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ETHNOARCHAEOLOGY AND THE PAST: OUR SEARCH FOR THE "REAL THING".

Abstract

Recent attempts by ethnoarchaeologists like Hodder and Binford to explain past human behavior lack credibility because they fail to identify or control for a wide enough range of relevant context. An alternative framework based upon "first order" and "higher order" questions is proposed to provide a better approximation of the realities of past behavior represented in the potential archaeological record. This hierarchical approach permits ethnoarchaeologists, who normally observe fleeting and momentary behavior in present-day human societies, to recognize and evaluate the widest possible range of relevant variables that structure the long-term historical reality referred to by Braudel as the *longue durée*.

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Ethnoarchaeology is an ethnographic approach to the study of contemporary, living human societies that seeks to identify behavioral realities that structure the potential archaeological record. To do this, it combines with other approaches that have increasingly come to play a similar role with respect to natural factors that affect archaeological deposits and associations. These latter include studies in processes of sedimentation and deposition, in the decay and dispersal of bone remains in geo-archaeological context (taphonomy), geochemistry, and other approaches that Schiffer (1987) groups under the heading of "natural formation processes" in the archaeological record. Schiffer's distinction between such natural processes – earlier referred to in his writing as "N-transforms" – and behavioral factors ("C-transforms" in Schiffer's terminology) has important implications for the way archaeologists attempt to infer past human behavior from archaeological materials.

First, it implies that the same degree of scientific control is needed in data collection and analysis for ethnographic materials as is already widely in use in geo-archaeology, taphonomy, and other explicitly "scientific" approaches within archaeology. These approaches fit within

an overall framework that belongs to the historical sciences (geology, paleontology, paleoecology and astronomy) which assume that the principle of uniformitarianism can serve as a bridge to the past. That is, processes in nature observed in the present can be assumed to have operated uniformly in a similar manner in the past. To do this, these sciences require that the widest possible range of relevant variables be identified, measured and controlled for within the context of the particular "past" being studied. By applying general uniformitarian principles such as, for example, neo-Darwinian concepts of natural selection and evolution in biology, or more recently, plate tectonic theory in earth history, general syntheses can be subjected to a continuous process of empirical testing, usually via lower-level operational principles that facilitate this testing process. An example would be the array of operational theories in use today within evolutionary biology and ecology – such as optimal foraging theory, succession theory, predation theory, limiting factor theory, input-output studies relating to energy flow, and others – which provide controlled frameworks for empirical testing of the more comprehensive theories that stand behind each discipline.

So a second implication is that there is a necessary and complementary relationship between theory and empirical research. While this relationship has been caricatured as a simplistic kind of Positivism by some of our leading theorists in archaeology (Binford, 1985: 583-9; Hodder, 1986), it continues to be a key component in the process of archaeological inference. It is a truism in the historical sciences, as in science generally, that the credibility of a theory or general principle depends upon its ability to account parsimoniously for the observed data in particular cases and, that, in each case, the theory must be able to withstand the test represented by that data.

In short, the operating principles of the historical sciences provide a starting-point for considering the place that ethnoarchaeology occupies within archaeology. Uniformitarian principles and assumptions are widely shared in Finnish archaeology today, as witnessed by articles that have appeared in the journal on subjects such as paleoenvironmental reconstruction and prehistoric plant use, especially by means of fossil pollen studies (Siiriäinen, 1982; Donner, 1984; Hicks, 1985; Tolonen, 1985), and are part of a long-standing Scandinavian scientific tradition of paleoenvironmental research related to Pleistocene geology (including glaciology and varve studies) and palynology. However, there is a third implication to Schiffer's view that underlies all of these studies and makes it especially important for archaeologists to view their discipline first and foremost as a historical science. This is the assumption that *there is a credible, knowable past - a historical reality that lies outside the domain of individual, subjective impressions and opinions*. It is the domain of behavioral realities that I referred to earlier, and it applies equally to our concept of the human past as structured by natural factors and those of a more culturally constructed nature. It is the "real thing" that we seek to know through our archaeological studies. Such knowledge requires an organized, controlled approach to the archaeological record (and, I would add, to the materials of historic archaeology as well as those of prehistory).

Not all archaeologists, however, share this view. Right now we are seeing a surge of interest in subjective and intensely particularistic modes of archaeological interpretation, as evidenced by the recent ethno-archaeological studies of Hodder (a leading proponent of this view), Paleolithic cave art research (Conkey, 1984), and the subject of Critical Theory in archaeology re-

viewed by (Earle and Prencel, 1987). This trend has provided some stimulating ideas and debate, but it could also damage the scientific credibility of the discipline. So far, such interpretations have not figured prominently in publications on Finnish archaeology, but students here are (quite rightly) asking questions about this trend and what it might mean for archaeology. Some archaeologists are dissatisfied with what they perceive to be the narrowly constrained and even antihumanistic direction to scientific archaeology. They regard scientific approaches and assumptions, particularly uniformitarian assumptions, as inadequate to explain all of the complexities of human expressive behavior as they often appear in the archaeological record. Such dissatisfaction is understandable, and nothing I might say here is intended to discourage archaeologists from attempting innovative approaches to overcoming the apparent limitations of the archaeological record. We should always try to generate the fullest possible explanations for the variability represented by material associations in the archaeological record. But neither can we ignore the limitations imposed by a scientifically controlled approach to the archaeological record if we want to produce credible explanations of past human behavior.

Competing "Ideas" of History

Ethnoarchaeological research initially consists of observing and recording the flux of everyday human activity. In many respects, it resembles the kind of participant-observation study of human behavior carried out by social/cultural anthropologists, and it calls for many of the same skills. Above all, there is a need to control for ethnocentrism. For the ethnoarchaeologist, there is a kind of "double ethnocentrism" that must be recognized and controlled for. First, there is the ethnocentrism imposed by one's own culture and personal experiences within that culture, and, second, there is the temptation to view past human behavior in relation to the cultural categories and experiences of the culture being studied. The Critical Theorists are not wrong when they point out how our ideas of the past are strongly conditioned by the culturally and historically dominant assumptions of the present. Learning the language of the society being studied and residing within that society so as to experience and understand the dominant cultural assumptions of that society are both essential elements in this process of what I term "interactive ethno-

archaeology." It is not enough to measure and control for such variables as diet, refuse and discard patterns, technological materials and skills, settlement patterning and other similar sorts of "external" forms of behavior, although I consider these to be an essential part of ethnoarchaeological field research. The ethnoarchaeologist must also experience and control for different ideas of history and the human past that arise in different cultural contexts.

Collingwood's concept of the "idea of history" (Collingwood, 1946) is often invoked by archaeologists as a model for our study of the human past. It is certainly true that the past is over, and that anything we might say about the past today consists of our ideas about it. However, during fieldwork, an ethnoarchaeologist should experience different, culturally constructed ideas about the human past. These "emic" or insiders' concepts of human history have their counterpart within our own, Western culture, which is internally complex and contains many subgroups constituted along religious, linguistic, and other lines. These are the "alternative Western archaeologies" referred to by Hodder (1986, 157-164), and they include historical and archaeological "pasts" that range from attempts by ethnic or special interest groups like Native Americans, Blacks and feminists to redress perceived imbalances in their history to such fringe products as Atlantis and the Chariot of the Gods. (In Helsinki I discovered a bookstore named "Rajatieta" where virtually all of the current examples of this genre are to be found, and they occur in other bookstores as well.) A relatively new and popular kind of novel based on archaeological themes from the remote past, usually the Paleolithic, has also appeared (best-sellers include titles like *Clan of the Cave Bear* and *Reindeer Moon*). The recent popularity of such books demonstrates the existence of alternative ideas of history.

Whether constructed from historic and archaeological evidence or by pure fantasy, these alternative ideas about the human past require choices. That is, they are competing for acceptance. Collingwood does not provide a framework for evaluating competing ideas of history, so archaeologists must construct one for themselves. I realize that some of my colleagues (Hodder, in particular) will object by claiming that such an effort to set limits on speculation represents an assault on human freedom of expression. My answer to that is that we are always free to speculate. I am sure the "rajatieta" will continue to flourish and will even be accepted as

true in some quarters. But if we wish to choose intelligently among these alternatives, a shared framework for evaluating these different competing ideas of the human past is needed. I am suggesting that the opportunity and ability to choose intelligently rather than speculation, however ingenious, logical or sincere, it may be, is a more appropriate kind of intellectual freedom for archaeology.

