



# History and explanation in archaeology

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## Abstract

Paukerat argued (2001: 73–98) that a new paradigm – historical processualism (HP) as operationalized by practice theory – is preferable to processual, behavioral, and evolutionary archaeologies as a source of explanation for culture change. To make his case, Paukerat sets up several contrasts between HP and the other three approaches. He claims that HP is superior because it neither essentializes behavior nor calls on potentially false universal laws to create explanations. He argues that HP holds that human practices – individual, particularistic human behaviors – generate new practices as they are continuously re-enacted and renegotiated, and thus practice is the proximate cause of cultural change. Evolutionary archaeology incorporates such particularistic and proximate causes but goes far beyond HP by providing an explanatory *theory* that specifies ultimate causes of culture change. It employs Darwin's scientific theory of historical change, rewritten in *testable*, archaeological terms. In contrast, HP provides no testable implications of historical change.

## Key Words

agency • evolution • explanation • historical processualism • science

Darwinism has long played a role in Americanist archaeology, although it has largely been a metaphorical role, such as when Kidder (1917) and Ford (1936) spoke of 'genetic' connections between similar artifact styles. Within the last quarter-century, however, there have been serious efforts to adapt evolutionary theory to archaeology. The primary goal of what has become known as evolutionary archaeology (EA hereafter) is twofold: to build sets of related cultural lineages and to construct explanations for those sets of lineages having the shape they do. Both steps employ concepts – transmission, natural selection, and heritability – that are embedded within evolutionary theory. Darwin (1859), however, did not have the archaeological record in mind when he developed his theory. As a result, archaeologists have spent considerable time developing both the bridging theory and the methods needed to apply Darwinism to the material record (e.g. Dunnell, 1980; Lipo and Madsen, 2001; Lipo et al., 1997; Lyman and O'Brien, 2000; O'Brien and Lyman, 2000b, 2003a; O'Brien et al., 2001, 2002).

These efforts have spawned a number of criticisms (e.g. Bamforth, 2002; Boone and Smith, 1998; Cowgill, 2000; Kehoe, 2000; Loney, 2000; Preucel, 1999; Schiffer, 1996; Shennan, 2002; Spencer, 1997; Trigger, 1989; Watson, 1986; Weiss and Hayashida, 2002; Wylie, 1995, 2000), even from other evolutionary archaeologists (e.g. Neff, 2000). We welcome these criticisms because they cause us to reconsider and/or amplify certain points we have made with respect to EA and its ability to explain culture change. In responding to various criticisms, we have been able both to highlight differences between EA and other approaches – human behavioral ecology (Lyman and O'Brien, 1998), processual archaeology (Lyman and O'Brien, 1998), and behavioral archaeology (O'Brien et al., 1998) – and to point out areas of epistemological and methodological agreement (O'Brien and Lyman, 2000a, 2002b). Our goal has been to work toward an evolutionary synthesis in archaeology similar to what occurred between biology and paleontology in the early 1940s (Lyman and O'Brien, 1998; O'Brien and Lyman, 1999).

One interesting criticism of EA appeared in the inaugural issue of *Anthropological Theory*. In it, Pauketat (2001) contrasted EA and 'historical processualism' (HP hereafter) in terms of how well each approach explains culture change. We use the term 'interesting' with respect to Pauketat's discussion because of the ontological and epistemological issues it raises. That article, together with a later one (Pauketat, 2003), provides a springboard from which to examine two topics – history and explanation – that we view as fundamental to understanding culture change. The different manners in which EA and HP handle these topics reflect contrasting views on the causes of culture change and where those causes are lodged: Are they internal or external to the phenomena being studied? HP views cause as being internal. 'Practice', or what people do, is seen as the actual process of culture change. EA looks to external processes such as selection and drift for explanations of change. It does not ignore practice as an agent of culture change, but instead views it as a proximate cause, as does HP. And also like HP, EA holds that an important role of practice is as a mechanism for the production of cultural variation – the fuel that feeds ultimate evolutionary causal processes such as selection and drift. EA does not, however, view practice as an *ultimate* explanation of anything cultural.

Our tack here is not to debate the merits of HP as an explanatory approach, nor is it to formulate a detailed, point-by-point rebuttal of Pauketat's sketch of EA. The tenets of EA, at least as we define them, have been laid out numerous times (e.g. Lyman and O'Brien, 1998, 2000; O'Brien and Lyman, 1999, 2000a, 2000b, 2002b, 2003b), and interested readers can review them at their leisure. Rather, our goal is to examine the claim that an emphasis on cultural practice provides a *superior* explanation of culture change than what is provided through an emphasis on evolutionary processes. This claim, at least as Pauketat makes it, is based on the fact that HP avoids 'a commitment to science and ultimate causality [in favor of] the less positivistic and proximate explanations of history' (Pauketat, 2001: 73). At issue is not only where cause is lodged but the broader and related issues of what constitutes science and explanation and what role history plays in them – topics that should have wide appeal to both anthropologists and archaeologists interested in culture change. As a case study of how HP uses human practice to explain the archaeological record, Pauketat (2001, 2003) discusses the rise of Cahokia, a large mound-and-palisade center in the American Bottom of west-central Illinois that dates from around AD 1050. We use that same case study to examine briefly how EA might begin to explain the rise of such a center.

## HP AND CAUSE

To Pauketat (2001: 73), HP produces 'historical explanations, in the process altering the questions that archaeologists ask and the data that they must gather to address those questions'. This alteration supposedly makes HP so distinct from its precursors that it can be labeled an 'emerging paradigm' (Pauketat, 2001: 73). Elsewhere, Pauketat (2003: 39) refers to HP as a 'theoretical groundswell in archaeology [that] is moving the study of demographic displacements and migrations to the fore of explanations of causation'. Pauketat (2001: 74) also reports that '[t]oday, history matters in archaeology'. At first glance that declaration seems odd, given that history has *always* mattered in archaeology (e.g. Lyman et al., 1997; O'Brien and Lyman, 1999), but note that by 'history' historical processualists mean 'what people did and how they negotiated their views of others and of their own pasts' (Pauketat, 2001: 73). This definition, then, restricts history to a chronicling of what people did and what was going through their minds as they made their way through life.

