AN ARCHAEOLOGICAL PERSPECTIVE

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Archaeological Perspectives*

A book whose title proclaims something new immediately challenges the reader to verify the claim to novelty or innovation. The purpose of this paper is to justify this book's title by making explicit what is new and, also, how familiar ideas and arguments gain a new significance when viewed in the perspective being developed.

This paper does not attempt an exhaustive historical analysis of the field of archaeology but is rather the selective treatment of several general areas of archaeological concern put into historical perspective. It is hoped that this background will offer the reader a greater depth of field against which to view the substantive papers which follow.

The Aims of Archaeology

> The most profitable inquiry [of archaeology] is the search for the origin of epoch-making ideas in order to comprehend the history of civilization (Mason, 1893, p. 403).
>
> Archaeology, by etymology the study of beginnings, has historical reconstruction for its objective (Kroeber, 1937, p. 163).

These early statements summarize the generally accepted view on the aims of archaeology. Taylor (1948, pp. 26 and 207) has thoroughly documented the fact that reconstruction of culture history was widely accepted as the end of archaeological research. Since Taylor's publication, this aim has been reiterated frequently and continues to be stated in very recent publications (Rouse, 1965, p. 2; Meggers *et al.*, 1965, p. 5; Willey, 1966, pp. 2–3; Deetz, 1967, p. 3).

If seeking origins and tracing the history of culture was one task of archaeology, some researchers considered a further aim to be the reconstruction of the lifeways of the peoples responsible for the archaeological remains. Such

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an aim appears early in the literature—for example, in H. I. Smith (1910) and Sollas (1924). Concern with the reconstruction of lifeways of extinct peoples has been expressed by many, but probably the most influential advocate for more attention toward this end has been Taylor:

> The conjunctive approach . . . has as its primary goal the elucidation of cultural conjunctives, the associations and relationships, the "affinities," within the manifestation under investigation. It aims at drawing the completest possible picture of past human life in terms of its human and geographic environment (1948, pp. 95–96).

Most archaeologists would agree that we should not lose sight of "the Indian behind the artifact" (Braidwood, 1959, p. 79) and would accept as a major aim of archaeology the reconstruction of lifeways.

While these aims of reconstructing culture history and lifeways cannot be said to have been satisfactorily achieved, a few archaeologists during the 1930's began to suggest aims reaching far beyond these:

> Some day world culture history will be known as far as archaeological materials and human intelligence permit. Every possible element of culture will have been placed in time and space. The invention, diffusion, mutation and association of elements will have been determined. When taxonomy and history are thus complete, shall we cease our labors and hope that the future Darwin of Anthropology will interpret the great historical scheme that will have been erected? . . . Candor would seem to compel the admission that archaeology could be made much more pertinent to general cultural studies if we paused to take stock of its possibilities. Surely we can shed some light not only on the chronological and spatial arrangements and associations of elements, but on conditions underlying their origin, development, diffusion, acceptance and interaction with one another. These are problems of cultural process . . . (Steward and Setzler, 1938, pp. 5–7).

One year earlier a Scandinavian archaeologist also urged that his colleagues take stock of where they have been and where they were going:

> It appears that archaeology, in spite of its remarkable achievements, has got into a cul-de-sac . . . . The whole subject consists merely of a comparison of forms and systematization . . . . Brilliant systematization, regarded as exact, has not led to and does not lead to an elucidation of the organic structure of the whole life of the period studied, to an understanding of social systems, of economic and social history . . . . Forms and types . . . have been regarded as much more real and alive than the society which created them and whole needs determined these manifestations of life . . . . Have we reached a crisis where the procedure and aim of our science must be revised? (Tallgren, 1937, pp. 154–155).

Statements urging archaeologists to concern themselves with problems of process appeared with increasing frequency in the literature of the next twenty years (Steward, 1942, p. 139; Bennett, 1943, p. 208; Childe, 1946, p. 248; G. Clark, 1953a, b; Barth, 1950; and especially Caldwell, 1958). As recently as 1958 this concern with process was still being defined and distinguished from other aims of archaeology:

> So little work has been done in American archaeology on the explanatory level that it is difficult to find a name for it . . . . In the context of archaeology, processual interpretation is the study of the nature of what is vaguely referred to as the culture-historical process. Practically speaking, it implies an attempt to discover regularities in the relationships given by the methods of culture-historical integration . . . . On this explanatory level of organization . . . we are no longer asking merely what but also how and even why (Willey and Phillips, 1958, pp. 5–6).

Willey and Phillips' statement about so little work having been done on the explanatory level was made despite such efforts as Steward's (1937) investigation of settlement patterns which were later elaborated on in the Viru Valley project. Willey himself had expressed great optimism about the possibilities for "processual interpretation" as well as for the reconstruction of cultural institutions (Willey, 1953, p. 1). Some of the other efforts made between the late 1930's and the late 1950's toward gaining an understanding of cultural process were White's arguments on the role of energy in the evolution of culture (White, 1943, pp. 335–356), Steward's "Cultural Causality and Law . . ." (1949), and Steward and Wittfogel's study of irrigation (Steward *et al.*, 1955).

In his 1962 Presidential Address to the American Anthropological Association, Willey again commented on the lack of progress in gaining a processual understanding of culture history:

> Certainly the answers to the . . .. causal questions as to why the ancient American civilizations began and flourished as they did and when they did still elude us, and what I can offer . . . will do little more . . . than describe and compare certain situations and series of events (Willey, 1962, p. 1).

There began to appear in the literature a general dampening of enthusiasm of those who some twenty years earlier had called for the archaeologist to turn his attention to processual investigations. There was a similar pessimism expressed in the writing of British scholars despite the work of such authors as Childe (1936), Crawford (1953), and G. Clark (1951, 1953a):

> We have lost the confidence of the nineteenth century, and are children of an age of doubt . . . . We must recognize that in archaeology . . . there are no facts other than those which are . . . "observational data." . . . What we have at our disposal, as prehistorians, is the accidentally surviving durable remnants of material culture, which we interpret as best we may and inevitably the peculiar quality of this evidence dictates the sort of information we can obtain from it (Piggott, 1965b, pp. 4–5).

