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Ethnoarchaeology Needs a General Theory of Behavior

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Ethnoarchaeology is the study of relationships between human behavior and its material consequences in the present. Practitioners hope to establish consistent links between the two that can be used to interpret archaeological evidence of human behavior in the past. Much of this work is descriptive: analysts seldom attempt to explain variation in the behavior they observe, instead simply documenting its archaeological implications. This limits the utility of their results. At best, they can only identify the past distribution of ethnographically known behavior. Evidence of anything else is uninterpretable; the behavioral variability it reflects inexplicable. This problem can be resolved only by linking ethnoarchaeology with a general theory of behavior. Neo-Darwinian behavioral ecology may provide the necessary framework. Recent ethnoarchaeological work on site structure and faunal remains, especially as applied in research on the Paleolithic, illustrates both the problem and the appeal of the proposed solution.

KEY WORDS: ethnoarchaeology; behavioral ecology; site structure; faunal analysis.

INTRODUCTION

Archaeology's ultimate goal is to describe and explain variability in past human behavior. To this end, it routinely appeals to two sources of information: patterns in the form and distribution of objects made or modified by humans in the past and knowledge of human behavior and its material consequences in the present. The first provides direct evidence of past behavior; the second, a basis for interpreting that evidence.

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Ethnoarchaeology contributes to the development of that interpretive framework. Its results have led to important changes in archaeological practice and significant improvements in understandings of human history. But a close look at current work also reveals a fundamental problem. With some exceptions, ethnoarchaeologists are much more concerned with describing and explaining the material consequences of behavior than with understanding behavior itself. This means that while their observations can help track the past distribution of ethnographically known behavior, they generally cannot be used to interpret archaeological evidence of anything else. In other words, they cannot reconstruct past behavior unknown in the ethnographic present. Further, and for the same reason, they cannot help explain variability in past behavior, however reconstructed.

I argue that this problem can be resolved only by broadening the focus of ethnoarchaeology to include the study of behavior from a theoretical perspective capable of predicting and explaining its variability under the broadest possible range of circumstances. Behavioral ecology may provide that perspective. Here I develop this argument, first, by defining ethnoarchaeology, characterizing its strengths and weaknesses, and outlining the potential role of behavioral ecology in addressing the latter, and then, by reviewing recent work on site structure and faunal remains, two of the most important topics in ethnoarchaeology. I illustrate the difficulties encountered in applying the results of research on these topics to substantive archaeological problems, as well as the promise of behavioral ecology, with examples drawn mainly from the paleolithic. I chose this particular time frame not because the problems it presents are unique but because they are easily described and readily appreciated. I conclude with some comments on the differences between the approach I advocate and others recently proposed, on potential objections to its application, on problems likely to be encountered in pursuing it, and the means by which these might best be resolved.

ETHNOARCHAEOLOGY DEFINED AND CHARACTERIZED

Ethnoarchaeology is commonly defined as the study of relationships between human behavior and its archaeological consequences in the present. Its immediate goal is to identify and explain patterns in this relationship and the processes that determine them. Its ultimate objective is to apply this knowledge to the investigation and explanation of variation in past human behavior. In general, it seeks to expand the range of inferences drawn about past behavior from archaeological data and improve confidence in their accuracy. It differs from other so-called "actualistic" research (notably taphonomy and experimental archaeology) in that it typically involves participant observation among living people. It is distinct from most ethnography in its explicit concern with archaeological phenomena.

Ethnoarchaeology has a long history, in a sense beginning with the first use of ethnographic data to interpret archaeological remains (Grayson, 1986; Heizer, 1962). The term "ethnoarchaeology" itself first appears at the end of the 19th Century (Fewkes, 1900), coincident with the growth of interest in comprehensive, empirically based reconstructions of past behavior (for discussion see Trigger, 1989, pp. 270–275). Detailed ethnographic studies of material culture undertaken primarily for the purpose of interpreting archaeological phenomena appear sporadically from this time onward, forming an important part of certain local research traditions [e.g., Colton (1939), Cushing (1886), DiPeso (1950), Guthe (1925), Holmes (1886), and Kroeber (1916) on southwestern U.S. ceramics]. The late 1950s and 1960s witnessed a sharp increase in the volume and diversity of ethnoarchaeological research, largely as part of (but sometimes in opposition to) the New Archaeology (e.g., Ascher, 1962; Brain, 1967; Gould, 1968; Heider, 1967; Kleindienst and Watson, 1956; Longacre and Ayres, 1968; Thompson, 1958; Tindale, 1965; White, 1968). The subsequent appearance of several monographs and volumes of collected papers marked its emergence as a widely recognized subdiscipline within archaeology (e.g., Binford, 1978a; Gould, 1978, 1980; Hayden, 1979; Kramer, 1979; Watson, 1979; Wright, 1977; Yellen, 1977a).

In recent years, most ethnoarchaeological research has been conducted among subsistence hunters and farmers living in the Americas, Africa, and Oceania (mainly Australia). Common topics of inquiry include, but are not limited to, site structure and settlement patterns, ceramics, and faunal remains. As indicated above, attention is paid primarily to the relationship between behavior and its archaeological consequences, particularly in the form of objects produced, used, or otherwise modified by humans and their distribution at various spatial scales. Nearly all ethnoarchaeologists have been trained primarily as archaeologists; few have any formal background in ethnography. As a result, most approach the behavioral aspect of their research inductively, with less than precise ideas about the kinds of data that might be useful or how they might best be collected. Not surprisingly their observations vary greatly in scope and rigor, from anecdotal accounts of single events to systematic samples of a wide range of activities drawn over relatively long periods of time. Comprehensive research designs are comparatively uncommon, but where present typically reflect a well-developed sense of question, likely answers, and their potential significance.

THE PROBLEM WITH ETHNOARCHAEOLOGY

The results of most of this research are applied in one of two ways, either as conventional analogies or as cautionary tales. In drawing an analogy, the analyst notes similarities between material phenomena observed ethnographically and archaeologically and infers that behavior responsible for the former also produced the latter. Cautionary tales highlight ambiguities in this approach, usually by observing that different behaviors or other processes may have similar material consequences (cf. "equifinality"), less commonly by showing that the same behavior may have variable consequences depending on context.

Though essential to current practice, both applications have widely recognized limitations. Cautionary tales point to interpretive problems but usually provide no means of resolving them. Analogies help one stipulate the temporal and spatial limits of ethnographically recorded behavior but offer no guidance in interpreting, sometimes even identifying, archaeological evidence of anything else. Even where they accurately characterize past behavior, they cannot explain it.

These problems are most apparent in (but certainly not limited to) research on the early stages of human evolution. The questions here are fairly straightforward: When and where do the patterns of behavior we take as fundamentally human first emerge? What other patterns of behavior have hominids displayed in the past? Why do all these patterns take the forms they do when and where they do? The archaeological record provides evidence pertinent to the resolution of these questions; the trick lies in identifying and interpreting it. For more than a century, modern or historically known subsistence hunters have been seen as a source of potentially useful analogy in this regard, largely on grounds of certain similarities (real or assumed) in technology and subsistence. Many have been the subjects of recent ethnoarchaeological research for the same reason. Results of this work influence nearly every aspect of paleolithic archaeology, from basic data recovery through analysis and interpretation.

Despite its pervasiveness, appeal to modern analogy in this context is subject to recurrent criticism. The grounds for objection are consistent and not easily dismissed. Modern hunters live in the modern world. They are the products of evolutionary history, not fossilized remnants of the distant past. Neither their behavioral capabilities nor the options open to them need necessarily be the same as those available to their predecessors in the distant past; indeed, the differences among them, their predecessors, and other now extinct hominids are precisely the object of interest. The fact that some aspects of the archaeology produced by modern hunters and paleolithic hominids match does not mean that their respective patterns of behavior must also be the same or even broadly similar. Differences in their morphologies alone make the analogy suspect; cautionary tales about the nature and ambiguity of the archaeological data amplify the basis for skepticism. Conventional ethnoarchaeology cannot resolve this problem. It lacks the capacity to generate comprehensive, well-warranted, *ethnographically unprecedented* models of hominid behavior that might account for the archaeological evidence.

Granted there are situations, even in the paleolithic, where a modern analogy may be appropriate. Assuming, for the sake of argument, that such situations can be confidently identified, the problem then becomes accounting for the behavior indicated by the analogy as well as any variability it may display (e.g., shifts in technology, diet, settlement pattern—all common in this time period). *Because ethnoarchaeology generally takes behavior as a given, it is in no position to address this question.* It may allow some aspects of past behavior to be described more fully and with greater accuracy and confidence in some circumstances, but it offers no help in explaining any variability that behavior may display.

A POSSIBLE SOLUTION

In my view, this problem can be addressed only by expanding the focus of ethnoarchaeological inquiry to include the study of variability in behavior itself under the aegis of a general theory capable of predicting and accounting for that variability. In principle, such a theory would enable archaeologists to develop potentially testable expectations about behavior and its archaeological consequences in any setting, past or present, even if these are unlike any known among modern humans.

