14. Ethnoarchaeology: Obnoxious Spectator, Trivial Pursuit, or the Keys to a Time Machine?

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Abstract: Prospecting a future for ethnoarchaeology will require attention to at least three issues. Ethnoarchaeology tends to employ a style of discourse that limits itself to cautionary tales. Most ethnoarchaeology fails to employ the techniques of typology and analyses common to field archaeology, especially in increasingly standardized CRM contexts, and that lack of operationalization also limits its contribution. Ethnoarchaeology can correct some of the problems with minor adjustments, but the pioneering research will come from an explicit attempt to go beyond consideration of middle-range issues and incorporate general theory into ethnoarchaeological research strategies. In this way, ethnoarchaeology can help explain behavioral systems in addition to furthering a descriptive decoding of the past.

Introduction

Predicting the future is treacherous, but since the future of scientific disciplines can be created, it may be more useful to anticipate, or prospect. The “history” of ethnoarchaeology has left some marks, and I identify some of them, not to review the literature or even to evaluate our progress so far, but as points of departure for anticipating a future. I speak to three points selected from a broad array of ethnoarchaeology:

1. Ethnoarchaeology is well known for its ability to evaluate assumptions used by archaeologists. While serving as useful correctives, the evaluations have most frequently been presented in a style of argumentation that has come to be known as “cautionary tales.” With the passage of time, that practice has put ethnoarchaeology in the unflattering role of obnoxious spectator. In the future, practitioners may transcend that limited role.

2. Numerous ethnographic analogies have been produced under the rubric of ethnoarchaeology. This combines with a high frequency of poor linkage to practical archaeology and leads to a trivial pursuit. I explore this phenomenon as evident in the cultural resource management (CRM) literature and suggest a need for better operationalization of the ethnographic with the archaeological in the course of ethnoarchaeological research. In this way, ethnoarchaeology can realize its potential to redirect archaeological research.

3. I conclude with a perhaps naïve vision of a time machine, but, after all, the future of science demands a certain optimism. Besides, pessimism, like a cautionary tale, is easy. I briefly discuss the prospects for linkages between the mundane of the middle range that preoccupies ethnoarchaeology with the excitement of the exploration of ultimate causation found only in general theory. General theory is not offered as a panacea. However, attention to the higher levels could help mitigate the negative effects of the obnoxious spectator. It could do the same for the trivial pursuit of an ethnoarchaeology spawning schools of ethnographic analogies with unstated relevance for practical archaeology.

A Boundless Ethnoarchaeology

Ethnoarchaeology was one of those terms engendered during the sixties, a time of reflection, upheaval, and idealism in archaeology. In the wake of debate over whether anything new was actually happening, whether it was simply ethnography or whether it was an invention of burgeoning numbers of archaeologists hungry to carve an intellectual niche, there is now clear recognition of the term *ethnoarchaeology* in the professional lexicon. For most, ethnoarchaeology is here to stay as a category to help explore something very old in archaeology—the means of inference (Grayson 1986). On this level there is nothing new. On the other hand, the concerns and some of the failures of the new archaeology of the 1960s attuned us to the importance of an explicit concern with middle-range issues to help “decode” a difficult and misleading archaeological record (Binford 1983b).

Ethnoarchaeology has typically been seen as one facet of middle-range research or theory. Whether ethnoarchaeologists tend toward the optimistic positivism of Schiffer (e.g., 1976), the methodological processualism (in practice) of Binford (e.g., 1983a), the more explicitly hierarchical view of Raab and Goodyear (1984), or all of the above, understanding the middle range is important.

A less bounded ethnoarchaeology is advocated here so I employ a definition that displays, but is not limited to, a concern with the middle range. Ethnoarchaeology is research attempting to bond the patterned relationships between the static material remains that constitute the archaeological record and some of the corresponding but dynamic behaviors responsible for the creation of that record. This definition is pliable enough to allow for ethnoarchaeology to grade into other named realms such as historic archaeology (Schiffer 1978) and experimental archaeology (Ingersoll et al. 1984).
It is also open enough to accommodate study of the systemic causes of behavioral form that subsequently shape the material record. As we look to the future, it may be useful to question the limitation of ethnoarchaeology to the middle range. Ethnoarchaeology should be just as useful in general theory building. The potential becomes clearer and more urgent as we examine some of the characteristics of ethnoarchaeology.