The philosophical objection to science as means of understanding the human past usually arises from a sense of human freedom of action and thought being constrained and even, to a degree, determined by factors outside the control of the individual. Science, among other things, discovers and measures the effects of "real world" conditions, usually in the form of general principles or laws, that can be observed and tested in the course of their operations in actual situations. Such general principles, not science, are what act to constrain human activity in particular situations. Failure to recognize such constraints when engaged in activities where such principles are relevant leads inevitably to bad results. In scientific archaeology, ideas about the human past lack credibility whenever they exceed the limitations set by "real world" factors known to have been operating during the period being studied.

For example, there was an attempt to posit a "Pleistocene Overkill" argument for the Australian continent (Martin, 1967) to explain the extinction of megafauna there at the end of the Pleistocene. Because of Australia's unique biogeographical history, with its early isolation from the Asiatic mainland, the indigenous mammals (with the possible exception of bats) there were all marsupials. It is safer, therefore, on uniformitarian grounds, to assume that the ancient marsupials of Australia behaved more like their modern marsupial counterparts in similar environments than like modern placental mammals. None of the modern marsupials of Australia exhibit herd behavior or could reasonably be termed "gregarious mammals." Even the so-called "mobs" of Australia's largest living marsupial, the red kangaroo (*Megaleia rufa*) disperse when pursued and lack any kind of herd instinct or herd leadership. If one accepts this assumption, it means that Aborigines living in Australia during the Pleistocene and until the arrival of Europeans depended primarily upon hunting game animals individually or in relatively small groups, rather than by means of large, organized mass kills of entire herds, as posited by Martin. Thus it is unlikely that the

extinction of large-bodied Pleistocene mammals could ever have been due to human hunting activities alone. This alternative view is supported by archaeological evidence from Pleistocene fossil deposits at Lancefield, Victoria, where excavators (Gillespie, Horton, et.al.1978) have found that large-bodied mammals like *Macropus titan* (a large-bodied animal probably ancestral to modern kangaroos) persisted for thousands of years while human hunting populations were present in the same area, as shown by archaeological sites not far away dated in excess of 30,000 years. That is, human hunters and Pleistocene megafauna co-existed in Australia for at least 7,000 years before these species became extinct. Martin's hypothesis, however, would require that we accept the idea of their extinction fairly shortly after the human species' arrival on the Australian continent, probably between about 30,000 and 40,000 years ago.

In short, Martin's idea of history exceeds the limits of what we know, based on scientifically-based uniformitarian assumptions, about past and present marsupial behavior. Therefore Martin's idea, which might perhaps be true in other parts of the world where similar Pleistocene extinctions occurred, such as North America and Europe, was probably not true for Australia, where different biogeographical conditions operated. When tested empirically by means of archaeological evidence, such as at Lancefield and at other archaeological sites of Pleistocene age in Australia, Martin's idea does not stand up. The archaeological evidence is more congruent with some other kind of explanation for Pleistocene extinctions of large-bodied mammals in Australia, perhaps due to climatic changes which appear to have been underway around 15,000 years ago (Bowler, 1976).

As a footnote to this example, an alternative idea of history regarding the spread of early human populations into the Australian continent which does not require mobile human hunters pursuing herds of large game has been proposed and is now undergoing archaeological testing. This is the "Strandlooper Hypothesis" (Bowdler, 1977) which argues that early human populations in Australia were primarily adapted to coastal and marine resources and spread along the shorelines and coastal plains of the continent. This idea fits the ecological facts of Australian biogeographical history better than Martin's. Bowdler points out how, as world sea levels rose towards the end of the Pleistocene due to the discharge of water into the world's oceans as the continental glaciers retreated, human

populations found an increasingly indented coastline with even greater abundance of coastal-marine and estuarine resources. She argues that the more arid interior of Australia was settled later, after the better-favored coastal areas were occupied. So far, the archaeological evidence tends to support her view, since most prehistoric sites in the 40,000 to 30,000-year range are located either along the coast or on islands that were once hills on a Pleistocene coastal plain that is now submerged. An example of this latter case would be Bowdler's (1984) excavations at Cave Bay Cave on Hunter Island, off the north coast of Tasmania and in what is now the Bass Strait.

There are some difficulties with this idea. For instance, it depends heavily upon negative evidence, such as the expectation that most Australian sites of Pleistocene age probably lie submerged on the continental shelf, where access by archaeologists is difficult (although not impossible, as recent Paleo-Indian studies in underwater archaeology at submerged sites in Florida demonstrate). Another example of negative evidence upon which this hypothesis depends is the relatively late date for the earliest sites of Pleistocene age in the arid central and western parts of Australia. So far the earliest site recorded for this region is only about 22,000 years old (Smith, 1987), so Bowdler's idea continues to stand. But it should also be remembered that arid Australia has never received the same degree of archaeological sampling and testing as the more coastal areas. So archaeological comparisons of this kind are not really appropriate until better controls are available, in this case, in the form of better geographical sampling. Sites like Lake Mungo in western New South Wales, where radiocarbon dates in excess of 30,000 years are available, could perhaps be viewed as an exception to Bowdler's argument. Today this site lies in a semi-arid area of ancient sand dunes. But these huge, semicircular dunes once enclosed freshwater lagoons connected to the Murray-Darling River system, which was more extensive during the Pleistocene than it is today. Thus the possibility exists that Lake Mungo represented a riverine extension during the Pleistocene of Bowdler's coastally adapted human populations.

These examples show that archaeologists in Australia must choose among competing ideas of Pleistocene prehistory in the same way that archaeologists everywhere must make similar choices. Collingwood recognized the importance of the "idea" of history as a construct within the

human mind, but he did not approach this construct from a scientifically controlled point of view. But if we accept Collingwood's view that history is an idea of the past, why should scientific approaches to the idea of the past be more credible than any others?

Historical Science and Braudel's Concept of "Longue Durée"

While ethnoarchaeologists observe and measure short-term behavior within the context of living, present-day societies, their goals are not the same as those of cultural anthropologists and ethnographers. For ethnoarchaeology, these observations are really a means to an end. That end is perhaps best described by historian Fernand Braudel, whose work is (belatedly but deservedly) receiving more attention from archaeologists. Braudel's credibility in archaeological circles rests mainly upon his monumental study, *The Mediterranean World in the Age of Phillip II* (1966), although it is his essays collected under the title of *On History* (1969; in English in 1980) that are most often cited by archaeologists. For Braudel the human past presents a series of paradoxes that historians must struggle to resolve. One such paradox involves the inherent difficulties of recording and integrating immediate, short-term human experiences of the moment with the long-range developments that can be observed in human history – Braudel's much-cited concept of *longue durée*. Related to this view, he also points out how long-term history involves patterns akin to structural processes in nature studied by scientists – a domain inhabited by archaeologists (who are mentioned but whose time-scales are longer than even the longest *durée* discussed by Braudel) – and the particularities of the present or short-term, which, in fact, does not exist except as a moment arbitrarily frozen in time for purposes of study. This latter idea, termed "social reality" by Braudel, is viewed as a kind of reality that does not lend itself readily to historical study, however much it may appeal to sociologists, anthropologists and geographers. It is also the domain of ethnoarchaeology, at least when dealing with the so-called "ethnographic present:"

"In its totality, social reality in flux is ideally, at every instant, *synchronous* with its history, a constantly changing image, although it might repeat a thousand previous details of a thousand previous realities. Who would deny it?" (Braudel, 1980:76)

What is important here is the assumption, made explicit elsewhere by Braudel, that there is such a thing as human history that exists as an external reality, beyond the flux and subjectivity of the moment. For Braudel, this external reality represents:

". . . a mathematical, godlike time, a notion easily mocked, time external to men, 'exogenous', as economists would say, pushing men, forcing them, and painting their own individual times the same color; it is indeed, the imperious time of the world." (1980: 78)

Attempts by historians to reconcile or integrate these two concepts of time constitute, for Braudel,

". . . the two contradictory movements of the mind, confining them within either the narrowest limits of the event or the most extended *longue durée*." (1980: 79)

A further paradox exists whenever we consider the *longue durée* described by Braudel. Does this time move in one direction ("time's arrow"), or does it operate cyclically, moving in a repetitive manner like the planets or the seasons? For Braudel, the *longue durée* of human history is a "great structure" that:

". . . travels through vast tracts of time without changing; if it deteriorates during the long journey, it simply restores itself as it goes along and regains its health, and in the final analysis its characteristics alter only very slowly." (1980: 75)

It is a kind of "slower tempo" in human affairs, bordering on the motionless, and forming a kind of infrastructure for observing and evaluating the more explosive, short-term events of human history. An example of this would be Braudel's view of the institution of mercantile capitalism in European history in relation to ships and nautical traditions from the 14th to the 18th Centuries. Within the context of this grand structural theme, Braudel, in his study of the Mediterranean world, observes and evaluates the particular effects of geography and short-term historical events during the 16th Century. The results of this monumental effort at historical analysis can serve as a model for the discipline of archaeology, with the potential to resolve persistent debates in archaeology between scientific generalists and historical particularists, materialists and mentalists, positivists and humanists, or other contending points of view, however they are labelled.

