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From Bones to Behavior
Ethnoarchaeological and Experimental Contributions to the Interpretation of Faunal Remains

Edited by Jean Hudson

Center for Archaeological Investigations
Southern Illinois University at Carbondale
Occasional Paper No. 21
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Diane Gifford-Gonzalez

Abstract: Ethnoarchaeological research on bones has given considerable attention to field processing and transport decisions and little attention to subsequent subdivision, processing, and discard. Likewise, experimental research has concentrated on primary butchery activities and uncooked bones, seldom addressing culinary processing. Various reasons may exist for this research bias: the atypical nature of faunal acquisition and processing decisions in several widely read studies; assumptions about carcass processing by early hominids, and narrowly construed applications of foraging theory models to modern humans. Another cause is a cultural bias toward seeing immediate prey acquisition as the aspect of hunting behavior most worthy of study, with social subdivision of prey and household processing commensurately undervalued as determinants of patterning in faunal deposits. This correlates with an androcentric view of human foraging. Notable exceptions to those biases indicate the strengths of an end-product-focused approach as a means of balancing actualistic research with bones.

Introduction

Over ten years ago, I had a conversation with Lewis Binford about the Nunamiat hunters he knew, and I was struck by a story he told me. He had asked some Nunamiat men if they usually ate the marrow from metapodials of caribou at the kill site. They asked him whether it was spring (the season of lean caribou) or fall (the season of fat caribou), how far they were from home, and whether their wives were pregnant (Lewis Binford, personal communication 1980).

This story has stayed with me since then, saying something about zoarc
archaeological research that I could not figure out for a long time. In a way, this
essay springs from it. It addresses what I perceive has been missing from
recent ethnarchaeological analyses of butchery and carcass processing. The
gaps alluded to in the title are unstudied topics, and behind them stand
unshedd persons and seldom-mentioned tasks. What is missing is from recent
ethnarchaeological analyses is serious treatment of the culinary end of the
processing spectrum and its major influence on the structure of faunal
assemblages.

My discussion takes two perspectives on this gap. First, I argue here that the
full range of activities that incorporate animal resources into human nutri-
tion—and the material effects of such tasks on bone—are not being suffi-
ciently studied. Specifically, culinary strategies and tactics are intimately tied
to the nutritional benefits derived from faunal resources, they can drive field
butchery decisions, and they determine the patterning of faunal refuse, even
in field situations. Second, I contend that lack of attention to the influence of
cooking and cuisine stems from a general devaluation of the work of women,
and of that of anyone who is not a hunter, in processing the bodies of animals.
This results not in “politically incorrect” research, but methodologically
flawed research, in which a narrow segment of the provisioning spectrum is
privileged for study, while the overall movement of animal bodies through a
subsistence system—and the impact on processing decisions at every stage of
it—is neglected.

I recently (Gifford-Gonzalez 1989a) made a detailed analysis of bone modi-
fications of some modern mammal bones assemblages created by Dassanetch
people on the northeastern shore of Lake Turkana, in Kenya. Dassanetch
people speak a Cushitic language and, when I worked with them, lived in
several territorial sections around the north end of Lake Turkana, practicing a
mix of pastoralism, hunting and fishing, and cultivation. The local landscape
contained a variety of abandoned sites, testifying to several aspects of their
mobile and flexible subsistence strategy. My earlier analyses focused on the
activities that gave the sites their form and produced the artifactual and
dietary debris left behind (Gifford 1977, 1978, 1980; Gifford and Behrensmeyer
1977). My reanalysis compared Dassanetch carcass processing practices with
those documented by other archaeologists and ethnoarchaeologists including
Prison (e.g., 1970, 1974; Prison and Todd 1987), Binford (1978, 1981, 1984),
Bunn and Kroll (1986), Bunn and colleagues (1988), O’Connell and colleagues
(1988), and Guilday and colleagues (1962). I was especially interested in the
effects of exposure to fire on bone breakage patterns, the impacts of cooking
on placement and integrity of cut marks, and the effects of situationally varied
processing equipment on patterns of bone modification among one cultural
group. On all counts, I found virtually no comparative ethnarchaeological
literature, even in subsequent publications (e.g., O’Connell et al. 1990;
O’Connell and Marshall 1989). An exception is Oliver (this volume). In the
process I identified what seem to me to be some serious gaps in the extant
literature on dismemberment, selective transport, and bone breakage.

What Is the Effect of Cooking on Long Bone Fracture?