**Ethnoarchaeology as an Obnoxious Spectator**

Prior to the crystallization of an ethnoarchaeology, the new archaeology pursued an ambitious “decoding” of the archaeological record. But many of the early (and decidedly ethnographic) studies of middle-range issues prompted easy critiques. In general, ethnoarchaeology opened the eyes of a discipline whose ambition to be prehistoric ethnography often outstripped the potential of available data (e.g., Bonnichsen 1973; David 1971; Donnan and Clewlow 1974; Gould 1971; Longacre and Ayres 1968; O’Connell 1974). At the same time, critiques of analogy as a general philosophical issue flowed anew, and cautions were plentiful.

In the mid-1970s Diane Gifford made an observation about ethnoarchaeology that could apply today. Soon after participating in the seminar that led to the volume *Explorations in Ethnoarchaeology* (Gould 1978), she spoke of the dangers of ethnoarchaeology’s negative commentary, an ethnoarchaeology that can only correct, or define, the limits of archaeology but that is less than explicit in directing *practical* archaeology.

Indeed, correctives are still needed, and the remedy need not be so extreme as Bambi’s admonition to Thumper, “If you can’t say something nice, don’t say anything at all.” But even in cases where ethnoarchaeological research actually includes an archaeological component potentially applicable to the realities of surveys and excavations, it is common to see only vague generalizations based more on ethnography than archaeology. I submit that the lack of clear linkage often stems from the cautionary style of the “suggestions,” testifying to the lack of operationalization typical in ethnoarchaeological comment. I do not offer these observations for historical purposes—the trend continues.

**Ethnoarchaeology’s Trivial Pursuit and the Redirection of Archaeological Research**

Ethnoarchaeology and middle-range research highlight the role of analogy in scientific reasoning. The important consideration is how analogy is employed, and a constructive reading of the cautionary literature shows that progress has been made. For instance, there is less ambition toward reconstructing past social systems as conceptualized by ethnographers and more emphasis on identifying behavioral realities such as group size, mobility, and gender (see, for instance, Whitelaw 1989).
This trend suggests that ethnoarchaeology is aware that analogies will always be produced and employed but cannot be employed well in the absence of a consideration of the archaeological. However, I see a huge gulf between the analogies generated by ethnoarchaeologists and their employment by practical field archaeologists. As long as ethnoarchaeology remains happy with the generation of vast numbers of possible analogies, leaving the archaeologists to figure out what to do with them, ethnoarchaeology will risk the label of a trivial pursuit.

One place to really see the effects of ethnoarchaeological effort is in the everyday technical reports produced in archaeology, especially the CRM literature. This literature is the modern engine of archaeological knowledge on an international scale (see Simmons, this volume), and it increasingly reflects interaction with ethnoarchaeology.

When the results of ethnoarchaeology are presented as cautious or vague implications to the practical archaeologist operating under ever-expanding constraints and standards, the problems associated with the abuse of analogy are exacerbated. The technical literature in CRM indicates this. One need not cite the extreme abuses of analogy in often regionally potent CRM reports because they are probably the exception. However, consider the potential for abuses of analogy by noting how many prehistoric cases in North America are interpreted with reference to the Nunamiat (especially Binford 1978), the !Kung (especially Yellen 1977), a few comparative treatises (especially Binford 1980), and an overlay of whatever direct historical analogs are available.1

It is easy to lay the blame on field archaeologists’ continuing to practice the traditional ex post facto and intuitive means of interpretation, with an overlay of ethnoarchaeologically generated analogs. However, it is arguable that ethnoarchaeology has with few exceptions gone the easy route—failing to make full linkage with archaeology in shaping how analogs are applied and how ethnoarchaeological research affects day-to-day field practices, typologies, analyses, and interpretation. The amount of ethnoarchaeology that involves no archaeology at all testifies to this. By archaeology, I mean the application of procedures, categorizations, and especially analyses that are becoming increasingly standardized in cultural resource bureaucracies. How often do we see the use of flotation for botanical or microrefuse analysis, plant phytolith analysis, lithic use wear analysis, or geomorphology in ethnoarchaeological settings? This degree of linkage represents the minority of ethnoarchaeology.

