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Human Ancestors: Changing Views of Their Behavior

LEWIS R. BINFORD

Department of Anthropology, University of New Mexico, Albuquerque, New Mexico 87131

INTRODUCTION

I think it is safe to say that during the latter years of the 1970s there developed in paleoanthropology a kind of "consensus" view of our hominid ancestors. This view was largely developed by Glynn Isaac (1971, 1978a, 1978b, 1983c), then of the University of California at Berkeley, and popularized in many articles and in several books aimed at lay audiences authored or coauthored by Richard Leakey (Leakey and Lewin 1977, 1978; Leakey 1981). Fundamental to this construction of our ancient past was the view that early man was a hunter. It was admitted that the early beginnings of mankind were impoverished relative to the achievements of later men; nevertheless, the history of the emergence of our modern condition was thought to be a story of progress. It was a history of gradual and accretional accomplishment, given the original presence of certain fundamental "human" characteristics. Isaac argued in a seemingly convincing manner that, at the very dawn of our appearance as tool-using hominids, "men" were hunters living in social groups characterized by a male-female division of labor. The products of the hunt were returned to sleeping places (home bases) in which altruistic sharing took place among adults as well as provisioning of children. Crucial to this contruction of early man's lifeway was Isaac's belief that food sharing was a major conditioner for many of the "essentially" human characteristics that he believed to have been already present at roughly the Plio-Pleistocene boundary (Isaac 1971, 1976, 1978a, 1978b; Isaac and Crader 1981). Food sharing thus was believed to have set the stage for much of the progressive emergence that seemed to characterize the later evolution of mankind.

FORESHADOWING OF CHALLENGE—SOME SKEPTICISM

During the time that this "consensus" view of early man was being formed I had been conducting research for purposes of developing a meth-

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odology for making behavioral inferences from animal bones believed to represent archaeological traces of ancient human–hominid behaviors (Binford and Bertram 1977; Binford 1978). During the course of this research the potential of the application of my nascent methods to the questions of the nature of ancient man's behavior appeared obvious. As a result of this interest I began to read more critically the literature dealing with the factual basis for the consensus view and to examine the rational basis for the inferences then being made, particularly by Africanists. My first excursion into this field of argument came in my review (Binford 1977) of Glynn Isaac's (1977) monograph on the important early site of Olorgesailie. I noted that, while Isaac was an innovator in considering the integrity of deposits yielding traces of early man, he never questioned that the *associations* among the items found in such modified deposits were all indicative of hominid behavior. He simply accepted the conventional "wisdom" that they were present because hominids had caused the association. This reinforced Isaac's beliefs that stone tools associated with animal bones were evidence of hunting. This permitted, even required, that he speculate on the hunting techniques used by ancient hominids to account for the extraordinary association between stone tools and the remains of a large number of now-extinct baboons found at the Olorgesailie site of DE/89B.

> A killing pattern such as is observed . . . might have been achieved if the baboons had been driven against a hazard or if their waterhole had been poisoned. But driving baboons or geladas against a hazard is almost inconceivable, since the species shows such guile and agility. Poisoning of waterholes, while a possible explanation for the killing pattern, would have resulted in a much broader spectrum of species in the refuse.
>
> An ethnographic analogy provides for a plausible . . . reconstruction. . . . [During] a communal baboon hunt . . . [the hunters] encircle the roosting place of a baboon troop in a grove of trees or on a small rock outcrop . . . [and] dislodge the baboons by . . . making a great noise. As the baboons try to break out of the circle they are clubbed to death (Isaac 1977:91).

I questioned that Isaac's acceptance of the facts of assemblage composition and faunal association were directly referable to past hominid behavior.

> I have said many times that one cannot reconstruct the past in ignorance of the processes which operated to bring the archaeological facts into being. . . . *A frontal attack is now needed on assemblage composition and tool–faunal associations for the earlier ranges of time. We need to know more of the formation processes for the archaeological record* (Binford 1977:203).

This statement, simply put, means that I did not think that archaeologists knew how to diagnose the different processes that could stand behind the early deposits that also yield evidence of hominids. What is most interesting is that during the next several years many different researchers were engaged in activities largely designed to address these very issues.

THE ACCOMMODATION OF NEW EVIDENCE

1980 and 1981 were big years in the literature of paleoanthropology. The results of the previous several years of focused research from Isaac's team were reported in *World Archaeology* (Bunn et al. 1980). In that report discussion was focused on the important site of Koobi Fora, FxJj50. This article reported pioneering work on faunal analysis, particularly concerning inspection for cut marks and breakage patterns that might be cited to link the fauna behaviorally to acts of hominid tool use. Site structural studies, designed to illustrate the spatial patterning in tool and faunal remains, were also reported. While clearly showing concern for inference justification, Isaac's team nevertheless drew the following conclusions:

> Many of our observations will have relevance for testing hypotheses about the nature of early hominid behavior. . . . Suffice it to say that the concentrations of artefacts and bones, the cut marks and the conjoining sets are all consistent with (but not final proof of) interpretations that attribute meat-eating and food transporting activities to the early tool makers. . . . Further, the characteristics of the bone assemblage invite serious consideration of scavenging rather than active hunting as a prominent mode of meat acquisition (Bunn et al. 1980:133).

In other words, the facts from Koobi Fora were cited to justify the continued defense of the consensus view of early man—with the caution, however, that scavenging might have been a mode of meat procurement. In spite of this insight, the site was seen as a "base camp" where the behaviors thought to be critical to the emergence of many of our basic human traits, such as sharing, regular meat eating, and perhaps early "language," were considered to have been present.

Taking up my challenge to the hunting interpretations rendered by Isaac for the Olorgesailie site of DE/89B, Pat Shipman reported her conclusions gleaned from a restudy of the DE/89B fauna (Shipman et al. 1981). In this publication the identification of a "distinctive breakage pattern" was used to justify the conclusion that butchering and probably hunting had occurred.

The Olorgesailie materials were not the only target of Shipman's investigations. Roger Lewin (1981a:211) reports on the events at the annual meeting of the American Association for the Advancement of Science (January 3–8, 1981). Lewin cites reports by Alan Walker on observations made in independent studies by Shipman and Richard Potts regarding

> "cut marks superimposed on marks made by carnivore teeth." This is the kind of incontrovertible evidence for prehuman scavenging that paleoanthropologists will

> welcome enthusiastically. "They've also found extensive cut marks on fossil horse limbs from Olduvai at a point where virtually no meat would be present, only tendons and skin," he added. "Either these creatures weren't smart enough to know where the meat was, which is unlikely, or they were interested in something other than meat. One can only speculate what they were after" (Lewin 1981a:372–373).[1,*]

In another review article in *Science,* Roger Lewin (1981b) summarizes conclusions from the previously published articles by Potts and Shipman (1981:577) and Bunn (1981:574) in *Nature.* Lewin comments,

> Two intriguing aspects of interpretation emerge from Potts and Shipman's work. The first concerns possible competition between early hominids and carnivores. The second relates to the uses to which protohumans put animal products.

Then Lewin summarizes accurately the thinking of the researchers on these subject as follows:

> Clearly, the hominids sometimes had first access to a carcass, and sometimes they followed carnivores. This is good evidence that at least some of the hominids' meat-eating was the result of scavenging in competition with carnivores rather than direct hunting. . . .
>
> There is apparently some indication that skin and ligaments from animals were important products for our ancestors. . . .
>
> It has frequently been stated that one of the earliest technological inventions made by our ancestors must have been some form of carrier bag in which to transport collected plant foods, though evidence for such receptacles is virtually nonexistent. It is intriguing to speculate that some of these cutmarks do in fact constitute such evidence, indirect though it is (Lewin 1981b:124).

My review of the major points made in literature through the summer of 1981 points out several things.

(1) Isaac is the first (in the sequence of current arguments) to suggest that there is evidence from early time periods perhaps indicative of scavenging.

(2) Later, the observation of tool marks overprinted on carnivore tooth marks is cited by Potts and Shipman (1981) and emphasized by Lewin (1981b) as being further evidence for scavenging. In addition, however, it is noted that tool-marked bones are commonly those from the lower legs of ungulates. This bias is interpreted as arising from the processing of tendons for the manufacture of carrying bags (Lewin 1981b).

(3) Finally, an additional idea appears in the literature, namely "that the hominids and carnivores were competing for carcasses or bones, perhaps to obtain different substances" (Potts and Shipman 1981).

While these arguments were developing regarding the Lower Paleolithic materials, Shipman was staunchly defending hominid hunting of fierce animals, perhaps in a ritual context, as an explanation for the data from Olorgesailie (see Shipman et al. 1981, 1982; Shipman 1983).[2]

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* See Notes section at end of paper for all footnotes.