Analyzing long bone breakage patterns in my Dassanetch assem-
blages, I encountered a gap in the literature on the effects of culinary pro-
cessing. I needed to explain high rates of jagged, transverse fractures on bones
drawn from a Dassanetch pastoral camp. The camp was inhabited for about six
weeks and then abandoned. Four weeks later, I mapped and collected the
bones. About one-third of the long bones from the camp displayed transverse
and jagged break surfaces, as did the same proportion of long bones from other
pastoral camps sampled in the region (Gifford-Gonzalez 1989a).

Such breaks are rare in long bone samples from African Stone Age arche-
ological sites I have analyzed. Illustrations and descriptions of bone fracture in
other ethnarchaeological studies (Binford 1978, 1981; Yellen 1977a, 1977b)
also convinced me that the breakage pattern was as yet unreported in the
literature. Colleagues who worked with paleontological and Stone Age bone
samples questioned whether the long bones actually were fractures of fresh
bone since they had never seen such breaks except on weathered bone in their
assemblages. I discounted weathering as a factor since the bones had been
exposed to the elements for a maximum of 10 weeks and since none of
hundreds of wild animal long bones I have longitudinally monitored in the
same region (Gifford-Gonzalez 1984) displayed such fractures at any stage of
weathering.

The experimental and ethnographic literature on fracture focused on un-
cooked bones (e.g., Binford 1978, 1981; Bonnichsen 1973, 1978, 1979; Bunn
et al. 1988; Davis 1985; Johnson 1985; Mengoni-Goñalons 1980; O’Connell
et al. 1988). Uncooked bones are a logical place to start since they provide a baseline
for understanding properties of bone as a material when subject to dynamic
loading. But the literature’s nearly exclusive emphasis on breakage of un-
cooked long bones presented me with an immediate interpretive problem and
a wider methodological question.

My immediate question was whether sustained heating, in either moist
(bloating) or dry (roasting) media, affects bones’ ability to respond to later
dynamic loading when the bone is cracked for narrow. The answer, I found,
was that at present we do not know because little systematic research has
been done in this area. Shipman and colleagues (1984) established that heating
bone to normal cooking temperature ranges does not create microscopically
detectable alterations in the inorganic component of bone. However, little
work has been done on the effects of cooking on bones’ moisture content or
collagen structure, which might affect their tensile strength. Johnson (1985)
argued that spiral fractures characterize very fresh bone, and more jagged
“horizontal tension failure” is typical of bone that has lost some of its original
moisture through exposure to the air. A voluminous literature exists on the
response of small segments of bone to a material to various stresses (static,
dynamic loading, etc.), but these studies neither aim to mimic cooking nor to
stress whole bones or bone segments (e.g., Amprino 1958; Ascenzi and Bell
1972; Bonfield 1981, 1987; Bonfield and Li 1966; Simkin and Roblin 1974).
A few articles exist on alterations to bone collagen fibrils at temperature ranges that replicate cooking (e.g., Bonar and Glimcher 1970; Richter 1986; Seldin 1965). Since collagen gives bone its resiliency under stress, the alterations are especially critical to the question of whether cooking can make bones break differently after cooking. The studies cited indicate that collagen fibers liquefy with heating and then reaggregate structurally with cooling. The reaggregated fibers may be shorter than the original, and full bone resiliency is not recovered. The results suggest that cooked bones may respond differently to dynamic loading than uncooked ones. Experimental breakage studies by Horwitz (1987) and Black (1989) suggest this may be the case, but more controlled experimentation is needed. In fact, Oliver’s chapter in this volume reports jagged, transverse diaphysis fractures on long bones that were exposed directly to fire by Hadza processors prior to breakage for marrow extraction. They are virtually identical to those in the Dassanetch assemblages, supporting the sequence of processing events originally proposed in my analysis (Gifford-Gonzalez 1989a).

Another recent example of this is the work of Richter, Horwitz, and Black, none of the investigations were presented by archaeologists. In fact, the situation could be seen as even bleaker since Black and Horwitz were in part motivated to do their studies after hearing the expiation in a zooarchaeology course on the lack of experimental data on the handling of cooked bones. One must comb the materials science and biological literature to find these citations.

