Major Aspects of the Interrelationship of Archaeology and Ethnology

by K. C. Chang

ANTHROPOLOGY HAS MOVED with amazing rapidity from the era of Boas and Kroeber into the age of the specialist. The omnifarious textbook in general anthropology is increasingly out of vogue, and great books in all of its fields are today as often as not put together by editors instead of single authors. This is particularly remarkable in the United States, where breadth of outlook had always been considered the hallmark of American anthropology. Many graduate and undergraduate curricula still cling to the tradition of an "anthropological whole" by insisting that degree candidates become familiar with physical anthropology, linguistics, archaeology, and ethnology; but a "general anthropologist," equally at home in all areas, is now generally regarded as a mythological hero whose like is no longer among us.

It is not my purpose here to endorse or to criticize this state of affairs, but it would be plainly unrealistic to insist that archaeology and ethnology—the latter term is used here in its conventional sense, including what is usually known as cultural or social anthropology—are logically interrelated simply because they are subdisciplines of anthropology. Archaeologists and ethnologists are two distinct groups of practitioners, each having its own tools of trade, its idiosyncratic formulations of problems and their solutions, its own parlance and jargon, its own journals, and professional societies. There are differences of opinion within each group, to be sure, but these can be ignored for the purpose of this paper. No matter how closely interwoven are their respective concerns in theory, the sheer mass of data and literature alone in each of these fields makes it increasingly difficult for any single person to become master of both. Intercommunication does take place, but it may become even rarer and more inconsequential as the trend of specialization continues and intensifies.

Although the merging of the two groups of scholars is neither practical nor advocated, several problem areas are of potential common interest. This paper is intended to be an exploratory survey of these areas, and it will not attempt to be comprehensive. Since I am an archaeologist in America, my points of reference will be reflected. Since the view is deliberately broad, rigid definitions will be kept to a minimum: when it is not necessary to define, it is necessary not to define.

TYPOLOGY

Like other disciplines that deal with variations and have to arrange their variables in order, archaeology cannot do without typology. Moreover, the variations and the variables that archaeology deals with are characteristically elusive; they are arbitrarily demarcated and defined, in either fluid or static situations, first by the natives and then by their students, unlike those commonly encountered in such fields as physics, which can be defined in terms of discreet elements with commonly accepted boundaries. Typology, therefore, must be the focal point of archaeological research.

If by typology we mean "a classification that is explicitly theoretical in intent as opposed to one intended purely as a descriptive categorization" (Kluckhohn 1960 : 134), then classification in archaeology is nearly always typological, because it is nearly always theoretical in intent. Archaeologists classify *in order to* reveal relevant information about the life and history of ancient peoples. Even a descriptive categorization can be explicitly theoretical in mtent as long as the archaeologist assembles it in the belief—right or wrong—that he is making a cultural inventory.

Are archaeological types discovered, or are they designed? This classic question remains current and meaningful among American archaeologists; we do not agree as to the extent to which cultural behavior can be recognized from artifacts. Rather than propose

KWANG-CHIH CHANG is Associate Professor and Associate Curator in Anthropology at Yale University. He was born in China in 1931 and received his B.A. at the National Taiwan University (Taipei, Taiwan, China) in 1954 and his Ph.D. at Harvard in 1960.

Chang has published on a wide range of topics on the archaeology of the Far East, ancient culture and society of China, and archaeological method and theory. His most recent book on the latter is *Rethinking Archaeology* (New York: Random House, 1966).

