

## Integrating Ethnoarchaeology: A Subtle Realist Perspective

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Ethnoarchaeology is characterized by a diversity of products that often seem to have little in common but which can for the most part be assigned to one or the other of two contrasting, although not incompatible, schools: the scientist and the hermeneutic. A subtle realist philosophical perspective and Goodenough's concepts of phenomenal and ideational orders are employed to situate ethnoarchaeological researches in the context of culture as a whole. It becomes apparent that the diversity of studies reflects the expanse of this domain, in the analysis of parts of which it is appropriate to employ a variety of methods and analytical styles. Selected ethnoarchaeological studies are reviewed in demonstration of the thesis that the relative simplicity or complexity and openness or restriction of the systems under investigation are the main factors influencing choices of methods and styles. Ethnoarchaeological and archaeological interpretation should indeed partake both of scientist explanation and hermeneutics. Last, it is noted that, while ethnoarchaeology remains closely linked to archaeology, its scope is widening to include a broader range of anthropological concerns that can be accessed through material culture studies. © 1992 Academic Press, Inc.

There is a strong anti-philosophical strand in ethnographic thinking that places value on the practice and products of research and has little patience with or interest in discussions *about* research. I have some sympathy with this  
Hammersley 1992:43

This is not a philosophical tract, rather an attempt to locate myself and others in a fast changing disciplinary landscape. Ethnoarchaeology was conceived to provide more focused and complete analogies for archaeological application than could be gleaned from ethnographic and other sources (Kleindienst and Watson 1956). Some 35 years later, it is characterized by such a disconcerting variety of topics, approaches, and methodologies that one may well ask whether it is not merely a grab bag: of questions raised, at least initially, by the interpretive needs of archae-

ology; of methods that run the ethnographic gamut from qualitative case study to quantitative survey; of philosophical attitudes that, while rarely explicit, range from positivist to idealist. It is symptomatic that it is difficult to design a coherent university course on ethnoarchaeology.

Perhaps this is not so surprising; a recent book, *What's wrong with ethnography?* (Hammersley 1992), signals that one of our parent disciplines is also in crisis. Hammersley's answer to the question is "an attempt to integrate both the methodological ideas and methods that the term "ethnography" normally refers to into a more appropriate methodological framework for the social sciences" (Hammersley 1992:203). The building of this framework is founded on a "subtle realist" philosophy of science, to be defined below. My aim in this paper is to extend a similar approach to ethnoarchaeology in the hope of finding some underlying unity that can accommodate a variety of problem orientations and research techniques. This is attempted not exhaustively but by reference to selected examples, several from research undertaken by members of the Mandara Archaeological Project in North Cameroon (David and Sterner 1987, 1989; David et al. 1991).

### REALIST PHILOSOPHY OF SCIENCE

Most (ethno)archaeologists avoid taking an explicit philosophical position and are probably wise to do so (see Kelley and Hanen 1988). Nonetheless, we operate *as if* we had answers to the following questions. What are the "things" we study? Are there "laws of human social life"? What is "explanation"? How do we "verify" our explanations? Realist philosophy of science in its "subtle" form provides answers to these questions that serve to orient and direct scientific enquiry without either overly constraining its scope or forcing us into bogus intellectual gymnastics.

The realist research program in social science is conveniently set out by Gibbon (1989:142-172), from whom the following summary of what are for our purposes its essential elements is taken.

Realists distinguish between three domains:

(a) the real—structures and processes, which are often unobservable and may be complex stratified composites (e.g., genes, migration); anything that can bring about changes in material things is real;

(b) the actual—observable events and phenomena; compounds and conjunctures formed by the real; and

(c) the empirical—experiences and facts generated by our theory laden perception of the actual.

Scientists are concerned to identify, define, and explain "things" in the domain of the real. We approach the real through our empirical reading of the actual, and according to the scientific knowledge of the day. It is this

that distinguishes “subtle” from “naive” realism. Naive realists “neglect . . . the way in which theoretical assumptions inform their descriptions and explanations”; they believe that they can have direct contact with reality and achieve knowledge that is certain (Hammersley 1992:32,50).

Society exists as a real object, a complex structure irreducible either to its effects or to people, consisting of the sum of relations, including relations with material culture and the environment, within which individuals and groups stand. Society exists by virtue of the intentional activity of people. It can only be detected by its effects; it generates social life, is manifest in social behavior and its products, and is conceptualized in the experience of its bearers. The causal power of social forms is mediated by people, and social forms are a necessary condition for social action. However, human behavior cannot be determined by or completely explained by reference to social forms and rules, because people are purposeful and possess intentionality and self-consciousness. Psychological and physiological as well as social reasons contribute to intentional human behavior. Thus, people act in open systems codetermined by a variety of mechanisms of which the social is one. Societies are continuously being transformed in practice, are only relatively enduring, and are thus irreducibly *historical*.

Explanation of social phenomena proceeds by the same general process as in the natural sciences:

1. Recognition of a pattern and resolution of events into their components. Events are viewed as conjunctural, the results of the combined effects of a variety of active structures.
2. Redescription of events in the language of social science.
3. Creative model-building, the search for generative mechanisms that might produce the observed pattern. This is an inductive, or more precisely a *retroductive* attempt to lay out the structural conditions that must have existed for the events to be present.
4. Theory construction. Candidate mechanisms are reduced to one as the reality of their postulated structures and powers is checked, in part by evaluation of each mechanism in terms of its plausibility and credibility in the light of other theories, especially those that we currently take to be beyond reasonable doubt, in part by gathering of independent evidence that will subject the theory to maximal threat. If they pass these tests, structures and their workings can then be defined in the form of causal “laws.” However, because of human intentionality and the openness of cultural systems, causal laws describing the way real social things operate must be analyzed as tendencies that may or may not be expressed in particular historic conjunctures.
5. Exploration of the stratum of reality revealed in the previous steps can now begin.

As social structures are only manifest in open systems, decisive tests of theories are impossible. We may be able to explain precisely and accurately, but our capacity to predict remains rudimentary. While the validity of claims is subject to stringent criteria of assessment, proposed definitions of the real and theories about the nature of society are ultimately accepted or rejected on the basis of their explanatory fruitfulness or power. It is this rather than predictive accuracy that decides which of a set of competing models becomes, for the time being, theory.

To this partial and abbreviated account of realist philosophy in social science, I would add a rider. Although humans live in open systems, certain aspects of their behavior are more constrained than others and can be conceptualized in terms of, if not closed, at least restricted systems, some of which are quite simple. Much of the behavior studied by ethnoarchaeologists and archaeologists, tool-making, for example, or subsistence, can be viewed in such terms. The simpler the system and the more it approximates to the closed condition, the greater the predictability of associated behavior. Thus, depending on the natures of the "real things" and system under investigation, different approaches and methodologies may be appropriate, and the ability to predict may be greater or less.

It is because ethnoarchaeology and to a much greater degree archaeology deal with material things and their relations to both the natural and the social environment that there is such disagreement between authorities as to what we should be doing and how we should do it. Binford (e.g., 1977, 1982, 1987) believes that archaeology should be more like a natural than a social science and advocates the development of "middle range theory" to relate the statics of the archaeological record to the dynamics of the living, systemic context, while Hodder (1986:118-146), who sees archaeology as social science, recommends a "contextual" approach in which context-specific structuring principles replace cross-culturally applicable middle range theories. As Kosso (1991) has convincingly shown, there is little difference in the epistemic structure of their approaches.<sup>1\*</sup> Middle range theories relate the empirical, the perceived actual, to the real; so do Hodder's structuring principles, which may indeed be regarded as middle range theories. The "content and justification of theories are [in both cases] strongly influenced by observations, and in turn the informational content and justification of observations are influenced by theories" (Kosso 1991:625). Where Binford and Hodder and other (ethno)archaeologists primarily differ is in (a) the behaviors, relating to more or less open or restricted, simpler or more complex, systems in

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

which they are most interested, (b) in their views of the real things that structure these behaviors, (c) in their understandings of what constitutes explanation and verification, and (d) the corresponding “scientist” or “hermeneutic” styles of their arguments.<sup>2</sup> I use the terms scientist and hermeneutic to contrast *styles* of analysis. Each encompasses a range of philosophical and methodological positions, and indeed individual studies often show blends of the two. Scientist analyses are characterized by preferential use of quantitative approaches and statistical inference, a focus on behavior rather than its meaning, an emphasis on the context of justification over that of discovery, on deduction as against induction, and on cross-cultural laws (whether deterministic or probabilistic) rather than cultural patterns. Hammersley’s (1992:159–173) chapter, “Deconstructing the qualitative–quantitative divide,” is a valuable in-depth treatment of these apparent polarities.

