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PREHISTORIC PATTERNS OF HUMAN BEHAVIOR

A Case Study in the Mississippi Valley

By BRUCE D. SMITH

With a Contribution by WILMA WETTERSTROM

A Volume in the STUDIES IN ARCHEOLOGY Series

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The book gives background information on the Powers Phase, a small Middle Mississippian site composed of two habitation structures and a series of nine pits, explaining the research procedures used, and describes the artifactual materials, ecofactual materials, and cultural features uncovered at the site. Of special interest is its analysis – using, in part, nearest-neighbor analysis – of the spatial patterning of material remains at the site. The interpretive core of the book uses the data to evaluate alternative hypotheses and their associated observational predictions, thereby demonstrating the application of the scientific method to archaeology.

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MONTE ALBÁN

Settlement Patterns at the Ancient Zapotec Capital

By RICHARD E. BLANTON

With Contributions by WILLIAM O. AUTRY, JR., STEPHEN KOWALEWSKI, CARL KUTTRUFF, ELSA REDMOND, and CHARLES SPENCER

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Nunamiut Ethnoarchaeology

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Nunamiut Ethnoarchaeology

Lewis R. Binford

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Contents

Introduction

Before I delve into the details of my work among the Nunamiut, I want to set forth the ideas that prompted the study. These are very basic ideas, indeed—they focus on the question of precisely what archaeologists do.

Archaeologists attempt to make systematic observations on the remains of past human behavior; that is, they investigate the archaeological record. The archaeological record, however, is contemporary, and any observations that I, as an archaeologist, make through the excavation of an archaeological site are contemporary observations. My interest is in the past but my observations are on the present.

To pursue my interests I must accomplish two quite separate kinds of acts: (*a*) I must project my contemporary observations accurately into the past and (*b*) I must assign meaning to my observations. I accomplish the first through methods of dating. I may then examine these projected and temporally arranged observations for forms of patterning. If I find variability in patterned configurations, I have some evidence for dynamics, changes that occurred in the past. I know that something happened, that some dynamics were operative, but I do not know why the changes occurred; neither do I know anything about the character of the changes. To make a statement about the character of changes I must first *assign meaning* to the contemporary facts of the archaeological record.

Suppose I observe that a metal tool is present in a particular archaeological site. Examining additional sites, I may note that metal tools are present in some but not all of the sites. Accurate dating tells me that all the sites without metal tools are older than the sites with metal tools. I may assert that this pattern reflects the events of the invention of metallurgy. If I also discover metal tools in archaeological sites in adjacent regions at time periods increasingly more recent as a function of their distance from the region initially observed, I may assert that this pattern reflects

the spread of metallurgy to other places. The definition of such patterns in no way tells me *why* they exist. My assertions are descriptions of the world as known and do not provide answers to the question of why the world is the way it appears.

Let's take another example. If I (*a*) observe a series of small, molded objects in the form of a human female, (*b*) assert that each object is a mother goddess, (*c*) project into the past a series of archaeological assemblages, some of which yield mother goddesses, (*d*) recognize a patterned distribution both temporally and spatially, and (*e*) ask the question, "Why were mother goddesses invented and increasingly distributed over wider geographical areas?" I have already restricted my thinking to a particular context involving religion, cults, ritual behavior, and the like. I may then seek to understand the distribution and the context of appearance of these little female effigies in terms of arguments about the role of religion in human life, the symbolic importance of females and fertility, and so on. But suppose I had a time machine and was able to determine that the objects are not mother goddesses but toys, or perhaps magical devices used to divine the sex of children before birth. Under these ascriptions of "meaning" I would be directed to pursue very different lines of thought in seeking an explanation for the effigies' appearance and geographical spread. If I am to make accurate statements about the past or even to engage in relevant forms of thought I must have a relatively accurate understanding of the context in which the facts of the archaeological record came into being.

The relevant past to a set of static facts of the contemporary archaeological record can only be the conditions that brought the observed facts into existence. Much of the history of archaeological work has been characterized by changing views as to the conditions producing archaeological facts. For many years—and even today in many places—the dynamic standing behind an archaeological fact was thought to be simply the maker of the artifact. Thus, the archaeologist might view a recovered artifact and make judgments as to the skill of the maker, his artistic sense, and the degree that he seemed to share certain artistic values of the archaeologist's culture. The meaning archaeological remains carried became a statement on the character and quality of the maker. If artifacts were crude by the archaeologist's standards then the makers were crude. If artifacts were "beautiful" then the makers were advanced and had "advanced" aesthetic senses.

Gradually, this paradigm for giving meaning to the contemporary facts gave way to other arguments. It was reasoned that a person's overall intelligence or capacity for "humanness" is not necessarily directly translatable into accomplishments, and that many intelligent men can produce crude products. Factors other than those intrinsic to the artifact's maker condition his behavior, so we can not legitimately use the human products recovered archaeologically as a statement on the "quality" of the producers; rather the products are to be seen as a statement on the "culture" of the makers. For instance, according to a commonly cited statement, cultures are "historically created designs for living, explicit and implicit, rational, irrational, and non-rational, which exist at any given time as potential guides for the behavior of man [Kroeber and Kluckhohn 1952:97]." Built into this definition of culture are the rules for its own explanation. Culture is said to be historically created. Thus it is not surprising that, viewing human products as reflections of the culture carried by the makers, we can hope to convert contemporary observations into statements about past culture. We can compare the contemporary facts uncovered as a result of excavation with other, similarly recovered remains, evaluate the differences and similarities, and arrange them taxonomically to express the degree to which they indicate shared culture. When a temporal assessment can be made, we can trace, through our assessments of similarities and differences, the history of cultural development.