The present article submitted to CURRENT ANTHROPOLOGY 14 XII 65, was sent for CA☆ treatment to 52 scholars of whom the following responded with written comments: Lewis R. Binford, Bernhard Bock, Alois Closs, George L. Cowgill, Samir Ghosh, Gutorm Gjessing, Shirley Gorenstein, Karl J. Narr, Carroll L. Riley, Philip E. L. Smith, and Julian H. Steward. The comments written for publication are printed in full after the author's text and are followed by a reply from the author.

a clear-cut solution here, I ask two questions: (1) Is it possible and fruitful to reconstruct culture and history by classifying artifacts without recognizing, or satisfactorily demonstrating, cultural behavior? (2) Is there a recognizable logical and causal relationship between the physical properties and contexts of the artifacts and their relevance to the behavioral and cognitive systems of the makers and users? A negative answer to the first question would naturally make the second more imperative, but an affirmative answer does not cancel the second. These are two independent inquiries that pertain to different aspects of the archaeological method. Neither of the questions can be answered without rigorous research into ethnology, and the archaeologist's work along such paths may prove to be of considerable ethnological interest. Let us consider the second question first.

The criteria for the classification of potsherds by their color—black, white, buff, red, gray, black-on-white, brown-on-red, and so on, as specified by some color scale—seem to be entirely objective. But Conklin's (1955) study of Hanunóo color categories makes it clear that as far as color is concerned "sensory reception" and "perceptual categorization" are two quite different things:

> Under laboratory conditions, color *discrimination* is probably the same for all human populations, irrespective of language; but the manner in which different languages classify the millions of "colors" which every normal individual can discriminate *differ*. Many stimuli are classified as equivalent, as extensive, cognitive—or perceptual—screening takes place. Requirements of specification may differ considerably from one culturally defined situation to another (Conklin 1955:340; italics in the original).

Each prehistoric community is surely a culturally defined situation, but can the archaeologist discover its cognitive categorizations relative to color? Color is but one of many cognitive systems for the archaeologist to consider.

Ethnographers of late have given notable attention to this aspect of research, and their basic tool is a culture's terminological systems—its systems of words (e.g., Frake 1961, 1962; Conklin 1962*a*, *b*). Words, alas, are hardly ever at the archaeologist's disposal. Even in a rare protohistoric situation where words can sometimes be matched against things, seemingly insurmountable obstacles loom, owing to the lack of informants. In a very suggestive illustration of the "important difference between the analysis of semantic structure and the presentation of an arbitrary arrangement," Conklin (1962*a*) compares two arrangements of monetary units used in the United States, one a folk classification (penny, nickel, dime, etc.), and the other an arbitrary key—one that an archaeologist could have designed—based on physically observable differences. He concludes,

> If we are concerned with the way in which a set of categories is cognitively interrelated contrastively and hierarchically, detailed examination of physically observable differences in an array of objects cannot—by itself—provide decisive answers to questions of cognitive distinctiveness (Conklin 1962*a*:90).

The implication of this statement for archaeology is evident: Not every arbitrary key—based upon no matter what objective physical criteria—is cognitively significant, and most are not. From experience, however, archaeologists know that of the countless ways of classifying their objects, some produce more significant and meaningful results than others. "Significant" and "meaningful" can be pragmatically defined. An archaeologist believes his classification to be significant and meaningful when and only if it *works*—that is, when and only if he can interpret his material, with reference to his knowledge of the larger context of his site and culture, more consistently by means of one classification than by another. Since his interpretation is inescapably in the realm of man's behavior, its facets and its history, the question arises whether a classification that agrees with the cognitive system of the makers of the artifacts would, for a variety of purposes, invariably work better than one that does not. The answer has to be affirmative as long as one recognizes causal relations between cognition and behavior and history of behavior. In fact, this has been the implicit assumption of the archaeologists:

> By the term "mode" is meant any standard, concept, or custom which governs the behavior of the artisans of a community, which they hand down from generation to generation, and which may spread from community to community over considerable distances.... Not all the attributes of the artifacts are indicative of modes. Some attributes will instead express personal idiosyncracies of the artisans. ... Other attributes fall within the realm of biology, chemistry, or physics rather than culture.... Analytic classification, then, must single out modes, which are cultural, and exclude those traits which are purely biological, chemical, or physical (Rouse 1960:313-14).