HP is Pauketat's version of what commonly is referred to as *practice theory* (Bourdieu, 1977) or *agency theory* (Giddens, 1979) – approaches that have seen widespread adoption in archaeology, despite the fact that the original concepts have not always been well scrutinized (Dobres and Robb, 2000). Its name notwithstanding, HP has little in common with the approach in Americanist archaeology known as *processualism*. This can be seen in how HP 'relocates the processes that we seek to explain and revises how we understand cause and effect' (Pauketat, 2001: 75). In HP, explanations involve identifying 'the proximate causes of *how* a certain social feature . . . developed in a particular time or place' (Pauketat, 2001: 74). Note the use of *proximate* with respect to cause. We do not doubt that HP can account for the near-term causes of culture change, much in the same way that White's (1949, 1959) version of cultural evolutionism identified human intent, or 'urges', as a cause (Dunnell, 1980; Lyman and O'Brien, 1997, 1998). In fact, one's analytical ability to account for near-term causes is assured, given comments such as 'people's actions and representations – "practices" – are generative . . . [P]ractices *are* the processes, not just consequences of processes. Thus, they generate change. That is, practices are always novel and creative, in some ways unlike those in other times or places. This means that practices are historical processes to the extent that they are shaped by what came before them and they give shape to what follows' (Pauketat, 2001: 74).

How could one argue with the statement that practices are generative – that they generate new practices? And how could one not agree that practices are in some ways unlike those that occurred in other places and at other times? Everything we know about humans past and present strongly indicates that practices produce practices and that they differ from each other. If, as Pauketat suggests, practices are the actual processes of culture change, then we can always explain change in terms of what humans do. All we need to do is 'focus attention on the creative moments in time and space where change was actually generated' (Pauketat, 2001: 87). As long as we can correctly identify the kind of practice – accommodation, coercion, collaboration, revitalization, and the like (Pauketat, 2001: 80) – then we have our precise proximate cause. The only problem is, how do we set up falsifiable hypotheses so that we know when we are wrong? HP is silent on this, primarily because falsification is a characteristic of science, and science is anathema to HP.

## SCIENCE, HISTORY, AND EXPLANATION

Science is a formal means of understanding and explaining how and why the world works the way it does. It consists of two components, theory and performance (empirical) standards. Theory specifies three things: the kinds of phenomena to be examined; how phenomena are to be structured and measured for examination; and how those phenomena should interrelate, interact, and respond to one another given particular contingencies. Theory thus provides the basis for explanation. A discipline is scientific if its theory is dynamically sufficient – that is, if its theory contains the proper elements in the proper structure to generate scientifically acceptable explanations. Dynamically sufficient theories generate explanations, not the other way around. We cannot, as Lewontin (1974b: 8) put it, ‘go out and describe the world any old way we please and then sit back and demand that an explanatory and predictive theory be built on that description’. A scientifically acceptable explanation is one that works well in an empirical context, meaning that the performance standards tell us that a conclusion accounts for the phenomena it is supposed to. By definition an empirically sufficient theory is one that has elements (units) that are directly measurable in the phenomenological world (Dunnell, 1986; O’Brien and Lyman, 2002a). Elements that are not measurable – and with respect to the archaeological record we would include many human practices and almost all human intentions – render empirical testing impossible.

Notice that nowhere in our definition of science is there mention of the term *law*. To be sure, science deals with laws, and in fact theory building ‘is a constant interaction between constructing laws and finding an appropriate set of descriptive [units] such that laws can be constructed’ (Lewontin, 1974b: 8). This statement, however, obscures the fact that there is more than one kind of law. Failure to appreciate this fact can lead to a rejection of science as a method of explaining history (Cooper, 2002; Gould, 1986; Moore, 2002; for a recent pertinent discussion by an anthropologist, see Carneiro, 2000). HP, for example, rejects science as an approach to explaining culture change because science involves laws. To Pauketat (2001: 74–5), science and laws came into archaeology through its fascination with discovering ‘“cultural processes” . . . abstract, law-like principles of *why* something occurred. These ultimate explanations tended to leapfrog over historical data, making them reductionist to the point of being trivial or easily debunked’.

Pauketat is correct that processual archaeology focuses on the identification of law-like principles of human behavior that purportedly explain why something occurred. And we would agree that in large part it has failed to identify such principles (O’Brien, 1996; O’Brien and Holland, 1995). But processual ‘explanations’ of culture change fail not because they are scientific or because they emphasize ultimate cause. Rather, they fail because they follow a model of science, essentialism (Popper, 1950), in which ‘kinds’ and ‘processes’ are invariant. Under this model, what happens today is an excellent proxy for what happened yesterday or will happen tomorrow. Any statement made about relations between and among units within a set of entities must by definition be universally true, given that the entities themselves are timeless and spaceless. This is why law-based explanations ‘leapfrog over historical data’. Such data are relevant only as instances of the action of laws (e.g. Trigger, 1970, 1973; Watson, 1973; Watson et al., 1971).

This kind of science explains the action of such things as atomic particles and elements, but it cannot lead to logical and consistent explanations of human actions

(Dunnell, 1980; Lyman and O'Brien, 1998; O'Brien, 1996; O'Brien and Holland, 1995; O'Brien et al., 1998). So do we do what Pauketat suggests and rid archaeology of science? No. Rather, we shift our attention to an alternative kind of science, materialism, in which history becomes all important (Dunnell, 1980; Ereshefsky, 1992; Gould, 1986; Hull, 1965; LaPorte, 1997; Lewontin, 1974a; Mayr, 1959, 1987; Simpson, 1963, 1970; Sober, 1980). Darwinism is the epitome of materialist science. It views relations among phenomena as being time- and space-bound, meaning that what happens today is no exact predictor of what happened yesterday or of what might happen tomorrow. Similarly, what happens in location A is no exact predictor of what happened in location B or of what might happen in location C. If organisms are governed by any law – and it is more of a limiting law than one that allows us to predict – it is the law of *contingency* (Beatty, 1995; Brandon, 1990). This law states that what happens at, say, point D is *conditioned* to some degree, not *determined*, by what happened at preceding points C, B, and A.