Thus, by the summer of 1981 scavenging had been suggested by Isaac and inferentially supported by the observations of Potts and Shipman (1981) of cut marks overprinted on carnivore gnawing. At no point, however, had the "consensus" view been basically challenged. It was still maintained that "meat eating" was important, thereby defending the argument emphasizing the importance of such behavior for a division of labor and the "sharing" hypothesis (Bunn 1981). Shipman appears also to support the consensus view with her claims for the hunting of giant geladas at Olorgesailie. She stresses the "human" appearance of mid-Pleistocene hominid behavior, adding that the biased presence of cut marks on ungulate lower limbs from Olduvai perhaps arose in the context of tendon removal for use in the production of carrying bags; this suggestion had previously been made by Richard Leakey regarding what was considered to be the most fundamental tool of home-base-living early hominids (Leakey and Lewin 1977:174). In addition, Shipman et al. (1981:260) examined cercopithecid remains from Olduvai "camps and living floors" as a clue to how hominids might have treated this species. The authors concluded that the remains were not hominid altered since the Olduvai data were similar to "nonhominid" data from Lake Turkana. The original research design indicates to me that Shipman believed that the Olduvai sites were high-integrity living or base-camp locations. In short, all the new data and observations were being accommodated to the then prevailing view of early man.

THE CONSENSUS VIEW IS CHALLENGED

In April of 1981, between the burst of publications cited above, another book appeared: *Bones, Ancient Men and Modern Myths* (Binford 1981). This book represented the state of my knowledge regarding factors conditioning the character and composition of faunal assemblages and the methods then available for unambiguously recognizing the agents responsible for, or the determinant conditions influencing, site formation. I discussed, among other things, patterns of bone modification (breakage and inflicted marks) as they may indicate the identity of agents contributing to deposits remaining from the past.

Archaeologists beginning the task of investigating the limitations of the knowledge of their time change their views of the past and the meanings that they give to their experiences as new knowledge is generated. This is done as they gain new perspective through their investigations on the limitations of the knowledge with which they work. In my book I summarized what was then known about the compositon of ungulate bone populations remaining on animal kill sites and bones accumulated in animal dens. The reasoning was simple: if we cannot tell the difference between the faunal assemblages associated with stone tools and forms of assemblage known to occur independent of human agency, then what, if anything, is implied by such ambiguous faunal remains as regards the behavior of the early hominids? Ambiguity must be resolved before defensible interpretation can be put forth. If it cannot be resolved, it must be eliminated from consideration—not because the ambiguous facts are not interesting, but because of the limitations of our present knowledge to resolve the ambiguity.

I chose a procedure that would permit the elimination from consideration of the faunal facts, as recorded, that were ambiguous in terms of our current knowledge. The results of the analysis were informative, since the faunal composition at the Olduvai sites (tool associated) was more complex than simple compounds of "kill" versus "den" assemblages common in nature. A "residual" pattern was isolated that, it was thought, could be informative about hominid behavior.[3] This pattern was characterized by parts of animals biased in favor of those yielding the least meat (Binford 1981:281). The most common bones in these assemblages were identified as those yielding only bone marrow as a potential food. It was pointed out that these are also the bones commonly surviving at kill–death sites after exploitation by nonhominid predator–scavengers. Inferences from this pattern were summarized as follows:

> 1. They (the hominids) were scavenging the consumed kills and scavenging death sites of animals after most of the other predator–scavengers had abandoned the carcass and scattered some of its parts.
> 2. The parts scavenged were primarily leg bones that appear to have already had the meat removed, or they were lower leg bones that had little meat present. . . .
> 3. The major, or in many cases the only, usable or edible parts consisted of bone marrow.
> 4. Hominids were using hammer tools to break open the leg bones and thereby expose the usable marrow. . . . There is no evidence supporting the idea that the hominids were removing food from the locations of procurement to a base camp for consumption. In fact, the covariant patterning among anatomical parts shows that the parts selected by hominids for use were taken from already consumed and abandoned carcasses: the coincidence of both the residual components of such an animal kill and the modified elements used by the hominids at the same site demonstrates nicely that the consumption was at the place of procurement. No evidence for base camps exists. Similarly, the argument that food was shared is totally unsupported (Binford 1981:294).

These conclusions were warranted by citing the information then available on the nature of large mammal faunal assemblages at "natural" kill–death sites where humans were known not to have been involved in modifying the materials. In addition, previous studies of the economic anatomy of ungulates (Binford 1978:15–46) were cited to warrant the conclusions regarding the economic or food potential of different bones.

The conclusions regarding base camps were justified by the argument that the only usable material indicated by bone frequencies—bone marrow—occurred in far too small a "package" for it to have been shared.[4] It will be recalled that the importance of the hunting hypothesis depended upon hunters killing animals of moderate to large size, therefore obtaining much more food than a single hominid could consume. It was thought that the only "motive" for killing such animals would have been if they were being returned to a base camp and shared among a large group of consumers. The conclusions were further justified by the fact that the frequencies of lower legs occurring on the Olduvai sites matched frequencies occurring on natural death sites. This strongly suggested that the hominids were not initially dismembering carcasses but that most commonly they were picking up already dismembered anatomical segments for consumption of the remnant food morsels.

It was pointed out that scavenging tactics would favor the biased exploitation of carcasses from animals of moderate to large body size, since these would be more likely to yield usable parts after other scavengers had finished feeding (Binford 1981:296). This suggestion was, of course, consistent with the already known pattern that many early sites consistently yielded bones from large animal species (see Sampson 1974:128, 215–216).

Very importantly, my analysis supported the observations of the excavators that other animals, particularly carnivores, had played a major role in conditioning the content of the faunal assemblages found in the same places as the stone tools. This meant that the total faunal assemblage could not simply be interpreted as resulting from hominid behavior. In addition, the analysis demonstrated that the archaeologically remaining faunal assemblages had been modified by destructive agents, in that a "signature" pattern of biased bone destruction was demonstrable. These facts condition the degree that interpretations about hominids can be made directly from the faunal frequencies as they were recorded by archaeologists. If sustained by further evidence, it means that interpretations of hominid behavior cannot be made directly from the raw data recovered from the Olduvai sites. In short, most of the Olduvai sites appear to be palimpsests, further modified by attritional agents.

The arguments in *Bones* challenged the models of the past then commonly accepted regarding the organization and behavior of early hominid life. I also challenged Shipman's interpretations of the data from Olorgesailie (Binford and Todd 1982) by pointing out that she lacked any clear form of inference justification and that the pattern that she claimed to be a distinctive indication of hominid butchering was the normal survival pattern expected in nature for bones of differing density.[5]

While of importance, these challenges to the prevailing views of the past were not as important as were the methodological challenges, which went directly to the root of interpretation. How does one use the knowledge of the time to justify or warrant interpretative conclusions offered regarding observations on the archaeological record? My major point had been that there was sufficient evidence available to clearly suggest that the Olduvai sites were palimpsests resulting from the perhaps semi-independent actions of a number of formation agents. Thus, one could not accept the raw data from these sites as directly referable to hominid behavior without first measuring or controlling for the effects of other agents on the form of the assemblage. The second major point had been to show that, when we did control for ambiguity, the composition of the faunal assemblage, which may well refer unambiguously to the hominids, did not appear to represent carcasses transported or processed for meat; instead, the tiny morsels of bone marrow from the lower limbs seemed to have been the food targets of the early hominids. The challenge went to the heart of both the then-current view of the past and the methods of inference justification then commonly being employed by the Africanists.

COPING WITH THE CHALLENGE

Just about a year after the publication of *Bones,* Shipman delivered a paper at the 4th International Council for Archaeozoology in London.

> Potts, Bunn, and I (Potts and Shipman 1981; Shipman 1981a, 1981b; Bunn 1981) have shown that the stone tools and bones at some early sites (FxJj50, Koobi Fora; various Bed I and II sites, Olduvai Gorge) are causally, not casually, associated. The main thrust of these papers had been to demonstrate the existence of cutmarks on various bones. Such evidence in part refutes Binford's claim that the sites were formed by carnivore or hydraulic activities, in that we know that at least some of the bones were acted upon by early hominids wielding stone tools (Shipman 1983:35–36).