The best available candidate may be neo-Darwinian behavioral (or evolutionary) ecology (e.g., Krebs and Davies, 1991). Its basic premise is that the behavior of all living organisms is shaped by natural selection. Its objective is to explain patterns in behavior by identifying the constraints that underlie them, specifically those that affect differences in reproductive success. It does this through the use of formal economic models. These require an analyst to stipulate a hypothetical fitness-related goal for the behavior of interest, the alternate strategies available to achieve that goal under the circumstances in question, and the costs and benefits associated with each. These in turn predict an optimal pattern of behavior. Any mismatch with behavior actually observed suggests one or more of the contingent hypotheses about goal, costs, benefits, or other situational constraints is false. Alternate hypotheses are then posed and the test reiterated until the match improves or the analyst concludes that the exercise is somehow improperly framed and looks for another, potentially more appropriate model with which to begin the process anew. This approach does not eliminate equifinality; it may (in fact often does) generate different explanations for the same behavior. But it also puts the process of identifying, evaluating, and, in at least some cases, eliminating alternate explanations on a much stronger analytic footing.

This approach has been productively employed in research on nonhuman organisms since the late 1950s (e.g., Hutchinson, 1959; Lack, 1954; Williams, 1966; for review see Krebs and Davies, 1991) and more recently applied with increasing success in ethnographic contexts (e.g., Borgerhoff Mulder, 1991; Cronk, 1991; Smith and Winterhalder, 1992). Its potential utility for prehistorians and other students of human evolution has been recognized for more than a decade (e.g., Bayham, 1979; Beaton, 1973; Foley 1987, 1992; Hawkes *et al.*, 1982; Hill, 1982; O'Connell *et al.*, 1982; Simms, 1987; Winterhalder and Smith, 1981; see also Grayson, 1993; Simms, 1992). From this particular perspective, its strength lies in its ability to generate predictions about variability in a range of phenomena (including but not limited to diet, foraging patterns, resource sharing, mating strategies, group composition, territoriality, and the formation of social hierarchies) long thought to be crucial for human evolution. Even more important, it provides a framework for integrating those predictions—for envisioning the implications of variability in one aspect of fitness-related behavior for variability in others. Some of its predictions are likely to have direct archaeological implications; others will almost certainly not. Theoretically informed ethnoarchaeology will be crucial to identifying both. The fact that all are products of a coherent theory of behavior means that speculation about aspects that lack direct archaeological manifestations can proceed in a more comprehensive, tightly structured, better-grounded manner than is currently possible.

One can illustrate the problems with conventional ethnoarchaeology and the gains potentially available from a more theoretically driven approach by review of work on two topics, site structure and faunal remains. Both have been the focus of a substantial amount of ethnoarchaeological inquiry; both figure prominently in the paleolithic literature. There is general consensus that both can provide information on important aspects of past behavior, even insofar as it differs from that in the present.

Ethnoarchaeological research on site structure exemplifies the difficulties inherent in the conventional approach. It is concerned almost exclusively with the connection between behavior and its archaeological consequences, seldom takes variability in site-related behavior as a question to be investigated, and rarely attempts to explain that variability except in ad hoc, common-sense terms. Its results are derived inductively and presented as empirical generalizations about behavior and site structure in a sample of cases in the present. As a result, they cannot be applied with confidence much beyond the context(s) in which they were derived, except in cautionary terms. The fact that this work has yet to produce a well-warranted, substantively informative inference about behavioral variation in the past unrepresented in the present is an important indication of its shortcomings. Even where the analogies it supports are appropriate, this work does not help explain variability in the behavior it reconstructs except in relatively trivial terms.

Ethnoarchaeological research on faunal assemblage composition shares these problems to a degree, but differs in that at least a few analysts have consistently focused on the determinants of variability in bone-related human behavior as well as its archaeological implications. Some operate explicitly from the perspective of behavioral ecology; others adopt less general but still somewhat similar approaches yielding partly commensurate results. Taken together, these studies suggest close, predictable relationships among fitness-related goals, carcass acquisition and processing strategies available to achieve those goals, their respective costs and benefits, actual carcass-related behavior, and its effect on archaeological bone assemblages—all in the present. Substantive application of these results to problems in prehistory has so far been limited. Nevertheless, their general form and potential importance are readily anticipated and, in a few cases, actually exemplified.

SITE STRUCTURE, AN EXAMPLE OF THE PROBLEM WITH ETHNOARCHAEOLOGY

The term "site structure" refers to the horizontal distribution of artifacts, features, and other refuse within a site. Archaeologists have long held that it can provide evidence of past human group size, composition, organization, and activities. Efforts to collect and assess this evidence include the careful plotting of refuse scatters on "floors" or "living surfaces" in ancient sites and the development of quantitative pattern recognition techniques designed to facilitate their analysis. Both approaches figure prominently in research on the paleolithic (e.g., Ammerman, 1992; Audouze, 1987; Blankenholm, 1991; Bosinski, 1979; Bunn *et al.*, 1980; Carr, 1985, 1991; Clark, 1954; Hietala, 1984; Howell *et al.*, 1962; Isaac, 1977; Keeley, 1991; Klein, 1969; Leakey, 1971; Leroi-Gourhan and Brezillon, 1972; Simek, 1984; Stapert, 1989a,b; Whallon, 1974).

Ethnoarchaeologists have contributed to this effort by identifying patterns in site structure linked with specific aspects of behavior and evaluating the accuracy of assumptions commonly made about these links. They have pursued this research in a variety of settings, mainly at residential sites occupied by subsistence hunters or farmers [see Gamble and Boismier (1991), Kent (1987, 1989, 1990), Kroll and Price (1991), MacEachern *et al.* (1989), Meehan and Jones (1988), and Staski and Sutro (1991) for recent critical reviews, case studies, and comprehensive bibliographies]. Pertinent behavioral data come from historical accounts, informant interviews, and direct participant observation. Related archaeological records range from simple plans of individual "activity areas" to detailed piece-plots of refuse items and features across entire sites. Treatments of the relationships between the two vary from normative descriptions to detailed quantitative analyses.

Results of this work are conveniently summarized in terms of three aspects of behavior, all of which have important implications for archaeological site structure: the distribution of activities within sites, patterns in refuse disposal, and the spatial scales at which on-site activities are pursued.

Distribution of Activities Within Sites

Early research on prehistoric site structure often entailed the assumption that people routinely divide sites into discrete, activity-specific areas. In combination with other assumptions about "tool kits" and patterns in refuse disposal (see below), this facilitated arguments about possible relationships between spatially covariant refuse categories and the activities that produced them (e.g., Whallon, 1973, 1974). These assumptions are sometimes referred to collectively as the "functional" model of site structure.

This model was overturned by the first wave of ethnoarchaeological research on the topic (e.g., Bonnichsen, 1973; Longacre and Ayres, 1968; Yellen, 1977a). Subsequent studies indicate that among hunters and small-scale farmers, residential sites are routinely divided into household and other activity areas (e.g., Arnold, 1991; Bartram *et al.*, 1991; Binford, 1983, 1991a; Brooks *et al.*, 1984; Deal, 1985; Fisher and Strickland, 1991; Hitchcock, 1987; Janes, 1983, 1989; Jones, 1983, 1993; Killion, 1990; O'Connell, 1987; O'Connell *et al.*, 1991; Parsons and Parsons, 1990; on pastoralists see also Cribb, 1991; Simms, 1988). Household areas are typically the settings for most of the activities undertaken at the site, but in every group described at least some are performed elsewhere as well, sometimes exclusively. Such segregation is often attributed by analysts to considerations of convenience or efficiency: the activities in question are said to require exclusive use of space for long periods of time or to produce large quantities of refuse, thereby interfering with the conduct of other activities (e.g., Arnold, 1991; Binford, 1983, pp. 144–192, 1987; Fisher and Strickland, 1991; Hitchcock, 1987; Janes, 1983; Killion, 1990; O'Connell, 1987; Yellen, 1977a). In other instances, the separation has a social dimension. In many groups, for example, men and women spend much of their time in parts of the site away from their respective household areas. Some of these spots are used by members of only one sex; others are used serially (sometimes in the course of a single day) by sexually segregated groups (e.g., Binford, 1983; O'Connell, 1987; O'Connell *et al.*, 1991). Activities differentially linked with the sexes are distributed accordingly.

Within activity areas, different tasks may be carried out consistently in the same spot or relocated periodically, depending on such factors as exposure to or availability of light, heat or shade, equipment or facilities required and costs of maintaining versus repositioning them, and variation in the composition [especially size and sexual makeup of the group(s) involved (e.g., Bartram *et al.*, 1991; Binford, 1978b, 1983, pp. 144–192, 1987, 1991a; Chang, 1988; Fisher and Strickland, 1991; Hitchcock, 1987; O'Connell, 1987; Oswald, 1987; Siegal, 1990; Yellen, 1977a)].

Binford (1983, 1987, 1990) and O'Connell (1987) propose that residential site structure among subsistence hunters can be described on a continuum broadly correlated with geographic setting, foraging tactics, and degree of reliance on stored foods (see also Binford, 1980; Hitchcock, 1987). Low-latitude "foragers" are said to gather resources daily, move residential bases frequently in response to the variation in the distribution of potential food sources, construct relatively simple shelters, perform most activities in organizationally redundant household areas, create relatively few special activity areas, and frequently reposition activities within household and other areas depending on variation in the physical and social conditions of performance. This leads to the inference that their base camps will be relatively unstructured, at least as a function of the spatial distribution of activities.