Materialism's emphasis on contingency in no way implies that essentialist laws do not apply to organisms. To the contrary, an organism's entire somatic being obeys physical and chemical laws. What is important is the recognition that organisms are mixes of *configurational properties* – a set of characteristic traits that a thing possesses and which depend on its position in time and space – and *immanent properties* – a set of characteristic traits that a thing possesses regardless of where it exists in time and space (Simpson, 1963, 1970). Although organisms are mixtures of those two distinct kinds of traits, the mixtures are like oil and water. Activities, or behaviors, are configurational properties, not immanent properties or some blend of the two. As such they cannot be timeless and spaceless (Wolverton and Lyman, 2000).

It should be clear from this brief discussion of laws and kinds of science that we strongly disagree with HP's dictum that archaeologists can be either scientists or historical processualists but not both. Various processual archaeologists (e.g. Bamforth and Spaulding, 1982; Trigger, 1973; Watson, 1973) would agree with us. Darwinists by definition *are* both scientists and historical processualists. Boyd and Richerson (1992: 179–80) note that 'Darwinian theory is both scientific and historical. The history of any evolving lineage or culture is a sequence of unique, contingent events'. They ask, 'What makes change historical' and 'What makes historical explanation scientific?' We agree with their conclusion that in 'the biological and social domains, "science" without "history" leaves many interesting phenomena unexplained, while "history" without "science" cannot produce an explanatory account of the past, only a listing of disconnected facts' (Boyd and Richerson, 1992: 201). So what *does* constitute explanation?

From an evolutionary perspective, to '*explain* means to identify a mechanism that causes evolution and to demonstrate the consequences of its operation' (Bell, 1997: 1). The mechanisms are selection and drift (transmission), and importantly, *the causes precede the consequence or effect of the working of the mechanisms*. Further, the causes are lodged *outside* the phenomena that change. Unlike in HP, the causes are not isomorphic with the consequences, meaning that Darwinism is neither teleological nor tautological. Selection and transmission are historical mechanisms; they operate continuously, at some times more strongly or more rapidly than at others, creating the varying tempo of evolutionary change (e.g. Gould et al., 1987). EA clearly recognizes the varying tempo of change (Lyman and O'Brien, 2000; O'Brien and Lyman, 2000b), despite the claim

that to evolutionary archaeologists 'punctuated transformations in social life or technology, if these rely on higher-order changes in organizations or institutions, are not even thought possible' (Pauketat, 2001: 76).

Change, irrespective of tempo or mode, is a historical process, but what is history? O'Hara (1988: 144) provides a useful discussion:

[G]enerally speaking a *chronicle* is a description of a series of events, arranged in chronological order but *not* accompanied by any causal statements, explanations, or interpretations. A chronicle says simply that *A* happened, and then *B* happened, and then *C* happened. A *history*, in contrast to a chronicle, contains statements about causal connections, explanations, or interpretations. It does not say simply that *A* happened before *B* and that *B* happened before *C*, but rather that *B* happened *because* of *A*, and *C* happened *because* of *B*. . . *Phylogeny* is the *evolutionary chronicle*: the branched sequence of character change in organisms through time. . . [H]istory, as distinct from chronicle, contains a class of statements called *narrative sentences*, and narrative sentences, which are essential to historical writing, will never appear in [chronicles]. A narrative sentence describes *an event*, taking place at a particular time, with reference to *another event* taking place at a *later* time. . . Just as narrative sentences distinguish history from chronicle, *evolutionary narrative sentences* distinguish evolutionary history from evolutionary chronicle.

The critical points of O'Hara's discussion are that (a) false or inaccurate chronicle cannot result in accurate history and (b) narrative statements provide the explanations for why chronicles read the way they do (Lyman and O'Brien, 1998). Culture historians recognized these distinctions decades ago (Lyman and O'Brien, 1997; Lyman et al., 1997), but they could not escape the same problem that continues to plague evolutionary studies (O'Hara, 1988) – conflating the explanation of *states* and the explanation of *events* of change. Explaining states is basic to essentialism. Explaining events of change is basic to materialism and distinguishes Darwinism as not only a different scientific theory but a different *kind* of scientific theory (Lewontin, 1974a).

## EA AND EXPLANATION

Darwinism is applicable to explaining change among any group of organisms, including humans. Although evolutionary studies are as diverse as their subject matter, their unifying feature is that they encompass 'description[s] of the historical patterns of differential trait representation and arguments as to how evolutionary [processes] acted to create those patterns' (Jones et al., 1995: 29). Both steps employ concepts embedded in Darwinian theory, such as (a) heritability, which denotes continuity such that similarity is homologous; (b) lineage, or a line of development owing its existence to heritability; (c) invention/innovation, a source of new variants; (d) a transmission mechanism, which itself is a source of new variants, given the imperfect fidelity of replication; and (e) natural selection, which is a mechanism of change. Darwin (1859) referred to evolution as 'descent with modification', which we define as 'any net directional change or any cumulative change in the characteristics of organisms or populations over many generations. . . It explicitly includes the *origin* as well as the *spread* of alleles, variants, trait values, or character states. Evolution may occur as a result of natural selection, genetic

drift, or both' (Endler, 1986: 5). Do not be misled by our use of this definition; it does not mean that we equate evolution with changes in the relative frequencies of either genes (O'Brien et al., 2003) or cultural traits (Lyman and O'Brien, 2003), although these are important components of evolution. Mayr (1991: 2) argues that evolution defined merely as 'a change in gene frequencies' is reductionist and that the concept is 'described far better as "a change in adaptation and in biological diversity"'. We agree with Mayr; no one seriously doubts the role played by genes in evolution, but it is only *a* role. The important point is, evolution means significantly more than simply changes in allelic frequencies, and it is in that expanded arena that archaeology, biology, and paleontology fit comfortably.