It should be clear that Shipman had not read the *Bones* book very carefully, since nowhere did I question the relationship of stone tools to the processing of animal remains. I had even offered suggestions as to how it was done! Nevertheless, in her paper she proposed to "test" the arguments of Lovejoy (1981) and Isaac (1978a) regarding provisioning, sharing, and living in base camps. This was to be accomplished by using a set of stipulated conventions for inferring or denying meat eating, food sharing, food carrying, base-camp living, and division of labor among ancient hominids. These stipulated indicators are asserted to be what recent hunter–gatherers "do," and a knowledge of these activities would therefore provide a "signature" for the behaviors she sought to recognize archaeologically.[6] Her criteria are as follows (from Shipman 1983:36):

Behavior	Correlate
Meat-eating	Cutmarks on bones
Food-sharing	Disarticulation of carcasses
Food-carrying	Differential carcass utilization
Base-camp-living	Disarticulation of medium to large carcasses, defleshing of very large carcasses
Divison of labor	Combination of bony and plant remains at the base camp

I think it should be clear that almost anyone familiar with hunters and butchery could point to situations in which any one of Shipman's correlates could occur and not indicate what she claims. In fact, even she discounts the correlate of cut marks on bones as indicative of meat eating by recounting her belief that the biased marking of lower leg bones was because of tendon removal! After a short discussion of her criteria she proceeds to demonstrate that the frequencies of cut marks and tooth marks are essentially the same on shafts as they are on articular ends of the Olduvai bones, with a minor cut mark bias in favor of shafts. In turn, the frequency patterning of cut marks at Olduvai was shown to be different from that observed by Gifford et al. (1981) on archaeological bones recovered from a Neolithic site, where a much higher proportional frequency of marks on articular ends was observed. This was taken as evidence that the Olduvai hominids rarely disarticulated carcasses! Such a conclusion is not warranted even given the data she presents. First, there are cut marks on the articular surfaces of bones reported by Potts and Shipman (1981) as well as by Shipman (1983). Second, a proportional difference as noted by Shipman, could be understood to represent a different processing strategy with respect to bone shafts at Olduvai while a similar disarticulation strategy could be common to Olduvai and the Neolithic site. In short, arguments from proportions or ratios are always ambiguous with regard to the sources of observed differences. Nevertheless, Shipman gave the following meanings to her observations.

> The *conclusions must be* that the Olduvai hominids are rarely if ever disarticulating carcasses. Thus both Isaac's and Lovejoy's hypothesis are refuted by these data: food-carrying and food-sharing (and by implication, base-camp-living) did not occur at these sites (Shipman 1983:40; emphasis added).

Here we see that her entire position on the significance of the early sites is based on her rejection of the proposition that the hominids were disarticulating carcasses. This is a point that, as I have suggested earlier, cannot be defended even with the data as she has presented them. Nevertheless, after linking the above suggestion with those previously advanced by Vrba regarding how one might identify scavenging, Shipman claims that she has formulated her own criteria for recognizing hunting versus scavenging: (1) "early hunters were more likely to have hit upon a strategy suitable for hunting a single type of animal rather than a more broadly applicable strategy" (Shipman 1983:45). [Author's commment: they would be specialists.] (2) ". . . smaller carcasses, whether juvenile or adult, are less likely to be available for scavenging than larger ones" (Shipman 1983:45).[7]

There appears to be no justification at all for the argument that early hunters would be species-specific specialists except perhaps her knowledge that the early sites contained many different species and hence she could strengthen her "position" by such an assertion. It is unclear why the second claim is made, since there is a relatively large small-mammal and rodent fauna from the Olduvai sites, which has regrettably remained largely unanalyzed.

Undaunted, however, Shipman seeks to warrant the following conclusion: "I propose as an alternative to the human-type hunter–gatherer models that early hominids were predominantly scavenging for meat and foraging for plant foods" (Shipman 1983:45). Shipman again reports her observations on cut marks but this time long after the appearance of the *Bones* book. In this setting she presents "her" insight that early hominids were scavengers! Citing as evidence a biased tool-using attention to shafts rather then articular ends, she then unjustifiably asserts that this pattern means a lack of dismemberment activity! Ironically, by failing to demonstrate that meat-yielding bones were relatively rare at Olduvai she defeats her own argument, since one can imagine the scavenging of already dismembered relatively large meat-yielding parts linked with the transport of such parts to a base camp where sharing could be as reasonable as it is in the hunting hypothesis!

This irony is further extended since she adds the previously stated idea that, for her, scavenging places hominids in direct confrontational competition with other predator–scavengers. Such a situation is only realistic if the early hominids were going primarily after meat, the target of many other predator–scavenger feeders.

> Perhaps the unusual distribution of cutmarks at Olduvai relative to the Neolithic site . . . is a direct consequence of the opportunistic foraging–scavenging mode of life. Removing meat and other useful materials from a carcass as rapidly and efficiently as possible makes sense if the hominids were likely to be displaced or attacked by carnivores (Shipman 1983:45).

It should be clear that Shipman has in mind a meaty carcass being stripped without disarticulation by tool-using hominids. In such a case, it would seem that the tool-marked bone (from filleting) would remain at the carcass or kill site. Is she saying the Olduvai sites are natural death locations where hominids sneak in and cut off food morsels? On the other hand, if she has in mind confrontational competition, why would the hominid not disarticulate parts (a more rapid strategy) and carry them to safe locations? Alas, we are not enlightened; we are only told that hominids were in direct competition at carcasses, yet they were not dismembering them—instead, they were essentially filleting or removing tendons for carrying bags!

There is perhaps another issue that is even more important than the organization of Shipman's arguments. This has to do with her attempt to model a "way of life" from patterning manifest in essentially medium to large mammal bone. It should be recalled that the participants in the arguments being summarized are talking about some of the "first" archaeological manifestations of hominid behavior. We are discussing the very dawn of tool use and its "early years." From what we know of technological change in general, how likely is it that the earliest experimentation with tool use would constitute the "core" of the tactical array used by a creature in the adaptive exploitation of its environment? The hominids performed archaeologically invisible activities previously; how wonderful it would be if their early use of tools would in fact inform us about their total adaptation! I think this is the most unlikely state of affairs one could imagine. The challenge in this situation is to understand the context of initial tool use and then to see how this context changes through time. We know that, much later, man and late hominids used tools in many facets of their adaptation; was that true in the early years? Based on the facts that only the medium- to large-sized mammals have been studied taphonomically and that the only unambiguous pattern in these remains seems clearly to point to scavenging for bone marrow and perhaps to the incidental eating of tiny morsels of dried or desiccated meat, it is hard to see this behavior as the normative core of their adaptation.

During the spring of 1984 several articles by Shipman regarding the "scavenging hypothesis" appeared. Perhaps the most important of these "new" arguments relates to bias in the anatomical parts exhibiting cut marks.

> Meat-eating would be indicated by the presence of cutmarks on the meat-bearing bones (upper forelimb and hindlimb bones) of prey animals; very few cutmarks would be present on non-meat-bearing bones, like those of the feet (Shipman 1984a:9).

Now we are told that Shipman actually carried out her work on the Olduvai materials with this argument in mind and that, much to her surprise, more than half of the cut marks occurred on non-meat-bearing bones. She then returns to her earlier idea that perhaps the cutmarks were produced while the hominids were removing tendons: "Many of these marks were on non-meat-bearing-bones. Many of these marks must have resulted from skinning and tendon removal as well as butchery."

The next article to appear was in the popular magazine *Natural History* under the title "Scavenger Hunt," and here a more sophisticated discussion of disarticulation versus filleting of meat from bones was introduced into the discussion (Shipman 1984b:22). It was suggested that the bias in favor of "meat removal" (from bones that do not have meat) was evidence that the hominids did not carry "their kills back to camp to share with others, since both transport and sharing are difficult unless carcasses are cut up" (Shipman 1984b:22). (This again implies that we are looking at kill sites?)

> When I looked for cut marks attributable to skinning or tendon removal, a more modern pattern emerged. On both the Neolithic and Olduvai bones, nearly 75 percent of all cut marks occurred on bones that bore little meat, these cut marks probably came from skinning. Carnivore tooth marks were much less common on such bones. Hominids were using carcasses as a source of skin and tendon. This made it seem more surprising that they disarticulated carcasses so rarely (Shipman 1984b:22).

If the early hominids were scavengers, what did they scavenge? The answer that Shipman gives us is "tendons." How can she postulate an overall way of life based on tendon extraction? Given the original context of this suggestion (tendons used for making carrying bags) it would seem that her arguments would lead her to propose that the hominids were more likely foragers of vegetal food! Nevertheless, in an article by Roger Lewin (1984) entitled "Man the Scavenger" the essential features of Shipman's arguments as seen in the *Natural History* article were recapitulated. The contents of the article are not new but the tone is surprising. For instance, "the idea that scavenging might have represented a complete ecological adaptation is only now being articulated" (Lewin 1984:861). Later in the article we are told, "In developing the scavenging hypothesis, Shipman has documented the various physical and behavioral characteristics among carnivores and compared them to hominids of 2 million years ago" (Lewin 1984:862).

We have gone from a biased distribution of cut marks on the shafts of lower limb bones at the Olduvai sites to a discussion of the overall niche occupied by early hominids in an African setting. The whole time, Shipman bases her reconstruction on the unjustified interpretation of (a) proportional differences in cut marks between Olduvai and Neolithic sites, and (b) the assumed significance of carnivore evidence at the site—direct competition between hominids and carnivores.

At the 4th International Council for Archaeozoology in London (April 1982), Richard Potts presented a paper designed to clarify the problem of early hominid subsistence. Potts launches into a series of arguments warranting his analytical strategy, which begins with the following assumptions:

> representation of different skeletal remains on sites in part relates to the timing of hominid access to carcasses. Hunting implies that the complete carcass is acquired by the predator. In contrast, scavenging produces wider variation in the availability of skeletal parts. Processing of an animal recently dead (early scavenging) may permit access to the entire skeleton and its resources. On the other hand, late scavenging—after other animals and agents have acted on the carcass—allows access only to skeletons which have undergone disarticulation and bone dispersal (Potts 1983:52).