In contrast, high-latitude "collectors" are said to depend on stored foods, relocate base camps less often, construct relatively elaborate shelters in response to colder climatic conditions, perform a greater proportion of activities (notably those connected with processing food for storage) in special activity areas, and display greater redundancy in the positioning of activities within areas, partly because of greater investment in facilities and partly because more effective shelter ensures greater consistency in the physical conditions of performance (but see Janes, 1983, 1989). It is also suggested that they display greater seasonal variation in site structure as a function of the variation in the array of tasks performed, the size and composition of associated groups, and the physical conditions of performance (e.g., Binford, 1983, 1993). As a result, their base camps are expected to be more highly structured than those used by foragers. The site structural aspect of this argument has yet to be adequately tested.

Refuse Disposal Relative to Location of Activities

As part of the "functional" model of site structure, archaeologists often assumed that refuse resulting from various activities was deposited at or near the point of production [i.e., in "primary" context (Schiffer, 1972)]. Items of uncertain utility consistently found together in covariant sets were thought to have been used and discarded in the course of the same or similar activities. Inferences about their function were drawn accordingly (e.g., Whallon, 1974).

Like other aspects of the model, this assumption was ultimately rejected as a general rule, partly on the basis of ethnoarchaeological observations (e.g., Schiffer, 1972; Yellen, 1977a). These indicate that the probability of refuse items being dropped in primary context varies inversely with such factors as duration of site occupation, investment in facilities, and size and number of items produced (for references and discussion see Metcalfe and Heath, 1991). Larger objects, in particular, seem more likely to be removed to secondary disposal areas (e.g., Arnold, 1990; Bartram *et al.*, 1991; Binford, 1978b, 1983; Clark, 1991; DeBoer, 1983; DeBoer and Lathrap, 1979; Hayden and Cannon, 1983, 1984; Hitchcock, 1987; McKellar, 1983; Murray, 1980; O'Connell, 1987; Simms, 1988; Stahl and Zeidler, 1990; Staski and Sutro, 1991). Smaller items are more often discarded in primary context and may escape subsequent cleanup and secondary disposal, depending on their shape and weight, the characteristics of the substrate on which they fall, and the nature of subsequent traffic on the surface (e.g., Gifford-Gonzalez *et al.*, 1985; Nielsen, 1991; Savalle, 1984; Stahl and Zeidler, 1990). Secondary discard may also be determined by an object's nuisance value (independent of its absolute size) and its anticipated future utility (e.g., Binford, 1983; Hayden and Cannon, 1983, 1984; South, 1977).

One can reduce the results of this work to a common-sense hypothesis: The discard location should reflect the interference value of debris and the relative costs of relocating activities and associated facilities versus those connected with shifting the refuse (Schiffer, 1972). Though probably true, it remains to be tested. Its operational utility will depend on the ease with which "interference value" and "relocation costs" can be calculated in archaeological context.

Spatial Scales

Early archaeological investigations of site structure often entailed two assumptions about the spatial scale of patterning, particularly at sites without elaborate architecture: first, that patterns will usually be apparent in exposures measuring no more than a few hundred square meters (e.g., Leakey, 1971); second, that there is a consistent relationship between the size of activity areas or sites and the size of groups that created them (e.g., Cook and Heizer, 1965; Naroll, 1962).

Ethnoarchaeological observations show that both assumptions are problematic. Even among subsistence hunters using simple structures, activity areas vary greatly in size, the largest covering >1000 m^2 (e.g., Binford, 1991a; O'Connell, 1987; on pastoralists, see also Cribb, 1991; Simms, 1988). In some cases, sampling the minimum number of areas needed to reveal *patterns* in their size and internal organization may require exposures *>10,000* m^2, at least two orders of magnitude greater than those commonly examined by archaeologists (e.g., O'Connell, 1987, 1994; Simms, 1988).

Systematic explorations of the relationship between spatial scale and population size have focused on two dimensions of site structure: size of household areas and the distances between them. Regarding the first, Binford (1978b, 1983) proposes a general relationship between human body size and the distribution of activities within areas, implying a consistent, perhaps cross-culturally constant link between the minimum number of individuals associated with a "space" and its areal extent. Data from the Alyawarra and !Kung provide some support for this argument, indicating (at least in these cases) that about 30% of the variance in size of household areas (defined as the zone of primary refuse disposal surrounding domestic hearths) is accountable by number of household occupants (O'Connell, 1987; Yellen, 1977a; see also Kent and Vierich, 1989). Even more interesting, the regression equations (area versus household size) for each group are quite similar. However, these and other studies also show that the relationship between population size and floor area may be affected by other factors, including the length of time the area is in use, the investment made in structures, and the variability in household size over the period of time during which structures are occupied (e.g., DeBoer, 1989; Hayden and Cannon, 1984; Hitchcock, 1987; Killion, 1990; Oswald, 1987; Siegal, 1990; Wandsnider, 1992).

Distances between contemporaneously occupied household areas vary widely within and between groups. Among the Ache, for example, nearest neighbors are <4 m apart on average (Jones, 1983, 1993), while at some Nunamiut camps, they are >70 m apart, again on average (Binford, 1991a). [See Whitelaw (1991) for a general survey and Bartram *et al.* (1991), Binford (1983, 1991a), Fisher and Strickland (1991), Gargett and Hayden (1991), Jones (1993), Marshall (1994), O'Connell (1987), and Yellen (1977a) for additional case studies]. Gould and Yellen (1987, 1991) argue that among subsistence hunters, this variation is a function of predator pressure: Where this is a threat, household areas are closely spaced; where not, they are far apart. Although some of the data they cite are consistent with this hypothesis, it cannot be the only consideration, primarily because it does not account for within-case variation. Among the Alyawarra, for example, mean nearest neighbor distance averages about 35 m but varies from 10 to 80 m even within single camps (O'Connell, 1987). Among the Nunamiut, within-camp variation is even greater (Binford, 1991a). Since predator pressure is a constant in each case, it cannot explain these differences (see also Binford, 1991b).

Whitelaw (1983, 1991) suggests that interhousehold distance varies inversely with economic interdependence: Households whose members frequently form cooperative work groups or consistently share food also camp close together; those less engaged place themselves further apart (see also Binford, 1991a,b; Brooks *et al.*, 1984; Fisher and Strickland, 1991; Gargett and Hayden, 1991; O'Connell, 1987; O'Connell *et al.*, 1991; Siegal, 1990). This proposition draws support from the common ethnographic observation that close kin often cooperate in various ways and that interhousehold distance sometimes varies with kinship distance. Still, few quantitative data for any group speak directly to the relationship between economic interaction and household spacing.

Archaeological Implications

These results have important cautionary implications: The functional model of site structure is inappropriate as a general guide to analysis and interpretation; sites are not routinely divided into activity-specific areas; refuse items that cooccur spatially need not necessarily have been used in the same activity; behaviorally significant patterning in refuse distribution and assemblage composition may be apparent only at much larger spatial scales and require correspondingly larger exposures than expected; and relationships between size of activity area or site and sizes of associated human households, task groups, or site populations are more complex than previously appreciated.

These same results can also be applied in more positive terms, as conventional analogies that either enable predictions about patterns in site structure likely to be encountered in certain situations or suggest hypotheses about the behavioral significance of patterns already observed. It is essential that such applications attend to problems of grain and scale: Not all sites contain the fine-grained record of site structure necessary to pursue this line of research; those that do must be investigated at much larger spatial scales than has customarily been the practice to ensure recovery of a representative sample of patterns actually present. Despite these problems, such analogies may prove useful in some circumstances (e.g., O'Connell, 1993b).

On the other hand, their utility is ultimately limited for the reasons rehearsed in the opening polemic: They generally appeal to common patterns in the relationship between behavior and its archaeological consequences while giving little or no systematic attention to behavior itself or its determinants. This means that they cannot predict or account for behavior unrepresented in the ethnographic sample on which they are based. Even where they permit accurate inferences about past behavior, they cannot explain the variability it may display, at least not in their current form. In the few cases in which determinants of site-related behavior have been addressed, the behavior in question is of descriptive interest only; their explanation has no important implications for knowledge or understanding of past human behavior.

These points are illustrated by two examples, the proposed site structural correlates of the forager–collector continuum (Binford, 1980, 1983; O'Connell, 1987) and Binford's (1978b, 1983) hearth-centered model of activity organization and refuse disposal. As indicated above, the forager–collector model describes a series of relationships among geographic setting, foraging tactics, and degree of reliance on stored foods. These in turn are said to have implications for other features of behavior, including the spatial organization of residential base camps (Binford, 1979, 1980, 1983; O'Connell, 1987, 1994; O'Connell *et al.*, 1991). The model is of general interest because the behaviors in question were probably important in human evolution, and because it provides a means of tracking at least some of them archaeologically.

