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Bones Are Not Enough: Analogues, Knowledge, and Interpretive Strategies in Zooarchaeology

DIANE GIFFORD-GONZALEZ

Board of Studies in Anthropology, University of California, Santa Cruz, California 95064

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Zooarchaeologists have become accustomed to high levels of confidence in their inferences about the origins, functions, and responses to stress of animal remains. This confidence rests on the causal and functional links between attributes of these remains and the processes and contexts which generate them. Their investigations are presently moving toward wider inferences about the context and functions of bones in ancient hominids' behavioral systems and in regional ecosystems. This transition involves a shift toward lower levels of inferential confidence. These arise from several sources and must be dealt with differently. Zooarchaeologists now need a different set of inferential strategies than that which characterized their preceding phase of research. This essay explores the various causes of lowered inferential confidence and suggests strategies for coping with them. It advances a philosophical argument for juxtaposing multiple, independent clusters of relational analogies, drawn from a wide variety of sources, including those outside zooarchaeology.

> It is not easy to define scientific progress. It is characterized by an improved understanding of previously puzzling phenomena, by the removal of contradictions, by the opening of black boxes, by the possibility of making better predictions, and by the establishment of causal connections between previously unconnected phenomena.
>
> Ernst Mayr 1982, *The Growth of Biological Thought*

INTRODUCTION

This essay deals with emergent problems in zooarchaeological inference, but it has implications for any research building or using "middle range theory." It examines the transition from agent identification studies, and analogical inferences drawn from them, to more ampliative in-

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ferences about hominid behavior and ecology, based on patterning in the frequencies of various agents' traces. Although I will cite recent controversies over the meaning of patterning in bone assemblages, my aim here is not to determine who is "right" or "wrong." Rather, I am interested in why these debates are happening, despite much general agreement about the agents responsible for the traces. This essay explores why sorting out more and less probable inferences in such debates is more difficult than it was in agent identification studies. It discusses what inferential paths appear to be most productive to follow. In the process, this essay raises for faunal analysts, and for any archaeologists working with "uniformitarian" materials, questions about how to proceed in studying wider problems of human behavior and ecology using their materials.

In the last 20 years, zooarchaeological research on bone modification has dealt with topics that lend themselves well to experimental verification and to inferring past events by analogical reasoning. Experimental and naturalistic observations have more closely defined the morphologically distinctive traces of human actions on bone (Binford 1978a, 1981; Bonnichsen 1973, 1979, 1983; Brain 1967a, 1969, 1981; Bromage and Boyde 1984; Bunn 1983, 1986; Johnson 1985; Mengoni Goñalons 1982; Morlan 1983, 1984; Olsen 1989; Potts and Shipman 1981; Shipman 1981a; 1989; Shipman and Rose 1983). As well, research has specified distinctive effects of carnivores (Binford 1981; Binford and Bertram 1977; Blumenschine 1986a, 1986b; Brain 1969, 1981; Haynes 1980, 1983a, 1983b; Hill 1979, 1980, 1989a; Shipman 1989; Shipman and Rose 1983), of bone-gnawing and -trampling ungulates (e.g., Behrensmeyer et al. 1986, 1989; Brain 1967b; Brothwell 1976; Fiorillo 1984, 1989; Laporte and Behrensmeyer 1980; Myers et al. 1980; Oliver 1989; Olsen 1989), and of rodent bone modifiers (Shipman 1981a; Shipman and Rose 1983). The effects of flowing water in both modifying bone surfaces and structuring element frequencies have been detailed (Behrensmeyer 1975, 1982; Bromage 1984; Shipman 1981b, 1989; Voorhies 1969), as have those of other attritional processes affecting bones (Behrensmeyer 1978; Behrensmeyer et al. 1986, 1989; Lyman 1984, 1985; Shipman 1989). Such research has successfully used contemporary observations to specify causal linkages between the action of various agents on bones and the physical results of those actions. Armed with such information, investigators have turned with considerable confidence to analyzing similar modifications in prehistoric bone assemblages.

Some zooarchaeological researchers, mainly North Americans, have written on the relationships among uniformitarian assumptions, reasoning by analogy, and actualistic research (e.g. Binford 1977, 1978a, 1981; Gifford 1981; Hill 1984, 1989b; Oliver 1989). These works on zooarchaeological epistemology and methodology are part of a wider set of discus-

sions about analogy and uniformitarian assumptions in English-speaking archaeology (e.g., Gould 1980; Gould and Watson 1982; Hodder 1982b; Wylie 1982, 1985, 1988, 1989). Why anthropologically based archaeologists have needed to define or reassert the basic terms of their practice is itself an interesting question, but it will not be dealt with here. This article addresses an emergent issue in zooarchaeological research based on analogue studies: the current shift to more complex analyses and more ampliative inferences about the life relations of prehistoric hominids.

Zooarchaeologists are now attempting to reconstruct patterns of human foraging behavior from complex patterns of multiple agents' "signatures" in bone assemblages. To date, much of the work attempting this transition has been controversial (e.g., Binford 1984, 1986; Bunn and Kroll 1986; Shipman 1986a, 1986b). These problems have been described by Lyman (1987a) as the result of weak assumptions, inconsistently applied analytic criteria, and as yet undeveloped research protocols. In this paper, I address the same issues, but from a somewhat different tack, focusing on the nature of analogical inference, our analytical categories, and our inferential expectations. In the first place, I argue that these problems are due to some unarticulated problems attending the transition to more ambitious inferences about the past. The present phase of zooarchaeological research requires strategies different from those that served us well in the previous phase of agent-identification studies for two reasons, each related to the nature of causation in the materials studied.

First, the relationship of patterning in archaeological assemblages to its causal agencies is more ambiguous. Some of the causes of this ambiguity—such as unclear analytical categories and levels—can be remedied relatively easily, and this essay offers suggestions in this area. However, some causes of the ambiguity are less tractable. For example, attributes commonly used to characterize and compare entire bone assemblages—frequencies of different anatomical elements or traces—may be produced by various combinations of processes. In other words, at this stage of their research program, zooarchaeologists are more likely to face problems of equifinality than in the previous stage. It follows that confidence about causal agency like that typical of the previous phase of research often may not be available to us. Moreover, I will argue that, although our ability to recognize the action of specific agents is well developed, our understanding of the role of faunal remains in human subsistence systems and in ecosystems is only rudimentary. Thus, our modern reference cases for discerning regularities in assemblage patterning are as yet few.

Behind these problems lies a second, and perhaps more critical, issue: that is the inferential confidence in biological, as opposed to physical, sciences. Being historical scientists, archaeologists must rely on statements of behavioral or systemic causation to give meaning to patterning

in the materials they study. Because hominids and the contexts in which they exist are biological entities, archaeologists need to examine their assumptions about causation and inferential confidence in light of thinking about causality in biological systems. Again, the levels of confidence enjoyed by zooarchaeologists when doing agent-identification studies may not be replicable when working with these larger and more complex systems.

The dilution of inferential confidence caused by these problems should not necessarily be viewed as an indication that something has gone awry in our research program. Although some aspects can be fixed, some of the problems are intrinsic to the complex ecological and behavioral systems we ultimately aim to study. These cannot be fixed by more hard work of the same sort that proved effective in the previous round of research. At the same time, these problems should not cause a retreat from intellectual rigor. In fact, more rigor is called for. Analogical reasoning based on well-controlled actualistic research is still the preferred strategy in this new stage of research. However, its application must be more complex than in the earlier phase, informed by a deeper recognition of how we generate knowledge and inferential confidence in these contexts.

Much of the philosophical terrain covered in this essay has recently been traversed by other archaeologists, in somewhat different ways. I will cite arguments and opinions advanced by Binford (1987a), Flannery (1986), Lyman (1987a), and Wylie (1985, 1988, 1989), exploring the implications of these and other works in specific relation to zooarchaeology. Like Flannery (1986), I have found Mayr's (1982) discussion of the nature of biological systems and of causality within them to be especially useful. I will explore the utility of Mayr's perspective for archaeological research from a somewhat different, though complementary, viewpoint to that taken by Flannery in his analysis of Mayr's work.

In this essay I first examine the nature of analogical reasoning in archaeology in relation to its role in other nonhistorical sciences, analogy and uniformitarianism, and the role of relational analogies. Then, I outline issues involved in the transition to more ampliative inferences, examining problems in analytic categories and suggesting some terminological revisions which may help. Zooarchaeological cases that illustrate the problems of moving from one analytic level to another will be cited. I also discuss the problem of equifinality and propose some alternative approaches, citing the relations of the concepts "context" and "frames of reference" (viz. Binford 1987a) to analogical and "uniformitarian" reasoning. Finally, I treat the problem of causation to biological systems, focusing especially on the concepts of "hierarchy" and "emergence" as developed by Mayr (1982) and suggest strategies for coping with lowered inferential confidence derived from this source.