The linking together of the limits of archaeological interpretation with the fragmentary nature of the archaeological record is a phenomenon we examine in some detail later (see pp. 91–96), but the points to be made here are: (1) There was general acceptance of the three aims of archaeology—reconstruction of

culture history, reconstruction of lifeways, and the delineation of cultural process; and (2) there has been increasing despair over the feasibility of achieving the third aim.

The Methods of Archaeology–Traditional Approaches

This section examines the methods traditionally used in attempts to achieve the aims of archaeology. We shall deal with each of the aims separately, attempt to describe the methods employed, and analyze some of the problems underlying the application of method to problem.

Reconstructing Culture History

Reconstructing culture history consists of arranging cultural units in a way which accurately reveals their generic affinities. Archaeologists have generally operated on the basis of the following two assumptions:

1. The degree of genealogical affinity between two cultural units varies directly with the similarities they exhibit in generically related characteristics (for example, whole culture traits or complexes, design elements on artifacts, etc.).
2. The degree of genealogical affinity between two cultural units can be measured by the ratio of shared generically related characteristics to the number of such traits not shared.

It is evident that each culture trait tabulated in obtaining the ratio which measures degree of genealogical affinity must be evaluated *to determine whether the similarity between traits arose as a function of lineal transmission, diffusion between cultural units, or independent development within each cultural unit.* It is here that a basic, unsolved problem lies: *How can archaeologists distinguish between homologous and analogous cultural similarities?*

As early as 1896 E. B. Tylor concerned himself with this problem and suggested a procedure for analyzing observed similarities by

> . . . division into constituent elements showing so little connection with one another that they may be reasonably treated as independent. The more numerous such elements, the more improbable the recurrence of the combination (1896, p. 66).

In other words, Tylor suggests that one might calculate the probabilities of independent occurrences of identical combinations among a set of independently varying characteristics.

Other workers worrying over the same problem offered similar suggestions. For example, Graebner (1911) cites two criteria for evaluating cultural similarities: the criterion of form and that of quantitative coincidence. For Graebner the criterion of form consisted of the degree to which there was a coincidence of characteristics which did not necessarily stem from "the nature of the objects compared"; the criterion of coincidence lay in determining whether or not the trait or item under study occurred as an isolated similarity or as an element of a greater cultural complex. On the basis of the criterion of form, this greater cultural complex could not reasonably be viewed as having arisen independently.

Robert Lowie pointed out some of the shortcomings of Graebner's reasoning: "The comparison of form can never do more than establish the identity of forms; that such identity is to be explained by genetic relationship is an hypothesis" (1912, p. 28). He also noted that Graebner's quantitative criterion was not probabalistic as was Tylor's but was simply the criterion of form raised to a higher level of abstraction and was therefore not an independent criterion for judgment (1912, p. 27).

A recent evaluation of the applications of Tylor's probability method notes that probability calculations of concrete cases have seldom been performed accurately, and in many instances the apparent accuracy of probability reasoning has been a semantic rather than a methodological addition to the anthropological literature (Erasmus, 1950, pp. 374–375). A more basic flaw in Tylor's procedure is the assumption of a worker's *ability to recognize constituent elements which are in fact independent variables.* This problem has been discussed (Erasmus, 1950, pp. 375–387; Rands and Riley, 1958; and indirectly by Sackett, 1966), but no methods have been advanced for the solution of the problem other than the intensive analysis of the distribution and patterns of covariation demonstrable among selected characteristics. Such studies have rarely been conducted by archaeologists and certainly have never been a routine analytical component of the works of archaeologists proposing historical reconstructions. This particular problem has been the almost exclusive concern of ethnographers and is one of which archaeologists involved in reconstructions of culture history have seemed deliciously unaware.

Lowie (1912, pp. 24–27) pointed out another problem in method—that while some workers have attempted to identify similarities which arose from generic connections between cultural units, no one had considered the means for evaluating the alternative of independent development, except by lack of ability to demonstrate historical connections. Without first gaining some understanding of laws of cultural development, such independent means for evaluating particular cases will continue to be lacking.

Despite these unsolved problems of method and our consequent inability to distinguish accurately analogies and homologies, archaeologists have continued

to formulate reconstructions using the procedures set forth by Tylor and Graebner on a common sense level, often adding distributional criteria. The principles of interpretation which have guided archaeologists' reconstructions of culture history can be summarized as follows:

1. The probability of diffusion having taken place increases directly with the degree of formal resemblance between items and traits (Jennings, 1957, p. 265; Linton, 1936, p. 372) and with the degree of componential complexity of the traits compared (Linton, 1936, p. 372).
2. The probability of diffusion having taken place decreases with the amount of temporal and spatial separation between the traits being compared (Linton, 1936, p. 370; for relevant discussions, see Wallis, 1928; Meggers *et al.*, 1965, pp.157–178; Rowe, 1966, pp. 334–337).

Such guides to interpretation ignore the inherent unsolved problems of method and epistemology, and most taxonomic schemes proposed as aids to historical reconstruction also fail to cope with them. For example, McKern in his discussion of the Midwestern Taxonomic System made it quite clear that classifications are to be made with respect to a list of culture traits undifferentiated as to the likelihood of their representing analogies or homologies:

> All the traits characteristic of a given culture manifestation comprise the culture complex for that manifestation . . . . In any comparison of this manifestation with another, made for purposes of classification, certain traits may be demonstrated as present in both complexes, and these linked traits [serve] to show cultural similarity between the two culture variants (1939, p. 205).

