MAN THE HUNTER

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28.

Methodological Considerations of the Archeological Use of Ethnographic Data

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Any consideration of the implications for archeological interpretation of new ethnographic data on hunter-gatherers requires an examination of the general relationship between ethnographic observations and archeological reasoning. Only then can the particulars of any new ethnographic data be placed in proper perspective.

It is frequently stated that one of the main tasks of the archeologist is the interpretation of the past and that the primary means available is reconstruction based on analogies to living peoples. Such a view presupposes that our knowledge of the past is only as good as our knowledge of the present and that our reconstructions are valid only insofar as we are justified in projecting knowledge of living peoples into the past. I would like to take exception to the above-stated position and set as the task of the archeologist and the anthropologist the explanation and explication of cultural differences and similarities. Ethnologists may, by virtue of their particular field of observation, explicate certain cultural forms not directly observable in the archeological record. The archeologist, on the other hand, may explicate forms of cultural phenomena not generally discussed by the ethnologist, although these phenomena may be available to the ethnologist for observation. However, some phenomena, particularly those of a processual nature covering considerable periods of time, may be unavailable for direct observation by the ethnologist.

Adequate explanations are in no way dependent upon the data having been collected within any particular frame of reference; ethnologists can and do use archeological data, and vice versa. Cultural systems function within an ecological field whose structural changes are most frequently not synchronously phased with the life span of an individual. It follows that even if an individual were to devote his life exclusively to observation, he could not be expected to give an accurate or replicable account of the operation of cultural evolutionary processes. We would expect that some explanations for phenomena explicated through ethnographic observation to be found in archeological data, and vice versa. Further, archeological research might be expected to yield explanations for some observations of cultural phenomena made exclusively through archeological data.

If we define an hypothesis as the statement of a relationship between two or more variables, and if both variables are observable in the archeological record, then the hypothesis formulated is testable. It is only through the testing of hypotheses logically related to a

series of theoretical propositions that we can increase or decrease the explanatory value of our propositions. The same procedure is the only means available to the ethnologist for generating adequate explanations. Methodologically, then, the archeologist is in no way dependent upon the ethnologist. Archeologists are dependent for building models upon the knowledge currently available on the range of variability in form, structure, and functioning of cultural systems. Much of this information has, of course, been provided by ethnographic investigation. It is this background information which serves the archeologist in offering explanatory propositions for some of the differences and similarities observed in the archeological record, many of which may not necessarily reveal differences or similarities between distinct cultural systems.

For example, some observed differences and similarities may be explained by differences in preservation; others may reveal differences in function between sites occupied by the same social unit; others may document different occupation histories by social units at separate locations. Many adequate and accurate explanations may refer to functional relationships between locations, material items, and classes of human activities, etc. However, as I have argued elsewhere (L. R. Binford, 1967) the "interpretation" of the archeological record by the citation of analogies between archeologically observed phenomena and phenomena from a known behavioral context simply allows one to offer his *postulate* that the behavioral context was the same in both cases. In order to increase the probability that the postulate is accurate, a number of testable hypotheses must be formulated and tested.

Archeologists are not limited to analogies to ethnographic data as the sole basis for offering explanatory postulates; models can be formulated in a theoretical calculus some of which may deal with forms without ethnographic analogs. Archeologists are certainly indebted to ethnographers for providing sources which can be used as inspirations for model-building. The crucial point, however, is that our understanding of the past is not simply a matter of interpreting the archeological record by analogy to living societies as has been commonly asserted (cf. Thompson, 1956, p. 329). Our knowledge is sound to the degree that we can verify our postulates scientifically, regardless of the source of their inspiration. Scientific verification for archeologists is the same as for other scientists; it involves testing hypotheses systematically.

If archeologists and ethnologists are to overcome the limitations of their observational fields and contribute to the general field of anthropology, they must develop methods which will allow explanatory propositions regarding the operation of cultural systems to be tested by both archeological and ethnographic data. The archeologist, independent of the ethnologist, must search for order in the data available to him. After the recognition of classes of order in the record, the archeologist must then develop models that will allow him to relate the archeologically observed phenomena to variables which, although observable in different form among living peoples, are thought to have explanatory value. The only other alternative is to advertise his findings as indicating a new explanatory variable previously not isolated which might still be operative in the cultural systems of living peoples.