As an example of the potential for ethnoarchaeology to redirect archaeology, even CRM policy, I refer to my own experiences in archaeological and ethnoarchaeological studies of perishable housing in the Great Basin (Simms 1989; Simms and Heath 1990) and portable housing in Arabia (Russell and Simms 1991; Simms 1988). A study of protohistoric and historic wickiup compares the location of structures to various types and patterns of debris (Simms 1989). The goal is to improve the means of inference for cases deeper in prehistory where the temporary houses have vanished from easy detection at the surface and may be difficult to directly recognize through subsurface investigation.
Without getting too involved here, one of the messages of that study for policy is that current criteria for defining and recording sites in the Great Basin, and especially criteria for test excavation, exacerbate the problem. We are left with potentially misleading interpretations about the function of sites based on negative evidence (i.e., there are no residential structures here so this must be a ______) spawned by years of routine practice that out of sheer tradition have evolved into agency policy. Furthermore, the apparent paucity of debris in and immediately around many such structures has led to the attitude that little can be learned.

If my study of the debris and structures in the preserved cases, a de facto form of ethnoarchaeology, had only played the role of obnoxious spectator and pointed out how misleading lithic scatters can be, the negative attitude toward the sites would only be strengthened. The work would have exemplified a trivial pursuit. Therefore, it became imperative for the "ethnoarchaeology" to spell out what could be done in such cases. This was attempted in an application of ethnoarchaeology to improve the interpretation of a fifteenth-century camp, a purely archaeological case (Simms and Heath 1990). Our ethnoarchaeological studies of portable bedouin housing in Jordan (Russell and Simms 1991; Simms 1988) brings this thinking to another region and may improve our ability to recognize early phases of pastoral nomadism.

An ethnoarchaeology that practices archaeology is in a position to redirect the lines of archaeological investigation toward the issues and questions that can be addressed in the mundane world of real archaeology. Despite the value of ethnoarchaeology for correcting interpretation, improving the means of inference, and shaping archaeological research or perhaps even policy, none of the studies cited in the above example evaluates or influences general theory. This is perhaps the most unrealized potential of ethnoarchaeology.

**Ethnoarchaeology and Theory: Another Level**

In 1982 Barbara Price wrote,

What is surprising, however, is that the "new archaeology" begins its downward deductions at so resolutely middle a level, precluding significant generalization and producing a corpus of work remarkable (at least in retrospect and given its initially revolutionary program) for its intellectual conservatism. Interest in the higher levels has, if anything, dwindled. Despite the early focus on the discovery of laws there remains a sense of trivialization. . . . The results “float,” unanchored at the top (1982:714).

Operating in the wake of the new archaeology, ethnoarchaeology takes much of its character from the habits of the former, including what may be termed a preoccupation with the middle range. To be sure, concern with middle-range research is of great importance, but explanation of archaeological data may still not be at hand simply by the piecemeal addition of bridging arguments to everyday archaeological data. There remains the role of generalizing from the empirical.

David L. Wilcox, for example, has not hesitated to compile data and apply that knowledge to real problems.

That is the range of projects that fieldwork in the modern day can and does provide. The same is true of the middle range, and a conclusion is that the method of empirical research is sound.

The search for a middle ground cannot be avoided by the archaeologist. The search is not a means of avoiding a middle ground but a means of finding one. For the moment, we must be satisfied with analogs and the formulations that imply them. In the future we may be able to articulate these analogs and achieve a real understanding of what lies behind them. But the search cannot be avoided.
of general theory, not as something rarified and "unanchored" to the empirical world, but as the very glass we use to perceive both the middle range and our explanation of the past.