In dealing with new materials from an area with which he is familiar, an archaeologist usually arrives, without much difficulty and hesitation, at a classification that works. In other words, archaeological classifications tend to be cognitively significant—or so the archaeologists presume. This is a point that deserves close attention by ethnologists.

To explain, we may make the following two assumptions: (1) Variations can always be reduced to minimal units that *exist* in the physical world, but these are hierarchically and contrastively grouped differently in different cultural situations. That is, even though cognitive systems are culturally determined, they do have absolute, however qualified, physical foundations. Therefore, in theory, it is always possible to recognize a cognitive system through observable physical differences by recognizing the meaningful hierarchies and contrasts. (2) When informants are not available to provide information on the hierarchical and contrastive meaningfulness of the variations, such information can be recognized in a context of change, i.e., in the context of history. There are, to be sure, countless ways in which variations can be contrastively and hierarchically structured, but those that are cognitively significant in specific cultural situations do not emerge, in time or in space, at random; they form a part of the heritage of the behavior of the artisans "which they hand down from generation to generation, and which may spread from community to community," as stated by Rouse. Therefore, cognitively significant attributes stand out in a patterned manner in the archaeological record *in the long run*.

In fact, this partially answers the first question posed above. It is implicit in the archaeologist's method that all classifications in archaeology that are workable in the realm of cultural and historical interpretation are cognitively meaningful, although the workability is always relative and subject to proof and the subjective elements can only be minimized by the continuous widening of the sphere of consideration. It is thus also implied that designed types are discovered types for the period and in the area for which they prove to be workable.

Rather than claim that no work remains to be done in archaeological typology, I merely point out that archaeologists and ethnologists have a common area of interest in the study of, and approach to, cognitive systems. That categorization as a theoretical pursuit occupies a central place in archaeology, on the one hand, and in ethnology (e.g., in ethnoscience, structural linguistics, kinship, and folklore), on the other, is both evident and natural. It is thus inexplicable that with rare exceptions students of these areas of study have seldom come to grips with each other's problems and potential contributions.

ANALOGY

Unlike typology, in which there is conceptual reciprocity between archaeology and ethnology, analogy is largely a one-way street. Analogy is the principal theoretical apparatus by which an archaeologist benefits from ethnological knowledge.

"Interpreting by analogy" is, according to Ascher (1961:317), "assaying any belief about non-observed behavior by referral to observed behavior which is thought to be relevant." He cites the claim that "archaeology depends on ethnographic data for interpretation" (p. 324). This dependence is explicated by Thompson (1958:5):

> The archaeologist who formulates an indicated conclusion is suggesting that there is a correlation between a certain set of archaeological material percepta and a particular range of sociocultural behavior. He must test this conclusion by demonstrating that an artifact-behavior correlation similar to the suggested one is a common occurrence in ethnographic reality.

These quotations are adequate indication that in analogy lies the most generally recognized interrelationship of archaeology with ethnology (Hole and Heizer 1965:211-14). No archaeologist is worth his salt, it can almost be said, unless he makes an analogy or two in every monograph he writes. The examples in literature cannot be enumerated.

The first and most obvious (the "straightforward") kind of analogy is found in folk-culture study in the Old World (Clark 1951) and the "direct historical" approach in the New (Steward 1942). Where there is demonstrable *cultural* continuity from the prehistoric to the ethnographic, as in the New World (as well as in the Pacific islands, India, the Near East, and much of Africa), the archaeological reconstruction of late prehistoric sites is often greatly aided by ethnological knowledge. One easily recognizes the same stage setting when an old play is staged at another theater. Cultural and social change sometimes alters, slightly or drastically, the correlation between artifacts and behavior, but this is a matter of detail rather than of principle.

