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Evolutionary Implications of Design and Performance Characteristics of Prehistoric Pottery

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A Darwinian evolutionary approach offers a powerful means of examining change archaeologically. The approach is based on the tenet that materials contained in the archaeological record were parts of human phenotypes, as were behaviors behind the manufacture, use, and discard of the materials. Engineering-design analysis and performance analysis of material remains — here variously tempered ceramic cooking vessels from the midwestern United States — extend the archaeologist's ability to see and map variation beyond that which is readily apparent and thus are logical points from which to begin examination of human adaptation. The approach is essentially indistinguishable from that used by biologists to study design and function of biologically based features as well as phylogenetic histories of such features.

KEY WORDS: evolutionary archaeology; cooking vessels; design analysis; performance characteristics; human behaviors.

INTRODUCTION

> For many years population genetics was an immensely rich and powerful theory with almost no suitable facts on which to operate. It was like a complex and exquisite machine, designed to process a raw material that no one had succeeded in mining. Occasionally some unusually clever or lucky prospector would come upon a natural outcrop of high-grade ore, and part of the machinery would be started up to prove to its backers that it really would work. But for the most part the machine was left to engineers, forever tinkering, forever making improvements, in anticipation of the day when it would be called upon to carry out full production. Lewontin (1974, p. 189)

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Increasingly in the last several years, archaeologists (e.g., Leonard, 1989; Leonard and Jones, 1987; Neff, 1992, 1993; O'Brien and Holland, 1990, 1992; Rindos, 1984, 1989) have called for the integration of a Darwinian evolutionary paradigm into American archaeology. Building on the base laid primarily by Dunnell (e.g., 1978, 1980, 1982, 1988, 1989), archaeologists interested in evolution and its attendant processes, especially selection, have begun to construct an impressive theoretical machine, but the machine has yet to stamp out more than a few marketable products. In effect, the Darwinian theory of archaeological change is in much the same position as Lewontin (1974) noted for population genetics: Although we have continued to fine-tune the theory, it remains basically that — a theory with very few applications. The failure of evolutionary archaeology to engage the discipline lies in large part in the areas of development of connections between theory and data and the construction of analytical units appropriate to the kinds of historical analysis mandated by a Darwinian evolutionary approach (Dunnell, 1987). The problem, in other words, is not that the theoretical machine is somehow flawed but rather that it lacks suitable raw materials to process.

Our objective here parallels that of two previous articles (O'Brien and Holland, 1990, 1992) — establishing the logical connection between evolutionary theory and archaeological remains — but we expand those earlier discussions significantly. Nowhere do we argue that rewriting evolutionary theory "in terms that have empirical representation in the archaeological record" (Dunnell, 1980, p. 89) is easy or that a cookbook for the application exists. Not only is archaeology steeped in a non-Darwinian mindset, but even the transfer to archaeology of Darwinian concepts such as adaptation and fitness (we also use the equivalent term *adaptedness*) has been anything but smooth and rigorous (O'Brien and Holland, 1992). Our objective is to demonstrate the logical connection between evolutionary theory and data derived through analysis of objects in the archaeological record, but we emphasize at the outset that the paper raises more problems than it solves. However, we also emphasize that this should be the normal course in science. Scientific progress *must* raise new questions and problems; it cannot, as Sober (1991, p. 275) notes, make "rashes on the skin of the body scientific" go away. In contrast, archaeology has enjoyed a long history of providing pat answers to questions of culture change — answers that unfortunately often turn truly complex and interesting questions into trivial-sounding problems that can be dispensed with quickly. We pose some of those questions, many of which earlier were thought settled, in evolutionary terms.

The key to linking evolutionary theory to the archaeological record successfully lies in how we view objects contained in the record and behaviors that led to their creation, use, and discard. Our argument (see Dunnell, 1988; Leonard and Jones, 1987; Neff, 1992; O'Brien and Holland, 1990, 1992) that a Darwinian evolutionary approach offers a powerful means of examining change archaeologically is based on a single tenet: Materials contained in the archaeological record were parts of human phenotypes, as were behaviors behind the manufacture, use, exchange, and eventual discard of the materials. Viewed as such, those materials and behaviors can contribute as much information regarding human adaptedness and adaptation (both of which are discussed later) as can analysis of purely biological features. We argue that engineering-design analysis of material remains — how they were manufactured — as well as performance analysis — how well they performed certain functions — are logical points from which to begin examination of human adaptation and adaptedness. This approach obviously is grounded in materialism and essentially is indistinguishable from the approach used by biologists to study (a) the design and function of biologically based features such as wings and beaks and (b) phylogenetic histories of such features. It also obviously has much in common with behavioral archaeology (see especially Schiffer, 1976).

To this point the evolutionary-archaeology literature has focused almost exclusively on technological histories of material remains at the expense of performance histories. But as we discuss later in detail, changes in technology are driven by selection for *performance characteristics,* which in turn are driven by changes in the various behaviors of the makers and users of the materials as they respond to the selective environment. We agree with Neff (1992, p. 141) that "like other phenotypic characteristics, the behaviors used to make [objects] at a particular place and time (and therefore the [objects] themselves) can be explained by reference to history (descent, inheritance of information) and selective retention." In other words, changes evident in the life histories of objects in the archaeological record are tied inextricably to changes in behaviors — *activities* in the sense of Schiffer (1976) — involved in the manufacture and use of the objects.

This paper is not intended to serve as a thorough review of the literature on either Darwinian evolution or behavioral and experimental archaeology. Instead, we discuss several aspects of the archaeological record that can be used to examine one of evolution's processes — selection — relative to prehistoric pottery-using groups residing in and adjacent to the central Mississippi River valley. Central to discussion are changes in ceramic-vessel manufacture and use and how the changes affected the evolution of those groups. Our choice of regions upon which to focus is a product of several factors, not the least of which is the growing number of studies from the Mississippi Valley that have examined various aspects of prehistoric ceramic technology and use. Earlier work by Braun (1983, 1985b, 1987), together with later work by Schiffer and Skibo (1987; also Skibo *et al.,* 1989) and Feathers

(1988, 1989a, b, 1990a, b; also Features and Scott, 1989; Dunnell and Feathers, 1991), has created sets of data appropriate for the task. This does not imply that all analyses were conducted within an evolutionary-archaeology framework, but the results are directly applicable. Although Schiffer and Skibo, for example, clearly are behavioral archaeologists, they are as interested in pottery-performance criteria as is any selectionist. Their analysis of fiber-tempered pottery is as important for understanding the selective forces driving early pottery making and use in the midwestern United States as is the analysis by Feathers (a selectionist) of later shell-tempered pottery.

BACKGROUND

Archaeology, as Dunnell (1980, 1982, 1987, 1989) and others (e.g., Rindos (1989) have pointed out, has had no shortage of evolutionary schemes, most based on orthogenetic principles, especially that of directed variation. Evolution in archaeology often is viewed as a series of transformations of one kind, or type, of thing into another, similar to one pervasive biological view of species origin (e.g., Kitcher, 1984a, b; Schwartz, 1981). Under this essentialist (Popper, 1950), or typological (Mayr, 1963, 1976), framework, entities are assumed to exist as bounded phenomena, and group cohesiveness is based on shared characteristics (that together form an "essence") of the objects under investigation. In other words, sets of objects (kinds) are viewed as discrete entities. The task of the analyst is to segregate observed variation into "significant and nonsignificant kinds in order to extract the essential (hence essentialism) nature of kinds from observed variation, usually by the pursuit of central tendencies" (Dunnell, 1988, p. 16). Single sets of entities are viewed as real, and relations between units within a set can be formulated without reference to space or time. Change is seen strictly as transformation (Hull, 1965), often *reversible* transformation. Forms simply are replaced by or transformed into other forms. At its very core, "essentialism is a doctrine about causal mechanisms" (Sober, 1984, p. 165).

But the subject of evolution is change, not transformation, and as Mayr (1982, p. 401) points out, "It is quite impossible to develop an evolutionary theory on the basis of essentialism." For the Darwinian model of evolution to work, change must be rendered as the inevitable outcome of a selective, or culling, process, not as the result of some transformational process that turns A into B. In other words, the causal mechanism for change is external — not internal — to the things that are evolving. Explanations of change are constructed around how and why processes external to the things that are evolving cause those things to evolve. Thus "cause is embedded in the theoretical system; it is not attributed to the phenomena being studied" (Dunnell, 1987, p. 444). The materialist, or population-thinking (Mayr, 1977; see also Dobzhansky, 1951), perspective views relations between and among phenomena as being time- and space-bound. "Kinds" are not empirical, though at any given moment in time and space kinds can be created based on observation (Dunnell, 1988). As long as analytical boundaries are held constant, variation between and among objects is rendered as change, as opposed to the essentialist view that variation is an "annoying distraction" (Lewontin, 1974, p. 5).[5]

Clearly archaeology, since its inception, has had a transformational perspective: "Because essentialism characterizes not only the predictive, ahistorical sciences but also the structure of common sense, it creeps into archaeological writing almost invisibly" (Dunnell, 1987, p. 444). Many of the

[5]The distinction between essentialism and materialism is anything but trivial. In biology the issue revolves around whether species should be thought of as individuals, a position advocated strongly by Mayr (1963, 1987), Ghiselin (1981), Hull (1976, 1978, 1980), and Sober (1980, 1984), or as natural kinds, a position supported by Schwartz (1981) and Kitcher (1984a, b), among others, (Kitcher actually uses the term *set* to refer to species, which, if we read him correctly, needs no defining properties. His appears to be a minority view even among essentialists.) Although the issue of species as individual is important, the very counterintuitiveness of that concept detracts from the real issue, which is whether species have essences. If species, i.e., natural kinds, have essences—properties possessed by all members of the species—then those essences logically cannot be things the species lacked initially (Sober, 1984, p. 165). It such were the case, then the original members would have been members of a different species. This is the logical inconsistency to the view that species are natural kinds. If the position *is* internally illogical, then does the species-as-individual paradigm imply that members of a species do not share properties in common? Is each individual within a species so unique that the concept of species is destroyed [see Schwartz (1981) for a defense of the essentialist position on these grounds]? This is totally unreasonable and is a result of confusing *essences* with *properties in common* (Mayr, 1987, p. 155): "To be sure every essence is characterized by properties in common, but a group sharing properties in common does not need to have an essence. The outstanding characteristic of an essence is its unchanging permanence. By contrast, properties in common of a biological group may be variable and have the propensity for evolutionary change. What is typical for a taxon may change through evolution at any time and then no longer be typical."

The critical relevance of this argument to archaeology follows directly from the view that behaviors and products of behavior are phenotypic. If objects in the archaeological record are phenotypic, i.e., are parts of past phenotypes, then how can they have an essence? The answer is that they cannot. They must be viewed instead as constantly changing entities that are as subject to evolutionary processes as is any somatic feature. Does this mean that archaeological *kinds* cannot be created? No. If such were the case, then, as Dunnell (1988) points out, we would reduce analysis to a purely metaphysical level. We *can* create kinds, but it cannot be overemphasized that these are strictly units of analysis. The kinds of units needed for an evolutionary archaeology are formed by the intersection of nonoverlapping attribute states of dimensions (Dunnell 1971) selected solely on the basis of analytical need. Objects in a class, by definition, have properties in common, but they do not share an essence. In fact, individual objects within a class can and probably do share little else in common. For example, we might be interested only in location and type of wear on stone tools, regardless of type of material on which the wear is located. Classes defined as such are timeless and spaceless entities; as such, they cannot "evolve." When one attribute state within an analytical dimension is replaced by another, the newly created entity is in a separate class. Considerable confusion will be alleviated when the distinctions between essence and properties in common and between units of selection and analytical units are kept separate.

things archaeologists routinely do are based on the premise that there is an essence to the objects examined. Ceramic types, for example, are formulated on the belief that enough distinguishing characteristics exist among sherds or vessels that the characteristics can be used to construct units that encompass individual objects that are alike enough to qualify for inclusion. In common archaeological practice, archetypes are identified, and all objects are matched against the essence of the archetype. The types are not viewed as changeable entities but rather as *replaceable* entities. On the other hand, an evolutionary archaeology focuses not on replacement of one type by another but on changes in frequency of individual attribute states within analytical dimensions. Changes in frequency distributions create the composite life histories of classes of phenomena in the archaeological record.