The distinction between the schools is made clearer by specification of the cultural domain that is our object of study.

### THE CULTURAL DOMAIN

The cultural domain is represented in Fig. 1, at the center of which is what Goodenough (1964:11) termed the *phenomenal order*. This is a stratum of real things, activities, and patterning, an abstraction from behaviors *empirically observed* in living cultures. As a soil may be represented as constituted by different proportions of sand, silt, and clay, so certain behaviors are classified as activities that may be categorized in terms of their ideological, social, and technical *aspects*.<sup>3</sup> Certain activities are responsible for that part of the material output of the phenomenal order (henceforth PO) in which archaeologists are primarily interested, that is to say artifacts or, more precisely, things affected by human action and differentiated by matter, form, and context. A small portion of these is recovered as the archaeological record.

POs exist as part of an interacting dyad, the other element of which is the *ideational order* (Goodenough 1964). This is made up of another order of real things, *unobservable* ideas, values, norms, representations, and is itself underlain by a psychocognitive order of reality, the hard wiring of the human mind. The ideational order (henceforth IO) is expressed in, although it does not wholly determine, the activities and their patterning defined as the PO. While all IOs are structured by certain real things relating to the biology of modern *Homo sapiens sapiens*, they vary as a consequence of differing historical trajectories, varying interactions between the dyadic complex and the environment, and within and between the IO and the PO themselves. Furthermore, since things in the IO do not interact directly with the environment, they are less constrained by it than

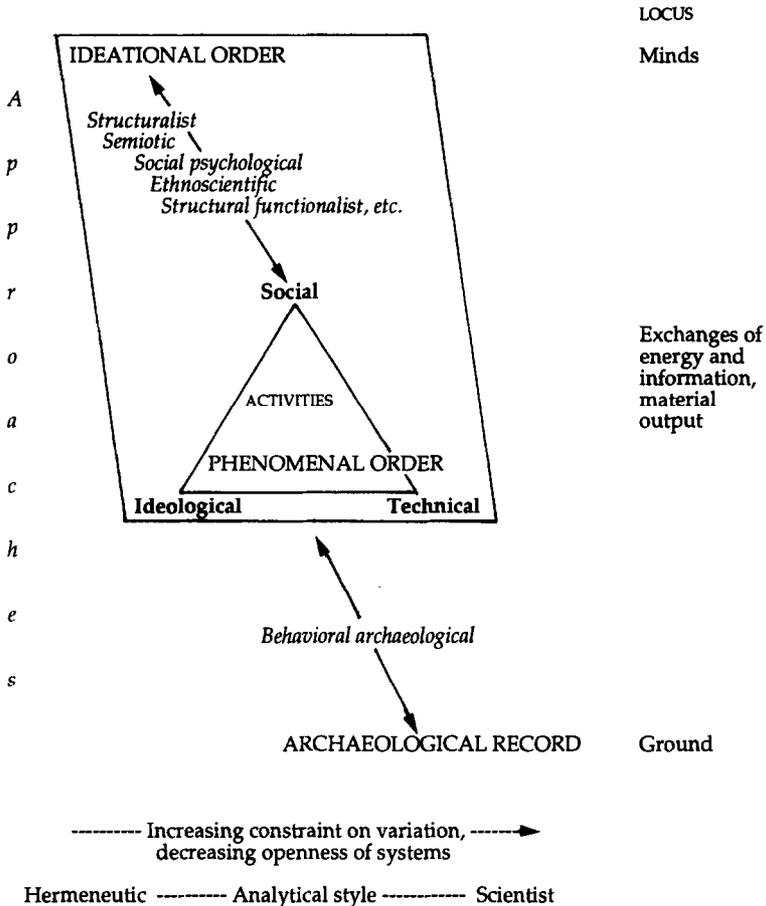


FIG. 1. The cultural domain and its relationship to interpretive approaches and analytical styles.

behaviors in the PO, which, particularly in its technical aspect, comprises relatively restricted subsystems. IOs are open systems characterized by some fuzziness and internal contradiction, competing representations of reality held by different individuals, genders, age groups, craftspeople, ranks, classes, and so on. The relations between IO and PO influence actual behavior and thus its material output. It follows that one cannot reconstruct a PO from cultural events without reference to the IO. The two form a hermeneutic circle: "system is the source for understanding structure, and structure is the background for understanding the system," as Kosso (1991:624) puts it.

An archetypal Binfordian scientist approach in ethnoarchaeology in-

volves attempts to define cross-cultural regularities or lawlike statements derived from ethnoarchaeological and other, including ethological, evidence, in order to apply these to the reconstruction of past POs and their development, and to generate new regularities regarding culture change. Before these regularities can be applied to the archaeological record, they must first be reformulated, in the terminology of “behavioral archaeology” as cultural (C-) and natural (N-) transforms, the former theoretical construct relating observed remains to unobservable past behaviors, the latter “variables of culturally deposited materials and variables of the noncultural environment in which the former materials are found” (Schiffer 1976:14–16). Archetypal Hodderians on the other hand dispute the existence of most cross-cultural regularities and thus their value in prediction. To them, artifacts are not only instruments but also signifiers of signifieds that exist at the IO level. Their informational content must therefore vary as a function of the IO–PO complex that generated them. An axe *means* something very different to a Canadian lumberjack, a Minoan, and a “Stone Age Australian.” Hodderians are therefore concerned to reconstruct specific IO–PO complexes and their diachronic development “from the inside” with, at least in theory, minimal appeal to cross-cultural—as opposed to cross-cognitive—regularities. “To discuss humanity in terms of general laws, is ultimately to deny people their freedom” (Hodder 1986:102) is a fine example of Hodderian oratory, although one which he has denied in practice (Hodder 1982:125–184). Hodderians emphasize the irreducible historicity of societies.

Binfordians and Hodderians are thus primarily interested in defining different sorts of real things, Binfordians the regularities realized in social and technical behavior in environmental context, Hodderians the structures and principles that underlie ideological and social activities in IO context. There are only certain ways in which hunter–gatherers can exploit a tropical rainforest and survive, and the freedom to devise toolkits and form social groupings is limited. Their religious symbolism, on the other hand, is comparatively free to vary. The greater the constraints acting on behavior, the less open and the simpler the system, the more predictable it becomes, and the greater the potential to derive from it a cross-cultural regularity with predictive power. This being the case it is scarcely inappropriate, at least in the first instance, for Binfordians to work in a style inspired by natural sciences in which variables can often be strictly controlled. Where, due to the greater complexity and openness of systems, variables are much harder to control, much less quantify, it is similarly appropriate for Hodderians to grapple with their materials in hermeneutic or symbolic interactionist mode.<sup>4</sup> Binfordians and Hodderians strive for different things, the former “explanations” in terms of laws

that apply cross-culturally, the latter "understandings" in which laws are apparent, if at all, only as tendencies in sets of historical conjunctions. Neither account of sociocultural life is complete without the other.

### MECHANISMS AND PROCESSES, THE PARTICULAR AND THE GENERAL

This contrast between schools, simplified though it may be, provides the basis for a consideration of ethnoarchaeology, the study of the relations between material things and the IO-PO complex in living cultures. Ethnoarchaeology involves field study of the production, typology, distribution, consumption, and discard of material culture, with particular attention to the mechanisms that relate variation and variability to socio-cultural context, and to inference from the mechanisms to processes of culture change. "Mechanisms" are here defined as configurations of the full range of environmental, material, and sociocultural variables that interact at one time horizon to generate patterning in material culture, and "processes" as diachronic changes in mechanisms including their structural change, breakdown, and transformation into others.

Archaeologists are eager consumers of cultural laws, regularities, or tendencies in relationships between material and total culture that can be applied retrodictively to the archaeological record, especially those provided by middle-level theories, "generalizations that attempt to account for the regularities that occur between two or more sets of variables in multiple instances" (Trigger 1989:21). They vary from the particular to the most general. As an example of the first take Yellen's (1977:130) equation: The number of days a Dobe-/Du/Da !Kung camp has been occupied =  $0.1 (\text{Area in square meters of the Absolute Limit of Scatter minus the Limit of Most Scatter}) + 1.87$ . Such a formula is of course of restricted value in archaeological prediction. Note that it refers to the social and technical. As a regularity at the other end of the scale, relating PO to IO, consider the following formulation (suggested by a reading of Kramer 1985:88), "Typology, in the general sense of patterned variation in classes of material culture, reinforces principles of social structure, including gender and power relationships, and reifies other aspects of world view." This candidate law, with which Carol Kramer herself would probably not agree, is applicable to architecture in Western society, or to ceramics in the Mandara region of North Cameroon (David et al. 1988),<sup>5</sup> and, in my opinion, offers a valuable, if only too rarely utilized, approach to interpretation of archaeological and ethnographic data of all times and all places.