The model can be challenged on several grounds (e.g., Bettinger, 1991a, pp. 62–82); among them, as noted above, that its proposed site structural correlates have yet to be demonstrated. More important to the present discussion is that the patterns in foraging behavior it describes are not in any sense explained. There are two aspects to this. First, at its heart, the model is an empirical generalization, an observation about the relationship between length of growing season and degree of reliance on storage across a sample of ethnographically known subsistence hunters (Binford, 1980, Fig. 4). The underlying argument is that length of growing season determines primary productivity, which determines resource availability to hunters, which determines storage practices. The key intervening term, resource availability, is never measured. Its relationships with length of growing season and primary productivity are quite complex. Thus, depending on the sample of cases selected, growing season may or may not predict resources and so may or may not account for storage (e.g., Thomas, 1983, pp. 10–23). Shifting the focus of attention to actual patterns of resource distribution and the opportunities and constraints they impose on hunters might well resolve the issue, but the model offers no guidance in how this might be done. It does not tell how resources are selected for exploitation or in what ways and in what circumstances selection, processing, and storage practices might be expected to vary.

The second problem with the model is that it takes certain features of behavior common to ethnographically known hunters (e.g., central place foraging, a sexual division of labor, nuclear- or extended-family social organization) as constants. Though these too are likely to be affected by features of local ecology, including available subsistence options, the model does not tell how, or how the outcome might be reflected archaeologically. In short, some of the most interesting and potentially provocative aspects of past hominid behavior are completely beyond the capacity of the model either to anticipate or to recognize and interpret if encountered archaeologically—a classic example of the limits of conventional ethnographic analogy.

At best, then, the model can be used to predict or interpret certain features of the archaeological record (including site structure) and, on the basis of the results, label the people who produced them as "foragers" or "collectors." This is now a popular practice in some quarters, notably with respect to the European and west Asian Upper Pleistocene (e.g., Enloe, 1993; Gamble, 1986; Lieberman, 1993; Peterkin *et al.*, 1993; Pike-Tay and Knecht, 1991; for review see also Kelly, 1992). Appealing as it may be, it will not help explain why people "foraged," "collected," or shifted from one practice to the other. Classification in the absence of an argument about causality begs the interesting question.

Worse, such applications run the risk of assuming the past existence of certain patterns in behavior in circumstances where ethnographically unanticipated alternatives are not only possible but objects of special interest. Recall, for example, the once commonly accepted suggestion that site structure in Plio-Pleistocene deposits at Olduvai resembles that found at sites created by modern African foragers (Leakey, 1971, pp. 24, 260). Assume, for the sake of argument, that this suggestion is accurate. Does this imply that hominids responsible for these deposits behaved like modern foragers, say, in terms of their subsistence and social organization? Despite past acceptance of arguments like this (e.g., Isaac, 1978), most archaeologists would now be skeptical (cf. Binford, 1981, 1985). Why not be equally skeptical of its applicability in the Upper Paleolithic? Where in the past are models like this appropriately applied, where not, and what determines the answers to these questions? The forager–collector model itself gives no guidance here. Where it is seen as an *in*appropriate analogue, say in the Lower Pleistocene, on what basis does one offer an alternative interpretation of the archaeology? Where the archaeology is clearly *in*consistent with ethnoarchaeologically derived expectations, how does one proceed beyond their description? The common answer, "Collect more archaeological data more carefully," will not suffice: It simply adds to the problem. Only by appealing to the determinants of behavior as well as its archaeological consequences can these questions be addressed.

The second example, Binford's (1978b, 1983, pp. 144–192) hearth-centered activity model, speaks to some of these issues but in ultimately unsatisfactory terms. It describes the distribution of activities and associated patterns of refuse disposal at a series of ethnographically observed hearths and uses the descriptions as the basis for reinterpretation of similar patterns observed archaeologically, notably at the well-known Magdalenian site called Pincevent (see also Carr, 1987, 1991; Stapert, 1989a). It has since been applied in other settings as well (e.g., Gamble, 1986, pp. 251–263; Kind, 1985; Kozlowski, 1985; Stapert, 1989b). Its central elements include the observation that activity organization around hearths is influenced by such factors as the presence or absence of a surrounding shelter, the prevailing weather conditions (especially wind direction, if the hearth is located in the open air), the number of actors and their respective postures (e.g., sitting or standing around the hearth), and the nature of activities in which they are engaged. Disposal of refuse produced in such situations is seen to be a function of some of these same factors (e.g., presence/absence of shelter, wind direction) as well as the nature of the refuse itself, especially the size of individual items.

Unlike the forager–collector example, the issue here is not how widely the model can be applied or whether it explains the behavior it describes. Insofar as it appeals to such considerations as the importance of maintaining body heat in cold climates, the relative advantages of sitting upwind versus downwind of a smoky fire, and the influence of the physical principle of impenetrability as a determinate of the spacing of actors and potentially disruptive objects in a work space, it probably holds everywhere and accounts for the behavior it permits one to reconstruct. The key question is whether its application yields any important insights on past human behavior and its evolution. It is difficult to see not only how it does but how it ever could. Ethnoarchaeological models not only need to be explanatory with respect to behavior, but also need to explain something important about it. Working in the context of a general theory of behavior helps an analyst identify what might qualify and why.

FAUNAL REMAINS: TESTING THE PROPOSED SOLUTION

Animal bones are a prominent part of the archaeological record from late Pliocene times onward. They are commonly seen as indicators of economic orientation and, where hunting is inferred or at issue, of predatory capability. For these reasons, they are often implicated in arguments about the evolution of other aspects of behavior, notably social organization (e.g., Binford, 1984; Dart, 1957; Hill, 1982; Isaac, 1978; Washburn and Lancaster, 1968). As a result, substantial attention has been paid to sampling, describing, and analyzing them (e.g., Grayson, 1984; Klein and Cruz-Uribe, 1984; Ringrose, 1993).

Ethnographic and ethnoarchaeological contributions to this effort include descriptions and analyses of prey selection, carcass transport and processing, and bone discard, mostly among subsistence hunters in Africa and the Americas. Unlike the work on site structure, pertinent behavioral data are obtained almost exclusively by direct participant observation; appeals to informant testimony about past events are rare. Also, a larger proportion of studies are relatively long-term, some carried out over periods of several years. Finally, a substantial fraction of the work has been conducted under the aegis of behavioral ecology and, as a consequence, is concerned as much with the explanation of bone-related behavior itself as with its archaeological consequences. In principle, its results can be used to anticipate variability in carcass procurement and handling practices in a very broad range of circumstances. They can also be connected, again by appeal to theory, with variability in other aspects of behavior as well.

Results of this research speak directly to three dimensions of faunal assemblage composition: taxonomic and body part representation and damage morphology.

Taxonomic Composition

Until recently, most archaeologists took the range of taxa represented in an assemblage as a simple, unambiguous index of hunters' choices among potential targets. Preferences were expected to correlate broadly with prey body size and abundance; variation through time and space was attributed to environmental change, differences in hunting technology or tactics, increased familiarity with local habitats, "cultural choice," or a combination of these factors (e.g., Flannery, 1969; Jones, 1978; Klein, 1976; Martin, 1967). While such interpretations were often plausible, they were seldom subject to systematic test nor were alternative explanations often explored.

Over the past two decades, actualistic research has forced a change in this approach by identifying the array of agents other than humans that shape assemblage composition (e.g., Bonnichsen and Sorg, 1989; Haynes, 1992; Hudson, 1993a), providing criteria for distinguishing their effects (see below, under "Skeletal Part Representation" and "Damage Morphology"), and exploring the determinants of prey choice. The last of these has been especially important in broadening the range of potential explanations for differences in the taxonomic composition of archaeological faunas and providing a basis for testing them. Though not normally considered ethnoarchaeology because of its lack of explicit concern with material consequences, this research has very important implications for archaeology.

Systematic investigations of prey choice are grounded largely in behavioral ecology and often make use of the optimal diet model (Stephens and Krebs, 1986; see also Bettinger, 1991a, pp. 83–111; Kaplan and Hill, 1992). This model is designed to explain patterns in the selection (and rejection) of targets from among an available array. It is based on the observation that prey vary in nutritional value, frequency of encounter, and cost of capture and processing once encountered. Like other foraging models, it stipulates a hypothetical currency in terms of which alternatives are evaluated and identifies morphological, behavioral, and ecological constraints on the predator's options. The proposition most commonly tested is that hunters pursue the subset of prey whose capture maximizes the mean rate of energy capture while foraging. In other words, once encountered, potential prey are expected to be pursued if, *and only if*, the energetic return rate likely to be gained exceeds that available from bypassing the target at hand and continuing to search for other, higher ranked items (i.e., those likely to yield postencounter returns high enough to offset the cost of additional search). As with other foraging models, the underlying assumption is that efficiency in the capture of energy is generally favored by natural selection. All else equal, foraging tactics (including patterns in prey choice) that achieve this goal most effectively are expected to persist and spread at the expense of alternatives (for additional discussion, see Kaplan and Hill, 1992; Krebs and Kacelnik, 1991; Stephens and Krebs, 1986).