In biology we might, at a general level, speak of brown-eyed individuals "replacing" blue-eyed individuals over successive generations, but what is of analytical interest are (a) the changing frequencies of eye-color alleles at given points in time and (b) the reasons for the changes. Likewise, in archaeology we might speak in shorthand notation of shell-tempered pots replacing sand-tempered pots, but what we are interested in from an evolutionary perspective is the changing frequencies of tempering agents (analogous to alleles) through time. This interest leads to the posing of different kinds of questions, which require different kinds of information, from those posed under a perspective based on transformation.

Adaptation and Adaptedness

It is impossible to advocate a Darwinian evolutionary archaeology without a detailed understanding of two terms, *adaptation* and *adaptedness.* Put more directly, in our view evolutionary archaeology as a theory must hinge on the two concepts. We discuss them in considerable detail elsewhere (O'Brien and Holland, 1992), in terms of both their development as biological concepts and their logical extension to archaeology. Our point in emphasizing them here is that conflation of the terms and incorrect or vague usage have led in archaeology to an inability to distinguish among three categories of materials and the behaviors behind manufacture and use of those materials: (1) objects and behaviors that increase adaptedness but that are not under selective control, (2) objects and behaviors that increase adaptedness and that *are* under selective control (true adaptations), and (3) objects and behaviors that are not under selective control and that do not increase adaptedness (O'Brien and Holland, 1992). Without a means to distinguish among the three categories, we are left with a never-ending series of stories concocted to explain the appearance of traits and behaviors and how they relate to increased adaptedness. It makes little sense to build inferentially based stories relative to changes in human adaptedness if the traits used to build the scenario never affected adaptedness. Likewise, it makes little sense to construct adaptationist arguments centered on changes in technology and function if the traits used to build the arguments do not exhibit the requisite kinds of histories to be adaptations. Given that analysis of function is one cornerstone of evolutionary archaeology (see Dunnell, 1978), paralleling its importance to Darwinian theory generally, it seems appropriate to raise the issue of function here.

Behaviors and their products are adaptations if (a) they increase the relative adaptedness (fitness) of the possessors and (b) they are under selective control. Materials/behaviors under selective control exhibit temporal-frequency curves that are radically different from curves of materials/behaviors not under selective control (Dunnell, 1978; O'Brien and Holland, 1990, 1992). If we plot the frequency distribution through time of an adaptation, we expect that in general the curve will shift upward suddenly, followed by constant deceleration as the feature becomes predominant in a population. After some period the curve suddenly will plunge toward the *X* axis (O'Brien and Holland, 1990, Fig. 1). What this tracks, presumably, is the "adaptive advantage" conferred on organisms that posses a trait — first the proliferation of the trait among succeeding generations, then a leveling-off as the trait reaches fixation in a population, followed by a rapid decline as the trait is replaced. This curve is decidedly different from the stochastic patterns created by traits not under selection (Dunnell, 1978; O'Brien and Holland, 1990, 1992). The plotting of life-history curves, however, in and of itself tells us nothing about why an adaptation arose: "By themselves, adaptive scenarios are merely models of functional relationships, in which adaptive needs are adduced to fit the details of an activity like pottery-making. Even if pottery-making or another activity meets some adaptive need, this begs the question of how the adaptive fit of potters to particular conditions originated" (Neff, 1992, p. 170). Later we discuss changes in subsistence behaviors of Woodland groups in the central Mississippi River valley, which were contemporary with dramatic changes in ceramic vessel manufacture. We do not, however, state that one *caused* the other. Rather, as emphasized by Braun (1987) and O'Brien (1987; see also Charles, 1992a), the trends are viewed as coevolutionary developments. Cause in this case, as in any analysis based on evolutionary theory, is viewed as external to the system — not as some vague process but rather in terms of selection working on variation. What we want to know more about is the nature of that selective process and how it shaped phenotypic behaviors of prehistoric midwestern groups.

One approach to using archaeological remains to examine the evolution of prehistoric groups involves two steps. First, engineering-design studies of objects in the archaeological record are undertaken to understand how things were put together and how their construction changed through time. Second, the objects are assessed in terms of how well they performed certain functions for which they were intended (recognizing that functions change through time). Understanding changes in function is integral to understanding changes in design. It makes little sense, for example, to talk about changes in temper if pots perform equally well for the same function regardless of what kinds of nonplastic inclusions are added to a paste. However, if performance parameters changed and certain kinds of pots no longer were suited to the new parameters, then temper *might* matter.

In short, knowledge of the selective environment is critical — here the nature of the selective regime that surrounded the making and using of pots: What conditioned the size of a pot or the thickness of its walls? What processes shaped the decisions with which prehistoric potters wrestled as they attempted to manufacture pots that met certain performance requirements? We believe that careful engineering-design analysis is the first step in answering the "Why?-type" questions. A similar position is taken by biologists (e.g., Gould, 1977; Lewontin, 1978; Mayr, 1988) and philosophers of biology (Sober, 1984) concerning biological features. The dorsal plates of a stegosaurus and a ceramic cooking vessel, for example, are both features that potentially affected the adaptedness of their bearers. If the features came under selective control, i.e., became adaptations, then we can infer logically that they allowed the organism to solve some environmental problem — not in any directed sense; i.e., they were not evolved to solve the problem but, rather, in terms of increasing the relative adaptedness of organisms (see below) that possessed the features (O'Brien and Holland, 1992).[6] But here we run into a second potential problem that, while related to the problem of causality, is a separate issue. The problem is that of assigning function.

Gould and Lewontin's (1979) article, "The Spandrels of San Marco and the Panglossian Paradigm: A Critique of the Adaptationist Programme," spawned a host of criticisms in biology directed at efforts to understand both the development and the function of biological features thought to be adaptations. Gould and Lewontin raise the point that many attempts to understand biological adaptations result in adaptive constructs

[6]Phenotypic features that serve one purpose can also be coopted for other purposes later in a lineage's phylogenetic history (see Fisher, 1985; Gould and Vrba, 1982). Also, phenotypic features can serve more than one purpose.

that, while plausible, are untestable. In other words, researchers create "just-so" stories that accommodate available data. They contend that organisms can be studied only as integrated wholes and that phylogenetic history, pathway development, and engineering constraints must be dealt with during analysis. We agree emphatically, but we also agree with Mayr (1988, pp. 152–153) that nothing is inherently wrong with a "try-and-try-again" approach to understanding the design and function of features, as long as care is exercised (a) in deciding that a trait is an adaptation, i.e., that it is functional, and (b) in concluding what the function of the feature is. The investigator may not always arrive at the correct solution — indeed biology is replete with examples of later analysis proving earlier analysis incorrect — but it is not necessarily the result of what Gould and Lewontin term a "reductionist" scope.

Gould and Lewontin's (1979) point regarding the need to link engineering studies to studies of phylogenetic histories is well taken and entirely appropriate to the study of archaeological remains. Engineering studies of prehistoric tools in and of themselves are interesting, as is the analysis of the dorsal plates of the stegosaurus, but unless such studies are keyed to a developmental analysis, they are merely quantified stories. Without a detailed knowledge of the *developmental* history of a feature, it is nearly impossible, in many cases, to figure out its *functional* history. For example, who would doubt that the tough plates along the dorsal crest of a stegosaurus gave the giant creature added protection from even larger predators? But did the plates evolve because they gave the animal added protection, or did they evolve because they conferred some other advantage (such as heat regulation), in the process contributing to the ability of the organism to ward off predators? Importantly, what was the evolutionary cost of evolving the plates (see Neff, 1993)?

When we speak of adaptedness we mean *potential,* or *expected,* adaptedness, as opposed to *realized* adaptedness (O'Brien and Holland, 1992). In other words, we might examine the adaptedness of humans in terms of their *potential* to reproduce successfully relative to conspecifics rather than in terms of whether they actually *did* outreproduce their conspecifics. The difference between these is anything but subtle, and our argument rests on the ability to distinguish between the two. In short, we argue that what we should be interested in from an evolutionary perspective is potential adaptedness, as opposed to *realized adaptedness.* As Burian (1983, p. 299) points out, realized fitness is not what Darwin had in mind: "Darwin almost certainly meant the phrase 'survival of the fittest' to stand for the *tendency* of organisms that are better engineered to be reproductively successful" (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, p. 11). Williams (1966, p. 159) sums up our position neatly: "Measuring reproductive success focuses attention on the rather trivial problem of the degree to which an organism actually achieves reproductive survival. The central biological problem is not survival as such, but *design for survival*" (italics added).

Another distinction must be made, that between adaptedness of humans and adaptedness [*replicative success* as used by Dunnell (1988) and Leonard and Jones (1987)] of objects in the archaeological record. In and of themselves, measurements of replicative success tell us nothing more than that a particular class of objects was replicated more frequently than was another class. The link between the realized replicative success of objects and the potential success of individuals or groups making and using the objects is provided through the concept of the "extended phenotype," a term borrowed from Dawkins (1990).

Extending the Phenotype

Certainly, consideration of behaviors and behavioral products as phenotypic components is common in biology (e.g., Bonner, 1980, 1988; Dawkins, 1990), but the proposal has rarely been applied to humans. Reasons for this lack of application are unclear, though we suspect that they are tied to the erroneous belief that somehow humans are not subject to selection because our ability to reason and to formulate goals — human intention — has severed the connection and made us immune to evolutionary processes. Therefore, material remains are really nothing more than intentional products constructed solely to adjust humans to environmental stresses. As long as humans are viewed as being above much of nature's reach and as long as human intentions are substituted for theory, we are doomed to examining trivial questions. We do not doubt that intent produces outcomes, but unlike the road to hell, the evolutionary pathway is not paved with good intentions. Intent is like any other generator of variation; it produces the variants upon which selection can act — no more, no less. The important point is, as Neff (1992, p. 146) points out, "that problem solving and inventiveness should be viewed as part of the mechanism by which variation, the raw material for selection, arises."

We see no *a priori* reason why human skeletal remains, which archaeologists would agree are phenotypic features, should be slotted into an analytical framework different from the one applied to grave goods interred with a body. The human body throughout its phylogenetic history has been shaped by selection and thus is an adaptive response. The difference between the body and associated grave goods lies solely in the degree to which the phenotypic expressions — the body and pots (in terms of pot-manufacturing and pot-using behaviors) — are encoded within the genotype. Chromosomes carry instructions for making a human body, a portion of which, the brain, has been under continuous selective pressure for millions of years. Brains impart to humans the behavioral ability to create objects, some of which serve to further the protection of the vehicle carrying the hard-wired recipe for reproducing itself and some of which do not. All that matters is that variation is present — generated by whatever means — and that some of the variants are selected more often than others.

No one has ever found a gene or series of genes that control(s) how a beaver builds a dam or how a spider spins a web. But never having found such genes does not destroy the logical proposal that such genes exist, given what we know about behavioral genetics. If one accepts the notion that individual organisms act as vehicles for replicative units — genes — then the vehicles, to a certain degree, must do their job of protecting the germ-line replicators. Nature has shaped an almost-infinite number of vehicles, some of which are better than others in a relative sense and some of which are more or less equal in terms of getting the job accomplished. The problem, apparently, arises in deciding what constitutes the vehicle that carries the germ line. Our point is that if certain genes control the formation of "bodily" portions of the phenotype, and those portions protect the germ-line replicators, then the genes that control nest building can be considered as producing further protection. The logic is identical. Dawkins (1990, p. 198) makes the same argument:

> The house of a caddis is strictly not a part of its cellular body, but it does fit snugly round the body. If the body is regarded as a gene vehicle, or survival machine, it is easy to see the stone house as a kind of extra protective wall, in a functional sense the outer portion of the vehicle. It just happens to be made of stone rather than chitin. Now consider a spider sitting at the centre of her web. If she is regarded as a gene vehicle, her web is not a part of that vehicle in quite the same obvious sense as a caddis house, since when she turns round the web does not turn with her. But the distinction is clearly a frivolous one. In a very real sense her web is a temporary functional extension of her body, a huge extension of the effective catchment area of her predatory organs.