UNIFORMITARIAN ASSUMPTIONS, ANALOGY, AND ACTUALISM

The uniformitarian theoretical perspective and the actualistic research strategy that follows from it have been basic to the generation of knowledge in the historical sciences since modern geology began (Lyell 1830; Hooykaas 1970). Researchers in paleontology and zooarchaeology at least implicitly acknowledge that their work rests on the assumption that bone and other animal tissues have responded to various stresses uniformly over time. They understand that modern cases analogous to ancient ones may be studied to clarify the origins of specific classes of prehistoric evidence. At the same time, paleontologists and zooarchaeologists have also long been aware of the pitfalls of misapplication of uniformitarian assumptions and faulty analogies (e.g., Herm 1972; Kitts 1977; Lawrence 1971; Simpson 1970). The most common of these involves what Lawrence (1971) called "transferred ecology," attributing most of a modern ecosystem's features to an ancient one on the basis of a few points of physical similarity between prehistoric and contemporary cases. This is in fact an example of the dangers of formal analogy and is familiar to archaeologists who have been warned about a parallel abuse of analogies from ethnographic cases in archaeological inference (e.g., Ascher 1961; Binford 1981; Freeman 1968).

Analogy in Nonhistorical and Historical Sciences

The uses of analogy in the historical sciences may differ from its more common applications in other sciences on which much of the writing in the philosophy of science is based. Debate exists among philosophers of science about relations among analogies, metaphors, models, and theories in constructing scientific knowledge. For example, Achinstein (1964, 1972) and Hesse (1966) would term as "models" some concepts which others (e.g., Leatherdale 1974) would call analogies and others might call theories (see also Agassi 1964; Braithwaite 1953; Bunge 1973; Carloye 1971; Girill 1971; Ruse 1973 on these differences). However, it is generally agreed that analogies serve to make concepts and relationships in a new field of inquiry more comprehensible by likening them to something more familiar. Analogies also serve to extend and develop a theory about something by assigning to a little known subject the qualities of better understood phenomena (Achinstein 1968). In other words, analogies help "revisualize" a phenomenon by asserting that it is like another admittedly nonidentical one (Carloye 1971).

Leatherdale (1974) distinguishes two types of analogies in science: manifest analogy and imported analogy. In manifest analogies, properties linking the analogues may be apprehended through ordinary experience

or perception; they lend themselves well to formal symbolism. Leatherdale contends that these analogies, although often employed in extending systems of classification, are less productive than the imported analogy. Imported analogies propose "novel or more esoteric relations" between the properties of two systems (Leatherdale 1974:4). These novel and sometimes seemingly illogical juxtapositions, he argues, are the synthetic breakthroughs by which science takes major steps forward. Among the examples he offers is Kekulé's dozing vision of dancing atoms and snakes which led to his model of the benzene ring. In these situations, analogy—and, according to Leatherdale, metaphor—can be a step toward better understanding of the phenomenon under study. However, at some point in the research cycle, it is at least possible that the actual qualities of the phenomenon may be apprehended directly, eliminating the need for either type of analogy.

By contrast, historical scientists never can dispense with analogies. Using Leatherdale's definitions, historical scientists commonly use a variant of manifest analogies to name prehistoric materials and to infer past events, processes, or contexts from static evidence. Inferences are based on formal delineations of similarities between prehistoric entities and modern ones, the latter of known associated events, processes, or contexts. I hasten to add that in formulating general theory archaeologists are as likely as any other scientist to use imported analogies creatively and productively. What differs is the extent to which archaeologists and other historical scientists can dispense with analogical reasoning when doing their basic disciplinary work. No matter how well understood the relationship of a contemporary entity to its present-day context and causes (what Wylie, e.g., 1985, 1989, has termed the "source context," or the "determining structures"), inferring the prehistoric context and cause forever remains analogic.

In the 1960s, claims were made that archaeologists could escape the widely acknowledged inferential problems of analogy by a strategy of deduction from uniformitarian "laws" (e.g., Binford 1967; Freeman 1968), a position reasserted more recently by Richard Gould (1980; Gould and Watson 1982). In examining this position, Wylie (1982, 1985) states that these writers have not perceived that using such principles in the form of an argument about the unobservable past constitutes a special type of very strongly warranted analogy. Thus, the "escape" from analogy offered by law-like generalizations in the natural sciences is really a refinement and elaboration of the analogical model of inference. I align myself with Wylie (e.g., 1982, 1985) and others (e.g., Binford 1981; Hodder 1982b) in asserting that archaeological epistemology, hypothesis formation and selection, and methods of inference are normally and unavoidably analogical (viz. Gifford 1981; Gifford-Gonzalez 1989). These points merit a brief summary.

Binford (1977, 1978a, 1981, 1987a) has argued that archaeologists name their materials, create relevant analytic categories, and assign meaning to patterning in their materials by analogy with modern cases. This assertion is the basis for his program of building "middle range of theory." Smith (1977) and Watson (Gould and Watson 1982) agree, noting that, prior to hypothesis testing, archaeologists employ analogy to evaluate the plausibilities of bridging arguments which give objects a functional meaning. Archaeologists also select the more plausible of alternative hypotheses about the functioning of prehistoric systems through analogy with contemporary cases. These uses of analogy are often inexplicit, what Wylie (1989) has called "suppressed analogy," but they are nonetheless powerful discriminatory tools in developing archaeological explanations. Such uses of analogy are not unique to archaeological reasoning and have been analyzed in other sciences (e.g., Salmon 1967).

Weitzenfeld (1984) discusses the tacit premises on which even explicit analogies rest and which enable them to yield precise and sensible information about a "target." He argues that analogies are based on understandings of their components which tacitly specify their ontological status and implicitly stipulate a background "determining structure"—such as chess rules would be in an analogy between two specific chess games. Thus, the archaeological uses of analogy are not exceptional in the phase of scientific research which defines problems and hypotheses.

Some archaeological examples of the use of determining structures are Binford's and Hodder's approaches to inference. For Binford, powerful determining structures are principles of evolutionary theory (e.g., adaptation, selection) and those involved in the functioning of ecosystems (viz. 1962, 1977, 1981). Despite profound differences with Binford's theoretical program, Hodder agrees (1982b) that controlled uses of analogy and assumptions of regularities over time are useful tools in archaeological inference. Although Hodder has condemned reductionist, "uniformitarian" perspectives on human behavior (1982a), he acknowledges the existence of "general principles of meaning and symbolism" (Hodder 1982b:25) which he uses to interpret archaeological cases. He thus admits that, within certain confines (from the examples cited, behaviorally modern humans living in more socially complex societies), uniformity of process and material effects can exist over time. In doing so, Hodder is implicitly stipulating the existence of a determining structure which governs the ontological categories "meaning" and "symbolism."

A Model of Analogical Inference

Because analogical reasoning permeates archaeological practice, as it does in other historical sciences, it merits the detailed treatment it has

received in the last decade by archaeological theorists. Among the most pressing questions for practitioners in the historical sciences is how to assess the validity or strength of a proposed analogical relationship. Figure 1 presents a schematic outline of simple analogical inference in historical science based on manifest analogy. We observe a phenomenon in the present-day world—as a bone, grooves on its surface, footprints. Although we study it as a contemporary phenomenon (viz. Binford 1977), we accept it as having originated long ago on the basis of background assumptions and arguments fundamental to the practice of any historical science. As archaeologists, our hope is that understanding these aspects of the entity's prehistoric context will shed light on the nature of hominid existence at that time.