Observations among modern subsistence hunters yield results broadly consistent with this basic hypothesis. Prey rank has been found to vary with taxonomic identity, body size, nutritional condition, behavior (including differences by age or sex within taxa), and procurement techniques (including available technology); encounter rates are conditioned by variation in prey population size, habitat characteristics, and search technology (e.g., Alvard and Kaplan, 1991; Bailey and Aunger, 1989; Bennett, 1991; Hames and Vickers, 1982; Hawkes *et al.*, 1991, 1995; Hill and Hawkes, 1983; Hudson, 1991; Hurtado and Hill, 1989; O'Connell and Hawkes, 1981, 1984; O'Connell *et al.*, 1988a; Smith, 1991; Wilkie and Curren, 1991; Winterhalder, 1981; Zeleznik and Bennett, 1991). Falsifications of the basic hypothesis have led to the investigation of alternate goals and currencies, including the proposition that in some circumstances men's prey choice may be determined by fitness-related considerations other than maximizing the average daily rate of energy capture (see especially Hawkes, 1990, 1991, 1992, 1993a,b; Hawkes *et al.*, 1982, 1991; Hill, 1988; Hill and Kaplan, 1993; Hill *et al.*, 1987; Hurtado and Hill, 1990; Hurtado *et al.*, 1985; Kaplan and Hill, 1992).

Skeletal Element Representation

Variation in the body part composition of archaeological faunal assemblages is attributable to one or both of two factors: differential transport to or from a death site and *in situ* attrition. The latter can be caused by any of a number of agents—biological, chemical, and mechanical. Its effects vary inversely with part density [see Blumenschine and Marean, (1993), Grayson (1989), Lyman (1991), Lyman *et al.* (1992) for literature reviews and basic discussion; see also below, under "Damage morphology"].

Differential transport may be caused by fluvial or biological processes (e.g., Behrensmeyer and Hill, 1980; Gifford, 1981; Haynes, 1992). Where transport by humans is implicated, differences in the proportions of parts represented by various taxa commonly provide the basis for inferences about carcass processing and transport tactics and, by extension, other features of behavior including settlement pattern and social organization (e.g., Binford, 1981; Bunn and Kroll, 1986; Klein, 1976; Klein and Scott, 1986). Arguments were modeled initially on T. E. White's (1952) idea that the probability that any bone would be taken from a kill varied directly with the weight of attached edible tissue and inversely with the weight of the bone itself. White suggested that, in general, limb elements should be moved more frequently than axial parts, the bones of smaller animals more frequently than those of larger ones. Bones not moved should be stripped of edible tissue and discarded at the kill. These suggestions were widely applied as interpretive conventions through the mid-1980s.

Binford's (1978a) ethnoarchaeological work with the Nunamiut falsified White's specific hypothesis about preferential limb transport and, by extension, any archaeological argument that appealed to it as a general rule. But it also supported part of the underlying proposition about economic utility, namely, that the weight of edible tissue attached to a bone predicted its rank for transport [see Metcalfe and Jones (1988) for clarification of Binford's argument on this point]. Binford's utility indices for caribou and domestic sheep measured this variable, provided a new basis for interpreting part representation (e.g., Speth, 1983; Thomas and Mayer, 1983; cf. Grayson, 1988), and prompted the construction of similar indices for other taxa [e.g., Blumenschine and Caro, 1986; Blumenschine and Madrigal, 1993; Borrero, 1990a,b; Brink and Dawe, 1989; Emerson, 1993; Kooyman, 1984, 1990; Lyman *et al.,* 1992b; O'Connell and Marshall, 1989; for additional reanalyses of Binford's Nunamiut data see also Jones and Metcalfe, 1988].

Recent ethnoarchaeological work in other settings adds to these results but also complicates them. Body part rank for transport is now seen to vary greatly across prey taxa independently of body size, by nutritional condition of individual prey within taxa, and by carcass completeness on encounter (O'Connell, 1993a; O'Connell *et al.*, 1988a,b, 1990; cf. Bunn, 1991, 1993; Bunn *et al.*, 1988; for pertinent experimental data and additional references, see also Blumenschine and Madrigal, 1993). Also, part rank is *not* consistently predicted simply by weight of attached edible tissue: The economic utility of any part may be adjusted by partially stripping meat from bone prior to removing either from the kill (Bartram, 1993; Bunn *et al.*, 1988; O'Connell *et al.*, 1988b, 1990). This observation implicates processing costs and available processing technology as determinants of rank (Jones and Metcalfe, 1988; Metcalfe and Jones, 1988). Finally, the overall proportion of bones removed from a kill varies with transport costs (including overall carcass size and distance from kill site to anticipated point of consumption), transport capacity (as determined by number of carriers or available transport technology), processing gear, and time constraints (Bartram, 1993; Bunn *et al.*, 1988; O'Connell and Marshall, 1989; O'Connell *et al.*, 1988b, 1990, 1992; see also Binford, 1978a).

Overall, these findings suggest that White's original argument about the importance of cost/benefit considerations in decisions about carcass butchery and transport is accurate in principle but operationally simplistic. Metcalfe and Barlow (1992) take an important step toward improving it by presenting a formal model of resource processing and transport grounded in Orians and Pearson's (1979) theoretical work on central place foraging. It turns on the observation that some resources (including animal carcasses) can be seen as sets of parts that not only differ in utility and associated processing and transport costs, but also can be reconfigured with respect to utility and transport costs by processing at the point of acquisition. The question is how to treat them: which to move, which to discard, and, most important, how to process them before doing either.

Metcalfe and Barlow argue that these decisions will be determined by four factors: the forager's goal, the options available to achieve it, the currencies in which the options are to be compared, and the constraints associated with each—all pertinent considerations in any foraging model. For central place foragers, a common goal may be maximizing the nutritional utility of the load transported back to a residential base to share with others. The options (cf. "decision variables") relevant to this goal include the range of parts to take, the subset to process prior to transport, and the processing techniques to employ. Currencies may be time and energy. Constraints may include the forager's transport capability, the time frame in which the goal is to be achieved, the distance to base, and the utilities and processing and transport costs of various parts and their respective components. Supplying values for the pertinent constraints in any given situation yields a potentially testable prediction about which parts will be processed and how, which moved, and which discarded at the point of acquisition. Changing the goal, decision variables, currencies, or constraints yields different but equally testable predictions.

The utility of the model is yet to be assessed but the circumstances needed to do so are all but present [see O'Connell and Marshall (1989) for a partial test on red kangaroo]. Sizable data sets on differential part processing and transport are available for at least two groups, the Nunamiut and Hadza (Binford, 1978a; Bunn *et al.*, 1988; O'Connell *et al.*, 1988b, 1990; see also Bartram, 1993; Marshall, 1994). Lacking are pertinent data on processing costs and, in the Hadza case, part utilities (but see Blumenschine and Caro, 1986; Blumenschine and Madrigal, 1993). Once these are assembled, one could use the model as a framework to test hypotheses about the factors that affect observed transport patterns in each group, including currently disputed notions about Hadza men's kill-site processing goals (Bunn *et al.*, 1988; Oliver, 1993; Speth, 1990; cf. O'Connell *et al.*, 1988b, 1990, 1992) and the importance of boiling technology as a determinant of element rank for transport (Bunn *et al.*, 1988; Gifford-Gonzalez, 1993; Oliver, 1993).

Damage Morphology

This term refers to patterns in the form, frequency, and anatomical distribution of damage to bone surfaces (e.g., cutmarks, tooth scores, fracture patterns). Archaeologists have often taken these as indicators of the identity of the agents responsible, their processing and consumption goals, and the relative priority of access to the carcass or body part in question. These in turn speak to issues as diverse as the role of scavenging in the diet of Plio-Pleistocene hominids and the arrival date of humans in the Americas (e.g., Bonnichsen, 1979; Bunn and Kroll, 1986; Dart, 1957). The potential ambiguity inherent in many interpretations has been recognized for some time and actualistic research pursued to resolve it (for summaries of the early literature see Binford, 1981; Bonnichsen and Sorg, 1989; Brain, 1981; Gifford-Gonzalez, 1991; Lyman, 1987; for recent case studies see Binford *et al.*, 1988; Blumenschine, 1988, 1993; Blumenschine and Marean, 1993; Blumenschine and Selvaggio, 1988, 1991; Enloe, 1993; Greenfield, 1988; Haynes, 1992; Hockett, 1991; Hudson, 1993b; Lam, 1992; Lupo, 1994; Marshall and Cosgrove, 1990; Schmitt and Juell, 1994; White, 1992; Yellen, 1991a,b).

Results are pertinent to two closely related questions: the first, whether particular agents or processes create distinctive damage patterns. Some apparently do (e.g., Blumenschine, 1993; Blumenschine and Selvaggio, 1988, 1991), but there is also broad overlap in the morphology of marks, fractures, and other kinds of bone damage produced by humans, other carnivores, and certain mechanical processes (e.g., Behrensmeyer *et al.*, 1986; Binford, 1981; Fiorello, 1989; Gifford-Gonzalez, 1989; Haynes, 1983; Hill, 1989; Lupo, 1994; Oliver, 1989).

The second question is whether the overall pattern of damage (i.e., mark and break morphology, frequency and distribution across skeletal elements) varies systematically with respect to the agents involved, their relative order of access to the carcass or skeletal element in question, the intensity with which they operate, and the condition of carcass or element at the time. Answers are pertinent to questions about priority of access to carcasses by various consumers (e.g., passive versus active scavenging and hunting) and, where humans are involved, the technologies used in processing them.

