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From Bones to Behavior

Ethnoarchaeological and Experimental Contributions to the Interpretation of Faunal Remains

Edited by
Jean Hudson

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11. Gaps in Zooarchaeological Analyses of Butchery: Is Gender an Issue?

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 Nunamiut hunters he knew, and I was struck by a story he told me. He had asked some Nunamiut 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).

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This story has stayed with me since then, saying something about zooarchaeological 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 ethnoarchaeological analyses of butchery and carcass processing. The gaps alluded to in the title are unstudied topics, and behind them stand unheeded persons and seldom-mentioned tasks. What is missing from recent ethnoarchaeological 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 nutrition—and the material effects of such tasks on bone—are not being sufficiently 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 slighted.

I recently (Gifford-Gonzalez 1989a) made a detailed analysis of bone modifications 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 Frison (e.g., 1970, 1974; Frison 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 intensity 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 ethnoarchaeological 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 assemblages, I encountered a gap in the literature on the effects of culinary processing. I needed to explain high rates of jagged, transverse fractures on bones 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 archaeological sites I have analyzed. Illustrations and descriptions of bone fracture in other ethnoarchaeological 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 uncooked 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 uncooked long bones presented me with an immediate interpretive problem and a wider methodological question.

My immediate question was whether sustained heating, in either moist (boiling) or dry (roasting) media, affects bones' ability to respond to later dynamic loading when the bone is cracked for marrow. 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 as 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 Robin 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; Sedlin 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 liquify 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)

With the exception of 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 me expatiate 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.[1]

Raw, Cooked, and Cut: Omitted Distinctions

Recent discussions of dismemberment versus filleting marks rest on the presumption that flesh is normally cut off when body 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 attest to many instances of cooking meat with the bones left in, as roasts, in stews, or baked or steamed in subterranean "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).

Butchery and Transport Decisions: Underreported Aspects

Literature on determinants of field butchery and transport (Binford 1978, 1981, 1984; Bunn and Kroll 1986; Bunn et al. 1988; O'Connell et al. 1988, 1990; O'Connell and Marshall 1989; Yellen 1977a) has focused on such factors as

1. size of the animal, relative to that of the human processors;
2. number of animals requiring immediate processing;
3. distance of the animal from the destination of its products;
4. number of persons in carrying party;
5. condition of the carcass at the time encountered; and
6. time of day.

I have argued (Gifford-Gonzalez 1989a), as have others in this volume, that an animal's size, anatomical structure, and nutritional composition appear to be *constraining* conditions or outer parameters within which a variety of dismemberment and defleshing tactics and strategies may successfully be employed, according to such considerations. Ethnographic literature indicates that factors additional to those noted above may be critical in determining tactics used:

7. gear at hand to effect field processing;
8. processing technology available at destination site; and
9. ultimate form or forms the animal products will take.

Those factors have been given less attention than they deserve, and the last two especially merit discussion.

Culinary techniques and technology are intimately related to rates of bone transport. How an animal is disjointed and filleted depends on whether a butcher aims to produce joints of meat to roast on a fire, segments of bones and flesh to boil in a pot, boneless cuts to be sliced and dried as jerky, or manageable and quickly frozen segments for winter storage. Ethnoarchaeological accounts by Binford (1978) for the Nunamiut, Bunn and colleagues (1988) and O'Connell and colleagues (1988) for the Hadza, and Yellen for the Dobe !Kung (1977b) indicate that kill-site butchery decisions and tactics are influenced by expectations about how the meat, marrow, and other useful parts of the dead animals will be stored or processed for consumption. For example, both Bunn and colleagues (1988) and Yellen (1977b) note special handling of carcass segments headed for boiling in pots. Bunn and colleagues (1988) in fact contend that the fundamental decision to transport larger animals' axial segments is predicated on the existence of a pot-boiling extractive technology.

Likewise, the ultimate form of the bone debris at residential or short-term camp sites is determined by cooking technology. Hadza, !Kung, Dassanetch, Nunamiut, and other documented modern groups all have metal or ceramic pots as part of their culinary equipment, and meat is often prepared in stews with this technology. Investigators report a range of hearthside chopping and cracking of bone and bone units prior to cooking, both to facilitate fitting the

bones into cook pots and to liberate 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 Dassanetch 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 (Frison 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 1918; 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 Nunamiut caribou hunters. Instead of extensive muscle lifting, selective flesh removal, situationally tuned dismemberment, and transport of higher-utility, bone-bearing body segments to secondary caches or residential camps are the norm.

As discussed by others in this volume, patterns of wholesale extraction of edible tissue and/or 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 (Guilday et al. 1962).

Despite those indications of the role of culinary goals and technology in structuring faunal assemblages, recent ethnoarchaeological literature has largely noted them as asides 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 Frison's Great Plains

research and Binford's Nunamiut 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 Plio-Pleistocene archaeological sites (e.g., Binford 1981, 1986; Bunn 1981, 1983, 1986; Bunn and Kroll 1986; Bunn et al. 1988; O'Connell et al. 1988, 1990). Those 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-hunting 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 products 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 cuts of a kill). No one has, 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 zooarchaeological practice to examining the structure of assumptions and the standards of research in zooarchaeology. Specifically, I want to examine the role of gender bias in differentially valuing certain types of research questions.