On the basis of our knowledge of the present-day world, we chose a modern counterpart for the prehistoric phenomenon. Comparison of the two objects of study verifies that they are similar in features we deem relevant to our inquiry. At the same time, we note features which respectively differ between them (cf. Copi 1982; Wylie 1985). Analogues are thus not expected to be homologues, and the differences may in fact be enlightening. Wylie (1989:12–13) points out that the process of establishing which similar features are relevant is actually very complex. It may or may not be explicit, but it rests on preexisting assumptions about the

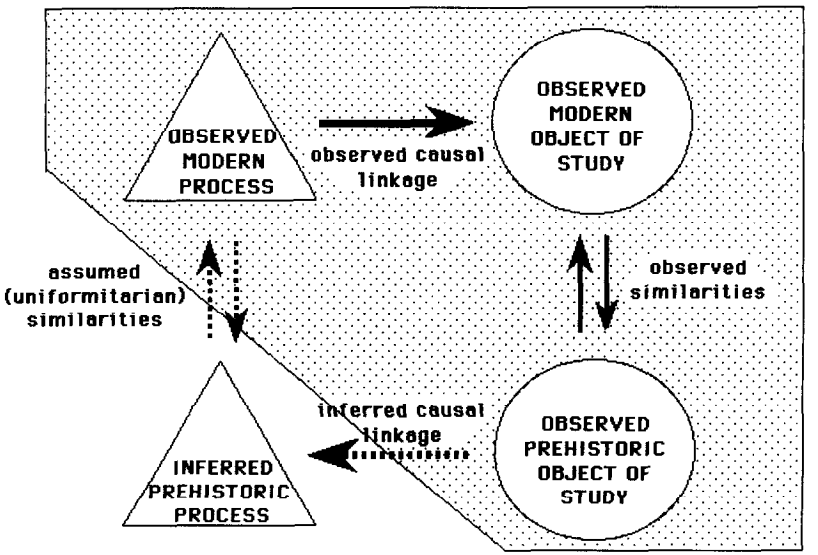

FIG. 1. A model of analogical reasoning in historical science. The shaded area indicates the realm of contemporary observations. The "inferred similarity" on the left may also be viewed as one or more uniformitarian assumptions.

relation of a determining structure, or causation, to the features chosen as relevant. To remain with our simple example, we specify a range of processes and contexts (the determining structure) which may have created the relevant features in our object of study. The plausibility that the specified processes could have generated these features is again assessed on the basis of our knowledge of the present world, often by a process of suppressed analogy.

Our inferential process may at this stage be complete, if we are satisfied with the extent of detail known about, and our degree of confidence in, the context and processes associated with the modern analogue (and, by extension, with the prehistoric entity). However, we may wish to know more about the modern analogue, or to have greater confidence in what we know, because we believe this might tell us more about the function, significance, or context of its counterparts in the remote past. To this end, we may engage in actualistic research, making observations of modern analogues under controlled conditions in the contemporary world. Such work is intended to further clarify the causes and contexts in which the entity or its relevant features are produced. Actualistic research with faunal remains has included laboratory experimentation (e.g., Bonnichsen 1973; Shipman and Rose 1983) and carefully monitored natural situations (e.g., Behrensmeyer 1978, 1982; Conybeare and Haynes 1984; Haynes 1980, 1983a, 1987, 1988a, 1988b).

In this observational phase we may discover that processes and contexts other than those originally specified may produce or affect the entity studies, necessitating another round of research. Sometimes, as Wylie (1989) points out, the results of our novel observations force a fundamental rethinking of the conceptual categories and assumptions on which we based our original research design. This recursive process of dialogue between analytic constructs and experimental findings was noted by Binford (1981, 1987a) as an essential aspect of investigating ambiguity in historical sciences.

Analogy and Uniformitarianism: Another View

We are accustomed to thinking of analogical inference in archaeology as permitted by the uniformitarian perspective, the central "leap of faith" (Yellen 1977) in all the historical sciences. However, methodological uniformitarianism can itself be seen as a product of analogical reasoning (see S. J. Gould 1967; Simpson 1970). Geologists, paleontologists, and archaeologicals feel warranted in taking the stand that prehistoric entities can be accounted for by invoking the past operation of presently observable processes because of a multitude of examples in which the past–present analogical relationship appears to be very strong. Lyell's (1830–1833)

original arguments for a uniformitarian position were induced from such a multitude of exemplary cases. Methodological uniformitarianism thus conforms to one of Carloye's (1971) descriptions of the role analogy plays in building general theory. He asserts that, in the development of theory through the use of analogue models, a shift to more formal statements of abstract relations is often first suggested by common features of several analogue models.

About Relational Analogies

Actualistic investigations, what Wylie (1985) has called "source-side" research, establish the basis for relational analogies which stipulate causal or structural linkages (Wylie's "determining structures") between the phenomena specified (viz. Copi 1982; Hesse 1966; Wylie 1985; see also Binford 1981; Gifford 1981; Hodder 1982b). Inferences from relational analogies are considered to be more strongly warranted than are those drawn from formal analogies in which the stipulated relationships are based solely upon similarities of formal qualities. In relational analogies, the links between source or context on the one hand, and relevant criteria of resemblance on the other, are thus systematic and causally based.

Many of the potential and actual problems entailed in the so-called abuses of ethnographic analogy in archaeological inference (e.g., Ascher 1961; Binford 1967, 1987b; Freeman 1968; Gould 1980; Gould and Watson 1982), as noted earlier, exemplify the failings of formal analogies. Most criticisms center on the unwarranted ascription of traits known to exist in an ethnographically documented group to a prehistoric group solely on the basis of some formal resemblances between modern and prehistoric artifacts or environmental contexts. Critics object that the traits do not inherently (functionally or structurally) or universally follow from the cited similarities. Such inferences are suspect because they impute functional relations among different aspects of a system where none necessarily exist. Binford (1981, 1987a) and Hodder (1982b) have stressed the preferability of relational analogies in archaeology and have advocated using ethnoarchaeology and other forms of actualistic research to build a larger reference set. Zooarchaeologists have done just this with great success over the last 20 years.

Relational Analogies and Zooarchaeological Inference

Zooarchaeologists and paleontologists use relational analogies in every phase of their work often in implicit yet complex ways. We name prehistoric bone specimens based on their resemblance to certain modern bones, but only with regard to what we deem to be relevant criteria of

similarity. For example, fossil bones may in fact differ markedly from modern ones in weight, color, or chemical composition. However, we deem these traits irrelevant when identifying anatomical element and taxon because they reflect processes which affected the ancient bone postmortem.

From simple naming, we readily engage in broader inferences by (usually unconsciously) employing reasoning by relational analogy. We accept that a fossil element once existed as one of many bones in a skeleton, although we may not have found any such associated bones. We also "know" that the bone once had specifiable muscles and ligaments attached to it which had certain functions in locomotion, etc. We find it reasonable to assume the bone was associated with a digestive system and other soft anatomical parts characteristic of the species. If epiphyses on the bone were fully fused, we would infer the animal was an adult when it died. We would probably be willing to explain that the element originated from a fertilized ovum, some cells of which diversified into specialized bone tissues. Given adequate modern reference sets, we might even infer that the fossil bone came from a male or female animal.

We thus make complex, ampliative inferences about the anatomy, physiology, feeding adaptation, embryology, and sex of an animal we have never seen, based on a set of features of an entity which resembles others we know to have these associations. Our security in these analogical inferences stems from a background knowledge—the suppressed analogies or tacit premises—of the causal and functional links between developmental histories, function, and morphological features of contemporary elements resembling the one under study. Of course, paleontologists or zooarchaeologists do not explicitly go through all the steps of logical argument about causality every time they pick up and talk about a fossil bone. However, we believe ourselves to be justified in our inferences because of this background of causal and functional relationships. Researchers accept that bones are produced by a specific biological pathway and exist as living entities in a restricted range of functional contexts. They accept this as true in the present, based on their experience, and true in the past, based on the assumption that these pathways and contexts are the sole necessary and sufficient conditions for the existence of entities we call bones. Biological theory, with its many descriptions of causal relationships, serves as the complex background of bridging arguments and warranting statements permitting such security.

It is possible, however, to make assertions about the former contexts and functions of a fossil bone which are not considered so secure. For example, we could say that it was raining when this individual died. Given the criteria deemed relevant above, this inference cannot be supported. The weather is not considered to be causally or functionally related to the

existence or form of the element under study. One might advance a series of arguments regarding the relation of weather conditions to the context or associations in which the bone was found or even devise actualistic research to discern more about this aspect of its context. However, the features of the entity as presently understood do not warrant the inference.

Over the past 20 years, actualistic investigations into the causes and contexts of production of various marks on bone have successfully distinguished the distinctive "signatures" of various agents. However, these solid relational analogies may not be enough to get us where we ultimately want to go. We should not assume that if we keep doing the same kind of work in the future that we will obtain cumulatively greater and equally confident knowledge about events and states in the prehistoric past.

THE CURRENT TRANSITION IN ZOOARCHAEOLOGICAL INFERENCE

What most zooarchaeologists really want to investigate are the life relationships—ecological, social, and cultural—of the prehistoric hominid species we study. The new generation of zooarchaeological research attempts to move toward more complex inferences about the systems or contexts in which the agents acted and hence toward such life relationships. Whereas the action of an agent can often be inferred from its trace with considerable confidence, it is more difficult to assign an unambiguous meaning to aggregate patterns of traces and remains in assemblages especially when evidence exists for the action of two or more agents. This is because linkages between cause and assemblage pattern effects are less certain. Given the analogical nature of our inferences about past events and systemic relationships, this results in lower levels of confidence about the causal conditions involved generating the patterning we see. There are multiple sources for this uncertainty, some remediable, some not. In this section, I discuss two sources of inferential uncertainty which need to be dealt with through different, though complementary, strategies. The first pertains to zooarchaeological epistemology and methodology: the relationship of patterning in faunal assemblages to ancient behaviors of hominids or other agents and their ecological contexts. The second area is one of general theory and its relation to methodology: the nature of causation in biological systems and its impact on expectable levels of inferential confidence.