This view proposes that culture, the ideas or "ideational guides for living" held in the minds of men, is simply projected into their products. The products can thus be viewed as accurate reflections of mental templates, so culture in turn can be seen as a model of past dynamics normally discussed in the context of artifact production. Few would disagree that planning is characteristic of acts of fabrication and that fabrication plans are guided by some ideas regarding the desired outcomes. This "fabrication model" comes into question when we ask whether it is relevant to all facts and patterns observable in the archaeological record. Is it an adequate and accurate dynamic model that accounts for the frequency variability in an archaeological assemblage? Do makers arrive on a site and proceed to fabricate an assemblage of tools relative to a mental template of what an appropriate "assemblage" should look like? Is it reasonable to expect that men carry in their heads fabrication plans for what archaeological sites should look like after they leave? An uneasiness with the basic model linking the dynamic past to the static facts of the contemporary archaeological record leads me to question the relevance of this linking model.

Under the "traditionalist" paradigm, the composition of an assemblage is measured by relative frequencies of recognized classes of artifacts. Redundancy is accepted as "patterning" and hence a manifestation of the "cultural norms" of behavior transmitted and shared among the people represented. The assemblage is equated with the community. The expectation is that if we are dealing with the remains of identical or related groups of people the composition of the assemblages should be similar since they share a common body of culture. I challenged this paradigm:

> The behavioral model recognizes that behavior is the dynamics of adaptation. People draw upon a repertoire of cultural background and experience to meet changing or variable conditions in their environment, both social and physical. Our expectations, then, are for variability in the archaeological record to reflect a variety of different kinds of coping situations. Activities will vary with the particular adaptive situation of the group and the character of tasks being performed. We would therefore expect variability in the archaeological record to reflect these different situations.
>
> Assemblages may therefore be expected to exhibit variability concomitant with the various "structural poses" (Gearing 1962) of a community through its annual adaptive cycle. In addition, many assemblages may be expected to vary directly with the degree to which the community may be partitioned into specific kinds of task groupings for performing work at different locations. In short, assemblage variability may be expected to exhibit or reflect a variety of segments of community life and cannot always be expected to exhibit similarities as a direct reflection of the continuities among the persons performing the acts. Similarities may equally reflect continuities in the character of the acts performed. Differences may arise when the organization of activities varies temporally and/or spatially, resulting in a variety of assemblage types characteristic of the life of a given community [Binford 1972:132].

At the time I wrote the foregoing statement there was no direct empirical support for the argument. It represented a plausible view of archaeological formation processes, but there were no empirical studies to demonstrate that what was plausible was indeed realistic.

In addition to questioning the character of the linkage between past dynamics and present static data, I was issuing another challenge to the then prevailing view of the past. Under the fabrication model characteristics of the archaeological record were linked directly to differing mental templates, which in turn were tabulated and summarized as a "trait list" of enumerated culture. I was suggesting not only that the archaeological record derives from an adaptation but also *that adaptations have all the properties of a system* in which various components are responsive to one another in their ongoing operation. The basic elements or components of the system are units of organization rather than discrete elements as viewed by us in the contemporary world. One cannot understand the workings of an automobile through a strategy that enumerates all the screws, nuts, springs, and other parts. One must first develop some way

of recognizing basic functional components, such as carburetors, distributors, and voltage regulators, and then seek to understand how these units articulate with one another and interact under differing conditions external to the system to which they are responsive. Ways of developing a realistic appreciation for the characteristics of a system of adaptation must be sought, since the model of an automobile or other handy mechanical system is apt to mislead us if we attempt to use it to appreciate the organizational properties of a flexible behavioral system. Such flexibility can be thought of as deriving from responsive modifications through the use of alternative strategies and variable means to accomplish similar ends.

Under this systematic view of past dynamics we might anticipate much variability in the archaeological record that is directly referable not to differences between systems, but to *differing states of a single system*. We might also imagine that systems differing in overall organization could well share very similar strategies of adaptation. Sites that actually represent analogous situational states might be grouped, under the traditional methods of analysis, as similar systems. Such arguments are plausible, but as with the basic challenge to the fabrication model itself there was no empirical demonstration supporting them.

While such "processual" views of the nature of the linkage between static archaeological facts and past dynamics were being developed, argued, and made available in the literature, I was engaged in research and controversy primarily with François Bordes regarding the appropriate meaning to be given to certain archaeological facts regarding Mousterian material from both Europe and the Near East (see Binford 1972; Binford and Binford 1966, 1969). These arguments were not related to the character of processes responsible for culture change, evolution, and the like; they were a direct confrontation regarding the assumed linkage between contemporary archaeological facts and past dynamics. We differed on the character of the dynamics believed responsible for observed archaeological facts and therefore on the meanings that could be rationally assigned to them.