Evolution comprises change in the composition of a population over time. In archaeology the population, not surprisingly, usually comprises artifacts. It is 'the differential representation of variation at all scales among artifacts for which [evolutionary archaeology] seeks explanations' (Jones et al., 1995: 28). One might legitimately ask why analytical emphasis is placed on artifacts when it is the makers of the artifacts who are evolving. The answer is, evolutionary archaeology rests on the premise that objects occurring in the archaeological record, because they were parts of past phenotypes, were shaped by the same evolutionary processes as were the somatic (bodily) features of their makers and users (Dunnell, 1989; Leonard and Jones, 1987; O'Brien and Holland, 1995). This is a shorthand way of saying that the *possessors* of the objects were acted on by evolutionary processes. No evolutionary archaeologist to our knowledge has ever claimed that changes in technology are a result of selection working on the artifacts themselves (but see Pauketat, 2001: 75).

That artifacts are phenotypic is nonproblematic to most biologists, who routinely view such things as a bird's nest, a beaver's dam, or a chimpanzee's twig tools as phenotypic traits (e.g. Bonner, 1980, 1988; Dawkins, 1990; Turner, 2000; von Frisch and von Frisch, 1974). Neither is it problematic to paleontologists, who have to rely on the hard parts of phenotypes (shells, bone, and the like) to study the evolution of extinct organisms and the lineages of which they were a part. Many anthropologists might buy into this argument, but they would draw the line when it is suggested that such things as tools, clothing, and houses are parts of the human phenotype. Why does this dual view exist? Surely we should have no trouble accepting that the *behaviors* that lead to creation of a stone tool or the way people dress or build their houses are phenotypic, irrespective of the origin of the behaviors. If the behaviors are phenotypic, then the results of the behaviors are phenotypic as well. But many of us have a problem with viewing human behavior as phenotypic. We see ourselves as being quantitatively and/or qualitatively *so* different from the rest of the natural world that we warrant not only a whole new set of laws but also a different set of philosophical questions with which to examine ourselves. EA does not accept that argument.

If we accept what to us is a rather noncontroversial notion that one of the functions of an organism as an interactor with its environment (Hull, 1988a, 1988b) is to act as a vehicle (Dawkins, 1982) for the replicative units it carries – the genes – then it stands to reason that the vehicle (the phenotype) has to do its job of protecting the replicators (the genotype) so that information is passed to the succeeding generation. If the vehicle does not do its job, or if it is prevented from doing its job, the germ line dies. Nature has shaped an almost infinite number of vehicles, some of which perform their jobs

better than others do in a given environment. The evolutionist's job is to figure out how and why under a given set of circumstances some individuals (vehicles) are more successful – more fit – than others in passing their genes on to succeeding generations. Lest this sound as if it is the most narrow reading of what an evolutionist's job is, we point out that the investigation of the myriad pathways to success that organisms, including humans, have taken is anything but narrow and uninteresting. It brings us into direct contact with all of the complex features produced by the evolutionary process, irrespective of whether those features are somatic or not, that potentially affect an organism's fitness (O'Brien and Lyman, 2003a).

Time is treated as a continuous variable in EA, and 'change is conceived [of] in terms of frequency changes in analytically discrete variants rather than the transformation of a variant' (Teltser, 1995: 53). Such changes might be the result of natural selection and thus represent shifts in adaptational state, or they might be the result of drift (Dunnell, 1978; O'Brien and Holland, 1992). The analytical challenge is to determine which is applicable in any given situation (O'Brien and Leonard, 2001; VanPool, 2001, 2002). On the one hand, such a challenge demands the study of immanent properties and processes and the construction of laws concerning them (Gould, 1986; Simpson, 1963, 1970), as well as the construction of a set of units for measuring and describing a lineage's fossil record – that is, for writing a historical chronicle (Dunnell, 1992). On the other hand, explaining *why* the lineage has the appearance it does demands that the uniqueness of historical contingencies and configurations be considered (Beatty, 1995).

Showing that a particular phenotypic trait has a positive fitness value is critical to evolutionary studies. In archaeology, this might require that the mechanical properties of artifacts be measured (e.g. Allen, 1996; O'Brien et al., 1994; VanPool, 2001; VanPool and Leonard, 2002) – an operation not so different from what is carried out in biology (e.g. Mayr, 1983). Does a particular kind of pottery work better within the particular time–space position it occupies than some other kind of pottery does? If so, why? In other words, how does that particular *state* of pottery work in that context? Additionally, what is the selective environment in which it is found, and what were the selective environments that led to its appearance? These are questions about the *history* of change in pottery. They are what make EA evolutionary. Answering the questions regarding pottery *state* requires the use of immanent properties and processes, or an essentialist ontology; answering the questions regarding pottery *change* requires the use of configurational properties and processes, or a materialist ontology (Simpson, 1963, 1970; Wolverton and Lyman, 2000).