Raw, Cooked, and Cut: Omitted Distinctions

Recent discussions of dismemberment versus filleting marks rest on the presumption that flesh is normally cut off when bone segments are fresh and uncooked. Average frequencies of occurrence of cut marks on bones processed by modern humans have been the basis of arguments about early hominid subsistence (e.g., Bunn and Kroll 1986; Shipman 1986a). But ethnographic cases suggest to many instances of cooking meat with the bones left in, as roasts, in stews, or baked or steamed in substranean “ovens,” with flesh removal after cooking (e.g., Catlin 1959; Wissler 1910). Because meat is easier to remove from bones after it is cooked, one could hypothesize that filleting of uncooked bones imposes time and energy costs that must be balanced against potential benefits of reducing the weight of tissue transported away from the acquisition site. For groups killing only one large animal or a few large animals at a time, transport costs may not be so crucial a consideration, and limb segments would have been transported to sites of consumption with bones included. Were marrow bones of meat-rich body segments transported and cooked in joints of meat, marrow extraction might be deferred until “dessert.”

More knowledgeable reconstruction of prehistoric processing patterns requires better ethnographic documentation of contrasts between cut mark patterns on bones cooked with the meat on and raw meat removal marks.

There is a need as well for extensive evaluation of the nutritional benefits and energetic costs of various cooking and other culinary processing techniques and technology (see Emerson, this volume).
bones into cook pots and to liborate fat, marrow, and blood (e.g., Binford 1978; Yellen 1977b). Among such groups, pots may be absent from hunting stands or other sites created by hunting parties, leading to different processing and damage patterns on bone assemblages produced by one group of people, in functionally disparate sites. I have outlined the differing impacts of situationally variable processing gear on Dusseanetch mammal bone assemblages (Gifford-Gonzalez 1989a) and will not elaborate on this important consideration here.

Among people who store animal products, the form of the products drives field butchery activities. Ethnographically documented muscle-stripping tactics at mass bison kills by historic Plains Indians (Prison 1970, 1974; Frison et al. 1976; Frison and Todd 1987; Johnson 1978; Wheat 1972) were aimed at quickly removing the most readily transported meat units from as many animals as possible. The overriding consideration was to dry flesh for transport and storage, sometimes augmented by extraction and processing of fat (Densmore 1916; Speth and Spielmann 1983). These culinary goals produce distinctive patterns of bone modification and discard at kill/butchery sites (e.g., Frison 1970; Frison and Todd 1987). Binford (1978) reports another pattern of mass-kill butchery among Nunavut caribou hunters. Instead of extensive muscle lifting, selective flesh removal, situationally tuned dismemberment, and transport of higher-utility, bone-bearing body segments to secondary cachers or residential camps are the norm.

As discussed by others in this volume, patterns of wholesale extraction of edible tissues and discard of bone at mass kill sites differ from those reported for hunting peoples who kill fewer large animals at a time and at a steadier rate over the year. The ethnographic literature suggests that bones are more rarely discarded in the field when animals are encountered on a one-by-one basis than when encountered in substantial numbers (e.g., Bunn et al. 1988; Marshall, this volume; O'Connell et al. 1988, 1990; Yellen 1977b). To take an archaeological example, processing marks on large ungulate bones at the Eschelman site, a residential settlement into which single deer and elk entered over a span of decades, reflect substantially different patterns of limb segmentation and muscle removal than do bones of like-sized animals at mass processing sites (Gilday et al. 1962).

Despite these indications of the role of culinary goals and technology in structuring faunal assemblages, recent ethnoarchaeological literature has largely noted them as addenda to discussions of field processing. To use a faunal metaphor, this approach may be putting the cart before the horse. When I considered this fact in combination with the scanty research on the influence of cooking on bone breakage and cut mark patterns, I was led to ask why the effects of culinary processing on bone had not been deemed a suitable research topic by archaeologists.

Why Has Culinary Processing Been Ignored?

Reasons for the research emphasis on uncooked bone are several. First, centrally influential works on butchery, such as Prison's Great Plains research and Binford's Nunamuit project, focus on mass kill assemblages. Simultaneous kills of many animals create a kind of "mass production" atmosphere, in which considerations of spoilage, transport, and storage interact to drive butchery in an efficiency-focused direction. Recent Hadza research (Bunn et al. 1988; O'Connell et al. 1988, 1990), as well as Yellen's (1977b) earlier work, indicate that single-animal processing may not parallel the starkly repetitive, cost-benefit, discard and modification patterns typical of mass kill situations. Mass kill situations also heighten the likelihood that meat will be removed prior to cooking, that long bones will be fractured raw, if they are fractured at all, and that bones will be discarded on-site, without cooking. These cases may not adequately direct our attention to the circumstances and strategies forming most archaeological bone deposits.

