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The Razor's Edge: Symbolic-Structuralist Archeology and the Expansion of Archeological Inference

In this two-part article, Watson summarizes and discusses a number of new themes in the literature on archeological theory with critical emphasis on symbolic-structural approaches. Fotiadis comments by applying a structuralist analysis to Watson's argument.

IN 1973, THE BRITISH THEORIST DAVID CLARKE characterized archeology as “an undisciplined empirical discipline.” The undisciplined part of his description is as apt now as it was then, but the characterization of archeology as empirical is currently subject to qualification.

The strong central tendencies of 19th and early 20th century archeology (chronology, art history or history of architecture, and narrative cultural history) have given way in the past 20 years to discussion and debate—oral and in print—concerning every aspect of archeological method and theory. The very existence of the archeological record has been questioned (Patrik 1985), and some archeologists no longer seek to know the past, whereas others seem to believe that, in any case, the past is ultimately inaccessible.

By now, there are numerous archeological schools, scholarly bands, or working groups intently pursuing a variety of approaches, a few of which are mutually exclusive and a few others of which are not even primarily focused on the human past. I provide a brief overview of this rather bewildering array before taking a more detailed look at one portion of it: symbolic-structuralist archeology.

From Processualist to Postprocessualist Archeology

Several recent summaries are available, so the task of overview is considerably simplified (Conkey 1989; Dunnell 1984, 1985, 1986; Hodder 1985; Leone 1982a, 1982b, 1986; Shennan 1986; Trigger 1984a; Watson 1986, 1991). Nevertheless, the scene is complex and there is ample room for differing interpretations. During the 1960s and 1970s, archeology in the United States—and to a certain extent in Europe and in Latin America—underwent a lively period of questioning, debate, and ferment that was then often called a revolution. The actual changes that occurred were from overwhelming concern with the chronology of diagnostic artifacts and their spatial distributions to a heavy emphasis on ecological, environmental, and subsistence-oriented paleoeconomic avenues to knowledge of the past, together with equally heavy emphasis on quantification and on the generalizing (nomothetic) potential of archeology overtly practiced as a form of social science rather than as a necessarily particularistic or narrowly historicist pursuit. The advocates

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of these and related new research foci called their kind of archeology “new archeology” or “processual archeology.”

By 1980, a variety of critical reactions to new archeology arose on both sides of the Atlantic: (1) Inadequate attention was paid to site-formational processes; that is, how a living community becomes an archeological site, and what cultural and noncultural forces affect it before an archeologist gets there. (2) The philosophy of science and the scientific method were oversimplified, or misinterpreted. (3) No attention was paid to the cultural *meanings* of material objects, and to the interplay between the symbol systems and the artifacts created by past human groups. New archeology dehumanized the study of the human past, and completely ignored ideology. (4) All causes for change or development through time were sought (and found) in the category of eco-utilitarian or environmental mechanisms (e.g., population increase or climatic change). (5) The research and results of new archeology were biased by the sociopolitical contexts of its practitioners.

Ian Hodder (1985) uses the phrase “postprocessualist archaeology” to include the approaches grounded by the most radical of those criticisms.

That Binford and the new archaeologists . . . did or did not allow generalizations about meaningful social action and do not allow explanation without prediction is, in my view, because they are caught within a language and a coping system that is based on technical control. . . . By emphasizing the meaningful construction of social acts and the historical particularity of human culture I seek to dissolve the timeless past both in its role as the ultimate legitimation of the modern technocratic West and in its function as the prop of the professional theoretician. . . . The social theory [I present] . . . seems to be as relevant for reconstructions of the past as it is for understanding the archaeologist at work in contemporary society. Culture and the cultural past are the media and results of practical actions. . . . The complacent supportive ideology of a timeless past in which Man the passive and efficient animal is controlled by laws that he cannot usurp must be at least criticized and can, it is hoped, be replaced by the individual actively and meaningfully creating his or her world. [Hodder 1985:22–23]

The diverse viewpoints and perspectives Hodder groups under the single phrase, “postprocessualist archaeology,” could be seen as belonging to two major groups. They are the cognitive, structural, and symbolic approaches, on the one hand, and the critical or Marxist approaches (including Marxist structuralists) on the other (Shennan 1986). Within the first category, the three major subdivisions—cognitive, structural, symbolic—overlap in various ways (Conkey 1989; Leone 1986).

Similarities and distinctions among these are approximately as follows: All three view material culture as “text,” an encoding of the symbol systems that ordered the lives of those people who created the material culture. Symbolic and structural archeologists are very much concerned with the recursive or interactive quality of culture—the interplay between and among people, their symbol systems, and each other; the daily and moment-by-moment creation of systems of meaning. Some structuralists differ from symbolic archeologists in believing that these symbol systems are always made up of coherent sets of fundamental oppositions (pure/impure, male/female, healthy/unhealthy, sacred/profane, etc.), but structuralist archeologists as a group (binarian or other) are not particularly concerned with the origins of these symbol systems, whereas symbolic archeologists are. Some cognitive archeologists use the binary opposition, Lévi-Straussian structuralist method (i.e., they phrase their descriptions of the cognitive patterns they believe to be encoded in material culture in the form of binary oppositions like blood/milk, red/white, life/death, raw/cooked), but are not especially concerned with the issue of recursiveness—the interactive quality of culture—nor with the origins, history, sociocultural context, and relevance to individual humans of the symbol systems they describe.

As to the second major category, proponents of critical archeology diverge from symbolic and structural archeologists because the latter two renounce the materialism that is so strong in new archeology, whereas critical archeologists do not reject a materialist tradition. Critical archeologists do, however, fault new archeologists for ignoring ideol-

ogy, the major part of culture that, in the critical archeologist's view, acts as a mask to conceal contradiction and conflict within a society. Ideology, they say, enables the continuity and reproduction of society.

