1966, 1974, 1981; Mayr 1987)? The former position has been labeled *essentialism*, or *typological thinking* (e.g., Mayr 1963, 1976; Sober 1980, 1984), and the latter *population thinking* (e.g., Mayr 1977; see also Dobzhansky 1951). The two positions have very little in common.

The differences between typological and population thinking have been extended to archaeology by Dunnell (1980, 1982, 1986, 1988), with an emphasis on how variation is treated under each approach. An essentialist perspective emphasizes group cohesiveness based on some set of characteristics shared by members and thus views the

phenomenological world as being composed of a series of discrete entities, variation *between* which is of explanatory significance but variation *within* which is noise: "In short, *kinds are empirical*. This view of the nature of reality spawns a particular suite of methodologies which have as their principal objective the segregation of observed variation into significant and nonsignificant kinds in order to extract the essential (hence essentialism) nature of kinds from observed variation, usually by the pursuit of central tendencies. Kinds are explained. Variation is rendered as difference" (Dunnell 1988:16, emphasis in original). Under an essentialist perspective, reality is seen as a unified, locally heterogeneous but universally homogeneous system. Entities are assumed to exist as bounded phenomena. Importantly, time is an elapsed-interval measure, and space is rendered as distance. Single sets of entities ("things") are presumed to be real; therefore, relations between units within a set can be formulated without reference to time or space and thus are redundant, universally true statements. Change is seen strictly in terms of conditionally reversible transformations. The key word is *transformation*. The essentialist view focuses on replacement of one form by another or on the transformation of one form into another (Hull 1965), not on shifts in attribute frequency.

The population-thinking, or materialist, view of reality does not assume that reality is a unified system. Phenomena ("things") do not exist but are always in a state of flux, i.e., in a state of becoming. In other words, relations between phenomena are not timeless, nor can universal statements be made about the relations, because no static set of phenomena exists. Space and time are kept separate, and relations between phenomena are space- and timebound. Under this view, "kind" is not empirical, though at any given moment in time and space we can create kinds based on observation. For it to be otherwise would doom evolutionary biology to a purely metaphysical level.

Materialism views kinds as constantly changing, nonempirical configurations that are observational consequences: "Since neither boundaries nor central tendencies exist apart from the effects of the observer variation is rendered as *change*. Difference may arise only in the epistemological context. Observations may *differ* because they are *samples*, but reality only changes" (Dunnell 1988:16, emphasis in original). The types of questions asked under a materialist perspective are of the "why?" sort. Cause is ultimate, and explanation is

historical. A key point in differentiating between materialism and essentialism is not that the latter treats difference and the former change, but that essentialism treats *only* difference, while materialism treats difference *and* change. Under a materialist view, explanation is tied intricately to observed variation; inferences are made about the nature of change *only* after variation has been identified and measured.

Archaeology and Views of Reality

On one hand, much of modern evolutionary biology clearly falls under a materialist view of reality (Lewontin 1974a) in its focus on variation and change and its emphasis on historical relations. On the other hand, archaeology throughout most of its existence has maintained an essentialist position. Objects are lumped or split into categories (types) according to perceived similarities, and change normally is viewed as transformation of one type into another or as replacement of one type by another. The concept of archetype is not limited to objects but also includes culture areas, a host of temporal and spatial units, and sociopolitical units. For example, regardless of myriad differences between what commonly are thought of as simple (bands and tribes) and complex (chiefdoms and states) societies, differences that are as real as they are apparent, the fact remains that enormous intragroup variation exists among ethnographic examples used to create and support the typological scheme (e.g., Leonard and Jones 1987). Being products of our anthropological upbringing, we naturally have an ad hoc, intuitive feel for whether or not a portion of the archaeological record under examination was left by a band or a chiefdom, but it must be remembered that such an assessment is speculative.

If we can derive methods to make such an assessment, then we ought to be able to derive methods to determine whether the group that created an archaeological site used an Omaha, Crow, or Iroquois kinship system. This clearly is a patently ridiculous idea, but one not so far removed from debates over interpreting the archaeological record in terms of marital residence patterns (e.g., Allen and Richardson 1971; Deetz 1968; Hill 1970; Longacre 1968). The bottom line is that we must admit that the archaeological record is not some extension of the ethnographic present. Unless we subscribe to the view that we have defied selection or tricked it into bypassing us, then we

cannot make extensive use of the present to understand the past. Observations of things going on around us are the same regardless of whether one uses an essentialist or a materialist perspective. But empirical (essentialist) generalizations created from modern observations weaken considerably when pushed back in time, because the phylogenetic histories to which organisms are tied inextricably are obscured, if not lost. This conclusion emphasizes a need to distinguish between empirical and theoretical laws.

Although much of Americanist archaeology falls into the category of essentialism, its practitioners traditionally have asked questions concerning "why" something developed. For example, the origin of agriculture has been a central focus of anthropological interest for decades. Except for the work of Rindos (1980, 1984) and those influenced by him (e.g., Braun 1987; Leonard 1989; O'Brien 1987; O'Brien and Wilson 1988), few attempts have been made to apply Darwinian concepts to the problem (see Binford 1983:203 for an interesting exception). Most attempts employ directional (e.g., Pryor 1986), orthogenetic, and/or flatly operationalistic models that clearly are not designed to answer questions of origins. In short, though archaeologists often are interested in "why?" questions, the strategies they employ are not designed to produce relevant answers. This predicament is summed up by Flannery (1986a:506-7) with regard to his work at the preceramic site of Guilá Naquitz in the valley of Oaxaca, Mexico. The curves to which he refers are graphs of efficiency of a simulated collector/forager group before and after introduction of cultivated plants.

As anthropologists, what we claim we most want to know about are the processes underlying these curves. In our grant proposals we talk about "preagricultural adaptation," about agriculture "reaching the takeoff point," about ancient cultures "achieving a new adaptive plateau." One would therefore expect that our preceramic chronologies would be based on the major landmarks of these sigmoid curves. We might expect to hear statements such as, "Guilá Naquitz E lies near the top of the curve's upswing, just before it levels off, while Guilá Naquitz D lies on the stable plateau formed after the upswing levels off." . . .

Do we hear such things? We do not. Instead, we have living floors assigned to archaeological phases that are based on projectile point styles. . . .

Thus, the whole sequence of plant collecting, incipient cultivation, and gradually developing preceramic agriculture in the valleys of Oaxaca and Tehuacán has been broken down into time segments based on stylistic changes in deer-hunting equipment. . . . We are therefore confronted with a paradox: the processes we wish to document proceed as a series of logistic curves, while our chronologies are composed of linear phases based on stylistic changes in artifacts that may have little or nothing to do with these processes. . . . [S]o long as our evolutionary sequences are tied to stylistic phases, we have an unsolved dilemma.

Flannery's dilemma encapsulates a desire to move beyond an essentialist framework to address evolutionary problems. How, then, do we develop a materialist perspective, and more important, how do we get beyond simply outlining what a materialist perspective should contain to actually using materialist theory in archaeology? The logical step is to examine major components of the materialist perspective—variation and selection—and to demonstrate how they operate within an evolutionary mode.

Evolution, Variation, and Selection

As Haldane (1985:161) noted, the trouble with scientists is that they "either use ordinary words with a special meaning, or invent words of their own which ordinary people do not understand." Evolution and its accompanying jargon are cases in point. Considerable misunderstanding has occurred during anthropological applications of Darwinian evolutionary theory, partly as a result of issue complexity and partly as a result of terms used. Everyday terms such as *evolution* and *selection* carry very different meanings in a Darwinian sense than they do in common parlance. Added to the problem are concepts such as *randomness* and *sampling error* that have slightly different nuances in evolutionary biology than they do in the common anthropological context. The net result is that unless these terms and concepts are defined, confusion will continue to cloud the issue.