The early hominid focus of much actualistic research also contributes to the concentration on uncooked body segment processing. Many ethnoarchaeological researchers want to apply their findings to interpreting bone assemblages in Pleistocene archaeological sites (e.g., Binford 1981, 1986; Bunn 1981, 1983, 1986; Bunn and Kroll 1986; Bunn et al. 1988, 1990). These who use experimental research on butchery marks have similar goals (e.g., Shipman 1981, 1986a, 1986b, 1989; Shipman and Rose 1983). Whether of the pro-butchery or pro-scavenging camp, those researchers share the assumption that their target populations lacked fire and—it seems implicit in their writings—subjected animal products to minimal processing once they were extracted from carcasses. Early hominids' transporting carcass segments or objects away from a carcass is considered likely (e.g., Binford 1984; Bunn and Kroll 1986), although different researchers say early hominids were differently motivated in distancing themselves from a large animal death site (the cowering scavenger hotfooting it for a safe haven versus the efficient hunter strolling home with the choicest cut of a kill). No one, to my knowledge, explicitly discussed what tactics early hominids might have employed to process animal foods, past an implicit "smash and/or slash and gulp" scenario.

However, I believe they are not the only reasons why attention is directed away from cooked bone and from the culinary end products of animal butchery. Here I shift focus from the internal logic of zoarchaeological practice to examining the structure of assumptions and the standards of research in zoarchaeology. Specifically, I want to examine the role of gender bias in differentially valuing certain types of research questions.

Zoarchaeological Research as Gendered Practice

The lack of attention to cooking and culinary end products in zoarchaeology is, I believe, attributable to unconscious androcentric bias within the field. Despite having undergone tremendous methodological ferment since the early 1970s, the subjects of study and debate in zoarchaeology have been chosen according to an unwritten valuation of what researchers see as important activities. This view favors hunting—especially male pursuit,
dispatch, and butchery of prey—over just about any other activity involving animals, even the supposedly patriarchal practice of pastoralism. The paradigmatic emphasis at least implicitly characterizes large animal hunting as the central part of past human adaptations, usually ignoring the role of food-processing technology and of the social allocation of food resources as pivotal adaptive issues. It characterizes field butchery and transport as male activity and central to understanding “adaptation,” while secondary processing—storage or culinary—is less interesting, less central to adaptation, and often “left to” marginally noted females, if discussed at all.

But it is even worse than that. Not even all men are deemed worthy of study in this context. The economic roles of nonhunting males and children of either sex are downplayed in ethnographic accounts of hunting, except insofar as they constitute part of the carrying party hunters mobilize to get their prey to residential camps. Nonhunter processing inputs and impacts on bone are not included in discussions of either energetic costs incurred or nutritional benefits reaped by such secondary processing of animal products, or treatments of the form of debris that hits the ground. I contend that they are seriously incomplete accounts of both hominin tool-mediated subsistence and the activities that form archaeological sites.

Concentration of carcass processing research on the pre-culinary, field phase reflects, I believe, the “male equals public, female equals private” dichotomy of our own culture, in which the really important decisions in life are seen as going on outside the sphere of women and home (Coller and Yangisako 1987; Conkey and Spector 1984; Rosaldo 1980; Strathern 1988). In fact, the dichotomy may actually be reproduced in the “Man the Hunter/Woman the Gatherer” stereotypes now common in the general anthropological literature. While it has served a useful role in documenting human females as active and productive foragers (e.g., Tanner 1981; Tanner and Zihlman 1976), the Woman the Gatherer concept can reify our own culture’s ideas about the division of labor between men and women. Specifically, it blinds us to the involvement of women and nonhunting personnel in tasks that seek, acquire, and process animal foods and other animal products. I contend that they are seriously incomplete accounts both of hominin tool-mediated subsistence and of processes that form archaeological sites. To the extent that we underplay certain processing tasks because we think of them as routine, simple, passive, or fundamentally unproductive (in our culture’s gender paradigm, female), we undermine our understandings of processes forming the archaeological record on a regional scale and so undermine the effective practice of zooarchaeology.

I want to make clear that I do not dispute that among modern peoples, most of the time, it is men rather than women who usually stalk and kill large animals and that it is reasonable on selective grounds to assume a strong tendency for males to have done so in the past. Rather, the purpose of my inquiry is to delineate the unrecognized shape of our discipline, as a conversation in which we all—men and women and perhaps and take to be our common knowledge. Let us explore some implications for zooarchaeological methodology. I am concerned that the implicit biases produce less useful find-

ings because our attention is directed away from an important source of patterning in archaeological materials.