Numerous cases of the application of the Midwestern Taxonomic System (B. L. Smith, 1940; Cole and Deuel, 1937, pp. 207–219; Griffin, 1943; Morse, 1963) demonstrate that there was no attempt made to distinguish between analogous and homologous traits. (It should be pointed out, however, that the McKern system is internally consistent and logical; most of the problems with it have arisen from those who have misused it.) Other schemes have also employed summations of observations whose relevance to discussions of cultural phylogeny and contact might well be questioned (Gladwin, 1934; Colton, 1939). Rouse (1955) recognized the difference between classification based on gross measures of similarity and "genetic correlations"; he went on to suggest that for the purpose of historical reconstruction

> . . . it would seem advisable first to eliminate all those resemblances which do not appear to have been accompanied by contact. Next, one must decide which of the remaining resemblances are due to genetic connection rather than to some other factor such as

adaptation to a similar environment or attainment of the same level of cultural development. Only then will it be safe to choose from among two various possible forms of genetic connection (1955, p. 719).

However, Rouse offers no guidelines for deciding which traits are generically related and which ones might exhibit similarity from other causes. In short, Rouse's statement shows an awareness of many of the shortcomings of taxonomic schemes but offers no solution to one of the major underlying methodological problems.

It is argued here that the accomplishment of the reconstruction of culture history is predicated upon an overhaul of method and theory, that traditional methodology and analytical procedures are inadequate for the successful achievement of the stated aims of the field. Given our current sophistication in dating techniques, we can fairly accurately place archaeological remains in their proper chronological relationships to one another. We can inventory the remains and discuss additions, deletions, and "hybridizations" in the inventories of sites through time. We can also formulate classifications of assemblages on the basis of summary measures of formal similarities between recovered items (see Ford, 1954); we can also measure likenesses by comparing the total composition of the sample of recovered materials (see Bordes, 1953). Arguments can then be formulated about the probability of one such taxon being the cultural ancestor, descendant, or collateral relative of another taxon (see Hodson *et al.*, 1966; Doran and Hodson, 1966), or whether another unit might be more appropriately considered (see Warren, 1967, pp. 168–185; Sanger, 1967, pp. 186–197; Aikens, 1967, pp. 198–209; Schlesier, 1967, pp. 210–222).

These procedures, however, do not help to achieve the stated aims of archaeology. An accurate and meaningful history is more than a generalized narrative of the changes in composition of the archaeological record through time (see, for example, Griffin, 1967); it is also more than a reconstruction from that record using interpretive principles such as those discussed above which can be shown to have inherent flaws. If we hope to achieve the aim of reconstructing culture history, we must develop means for using archaeological remains as a record of the past and as a source of data for testing propositions which we set forth regarding past events, rather than as a record we can read according to a set of *a priori* rules or interpretive principles whose application allow the skilled interpreter to "reconstruct" the past. We know much too little about both archaeological data and processes of cultural development to make "reading the archaeological record" anything but a shallow and suspicious pastime. What we seek to investigate is cultural process, and only with an understanding of such processes can we construct the events which form the context in which the archaeological record was produced.

Reconstructing Past Lifeways

The reconstruction of the lifeways of extinct peoples is the second aim of archaeology which we will examine in order to evaluate traditional methods. The standard operating procedure for achieving this aim is set forth in the following quotation:

> Everyone is aware of the fact that it is impossible to explain and to give absolute meaning to all the discoveries which are made while digging ancient villages. All we can do is to interpret what we find in the light of our knowledge of modern . . . [peoples] . . . . In this way, it is possible to moderate our conjectures, and piece them together by means of reasonable imagination. Thus, the cold, unrelated and often dull archaeological facts are vivified and the reader may have some sort of reconstruction in his mind's eye of what [past peoples] . . . were like and how they lived (Martin and Rinaldo, 1939, p. 457). [This statement is one of the first in the literature of American archaeology that deals with the reconstruction of lifeways. Paul Martin was in the avant-garde of archaeological thought in the 1930's, and he still is today. This quotation should in no way be considered a statement of his current views, which have grown and changed remarkably in thirty years–Eds.].

Most archaeologists would agree with this statement (see Willey, 1966, p. 3; Chang, 1967a, p. 109; Ascher, 1961). Analogy to living peoples has been the traditional answer to the question of how one goes about reconstructing lifeways (see Randall-MacIver, 1932, pp. 6–7; Hawkes, 1954, pp. 157–158; Vogt, 1956, p. 175; Piggott, 1965b, p. 12; Rouse, 1965, p. 10; Willey, 1966, pp. 3–4). The major controversy has concerned the appropriateness of a given ethnographically known group or set of conditions as a model for the lifeways of the groups under archaeological study (see Lowie, 1940, pp. 369–370; Slotkin, 1952; S. R. Binford, 1968).

Given the method of analogy to living peoples, appeals have been made by archaeologists to explore the record in search of units which can be meaningfully compared in analogies to living peoples. One obvious plea has been for archaeologists to excavate the remains of entire communities, to concern themselves with the comparative study of settlement, as well as with the internal organization of sites. Taylor (1948), in appealing for archaeologists to study in detail the contextual relationships among the archaeological remains, asked for a search for order demonstrable among the elements in an archaeological deposit. Willey (1953, 1956), Chang (1958, 1967a), and Trigger (1967), among others, have stressed the desirability of the investigation of settlement patterns, since these are observable among living peoples and are said to be informative about social organization.

Pleistocene archaeologists also are increasingly viewing sites as the remains of activities conducted by social units; this kind of data collection is stressed in the search for living floors and in attempts at fairly complete excavation of sites.

> The living places of Pleistocene peoples are capable of yielding the same kind of evidence as to the behavior and ecology as do those of much later times when the appropriate techniques of exposure and excavation are applied to their recovery . . . . Such field studies . . . of . . . Paleolithic sites [are] infinitely more rewarding and significant, as can be . . . appreciated from papers . . . relating to living floor excavation (J. D. Clark and Howell, 1966, pp. v–vi).

Another aspect of data collection which has been dealt with in recent years is the problem of sampling. There has been frequent discussion of the use of sampling techniques which are designed to increase the probability that archaeological samples taken are in fact representative of what remains from the past (see L. R. Binford, 1964; Rootenburg, 1964).