Unless one wishes to maintain a sacrosanct category for human artifacts, what, logically, is the difference between a mud-dauber's nest and daub from a Mississippian house? Biologists would have no difficulty in dealing with the dauber's nest within the framework of the extended phenotype, yet most archaeologists would not view houses similarly. If, as Dawkins (1990) argues, the step from a genetic basis for morphological development to a genetic basis for behavior is conceptually negligible (and we agree it is), then the step from behavior to extended phenotype — here mud-daubers' nests and Mississippian houses or caddis stone houses and spider webs — also is negligible. In no sense does this argument negate the importance of the social and physical environment in helping to shape the response.

One important point worth noting is that behavior must have a physical consequence if it is to enter the selective process. Birds, for example, did not evolve because of egg-laying behavior but rather because they produced a physical consequence — an egg — that could either break too soon, resulting in the death of the embryo, or break when the fledgling was ready to go out on its own. Consider the plight of the bald eagle in the 1950s and 1960s. DDT in the food chain resulted in fragile eggs that all too often broke before hatching. The birds faced extinction not because of their egg-laying behavior but because of selection against thin-walled eggs. Their eating behaviors, to be sure, led to the ingestion of DDT, but it was the physical consequence of the behavior upon which selection acted.

For this very important reason we state that *both* behaviors and the products of behavior, whether those products be pots or eggs, are important in an evolutionary sense. In a real sense, they are inseparable. Some behaviors lead to creation of objects, possession of which could affect the success of the possessor. If those behaviors vary and are transmitted, by whatever means, then they are ripe for selection. This does not imply that selection *will* take place but rather that it *can* take place.

SELECTION AND THE MANUFACTURE AND USE OF CERAMIC VESSELS

From an archaeological viewpoint, ceramic vessels represent an excellent set of materials to use in examining the role of selection relative to adaptation and adaptedness. With respect to pottery makers,

> Selection takes place in part because pottery-making aspects of potters' phenotypes enjoy different degrees of success, and relative success depends in part on environmental conditions. Potters interact with the environment throughout the production process: materials that will make an adequate clay body must be chosen by the potter; he/she must employ forming and firing practices that consistently produce whole pots; the pots must fulfill the cooking, carrying, storage, communication, ceremonial, or other needs of the potter or other individuals for whom they are intended; and, if pots are to be exchanged for other goods, they must bring an adequate return. (Neff, 1992, p. 169)

We examine below some of the behaviors and behavioral products of pottery-making midwestern groups residing in and adjacent to the Mississippi River Valley. Again, our interest focuses on one aspect of the human phenotype — the manufacture and use of ceramic containers — with an emphasis on monitoring changes in technological and use-related dimensions through time. Importantly, changes in the design and manufacture of vessels are viewed in terms of performance characteristics, which are then examined in light of proposed related changes evident in the archaeological record.

Our position is that these changes directly affected the adaptedness of the groups, but our real interest is in determining *how* they affected adaptedness. The data needed to address that issue exist in various forms, scattered throughout the archaeological literature.

We limit our discussion almost exclusively to one class of ceramic container, the cooking pot. We suggest that a detailed analysis of change in how cooking vessels were produced and used—phenotypic behaviors—will yield a better understanding of the selective environments faced by riverine midwestern groups. Our position is identical to that of Braun (1983, p. 107): "Many of the attributes of pottery . . .are, in fact, evidence of the techniques used by potters to achieve particular characteristics of utility in the finished vessels. When examined in an appropriate theoretical framework, these attributes inform us about variation in vessel use, providing complementary evidence of variation, for example, in practices of food preparation and storage and other aspects of prehistoric behavior." Our views on the subject of selection as it relates to the evolution of cooking vessels also parallel those of Neff (1992, p. 173): "The ceramic attributes that are the most likely targets of selection are those that affect a pot's ability to perform the basic cooking, carrying, and storage needs of the social unit within which it is made and used. Selective pressures on pottery-making may stem from changes in the uses to which vessels are put or from key technological innovations that alter the relative efficiencies of a whole complex of ceramic technological practices."

Unfortunately, there are, as Braun (1983, pp. 107–108) points out, "two all-too-common conditions in archaeology: (1) a lack of integration of analytical methods with interpretive theory, and (2) a sophistication with measurement that runs ahead of our sense of what the measured variation means." In effect, these two points, especially the second, summarize the tenets of this paper—shortcomings that have impaired attempts to develop an evolutionary archaeology. We cannot overemphasize the point that evolutionary archaeology does not consist simply of measuring variation. Infinite variation exists in the archaeological record, and it is fairly straightforward to measure much of it. We might suppose that selection has worked on a considerable amount of that variation, but until we examine the frequency of occurrence of certain traits through time and compute the variance, we have no a priori knowledge of which traits were under selective control and which were not. However, even after demonstrating that a particular trait was being selected for (actually, which traits were being selected *against*), we have not begun to address the question of what the measured variation means.

Temporal and Cultural Setting

The time span under consideration — subdivided for convenience into the Late Archaic (ca. 3000–1000 B.C.), Early Woodland (ca. 1000–200 B.C.), Middle Woodland (200 B.C.–A.D. 400), Late Woodland (A.D. 400–900), and Mississippian (post-A.D. 900) periods — witnessed wholesale changes in the subsistence, settlement, and sociopolitical practices of midwestern groups living in and adjacent to the central Mississippi River valley. Topical treatments of the region tend to emphasize two cultural developments — Hopewell and Mississippian — as hallmarks of prehistory (e.g., Hall, 1980), based in large part on the presence of (a) exotic goods in association with mound interments (Hopewell); (b) large, mounded, fortified communities and a shift to maize agriculture (Mississippian); and (c) marked social differentiation (Hopewell and Mississippian). The presence of these features has played a significant role in the imposition of a transformational, cultural-evolution-based approach to analyzing the archaeological record. Direct correlations between "Hopewellian" polities and Big-Man societies have appeared in print (e.g., papers in Smith, 1990). Important considerations under these perspectives include identification and analysis of mechanisms that transformed one type of sociopolitical organization into another. Despite this emphasis on transformation, archaeological work in and around the Mississippi Valley has produced a broad database that now contains enough redundancy that we can outline in considerable detail several key aspects of prehistoric life.

Much of what is known about Woodland groups is derived from several decades of survey and excavation in the lower Illinois River valley (Fig. 1) and tributary valleys. During the period 600 B.C.–A.D. 750, the Illinois River valley apparently witnessed increased population influx [and perhaps changes in the fertility rate (Buikstra *et al.*, 1986)] and increased sedentary settlement (Struever, 1968) as groups became "tethered" to highly productive plant and animal habitats (O'Brien, 1987; Styles, 1981). The archaeobotanical record for the lower Illinois and central Mississippi valleys shows a change in procurement and eating behaviors ca. 200 B.C., with a significant rise in the use of native starchy and oily annuals such as sumpweed, chenopodium, and little barley (Asch and Asch, 1985; Asch *et al.*, 1979; Braun, 1987; O'Brien, 1987; Pulliam, 1987). Similar trends in plant use have been found in southern Illinois (Hargrave, 1981; Hargrave and Braun, 1981) and eastern Missouri (O'Brien, 1987; Pulliam, 1987); the zooarchaeological record for those areas shows a concomitant increased localization in fish procurement (O'Brien, 1987; Styles, 1981). Two additional trends occurred during the period: trafficking in exotic goods and considerable variation in treatment of the dead (Charles, 1985, 1992b). How widespread these trends were in areas adjacent to western Illinois is unclear.

Data for the post-A.D. 750 period come primarily from several regions south of the confluence of the Mississippi and Illinois rivers, most notably the American Bottom of Illinois and the Eastern Lowlands of southeastern Missouri and northeastern Arkansas. The American Bottom was dominated after A.D. 900 by Cahokia (for review see Milner, 1990), though numerous contemporary communities also existed in the region and outlying areas (for summaries see Emerson and Lewis, 1991; Kelly, 1990). Detailed excavations of sites in the path of I-270 (for summary see Bareis and Porter, 1984) have led to an unparalleled view of the life histories of Mississippian communities in the Midwest. By A.D. 950 many large communities apparently of up to several hundred persons, along with dozens of smaller settlements, were evident in the American Bottom. The ubiquity of corn at these Mississippian sites is in contrast to its absence in Late Woodland sites in the region. In southeastern Missouri and northeastern Arkansas, archaeological surveys (e.g., Phillips, 1970; Phillips *et al.,* 1951) and excavations (see summaries by Morse and Morse, 1983, 1990) in the Eastern Lowlands, especially on the Malden Plain (e.g., Dunnell and Feathers, 1991; Morse and Morse, 1980, 1990), have recorded a complex development of communities.

Design and Performance Analysis of Midwestern Pottery

Despite these broad views of ceramic-period lifeways, it is difficult both to understand the changing nature of the selective environment (both social and physical) and to track the rapid changes that potentially affected adaptedness. For example, if, as Braun (1985a, p. 131) notes, areas such as the lower Illinois River valley were witnessing increasing population density during the Middle Woodland period, did it lead to changes in settlement and subsistence behaviors and in subsistence-related technologies?

Ideally, examination of selective pressures relative to changing technological and functional characteristics of prehistoric ceramic vessels, especially the resistance of vessels to mechanical and thermal stresses that occur through use, would employ the actual archaeological materials themselves. But prehistoric pottery, almost without exception, is a low-fired, usually porous ware that no longer retains its original properties. Although sherds can serve as accurate measures of some dimensions such as vessel-wall thickness and vessel size, and tempering agents can be identified, postdepositional processes (see Schiffer, 1983, 1987) such as leaching of temper particles by groundwater and rehydration of clay minerals have led to compositional change and therefore a reduction in strength.

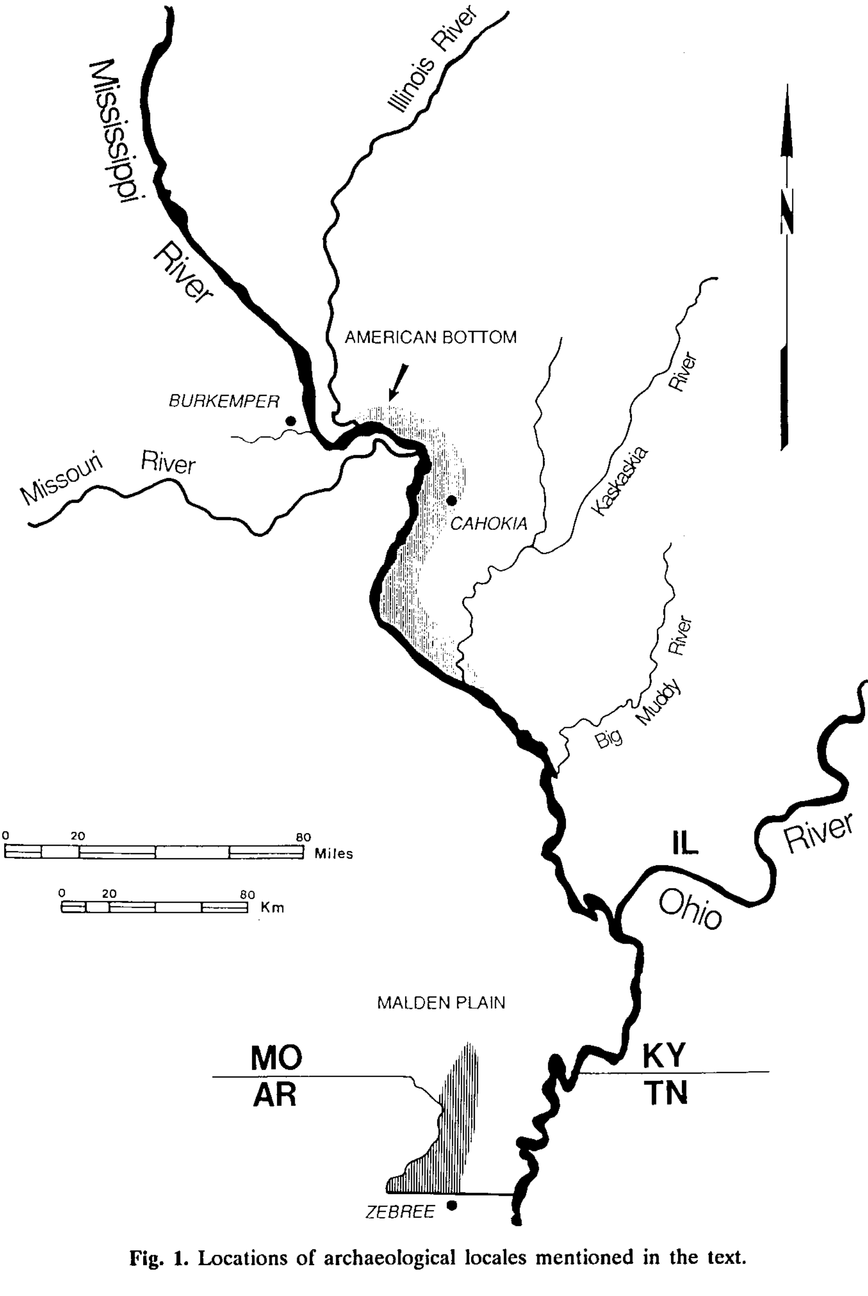

Fig. 1. Locations of archaeological locales mentioned in the text.