Some critics (e.g. Bamforth, 2002; Pauketat, 2001; Watson, 1986) make it sound as if EA deals only with artifacts, and often only at the microanalytical level. This is untrue, although evolutionary archaeologists have not stressed the point well enough that EA *ultimately* deals with people. It does so through analysis of the hard parts of their phenotypes. Neither have evolutionary archaeologists stressed that EA is both a macro- *and* a microanalytical approach. Recall the quotation by Jones et al. (1995: 28) cited earlier: It is 'the differential representation of variation at all scales among artifacts for which [evolutionary archaeology] seeks explanations'. The words 'at all scales' are important because they aptly describe the purview of EA. As we noted earlier, demonstrating that a particular phenotypic trait has a positive fitness value is critical to evolutionary studies, but that trait could encompass any number of scales. Sometimes the scale might be at



the object level – a ceramic vessel, for example – and sometimes it might be at a smaller scale – the kind of material used to temper a ceramic vessel. It also might be at the level of sets of objects – assemblages, as they usually are labeled. Objects, however, can be more than simply ceramic vessels or projectile points. They can be such things as earthen mounds. A set of objects could include the mounds at a site such as Cahokia. From a human evolutionary standpoint, those mounds are every bit as much a part of the human phenotype as bone, skin, language, and stone tools are. The kinds of sorting processes – selection and drift being the most prominent – that affect all organisms affected the makers of those mounds. What can the mounds – those highly visible phenotypic features – tell us about those sorting processes?

## EXPLAINING THE RISE OF CAHOKIA

Recent work by Pauketat and his colleagues (e.g. Pauketat, 1997a, 1997b, 1998, 2003; Pauketat and Emerson, 1999; Pauketat and Lopinot, 1997; Pauketat et al., 2002) has given us significant insights into the rise and development of Cahokia as a powerful community in the central Mississippi River valley. Sometime around AD 1050 a disruption of the previous social landscape occurred in and around the American Bottom of west-central Illinois:

A central plaza, earthen pyramids, and perimeter features were built as center points of a planned quadrilateral order. That order seems fixed into the landscape such that even residential buildings were aligned to it . . . The order was, however, dynamic and subject to ongoing 'negotiations' as witnessed in the planned but accretional appearance of the site, likely a consequence of the disparate developmental histories of attached kin groups. (Pauketat, 2003: 43)

Pauketat makes the case that Cahokian society was pluralistic, meaning that it absorbed nonlocal people into its fabric. Certainly in some outlying areas there is evidence that nonlocal 'Mississippianized' people practiced a distinctive mortuary tradition alongside that practiced by Cahokians (Emerson and Hargrave, 2000). There also is considerable evidence that flood-plain villagers resettled into upland farmsteads as part of the process of 'Cahokianization'. That process was not merely 'an outcome of an *in situ* development where management of or competition over pre-Mississippian household surpluses supposedly led to institutionalized social inequalities . . . [Rather,] Mississippian farmers had agency' (Pauketat, 2003: 56). Pauketat doesn't define *agency*, but we take it to mean the behavior on the part of individuals who believe they can influence the outcome of a future event (Cohen, 1987; Giddens, 1984). Given this definition, no doubt what Pauketat says is true: The rise of Cahokia was *not* merely an outcome of household surplus leading to institutionalized social inequality. Further, there is little doubt that, in general, any resettlement strategies cooked up by Cahokia's elites were

contingent on farmers accepting, accommodating, or resisting top-down power. The strategies of 'the few' matter little, after all, if 'the many' refuse to heed them or accommodate them; power and hegemony are clearly 'relational' if not 'consensual'. . . . Prestigious or high-ranking families – even charismatic individuals – may have

been part of the abrupt Cahokian coalescence, but identifying their actions alone is insufficient to understand the larger process of cultural construction. In the Greater Cahokia region, that process was demonstrably collaborative, consensual, and communal to the extent that it involved regional participation and a pervasive alteration of all people's practices within the Greater Cahokian sphere. (Pauketat, 2003: 56–7)

Pauketat sees his perspective as running counter to the traditional, processualist view of how a complex unit such as Cahokia arose. That view (e.g. Milner, 1998) emphasizes such things as agrarian rules of risk reduction and subsistence production and the goal on the part of farmers to produce an agricultural surplus. That surplus then

was available for expropriation by 'high ranked people', gradually leading up to the founding of Cahokia (or any other such capital). Once established, such chiefdoms were unstable, subject to fracture along various old cleavage planes. Any physical-environmental problem of sufficient magnitude would provide the wedge to split the chiefdom into its component parts, those loosely articulated households with their low-order administrations. (Pauketat, 2001: 84)

The kind of model summarized by Pauketat is similar to numerous processual-archaeological schemes devised to account for the advent, development, and decline of powerful prehistoric polities. And as Pauketat (2001) points out, the models suffer from at least three weaknesses: (1) they ignore the actions and representations of people other than high-ranking individuals; (2) they view behavior as invariant (one might say *normative*); and (3) they tend to group complex societies into a small number of units (e.g. chiefdoms and states). Pauketat wants to turn processual models on their heads and cast the analytical spotlight on the little guys instead of on the elites. It was those little guys – Mississippian farmers with agency – who, according to Pauketat, shaped the history of Cahokia.

Granted, processual models have been criticized on various fronts by evolutionists (e.g. Dunnell, 1980; Leonard and Jones, 1987; Lyman and O'Brien, 1998, 2001; O'Brien, 1996; O'Brien and Holland, 1990) and non-evolutionists (e.g. Brumfiel, 1994; Cowgill, 1975) alike, but is the way around the problem to do what Pauketat (2001: 88) suggests and focus attention only on 'what people did and how they negotiated their views of others and of their own pasts'? We don't think so, and neither do most processualists (e.g. Spencer, 1993). Although we would not lessen the importance Pauketat places on the whole of a population – farmers as well as elites – and on human negotiation, we would argue that we will never know the precise nature of those interpersonal negotiations. Neither will we ever know what one group of prehistoric people thought of another group nor how either group negotiated its view of its own past. Pauketat (2001: 76) derides this rather pessimistic sentiment, laying its genesis at the door of a supposed tenet of EA that human agency and social change are reliant on essentialist abstractions. Further, he claims that 'only instances about the gradual (unintentional, uninstitutionalized) evolution of technology and the behavior that produced it are seen as legitimate in the [EA] agenda' (Pauketat, 2001: 76).