Along with these refinements in data collection, there has been a growing interest in the study of living peoples by archaeologists (Crawford, 1953; Kleindienst and Watson, 1956; Thompson, 1958; Ascher, 1962; Watson, 1966). Such studies have as their aim the delineation of behavioral correlates for material items (Chang, 1958; Robbins, 1966), and the purpose of archaeologists undertaking such research has been to maximize their interpretive powers by increasing their knowledge of living peoples—that is, to make more secure the analogies they draw between lifeways of peoples known archaeologically and those known ethnographically.

While we applaud all attempts to increase the reliability of data collected archaeologically and while we certainly favor a firmer basis for determining the behavioral correlates of material culture, both refinements in data collection and increased ethnographic knowledge cannot by themselves increase our knowledge of the past. Facts do not speak for themselves, and even if we had complete living floors from the beginning of the Pleistocene through the rise of urban centers, such data would tell us nothing about cultural process or past lifeways unless we asked the appropriate questions. We can infinitely expand our knowledge of the lifeways of living peoples, yet we cannot reconstruct the lifeways of extinct peoples unless we employ a more sophisticated methodology. Fitting archaeological remains into ethnographically known patterns of life adds nothing to our knowledge of the past. In fact, such a procedure denies to archaeology the possibility of dealing with forms of cultural adaptation outside the range of variation known ethnographically (see S. R. Binford, 1968). In view of the high probability that cultural forms existed in the past for which we have no ethnographic examples, reconstruction of the lifeways of such sociocultural systems demands the rigorous testing of deductively drawn hypotheses against independent sets of data.

This perspective is in marked contrast to the epistemological basis of traditional method, whose implications can readily be seen in a recent statement:

> As to analogy, archaeology as a whole is analogy, for to claim any knowledge other than the objects themselves is to assume knowledge of patterns in culture and history and to apply these patterns to the facts (Chang, 1967a, p. 109).

I have criticized this view elsewhere (L. R. Binford, 1967a, b, 1968) and would state here that so long as we insist that our knowledge of the past is limited by our knowledge of the present, we are painting ourselves into a methodological corner. The archaeologist must make use of his data as documents of past conditions, proceed to formulate propositions about the past, and devise means for testing them against archaeological remains. It is the testing of hypotheses that makes our knowledge of the past more certain, and this is admittedly a difficult business. Archaeology as part of anthropology and anthropology as a social science are often guilty of the charges made against them by the "harder" scientists:

> The most important feature about a hypothesis is that it is a mere trial idea . . . [and] until it has been *tested*, it should not be confused with a *law* . . . . The difficulty of testing hypotheses in the social sciences has led to an abbreviation of the scientific method in which this step is simply omitted. Plausible hypotheses are merely set down as facts without further ado (Wilson, 1952, pp. 26–27).

Traditional archaeological methodology has not developed this final link in scientific procedure. For this reason, reconstruction of lifeways has remained an art which could be evaluated only by judging the competence and honesty of the person offering the reconstruction (Thompson, 1956).

The Study of Cultural Process

Different authors have referred to different phenomena in their discussions of culture process. The phrase has been used to refer to the dynamic relationships (causes and effects) operative among sociocultural systems, to those processes responsible for changes observed in the organization and/or content of the systems, or to the integration of new formal components into the system. The term cultural process has been used by others to refer to patterns or configurations in the temporal or spatial distributions of the archaeological materials themselves (see Wauchope, 1966, pp. 19–38). The first set of meanings—that of dynamic relationships operative among cultural systems—is the one used by this author and by the other authors in this volume.

Let us examine the methods and procedures traditionally followed in seeking an understanding of culture process, regardless of the meaning given to the term. Most often, the procedure has been to equate process to a transformational sequence of forms, normally summarized in a stage classification. A second, or

sometimes an alternative, procedure has been to pursue a comparative study of temporal and spatial changes of archaeologically known cultural forms, to note certain trends or regularities. These trends are then stated as empirical generalizations which, in turn, are taken as statements regarding culture process (see Steward, 1949; Braidwood, 1952, 1960; Braidwood and Reed, 1957; Willey and Phillips, 1958; Willey, 1960; Beardsley *et al.*, 1956). The criticism to be offered here is that any stage classification is simply an ordinal scale for measurement. The application of such a scale to innumerable empirical cases, or even the ultimate systematization of all archaeological materials, can never provide us with an understanding of the processes operative in the past which resulted in the stadial sequence. An empirical generalization of data—no matter how accurate it is—is never an explanation for the data. The ordering of forms of life, the end-products of evolution, by Linnaeus, did not describe or define the process of organic evolution.

Steward has suggested that the comparative study of distribution of cultural forms in space and through time will reveal certain trends, regularities, or patterns for which historical or generic interpretations are appropriate; he suggests further that these trends or patterns reflect cultural process (Steward, 1949, p. 3). This suggestion is, however, predicated on our ability to discriminate between cultural analogies and homologies. As pointed out above methods for such discrimination have yet to be developed. Even if we were capable of making this distinction, the demonstration of empirical "regularities" simply documents similarities which need to be explained; it is to be hoped that the explanations offered would deal with cultural or ecological processes operative in the past.

Rouse (1964, 1965) has offered archaeologists an "out," and his ideas undoubtedly have great appeal for those who would like to study cultural processes but lack the methods for doing so. He states that since we recognize a difference between the *process* of evolution and the *products* of evolution, that the study of the process should properly be the domain of ethnologists, "who are able to observe change as it is still going on" (Rouse, 1964, p. 465). He suggests further that the archaeologists might more appropriately study the products of evolution in systematic terms—by descriptive taxonomic and distributional schemes. In this view, processes of cause and effect cannot legitimately be studied by archaeologists since they are not part of the archaeological record, cannot be dug up, and are not available for direct observation.

Others, working within the traditional framework, have stated that archaeologists *can* gain understanding of cultural process and that the means for doing so is to interpret data from the past in the light of our understanding of the present. An example of this approach can be seen in what Willey and Phillips term "developmental interpretation"—a process which allows the archaeologist

to "abstract . . . certain characteristics that seem to have significance from the point of view of the general development of . . . culture" (Willey and Phillips, 1958, p. 77).