In a more basic sense, we were arguing about the relevance of concepts and in turn the operational definitions associated with these concepts. Definitions are basic tools used to give meaning to what we see. They provide the links between concepts, our tools for thinking about the world, and observable properties of the world. It is the operation of translating perceptions into cognitive units that provides the meaning for sensory experiences. The concepts are the conventions that give the "paradigmatic" character to any field of endeavor. If we question the utility either of concepts or of the definitions associated with them, we are questioning the basic "culture" of a science. At this point we are not questioning a theory about the way the world works, since such theories are always phrased in terms of understandings about the way the world is. Theories attempt to answer the question, "Why is the world the way it appears to be?" Of course the way the world "appears to be" is conditioned quite directly by our paradigm or set of conventions for giving meaning to experience. We may question the usefulness of theories and still operate in terms of what Kuhn (1964) has called *normal science*. That is, although we may be skeptical about a given explanation for the world as known, such skepticism in no way questions our tools for knowing. We do not challenge the conventions whereby we give meaning to experience.

On the other hand, if we do question these conventions—the utility of our concepts and the rules for their appropriate use—we are questioning the very character of our alleged knowing, the character of our understanding of the way the world is, not simply ideas as to why it is that way. Normal science in Kuhn's (1964) terms proceeds in the context of an assumed paradigm. Normal science is concerned with evaluating the usefulness of explanations offered in answer to the question of why the world is the way it appears to be. The methods of hypothesis testing, deductive reasoning, and so on that characterize an epistemology based on scientific methods in no way address themselves directly to the problem of knowing what the world is like; they are part of a procedure for evaluating propositions as to why it is the way it appears.

When we do question our conventions for knowing we are questioning quite directly our view of the world, the meanings we give to experience. This questioning produces what Kuhn (1964) has called a *crisis period*—a period during which the conventions for knowing are uncertain and no one knows how to give meaning to what he sees. During such a period, therefore, there is no agreement on problem, procedure, or aims of a science.

> In such times of crisis this conflict over the aims of science will become acute. We and those who share our attitude will hope to make new discoveries; and we shall hope to be helped in this by a newly erected scientific system. Thus, we shall take the greatest interest in the falsifying experiment. We shall hail it as a success, for it has opened up new vistas into a world of new experience. And we shall hail it even if these new experiences should furnish us with new arguments against our own most recent theories. But the newly rising structure, the boldness of which we admire, is seen by the conventionalist as a monument to the total collapse of science. . . . In the eyes of the conventionalist one principle only can help us to select a system as the chosen one from among all other possible systems, it is the principle of selecting the simplest system—*the simplest system of implicit definition*, which of course means in practice the "classical" system of the day [Popper 1959: 80–81].

During times when the paradigm has been questioned, when the definitional conventions for giving meaning to experience are under attack, there is great confusion. Some seek to evaluate "new" concepts and definitions through the methods of normal science—the use of deductive reasoning, hypothesis testing, and the like. These methods do not work. They were designed as methods for evaluating ideas about the way the world works. Under crisis conditions we must evaluate our rules for knowing, not proposals for understanding what is known. To my knowledge there are no procedures that are clear-cut when a paradigm is questioned. At such times we can only attempt to demonstrate that the world is different from the picture we would obtain had the conventions previously assumed been applied to given experiences. We must attempt to invent new conventions more appropriate to our experience and seek to eliminate ambiguities that might plague the use of these new conventions. This is a problem of developing new concepts and associated "operational definitions," new links between experience and our tools for meaningfully describing it. We may seek to justify both the concepts and their definitions to our colleagues. Such justification normally consists of arguments as to the reality basis for the concept, and in turn the utility of the concept for thinking about experience.

Science is based on the premise that the external world is knowable, and knowable *directly*; that is, it is accessible. When our tools for apprehending the world are questioned there is but one recourse: *to seek experiences in the world*, experiences that can elucidate the usefulness and accuracy of our tools for apprehending and describing reality. In turn we follow a strategy that, it is hoped, will prompt our inventiveness such that new concepts will result, new tools for describing more accurately the properties of nature. As Francis Bacon suggested (in Commins and Linscott 1947: 78–85),

> Man, being the servant and interpreter of nature, can do and understand so much and so much only as he has observed in fact or in thought of the course of nature; beyond this he neither knows anything nor can do anything. . . .
>
> . . . we must lead men to the particulars themselves, and their series and order; while men on their side must force themselves for a while to lay their notions by and begin to familiarize themselves with facts.

The search for certain, relevant experience is the concern of this book. I am not directly

involved in hypothesis testing. I am not involved in a direct way with the problem of explanations. I am concerned with sharing a series of concrete experiences sought in the hope of uncovering some of the links between an ongoing living system and the static archaeological products resulting from the dynamics of the situation.

On the other hand, this is not a blind appeal for empiricism. Nor is it a contradiction of my earlier arguments in favor of the use of logico-deductive strategies in the important task of seeking verification for our ideas as to how the world works. Here, however, we face the serious problem of what the world is like.