The first aspect of the problem centers on the meanings which can be assigned to patterning in faunal assemblages. This could be called a problem of middle range theory, as defined by Binford (e.g., 1977, 1981). It is the epistemological problem of reliably establishing what we think such

patterning means, in terms of the behaviors and contexts we ultimately want to study. This problem may be broken down into three related parts, each of which has a different, discipline-specific "remedy." These are (1) unclear analytic concepts, (2) equifinality of causes in patterning of assemblages, and (3) lack of information on faunal remains in behavioral or ecological contexts.

Problems with Analytic Categories: Immediate Cause versus Context of Production

Inferential problems can begin at the level of relating a trace's immediate causal agency to its broader context of production. This issue has been recently addressed by several archaeologists. Young (1989) presents a carefully developed discussion of the role of analogy in lithic replication studies, which parallels actualistic research in bone modification. Young notes that it has often been possible to specify experimentally the force conditions producing a certain flake morphology in a given raw material, and whether the percussor was hard or soft hammer. However, despite very tight control, it has not been possible to closely specify the raw material of the percussor, or the angles at which the core and percussor were held. These variables may seem very close to "true causation" for most of us, and yet their effects cannot be experimentally isolated. Thus, wider inferences, as Young stresses, may not naturally unfold from meticulously controlled experimental situations.

Oliver (1989) makes a similar point about physical evidences for striations and stone impact marks on bones in a natural sinkhole situation, where animal bones developed traces identical to those produced by humans. He states,

> It is important to note that in all of the examples described in this study, the materials modifying the bones are similar to those which hominids might use and/or work (e.g., rock-fall block hammerstone; stone tool = sharp-edged rock; flesh, hide, and bones = flesh, hide, and bone). Only the agent or energy source that sets the materials in motion is different. (Oliver 1989:93)

Thus, the immediate conditions of production (e.g., stone, bone, and certain physical attributes of force in their encounter) are specifiable, but the circumstances—what most of us might call the agents—bringing these factors together (e.g., a hominid bone smasher versus a rock-fall in a cave containing carcasses) are not, from the trace itself. The same argument was made by Lyman (1987a) in his discussion of equifinality of cause.

Fiorillo (1989) and Behrensmeyer et al. (1986, 1989) underline the same problem in their discussions of "pseudo-cut marks," striations produced on bones trampled on coarser-grained substrates which are morphologi-

cally indistinguishable from those made with a flaked stone cutting edge. The striations and the cut marks identically reflect the mechanical conditions of force and the interactions of constituent materials, which can actually be brought together in a number of contexts by several agents. Neither the exact agent exerting the force nor the context of interaction is evident from the trace in and of itself because several causal contexts or agents can produce them. Lyman, Oliver, Fiorillo, and Behrensmeyer et al. all argue that other, independent lines of contextual evidence must be combined with the physical traces on bone to better specify the agencies.

Causal Agent, Effector, Actor, and Behavioral and Ecological Contexts

We have seen that inferring "agency" in the sense that we usually mean it as archaeologists (e.g., hominid versus nonhominid, hunter versus scavenger) may not necessarily follow from closely specifying the immediate causes of a given trace. In recognition of a distinction between the immediate causal agency and our "targets" as archaeologists, I propose some terminological distinctions which may permit us to ask clearer questions about the linkages of our study materials to the processes we wish to know better. This would allow us to build more explicit and warranted bridging arguments between our objects of study and the relationships about which we wish to know. We might begin discussing examples of immediate physical causes as the *causal agency,* while naming what we have formerly called the "agent"—the hominid holding the hammerstone or the hyena gnawing the bone—an *actor.*

This distinction between the immediate interaction of the materials which produce a trace and our inferential target would permit us to construct and evaluate bridging arguments relating the two. For example, under which archaeologically observable conditions could an actor (e.g., a hominid, a hyena, a trampling ungulate, etc.) be inferred confidently from traces of a certain causal agent? What other lines of evidence need to be brought together with the traces to create higher levels of confidence in the linkage? This is an empirically investigable issue which can be addressed through actualistic research and comparative assemblage analysis.

However, this still leaves a category of object between the immediate causal conditions and the actor unrepresented. For example, what of the stone tool itself in the hominid's hand versus the sand grain grating against a bone? While this distinction may not always be necessary, it is important to hold the distinction between these and other objects that effect the modification of a material, and the force conditions and material interactions creating it. I call this analytic category effectors.

Likewise, among our targets, it may be useful to distinguish two levels

of "context": *behavioral context* and *ecological context*. The first would include those patterns of behavior targeted for study (e.g., "scavenging," "storage," "herd management," etc.). In fact, we should recognize that these are conceptual frameworks imposed on an array of hominid or nonhominid behaviors. *Ecological context,* in this usage, would refer to the type of ecosystem and environment in which the actors lived. As with the relations between immediate causal agent, effector, and actor, so the relationship between an actor and the context in which it carries out certain actions can be specified and empirically investigated, as can linkages between certain behavioral systems and regional ecology.

I am thus proposing a nested system of categories, each linked to the other by empirically evaluable bridging statements (Fig. 2). The most useful of these statements will specify causal or functional linkages. Because analogical inferences based on a single line of linkage between immediate causal agency and actor, or between actor and context, are less strongly warranted, arguments for a specific linkage between levels of this system are strengthened by additional, independent lines of information.

Recent Zooarchaeological Controversies

Some controversies in the recent literature on bone modification and assemblage composition reflect problems of the transition between

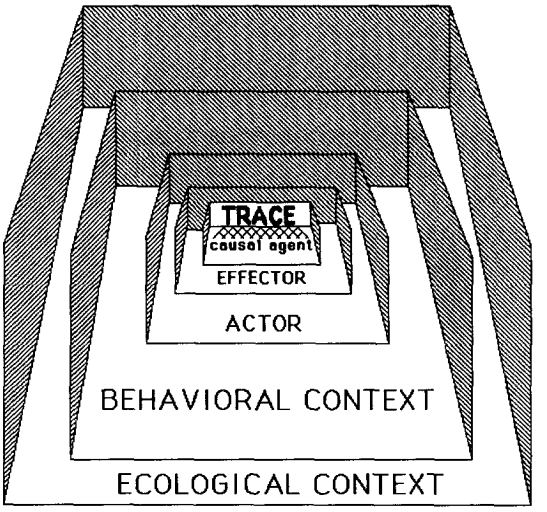

FIG. 2. Nested system of analytical categories linking a trace, its immediate causal agent, the effector of the causal conditions, the actor setting the cause in motion, and behavioral and ecological contexts.

clearly articulated causal linkages and wider inferences about actor and context. Specifically, they demonstrate how security about the immediate physical cause of traces does not automatically translate into security about higher-level inferences. I cite two examples of contested behavioral and ecological meanings for mammal bones from the so-called "living floors" of Olduvai Gorge, Tanzania. In one such debate, Shipman (1986a, 1986b) first defined marks on the middle of long bone shafts from these sites as stone tool cut marks made by hominids. She grounded her identification of agency in extensive experimental replication studies, isolating distinctive morphological criteria for cut marks with scanning electron microscopy (e.g., Shipman 1981a; Shipman and Rose 1983). Shipman went on to assert that the marks reflect a scavenging strategy on the part of the early hominids, since the cuts occur on meat-poor sections of the bones, many of which were themselves less meaty than other anatomical elements. Other researchers (e.g., Bunn and Blumenschine 1987; Gifford-Gonzalez 1988; Lyman 1987b) agree with Shipman's attribution of the cut marks to stone tools and to hominid actors, each in the face of recent evidence for the existence of "pseudo-cut marks" (see next section). However, they disagree with her wider inference of the behavioral context in which they were produced. They assert that it is now warranted to infer a foraging strategy from them, since butchery tactics which might produce them are not tied to a scavenging mode of carnivory as a necessary and sufficient condition. Each critic marshalls evidence to dispute the behavioral meaning of the location of the cuts, citing countercases in which similar patterning might be produced in other causal contexts. From the standpoint of this essay, the controversy stems from the equivocal linkage between the immediate causal agency and actor and wider inferences about behavioral context.