David Thomas's (1983a, 1983b, 1988) study in Monitor Valley provides an example of the need for this link. A study of changing subsistence and settlement over several millennia, this work has been deservedly complimented as an example (as one reviewer put it, the best example) of the application of ethnoarchaeology and the middle range to a substantive problem in prehistory.

Thomas attempted to operationalize ethnoarchaeology and other middle-range research to distinguish up to five contrasting adaptive strategies. The project was a model of modern research design and linkage between fieldwork and middle-range research but remained chained to Great Basin ethnographic analogy, especially the work of Julian Steward. Thomas tapped into everything ethnoarchaeology had to offer to try and operationalize middle-range knowledge (much of it ethnoarchaeological) in a specific case, but there was nothing in the middle-range work that helped him to break the confines of the historical analogies he employed and nothing to indicate how and why these systems evolved. In the end, the conclusions were limited to empirical generalizations.

The fault does not lie in Thomas's choosing to use the historical analogs, nor is something wrong with empirical generalization. The problem is that middle-range research based on cross-cultural comparisons (analogies) by itself cannot lead to an explanation of the system. Such knowledge would enable predictions beyond those analogs provided by history or modern experience. The study suffers from a lack of general theory.

For the purposes of argumentation I offer the challenge that archaeology must be able to identify past behavioral systems with no modern systemic analogy. Portions of such past systems (individual sites, site types, site formation processes, etc.) may be found in modern analogs—hence, the importance of middle-range research—but the systemic consequence of those analogies could conceivably be systems previously unknown, unimagined, and even "counter-intuitive" (intuition being a culture-bound concept). If we really want to step into our time machine, into prehistory, this is appropriate, at least as a guiding goal. As with all lofty goals, complete realization of the above challenge may be impossible. But let's not throw the ethnoarchaeological baby out with the bath of impracticality.

In order to approach this challenge, explorations of the middle range will have to co-occur with the application of models about the expected shape of the world generated from general theories of behavior. Some of the postprocessualists may be talking about this (Hodder 1982; but more explicitly, Kobylinski 1989), but I suspect that the paradigm(s) of this group will prevent any work along these lines from going beyond more cautions. Gould (1985) realizes the need to go beyond analogy but seems to see the different approaches as just different kinds of ethnoarchaeology, a matter of "themes." Binford (1977, 1985) is closer to the mark in seeing the different lines of questioning in ethnoarchaeology as stemming from fundamental
differences in paradigms. He has long argued for more explicit attention to higher level constructs, but in practice he remains decidedly middle range. Nevertheless, much recent thinking sets the stage for a move to an ethnoarchaeological-archaeological research program that integrates what is currently isolated as middle- and high-level theory under the tacit assumption that theory is the simple sum of empirical generalizations decoded from the archaeological record.

Maybe Price was right when she accused the new archaeology of "intellectual conservatism" and "a sense of trivialization." The label may in fact be appropriate for the humanist and social science enterprise in general (see Dunnell, this volume). We often seem preoccupied with validating the complexity we can all readily acknowledge rather than finding ways through it. Historians of archaeology continue to juxtapose complexity with determinism (Trigger 1989:367), wrongly assuming that we can only have one of the two and ensuring that explanation will never amount to more than some agreed-upon increase in descriptive "understanding." We can hope to transcend the knee-jerk denunciations of the determinism and the reductionism common to disciplines such as evolutionary biology (see Dunnell, this volume, for some reasons why this may not happen).

Behavioral ecology, for one, seems more comfortable in braving the dangerous waters of reductionism, realizing that models must begin simple and reduced in order to evaluate the role(s) of different variables in shaping the consequent complex system (humans do not corner the market on learned behavioral and social complexity). By making simple and expectedly inadequate predictions, the amount of variation that can be explained by a simple model can be compared to that which cannot be explained. Additional variables can then be brought into play, building a more comprehensive model. In that way, alternative models predicting what the organism should be doing as circumstances vary can be offered and then compared with ethnoarchaeologically documented behaviors, other middle-range studies, and the bridges they build to the actual material record.