Ethnoarchaeology is a powerful tool for constructing and testing better approximations of the historical reality proposed by Braudel in his concept of the *longue durée*. That reality, however, is not self evident and must be constructed and tested in a particular way if it is to serve as an acceptable idea of history. Ethnoarchaeology operates at two levels that may appear contradictory. On the one hand, ethnoarchaeology permits one to observe the widest possible range of differences in present day and recent human behavior, any of which could serve as a potential model for an alternative idea of the past. This aspect of ethnoarchaeology possesses a wide appeal and is widely recognized by archaeologists (Watson, 1979:286). On the other hand, ethnoarchaeology has the even more important role of setting limits within which only certain possible concepts of the human past, whether arrived at logically or ethnographically, could reasonably have been expected to operate. Since these two levels of ethnoarchaeological analysis are not entirely congruent, there will always be debates about the appropriateness of particular ethnoarchaeological observations to interpreting the archaeological record.

Controlling for Context: The Critique

"Context is everything!" Anthropologists and archaeologists usually agree that it is always essential to recognize and control for context in any kind of explanation of variability in human cultural behavior, past or present. Recognition of this fact has been one of anthropology's most important and enduring contributions. Disagreement, however, often arises over what constitutes relevant context, and it is here that the strongest differences arise over the role of ethnoarchaeology. These differences are especially strong today, as shown by the contrasting views of two contemporary theorists and ethnoarchaeologists; Binford and Hodder. Each of these ethnoarchaeologists asserts that one must control for context in order to draw archaeological conclusions from ethnographic evidence. But each limits his definition of relevant context so narrowly that the resulting conclusions lack credibility. Their failures are useful, however, in helping us to arrive at an ethnoarchaeological approach that will overcome these difficulties.

In a series of studies among the Nunamiut Eskimo (Binford, 1978) and, more recently, the Alyawara Aborigines of Central Australia (1984;

1986), Binford has argued that circumstantial and situational variables have more to do with explaining variability in material assemblages than do cultural differences. This is a classic theme in anthropology, and it has always enjoyed a close relationship to ethnoarchaeological interpretation. For example, the study by Thomson (1939) of seasonality among the Wik Munkan Aborigines of Cape York, in the tropical north of Australia, is often cited by ethnoarchaeologists as a case of how seasonal differences in economic activities produce differences in material culture that could easily be mistaken for the material by-products of different subcultures. While not exactly a new idea, this theme has been repeated and refined by Binford into a major position. It stands in clear opposition to the more symbolic and ideational explanations offered by Hodder and others who prefer to emphasize the culturally constructed nature of variability in the archaeological record.

In one of his most recent efforts in this direction, Binford (1986) argued that the notions of style proposed by Sackett (1982) as potential indicators of ethnic differences are unconvincing. Binford stresses, instead, the role of functional variables based upon the interaction of technology (in this case, related to Alyawara Aborigine stone knife production) and such situational aspects of behavior as camp organization, personnel, raw materials, and other factors of this kind that come together under certain circumstances. In this paper, Binford uses the term, function, to refer not to the use or role of a particular kind of artifact, but to the relationship between technological variables within a particular cultural system and other situational aspects of the circumstances under which members of that cultural system behave. He sees this functional relationship as a more potent source of explanations for unambiguous archaeological inferences than references to differences in ethnicity, symbolic meanings, or other cultural norms.

I agree with Binford on the potential that such a general view has for interpretation of the archaeological record. Elsewhere (Gould, 1980) I have argued that all ethnoarchaeological explanation must be derived from an understanding of how each cultural system operates within the context of its total biogeographical and sociocultural environment, with the aim of identifying potential "archaeological signatures" of characteristic kinds of situationally adaptive behavior. What disappoints me in Binford's presentation, therefore, is how unconvincing his use of evi-

dence is in making the very sort of argument that is needed in ethnoarchaeology.

For example, Binford never recognizes or controls for the fact that the Alyawara Aborigines in his study live in relatively close proximity to Europeans and have regular access to important elements of European-Australian economy and technology. My notes from a week spent at this same locality with James O'Connell in 1974 (very close to the time Binford was there) recall Aborigines with strongly-held traditional values but who also drove cars, used rifles and shotguns, listened to radios in camp, ate from tins and boxes of prepared foods purchased at a nearby store, depended upon water from a nearby mechanical well, and relied upon a variety of metal tools such as axes and knives. These are, in fact, the same people pictured in the illustrations accompanying Binford's paper, and these illustrations show metal tools in use for various parts of the manufacturing process for stone knives. In short, elements of European-Australian culture pervaded at every level. When I visited the Alyawara, I never saw anyone actually using a stone knife of the kind described in this study for domestic or other activities in or around the camp. Perhaps I missed something, but it seems to me that there is a question here, which is: Who were these knives being produced for? Such artifacts appear from time to time for sale in various tourist shops in Alice Springs, so perhaps this is where they went. Or perhaps they were being produced for a museum collection somewhere (a fairly common occurrence at that time). If the Alyawara were producing these items for their own use, it would be useful to know more about this. As it is, we have control whatever over the systemic relationship between what, by 1974, was an anachronistic technology for the Alyawara and the wider context of contemporary Euro-Australian culture.

This is not a minor point. One of the most important interpretive arguments presented by Binford in this paper has to do with the way Alyawara Aborigines parcelled out tasks, with different individuals picking up and carrying on tasks in a rather informal manner until the artifact was completed. Binford contrasts this behavior with the Nunamiut, where an individual will generally perform all of the tasks related to producing an artifact. Of course, Binford is correct in questioning the assumption which he feels at least some archaeologists share that there is a 1 person = 1 tool equation in artifact production, and he cites his Alyawara case to show how sev-

eral individuals may participate in the production of a single item like a stone knife. However appealing Binford's argument may be, I should point out that I saw this same kind of behavior in which production activities related to the manufacture of domestic tools and implements are parcelled out informally and performed by different individuals among the Ngatjara Aborigines of the Western Desert in 1966-70. Such task-splitting characteristically occurred in the context of Aboriginal reserves, missions, and other settlements close to Euro-Australians - that is, in contexts similar to those I observed among the Alyawara living at Macdonald Downs. With respect to domestic technology, such task splitting rarely, if ever, occurred among traditional, desert dwelling Aborigines in the Western Desert. There was one important exception to this, however. During ceremonies, intense task-splitting occurred in relation to the applications of body decorations and in the manufacture of sacred paraphernalia.