In another argument over the same bones, Bunn (e.g., Bunn 1983; Bunn and Kroll 1986, 1988) asserts that patterns of cut marks and element representation in the FLK *Zinjanthropus* Floor assemblage indicate that hominids were predaceous carnivores, while Binford (1986, 1987b, 1988) infers that the patterns reflect hominid scavenging of carnivore-modified bone assemblages. Both sets of researchers rely on the same data bases: cut marks, carnivore tooth marks, and, most importantly, element frequencies. As in the case of Shipman debate, there is little dispute among the researchers about the immediate causal agencies and patterning of traces on the bones. Both agree that cut marks and carnivore tooth marks (traces of agents of modification) exist, and that an absence of long bone epiphyses is evidence of reduction by carnivores. Moreover, both agree that hominids were heavily involved in creating the patterning observed in the assemblage. Yet, despite their agreements on the immediate agents of

bone modification, the authors draw diametrically opposed ampliative inferences from the evidence.

Some of the disagreement between the researchers may stem from their differing methods of reconstructing element frequencies. For example, Bunn and Kroll (1986, 1988) estimate the original number of intact epiphyseal ends of long bones through a comparative, conjoining study of diaphyseal specimens from the site, whereas Binford (1981) relies on counts of the extant epiphyses as published by M. D. Leakey. He rejects Bunn and Kroll's estimates as inflated, based on the number of diaphysis fragments per long bone produced by experimental long bone breakage. More is at work here, I believe, than either methodological details or "bad science," in which one or the other reseachers' preexisting interpretive agendas colored their approaches to the Olduvai materials. Certainly, earlier writings by both Bunn (e.g., 1981, 1983) and Binford (e.g., 1981, 1984) reflect preexisting positions on predation versus scavenging among Plio–Pleistocene hominids. But what is of interest to the themes of this essay is the fact that the inferential "space" exists for both interpretations to come forward from the agreed-upon meanings of the traces.

This disagreement shows once again the problematic aspects of moving from strongly warranted inferences about a trace's immediate causation, and even of actor, to broader inferences about its context of production. The latter, although not necessarily incorrect, are not warranted at the same levels of confidence as the former ones, when using these data alone. In the environment of lowered inferential confidence, inexplicit, suppressed analogies about the physical outcomes of hominid predation and scavenging and citations of individual rather than exhaustive contemporary cases are given free play in structuring interpretation. The question of zooarchaeologists is this: Given this dilution of certainty, can we do better than lapse into a variety of interpretations of past events and contexts, all equally defensible or assailable? If so, we could be stuck, as Binford (1968:16) put it, with evaluating the credibility of an argument by assessing its advocate's competence as archaeologist. I believe we can do better than this, based on a recognition of the new terrain in which we are beginning to work. This may require drawing some new distinctions in our vocabulary, such as those suggested here, as well as extending our inferential strategies to include more contextual data.

The Problem of "Equifinality"

That a given attribute may not inevitably signify a closely specifiable actor, behavioral or ecological context or process has been discussed by

Lyman as a problem of equifinality. Among the best known examples is the argument, advanced by Lyman (1985), Grayson (1988, 1989), and Klein (1989), that in-place destruction of vertebrate bones by various attritional agents may produce patterns of element representation very similar to those produced by selective transport of high-utility body parts away from a death site. In this case, different actors and behavioral contexts are alleged to create very similar patterns of element representation. The interpretive dilemma presented by pseudo-cut marks represents another such case, in which different actors produce the same effects on bones.

Lyman (1987a) has discussed these and other examples as a problem of equifinality in recent taphonomic analyses of bone. He argues (Lyman 1987a:269) that the pattern recognition approach which has typified many recent zooarchaeological analyses has failed to describe unequivocal linkages between the pattern observed and actions of a specific causal agent (what I have here called an actor). He notes that this approach fails to acknowledge that the physical effects of several agents (actors) may overlap. Based on the argument developed in the previous section of this essay, I argue that in some of these cases the issue is not that there are really multiple causes for the same trace, true equifinality. Rather, the inferential problem is that one specific causal event can occur in what we consider significantly different circumstances, involving different actors and behaviors. However, I agree with Lyman that in other cases different processes could create the same pattern perceived. In each case, the strategies for coping with the problems will differ.

The cases of cut marks and transport versus in-place destruction illustrate this distinction. In the case of cut marks and pseudo-cut marks, the immediate causal agency of the striations is identical: the problem is that the interactions of materials can be effected by a stone flake wielded by a hominid or by an ungulate hoof grating a bone against a sharp grain of sand or gravel. The first step in coping with this kind of equifinality is to recognize that it is situated at the analytic level of the actor and its behavioral context. The second is to ask what other types of evidence may be brought forward to justify arguing for one or the other behavioral context and actor. This is the strategy advocated by Behrensmeyer et al., Fiorillo, and Oliver for distinguishing cut marks from trampling striations. The evidence for one or the other has been said to include substrate texture (assuming that striations produced in fine-grained sedimentary environments are less likely to be effects of trampling), redundancy of marks at locations which make anatomical "sense" as by-products of intentional butchery (assuming that vertebrate anatomy presents consistent challenges to hominid butchers which are often resolved by intervention of a cutting edge at consistent anatomical locations). Such tactics

of argumentation have been used repeatedly and convincingly in zooarchaeological literature, for example as in the work of Guilday et al. (1962) and Parmalee (1975), to argue for human butchery of carcasses. Lyman (1987a) discusses potential problems of criteria of redundancy and purposiveness in detail, and I will not repeat them here. For the purposes of this essay, it is important to note that the premises upon which contextual evidence is marshalled are themselves empirically investigable and are mobilized within an analogical framework. The analogical nature of contextual evidence will be discussed in the next section.

In the case of transport versus in-place destruction, two entirely different sets of causal agencies and contexts create similar patterns of element representation. The actors effecting these patterns might in fact be the same: bone-smashing versus bone-transporting hominids, gnawing versus bone-transporting wolves or hyenas. In this case, one might move directly to ask what other types of evidence may be brought forward to justify arguing for one or the other agency or behavioral context. This is the path taken by Grayson (1988, 1989) in arguing for in-place destruction by biological (carnivore and/or human) actors as the primary cause of element frequencies in assemblages with which he worked. In the case of the bighorn sheep assemblage from a level in Gatecliff Rock Shelter, he cites carnivore tooth marks on bones in the assemblage. In the case of marmots from aboriginal settlements in the White Mountains, he cites the size of the prey animal in relation to human ability to transport entire carcasses, the placement of cut marks on surviving marmot bones, and the residential nature of the sites in which these remains are encountered. In this case too, contextual information is seen as critical to creating greater certainty about the meaning of patterning the assemblage which is ambiguous standing on its own.

"Context" in Analogical Reasoning

The strategy of marshaling contextual data is a common-sense one in archaeology, but its underlying logic is interesting. All reseachers cited above assert that the best way to select the most probable of several alternative actors or causal contexts is to turn to other, independent lines of evidence. These, in combination with the physical traces on bone, are thought to permit a more warranted identification of actors or contexts. Reseachers who emphasize using "context" in archaeological analysis are varied, and the term may mean different things to different people. Here I pursue the relation of "context" to the process of analogical inference. In the cases cited above, "context" implicitly or explicitly refers to other bodies of knowledge in which the correlative, if not the causal, relations among their components are well understood. For ex-

ample, consistencies in animal anatomy, and the problems these present to human butchers, are a background against which striations on bone surfaces can be read with greater certainty. When assuming such consistencies, the analyst relies on the usually unarticulated "suppressed analogies" regarding animal bodies outlined earlier in this article.

Among the bodies of knowledge most widely used to better understand patterning in faunal remains is geological "context." The environmental and depositional information offered by sedimentary context has been emphasized in so many analyses of prehistoric faunal assemblages that it may be ascribed a privileged status, seemingly more "real" than other types of prehistoric evidence. It is an archaeological and paleontological truism that meticulous comparison of the geological contexts and the preserved remains of actual fossil assemblages is a secure and productive analytic method. However, relying on the "reality" of geological context to lessen the ambiguities of faunal assemblages may obscure the fact that inference of geological processes itself depends on a universe of relational analogies assembled and systematized by geologists over the past 200 years. Sedimentologists use the same strategy of establishing causation through contemporary observations, assess the range of possible effects of the same cause (e.g., Walker 1980), and face the same problems of equifinality in their analogues (e.g., Reineck and Singh 1975; Selley 1978:272–277). In other words, like uniformitarian assumptions, the "facts" of geological context are based in analogical reasoning and are best used when understood as such. From the perspective advanced in this essay, contextual data are (usually implicitly applied) bodies of relational analogies which circumscribe the variety and thus reduce the ambiguities of inferences from one's own data set. In this light, one person's "context" is another person's primary and problematized field of inquiry.