Archaeologists and ethnoarchaeologists are faced with inverse observational problems. Archaeological data have a temporal dimension and

manifest the workings over time of cultural processes. The interpretation of a given body of archaeological data must necessarily account for continuity and change through time in terms of both mechanisms and processes. Neither are directly observable. Ethnoarchaeologists work in the "ethnographic present." While they can infer from their observations the existence of mechanisms, they lack opportunities to observe, except over short time spans, the material manifestations of processes. Ethnoarchaeological inference to process therefore takes the following form. A mechanism can be considered as an interplay of variables structured by a relational system that generates a material output. Although we cannot observe them, we may imagine the effects of changes in the value of one or more variables on output. As an example I might point to the possible outcomes under varying conditions of the socioeconomic mechanism underlying the production of Fulani pottery (David and Hennig 1972:26). Within an Islamic context in which male authority over women was marked and there was great desire for children, household instability and income, influencing the desirability and accessibility of nonpottery substitutes, were deemed to be critical variables affecting pottery supply and quality. Such exercises, in which the observer's imagination interacts with informants' accounts of past events and future plans, can extend the data span to several decades while remaining anchored to reality via the inferred mechanism. More difficult is determination of the boundary conditions under which a mechanism will continue to operate without systemic change in its nature. In the example cited, changes as different as acceptance of Christian attitudes to monogamy or the eradication of sexually transmitted diseases would both result in systemic change (see David and Voas 1981). More difficult still is imagining mechanisms that existed in the past but that have no even fairly close analogues today. This is of course not strictly the task of ethnoarchaeologists, although I would argue that our contribution to the world store of analogies is a fertile source of inspiration in this area.

An ultimate aim of Binfordian archaeology and ethnoarchaeology is to arrive at general laws regarding the nature of cultural variability and change. While the ultimate aim of Hodderians is different—to render intelligible the human condition—there is always tension between the particular and the general, any particular interpretation ultimately relying upon assumptions regarding the nature of culture and of culture change. Developments in interpretation occur as the result of a dialectic between the particular and the general that, whether explicitly or not, takes the form of retroductive model building and the "testing," formal or informal, of hypotheses derived from them against a database. While ethnoarchaeology is of necessity particularistic, every effort should be made to draw out the broader implications of each study. It is here that, for what-

ever reason, many ethnoarchaeological studies leave us unsatisfied (see Schiffer 1978).

### ETHNOARCHAEOLOGY IN SCIENTIST AND LOGICIST MODE

In *Archaeological approaches to the present* Yellen exemplified and evaluated four types of archaeological use of ethnographic data: what he termed the laboratory, buckshot, spoiler, and general models.<sup>6</sup> The main aim of his book was the development of transforms relating the dynamics of San behavior to the statics of their abandoned camps. His Ring Model is a general model of a regularity of relevance to the interpretation not only of !Kung camps, but also, with modifications, of any community organized on a circular plan. Although length of stay was an important variable, Yellen was not much concerned with process as defined above, and the model relates to the social and technical aspects of the PO, treating the IO as irrelevant in this context. The symbolic significance of the San hut did not enter into his equation. Neither, one might add, do economists consider the symbolism of sports cars when predicting General Motors' third quarter earnings. That economists are better at explanation than prediction points to the openness and complexity of economic systems and to the exclusion from them of any consideration of the ideational order. "[T]he tastes and habits of the consumer," indeed most of culture, are regarded as givens (J. M. Keynes 1939:245, cited in Herskovits 1960: 47), and this constitutes a real weakness. The foregoing comparison also helps us to understand why it is appropriate that the scientist style of Yellen's analysis is modeled on that of economics and much natural science.

Comparable although less quantitative in its approach is a study by A.-M. and P. Petrequin (1984) of Toffinu villages built in and around Lac Nokoué in Bénin. This was undertaken with the aim of improving and controlling interpretations of housing in Alpine and other European lacustrine habitats. Variations in architecture and community plans are set in their geographic, cultural, sociopolitical, and economic contexts, and close attention is paid to differential processes of "fossilization" of structures and features in relationship to their dry land, liable to flooding, emerging at low water, and fully aquatic situations. The study spans the uncertain boundary between C- and N-transforms and, as might be expected given the marked cultural contrasts between the ethnographic and archaeological situations, its major contribution is taphonomic.

Inferences regarding length of stay at a !Kung camp site and as to whether a particular dwelling was built over open water, in a marsh, or on dry land differ in their applicability to the archaeological record. In the former case the predictive value of the inference rests primarily upon the

extent to which specific sociocultural and economic behaviors are common to the ethnographic instance and the archaeological case under investigation. As such commonality is very hard to demonstrate in the absence of witnesses, the numerical value of the equation even in a Botswana Later Stone Age context can only be considered as broadly indicative. Its real value lies in its contribution to research design. In the case of the Petrequins' study, the predictive value is greater precisely because, while cultural behaviors are of course implicated in production of the data, natural processes (wave action, decay under aerobic and anaerobic conditions, fires, the falling of objects in water, etc.) and mechanical contingencies such as susceptibility of objects to comminution by trampling, largely determine the patterns of deposition that are the signatures of different settings of structures.

It is much easier to reconstruct taphonomic, that is to say largely natural, processes such as these than cultural behaviors that are less immediately constrained by physical laws, and that are not amenable to experimental confirmation. The systems differ in their relative simplicity and openness. Ease of validation of inferences regarding cultural behavior varies inversely with (a) the extent to which that behavior is controlled by properties of matter and (b) the specificity of the behavior in question. Thus, we may well be able to reconstruct details of pressure flaking in the production of arrowheads, but it may be foolish to characterize the remains of a compound as the residence of a polygynous family (see David 1971).

Still in the scientist mode, the marriage of ethnoarchaeology and experimental archaeology, advocated by Tringham (1978) and others, seeks to capitalize upon the enhanced potential of the combination for securing inference. The recent and ongoing work of Longacre (1991a, 1991b) and his Kalinga Ethnoarchaeological Project team in the Phillipines provides some nice examples. Employing an amalgam of both approaches, these researchers have achieved a good understanding of the "surface alterations [of pots] as a direct reflection of a variety of cooking and cleaning behaviors," and of material and functional characteristics that have on the one hand influenced the replacement of certain kinds of pots by metal analogues, and on the other have encouraged a trend toward specialization in pottery production (Longacre 1991b:78). While we must admire such work, we must also be conscious of its limitation in scope to the social and technical, and to the mechanistic rather than the processual, and in the range of cultural behaviors to which it is applicable.<sup>7</sup> We have only to remember Gould's (1980:141-160) demonstration that Australian aborigines on occasion used poor quality stone to manufacture tools because they were "righteous rocks," to realize that cultural mechanisms strongly influenced by the IO might very well lead to the monopolization

of pottery manufacture by a single village even if their clay and other material conditions of production were objectively inferior. Nonetheless, and again following Gould, the recognition through experimentation of such an anomaly could well be the first step toward identification of the full range of cultural mechanisms involved.

How then do we proceed beyond the taphonomic and technical to the valid construction of cultural mechanisms occurring at a specific place and time? Perhaps we can learn from ethnoarchaeologists of the Francophone logicist school, trans-Atlantic cousins of the New Archaeologists. In the words of Gardin (1980), its erudite originator, logicism is a rigorous methodology for the deconstruction of archaeological texts, its goal being "to reformulate archaeological constructions in order to come close to the requirements of logical thinking" (Gardin 1980:15). While Gardin is in sympathy with realism, his follower Gally (1989) takes an explicitly positivist position, aiming to exemplify this in his ethnoarchaeology. We take as an example some recent research on ceramics in the Inland Niger Delta (Gally 1991). Gally aims to use ethnoarchaeology in the demonstration of past cultural mechanisms through empirically verifiable interpretation. He identifies four successive steps in such research:

1. Description of the variability observed ethnographically.
2. Description of the mechanisms responsible.
3. Formulation of "transcultural rules," and
4. Application of the rules in archaeological context.

Steps 1 and 2 are comparable to steps 1 through 3 of the realist procedure described above. Gally's third step involves a reversal of perspective, reformulation of the mechanisms as regularities or rules of inference that can be applied to archaeological remains.<sup>8</sup> Proposed regularities are presented "selon les principes de l'analyse des données en science et du logicisme en sciences humaines" (according to the principles of the analysis of data in science and of logicism in the social sciences) (Gally 1991:47). These in order of, according to him, decreasing accuracy are:

1. statistical correlations between two types of phenomena, continuous or discontinuous,
2. "typologies integrating two or more domains of reality, after each one has been partitioned" (Gally 1989:31), which I interpret to mean the construction of a contingency table, or
3. relations that can be formalized as a sequence of rewrite expressions (propositions) of the type: "if P then  $P_i + 1$ ."