Zooarchaeological Research as Gendered Practice

The lack of attention to cooking and culinary end products in zooarchaeology 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 zooarchaeology have been chosen according to an underlying 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 hominid 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 (Collier and Yanagisako 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 hominid 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—participate 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 findings 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 nonhunting personnel are not themselves active field butchers whose decisions affect the ultimate form of the animal food base and its bony refuse. The ethnographic literature abounds with examples of women as major actors in helping acquire large animals (Mason 1907), 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. 1988, 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., Blurton 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

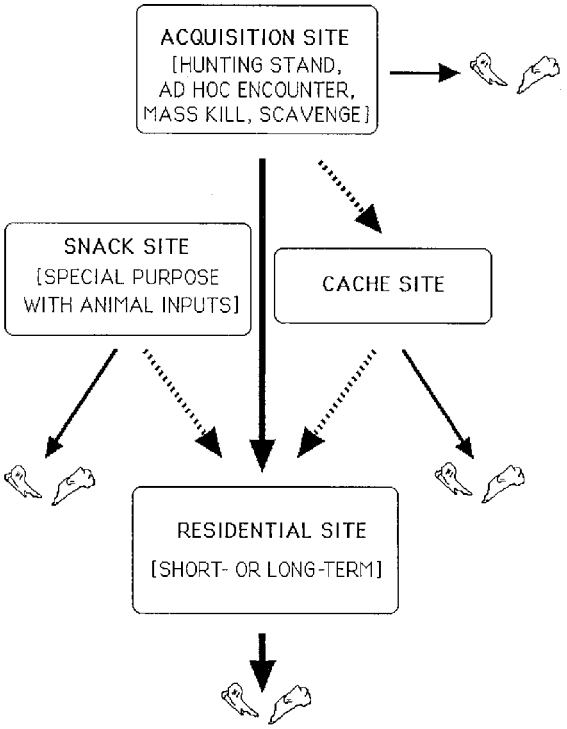

Figure 11-1. *A visual model of the movement of faunal remains through various sites in a subsistence system that provisions meat through large game hunting, specifying several site types at which animal carcasses may be processed.*

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

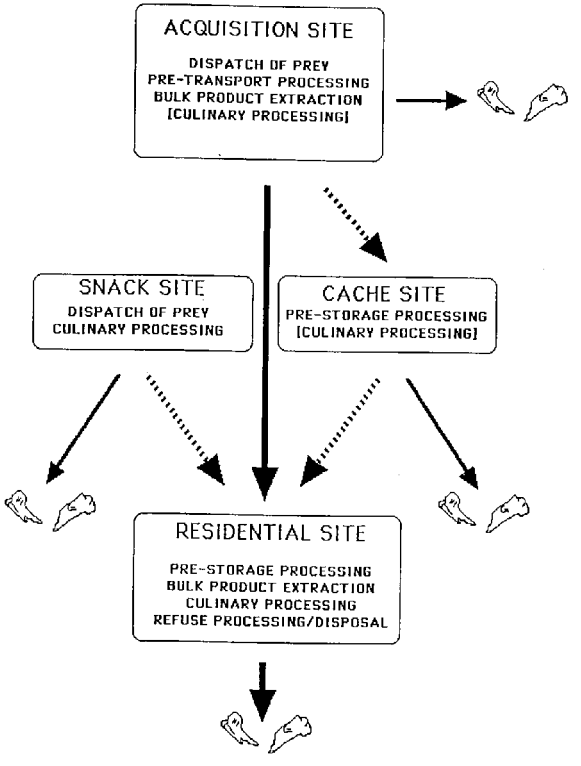

Figure 11-2. *The same model, showing animal processing activities likely to occur at each locality type.*

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 Nunamiut 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.

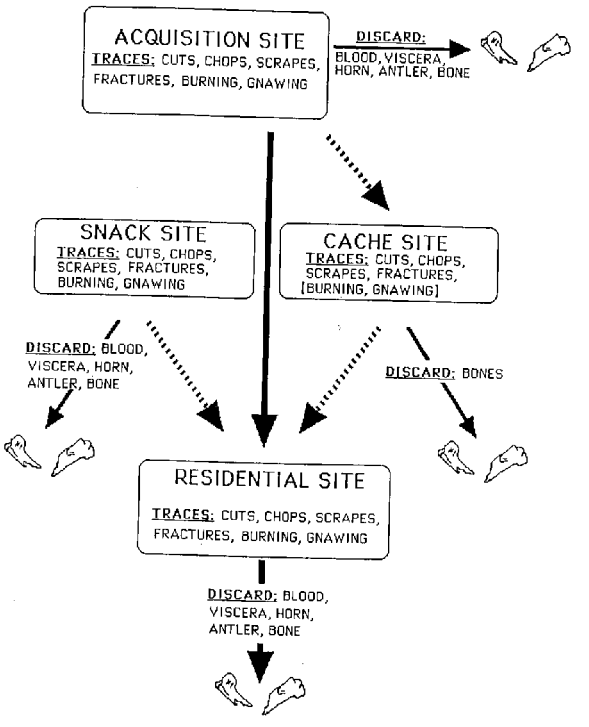

Figure 11-3. *The same model, showing bone modifications and discards likely to occur at each locality type.*

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 (Conkey 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 hominid 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 1977; 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 preexisting 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 nonhunting 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.

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