

The Archaeo-Ethnology of Hunter-Gatherers or the Tyranny of the Ethnographic Record
in Archaeology

Author(s): H. Martin Wobst

Source: *American Antiquity*, Vol. 43, No. 2, Contributions to Archaeological Method and
Theory (Apr., 1978), pp. 303-309

Published by: Cambridge University Press

Stable URL: <http://www.jstor.org/stable/279256>

Accessed: 07-11-2017 12:45 UTC

JSTOR is a not-for-profit service that helps scholars, researchers, and students discover, use, and build upon a wide range of content in a trusted digital archive. We use information technology and tools to increase productivity and facilitate new forms of scholarship. For more information about JSTOR, please contact support@jstor.org.

Your use of the JSTOR archive indicates your acceptance of the Terms & Conditions of Use, available at
<http://about.jstor.org/terms>



JSTOR

Cambridge University Press is collaborating with JSTOR to digitize, preserve and extend
access to *American Antiquity*

THE ARCHAEO-ETHNOLOGY OF HUNTER-GATHERERS OR THE TYRANNY OF THE ETHNOGRAPHIC RECORD IN ARCHAEOLOGY

H. MARTIN WOBST

Many of the constructs of space, time and behavior in the ethnographic literature on hunter-gatherers may be partly determined by the severe constraints on ethnographic fieldwork. This paper discusses the genesis of some of these constructs, points out that the anthropological theory consumed by archaeologists is often based on, or developed for these constructs, and suggests that some of these constructs may be insensitive to deal with behavioral variability expressed in the archaeological record, even though they can be made to fit any data. Their application to the archaeological record may merely be ethnography with a shovel in which the form and the structure of the ethnographic record are reproduced in the archaeological one.

THE ARCHAEOLOGICAL RECORD does not consist of behavior but, at best, of the precedents and products of behavior. Even ethnographers lack access to certain behavioral realms. For example, they cannot observe behavior in private (e.g., infanticide); behavior distorted by observers (e.g., hunting); and variability in behavior in large units of space and time. These obstacles are overcome only by means of hypotheses that have implications for those behaviors (or their precedents and products) that can be observed. Logically speaking, therefore, ethnographers and archaeologists face similar constraints in building a science of human behavior. Yet, anthropological theory has remained curiously ethnographic in character. By this I mean that it is primarily designed to fit and tends to be formally evaluated only against behavior recorded in the extant ethnographies and ethnoarchaeologies. If not contradicted by this universe of observation, it is used freely to order archaeological data, to guide archaeological expectations, and to explain pattern and variability in the archaeological record.

Obviously, the universe of ethnographic observation is not a clone of pattern and variability in human behavior. If the implications of a hypothesis are born out in the ethnographic record, it does not necessarily mean that the hypothesis will predict human behavior. It only means that it predicts human behavior that ethnographers can observe and have observed, in the way that ethnographers have summarized it. Whether or not this hypothesis also predicts behavioral variability which, while present, ethnographers cannot observe or have not observed, remains an open question. Unless our theories are subjected to strong inference against actual behavior and its precedents and products, as opposed to the behavioral images codified in the ethnographic literature, archaeologists should treat them as untested hypotheses. For, the data of the archaeologist are the precedents and products of actual behavior, rather than of recorded behavior. If archaeologists consume ethnographically derived theory without prior testing, there is a great danger that they merely reproduce the form and structure of ethnographically perceived reality in the archaeological record. This form and structure may spuriously confirm the ethnographically derived theoretical expectations, in a never ending vicious circle.

If we want to build a truly anthropological theory, capable of predicting behavior whether archaeological or ethnographic, we have to liberate our theories from the biases imposed upon them by the ethnographic record. In this paper, I want to alert my readers to some aspects of hunter-gatherer archaeology where “archaeo-ethnology”—that is, archaeological research with expectations, implications, and measurements derived from ethnography—has been particularly rampant.