Thus strength measurements conducted on sherds or pots from archaeological contexts are residual measurements (Braun, 1983, p. 114; Mabry *et al.*, 1988, p. 838; Steponaitis, 1983), and not necessarily (probably are not) accurate measures of original properties. To escape this dilemma, much of the experimental work discussed below was carried out using modern ceramic replicas manufactured to resemble in several important respects prehistoric materials.

Schiffer and Skibo (1987; see also Neff, 1992) present a useful framework for examining the nature of technological knowledge through the use of experimental data, and they identify three key components associated with producing any goods, here restricted to pottery. First, potters start with raw materials and an acquired, basic knowledge of those materials and their performance characteristics. Schiffer and Skibo make the important observation *that there is no way of knowing precisely what this knowledge might have been* — nor do we need to — and that even among potters studied ethnographically, there is little reason to assume that practitioners of a craft can articulate the rules that underlie their production-related behaviors. Schiffer and Skibo construct "recipes for action" (see Krause, 1985, pp. 29–31), which are summaries of knowledge that "if possessed by the artisan (or artisans), would account for the technological behavior . . . *recipes for action are models built by the investigator* on the basis of visible behavior or its archaeological traces" (Schiffer and Skibo, 1987, p. 597; original italics). Second, a formula is passed intra- or intergenerationally by teaching and encompasses imitation, verbal instruction, demonstration, and trial and error (see Boyd and Richerson, 1985; Neff, 1992). Third, there exist what Schiffer and Skibo term "techno-scientific" principles that underlie a technology's operation, though producers may be partially or completely unfamiliar with such principles:

> The techno-science content of a technology is revealed by applying the principles of modern science to its processes and products; these principles must be frequently augmented . . .by new experiments carried out by archaeologists under controlled conditions.
>
> . . .We want to stress that an understanding of the techno-science content of a technology is a prerequisite for explaining technological variability and change. In order to illuminate the techno-science embedded in any technology, archaeologists must develop their general understandings — modern science — to a high level of sophistication . . .Without a substantial foundation of modern science for identifying techno-science, archaeologists can scarcely hope to explain technological variability and change. (Schiffer and Skibo (1987, p. 598)

We agree completely with their statement that "although formal properties are the archaeologist's window into processes of technological change, one must operate analytically *at the level of performance characteristics*" (Schiffer and Skibo, 1987, p. 600; italics added). Failure to distinguish between the technological properties of an object and how well it performs, which is an indicator of the potential replicative success of a class of objects, has been a weakness of evolutionary archaeology. Schiffer and Skibo's work on technology and performance has direct application to midwestern archaeology since they link their general discussion to a performance analysis of two of the earliest kinds of wares found in the Midwest: fiber-tempered pottery and mineral-tempered pottery.

Late Archaic and Early Woodland Pottery

Fiber-tempered wares represent the earliest vessel pottery in the United States, dating to the third millennium B.C. in the Southeast. Vessels often were crudely made and of variable quality. Early fiber-tempered wares are widespread over portions of the Southeast, but in the Midwest their occurrence is restricted to the Late Archaic Nebo Hill site near Kansas City (Reid, 1984a b), with dates that fall in the 2600–1500 B.C. range (Reid, 1984a, p. 59). Nebo Hill sherds are soft (<2.5 on the Mohs' scale); pastes contain 5–15% silt-sized particles and no sand. A mixture of two types of temper was used: (a) crushed potsherds or fired clay, with particle sizes up to 5 mm, and (b) organic material, including switchgrass, big bluestem, and sedge (Reid, 1984a, p. 61). Sherd thickness ranges from about 5 to slightly over 12 mm (Reid, 1984a, p. 59); sherds are too small to infer vessel form.

To our knowledge no pottery dating post-Nebo Hill and pre-ca. 600 B.C. has been documented firmly in the Midwest. By ca. 500 B.C., however, mineral-tempered wares were becoming common over large portions of the midlatitude United States east of the Mississippi River. Localized type designations — Marion thick in the upper and central Mississippi Valley, Fayette thick in Ohio and Kentucky, Halfmoon cordmarked in West Virginia and Pennsylvania, and Vinette I in western New York — belie similarities in ceramic technologies and shapes. Farnsworth and Asch's (1986, p. 352) descriptio of Marion thick (defined by Helmen, 1951; see also Begg and Riley, 1990; Garland, 1986) could be used to describe related materials: "coarse, thick-walled, usually flat-bottomed 'tub,' 'barrel,' or 'flower-pot' shaped vessels." Temper varies regionally; Marion thick usually contains large angular to subangular pieces of crushed igneous (and sometimes metamorphic) rock and, in at least one instance, igneous rock and fired clay (Begg and Riley 1990).

Morgan *et al.* (1986, p. 212) describe the 5000-plus Marion thick sherds from the Mississippi-flood-plain Ambrose Flick site (western Illinois) as follows: "Sherds are on the order of 9–18 mm in thickness with a cord-marked or fabric-impressed interior . . .The dominant temper is a coarse, dark igneous or mafic material. Temper particles, which may be as large as 5 mm in diameter,[7] are typically an abundant inclusion. Commonly the paste is soft, and in many instances surfaces are highly eroded, with temper protruding through the surface." This description is typical of contemporary materials from other sites in the Midwest (e.g., Begg and Riley, 1990; Harn, 1986).

Beginning around 400 B.C. and in some areas of the Midwest overlapping with Marion thick, a new, thinner, hard-paste ware — commonly referred to as Dane incised and Prairie incised in Wisconsin and Black Sand incised in Illinois and Missouri [Farnsworth and Asch (1986) use the term Liverpool series] — became predominant in portions of the central and upper Mississippi Vally.[8] Incised-over-cordmarked vessels were tempered with large amounts of sand and/or finely crushed igneous rock, often to the point where sherds have an extremely sandy feel. In some areas, other minerals occur frequently in pastes [for example, in the lower Illinois River valley, chert was a significant addition (Farnsworth and Asch, 1986, p. 370)].

In terms of the behaviors associated with early fiber- and mineral-tempered ceramic products, Schiffer and Skibo (1987, p. 602) observe that comparison of "design priorities for Archaic and Woodland Pottery furnishes a basis for exploring changes in the societal contexts of these ceramics that might have promoted the technological change [from Archaic to Woodland pottery]." As a basis for comparison they examined three sets of 8-cm-square briquettes (one set untempered, one set tempered with fiber, and one set mineral-tempered) relative to six performance characteristics: ease of manufacture, heating effectiveness, portability, impact resistance, thermal-shock resistance, and abrasion resistance. Based on experimental evidence — some their own, some produced previously — Schiffer and Skibo (1987, p. 607; see also Skibo *et al.*, 1989) propose that "Archaic technology placed a high priority on ease of manufacture and portability, whereas Woodland — especially Late Woodland — technology stressed heating effectiveness and characteristics that promote longer uselives (e.g., impact resistance, thermal shock resistance, and abrasion resistance)."

[7]Some particles in sherds from eastern Missouri and western Illinois actually range as high as 10 mm in diameter.

[8]This general statement obscures the important point that at least in the American Bottom (Illinois) region of the central Mississippi River flood plain, crushed sherds (grog), not minerals, were the primary tempering material (Emerson, 1986; Emerson and Fortier, 1986). This is discussed later in more detail.

In a behavioral sense, the ability of Woodland potters to control various shape-related dimensions of their vessels was conditioned in part by their knowledge of tempering materials. As Schiffer and Skibo (1987) show, mineral-tempered vessels exhibit certain performance characteristics not exhibited by untempered or organic-tempered vessels. The literature (e.g., Bronitsky, 1986; Bronitsky and Hamer, 1986; Feathers, 1989a; Lawrence, 1972; Rado, 1969; Rye, 1976; Shepard, 1968) well documents several advantages served by temper, such as for controlling paste plasticity and binding the paste. Temper, under certain conditions, can also disperse mechanical- or heat-related stresses that otherwise would extend a preexisting crack and result in vessel failure (Bronitsky, 1986).

Resistance to crack propagation increases with an increase in temper size (Kingery *et al.*, 1976; Rado, 1969). As Braun (1983, p. 123) and others (e.g., Lawrence, 1972; Shepard, 1968) have noted, shape, density, and angularity also contribute to resistance. Despite these advantages, tempers with expansion coefficients that differ greatly from the coefficients of the clay paste in which they are embedded can, at least in theory, cause cracking and spalling during firing or later cooking (Rye, 1976). Quartz, for example, undergoes three changes (inversions) in atomic structure and bonding, one of which occurs at about 573°C, where quartz grains expand 2% in volume and 1.03% in length (Rice, 1987, p. 95). However, the widespread use of quartz sands in midwestern ceramic vessels, which were fired at temperatures greater than 573°C, suggests that the problem was not severe.

Open pore spaces, such as those that occur when organic material is burned out of a clay body, are another retardant to crack propagation: "High porosity (at least 10% surficial porosity) is a desirable property in low-fired earthenware cooking pots because the larger pores function to arrest the propagation of cracks that are generated by the differential expansion of heated exterior and interior wall surfaces" (Reid, 1984, p. 63). Thus, Reid (1984, p. 63) continues:

> These considerations suggest that the simplest strategy for producing an earthenware cooking vessel is to use a temper combination composed of pulverized sherd particles to improve paste workability, and chopped or crushed biosiliceous plant fibers to both improve the tensile strength of the malleable paste, and to produce large, crack-arresting pores in the fired fabric . . . .
>
> A similar tempering compromise is seen in the Nebo Hill sherd fabrics. The grog particles would thicken the homogeneous clay paste, and the grass and sedge fibers would provide tensile strength to the unfired body, preventing the walls from sagging.

Reid might be correct in asserting that the above recipe is the "simplest strategy for producing an earthenware cooking vessel," but it appears that the resulting product did not have wide appeal. As we noted, no other midwestern pottery contemporary with materials from Nebo Hill has been found, and a large temporal gap exists between its production and the earliest occurrence of mineral-tempered wares in the region. Thus, unless Reid (1984a) is correct in assuming that mechanical forces in the depositional environment have destroyed most traces of this early fiber-tempered ware, it hardly qualifies as an adaptation.

On the other hand, the widespread occurrence after 600 B.C. of thick, mineral-tempered vessels is more characteristic of an adaptation (though widespread occurrence by itself is not a sufficient condition for something to be an adaptation). The large size of the temper particles and the thickness of the vessels lead us to suspect that strength and the ability to arrest cracks during both initial firing and subsequent firings were two important considerations. The extreme thickness of the vessels, especially of the bases and walls, precluded the ability to keep a vessel from developing literally hundreds or thousands of tiny cracks, which are plainly evident on archaeological specimens. Therefore, we suggest that there was a great need to increase the resistance to crack propagation. The technology was adequate to produce a serviceable vessel for use over fires,[9] but design constraints kept the vessel large (not to mention relatively simple in form). Cracks inevitably were going to occur, but the technology was adequate to keep the cracks from destroying the vessel during manufacture and use. By ca. 400 B.C. in much of the central Mississippi River valley vessels were on average several millimeters thinner, temper size declined dramatically, and vessels were more conical (see articles in Farnsworth and Asch 1986). Importantly, mineral temper exhibited a long history of use, as discussed below, as opposed to being a historical anomaly.