There is, in fact, a double-barreled denial by symbolic and critical archeologists of scientific neutrality in the practice of archeology: archeologists do not produce objective knowledge about the past. Symbolic archeologists take the past to be a social creation in our own as in all other societies. Hence, Euro-American archeologists, for example, who produce some version of the past within and for their society, are not exempt from all manner of biases arising from their individual positions, as well as from the institutional setting of their discipline in their own society. Critical archeologists believe with Marx that history is always created to serve class interests.

Symbolic and critical archeologists attack the positivism that was so central to new archeology, with the symbolic archeologists in the vanguard. They deny a value-free status to Western scientific logic, noting that an unaware science is ignorant of its own culture and of the fact that method itself is of cultural origin. As Leone (1986:429) remarks, "the relationship between the present and interpretations of the past is assumed to be a political and economic one."

Finally, all cognitive and symbolic archeologists are deeply concerned with intracultural *meaning* (i.e., understanding from the viewpoint of the natives inside the culture) as they strive for what they call interpretation rather than for the defining of variables, the testing of hypotheses, and the formulation of scientific explanations, as in new archeology (e.g., Watson, LeBlanc, and Redman 1984). Shennan notes, however, as regards "neo-marxist" archeology (a variety of critical archeology), that similarities to new archeology "are not altogether lacking. Both are concerned with what they see as real socio-economic processes in the past and for both the task of reconstructing the past in prehistory involves the use of the archaeological record to 'monitor' . . . past processes of change" (Shennan 1986:331; see also Hodder 1987b). It would seem, then, that the symbolic-structural postprocessualists are more at variance with the processualist archeologists than are neo-marxist archeologists.

Although explicit concern with the cognitive systems, symbol systems, and/or structural categories of past peoples is central to postprocessualist archeology, it is not altogether new. For example, Deetz was pioneering cognitive approaches to archeology in the 1960s (Deetz 1967:83–101), and during the 1970s Robert Hall advocated a kind of cognitive archeology emphasizing ancient ritual and belief systems (Hall 1977). In addition, Conkey (1989:137, 147) notes that there were several attempts at structuralist analysis within archeology in the 1970s and early 1980s. Among others, she lists Clarke (1972), Conkey (1978a, 1982), Deetz (1977), Fritz (1978), Glassie (1975), Leone (1977), Muller (1966, 1977), and Washburn (1977). These all exemplify a kind of structuralism she calls "formalist," which can be and often was assimilated or accommodated to processualist archeology because the objective was usually to delineate implicit structural systems thought to express the boundaries and activities of social groups. Delineation and "monitoring" of social groups in the archeological record was a popular goal among new archeologists. As with much of structuralist research, the emphasis of these formalists is often more on form than on meaning, although some formalist scholars do address function of stylistic or design systems (e.g., Conkey 1978a; Weissner 1983).

One of the strongest challenges to the processualist school by the symbolic-structuralist postprocessualists is concern with intracultural *meaning*, especially the investigation of meaning via the prehistoric archeological record. At this point I turn to an illustrative example that largely predates postprocessualism: interpretation of the Upper Paleolithic cave paintings in Franco-Cantabria. This case is interesting for a number of reasons, at least two of which are relevant here: (1) It has not escaped the notice of processualists, and others who are not persuaded by the symbolic-structuralist postprocessualists, that virtually all of their published work so far has been within or has relied heavily upon ethnographic and historical data (e.g., Leone and Potter 1988), leaving the question of

relevance for prehistoric archeology ambiguous. (2) Interpretation of the Upper Paleolithic cave paintings, sculptures, carvings, bas-reliefs, and engravings has a long history (more than 100 years), much of it featuring cognitive and symbolic approaches. Therefore, this rather well-developed example may indicate the advantages and disadvantages of such approaches more clearly than less intensively examined archeological cases.

Upper Paleolithic Art

One of the first professional prehistorians to commit himself to systematic research on Upper Paleolithic cave art is Abbé Henri Breuil. To the extent that Breuil offers interpretations, he follows Reinach (1903), who suggests that Upper Paleolithic art was created in a context of hunting magic and increase magic. That is, the artists were attempting, by means of sympathetic magic, to ensure hunting success and the continued well-being of the food animals upon which they depended.

Largely through Breuil's influence, Reinach's ideas were accepted by nearly everyone for decades, until André Leroi-Gourhan (1958a, 1958b, 1965, 1967) and Annette Laming-Emperaire (1959, 1962) independently produced a fascinating and quite different set of interpretive formulations based on structuralist assumptions. Leroi-Gourhan concluded that the cave-wall pictures are carefully arranged compositions, not casual agglomerations, that certain animals and signs are characteristically central whereas others are peripheral (both in the cave itself and in the individual painted tableaux), and that everything seems to be arranged according to a dualistic system.

The organization of the figures, which seem scattered at random over the wildly uneven surfaces of the cave walls, is so extraordinary that it must be shown in greater detail. By this route alone shall we reach toward the thoughts of those men who are the only people anywhere in the world, at any epoch, to have sheltered their works of art in the dank depths of caves. [Leroi-Gourhan 1967:111]

Here, Leroi-Gourhan is clearly intent not only on delineating the structure or formal system underlying the cave art but also on revealing the mental processes of the artists whose works were created 15,000 to 30,000 years ago. As Conkey points out, his method combines structuralist and symbolic analyses:

his structuralism treats the imagery as a system of signs. . . . Leroi-Gourhan considered a well-preserved cave to be a message (cave-as-text) with elements (frame and figures); and these elements, he notes, are in the very position chosen by the author of the figures. . . . the mode governing the assemblage was that of a *mythogram* in which the figures are arranged around a central point, as in a picture. [Conkey 1989:142, 143]