The additional ingredient in a theory that can model the historically unknown is some knowledge of and assumptions about the fundamental processes of the system. Middle-range research, with the analogs we have at hand in the modern world provides this, but the critique of Raab and Goodyear (1984) still stands—the practice of middle-range research is decidedly methodological. Archaeologists have remained reluctant to employ general evolutionary theory, especially that occurring under the rubric of behavioral ecology (note the absence of references in this vein even in Dunnell’s advocacy for evolutionary thinking, this volume).

In essence, what I have advocated here is the use of general theory to generate hypotheses that are tested ethnoarchaeologically prior to archaeological application. The hypotheses for testing would be the same as those we would want to test archaeologically but would allow us to evaluate the proposition more expeditiously than if forced to rely only upon the difficult and fragmentary archaeological record.

All theories employ assumptions, and in the case of behavioral ecology, the
explicit attention to
the middle range. It is for a move to an
enterprise in general sys-
tematic biology. The day
of the tacit assumption is past. The day of
the ad hoc, of patchwork explanations is past. The day of
an "archaeology of facts" is over. The "archaeology of facts"
now must begin simple
in the market on learned
heuristics. In the same
way, all problems, all research
must be explained by a
model, a theory. Additional
variables need to be
considered, the organism should
be viewed as a whole, compared with
the whole of the middle-range studies,
research.

Model the historically
participating systems of the fundamental
behavioral science. The basic
allegories we have at
our disposal are the
torque of Raab and
the Taylor. Middle-range research is
reluctant to employ
the same. Middle-range research is reluctant to employ
the same under the rubric of
the very same
past (see this vein even in
this document).

The problem of general theory to
systematic biology. General theory
would be the same as
systematic biology. General theory
would allow us to evaluate
what to do, not just what to do today but what to do tomorrow,
what to do forever, not just what to do in the near
future. General theory, the

theory of natural selection is the driving force to develop models based on

"selection thinking" (e.g., Blurton-Jones 1976; Foley 1987; Krebs and Davies
1984; Standen and Foley 1989; Stephens and Krebs 1986).2

Recognizing the work is nascent, I offer some examples of studies stimulated by general evolutionary theory as it might be applied to

ethnoarchaeology and archaeology. I offer behavioral ecology as one example of general theory capable of directing ethnoarchaeological inquiry;

anthropology still holds the potential to generate its own.

Ethnoarchaeological Explorations and General Theory

Kelly and Todd (1988) developed a model of North American

Paleindian hunting and mobility behavior different from any single modern

systemic analog. To model the character of adaptive strategies during the

initial wave of New World colonization, they meshed various analogs from

around the world with ecological studies (from highly general theory) of

foraging, risk, and the like in a general but untested context of selection

thinking to envision an ecological niche and an adaptive strategy that no

longer exists anywhere.

Even their implicit appeal to general principles informed their model

beyond what could be expected of a strict use of ethnographic analogy and an

understanding of middle-range processes. Furthermore, they were able to

present a model that could potentially explain why Paleindian adaptive

strategies should be as they described, in addition to merely describing the

prehistoric behavior.

An appropriate topic for the application of ethnoarchaeology to situations

likely divergent from modern analogs is early hominid behavior. Gould

(1980) has advocated the use of analogs from both human and carnivore

behavior to help model early hominid systems, and this has been pursued.

More analogies and empirical generalizations are always helpful, but higher-

level theory will not to be reached by simply adding more of them while

acknowledging that cultural systems are evolutionary. "Any evolutionary

scheme presented must be consistent with our current understanding of how

natural selection operates" (Hill 1982). Yet the employment of the theory of

natural selection, as currently understood, has been rare in archaeology—ethno-

or otherwise.

Recent ethnographic work among the Hadza in Tanzania (O'Connell et al.

