Chapter 2

The Historical Development of an Evolutionary Archaeology

A Selectionist Approach

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Increasingly in the last several years there has been a growing number of archaeologists who are beginning to take note of the fact that Darwinian evolution offers a powerful means of explaining variation in the material record. The approach has been variously termed *evolutionary*, or *selectionist*, *archaeology*, and though it is still in a formative stage, there are clear signs of future growth and development. Although Darwinian evolutionary archaeology has not enjoyed the meteoric rise seen in the overnight sensation of the 1960s, processual archaeology, there are now in preparation or in press several edited books on the subject (e.g., Teltser 1995; O'Brien 1996), as well as numerous evolutionarily focused articles in leading archaeological journal (e.g., Dunnell 1978a, 1980; Leonard and Jones 1987; Rindos 1989; O'Brien and Holland 1990, 1992; Neff 1992; O'Brien *et al.* 1994) and monographs (e.g., Feathers 1989; Braun 1990; Dunnell 1992, 1995; O'Brien and Holland 1995a,b).

Despite the attention that scientific evolutionism in receiving from archaeologists, unless the discipline understands the basic tenets of the approach and is convinced of its power in explaining variation in the archaeological record, there is no reason to suspect that it will be widely accepted. The field of archaeology is a veritable graveyard of paradigms that have waxed and waned

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over the years, and, without considerable effort to show that evolutionary theory is the *only* means available for actually explaining the archaeological record as opposed to simply interpreting it, evolutionary archaeology will simply be another in the long line of casualties. It, unlike many other approaches that have been proposed in the discipline, needs full participation by researchers. This is because no other approach requires such a massive amount of meticulously generated data. Its success—measured in terms of its own performance in successfully explaining the archaeological record—depends on technological and functional data over which there exists tight temporal control. Unfortunately, most currently available data are not useful in addressing evolutionary questions because they were not generated for that purpose.

Data generated to examine culture-historical issues (including chronological ordering), while often suited to that purpose, cannot legitimately be extended to examination of issues of function. And it appears obvious that it is the functional aspects of the archaeological record that are most readily incorporated into a scientific evolutionary framework (Haag 1959; Dunnell 1978a,b; O'Brien and Holland 1990, 1992; O'Brien *et al.* 1994). The fact that currently available archaeological data are, for the most part, inappropriate for inclusion in a scientific archaeology might sound counterintuitive to some archaeologists—a situation that appears to have arisen from confusion over fundamental differences between typological thinking, with its emphasis on *transformation*, and population thinking, with its emphasis on *variation* and *replacement*. This issue is as pertinent to archaeology (Dunnell 1980, 1982, 1985, 1986, 1988; O'Brien and Holland 1990, 1992; O'Brien *et al.* 1994) as it is to biology (Dobzhansky 1951; Mayr 1963, 1973, 1976, 1977; 1987; Ghiselin 1966, 1974, 1981; Sober 1980, 1984).

Clearly, as Dunnell has pointed out *ad nauseam* (e.g., Dunnell 1989a), evolution is a materialist strategy that has its roots in population thinking. Equally clearly, archaeologists still do not understand the ramifications of this statement. We still speak of *types*, for example, not as theoretical units but rather as empirical units, i.e., as "real" things. How can this be, if we are seriously interested in incorporating scientific evolution in archaeology? Could it be that we do not understand the difference between theoretical units and empirical units? Are we missing the distinction between *essentialism*—which by its very name signals an interest in the "essential" qualities that something possesses—and *materialism*? At another level, are we viewing science as a monolithic entity, not realizing that there is a world of difference between physical science and the things in which it is interested and life science and its fields of interests?

Perhaps this is a good place to review briefly what evolutionary archaeology is and what it is not. Collections of essays such as this one play an important role in furthering a general understanding of the scientific evolutionary

approach, if for no other reason than they cause its proponents to rethink their position in terms of clarity and logic. I say this is a good place for reviewing the tenets of scientific evolution precisely because of the growth in its visibility. There is, however, a danger in its increased visibility. History bears out that archaeologists, for a variety of reasons, have been quick to jump on bandwagons without the slightest notion why they are doing it and certainly without the background necessary to understand the nuances of the approaches they begin advocating. Lessons learned from archaeology conducted in the 1960s and 1970s are informative here and perhaps shed light on the question of why scientific evolution has only recently begun to be incorporated in archaeology. Perhaps more importantly, though space precludes anything more than brief mention, we can also learn from the lessons of biologists in the 1930s and 1940s as they grappled with evolutionary issues.

THE SEARCH FOR A SCIENTIFIC ARCHAEOLOGY

Archaeologists of the 1960s and 1970s might have understood what Lewis Binford (e.g., 1962, 1968) and others meant by the term *culture process* (e.g., various papers in S. R. Binford and L. R. Binford 1968), but many of them certainly did not understand exactly what it meant to be a processual archaeologist. In fact, only now is it becoming clear what archaeologists really were accepting when they called themselves processualists. Beyond question, the discipline was becoming increasingly bored with a singular focus on issues such as time and space—a movement that can be traced back at least 25 years before Binford (1962) wrote "Archaeology as Anthropology" (e.g., Steward and Setzler 1938; Steward 1942; Bennett 1943; Taylor 1948; Caldwell 1958; Willey and Phillips 1958; Willey 1962). Archaeologists, at least some of them, were concerned that their discipline had, in its emphasis on time-and-space systematics, overlooked culture—that nebulous concept that makes us human and which had, by the middle of the 20th century, become the unifying principle of anthropology. In fact, Binford (1962:217) began his essay "Archaeology as Anthropology" by praising Willey and Phillips's (1958:2) famous quote "American archaeology is anthropology or it is nothing" and then proceeding to state that "the purpose of this discussion is to evaluate the role which the archaeological discipline is playing in furthering the aims of anthropology and to offer certain suggestions as to how we, as archaeologists, may profitably shoulder more responsibility for furthering the aims of our field."