Middle Woodland and Late Woodland Pottery

Braun's (1983, 1985b, 1987; Hargrave and Braun, 1981) work focused on ceramic materials — primarily cooking vessels from the Middle Woodland (ca. 200 B.C.–A.D. 400) and Late Woodland (ca. A.D. 400–750) periods — from 64 contexts in three river valleys (the Illinois, the Kaskaskia, and the Big Muddy) of western Illinois (Fig. 1).[10] The thrust of his research was twofold, though the two points are tightly interconnected: By tracking minute changes in technological dimensions, he simultaneously was constructing

[9]Some Marion thick sherds from lower halves of vessels from Ambrose Flick (eastern Illinois) and similar sherds in the University of Missouri — Columbia collections exhibit extensive sooting on the exteriors. One sherd from the Burkemper site in eastern Missouri exhibits carbonized residue on the interior surface.

[10]Braun (1985b) discusses 56 contexts; 8 additional samples were included later (Braun, 1987).

a device that potentially would be of use in chronologically ordering the materials under consideration (for analytical methods see Braun, 1985b). Given the shifts observed in at least one dimension —vessel-wall thickness — that potential was realized.

In brief, the trend documented by Braun for the period ca. 200 B.C.–A.D. 750 shows a short-lived increase in wall thickness followed by a long-lived decrease. Mean wall thickness ca. 200 B.C. was approximately 7.7 mm, which increased to a maximum of approximately 8.2 mm by the birth of Christ (Fig. 2). After approximately A.D. 50, wall thickness declined monotonically at a rapid rate until ca. A.D. 300 (mean wall thickness of approximately 6.1 mm), at which point it decreased at a slower rate until ca. A.D. 550 (mean wall thickness of approximately 5.8 mm), when the rate of decrease again accelerated. By A.D. 750, mean wall thickness was approximately 5.3 mm.

Application of Braun's methods to samples from contemporary contexts on the western margin of the central Mississippi valley in eastern Missouri yielded a general trend almost identical to that constructed for Illinois (Fig. 2). A sample of 174 rim and body sherds from eight apparently quickly filled pits uncovered at the Burkemper site (Fig. 1) (O'Brien 1987)[11] were used with radiocarbon assays to construct the curve. Although the general trends between the two curves are almost identical, there is a striking difference: Mean thicknesses of sherds from seven of the eight pits at Burkemper are thicker than mean thicknesses of contemporary sherds from Illinois. The maximum difference — approximately 3 mm — occurred ca. A.D. 60; the mean thickness values were identical ca. A.D. 750. In general, the amount of divergence between the two curves increased throughout the Middle Woodland period, then dropped significantly at the end of that period (ca. A.D. 400), picked up and then held constant during the early Late Woodland period (ca. A.D. 400–650), then decreased to zero ca. A.D. 750. We examine possible reasons for the differences between the Burkemper data and Braun's data later; the important point here is that the shapes of the curves are almost identical.

How thick to make the walls of a pot is of basic concern to any potter interested in performance characteristics of the finished vessel, since that consideration affects not only the cost of manufacture but also at least "three aspects of mechanical performance: thermal conductivity, flexural strength (breakage load), and resistance to thermal shock" (Braun, 1983, p. 118). Thermal conductivity obviously is correlated negatively with increased wall thickness. Thermal shock — the ability of a vessel to withstand

[11]Ten pits actually were dated, though the problematic nature of the dates received from two features led to their being dropped from this discussion.

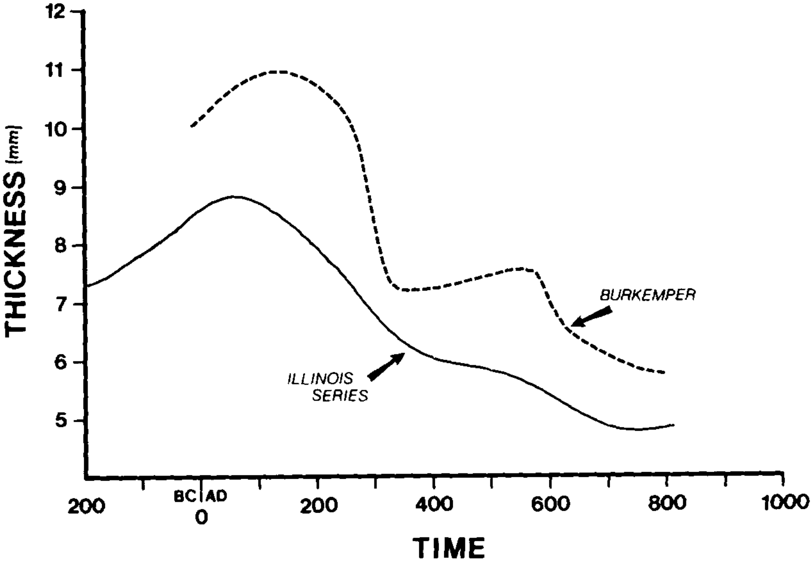

Fig. 2. Curves of mean vessel-wall thickness based on sherds from western Illinois (after Braun, 1987, Fig. 7-2) and from Burkemper (eastern Missouri).

sudden and extreme changes in temperature — also is correlated negatively, i.e., everything else being equal, the thicker the walls, the more poorly a vessel withstands thermal stress. However, flexural strength — the ability of a vessel to withstand mechanical stresses without distorting or breaking — correlates positively with increased thickness, given that variables such as temper type and size are held constant. Therefore, Woodland potters moved (intentionally or not) toward producing vessels that had a higher thermal conductivity and thermal-stress resistance — a move accompanied by a technology that allowed for production of an increasingly thin-walled vessel that would not collapse during manufacture and firing.

Vessels constructed during the 200 years just before the Christian era contrast with those constructed after ca. A.D. 50. The earlier vessels, as noted above, were constructed with increasingly thicker walls (Braun, 1987, p. 165), as Middle Woodland potters increased the general size of their wares. Later the positive correlation between increased vessel size and increased wall thickness disappeared. This trend is most evident in ceramic materials from Burkemper. Burkemper vessels manufactured between ca. A.D. 1 and A.D. 250 tended to average between 9.5 and 11 mm in thickness

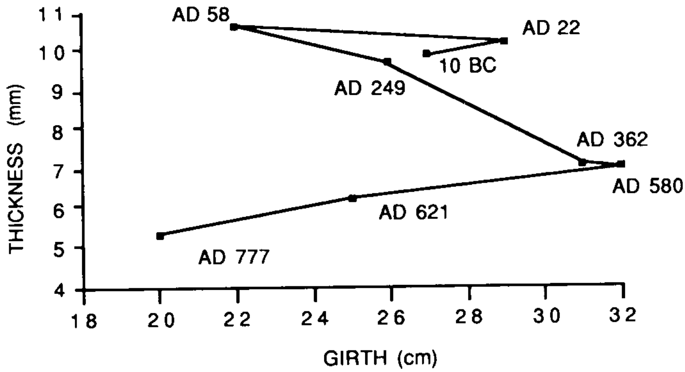

Fig. 3. Plot of average sherd thickness (mm) versus average maximum girth (cm) for materials from eight radiocarbon-dated contexts at Burkemper (eastern Missouri).

(Fig. 3); average maximum vessel diameters ranged from 22 to 29 cm (Fig. 3). After ca. A.D. 300, however, and lasting until ca. A.D. 600, the trend was toward production of thin-walled (average, 7-mm) jars with average maximum diameters of 32 cm. After A.D. 600, vessels tended to become smaller and more thin-walled.

Braun (1982, 1983) found that in western and west–central Illinois the trend in tempering characteristics through time included a reduction both in the density of particle sizes greater than 1 mm in average diameter and in the average particle size: "The tendencies suggest both an increasing use of temper to manipulate flexural strength, and an increasing concern for stresses resulting from differential thermal behavior of the temper. Viewed alongside the changes in wall-sectional shape, then, the changes in tempering characteristics also indicate an engineering trend of increasing attention to the accommodation of use-related thermal stresses" (Braun 1983, pp. 124–125). The relation among temper type, temper-particle size, and thermal stress is anything but straightforward. Temper may add strength to a clay body (both prior to and after firing), but as discussed previously, different types of temper exhibit different expansion coefficients when heated. Despite experiments by Bronitsky and Hamer (1986), who suggest that (under certain firing conditions) vessels tempered with smaller (≤0.5-mm) quartz grains are better able to resist thermal stress than are vessels tempered with large (≤1.0-mm) grains, it is difficult to believe, as mentioned above, that a maximum 2% expansion of quartz particles could have caused a significant number of failures in Woodland vessels. Rather, as Bronitsky and Hamer (1986) also found, a smaller temper size leads to a reduction in failure from *mechanical* stresses. We propose, but cannot yet demonstrate, that the reduction in the size of quartz grains used as temper was not related to a concern for thermal stress but rather to a concern for mechanical stress.

Based on particle-size analysis (by use of x-rays) of grit-tempered (predominantly angular pieces of quartz and feldspar) sherds from Burkemper, grit temper started out small around the birth of Christ and stayed small throughout the Middle Woodland and Late Woodland periods (Hoard, 1992). Examination of 39 sherds (admittedly a small sample but one over which there exists tight temporal control) showed that only 20% of observed particles were over 0.785 mm^2 (equivalent to the area of a rounded particle 1 mm in diameter). Particle-size variances for individual sherds were remarkably low, with an average per-sample variance of 0.384. Burkemper potters apparently quickly learned how to make thinner-walled vessels (but thicker than those made by contemporary potters in western Illinois) without giving up much in the way of flexural strength.

A shift to limestone as a temper is correlated with but does not necessarily explain the substantial changes in diameter and thickness shown in Fig. 3. Limestone as a temper has two advantages: Its chemical composition ($CaCO_3$) greatly increases the workability of clay over that gained through using other tempers [see Stimmell *et al.* (1982, p 220) for data on montmorillonitic clays], and its expansion rate is similar to that of most clays. One potential disadvantage is that, given certain firing conditions (discussed in detail later), calcium-carbonate-tempered vessels fail because of lime spalling. Regardless, ca. A.D. 350 the use of limestone tempering increased in Burkemper vessels, becoming the dominant temper type at least by ca. A.D. 750 (Fig. 4). In sherds from a prehistoric pit with a mean radiocarbon date of A.D. 777, temper particles are extremely small — well under 0.5 mm — and sherd thickness is between 5 and 6 mm. We were interested in two aspects of limestone-tempered pottery: strength of the finished product and how the temper reacts during firing.

Strength was examined through a three-point static bend test, similar to that employed by Feathers (1989b). Although impact testing more closely simulates the kinds of stress to which a pot is subjected, bend testing was chosen because it allows more accurate comparison of resistance to initial fracture and final failure. This is an important distinction: Different types of temper may be more efficient in preventing the spread of microcracks, thus preventing or at least delaying final failure of a vessel. Three kinds of temper were examined: crushed granite, crushed fired clay (grog), and crushed limestone. Five test bars of each temper type were loaded

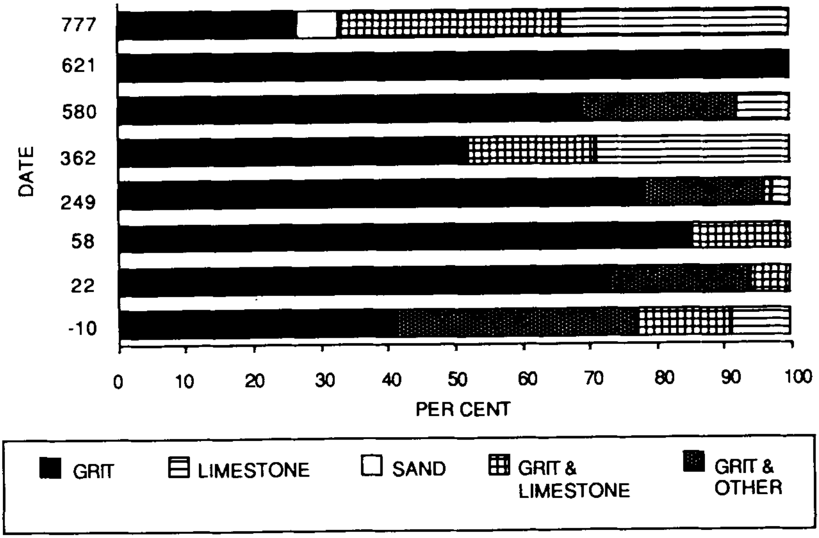

Fig. 4. Percentages of temper type in sherds from eight radiocarbon-dated contexts at Burkemper (eastern Missouri) (data from Hoard, 1992).

sequentially on a MTS testing machine. A head span of 95.25 mm and a crosshead speed of 0.1 mm/sec were used. The resulting curves (samples of which are shown in Fig. 5) document that the limestone-tempered tiles were substantially stronger than were either the grit- or grog-tempered bars, requiring more pressure to cause initial fracture. However, both grog- and grit-tempered bars were more elastic (able to withstand stress longer before initial fracture, as shown by the gentle slope before the peak). Finally, bars tempered with coarse limestone particles showed greater strength than did those tempered with finer particles, probably a result of the larger particles acting to arrest cracks development (Kingery *et al.*, 1976).