However, the decisions as to which characteristics are significant in the general development of culture do not derive from the data themselves; they are given meaning by the ideas we hold about the processes of cultural development. If we simply employ these ideas for interpreting archaeological remains, then no new information can be gained from the archaeological record about processes which operated in the past. In short, traditional archaeological studies have often recognized the desirability of investigating process, but methods for successfully conducting such studies have not been developed. It is toward this end that much of the thought and work of the authors in this volume have been directed.

Archaeological Theory and Method—New Perspectives

We have offered a brief review of the methods commonly employed for achieving the stated aims of archaeology. In this section we hope to compare and contrast some aspects of traditional method and theory with very recent developments in the field which are substantively illustrated in this book. This discussion of theory and method will be conducted under several problem headings.

Induction and Deduction

One striking feature of traditional archaeological method, regardless of the aims of the research, has been the lack of any rigorous means of testing, and thereby gaining confidence in, propositions about the past. Statements about the historical, functional, or processual significance of observed characteristics of the archaeological record have been evaluated by two criteria: (1) the degree to which our knowledge of contemporary peoples might justifiably be projected back to extinct sociocultural systems, and (2) the degree to which we might have confidence in the professional competence and intellectual honesty of the archaeologist advancing interpretations (see Thompson, 1956, p. 33). Traditional methodology almost universally espouses simple induction as the appropriate procedure, and the archaeological record is viewed as a body of phenomena from which one makes inductive inferences about the past. Such inferences are to be guided by our knowledge of contemporary peoples and also by certain principles, such as mechanical principles which govern the fracture of flint. The application of ethnographic knowledge and of guiding principles are the traditional means for increasing confidence in our inferential generalizations about the past.

> Inference is the key or the methodological pivot of archaeology, for it is only through inference that inanimate objects are reassembled into the milieu of life. Inferences are drawn from analogies (Willey, 1966, p. 3).
>
> At the inferential level, the archaeologist is at last providing the flesh for the bare bones of his data, and, if done with care and imagination, such a procedure makes possible the delineation and ultimate understanding of past cultures (Deetz, 1967, p. 11).

The changes in archaeology which are documented in this book are more than simply new methods and new theories; the changes consist of theories and methods developed in the context of a new epistemological perspective on such basic issues as the appropriate scientific procedures to be followed in investigating the past. In this perspective, a central point to be made concerns the role of induction in science:

> There can be no general rules of induction; the demand for them rests on a confusion of logical and psychological issues . . . . What determines the soundness of a hypothesis is not the way it is arrived at (it may have been suggested by a dream or a hallucination), but the way it stands up when tested, i.e., when confronted with relevant observational data (Hempel, 1965, p. 6).

In stressing induction and the drawing of sound inferences, then, the stress falls on the psychological issue, as pointed out by Hempel, of how to make meaningful statements about archaeological remains and what they represent from the past. What is argued here is that the generation of inferences regarding the past should not be the end-product of the archaeologist's work. While an awareness of as great a range of variability in sociocultural phenomena as possible and the citation of analogy to living peoples are not belittled here, the main point of our argument is that independent means of testing propositions about the past must be developed. Such means must be considerably more rigorous than evaluating an author's propositions by judging his professional competence or intellectual honesty.

We assert that our knowledge of the past is more than a projection of our ethnographic understanding. The accuracy of our knowledge of the past can be measured; it is this assertion which most sharply differentiates the new perspective from more traditional approaches. The yardstick of measurement is the degree to which propositions about the past can be confirmed or refuted through hypothesis testing—not by passing judgment on the personal qualifications of the person putting forth the propositions. The role of ethnographic training for archaeologists, the use of analogy, and the use of imagination and conjecture are all fully acknowledged. However, once a proposition has been advanced—no matter by what means it was reached—the next task is to deduce a series of testable hypotheses which, if verified against independent empirical data, would tend to verify the proposition.

The shift to a consciously deductive philosophy, with the attendant emphasis

on the verification of propositions through hypothesis testing, has far-reaching consequences for archaeology. As an example of such consequences I will discuss briefly two topics commonly treated in presentations on archaeological theory and method: the limitations of the archaeological record, and the appropriate units of archaeological observation.

Limitations of the Archaeological Record

The arguments on this topic generally begin by citing the fact that much of the material content of an ongoing sociocultural system is lost through decay or the action of other physical agents (such as fire) before the time the archaeologist can make his observations. It is then asserted that our knowledge of the past is limited to those classes of data which survive and that, depending on variations in past behavior, our knowledge of the operation of the sociocultural system in question may be enormously distorted (see, for example, Piggott, 1965a, p. 8). Such arguments also frequently take the form of asserting that since we can never know what is missing from the archaeological record, we can never correctly evaluate what *is* present. How can we know that an empirical generalization about archaeological data is accurate since there may be pertinent and noncomforming evidence that has been lost? (See M. A. Smith, 1955, p. 6; Heider, 1967, p. 62; Deetz, 1968a.)

An excellent example of reasoning of this kind is found in a recent discussion of the proper historical interpretation of distributions of African art styles:

> It is a curious fact that, with certain exceptions in Tanganyika, little rock art in the form of either painting or engraving, has been found north of the Zambezi . . . . It would appear that there is an almost complete break between the painting and engraving traditions of southern Africa and those of the Sahara. If this is so it makes the similarity between the two groups . . . appear as a striking example of parallel development. This would be a very hard case to prove . . . in view of the practice in many parts of the world of painting and engraving on such perishable substances as wood . . . . Indeed there is no reason to suppose that Late Stone Age man in East Africa and in the Congo did not paint or draw or engrave, simply because his work has not been preserved (Allchin, 1966, p. 41).

Allchin's dilemma arises directly and inevitably from the fact that she is offering an empirical generalization directly from the data and makes use of an *a priori* principle for interpreting the historical-cultural significance of the generalization. In this case the unstated principle would be that an interrupted distribution signifies a cultural boundary and independence for the two traditions represented. If one accepts the interpretive principle, the only possible way of invalidating the interpretation is to question the validity of the empirical generalization itself (namely, that there is a geographical break between the painting and engraving traditions of southern Africa and those of the Sahara). The validity of the generalization can be destroyed by citing an empirical case to

the contrary (an instance of painting or engraving in the "empty zone"). The generalization can also be challenged, and this is what Allchin does, by suggesting the possibility of such an empirical case to the contrary.