Furthering the aims of "our" field, indeed. Through the efforts of Binford and others, especially Kent Flannery (e.g, 1968a,b, 1972; Flannery and Coe 1968), archaeologists soon learned that culture could indeed be added back to the equation through such things as ecology and general systems theory, and

they began not only to steep themselves in the principles of those disciplines but also to incorporate the cultural-evolutionary pronouncements of Leslie White (e.g., 1945, 1959a,b), Julian Steward (e.g., 1955), V. Gordon Childe (1951a), and their followers [e.g., Sahlins and Service 1960; Fried 1967; Service 1975; various papers in Rambo and Gillogly 1991 (cf. Rambo 1991)] into routine archaeological studies. Soon, archaeologists were taking Binford's exhortation seriously and were doing everything they could so as not to lose sight of the "Indian behind the artifact," as Robert Braidwood (1959:79), echoing Walter Taylor (1948), had so appropriately put it. Despite caveats raised by a few ethnographers—one of whom (Harris 1968:360) even encouraged archaeologists to "shrive yourselves of the notion that the units which you seek to reconstruct must match the units in social organization which contemporary ethnographers have attempted to tell you exist"—archaeologists began devising methods to determine whether the group that was responsible for "creating" a particular archaeological site was matrilineal or patrilineal (e.g., Deetz 1968; Longacre 1968; Hill 1970; Allen and Richardson 1971). These exercises were at first entertaining, but they began to lose some of the charm when holes began to appear in the anthropological armor in which archaeologists had clothed themselves. For one thing, such exercises were too particularistic. Although they might contribute tidbits of information that the ethnologists could use, the results were unsatisfying to the archaeologist, who wanted big answers to big questions. What about all of the regularities that ethnologists such as White said were there? How could *they* be found?

The answer was provided by Binford, who urged archaeologists to study the philosophy of science, which, he claimed (Binford 1972:17), he had been told to do by White. When the philosophy of science was then added to the equation—literally, when archaeologists were told that not only could one investigate culture process but also could do it *scientifically*—the stage was set for a mass exodus from the stifling constraints of such mundane pursuits as culture history. Now archaeology could get on with the exciting voyage of science, perhaps even discovering a few laws (empirical, covering, or otherwise) along the way. The self-described new archaeologists began paying homage to Carl Hempel and Ernest Nagel (see below)—in large part because Binford told them that was the correct thing to do—though few if any of them really understood such concepts as *hypothetico-deductive framework, deductive-nomological approach*, and *bridging arguments*. These simply were words that someone heard a philosopher or an archaeologist-turned-philosopher utter, and he or she was impressed because the words sounded scientific. News of the new and exciting terminology spread like wildfire, and soon an entire generation of archaeologists was (supposedly) doing science.

One highly influential book written during this halcyon period was *Explanation in Archaeology: An Explicitly Scientific Approach* (Watson *et al.* 1971),

which proved to be so popular that is was revised and published over a decade later as *Archaeological Explanation: The Scientific Method in Archaeology* (Watson *et al.* 1984). Patty Jo Watson and her coauthors, Charles Redman and Steven LeBlanc, argued that archaeology could and should be a science and that one would know he or she had reached that goal if explanation was the end product of archaeological inquiry. To get there would require rigorous adherence to the scientific method: "Archaeologists should begin with clearly stated problems and then formulate testable hypothetical solutions. The degree of confirmation of conclusions should be exhibited by describing fully the field and laboratory data and the reasoning used to support these conclusions. This is what we mean by an explicitly scientific archaeological method" (Watson *et al.* 1984:129).

It is difficult to argue with the statement that archaeologists should state problems clearly and should describe data as completely as possible. And I find it difficult to argue against testing hypotheses, though technically what one actually is doing is examining the testable *implications* of a hypothesis. Watson *et al.* certainly were clear on their definition of science:

> ...science is based on the working assumption or belief by scientists that past and present regularities *are* pertinent to future events and that under similar circumstances similar phenomena will behave in the future as they have in the past and do in the present. This practical assumption of the regularity or conformity of nature is the necessary foundation for all scientific work. Scientific descriptions, explanations, and predictions all utilize lawlike generalizations hypothesized on the presumption that natural phenomena are orderly. (Watson *et al.* 1984:5–6)

> The ultimate goal of any science is construction of an axiomatized theory such that observed regularities can be derived from a few basic laws as premised. Such theories are used to explain past events and to predict future ones. Good theories lead to prediction of previously unsuspected regularities. Logical and mathematical axiomatic systems are essential as models of scientific theories, but no empirical science has yet been completely axiomatized. As Hempel indicates, it may ultimately turn out for any science, or for all sciences, that the goal is actually unattainable (Watson *et al.* 1984:14).