Leroi-Gourhan began his research by defining and quantifying the universe he was investigating. In his first comprehensive account (Leroi-Gourhan 1967), for example, he says there are 2,188 identifiable animal figures in the 66 caves he visited and studied. Of these, 610 are horses, 510 are bison, 205 are mammoths, 176 ibex, 137 wild cattle. These animals are differentially distributed within a cave, and there are also preferential associations among the different animal types: for example, 64% of all the occurrences of bison also contain depictions of horses. Ibex and stag occur more commonly at entrances, in peripheral positions, and at the backs of caves. Horse, ibex, cervid (the "framing" or "complementary" animals) are also those usually found on objects Leroi-Gourhan believed to have been used by men rather than women (spears, harpoons, spear-throwers, and pierced staff-handles). He thinks there are two major associational groupings among the hundreds of animal representations—(1) horses/ibex/cervids and (2) bison/cattle (and perhaps also mammoths)—and that the division of animal types derives from bipolar sexual symbolism. The male species (horses, ibex, cervids) are usually in the backs of caves or on the peripheries of wall panels, whereas the female species (bison, cattle) are centrally distributed both in the caves and in the panels.

Leroi-Gourhan also finds the enigmatic "signs" (geometric shapes such as ovals, rectangles, straight or hooked lines, rows of dots) to fall into two morphological divisions—

wide and narrow—which he believes to be sexually symbolic, female and male, respectively. They also are often paired, he says, one narrow with one broad: “realistic sexual symbols were numerous enough that, even without the help of abstract signs, the presence of sexual polarization is evident” (Leroi-Gourhan 1986:16, quoted in Conkey 1989:145). Leroi-Gourhan comes to a number of general conclusions, among which is a suggestion that the cave itself was thought of as female. He believes the cave art as a whole expresses a symbolic system centering on sexual union and death (spear and wound) integrated into a life-renewal cycle with two series of actors: man/horse/spear and woman/bison/wound.

There are serious weaknesses in Leroi-Gourhan’s study (Parkington 1969; Roussot, Frost, and Daubisse 1984; Ucko and Rosenfeld 1967), and in a second volume published only a few years before his death, he presents a modified view in which the sexual polarization highlighted in his earlier accounts is not so prominent (Leroi-Gourhan 1982; see Conkey’s recent summary, 1989:146). Nevertheless, even the strongest critics acknowledge that Laming-Emperaire’s and Leroi-Gourhan’s work opened new realms in the interpretation, not only of Upper Paleolithic Europe, but also of the human past in general. These new realms lie for the most part within the cognitive-symbolic-structuralist sphere.

In the years since Laming-Emperaire’s and Leroi-Gourhan’s initial publications, other scholars have examined Upper Paleolithic art from similar as well as differing perspectives (Ucko and Rosenfeld 1967; Conkey 1978a, 1978b, 1982; Lorblanchet 1977, 1980; Marshack 1972; Pfeiffer 1982; Vialou 1982; White 1986). Although few of these more recent investigators follow Leroi-Gourhan closely, all do emphasize the cognitive processes of the prehistoric artists as central to a comprehensive understanding of the cave art. Marshack, for instance, believes that notational systems of various kinds (especially those recording lunar phases and seasonal cycles) are encoded in both the *art mobilier* and *art rupestre*, and that these played an important role in the day-to-day and long-term survival of prehistoric human groups. Conkey (1978a) also stresses the importance of style in general, and in the Upper Paleolithic *art mobilier* in particular, as a means of transmitting information about ethnicity and social boundaries.

One recent publication (Halverson 1987) takes the deliberately provocative view that Upper Paleolithic art is essentially meaningless play, “representation for representation’s sake,” but most interpreters base their investigations on the assumption that it *was* meaningful, and that we—15,000 to 30,000 years after the fact—can discern something about that meaning (see “Comments” in Halverson 1987; Lewin 1986). The orientation of most current research on Upper Paleolithic Franco-Cantabrian art is thus the cognitive one of Leroi-Gourhan and Laming-Emperaire: a central issue addressed by nearly all investigators is the symbolic significance of the painted and engraved representations to their creators. Although one recent study (Rice and Paterson 1985, 1986) provides empirical support for the original interpretation that cave art was part of a hunting complex, the authors are careful to note that their statistical comparison of relative species frequencies between the bones and the cave paintings produces results compatible with a variety of hypotheses: “hunting magic, storytelling about hunts, fertility magic, hunt education, and variations on these themes” (Rice and Paterson 1986:658).

I return to symbolic-structural analyses of Upper Paleolithic art below, but now take up another example of symbolic-structuralist archeology.

The Postprocessualism of Ian Hodder

One of the most influential of the postprocessualists is the British archeologist Ian Hodder, who—together with his students—has produced a flood of articles and books in the past few years (e.g., Hodder 1979, 1981, 1982a, 1982b, 1982c, 1982d, 1982e, 1984, 1986, 1987a, 1987b; Miller and Tilley 1984). Beginning from a more or less conventional, late 1960s–early 1970s, new-archeology-like, David Clarkean position (e.g., Hodder and Or-

ton 1976), Hodder has moved to a critical, symbolic-structuralist approach, and—in at least some of his writing—beyond that to a skeptical crisis.

I illustrate the complexity of Hodder's views here by referring to two of his recent papers in some detail (Hodder 1982d, 1987a). In the earlier paper, he refers to pottery decoration, battle ax types, burial modes, and settlement pattern characteristics of the *Trichterrandbecher* (TRB), Protruding Foot Beaker (PFB), *Linearbandkeramik* (LBK), and other early agricultural societies of the 5th, 4th, and 3rd millennia B.C. in northern and central Europe (Hodder 1982d). For the TRB and PFB, for example, he notes intriguing parallels between changes through time in the organization of pottery decoration on the one hand, and in mortuary patterns on the other. The incised decoration on earlier TRB pottery comprises a basic contrast between vertical and horizontal arrangements proliferated in a dendritic manner.