None of these assertions is true. EA does not focus solely on gradual (and unintentional) evolution of technology, nor does it hold that human agency and social change

rely on essentialist abstractions. Nor has EA ever said that agency is unimportant. Rather, EA has said that it oftentimes is impossible to identify human agency archaeologically. That is why we said that we probably will never know what one group of prehistoric people thought of another group or how either group negotiated its view of its own past. We treat agency the same way we treat human intent: It's there, it's important at least as a proximate cause of change (O'Brien and Holland, 1992), but we cannot see it. If we cannot see it, how can we establish falsifiable statements about its role in culture change (Flannery, 1967; Leonard and Reed, 1993; Rindos, 1985)?

Pauketat's comments represent a complete misunderstanding of EA, but they shed light on why Darwinism often is viewed as nonapplicable to the archaeological record. Pauketat has confused essentialist abstractions with the units that EA uses to measure change. There is nothing essentialist about those units, unless one holds to a strange notion that all units are essentialist constructs. Change cannot be measured without units. Importantly, the units EA uses are intensional – meaning that they are created for a specific purpose and then imposed on the data – as opposed to extensional – meaning that they are extracted from the data (Dunnell, 1986; Lyman and O'Brien, 2002; O'Brien and Lyman, 2002a). Pauketat's conflation of EA's measurement units and the essentialist abstractions that archaeologists have long borrowed from ethnology leads to his statement that EA defines behavior as 'the repetitive and [invariant] sets of actions that typify human beings' (Pauketat, 2001: 76). In his view, this definition 'essentializes behavior' (Pauketat, 2001: 76) and thus renders the historical processes comprising practice inaccessible. EA has been called a lot of things, but essentialist is not one of them.

Can EA do any better than HP at providing an explanation for the rise of a center such as Cahokia? We do not pretend to be experts on the archaeology of Cahokia, but there are several ways we might approach the problem from an evolutionary standpoint. One involves the wasteful-advertising model developed by Neiman (1997; see also Trigger, 1990), in which costly architecture (e.g. mounds) serves as a means of display among competing elites, similar to how bright plumage functions among male birds. Another way, and the one we explore here briefly, involves the 'waste hypothesis' introduced by Dunnell (1989) and later elaborated theoretically and empirically by him and others (see later in this article). The concept of waste derives from the central place that reproductive success, often referred to as *fitness*, has always had in Darwinism. Under a neo-Darwinian interpretation, any activity in which an organism engages that does not work to maximize its reproductive success can be considered 'neutral' behavior. We know, however, that organisms, including humans, engage in significant amounts of behavior that appears to have nothing to do with maximizing immediate reproductive success. In fact, some of the behavior appears downright wasteful in terms of energy, at least as far as reproduction is concerned. Why are organisms not putting as much energy as possible into reproductive activities if getting one's genes into succeeding generations is what counts in evolution?

Let's highlight one word from the foregoing argument: 'behavior that *appears* to have nothing to do with maximizing immediate reproductive success'. Reproductive strategies of any species are complex, and with humans exceedingly so (e.g. Dunbar, 1995; Voland, 1998). But regardless of species we cannot mindlessly use *short-term* reproductive success as an all-inclusive measure of an organism's fitness. After all, what is important to an

organism is not how many children it leaves, but how many *grandchildren*. The question is, can a decrease in reproductive success in the *short* run enhance fitness in the *long* run? If the answer is yes, what set of conditions would favor diverting energy *temporarily* away from reproduction? We might propose that one set consists of an unstable/unpredictable environment. By engaging in wasteful behavior – which in the long run is not wasteful at all – organisms in such an environment lower their population size through lower fecundity. They set up an energy sink (measured in time and resources) that can be devoted if need be to subsistence and reproduction under stressful conditions (Dunnell, 1999). One kind of energy sink could be mound construction:

Under normal conditions, individuals or populations that produce the largest number of young (both culturally and biologically) will pass on the codes (either genetic or cultural) for constructing new individuals or populations most successfully. This means that populations will tend to approach carrying capacity, how closely being a function of variability in the near-term carrying capacity, direct storage (income averaging), and indirect storage (waste, partly). One easily deduced consequence of this line of reasoning is that when environmental perturbations that adversely affect the carrying capacity for a particular set of people are on a large scale and unpredictable . . . populations nearing carrying capacity would be catastrophically eliminated. Any populations with large amounts of waste would suddenly find themselves at a distinct advantage. They would have smaller populations and thus lower resource requirements as well as a reservoir of time to intensify subsistence. Thus given variability in waste/reproductive energy allocations among individuals or populations of individuals, waste would be fixed in highly unpredictable and variable environments. (Dunnell, 1999: 245–6)

What does this mean, exactly? It means that organisms that are used to having extra time on their hands – ‘extra’ meaning time that is not related to reproduction – will do better than their conspecifics if that ‘extra’ time is removed from the equation. What might organisms do with ‘extra’ time on their hands? Well, the members of one species might take some of its more athletic individuals – athleticism being one measure of fitness, perhaps – and organize them into baseball teams. Or, in the absence of possessing the requisite know-how for that, they might have them build mounds. What appears at first glance to be a counter-intuitive proposition is not so difficult to understand when we redefine fitness not as individual reproductive success but as a statistical summary of genetically and culturally transmitted traits that have a bearing on reproduction. With this redefinition comes an analytical interest in long-term as opposed to short-term trends in fitness. What matters is who eventually comes out ahead between those organisms (or groups of organisms) that adopt a high-risk, high-fecundity strategy versus those that adopt a low-risk, low-fecundity strategy. In predictable environments, high-risk takers should always outperform their more conservative counterparts, but in an unpredictable environment, the opposite should be the case. As Madsen et al. (1999) show, good years (in terms of resource return) select for the high-risk, high-fecundity strategy because of the high payoff in terms of offspring. But during bad years that payoff declines precipitously because child mortality preferentially affects those individuals/groups with large families. Importantly, in a random mix of good and bad years, the high-risk strategy

experiences a greater variance in the rate at which it spreads through the population. Everything else being equal, the lower-variance strategy – here low risk, low fecundity – will exhibit the higher rate of increase over evolutionary time.