THE PAROCHIAL MODEL OF HUNTER-GATHERERS

All hunter-gatherers in the ethnographic era were intimately tied into continent-wide cultural matrices, be it through the world market or through other direct and indirect contacts with more complex societies. Thus, the ethnographic record should be a veritable gold mine of information on regional and interregional process, among hunter-gatherers and between hunter-gatherers and other populations. Yet, the literature is remarkably silent on the behavior of hunter-gatherers as it ar-

ticulates within larger social and spatial entities. Similarly, our theories designed to fit this body of observation are vague on regional and interregional process in the universe of hunter-gatherers. The worst enemies of this kind of information appear to have been salvage ethnography and the "ethnographic present."

Salvage-ethnographers studied the rapidly acculturating or disappearing hunter-gatherers to generate a maximum of contrastive information, that is, to document all those behaviors that still differentiated hunter-gatherers from the encroaching agriculturalists. It is not surprising that salvage ethnographers, in their vast majority, focused on locally distinct behavior, rather than on those (intra- and interregional) processes that the studied populations shared with others in culture contact situations—processes, moreover, that continually eroded the behaviors that remained locally distinct. In other words, the rationale of salvage ethnography left little room to worry about regional and interregional process.

This kind of information also tends to become lost in the process of reconstructing the "ethnographic present"—the imaginary point in time when the studied populations were less affected by culture contact. In residualizing for the effects of cattle ranches, forts, trade and missions, one may easily filter out all behaviors that articulate hunter-gatherers within larger population matrices. What remains are populations attuned to the most parochial units of social, economic, and geographic space. Deprived of all regional and interregional variables, ethnographers are forced to attribute what remains to the most local and internalized stresses.

The search for parochial causation is particularly tempting with behaviors in which hunter-gatherers differ from agriculturally-based groups. After all, these differences "survived" up to the time of the ethnographer's visit although the populations had been under severe stress from more complex societies. Thus, it is difficult to see how these behaviors could relate to regional articulation. Given Occam's razor, they may instead be conveniently related to strictly local variables. That this is a very dangerous procedure has been demonstrated recently in a number of different contexts (see, for example, the discussions of "family hunting territories" [Speck and Eiseley 1939 *versus* Leacock 1954] and of Bushman behavior [Lee 1968; Lee and DeVore 1976 *versus* Schrire 1977; Williams 1974:101]).

Ethnographers contribute a steady stream of support for the parochial model of hunter-gatherers because of time and space constraints on their fieldwork. If human populations behave in certain ways to avoid, or minimize, exposure to major stresses, hazards, and catastrophes (compare with Vayda and McKay 1975), the shorter the observation period the less likely it is that ethnographers will observe the major driving variables behind the behaviors they observe. Major stresses are usually not observed because, under normal circumstances, normal behavior circumvents the most stressful situations. This makes it easy to attribute what is observed to what is within the ethnographic field of vision: small group dynamics, small units of space, and temporal and spatial variability of low amplitude.

In this way, the ethnographic literature perpetuates a worm's-eye view of reality, exemplified by such constructs as the *two-hour-walk territory* (Lee 1969) or the *catchment area* (Jarman 1972). Archaeologists happily consume this bit of theory. After all, it is always simpler (Occam's razor) to attribute local variability to local variability and, in site-centered archaeology, two-hour-walk territories are all that is in the archaeologist's field of vision. Thus, in the absence of strong inference (Platt 1964), we might expect archaeological research to reinforce the overwhelming ethnographic stereotype that hunter-gatherers articulate exclusively with local variability, and that regional and interregional process among hunter-gatherers is a symptom of degeneration and culture contact.