Binford's "Frames of Reference," Lyman's "Forensics," and Wylie's Account of Inference

Binford (1987a) and Lyman (1987a) have recently proposed approaches that augment our understandings of the research process and which converge on the arguments developed here. In her account of what archaeologists actually do in making complex inferences, Wylie (1989) describes a process similar to that advocated by Binford and Lyman. Binford (1987a) developed an alternative, and somewhat more descriptive, language for "contextual" analysis. He argues that ambiguity in one field of investigation can be reduced by juxtaposing independent "frames of reference" against which the facts (in his example, bone element frequencies at different sites) can be "projected" and understood. By a "frame of reference," Binford appears to mean a body of internally related knowl-

edge about processes and their products which operate in a uniformitarian way. In the language of this essay, a "frame of reference" is a complex configuration of relational analogies that have an internal coherence. For example, one of Binford's frames of reference is economic anatomy. In this body of knowledge, he links a functionally related set of entities (bones and soft tissues of the mammal body) to another functional cluster, human nutritional needs and choices, through adaptionalist bridging arguments (optimization, reproductive advantage, etc.).

Binford (1987a) proposes that knowledge of the past can best be obtained by juxtaposing several independent, uniformitarian "frames of reference." He gives as his own example economic anatomy juxtaposed with the use of space by foragers. In the language used of this essay, Binford suggests that higher levels of inferential confidence can be obtained by a mutual juxtaposition of independent sets of complex relational analogies. Each frame of reference is a web of relational analogies. It follows that the most strongly warranted inferences from contextual analyses depend—as do simpler analogical inferences involving a few variables—on defining strong causal or structural linkages between the processes and their physical effects.

Binford (1987a) has applied his argument specifically to bone assemblage analysis. He asserts, in much the same vein as writers mentioned earlier, that bone assemblages will not reveal much about the organization of human subsistence and movement in a landscape if studied in isolation from other evidence. He states, as already noted, that the optimal strategy is to juxtapose several independent, uniformitarian "frames of reference," economic anatomy being one of several he proposes.

In his review of the zooarchaeological literature, Lyman (1987a) takes pains to point out how infrequently a specific trace can unequivocally be taken to represent the action of a certain agent (in this essay, the actor). He notes that, even when certain morphological criteria seem to be clearly associated with hominid action, such as the "shoulder effects" on stone tool cut marks (Shipman and Rose 1983), the precise causal mechanism is not specified. In the face of this level of ambiguity in "signatures," Lyman (1987a:278–279) argues the best way to reduce ambiguous meanings in patterned faunal data is to use a strategy similar to that used by forensic investigators, who often attempt to identify the circumstances and causes of a crime from suggestive but equivocal evidence. Lyman notes that they evaluate the likelihood that a certain agent was responsible for an event in light of the aggregate of independent indications, each in itself somewhat ambiguous but more compelling when taken together. Clearly, the stronger the relational statements embodied in each independent line of evidence, the stronger the inferential case. Yet Lyman's treatment of the probabilistic nature of inference in

forensics and archaeology signals a further aspect of the inferential complexities attending inferences of actor and context in biological systems, a point to be taken up in detail later (see "The Nature of Causation in Biological Systems").

From the perspective of asking how archaeologists actually get on with assigning meaning in their analyses, Wylie (1989:15) notes that they regularly marshal "independently constituted lines of evidence" which "converge either in supporting or refuting" their proposed linkages between past practices and the materials studied. This, she argues, is a means of coping with a lack of strongly deterministic epistemic foundations for their inferences. She argues that multiple, independent sources of knowledge about analogues to archaeological entities are required both to generate and to evaluate "reconstructive hypotheses" drawn from archaeological data.

A sophisticated example of this strategy is the work of C. K. Brain (e.g., 1967a, 1969, 1981, 1984), in seeking to determine the primary causes of modifications to the bones associated with australopithecine fossils in southern Africa. While he has seldom devoted much time to discussing the philosophical underpinnings of his research, Brain has consistently combined multiple, independent lines of evidence, grounded in strong bodies of theory (e.g., cave formation, sedimentology) or in relational analogies he established through experimental and naturalistic observations to assign meaning to prehistoric materials (Brain 1981, 1984).

In sum, greater conceptual clarity is a first step for gathering and productively using relevant analogue data, and the juxtaposition of multiple, independently derived lines of evidence—each strongly grounded in relational analogies—creates greater confidence in inferences drawn from archaeological data. Another area of concern, related to the lack of clarity in our analytical categories, is a pervasive ignorance of the full range of roles played by faunal remains in behavioral and ecological systems.

Lack of Data on the Role of Faunal Remains in Higher-Order Systems

If we espouse the view that modern analogues are the central field for discerning systematic regularities between the actors and processes we want to know about and the materials we have to study, then we must turn to contemporary experiences to build relevant references for our work. Some realms of theory and data relevant to understanding life relationships are classic sources of contextual data for archaeologists, such as sedimentology and ecology. However, the extant geological, biological, and ecological literature will seldom provide an immediate remedy to zooarchaeologists'—or other archaeologists'—inferential problems because these bodies of knowledge seldom focus specifically on the

preservable components of ecological or geomorphical systems. Taphonomic researchers (e.g., Behrensmeyer 1975; Behrensmeyer et al. 1986, 1989; Binford 1978a, 1981; Blumenschine 1986a, 1986b; Brain 1967a, 1967b, 1969, 1981; Fiorillo 1984, 1989; Haynes 1980, 1983a, 1983b; Hill 1979, 1980, 1984, 1989a; Morlan 1983, 1984; Shipman and Rose 1983; Voorhies 1969) have carried out their research in acknowledgment of the unique aspect of both our goals and our research materials. At present, however, we know a lot about the effects of specific agents on faunal remains and much less about the relation of patterning in faunal assemblages to behavioral strategies of various contemporary species, including human and nonhuman carnivores, or to the operation of particular ecosystems. The remedy is indeed more actualistic research in zooarchaeology, with a redefined focus.

The next round of actualistic research should focus on the role of faunal remains in the complex systems of life relations we wish to study archaeologically. This work can be informed by four fundamental realizations. First, mechanically produced traces and patterns of element representation in faunal assemblages may not signal one actor or context, but rather a specifiable range of them. Second, the actors or contexts most likely to have been responsible for patterning in a given prehistoric assemblage can best be discovered by juxtaposing independent systems of relational analogies. Third, to infer significant behavioral or ecological information from faunal remains, we must document contemporary analogues, focusing on them as elements in temporally and spatially variable behavioral and ecological systems. Fourth, given the nature of these systems (see below), we should not expect outcomes of the regularities revealed by such research to be so closely specifiable as those observed in the mechanical interactions of materials. Despite this, we can and should strive to isolate features of these systems suitable for relational analogies. In the process, we may have to refine our notions of function and causation.

Some reseachers are already working in this mode. Binford's work with the Nunamiut (e.g., 1978a, 1980, 1981) was a bold attempt to bring together economic anatomy, ecology, and uniformitarian assumptions about the behavior of bones. Research by Blumenschine on carcass utilization by Serengeti Plains carnivores (e.g., Blumenschine 1986a, 1988, 1989) combines economic anatomy and ecological principles, while documenting the physical signatures of bone processors along several different evidential axes. O'Connell et al. (1988, 1990) have sought to combine economic anatomy and ecologically-based foraging theory to account for modern Hadza foragers' butchery and transport strategies and the patterning of resulting bone assemblages. Behrensmeyer's (e.g., 1983, 1986; Behrensmeyer et al. 1979) ongoing longitudinal study of bones in the Amboseli basin landscape combines ecological, geomorphological, and

bone durability data in a search for determining structures in bone assemblage patterning. Haynes' (1988a, 1988b) longitudinal work on the taphonomic consequences of elephant mortality incorporates considerations of rainfall, demography, and geomorphology. Lyman's (e.g., 1985; Lyman and Fox 1989) research on bones' mechanical durabilities in relation to their nutritional values is an attempt to assess expectations drawn from one set of relational analogies in light of another.

In addition, there are as yet unsynthesized but promising areas of research on faunal remains in ecosystems including carnivore behavioral ecology and regularities in impacts on bones. For example, several patterns of element representation and damage described by Hill (1989a) for bones from a spotted hyena den in Kenya can be explained by Blumenschine's (1986a, 1986b) observations of hyenas' effects on bones in his Serengeti study. The lack of femora, for example, may be the result of the fact that this element is reported as the first bone to be consumed by hyenas, ranking over head flesh (Blumenschine 1986b). Femora rank very high in nutritional indices in other bovid and cervic species for which these indices have been calculated (e.g., Binford 1978a; Emerson 1990). They are also the one bone element reported by Blumenschine (1986b) to be breached for marrow by lions. One might thus propose that hyena's nontransport of femora to a denning site reflects either their consumption of the elements in the field or to prior consumption of its marrow contents by lions. Either option relates to the place of hyenas in such communities as Amboseli, in which direct and indirect competition with lions over prey and carcasses has been shown to affect their behavior (e.g., Kruuk 1972; Schaller 1972). It is thus possible to envision linkages between bone assemblage patterns and prey densities, predator packing and competition, which in turn are related to rainfall and other basic ecological variables, as well as to historically particular aspects of community structure. This example proposes admittedly speculative linkages, but empirically evaluable ones.