This is a totally different approach to explaining the presence of cultural phenomena such as elaborate graves, mounds, stockpiles of fancy goods, and the like – one that has little or nothing in common with many current archaeological approaches, including HP. Why? Because it recognizes that evolution is a probabilistic process, not a deterministic one. Probabilistic hypotheses are different than those that characterize essentialist science, where one negative instance disproves a hypothesis. As Dunnell (1999: 247) points out, 'it is the distribution of results, not single cases, that are needed to falsify a [probabilistic] hypothesis'. This is an important point. No evolutionary archaeologist has proposed that humans everywhere and at all times engage in wasteful behavior as a bet-hedging strategy: 'In practical terms, there is no guarantee that waste will appear when "needed", especially when it will have lowered the fitness of its transmitters in most if not all other situations' (Dunnell, 1999: 247). The waste hypothesis simply provides 'a *mechanism* that explains some correlation between the occurrence of cultural elaborations identified as waste and variation in the environment. The "cause" of waste is natural selection acting in the usual fashion in somewhat unusual circumstances' (Dunnell, 1999: 246).

The key words in the foregoing quotation are 'some correlation'. We cannot expect a probabilistic process to act in a deterministic manner. Madsen et al. (1999: 252) put it this way:

Even if one assumes a Darwinian perspective for building explanations of cultural elaboration, no *single* explanation can be expected. . . . A general explanation cannot exist because evolution is an endless interplay of general principles (e.g. natural selection, physiology, and constraints and rules for behavior) with the specific and contingent history of particular populations. . . . The best one can do toward providing a 'general' explanation for a phenomenon is to build increasingly comprehensive models that show how a set of invariant principles interact with variability to shape historical phenomena in consistent ways. General theories about a class of phenomena such as cultural elaboration can be *sufficient* for explaining the material world, but never *necessary*, since these theories cannot specify how the contingent history of a situation interacts with general principles. Unlike general theory, explanations for specific instances of elaboration take the form of a narrative, showing how general principles interact with the history of a population to produce the archaeological record. (O'Hara, 1988)

Will such narratives always be correct? Probably not in terms of 'absolute' truth (as if there were such a thing), but the point of science is not and cannot be to provide 'correct' explanations. Its purpose is to provide explanations that are consistent with both theory and empirical expectations (performance standards) derived from that theory. Consistency is measured through the goodness of fit between empirical expectations and data. To date the waste hypothesis has been tested in various geographic regions – the Ohio River valley (Dunnell and Greenlee, 1999), the lower Mississippi River valley (Hamilton, 1999), Egypt (Sterling, 1999), Ireland (Aranyosi, 1999), and Peru (Kornbacher, 1999) – and the

results (some more preliminary than others) have been consistent with the expectations. In-depth application of the waste hypothesis to mound building and other kinds of cultural elaboration at Cahokia is beyond the scope of this article, but we note that the environmental conditions on which the model rests – unpredictability – appear to be met. By AD 1050 Mississippian farmers across the American Bottom were involved in maize agriculture, a risky proposition in a flood-plain environment (Emerson and Milner, 1982). As Rindos and Johannessen (1991: 44) put it, ‘The close interdependency of the maize-human relationship that developed after AD 750 was such that even slight changes in the environment produced severe perturbations in the subsistence economy. Never before . . . had so many people been so closely tied to each other, to a single crop, and to the floodplain’.

Whether Cahokians actually hedged their bets through ‘wasteful’ behavior remains to be determined through such things as demographic structure and diet (as determined by stable-carbon-isotope ratios). In other words, we need detailed data on the responses that Cahokians made to living on what appears to have been an unpredictable landscape. This is typical of historical sciences such as evolutionary biology; a possible explanation *should* have multiple independent test implications. If Cahokians hedged their bets through wasting, undoubtedly they developed other responses, some of which must have gone hand in hand with wasting. Madsen et al. (1999: 275) make an important observation along this line:

[T]he form that ‘wasteful’ artifacts take potentially provides the variability for other kinds of selective processes. For example, artifacts involved in trade-offs in reproductive effort and success may also be related to costly signaling, functional specialization, and redistribution. It is important to recognize that because of diminishing returns for any one kind of energy expenditure, there are often multiple evolutionary solutions for reducing variance . . . The fixation of any particular trait may require additional fitness consequences resulting from food redistribution and other kinds of functional organizations.

In such cases, these proximate mechanisms can act to intensify selection for costly artifact classes. Increased investment in mound building, for example, may be driven by the bet-hedging effect. However, the fixation of mound building within the population may be due to its role in creating a large-scale food sharing system.

From an evolutionary standpoint the key to long-term fitness appears to lie in variance reduction, and as Madsen et al. (1999) point out, there is no one correct solution. Certainly wasteful behavior itself can take many forms – mound construction, production of ‘prestige’ items, elaborate mortuary practices – all of which are evident at Cahokia (e.g. Fowler, 1991; Fowler et al., 1999; Pauketat, 1997b; Trubitt, 2000). In terms of mound construction, Cahokia is on a par only with itself, its 104 mounds (a conservative number) spread over roughly 5 sq mi (13 km<sup>2</sup>) of rich alluvium. Monks Mound, the largest entirely earthen man-made structure in prehistoric North America, measures 1000 ft (305 m) long and 775 ft (236 m) wide, which means it covers over 17 acres (7.2 ha). Its peak tops out at around 100 ft (30.5 m). Estimates of how much earth went into its construction vary, but one guess (Fowler, 1989) is around 22 million ft<sup>3</sup>

(624,000 m<sup>3</sup>). As Henry Brackenridge (1942: 187) put it early in the 19th century, 'What a stupendous pile of earth!' As impressive as mound building might be, its evolution did not occur in a vacuum but instead coevolved with other strategies such as large-scale food-sharing systems (Madsen et al., 1999). The cascading effects that such coevolution had on Cahokian social and political systems, on the diet and health of Mississippian farmers and elites, and so forth will not be easy to tease apart, but our guess is that the results will be well worth the effort.