There is no doubt that hunter-gatherers, like other humans, articulate strongly with spatially circumscribed, local variability. But paradoxically, hunter-gatherers may be least constrained by localized catchment areas. This is partially due to their broad spectrum exploitation of food sources, partially to the weak development of facilities, storage, ownership, and claims to land. Where agriculturalists encounter a sizeable threshold to movement outside of their catchment area, hunter-gatherers, individually or as a group, can shift camp under the extremes of local variability, thus redefining their catchment area, or shift resources, thus redefining the food supply. At the same time, the plant or animal populations that hunter-gatherers exploit, with very few exceptions, are not spatially circumscribed within the catchment area like those of most agriculturalists. Instead, they are spatially continuous and subject to regional exploitation by predators and other populations of hunter-

gatherers. If neither the resources nor the personnel are confined to the small unit of space that some general models want to allot to hunter-gatherers, it is difficult to see why most archaeologists use its contents as their sole means to account for hunter-gatherer behavior. The recent demise of the "tribal" model (Fried 1975 and elsewhere) and "nation-state" model (Wallerstein 1974) suggests that the parochial model of hunter-gatherers may be the next candidate for revision.

A strong clash between the parochial model and ethnographic reality is apparent in the fact that Hopewell and other precolumbian exchange in the New World (Struever and Houart 1972:Fig. 5) and Egyptian (Hofmann 1975), Arabic (Thorbahn 1976), and Chinese (Okladnikov 1968) trade in the Old World articulated with hunter-gatherers in exactly the same areas where ethnographers of the last two centuries attributed all behavioral variability again to the most localized sources. Late Paleolithic hunter-gatherers already are shown to have participated in regional exchange, and this exchange is reflected in their local adaptive posture (see, for example, Gabori 1969; Kozlowski 1972/3; and Soffer 1977). At low population densities, even such a basic process as mating forces local populations to interact within a regional population matrix (Weiss 1976; Yellen and Harpending 1972) and has locational implications for the individual groups (Wobst 1976, 1977). Certainly, the momentum for theory at this level of integration is steadily increasing. Yet, this revision is difficult to achieve with ethnographic data (for an interesting attempt, see Lee 1972, the originator of the two-hour-walk territory). Instead, it is quite properly the job of the archaeologist, because archaeologists are the only anthropologists who can observe "simultaneous" (products of) behavior along a regional or inter-regional transect. Thus, regional culture process among hunter-gatherers may be one of the areas where archaeologists have to contribute to the theory of human behavior with little or no guidance from ethnographically recorded precedent.

THE GENESIS OF CULTURE AND SOCIETY

Unable to observe simultaneous spatial variability in behavior, ethnographers are forced to reconstruct it with the help of informants. But informants are not much better off than ethnographers. Their knowledge of human behavior is also acquired by observation and hearsay. Both modes structure their knowledge in ways that distort reality, even with the best of intentions. This is because the information field of individuals is bounded and patterned.

Informants most frequently partake in, and observe, behavior closest to their habitual location (the social and geographic space in which they spend most of their time; their camp, group, or minimal band). If the distance from this location is increased, eventually a point is reached beyond which informants do not know human behavior any more by observation. This distance will usually encompass at least as many people as informants have to interact with in a regular or predictable manner. As a result, informants tend to dichotomize the continuum of space into a bounded unit with predictable behavior on the inside and unpredictable behavior on the outside.

Interaction with personnel from distant locales in this information field usually involves only certain well defined contexts and tends to be confined to the most "social" and public situations; that is, it requires the most stereotyped behavior. This high degree of patterning is necessary to reduce potential stress and to bring potentially unpredictable social contexts into the realm of predictability. In other words, behavior that articulates informants with individuals from other locales is highly patterned in reality. Ironically, the degree of patterning in these interactions should increase the rarer they are, the more the other realms of behavior (those not involving interaction between different locales) are dissimilar and unpredictable, or the greater the potential stress anticipated. Thus, informants should provide ethnographers with more patterned behavior in their information field, the greater the behavioral variability along the given spatial transect.