It is well documented in both the ceramics and the archaeological literature that lime spalling occurs as an end process of heating calcite ($CaCO_3$), which is converted to calcium oxide (CaO) and carbon dioxide (CO_2). The gas dissipates and, upon cooling, the CaO (which is hygroscopic) hydrates to form calcium hydroxide $Ca(OH)_2$. As the volume of the inclusion expands, stresses cause popping and cracking. The degree of damage can be controlled by reducing the size of the lime particles (Laird and Worcester, 1956), by wetting the fired (and still hot) items with cold water (Klempter and Johnson, 1986), or by adding salt to the paste prior

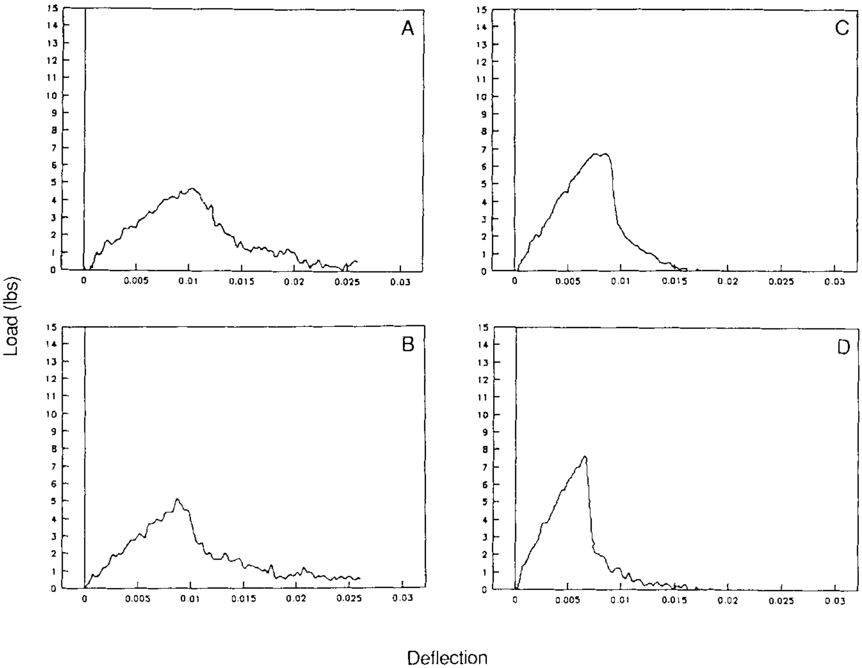

Fig. 5. Graphs of load (lb) versus deflection (inches) for test bars tempered with (A) grit, (B) grog, (C) coarse limestone, and (D) fine limestone (test data and notes on laboratory protocol on file, Museum of Anthropology, University of Missouri — Columbia). Compare with Fig. 6, which shows results from similar tests of sand- and shell-tempered bars undertaken by Feathers (1989a).

to firing (Klempter and Johnson, 1986; Laird and Worcester, 1956). Conventional wisdom states that in oxidizing atmospheres the critical temperature above which lime spalling occurs is about 870°C (Rice, 1987, p. 98). However, as Rice (1987, p. 98) notes, "The exact temperature is a matter of disagreement: some researchers say it may occur at 850–900°C while others contend it may take place at as low as 650–750°C. That the argument exists highlights how time *and* atmosphere act in addition to temperature in governing firing behavior" (italics added). Rice (1987, p. 98) states that "alternatively, the clay may be fired in a reducing atmosphere, or oxidation fired to a temperature either below 700°C or above 1000°C. At temperatures over 1000°C rehydration does not occur, since at those temperatures the calcium in most clays becomes a part of the liquid phase with sintering and vitrification" (or it becomes part of high-temperature calcium silicates such as gehlenite). While we do not doubt that reducing atmospheres may, under certain conditions, allow calcium-tempered vessels to be fired at higher temperatures than will oxidizing atmospheres, especially if the reducing atmosphere has an abundance of CO_2, the *rate* at which the temperature is raised within the environment probably is critical, as is the soak time.

Although considerably more experimental work needs to be done to understand the various technologies of limestone-tempered-pottery manufacture in the Midwest, our admittedly preliminary experimental data indicate that limestone-tempered pottery is stronger than grit- or grog-tempered pottery *when fired at 600°C.* Several questions remain unanswered. Is the increased strength because of chemical properties, physical properties, or both? Do limestone particles bond more firmly to the clay matrix? Would fine grit or grog particles affect the clay body in the same way as fine limestone particles do? In other words, are the differences in strength the result only of textural differences? Regarding firing temperature, it now *appears* to be the case — and we emphasize the word *appears* — that prehistoric potters either (a) fired their limestone-tempered pots at temperatures under 600°C (which we seriously doubt); (b) used higher temperatures but fired their wares in reducing atmospheres with a faster heat-up period (slower rates allow more time for carbonate decomposition to occur), a longer soaking period, or a longer cooling period than we used; (c) used salt in the paste and fired at temperatures higher than 600–700°C [though we emphasize that salt has *never* been found in any midwestern carbonate-paste ceramic (see Stimmell *et al.,* 1982)]; (d) immersed vessels in cold water immediately after heating; or (e) used a combination of methods.

Mississippian Pottery

Feathers' (1988, 1989a, b, 1990a, b; Dunnell and Feathers, 1991; Feathers and Scott, 1989; cf. Bronitsky and Hamer, 1986) work on shell-tempered pottery from flood-plain contexts in southeastern Missouri extends our understanding of ceramic manufacture and use in the Mississippi Valley. Feathers (1989, pp. 78–79) asks

> whether shell tempering was a technique already known and therefore part of the pool of available variability at the time it began to predominate in the manufacturing process or whether its rapid spread is due to diffusion of a technique that once available quickly outcompeted alternatives.

> While this question is largely historical in nature, the two alternatives require different functional arguments. In the first case, shell tempering was already known but was not selected earlier because the advantages it bestows did not outweigh the disadvantages, because of the way pottery was used or made (e.g., poor control over firing). In the second case there is no question the advantages outweighed the disadvantages. The question is simply one of availability. Once introduced it spread rapidly.

Archaeological work in the eastern Ozarks of southeastern Missouri has produced shell-tempered materials that have been dated directly through thermoluminescence or indirectly through radiocarbon assays at about A.D. 600 (Lynott, 1986; Lynott and Price, 1989; Price, 1986). Price and Lynott view the region as the heartland of shell-tempered ceramics, with adjacent regions receiving the requisite technological knowledge at later dates through diffusion. As Feathers (1989b, p. 179) notes, too few dates exist in other regions to test the implications of this scenario. The archaeological literature (e.g., Morse and Morse, 1983) also contains scenarios in which Mississippian peoples (the bearers of shell-tempered-pottery) moved into a region and replaced sand/grog-tempered-pottery users.

Reliance on a typological approach, as Feathers (1989b, p. 79) points out, has shifted attention away from the life histories of various technologies such as shell-tempered-pottery manufacture. The archaeological literature for the most part is straightforward: Shell tempering signifies the end of the Late Woodland period and the beginning of the Mississippian period. In other words, shell-tempered pots replaced mineral- and/or clay-tempered pots. And nowhere in the literature is there more than passing mention that a ceramic vessel might be tempered with more than one material. But several studies (e.g., Feathers, 1988, 1989b; Philips *et al.*, 1951; Teltser, 1988) have shown that the shift from sand (or grog) to shell initially was not one of complete replacement. Where extensive archaeological work has produced large ceramic assemblages, such as the American Bottom and the Cairo Lowland of southeastern Missouri, clearly (a) sand- and/or grog-tempered pots were still being manufactured several hundred years after the advent of shell-tempered pottery and (b) combinations of temper can occur in the same vessel. Feathers (1990a), on the other hand, reports very few sherds from the Malden Plain of southeastern Missouri and northeastern Arkansas tempered with both sand and shell. Studies emphasizing dimensions of vessel form and paste characteristics (e.g., Dunnell and Feathers, 1991) suggest that, at least on the Malden Plain, indigenous Late Woodland peoples were responsible for both traditions. There, the

> earliest shell-tempered pottery is quite distinctive from the sand-tempered pottery it replaced. The shell temper is abundant (up to 40%) and coarse (ranging up to 2 mm in diameter). A red slip was often applied to one or both surfaces which were first smoothed by scraping. The pottery is quite porous (about 45% apparent porosity) and soft (between 2.0 and 2.5 on the Mohs' scale). In contrast, the sand-tempered ware is rarely slipped, the exteriors are roughened with a cord-wrapped paddle, and the pottery is less porous (about 23%) and harder (2.5 to 3.0 on the Mohs scale). The sand is rounded quartz, ranging from 20% to 40% of the body with only occasional grains larger than 1 mm in diameter. (Feathers and Scott, 1989, p. 554)

What, if any, selective advantage did shell-tempered vessels exhibit? For example, were they stronger than pots made with other tempers, and if so, how? At face value, Mississippian shell-tempered pottery appears inferior to its local antecedent on the Malden Plain — the hard, sandy-paste Barnes type. In terms of raw strength, Feathers (1989a) found that test bars tempered with 25% fine sand (95% of the particles ≤0.5 mm) were stronger on average than those tempered with 25% fine shell (80% ≤0.5 mm) or 25% coarse shell (50% ≤0.5 mm, 30% ≤1.0 mm but ≥0.5 mm, 20% 1.0–2.0 mm). Test bars tempered with 45% coarse or fine sand performed decidedly poorer than did other test bars, probably as a function of weak bonds between the numerous quartz particles and the clay matrix.

Feathers (1989a) also measured "toughness," which tracks the time between crack initiation and failure. Figure 6, which shows average values of the test specimens discussed above, illustrates not only that it took longer for shell-tempered specimens to break, but also that there was considerable time lag in the shell-tempered specimens between crack initiation and failure. Figure 6 also shows differences among shell-tempered specimens by fine versus coarse grain and by percentage of temper. Coarse-tempered specimens with 45% shell temper outperformed all other classes. Performance tests conducted by Feathers and Scott (1989) yielded quantitative differences between shell- and sand-tempered specimens. They found that coarse shell-tempered pieces were 61% tougher than those tempered with sand and that there was a 135% increase in work of fracture. Furthermore, shell replicates did not break catastrophically, in contrast to sand-tempered replicates. Apparently the large platelike structures created by crushed shell are better deterrents to crack propagation than are sand particles:

> The higher strength and toughness of the shell-tempered samples can be explained by the nature of the calcite grains. When viewed under high magnification (160×), the particles appear as bundles of longitudinal fibers. When these fibers are aligned parallel to the direction of stress, they increase strength because of the greater force required to break them as compared to the force required to break through the clay matrix . . .Because of the difficulty in propagating a crack across the fibers, the particles tend to be pulled out from the matrix rather than broken. This pulling action absorbs considerable energy. (Feathers, 1989a, p. 581)

Million (1975) argues that paste workability increases with the addition of burned shell, a phenomenon apparently caused by increased flocculation due to the presence of calcium cations (Feathers, 1988).[12] Given the added strength afforded by the inclusion of shell as temper, one might wonder why it was not used earlier, if indeed vessel strength was a favorable feature. It is hardly enlightening to say that Woodland potters in the central Mississippi Valley did not realize some advantages afforded by crushed shell. We thus are forced to seek other reasons for its late development. Mussel shell is a combination of organic material and a crystalline carbonate, aragonite, and has a hardness of 3.5–4.0 on the Mohs' scale. Burning the shell, which Mississippian potters apparently did (Feathers, 1989a, p. 581; Million, 1980), removes the organic material and converts the crystalline structure from aragonite to calcite (hardness of about 3.0). Without burning, not only is the shell harder to crush, but an increase in particle volume during the aragonite-to-calcite shift between 200 and 400°C can cause pots to fail during firing. Bronitsky and Hamer (1986) suggest that raw shell was burned at temperatures that exceed the critical point of carbonate decomposition (650°C or higher, depending on other factors), but if this were the case, we would not recognize the temper as shell, since the $CaCO_3$ conversion to calcium hydroxide would reduce the platelets to a fine powder. Thus, the presence of visible shell platelets in the paste of shell-tempered pottery indicates that the raw shell was burned at temperatures below the critical point and that shell-tempered pots were also fired at temperatures below the critical temperature or, as Feathers (1989a, p. 581) suggests, at higher temperatures but in a reducing atmosphere [and perhaps for shorter periods (Dunnell and Feathers, 1991, p. 31)].