## CONCLUSION

Darwin provided the theoretical basis for studying change in organisms – not *all* kinds of change, but certainly all kinds of change that take place over extended (multigenerational) periods of time. It has been the role of scientists ever since to refine that basis and to examine the particulars spawned by nature, some of which form the archaeological record. To us, organisms, including humans, change in part because of selection, regardless of how that selection is presented to the organism. In fact, it really makes no difference to the organism what the source of selection, or of any other evolutionary mechanism, is. It could be the physical environment, the social environment, or both. Or it might be chance that causes change within a lineage of organisms. Our job as archaeologists is to understand not only the context in which evolution occurred but also the outcome of the process. And it is the myriad outcomes that underscore the fascinating story of the emergence and development of humankind.

We view EA as a better approach to explaining the archaeological record than HP based on the fact that science demands that theories have empirical content. Hypotheses derived from theories *must* have testable implications. We are not sure how one would frame a hypothesis regarding practice, let alone how we would derive archaeologically testable implications. We could say that all visible traces are the product of *some* human practice, but this gets us nowhere. A hypothesized specific prehistoric instance of human practice cannot be empirically confirmed or refuted. In HP, 'causes do not exist as abstract phenomena outside the realm of practices' (Pauketat, 2001: 85) because practices *are* the causes of change: 'The locus of change [is] practice, set in the context of a continually redefined and revalued tradition' (Pauketat, 2001: 86). The critical bridge is this: 'Material culture [read *artifacts*], as a dimension of practice, is itself causal' (Pauketat, 2001: 88). The production of artifacts is 'an enactment or an embodiment of people's dispositions – a social negotiation – that brings about changes in meanings, dispositions, identities, and traditions' (Pauketat, 2001: 88). Practices change because they are reworked with every manifestation. HP does not necessarily need to know the 'meanings' of particular past practices (Pauketat, 2001: 87). Given that practice is reworking, and reworking is practice, it suffices to know that a change in the archaeological record comprises a change in practice. As a result, there is no leftover inexplicable cultural or artifactual residue. This actually sounds to us much like some of the characterization of normative theory provided nearly 40 years ago by Binford (1965), except that rather than norms, practice is the key concept.

As an aside, we are unsure how representative of practice theory Pauketat's version of HP really is. We say this after contrasting his position with those of, say, Dobres (2000; see also Dobres and Hoffman, 1994) and Drennan (2000). In Drennan's case, he carefully considers how individual decision making is part of larger processes of the kind

that archaeologists have long had an interest in examining. Drennan (2000: 190) demonstrates in convincing fashion that 'one of the ways in which explicit attention to individual decision making enriches our accounts is by offering at least some possibility of a harmonious union between macro- and micro-analytic levels'. We find his treatment of decision making at the individual level to be a source of several interesting evolution-based propositions – a finding in line with that of VanPool and VanPool (2003) after they surveyed the work of others with an interest in developing an agency theory applicable to the archaeological record. Drennan's treatment certainly is an improvement over simply stating that 'practice and tradition are *what* people do and *how* they do it, with no strings attached to functionalist equations of why people do it' (Pauketat, 2001: 76).

Darwinian explanations of the organic world are not simply 'functionalist equations'. Rather, they are historical narratives directed at identifying and understanding ultimate cause. As Campbell (1974: 420) put it, Darwinism is 'the universal nonteleological explanation of teleological achievements'. This means that its explanations depend on processes – selection and drift, for example – that are *external*, not *internal*, to the phenomena being explained. HP, with its emphasis on negotiation and other practices, lodges cause in the phenomena to be explained. To an evolutionist, 'why-type' questions are the most interesting questions one can ask. To use HP's term, why did one set of practices work in one situation but not in another? Why did a certain practice become fixed within one population but not in another? We are not saying that EA can always answer these questions. As with science in general, its data requirements are high, and we have to face the fact that some questions are beyond our reach, despite statements to the contrary.

Bettinger and Richerson (1996: 226) suggest that 'few archaeologists will ever be privileged to participate in constructing a "how actually" explanation'. They are pointing out that the historical chronicles and narratives of EA are merely plausible stories. We agree, but we also underscore that the stories constructed under EA are *theoretically informed* and thus are *testable*. Bettinger and Richerson are also arguing that the real story will never be known. Apparently, by distinguishing between 'how possibly' and 'how actually' explanations, they are suggesting that they find little satisfaction with the former, characterized by O'Hara (1988: 149) as statements regarding 'how a change *may* have taken place', and would much prefer the latter, or how a change '*did* take place' (O'Hara, 1988: 150). Bettinger and Richerson's point is that the latter is impossible to attain. We do not disagree with this point (Lyman and O'Brien, 1998; O'Brien and Lyman, 2000b; O'Brien et al., 2003).

Bettinger and Richerson (1996) reference Brandon (1990) when making the distinction between 'how possibly' and 'how actually' explanations. Brandon's point was that 'how possibly' explanations are quite valuable to Darwinism and in many cases can be distinguished epistemologically from explanations of the 'how actually' sort. When a 'how possibly' explanation accounts for numerous observations and provides an empirically and logically – both founded in theory – coherent explanation, it attains the status of a 'how actually' explanation yet remains testable in light of new evidence. Brandon (1990: 183) acknowledges that we may never know when we have truly attained the latter, though he also states that 'no one can fairly describe [such a "how possibly" explanation] as merely an imaginative bit of story telling'. It is for this reason that evolutionary biologists interested in historical questions are unafraid to accept 'how possibly' sorts of



answers to the questions addressed to the fossil record (Cooper, 2002; Gould, 1986; Moore, 2002). As we have said before, archaeologists might do well to pay heed.

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