A large proportion of all the different kinds of behavior which are transacted in a given region are observationally accessible to informants only at their habitual location (for example, behaviors not involving interaction between individuals from different locales). Only at their habitual location can they observe a given category of behavior often enough to generate a histogram with the range and frequency of alternative expressions and to produce a plot with means, modes and medians (regardless of whether these parameters have any adaptive significance for the given population). Clearly, they can-

not induce these parameters elsewhere in their information field. Thus, they are forced to evaluate their infrequent encounter with a given behavior at other locales in terms of the parameters for this category at their habitual location. If the observation elsewhere differs from the central tendency at this habitual location, it is much easier to accommodate the difference as a chance deviate of this central tendency, rather than as a sample of a distinct distribution—a distribution moreover which could not be observed in any case. Thus, even common, every day behavior which does not involve intergroup interaction is bestowed with pattern and homogeneity across the information field of the informant. The less frequently this kind of behavior is observed or observable, the greater the tendency of the informant should be to replicate his histogram of reality across his interaction field.

Unless ethnographers apply a heavy dose of strong inference, they may automatically translate this spatial construct into a bounded social unit ("society") circumscribing a finite set of individuals with shared behavior patterns ("culture"). Ethnographic practice tends to amplify the finiteness and boundedness of this construct. For example, dissertation committees and review panels are not usually supportive of repetition and replication of fieldwork. Instead, professional colleagues normally avoid infringing upon each others' "people" and ethnographers tend to look for "unique" niches (an exciting exception is the Bushman Research project, see Lee and DeVore 1976). Thus, new fieldwork in a region usually attempts to maximize the social and spatial distance to previous fieldwork. This is easiest to achieve with informants whose information does not overlap that of previous informants. In this way, "social boundaries" are created, for the patterned information field of one set of informants clashes somewhere in space with the equally patterned construct of the informants of previous ethnographers. At the imaginary line where the two clash, a large amount of simultaneous behavioral contrast is predicted by the information that the ethnographers have obtained at their respective field locales. The actual behavior at this boundary, if it should exist, is rarely observed among hunter-gatherers and even less frequently has been the topic of problem-solving research.

Thus, ethnographic fieldwork may dichotomize the continuum of spatial process among hunter-gatherers into populations surrounded by boundaries, regardless of whether these boundaries have behavioral significance or not. Inside of these boundaries, ethnographic fieldwork posits a set of personnel that shares patterns of behavior to a significant degree. And at the "boundary" itself, a marked discontinuity in behavior is expected. As a result, spatial variability is reduced, pattern and homogeneity are artificially produced or exaggerated, and "cultures" and "societies" are created.

In the meantime, the archaeological record is easily fitted into these ethnographic constructs because this constitutes least effort behavior on the part of archaeologists. For example, the archaeological remains along a spatial transect are easily sorted as if they had a central tendency that applied to the entire unit under scrutiny (regardless of the relevance of this value to the population which produced the data). After all, cyclical, random, and directionally changing variance all have a mean, a mode, or a central tendency, although their predictive value along the transect would be virtually nil. This kind of sorting is made even simpler because the archaeological record tends to be spatially discontinuous. Archaeological discoveries, unlike ethnographic fieldwork, attract more research to an area. Thus, clusters of sites are excavated which are separated from other site clusters by intervening, sterile space. If the archaeological record in these clusters is described in terms of central tendencies applicable to their entirety—a simple task even if hunter-gatherers in the real world should not behave in this way—an internally homogeneous spatial unit is generated. At its margins, this unit clashes sharply with similar constructs (even if these contrasts are due to nothing but distance). It is easy to mistake these constructs as evidence of bounded social units akin to the societies of the ethnographic literature. Thus, the ethnographic record is replicated in the archaeological one and, in the absence of strong inference, ethnographically inspired anthropological theory receives spurious support from archaeological data.

These comments are not only addressed to the mentalist culture construct that has derived much of its "empirical support" from this practice. This construct has been so effectively laid to rest by materialist ethnography (see, for example, Barth 1969 or Harris 1968) and scientific archaeology (see, for example, Binford 1965) that few hunter-gatherer specialists continue to endorse it. The argument applies as well to a behaviorist culture and society construct: a unit bounded in space and personnel whose members carry out a number of tightly constrained, closely replicated behaviors concerned with

boundary maintenance, group affiliation and group identity (and symbolize the same in other behaviors) to set themselves off from members of similar such units, in response to stimuli from their natural and social environment. In the hunter-gatherer literature, this construct is usually visualized as integrating around 500 people into a social and linguistic unit that is behaviorally differentiated from other such entities much like the ethnic groups of more complex society (see, for example, Birdsell 1958, Tindale 1974 or Williams 1974).