The validity of inferences drawn from regularities of the first two types relies upon statistical measures of correlation or association. But it is well known that these may be spurious or misleading. Thus, Gally's preferred

methods both require to be supported by an analysis of the third type. This involves situation of the regularity in an ethnological context that defines the limits of applicability of the regularities proposed. These limits are expressed in the form of hypotheses. Inferences will be valid only if the mechanisms from which the transcultural rules have been developed were in fact operating in the segment of the archaeological record under investigation.

The regularity derived by Gally from his ethnoarchaeological research is that:

*if potter's tools are found in a compound, the larger part of the associated pots is representative of a homogeneous ethnic tradition.* This requires establishment of the truth (instantiation) of a number of hypotheses regarding context, as follows:

1. There are distinct sympatric ethnic groups.
2. Production of ceramics is a specialist activity.
3. There are distinct ethnic traditions of pottery manufacture.
4. Ceramic production covers the whole range of needs that require the use of pots.
5. The economy is characterized by markets ("marchés périphériques").
6. The modes of distribution and purchase of pots result in mixtures of pots of different ethnic traditions.

Again according to Gally, application of the regularity proposed to the archaeological record requires that one is able to demonstrate the following propositions.

1. Pots in potters' compounds represent their own work.
2. Pots of other traditions occur in limited numbers.
3. Potter's compounds are identifiable.
4. Permanent settlements are identifiable.
5. Potters' tools vary by ethnic group.
6. Technological and morphological variation in pots can be linked with ethnically varying toolkits.

I treat this example at some length for a number of reasons. First, as Gally acknowledges, the hypotheses and propositions are open to challenge. Not only do certain hypotheses appear unnecessary (e.g., 2 and 5), and others missing (e.g., that potters of different ethnic groups do not coreside), but also it would be quite impracticable to instantiate a cluster of such hypotheses in any archaeological instance where there is not other confirmatory evidence (e.g., ethnohistoric sources or oral traditions). As for the hypotheses so for the propositions. Second, the regularity proposed is trivial, and, while this is perhaps not inherent in the method,

neither is it, I contend, a particularly productive function of ethnoarchaeology to provide analogical support for reconstruction of detailed socio-cultural arrangements on specific sites. Gally (1989:31) is regretfully prepared to limit the interpretive ambitions of his school, "we shall probably have to definitively renounce trying to attain certain ambitious [archaeological] objectives, particularly those concerning the organization of society or religious beliefs." This would seem under this methodology to be as true for the discovery of ethnoarchaeological regularities as it is for their application to the archaeological record. In which case let us abandon this alchemy (Gardin 1980:173) and seek another way.

Neither logicist nor scientist analysis (and the style of fieldwork with which the latter is generally associated)<sup>9</sup> is appropriate to the ethnographic (as opposed for example to the experimental psychological) study of mechanisms involving the ideational order. Whereas, in a computer program, constructs and their relations are precisely defined within a closed system, human activities are interdependent with the IO, which not only incorporates a degree of contradiction but also symbols that are protean and polysemic. To logicist-positivist deconstruction must be added hermeneutics.

There are further conclusions to be drawn from this attempt to inject logicist rigor into our practice of inference. While it may never be possible to instantiate the hypotheses and demonstrate the propositions required to establish the working of nontrivial mechanisms in the past, logicists at least make their reasoning explicit and, by this courtesy, open up the possibilities for constructive debate. Last, rigorous methodology cannot compensate for inadequate research design.

### TOWARD HERMENEUTICS

We should not be limiting ourselves to the study of mechanisms involving the social and technical but should also attempt to comprehend dimensions of variability in IO-PO complexes, both in particular instances and in general. To do this the ethnoarchaeological strategy must be to employ a hermeneutic style in the development of theories regarding the existence of IO constructs and principles and their working in the PO. The theories must then be validated by assessing their internal plausibility and credibility in the context of (a) the analytic model and other theories and (b) empirical constraints imposed by data that are "sufficiently independent of superimposed interpretation to challenge, force revision and even rejection of the theoretical constructs intended to explain them" (Wylie 1982:42,44). A useful criterion is the extent to which proposed constructs and principles can interrelate and account for variation across classes of material culture. While logicism is not directed to the context of discov-

ery—logicist programs of fieldwork are likely to focus on questions of technology (e.g., Roux 1990)—our texts will be available for logicist analysis, preferably (but rarely) by ourselves and before publication. But logicist explication is not prerequisite either to practical understanding or to insightful interpretation.

Since 1984 members of the Mandara Archaeological Project team have been studying the culture history and ethnoarchaeology of the Mandara highlands and surrounding plains of North Cameroon, a region designedly chosen for its maximal topographical, social, and ethnic variety (Boutrais 1984). Within the 7000 km<sup>2</sup> in which, between 1986 and 1990, eight ethnoarchaeologists spent a total of 80 months accumulating a very large data base (see David and Sterner 1987, 1989), there are societies descended from both precolonial states and small-scale unhierarchized communities. At least 28 mother tongues are spoken, mainly of the Central branch of Chadic (Fig. 2). A primary aim of our research is the development of a theory of style of general, indeed broadly predictive, value in ethnology and archaeology. We have chosen this focus because the concept of style

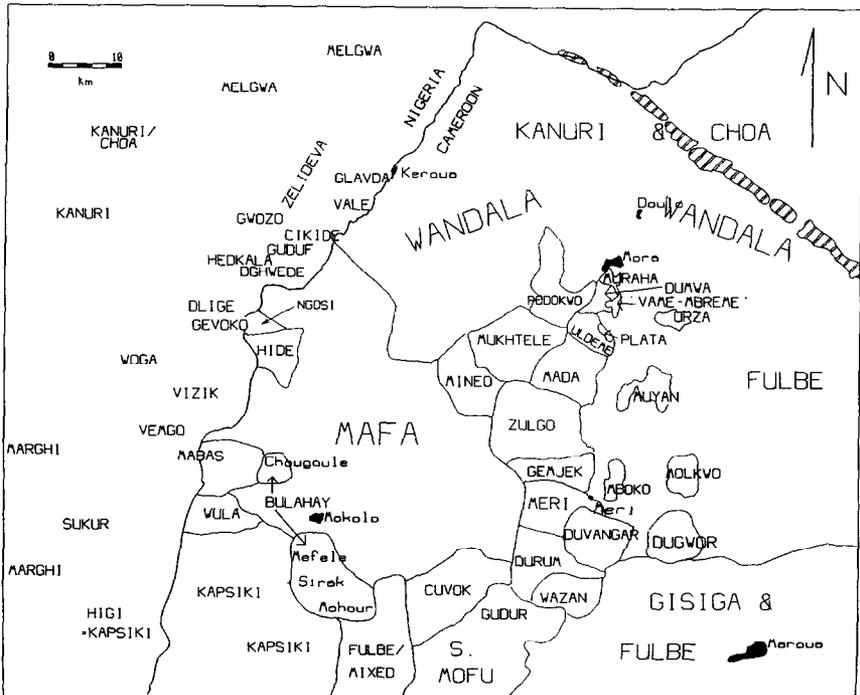


FIG. 2. Parts of northeastern Nigeria and north Cameroon, showing "ethnolinguistic" groups. The cross-hatched areas in the northeast represent the Bama ridge. (Map by A. S. MacEachern.)

is a key to all archaeological interpretation that is concerned with the behavior of humans as members of sociocultural groups. We are interested both in its instrumental aspect, its uses as a medium of communication, and in accounting for the specific forms style takes in Mandara cultures. We have emphasized style and stylistic variance within domains of material culture chosen in part for their intrinsic interest and in part for the likelihood that they will be represented in the archaeological record. These are architecture, ceramics, metallurgy, and mortuary practices. While some researchers have undertaken in depth studies of single communities (e.g., Gavua 1990), others were less narrowly focused. The combination of the two approaches provides a broad comparative base for analysis of the PO and IO (see David et al. 1991).

We take an isochrestic (Sackett 1990) approach to style, accepting that it may reside in aspects of form either that are adjunct to utilitarian function or that involve choice between viable functional alternatives. A style is a polythetic set of attributes present by virtue of common descent from an identifiable artifact-production system (David 1990:19). This implies the fundamental importance of studying modalities of production.