Work of this sort is beginning, as with Blumenschine's (1989) own "landscape taphonomic model" of carcass availability. Borrero (1990) has specified some of the ecological factors affecting rates of carcass disarticulation in different environments. At a more specific level, it may be possible to formalize a body of predictive theory to describe carnivore feeding strategies in relation to mechanical, nutritional, and ecological principles of broad applicability. For example, Haynes' (1980) and Blumenschine's (1986a, 1986b) descriptions of carcass consumption sequences for wolves, hyenas, and lions show remarkably similar patterns. These regularities in consumers' behavior may be explained by analyzing three bodies of knowledge with predictive potential: economic anatomy of ungulate carcasses, the relative strengths of different body segment

articulations and strengths of the consumers in relation to the resistance of the carcass to disarticulation and destruction (see Borrero 1990). When cast within an optimal foraging theory framework, and informed by biogeographic or paleobiogeographic data, these three areas may be synthesized into a body of theory that predicts both the carcass utilization strategies of various species in regional predator guilds and their most likely impacts on bone. These data, like sedimentological theory, could then be extended to predict likely behaviors and impacts of extinct carnivores coeval with ancient hominids. This would form a useful uniformitarian background for analyzing evidence of carnivore interventions in prehistoric bone assemblages.

With regard to faunal remains in human behavioral systems, I believe that Binford's Nunamiut research (e.g., 1978a, 1978b, 1980, 1981; Binford and Bertram 1977) remains the most comprehensive attempt to describe the linkages among ecologically structured resources, landforms, human nutritional needs, technology, and patterning in food remains and cultural materials. The last decade has seen much research aimed at clarifying or critiquing linkages proposed by Binford, often to good effect. However, none of these have equaled the regional scope of the Nunamiut study. I believe such scope is essential for developing models appropriate to studying faunal remains as components in strategically variable human subsistence systems. In this, I echo Lyman's (1987a) concern that studies of bone modification need to document the different responses of bone modifiers in a variety of circumstances, rather than taking a typological, pattern recognition approach.

Up to this point, discussion of the present inferential problems in zooarchaeology has focused on issues of epistemology and methodology, where a number of coping strategies have been suggested or described. I now turn to the second aspect of the problem: considerations of general theory, specifically, the nature of causation in biological systems, and its effect on analogical inference.

The Nature of Causation in Biological Systems

Archaeologists seeking to make strongly warranted inferences about ancient life relationships from prehistoric materials need to know how causation is conceptualized in biological theories. This area has seldom been explored by zooarchaeological practitioners. An exception is Flannery's (1986) discussion of the nature of explanation in archaeology in his monograph on the Guilá Naquitz rock shelter. This section owes much to his reworking of Mayr's (1982) thinking, as well as to my own reading of Mayr.

Causal relationships in contemporary biological systems are complex,

and there is little reason to believe that they were otherwise at any phase in hominid evolution. The complexities of such relationships have implications for the types of analogical inferences we wish to derive from modern observations or theory drawn from them. Determining structures do certainly exist in the form and functioning of ecosystems. The amount of rainfall, for example, does have a consistent relationship to standing biomass (e.g., Coe et al. 1976; Le Houerou and Hoste 1977). However, the nature of determination in these systems is not simple. For example, biomass can sometimes exist at levels well above those predicted by regressions against rainfall (e.g., Lamprey and Waller 1990). Outcomes of the interaction of multiple variables in ecosystems may be regular, but the regularities are often better described in probabilistic terms, as they usually are in the statistical descriptions typical of biological and ecological observations.

As part of his argument that the conduct of biological science is qualitatively different from that of the physical sciences, Mayr (1982:37–38) notes that recent philosophers of science have disagreed over whether "laws" such as those found in the physical sciences exist in biological sciences. Mayr emphasizes that few contemporary practicing biologists refer to laws when accounting for features of biological systems. "This does not mean that regularities do not occur in biology: it simply means that they are either too obvious to be mentioned, or too trivial" (Mayr 1982:37). Archaeologists will recognize in this passage a parallel to Flannery's (1973) ridicule of "Mickey Mouse laws" in archaeology. Mayr classifies most recently formulated biological "laws" (e.g., Rensch 1971) as "adaptive trends effected by natural selection" (Mayr 1982:37). In using the word "trends," Mayr stresses the probabilistic nature of generalizations in biology. Mayr's approach rejects reductive models of explanation drawn from the physical sciences, stating that the complex nature of biological systems are not well accommodated by law-like formulations. In doing so, Mayr does not reject that regular patterns of causation exist in biological systems, but he argues that probabilistic statements best accommodate the complexities of such hierarchically integrated systems.

Variation, selection, and so forth are assumed to have operated throughout the history of life on the planet in regular ways, but the outcomes of their actions—or interactions—must be specified in probabilistic terms. They are nonetheless essential to exegeses of prehistoric cases. From the perspective developed in this essay, the operation of such processes in the past assumes a unique form of uniformitarianism, in which the relation of cause to effect is probabilistic (often highly likely), rather than as necessary and sufficient conditions. Biological principles are thus applied in a special and complex kind of analogical inference.

Thus, it appears that when we seek to recover information about behavioral complexes and ecological contexts from faunal remains, we are entering a new realm of knowledge and confidence claims. Analogical inferences based on these more complex causal relations in biological systems are less strongly warranted, since a given attribute may not inevitably signify only one biological, behavioral, or ecological context or process. This problem is another kind of "equifinality," one not specifically treated by Lyman, but one which I believe lies behind some of the nontechnical problematics of cause he raises. To put it in terms of archaeology, no matter how deterministic the relationship between the immediate causes of certain archaeological traces—and even their links to specific actors—are, when archaeologists seek to set these traces and actors in behavioral and ecological systems, the probabilistic nature of the operations of these systems preclude extending "if a, then b" deterministic statements into those realms. Explanation becomes a probabilistic account in which certain tactics may be employed to reduce uncertainty. In fact, I believe that a parallel exists here between the process outlined by Wylie (1989) as common in archaeological inference and the complex inferential process that underlies explanation in evolutionary biology. Other relevant discussions of the unique aspects of probabilistic causation and explanation are those of Merrilee Salmon (1982) and Wesley Salmon (1982).

Other aspects of Mayr's thinking about the nature of biological systems may prove valuable to archaeologists moving from agent-identification to life-relation studies. In arguing for a distinct philosophy of biology, Mayr (1982:63–66) develops an argument that emphasizes unique aspects of the hierarchical structure of biological systems. He asserts that each level is more than the sum of its parts. This is true not only in a simple functional sense (e.g., a body versus an organ) but also because of the quality of "emergence." By this, Mayr stresses, he does not mean any uninvestigable, vitalistic qualities of biological systems but rather novel features of an organizational level that cannot be predicted from the qualities of constituents at lower levels of organization (Mayr 1982:64; see also Flannery 1986:513–514). Mayr portrays his view of biological science as in conflict with the perspective that "proper science" is by nature reductionist. He (1982:62–63) argues strongly against "theory reductionism" in biological explanation, by which he means seeking to account conceptually for the salient aspects of an organizational level by using only concepts applicable at lower organizational levels. He takes pains to distinguish this type of reductionism from the often productive study of constituent materials and processes. Mayr's argument is that emergent features of biological hierarchies must be studied at their relevant organizational level. Further, he believes "physicalist–reductionist" ap-

proaches, modeled on strategies pursued in the physical sciences, are seldom fruitful in accounting for the unique features of biological systems.

The transition zooarchaeologists are now making can be seen as a move from one to another level of organization in biological systems. We must be prepared to study human behavioral and ecosystemic relationships not only in terms of the systems we have previously studied but also in terms of the unique features emergent at these levels of organization. Flannery (1986:516–518) discusses "intentionality" as one of the unique features of the organization of human social life and action, and consideration of intentional decisions by Nunamiut actors permeates Binford's work as well. I do not attempt to enumerate what might be unique about modern humans or hazard guesses about unique, "emergent" qualities of earlier hominids. The point here is that simple causal models derived from lower hierarchical levels may not account for relevant aspects of the life relationships we wish to study.