The Hempelian notion of science and how it operates formed the basis of the reintroduction into philosophy of 19th-century empiricism, though the term usually applied to Hempel's view is *logical positivism.* One of his books, *Aspects of Scientific Explanation, and Other Essays in the Philosophy of Science* (Hempel 1965), became as widely cited in the archaeological literature of the 1970s as it did in the philosophical literature of the 1960s, and it was his notion of science that Watson *et al.* (1971, 1984) assumed as the basis of their argument that archaeology could and should become scientific. For them, science was "an axiomatized theory such that observed regularities can be derived from a few basic laws as premises." Watson *et al.* were joined in their efforts to make the philosophy of science accessible to the archaeological community by other

neopositivists such as Merrilee and Wesley Salmon (e.g., M. H. Salmon 1975, 1982; M. H. Salmon and W. C. Salmon 1979).

Central to all of their arguments was the Hempelian account of how one arrived at explanation—what Hempel termed the *deductive-nomological* approach. Despite his use of unusual terms, Hempel's basic tenet was simple: Whatever is to be explained (he used the term *explanandum* to refer to the "whatever") is derived logically from one or more universal statements, or laws, keeping in mind that certain boundary conditions might apply—hence, Watson and colleagues' (1984:5) above-cited definition of science as the "belief by scientists that past and present regularities *are* pertinent to future events and that under similar circumstances similar phenomena will behave in the future as they have in the past and do in the present."

There are still a few philosophers around who view science in Hempelian terms, but by the middle of the 1970s it was becoming clear that the deductive-nomological approach was dying a natural death. There were attempts to keep it alive, for example by linking it to the *bridging-law* concept of philosopher Ernest Nagel (e.g., 1961), but these also died out—except among archaeologists, who began making bridges between the archaeological present and the archaeological past through such things as ethnographic analogy and ethnoarchaeology (see Fritz 1972). In other words, archaeologists were using the present as an analogue of the past. In fact, they *had* to resort to analogy; how else were they going to find the laws that Hempel said were there—the very laws that, once discovered, led to the formulation of "axiomatized theory" and thus ultimately to explanation?

What a blessing it was that archaeologists now had access to the past through the present. They could find patterning in their archaeological data sets and interpret the patterning in terms of modern analogues. Or, conversely, they could use present behavior as a guide to what to look for in the prehistoric archaeological record. If one found enough correlations between the past and the present, then surely laws could be constructed to account for the similarity in pattern. Any slight deviations could be explained away in terms of slightly different "boundary conditions," to use Hempel's term, that had impinged on the creators of the past and present signatures. The end result of this exercise was scientific explanation—defined as interpretation by way of law formulation. This is the reason why Watson (1986:452) equates archaeological interpretation with "describing and explaining the real past."

There are, however, several archaeologists, myself included, who do not agree with this conflation of interpretation and explanation nor with the belief that the Hempelian view of science can be applied to the study of organisms, including humans. The type of science Watson has in mind—a predictive, law-driven science—will not work in archaeology. Hempelian science is not particularly useful for studying humans—or any other organism—because of the as-

sumptions it makes relative to laws. No one has ever denied that chemical–physical laws do not apply to organisms, but at the level that concerns most archaeologists—behavior (why we do what we do) and the products of those behaviors (artifacts)—they do not appear to play a deterministic role. And determinism, i.e., the intrinsic properties that something has that makes that something predictable, is the basis of Hempelian explanation (again, within reason—remember Hempel's "boundary conditions").

Deterministic laws work well for physical things such as elements and molecules and their chemical interactions, but they do not work well for organisms. A carbon atom, for example, is always a carbon atom, regardless of time or place. And there are deterministic laws that govern how carbon atoms interact with other atoms. For example, if four hydrogen atoms happen to pass near a carbon atom, it is a safe bet that the carbon atom will grab them and form a molecule of methane. We can make that bet today, tomorrow, or 10 years from now and we will win it—just as Hempelian science says we will. The safety of the bet resides in our knowing what the laws are that govern the behaviors of atoms *and* in our understanding the various chemical–physical mechanisms that carry out the dictates of the laws. Those kinds of laws apply the invariant properties of *inanimate* objects, but they do not work on such things as the behavior of organisms (O'Brien and Holland 1990, 1995b). They are, however, precisely the kinds of laws archaeologists wanted to apply to humans. Fritz and Plog (1970:405), for example, were explicit about the definition of law: "A statement of relation between two or more variables which is true *for all times and places*" (italics added).

One could, I suppose, dance around the issue and claim a distinction between "universal facts" and "laws"—Binford (1972:18) claimed that Leslie White once noted that "Julian Steward doesn't know the difference between a universal fact and a law" (I'm not sure I do either)—but this obscures the real issue, namely, are there invariant laws that govern human behavior? If there are, then the Hempelian notion of science is quite adequate. If there are not, then where do we look for explanation? We might start by looking at scientific evolutionary theory, which has little or nothing to do with invariant laws.

EVOLUTIONARY THEORY

The new archaeologists forgot (or never knew) that invariant laws are not the only kind of laws around. What about the law of contingency, which says that whatever happens at point D is conditioned in part by what happened at points A, B, and C? Point D is not *determined* by what happened at the other points but rather is *contingent* on what happened at those points. Whatever is manifest at point D is *stochastic* as opposed to *random*—meaning that the ex-

pression of D is derived from a limited, rather than an *infinite*, number of possibilities. The theory of Darwinian evolution is built around contingency as a historical process. Organisms evolve, and in the process usually change shape, but there is not an unlimited number of shapes into which they can change from generation to generation. There are certain forms that hang on for hundreds, thousands, or millions of generations, sometimes with little variation evident from one generation to the next. In other words, forms are *channeled* in certain directions because of their history (Mayr 1988:108). We do not, for example, expect a newborn baby to have three legs. It might have six toes—that's not too uncommon—but an extra leg is almost impossible to imagine. Few of those knowledgeable in the natural sciences would argue the Darwinian evolution is not a theory or that in its modified form (modified in the sense that we now understand genetic transmission, embryonic development, and the like) it is not capable of providing *explanations* for how and why we are the way we are. It is not a perfect theory—yet—but it is a good one because it works.