As a basis for discussion, we define *evolution* in several interrelated ways. As a phenomenon, it can be defined as the result of heritable variation or as changes in gene frequencies in effective breeding

populations. In a nonphenomenological sense, it can be defined as "a particular framework for explaining change as differential persistence of variability" (Dunnell 1980:38; see Campbell 1970; Lewontin 1970). Note the use of the term *variability* as opposed to *variation*. Variability is the potential for variation to be produced. Selection operates upon actual *variants* that are produced. The above definitions of evolution are not all-encompassing but rather represent a middle-ground position that focuses attention on variability. They tell us nothing about the origin of variation or about synchronous variation (Mayr 1982:400).

The subject of evolution is change, not similarities and differences. Change as used here is a selective rather than a transformational process. Care should be exercised, however, in not pressing this point too far. Biologists (e.g., Mayr 1982) often use *transformation* in a general sense to refer to the vertical (adaptive) component of change through time. In fact, Darwin's (1859) phrase "descent with modification" embodies the concept of transformation. However, it is clear that biology views transformation (change) as shifts in frequencies of units rather than as a simple chameleonlike alteration of one unit into another.

The Darwinian model of evolution embodies essentially a two-step process. Indeed, identification of the process is one of Darwin's chief contributions to the theory of evolution. The first step is the generation of variation within the phenotypic pool. Variation occurs initially at the level of the genotype, but it is at the level of the phenotype that Darwin's second step, the selection of variants, takes place. In biological evolution variants result from mutations at the level of the gene or from novel recombinations of existing gene stocks. How the variation is produced is largely immaterial in understanding the effects of evolution, a fact underscored by Darwin's own ignorance on the matter. What is important, however, is that the production of variation is *undirected*, which effectively uncouples Darwinian evolution from other evolutionary theories.

The distinction between directed and undirected production of variation is so critical to Darwinian evolution that its importance is difficult to overemphasize. The evolutionary schemes developed in part by Lamarck for biological phenomena and by Spencer for human cultural phenomena both emphasize directed changes that permit survival in the face of changing environments. In Lamarckian theory a force directs the pool of variation to produce variants that allow an

organism to adapt to changing conditions. These variants then become favored in the course of reproduction and transmission. How would Lamarck explain the facts that polar bears are white and giraffes have long necks? It would not be because of the random occurrence of a white mutant among the thousands of brown or black bears born and the chance appearance of a slightly longer-necked giraffe among the short-necked throng, which in turn are selected for and thus pass on their coloring and shape. Ultimately, Lamarckism views organisms as being the way they are because they *need* to be white or they *need* long necks. Selection becomes not only unnecessary but also undefinable (Rindos 1989). Evolution, then, becomes nothing more than a set of solutions to problems presented by the environment (Lewontin 1983). This is precisely the way the concept of evolution usually is employed in anthropology.

Although Darwinian evolution has undirected production of variation as a central tenet, it is absurd to claim that variation is generated randomly in the mathematical sense of the term; i.e., that the process is purely stochastic and unbounded. Clearly, humans will not exhibit variation in tail length, nor will dogs exhibit phenotypic variation in wingspan. Organisms are tied to their phylogenetic histories; thus variation at any point in time is bounded by the nature of the organism itself. This point will be of paramount interest when we later discuss the random component of evolution.

Important as variation is, it is in no sense causal. As Dobzhansky (1982:119) points out, "variations may be compared with building materials, but the presence of an unlimited supply of materials does not in itself give assurance that a building is going to be constructed." Construction of the building requires selection, the ultimate arbiter of *fitness*. Perhaps the single most common misconception about selection involves equating it with force. Just as relativity in general replaced the concept of force in physics, Darwinian evolution requires no active force for selection. Rather, selection is viewed as a process by which variants are replicated differentially. The use of force-denoting terminology is acceptable only as long as selection is not allowed to assume a goal-directed quality. Unfortunately, the English language is ill designed to deal with concepts except in terms of actor and action. For anthropology the issue is confounded by the strong progress-oriented roots of Western thought. This might not present a problem if it did not result in selection usually being identified with some physical aspect of the environment—a perception

that generally leads to purely mechanical models of cause and effect that simply dress up existing orthogenetic models in selectionist terminology. Rather than vague evolutionary forces, these models posit equally vague "selective forces" or "selective pressures" that lead to greater adaptation—examples of what Lewontin (1977) terms "idle Darwinizing."

If selection is to be invoked, two distinct concepts—*selection of* and *selection for*—must be kept separate. Sober (1984:99) presents a useful example of the difference between the two: Balls of four different sizes are inserted into a plastic cylinder that contains four horizontal levels, each of which contains a series of equal-size holes. The holes on each level are larger than those on the level below. The object is to get as many balls as possible to the bottom of the cylinder. Shaking the cylinder distributes the balls to their respective levels. After performing the operation, we notice that all the balls on the bottom level are green. Balls on each of the other levels also are segregated by color. The selection process obviously selected the green balls, so there must be some reason why. In other words, they must have some property that was being selected for. But is that property smallness or greenness? Obviously it is smallness. There was *selection of* green objects, but no *selection for* greenness. Put another way, we can speak of *selection of* objects and *selection for* properties. (It is useful semantically to contrast selection "of" and selection "for," but this can obscure the fact that selection actually is selecting *against* phenotypes that do not exhibit the feature.) If selection for a particular property occurs (in reality, selection against phenotypes not possessing the property), then we can say that a certain causal process actually is in motion (Sober 1984:100). This causal process should not be confused with the naïve notion of cause and effect, i.e., a drive toward adaptation, embodied by directed variation.

If we eliminate direction, it seems we are tampering with the sacrosanct. Direction, relative to humans, implies intention, and intention is, if not the soul of Western man, at least a close approximation thereof. The role of human intentionality and motivation is perhaps the single most misunderstood concept in selectionist theory. It certainly elicits the most visceral and reflexive objections from critics of evolution. Humans are doers. Humans are thinkers. How, then, can human rationality and intent be ignored and discounted? The answer to this question is that they cannot, nor should they be. But, as Steward (1956:72) noted over thirty years ago, "a specific invention

is not explained by saying that man is creative." Far from discounting human intentions, selectionist theory assigns an important role to them—that of introducing variation into a cultural system. Variation, not intent, is the important ingredient. As we noted above, in biological evolution variation results from mutations at the level of the gene or from novel recombinations of existing gene stocks. However, in cultural systems, variation results in part from differences in human perception and intention. Indeed, it might be argued that the so-called genetic capacity for culture that humans are supposed to possess is actually a capacity for intentional behavior (Rindos 1985, 1986).

We speak colloquially of intent as a human behavior in which the mind conjures up an idea and the body carries it through. Implicit in most discussions of intent is that an action—the phenomenological result—is the intended outcome of an idea. If this were the case, i.e., that the human brain were capable of predicting all outcomes and ramifications of an idea, then the only variation possible would be that which was intended. We then could think of ideas themselves as sources of variation and results as directed variation. But results of ideas often do not mirror what the actor had in mind; i.e., the outcome was not predicted or even predictable (see Reynolds 1984). Consequences of what appear to be the simplest of actions spawn outcomes *ad infinitum*, spinning off a dizzying array of variation upon which selection can operate. Consider how many significant inventions and discoveries have come about because the inventor was focusing attention in some other direction. From Columbus to Kepler, humans have had a proclivity for succeeding in spite of their intentions—what Koestler (1959) terms "sleepwalking."

At an intuitive level, no one, Darwinist or not, would argue that a human who sits down to manufacture a projectile point does not *intend* to reduce a piece of stone into a useful object with which to hunt. Likewise, dropping a seed into a hole is done with the intent of having a plant to harvest. In one sense we can speak, rather trivially, of intent being a proximate cause of something, but of what analytical value is such a statement? Proximate causes, in any scientific framework, are functional causes, i.e., how things work. To invoke intent as an explanation robs valid functional questions of their interesting parts and replaces them with vitalistic, directional components.