1988, 1990) is important here. It explicitly employs "selection thinking" that
document the workings of the contemporary system (hunting, butchering,

and bone transport among others), identifying the contexts in which certain

behaviors obtain. The studies show how general features of systems can be

examined from an empirical standpoint via the integration of middle-range

and general theory. Take the case of Hadza scavenging, a kind of behavior

suspected of early hominids based on archaeological finds. Ethnographic

observations of Hadza scavenging from carnivore kills and knowledge of the

role of scavenging in the larger system, coupled with assumptions stemming

from "selection thinking," led to several propositions about the characteristics
of early hominid scavenging (O’Connell et al. 1988). The arena of early hominid behavior is sure to generate additional ethnoarchaeology and perhaps applications of general theory in the coming years.

The deep past is not the only place where behaviors different from the present may be expected. Ethnographic and archaeological observations can be guided by the theory of natural selection to offer general propositions. This approach has been widely employed and successful in nonhuman evolutionary ecology. It has been employed in the analysis of existing ethnographic, archaeological, and ethnoarchaeological data in studies of hunter-gatherer foraging (O’Connell and Hawkes 1984), mobility (Kelly 1983), resource transport (Jones and Madsen 1989a), transport and diet choice (Jones and Madsen 1989b), and bone transport (Metcalfe 1989), to name a few.

These and other studies suggest the goal is not only to evaluate and interpret the archaeological record, the typical end point for archaeology, but also to evaluate explanations of human behavior over time spans that transcend the lives of individuals. Of course, the benefit is the potential for the work to contribute to the general theory that informed the research. Could there be such a future for ethnoarchaeology?

Program for Ethnoarchaeology

Looking to advocate more than anticipate, I present the following prospectus for the future of ethnoarchaeology. Ethnoarchaeology will go beyond an expose of archaeological naivete; empty “conclusions” about the multivariate nature of cultural causality; or the truism, “we need more research,” along yet more and more lines of, yes, you guessed it, middle-range investigation. Ethnoarchaeological conclusions will not stop at the cautionary but will elicit the expected character of the material record under various archaeological conditions, enabling application and test. Pursuant to this prospectus, one way for ethnoarchaeology to stretch itself is to explicitly redirect archaeological research—a goal that would force the operationalization I advocate.

Some work will continue along the lines of the recent past, including studies of the relationships between material culture and the symbolic realm. While it is conceivable that such work can be linked to larger theory and the past, I suspect it will remain divergent. The direct historical analogies that have been archaeology’s bread and butter will continue to be used, carefully, as they should. There will be increasing realization that all analogy extracted from a dynamic world carries problems and that the key is not to be found in the location of the Perfect Analogy. Some of this realization will stem from the beating the Direct Historical Approach in the New World has taken from evidence for disease-induced upheaval long before Europeans could make the observations we use for many of our analogs.

Middle-range ethnoarchaeological research will continue, but there will be a greater consciousness about including an archaeological component in research claiming to be ethnoarchaeology. The most pioneering ethnoarchae-
Ethnoarchaeology will be that which escapes the trivial by becoming explicitly but not exclusively concerned with general theory. Fewer ethnoarchaeologists will resist the temptation to isolate the middle range, a habit that tacitly assumes explanation will be an intrinsic outcome of merely “decoding” the record. Since general theory guides the development of the middle-range questions, the two are essentially coterminous. General theory does not wait for the middle-range research to be “done.” The reward of these efforts will be an improved position from which to enter the archaeological record and understand a past that may have been far different from our present. That would be the closest thing to a time machine.

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Notes

1. This is based on an informal survey of CRM bibliographies for the western United States, showing that these references are the all-time favorites. The use of favorites has to do with a variety of things ranging from the quality of a study to publishing inertia, exposure, and academic bloodlines favoring the easy location of a few studies by a hard-pressed group of “consumers” making their living in CRM.
2. These references reflect a range of descriptions of behavioral-evolutionary ecology, a range of applications from nonhumans to humans, and a responsible comparison between evolutionary biology and anthropology. Far from exhaustive, it stresses the need for anthropologists to comprehend behavioral ecology through interdisciplinary links rather than the interpretive writings on contemporary evolution found in some recent treatises by archaeologists.

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Quandaries and Quests

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