Specifically, how does this bias negatively impact ethnoarchaeological studies of animal processors? Gaps are created in two areas. First, and perhaps most superficially, is the a priori assumption that women and non-hunting personnel are not themselves active field butchers whose decisions affect the ultimate form of the animal food base and its by-products. The ethnographic literature abounds with examples of women as major actors in helping acquire large animals (Mason 1987), in field processing large animals for storage or consumption (Demallie 1983; Denig 1930; Dodge 1959; Mackenzie 1960; Medicine 1983; Weist 1983), in transporting them (Weist 1983; Wheat 1972), in extracting bulk carcass products such as marrow, bone grease, and bone juice (Binford 1978; Fletcher and La Flesche 1911), and in managing stored products (Binford 1978). Women, from ethnographic accounts, can be up to their elbows in large animal carcasses. Given that men and women, even of the same household, may have divergent agendas in the use of animal products, women’s decisions and their consequences need to be monitored.

Second, and more seriously, recent discussions (Binford 1986; Bunn and Kroll 1986; Bunn et al. 1988; O’Connell et al. 1986, 1990) appear to assume that field butchery decisions are primarily driven by the particular circumstances of prey acquisition in the field (the first six factors listed earlier) rather than by decisions and strategies grounded in the overall nutritional, technological, and social context in which hunters live. The assumption that hunters approach their field butchery tasks without such end-product-based considerations in mind is, I believe, problematic. Ethnoarchaeological research based on selectionist theory might be expected to take a more comprehensive approach to meat provisioning but in fact has not. Some recent ethnography in the optimal foraging framework has focused on women and children (e.g., Blumloff Jones et al. 1989; Hawkes et al. 1989), as is reasonable, given the theoretical underpinnings of this approach. Although the costs and benefits of the entire chain of animal processing would seem to require study as well, studies by O’Connell and colleagues (e.g., 1988, 1990) have thus far omitted from their cost-benefit calculations the time and energy invested in secondary, culinary tasks—and the nutritional benefits of such processing. Nor have the impacts of such processing on actual on-the-ground element frequencies been assessed.

A behavioral ecological approach may indeed be a better means of discovering uniformitarian relationships with which to understand archaeological cases than less theory-informed, ad hoc investigations. But I believe their scope can be expanded beyond what is apparent in published studies. Modern humans are unique in the degree that social cooperation supports individual survival and reproduction. To evaluate human prey processing inputs as simply analogous to those of non-tool-using, less social predators—that is, in a primarily kill-site-based perspective—is to ignore major factors that structure bone assemblages among modern hunting hominids. Even if one’s goal is to discern uniformitarian relationships with which to study
earlier hominid forms, whose carcass processing strategies may have been closer to those of nonhuman carnivores, it is essential to define the "modern" impacts that should be lacking in earlier by-products.

The question, which needs to be addressed ethnographically, is how much do nonhunters' agendas, either home-economic or reproductive, structure hunter's actions in the field? Binford's story about the hunters and their wives led me to reflect on the possibility that, beyond the many factors enumerated in the literature on carcass processing, we need to consider the imperatives of the household in driving field processing decisions. Men and women in households engage in common and basic projects of feeding themselves, rearing and feeding children, supplying dependent elders with shelter and sustenance, and developing their social lives according to their respective goals. Given that hunting is largely aimed at satisfying such needs, we might, as a radical alternative, envision it as an extension of home economics, in which verbal and nonverbal information from all members of households heavily influences hunting and field butchery decisions. These are empirically investigatable issues, accessible to various established forms of systematic analysis.

The problem with recent studies' focus—whatever their theoretical grounding—may really be that they have been overly determined by simplistic archaeological visions and versions of the past, including a nearly fetishistic obsession with the search and pursuit phases of hunting. It would be a waste of human effort if we only used our ethnographic research to flesh out a priori (and often profoundly ethnocentric) ideas about the sexual division of labor—and the process of food-getting itself—rather than seeing what is really going on in our study cases. I am not advocating a purely inductive approach, but rather one in which research is a continual confrontation of analytical models and concepts with empirical evidence, with modification of the former in light of the latter when necessary.

What Are Productive Research Approaches?