One basic finding concerns the use of style and is supported by Wiessner's (1983) study of San projectile points. In the absence of corporations responsible for the production of a material culture set, the vast majority of stylistic signaling is directed by group members to themselves, serving to reinforce group identity, structure, and values (Stern 1989; contra Wobst 1977). Where corporations that control the production of material culture exist, style is not only manipulated for a variety of ideological, commercial, and other intrasocietal purposes, but is liable also to be used for organized external messaging. This general transcultural regularity must be taken into consideration when making use of typologies for purposes of inference to sociocultural mechanisms. It is partly for this reason that the reconstruction of manufacturing sequences, "chaines opératoires," has become such a focus of research in France (e.g., Binder and Perlès 1990), and the concept of "technological style" (Lechtman 1977) in the Americas. In our area, Robertson (in press) has shown how the guild of Wandala smiths uses metal—and not only metal—templates to produce varieties of hoe forms designed for different ethnic markets. Such "packaging" is common; style can only be meaningfully interpreted in the context of the production system responsible for its expression. Archaeologists can scarcely secure inferences regarding exchange, trade, or any form of social differentiation without reference to modes of production.

Production systems mediate the material culture instrumental in the constitution of ethnicity, defined here following Eriksen (1991) as the aspect of behavior that communicates cultural similarity and difference. Much if not most archaeological interpretation has as a prerequisite the

definition of cultural groupings. While in our area it is possible to delimit ethnolinguistic groupings on a map and to assign individuals to them for administrative and other purposes, this distorts cultural reality. Individual ethnicity is in fact situational and experienced at several levels (David et al. 1991). Thus, instead of discrete, formally differentiated, and geographically bounded assemblages of material culture, we find zones with fuzzy boundaries, within and to a lesser extent between which, at the levels of the lineage, the multiclan settlement, the political alliance, the dialect, sometimes the language, there takes place a continuous, kaleidoscopic reassembly of forms. Each of these levels may be associated with an IO, but individuals, depending upon the context in which they find themselves, may relate to more than one, possibly several, the distinctions between which are by no means clear. Borrowing, reinterpretation, inversion, montage, and jumbling of attributes and motifs create a material world that is both the context that renders interaction comprehensible, and is itself constantly being invented through that interaction.<sup>10</sup> While ethnographers are very familiar with such situations (Sanjek 1991:622 and references there cited), the very nature of the archaeological record, comprising artificial and static segments of cultural reality, has led archaeologists first to reify aggregates of materials as archaeological cultures and then to puzzle over their relationships to ethnographic analogues. It is in fact rare for either to exist as a bounded entity.

Although the history of the Mandara has exerted particular influences on stylistic expression, the pattern described is by no means unique but characterizes other societal and cultural groupings. Responding to environmental, economic, social, and political change, culture sets experience a continual flux of persons and ideas, moving between cultural subsets that in order to function must both be differentiated and mutually intelligible. We can account for continuities in the material expression of such patterns in terms of the whole IO-PO complex. The environment of course imposes significant constraints on the form of many artifacts, but other formal aspects including decoration, the aspect apparently least restricted, often show very limited variation. In all societies a significant portion of stylistic expression refers to and embodies long-lived cosmological and religious themes existing at the IO level and expressed primarily through ideological activities. Here style operates as a potent agent in the recreation of society and the instilling of its values. Thus, we can account for continuity and standardization through space and time and, incidentally, justify—within a certain societal range—the commonplace use of decoration as the prime index of ethnicity preserved in the archaeological record (David et al. 1988). In recognition of the connection between style and society's "ultimate concern, its religious substance," we borrowed from McIntosh (1989) the concept of "symbolic reservoir" to

describe the long-lived Saussurian *langue* of “founding ideas” which are made manifest in behaviors that include artifactual *paroles* that vary clinally and stepwise in space and time while remaining recognizably the same (Sterner in press). McIntosh’s symbolic reservoir in fact comprises much of the content of our IO. Such symbolic reservoirs, maintained by the participation of individuals in a variety of ethnic contexts, are the true referents of many “cultures,” ethnographic and archaeological, and the population that draws on one symbolic reservoir is the significant unit for understanding cultural process at the macroscale. Hodder’s (1990) recent book, *The domestication of Europe*, is on precisely this subject, the evolution of a Neolithic European symbolic reservoir.

It may be instructive to consider how we arrived at this conclusion, one that relates IO to PO. First we carried out archaeological surveys and test excavation, which gave us some limited insight into regional cultural development during the Iron Age. We then carried out a combination of comparative and in-depth ethnoarchaeological studies. The scale of our work through time and across space helps free us from the constraint described by Wobst (1978) as the “tyranny of the ethnographic record.” We obtained evidence of synchronic and diachronic patterns in the distribution of material forms—and particularly ceramics which are greatly elaborated in the Mandara—and set them in context by reference to social and cultural anthropological and ethnohistoric sources. Some of these, for example Juillerat (1971), gave us early insights into mechanisms and process that we were able to follow up and confirm. It is the mutual support provided by all these approaches—and not rigorous hypothesis testing or logicist specification—that led us to our conclusions. The extent to which we are justified in generalizing from the Mandara to other regions and areas depends in large part upon the plausibility of our case in the larger context of anthropological theory, and on comparative studies. Validation of our conclusions must include study of other areas, of which more below.

The discussion to this point has been concerned with mechanism and process, with dimensions of variability without regard to culture’s content of meaning. To what extent can we, in Hodder’s (1986:6) phrase, “come to an understanding of each cultural context in its own right, as a unique set of cultural dispositions and practices.” Hodder is concerned here with an entirely legitimate subject of archaeological enquiry, one that must involve some application of regularities of IO–PO behavior to the particular case.<sup>11</sup> Having struggled over many months and with only partial success to gain some intellectual appreciation of the Mandara symbolic reservoir, I am pessimistic as to prehistorians’ hopes of achieving access to complex structures of prehistoric meaning, far less to any deep empathy with IOs. In our work we have approached the study of meaning

through contextual analysis, that is to say by tracking and interrelating forms and motifs as they appear and reappear in a variety of domains of life and classes of material culture. In the course of ethnographic enquiry, perhaps during discussion with my assistant, E. Isa Kawalde, as we walk back through the terraced and domesticated landscape after visiting with a smith, perhaps in the course of argument with Judy Sterner when she returns from "her" village, ideas are constantly being proposed, abandoned, developed, refined, and tested on their way to becoming models and theories (cf. Wylie 1982:45–46). This may work well when researching the dynamic present, with recursive access to informants and materials, but the cultural and natural transforms affecting material remains as they enter the archaeological record result, except in very special circumstances, in the collapse of the contextual frames that render ethnographic analysis possible and fruitful. Paradoxically, it may be that the more complex the society, the richer its iconography, the more capital and labor expended on its material culture, the better are prehistorians' chances of penetrating the ideology, at least of the elite, through hermeneutics. Inversely, it may be that at the other end of the societal scale, restriction of range of images and metaphor, and omnipresence of those relating to the human body and its processes, will facilitate the approach to meaning content.

Attempts by archaeologists to comprehend prehistoric meaning have, and I believe necessarily, emphasized its structure at the expense of content, and domains of material culture that maximally retain the integrity they possessed in the systemic context. Rock art is a classic example (e.g., Leroi-Gourhan 1982; Lewis-Williams 1981), as are architecture and the intentional time capsules that we call burials. Thus, Hodder (1984) found structural equivalences between Neolithic domestic and mortuary architecture and related these to transformations in the status of women and in the productive base. But we are far from experiencing their original richly textured meanings.

The domain of mortuary practice, relating to the IO primarily through ideological and social activities, has great potential for ethnoarchaeological contributions to the IO—although curiously it is one in which there is an extraordinary lack of useful sources. In studying mortuary practices in the Mandara (David in press), I found that, despite the realization that disposal of the dead involves a conceptual *transformation* of living society (Parker Pearson 1982), this is not in itself adequate as an approach. We must also take into consideration what Huntington and Metcalf (1979) have called the Hertzian triangle (see Hertz 1907). The triangle (Fig. 3) represents mortuary practices as comprising three pairs of relationships. That between the "Living and Mourners" and the "Corpse and its Disposal" determines the *scale* of the rites and the *differentiation of social*