Through time this structure is gradually transformed so that clear boundaries are not drawn, but there is cross-referencing and contrast between the different zones. In the latest phases . . . the overall dendritic system becomes lost in favour of a series of horizontal bands of zones, and the proportion of the pot surface that is decorated gradually decreases. [Hodder 1982d:166]

Hodder notes that there are important changes in the relationship of burial mode to settlement pattern contemporaneous with these ceramic changes. Complex communal burial had begun prior to the TRB and is thought by some archeologists to reflect competing corporate groups (clans or lineages) jockeying for dominant positions in regional, sociopolitical hierarchies. Communal burial continues into the early TRB period, but, at least in the Netherlands, settlement pattern changes to short-term, transitory communities. Hence, burial mode (long-term, stable) contrasts with settlement mode (short-term, unstable).

Hodder relates the burial to the pottery evidence as follows:

Within the [TRB] social and economic system as described, a series of contradictions and contrasts are emerging. There is a concern by dominant and subordinate groups to emphasise traditional, stable ties to ancestors, in the context of shorter term, expanding settlement. . . . It is at the locus of this tension and contradiction that the burial ritual acts. There may also be tensions between senior and younger men, and between men and women. Regional distinctions in pottery styles . . . and battle axe types emerge . . . indicating a social contrast between spatial groups. The various distinct categories of pottery form and decoration, being involved in daily activities, would serve to separate these activities, and the individuals associated with them, setting up symbolic contrasts and forming the social distinctions in a context in which there was a concern about the contradiction of a traditional social structure. The contrasts and oppositions in the [pottery] decoration would act, perhaps as a mnemonic, perhaps as a comment, to reproduce and to "naturalise" the social categories. [Hodder 1982d:171]

Several of the significant themes identified by Leone (1986) and summarized above are clearly present here: Material culture is viewed as a dynamic product of the society in question, changing it and being changed in response to human actions, and thus resulting in social and political trends. Conflicts like that between the older (pre-TRB) principle of stable groups and the newer (TRB) practice of settlement dispersal are softened, masked, or legitimated by details of pottery form and decoration. These details help delineate the various social groups and make them seem "natural."

Even a sympathetic reader may find some of this argument a bit tenuous, and I think it would be difficult to confirm the postulated link between social organization and pottery decoration in a living society, let alone one that has been extinct for more than 4,000 years. (Hodder himself [1986:41–42] has criticized this earlier work of his for not giving adequate attention to the meanings of the pottery vessels and their decoration.) Still, I am willing to go along with Hodder in his claim that the general interpretation he offers for the nature of changes in the archeological record between TRB and PFB could be at least partially tested for fit by seeking more detailed evidence of settlement pattern and duration, subsistence trends, mortuary programs, and the functions of ceramic classes.

Although he explicitly rejects a nomothetic approach (1982d:176), Hodder is *not* here divorcing his work from the empirical reality of the archeological record. Rather, he seems concerned to preserve empirical content in his formulation.

In a more recent publication, "The Meaning of Discard: Ash and Domestic Space in Baringo," he expresses a particularistic perspective so thoroughgoing that it would seem to rule out any hope of obtaining knowledge of the past, or at any rate knowledge of the only sort he believes to be interesting and worthwhile about the human societies that the archeological record represents.

[1.] . . . the particular case must initially be understood in its own terms, rather than by reference to general covariation with external variables, such as social complexity, duration of occupation. . . . there can be no general theory and no universal method for measuring and interpreting activity residues, except in relation to physical, nonhuman, processes of decay and deposition. [2.] . . . Ethnoarchaeological studies . . . cannot contribute directly to our understanding of the past since the meaning of settlement organization and discard can only be derived from the context (present or past) within which settlement use and artifact discard take place. . . .

To understand ash discard among the Ilchamus [for example] I need to follow all the contextual clues available. I need to discuss women and their roles in society because it is largely women who produce and discard ash. I need to discuss the meanings of the color white because ash is described as white. I need to discuss the color red because white ash is often contrasted with red soil. I need to examine the other activities which take place by the hearth where ash is produced, and so on. There is not the space here to follow up all the webs of associations and meanings surrounding ash in Ilchamus society. [Hodder 1987a:424–425]

To most field archeologists, the above two points would seem sufficient to divorce a Hodderian pursuit of anthropological knowledge from access to any prehistoric society, but Hodder adds a third point: culture is *interactive*; the contextual meanings of objects and actions are *negotiated* in the normal course of social process.

[3.] . . . human behavior is creatively and actively adaptive. The term "context" aims to capture, and to bring together, both sides of the coin: material adaptation and cultural ideal. The word as used in this paper refers both to the framework of meaning within which individuals act, and to the situated improvisations which make up daily life. [Hodder 1987a:425]

Hodder discusses those aspects of the Ilchamus belief system that provide a meaningful context for ash discard. These are manifold, intricate, and almost endlessly ramifying. For instance, ash has many positive connotations, evoking women's activities, little children, cooking, food, and healing (because it is sometimes used to dress wounds). But it also has strong negative meanings: if a man touches hearth ashes, or if women discard domestic ash in the presence of a man, he will die. The spirits of the dead gather around the hearth; and when taken outside the compound, hearth ash becomes a powerful curse.

The white color of ash ties it to other qualities symbolized by the color white: fertility and reproduction in the domestic realm, for example. White is contrasted with red, which means strength and danger in the outer world.