Perhaps we should reconsider the Alyawara as a group of traditionally-oriented people who, by 1974, were becoming integrated into a larger, market based, dominant cultural system in a manner not unlike American Indian reservations of the late 19th and early 20th Centuries. The same can be said for Western Desert Aborigines the living in close proximity to Euro-Australian settlements, and several accounts of this kind of situation are available (Gould, 1969; Gould, Fowler and Fowler, 1972; Tonkinson, 1974, 1978). At the Laverton and Warburton Reserves in Western Australia in 1966-70, most traditional material culture items were produced for sale to Euro-Australians, either directly or, more often, indirectly through the agency of the Native Welfare Department of Western Australia. Money received from the sale of these items was used to purchase tinned and packaged foods, ammunition, petrol for old cars, and a range of goods and supplies similar to those observed at Macdonald Downs in 1974. In keeping with Binford's situational orientation in ethnoarchaeological explanation, the Western Desert people at Laverton and Warburton in 1966-70 were behaving differently in relation to their mode of production of domestic artifacts under "reservation" circumstances than they had been while living under desert conditions with a minimum of contact (in some cases, no direct contact) with Europeans. That is, the functional relationship - according to Binford's usage - between technological behavior and circumstances was of paramount importance in accounting for

this difference. These "reservation" circumstances included a new demand for artifacts of a traditional character for a mass market, resulting directly in a sort of low level mass-production involving small, informally constituted task groups using steel axes, metal files, steel knives, metal pen-nibs, and other hand tools of European origin. I shall even suggest that in this case we may be seeing an example of the kind of alienation of the worker from the completed product described by Marx as one of the essential attributes of mass-production under the conditions of an industrializing capitalist economic and social system. In order to find income to feed themselves and their families, desert-dwelling Aborigines arriving on these reserves from the Western Desert changed their mode of producing domestic artifacts literally overnight in response to altered circumstances brought about by a European-introduced economic demand structure.

Perhaps the Alyawara case is not an exact parallel to that of the Western Desert people. However, due to a failure to recognize or to control for the context of recent historical circumstances surrounding the functional behavior of the Alyawara stone knife producers at Macdonald Downs in 1974, we cannot tell what it was that actually structured their behavior in relation to the variability that might appear in the material record. Binford's evidence is ambiguous and fails to provide a convincing basis for his conclusions, mainly because he defines the context of human situations and circumstances too narrowly. The real lesson of the "Alyawara Day" described by Binford is that we need to avoid tunnel vision when developing situational or circumstantial explanations, since most human situations involve levels of context that go beyond the most obvious or immediate "facts" of the externally-observed situation. The Alyawara case is especially important, because this kind of situation commonly arises in ethnoarchaeological research today. Instead of pristine, un sullied traditional societies (did these ever really exist?) we now find human societies with varying degrees and modes of involvement with world market systems and nation states. To deal adequately with these kinds of complex contemporary situations, ethnoarchaeologists will need to develop a more comprehensive and better organized approach to the recognition and control of the widest range of relevant variables in their explanations.

At the opposite extreme from Binford's circumstantial approach, we have Hodder's efforts

to explain variability in contemporary material culture and in archaeology with reference to culturally constructed elements of human behavior. Here, too, we can see a failure to recognize or control for relevant variables – in this case, of a more circumstantial nature. Hodder recently analyzed data collected during successive field studies among the Ilchamus (also referred to as the Njemps) of the Baringo District of Kenya, to address the question of why it is that the Ilchamus were the only society in that region to decorate their calabashes with incised, rectilinear designs. He rightly notes the inadequacy of explanations that rely upon empirical, correlative evidence, such as the idea that increases in the size of a social group will produce greater stylistic complexity in order to facilitate social interaction. Or that increased social competition for resources leads to increased stress and a need to mark resources by means of symbolic devices like decoration.

Cordell, et. al. (1987) describe arguments of this kind as "empirical generalizations," and they contrast such approaches with explanations based on laws:

"Empirical generalizations, even when they are correct, are not explanations. Because they are not derived from any theory about the way in which the world or some portion of it works, we cannot know within what contexts they are to apply. There are no procedures for evaluating empirical generalizations." (Cordell, Upham and Brock, 1987: 574).

One can, therefore appreciate Hodder's efforts to move beyond such empirical generalizations. In an earlier publication (1980) I addressed this problem by noting that empirically observed regularities in archaeological patterning, so much favored by the first generation of "new" archaeologists, cannot be taken seriously as cultural laws because of their failure to confront and explain as fully as possible the totality of evidence represented by material associations in the archaeological record. Scientifically constructed laws or "lawlike propositions" along the lines advocated by Schiffer (1976), whether derived from ecological sources, like Optimal Foraging Theory and Predation Theory in the study of hunter-gather behavior, or more strictly utilitarian concepts like Zipf's Law (a variant of the Principle of Least Effort) or Christaller's idea of Central Place Theory applied to archaeological settlement pattern studies, have, on the other hand, proved useful to explaining patterning in

the archaeological record. But the use of such laws is not intended to account for all of the specific attributes of cultural embellishment in such areas as social organization, artistic expression, or cosmology and religious beliefs. Hodder's dissatisfaction with such "incomplete" explanations is understandable, although it misses the point that these are not covering laws. They are intended only as relative approximations of behavioral reality, without pretensions to the sort of completeness that Hodder seems to expect.

Hodder's explanation of Ilchamus calabash decoration is based upon a symbolic linkage in Ilchamus culture between decorated calabashes as "female" objects and the female activities of milking cows and reproduction. If, as Hodder argues, Ilchamus males are to achieve their aims of creating wealth for themselves and their lineages by accumulating surpluses of cattle and by increasing the number of children they have, they must depend upon women both as producers (milkers and tenders of cattle) and as reproducers. Since women in Ilchamus society are effectively denied any public role in political matters, which resides entirely with male elders who make decisions collectively, Hodder regards calabash decoration as a mode of expression exclusive to the women to enable them to overcome their "severe mutedness" in an otherwise public male world.

Hodder expands upon the theme of the sexual symbolism of decorated calabashes in other ways as well, especially with respect to ceremonies related to reproduction and witchcraft. However, recognizing that an ethnographic description, no matter how carefully analyzed in insider or "emic" terms, is still not an archaeological explanation, Hodder incorporates an historic component into his explanation. He notes that in the 19th Century the Ilchamus lived in large, crowded and defended settlements, in contrast to their present arrangement of dispersed individual homesteads. They subsisted primarily on intensive irrigation agriculture, with few cattle. Calabashes were not decorated, and the same was true for pottery. However, Hodder notes that body decoration was prevalent then for women and young male warriors. And the villages' primary ancestral figure then was renowned for his possession of unique decorated skin clothing and strong magical powers. The load of symbolic associations with this male ritual figure, called the "decorated one," included social and ritual prerogatives and was another important domain for Ilchamus decorative art. Women then served as field laborers in what the Ilchamus regarded

as a low-status occupation. But Hodder argues that their field labor was important enough to discourage the Ilchamus then from having many children, since women could not function effectively as producers (farm field laborers) and reproducers at the same time.

However, the Ilchamus dispersed their settlements around 1900, abandoned intensive agriculture, and turned to cattle-raising as their principal livelihood. Hodder mentions several factors that might have affected this change. The rivers, along which the earlier villages were sited, silted up. The village populations were getting too big. The British colonial presence was being felt then, with pacification as one of its main effects, so inter-tribal raiding was stopped. Hodder (1986: 113) observes:

"But all of these factors are not reasons for change – they are only conditions of change, since in all cases the Ilchamus *could* have stayed living in the same or other large villages."

Instead, Hodder attributes these changes to a set of intentions based upon a pre-existing valuation of cattle as wealth, with a corresponding devaluation of agriculture and women's labor associated with agriculture. Ilchamus women no longer worked in the fields. He asserts that:

"It seemed 'natural' in this context for the women to begin decorating milk calabashes – items connected with an aspect of life that everyone valued positively and thought important for various reasons. The calabashes became decorated as part of existing cultural dispositions within a new context. The principles and aesthetic sense concerning decoration were extended from female and young male bodies to the new arena of child care and milk provision in order to make *them* beautiful." (Hodder, 1986: 113)

Perhaps. But is this transformational mode of explanation any more convincing than the sort of empirical generalizations discarded earlier by Hodder? His argument suffers from an inverted notion of context in archaeological explanation. When Hodder exhorts archaeologists to consider variability and change in relation to context, he really means only one kind of context – namely, the specific, constructed context of the particular culture or culture-historical tradition. In his ethnohistorical argument for the transformation of decoration from human body designs to calabashes among the Ilchamus, the problem is reduced by Hodder to a search for historical ante-

cedents which, in the retrospective view generated by present-day Ilchamus values, is rationalized by an essential continuity of aesthetic associations between females and decoration. From an Ilchamus-centric point of view, it seems an inevitable transformation from pre-existing conditions. But Hodder's aesthetic explanation is not convincing because he fails to control for other, even more fundamental or "first order" elements of the total context of Ilchamus culture change. Hodder presents an explanation for symbolic changes that arose during a period of rapid and profound change for the Ilchamus, yet he does not deal in a detailed or controlled manner with the shift in settlement and economy by the Ilchamus around 1900.