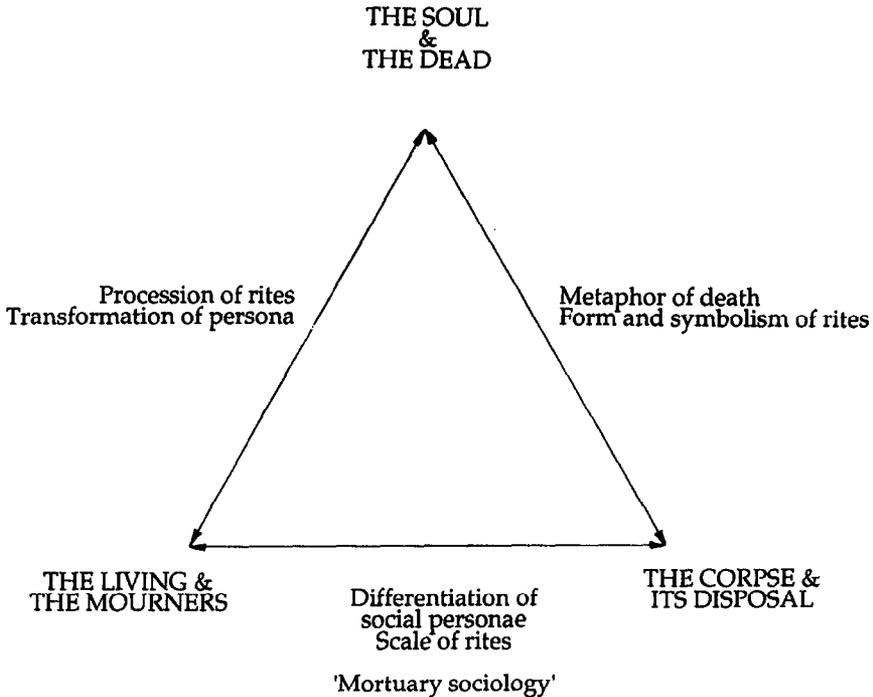


FIG. 3. The Hertzian triangle.

*personae*. It is this social aspect with which archaeologists were almost exclusively concerned through the 1970s. Until the 1980s little attention was paid to the relationship between the "Soul and the Dead" and the "Corpse and its Disposal." This is apprehended through *metaphors* (e.g., sleep and awakening to resurrection, reverse birth, or germination of an ancestor) that are expressed in the *symbolism* of the rites. The third pair, linking "Living and Mourners" to the "Soul and the Dead," accounts for the *sequence of rites* as the social persona is transformed or extinguished.<sup>12</sup>

The study of mortuary practices in the Mandara strongly reaffirmed the existence of the symbolic reservoir, emphasizing that material culture and practices associated with disposal of the dead are intimately linked with ideas, values, attitudes, and themes that repeat in many different aspects of the lives of these societies. Very striking is the polysemic nature of traits and the ambiguity and multivalence of metaphors; thus, the tomb partakes of the nature of a hut, a pot, a womb, and an inverted granary. Also apparent is the great variability in the data and that the seven groups studied choose to emphasize different aspects of this common ideological heritage in differing ways and in different dimensions of mortuary practice.

In this domain, ideology was shown to operate in all three of the ways identified by Giddens (1979:193–197), by:

1. representation of the interests of dominant sections of society (male elders) as universal,
2. denial or transmutation of contradictions (between castes), and
3. naturalization of the present (structuring of mortuary practices by “natural” social statuses: infant/fully human, man/woman, older/younger, etc.).

Given the importance of disposal of the dead as a dramatic institutional framework for the ongoing invention of society, it is *inevitable* that this will always be the case.<sup>13</sup> Note that this is a candidate cross-cultural law relating IO to PO that has been arrived at via a hermeneutic approach, and that the absence of scientist methodology is irrelevant. I have been present at only one burial and have measured none.

Again we may ask how such very general transcultural regularities are validated. Principles have been presented above. A first crude test is to use oneself as informant and to ask whether the regularity applies both in one’s own and in other, preferably very different, cultures with which one is familiar. However, we should go much further than this and design research that will analyze the differential output of mechanisms and processes in cultures that differ in historical and other contexts but that can be shown to be structurally similar. This is as close as we may aspire to controlled experimentation. We have taken initial steps to extend our research to the Upper East Region of Ghana where the societies, economies, and political histories of the Tallensi and their neighbors have much in common with those of Mandara peoples, but whose cultural heritage is markedly different, as is shown by their speaking languages of a different phylum and family (Congo-Kordofanian Gur as against Afro-Asiatic Chadic). We expect, for example, that similar mechanisms operating on different cultural materials will be expressed in output that is structurally analogous but different in content. Such propositions are not amenable to simple forms of testing; rather we must again appeal to consistency of inference and data, plausibility of analytical model and coherence of theories. There will be dangers of falling into the trap of pseudovalidation by confirmation (Stockowzcki 1991). But if, despite searching for counter examples and counter models, our theories are supported by the new evidence, this would suggest that certain at least of the mechanisms and, by extension, processes in question express IO and PO patterns characteristic of the psychology, society, and culture of *Homo sapiens sapiens*.

#### ETHNOARCHAEOLOGY AND ARCHAEOLOGY

Some regularities are specific to one or a limited range of societies,

others more widely or even generally applicable. Once recognized these may appear to be truisms, for example that "Buildings are liable to fulfill a series of functions over their useful life, and the precession of uses will in any one society show regularities characteristic of one or more devolutionary cycles" (modified from David 1971:117–119). Nevertheless they help direct and constrain archaeological observation and interpretation. Middle-range theories explicating the relationship between dynamics and statics are of immediate practical use in planning and interpreting research. In such cases the approach is generally scientist and the behavior investigated primarily technical and/or social. The scale can be large; Carol Kramer has a major monographic study, *Village Ethnoarchaeology* (1982), on cultural transformations, relating primarily to technical and social aspects of the PO, in rural Iran, and an analysis at the local and regional levels of scalar effects on the distribution of pottery in two cities in Rajasthan (Kramer 1991). Whereas the former must influence the research design of any excavator of a village site in the Near East, and indeed elsewhere, the second, pioneering in its scope, should be taken into account in constructing regional prehistories.

However, it is in the context of discovery that ethnoarchaeology, employing a less obviously rigorous hermeneutic approach, can contribute most to larger issues of archaeological methodology and to research design. More important than the generation by ethnoarchaeologists of regularities that can be applied in more or less formulaic fashion to archaeological databases, is the deepened understanding of the expressions in material culture of linkages between IO and PO.

Naturally ethnoarchaeology's relationship to archaeology, the other anthropological subdiscipline that focuses on things, is very close and will remain so. Its contribution to archaeology is invaluable at least in theory and on condition that rigor is used, which too rarely occurs, often for reasons of incomparability of analytical and observational units—to which we should pay more attention (Skibo et al. 1989). However, in the sense that several of its practitioners are now engaged in more general anthropological studies of material culture (e.g., Miller 1987), ethnoarchaeology is becoming less restricted to topics of specific and immediate archaeological concern. I am, for example, presently engaged in showing how the technology of Mafa iron smelting depends upon a parallel technology of plant medicines that draws on metaphors of human digestion and pregnancy. Like the two sides of a ladder, the siderological and magicobotanical constructs are linked together, reinforcing each other in a powerful mnemonic that relates IO and PO, ideological and technical activities. Similar systems underpin the intergenerational control and transmission of technology in many if not all nonliterate societies—another hypothesis to be explored.

Ethnoarchaeology's primary *service* mission is still the raising of the analogical consciousness of archaeologists, many of whom prefer their culture dead, sensitizing them to dimensions of variability and the richness of the relationship between humans and their artifacts, including their own bodies. They need, if only vicariously, to experience cultural realities other than their own in order to combat the ethnocentrism that colors arguments and distorts inferences. But only some ethnoarchaeologically generated insights offer archaeologists the analogies, tactics, and strategies needed for interpretation; others relate to broader anthropological concerns.

The realist philosophy of science is liberating in that it integrates history and science, and encourages archaeologists and ethnoarchaeologists to get on with what they are best at—doing archaeology and ethnoarchaeology. Realism does not, for example, require us to engage in mental gymnastics in order to reformulate our retroductive insights as hypotheticonomological deductions. A realist approach offers much more than what Binford (1987:391) called “a kind of intellectual comfort” in a misguided paper that would of itself justify the present attempt to reintegrate the field. His attack on archaeological hermeneutics is there founded on misrepresentation of ethnography—we learn, for example, that ethnographers, unlike archaeologists, do not report data but only “information”—but I read the conclusion to his paper as falling fairly and squarely within the realist program advocated above. Realist praxis amid systems that are more or less complex and restricted in fact characterizes most recent research, allowing research variety while providing firm although flexible guidelines to research process and the criteria to be used in evaluating inferences and theories. Knowledge can progress in a context of transitive facts, the theory and politics laden science of the day, incorporating the insights, say, of feminism and postmodernism while retaining critical control. Meanwhile the intransitive objects of our knowledge, the real things, may never be known but can be ever more closely approximated.