Following the introductory two pages containing the three points already alluded to, this extended discussion of meaning, context, and symbolism of ash—and the manipulations of those meanings by Ilchamus women and men—makes up the bulk of Hodder's article (17 pages out of 24). It is difficult for me to believe that a practicing field archeologist reading the article would conclude that a Hodderian approach as represented there could be or should be applied to the prehistoric archeological record. Hodder himself has not done this. His analysis of the Dutch Neolithic discussed above (Hodder 1982d) was applied to archeological materials, but was not part of an archeological (i.e., excavation-recovery-analysis) research design. Rather, Hodder examined materials collected previously. In the Netherlands Neolithic paper, however, he does indicate the directions an actual archeological project might take if it were intended to check his interpretation. And in *Reading the Past*, although it is as programmatic as most of the rest of his publications, he is explicitly concerned with verification (Hodder 1986:49–53, 93ff.,

135). In the 1987 Ilchamus paper, Hodder also makes a pass at situating his concerns within the realm of field archeology and of an empirical archeological record. He speaks of general, noncausal (archeological) principles that underlie all human culture, such as his belief that meaning is created and expressed by similarities and differences along four dimensions: space, time, typology, and unit of deposition (a pit or hearth, for example).

These relationships are built at both the surface (systemic functional) and at deeper (structural) levels. Where archaeological data show repeated patterning using these varied principles, it is possible to begin to "read" the text, making interpretations of things "with their text," context. . . . Every term used, every assumption made, must be criticized and examined carefully to see if there is contextual data to suggest that different meanings may have held in the past society being studied. . . . In each particular archaeological case, relationships, similarities, contrasts, structures are interpreted and continually and critically checked against the subjectively perceived data. Analogies and the historical imagination are used and argued through in relation to the data. The ultimate aim may be generalization, and generalization may play a part in the choice of analogies and in the initial formulation of questions and hypotheses. But the primary concern is with context. [Hodder 1987a:445]

I do not believe this argument would be found either inviting or compelling when viewed by the cold eye of an experienced archeological field supervisor, who had also read the body of the paper to which it is attached. Hodder's stated goal (to correct what he perceives as the weaknesses and failings of processual archeology while building on its strengths) is a very worthy endeavor, but discussions like the 1987 Ilchamus paper are counterproductive to that goal because they leave the reader with the strong impression that the author has gone native, has lost his analytical perspective, having come to believe that the only true understanding of a particular society is that in the minds of its creators and participants, and that this understanding can be obtained only by living and talking with them, and hearing them talk to each other, on a long-term, daily basis.

The Razor's Edge

In both the examples used here—interpretations of Upper Paleolithic art, and Hodder's postprocessualist approach to archeological materials—we see a common striving to push archeological inference to its limits, while also expanding those limits in directions ignored or declared out of bounds by the processualist new archeologists (e.g., Binford 1967, 1968). Leroi-Gourhan's analysis—although strongly contrastive to much of contemporary new archeology in content—is positivistic, mainstream new archeology in orientation and basic procedure. Setting himself against Breuil and practically everyone else, Leroi-Gourhan took as a fundamental operating assumption that cave art represents a coherent, orderly, knowable system (compare Watson, LeBlanc, and Redman 1971:4). He then sets out to define and describe the system via a set of quantification procedures, and to explain it, albeit in a direction (paleocognition) not deemed productive by Binford and his followers. Moreover, even in the most elaborate formulations stressing paleocognition—those of Leroi-Gourhan and Marshack—there is concern for the empirical content of the interpretation.

Like Leroi-Gourhan, Hodder also wants to expand archeological inference much further in the direction of intracultural meaning (ideology, social action, and cognition) than the new archeologists were willing to contemplate. And he also explicitly disavows their positivistic insistence on hypothetico-deductive approaches and nomothetic purposes, which he finds dehumanizing and repugnant. Yet he does speak of testing and validation, and is usually careful not to go so flagrantly beyond the limits of what would be construed as valid inference by his processualist archeological peers that they can dismiss his entire program as irrelevant to real archeology.

Both the Upper Paleolithic art interpreters and Hodder operate near the boundaries of empirically based research, but Hodder seems to have placed himself in a much more precariously balanced position on the very edge of that research universe. In the Ilchamus ash-discard paper, we see him not only walking the razor's edge that separates far-

extended inference about the past from imaginative formulations free of empirical contact or content, but also slipping off it into the realm of ingenious but non-empirical and archeologically irrelevant literary exercises.

There is often a rather thin line between archeological inference and the writing of fiction because archeology is, by nature, an underdetermined kind of scholarship. One of the principal aims of the new archeologists was to make their discipline a more robust form of social science than it had previously been. Hodder and the postprocessualists believe that the processualist approach took the soul out of archeology and out of the cultural past by denying, or at any rate ignoring, the basic humanity of those who created the archeological record. But in Hodder's well-intended attempt to redress that situation, he has brought himself—and, perhaps, a certain number of his archeological colleagues as well—to the brink of a serious skeptical crisis about any meaningful accessibility to the prehistoric human past.

Perhaps one reason for this is that the task he has set is difficult, so difficult that he himself remains within the realm of ethnoarcheology, rather than pressing on to apply his theoretical program to the subject matter of prehistoric archeology. This does not mean the task is impossible, however, and in fact some of Hodder's own students and young colleagues (e.g., Miller and Tilley 1984), as well as other archeologists elsewhere (e.g., Conkey 1978b, 1989; Hall 1977, 1983, 1985), continue to walk the razor's edge in the interests of expanding and strengthening archeological inference about prehistory while not relinquishing the gains made by their processualist predecessors.

Ian Hodder is a gifted, articulate, and charismatic individual, but if he wants to influence the field archeologists who are primary producers of archeological information about the prehistoric past, then he must not flirt publicly with a symbolic-structuralist-induced, skeptical archeological crisis. He cannot convince such archeologists that symbolic-structural postprocessualism is desirable and feasible if he is simultaneously distracting and confusing them by subliminal advocacy of views that run counter to their necessarily empirically based procedures and approaches. Hodder must decide very soon, and then show us unequivocally where he means to direct his talents: into the intricate labyrinths of symbolic-structuralist mind-games, or toward the hard labor of reshaping and expanding archeological inference.

Comments

Michael Fotiadis

Watson's principal point is that the intellectual concerns of Ian Hodder, and of his sympathizers, prescribe a research program that cannot be implemented easily, if at all, in prehistoric archeology; such a program broaches questions that cannot be readily investigated by means of the archeological record.