In order to accept Hodder's symbolic and aesthetic explanation, one must first accept his assertion that the shifts from nucleated villages to dispersed settlements and from irrigation agriculture to cattle herding were *not* important. From an archaeological point of view, this is potentially the most important aspect of Hodder's argument, yet he deals with it only in passing. If, in fact, it were possible to demonstrate that changes of this magnitude and rapidity in the economy and settlement of any cultural settlement are of minor importance, we could accept his claim that symbolic expression represents the principal domain of archaeological study. But Hodder provides no details about these changes, and the reader is unable to control for these "first order" variables. For example, the Ilchamus must have been extraordinarily ineffective farmers if, as Hodder asserts, their irrigation agriculture could not support populations as large or larger than those subsisting later by means of herding. On general cross-cultural grounds, agricultural intensification, especially in the form of irrigation, usually affords opportunities for population growth that exceed the potential of any other mode of subsistence (Geertz, 1971; Boserup, 1965). If the Ilchamus prior to 1900 departed from that general pattern, we need to know why. What combination of ecological, technological, social and agronomic factors might have led the Ilchamus to be such poor farmers that they had to restrict their population size? And what opportunities did cattle-herding afford for sustaining larger human populations than were possible under irrigation agriculture? If Hodder had answered these questions in a controlled manner, we could evaluate the importance of his symbolic arguments. As it is, his symbolic approach raises more questions about Ilchamus culture change than it answers.

Except for the presumed internal consistency of decoration aesthetics within Ilchamus culture through the last 200 years, Hodder's symbolic explanation lacks controls within the domain of economic and residential behavior and is therefore no easier for us to evaluate than the "empirical generalizations" criticized by Cordell, et. al. Hodder's symbolic/transformational mode of explanation is not necessarily wrong. There may, indeed, be historical continuity and logical consistency in the Ilchamus decorative art tradition, and the transformations in this art style may be causally related to changes in women's roles within Ilchamus society. But for these claims to be credible explanations as opposed to merely stimulating possibilities, we need to see a better definition of the ecological, economic, and residential parameters of existence for the Ilchamus under both their pre-1900 irrigation agriculture and post-1900 life as cattle-herders. The failure in this case lies in the assumption that the only context that matters in archaeological explanation – that is, which needs to be controlled for – lies within the domain of culturally constructed values and meanings. The ingenuity and internal consistency of Hodder's argument has a certain appeal as an exercise in anthropological aesthetics. It may be good "emic" ethnology, in this case applied to the expressive aspects of Ilchamus material culture. But it has very little to do with archaeological explanation.

Controlling for Context: The Scientific Alternative

If, on the other hand, we approach archaeology as a historical science and consider ethnoarchaeology as a way of building bridging arguments between the flux of present-day human behavior and the *longue durée* of the human past, we can avoid becoming trapped by overly restrictive notions of what constitutes relevant context in archaeological explanation. We can avoid pitfalls presented by the extreme positions taken by Binford and Hodder and the adherents of their respective views. While Hodder would like to restrict the role of ethnoarchaeology to a form of cultural anthropology for the purpose of generating ideas about particular historical pasts in different cultures, he does not seem willing to accept the constraining or limiting role that ethnoarchaeology also plays in allowing us to choose intelligently among these ideas. This may stem from an unwillingness to recognize the

existence of an historical reality in the human past comparable to, say, the concept of plate tectonics in geology, neo-Darwinian evolutionism in biology (and, particularly, in paleontology), and similar sorts of syntheses achieved in other historical sciences. These are all ideas about the past that require constant testing and evaluation with respect to the limits of their credibility. Without recognized test implications, such ideas of the past would be impossible to evaluate. The situation regarding the human past is the same, whether or not the symbolists and phenomenologists in our discipline care to recognize it. Real world constraints apply to our ideas about the past, and one of the most important roles of ethnoarchaeology in developing credible ideas about the human past is to inform archaeologists about what kinds of ideas are *not* possible by evaluating them in relation to real world conditions. Before zooming off to "higher order" explanations based on symbolic and expressive aspects of human behavior, we need to take a controlled look at how much of the variability in the archaeological record we can account for effectively in relation to human behavior appropriate to these "first order" constraints.

Although attacked repeatedly and often by Hodder and others, the earlier work of David Clarke (especially Clarke, 1972) offers a useful guide toward a scientific approach to the study of the human past. The use of scientific principles and empirical testing of these general relationships in particular cases and under controlled conditions has been caricatured by Binford (1985) who views this as a crudely positivist kind of "natural scientific" approach to present and past human behavior and by Hodder (1986), who views it as an equally crude positivist assault on the humanistic dimension of archaeology. Anthropological critics, steeped in the traditions of "emic" anthropology, are quick to point out that this kind of science is merely one of many forms of human knowledge. From this point of view of cultural relativism, science, as the product of Western culture, is seen as relatively "no better" than other culturally constructed systems of knowledge. Logically, this is true, but so, too, is the opposite view derived from cultural relativism in anthropology (although sometimes overlooked) that each culturally constructed system of knowledge serves the needs of its own cultural system. Ngatatjara Aborigine cosmology, as a system of knowledge, may be right for the Ngatatjara and should be understood relative to the internal needs of Ngatatjara society. But recognition of the validity of Ngatatjara cosmo-

logy for the Ngatatjara in no way reduces the needs for Western social scientists (including archaeologists) to understand and apply the rules of Western science in appropriate ways to their materials. The same anthropological concept of cultural relativism that enjoins us to understand and appreciate each culture's own system of knowledge in its own right also requires us to recognize the importance of our own Western scientific system of knowledge for ourselves. Not only is the "game" of science, with its particular rules, the only game in town for Western social scientists, but there is irony in the fact that this very concept of cultural relativism is itself the product of and an integral part of Western social science. It will not be found in Ngatatjara cosmology or in any other non-Western system of knowledge that I am aware of. Instead of appealing to the relativistic nature of Western scientific knowledge as an excuse for not doing science, archaeologists and anthropologists need to look for ways to use the improved self-awareness afforded by "emic" ethnology, Critical Theory, and other self-reflexive approaches to provide better levels of control when they try to use scientific methods to account for variability in past human behavior represented in the archaeological record.

The key concept here is the use of controls. If "context is everything", then the recognition and control of relevant context should be of primary concern in any attempt at archaeological explanation. If most differences among archaeologists arise from disagreement over what constitutes relevant context, then it can also be said that most failures in archaeological inference can be traced to inadequate controls, often in the form of assumptions about the context that were overlooked and were, therefore, uncontrolled. It is interesting to see how, despite their diametrically opposed positions regarding the way archaeological explanations should be developed from ethnoarchaeological findings, both Binford and Hodder believe it is possible to "read the past" directly from the archaeological record. Yet, as Schiffer (1987) effectively demonstrates, the archaeological record is subject to a wide range of transformation processes of both natural and cultural origin which must be identified, disaggregated, and controlled for before one can begin to infer the nature of the human behavior indicated by these remains. That is, archaeological explanation is an indirect process that must proceed in an organized manner.

Ethnoarchaeological observations are needed for this indirect process, just as are approaches

like geo-archaeology, taphonomy, paleobotany, and other methods that control for what Schiffer refers to as the archaeological context. Acceptance of this scientifically-controlled view effectively precludes direct explanations about the human past from archaeological remains, whether these take the form of inferences about "fossilized human behavior" or a search for indications of more culturally constructed kinds of expressive and symbolic behavior. An experienced ethnoarchaeologist learns to suspect direct inferences about prehistoric human behavior based upon ethnographic analogues, because so often they fail to control for critical natural factors that have affected the archaeological record. Along with such natural factors can be included later cultural factors that may be totally unrelated to the prehistoric cultural system that produced the archaeological associations in question. Not only is this kind of argument developed in detail by Schiffer, but it also applies in underwater archaeology and has been discussed in detail by Muckelroy (1978).