A second kind of analogy has been called "general comparative" (Willey 1953*a*:229). In contrast to the specific historical analogy, the second depends on artifact-behavior correlation that "derives from a pattern of repeated occurrences in a large number of cultures" (Thompson 1958:5). Since the applicability of the first kind of analogy is obviously limited, and the second kind is thus of prime concern to archaeologists both in theory and in practice, one might have assumed a theoretical sophistication in analogical problems among archaeologists. Ascher, who made a point of finding out, concluded otherwise (1961 : 322):

> It is apparent that there is no general agreement on the new analogy, either in theory or practice. . . . If it were not for the fact that analogy in archaeological interpretation has suffered chronic ambiguity since the nadir of classical evolutionary simplicity, an impasse could be said to exist.

Ascher's prescription: More ethnology, but of a special kind:

> Every living community is in the process of continuous change with respect to the materials which it utilizes. At any point in its existence some proportion of materials are falling into disuse and decomposing, while new materials are being added as replacement. In a certain sense a part of every community is becoming, but is not yet, archaeological data. The community becomes archaeological data when replacement ceases. . . . The observational fields of ethnology and archaeology overlap on that proportion of a living community which is in the process of transformation It is the study of this very special corpus of data within the living community which holds the most fruitful promise for analogy in archaeological interpretation (Ascher 1961:324).

There is no doubt that Ascher has brought to our attention an area of study of great interest and import that has been heretofore neglected. This, nevertheless, is a problem distinct from the main purpose of analogy: reconstruction of the *living*, *before* as well as *during* and *after* the "transformation." The process of transformation from ethnology to archaeology can only be of archaeological interest; the pattern of artifact-behavior correlation, of greater consequence to archaeology, is in itself an ethnological problem.

The microcosmic regularities of culture, in essence accounted for by the intercausality of diverse elements when integrated, are regularities of variations and the variables. Each element, under study, is, as pointed out by Thompson (1958:6), a type and is phonemic in nature, abstracted from variables which are comparable to allophones (to use an old-fashioned linguistic analogy). The more elements one considers for each complex of relations, the more specifically their respective allophonic realities can be characterized. In ethnology, the basic method of working out such regularities has been variously called the sociological or structural principle, the limited generalization, and the concomitant variation. To place his analogies on a firmer ground, the archaeologist asks this of the ethno-

logist: In your network of variations of each delineable intercausal area of behavior, will you please consider, include, and specify as many physically observable and imperishable elements as you can? The more such elements are available, the greater number of unobservables can be restored, and the more specific and realistic characterizations they can be given. This, of course, is easier said than done, but there is no question that such attempts will prove rewarding in providing some concrete and demonstrable guidelines for the archaeologist's analogies.

For example, slash-and-burn cultivation as a behavioral network includes many manual activities, social relations and interactions, temporal sequences, and utterances that can never be archaeologically retrieved. But such activities and interactions always take place in spatial loci and contexts and with reference to material instruments and symbols that come to be especially adapted to such purposes, and such spatial contexts and instruments could survive in the right sequence. What distinguishes them from spatial contexts and instruments for other kinds of cultivation processes? If there is more than one kind of slash-and-burn cultivation process, do variations leave impressions on their material correlates? What is the relationship between residential patterns and kinship patterns? What is indicated by the horizontal distribution patterns of minute variations in contemporaneous ceramics in the light of ethnological knowledge of the potter's social status and social roles? Once one begins to think along such lines, the possibilities are alarmingly unlimited. Bride-prices are usually material goods; political prestige has visible symbols; and a medicine-man leaves his paraphernalia bag in some houses in a village but not in others. Many small things that have to do with location, form, association, and sequence of seemingly intangible or even unobservable behavior are occasionally taken for granted and left unrecorded by the ethnologist, but these are precisely the kind of things on which an analogical archaeologist must depend. Should the ethnologist observe and record these data so that they might someday be of some use to an archaeologist? Or should he do so in any event? Or should there perhaps be a branch of archaeology (ethnoarchaeology) to take care of such things? The solutions to these problems are left to anyone who is willing to attempt them.