I leave the task of defending Hodder to Hodder. "He writes as a crusader," notes a reviewer (Cullen 1984) of *The Present Past* (Hodder 1982b). Transgression of the rules of scholarly rhetoric is a conspicuous aspect of the crusader role and, to my mind, the most severe limitation of the crusade. "Whether one's emphasis is structuralist or behaviorist . . . the links between the components of our arguments must be logically defined and documented" (Cullen 1984:267). Without those links, Hodder's constructions on some days appear indeed precarious.

Watson's objection is solidly founded in a disciplinary thought contiguous with common sense. Among the constituents of that relatively stable ground are ideas whose coherence depends solely on other ideas, and so forth, so that the assertions they contain are controvertible. I attempt below to identify a few of those ideas, their sources of coherence and their problematical character. In the process I violate the integrity of Watson's text, constructing my points by highlighting, simplifying, and exaggerating hers. I begin by rephrasing her principal point: How can the symbolic-structuralist program be applied to prehistoric archeology? I assume from the start that the question is rhetorical. What lends weight and conviction to the rhetoric?

With regard to Leroi-Gourhan's work on Paleolithic cave paintings, Watson writes, "Leroi-Gourhan is clearly intent . . . on revealing the mental processes of the artists whose works were created 15,000 to 30,000 years ago" (p. 616), and, about Hodder's work, "it would be difficult to confirm the postulated link between social organization and pottery decoration in a living society, let alone one that has been extinct for more than 4,000 years" (p. 618) and "It is difficult for me to believe that a practicing field archeologist reading the article [about the Ilchamus] would conclude that a Hodderian approach as represented there could be or should be applied to the prehistoric archeological record" (p. 619). Once those three passages are juxtaposed to form a short text, an emphasis on the distance—in fact, absence—of the societies on which the structuralist program must apply becomes apparent. The master opposition here is not simply "present/past," but, metaphorically and irrevocably, "living/extinct," hence also "present/absent." Founded on such a stark and familiar metaphor, Watson's challenge to a "Hodderian" approach to the prehistoric past follows forcefully: to claim that the study of the mental processes of prehistoric men and women are within the reach of science is to claim that we can communicate with the nonexistent.

The question arising here concerns the appropriateness of the "living/extinct" metaphor for shaping the relationship of present to past societies and for justifying the difficulty that Watson notes. In illuminating that relationship one would need to address such perplexed issues as continuity and the ideological tension between the concepts "the human past" and "our (viz. ethnic, cultural, national) past" (e.g., Leone 1982a; Trigger 1984b; Herzfeld 1986). At the end, in the case of Leroi-Gourhan at least, the difficulty seems to me to arise not from the "extinction" of Paleolithic society but from an inadequate theory of meaning.

A second opposition, in fact, which intersects that between present and past, concerns the locus of meaning vis-à-vis "the empirical reality of the archeological record" (Watson, here p. 619). For example, Watson writes that in the Ilchamus paper, "Hodder discusses those aspects of the Ilchamus belief system that provide a meaningful context for ash discard" (p. 619). At first, no opposition is apparent in this sentence, only a disjunction—that between an *action* (ash discard) and its *meaningful context* (belief system). Immediately preceding the sentence in Watson's text is a short quotation from Hodder (1987a), where the latter makes it fairly clear that under "context" both meaning and action are to be subsumed (see also Hodder 1985:2). Hodder could, therefore, charge here that Watson's formulation distorts his conceptualization. Yet Watson's formulation still makes a good deal more sense than Hodder's, and the reason is that hers is contingent on well-established oppositions in disciplinary discourse (and not exclusively in archeology).

Specifically, "ash discard" is an activity concept, and although we do not excavate activities, we normally make inferences about them from their material effects (much in the way one infers many characteristics of a fire from the smoke it produces; see Patrik 1985:38, 44–46). The locus of "ash discard" as a concept of the "activity" class is therefore firmly within the archeologist's empirical universe, the realities of the record. The locus of "meaning" and "belief systems," on the other hand, in our firmament of concepts is the human mind. The conceptual distance between the disjuncts "action" and "context" in Watson's text is far greater than it at first appears, contingent on the disjunction "archeological record/human mind." To cross that distance would in fact require crossing historically maintained, formidable disciplinary boundaries ("sciences of the past/sciences of the mind"). Furthermore, if the record has an aspect that is literally visible and tangible, and is thereby situated *before* the senses, the mind—as emanating from the brain—is situated wholly behind the senses. In other words, the reality of the record partakes of the world of phenomena and is, therefore, directly accessible through senses and instrumental measurements. The reality of the mind, hence also of meaning, on the other hand, is hidden in the depths of men's and women's heads.

The opposition I have been outlining is firmly entrenched in disciplinary thought, where it recurs in a variety of expressions (e.g., "surface/deep structure," "object world/

internal representation"). Watson's argument gains force from appealing to this opposition, for, as long as meaning is tied to ideas, belief systems, thoughts, and mental processes—the mind as an *internalized* entity—to contemplate that archeology can recover it would rapidly lead to absurdities. Even, however, when the conceptual association of meaning with the internalized domain of "the mental" is relaxed, disciplinary language in most quarters still places meaning in opposition to the observable world: meaning remains *contained in* that which stands for it and which is directly, empirically accessible. Familiar instances of the opposition include the Saussurean "signifier/signified" and the celebrated "conduit metaphor," the notion that words carry meaning (see, for example, Lakoff and Johnson 1980:10–12). In such instances, meaning is assigned an ambiguous, elusive ontological status, opposed to the material reality (tangible artifacts, audible words, visible signs). To locate it, one must search behind the phenomena. Hodder's own formulations have not always overcome this opposition (see, for example, 1982b:7).