Explanation, Narrative, and Reconstruction in Historical and Evolutionary Sciences

A final point concerning Mayr's view of explanation in evolutionary biology, which Flannery reframed from an archaeological perspective, involves what archaeologists are doing when they work through their particular cases. Both Mayr (1982:71–73) and Flannery (1986:513–514) discuss the dual nature of explanation in biological science. They distinguish between a "what and how?" account of proximate causation—such as explaining male versus female plumage in a bird species by invoking hormonal causation—and a "why?" explanation of ultimate (or evolutionary) causation—such as an account of why the hormonally mediated differentiation of plumage came into being over many generations of organisms. Mayr (1982:73) contends that, "no biological problem is fully solved until both the proximate and the evolutionary [ultimate] causation has been elucidated." Flannery, seeking to integrate evidence for the emergence of food production in Mesoamerica with a general model that would accommodate other cases, opts for Mayr's approach to explanation. Both stress the unique form of "historical narrative" in evolutionary explanations. Mayr (1982:71) works from writings of philosopher of science Thomas Goudge (1961) and philosopher Morton White (1963) in proposing that satisfactory evolutionary explanations of biological phenomena take the form of historical narratives, in which central concepts of biology rather than general laws are invoked. These would include adaptation, naturally occurring variation, selection, as well as certain aspects of the organization of systems. Together, these are brought to bear in relation to the unique factual aspects of the case under study. I

view these central concepts as special types of uniformitarian principles in this essay.

How does current zooarchaeological work relate to either type of explanatory account? The work in which we are presently engaged, including the broader ecosystemic and behavioral syntheses outlined earlier, is *not* aimed at constructing historical narratives. Rather, we are engaged in a simultaneous process of (1) constructing and evaluating the means by which we investigate past human behavior and adaptation, and (2) reconstructing specific cases of the past states of systems (see Binford 1978a; Wylie 1989). These reconstructed "past states" *do* relate to historical narratives, both as cases demanding an evolutionary account and as challenges to existing narratives. For example, historical narratives exist in archaeology for the emergence of uniquely hominid capacities and behaviors, phrased in ecological and evolutionary terms. Reconstructions of relevant system states may challenge those narratives and demand alternative accounts. Other narratives to which some zooarchaeologists may refer pertain to later periods of time, assume more about uniformities in behavior between modern humans and the "target" populations of hominids, and address issues from a nonevolutionary perspective. Present zooarchaeological research may be seen as a testing of alternative narratives which are equally plausible, given the central concepts mobilized (evolutionary theory being only one example). The challenges I have discussed are those involved with reconstituting past states of systems in sufficiently reliable ways that extant historical narratives may be confronted.

Finally, what strategies can be used to cope with the probabilistic nature of determination in biological systems and with the problems this presents to an analogically based means of learning about the past? I believe the same ones enumerated earlier apply. Independent lines of evidence, derived from distinct systems of causation, can be mobilized to challenge and/or support one another, leading to more strongly warranted inferences regarding the past life relations that produced certain configurations of material in our sites.

DISCUSSION AND CONCLUSION

How we archaeologists think about agency and causation in the systems we study affects how we do our science. In the previous section I presented a view of causality in biological systems that emphasizes probabilities and complex explanatory strategies. This is not the pattern of explanation current in zooarchaeological discussions. Even Lyman, whose work (e.g., 1987a) represents some of the best grappling with inferential problems in zooarchaeology, appears to be approaching zooar-

chaeological analyses within what Mayr calls the physicalist–reductionist paradigm. He largely characterizes problems in determining causality as methodological, which can be remedied by better or more detailed work on "signatures." I agree that this is the remedy in agent-identification studies, and that in any case methodological and conceptual rigor should always be sought. But we need to evaluate whether some of our relevant data and relationships are better accommodated by the approach advocated by Mayr for biological sciences.

Zooarchaeologists have been led to work within a physicalist–reductionist paradigm by two factors, one historical and one substantive. First, the heritage of the "new archaeology," with its strong positivist flavor, continues to influence research practices. Many processual archaeologists have rejected the most extreme positions that grew out of the nomothetic, logico-deductive approach to inference (e.g., Binford 1977; Binford and Sabloff 1982; Flannery 1986). Yet, even for those who do not espouse an extreme "law and order" viewpoint, there remains, as Flannery (1986) has cogently pointed out, the nagging sense that we should be doing science like physicists do—discovering laws, making clear, unequivocal statements of causation, and hopefully reducing these statements to symbolic or mathematical formulations. The perspectives outlined in the previous section present us with an alternative vision of science, at least for the part of our work involved with reconstructing life relationships.

The second source of encouragement to work within a physicalist–reductionist paradigm is the nature of the evidence itself, and how we have successfully dealt with it to date. Bones, shells, and other faunal remains *are* uniformitarian materials, which we feel justified in assuming *have* responded to stresses consistently over long time spans. Moreover, we have just completed a very successful round of experimentally treating these materials as physicochemically constituted and modified materials. Our experiments and tightly controlled naturalistic observations have been very much in the mold of the "hard" sciences, and they "worked"—or so it seemed.

As I see it, these two factors—our implicit assumptions about what "good science" should look like and our experience with faunal remains as physical materials—hamper us at our next stage of research. We are dealing with the role of faunal remains in higher hierarchical levels in hominid behavioral and ecological systems. We must handle several aspects of this fact. First, we need to sort out how many levels we may currently be conflating in the terms "agent" and "context." We must establish more clearly the logical relations of the immediate physical causes of traces, objects effecting the interaction, the actors setting the causes in motion, and the contexts in which they acted. Second, we must

assess the unique, irreducible features of the levels of systemic organization we now want to study, as well as what generalizations from the study of lower-level systems remain useful tools when applied in this context. Third, we must develop expectations about the nature of causation in the hierarchical level or levels under study, both in terms of specifying key determinative features and in terms of assessing the probabilities of specific outcomes from specific causes. Fourth, we must understand the roles of biological remains in the level of organization we wish to study, looking for strong associations between distinctive patterns of evidence and immediate causes, actors, and contexts.

The best context for obtaining these understandings is the contemporary world. A considerable amount of information already exists on the operation of ecosystems and the interactions of individual animal species. We now need to concentrate research on faunal remains as components of human subsistence systems and ecosystems. Such work has already begun. At this juncture in zooarchaeological studies, the emerging question is: What aspects of faunal remains in hominid and nonhominid behavioral systems, and in regional ecosystems, are amenable to relational analogies? What about relational analogies that are probabilistic in their predictions? Behind this lies another methodological question: How do we discern and ascertain these relations? To establish whether strong ties (necessary and sufficient conditions, determining structures, etc.) exist between features of bone assemblages and life relations, we need enough contemporary cases to assess the stipulated regularities of the relationships. To evaluate inductive generalizations drawn from such observations, we need controlled follow-up research which isolates the functional linkages in these regularities and assesses whether these operate in a uniformitarian manner.

But even this rigor only reduces, not eliminates, inferential uncertainties. A forensic model of inference, using multiple, independent lines of evidence to indicate the most likely causal agent or context is appropriate. Faunal analysts' recent tendency to rely solely on osteological evidence for inferences about hominid subsistence behavior must now be revised to include other types of information. The most prudent path toward inferential confidence in this new phase of research entails careful examination of not only faunal remains and their attributes but other lines of evidence also amenable to relational analogy—botanical, geological, artifactual, site structural, site locational, and others.

Accepting the analogical nature of most archaeological and paleontological reasoning requires that we face fundamental epistemological and methodological questions. How do we know what we purport to know, and how do we evaluate the reliability of our knowledge? Our analytic categories and interpretations of our materials ultimately rest on our

knowledge of similar entities in the present-day world. What we know to be so about prehistoric remains rests largely upon analogical reasoning, with the uncertainties and potential pitfalls this logical process presents. We have widely acknowledged this, and we have discerned the best routes to inferential confidence: establishing strong relational analogies.

We now face another kind of uncertainty: bringing methodological rigor to reconstructing the states of past systems in which we believe causal relations were complex and probabilistic. Despite their diverse starting points and study materials, researchers cited in this essay converge in seeing archaeological inference as a complex process and acknowledge its inherent uncertainties. Those who discuss the roots of this uncertainty often site the multivariate nature of the systems we wish to indirectly observe. They do not deny that deterministic relationships exist in such systems, but they contend that, from the standpoint of imputing causal agency or context, the outcomes of such interactions may bespeak several alternative combinations of cause. They all prescribe the use of multiple lines of evidence and of general uniformitarian principles as a means of coping with the equivocal nature of any one set of evidence.

I am aware that, under all the philosophical rhetoric, I have not said anything very new. Good archaeology has always involved careful contextual analyses of archaeological evidence. By raising these issues for my own specialized branch of archaeology and from a certain philosophical position, I reassert the importance of comprehensive, conjunctive analysis at a time when archaeology as a whole has moved increasingly deeply into specialization. Zooarchaeologists themselves are just emerging from an extraordinarily successful 20-year phase of revealing ancient causes of bone modification, largely by going it alone. Now, given the challenges that face us at this point in our research program, we need to move into a more collaborative mode of investigation with other specialists, for bones themselves are not enough.

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