Efforts currently are under way to examine an extensive sample of shell-tempered sherds from various localities in southeastern Missouri in terms of changing firing regimes. Cursory examination demonstrates considerable areal variation through time. In some areas, such as the Cairo Lowland, it appears that the majority of sherds containing fairly large, platey shell particles (the "coarse" shell-tempered ware known locally as Mississippi plain or Neeley's Ferry plain) is from vessels fired in oxidizing atmospheres; most "fine"-tempered sherds (Bell plain), which often contain almost microscopic-size shell particles and hard, compact pastes, are from vessels fired in a reducing atmosphere. This two-technology phenomenon at first glance makes sense to us in terms of intended vessel use. For special-purpose vessels such as funerary offerings, use fine shell and fire the

[12]Feathers (personal communication, 1992) stated that several geologists with whom he has spoken question whether calcite is soluble enough to cause flocculation. It could be particle shape that influences workability.

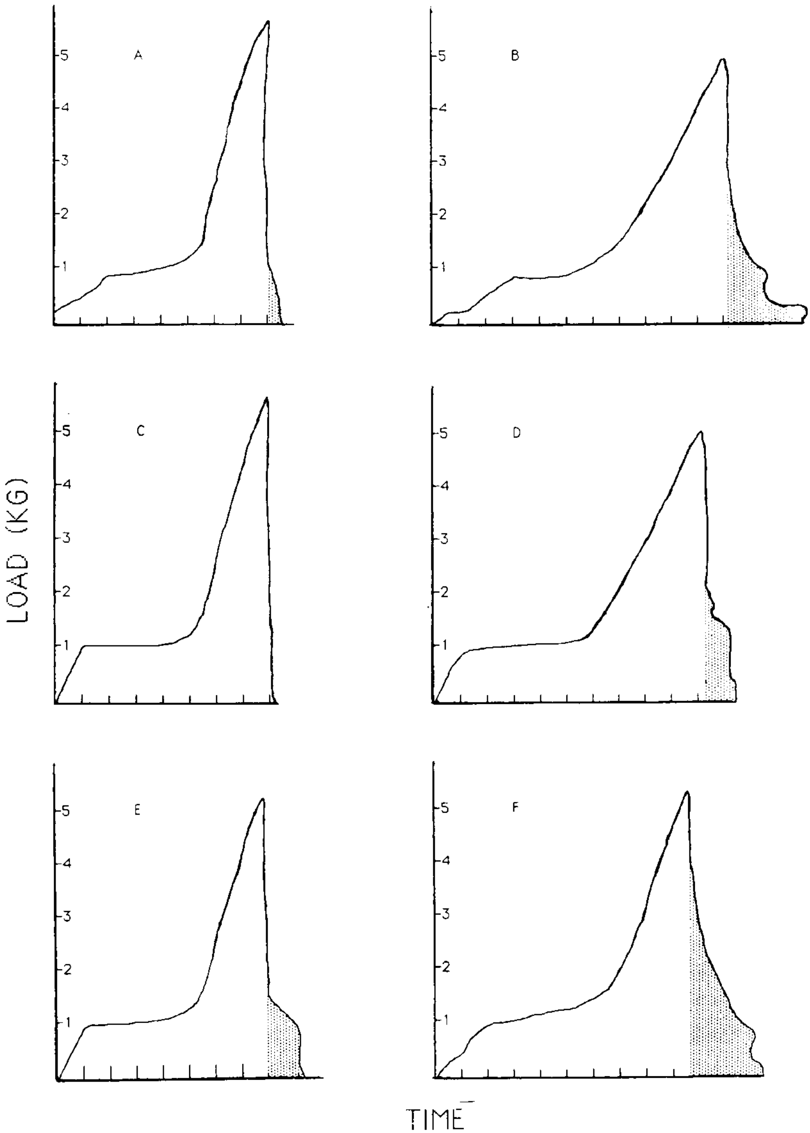

Fig. 6. Graphs of load versus time for test bars tempered with (A) sand (composite of multiple samples), (B) shell (composite), (C) 25% fine shell, (D) 25% coarse shell, (E) 45% fine shell, and (F) 45% coarse shell (C–F, single samples) (redrawn from Feathers, 1989a, Figs. 1 and 2).

vessel at high temperatures in a reducing atmosphere, thus increasing the hardness of the vessel (see Dunnell and Feathers, 1991, Table 3). For vessels that require increased strength, use large shell particles, fire the pot in an oxidizing atmosphere, and keep the fire below the point at which lime spalling will occur.

We need to insert a cautionary note lest we be interpreted as saying that no coarse-shell vessels were fired in reducing atmospheres. What we are saying is that sherds from such vessels are in a minority in sizable University of Missouri–Columbia collections from certain areas of the Eastern Lowlands of southeastern Missouri. There is, however, considerable regional variation. Dunnell and Feathers (personal communication, 1992; see also Feathers, 1990a, p. 11), for example, point out that considerable late-period reduced pottery from the Malden Plain contains coarse temper. But on the whole, we strongly suspect, but again cannot yet demonstrate, that it was easier to produce a harder-paste, reduced ware (fired at higher temperatures than coarse shell-tempered vessels) using extremely fine shell particles as tempering than it was to produce similar wares using large shell particles. We would expect more failures in coarse-shell vessels during firing, even in reducing atmospheres, because of the larger surface areas of individual particles — especially those at or near the vessel surfaces — despite the fact that for a given volume of temper, finer particles will have more surface area than will larger particles. Thus fine-shell vessels *might* make it through firings with fewer failures than would coarse-shell vessels.

Dunnell and Feathers (1991) point out two disadvantages of oxidized shell-tempered pottery: its softness and its porosity. Vessel failure because of abrasion might not have been an important consideration, but porosity could have affected potential vessel uses. Early oxidized shell-tempered pottery from the Malden Plain is twice as porous as local sand-tempered pottery (Dunnell and Feathers, 1991, p. 34). Not coincidentally, early shell-tempered vessels, not only those from the Malden Plain but also vessels from other regions in the central Mississippi Valley, often were slipped on the interior and exterior. Earlier, sand-tempered vessels also were occasionally slipped, but this probably had little to do with function. Slipping may be, as Dunnell and Feathers (1991, p. 34) note, an example of a trait that starts out "as a component of stylistic variation ('decoration') in [sand-tempered] Barnes ceramics, shifting to function (reduction of permeability) in the early shell-tempered ceramics in a new selective environment (shell tempering), and ending up once again as style in Middle and Late Mississippian when wholesale and unrelated changes in ceramic technology [including firing in reducing atmospheres] decrease porosity to earlier levels."

DISCUSSION

The data presented above are interesting in their own right as detailed analyses of technological change in one group of prehistoric materials — cooking vessels — but what do the results tell us about changes in behaviors of the groups responsible for making and using the pots? In other words, how did changes in technology translate into evolving behaviors related to how the pots were used? And, specifically, why did the technological and functional behaviors change? This question obviously cannot be answered through reference to the materials under analysis. Dunnell and Feathers (1991, pp. 30–31) make this same point with reference to their analysis of firing regimes: "If we are correct in our suppositions about the Barnes firing regime . . .then a change to better controlled, lower temperatures, and/or shorter firing time regime would make it possible for shell temper to replace sand. This does not explain why it happened or happened so rapidly. To do this requires identification of those selective factors responsible for the attribute association typical of Big Lake ceramics." Braun's (1983, 1985b, 1987) work was one of the first attempts to link detailed variation in cooking-vessel technology to other systemic features. Although his work was not carried out under an explicit evolutionary paradigm, it is clear that his interest included examination of the selective regime in which pots were produced and used. His data, coupled both with data from similar analyses and with contextual information, produce significant insights into the changing adaptedness of midwestern Woodland groups.

Recall that Braun's plot of vessel-wall thickness against time is a composite of 64 contexts — in essence an amalgam of many local technologies. Importantly, other assemblages have been examined in similar fashion to determine whether, on local levels, the trend toward decreasing wall thickness is evident. As discussed, the trend was documented in materials from contemporary ceramic assemblages from eastern Missouri (Burkemper), but it also occurs in the Saline Valley of southern Illinois. There, Hargrave (1981) found that although vessel-wall thickness decreased through time, vessels were on average 5 mm thicker than contemporary vessels from along the Big Muddy drainage approximately 60 km to the west (Fig. 1). Vessels from Burkemper were on average 2 mm thicker than the composite Braun curve. What we appear to be observing here are similar responses by Woodland groups to similar problems, but with slightly different results.

The precise nature of those problems is not completely understood, but it appears that the post-A.D. 50 trend in decreasing vessel-wall thickness was tied to evolving food-preparation systems: "The parallel [between decreasing wall thickness and increasing appearance of starchy and oily seeds in the archaeobotanical record] strongly suggests that these ceramic changes all reflect increasing attention to the cooking of seed broths in meal preparation, increasing nutrient extraction and possibly improving palatability" (Braun, 1987, p. 164). Localized differences in vessel manufacture probably covaried in relation to the demands placed on the vessels by the users. For example, Braun (1985, p. 527) notes that the mean wall-thickness difference between materials in his master sample and those from the Saline Valley parallels mean paleobotanical evidence for a much lower importance of edible starchy and oily seeds in the Saline Valley diet throughout the Woodland sequence (Lopinot, 1982, pp. 804–806). Evidence of the role played by vessels in cooking native-annual seeds is provided by hundreds of sherds from Burkemper, which exhibit thick (often up to 1-mm) carbonized residues on the interiors. In most cases where the sherds can be identified as to original position on a vessel, the coatings occur on the lower two-thirds of the vessel walls, especially on the lower half. Similar residues occur on Middle Woodland and Late Woodland sherds from western Illinois.

As neat as the vessel-wall thickness curves from western Illinois and eastern Missouri are, they oversimplify matters considerably. The general trends toward decreasing wall thickness obscure several underlying components that are not as obvious as the general trend — components that may signal the presence of selective pressures as yet unknown. One trend, the pre-Christian-era rise in wall thickness, is plainly evident. Braun (1987, p. 168) ties this trend to the rise in sedentary behavior indicated in other areas of the archaeological record (e.g., larger house-floor areas); we would also add the prospect that potters were concerned with constructing larger pots, which, given the state of their technological knowledge, required thicker walls. Braun (1987, p. 168) further speculates that two minor trends — one toward slightly thicker-walled pottery (A.D. 100–400) followed by a trend toward thinner-walled pottery (A.D. 400–750) — may have been connected to a climatic shift (the Scandic climatic episode) he believes is documented in the paleobotanical sequence of the region. However, conflicting opinions over the precise nature and geographic extent of this 400-year-long (ca. A.D. 270–690) climatic episode make it difficult to support Braun's suggestion of a shorter growing season leading to a change in crop mixes. An even less well-defined trend, toward thinner-walled vessels, began around A.D. 200 and ended around A.D. 400. Braun (1987, p. 169) initially thought the trend correlated directly with a decline in house-floor size — suggesting a reduction in average commensal group size — but the end of the trend preceded the house-floor area reduction by perhaps as much as 200 years.