In seeking experience, the problem of relevance and relevant experience is crucial. The paths that led me to decide in favor of the experiences reported here as relevant are perhaps informative.

In this book I discuss in some detail my observations on animal anatomy and the ways the Nunamiut Eskimo behaved toward animals. Why have I focused on fauna, and why have I chosen to study the Nunamiut Eskimo? Several lines of reasoning led me to concentrate my studies in these two areas. In 1966 my former wife and I argued that:

> if we assume that variation in the structure and content of an archaeological assemblage is directly related to the form, nature, and spatial arrangement of human activities, several steps follow logically. We are forced to seek explanations for the composition of assemblages in terms of variations in human activities. The factors determining the range and form of human activities conducted by any group at a single location (the site) may vary in terms of a large number of possible "causes" in various combinations. The broader among these may be seasonally regulated phenomena, environmental conditions, ethnic composition of the group, size and structure of the group regardless of ethnic affiliation. Other determining variables might be the particular situation of the group with respect to food, shelter, supply of tools on hand, etc. In short, the units of "causation" of assemblage variability are separate activities, each of which may be related to both the physical and social environment, as well as interrelated in different ways [Binford and Binford 1966: 241].

This "assumed" view questioned the relevance of the previously discussed fabrication model and the linked additive or enumerative view of an adaptation. We were suggesting that the dynamics out of which the properties of an assemblage were derived were different from the dynamics traditionally argued for the production of an artifact. In short, what we questioned was the relevance and utility of the concept of culture to the facts of assemblage composition when viewed in terms of a fabrication dynamic. In the case of the mother goddess, if we accepted the equating of assemblage composition with "mental template" or "design for living" or "culture," then an explanation for the patterning demonstrated by Bordes among assemblage types would rest with arguments as to what would condition the differential presence of different cultures in certain regions of France during the Mousterian era. On the other hand, if we rejected the relevance of the concept of culture to the facts observed by Bordes such an explanation would not be acceptable. The solution to such a dilemma does not rest in hypothesis testing; it remains in the domain of evaluating the utility of a concept and in this case its linked model of dynamics. Throughout the 1966 study we sought to justify as plausible such a view of the dynamics standing behind the facts of assemblage composition. At that time, since the targets of controversy were stone tools and archaeologically observed variations in the relative frequencies of similar tool classes, we thought we might be able to demonstrate certain organizational facts about tool assemblages that would support our view that assemblages were composed of organized sets of tools and that assemblage variability derived from the varying degrees in which these sets were represented. We acknowledged that we had no way of reliably giving behavioral meaning to the tools themselves; however, this did not stop us from offering certain guesses.

Although I was "impressed" with the results of the 1966 study I was very uncomfortable with the situation. I was proposing that

tool frequencies varied with activity differences. Clearly what was needed was some way of identifying activities, some concepts with linked definitions that would permit me to recognize a past activity from empirical properties of sites, assemblages, or the tools themselves. I first thought that detailed studies of wear patterns on the tools might permit the identification of function. However, results of lithic studies overwhelmingly demonstrated that wear-pattern analysis yielded ambiguous results. A variety of activities could generate similar wear patterns, and, conversely, similar activities could generate different wear patterns.

I became discouraged with this avenue of research and turned to the problem of developing techniques that could be used for isolating activity areas. I hoped that if we could see such areas then we might be able to develop concepts and definitions sufficient to identify activities. I hoped to be able to give meaning to archaeological facts in terms of past behavior.

In 1967 I began to develop another line of argument. Activities are investments of labor in modifying resources for use. If I could relate patterns of tool association, and spatial ordering on living floors, to certain kinds of resources then I might be able to make statements from the static facts of the archaeological record about the behaviors that generated the facts. Under a National Science Foundation grant, I began the detailed observation of the faunal remains and the horizontal distributions of fauna and tools recovered from the deeply stratified Mousterian site of Combe Grenal. A year spent in France recording the bones and laboriously developing the distribution maps of the different levels of Combe Grenal generated a wealth of new data about the site. I found variability among the levels in the species present, and some correlations between species frequencies and tool frequencies. Distribution patterns of tools and fauna varied from level to level, and in turn there was some correlation between pattern differences, tool frequencies, and environmental variables. I was able to make the following summary statements:

A. The number of animals represented in any one occupation zone is relatively small. It is therefore reasonable to suggest that the occupations at Combe Grenal were of relatively short duration, and that group sizes, although variable, were generally small.
B. There are clear differences in the relative frequencies of anatomical parts of various animal species.
    1. *Bovids and horses* are represented by analogous anatomical parts and are clearly differentiated from reindeer and deer in the parts present.
        a. Bovids are primarily represented by mandibular fragments, lower teeth, fragments of the tibia, femur, humerus, and radio-cubitus. Ribs, vertebrae, pelvis parts, skull fragments, metapodials, and phalanges are rare.
        b. Horses are primarily represented by mandibular fragments, lower teeth, and fragments of the tibia, femur, humerus, and radio-cubitus. In contrast to the bovids there is much greater variability in the frequency of maxillary teeth. In some levels there are more maxillary teeth than mandibular teeth. The latter generally occur in levels where numerous horses are represented. Ribs, vertebrae, pelvis parts, skull fragments, metapodials, and phalanges are rare, as they are for bovids.
    2. *Deer and reindeer* bone remains are not as similar as the remains of bovids and horses. However, the similarities are such that they can be

considered in a grouping separate from bovids and horses.