Despite substantial debate on this issue (e.g., Binford, 1986, 1988; Bunn and Kroll, 1986, 1988), there is little ethnoarchaeological basis for discussion. Only a few comprehensive accounts of butchery and resulting damage are available (Binford, 1981; Brain, 1981; Gifford-Gonzalez, 1989; Yellen, 1991a). All are basically descriptive with little systematic attention to explaining patterns observed or the variation they display. Binford (1981), Gifford-Gonzalez (1989), Lupo (1994), and Oliver (1993) plausibly suggest that damage patterns vary as a function of the energetic costs and benefits of alternate processing techniques applied under different situational constraints (e.g., carcass size, completeness and condition on encounter, available technology, processing goals) but their arguments are informally phrased and untested. Yellen (1991a) takes a different tack, attributing differences in !Kung treatment of small mammals to cultural preference or "style" (see also Yellen, 1977b). In addition to begging the question of preference, Yellen's assertion is at odds with most of his description, which implies (though again does not demonstrate) that carcass treatment and resulting bone damage are driven at least in part by features of prey anatomy, including nutritional value and processing costs, relative to available technology.

This work could be improved dramatically by means of experiments designed to test cost/benefit explanations for patterns observed actualistically or archaeologically. As with the foraging models reviewed above, analysis would attend to alternative processing goals, options, currencies, and constraints (including available technology and condition of the carcass) and their respective implications for overall damage. Recent research by Blumenschine, Marean, and associates on damage to bone by nonhuman carnivores (mainly hyena) offers an instructive lead (for summary treatment and references to previous work, see Blumenschine and Marean, 1993, see also Binford *et al.*, 1988). In a series of controlled experiments, they demonstrate that carcass consumption patterns and resulting damage to bone surfaces vary directly with nutrient content and the costs of extraction as measured by bone density. Analyses of human-produced assemblages would benefit from a similar approach. Blumenschine and Selvaggio (1988, 1991) have taken an important step in this direction, showing that the distribution of hammerstone damage on long bones broken to extract marrow varies with processing costs as determined by element size and shape.

Archaeological Implications

As with site structure, ethnoarchaeological research on faunal remains has important cautionary implications. Some are generally appreciated; among them, that hunters do not necessarily take a random or representative sample of potentially available prey, that body part transport practices vary situationally, and that different agents may inflict similar patterns of damage on bone. Conventional interpretations of various archaeological phenomena are complicated (if not entirely compromised) as a result (e.g., O'Connell *et al.*, 1988b, 1990, 1992).

Other cautionary points may be less widely recognized. One especially pertinent to the paleolithic emerges from the comparison of ethnoarchaeological research on site structure and faunal remains. As indicated above, paleolithic archaeologists have long seen refuse scatters on geologically stable land surfaces as important sources of information about past human behavior and, as a result, have recorded their contents with great precision, typically piece-plotting the location of every item. The rationale for this approach was originally provided by the functional model of site structure.

Though problems with this model were recognized more than a decade ago (Yellen, 1977a; see also O'Connell, 1987; O'Connell *et al.*, 1991, 1992), the practice of piece-plotting persists as a matter of routine, even though its cost almost always prevents exploration at larger spatial scales where behaviorally significant patterns in site structure might well be apparent (for recent examples, see Audouze, 1987; Keeley, 1991). The illusion of precision in data recovery distracts attention from the original purpose of the exercise.

The absurdity of this situation is underlined by two points emerging from actualistic research on faunal assemblage composition. First, the commonly observed practice of scattering parts of large animal carcasses widely across sites in the course of processing, consumption, and disposal suggests that archaeological sampling schemes designed to assess part representation must be conducted at equally broad scales. Second, the fact that carcass treatment practices vary in several dimensions, notably by taxon, points to the need for large samples to support systematic analysis. Both requirements are frustrated by the trade-offs in scale of exposure commonly associated with piece-plotting.

The Zinj floor at Olduvai (Leakey, 1971) is a case in point. This is a relatively large exposure by paleolithic standards, measuring about 300 m^2. It produced a sizable faunal collection, more than 60,000 individual specimens, including the remains of an estimated minimum of 48 large animals, mainly ungulates (Bunn and Kroll, 1986). Nearly all this material and the associated lithics are piece-plotted. The size and excellent condition of the collection make it the single most important source of archaeological information now available on Plio-Pleistocene hominid subsistence (e.g., Binford, 1981; Blumenschine, 1993; Bunn and Kroll, 1986; Potts, 1989).

Though many regard the piece-plot data as essential to the behavioral interpretation of this site (e.g., Kroll and Isaac, 1984), their actual significance remains completely opaque. In hindsight, the effort devoted to recording them would arguably have been better spent enlarging the scale of exposure and increasing the sample of faunal remains. If a cautionary cue from the Hadza is appropriate, analysis of this collection might begin with a division of remains by taxon, probably to genus (O'Connell *et al.*, 1988b, 1990). Current approaches which lump taxa by body size class (e.g., Bunn and Kroll, 1986) run the risk of conflating potentially important intertaxonomic differences in part representation and damage morphology. The most common large animal in the collection is *Kobus*, represented by an estimated nine individuals (Bunn and Kroll, 1986), probably too few to support a serious analysis, especially if carcass acquisition and site formation processes are at all complex. If the Zinj floor is a discontinuous segment of a larger site, the sample may also be skewed as a function of site structure. The obvious solution in cases like this (and there are many in the paleolithic) is to abandon piece-plotting (at least as a routine procedure), record provenience at a coarser scale, and expose a large area, thereby increasing the absolute numbers of bones recovered and gaining a better picture of their distribution in space. Unfortunately, the cautionary observations that signal the need for these adjustments give no positive guidance in carrying them out.

Ethnoarchaeological work on faunal remains can also be used as the basis for conventional analogies but problems with equifinality and unaccountable mismatches quickly become apparent. Binford's (1984) work on large animal bones from early Upper Pleistocene deposits at Klasies River Mouth provides examples. Based on his experience with hack marks on frozen carcasses processed by the Nunamiut, Binford interprets similar marks on specimens from Klasies as an indication carcasses brought to that site were encountered in a desiccated state, supporting his notion that they were scavenged rather than hunted. Lupo's (1994) analysis of faunal remains from modern Hadza sites shows that such marks can also be found on carcasses known to have been killed by hunters and acquired fully intact but in a state of rigor mortis—a classic example of equifinality.

Binford's analysis also revealed patterns in body part representation unlike any reported ethnographically, leading him to imagine similarly unprecedented patterns of prey procurement, consumption, and part transport. Novel propositions about middle paleolithic hominid social organization follow. Binford may be right here, but there is no good reason to think so and no obvious test. Like most conventional analogies, it shows either that the past might have been like the present or that it was not, but in ways one can only guess about. Absent a theoretical framework, such guesses are generally unwarranted and untestable.

Because ethnoarchaeological research on bone assemblages has been concerned as much with the determinants of variability in human behavior as with its archaeological consequences, it is possible to avoid these problems in application, at least in principle. If the behavioral variability observed in the present can be explained in general terms, one should be in a position to anticipate or account for its form and material reflection in the past, even if these lack a direct modern analogue. Work along these lines is still at a very early stage but enough has been done to indicate its general direction and likely results by concrete example.

Take taxonomic representation as an issue. As indicated above, this is an important aspect of variation in archaeological faunal assemblages. Comparable variation is widely observed among modern hunters and has been successfully explored and explained with the use of the optimal diet model. Broughton (1994a; see also 1994b) provides an illustration of its utility in archaeological settings. At issue is a disagreement about subsistence in prehistoric California. Specialists (e.g., Kroeber, 1925) have long maintained that local populations had access to an abundance of resources and were comparatively affluent as a result. Recent work takes a different view, based on the observation that certain plant staples (such as acorns) are expensive to process relative to energetic returns and, as a result, apparently entered local diets relatively late. These data can be read to imply that Native Californians steadily broadened their subsistence base through time, perhaps in response to long-term population growth, resource depletion, and nutritional stress (e.g., Basgall, 1987; Cohen, 1981).

Broughton proposes a simple test of this argument based on variation in the taxonomic composition of faunal assemblages. He assumes that the goal of resource acquisition is to maximize energetic return rates as a means of enhancing fitness, identifies the broad categories of animal prey available to meet this goal, and points to a general correlation between prey body size and rank in diet breadth terms supported by actualistic work on both human and nonhuman predators. This leads to predictions about the order in which particular taxa should have been added to the diet or the changes in relative frequency they should display if diet were indeed expanding. Data from several well-sampled archaeological sites are found to be consistent with this proposition. Exceptions involve local differences in resource rank as a function of available capture technology. As Broughton notes, this result is subject to further tests involving other aspects of these assemblages, including damage morphology and body part representation. These remain to be conducted. The exercise is interesting and distinctive in that it anticipates from theory patterns in diet not represented ethnographically, a very different approach from standard analogy. It also shows how a general theory can be used to link arguments about variability in one aspect of behavior with others.