Flannery's solution to the earlier-cited dilemma over how to understand change in the preceramic levels of a small rockshelter in Mexico is an example of using human intent as an explanatory mechanism. His concern is to give equal billing to proximate and ultimate (evolutionary) causes of change in the paleobotanical record in highland Mexico, the former emerging from "worldwide end-Pleistocene phenomena" (1986b:519) and the latter from identifying "the selective advantage conferred on early cultivators, and the fact that they became the means of seed dispersal for a group of plants whose genetic program they had altered, in defiance of natural selection" (1986b:519). Flannery's plan is laudable except in one detail. He cannot escape the idea that human intent is part of ultimate causality. Otherwise he would not use the phrase "in defiance of natural selection." To state that natural selection can be defied by intentional action shows little regard for the power of selection and completely confuses the way in which selection works. Clearly, natural selection continues to operate on *Homo sapiens* (see Dobzhansky 1960, 1961), as it does on other organisms. We have, with technological and medical advances, perhaps modified some of the aspects of selective pressure, but this in no way implies that we have defied selection. With regard to plants, humans simply have become selective agents, albeit very effective ones.

Undoubtedly some of the confusion surrounding the nature of selection can be attributed to use of the concept *cultural selection* (Rindos 1984, 1985, 1986; see also Boyd and Richerson 1985; Cavalli-Sforza and Feldman 1981; Durham 1978), the basic tenet of which is that evolutionary processes can act independently on genes and on culturally transmitted behavior. No one should doubt Mayr's assertion that "Behavior is perhaps the strongest selection pressure operating in the animal kingdom" (1973:388) or Rindos's statement that learning is "one of the most important determinants of behavior" (1986:316). If variation of a cultural trait, whether by accident or design, confers an increased fitness on an individual, and if the trait is inheritable or transmissible by any means, then all else being equal, the trait will be represented differentially in the next generation. All that is important to the selectionist, then, is that variation is available in the cultural phenotype. For this reason selectionist theory is not inherently antagonistic to structuralism and cognitivism, because the selectionist model is not dependent on how

variation is produced. Likewise, an evolutionary approach is not at odds with functional models, which are used to structure explanations of how the phenotype operates.

Having considered briefly the concepts of evolution, variation, and selection, we now are in a position to consider two complex, interrelated issues—adaptation and neutrality—that are as critical for archaeology as they are for biology.

Adaptation

Adaptation, a venerable icon in archaeology and anthropology, as well as in biology, often is afforded the vacuous role of being an *ex post facto* accommodating argument used to “explain” the persistence of organisms or traits over time. As Mark Twain is said to have noted, “How wonderful nature is: Our legs are always just long enough to reach the ground.” The archaeological record is largely a record of successes; things that leave an archaeological trace, by definition, are adapted. In this sense, archaeological remains are no different from other sort of fossil remains. Thus, when we compare variation in the archaeological record, we usually are not comparing successful variants with *unsuccessful* variants but rather successful variants with other successful variants—a subtle but important distinction. Explaining why the walls of ceramic vessels thin through time from 8 mm to 5 mm is not the same as explaining why ceramic vessels have walls 5–8 mm thick rather than 100 mm thick. The former compares two “adapted” forms, while the latter contrasts successful forms with a clearly unsuccessful variant, and more important, with a variant unknown in the archaeological record.

Evolutionary biology views adaptations as features that increase the Darwinian fitness of an organism, i.e., as results of natural selection working on genetic variation. Mayr (1988:135) defines *adaptedness* as “morphological, physiological, and behavioral equipment of a species or a member of a species that permits it to compete successfully with other members of its own species or with individuals of other species and that permits it to tolerate the extant physical environment.” Analysis of adaptations comprises two essential components: showing that a trait was selected and specifying the nature of the selective agent or agents that favored the trait (Futuyma

1986:251). Adaptation through selection sometimes can be inferred even though the adaptive value of a particular structure is unclear. For example, paleontologists might not agree on the functional role of the dorsal plate on *Stegosaurus*, but "one cannot escape the conclusion that it could not have evolved except through natural selection because it provided some benefit to the possessors of this structure" (Mayr 1988:138). Identifying the specific nature of the selective agents that favored a trait is more difficult. One problem is that not all features of organisms, including material remains and/or behaviors that created the materials, are adaptations. We often assume that traits of objects occurring in the archaeological record had adaptive ends—an outlook clearly in the realm of Gould and Lewontin's (1979) "adaptationist program." But some traits may simply be consequences of physical laws or of developmental constraints, in essence consequences of engineering limitations. For example, Early Woodland potters may have had to use thick walls to construct large ceramic vessels. They were not selecting *for* thick walls (in fact, they may have desired thin walls), but to make serviceable large vessels they had to thicken the walls. Other traits may be the result of integrated development. Gould and Lewontin (1979) suggest that the human chin is not adaptive. In a sense, the chin does not exist as a true feature, being simply a consequence of different growth rates in parts of the mandible during the course of human evolution. Features also can change their function or *become* functional in the face of new selective pressures—*exaptations* in the parlance of Gould and Vrba (1982).

Neutrality

As if the problem were not complicated enough, we are faced with the concept of neutrality, commonly defined as a state in which traits have neither positive nor negative selective value, but defined more properly as any of several *different* states of a single trait, each of which confers equivalent fitness on the bearer *within a given environment*. Debate over neutrality has assumed an important position in biology (e.g., Kimura 1968, 1979, 1983; King and Jukes 1969; Lewontin 1974b), though almost exclusively at the genic level, and a lesser position in archaeology (e.g., Dunnell 1978, 1980; Leonard and Jones 1987; Teltser 1988). Gould (1977) suggests that

detailed engineering studies of structures may help to distinguish neutral from nonneutral traits, and Sober (1984:81–82) offers a strategy he terms “bootstrapping” for obtaining an independent criterion for assessing fitness: “One may begin by investigating fitness *ex post facto*—that is, by looking at differences in survival and reproductive success in one population and reasoning that these were a consequence of certain fitness differences. Then, when confronted with a second, similar population, one may be able to use one’s previous experience to reason in the opposite direction. The hypothesis would be that the characteristics that determined fitness differences in the first population also do so in the second.”

Both methods of determining whether traits (features) are adaptive rest on inference. Engineering studies can show us how certain structures confer on an organism the ability to run fast, but *we* make the assumption that speed has something to do with outrunning predators, based on our knowledge of predator-prey relations. Likewise, we can relate the presence of water-retaining mechanisms in a host of different plant species to their distributions in xeric environments. We can use Sober’s method to examine features in one group vis-à-vis survival and reproductive success, where our inferences are on fairly solid footing, and then apply those results, inferentially, to another group. This appears to us to be the proper use of ethnographic data in solving archaeological problems.

At this point we turn our attention to a critical question that, if not examined carefully, can lead to gross misinterpretation: Are features that are evident in the archaeological record capable of providing insights into the reproductive success of their bearers and if so, how can we elicit such information? It appears reasonable to assume that the archaeological record contains fossilized traces of traits that served an adaptive purpose, i.e., that directly affected the reproductive success of their bearers, as well as traits that did not. It also appears reasonable to assume that the “neutrality” of some traits was lost, i.e., the traits became functional, because of environmental change. Under the new environmental regime, previously neutral traits directly affected the fitness of the bearers. There should be no argument with these assumptions. At this point we are less interested in the source of the traits and, as we explore below, more interested in how the traits might behave through time.