The careful reader will recognize this as the same principle that I used earlier to argue that the residual pattern noted on the Olduvai sites was most likely the result of scavenging (Binford 1981:253). This is the principle upon which I based my earlier studies of human hunting and faunal assemblage diversity. A biased deletion of parts was made from an original population, resulting in a derivative population found elsewhere (see Binford 1978:81, 242–245). There is, however, a major difference, which Potts has overlooked, between the forms of the original population and the derivative population. It was shown empirically time after time that the first derivative population, the one removed from a parent population, was in fact different from the parent population. This difference derives from the fact that modern humans exploit a carcass in a biased manner. They do not just pick what is most common or accessible but what is most useful to them in the context of availability. It is the pattern inherent in the first derivative population that is a clue to the exploitive bias standing behind the selection of parts from a parent population. Potts simply assumed that early hominids were not interested in usable material, that they picked up bones in direct proportion to what was available to them as already disarticulated parts. I think such an assumption is very hard to justify. The hominids must have been interested in some usable materials on a carcass and not just in the bones themselves considered in anatomical terms. One could easily argue that the most usable material would remain on those parts that were least likely to have been exploited earlier by other kill-scavengers! Of equal importance must be the recognition that the hominids *had tools* and could and apparently did *disarticulate* animal parts, presumably with reference to judgments as to their utility. Such behavior would ensure that the removed or transported assemblage of parts would be different from the parent population of parts from which selections were made.

Ignoring such problems, Potts proceeds to justify the argument that there was a signature pattern to anatomical parts remaining on nonhominid kill–death sites. Potts focuses on an observation by A. Hill (1975, 1979) that forelimbs tend to be disarticulated early in the normal sequence of anatomical disorganization expected among carcasses. It should be noted that research conducted on decaying bovid carcasses in the New

World, where it is known that no active scavengers were present, does not yield such a pattern (Todd 1983). On such sites both front and rear legs do not "fall off" in the sequence noted by Hill. Presumbly the pattern Hill observed is related to predator dismemberment rather than a regular pattern of anatomical disorganization independent of mammal agents. Nevertheless, Potts proposes an analytical strategy to determine whether hominids consistently had access to carcasses early or late in the alleged normal sequence of anatomical disorganization. His proposal is a simple one: calculate two ratios from the actual counts of bones on the Olduvai sites, the ratio of front to rear leg elements and the ratio of axial skeleton parts to all limb parts. He calculates his ratios for a number of key Olduvai sites and finds that there is little patterning recognizable, an exception being noted for bovids of medium size.

At this point we are forced to ask why Potts insists on treating the remains on the Olduvai sites as if hominids were responsible for all the bones present. There is clear evidence in the form of extensive animal gnawing (Potts and Shipman 1981; Shipman 1983), coprolites believed to be from lions and hyenas (M. Leakey 1971:43, 50), as well as breakage and part-survival patterning (Binford 1981:256–262) that is nearly identical to control cases where gnawing animals broke and consumed a faunal assemblage. In addition, water sorting can be expected to condition at least three of the assemblages he studied.[8] Finally, there is an anatomical part association pattern that is partially indistinguishable from that known to result from ravaged carcasses where man played no role in their modification (Binford 1981:262–288). In spite of the evidence to the contrary, Potts proceeds as if all the bones on the studied Olduvai sites were directly referable to hominid actions. This procedure is simply not defensible. The problems of potential transport of parts by animals and by water, and the differential and biased destruction of anatomical parts by gnawing animals, are simply ignored. We see here a failure to deal realistically with the archaeological record.

With this unrealistic approach Potts moves directly to inferences about hominid behavior and concludes that they were nonspecialized omnivores using a "broad base of faunal exploitation and a varied mode of foraging . . . that at least occasionally utilized animal resources" (Potts 1983:61). Such conclusions are simply not warranted. First, the model used for monitoring hominid strategy was not a realistic one, in that it assumed that the hominids did not exploit carcasses in terms of the subsistence potential of parts, only the simple availability of bones anatomically considered. Second, the analysis assumed that all the bones on the Olduvai sites were accumulated by hominids and that the anatomical frequencies present were directly reflective of hominid behavior, unmodified by scavenging animals. These assumptions are demonstrably inappropriate. Finally, his conclusions tell us that we can expect great flexibility and variability in the subsistence behavior of the early hominids. Such conclusions simply do not add to our knowledge since they derive from the vague lack of patterning that always results from treating a complex, multidimensional phenomena unidimensionally.

In the beginning of his paper, Potts (1983:51) asserted that he considered the Olduvai sites to have been "stone caches which were used repeatedly for carcass processing." The "stone cache" argument to which Potts alludes was developed in his dissertation (Potts 1982) and has recently been expounded in a more complete fashion (Potts 1984a). This argument starts with the observation that most of the stone found in the Olduvai sites had been transported from "several kilometers away." From this observation the suggestion is made that the hominids transported the stone to appropriate places in their environment in anticipation of subsequent use. Later, when hominids secured a carcass or parts thereof, they would transport the animal parts and perhaps other foods to these caches for processing. This strategy is evaluated as being more efficient than having a single home base and also tending to reduce competition at carcasses.

> Because many animals were attracted to carcasses, it was necessary for hominids to transport portions of them away from the death sites at which they had been obtained. . . . Time and energy spent in handling and transporting portions of meat could be minimized by taking the bones to the nearest cache, where there remained stone tools. . . . Time spent at the cache was then minimized by processing the new material quickly to obtain whatever . . . were needed. By abandoning the site immediately, hominids could probably often avoid direct confrontation with carnivores attracted to the remains (Potts 1984a:345).

These arguments are interesting in that they attempt to model a non-home-base formation context for the Olduvai sites. They are seemingly plausible, in that currently popular appeals to efficiency and risk reduction are built into the arguments. They further imply a very different subsistence–settlement pattern that is common with modern man. Thinking of this sort is needed when treating the early materials. However, the model fails in several ways. Perhaps the greatest difficulty lies with the fact that it cannot be tested as presented, beyond its plausible accommodation of the facts as known or admitted at present. As such, it is a classic post hoc accommodative argument. Secondly, it fails to acknowledge the data relative to nonhominid roles in site formation since it accommodates ambiguous data. It makes the convenient assumption that the evidence for nonhominid presence at the site was added after the hominids had abandoned the site. Finally, it assumes a set of planning strategies on the part of the hominids that is only known among the most complex modern hunter–gatherers, namely a caching strategy for materials and the outfitting of places. This strategy is presently documented only for complex, logistically organized, modern hunter–gatherers (Binford 1980).

In a more recent paper, Potts (1984b) acknowledges some of these problems but adopts what is best described as an "uncertainty averaging" strategy rather than treating the analytical problem presented by the Olduvai sites realistically. Not surprisingly, he concludes that things are uncertain and resorts to opinions warranted by appeals to general ecology and the plausible suggestion that life was different in the Lower Paleolithic (Potts 1984b:159–161).

I applaud Potts for his attempt to introduce new ideas into the field of early man studies. I agree that the Olduvai sites are not home bases, and I agree that they are more likely multiple occupational, "functionally specific" locations. I doubt strongly that they are tactically planned locations of the type he suggests. I will have more to say on this point in the summary.

The third paper of interest delivered at the London conference was by Glynn Isaac (1983b). This paper summarized the knowledge that he considered to be secure about the behavior of the early hominids. It essentially acknowledged the skeptical excitement of the preceding years and then emphasized the evidence for the transport of anatomical parts by hominids. In addition, it emphasized that the evidence indicated "meat eating" on a significant scale (Isaac 1983b:13). (This is a misleading statement since we actually have no evidence bearing on the relative contributions of different food sources to the overall diet of the early hominids.) Isaac then goes on to suggest that, while his earlier position on "home bases" was probably premature (he made early man appear "too human"), nevertheless the "significant" role of meat eating and the evidence for transport of animal-derived "foods" justifies what he calls the "central-place foraging" view of early man. He then cites his earlier papers (Isaac 1978a, 1981a, 1981b; Isaac and Crader 1981) as well as one by Jane Lancaster (1978) as germane to this view. These are all statements of the old "home base" consensus view. This position was well summarized in Isaac and Crader (1981:93) as follows:

> In devising models of early stages in the evolution of a system in which food-sharing became a pivotal ingredient, meat does have critical importance: it is a highly concentrated, highly portable form of nourishment. Portions of a carcass are readily carried and are an important food prize when consumed at the destination. We thus favor a model in which the active delivery of some meat to fellow members of a social group developed in a reciprocal relationship with the practice of transporting and sharing some surplus plant foods. We see the model as representing a functionally integrated behavioral complex, in which any attempt to isolate one or another component as an initial or prime mover is probably misleading.