The possibility of an undocumented case to the contrary normally takes the form used in Allchin's argument—speculation about conditions under which data might be destroyed, overlooked, or "hidden." The validity of all generalizations may be questioned if this procedure is followed, since the possibilities for speculation about "hidden data" are infinite. Further, the validity of the interpretive principle itself can never be independently tested since its accuracy is tested only by reference to the empirical generalization it is said to cover. Extension of the generalization to cover new cases simply provides more instances for which the principle might be relevant; it in no way tests the principle itself. Cases to the contrary of the generalization only show that the data generalized are inappropriate to the principle employed; they in no way serve to test the principle itself. This is one of the crucially weak points of a purely inductive methodology. Thus, Allchin's principle implicitly used for interpretation of her generalization cannot, with the methodology employed, be validated or refuted, and the generalization itself can always be questioned by the possibility of citing hidden data or the incompleteness of the archaeological record.

The procedure we would advocate as a way out of Allchin's dilemma would be as follows:

Observations

1. There is a geographical break in the archaeological distribution of rock paintings and engravings between southern Africa and the Sahara.
2. The style of paintings and engravings from the two areas are very similar.

Proposition

The geographic break is the result of there having been two independent cultural traditions in the respective areas.

Deduction

Therefore, the similarity in form of painting and engraving is the result of parallel development.

Prediction

We would expect a similar break in the distribution of stylistic attributes of other items—for example, bead forms, decoration on bone implements, projectile point forms, etc.

Bridging Arguments

Here we would attempt to establish the relevance of some classes of

archaeological data to our deduction and prediction. We would try to establish that certain formal characteristics of artifacts, other than rock paintings and engravings, were stylistic and would therefore vary as a function of tradition.

Hypothesis

The distribution of the data whose relevance has been argued will exhibit interrupted distributions between southern Africa and the Sahara.

If the hypothesis were confirmed, then arguments about hidden data would be irrelevant since the existence of cultural boundaries would have been established by independent data. If the hypothesis were refuted, arguments of hidden data, while possibly relevant to the original generalization, would in no way place limits on our ability to gain knowledge of cultural boundaries from the archaeological record.

High-probability statements covering a broad range of phenomena are the aim of science, not empirical generalizations which can be destroyed by the citation of a single empirical case to the contrary. The endless search for data in harmony with empirical generalizations is a wasteful procedure at best, and the data can never serve to validate the generalization. Propositions can be evaluated by deducing hypotheses which must be tested against independent data. The argument of hidden data can always be made about generalizations, but it is significant only insofar as it prompts testing the validity of propositions made regarding the significance of the generalization. The citation of possible hidden data has no inherent value as a statement of limitation of our knowledge of the past, nor is it applicable to the truth or falsity of propositions. Confidence in any given proposition can be evaluated only with respect to the history of hypothesis formulation and with testing relevant to that proposition.

Another common argument on the limitations of the archaeological record asserts that the reliability of conclusions reached by an archaeologist varies directly with the degree to which the subject is removed from discussions of artifacts themselves (see MacWhite, 1956, pp. 4–6; Hawkes, 1954, p. 161; M. A. Smith, 1955, pp. 3–4; Piggott, 1965a, pp. 10–11).

> Artifacts and the study of artifacts—including typologies—are placed at the lowest level, and historic interpretations based upon such studies are considered to be of the greatest reliability. Moving into the socio-cultural system is moving up the levels of abstraction with increased use of inferences, and moving down the ladder of reliability . . . . Those who want to make inferences and to step beyond the limitations of archaeological remains can do so and engage in the fancy game of socio-cultural reconstruction (Chang, 1967a, pp. 12–13).

A frequent way of stating this argument is to propose a formal ladder of reliability:

> 1. To infer from the archaeological phenomena to the techniques producing them I take to be relatively easy.

2. To infer to the subsistence-economies of the human groups concerned is fairly easy.
3. To infer to the socio/political institutions of the groups, however, is considerably harder.
4. To infer to the religious institutions and spiritual life . . . is the hardest inference of all (Hawkes, 1954, pp. 161–162).

These statements are predicated upon two major premises: first, that the archaeological record is incomplete, that many items of the material culture have been lost through decay, destruction, etc.; second, that the archaeological record is lacking in all the nonmaterial features of the sociocultural system under study. The conclusion is then drawn that the reliability of our interpretations will vary directly with the degree to which we can justify the acceptance of a partial record as representative of the total material culture, and also with the degree to which we can believe that the nonmaterial components of any sociocultural system are reflected in the imperfectly preserved material items.

This reasoning is functionally linked to a methodology that limits the archaeologist to generalizing about the "facts" he uncovers. Since preservation is always imperfect, inferences from the facts of material culture to statements about the nonmaterial culture move us away from the primary data and thus diminish the reliability of our statements.

There has been a wide range of opinion expressed on this latter point—the degree to which nonmaterial aspects of culture can be inferred from material facts; the ultraconservative range of this spectrum can be seen in the following statement:

Since historical events and essential social divisions of prehistoric peoples don't find an adequate expression in material remains, it cannot be right to try to arrive at a knowledge of them through archaeological interpretation (M. A. Smith, 1955, p. 7).

Most of the authors in this volume would take strong exception to this statement. In the first place, the argument that archaeologists must limit their knowledge to features of material culture is open to serious question; and second, the dichotomy between material and nonmaterial aspects of culture itself and the relevance of this dichotomy for a proposed hierarchy of reliability have also been the subject of critical discussion (Service, 1964; L. R. Binford, 1962, 1965). It is virtually impossible to imagine that any given cultural item functioned in a sociocultural system independently of the operation of "nonmaterial" variables. Every item has its history within a sociocultural system—its phases of procurement of raw material, manufacture, use, and final discarding (see Deetz, 1968b). There is every reason to expect that the empirical properties of artifacts and their arrangement in the archaeological record will exhibit attributes which can inform on different phases of the artifact's life history.