In the absence of such networks of tight intercausal relationship, or at least concomitant occurrence, of behavior-cum-physical-manifestation, the archaeologist must continue to make analogies, and on a broader basis, resulting in reconstructions of mere possibilities. Indeed, in a broad sense, archaeological reconstruction *is* analogy, with or without explicit ethnological recourse. To claim any information at all, other than the stone or the potsherd that is actually discovered, is necessarily to presume knowledge of man and culture in general and to assume the existence of cultural regularities, however broadly conceived. Since each archaeological object and situation is unique, every archaeological reconstruction is analogy based upon a number of such presumptions and assumptions. The ethnological recourse does not make analogy possible; it only renders its results probable or even scientifically true.

RECONSTRUCTION OF SOCIOCULTURAL SYSTEMS

Archaeology, along with linguistics, has been characterized as the most "culturological" of the culturologies (Nicholson 1958). To resort to so-called correlation, as is done above in regard to analogy, may seem to be reductionistic. If so, and if reductionism is sin, so be it. An archaeologist, no less than his ethnologist colleague, has to take a culturological position.

In an article entitled "Archaeology as Anthropology," Binford (1962:217-18) makes some accusations and then follows with some suggestions:

> Archaeologists tacitly assume that artifacts, regardless of their functional context, can be treated as equal and comparable "traits." Once differences and similarities are "defined" in terms of these "traits," interpretation proceeds within something of a theoretical vacuum that conceives of differences and similarities as the result of "blending," "directional influences," and "stimulation" between and among "historical traditions" defined largely on the basis of postulated local or regional continuity in the human populations.
>
> I suggest that this undifferentiated and unstructured view is inadequate, that artifacts having their primary functional context in different operational sub-systems of the total cultural system will exhibit differences and similarities differentially, in terms of the structure of the cultural system of which they were a part. Further, that the temporal and spatial spans within and between broad functional categories will vary with the structure of the systematic relationships between socio-cultural systems. Study of these differential distributions can potentially yield valuable information concerning the nature of social organization within, and changing relationships between, socio-cultural systems. In short, the explanation of differences and similarities between archaeological complexes must be offered in terms of our current knowledge of the structural and functional characteristics of cultural systems.

Cultural items under comparison can be considered either within or without their context, and their methodological problems will be discussed later. The importance of providing that context—the reconstruction (or formulation, if one dislikes the word *re*construction) of the sociocultural systems—cannot be overstated, and these remarks of Binford's reiterate in effect the views of Childe (1936), Tallgren (1937), Steward and Setzler (1938), and Taylor (1948).

Even if further elaboration of principle were superfluous, pertinent questions still should be asked regarding the specific procedures in which sociocultural systems could be reconstructed or "structured." Let us first list some of the questions that are begging for answers:

—In what manner can cultural universals as against cultural relatives be said to fossilize in the archaeological record?

—In the formulation of particular sociocultural systems and subsystems, is there only one way, the correct way, or are there alternatives?

—What is a "sociocultural system" in archaeology? Is it a list of archaeological finds? An abstract model linking actual artifacts together? A bundle of facts, classes, and inferences?

—How much of the reconstructed system must depend on chance of preservation and discovery?

—Are archaeological variations within the site significant only contrastively, or have their physical attributes their own inherent significance?
—Can sociocultural reconstructions be interpolated in the same culture at different sites?
—Can "use" and "function" be differentiated in archaeology? Is it meaningful to do so? Can functional reconstruction be accomplished without certain knowledge of all the uses?
—Must reconstruction depend upon an extreme deterministic viewpoint? Must one assume circular intercausality to avoid being—or being labeled—extremely deterministic?