Since 1983 Isaac has presented his views many times and currently concedes that the hominids were probably not sleeping at "central places," but he continues to insist that the transport of animal products (and "significant" meat eating) are sufficient empirical indicators of the accuracy of his "model" of early hominid behavior and in turn of the conditioning role that such behavior played in bringing forth our human characteristics.

The point made by Isaac about "significant" meat eating seemingly stems from work done by one of his former students, Henry Bunn, who also delivered a paper at the London conference. Bunn's paper was short and to the point. He analyzed the form and distribution of what he judges to be tool-inflicted marks on the bones from Isaac's site (Fxjj 50) at Koobi Fora and from the important floor at Olduvai Gorge (FLK-22) that yielded the "Zinjanthropus" hominid remains. He reports the *proportion* of different anatomical parts that, in his judgment, yield tool-inflicted marks. He stresses the fact that cut marks consistently occur in high proportions on the articular ends of long bones. This clearly conflicts with the claims by Shipman, discussed earlier, that "hominids are rarely if ever disarticulating carcasses" (Shipman 1983:40). Like Shipman, however, Bunn (1983:27) does report high frequencies of cut marks on the shafts of long bones, seemingly indicative of different behavior than is commonly reported from relatively modern assemblages.

Like earlier apologists for the tooth marks from Olduvai, Bunn notes the high frequency of carnivore marks on the bones and offers what can be considered the "standard" argument for their dismissal. [See previous statements of this position: M. Leakey 1971:43; Isaac 1971:288; Bunn 1982:495; as well as my comments on it (Binford 1983b:373.)]

> The simplest explanation for the presence of several hundred bones with carnivore gnaw marks, some of which occur on bones that also retain cut marks, is that scavenging carnivores, possibly hyenas, were attracted to the FLK Zinjanthropus bone and artifact concentration after hominids created it (Bunn 1983:28).

This may be the "simplest" assumption, but it is also the assumption that saves the "consensus" view of the past. If animals were admitted to have played a role in the accumulation of the deposits, the case for bone transport by hominids would be rendered at best ambiguous. Secondly, if animals were to be admitted as potential bone transporters, then the case for "significant meat eating" would be less secure. Seeing scavengers coming on the site after the hominds had produced it makes it possible to salvage the consensus view. It also makes it possible to offer alternatives to the consensus view and still not face the issue of the methodological sophistication needed to analyze data when multiple agents are suspected as having been operative in site formation, as was done in Potts' (1983) arguments.

SUMMARY OF CRITICISM

What seems clear is that essentially all the major authors treating the early eras of tool use agree that much of the fauna on the sites was accumulated by virtue of hominid scavenging activities. It appears that all currently also agree that these lake- and waterside locations were not places where the hominids slept. All authors (except perhaps Shipman, who is unclear on this issue) seem to agree that the hominids transported parts of animals to the places we see today as sites. All also recognize a major role of nonhominid predator–scavengers in the formational history of these sites. These views represent major and deeply provocative changes in our ideas about early man, which have largely come about since 1980. Major disagreements remain regarding (a) the presence and, if so, the quantity and character of hunting carried out by the early hominids, (b) the motives for transporting animal parts, (c) the functional character of the sites with respect to the broader subsistence–settlement pattern of the hominids, and (d) the role of the nonhominid predator–scavengers both in site formation and in the ecology of the early hominids. It seems appropriate to summarize the arguments regarding these points of controversy.

I think it should be clear that the role of carnivores as contributors to the site formation processes is the central and currently unresolved issue complicating much of the interpretation of the data from the early hominid sites. I argued that nonhominid predator–scavengers were important agents responsible for some of the characteristics of the Olduvai faunal assemblages (Binford 1981). I also suggested that what appeared unambiguous at these sites was an assemblage of bones biased in favor of ungulate lower-limb bones, which were problablly selected and transported for purposes of processing for bone marrow. I used by analysis to challenge the view that hunting and transport of meat-yielding parts to base camps for sharing were justifiable interpretations of the Olduvai data. It should be emphasized that the challenge was offered by reasoned argument from the facts of faunal assemblage composition, both in terms of anatomical part frequencies and in terms of breakage categories justified by the independent study of the economic anatomy of ungulates.

The response was interesting. Shipman did a complete about-face. She had quite obviously been defending the consensus view with her "very human" interpretation of middle Pleistocene hominids. This clearly implies humanlike characteristics for the earlier hominids. All at once, using two data sets (the proportions of animal-tooth to tool-inflicted marks on bones and the proportions of tool-inflicted marks on bone shafts versus articular ends), she argues that early hominids were not hunters, they were scavengers. The alleged lack of evidence for disarticulation in the Lower Paleolithic is said to show that hominids were not transporting meat-yielding parts to "home bases." Finally, the evidence of carnivore tooth marks is suggested as indicative of direct competition between hominids and other scavenger–predators for animal products. As previously pointed out, there is no lack of evidence for disarticulation by the hominids, and the proportional data cited by Shipman is inappropriate to her argument. The important facts of faunal assemblage composition are not mentioned, nor are the problems of multiple agents as contributors to the Olduvai assemblages dealt with except by avoidance. The argument for competition with carnivores is hard to accept in the absence of any clear suggestion as to how the tool- and tooth-marked pieces came to be associated at sites. Consideration of this problem should lead directly to an evaluation of the kinds of sites represented by the Olduvai material, and we see that this issue is generally ignored except to say that they are not base camps (although implications seemingly unappreciated by Shipman haunt her arguments). Of all the contributors to debate, Shipman seems to be the only writer who suffers from problems with both the appropriateness of the analytical procedures chosen and the internal logic of her argument, even if one grants her the assumptions made.

This is not to say that someone skilled in analysis could not use Shipman's observations, but she would have to present her data on gnawing and cut marks broken down by anatomical part since *the only frame of reference we currently have for evaluating biased behavior* is relative to the use of anatomical parts of known utility. If she were to present her material in this fashion she might be able to demonstrate a biased use of animal products in favor of non-meat-yielding parts. She could not, however, demonstrate the absence of disarticulatory behavior or the absence of transport. The second major problem with Shipman's conclusions rests with her argument for direct competition among hominids and other predator–scavengers. Instead of a hard-headed attempt to understand the common occurrence of tooth-inflicted marks on bones associated with hominid remains she moves directly to arguments of "evolutionary functionalism," which appear to be implausible and poorly grounded.

The work of Richard Potts appears somewhat more sophisticated. Rather than denying transport, he clearly goes with the data that seem to indicate the transport of animal parts by hominids. Potts' work suffers from two major weaknesses: (a) the failure to treat the problem of the integrity of the deposits, adopting the explanation for animal-tooth-inflicted marks as inflicted *after* the hominids had accumulated meat at "stone caches;" and (b) the assumption of the very behavioral characteristics for the hominids that we would like to monitor—that is, planning depth in their subsistence strategies. Potts points out that the stone was transported to the "sites" in both tool and manuport form. He assumes that this was done in *anticipation* of the use of such locations for processing animals and perhaps other food products. Such behavior requires considerable abilities in *planning ahead.* I have stressed elsewhere (Binford 1982:178; 1984:97, 98, 195, 224) that planning depth with regard to technology is not something in which the early hominids appear to have been very skilled.

Potts' model assumes motivation and, in so doing, suffers from the same tautological properties that I will show characterize Glynn Isaac's arguments—namely, the facts of the case are argued in a post hoc manner to result from an assumed motivation. The evidence for the motivational component of the argument consists of the very facts that it is supposed to explain (see Potts 1984:345)! In fact, the only data currently available for which there is some independent basis for inferring motivation would permit recognition of a bias in the use of animal body parts (see Binford 1978), yet these investigators strangely avoid these data. We need much more work at the middle-range level to provide independent frames of reference against which to evaluate motivational arguments. At present the only basis for believing Potts' accommodative argument is that it fits the data. Of course, it was developed to accomplish just that and hence remains a tactical tautology.

The arguments of Glynn Isaac and his student, Henry Bunn, both fail to treat the problem of the integrity of the Olduvai sites by assuming the "standard" explanation for the evidence of nonhominid predator–scavenger presence—these animals gnawed the bones after the hominids had left the sites. Isaac's current defense of his slightly modified "home base" argument, central-place foraging, assumes that the motive for transporting animal parts is for sharing; hence, the evidence he is willing to accept for sharing is the demonstration of transport, another tactical tautology. Finally, both Isaac and Bunn appear willing to accept the bones as evidence for meat eating by arguing for disarticulation of anatomical segments by the hominids, and by citing the quantity of bones. Once again, the only evidence currently independently justifiable for demonstrating a bias toward the use of meat is from anatomical part frequencies, a data domain they both seem to judiciously avoid!