But, as Holland and I have pointed out (O'Brien and Holland 1995a), a review of the history of scientific paradigms makes it clear that for any paradigm to take hold requires a considerable amount of time and reiteration. To say that evolutionary archaeology is a good paradigm because it works really says nothing about *how* and, more importantly, *why* it works. Those two issues—the how and why it works—can only be addressed through a careful reading of the biological, not the archaeological, literature. Application of the theory to archaeological phenomena is entirely appropriate, and it is equally clear that archaeology has something to contribute to evolutionary theory. But the theory itself is a biological one, not an archaeological one.

I personally do not find the fact that Darwinian evolution has been applied to the archaeological record particularly novel. I suspect that it was only a matter of time before archaeologists began to see the archaeological record for what it is—a record of the histories of past human phenotypes. It is rather surprising, however, that anthropologists, as opposed to archaeologists, were not the first to seize on the idea that Darwinian evolution is entirely appropriate to the study of all humans, their behaviors, and their behavioral by-products. The notion that selection, the centerpiece of Darwinian evolution, operates on humans might have been profound back in 1859, but after Darwin published *On the Origin of Species*, it theoretically should have been a relatively uncomplicated matter to extrapolate "descent with modification" to humans and, by extension, to features that affect their fitness. However, this extrapolation was slow to be made. Not even Darwin wanted to admit that humans were necessarily a product of natural selection and other evolutionary processes (many of which were unknown or misunderstood), a view that still pervades anthropology and inhibits the acceptance of an internally consistent approach to the study of humans and the materials they manufacture, use, and discard. Evolutionary archaeology,

however, has made the claim that humans, like any other organisms, are directly affected by selection and that some aspects of the material record reflect the effects of selection (Dunnell 1982; Leonard and Jones 1987; O'Brien and Holland 1990, 1992, 1995a; O'Brien *et al.* 1994).

Evolution, of course, has been around in Americanist archaeology for a long time. In the late 19th century it was evolution as espoused by Lewis Henry Morgan and others to which most Bureau of American Ethnology prehistorians and ethnologists subscribed, at least in part. Later, as we have seen, it was evolution according to White, Steward, and friends that caught the attention of archaeologists. Interspersed were the purely functional arguments of anthropologists such as Marvin Harris (e.g., 1979) and the aforementioned systems-theory formulations of archaeologists such as Flannery. Evolution often was invoked in such formulations, though it bore little or no resemblance to anything familiar to biologists. For the most part, anthropological brands of evolution were and still are little more than unidirectional, progressive formulations grounded in the notion of some kind of cultural transformation (Dunnell 1980; Rambo 1991). Change is viewed simplistically as an outcome of need. For example, if a group is facing food shortage, it simply forms alliances with other groups to develop a different means of obtaining food. In most anthropological schemes, groups (and, by extension, individuals within the groups) always come out as winners.

Selection, other than some vague notion of *cultural* selection, plays no role in most evolutionary scenarios concocted by anthropologists and archaeologists, since evolution becomes little more than a set of invented solutions to problems posed by the environment (Lewontin 1983). In other words, humans go out and get whatever it is they need to adapt to their social and physical environment. In a real sense, anthropologists emphasize humans as intent-driven, *maximizing* creatures, a concept that has been amplified in anthropology through the addition of sociobiology as an area of interest. Why should we believe that humans act any differently than other organisms when it comes to behaviors? Certainly there is nothing in evolutionary theory that states that organisms must always act in accordance with some maximizing strategy. As Dawkins (1990:188–189) notes, "Individuals do not consciously strive to maximize anything; they behave *as if* maximizing something....individuals may strive for something, but it will be a morsel of food, an attractive female, or a desirable territory." As Darwin himself figured out, no such thing as a perfectly adapted organism has existed or will ever exist. All he ever had in mind when he used the phrase "survival of the fittest" was for "the *tendency* of organisms that are better engineered to be reproductively successful" (Burian 1983:299; italics added). In other words, "If *a* is better adapted than *b* in environment *E*, then (probably) *a* will have greater reproductive success than *b* in *E*" (Brandon 1990:11). The kinds of "explanations" that usually result from mechanistic ap-

plication to humans of concepts such as optimal foraging strategy are not science, they're just-so stories (Dunnell and Wenke 1979).

A single example should serve to demonstrate not only the way intentionality is interwoven in adaptationist stories but also the pervasiveness of the adaptationist perspective in archaeology. Some of the greatest just-so stories in archaeology have centered around the origin(s) of agriculture, as if domestication and attendant processes are the result of, to borrow a phrase from Childe (1951b), man's attempt to make himself. In other words, agriculture is viewed as a solution to an environmental problem, be it population pressure (Cohen 1975, 1977) or a host of other problems. In two works, David Rindos (1980, 1984) provided a clear exposition of how Darwinian evolutionary theory can, in essence, explain the origin and spread of domesticatory systems. Importantly, his explanation says nothing about human intent and invention, a fact he points out explicitly: "Parsimony would suggest that if agricultural origins may be explained without the use of intent or invention, then these concepts may, for the purposes of this model, be set aside" (Rindos 1980:751). As might have been anticipated, not all anthropologists and archaeologists were kind in their assessments of Rindos's evolutionary explanation (e.g., Ceci 1980; Shaffer 1980; Yarnell 1985; Flannery 1986), pointing out repeatedly that any "model" of the origins of agriculture *must* take into account human intention and problem-solving abilities.