One can readily identify many other examples of diachronic variability in taxonomic representation that might benefit from a similar approach; for example, those associated with the Middle-Upper Paleolithic technological transition (e.g., Binford, 1984; Klein, 1976; Stiner, 1993). Use of the optimal diet model to address them may be complicated by difficulties in establishing prey rank and encounter rates, and it remains to be seen how these will be resolved. [Because rank is more readily established for plant resources, archaeological applications of the model are more common and the implications of their results for inferences about other related aspects of past behavior more thoroughly explored (e.g., Barlow and Metcalfe, 1994; Hawkes *et al.*, 1982; O'Connell and Hawkes, 1981; Simms, 1985a,b, 1987; Wright, 1994; see especially Hawkes and O'Connell (1992) for an argument about diet breadth, plant domestication, and its implications for variability in women's time allocation, reproductive success, and population growth rates in the terminal Pleistocene)].

Theoretically driven work on body part representation provides an equally good, if less fully realized, example. As indicated above, variation in this aspect of assemblage composition is often a basis for hypotheses about carcass acquisition, processing, and transport practices. Determining that one rather than another set of practices is represented may have implications for arguments about other aspects of behavior, such as foraging strategies, settlement patterns, and features of social organization. The Metcalfe/Barlow model offers the prospect of evaluating some of these hypotheses empirically.

Consider the sizable assemblages of large animal bones from Plio-Pleistocene archaeological sites at Olduvai and Koobi Fora (Bunn, 1982; Leakey, 1971). Some have argued that these are the product of hunting, active scavenging, and differential transport of high utility carcass parts by hominids from various points of acquisition to central places for further processing and consumption (e.g. Bunn and Kroll, 1986; Isaac, 1978). By comparison with modern hunters, this is seen in turn as evidence for a sexual division of labor (especially with respect to foraging), routine food sharing among adults, offspring provisioning by adult males, and relatively stable (perhaps monogamous) mating arrangements: in short, a "typically human" as opposed to "primate" pattern of behavior (Isaac, 1978; Washburn and Lancaster, 1968). More recently, these same assemblages have been interpreted very differently—as the remains of animals killed and largely consumed by other predators and subsequently subjected to passive scavenging by hominids who may or may not have moved parts away to another place in the process (e.g., Binford, 1981, 1983, 1985; Blumenschine, 1991, 1993; Blumenschine and Marean, 1993). The implications of this interpretation for arguments about other aspects of hominid behavior are generally unstated except in negative terms: To the degree that the emergence of the "human" pattern depends on routine access to large quantities of meat, its presence at this early date becomes unlikely (see below for further comment).

Both interpretations are open to familiar objections. The first is a conventional analogy, grounded in an empirical generalization. The assemblages in question contain disproportionately high percentages of appendicular parts; T. E. White (1952) asserts that hunters always favor such parts for transport to central bases; hence the assemblages are evidence of central place foraging. All other inferences follow. As indicated above, recent ethnoarchaeological work contradicts White's assertion (O'Connell *et al.*, 1988b, 1990; cf. Bunn *et al.*, 1988). Thus, while these sites may indeed be central places, large animal body part representation provides no support for the idea. Even if White were right, the conventional interpretation of these ancient assemblages would still be questionable insofar as it turns on an empirical generalization about modern human behavior, rather than an explanation of it.

The counterargument—that the bones reflect passive scavenging on the part of hominids—is largely by default and hence equally weak, especially as presented by Binford (1981). It effectively asserts that since much of the patterning in part representation and damage morphology could have been created by nonhuman predators (mainly hyena), it must have been so created. Hominids are therefore responsible only for the residue of evidence inexplicable in these terms. Their involvement becomes peripheral *by definition.* Questions about the relationship between scavenging and other aspects of early hominid behavior, or about the general nature of that behavior itself, are never systematically addressed.

Because it is theoretically driven, the Metcalfe/Barlow model may enable one to do better, both at interpreting patterns in faunal assemblage composition and at linking the behavior they represent to other aspects of past hominid life. Recall its main elements. It is a model of resource transport for central place foragers. It identifies a hypothetical but theoretically and empirically well-warranted behavioral goal—maximizing the utility of resources transported from the point of acquisition to the forager's residential base. It stipulates a set of decision variables—which parts to take, which to process prior to transport, and how to process them. It nominates pertinent currencies for the comparison of alternatives—time and energy. It specifies key constraints—among them, the forager's transport capability, the time frame in which the goal is to be achieved, the distance to base, and the utilities, processing, and transport costs of various parts and their respective components. Supplying pertinent values yields an optimal solution with consequences for the spatial distribution of body parts and, in some cases (depending on processing considerations), their respective damage morphologies. Inconsistencies between predicted and observed behavior or its archaeological consequences indicate that one or more hypotheses about goals, currencies, and constraints is falsified. Alternative hypotheses about these variables and/or their respective values can then be posed, allowing further tests.

Suppose, for the sake of argument, that hominids responsible for these assemblages generally acquired large animal carcasses intact or nearly so, that they did this at some distance from a central place, and that they processed the carcasses with the goal of maximizing the weight of edible tissue carried back to that central place, given pertinent situational constraints. Using ethnographically and/or experimentally derived data on part utilities of potential large animal prey, the costs of butchery with simple tools (i.e., sharp-edged stones), and the resulting changes in utility, one should be able to predict the rank order of parts for transport and their relative frequencies in both acquisition site and central place assemblages. To the degree that part utilities and processing and transport costs differ across potential prey taxa, predictions about rank order may vary accordingly. Hypotheses about the weights individual hominids could carry, the number in a carrying party, and the distance-related costs of transport under various climatic and/or topographic conditions could be used to develop further predictions about the range of parts likely to be found at both acquisition and destination sites. These predictions may also vary across taxa. Many points of comparison with archaeological assemblages are implied, establishing the basis for a critical test.

One might also construct a parallel set of hypotheses and predictions based on the initial assumption that carcasses were generally acquired in heavily ravaged condition by hominids acting as secondary consumers. Knowledge of modern carnivore consumption tactics (e.g., Blumenschine, 1986) could be used to develop estimates of the range, condition, and utilities of parts likely to be available. Experimental data on processing and transport costs associated with handling these parts could be used to predict their treatment, including whether they are likely to be moved at all, in what order, and over what distances. Again, predictions might well vary across taxa. The archaeological implications of this scenario seem likely to be very different from the preceding one.

Clearly this exercise has many potential permutations, depending on the specific hypotheses developed about goals, currencies, constraints, and their respective values. Apart from reinforcing the admonition offered above concerning size of samples required to evaluate arguments about faunal assemblage composition and its determinants, two important implications follow. First, given the variation in situational constraints, only a small subset of the range of carcass treatment practices imaginable need necessarily have an ethnographic correlate. The prospect of learning something really different about past human or hominid behavior becomes a distinct possibility.

Second, to the degree that carcass acquisition tactics are determined by other aspects of behavior, establishing certain patterns in the former may have implications for the latter, some of which may themselves have material consequences, allowing further archaeological tests. For example, carcass acquisition and treatment practices may be partly a function of patterns in foraging range. Inferences about the latter based on the former may lead to predictions about toolstone access and features of lithic assemblage composition. In other words, the same aspect of behavior, foraging range, may be addressed by appeal to more than one archaeological data set. The same line of argument may permit inferences about features of social organization, including those aspects that drew anthropological attention to the characteristics of Plio-Pleistocene faunal assemblages in the first place. Though links among meat acquisition, food sharing, and a sexual division of labor are now seen to be more problematic than initially imagined (e.g., Blurton Jones, 1984; Hawkes, 1990, 1991, 1992, 1993a,b, 1994; Hill and Kaplan, 1993), some such connection seems likely, if only on theoretical grounds. Depending on how it is ultimately understood, it may be that establishing the existence of certain acquisition (and transport?) practices has more or less clear-cut implications for these other phenomena. The process of developing and testing such inferences may not be as simple as it once seemed to be, but in the long run one might well have greater confidence in its outcome.

CONCLUDING REMARKS

Any critique of ethnoarchaeology depends on how one imagines its results are to be applied. Current work emphasizes the production of cautionary tales and conventional analogies. No doubt these are useful for many purposes. My purpose here is not to dismiss them but to call attention to their limitations in pursuit of certain problems, namely that they cannot help reconstruct past patterns in behavior unrepresented in the present or contribute to the explanation of variability in behavior wherever indicated and however inferred. Both of these problems are best appreciated with respect to research in the paleolithic, where continued appeal to conventional ethnographic analogy precludes the possibility of learning how past behavior differs from that in the present, let alone how those differences might be explained. But it is important to emphasize that neither problem is limited to that arena: On the contrary, they are ubiquitous.

I have argued that these problems may be overcome by linking ethnoarchaeology with a general theory of behavior, one that permits an analyst to develop well-warranted, potentially testable hypotheses about variation in past behavior and the factors that may determine it. I have nominated the theory of natural selection and the operational framework known as behavioral ecology as particularly suitable for this endeavor. Over the past 30 years, this approach has proven useful in exploring behavioral variation in many living organisms, including humans. If, as many contend, the archaeological record provides systematic evidence of past human behavior and its evolution, then it should be possible to use this framework, in tandem with ethnoarchaeology, to explore it, at all times and in all places.

This argument is not unprecedented. Others have advanced it in principle (e.g., Grayson, 1993; Simms, 1987, 1992) and, in some cases, taken preliminary steps toward putting it in practice, notably in experimental (rather than strictly ethnoarchaeological) research on prehistoric subsistence and settlement in western North America (e.g., Barlow and Metcalfe, 1993, 1994; Barlow *et al.*, 1993; Brannan, 1992; Jones and Madsen, 1989; Metcalfe and Barlow, 1992; O'Connell *et al.*, 1982; Raven, 1990; Raven and Elston, 1989; Rhode, 1990; Simms, 1985a,b, 1987; Zeanah *et al.*, 1994).