And, lest we be misunderstood, we are *not* suggesting that all traits can be modeled using a genetic analog, and we certainly are not

suggesting that the sole focus of an evolutionary archaeology is on traits. Often forgotten is the point that traits in and of themselves are not fit; they only confer (or do not confer) fitness on an organism. Traits, regardless of what they are traits of, do not reproduce. Organisms reproduce, and the features tag along accordingly. Therefore it makes sense ultimately to examine populations and to define a population in terms of members having one or more specific traits in common. Is this perspective at odds with our earlier discussion of typological versus population thinking? Not at all. Remember that populations, like species, are always in the process of "becoming." They take on certain traits, and they give up certain traits. And regardless of *how* populations receive the traits, the traits *may* affect fitness. We simply are asking if we can distinguish between features that do and do not affect fitness.

Leonard and Jones (1987) argue that traits themselves exhibit a type of success, *replicative success*, that may or may not affect reproductive success (Dunnell 1988; Rindos 1985). Cultural features obviously can be passed along horizontally or obliquely (Boyd and Richerson 1985), and thus little is to be gained by modeling either the features themselves or how they are transmitted using biological analogs. Importantly, and we cannot stress this enough, replicative success implies neither a mode of trait transmission nor that a selective advantage is attained by an organism as a result of trait transmission and retention. Leonard and Jones (1987) echo this caveat. Success is gauged *solely* in terms of variance in trait frequency through time. Again, the source of variation is an issue separate from whether selection is operating on a particular trait, as is the issue of how differential representation of traits relates to individual fitness.

Care obviously must be exercised in distinguishing between reproductive and replicative success. Consider the case of sand- and shell-tempered ceramics. If an archaeological site is excavated and found to contain both "types" of ceramics, and if it can be shown that the mean frequency of shell-tempered vessels increased over a period of time at the expense of sand-tempered vessels, two conclusions can be drawn (assuming, for the moment, that postdepositional processes affected both types equally and therefore can be held constant): (1) the population was segregated into shell-tempered-vessel users and sand-tempered-vessels users, and the former expanded at the expense of the latter (reproductive success); or (2) everyone in the population used both temper types, and shell tempering increased at

the expense of sand tempering (replicative success). Leonard and Jones (1987:213–14) go directly to the center of the problem: “While the differential reproductive success of individuals may influence the *expression* of cultural variation, it does not determine it. How then is the fitness of cultural traits evaluated?” (emphasis added). Replicative success depends simply on an increase in frequency of the trait over time. In terms of reproductive success, *possession* of a trait *may* make individuals more fit by giving them a selective advantage over individuals not possessing the trait. But if possession of that trait has no effect on the reproductive success of the possessor, then the trait is neutral.

As an initial step in sorting out adaptive from neutral traits, we might propose that traits in each category behave differently over time when their frequencies are plotted. If we know how adaptive traits behave relative to neutral traits, then we should be able to compare the behaviors of individual traits to the established patterns as a first step in studying effects of selection. Three things must be kept in mind while reading the following section. First, although we divide traits into categories, there is, at this point, no a priori reason for doing so other than to follow standard terminology. Second, although we often refer to a *trait* as conferring fitness on the possessor, what we really are referring to are the differential fitness values one particular *state* of a trait may confer relative to another state of that trait—similar (not analogous) to alleles of a particular gene locus. Third, and most important, our discussion does not cover the wide topical range that deserves treatment under a selectionist perspective. Primary emphasis is placed on clearing up what we see as a confusing issue at this stage of theory development—the difference between style and function and how stylistic (nonfunctional) and functional features behave through time.

Kinds of Traits

Three broad categories are used widely in archaeology to partition traits and, by extension, objects found in the archaeological record: technological, functional, and stylistic. Similar categories are employed to tie a wide range of traits to either technology (e.g., Binford's [1962] technomic, sociotechnic, and ideotechnic categories) or function (e.g., Rathje and Schiffer's [1982] techno-functional, socio-

functional, and ideo-functional categories]. The explicit separation of what loosely can be termed style and function has a long history in Americanist archaeology (e.g., Binford 1962, 1968; Jelinek 1976; Sackett 1982), based on the assumptions that each refers to different kinds of empirical phenomena and that each is produced by a different process. Dunnell (1978a) ties the concepts of style and function into an evolutionary framework by using *function* to refer to those forms that directly affect the Darwinian fitness of populations in which they occur and *style* to refer to those forms that have no detectable selective values.

Dunnell's (1978a:200) definition of function is a departure from traditional use of the word in archaeology, i.e., the mental association we make between an object and what it is (was) employed to do. But, as he emphasizes, function used in this sense, especially when terms are employed that draw upon contemporary forms for meaning, denies the possibility of evolution of separate functions. Features, or traits, that confer Darwinian fitness on an organism may arise among different breeding populations as independently generated features (analogs) or as products of common developmental histories (homologs). Analogs are ahistorical in that they arise not from common phylogenetic backgrounds 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 them from consideration.

In biology, homologs are differentiated from analogs on the basis of structural differences and developmental histories, though recent analysis at the molecular level (e.g., Sibley and Ahlquist 1985) has shown that mistakes can be made in assignment. 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 are features similar in *form* but different in structure. Homomorphy is a *superficial* resemblance due strictly to convergence and not to common origin. Axes can be considered homomorphs. 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.

Stylistic features commonly are treated as homologs based on the assumption that styles are so complex that the probability of duplication by chance (analogs) is astronomically low (Gould 1986). Following this line of reasoning, occurrence of similar stylistic features among separate groups of people can be accounted for only by diffusion, migration, or some similar mode of transmission. One might argue that functional features, in the sense used by Dunnell, also can be transmitted, and we would agree. But again, care must be exercised in distinguishing form from function.

Dunnell (1978a) makes the rather curious and perhaps counterintuitive statement that style is neutral. Misinterpretation of what we believe Dunnell's position to be has caused some confusion in the literature (e.g., Rindos 1989), and we believe the position needs clarification. We begin by examining Dunnell's (1978a:199) view of 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 is correct; variants under selective control behave differently than do those not under selective control. Using a biological analog, and in extremely simplified form with no attention paid to intervening agents, we expect an allele that is being selected *for* (or more precisely, an allele that is not being selected *against*) to begin at some arbitrary point above zero and to increase in frequency at a steadily decelerating rate toward some optimal value (Fig. 2.1). This gives selection its apparent directional component. Selection *against* the allele (in reality, selection against bearers of the allele) reverses the trend and sends the curve downward. Two possible outcomes exist: Either one allele eventually disappears from the genotype or, if each allele confers equivalent (not necessarily equal under all environmental conditions) fitness to some of the possible bearers, the result can be a balanced polymorphism.

On the other hand, an allele not under selection can drift through a breeding population from generation to generation, as shown in

Figure 2.1. Hypothetical changes in gene frequency in an allele under selection and in one under random drift for 25 generations. (After Lasker 1976, Fig. 8.1)