It is not going to be a simple matter to eradicate storytelling from archaeology, and, in fact, without a real understanding of Darwinian evolution and its attendant processes such as selection and drift, we run the risk of substituting evolutionary-based "adaptationist" stories for the orthogenetic ones stemming from cultural evolution. In other words, we cannot assume that by wrapping ourselves in Darwin's mantle that our stories are any better than those from someone wrapped in the mantle of White or Steward. Silly adaptationist stories are as much a problem in archaeology (O'Brien and Holland 1990, 1992, 1995a) as they are in biology (Gould and Lewontin 1979) and, importantly, pose a serious threat to the profession taking evolutionary archaeology seriously. These can be minimized, especially through reliance on engineering-design analysis (Mayr 1983; O'Brien and Holland 1990, 1992; O'Brien *et al.* 1994).

DISCUSSION

It might appear that the amount of time that has passed since the first seeds of an evolutionary archaeology were planted—I use 1978 as a benchmark, for it was in that year that Dunnell (1978a,b,c) sketched out in three papers some of the essential points of such an approach—is an inordinately long time for an approach to have been around with few if any takers. Despite the

theoretical and methodological advances that have been made in the following decade and a half (e.g., Dunnell 1982, 1985, 1988, 1989b, 1992, 1995; Leonard and Jones 1987; O'Brien and Hollard 1990, 1992, 1995a,b; Neff 1992; Teltser 1995), critics might argue that even now the number of actual case studies that employ evolutionary theory is small. Proponents of the approach might take heart in knowing that as late as the middle 1930s it was by no means clear to biologists exactly how Darwinian evolution worked. On one side were the naturalists and their ideas on geographic isolation as a major cause of speciation. On the other side were the experimentalists and their ideas on mutationism. The gulf between gradual evolution by means of natural selection and rapid evolution by means of mutation seemed unbridgeable, but by the middle 1930s the situation changed dramatically. As Mayr (1982:566–567) points out, two things had to happen before a bridge could be constructed: (1) geneticists had to take an interest in both diversity and the populational aspects of evolution and (2) naturalists had to understand that the experimentalists (geneticists) no longer were opposed to natural selection and gradualism. The latter group also had to abandon its emphasis on the transmission of acquired characteristics. Within about a decade, biologists reached what Huxley (1942) termed the *evolutionary synthesis*.

I imagine the same thing will happen in archaeology. As archaeologists become more familiar with evolutionary theory and begin to move outside their narrow specialties, applications will grow exponentially. If we can escape the temptation to construct patently absurd adaptationist scenarios that ostensibly "explain" variation in the archaeological record, evolutionary archaeology will become widely accepted as a legitimate approach. I take sharp exception with those who note with derision that the number of case studies in evolutionary archaeology is still so small after all these years, as if this is evidence that somehow the approach is flawed. There is nothing flawed with the approach; what is flawed is our thinking. It is still difficult for many people to believe that selection works on humans, as if the fact that we have "culture" somehow makes us immune to selection and drift. This is patently nonsense. And neither do we have to invoke a special kind of selection—"cultural selection"—to address the issue of human evolution. Selection does not need to be gussied up in new clothes for application to humans. Neither do we need to be worried at this stage about the type of vehicle by which variation is transmitted or how the variation arose. Selection, in fact, is blind to the source of variation (O'Brien and Holland 1990), and all that matters is that the variation is present and that it can be transmitted. Humans might have a few more cards with which to play the game than other animals do, but the rules are the same. I suggest that instead of searching for a separate set of rules, which does not exist, archaeologists should examine who has won and lost the games played over the last 10,000 years or so and attempt to figure out *how* and *why* the winners won and

the losers lost. Evolutionary theory offers a means of doing this without recourse to inventing little stories.

Above all, archaeologists who want to make the discipline scientific need to keep firmly in mind that the type of science to which they aspire is historical as opposed to physical in nature. In physical science, prediction is symmetrical to causation (Mayr 1982:71); in historical science there is no prediction. Thus, attention spent on law formulation in archaeology is pointless, since there cannot *be* any laws except that of contingency. Rather than search for "explanation" in terms of "universal facts" and "laws," archaeologists should realize that explanation is derived from the theory itself. Mayr's (1982:76) admonition to biologists is equally appropriate for archaeologist: "what is needed is an uncommitted philosophy of biology which stays equally far away from vitalism and other unscientific ideologies and from a physicalist reductionism that is unable to do justice to specifically biological phenomena and systems."

REFERENCES

Allen, W.L., and Richardson, J.B., III, 1971, The Reconstruction of Kinship From Archaeological Data: The Concepts, the Methods, and the Feasibility, *American Antiquity* 36:41–53.

Bennett, J.W., 1943, Recent Developments in the Functional Interpretation of Archaeological Data, *American Antiquity* 8:208–219.

Binford, L.R., 1962, Archaeology as Anthropology, *American Antiquity* 28:217–225.

Binford, L.R., 1968, Archeological Perspectives, in: *New Perspectives in Archeology* (S.R. Binford and L.R. Binford, eds.), Aldine Press, Chicago, pp. 5–32.