So far, the work of Binford (1973), Binford and Binford (1966) and others has shown only that the existence of such bounded units is unlikely before the Upper Paleolithic. Binford himself, though, leaves open the possibility that later hunter-gatherers were organized in this fashion (Binford 1973:234, 235, 237, 250). I have suggested that socio-cultural boundaries of this kind are associated with such severe locational penalties under the low population densities characteristic of most hunter-gatherers that they should be expected only in very unusual ecological circumstances (Wobst 1976). Elsewhere I have pointed out that the functional matrix for messages of group affiliation and boundary maintenance is bound to be only weakly developed, if present at all, under the same conditions (Wobst 1977, see also Binford 1973). Only problem-directed research, with a good deal of strong inference, can establish how prevalent "ethnic groups" have been among the hunter-gatherers of the ethnographic and archaeological literature and what cultural processes facilitate their formation and demise. Clearly, the consumption of untested theoretical constructs from the ethnographic literature merely perpetuates the extant notions.

Short of modern mega-surveillance, synchronic behavioral variability along spatial transects cannot be *observed*. It has to be reconstructed through verbal interaction with informants (ethnographic mode) or through work with the products and precedents of this behavior (archaeological mode). In both cases, the reconstructions are hypotheses that have to have implications in the observable world. Given the virtual demise of hunter-gatherers accessible to ethnographic techniques, archaeologists will have to shoulder the responsibility of replacing the culture and society monolith, and similar processually insensitive constructs in the ethnographic and archaeological literature, with theory more permissive of behavioral variability and more sensitive to culture process in all of its spatial dimensions.

CONCLUSIONS

If observation were our only source of data on human behavior, we would know little about its variability. Fortunately, we have means of data acquisition that do not require our presence where behavior is transacted. We can observe the material precedents and products of behavior (archaeological and ethnoarchaeological mode) and we can acquire verbal information about behavior (ethnographic mode). These modes of data acquisition allow us to overcome the constraints on our field of vision and to deal with human behavior in all of its temporal and spatial expressions. Yet neither the material precedents and products of behavior nor verbalized information about behavior are behavior. Rather, they can be linked with behavior through hypotheses in a framework of strong inference. In the absence of strong inference, behavior inferred from these sources is nothing but an untested hypothesis or, if the construct cannot be evaluated, metaphysics. Archaeologists are the only anthropologists whose data contain information about behavioral variance in all of its dimensions: in personnel from single individuals in private to the largest structural poses; in space from the smallest catchment area to the largest continent-wide population matrix, and in time from single events to millennia. Long after the ethnographic era of hunter-gatherer research will have passed into history, archaeologists will be busy removing the ethnographically imposed form and structure from their data and retrodicting both the ethnographic and archaeological record. Only then will our theory become truly anthropological, that is, capable of predicting variability of behavior in all of its spatial and temporal dimensions.

Acknowledgments. I would like to dedicate this paper to Provost Dr. Paul Puryear, without whose failing support of Social Sciences at the University of Massachusetts, Amherst, I would have been done much earlier. Robert W. Paynter read and commented on a first draft. Thanks are due also to four anonymous reviewers. The flaws in logic, as usual, are my own.