We must think of meaning in different terms. A possibility arises when we open a dictionary. On the left of any given page we find words—that is, *signs*—and to the right we find the meanings of those signs, which, however, are words as well. In short, on either side of the definition we find entities of fundamentally the same nature. No ontological difference whatsoever can be said to hold here between two levels of reality, a tangible/empirical one (signs) and another, which is hidden and empirically inaccessible (meaning).

Furthermore, it is of interest that what appears on the meaning-side of the dictionary entry (definiens) are signs whose meanings are other signs (other dictionary entries), and the meanings of the latter signs are yet other ones, and so forth. With this realization we approach Peirce's notion of *unlimited semiosis*. It is this notion that Hodder (1987a) has effectively expressed, when he argues that he cannot meaningfully speak of the ash until he has spoken of virtually everything else in the culture. Watson's most serious objection concerns precisely the relevance of this argument for archeological research: how would you hope to make use of unlimited semiosis in the archeological record, to uncover the full meaning of an instance of cultural residue, to trace the "global semantic system" (Eco 1976) of an "extinct" society?

I give a short answer to this, cite a second, more substantial one, and look forward to a third, Hodder's own. The goal of an archeology concerned with meanings is not to trace global semantic systems in their entirety any more than the goal of an ecological archeology is to trace entire ecosystems. The fact that archeology's semantic systems and ecosystems are "extinct" has nothing to do with the issue. The notions of unlimited semiosis and global semantic system (and of ecosystem) serve only as methodological postulates, as realistic assumptions, based on limited knowledge, about the workings of our empirical universes. They are conceptual formulations that reveal something fundamental about those universes. (In fact, such postulates are constitutive of the empirical universe of any discipline: no scientific theory with empirical purport can emerge without them.) A "global semantic system" exists only insofar as its postulation permits us to advance models of the production of finite instances of semiosis. The postulate, and the effort to formulate it, are justifiable in the light of the efficacy of those models.

The more substantial answer is provided by Eco (1976, esp. 123–129), from whom I largely have derived mine. Here I will carry the dictionary experiment a little further. Of the vast, chainlike continuum of signs linked with any definiendum, a dictionary, regardless of its weight, lists under that definiendum but a minute fraction. Indeed, that list, which may also contain examples of usage of the definiendum in sentence-long contexts, must be considered only as a shorthand of the meaning of the definiendum, as a "chapter heading," useful for certain purposes. To uncover a fuller meaning, one might perhaps follow the chain of signs from entry to entry, and hope to read in this way the entire dictionary (cf. Quillian 1968:234–240). Suppose that this meaning-zealot managed to carry the search through the first dozen or so links, before despairing. It is clear that:

1. The meaning-zealot abuses the dictionary, for dictionaries are not designed to yield large semantic fields readily; rather, they are designed for specific uses, and the meanings of the words they furnish are so circumscribed as to be adequate for those uses.

2. As the zealot flips the pages back and forth traveling from one entry to another, the dictionary is conceptually transformed. Imagine, for instance, that we record the zealot's travels in a log—what would that log look like? Its organization would be radically different from that of a dictionary, resembling a labyrinthine network without beginning, end, or center. We may say, then, that the transformation amounts to replacing a conception of language as *set of signs* with a conception of language as a network of intersecting codes (cf. Eco 1984:ch. 2).

The work of the meaning-zealot, then, is subversive: the zealot forces the dictionary to reveal some of its rules of production—rules that “fix” the dictionary as a sign and as an artifact. It is revealed, for example, that the dictionary is so conceived and constructed as to interrupt unlimited semiosis, to withhold the fuller meaning of words, and thereby it stands for a particular conception of language (as a set of discrete units); it has, therefore, uses and meanings beyond those it acknowledges about itself in the entry “dictionary.” The dictionary is “fixed” as object and as sign by concealing its rules of production.

Granted that a theory of meaning based on a single thought experiment cannot be too profound, what can we draw from the above? A close tie of meaning with use, first, is suggested. Meanings are utilitarian, in the sense that we do something with them. For instance, we can use the concept of labyrinth to restructure and subvert our concept of language, and we can coalesce behind the subverted concept to resist domination. (See also Friedrich 1989:303, for an analogy between messages and commodities.) Second, it appears that a sign encompasses meanings that become manifest only as one attempts to abuse it. Meanings *are*, after all, elusive and indeterminate. Language, whether ephemeral sounds or the archeologist's lasting material culture, casts a message as it diverts attention from another, concealing as it reveals.

Language, then, offers an immense potential for manipulating the experience of the social universe, especially for inverting causal orders, making appear natural what is cultural convention. We have no reason to suppose either that language acquired this potential only among modern societies or that material culture qua language is unfit as accomplice in the task. We cannot dispense lightly with this double supposition and still claim that we are doing social science. Minimally, we ought to turn the supposition into a pair of research questions: “where and when” or, better, “under what social conditions did material culture qua language acquire that potential?” and “how does that potential differ among human populations at different levels of sociopolitical complexity?” Questions of this kind are perfectly scientific, although equally scientific answers cannot be reached in a few seasons' research. The scientific quality of the questions consists at least in the placing of language in a Cartesian frame as an object that can be analytically and, to a point, observationally isolated. I imagine, therefore, that those who by “social science” hear “science” more than they hear “society” will find such research appealing.

What is indeed disappointing about such a research program is that it undermines its own claim to being *social*. The research questions are about language (as material culture), not about society, which is granted the peripheral status of conditions. Abstracted from the latter, the former as the object of study is rapidly reified: the distinction between the two concepts may seem purely methodological, but is there anything in the research questions that would require at some point a reconsideration of that distinction? The questions indeed are so formulated as to detract attention from the problematic character of the concepts or tools of the investigation—society, language, and their relationship to each other. A hypothesis about what society is, to be negotiated through the empirical research, is rendered irrelevant to the project. Disguised as a methodological distinction, the social theory in the case of such a program is only the residue of the practice.