a. There is much greater variability between different occupations in the anatomical parts of deer and reindeer represented than is the case for either bovids or horses.
b. All previously published patterns of variation in anatomical parts are represented among the deer and reindeer remains from the occupations of Combe Grenal. Frequencies analogous to those noted on kill sites (Dibble and Lorrain 1968; Kehoe 1967; White 1954) are represented. Similarly, frequencies analogous to two recognized patterns documented for semipermanent settlements on the Plains of North America (Wood 1962) are also represented, and there are other patterns of variation not previously documented.

C. There are marked and contrasting patterns of variability in the anatomical parts represented from a single species recovered from different occupational zones in Combe Grenal.
D. There are no bone samples from Combe Grenal in which all the anatomical parts of any animal are represented in expected proportional frequencies based on their frequency in the skeleton of the animal.
E. There are clear correlations crosscutting the recognized types of assemblages between some tool types and the pounds of meat represented by certain species. In addition, there are correlations crosscutting the recognized types of assemblages between some tools and the total amount of meat represented regardless of species.
F. There are correlations crosscutting recognized types of assemblages between some tool types and particular parts of certain species.
G. There is no demonstrable directional change in the patterns of variation among anatomical parts from the bottom to the top of the deposit.
H. There are some correlations between faunal components and the four types of Mousterian assemblage recognized by Bordes.

Despite this new knowledge, the facts did not speak for themselves; I had no way of reliably giving meaning to what I had seen, of relating my observations to the dynamics of the past from which they derived. I was frustrated. There seemed to be no unambiguous way of demonstrating the inadequacy of the fabrication model of dynamics as an explanation for the facts of assemblage composition.

One of the findings that emerged during the Combe Grenal study was the existence of considerable interspecies and intraspecies variability in the relative frequencies of anatomical parts among the several archaeological levels. Other investigators had attempted to give meaning to such differential frequencies. Theodore E. White was one of the first to do so. In general, White viewed frequency variations as resulting from varying butchering practices and considered that these practices might well differ between societies (White 1952).

Working independently of White, Raymond Dart investigated over 7000 animal bones associated with *Australopithecus*. Dart found serious disproportions in the skeletal parts preserved as fossils. Certain parts were common in the assemblage, whereas others were generally missing. Dart interpreted these differences as the result of australopithecine hunting and tool-using behavior:

> The disappearance of tails was probably due to their use as signals and whips in hunting outside the cavern. Caudal and other vertebrae may also have disappeared because of the potential value of their bodies as projectiles and their processes as levers and points... femora and tibiae would be the heaviest clubs to use outside the cavern, that is probably why these bones are the least common. Humeri are the commonest of the long bones; probably because they would be the most convenient clubs for the women-folk and children to use at home [Dart 1957:85].

In 1968, Dexter Perkins and Patricia Daly reported on the fauna from a "Neolithic" village excavated in Turkey. They investigated the relative frequencies of anatomical parts recovered for several species. They observed that, among the bovids, bones of the upper legs were strongly underrepresented compared to the bones of the lower legs. They argued that

> the missing leg bones indicate that the cattle were slaughtered near the village....
>
> When a... hunting party killed a wild ox, they apparently butchered it on the spot and used the animal's own hide as a container for carrying the meat home. They evidently stripped the forequarters and hindquarters of meat and threw the leg bones away. They apparently left the feet attached to the hide, perhaps because the feet made convenient handles for dragging the meat filled hide [Perkins and Daly 1968:104].

R. E. Chaplin discussed the low frequency of sheep upper-leg bones compared to the frequency of lower-leg bones at a Saxon site near London. He concluded that this discrepancy resulted from the "export" of the meat joints from the site and was therefore indicative of trade (Chaplin 1969:233–244).

White (1954) suggested that low frequencies of the upper-leg bones at the site studied from the Plains was the result of their destruction beyond recognition during the aboriginal manufacture of bone grease (White 1954:256). Underrepresentation of the bones of the lower leg at living sites was a result of their having been abandoned at the kill site (White 1954:256). The latter suggestion was borne out by the higher frequencies of lower limbs on demonstrated kill sites (Dibble and Lorrain 1968:100; Kehoe 1967:107).

In these examples the same phenomena—high frequencies of lower limbs and low frequencies of upper limbs—are interpreted as indicative of (*a*) use of the upper limbs as clubs away from the site, (*b*) abandonment of upper limbs at kill sites and introduction of lower limbs into living sites, (*c*) removal of upper limbs from the living site through trade in meat, (*d*) destruction of upper limbs at living sites through the manufacture of bone grease, and (*e*) abandonment of lower limbs at kill sites and the removal of meat bones to living sites. The only interpretation that appears to be borne out through comparative study of documented cases is the last one, which is directly opposite to the interpretation placed on the same phenomena by Perkins and Daly (*b*).