In general the responsive arguments have been encouraging in that the challenge to the consensus view is taken seriously and even supported by some, while all are struggling to view the data from the early hominids in new ways. What is perhaps regrettable is that the types of work necessary to forging a more realistic view of the past are not thus far forthcoming. We need solid descriptive studies of the important Olduvai sites in terms consistent with current methods of analysis. The faunas need to be described in detail: the frequencies of cut marks, animal gnawing, and breakage patterns must be reported in such a way that the patterning regarding the interaction among agents, which most certainly stands behind the archaeological record, can be recognized. Short articles that merely summarize "new facts" in ways that are not amenable to alternative analyses do not help. I look forward to a period of more substantive research and more meaningful argument than has thus far materialized.

BEYOND THE REACTIVE ARGUMENTS

It is perhaps appropriate to point out that I became involved in arguing about the lifeways of the early hominids from the perspective of working with archaeological materials from a later time period, the Mousterian of Europe (see Binford and Binford 1966; Binford 1972, 1973, 1982a, 1982b, 1983a, 1983b). During the course of work on the "Mousterian problem" I became convinced that the organization of the hunting and gathering way of life, among these relatively recent ancestors, was quite different than that among fully modern *Homo sapiens sapiens*. If this was true then the almost "human" lifeways depicted in the "consensus" view of the very early hominids stood out as an extremely unlikely condition. It was for this reason that I began to "worry" about the early Pleistocene materials. I think that I have been successful in casting strong doubt on the characterizations of hominid life that were so current in the 1970s. As suggested in the summary, many of the points of the challenge are currently conceded by many Africanists.

It might be reasonably asked, where does this leave our understanding of the processes leading to our own characteristic humanity? It should be clear that large-game hunting has been considered by many as a formative step in our evolutionary background. As suggested here, the practice of such predation during the early phases of hominid experience has been strongly challenged, and there is a growing suspicion that active large-game hunting may not have been a part of the early hominid lifeway. In view of this uncertainty some have simply shifted the hunting argument to later time periods, suggesting that hunting was a behavior characteristic of *Homo erectus*.

> There are after all some persuasive records of systematic hunting later in human history. One of the oldest of these is at Olorgesailie in Kenya, where hominids repeatedly killed and butchered giant baboons. . . . The hominids of the time were *Homo erectus*, which had appeared in Africa some million years earlier than the Olorgesailie site.
>
> As there are no equivalent records of systematic hunting between 1.5 million and 0.5 million years ago, the question of when the practice began and when the scavenging mode ended remains a matter of speculation (Lewin 1984:862).

As has been pointed out, the evidence presented by Shipman et al. (1981) for systematic hunting at Olorgesailie is far from convincing;[9] nevertheless, it is appealing to some to hold to the old view that it was hunting that "made us human" and hence translate the argument to a more recent era.

At the same time that the history of early hominid large-game hunting was being reevaluated, there were intellectual challenges to the idea that hunting was a crucial behavioral context for understanding the emergence of our "humanness." I have already discussed the consensus view of early man, but Glynn Isaac's argument is not totally dependent upon hunting although a sexual division of labor seems crucial to his arguments.

> Once food transport was initiated, novel selection pressures would come to bear on (1) ability to communicate about the past, future, and the spatially remote, and (2) enhanced abilities to plan complex chains of eventualities and to play what one might call "social chess" in one's mind. That is, the adoption of food-sharing would have favored the development of language, social reciprocity, and the intellect (Isaac 1983c:535).

Another view has been articulated by Owen Lovejoy (1981), writing from a more biological perspective, in which he suggests that reproductive advantages and disadvantages accrue to differing kinds of social arrangements and, by implication, subsistence behavior. His arguments have the advantage of suggesting mechanisms for the appearance of food transport and "provisioning," while Isaac's take a more historical approach and postulate such conditions as necessary prerequisites to the human characteristics we know must have appeared more recently. Given the implied importance of a male–female division of labor in Isaac's argument, Lovejoy makes a most provocative point regarding male–female subsistence activities.

> Greater seasonality and the need to increase both birthrate and survivorship would also favor at least partial separation of male and female day ranges since this strategy would increase carrying capacity and improve the protein and calorie supply of females and their offspring. . . . Lowered mobility of females would reduce accident rate during travel, maximize familiarity with the core area, reduce exposure to predators, and allow intensification of parenting behavior. . . . Monogamous pair bonding would favor feeding divergence by "assuring" males of biological paternity and by reducing feeding competition with their own offspring and mates (Lovejoy 1981:344–345).

Although Lovejoy is making the above argument with respect to changes he believes occurred during the Miocene, the argument is perhaps more germane to the conditions facing a radiating hominid population penetrating the temperate zone during the mid-Pleistocene. It is interesting that when Lovejoy's argument is considered in a Miocene context the focus would be exclusively on equatorial and subequatorial environments, where temperature-related seasonality is not a major factor exacerbating patchy food distributions.[10] If, as many have thought, hunting is strongly tied to a male–female division of labor, a separation in feeding ranges would provide a natural basis on which a division of labor and sharing could later be developed between the sexes. The latter characteristic, sharing, could be related to the beginning steps toward a more hunting-dependent subsistence strategy.

As we have seen, recent research strongly supports the view that the early hominids living in essentially equatorial and subequatorial environments were not large-mammal hunters in any systematic way. On the other hand, a separation in feeding ranges could well be exacerbated by the lowered food abundance in temperate settings and in turn could provide the selective context upon which hunting and a more systematic division of labor could be biologically favored. I have long thought that hunting was a strategy that would increasingly serve an omnivorous primate in environments with shorter and shorter growing seasons (Binford 1981:296). A temperate-zone context of selection could be summarized as the need to cope with the overwintering problem. One solution to this problem is to eat animals who have already solved this problem in their region. Viewed against a suspected pressure to separate the feeding ranges of males and females, plus the advantage of predation as a means of solving the overwintering problem in temperate settings, we can envision a more realistic framework to investigate the important changes in adaptation that must have occurred in our remote past. In support of the temperate-zone focus there is evidence from modern hunter–gatherers that clearly indicates the advantage of hunting in environments with short growing seasons. Richard Lee (1968) has demonstrated that dependence upon nonplant foods increases as one moves away from the equator. I have shown (Binford 1980) that, in addition, modern hunter–gatherers deal with the overwintering problem by increased dependence upon storage as one moves farther from the equator. Surely, early man had to cope with these same problems!

I cannot imagine that the earliest hominids to radiate into temperate settings practiced storage strategies, but one can imagine them being under pressure to become at least seasonal predators. Harking back to Lovejoy's arguments, we could also expect that one of the first moves in this direction might well have been an increased separation in the feeding ranges of males and females. In addition, the increased shelter requirements in the temperate zone would render it even more reproductively advantageous to reduce the mobility of females and their offspring, providing still further advantage to increases in provisioning behavior on the part of males.

Arguments such as these may turn out to be wrong, but at present they can provide provocative guides for the development of research programs aimed at furthering our understanding of hominid evolution. It was in the context of such speculations that I turned my attention to the study of temperate-zone sites with an eye to isolating both when and where unambiguous evidence of hunting appears and, further, how we might use archaeological traces to enlighten us concerning the interesting possibility of semi-independent feeding ranges among hominids as well as the interesting question of recognizing the presence of a division of labor.

WHEN AND WHERE DID HUNTING BECOME A MAJOR HOMINID STRATEGY?

At the southern tip of Africa is a geographically small temperate environment. In 1981 I had the opportunity to study the fauna from the South African site of Klasies River Mouth, which spans the period from around 125,000 until about 35,000 years ago and documents the shift to the presence of fully modern forms of men. Based on my European experience I had fully expected the occupants of this site to have been regular hunters. Much to my surprise, however, my interpretation of the data indicated that scavenging of medium- to large-body-sized animals was still a regular and significant subsistence strategy. Also, while hunting of small mammals was practiced, there was a trend toward increases in hunting through the sequence at Klasies that was roughly coincident with the anatomical shift to more modern types of humans. It is true that the scavenging appears more regular than thus far indicated for the early hominids at Olduvai Gorge, and that fire and other technical aids to processing food and almost certainly protecting sleeping areas are indicated; nevertheless, the overall pattern of life appeared remarkably similar to the strategies of the hominids of the Plio-Pleistocene boundary, particularly for the earlier phases of the sequence. While there were other provocative results of this study, certainly the most surprising was the conclusion that not only was early man not a "mighty hunter" but very late hominids on the very threshold of the appearance of our own species were also *not technically aided hunters and gatherers* in the sense that archaeologists have generally assumed for the earliest hominids or have considered likely for mid-Pleistocene *Homo erectus*.

A LOOK AT THE NORTHERN TEMPERATE ZONE

Since the small and relatively recently settled temperate zone on the southern tip of Africa may have a very different history than the large and anciently colonized temperate areas of Europe and Asia, it was clear that the next research step was to investigate the early evidence for hominid radiation into Europe and Asia. During the spring and summer of 1984 I had the opportunity to conduct such investigations. These studies are as yet incomplete in that all the planned analysis has not yet been performed. Nevertheless, I have gained some interesting impressions of the data at this stage of their preparation for publication.