We agree with Braun (1987, p. 170) that "it seems plausible that at least some of the changes in pottery technique associated with the Middle to Late Woodland transition were not simply adjustments among existing techniques, but true innovations." We view "true innovations" as being analogous to biological mutations (O'Brien and Holland, 1990). Analysis of Burkemper materials bears this out. The major shift in maximum vessel diameter versus wall thickness (Fig. 3) occurred between ca. A.D. 250 and A.D. 340, when the average maximum diameter increased substantially and the average vessel-wall thickness declined dramatically (by almost 3 mm). Braun (1983, 1987) probably is correct that the technology needed to produce larger, thinner-walled jars grew out of a specialized technology that developed ca. A.D. 200 and produced small, thin-walled (Hopewell) limestone-tempered jars and bowls that were used for non-food-related purposes. The coincidence in timing suggests this as an avenue for further research. At Burkemper, the frequency of limestone tempering in cooking vessels rose after A.D. 250, though it was not until the seventh or eighth century A.D. that limestone was used as frequently as grit [see Chapman (1980) for descriptions and dates of limestone-tempered types from Missouri].

What do these data tell us about the adaptedness of groups living in the riverine Midwest? In one interesting case, the trend toward thinner cooking-vessel walls and the concomitant rise in the use of native annuals as dietary staples led J. Buikstra and her associates to propose that this shift could have led directly to changes in fertility of Woodland women. Infants, for example, could have been weaned at an earlier age than they were previously and placed on a carbohydrate-rich diet. Therefore, decreased lactation in Woodland mothers, coupled with a corresponding resumption of ovulation, could have led to a rise in fertility — a rise that might eventually be evident in archaeologically derived population profiles. This speculation was clearly a result of using one trend evident in the archaeological record to posit, in logical fashion, the existence of another. As it turned out, Buikstra was correct (Buikstra *et al.*, 1986; see also Holland, 1989); evidence of such a change in fertility during the early Late Woodland period (ca. A.D. 400–750) was found in skeletal series from western Illinois.

Buikstra and co-workers' (1986) findings support the notion, in strictly biological terms, that at least some early Late Woodland groups were becoming better adapted (measured in terms of reproductive capability). Unfortunately, no other studies paralleling that of Buikstra *et al.* exist. However, as we discussed earlier, our primary focus is on potential fitness and understanding the role played by adaptations in producing that fitness. Approaching fitness from an engineering standpoint, Woodland peoples became more adept at producing vessels that would better withstand heating stresses while simultaneously allowing better heat conductivity. Archaeobotanical data and sherd residues indicate increased use of native annuals as dietary staples, which leads us to believe that Woodland groups were taking advantage of the innovative technology. The remains indicate increased fitness as midwestern riverine groups evolved a means of increasing their realized niche (Hutchinson, 1965), i.e., bringing under cultivation a broad spectrum of previously untapped, or at least underused, resources. If O'Brien (1987) and Braun (1987) are correct, the mutualism that developed from at first casual, then intensive, human and plant interactions in the Midwest resulted in competition among sedentary groups for resources and perhaps, as Braun and Plog (1982) suggest, mechanisms for exchanging information and reducing conflict.

Despite our heightened knowledge of prehistoric lifeways in the central Mississippi River valley after ca. 600 B.C., it currently is difficult to assess potential adaptedness of individuals or groups *relative* to one another. What are needed are detailed technological studies of ceramic materials from different midwestern drainages and an understanding of how those technologies were related to changing subsistence- and settlement-related behaviors. we also need a much better understanding of what selective pressures operated in those drainages through time. Early Woodland materials, for example, are critical for examining what archaeologists (e.g., Braun, 1985b, 1987; Farnsworth and Asch, 1986; O'Brien, 1987; Styles, 1981) see as the beginnings of sedentary life in the central Mississippi Valley and neighboring valleys. We suspect that across the region the period 600–200 B.C. witnessed the development of literally dozens of local ceramic industries. In several cases potters in widely separated communities produced goods that were strikingly uniform, leading to cultural-causation explanations centered around diffusion (e.g., Begg and Riley, 1990, p. 250). In other areas, goods were strikingly dissimilar, leading to explanations centered around group intrusion (e.g., Munson, 1982, 1986).

In the same vein, adjacent regions often produced contemporary yet strikingly dissimilar materials — such as the appearance of grog-tempered Early Woodland pottery in the American Bottom (Emerson *et al.*, 1983; Fortier, 1985; Fortier *et al.*, 1984), a locality that is surrounded by sites that have produced mineral-tempered Black Sand and Marion thick pottery. The American Bottom also witnessed another case of two contemporary but different technologies developing in close proximity — a phenomenon that spanned the period A.D. 750–1150 (data from Kelly, 1990). Prior to that period over 95% of Late Woodland vessels were tempered with grit or grog, and the vessel exteriors were cordmarked. After A.D. 750 limestone became the dominant temper in the southern half of the American Bottom; over 70% of all vessels manufactured between A.D. 750 and A.D. 1050 were limestone tempered. In the northern half of the American Bottom, plain-surface grog- and grit-tempered vessels compose 75–90% of assemblages that date to A.D. 800–1000. After ca. A.D. 1150 the vast majority of vessels from the American Bottom, regardless of location, was shell tempered.

What do these dichotomous distributions tell us? Does the existence of two distinct ceramic technologies side by side indicate the presence of two culturally distinct groups, each having found its own unique solution to common problems? As one reviewer pointed out rather emphatically, the question of group intrusion and/or geographic movement of technological knowledge will eventually have to be addressed. We agree, although without detailed knowledge of manufacturing technologies, it is difficult, (a) to quantify the variation among technologies and (b) to understand how that variation translated into vessel performance. Precise documentation of technological variation and the tracking of technological change within individual regions would allow us to begin to examine intergroup differences in adaptedness. Can we, for example, based on engineering-design studies, propose that users of certain kinds of pots were potentially more successful than were contemporary groups that did not have that technology? This type of information obviously must come not only from engineering studies per se but from engineering studies *and* from an understanding of function and associated behavioral implications — a point that certainly will not strike behavioral archaeologists as new. We offer the following example to show precisely what we mean.

It originally seemed odd to us that mineral-tempered wares were preferred in many areas of the central Mississippi Valley until the "advent" of shell-tempered pottery ca. A.D. 900. Our assumption, erroneous as it turns out, was that grog-tempered vessels, because of similar expansion coefficients between paste and temper, would have withstood stresses introduced by reheating much better than mineral-tempered wares would have. We were curious why, when Early Woodland groups in the American Bottom were producing grog-tempered vessels, the supposed superior technology did not spread and soon become fixed among all or most groups. Left out of the equation, however, was the matter of performance. Vaz Pinto *et al.* (1987), for example, clearly demonstrate the inferiority of grog-tempered briquettes to abrasion — an inferiority that, counterintuitively, rises with firing temperature. Did vessel failure through abrasion-causing activities lead to an early rejection of grog as a temper? On the other hand, when grog became a major temper in eastern and southeastern Missouri vessels during the Late Woodland period, was this trend accompanied by major technological change in vessel manufacture and/or by change in how the vessels were used? These questions presently are unanswerable because no one has examined either the technological histories of the vessels or their use histories. At this point it is debatable whether in Late Woodland contexts — many of which contain both sand-tempered as well as clay-tempered materials — a functional difference existed between vessels of different temper or whether it did not matter which temper was used, i.e., both performed equally well. If the latter case is true, then choice of temper was neutral; what perhaps was adaptive was that the vessels were tempered.

To our knowledge, little work has been done on interregional differences in the performance characteristics of cooking vessels or, except for pottery from western Illinois and east-central Missouri, on temporal changes in technology. Given what normatively are viewed as wholesale changes in social and political organization in the central Mississippi River valley after A. D. 900, for example, we cannot believe that vessel-production technology was static. Except for the work of Dunnell and Feathers (1991) on the shift from sand-tempered to shell-tempered vessels on the Malden Plain of southeastern Missouri and northeastern Arkansas, our knowledge of Late Woodland/Early Mississippian technologies in the central Mississippi Valley is based on cursory examinations of vessel form as part of efforts in type description. And yet the shift that occurred in many areas from grog- or grit/sand-tempered ceramics to shell-tempered ceramics must have had extremely important consequences in terms of potential adaptedness of prehistoric groups (Feathers, 1989b, 1990b) — consequences that as yet are poorly understood.

CONCLUSION

Our objective has been to show that Darwinian evolutionary theory, with its emphasis on the selection of variation, is entirely appropriate to the study of past human groups. The subject of evolution is change, which is rendered as the inevitable outcome of a selective process that works on variation among organisms. Humans are organisms — hence they evolve — and they evolve at rates that are almost unknown in the rest of the biological world. To deny that humans evolve is to hold either to a very narrow definition of evolution (for example, evolution has occurred only when gene frequencies have changed) or to the strange notion that humans somehow are immune to evolutionary processes such as selection. Our notion of evolution is the same as Darwin's — descent with modification — a concept that at once calls attention to the fact that variation exists and that it is heritable. However it is observed and measured, that variation must be cast in a materialist light, reflecting the fact that selection works on the tangible — things produced ultimately by genes but more proximally by behaviors (even intentional behaviors). Our view is that objects in the archaeological record were parts of past human phenotypes, as were the behaviors involved in producing and using the objects. As such, the objects as well as the behaviors were acted upon by selection. The goal of an evolutionary archaeology must be, as one reviewer put it, to explain how and why we got to be the way we are. We view this objective as being tied inextricably to the issues of adaptation and adaptedness. Did certain features — behaviors and associated objects — lead to increased adaptedness? Were they functional, i.e., did they come under selective control? and Do they exhibit the requisite histories to be labeled adaptations? We argue that analysis should begin at the point of design. Changes in design of objects in the archaeological record are both observable and measurable, and if enough materials are available from chronologically controlled contexts, life histories of kinds of objects can be constructed with a high degree of certainty. Behaviors, on the other hand, are not directly observable but rather must be inferred through design analysis (manufacturing behavior) and functional analysis (itself an inference based on such things as replication, wear studies, and contextual information).

Our emphasis on change in no way implies that archaeology has not always had, to one degree or another, change as a focus, but more often than not, change has been rendered as the transformation of one form into another or the replacement of one form by another. However, the kind of change in which we are interested is (a) a gradual (usually) shift in frequency of the components of prehistoric technologies and (b) concomitant changes in behaviors. Instead of talking about the "emergence" of shell-tempered pottery, for example, we place analytical interest, first, on obtaining a basic working knowledge of carbonate-tempered technologies; second, on creating catalogs of performance characteristics of the products of that technology; third, on analyzing the functions of those products; and finally, on connecting changes evident at the technological and functional levels to other pieces of the archaeological record.

The visible role of design and performance analysis, then, is to extend the archaeologist's ability to see and map variation beyond that which is readily apparent. Has this not always been the role of technological analysis in archaeology? Certainly, technological work has usually quantified variation, but when, for example, Schiffer and Skibo (1987) bounce ball bearings off ceramic briquettes, or when Bronitsky and Hamer (1986) plunge heated ceramic blocks into icy water, they are doing more than merely examining some variation in the reaction of a ceramic object to stress. They also are illuminating some of the myriad properties — often previously unknown properties — that might have been subject to selection. The hidden role of such research is not in answering existing questions but in helping to formulate new questions. And the more familiar archaeologists become with such analytical methods and techniques, the more questions will be generated.

At this point there appear to be many more questions than there are answers. Certainly we raise questions here that currently are unanswerable with the information available. This certainly is no weakness of evolutionary theory or of the type of direction for archaeology as laid out here. As we stated in the introductory remarks, one measure of scientific progress is the number of problems that are posed by new ways of asking questions. There is, as Sober (1991, p. 275) notes, a reciprocal aspect of scientific change: "The creation of problems — the discovery that some fact needs to be explained, where earlier it was accepted as obvious and commonsensical — is fundamental to the scientific enterprise. In fact, it is arguable that major scientific breakthroughs often involve the discovery of new problems, as well as the formulation of solutions. A new way of posing a question is often the prelude to a new answer." We do not pretend to be posing new avenues of research in archaeology; all we are suggesting is that the research questions be posed in terms of evolutionary theory.

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