For example, archeologists and ethnologists may observe different phenomena to gain knowledge of a common variable. Ethnologists may interview informants and determine verbally the jural rules regarding postmarital residence. In addition they may take a census to determine the degree of conformity between the stated jural rule and actual decisions. These data may then be descriptively summarized and later treated as a case in testing various hypotheses regarding the conditions determining form of postmarital residence.

For many years it would have been argued that knowledge of postmarital residence rules of prehistoric communities was unobtainable by archeologists since it was not "material culture." A Soviet archeologist, Tretyakov, was the first to my knowledge to suggest that there was evidence for the forms of postmarital residence preserved in the archeological record (Tretyakov, 1934, p. 141). His work was summarized and commented upon favorably by Childe (1943, p. 6). Tretyakov's argument was fairly straightforward: the form of fingerprints on the inside of vessels indicated that it was females who manufactured pottery. In societies

where matrilocal residence was the rule, there would be less formal variability expected in the execution of ceramic designs within any single community than under conditions where patrilocality was the rule, since patrilocality brings about a mixed population of female potters. Quimby (1956) suggested a similar argument to account for observed differences in variability between samples of pottery from Huron and Chippewan historic sites in the Great Lakes. The development and refinement of this particular argument and the perfection of reliable analytical methods have been accomplished by several recent workers (Deetz, 1965; Freeman and Brown, 1964; Hill, 1965, 1966; Longacre, 1964a, 1964b; McPherron, 1965; Whallon, 1965). As a result of this work, we can today state within definable confidence limits the postmarital residence patterns of prehistoric communities. Given the discussions at this conference over the nature of the determinants of residence rules and the degree to which patrilocality characterizes "pristine" hunter-gathers, we may find that some of the answers will be found in archeological data.

This case provides a good example of a study in which different forms of information were relevant in the elucidation of a single variable. It also exemplifies another important point—the interpretive model for the archeological data was not based on simple analogy to ethnographically known societies. The model was drawn deductively from several assumptions and propositions:

1. Females were the potters.
2. Homogeneity of cultural expression within a group varies directly with the homogeneity of the group's composition.
3. Many formal characteristics of pottery are stylistic and tend to vary with tradition rather than utiliarian or mechanical factors.

The model developed from these propositions states the nature of the expected relationship between two variables: formal variability in items produced at a given location by females and variability in premarital residence of these females. While the points of premarital residence of females of an archeologically known community are not directly observable, this proposition remains a postulate. However, among contemporary groups, or groups whose residence patterns are known through documents, the proposition may be stated as a testable hypothesis. This illustrates one of the functions of ethnographic data in archeological reasoning. These data may be used for testing hypotheses for which information on one or more of the relevant variables is not obtainable through archeology. While models for the interpretation of archeological data may be tested and verified on other than ethnographic sources, it may often be more impressive or scientifically more efficient in obtaining high levels of confidence to make such tests with ethnographic data.

This latter kind of investigation was strongly advocated by Kleindienst and Watson (1956) in urging a kind of inquiry they termed "action archeology." Watson actually did carry out a study of a living community from the perspective of an archeologist while she was a member of the staff of Braidwood's 1959–60 Iranian project. Since then there have been increasing numbers of workers studying archeologically relevant data among living peoples. Ascher (1962) studied the Seri; Leshnik (1964) lived with village agriculturalists in India; Richard Gould of the American Museum of Natural History is currently engaged in research among the Australian aborigines. Margaret Hardin, a graduate student at the University of Chicago, is currently studying functional variability in ceramic styles among Mexican potters. Longacre and Ayres recently reported their analysis of an abandoned Apache wickiup (1966).