I have studied the fauna from the lower gravels and the lower loam, as excavated by John Waechter, from the famous site of Swanscombe–Barnfield pit (see Wymer 1968:332–361; Roe 1981:67–72). These materials are associated with a Clactonian tool industry and are generally assigned to the Hoxnian Interglacial (Mindel–Riss in the old Alpine sequence), which is estimated to have occurred between 350,000 and 400,000 years ago. I have also studied the fauna, excavated by John Wymer, that was found in association with a hand axe industry (Lower Industry) from the famous site of Hoxne. This is thought to be more recent than the Swanscombe material and could be as recent as 200,000–240,000 years old (see Wymer 1983:187).

Several preliminary observations are of interest with regard to these faunas. At Swanscombe only six bones exhibited tool-inflicted marks.[11] Three of these were related to disarticulation: a mandible, an atlas vertebra, and a distal tibia. The other three marked pieces bore clear evidence of meat removal. These were on a proximal tibia, the shaft of a distal tibia, and the shaft of a distal humerus. Importantly, no metapodial bones bore tool-inflicted marks and there were no scraping or chop marks on any of the bones from Swanscombe. This suggests that the hominids were interested in meat and that their involvement as agents responsible for the accumulation of much of the fauna at Swanscombe was minimal. The latter inference is born out by high frequencies of shed antler and a pattern of anatomical part frequencies strongly suggestive of a normal "background" fauna, which might be expected to have accumulated naturally on an exposed waterside location where some hydrological sorting had also occurred. There was very little evidence of marrow bone breakage and there was also minimal evidence pointing to the action of other carnivore–predators. Very few animal-gnawed bones were present.

At Hoxne, as at Swanscombe, there were only a very few bones with tool-inflicted marks. One astragalus of fallow deer had been cut by stone tools during disarticulation, and all other cut marks were on horse bones. Nine horse bones bore tool-inflicted marks, of which three are believed to be related to disarticulation while six are thought to have been inflicted during meat removal. Two of the latter were ribs, one was a metacarpal, and the remainder were on long-bone splinters. In marked contrast to the fauna at Swanscombe there was considerable evidence of systematic marrow-bone breakage, particularly of horse bones. In terms of anatomical part frequencies the horses were represented by a head- and lower-limb-dominated frequency graph, as were the clearly scavenged large-mammal remains from Klasies River Mouth. On the other hand, the fallow deer were represented by primarily meat-yielding bones while red deer exhibited a graph with properties of both horses and fallow deer. As was true at Swanscombe, animal gnawing, while present, was relatively uncommon. For all intents and purposes the Hoxne fauna has the characteristics of a transported and accumulated assemblage scavenged from medium to large mammals, in which heads and marrow-yielding bone were the parts most commonly transported for processing. In addition there is a meat-biased assemblage evidenced by the remains of fallow deer, but these creatures are infrequent at the site. The latter pattern is very similar to that noted as Klasies River Mouth for the time period around 125,000 years ago; however, the degree that these smaller animals were hunted cannot be established from these data.

While preliminary, it is my impression that the Swanscombe fauna represents hominid scavenging at the source of carcasses. There appears to be a meat bias in the treatment of bones and there is no evidence for systematic marrow-bone processing. The assemblage does not appear to be transported or aggregated. In marked contrast is the fauna from Hoxne, where transport and aggregation of heads and marrow-yielding bones of both horse and red deer is indicated. Systematic marrow-bone processing is the most robust evidence for hominid modification of the bones. There is a provocative suggestion that the small fallow deer may have been rarely killed for meat. This site has much in common with the much earlier Olduvai sites; however, the situation is clarified by the general absence of evidence indicative of carnivore involvement in the accumulation of the deposits. Thus, by the much later era, when hominids made their first appearance in the British Isles, the way of life indicated by the medium to large mammal remains does not appear to be greatly different from that of their much earlier ancestors at Olduvai Gorge!

In further search of evidence for changes in subsistence practices I studied the deep, stratified sequence of depositis from the French site of Abri Vaufrey (Rigaud 1982). This site is particularly interesting in that it is a cave, and its deposits span the period from the Mindel/Riss interglacial (Hoxne) through the first interstadial of the Würm (300,000 to about 60,000 years ago). Thus far I have analyzed the remains from layer VIII (235,000 years ago, perhaps roughly contemporary with Hoxne) and am familiar with the tabulation and recording of the more recent layers. Layer VIII yielded a "flake" industry that is "typical Mousterian" in the Bordean sense of later Würm Mousterian assemblages. The fauna that is referable to hominid transport to the cave was primarily red deer, horse, and rare occurrences of aurochs. Surprisingly, *there were no tool-*

inflicted marks on these bones resulting either from dismemberment or removal of meat. Equally surprising was the fact that the anatomical parts commonly represented were upper-limb bones or the meat-yielding bones from these animals. These bones had been gnawed by nonhominid predator–scavengers in exactly the same pattern as is shown by controlled data from predator-exploited kill and death sites, indicating that the hominids had transported meat-yielding bones from previously ravaged carcasses, not from hunted animals. In the Vaufrey level VIII assemblage there were few lower-limb bones, and *even these were rarely exploited for bone marrow.* There was little evidence for the regular use of fire in this level and no evidence for cooking.

In marked contrast was the fauna of level VII, believed to date around 200,000 years ago. In this assemblage tool-inflicted marks from both dismemberment and filleting meat were relatively common, as were numerous long-bone splinters exhibiting longitudinal scrape marks. Long-bone splinters with numerous pitted scars, known in the European literature as "compressors," were also present. There was clear evidence for regular processing of long bones for marrow. The regular use of fire was clearly indicated, and there was a biased pattern of burning on bones. In terms of anatomical part frequencies, preliminary tabulations suggest a head- and lower-leg-dominated assemblage. Except for the greater frequency of tool-inflicted marks and the evidence for the regular use of fire, this assemblage resembles that from Hoxne. On the other hand, the associated tool assemblage, like level VIII, is classifiable as typical Mousterian! As in the case of level VIII there is a regular pattern of animal-tooth markings that is indistinguishable from the known pattern remaining on ravaged carcasses after nonhominid predator–scavengers have finished feeding. This is taken as evidence that, as in the case of level VIII, the majority of the transported fauna was scavenged from previously ravaged carcasses. This view is further supported, in the case of level VII, by tool-inflicted marks that overprint animal-tooth marks.

Very little analysis has been thus far conducted on materials from the upper levels of Abri Vaufrey; nevertheless, it can be reported that the levels deposited during the Würm I phase of the French sequence exhibit a very different treatment of fauna. The pattern is very similar to that with which I have become familiar from having studied the remains excavated by F. Bordes from the important site of Combe Grenal. Combe Grenal spans the first two phases of the Würm, dating between 85,000 and 45,000 years ago; like the previously discussed Klasies River Mouth site in South Africa, it documents the period just prior to the appearance of our species. At Combe Grenal the large animals, such as aurochs and horses, tend to be represented by essentially meat-yielding upper-limb bones (see Binford 1981:99) but unlike at Vaufrey level VIII some of the marrow-yielding bones occur in reduced numbers, suggestive of "riding" or transport of more complete limbs back to the site. Additionally, marrow-bone cracking was seemingly done as "a matter of course" at Combe Grenal. That is, most marrow-yielding bones that were introduced showed evidence of cracking. What is most variable at Combe Grenal is the incidence of their introduction. For moderate-sized animals, particularly red deer and reindeer, there is good evidence that a much more representative anatomical inventory was introduced into the site. Meat-yielding bones are common, and there is considerable variability among the levels in the degree to which lower limbs were introduced. The lower-limb bones that were introduced were regularly exploited for marrow; sometimes even the phalanges of reindeer were broken. Particularly striking is the general absence of nonhominid gnawing of the bones from moderate-sized animals. This contrasts markedly with Klasies River Mouth and all the earlier sites discussed here. This is taken as good evidence that the majority of the moderate-body-sized animals at Combe Grenal *were hunted for meat.* This pattern is particularly striking for the faunas of Würm II levels at the site.

Particularly interesting is the distribution of tool-inflicted marks on the Combe Grenal bones. First, the number of such marks is relatively rare compared to the frequencies reported at Olduvai Gorge (see Binford 1981:99; cf. Bunn 1983:27). In addition, most of the marks are clearly related to dismemberment of carcasses and removal of muscle tissue. As has been pointed out before, many of the tool-inflicted marks on the much earlier faunas are biased in favor of lower-limb bones and along the shafts of nonmeaty bones. This was also true of many of the marks from the Vaufrey level VII bones. These summary statements should not be read to imply that there is little variability at Combe Grenal; there most certainly is. Nevertheless, the overall picture contrasts markedly with the earlier European sites thus far known.