The complexities of the problem can be further demonstrated by a summary of the suggestions offered in the literature to account for observed frequency patterning:

A. Suggestions offered to account for differing proportional frequencies observed at a single site between anatomical parts of a single species:
   1. Removal or destruction of some parts as a function of their use as tools or as raw materials for tools (Dart 1957:85; Kitching 1963:49–53; White 1953b:162, 1954:177)
   2. The differential destruction of parts during butchering (Kehoe 1967:72; White 1952:338)
   3. The differential transport of anatomical parts from kill sites to sites of consumption (Dibble and Lorrain 1968:103–103; Kehoe 1967:72; Perkins and Daly 1968:104; White 1952:162)
   4. The differential transport of parts away from sites of consumption in the context of trade (Chaplin 1969:223)
   5. The differential destruction of anatomical parts during consumption by man

a. Direct eating of soft bone parts (Brain 1969:15–16; White 1954:171)
b. Destruction of bone parts as a result of marrow extraction (Brain 1969:15; Kehoe 1967:72; White 1954:258)
c. Destruction of bone parts as a result of pounding of bones for the extraction of bone grease (Kehoe 1967:72; White 1953b:162)

6. The differential destruction of bone parts on sites of consumption by domesticated dogs (Brain 1969:15; Dibble and Lorrain 1968:93; Guilday n.d.:7; White 1954:256)
7. The differential destruction of anatomical parts on kill sites or abandoned living sites by carrion feeders (Brain 1969; Kitching 1963:22–23; Voorhies 1969:20)

B. Suggestions offered to account for differences noted between species in the proportional frequencies of anatomical parts represented within a single deposit:

1. The differential degree of butchering of different species at kill sites as a fuction of their size and portability (White 1953b:160, 1954:255)
2. Differences between domesticated and wild forms, resulting in domesticated forms being butchered at the site of consumption and wild forms being butchered at kill sites (Perkins and Daly 1968:104; White 1954:172)
3. Differential destruction of parts from different species as a function of differences in the size and strength of analogous anatomical parts (White 1954:256)
4. Differential food preferences for analogous anatomical parts from different species (White 1952:337)

C. Suggestions offered to account for differences noted between proportional frequencies of anatomical parts from a single species recovered from different archaeological sites:

1. Functional differences between the sites, notably kill sites versus village sites (Clark and Haynes 1970; Dibble and Lorrain 1968:102; Kehoe 1967:72; White 1952:337)
2. Ethnic differences in food preferences and butchering practice between the social groups represented at different sites (Dibble and Lorrain 1968:102; Kehoe 1967:72; White 1954:254; Wood 1962:203)

Most if not all of these suggestions have some basis in ethnographically known behavior. Many are specifically documented as affecting the differential introduction or destruction of anatomical parts at archaeological sites. What is clearly lacking from our current understanding is a *specific knowledge* of the particular effects that might be expected to result from any of the activities mentioned.

The picture is further complicated by the research of C. K. Brain (1969). Largely stimulated by the work of Dart, Brain investigated the remains of goats found in recently abandoned Hottentot villages. In a situation where the animals had been slaughtered in the village and no meat traded, he found that there was still a marked discrepancy in the frequencies of anatomical parts recovered. In an attempt to account for the missing parts, Brain conducted preliminary studies of the specific gravity of the different bones and the differential fusion time of articulator ends of long bones. He demonstrated that the survival of identifiable bones varied directly with the specific gravity of the part and inversely with the fusion time of the articulator end. On the basis of these findings, he suggested that most if not all the discrepancies observed by Dart in the Makapansgat fauna could be understood solely as a function of the differential durability of

bones subjected to destructive natural agencies. Thus, Brain's data appear to be a major challenge to those who see behavioral or cultural significance in differential bone frequencies. Brain's conclusions are supported by the observed differential frequencies of anatomical parts observed in pre-Pleistocene fauna from North America. Voorhies (1969) has reported in detail on the frequencies of parts of animals preserved in an early Pliocene deposit in Nebraska. The animals represented in this deposit lived in North America long before man was present, so human behavior and cultural practices could not possibly have contributed to the observed discrepancies in anatomical part frequencies. Voorhies concluded that the activity of carrion feeders, differential sorting by natural agents, and the differential breakdown of parts as a function of their strength all contributed to the observed frequencies.

Clearly this is a provocative set of findings and one that is of great importance to the archaeologist. I have followed up on these suggestions and my findings regarding processes of natural attrition have been published elsewhere (Binford and Bertram 1977).

In spite of the ambiguities and the clear lack of any reliable procedures for giving meaning to faunal facts, I reasoned that research in this area might provide the needed context for evaluating the models of past dynamics and therefore the general relevance of the concept of culture to facts of assemblage composition.

The obvious advantage of research with fauna is that there is no question about the irrelevance of the fabrication model with regard to the character and formal properties of the bones. Man did not make the bones; their form is not a product of any mental templates or designs for living. Therefore it becomes possible to set forth a completely culture-free taxonomy of bones. Any variability observed in the relative frequencies of anatomical parts among archaeological sites must derive from the dynamics of their use. Man's role is only one of partitioning, segmenting, and differentially distributing the segments of animal anatomy during the course of his exploitation of the animal. Little question could exist that any patterning derived from the faunal remains reflected use.