In this latter case, Muckelroy developed the concept of "filters" that structure the archaeological record to varying degrees and must be recognized and controlled for before cultural inferences about the human behavior that might have originally produced a particular set of underwater archaeological associations are attempted. The actions of shipworms, silting, corrosion, marine growth, currents, wave action, and subsidence are all examples of filters operating in the natural environment upon submerged cultural remains. But there are also factors like salvage, dredging, industrial development, and the use of explosives in underwater warfare which represent cultural factors unrelated to the original formation of the site but which also can have important effects on the structure of submerged sites and associated materials. Ethnoarchaeological concepts like Murphy's "One More Voyage Hypothesis" (Murphy, 1983) can be evaluated in relation to archaeological materials only after these other controls have been applied in the context of particular sites.

Before going on to evaluate this example of ethnoarchaeological analysis more fully, let me stress that a controlled, scientifically credible approach to ethnoarchaeological explanations of archaeological materials and associations requires not only that we ask the right kinds of questions (questions based upon test implications that can be evaluated) but also that *these questions must be asked in the right order*. "First order" questions relating to the real world con-

straints that could potentially structure the archaeological record must be asked first. If, as often happens, these do not account fully and parsimoniously for the material associations, then we can proceed on to consider "higher order" questions of a more culturally constructed nature. Earlier (Gould, 1980) I pointed out that systematic efforts to account for material associations by positing and testing first-order questions of an eco-utilitarian nature in ethnoarchaeological analysis can be expected to produce "anomalies" that will require higher-order explanations that will exceed the uniformitarian assumptions normally used in the historical sciences. While it is certainly true, as Watson (Gould & Watson, 1982:358) has pointed out, that there is no such thing as "cultural uniformitarianism" it is also true that there are scientifically controlled and convincing ways to examine general propositions about culturally constructed human behavior in the past in relation to particular cases. But this can happen only if one initially exhausts the first-order possibilities for explanation that could be expected to structure the potential archaeological record. These first-order questions include Schiffer's "formation processes" and Muckelroy's "filters," and they also include eco-utilitarian factors that apply to the ethnographic behavior studied by ethnoarchaeologists.

The most effective litmus test for ethnoarchaeological explanations is to look for higher-order explanations that fail to consider more comprehensive and parsimonious first-order explanations for the same material associations in the archaeological record. In a scientifically credible approach, one cannot proceed to higher-order explanations of material associations until one has exhausted the first-order possibilities. This is a materialist strategy for dealing with archaeological and ethnoarchaeological materials, but, as I have emphasized elsewhere (Gould, 1980: 159-60), it is not a materialist philosophy. Once behavioral anomalies have been identified using a step-by-step, controlled materialist approach, it is necessary to turn to explanations based upon the culturally constructed principles that *are demonstrably particular* to the culture-historical tradition being examined. This is not a plea for a "balanced view". Nor is it an attempt at a compromise position that will provide archaeologists with a warrant to accept any explanation of past human behavior that appeals to them because of its ingenuity or consistency with respect to a present day, existing culture.

One More Voyage?

Murphy's hypothesis rests upon a kind of limited uniformitarianism that applies only to contemporary maritime behavior and its historical antecedents in the evolution of the Western capitalistic-mercantile tradition. This is the same culture-historical tradition identified by Braudel as an enduring theme (or *longue durée*) in European history in his monumental study of the Mediterranean region. Murphy (1983:75) notes that modern shipowners, as well as those of the recent historic past, tend to operate their vessels "just one more voyage" beyond their designed or safe use-lives. Initially, this model of human behavior depends upon observations of contemporary and recent historical merchant shipping practices in relation to the total context in which they occur.

The definition of exactly what constitutes "one more voyage" depends very much upon that context and the ethnoarchaeologist's ability to identify and control for it as fully as possible. We do not have to search far for contemporary or recent examples of this kind of behavior. In 1973, following the Arab oil embargo in the Middle East, it became necessary to ship oil by tanker around Africa via the Cape of Good Hope to European and American markets. With the high price of oil then, shippers were encouraged to order large numbers of giant ships, commonly termed "supertankers". These ships had to be built rapidly to meet this demand, so elements of mass production and simplified construction were emphasized along with great size. These were among the largest ships ever built, and their history and development are well documented (Mostert, 1974). Prefabricated structures and all-welded steel hulls were used in building these ships, many of which had single-screw propulsion. As Mostert points out, these features, along with the ever-present danger of fire and explosion aboard tankers of any size, present real dangers at sea. Very large all-welded steel hulls can, under certain conditions involving wave action, split from stresses imposed along the ship's length (termed "hogging" and "sagging"). The single-screw system of propulsion is cheap to build and to operate, but it provides much poorer maneuvering than a twin-screw system. Again, due to their great size, supertankers were hard to steer and even harder to stop when under way. So the risk of collision in crowded sea lanes, like the English Channel, was greater with supertankers than with ships of more conventional construction. Mostert's dire predic-

tions about the dangers of operating supertankers were borne out in the years following the publication of his book, the most spectacular example being the wreck of the supertanker, *Amoco Cadiz*, off the northwest coast of France in 1978 (Chelminski, 1987).

These ships were designed for short use-lives, — around five years in most cases. Because of the high price and demand for oil, shipowners expected to amortize their ships within that amount of time and to clear a substantial profit, and in many cases they did. But, as Mostert points out, this did not always mean the end of the ships' use at sea. Often, such ships were sold to other owners who continued to operate them beyond their intended or designed use-lives. Such ships became increasingly vulnerable to damage, due to metal fatigue in the hulls and worn-out machinery. And their subsequent owners were not always careful to maintain or operate the ships according to their original standards. Since such practices produce lowered safety standards, these ships could not be expected to meet the requirements for registration in the leading maritime nations. So their registration has been progressively transferred to countries that provide a specialized service to shipowners whose ships cannot meet these standards. Such "flags of convenience" have led to an extraordinary number of older ships with registrations in such countries as Liberia, Panama and Malta, none of which is known for its indigenous maritime industry.

These ships continue to operate around the world, often being passed from one set of owners to another, until, as Murphy's hypothesis predicts, some critical element of the ship's construction or machinery fails. The existence of socio-economic institutions such as the "flag of convenience" indicates how well established this practice of always trying to squeeze one more voyage out of a ship has become.

An earlier example of this practice can be found in the use of American-built "Liberty Ships" in the years following World War II. During the war it was apparent that cargo ships sunk by German submarines had to be replaced quickly and that shipping tonnage in both the Atlantic and Pacific theatres had to be expanded. This led to one of the greatest shipbuilding expansions in history. Techniques of prefabrication and mass-production were combined in American shipyards with standardized design and simplified engineering to produce 2751 of these ships between 1941 and 1945. As in the case of supertankers in the 1970s, welded

hulls and single-screw propulsion were important features. These ships also used triple-expansion reciprocating steam engines of a type not built or seen in widespread use since the early 1900s. These engines of old-fashioned design produced relatively slow operating speeds and were not fuel-efficient, but they could be maintained and repaired almost anywhere in the world (where the older, reciprocating steam technology was still used in many places). Unlike supertankers, these ships were smaller and less vulnerable to the stresses imposed by larger structures and load. In general, Liberty Ships performed well during the war, but they were designed as a wartime expedient and intended for a use-life of uncertain but limited duration (Bunker, 1985; Sawyer & Mitchell, 1985).

Immediately following the end of World War II, however, 221 of these ships were transferred to private owners in the United States and over 600 to owners abroad to help meet the increased demand for commercial shipping during the postwar reconstruction period. Some of these ships continued in use until as late as the 1970s, but many were lost in circumstances that fit Murphy's hypothesis. Splitting of the hull due to the combined effects of metal fatigue and general neglect as well as machinery failures were factors in at least 14 cases, usually in the immediate context of a storm or some other hazard to navigation (Bunker, 1985:191-204). Liberty Ships no longer operate commercially today, but their wrecks can still be seen in some parts of the world and there is one preserved, operating example restored and maintained by the U.S. National Park Service in San Francisco Bay (Butowsky, 1985, Section 25; Sawyer & Mitchell, 1985: 229-35).

Other potential cases of the "one more voyage hypothesis" abound, often in areas like the Great Lakes of North America. Here, ship-owners were tempted to push their ships up to and beyond the limits imposed by severe storms that arrive in the fall. Ore- and grain-carriers sometimes were operated late into the fall season and were lost in these storms (Nordby, pers. comm.). In the Great Lakes, conditions differ from the world's oceans because of the freshwater operating environment. In most ocean areas, ships' hulls deteriorate more rapidly than their machinery owing to the corrosive effects of salt water. But in a freshwater environment, it is the machinery that is most likely to fail first. A typical scenario for this region, as predicted by Murphy's hypothesis, is for an aging ship to attempt to operate late into the

season. When confronted by a storm, the engine or perhaps the steering machinery fails, and the ship then drifts out of control onto a reef or other obstacle and is wrecked. The Great Lakes contain many known "ship traps" where repeated examples of this sequence of events have produced concentrations of shipwrecks that have attracted the attention of sport divers and archaeologists.