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## NOTES

<sup>1</sup> I follow Kosso in treating Lewis Binford and Ian Hodder as archetypes. Binford's philosophy of science and more obviously his rhetoric have developed over the years from logical empiricism/positivism toward the realism which his practice has always tended to exhibit (see Wylie 1989 and below). Hodder's views, approaches, and interests have changed even more rapidly, and I exclude from this paper any consideration of his and others' postmodernist concern with reflexivity and critical theory (on which see Hammerley 1992:96–122).

<sup>2</sup> Hermeneutics is a word that turns many people off. I use it here in the broad sense of the interpretation of layers of meaning of "texts," broadly defined to include such things as material culture sets, in the context of other such texts.

<sup>3</sup> The term "ideological" is only used in preference to the more correct "ideational" in order to avoid confusion with "Ideational Order" (see below).

<sup>4</sup> Given the existence of contradiction within the ideational order, symbolic interactionist analysis is in some cases a more appropriate term than hermeneutics. Hodder's (1991) study of Ilchamus decorated calabashes is a good case in point. Hatt (1992) provides both a historical example of symbolic interactionist analysis and a clear explanation of the difference between the kindred methods.

<sup>5</sup> In the article cited we argued an assimilation of pots to people. We can now demonstrate that the caste division of central Mandara societies is expressed in the manufacture of red and black ceramic wares (David 1990).

<sup>6</sup> The buckshot approach refers to the hit-or-miss use of a specific ethnographic analogy to suggest an answer to some equally specific archaeological question. A researcher taking a laboratory approach examines the relationship between known ethnographic behavior and observable archaeological remains. The spoiler approach, related to the "cautionary tale" of Heider (1967), uses such correlations to "evaluate statements, models, and assumptions of a generally deductive nature" applicable to a certain range of societies, and often leads to their being discarded or reformulated. General models "include general analogies and deductive hypotheses as well as . . . 'lawlike generalizations,'" which Yellen (1977:6) argues should be stated in the form of hypotheses susceptible to testing.

<sup>7</sup> This statement does not of course apply to other research, not involving experimentation, carried out by members of the Kalinga project, for example Miriam Stark's (1991) thoughtful study of ceramic production and community specialization.

<sup>8</sup> With regard to theory construction, I agree with Gallay that regularities (and thus mechanisms) cannot be validated by reference to the archaeological record, but only by the extension of ethnoarchaeological observations to other ethnographic contexts, and ultimately by reformulation of the regularity in the light of new evidence (i.e., the standard practice of model building and testing).

<sup>9</sup> There is no space here to explore the styles of ethnoarchaeological fieldwork necessarily associated with the scientist and hermeneutic approaches. The former is likely to involve censuses, questionnaires, mapping, and measurement of material culture and the asking of specific questions. The second, while not spurning the techniques of the first, requires longer and deeper exposure to the culture and language, and more argument over the meaning of concepts underlying behavior and informants' answers.

<sup>10</sup> Thus, in the central Mandara, there is a "transformer" caste responsible among other things for specialist iron working and ceramics (Stern and David 1991). Caste members are much less constrained in their marital, social, and economic relations than the mass of ordinary folk, and behave in some ways as a supraethnic-linguistic group, and they are disproportionately responsible for creation of the artifactual environment. A little further north different production systems give rise to different effects (MacEachern 1990).

<sup>11</sup> In fact a weakness of Hodder's (1982:125-184) fascinating "Dirt, women and men: A study in the Nuba Mountains, Sudan" is that his argument that beliefs in sex pollution are important in structuring the patterning of refuse and decoration is overly dependent upon a cross-cultural regularity proposed by Mary Douglas (1966:142) and not enough on observation and discussion with informants. The symbolic principle thus appears somewhat of an *ex post facto* imposition on the data, despite his exemplary attempt to show, by comparative analysis, contrasting expressions of the principle in two fields of human behavior in two ethnic groups. Hodder and his team were in the field for only a short time.

<sup>12</sup> Shanks and Tilley (1982) attempted to demonstrate the existence of ideological naturalization (the representation of social constructs as part of the natural order of things) and denial of contradiction (the concealment or misrepresentation of social relations) expressed in "regularities in the spatial arrangements of different classes of bone within [Neolithic] tombs." But their arguments must be faulted for ignoring the left-hand side of the triangle, the procession of rites during which decarnization and probably "ancestralization"—becoming an ancestor—took place. Instead they assumed that only the final jumbled disposition of the dismembered and partial skeletons was the *explanandum*.

<sup>13</sup> A major problem facing the archaeologist is then to determine which elements of mortuary practices are operating in which way. To this there can be no general answer, particularly as, since denial of contradiction seems likely in most instances to imply a *lack* of material differentiation, its identification will require especially full contextual knowledge of other aspects of the culture.

## REFERENCES CITED

- Binder, D., and C. Perlès  
1990 Stratégies de gestion des outillages lithiques au Néolithique. *Paléo* 2:257-283.
- Binford, L. R.  
1977 General introduction. In *For theory building in archaeology*, edited by L. R. Binford, pp. 1-10. Academic Press, New York.
- Binford, L. R.  
1982 Objectivity—explanation—archaeology 1981. In *Theory and explanation in archaeology*, edited by C. Renfrew, M. J. Rowlands, and B. A. Seagraves, pp. 125-138. Academic Press, New York.
- Binford, L. R.  
1987 Data, relativism and archaeological science. *Man* 22:391-404.
- Boutrais, J. (editor)  
1984 *Le Nord du Cameroun: des hommes, une région*. Editions de l'ORSTOM (Collection Mémoires, No. 102), Paris.
- David, N.  
1971 The Fulani compound and the archaeologist. *World Archaeology* 3:111-131.
- David, N.  
1990 *Vessels of the spirits: Pots and people in North Cameroon*. Video (50 min). Dept. of Communications Media, University of Calgary, Calgary.

- David, N.  
 In press The archaeology of ideology: Mortuary practices in the central Mandara highlands, North Cameroon. In *An African commitment: Papers in honour of Peter Lewis Shinnie*, edited by J. Sterner and N. David. University of Calgary Press, Calgary.
- David, N., K. Gavua, A. S. MacEachern, and J. Sterner  
 1991 Ethnicity and material culture in North Cameroon. *Canadian Journal of Archaeology* 15:171-177.
- David, N., and H. Hennig  
 1972 *The ethnology of pottery: A Fulani case seen in archaeological perspective*. Addison-Wesley Vol. 21, Modular Publications. Addison-Wesley, Reading, MA.
- David, N., and J. Sterner  
 1987 The Mandara Archaeological Project 1984-87. *Nyame Akuma* 29:2-8.
- David, N., and J. Sterner  
 1989 The Mandara Archaeological Project 1988-89. *Nyame Akuma* 32:5-9.
- David, N., J. Sterner, and K. Gavua  
 1988 Why pots are decorated. *Current Anthropology* 29:365-389.
- David, N., and D. Voas  
 1981 The societal causes of infertility and population decline among the settled Fulani of North Cameroon. *Man* 16:644-664.
- David, W.  
 1990 Style and history in art history. In *The uses of style in archaeology*, edited by M. Conkey and C. Hastorf, pp. 18-31. Cambridge University Press, Cambridge/London.
- Douglas, M.  
 1966 *Purity and danger*. Routledge and Kegan Paul, London.
- Eriksen, T. H.  
 1991 The cultural contexts of ethnic differences. *Man* 26:127-144.
- Gallay, A.  
 1989 Logicism: A French view of archaeological theory founded in computational perspective. *Antiquity* 63:27-39.
- Gallay, A.  
 1991 A propos de la céramique actuelle du delta intérieur du Niger (Mali). Approche ethnoarchéologique et règles transculturelles. Prétirages de communications, *Ethnoarchéologie: Justification, problèmes, limites*, pp. 45-53. 12e Rencontre Internationale d'Archéologie et d'Histoire d'Antibes, October 1991.
- Gardin, J.-C.  
 1980 *Archaeological constructs. An aspect of theoretical archaeology*. Cambridge University Press, Cambridge/London.
- Gavua, K.  
 1990 *Style in Mafa material culture*. Unpublished Ph.D. dissertation, University of Calgary.
- Gibbon, G.  
 1989 *Explanation in archaeology*. Blackwell, Oxford.
- Giddens, A.  
 1979 *Central problems in social theory*. Allen and Unwin, London.
- Goodenough, W. H.  
 1964 Introduction. In *Explorations in cultural anthropology*, edited by W. H. Goodenough, pp. 1-24. McGraw-Hill, New York.
- Gould, R. A.  
 1980 *Living archaeology*. Cambridge University Press, Cambridge/London.