Many of these sample questions are obviously ethnological, and I am not sure that they do not involve ethnological controversies. If practical archaeology must depend upon answers to these questions and others of the same nature for its guide, the notion, then, cannot be tenable that archaeology is a more solid, factual, unsubjective, and dependable discipline than is social anthropology. Archaeological reconstruction of sociocultural systems is model-construction, governed by theory and guided by understanding of the human psyche and human behavior. In short, "archaeology is the ethnography and culture history of past peoples" (Kluckhohn 1957:46), and its cornerstone is analogy.

More than 14 years ago, Willey, remarking on "What Archaeologists Want" from ethnology, listed the following "areas of specific information where reconstructions of context are attempted" as those in which "we need facts bearing upon the use and background of material objects or features":

> The first ... I have listed as ecology. For instance, how do a particular people exploit an environment? How do they modify it—knowingly or unknowingly?
>
> A second area would be subsistence techniques and their relations to artifacts or features in prehistoric context, social participation in subsistence activities, or cultural elaboration and integration of subsistence activities.
>
> Third is settlement: houses, construction methods, uses or functions of house types or other buildings, the number of people in a community, general demographic problems, the cultural significance of spatial arrangements of buildings of different types in a community, or the arrangement of one community with relation to another community.
>
> Fourth, technology: methods and techniques of manufacture; uses of specific material culture forms; social divisions of labor—for example, do captive women or women married out of one group into another make pottery?—which would help us in the matter of diffusion of pottery styles; social and class divisions in usages and possession of material; priest class or shamanistic paraphernalia.
>
> Fifth, art: ceremonialism, burials, beliefs in the afterlife. We are interested in the context of art on socioeconomic or sociopolitical levels, as well as in interpretations placed on the particular iconography. How is style or ceremonialism integrated into the culture?
>
> Sixth, we might consider, under problems of development, diffusion, and acculturation, what ethnology could offer to archaeology. The ethnologist is hampered by a short-time view, but his observations on acculturation may provide some very helpful analogies to project the present back into the past. ... Under this, we could look for acceptances or rejections of innovations, as noted by the ethnologist, perhaps in some cases reflecting deep-seated or ancient attitudes of a cultural tradition. ... How do ideas spread between communities, tribes, regions? What are the means of diffusion, trade, imitation, exogamous marriage, etc.? ... Can the ethnologist help us understand why technology or certain broad types will diffuse much more rapidly and easily than specific art styles? We might even ask what happens to the individual during periods of change or relative stability, if this can be viewed by the ethnologist. This may be perstonality and culture, but the reconstruction of ancient personality types is interesting to the archaeologist.
>
> Seventh, the problem of cultural types and models: for instance, Rouse, some years ago, had a type concept in pottery. He tried to achieve or recapture what was the original model or type in the mind of the prehistoric artist. Perhaps the ethnologist could find out how the craftsman in a particular group felt about a style or type, if he in any way verbalized or conceived of types and realized that the modes of a certain type were changing. Culture types or models also bear on the question of culture units. Did various communities of an area consider themselves politically or culturally linked? In general, how do their concepts of such divisions, on the present time level and political and cultural divisions, check with what we can find from prehistory (Willey 1953*a*)?

These remarks serve well as a checklist to determine to what extent the archaeologists have now received what they wanted a decade and a half ago from the ethnologists.