Murphy's hypothesis has been a guiding theme in the underwater research program carried out by the Submerged Cultural Resources Unit of the U.S. National Park Service at Isle Royale, in Lake Superior (Lenihan, 1987). This island is a good example of a Great Lakes "ship trap", and the waters around it contain the wrecks of numerous 19th and early 20th Century steamships. One of the important test implications of Murphy's hypothesis, as applied to the wrecks of Isle Royale, is that, if these ships were pushed "one more voyage" beyond the limits of their designed use-lives, there should be indications of just what it was in each ship's machinery or engines that failed. Ships that broke apart and sank due to the failure of the hull, on the other hand, could disprove this hypothesis. The purpose of the hypothesis, of course, is to direct attention of researchers to the question of what it was that failed when these ships sank at Isle Royale (and in deeper waters) in an organized and controlled manner. The case of Isle Royale is an example of how important it is to identify and control for the widest range of relevant contextual variables. This starts with "first order" aspects of the physical environment, such as weather, wave conditions, currents, geography, and, in this case, the relative effects of a freshwater as opposed to salt water environment on iron and steel steamships, and it extends to "higher order" variables having to do with the larger culture-historical context of commercial shipping in the Great Lakes region as a part of Western mercantile capitalism.

One must always remember, of course, that ships can be wrecked as a result of many different kinds of proximate causes. Bad weather, hazards to navigation such as uncharted reefs, accidental collisions, and incompetence are only a few examples of such factors which lead to the loss of ships — even ships that were new when they were lost, like the *Titanic* and the *Vasa*. Use of hypotheses like the "one more voyage" argument is intended to call attention to possible systemic factors that led to a particular ship's loss under these kinds of proximate conditions. Why, for example, did the Captain of the *Titanic* drive

his ship at high speed at night through seas known to contain icebergs? What social and cultural factors arising from that period can help to account for such high-risk behavior? A controlled study of the proximate conditions under which the loss of the *Titanic* occurred indicates that the captain's behavior was anomalous (a conclusion also reached by the inquest that followed the loss [Wade, 1986]). This, in turn, requires us to seek higher-order explanations related to post-Victorian values such as misplaced confidence in engineering (the idea that any ship could be "unsinkable"), the English class system (was the Captain trying to impress his influential first-class passengers with his ship's performance?), or pressure from the White Star management to meet unrealistic schedules in an atmosphere of economic competition between passenger lines, symbolized by the Blue Riband award for speed at sea (a variation, perhaps, on the "one more voyage" theme?). The "one more voyage" hypothesis is not intended to account for all shipwrecks. But it does represent an example of how an ethnoarchaeological approach can, through controlled observations of contemporary human behavior and use of historical sources, identify and evaluate potential systemic factors in the culture that lead, ultimately, to particular kinds of shipwrecks.

This hypothesis can be extended and tested in any area or period in which the tradition of Western mercantile capitalism occurred. The research at Isle Royale has particular importance for the Scandinavian region, because, aside from the Great lakes of North America, the Baltic Sea is the largest body of fresh (or nearly fresh) water in the world. The ethnoarchaeological model described here briefly under the label of the "one more voyage" hypothesis can be tested under physical circumstances that are more like the Great Lakes than anywhere else. The Baltic, in fact, is potentially a better area for such ethnoarchaeological hypothesis-testing for other reasons as well. The maritime history of the Baltic is much longer than in the Great Lakes, as is the history of mercantile capitalism there. This great theme or *longue durée* evolved in the Baltic much earlier than anywhere in North America, thus offering nautical archaeologists an opportunity for observing the development of this culture-historical tradition in relation to the material remains it produced over an optimal time-span. Furthermore, the same freshwater conditions that act to preserve metal hulls on ships in both the Baltic and in the Great Lakes

also inhibit the activity of shipworms (*Teredo navalis*), which are the principal agents of destruction of wooden ship remains exposed above the siltline in saltwater environments. So wooden ships are extraordinarily well preserved in these two great bodies of water, as shown by the example of the *Vasa* (sunk in 1628) in Sweden and the wrecks of the *Hamilton* and *Scourge* (from the War of 1812) in lake Ontario. These wrecks and others like them recorded by Cederlund (1983) in Sweden and by Kehusmaa (1981; 1986, also Grönhagen & Kontinen, 1988: 130) on the *Sofia Maria* in the northern part of the Gulf of Bothnia, off the coast of Finland show how widespread these conditions of good preservation are. Along with shipwrecks, one should also be aware of the possibilities for discovering and recording well preserved wooden pilings and other underwater structures associated with harbors, port facilities, and fortifications.

But the attraction for archaeologists of the Great Lakes and the Baltic region lies not only in the excellent preservation of shipwrecks but, more importantly, in the ways in which these remains can be examined from an ethnoarchaeological perspective, by controlling for the total relevant context and by evaluating these remains in relation to these controls by asking appropriate questions in the right order. Since the "one more voyage" hypothesis assumes a particular culture-historical context, it cannot be expected to have operated in other culture-historical traditions where long-distance seafaring was important. For example, there is no reason to expect the material remains of the ancient Polynesians to conform to the test implications of this hypothesis. Ethnographic studies on traditional Pacific Island systems of sailing, boat-building, and navigation (Gladwin, 1970; Lewis, 1972; Finney, 1977; 1985) point to an entirely different nautical historical *longue durée* in which concepts of mercantile capitalism played no part. Limited archaeological finds from a Polynesian voyaging canoe on Huahine, in the Society Islands, generally support this view (Sinoto, 1983). It remains to be seen to what extent an idea like the "one more voyage" hypothesis could apply to other non-Western nautical traditions and their archaeological by-products, as, for example, in Asia, where shipwreck studies are also gaining momentum. On the other hand, archaeological evidence for the "one more voyage" hypothesis can serve to identify the penetration of Western mercantile capitalism into non-Western contexts, such as the Pacific.

Perhaps the best potential example so far in nautical archaeology of the operation of the "one more voyage" hypothesis comes from the wreck of the *Trinidad Valencera*, a ship from the Spanish Armada of 1588 wrecked on the west coast of Ireland. This is a particularly important case, because it is solidly embedded in the culture-historical context of the *longue durée* described in detail by Braudel – that is, the evolving tradition of mercantile capitalism in relation to maritime technology and commerce of the Mediterranean region during the time of Philip II of Spain. A published account by Martin (1979) of the *Trinidad Valencera* indicates that this ship was built originally for the Venetians at Ragusa (Dubrovnik) as a merchant ship. Later, this ship was taken over by Philip II as he requisitioned ships and supplies throughout his European empire for his assault on England in 1588. During her short career with the Armada, the *Trinidad Valencera* was used as a transport for carrying troops and heavy equipment for the planned invasion of England. This ship's history was thus a mirror-image of the Liberty Ships described earlier. In this case, we have a merchant ship converted to military, wartime use, but still bearing the marks of its mercantile origins. In the case of the Liberty Ships, we find military ships that later served as merchant vessels.

In both cases, elements of circumstantial expediency (a favorite theme of Binford's) played a critical role in structuring the potential or actual archaeological record, and these elements must be recognized and controlled for. On the other hand, this line of reasoning is quickly exhausted when it confronts anomalous associations. Why, for example, would ships with steam engines of antiquated design, single screw propulsion and other measurably inefficient technological features (that is, below the contemporary state-of-the-art in marine engineering) be found in association with other less-than-proven features in relation to operating conditions (such as welded steel hulls)? To account for such anomalies in American-built Liberty Ships, one must move to a narrower but higher-order, culture-historical context and *control for changes that took place in the nature of expediency*, – that is, in the shift from wartime to peacetime operating needs and conditions. It is in this latter context that the approach advocated by Hodder begins to make sense, but only *after* one has effectively recognized and controlled for the circumstantial factors that are affected by this culture-historically determined shift in expediency. Welded steel hulls were adaptive

under wartime circumstances that called for rapid, mass-production, but they proved to be maladaptive under the new circumstances imposed by postwar commerce, where we find the culturally-structured "one more voyage" effect taking its toll.