Many different determinants which were operative in the past might be cited as proper explanatory variables for archaeologically recovered items. For example, pottery vessels manufactured in two different communities for use in identical tasks may vary significantly in form, depending on local habits of ceramic manufacture and on local design and decorative concepts. On the other hand, different forms of vessels made for different uses (for example, cooking versus storage) might be produced with the same techniques and have similar decorative elements. In this latter case, the formal properties of the vessels relating to use would vary independently of formal properties relating to local ceramic techniques. It is conceivable that many other independently varying classes of attributes in combination might characterize the final form of any given class of item. Each kind of independently varying attribute might be relevant to a different set of determinants and would thus require independent explanation for their form and distribution in the archaeological record. Each such independent explanation would, upon verification, inform us about the operation of different variables in the cultural system under study. It is highly improbable that the multiple, independent variables which determined the form of any item or the distribution of items should be restricted to only one component of a cultural system. This means that data relevant to most, if not all, the components of past sociocultural system *are* preserved in the archaeological record (L. R. Binford, 1962, pp. 218–219).

Our task, then, is to devise means for extracting this information from our data, and this demands more than making summary generalizations about items of material culture. There is no reason to expect that our explanations of the archaeological record should necessarily refer to the same order of phenomena as that being explained. If this is so, it follows that we cannot be restricted to the knowledge of "material culture"; rather, to explain our observations from the archaeological record, we must deal with the full range of determinants which operate within any sociocultural system, extant or extinct.

There has been as yet no attempt to assess the limitations of the archaeological record for yielding different kinds of information; nor does there seem to be the means of accurately determining these limits short of total knowledge of all the systematic relationships which characterized past cultural systems. Thus, present discussions of limitations of reliability are inappropriate and are based on speculation. And it is speculation which the more conservative exponents of such arguments have sought to avoid!

The position being taken here is that different kinds of phenomena are never remote; they are either accessible or they are not. "Nonmaterial" aspects of culture are accessible in direct measure with the testability of propositions being advanced about them. Propositions concerning any realm of culture—technology, social organization, psychology, philosophy, etc.—for which arguments of relevance and empirically testable hypotheses can be offered are as

sound as the history of hypothesis confirmation. The practical limitations on our knowledge of the past are not inherent in the nature of the archaeological record; the limitations lie in our methodological naiveté, in our lack of development for principles determining the relevance of archaeological remains to propositions regarding processes and events of the past.

Units of Observation and Units of Relevance: A Basis for Analysis

The shift to a rigorous hypotheticodeductive method with the goal of explanation implies changes also in our perception and use of the archaeological record. Archaeologists have normally accepted certain observational units—such as the item, the industry, or the assemblage—as the appropriate units for comparative investigation. Such investigation generally proceeds by breaking down archaeological remains into categories based on raw materials: bone, stone, ceramics, basketry, etc. Or, in other cases, the investigator may use functional classes, such as projectile points, knives, axes, etc. Whatever the breakdown used, such analysis serves only to clarify information already available; it cannot increase our knowledge. After his initial comparative analysis, the archaeologist may offer descriptive generalizations regarding his analytical categories; he may also offer some kind of synthetic statement, assigning categories to proposed events which presumably were the context in which the materials in question were produced. The end-product of this kind of analysis is normally comparison, either by verbal generalizations or summary statistics, among a series of sites in order to evaluate differences and similarities which are then used to reconstruct culture history or formulate statements about culture process.

One of the assumptions underlying such a procedure is that the analytical categories used are adequate and useful components of a nominal scale for measuring cultural differences and similarities. By definition the categories of a nominal scale are mutually exclusive and presumably part of an exhaustive scale which can accommodate all archaeological observations (see Siegel, 1956, pp. 21–30; Blalock, 1960, pp. 11–16, for a discussion of scales for measurement). One other linked assumption is that information tabulated by such a scale is additive (this is well documented in Thompson, 1956, pp. 42–45). Stated another way, the assumption is that culture consists of a single class of phenomena which can be accurately measured by our analytical units and about which accurate summary statements, based on those analytical units, can be made. When we compare the summary statements or statistics from a number of sites and observe differences or similarities, these are generally taken as indicators of degrees of cultural relationship.

We can criticize this kind of analysis on two grounds. First, it is highly questionable that the analytical categories used by archaeologists actually

measure a single class of phenomena; we would argue that they are measuring along several dimensions simultaneously, that culture is neither simple nor additive. Second, intuitively established analytical units, whose significance is not specified, can at best be of limited utility in testing hypotheses. For in hypothesis testing we must always be able to justify our observations as relevant measures of the variables identified in the propositions we have formulated (see Nagel, 1967, p. 10).

With respect to the first criticism—that culture is not additive and consists of more than summed traits—we would argue further that culture is a system of interrelated components. The archaeological record must be viewed as the byproduct of the operation of such a system, and any single facet of that record can be referred back to multiple variables or components of that system. The determinants which operated to produce one part of the archaeological record need not be, and probably are not, the same determinants which produced another part of the archaeological record.

We may explain changes or differences in certain attributes of artifacts or features in terms of variations in prehistoric economy; such explanations may be largely irrelevant for explaining variations in motor habits as documented in the same artifacts. If we treat both these kinds of variation as undifferentiated measures of cultural difference, we are scarcely getting reliable information about past cultural systems. This same criticism is applicable to consideration of a single attribute and also to generalizations about summed attributes. A single characteristic observed in the archaeological record might well be the compounded byproduct of a number of codeterminant variables.