Instead of reviewing each of these areas and indicating what has been achieved and what has not, I would like to take an over-all view of the methodological problem with reference to sociocultural reconstruction as a whole. I assume that there are various possible approaches in social anthropology for the formulation of structural models and further, that some of these approaches are archaeologically practical or feasible, but others are not. (For instance, semantic analysis and interaction theory can be ruled out immediately.) I suggest that the most feasible and fruitful approach for the archaeologist in reconstructing the sociocultural system is to isolate social groups and to characterize their activities. An archaeological sociocultural system can be construed as a model of a series of such groups of various kinds and at various levels, ordered hierarchically and contrastively and integrated with a series of activity classes. For if, as Eric Wolf (1964:68-69) says, "a new archaeology, freeing itself from both the collector's madness of obtaining show pieces and from the infantile wish to restore the lost splendor of ruins long covered by earth or jungle, [turns] to the recovery of entire settlements of past populations," then it must "look beyond the mechanical gathering of isolated bits of material culture to the reconstruction of past communities, attempting to grasp the archaeological equivalent of the ecologists' group and the social anthropologist's organization-bearing unit." I have proposed a methodological procedure "to identify and characterize the social groups of archaeological cultures. The a priori assumption is that one must look at archaeological sites as local social groups instead of as cultures or phases. Cultures are fluctuant, but social groups are clear-cut" (Chang 1958:324). This plea echoes Childe (1936:3):

> The study of living human societies as functioning organisms has revealed to archaeologists this approach to their materials. ... The culture is not an a priori category elaborated

in the studies of philosophers and then imposed from outside upon working archaeologists. Cultures are observed facts. The field-worker does find specific types of tools, weapons, and ornaments repeatedly associated together in graves and habitations of one kind and contrasted with the artifacts found in graves and settlements of another kind. The interpretation of the observed phenomenon is supplied by ethnography. The traits of a culture are thus presented together to the archaeologists because they are the creations of a single people, adjustments to its environment approved by its collective experience; they thus express the individuality of a human group united by common social traditions. With this idea prehistory vindicates its character as a human, in contrast to a natural, science.

It seems quite feasible in archaeology to identify "the individuality of a human group united by common social traditions." One approach is the categorization of dwellings and other kinds of architecture that provide the loci for group activities. This is included in so-called settlement pattern studies (e.g., Willey 1953*b*, 1956; Chang 1958, 1962). Whether house remains are available or not, social divisions within a prehistoric community can be categorized by grouping artifacts and/or attributes of artifacts in relation to loci or to assemblages. Lothrop (1942:5) and Karlgren (1937:91-92) have applied this principle to the division of art styles within a single site, and Longacre (1964) and Deetz (1965) have attempted to do the same thing in greater detail with minute ceramic attributes (see also Smith 1962; Foster 1965). Clark's (1957) study of flints is of the same kind. What can be done with variations within a prehistoric community can also be done with the site's culture as a whole in relation to and contrasting with a larger sphere of interaction. A grouping of sites is a hierarchy of classes of sites; sites as variations grouped structurally take on sociological significance at a higher societal level, and the variations within this larger sphere bespeak its range and kinds of behavior. At the broader end of the spectrum, the archaeologists, inspired by social typology, unabashedly classify whole civilizations (Coe 1961; Willey 1962). These methods, with social groups delineated by the attributes of the remains as the theoretical units of departure, are obviously ethnologically suggested, and their operation must be sharpened with precise ethnological knowledge and techniques. On the other hand, such studies will provide information for the ethnologists. Can information additional to what is already available in living societies be thus obtained when the model of archaeological social grouping necessarily depends upon knowledge provided by existing societies? The answer must, perhaps, be negative until archaeological techniques are better and more self-contained; but archaeological reconstruction will at least be able to supply a greater variety of models of considerably greater time depth.

Variations indicating group behavior must be sharply distinguished from those indicating individual behavior. Archaeologists no less than historians must emphasize the uniqueness of each individual act; but patterns of group and patterns of individuals are not dichotomic, and a broad range of behavior can also be identified within a group (cf. Oliver 1958:803). To take the decorative art of a prehistoric community as an example, what are the stylistic norms or ideals, and what are the deviations and variations? Is a distinction between norms and deviations meaningful? Are "norms" the common denominators of all the variations within a class, and are they in agreement with the archaeological types or modes? These, together with the problems raised above concerning artifact variations and cognitive systems, must provoke some thought among archaeologists and ethnologists alike.