The argument is also similar in some respects to widely cited general statements by Bettinger (1987, 1991a), Binford (1977, 1978a, 1981, 1992), Dunnell (1980, 1982, 1985, 1989, 1992), Foley (1987, 1992), and Gifford-Gonzalez (1991) but differs from each on key points. While this is not the place for an extended comparison, readers who pursue one themselves may note the following. Like Binford and Gifford-Gonzalez, I take ethnoarchaeology to be an essential part of the practice of prehistoric archaeology, a view that Dunnell does not share. On the other hand, like Dunnell, I regard natural selection as an appropriate paradigm for archaeological inquiry, a position anathema to Binford and evidently not entirely comfortable for Gifford-Gonzalez. I depart from Dunnell in appealing to behavioral ecology, a literature he does not cite and an operational framework for which he provides no alternative.

My position is closer to those of Bettinger and Foley. It differs from Bettinger's in that I give greater priority to theoretically driven actualistic research, am more optimistic about the operational utility of behavioral ecology, and correspondingly unpersuaded that "dual-inheritance" models designed to account for the behavioral "peculiarities" of humans are useful, let alone necessary. It is essentially identical to Foley's on a general theoretical level, differing in that it is less concerned with empirical generalizations about the relationship between aspects of ecology and behavior, more concerned with actually exploring that relationship with the use of optimality models.

Some will also note similarities between the approach I advocate and that pursued by Blumenschine and associates on Plio-Pleistocene hominid scavenging (e.g., Blumenschine, 1986, 1987, 1989, 1991, 1993; Blumenschine and Selvaggio, 1988, 1991; Blumenschine *et al.*, 1994; Cavallo and Blumenschine, 1989). There is indeed some overlap, but there are also some significant differences. It is important to be clear about these, particularly in view of Blumenschine and co-workers' (1994) use of the term "behavioral ecology" to describe their work.

As indicated at several points above, appeal to the framework of behavioral ecology typically involves the use of formal optimality models that stipulate goals, the options available to achieve them, and the costs and benefits associated with each option. The overall theoretical framework allows models describing different aspects of behavior (say, foraging and mating) to be linked together analytically. It is assumed, in fact seen as essential, that the models be applied first among living organisms, including modern humans. The results then guide their application in the past.

Blumenschine and associates pursue parts of this strategy but omit key elements. They define aspects of the scavenging opportunities likely to have been available to early hominids, and quantify their respective benefits, but establish none of the associated costs, either those directly connected with scavenging (i.e., search, acquisition, processing, transport) or those incurred indirectly by ignoring other subsistence options, some of which might preclude or significantly inhibit pursuit of scavenging opportunities, however attractive they may seem in isolation. Cost considerations of this kind are basic to the application of an optimality approach (cf. Blurton Jones, 1984; Hawkes *et al.*, 1995; O'Connell *et al.*, 1988a,b, 1990). Nor do Blumenschine and his colleagues systematically consider the links between scavenging and other aspects of hominid behavior, notably mating patterns, food sharing, and reproduction, the phenomena that have consistently stimulated interest in this part of the archaeological record, and in scavenging in particular, over the last 15 years. Nothing they have published so far indicates how such issues might be more effectively addressed as a result of their work on scavenging opportunities. Other research, dealing with different aspects of subsistence from the perspective of behavioral ecology, has begun to do so (cf. Hawkes *et al.*, 1989, 1991). Finally, Blumenschine's work gives little or no attention to the results of ethnographic and ethnoarchaeological investigations (except in cautionary terms), implicitly endorsing the proposition that knowledge of modern humans cannot inform research on the distant past. The approach I favor puts such research at center stage. In short, despite the unambiguous importance of Blumenschine's work on pertinent taphonomic issues, it is different from the "behavioral ecology" I have in mind.

Returning to more general comparisons, I can anticipate great skepticism in some quarters concerning the approach I propose, notably from those who regard science as a fundamentally flawed approach to learning anything about humans (e.g., Shanks and Tilley, 1987), as well as from others who consider comparative analyses, particularly those rooted in biological theory, as insensitive to historical circumstance and the role of culturally constrained human intention (e.g., Schrire, 1980, 1984). One could rehearse the arguments on these and other critical themes but this has been done at length, sometimes quite well, elsewhere. Despite our various differences, Bettinger, Binford, and Dunnell have reacted to such objections in ways with which I generally agree. In any case, the proof lies in the proverbial pudding, in the results that emerge from the actual pursuit of the research trajectory I advocate, which has the distinct advantage associated with any scientific inquiry—it is at least potentially self-correcting. By adopting a single theoretical framework and operationalizing it by means of formal predictive models, analysts who pursue it should be able to determine when their inferences about past behavior are wrong and in what ways they might be adjusted to better fit the empirical record.

Some may contend that while this approach is essential in certain circumstances, it is not necessarily so in all: for example, those in which the archaeology of interest is of recent age and for which there is a direct historical connection with an ethnographic present. Surely, one might argue, analogies are useful here if anywhere: and simpler to develop and apply than the more involved, formal exercise I propose. If the goal were merely to reconstruct the behavior that produced the archaeology in question, one might agree, though one would be forced to observe that both equifinality and ethnographically unanticipated aspects of the archaeology remain potential problems. Both are evident, for example, in recent research on prehistoric Great Basin subsistence and settlement patterns (e.g., Bettinger, 1991b; Thomas, 1988), an arena in which the direct historical approach has long been seen as unusually effective (e.g., Jennings, 1957; Thomas, 1973). Local specialists are now turning to theory to resolve these issues. To the degree that differences between prehistoric and ethnographically observed patterns in behavior can be accurately characterized, the problem then becomes one of accounting for the variation. Even if this is of fairly recent vintage and involves the ancestors of historically known groups, analogy will not explain it. Again, appeal to a theoretical framework capable of accounting for the changes observed becomes essential. Whatever objections one may have to particular applications, the utility of behavioral ecology in this context is now becoming apparent (e.g., Bettinger, 1991b, 1993; Bettinger and Baumhoff, 1982; Broughton and Grayson, 1992; Grayson, 1989; Madsen, 1993; Raven, 1990; Raven and Elston, 1989; Simms, 1985b; Zeanah *et al.*, 1994).

Productive as the approach I advocate may ultimately be, it is important to keep three points in mind while pursuing it. First, despite any contrary impression that may have been conveyed by discussion of specific examples above, neither Darwinian theory nor the operational models used by behavioral ecologists are cookbooks; they do not yield simple, unambiguous predictions about behavior in all circumstances, even in the living world. As indicated at several points, they are better thought of as *frameworks* for the organization of specific hypotheses about goals, currencies, constraints, and their respective values, all of which are simultaneously at risk in any particular test. Pertinent variables are often hard to identify;

critical values difficult to estimate. Even where predictions are met, users must be sensitive to potential equifinalities—different combinations of goals, currencies, constraints, and values with the same or similar behavioral implications—and, where possible, evaluate them (for instructive discussion, see Hawkes, 1990, 1991, 1992, 1993a,b; Hawkes *et al.*, 1991; Hill, 1988; Hill and Kaplan, 1993; Hill *et al.*, 1987; Hurtado and Hill, 1990; Kaplan and Hill, 1992). Users (and critics) must also be clear about the implications of mismatches. Assuming that a model accurately captures the pertinent decision variables, inconsistencies between expected and observed results mean one or more specific hypotheses about goals, currencies, or constraints are incorrect—empirically falsified—and in need of reevaluation. Critics who characterize subsequent adjustments as inappropriate post hoc special pleading misunderstand the enterprise. Models themselves are tautologies. They stipulate what subjects would *necessarily* do *if* indeed they were maximizing the stipulated currency under the hypothesized constraints. Adjusting one's hypotheses in light of experimental results is a fundamental part of any science.

Second, applying models from behavioral ecology to problems in prehistory will not be easy. They were designed to analyze the behavior of individual organisms directly observed in ecological time, not the behavior of aggregates indirectly reflected in material evidence accumulated and incompletely preserved in archaeological time. Key variables will be difficult to monitor, critical values difficult to supply, the effects of various processes affecting the record difficult to disentangle. Analytic results will often be ambiguous. On the other hand, these problems are not unique to this approach. They have complicated systematic attempts to interpret archaeological data in behavioral terms for many years and will continue to do so regardless of how the record is treated simply because of its fundamental nature. Again, the key question is not whether to adopt a theoretical perspective grounded in knowledge of the present as a basis for investigating the past; it is which perspective works best—which one yields interesting, consistent, scientifically coherent answers to important questions about past human behavior. At the moment, the perspective of behavioral ecology looks as promising as any despite its operational constraints.

Third and finally, as with conventional ethnoarchaeology, the groundwork essential to the archaeological application of this approach must be laid in the present, simply because this is the only context in which potentially important determinants of behavior—goals, decisions, currencies, and constraints—can be observed directly. Only after having understood how these interact empirically is one in a position to apply the results archaeologically. There are no shortcuts.

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