Binford, L.R., 1972, *An Archaeological Perspective*, Seminar Press, New York.

Binford, S.R., and Binford, L.R. (eds.), 1968, *New Perspectives in Archeology*, Aldine Press, Chicago.

Braidwood, R.J., 1959, Archaeology and the Evolutionary Theory, in: *Evolution and Anthropology: A Centennial Appraisal* (B.J. Meggers, ed.), The Anthropological Society of Washington, Washington, DC, pp. 76–89.

Brandon, R.N., 1990, *Adaptation and Environment*, Princeton University Press, Princeton.

Braun, D.P., 1990, Selection and Evolution in Nonhierarchical Organization, in: *The Evolution of Political Systems: Sociopolitics in Small-Scale Sedentary Societies* (S. Upham, ed.), Cambridge University Press, Cambridge, pp. 62–86.

Burian, R., 1983, Adaptation, in: *Dimensions of Darwinism* (M. Grene, ed.), Cambridge University Press, Cambridge, pp. 287–314.

Caldwell, J.C., 1958, The New American Archaeology, *Science* 129:303–307.

Ceci, L., 1980, Comment on "Symbiosis, Instability, and the Origins and Spread of Agriculture," by D. Rindos, *Current Anthropology* 21:766.

Childe, V.G., 1951a, *Social Evolution*, Schuman, New York.

Childe, V.G., 1951b, *Man Makes Himself*, Mentor, New York.

Cohen, M.N., 1975, Population Pressure and the Origin of Agriculture, in: *Population, Ecology, and Social Evolution* (S. Polgar, ed.), Mouton, The Hague, pp. 79–121.

Cohen, M.N., 1977, *The Food Crisis in Prehistory: Overpopulation and the Origins of Agriculture*, Yale University Press, New Haven.

Dawkins, R., 1990, *The Extended Phenotype: The Long Reach of the Gene*, 2 Ed., Oxford University Press, Oxford.

Deetz, J., 1968, The Inference of Residence and Descent Rules from Archeological Data, in: *New Perspectives in Archeology* (S.R. Binford and L.R. Binford, eds.), Aldine Press, Chicago, pp. 41–48.

Dobzhansky, T., 1951, Mendelian Populations and Their Evolution, in: *Genetics in the 20th Century* (L.C. Dunn, ed.), Macmillan Co., New York, pp. 573–589.

Dunnell, R.C., 1978a, Style and Function: A Fundamental Dichotomy, *American Antiquity* 43:192–202.

Dunnell, R.C., 1978b, Archaeological Potential of Anthropological and Scientific Models of Function, in: *Archaeological Essays in Honor of Irving B. Rouse* (R.C. Dunnell and E.S. Hall, Jr., eds.) Mouton, The Hague, pp. 41–73.

Dunnell, R.C., 1978c, Natural Selection, Scale, and Cultural Evolution: Some Preliminary Considerations, Paper presented at the 77th Annual Meeting of the American Anthropological Association, Los Angeles.

Dunnell, R.C., 1980, Evolutionary Theory and Archaeology, in: *Advances in Archaeological Method and Theory*, Vol. 3 (M.B. Schiffer, ed.), Academic Press, New York, pp. 35–99.

Dunnell, R.C., 1982, Science, Social Science, and Common Sense: The Agonizing Dilemma of Modern Archaeology, *Journal of Anthropological Research* 38:1–25.

Dunnell, R.C., 1985, Methodological Issues in Contemporary Americanist Archaeology, in: *Proceedings of the 1984 Biennial Meeting of the Philosophy of Science Association*, Vol. 2 (P.D. Asquith and P. Kitcher, eds.), Philosophy of Science Association, East Lansing, MI, pp. 717–744.

Dunnell, R.C., 1986, Methodological Issues in Americanist Artifact Classification, in: *Advances in Archaeological Method and Theory*, Vol. 9 (M.B. Schiffer, ed.), Academic Press, New York, pp. 149–207.

Dunnell, R.C., 1988, The Concept of Progress in Cultural Evolution, in: *Evolutionary Progress* (M.H. Nitecki, ed.), University of Chicago Press, Chicago, pp. 169–194.

Dunnell, R.C., 1989a, Philosophy of Science and Archaeology, in: *Critical Traditions in Contemporary Archaeology* (V. Pinsky and A. Wylie, eds.), Cambridge University Press, Cambridge, pp. 5–9.

Dunnell, R.C., 1989b, Aspects of the Application of Evolutionary Theory in Archaeology, in: *Archaeological Thought in America* (C.C. Lamberg-Karlovsky, ed.), Cambridge University Press, Cambridge, pp. 35–49.

Dunnell, R.C., 1992, Archaeology and Evolutionary Science, in: *Quandaries and Quests: Visions of Archaeology's Future* (L. Wandsnider, ed.), Occasional Paper No. 20, Center for Archaeological Investigations, Southern Illinois University, Carbondale, pp. 209–224.

Dunnell, R.C., 1995, What Is It That Actually Evolves?, in: *Evolutionary Archaeology: Methodological Issues* (P. A. Teltser, ed.), University of Arizona Press, Tucson, pp. 33–50.

Dunnell, R.C., and Leonard, R.D. (eds.), n.d., *Archaeological Applications of Evolutionary Theory*.

Dunnell, R.C., and Wenke, R.J., 1979, An Evolutionary Model of the Development of Complex Society, Paper presented at the Annual Meeting of the American Association for the Advancement of Science, San Francisco.