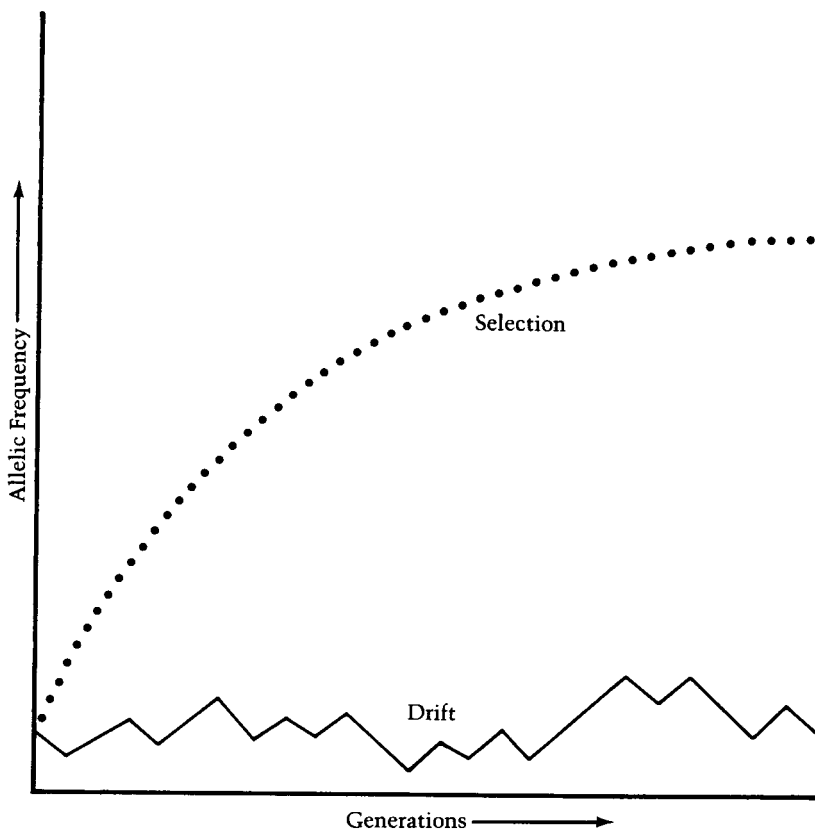


Figure 2.1. Its frequency fluctuates randomly, sometimes in one direction for a few generations, then in another, and so on. Given infinite time, one of two outcomes will occur: Either the allele will hit zero and thus be eliminated from the population, or it will reach one (100%) and be fixed in a population. If zero is reached, the allele will remain absent from the population unless it is reintroduced.

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. In theory, stylistic traits of pots act no differently than other stochastically propelled

traits. But, one might ask logically, how do we get from the randomly fluctuating pattern seen in Figure 2.1 to the neatly defined battleship curves of a seriation? Life histories appear orderly, whereas a random walk has no similar order. The question becomes, Do battleship curves—life histories—reflect a random distribution? The answer is, yes, they can. Even randomly generated life histories (e.g., Gould et al. 1977, 1987) can exhibit apparent order. This is neither startling nor revolutionary but simply the product of the Markovian side of evolution's nature, where the form of each state is in part dependent upon the previous state. 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 (Gould et al. 1977; Raup and Gould 1974; Raup et al. 1973).

The level at which style is analyzed also plays a part in the difference between the random-walk pattern and battleship curves. Figure 2.1 illustrates what happens to a *single* allele that drifts; battleship curves illustrate life histories of complex units composed of many elements. The latter are much easier to construct and may convey more information, but they tell us nothing about shifts in frequency of individual states in which a trait might reside. The difference between the random pattern seen in Figure 2.1 and the curves seen in seriation diagrams is due in part to the Markovian nature of style. Styles often are constructed from other styles; they build on previous developments, with one step leading to another. Selection will attempt to modify any variation that comes along, but no modification is made from scratch. Thus, evolutionary pathways are said to be "channeled" (Mayr 1988:108), with some developments being possible and others not. This is best illustrated by the nature of Bauplans (what comparative anatomists used to call the morphotype) of many organisms. Bauplans can be preadapted to dramatic evolutionary departures, as were the lobe-finned coelacanths for terrestrial life or the early archosaurs for flying (whereas no amount of natural selection will preadapt the Bauplan of a turtle for flying). "The existing genotype, thus, prescribes definite channels for future evolution" (Mayr 1988:109). Likewise, styles in many cases are products of their histories, and a *range* of variation is established for the production of new styles. Most variants that arise anew fall within the production range. Importantly, any of the variants may be capable of conferring

fitness (reproductive and/or social) on the bearer, if indeed fitness is even conferred.

In summary, we agree with Dunnell's (1978a) definition of style as neutral, but as we explain below, this definition does *not* imply that style can be ignored in an evolutionary framework. Dunnell never argues that it should. 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. What we see as the persistence and spread of stylistic traits may speak more about the fitness of the trait in terms of itself (replicative success) than about the success of the possessor(s) of the trait. And, as we point out later, traits that are stylistic (non-functional) under one environmental regime may take on functional roles in a different environment. Although we use the terms style and function, we strongly advocate abandoning the former term—because of both its connotations and the essentialness of the dichotomy between style and function—and substituting the term *nonfunctional*. Also, archaeology will have to come to grips with the fact that social success and reproductive success may, at times, have little to do with each other.

Style in Evolutionary Perspective

Drift has filled an important role in our understanding of the nature of evolution ever since Wright (1931, 1932 [see also Dobzhansky 1982]) introduced the concept. In its early years it was used to explain the presence of apparently adaptively neutral interspecies differences. But later, when it was shown that many of the supposed neutral differences were of adaptive significance, the concept took on even greater importance by emphasizing that drift provided *new* variation upon which selection could act (Provine 1983:65). For example, at every point (generation) along the X axis in Figure 2.1, a randomly generated frequency of the allele will occur in an effective breeding population. But we have no way of predicting whether an allele that is drifting might come suddenly under selective control when its presence becomes advantageous. And we also have no way of predicting if and how an allele at a given frequency might interact with another allele to produce a polygenic trait that begins to come under selective control.

To use a biological example, certainly in the phylogenetic history of a lineage a wide range of body color may have been available, conditioned only by historical constraints imposed on the genome. At a given time, any in a wide range of colors may have an equal chance both of arising in a phenotype and of staying in the pool of variation. We might speculate that two conditions remain random. First, predators are blind to color differences; i.e., they do not select their prey on the basis of color. Second, organisms of any color within the range have equal chances of being successful (e.g., of attracting mates). These conditions are maintained for a while, but then, because of drift, certain colors disappear from the population, the range of variation is reduced, and certain colors become dominant. Perhaps females begin selecting males of a certain color as their reproductive partners and excluding males of other colors, further reducing the variation range. Alternatively, a new variant may initiate selection. Importantly, though we might consider color to have been neutral at one time, we have not addressed the question of the fitness of individuals who fell outside the acceptable range. Neither have we assessed the environment in which a feature arose nor the environmental changes that occurred during the lifetime of the feature.

Animal coloration and patterning are examples of neutrality under certain conditions and nonneutrality under others. For example, newly hatched grouse and ptarmigan chicks exhibit a wide range of plumage patterns, and while the patterns provide camouflage, the interspecies differences probably have little adaptive significance (Johnsgard 1983). Coloring on the moth *Biston betularia*, however, is adaptive because of the nature of the selective environment. Kettlewell's (1961) famous experiments with the peppered and dark forms of the moth demonstrated the correlation between changing environments in industrialized England and the decline in the peppered morph. Trees covered with lichens, against which the peppered morph was camouflaged, became soot-covered after industrialization, providing a protective haven for the dark morph but allowing the peppered morph to be picked off by birds. Clearly, coloration under those conditions is not a neutral trait. Similarly, red paint on the interior, exterior, or both surfaces of a clay vessel may provide little more than decoration. If, however, a red slip is used to seal a permeable surface, as on early shell-tempered pottery in the central Mississippi River valley (Dunnell and Feathers 1986), then the trait could have functional significance.

In the case of nonhuman organisms we often see a clear and unambiguous source of selection. In the case of humans, however, the source of selection is tied up in intent, or so it appears. We often argue that humans select ceramic styles, dress lengths, and automobile models on the basis of culturally influenced choice. Obviously this statement contains some truth. It also is true that after a time our choices change, in large part because of new variants that arise. An expanded range gives us more options from which to select. But, as Rindos states, "Man may indeed select, but he cannot direct the variation from which he must select" (1984:4). This is as true for selecting one style of dress over another as it is for selecting one species of butterfly over another because of its bright colors. The seemingly capricious (but patterned) choice by butterfly collectors of a butterfly of one color over another is as potent an agent of selection as is the choice by any biologically driven bird. In both cases the butterfly meets a less-than-happy end, and the composition of the gene pool to which it contributed is adjusted accordingly.