From the preceding discussion, we see that ethnographic data can play two basic roles in archeological investigation: first, they serve as resources for testing hypotheses which seek to relate material and behavioral cultural phenomena; second, they may often (but need not always) serve as the basis for models of particular social relations which are postulated to have been the context for an observed archeological structure. In the former case, "action archeology" studies are relevant; in the latter, model building and testing can be related to ethnographic facts, but verification of propositions would remain a problem to be solved by the formulation of hypotheses testable by archeological data.

Given this relationship between ethnographic data and archeological inquiry, how

can cooperation between the two specialists be maximized for the solution of common problems? In the first place, if archeologists and ethnologists are to work with common problems, their observations must be geared toward gathering data on the same variables, despite the obvious differences in their fields of observation. Second, they must work in terms of comparable sociocultural units. Finally, there must be a free exchange of information between archeologists and ethnologists to achieve the first two aims.

The kind of profitable feedback that can occur through such interdisciplinary exchange is nicely illustrated by this conference. Archeologists have worked too long without the benefits of the understandings of ethnology as to the operation of sociocultural systems. Further, some of the work discussed at this conference was directly relevant to the perfecting of techniques for gaining information on identical variables. I am referring to the "action archeology" studies of Richard Lee among the Bushmen and Bob Williams among the Birhor. These two studies add tremendously to our knowledge and are two of the most comprehensive in seeking to document the relationships between behavior and the spatial structure of artifacts which would be observable in the archeological record. Such information will be very useful in testing some of the propositions of Cook and Treganza (1950), Cook and Heizer (1965), and Naroll (1962) as to the relationships between population size and site size as well as population size and amount of enclosed space required. The site maps and structural details illustrating the internal spatial-formal structure of settlements, documented by Lee and Williams, will be valuable in testing a number of propositions regarding spatial disposition and correlates of social status. With the data now available it would be possible to measure the degree to which proximity between living areas in a settlement is correlated with social distance as measured in kinship affiliations. Exceptions to such correlations and the kinds of contingencies which intervene can be more easily spotted with data like Lee's and Williams' available.

On a slightly more critical note, and writing from only a superficial knowledge of the data gathered by Lee, some of the observations made and some of the emphases stressed at this conference bear witness to the need for freer communication between ethnographers and archeologists. For example, Lee's site maps are informative, yet he uses them to illustrate how little of the physical remains of a living people are left for the archeologist to observe. However, it should be noted that Lee made only surface observations and did no excavation. In the discussion of the relative importance of meat versus plant food, Lee's exact measurements of intake of the !Kung Bushmen is good to have, but it does duplicate many of the points raised and resolved fourteen years ago and reported in *An Appraisal of Anthropology Today* (Tax, 1953). Linton discussed many of the points raised here and stated:

> there are very few places in the world where people live entirely by hunting . . . you may remember that the occupation layers of Peking Man at Choukoutien have hackberry seeds (1953).

I suspect that an archeologist actively engaged in research might have made many different observations than those made by Lee. In his site maps Lee does plot hearths and fire areas but treats them all as the same. Detailed descriptive data on the formal differences in discrete and metrical attributes of hearths used in lighting, heating, and cooking of various kinds of food might have contributed greatly to our understanding of the functional variables which must be considered in dealing with archeologically observed hearths. This kind of information might help to prevent the simplistic kind of archeological interpretation recently made by Movius in which the size of the hearth is taken as an index of the size of the social unit occupying the site (Movius, 1966, p. 321).

This critical note is not meant to discourage "action archeology" studies, but it is to be hoped that ethnologists in making such observations might do well to put their observations in the framework of archeological question-asking.

The emphasis by the participants in this symposium on questioning certain propositions which have generally been accepted as truisms has far-reaching implications for archeological research. To my knowledge all archeological

theorists who have considered the role of subsistence technology in evolutionary change have used a rather simplistic Malthusian model for population dynamics. The traditional view, superbly dealt with here by Sahlins, has been termed by Boulding (1955, p. 197) the Dismal Theorem. It holds that the ultimate check on population is misery; population will grow until the nutritional level falls and disease brings about population equilibrium. The fresh and challenging viewpoint put forth by Sahlins, and less playfully by Birdsell and Williams certainly brings into question many anthropological cliches about the origins of agriculture and animal domestication.