Feathers, J.K., 1989, Ceramic Analysis, Variation and Database Construction: A Selectionist Perspective, in: *Analysis and Publication of Ceramics: The Computer Data-Base in Archaeology* (J.A. Blakely and W.J. Bennett, Jr., eds.), British Archaeological Reports International Series No. 551, pp. 71–80.

Flannery, K.V., 1968a, Archaeological Systems Theory and Early Mesoamerica, in: *Anthropological Archaeology in the Americas* (B.J. Meggers, ed.), Anthropological Society of Washington, Washington, DC, pp. 67–87.

Flannery, K.V., 1968b, The Olmec and the Valley of Oaxaca: A Model of Interregional Interaction in Formative Times, in: *Dunbarton Oaks Conference on the Olmec* (E.P. Benson, ed.), Dunbarton Oaks, Washington, DC, pp. 79–110.

Flannery, K.V., 1972, The Cultural Evolution of Civilizations, *Annual Review of Ecology and Systematics* 3:399–426.

Flannery, K.V., 1986, A Visit to the Master, in: *Guila Naquitz: Archaic Foraging and Early Agriculture in Oaxaca, Mexico* (K.V. Flannery, ed.), Academic Press, New York, pp. 511–519.

Flannery, K. V., and Coe, M.D., 1968, Social and Economic Systems in Formative Mesoamerica, in: *New Perspectives in Archaeology* (S.R. Binford and L.R. Binford, eds.), Aldine Press, Chicago, pp. 267–283.

Fried, M.H., 1967, *The Evolution of Political Society*, Random House, New York.

Fritz, J.M., 1972, Archaeological Systems from Indirect Observation of the Past, in: *Contemporary Archaeology: A Guide to Theory and Contributions* (M.P. Leone, ed.), Southern Illinois Press, Carbondale, pp. 135–157.

Fritz, J.M., and Plog, F.T., 1970, The Nature of Archaeological Explanation, *American Antiquity* 35:405–412.

Ghiselin, M.T., 1966, On Psychologism in the Logic of Taxonomic Controversies, *Systematic Zoology* 15:207–215.

Ghiselin, M.T., 1974, A Radical Solution to the Species Problem, *Systematic Zoology* 25:536–544.

Ghiselin, M.T., 1981, Categories, Life and Thinking, *Behavioral and Brain Sciences* 4:269–283.

Gould, S.J., and Lewontin, R., 1979, The Spandrels of San Marco and the Panglossian Paradigm: A Critique of the Adaptationalist Program, *Proceedings of the Royal Society of London* B205:581–598.

Haag, W.G., 1959, The Status of Evolutionary Theory in American Archaeology, in: *Evolution and Anthropology: A Centennial Appraisal* (B.J. Meggers, ed.), The Anthropological Society of Washington, Washington, DC, pp. 90–105.

Harris, M., 1968, Comments by Marvin Harris, in: *New Perspectives in Archeology* (S.R. Binford and L.R. Binford, eds.), Aldine Press, Chicago, pp. 359–361.

Harris, M., 1979, *Cultural Materialism: The Struggle for a Science of Culture*, Random House, New York.

Hempel, C.G., 1965, *Aspects of Scientific Explanation, and Other Essays in the Philosophy of Science*, Free Press, New York.

Hill, J.N., 1970, *Broken K Pueblo: Prehistoric Social Organization in the American Southwest*, Anthropological Papers of the University of Arizona No. 18.

Huxley, J.S., 1942, *Evolution, the Modern Synthesis*, Allen and Unwin, London.

Leonard, R.D., and Jones, G.T., 1987, Elements of an Inclusive Evolutionary Model for Archaeology, *Journal of Anthropological Archaeology* 6:199–219.

Lewontin, R.C., 1983, The Organism as the Subject and Object of Evolution, *Scientia* 118:65–82.

Longacre, W.A., 1968, Some Aspects of Prehistoric Society in East-central Arizona, in: *New Perspectives in Archeology* (S.R. Binford and L.R. Binford, eds.), Aldine Press, Chicago, pp. 89–102.

Mayr, E., 1963, *Animal Species and Evolution*, Harvard University Press, Cambridge, MA.

Mayr, E., 1973, *Populations, Species, and Evolution*, Harvard University Press, Cambridge, MA.

Mayr, E., 1976, Typological versus Population Thinking, in: *Evolution and the Diversity of Life*, Harvard University Press, Cambridge, MA, pp. 26–29.

Mayr, E., 1977, Darwin and Natural Selection, *American Scientist* 65:321–337.

Mayr, E., 1982, *The Growth of Biological Thought*, Harvard University Press, Cambridge, MA.

Mayr, E., 1983, How to Carry Out the Adaptationist Program?, *The American Naturalist* 121:324–334.

Mayr, E., 1987, The Ontological Status of Species, *Biology and Philosophy* 2:145–166.

Mayr, E., 1988, *Toward a New Philosophy of Biology: Observations of an Evolutionist*, Harvard University Press, Cambridge, MA.

Nagel, E., 1961, *The Structure of Science*, Harcourt, Brace & World, New York.

Neff, H., 1992, Ceramics and Evolution, in: *Advances in Archaeological Method and Theory*, Vol. 2 (M.B. Schiffer, ed.), University of Arizona Press, Tucson, pp. 141–194.

O'Brien, M.J., 1996, *Evolutionary Archaeology: Theory and Applications*. University of Utah Press, Salt Lake City.