Pools of variation that make up cultural features—for example, the pool comprising women's dresses—change at exponentially faster rates than do gene pools. Variants are introduced, they are acted upon immediately, and they run their course toward fixation or elimination. We argued earlier that stylistic features—composed of variants arising from culturally derived pools—are important components of an evolutionary-archaeological perspective. One approach to the study of style comes at the problem not from the point of neutrality versus nonneutrality but from the point of view developed earlier: Is there a *range* in variation of a feature, accepting that individual values within the range are neutral, that, as long as organisms fall within that range, confers fitness on the bearers? Note that this is an entirely separate issue from whether style is neutral.

We pose the problem not from the standpoint of group fitness, which clearly is an area ripe for examination archaeologically, but rather as an alternative to measuring individual fitness values for various states a trait might reside in. The drive toward measuring fitness values of any trait often is based erroneously on an assumption of optimality—over the long term only the fittest survive. Overlooked is the fact that "it is sufficient to be superior and not at all necessary to be perfect" (Mayr 1982:589). One might even suggest that it is sufficient simply to be adequate. Examining the range of variation possible before selection occurs works with both neutral

and nonneutral traits. We use decorations on ceramic vessels, usually inferred to be stylistic phenomena, as an example.

It seems reasonable to assume that pottery designs carry information, and that the information can be read by those who know the grammar. Several theories concerning the role of decoration relative to social integration and information exchange have been developed (e.g., Braun and Plog 1982; Graves 1985; Wobst 1977), and although the theories differ over the precise role played by decoration, it is seen as something that serves as social identification. Under certain conditions it might be advantageous to participate in the social system identified by a set of decorations and to make those decorations part of one's phenotypic expression. By participating, one increases his/her success—new mates are available, food can be shared in times of stress, and newfound partners can be counted on for defensive purposes.

From the standpoint of the group making and decorating the vessels, there is an acceptable range of decorative variants. It is irrelevant whether the group uses circles, squares, or spoonbills as decorative motifs. Among nonhuman animals, a wide range of phenotypic expressions also is possible for some traits, and, as it is with humans, the question of why *particular* variants arose is immaterial. For a group as a whole, some traits might, in one sense, be considered neutral. For example, Darwin's finches exhibit a wide variety of beak shapes and colors, which are used by potential mates to recognize conspecifics. But what about the finch that is born with a purple-colored toucan's beak and thus cannot attract a mate? The point is, although at one scale some traits can be thought of as being neutral, we cannot blindly assume that all individuals will fall within the range of neutrality. Among humans there is apparently a wide range of equivalent fitness values for some traits. And, importantly, the range constantly changes. As soon as individuals fall outside the range, they could be selected *against*.

Several points need reiteration lest our position be misinterpreted. First, selection, regardless of the "type" of selection, maintains a constant vigil over a pool of variants. By definition, if new variants are acted upon by selection, they have an effect on the success of the relevant organisms. It is worth remembering that variability subsumes all the variations that could ever arise during any time and in any place. However, certain restrictions (channels) are placed on which variants can arise at any particular place and time—restric-

tions that are tied to developmental histories and physical laws. But history is full of surprises—features that arise seemingly out of nowhere and lead us to wonder about their origins. The archaeological record contains such features, and in one sense they are similar to biological variants. A clay-lined basket that happened to be dropped into an extremely hot fire and, when the fired clay lining was retrieved, was found to hold water is such a feature.

Second, just because selection monitors the pool of variants does not imply that each variant that arises necessarily comes under selective control. If possession of a trait in *any* of its variant states confers no additional fitness on the possessor, then those states can be termed, for analytical purposes, neutral. Although a trait may be neutral at one time and in one environment, a change in conditions can bring that trait under selective control. Neutrality can exist at the scale of the group, where most members exhibit a trait variant that falls within a tolerance range, but if an individual exhibits a variant outside the range, he or she might be selected against.

Third, although we maintain a distinction between functional and stylistic traits, we do so with the caveat that any such distinction is only an analytical device, not a measure of neutrality. Without close inspection of the behavior of any given trait, the assignment of that trait to either category is based solely on intuition. Although it is tempting to make a priori assignments of features to the category of style, mistakes can be made. By definition, anything that does not behave through time in a random manner cannot be assigned to style and thus cannot be neutral. The problem, then, is how do we demonstrate neutrality? Jacob observed that “natural selection does not work as an engineer works. It works like a tinkerer—a tinkerer who does not know exactly what he is going to produce but uses whatever he finds around him” (1977:1163; see also Lewontin 1978). Perhaps this should be our guide. Biologists face the problem of determining the role of chance by first attempting to account for biological phenomena as products of natural selection. “Only after all attempts to do so have failed [are biologists] justified in designating the unexplained residue tentatively as a product of chance” (Mayr 1983:326). While Gould and Lewontin (1979) have ridiculed this try-and-try-again approach, proponents (e.g., Mayr 1988) point out that this is precisely the strategy employed in all other branches of science. Sober raises a related issue: “Even when selection accounts for the evolutionary trajectories found in each of several populations, it

needn't follow that *differences* in morphology, physiology, and behavior must have adaptive significance" (1984:26). In other words, to use Lewontin's (1978) example, rhinoceroses presumably developed horns as a means of defense. But why do Indian rhinoceroses have one horn and African rhinoceroses two? Did a chance process produce the traits in question, or did two ancestral populations "find" a similar solution to the problem, with slightly different phenotypic results? Clearly the latter. In other words, "historical differences, rather than differences in selective significance, may account for variation" (Sober 1984:26).

Fourth, the scale of analysis can affect observations of trait behavior and thus interpretations as to whether the variable states exhibited by a trait are under selective control. For example, tempering agents included in ceramics are not, at least in most modern archaeological thinking, considered to be stylistic features. And yet at the scale of the southeastern United States, different tempering agents came in at various times, became popular, and then were replaced by other materials. Localized histories many times reflect regional-scale history, and thus temper classes can be used to order sites chronologically. How, then, do we determine whether changes in tempering agents resulted from selection? One step is by being able to separate analogs from homologs. Is it possible to separate convergence—similar solutions to similar problems—from divergence? Functional traits can be products of homology, but we would not expect identical stylistic traits to be products of analogy. Another possible solution to the problem is through understanding ceramic technologies from an engineering standpoint (e.g., Bronitsky 1986; Bronitsky and Hamer 1986; Feathers 1989; Schiffer and Skibo 1987). As we discuss briefly in the next section, engineering studies, whether of objects manufactured by additive (e.g., ceramics) or subtractive (e.g., stone tools) processes, have given us entirely new perspectives on the interrelatedness not only of technological properties within an object but of the functional roles played by the objects. Whether inferences made relative to objects' functions later are shown to be inaccurate is immaterial. Engineering studies and the accurate measurement of trait changes over time place the inferences on much sounder footing than is otherwise possible.

If variation—the fuel for selection—is of analytical concern, then objective means of identifying and measuring variation are needed.

We have, to this point, examined trajectories that traits can take through time; we discuss below several topics related to measuring variation in the archaeological record.

Measuring Variation

How we choose to measure variation and at what level we choose to measure it are tied closely to the questions asked. Americanist archaeology's essentialist philosophy has ensured that the concept of type, or kind, has been at the heart of analytical procedures. Types are formed on the basis of degree of centralness exhibited by analytically chosen attributes, the underlying assumption being that a certain amount of noise—deviations from central tendencies—can be tolerated, especially in light of human idiosyncrasies. When a critical threshold is reached, i.e., when the analyst decides that too much noise is present, new types are created to account for the noise. One critical issue is that thresholds rarely are delimited by the analyst. Thus at some point hafted, bifacially flaked objects with concave bases begin to grade into similar forms that have slightly concave or even straight bases. Projectile-point-type guides are replete with examples of types that are identical except for morphological characteristics due entirely to things such as stage of wear and degree of maintenance (Hoffman 1985).