An example of the confusion produced by treating independent variables as though they were one compounded variable can be seen if we take the case of measuring attributes of people rather than of artifacts. Let us assume that what we wish to explain is variation in human size, and the attribute we select as informing most economically on size is that of volume. We might proceed to measure a large number of people and even work out a taxonomy based on variation as measured by volume. The next step would be to attempt to explain variability in size and the distribution of size among human groups. We might investigate the degree to which size as measured by volume tends to covary with other variables such as environment, diet, disease, etc. Any such attempt would necessarily be doomed to failure, since at least two independent variables—height and weight—were being observed compounded into a single variable—volume. Someone who is 6½ feet tall and very thin might yield an identical value for volume as someone who is 5 feet tall and exceedingly stout. In studying the archaeological record, there is no reason to expect that our units of observation are, in their form and distribution, referrable to the operation of a single variable in the past.

The crucial question for archaeology is the relationship of our observations to

the operation of past cultural systems. What are we measuring when we apply various scales to the archaeological record: either nominal scales (typologies) or ordinal scales (stage classifications)? Do our stone tool typologies, for example, measure function or style, or do the attributes which define types involve two or more variables? At each juncture of explaining observations from the archaeological record, we must question anew to what variables operative in the past our observations refer. Any explanatory proposition must be reasoned in terms of relevance to the operation of the cultural system under study (see Spaulding, 1957, p. 87). These arguments of relevance frequently result in the modification of our analytical units and the generation of further analytical categories. This procedure insures the expansion of our knowledge of the past since it facilitates the testing of propositions. With the acceptance of a hypotheticodeductive method for archaeology and the use of a multiple-stage scientific procedure—observation and generalization, formulation of explanatory propositions, testing these against the archaeological data—it becomes evident that the analytical units employed in the initial stage may not be very useful during the final stages of testing. The sets of phenomena selected for observation, from the infinite number of possible observations, are not most profitably determined by the formal structure of the archaeological record itself. On the contrary, they are data which we must justify as relevant to the particular propositions advanced and as useful for hypothesis testing. A crucial role is thus given to the development of analytical techniques and to the generation of increasingly accurate analytical units for measuring cultural and environmental variables. During the past thirty years archaeologists have warned against the mixing of levels and inaccurate partitioning of archaeological deposits; the warning offered here is against the analytical mixing of variables and against the partitioning of our observational universe into irrelevant analytical units.

Relevance is established by reference to the propositions being advanced and by the theoretical context of those propositions. We can anticipate that progress toward achieving the goals of archaeology will be marked by continued refinement of the units of observation by which the archaeological record can be summarized and by the development of more accurate and less multivariate scales for measurement.

Conclusions

I have attempted to point out rather specifically what is new about the new perspectives. In doing so, I have made several points of contrast with more traditional approaches. I have noted that most archaeologists of whatever theoretical persuasion would agree on the triple aims of the discipline—

reconstruction of culture history, reconstruction of extinct lifeways, and the delineation of culture process. There are, however, major differences among archaeologists when it comes to theory and method, and it is argued that revamping traditional theory and method is essential for achieving any or all of the generally agreed-upon aims of the field.

The major methodological and theoretical points of contrast involve distinctions between cultural analogies and homologies, between culture viewed as a summation of traits and culture viewed as a system, between units of observation and units of analysis, between inductive and deductive approaches to the archaeological record. A basic underlying problem involves the use of scales of measurement. It was argued that traditional archaeological measures compound variables which probably operated independently in the past, and that a solution of the problem of measuring along several dimensions simultaneously must be reached in order to determine just what it is we *are* measuring. Despite remarkable advances in data collection techniques and in techniques of analysis, so long as the data from the past are considered within the framework of traditional theory, they can bring nothing new to bear on our knowledge of the past. It is a concern with the nature of knowledge, with the testing and verification of hypotheses, and with the relevance of questions asked that distinguishes much of the work in this book. We assume that the past is knowable; that with enough methodological ingenuity, propositions about the past are testable; and that there are valid scientific criteria for judging the probability of a statement about the past besides ad hominem arguments or "common sense."

The problems raised by the relationship of theory, method, and question-asking were elegantly dealt with fifteen years ago by Sherwood L. Washburn. Although Washburn was writing specifically about physical anthropology, his statement seems uncannily relevant for archaeology in the 1960's:

> The assumption seems to have been that description (whether morphological or metrical), if accurate enough and in sufficient quantity, could solve problems of process, pattern, and interpretation .... But all that can be done with the initial descriptive information is to gain a first understanding, a sense of problem, and a preliminary classification. To get further requires an elaboration of theory and method along different lines (Washburn, 1953, pp. 714–715).

The elaboration of theory and method which characterizes much of the recent work in archaeology consists minimally of two elements: First, the active search for understanding variability in the archaeological record—all of the variability and not just that judged *a priori* to be significant; second, an attempt to explain variability scientifically, rather than by conjecture or by "hunch." Some variability may be more apparent than real and may reflect sampling error,

partial erosion, redeposition, etc. Only with the self-conscious use of sophisticated method can this "noise" be factored out. Many kinds of variation will be shown to be the result of the normal functioning of internally differentiated cultural systems; others may document evolutionary changes within cultural systems. Still other kinds of variation may reflect changes in content within an essentially stable cultural system. In our search for explanations of differences and similarities in the archaeological record, our ultimate goal is the formulation of laws of cultural dynamics.

Many of the authors in this volume would agree that advances in achieving the aims of archaeology necessitate the enforced obsolescence of much of traditional theory and method, and thus many of the papers in this book are radical in the original sense of the word. If we are successful, many traditional archaeological problems will prove to be irrelevant, and we will see an expansion of the scope of our question-asking which today would make us giddy to contemplate. Despite a recent statement that one should not speak of a "new archaeology" since this alienates it from the old (Chang, 1967a, p. 3), we feel that archaeology in the 1960's is at a major point of evolutionary change. Evolution always builds on what went before, but it always involves basic structural changes.

In a rather caustic analysis of the field of archaeology, Spaulding has stated that apparently

> . . . truth is to be determined by some sort of polling of archaeologists, that productivity is doing what other archaeologists do, and that the only purpose of archaeology is to make archaeologists happy (Spaulding, 1953, p. 590).

We think that this statement was more appropriate in 1953 than it is today, and its inappropriateness today is a rough measure of the extent to which our field has advanced.

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