The European sites from the Rissian age (100,000–300,000 years ago) exhibit a very different pattern than that noted above. It is my impression that hunting seems most likely indicated for only small animals and rodents—particularly rabbits, which are common in such early European cave sites as Lazaret (Jullien and Pillard 1969) and the earlier Rissian levels of Combe Grenal (personal observation). Scavenging appears to have been the major method of meat procurement, and it should be emphasized that there is a bias in favor of scavenging meat as opposed to marginal, marrow-yielding parts, which dominate the exploitation pattern for moderate to large animals at Klasies River Mouth. On the other hand, the exploitation of marginal parts is certainly carried out in Europe, as is evidenced at Vaufrey level VII and Hoxne. Particularly interesting are the observations at Swanscombe (Mindel/Riss interglacial) and Vaufrey level VIII (early Riss). As noted, there are very few tool-inflicted marks at Swanscombe, which is believed to be a "field" location roughly at the source of scavenged carcasses. At Vaufrey, where there is definitely a bias in favor of meat-yielding parts transported to the location, there were *no tool-inflicted marks* on the bones. It is my guess that this situation reflects the fact that meat was frequently eaten directly from the carcass and rarely transported. This view is consistent with the situation at Swanscombe.

Tool use appears to be primarily related to processing of parts for rendering foods accessible. This suggestion is consistent with the pattern noted at Klasies River Mouth, where many of the lower limbs appear to have been at least partially desiccated before they were processed for marrow removal. When this was the case there appears to be much more tool-assisted processing related to the removal of dried skin and tendon prior to marrow removal than there is tool-assisted processing of fresh meat. If this suggestion is sustained by future research, then the high incidence of tool-inflicted marks reported from the Olduvai sites (Bunn 1983) may well be telling us that scavenging there was primarily of at least partially desiccated carcasses and consequently that meat would not have been a significant component of the scavenged diet.

I think it can be safely inferred that by Würm II times in south-central France hominids were regularly hunting moderate-body-sized animals. In addition, they were scavenging large-sized animals, such as aurochs and horses. Unlike the situation in southern Africa at roughly the same time, however, *scavenging was systematically for meat, not for marginal parts (such as marrow bones), although head parts appear to continue to be a major target of scavengers*. It is also my impression that, as hunting of moderate-body-sized animals increased in the Würm, less use was made of such small animals as rabbits, which are common in some earlier European cave sites.

During the Riss glacial episode and earlier the picture is quite different. There is no convincing evidence in the studied cases for regular hunting of anything except perhaps very small mammals and rodents. Scavenging appears to have been the primary method of obtaining meat from moderate- to large-body-sized animals. There appears to be a clear contrastive pattern between locations where essentially non-meat-yielding marrow bones were processed and sites to which meat-yielding bones were transported. There is also the hint that when meat was available, either at carcasses or as transported body parts, consumption was direct and largely not tool-assisted except perhaps in disarticulation tasks.

CONCLUSIONS

The picture that emerges from these studies is very different from what is generally considered to be the history of hominid subsistence evolution.

At the dawn of tool use the early hominids appear to have been scavenging carcasses largely for marginal foods and using tools to gain access to these tiny morsels, mainly bone marrow. That this can be taken as evidence for a "scavenging" mode of adaptation is highly unlikely, since the tactics indicated must have contributed only slightly to the modal subsistence security of these creatures.

Jumping far ahead in time to the period of the first appearance of hominids in the British Isles we see a very similar pattern but with perhaps a slightly different focus—scavenging appears to be much more meat oriented. This is also in a context that demonstrates much less nonhominid predator–scavenger involvement in the formation history of the sites and may point to regular consumption of meat largely unassisted by tools. At this time there is little that can be reliably inferred regarding "home bases" or sharing of food.

By the early phases of the Riss glacial sequence we note some interesting variety in the faunal assemblages associated with tools. There appears to be at least a dichotomous pattern of sites where marginal parts were transported for marrow-bone processing versus sites where meat-yielding bones were transported for meat consumption; however, there is little evidence that in the latter situation consumption was strongly tool assisted. Surprisingly, the tool assemblages associated with these two types of faunal assemblages are "typologically" the same, differing only in the amounts of exotic raw materials used in tool manufacture. Exotic raw materials were more frequent in the assemblage from the "meat"-dominated site than in the assemblage from the marrow-bone-processing site.

During the Würm some major changes occur. Hunting for medium-sized animals seems to be indicated. A more integrated exploitation of animal products seems to have been practiced, and the transport of parts regularly yielding more food than a single person might reasonably consume is present. In the geographically distant southern tip of Africa an analogous trend seems to be indicated in which hunting of small animals increases throughout the sequence and scavenging of marginal anatomical parts begins to fall off as a regular strategy.

Given differences in geography and environment the patterns from the northern temperate zone appear remarkably similar to the pattern seen in South Africa. *At present the inevitable conclusion seems to be that regular, moderate- to large-mammal hunting appears simultaneously with the foreshadowing changes occurring just prior to the appearance of fully modern man.* According to the principle laid down by Gilbert and Sullivan, "You can't be your own grandpa." *Systematic hunting of moderate to large animals appears to be a part of our modern condition, not its cause.*

IMPLICATIONS

It is perhaps even presumptuous to open up this topic since these are revolutionary and even "shocking" conclusions. I am convinced that the taphonomic study of faunal remains must be integrated with our more traditional studies of tool assemblages and site locational data. We have long recognized problems of tool assemblage variability, such as the alternation of developed Oldowan with Acheulian tools at Gadeb (Williams et al. 1979) or the alternation of flake- versus biface-dominated industries at Olorgesailie (Isaac 1977) and the arguments over the Acheulian and Clactonian of Europe (Collins 1969; Ohel 1979). We still do not know what these fascinating patterns indicate in terms of past behavior. It is true that the old idea that they represent "cultural traditions" is still common in the literature and most current textbooks present the known patterns of variability in this paradigm. Regardless of the conservative adherence to the "cultural tradition" point of view, the fact remains that we do not know what such variability indicates in terms of hominid lifeways. The arguments being currently set forth regarding the early hominids and the conclusions presented here about later hominid history clearly point to the fact that we are dealing with creatures very different from ourselves. The imposition of the assumption of culture on the premodern ancestors of man is no more justified than the imposition of a Bushman-like way of life on the early hominids.

The first important step in research growth is the recognition of ignorance, the acknowledgment that we do not know something. It is time to abandon our assumptive "knowledge" regarding hominid behavior and begin the hard-headed task of reducing our ignorance and hence contributing to our greater understanding of man's ancestors. This will not come about by fanciful "reconstructions" of the past or arguments about the plausibility of a "scavenging" adaptation. It will come about by the development of sound methods for reliable inference regarding the characteristics of the past implicated in such speculations. It may not be a bad place to start by considering the possibility that much of the interassemblage tool variability could have arisen in the context of independent feeding strategies between males and females in particular ecological settings during the Pleistocene. We may be seeing more female versus male "culture" tempered by situational conditions rather than ethnic "culture," as has been commonly assumed by archaeologists.

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NOTES

[1] Shipman has consistently speculated that they were after skin and tendons (see Shipman 1984b:22).

[2] One could argue that in fact the materials referrable to *Homo erectus* do not bear on the arguments regarding the behavior of early hominids. This is not likely given the materials I will discuss later, which represent hominids even more recent than *Homo erectus.*

[3] My strategy was one in which recognized ambiguity was eliminated from the assemblages being considered. Shipman (et al. 1981) followed no such strategy.

[4] This same argument would apply to small rodents and other animals that might have also been part of the early hominids' diet.

[5] Shipman et al. (1982) answered these criticisms by referring to her results of a chi-square test. Such a technique is inappropriate when a curvilinear relationship is known to exist, as it does between her data on known baboon carcasses unmodified by humans. Nevertheless, Shipman continues to maintain her interpretations (see Shipman 1983:43).

[6] Shipman (1983:37) is vague on the utility of these "signatures." She nevertheless uses them as if no ambiguity exists (see Shipman 1983:40).

[7] This is one of the points I made in the *Bones* book.

[8] Potts (1982) considered the evidence for water sorting in his dissertation, and after demonstrating much ambiguity on this issue he nevertheless concludes that water sorting played no significant role in conditioning the composition of the Olduvai fauna. Based on the data that he presents, this judgment is unjustified.

[9] This argument is presented as fact in recent textbooks; see Gowlett 1984.

[10] I do not imply seasonal "stability" in equatorial settings, only that temperature-related seasonality increases temporal and spatial variability in food availability by a large measure.

[11] I am well aware of the controversy (Shipman 1983 vs Bunn 1983) regarding the recognition criteria for cut marks. I am very skeptical of Shipman's claims for lack of ambiguity in identifications when her methods are used. I have seen many bones from geologic contexts in North America where human presence is unequivocally *absent* yet marks having all the characteristics she describes are nonetheless present! Similarly, many marks reported by Bunn as cut marks are in my opinion matched in control collections where young carnivores gnawed bones. At present I trust my experience in cut-mark recognition, which is based on a great deal of experience and employs a configurational approach (see Binford 1984) and recognized ambiguous cases.

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