The ethnoarchaeology of human subsistence profits when it takes a task- and product-focused approach. This approach envisions each element in a faunal assemblage as an end product of the chain of events through which it has passed (Figures 11-1 through 11-3). From this perspective, it is important to monitor processing impacts and their benefits and costs at all phases of the chain. All phases of animal processing, from acquisition through final refuse disposal, are studied. Relationships between processing decisions and tactics and their material consequences are isolated at each phase. A task-focused and product-focused approach to faunal materials in ethnographic settings thus asks how sites and their contents form. I advocated this approach from a taphonomic perspective ten years ago, as have Binford (1981) and Lyman (1987), and Behrensmeyer and Kidwell (1985) in paleontology. A task-focused approach was also advocated by Conkey and Spector (1984) as a means of highlighting the social roles involved in forming archaeological sites. In this approach, assemblages are viewed not as "biased" remnants of extinct systems but as aggregate evidence about the past states of those systems. A product-focused research strategy necessarily attends to the tasks affecting faunal remains—including storage, cooking, and refuse disposal practices—and the practitioners, whoever they are, whether they act "at home" or in the field.

Somewhat paradoxically, the strategy does not require paying special attention to women. It just involves not ignoring them and their—and other nonhunters'—participation in processing chains and tasks that create potential archaeological sites. Ethnographic cases suggest that participation of
different age and sex classes in animal processing activities varies tremendously, both situationally within a culture and from one culture to another. Our task is really to open our eyes to the impacts of such variability.

One researcher who has taken a product-focused approach is Binford, in his Nunavut research (e.g., 1978, 1981). It meant that he actually spent considerable time observing women's work. I doubt that Binford did it out of a feminist agenda—in fact, he too emphasizes male actions and the decisions underlying them. Binford's orientation is, rather, a logical outcome of his persistent concern with the archaeological products of human actions. In other words, in order to follow bones to their final resting places, as well as to understand butchery decisions, Binford needed to pay attention to women's—and other nonhunters'—work.

Figure 11-2. The same model, showing animal processing activities likely to occur at each locality type.

Figure 11-3. The same model, showing bone modifications and discards likely to occur at each locality type.

Conclusion

My thesis is that our own culture's gender paradigms have led us to develop less than comprehensive approaches to human actions affecting faunal remains. I have not argued here that it is easy—or even that it is necessarily possible—to "dig up gender" or other social aspects of prehistoric humans' social lives, not, at least, relying on bones alone. The bones we excavate may be products of gendered actions, but what we can reasonably expect to access—short of uniformitarian flights of fancy making all men and all women the same in all places for all times—is the indirect evidence of hominid decisions and actions affecting animals' remains. We may be able to combine faunal data with other, independent lines of evidence to ask about the activities of different ages and genders in that past system (Comakov and
Spector 1984; Gifford-Gonzalez 1991). But to do this we must first expand our understanding of the linkages between human decisions and actions and their products. That is what should be studied ethnoarchaeologically.

We zooarchaeologists have had great success pursuing a research program based on a uniformitarian approach to faunal materials as an entry point to the unknown terrain of ancient human life. I do not dispute this methodological stance since I fully support it (Gifford 1981; Gifford-Gonzalez 1989b, 1991). But I think we are not using ethnographic cases to their full potential. Ethnoarchaeology is important not only because it lets us see the dynamic processes that produce archaeological sites and assemblages. It is also crucial because it and other actualistic studies in archaeology often lead us to question the realism of our research questions and analytical categories (viz., Binford 1987; Wylie 1989). We use actualistic research to our fullest advantage when it informs and re-forms our ideas about what might have happened in the past, rather than simply using it to flesh out our pre-existing ideas about it.

To study the use of animal resources in a more realistic way requires a wider scope and attention to the influence and impacts of non-hunting household members. To attend to these aspects of animal-based subsistence and its archaeological consequences is not to force gender on our analytical approaches; rather, it is to ungender categories that up to now have been falsely gendered.

**Note**

1. Analysts dealing with later prehistoric Mediterranean and European faunal assemblages have emphasized cooking techniques more than have most North American researchers. It probably stems from their concentration on food-producing economies in which houses and food-processing and distribution methods are central objects of study. However, as I will discuss later in this paper, I suspect that hunting and herding are studied according to androcentric models within Europeanist research paradigms as well.

**Acknowledgments**

A preliminary version of this paper was drafted during tenure of a National Science Foundation Research Opportunities for Women Award, BNS-8711024. I am grateful to Jean Hudson for inviting me to participate in the From Bones to Behavior conference. I thank Carolyn Clark, Shelly Errington, Joan Gero, Donald Grayson, Lee Lyman, and Anna Tsing for their valuable comments on drafts of this manuscript.

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