In the case of the *Trinidad Valencera*, Martin points out that the physical remains of the ship's hull bear the marks of mass-production for mercantile purposes. He notes that, during the 16th Century, Venice was struggling to maintain its position as an important trading center. In order to be more competitive, Venetian shipowners ordered ships built rapidly and in large numbers, using manufacturing shortcuts to accomplish this. These included the use of iron nails to fasten hull planks instead of the traditional but more laborious (and durable) method of drilling holes and inserting wooden pegs. And it included the placement of these nails in straight lines (easier for untrained or unskilled shipyard workers to accomplish) rather than in staggered positions. Of course, iron nails corrode fairly rapidly in a saltwater environment, and nails set in straight rows along wooden planks will risk splitting the planks when the hull is stressed by movement at sea. Like the supertankers of the 1970s, these ships were designed for relatively short use-lives. Only in this case, wartime needs instead of commercial expediency may have led to "one more voyage" and the loss of the ship.

There is documentary evidence (Martin, 1979:16) that the *Trinidad Valencera* was leaking badly by the time she reached the Irish coast, and that she was brought inshore before she could sink in deeper water. Contrary to popular impressions, the sea battles between the English fleet and the Spanish Armada produced few sinkings and did little direct damage to the Armada. But the English did finally succeed in breaking up the Spanish formation, causing their ships to disperse to the north and west. Not only did this mean they were unable to complete their mission of transporting the Duke of Parma's armies to England, but they were also unable to retreat southward to Spain back through the English Channel (that is, against the prevailing winds). The real test for the Armada ships came, not at the hands of the English, but in the face of strong autumn gales in the North Sea and off the north and west coasts of the British Isles. Many Armada ships, some weakened by battle damage, others by the failure of a Mediterranean hull-first type of construction, and still others that were trapped by a combination of poor navigation and bad seamanship, were lost

at sea or came ashore on the coasts of Ireland, Scotland, and islands to the north (Mattingly, 1962; Martin, 1975; Martin & Parker, 1988; Fallon, 1978; Gould, 1983). So the *Trinidad Valencera* was not alone in her plight, which raises a problem of explanation for archaeologists. It seems clear that in some cases these sinkings were due to failures in their essentially Mediterranean-type construction, in which the strength of the ship's structure was based on the integrity of the outer "skin" of closely fitted planks. This egg-shell-like structure could collapse quickly if broken at any point, and this does, indeed, seem to be what happened in the case of another famous Armada wreck, the *Santa Maria de la Rosa* (Martin, 1973). It is still not altogether clear to what extent the *Trinidad Valencera* shared the problems of other Mediterranean-built ships in the Armada, due to this type of construction, or whether her plight was uniquely due to a failure of her hull due to shortcuts in 16th Century Venetian shipbuilding.

In the case of the *Trinidad Valencera*, a credible idea of the past pertaining to the loss of this ship (and other Armada ships as well) will depend upon systematic recognition, disaggregation, and control for each of these different technologies in relation to the immediate circumstances of the sinking (such as weather, currents, shoreline conditions) before higher order factors akin to the "one more voyage" hypothesis can be effectively brought to bear. The patterning of material remains from the *Trinidad Valencera* is quite scattered, even for heavy, compact objects like cannons and anchors, which do not generally move once they come to rest on the seabed, and this scattering requires an explanation. Where was the ballast (if any)? Was the ship overloaded? I am not questioning the historical accuracy of Martin's observations on the nature of 16th Century Venetian shipbuilding or his identification of relevant details of ship construction in the preserved planks of the *Trinidad Valencera* wreck. But in order to evaluate the importance of his higher order explanation for this wreck as an indirect expression of the dominant values of the particular maritime-mercantile society that produced the ship, we must first control for the first-order variables that may have operated to structure the archaeological record and attempt to exhaust these alternatives. The problem with explaining the wreck of the *Trinidad Valencera* is that we have too many competing ideas about how it may have come about and no clear framework yet for choosing the most convincing of these ideas.

Martin's explanation may be correct, and it has a strong logical appeal and consistency with the urgent, *ad hoc* manner in which the Armada's ships, guns, supplies and crews were assembled (Mattingly, 1962; 202-16). But more than logic or consistency is needed for a credible idea of the past based upon this variation on the theme of Murphy's "one more voyage" hypothesis (represented by Martin's explanation for the wreck of the *Trinidad Valencera*). The use of this ethnoarchaeological mode of explanation must arise from an organized and controlled study of the effects of various natural and cultural factors that could have structured the archaeological associations at the wreck site, along the lines proposed by Muckelroy through the examination of "filters" on underwater site assemblages or by Schiffer in his discussion of archaeological formation processes. I understand that analysis of materials from the *Trinidad Valencera* is continuing, and I hope that a more detailed final report will be forthcoming that will address and control for widest possible range of relevant context at this important wreck site. Meanwhile, the "one more voyage" hypothesis and its variations stand as potential ethnoarchaeological explanations once the first-order levels of explanations at the *Trinidad Valencera* site have been exhausted.

The Future of Ethnoarchaeology

I found it interesting that two of the most recently published analytical treatments of archaeological theory arrive at diametrically opposite conclusions about the usefulness of ethnoarchaeology for future archaeological research. While discussing ambiguities in the archaeological record, Schiffer (1987: 363) notes that:

"...future investigators need not suffer from these uncertainties because ethnoarchaeology, experimental archaeology, historical archaeology, geoarchaeology, vertebrate taphonomy, and other fields have begun to supply relevant general principles."

Hodder, (1986:117) on the other hand, takes the position that:

"As ethnography becomes more like anthropology and ethnohistory, and it needs to incorporate the methods of these adjacent disciplines more fully, its independent existence comes under threat - at least in its present form. In its place we are likely to find material culture studies sitting astride many

disciplines, and a different ethnoarchaeology, concerned with the archaeology of ethnic groups and with an archaeological dimension to ethnohistory."

Which, then, is it to be? Can ethnoarchaeology serve the basic needs of archaeology and provide a body of relevant theory that will help to resolve the potential ambiguities of the archaeological record for the purposes of inferring and accounting for variability in past human behavior? Or is ethnoarchaeology destined to be a prisoner of the present, becoming simply a kind of ethnology of material culture with little relevance to the archaeological record?

The answer depends on how ethnoarchaeology is used. In this paper I have argued that archaeologists cannot "read the past," as both Hodder and Binford, in their different ways, have claimed. For them, ethnographic observations serve as analogues for a direct interpretation of past human behavior. Whether these analogues are based on circumstantial or symbolic elements of human behavior, they fail to control adequately for the total range of factors that structure the archaeological record. Such direct ethnographic analogues may serve as interesting potential alternative ideas about prehistoric human behavior represented by archaeological remains, but they remain speculative and lack credibility when they fail to recognize or observe the limitations imposed by real world constraints to speculation. This failure arises from a kind of tunnel vision with respect to what constitutes relevant context in archaeological inference. *A priori* assumptions that limit ideas about what constitutes relevant context in archaeological reasoning are the single greatest source of disagreement in archaeological interpretation, with all that this implies about uncontrolled ethnocentrism by archaeologists.

What ethnoarchaeology offers is an organized approach that can define relevant context in human behavior for purposes of archaeological inference and control for this context in a way that will select the most credible idea of the past from among competing alternative ideas. This limiting or constraining role of ethnoarchaeology may be unpalatable to some archaeologists, especially those of the symbolist persuasion, who see it as an affront to human freedom. But I would argue that the opportunity and ability to make intelligent choices based on the use of scientifically organized and controlled frameworks is a better way to think of freedom as it applies to archaeological reasoning and our ideas about the human

past. There is nothing dehumanizing about the use of controlled arguments applied in the right order, — that is, from "first order" to "higher order" variables of human behavior that might have structured the archaeological record — to construct a credible approximation of the "real thing" represented by Braudel's concept of the *longue durée* as it applies to different cultural traditions. The future of ethnoarchaeology depends, therefore, upon our willingness to use ethnographic observations in an indirect manner as an instrument of choice to provide the best idea of the human past in relation to what the recognition and control for the widest possible range of relevant context will allow.

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