One of the most encouraging aspects of this conference is that archeologists and ethnologists are moving in the direction of dealing with comparable units. For example, archeologists assume that the size, composition, and spatial structure of an assemblage are jointly determined by: first, the size and composition of the social unit responsible; and second, the form of differential task performance carried out by individuals and segments of the occupying social unit. Such assumptions allow us to analyze in structural terms the contents of sites and permit the definition of the different tasks represented (see Binford and Binford, 1966b). In all his work the archeologist has available information on the behavior of persons making up either task-specific work groups or local residential groups. Any social unit larger than these are known archeologically only through comparative analysis of differences and similarities in form, composition, and distribution in a generally unbounded universe of sites yielding archeological data. Local groups and task forces are the social units about which archeologists can get information without having to work their data through a fairly elaborate body of culture theory.

In view of this, it is very heartening to hear at this conference considerable discussion on form and cyclically varying composition of local groups. The work of Helm and Watanabe is especially useful in this regard and will undoubtedly serve archeologists as a basis for model-building.

But perhaps the major contribution of this conference for me is the stimulation it has provided; the specific data as they were presented were literally food for thought. At the risk of presenting some of these ideas before—to continue the metaphor—they have been properly digested, I would like to offer an argument which is the direct result of on-the-spot linkages made between data presented here and archeological problems. The following observations are drawn largely from archeological data:

1. Judging from the scanty information on settlements and from the inferred functions of recovered tools, man's adaptations during the Pleistocene were accomplished almost exclusively through the use of *implements*. Implements have been defined by Wagner (1960) as tools which serve to translate or enhance energy exchanges; examples would be spears, knives, digging sticks, atlatls, etc.

2. Near the close of the Pleistocene and during the immediately post-Pleistocene period there was increased elaboration in the use of *facilities* (this is Wagner's term also). Facilities are objects which serve to prevent motion and/or energy transfers—that is, fish weirs, nets, pottery.

Let us add to these observations some points made at this meeting. Hunters and gatherers whose subsistence is largely obtained through the use of implements (Bushmen, Hadza, forest Pygmies, and during the winter the Central Eskimo) are said to be somewhat casual about death and nonchalant in their treatment of the dead or dying. An exception was the Birhor discussed by Williams who hunt with facilities—nets.

Let us now add one more archeological observation: Systematic burial of the dead and elaborate mortuary ritual are greatly increased on a worldwide basis at the close of the Pleistocene. True cemeteries appear first in the Mesolithic in the Old World and the Archaic in the New World; the Archaic and the Mesolithic are further characterized by a heavy dependence on facilities in their subsistence activities.

It is tempting to make functional linkages between the structure of technological adjustment, nature of status definition, and hence of attitudinal involvement with persons occupying these statuses as expressed in mortuary ritual. Facilities to be efficient require precise placement in space; their effectiveness is dependent upon energy flow and they must be placed so as to maximize the interruption of this flow. Fish weirs are an excellent example.

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Facilities further require cooperative labor for their construction and maintenance.

All of this implies that the responsibility for coordination of effort will be assumed and, more important, that stewardships involving maintenance will be established. There would also be a necessary development of rules governing access to the facility and the distribution of its yields. Many other elements of role content would become part of newly defined status positions arising out of the use of facilities at the close of the Pleistocene. It is also suggested that emotional involvements would thus have been linked to interstatus dependencies of an order unknown in societies in which implements dominated the technology. In this new kind of facility-dependent society the death of an individual occupying a structured status position would necessitate the reallocation of his position to others, the retirement of "debts" to his descendants, and many other kinds of socially defined obligations which may well have been effected through mortuary rites.

If some of the ideas briefly outlined above should prove upon testing to have explanatory value, the coincident appearance of cemeteries in the New and Old Worlds at the close of the Pleistocene (as well as some of the "aberrant" features of the Birhor) might be elucidated simultaneously. The development and refinement of ideas stimulated by this conference will be one of the chief profits of having been in attendance.