As mentioned previously, use of the type concept in archaeology has transcended material remains. For example, archaeology is involved in a constant search for archaeological correlates of sociopolitical types borrowed from ethnology. Such an exercise hinges on the perceived ability to deduce the key criteria of groupings such as bands and chiefdoms, to reduce the list of criteria to those that might leave traces in the archaeological record, and then to search for traces that fit preconceived models. The result of such an endeavor usually is a loose fit between archetype and archaeological example, and it is up to the analyst to decide the goodness of fit (and to convince others as to how good a fit it is). Lack of fit often is attributed to the paucity of the archaeological record when in reality it is because of variation, which we have dismissed so cavalierly as noise.

As so often is the case in archaeology, form, function, and structure are conflated, making it difficult, if not impossible, to sort out

analogs from homologs. Implicit in the concept of social types are formal, structural, and/or functional tendencies, often intertwined to the point that they are difficult to separate. For example, kinship systems have formal properties (e.g., symmetry versus asymmetry), structural properties (e.g., who may marry whom), and functional properties (e.g., control over reproductive rights). Again, the constructs used in ethnology (e.g., kinship systems and sociopolitical types) emphasize central tendencies and ignore variation. When enough central tendencies change—for example, when a group “shifts” its economic pursuits from hunting and collecting to “agriculture” and concurrently intensifies its emphasis on kinship—then the group has become more tribelike. In this sense ethnology has been more akin to the catastrophism of Cuvier (stability over time with abrupt transitions) than to the uniformitarianism of Lyell (gradual but continuous change).

The inability to distinguish between form and function also has affected the way in which archaeological objects are viewed. The use of shape-related, or morphological, characteristics to derive functional meaning has a long history in archaeological reconstructions. Objects are given names based on the analyst's perception of how they were used, which derives from an a priori assumption that modern objects, similar in shape or design to older objects, can be used to infer function. As time slices move farther and farther apart, analogies between ethnographic function and prehistoric function become more speculative, and it is impossible to show equivalency.

Clearly, traditional types are inappropriate for examining functional aspects of the archaeological record. What, then, are alternative avenues? One option is paradigmatic classification (Dunnell 1971, 1986), so termed because classes are defined on the basis of the paradigm deemed suitable by the analyst for solving a problem at hand. The essence of paradigmatic classification is in its use of intersecting attribute states to create classes, with the attributes actually being the units of variation contained under analytically predetermined dimensions of variability. For example, if two dimensions, X and Y , are used to classify a set of objects, the intersections of attribute states $X_1, X_2 \dots X_i$ and $Y_1, Y_2 \dots Y_j$ form individual analytical classes (e.g., class 1 = X_1, Y_2 ; class 2 = X_3, Y_1). The only limitation imposed on the number of possible classes is that of measurement scale. If dimension X is color, and we use only the primary and second-

dary colors as possible attribute states, then we limit the number of variants that can be observed. Likewise, if dimension *Y* is length, a ratio-scale dimension, and we impose certain intervals on the analysis (<1 cm, 1–2 cm, and so on), then for most purposes the number of attribute states will be small. Imposition of the attribute state “greater than [an absolute value]” assures a finite number of possible variants. Conversely, the use of a detailed color chart and halving the intervals used to measure length expands the number of possible classes considerably.

Important properties of paradigmatic classification include its expandability (attribute states can be added as they are encountered) and adaptability for new purposes as they arise. Since classes do not contain intuitively derived “essential properties,” analytical interest is shifted from the study of sameness to the identification of variation. At one level, admittedly a superficial one, classes containing more than one member are redundant in terms of multiple membership; i.e., class members are identical, but, and this is an important but, *only* in terms of the attribute states used. *Analytical* similarity is quite distinct from *essential* similarity.

Paradigmatic classification of functional dimensions has been performed on a variety of kinds of objects, including stone items (e.g., Dancey 1973; Lewarch 1982; O’Brien 1985; Thompson 1978) and ceramics (e.g., Teltser 1988). The express purpose of these studies is the identification of change in function across time through close examination of variation and changes in frequency of individual variants through time. What is clear is that functional classes, however derived, must be related to a host of other phenomena such as breakage patterns, object reuse, and the physical environment.

It is possible, and entirely appropriate, to move the scale of analysis from the object to portions of the object that actually are used in carrying out an activity. Especially with the work of Semenov (1964), archaeologists began to view function in terms of the damage done to an artifact. A host of studies (e.g., Ahler 1979; Keeley 1980; Tringham et al. 1974) has emerged over the last two decades in which analysis has shifted from the study of shape to the study of wear patterns on edges and surfaces of objects. Most approaches have emphasized (1) observation and description of wear patterns and (2) replicative experiments to produce the kinds and locations of wear evident on archaeological specimens. Implicit in

many of these studies are Dunnell's (1978b:51) definitions of prehistoric *function*—"the artificial relationship that obtains between an object at whatever scale conceived and its environment both natural and artificial"—and prehistoric *use*—"the special case of prehistoric function in which the artificial relationship is motion."

And yet the units employed to examine the types of wear caused by moving an object against another object, i.e., units used to examine damage to the object, clearly are not the units upon which selection acts. Dimensions such as "kind of damage," while useful for some purposes, are in themselves not tied to selection. In other words, selection does not select for kinds of damage; it selects for traits or properties that an object possesses, including such dimensions as type of raw material and performance characteristics (Schiffer and Skibo 1987). Importantly, attributes occur as complexes, and perhaps we can think in terms of attributes that control other attributes or at least that piggyback on other attributes. It is these complexes, in effect classes, upon which we should focus analytical attention. The behavior of attribute associations over time may be a far more meaningful measure of selection than the behavior of single attribute states.

One example of how measurement of covariation of attributes within a set of objects has been applied to an archaeological examination of selection and change is Braun's (1982, 1983, 1985, 1987) use of sherds from well-dated contexts in west-central Illinois to examine change in wall thickness of Woodland-period cooking vessels. Braun's analysis is noteworthy from several standpoints, not the least of which is demonstrating the difficulty encountered in understanding developmental histories of features. Braun's early work (1983, 1985) dealt only with examining central tendency in pottery-manufacturing practices, specifically in wall construction of cooking vessels. He assumed that by focusing on a central tendency, he was tracking a gradualistic process in which selection worked against first one side and then the other of a single, unimodal, normal distribution of pottery-making practices, so that the mean shifted smoothly over time. His resulting time-series curve pointed in general to a gradual thinning of vessel walls through time. He then inferred (Braun 1987) that adjustments in pottery technology track changes that took place among Woodland potters producing the vessels, principal among which were increased sedentariness, shifts in

household composition, and increased use of starchy seeds. Braun (1987) argued that thin-walled vessels were being selected for their increased ability to conduct heat and their increased resistance to thermal shock.

Despite the neatness of this scenario, further analysis (Braun, pers. comm.) demonstrated that the logistic curve created by measuring wall thickness through time obscured the presence of several logistic components that together created the curve. The logistic components reflect the decisions and limitations facing Woodland potters, and the points of balance (e.g., how much temper to add, how thin the walls could be made) shifted through time according to demands for different performance characteristics (Schiffer and Skibo 1987). Some of the change in vessel technology during the Middle and Late Woodland periods (ca. 200 B.C.—A.D. 800) did involve a gradual shift in mean wall thickness for the construction of moderate-size vessels, but some of the change also involved an upswing and then a radical drop-off in the use of a distinct class of thin-walled vessels. And still another part of the change involved the introduction of, and eventual complete replacement by, an entirely new technology. Users of the new technology could produce vessels with far thinner walls per unit of vessel size than could users of the older technologies. This thinner-walled technology came in during Middle Woodland times (evident as “Hopewellian” bowls) and became the dominant technology by ca. A.D. 300–400, continuing until the later shift to shell tempering.