O'Brien, M.J., and Holland, T.D., 1990, Variation, Selection, and the Archaeological Record, in: *Advances in Archaeological Method and Theory*, Vol. 2 (M.B. Schiffer, ed.), University of Arizona Press, Tucson, pp. 31–79.

O'Brien, M.J., and Holland, T.D., 1992, The Role of Adaptation in Archaeological Explanation, *American Antiquity* 57:36–59.

O'Brien, M.J., and Holland, T.D., 1995a, The Nature and Premise of a Selection-based Archaeology, in: *Evolutionary Archaeology: Methodological Issues* (P.A. Teltser, ed.), University of Arizona Press, Tucson, pp. 175–200.

O'Brien, M.J., and Holland, T.D., 1995b, Behavioral Archaeology and the Extended Phenotype, in: *Expanding Archaeology* (J.M. Skibo, W.H. Walker, and A.E. Nielson, eds.), University of Utah Press, Salt Lake City, pp. 143–161.

O'Brien, M.J., Holland, T.D., Hoard, R.J., and Fox, G.L., 1994, Evolutionary Implications of Design and Performance Characteristics of Prehistoric Pottery, *Journal of Archaeological Method and Theory* 1:259–304.

Rambo, A.T., 1991, The Study of Cultural Evolution, in: *Profiles in Cultural Evolution* (A.T. Rambo and K. Gillogly, eds.), Anthropological Papers No. 85, Museum of Anthropology, University of Michigan, Ann Arbor, pp. 23–109.

Rambo, A.T., and Gillogly, K. (eds.), 1991, Profiles in Cultural Evolution, Anthropological Papers No. 85, Museum of Anthropology, University of Michigan, Ann Arbor.

Rindos, D., 1980, Symbiosis, Instability, and the Origins and Spread of Agriculture: A New Model, *Current Anthropology* 21:751–772.

Rindos, D., 1984, *The Origins of Agriculture: An Evolutionary Perspective*, Academic Press, New York.

Rindos, D., 1989, Undirected Variation and the Darwinian Explanation of Cultural Change, in: *Advances in Archaeological Method and Theory*, Vol. 1 (M.B. Schiffer, ed.), University of Arizona Press, Tucson, pp. 1–45.

Sahlins, M.D., and Service, E.R. (eds.), 1960, *Evolution and Culture*, University of Michigan Press, Ann Arbor.

Salmon, M.H., 1975, Confirmation and Explanation in Archeology, *American Antiquity* 40:459–464.

Salmon, M.H., 1982, *Philosophy and Archaeology*, Academic Press, New York.

Salmon, M.H., and Salmon, W.C., 1979, Alternative Models of Scientific Explanation, *American Anthropologist* 81:61–74.

Service, E.R., 1975, *Origins of the State and Civilization*, Norton, New York.

Shaffer, J.G., 1980, Comment on "Symbiosis, Instability, and the Origins and Spread of Agriculture," by D. Rindos, *Current Anthropology* 21:768.

Sober, E., 1980, Evolution, Population Thinking, and Essentialism, *Philosophy of Science* 47:350–383.

Sober, E., 1984, *The Nature of Selection: Evolutionary Theory in Philosophical Focus*, MIT Press, Cambridge, MA.

Steward, J.H., 1942, The Direct Historical Approach to Archaeology, *American Antiquity* 7:337–343.

Steward, J.H., 1955, *Theory of Culture Change: The Methodology of Multilinear Evolution*, University of Illinois Press, Urbana.

Steward, J.H., and Setzler, F.M., 1938, Function and Configuration in Archaeology, *American Antiquity* 4:4–10.

Taylor, W.W., 1948, *A Study of Archeology*, American Anthropological Association, Memoir No. 69.

Teltser, P.A. (ed.), 1995, *Evolutionary Archaeology: Methodological Issues*, University of Arizona Press, Tucson.

Watson, P.J., 1986, Archaeological Interpretation, 1985, in: *American Archaeology Past and Future: A Celebration of the Society for American Archaeology, 1935–1985* (D.J. Meltzer, D.D. Fowler, and J.A. Sabloff, eds.), Smithsonian Institution Press, Washington, DC, pp. 439–457.

Watson, P.J., LeBlanc, S.A., and Redman, C., 1971, *Explanation in Archeology: An Explicitly Scientific Approach*, Columbia University Press, New York.

Watson, P.J., LeBlanc, S.A., and Redman, C., 1984, *Archeological Explanation: The Scientific Method in Archaeology*, Columbia University Press, New York.

White, L.A., 1945, History, Evolution, and Functionalism: Three Types of Interpretation of Culture, *Southwestern Journal of Anthropology* 1:221–248.

White, L.A., 1959a, The Concept of Evolution in Anthropology, in: *Evolution and Anthropology: A Centennial Appraisal* (B.J. Meggers, ed.), The Anthropological Society of Washington, Washington, DC, pp. 106–124.

White, L.A., 1959b, *The Evolution of Culture*, McGraw–Hill, New York.

Willey, G.R., 1962, The Early Great Styles and the Rise of the Pre-Columbian Civilizations, *American Anthropologist* 64:1–14.

Willey, G.R., and Phillips, P., 1958, *Method and Theory in American Archaeology*, University of Chicago Press, Chicago.

Yarnell, R., 1985, Review of *The Origins of Agriculture: An Evolutionary Perspective*, by D. Rindos, *American Antiquity* 50:698–699.