Scientific though it is, this program fails to address its own fundamental premises, precisely those that permit the reification of such ambivalent signs as “society” and “language.” It could, then, be supplemented by an alternative program, guided by such questions as “how did a given society construe itself through its material culture?” and “what

could have been the principles of such construal?" Material culture is given here the place of symbolic system and, also, of a medium—something to *construe with*. Note, however, that what is construed is also what construes—society. Material culture and society are thus caught (conceptually) in a reflexive relationship, and, as the Cartesian frame collapses, the problematic character of the critical concepts is underlined rather than ignored. Still, this conceptualization by no means prohibits analytical distinctions: from the empirical-analytical viewpoint of the investigation, material culture qua language is seen as socially informed semiosis. But this can be said of "natural" language as well, including the disciplinary idioms in which we (archeologists) construct the past. The analytical-empirical viewpoint, then, reverberates the problematic character of the critical concepts: we are ultimately obligated, in the name of consistency, to treat our own constructions the same way we treat the material culture from the past. We are obligated, that is, to switch our analytical-empirical focus back and forth between the material culture as socially informed semiosis and our own socially produced knowledge (cf. Leone 1982b:752–753). Our critical concepts no longer run the risk of reification (or, perhaps, ossification), as their problematic character is never lost from the focus of the investigation. We are constantly reminded of the question "what is society," that is, "how do we construe society?" There can be no social and cognitive archeology the moment we cease raising this question.

The applicability of the analytical-empirical viewpoint (material culture as socially informed semiosis) is well demonstrated by the work of Shanks and Tilley (1982) on some Neolithic burial practices in Britain and Sweden. Their focus is the manipulation of the human body at death, the dismemberment of the skeletons, and the arrangement of the bones in spatially distinct piles in the grave. The aim is to advance rules that could account for the observed distribution of bones in the graves. The rules to be discovered could, of course, be purely formal (a "grammar," in the usual sense, of sign-vehicles; certain standards about the appropriate treatment of the dead), or they could be viewed as translating into material culture aspects of the social universe (a "semantics" where each aspect of material culture represents a social aspect, e.g., group solidarity, distinctions according to age). Both of these views echo familiar approaches to mortuary ritual in today's archeology.

Shanks and Tilley, however, go beyond these two approaches. Once some rules are advanced (1982:150), certain contradictions among those rules can be detected. Such contradictions are not accountable by the familiar social theory. They necessitate further theorizing—the postulation of second-level rules, or meta-rules—if they are to be explained. These meta-rules pertain neither to pure language (grammar or semantics, in the senses specified) nor to pure social structure. Above all, they do not deserve an independent ontological status, and to call them "deep structures" in the minds of the Neolithic folk would serve no purpose. In fact, those meta-rules stubbornly resist ontological categorization and reification as much for the investigators as they would have—and this is the point of Shanks and Tilley—for the investigated. If anything, for the latter, the meta-rules were utilitarian, serving material interests: the juxtaposition of contradictory rules (i.e., their operation together in spatial-temporal proximity in the context of a unified activity, mortuary ritual) at once affirmed the asymmetry of power relations within the group and detracted attention from it by also emphasizing the symmetry of kin bonds. By virtue of the juxtaposition, power relations were also vested with the authority of the kin bonds. By fusing the arbitrariness of its existence with the necessity of kin relations, power thus concealed its material motives and mystified the rules of its reproduction (Shanks and Tilley 1982:151). Mortuary ritual as semiosis—I would add—concealed the rules of its own production. Was the stratagem successful? We cannot speak of the Neolithic people, but the stratagem continues to elude our social science.

In conducting their research, Shanks and Tilley did not compromise established standards of rigor. Almost two-thirds of their report concerns the documentation of patterning in the record that could not be attributed to post-depositional processes or to the biological structure

of the human skeleton. The cultural rules and meta-rules they propose in order to account for the patterning are informed by a substantial body of social theory, a theory that pertains to power relations and their reproduction, and accords, significantly, material status to ideology. Moreover, the presentation is free of "crusade" rhetoric, to which the authors succumb in some later works (e.g., Shanks and Tilley 1987).

I drew attention to the work of Shanks and Tilley to indicate that the "material culture as socially informed semiosis" viewpoint is perfectly compatible with the empirical reality of the archeological record. Their work is scientific in that it has isolated in the archeological record, and accounted theoretically for, *observable* patterns that remained unaccounted for within the frame of the eco-utilitarian social theory dominant in archeology. What is and what is not empirical reality depends, after all, on the interaction of a subject with its world, both the subject and the world gaining coherence in the course of that interaction. To reduce this reflexive relationship to a more familiar formulation, a realistic model of the world is the outcome of coherent and purposive operations. As the operations change, so does the model. For the social sciences at least, the operations are to an overwhelming degree conceptual. By rearticulating our concepts, then, we also rearrange the empirical reality of the archeological record (see Patrik 1985). That is not to assert that we can see in the record anything we wish, for archeology is not, one hopes, a matter of seeing and wishing but a matter of discipline: our concepts rearticulated still are subject to scrutiny for coherence. Furthermore, it cannot be claimed that archeology, cognitive or other, is at this point capable of testing its coherence wholly by its own means (by engaging in dialogue solely with its empirical universe; cf. Wylie 1982). It has been clear for some time, for example, that the vigor of the new archeology, up to the 1970s at least, rested more with a new, intellectually plausible conceptualization of the archeological record and less with empirical successes (Dunnell 1986:33–40). Coherence there was largely provided, as it is today, by an already existing conceptual framework, that of the language of social science. In the best cases, the framework is not only given explicit attention within the project but also acknowledges its own political content (see, for example, the first section in Shanks and Tilley [1982:124–134]). To purify our science from such politically conscious frameworks is to deny the materiality of the past and to render our profession obsolete.

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