The level at which Braun conducted his analysis and the precision of his measurements give us an unparalleled view of the selective milieu of midwestern riverine groups during 200 B.C.—A.D. 800. From a selectionist perspective, his data allow us (1) to track several dimensions of variability faced by Woodland potters as they made their “selective” decisions, and (2) to observe the behavior of various technological attributes (states) through time. In essence, Braun’s sequence is a combination of evolutionary phenomena—gradational changes along a continuum plus innovations that follow different selective histories.

Similar studies (e.g., Dunnell and Feathers 1986; Feathers 1988, 1989; Teltser 1988) have been conducted on ceramic materials from Pleistocene surfaces connected with ancient braided courses of the Mississippi River in southeastern Missouri. A long-standing topic of interest in Mississippi Valley archaeology is the advent of shell-

tempered pottery, which usually is identified as a hallmark of Mississippian culture. By A.D. 900 shell-tempered vessels occurred throughout the valley and across large portions of the southeastern United States, often on sites containing one or more flat-topped pyramidal mounds, pits, and other features from which corn has been recovered. Considerable recent work in the central Mississippi Valley has centered around pushing back the advent of shell tempering, with the express purpose of defining Emergent Mississippian—a cultural/temporal conflation serving to identify the first users of shell tempering. But a shift in tempering material should have considerably more analytic appeal than simply being a way to push back the emergence of Mississippian culture. From a selectionist standpoint, the appearance of a new technology, especially one that became fixed so quickly over such a large region, is a prime candidate for examination. The advantage of studying technologies of this sort lies in the facts that (1) they are conditioned by ahistorical chemical and physical processes that make engineering evaluations possible, and (2) in the case of ceramics, selection culls mistakes rapidly.

Studies to date (e.g., Feathers 1988, 1989; Teltser 1988) have shown repeatedly that the catchall terms "sand tempered" and "shell tempered" mask significant variation in paste consistencies of post-A.D. 600 central Mississippi Valley ceramics, and that the shift from sand to shell was not one of complete replacement. Detailed comparison of earlier vessels to later "Mississippian" vessels casts doubt on the often-held notion that Mississippian peoples replaced Woodland peoples in the region. Rather, the ability to control firing temperatures below about 700°C—the point at which calcite decomposes to hygroscopic CaO (Dunnell and Feathers 1986)—led to the appearance of a new technology by indigenous Woodland groups.

Obviously, myriad archaeological examples similar to those discussed above exist in the literature. Our choice is predicated on the fact that those studies were constructed explicitly around a selectionist perspective. Each employed units designed to measure and describe certain phenomena that were of analytical interest. This by no means implies that the only good archaeology is that done under the selectionist banner, but again we point out that if evolution is invoked as an archaeological concern, then it is essential to structure research accordingly. And this requires the use of appropriately designed measures of variation and the use of methods to assess the temporal dimension in as detailed a manner as possible.

Conclusion

A biologist once told us—he was quite open about the matter—that the reason he had switched majors in graduate school from anthropology to biology was because humans were too difficult and complex to understand. In a comparative sense, humans *are* complex organisms, if for no other reason than because we know, at a rudimentary level, how the game is played. Nonhuman animals are dealt hands face up, and nature places their bets for them, card by card. Hands can be kept from deal to deal, but game conditions change, and cards that were not wild in one deal may be wild in the next. The game essentially is the same for all organisms, though the rules set up by nature to govern play by humans allow them to have more cards.

Evolution by means of differential persistence of variants applies equally to all organisms, whether we view them as simple or complex. Darwinian evolutionary theory is perfectly capable of subsuming humans under its umbrella of explanation. What remains to be accomplished is to understand important nuances of the game that apply only to humans. We *know*, in a general way, how the game is played, and we have known it since 1859. But anthropology and archaeology continue to hold fast to the notion that the game played by humans is distinctly different from that played by other organisms, based mainly on the vague premise that humans can control the outcome. Oddly, we have spent considerable time paying lip service to the biological commonality of all organisms, especially when fending off creationism, but have turned around and deified humans by making them immune to selection.

Humans are not immune to selection any more than nonhuman organisms are. But as the pigs running Animal Farm so cogently noted, "All animals are equal, but some animals are more equal than others." We do have intelligence, we *can* consciously choose among the variants that are present, and, most important, everything we do is *not* directed toward maximizing our reproductive success. We hope that statement satisfies critics who otherwise might claim that we are biologically based reductionists who view no differences between humans and other organisms. Considerable middle ground exists between hard-core sociobiology and the view that culture is an entirely extrasomatic means of adaptation. There even is a defensible position between the dual-inheritance approach of Boyd and

Richerson (1985) and the cultural-selectionist approach of Rindos (1986). We have tried to outline key elements of that middle ground, at least as it applies to the analysis of past behaviors as elicited from the archaeological record.

We still hold fast to the belief that anthropology does not need a new form of Darwinism dressed in the mantle of expressly human features (Leonard and Jones 1987). Neither does it need a restrictive explanatory device that deals with culture on a hit-or-miss basis. Anthropology does not need a *new* theory, it needs a *real* theory—one that seeks to include rather than exclude; one that explains rather than one that explains away. Selectionism has the potential to be that theory—to account for humans in their totality—but it cannot begin its life either split into pieces or emasculated to the point that it explains nothing.

The time has come to stop fretting over the lack of a viable paradigm in anthropology and archaeology and to get on with the business of science. As Taylor (1983:158) noted over forty years ago, "A policy of wait-until-all-the-evidence-is-in can stunt the growth of archaeology to a dangerous degree." The evenhanded application of selectionism to the study of humans *and* their culture is the place to begin, and the identification of variation is the starting point. We are, after all, as are all organisms, products of the oddball. It is not the conservative but the new variant that interests selection, yet archaeology traditionally has ignored variation, relegating it to the category of background noise. In theory the identification of variation is a relatively simple process. Any state, or attribute, of an artifact can be measured, just as biological states can be measured. In essence, if selection can take something's measure, so can we. The choice of measurement technique is conditioned only by the problems we wish to solve. Any measured set of attributes usually will yield some degree of observable variation.

The organization and classification of variation into an analytical framework is perhaps the most important aspect of selectionist theory, for it is around that framework that explanations will be built. As Gould (1986:61) points out, "science is productive doing, not just clever thinking." We suggest that paradigmatic classification is an important, but certainly not the only, means of measuring variation. Such a technique allows for systematic examination of a wide range of variation and potential covariation without masking the variation behind a semantic barrier composed of vague references to

similarities and differences. Once classified, analytic units can be organized such that the variable history of a dimension can be examined, and, if possible, agents of selection can be inferred and correlated.

At this point there is no a priori reason to eliminate a set of objects or traits from consideration under a selectionist perspective. Some objects or traits may ultimately prove to be beyond selective control, or, phrased more precisely, some states of some traits may, under certain conditions at some points in their histories, prove to have been what commonly is referred to as neutral. Some traits may be beyond our ability to explain. This makes no difference. The advantage of selectionist theory is that it *allows* us to handle any trait in similar analytic fashion. Debates over style versus function, natural versus cultural selection, intentionality, and trait transmission need no longer present obstacles. These are debates that are meaningless until we begin to identify variation over large amounts of space and time and begin to construct logical inferences about how selection operates on individual variants in restricted space and during short time intervals. Darwinian evolution provides a guide for interpreting the past; future research will have to work at explaining it. We cannot, as Rindos (1989:5) notes, blame Darwin for not doing our work for us.

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