- Barth, F. (Editor)
1969 *Ethnic groups and boundaries*. Little Brown and Co., Boston.
- Binford, L. R.
1965 Archaeological systematics and the study of culture process. *American Antiquity* 31:203-10.
1973 Contemporary model building: paradigms and the current state of Paleolithic research. In *Models in archaeology*, edited by D. L. Clarke, pp. 109-66. Methuen: London.
- Binford, L. R. and S. R. Binford
1966 A preliminary analysis of functional variability in the Mousterian of Levallois facies. *American Anthropologist* 68, part 2:238-95.
- Birdsell, J. B.
1958 On population structure in generalized hunting and collecting populations. *Evolution* 12:189-205.
- Fried, M. H.
1975 *The notion of tribe*. Cummings, Menlo Park, CA.
- Gábori, M.
1969 Paläolithische Schnecken-Depots von Szob. *Acta Archaeologica* (Budapest) 21:3-11.
- Harris, M.
1968 *The rise of anthropological theory*. Crowell, New York.
- Hofmann, I.
1975 Wege und Möglichkeiten eines Indischen Einflusses auf die Meroitische Kultur. *Studia Institui Anthropos* 23.
- Jarman, M.
1972 A territorial model for archaeology. In *Models in Archaeology*, edited by D. L. Clarke, pp. 705-34. Methuen, London.
- Kozłowski, J. K.
1972/3 The origin of lithic raw materials used in the Palaeolithic of the Carpathian countries. *Acta Archaeologica Carpathica* 13:5-19.
- Leacock, E. G.
1954 The Montagnais "hunting territory" and fur trade. *American Anthropological Association Memoir* 78.
- Lee, R. B.
1968 What hunters do for a living, or how to make out on scarce resources. In *Man the hunter*, edited by R. B. Lee and I. DeVore, pp. 30-48. Aldine, Chicago.
1969 !Kung Bushman subsistence. In *Environment and cultural behavior*, edited by A. P. Vayda, pp. 47-79. Natural History Press, Garden City.
1972 !Kung spatial organization: an ecological and historical perspective. *Human Ecology* 1:125-48.
- Lee, R. B. and I. DeVore (Editors)
1976 *Kalahari hunter-gatherers: studies of the !Kung San and their neighbors*. Harvard University Press, Cambridge.
- Okladnikov, A. P. (Editor)
1968 *Istorija Sibiri*. Vol. 1. Nauka: Leningrad.
- Platt, J. R.
1964 Strong inference. *Science* 146:347-53.
- Schrire, C.
1977 Hunter-gatherer studies and interpreting Paleolithic data: how far have we come. Paper delivered at the Symposium on Contemporary Theory and Paleolithic Data. SUNY Binghamton, March 5, 1977.
- Soffer, O.
1977 Models for trade in the Upper Paleolithic. Paper delivered at the Symposium on Contemporary Theory and Paleolithic Data. SUNY Binghamton, March 5, 1977.
- Speck, F. G. and L. C. Eiseley
1939 The significance of hunting territory systems of the Algonkian in social theory. *American Anthropologist* 41:269-80.
- Struever, S. and G. L. Houart
1972 An analysis of the Hopewell Interaction Sphere. In *Social exchange and interaction*, edited by E. N. Wilmsen, pp. 47-80. *University of Michigan Museum of Anthropology, Anthropological Papers* 46.
- Thorbahn, P. F.
1976 The precolonial ivory trade in eastern Africa. Paper on file at the Department of Anthropology, University of Massachusetts, Amherst.
- Tindale, N. B.
1974 *Aboriginal tribes of Australia*. University of California Press, Berkeley.
- Vayda, A. P. and B. J. McCay
1975 New directions in ecology and ecological anthropology. *Annual Review of Anthropology* 4:293-306.
- Wallerstein, I.
1974 *The modern world-system*. Academic Press, New York.

- Weiss, K. M.
1976 Demographic theory and anthropological inference. *Annual Review of Anthropology* 5:351-82.
- Williams, B. J.
1974 *A model of band society*. Society for American Archaeology Memoir 29.
- Wobst, H. M.
1976 Locational relationships in Paleolithic society. In *The demographic evolution of human populations*, edited by R. H. Ward and K. M. Weiss, pp. 49-58. Academic Press, London.
1977 Stylistic behavior and information exchange. *University of Michigan Museum of Anthropology, Anthropological Paper* 61:317-42.
- Yellen, J. and H. Harpending
1972 Hunter-gatherer populations and archaeological inference. *World Archaeology* 4:244-53.