- Hammersley, M.  
1992 *What's wrong with ethnography: Methodological explorations*. Routledge, London.
- Hatt, D.  
1992 *The symbolic interactionist perspective in historical analysis*. Paper presented at the Qualitative Analysis Conference, Ottawa, May 1992.
- Heider, K. G.  
1967 Archaeological assumptions and ethnographical facts: A cautionary tale from New Guinea. *Southwestern Journal of Archaeology* 23:52–64.
- Hertz, R.  
1907 Contribution à une étude sur la représentation collective de la mort. *Année sociologique* 10:48–137.
- Herskovits, M. J.  
1960 *Economic anthropology: A study in comparative economics*. Alfred A. Knopf, New York.
- Hodder, I.  
1979 Economic and social stress and material culture patterning. *American Antiquity* 44:446–454.
- Hodder, I.  
1982 *Symbols in action*. Cambridge University Press, Cambridge/London.
- Hodder, I.  
1984 Burials, houses, women and men in the European Neolithic. In *Ideology, power and prehistory*, edited by D. Miller and C. Tilley, pp. 51–68. Cambridge University Press, Cambridge/London.
- Hodder, I.  
1986 *Reading the past*. Cambridge University Press, Cambridge/London.
- Hodder, I.  
1990 *The domestication of Europe: Structure and contingency in neolithic societies*. Blackwell, Oxford.
- Hodder, I.  
1991 The decoration of containers: an ethnographic and historical study. In *Ceramic ethnoarchaeology*, edited by W. A. Longacre, pp. 71–94, University of Arizona Press, Tucson, AZ.
- Huntington, R., and P. Metcalf  
1979 *Celebrations of death: The anthropology of mortuary ritual*. Cambridge University Press, Cambridge/London.
- Juillerat, B.  
1971 *Les bases de l'organisation sociale chez les Mouktélé (nord-Cameroun)*. Mémoire de l'Institut d'Ethnologie 8. Institut d'Ethnologie–Musée de l'Homme, Paris.
- Kelley, J. H., and M. P. Hanen  
1988 *Archaeology and the methodology of science*. University of New Mexico Press, Albuquerque, NM.
- Keynes, J. M.  
1939 *The general theory of employment, interest and money*. Macmillan, London.
- Kleindienst, M. R., and P. J. Watson  
1956 'Action archaeology': The archaeological inventory of a living community. *Anthropology Tomorrow* 5:75–78.
- Kosso, P.  
1991 Method in archaeology: Middle range theory as hermeneutics. *American Antiquity* 56:621–627.

- Kramer, C.  
1982 *Village ethnoarchaeology: Rural Iran in archaeological perspective*. Academic Press, New York.
- Kramer, C.  
1985 Ceramic ethnoarchaeology. *Annual review of anthropology*, edited by B. Sigelman, pp. 77–102. Annual Reviews, Inc., Palo Alto, CA.
- Kramer, C.  
1991 Ceramics in two Indian cities. In *Ceramic ethnoarchaeology*, edited by W. A. Longacre, pp. 205–30. University of Arizona Press, Tucson, AZ.
- Lechtman, H.  
1977 Style in technology—Some early thoughts. In *Material culture: Styles, organization and dynamics of technology*, edited by H. Lechtman and R. S. Merrill, pp. 3–20. (1975 Proceedings of the American Ethnological Society.) West Publishing Co., St. Paul/New York.
- Leroi-Gourhan, A.  
1982 *The dawn of European art*. Cambridge University Press, Cambridge/London.
- Lewis-Williams, J. D.  
1981 *Believing and seeing: Symbolic meanings in southern San rock paintings*. Academic Press, London.
- Longacre, W. A. (editor)  
1991a *Ceramic ethnoarchaeology*. University of Arizona Press, Tucson, AZ.
- Longacre, W. A.  
1991b The perfect marriage: The essential joining of ethnoarchaeology and experimental archaeology. *Prétirages de communications. Ethnoarchéologie: justification, problèmes, limites*, pp. 77–79. 12e Rencontre Internationale d'Archéologie et d'Histoire d'Antibes, October 1991.
- MacEachern, A. S.  
1990 *Du Kunde: Processes of montagnard ethnogenesis in the northern Mandara mountains of Cameroon*. Unpublished Ph.D. dissertation, University of Calgary.
- McIntosh, R. J.  
1989 Middle Niger terracottas before the Symplegades gateway. *African Arts* 22:74–83.
- Miller, D.  
1987 *Material culture and mass consumption*. Blackwell, Oxford.
- Parker Pearson, M.  
1982 Mortuary practices, society and ideology: An ethnoarchaeological study. In *Symbolic and structural archaeology*, edited by I. Hodder, pp. 99–113. Cambridge University Press, Cambridge/London.
- Petrequin, A.-M., and P. Petrequin  
1984 *Habitat lacustre du Bénin: Une approche ethnoarchéologique*. Mémoire 39. Editions Recherche sur les Civilisations, Paris.
- Robertson, I. G.  
In press Hoes and metal templates in North Cameroon. In *An African commitment: Papers in honour of Peter Lewis Shinnie*, edited by J. Sterner and N. David. University of Calgary Press, Calgary.
- Roux, V. (in collaboration with D. Corbetta)  
1990 *Le tour du potier. Spécialisation artisanale et compétences techniques*. Monographie du CRA 41. Editions du CNRS, Paris.
- Sackett, J. R.  
1990 Style and ethnicity in archaeology: the case for isochrestism. In *The uses of style*

- in archaeology*, edited by M. Conkey, and C. Hastorf, pp. 32–43. Cambridge University Press, Cambridge/London.
- Sanjek, R.  
1991 The ethnographic present. *Man* 26:609–628.
- Schiffer, M.  
1976 *Behavioral archaeology*. Academic Press, New York.
- Schiffer, M.  
1978 Methodological issues in ethnoarchaeology. In *Explorations in ethnoarchaeology*, edited by R. Gould, pp. 229–247. University of New Mexico Press, Albuquerque, NM.
- Shanks, M., and C. Tilley  
1982 Ideology, symbolic power and ritual communication: a reinterpretation of Neolithic mortuary practices. In *Symbolic and structural archaeology*, edited by I. Hodder, pp. 129–154. Cambridge University Press, Cambridge/London.
- Skibo, J. M., M. B. Schiffer, and N. Kowalski  
1989 Ceramic style analysis in archaeology and ethnoarchaeology: Bridging the analytical gap. *Journal of Anthropological Archaeology* 8:388–409.
- Stark, M.  
1991 Ceramic production and community specialization: A Kalinga ethnoarchaeological study. *World Archaeology* 23:64–78.
- Sterner, J.  
1989 Who is signalling whom? Ceramic style, ethnicity and taphonomy among the Sirak Bulahay. *Antiquity* 63:451–459.
- Sterner, J.  
In press Sacred pots and 'symbolic reservoirs' in the Mandara Highlands of North Cameroon. In *An African commitment: Papers in honour of Peter Lewis Shinnie*, edited by J. Sterner and N. David. University of Calgary Press, Calgary.
- Sterner, J., and N. David  
1991 Gender and caste in the Mandara highlands: Northeastern Nigeria and northern Cameroon. *Ethnology* 30:355–369.
- Stoczkowski, W.  
1991 Préhistoire, ethnologie et approche prédictive: tentation d'une épistémologie naïve. Prétirages de communications, *Ethnoarchéologie: Justification, problèmes, limites*, pp. 117–119. 12e Rencontre Internationale d'Archéologie et d'Histoire d'Antibes, October 1991.
- Trigger, B.  
1989 *A history of archaeological thought*. Cambridge University Press, Cambridge/London.
- Tringham, R.  
1978 Experimentation, ethnoarchaeology, and the leapfrogs in archaeological methodology. In *Explorations in ethnoarchaeology*, edited by R. Gould, pp. 169–199. University of New Mexico Press, Albuquerque, NM.
- Wiessner, P.  
1983 Style and social information in Kalahari San projectile points. *American Antiquity* 48:253–276.
- Wobst, H. M.  
1977 Stylistic behavior and information exchange. In *For the director: Research essays in honour of J. B. Griffin*, edited by C. Cleland, *Museum of Anthropology Memoir, University of Michigan*, 13, pp. 317–342.

- Wobst, H. M.  
1978 The archaeo-ethnology of hunter-gatherers or the tyranny of the ethnographic record in archaeology. *American Antiquity* 43:303-09.
- Wylie, A.  
1982 Epistemological issues raised by a structuralist archaeology. In *Symbolic and structural archaeology*, edited by I. Hodder, pp. 39-46. Cambridge University Press, Cambridge/London.
- Wylie, A.  
1989 The interpretive dilemma. In *Critical traditions in contemporary archaeology*, edited by V. Pinsky, and A. Wylie, pp. 18-27. Cambridge University Press, Cambridge/London.
- Yellen, J.  
1977 *Archaeological approaches to the present*. Academic Press, New York.