In spite of these obvious advantages, I still could not see how to demonstrate that the activities of use were inappropriate to a fabrication model for the formation processes of the archaeological record. Such a situation had frequently been pointed out by my critics. It had been asserted that activities are also cultural—that is, the fabrication model of dynamics applies equally to activities of use as it does to the activity of tool production. To this assertion there is no appeal except to the empirical world. Do people behave this way? Do people conduct their ongoing activities in terms of invariant mental templates as to the appropriate strategies regardless of the setting in which they find themselves? Do members of a given cultural unit, equal participants in a tradition, fabricate tools for use in their activities in terms of a shared ideal as to what their assemblage should look like in terms of the relative frequencies of tools or other elements? Are the results of actually coping with the world isomorphic with the traditionally passed on ideas as to means for coping? Are the cultural means independent of the problems presented to a group for solution?

To obtain answers to such questions about the way the world is, one must investigate the relevant world. What is relevant here? Would seeking additional empirical experience through the excavation and subsequent analysis of additional archaeological facts provide the relevant experience? The answer must be no. The relevant experience is one where we can directly experience the character of the linkage between the archaeological by-products and behavior. At the same time we must be able to evaluate the behavior relative to the degree to which it is differentially responsive to situational variables, the degree to which individuals sharing a common tradition or body of common knowledge use this shared culture differentially in dealing with situational differences arising from the

dynamics of their environment, and the character of the adaptive interaction between persons and their environment. It is necessary to experience directly the process of adaptation and in turn the archaeological products of this process. Relevance is achieved when we can examine variability in the archaeological products and hold culture a constant. In this situation, we could directly evaluate the utility of the fabrication model of behavior as the assumed link between the dynamics of behavior and the static facts remaining for us to observe.

Such experience can never be obtained from empirical work with archaeological remains. We must therefore proceed along the research path forged quite eloquently by our sister discipline geology in its adoption of the proposition of uniformitarianism. Is the formation of archaeological remains as a by-product of adaptive behavior a process that is operative in the contemporary world? Can we directly experience this process relative to a domain of facts that are observable in the archaeological remains from the past? If so, we may experience this crucial linkage of behavioral dynamics and statics. If we find our assumptions about the nature of this linkage to be inadequate, we are perfectly justified in rejecting these assumptions, since their general adequacy must be demonstrable if they are to be used universally to give meaning to the archaeological record.

My conclusion was that the formation processes of archaeological remains were indeed common to both contemporary and past eras. Many of the animal species present in assemblages are still extant, and the processes of exploitation and use operative in the past are still operative today.

The study of fauna offers another advantage: Since we can assume that earlier populations used animals primarily as food we can evaluate the food utility of various anatomical parts by studying representatives of modern species. This would permit the objective assignment of "utility" values to anatomical parts, thereby providing a meaningful reference dimension for our taxonomy of bones. Such an assignment of utility seemed impossible to work out for stone tools. How could we develop a procedure for giving utility values to stone tools relative to different potential uses? Using fauna we could not only employ a culture-free taxonomy for tabulating relative frequencies of faunal elements in different sites, but we could develop a reference dimension of food value for the taxonomy. We could display patterns of assemblage variability against scales of resource utility as a basis for evaluating the degree to which the patterning reflected consistent strategies relative to the use of the animals. Patterning in faunal frequencies might then be reasonably viewed as resulting from variable strategies in the use of food sources.

Although the advantages evident in working with fauna were exciting, it was recognized that following up these advantages in whatever detail would not directly solve the problem of relevance. What was needed was a set of concrete descriptions of the dynamics of behavior resulting in static patterning in the archaeological record. The static patterning had then to be related to the behavioral dynamics so that we might evaluate whether differences in culture were or were not manifest in the patterning. It was deemed desirable to seek an opportunity to observe such dynamics with respect to the genesis of faunal assemblages. If we could elucidate the models of dynamics standing behind faunal variability we might then use such understanding as a reference dimension for evaluating variability in stone tools.

To achieve these ends I chose to conduct ethnographic work among the Nunamiut Eskimo of the central Brooks Range in Alaska. These people are hunters, reported to be over 80% dependent for their subsistence on a single species, *Rangifer tarandus* or caribou. If there was any place in the world where I could learn about the problems presented by a strong dependence on hunted food, how

these problems are solved, and how such solutions are manifest archaeologically in faunal remains, it would be with the Nunamiut.

My aims at the time of initiating fieldwork were simple: to learn as much as possible about all aspects of the procurement, processing, and consumption strategies of the Nunamiut Eskimo and in turn relate these behaviors directly to their faunal consequences. I hoped to accomplish this for most if not all of the locations used by the Eskimo throughout a full seasonal cycle of their subsistence-settlement round.

The Nunamiut are inland Eskimo, currently localized in a sedentary community at Anaktuvuk Pass, at the drainage divide of the Brooks Range. (See Figure I.1 for the location of Anaktuvuk Pass and its relationship to the caribou migration routes.) The contemporary community is composed of two amalgamated bands, the Tulugakmiut and the Killikmiut, plus two attached families of the Ulumiut, a local band that broke up in 1942.

During the month of August 1969, the village of Anaktuvuk was composed of 126 permanent residents and 4 visiting Eskimos. The population was segmented into 21 households, 17 of which were composed of nuclear families (a husband and wife and their offspring only). Two households were composed of extended families—the nuclear family plus a widowed parent of the husband. The remaining 2 households were composed of unmarried adults with their offspring in one case and adult unmarried brothers in the other.

The Nunamiut have been studied pre-

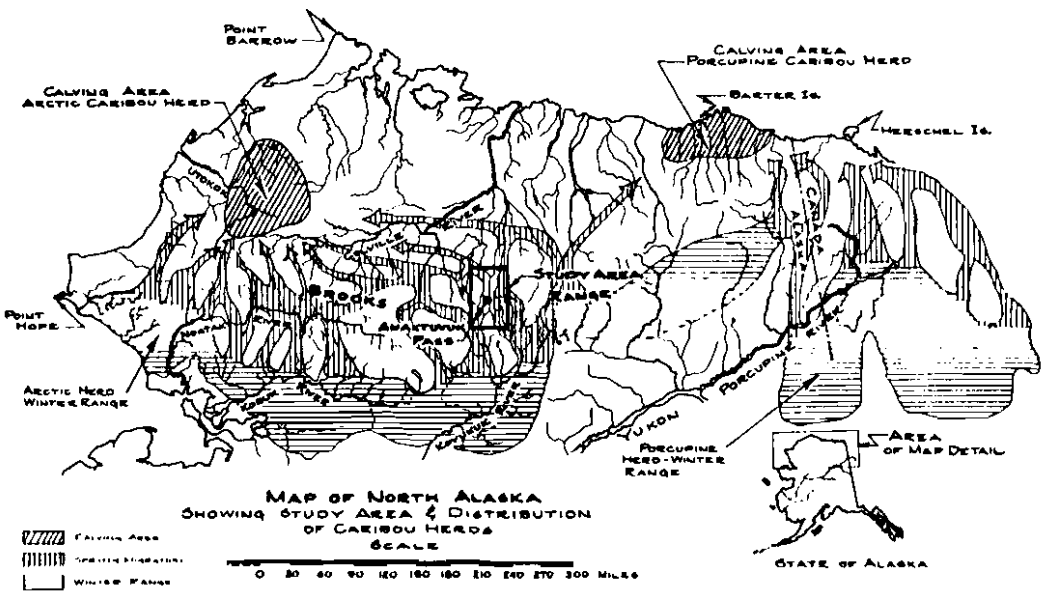

Figure I.1. Map of North Alaska showing location of research area. (Data on caribou movement from Hemming 1971.)

viously and a considerable literature covering various aspects of their society and patterns of adaptation is available (Binford 1975, 1976; Binford and Chasko 1976; Campbell 1968, 1970; Gubser 1965; Ingsted 1951; Pospisil 1964; Rausch 1951). Other researchers have summarized various aspects of Nunamiut life or conducted short-term interviews with some of the more famous "old men" at Anaktuvuk (Burch 1972; Solecki 1950; Spencer 1959; these are just a few).

This book reports the results of the study of the economic anatomy of sheep and caribou, and my experiences with the Nunamiut Eskimo. Throughout I have described the faunal materials collected from the sites of known behavioral contexts among the Nunamiut. These assemblages have then been referred to anatomical scales of value developed through the study of animals, as a means to the evaluation of the Eskimos' behavior and their adaptive strategies.

1

The Economic Anatomy of Sheep and Caribou

The Nunamiut life-style is dependent primarily on caribou and secondarily on sheep. In order to study the archaeological remains of these animals, I had to have a measure of the utility of the various parts. What follows is an account of the butchering and analysis of representative animals and the establishing of utility indices for various anatomical parts.

MEAT UTILITY (the Distribution of Usable Muscle Mass on the Skeletal Anatomy of Caribou and Sheep)

The first step is to explain how I determined the relative distribution of muscle to bone in the anatomy of sheep and caribou. The anatomical data presented on live body proportions were obtained through the study of one caribou and two domestic sheep. The caribou, a prime (approximately 3- to 5-year-old) bull, was taken in August 1969 at Anaktuvuk. This animal was nutritionally sound and weighed 243.5 lb (live weight). The sheep—a 90-month-old female in poor health and suffering from poor nutrition and a 6-month-old lamb in good health and showing good nutrition—were butchered on November 21, 1973 and studied under ideal conditions in Albuquerque.

The sheep were butchered primarily with stone tools. Two different procedures were used for dismembering. The lamb was butchered the Navajo way (see Binford and Bertram 1977) and the old female was butchered the Eskimo way (with one exception—we did not remove the tenderloin).

At each butchering a dairy scale with a hanging bucket was set up. As each part was removed it was weighed and then cut into unit bone parts and weighed again. A knife was then used to clean each bone of all meat and tendon, and the bone was weighed again, providing data on the quantity of meat that would result from boning the animal, as was frequently done for purposes of drying.

A 8
B 9
C 0
D 1
E 2
F 3
G 4
H 5
I 6
J 7