Logically related to group identification but potentially independent of it is the archaeological identification of "activity systems" as formulated by ethnologists (e.g., Howard 1963; Nash 1964). "Instead of conceiving of a society as having a social structure," Howard (1963:410) suggests that "we conceive of social behavior as being structured by participation in given activities within which behavioral choices (decisions) are regular and predictable." Activities can be physically indicated by loci and instruments, and behavioral choices materialize in artifactual variations. The archaeologists can thus structure their types around a series of activity systems such as subsistence, domestic, technological, and other behavioral categories (or their subdivisions). While such works as *Notes and Queries* (RAI 1951) and *Outline of Culture Materials* (Murdock *et al.* 1961), which purport to provide a universal categorization of culture materials for ethnographers, are indispensible to archaeologists for designing such activity systems, these designs must primarily be determined by the nature and preservation of their material in the field.

It remains to be reiterated that the reconstruction of sociocultural systems described above can be greatly aided by direct historic or ethnological analogy; the best examples can perhaps be found in the American Southwest (Parsons 1940; e.g., Di Peso 1956).

PROCESS AND THE COMPARATIVE METHOD

"The interest of modern archaeology," says Kluckhohn (1957:44) "is focused upon helping to establish the principles of cultural growth and change." Of all anthropological disciplines, archaeology alone uses "time" as a fact rather than as a reconstructed or restored dimension.

> By extending in time as well as in space the comparisons that can be made as to how different peoples have solved or failed to solve their problems, the chances for testing scientifically certain theories about human nature and the course of human progress are much improved. ... As the archaeologists inject chronology into a confusing mass of descriptive facts, one gets a sense not only of the cumulative nature of culture but also of pattern in history (Kluckhohn 1957:45, 41).

The most obvious application of archaeological results to ethnology in matters of cultural process concerns the history of material culture elements and the history of ethnic elements and entities. Equally understandable is the fact that recent contributors to the theory of cultural evolution either are themselves archaeologists (Childe 1951; Braidwood 1960; Rouse 1964) or have utilized archaeological results as the backbone of an evolutionary scheme conceived from ethnological theories (Steward 1955). These need not be elaborated but cannot be overemphasized.

More problematical is the matter of social history. Social evolution is interpretation of sociocultural systems seriated temporally, or the sum of archaeological variations of a social and societal nature oriented according to a time scale. Ethnologists can construct quasi-static systems without regard to difference in time; they can also conjecture about what might have happened in the past, on the basis of their understanding of the mechanisms and patterns of cultural and social change. But they can never empirically verify a historical hypothesis without archaeological recourse. S. F. Nadel says that "archaeology by itself can never be social history" (1957:7). The collateral is also true: ethnology by itself can never be social *history*. The assistance ethnologists offer archaeologists in this connection really serves their own interest.

Patterns of historical process cannot be revealed without comparative study. No matter how systematic an archaeologist's analysis, he must compare finds from different times and places. "One-people ethnologists" there may be, but "one-site archaeologists" are only a joke. Occasional comparisons are undertaken at a higher level, but such comparative studies must frequently proceed on the basis of single traits.

Service (1964:364) has recently reminded us that "There should ... be ways in which archaeological theory and method could profit from greater attention to ethnological fact." Since "historical reconstructions in archaeology are based on comparisons of traits and attributes of traits representing different cultures," the archaeological method apparently centers in the comparative study of similarities:

> Similarities in two lists of traits could suggest any of three distinct kinds of occurrences. (1) The traits might be similar because they represent two descendant cultures of one homogeneous parent culture; ... (2) Similarity may also have been created in certain respects by diffusion or trade between two otherwise historically distinct societies. (3) Some similarities may be chance parallelisms or adaptive convergences to like environmental or historical influences.
>
> A major methodological problem is revealed here: How can comparisons of traits and attributes be made so that a conclusion is reached as to which of the above possibilities is responsible for the similarities?

He suggests that the answer lies in a better understanding of the nature of culture:

> Culture is composed of conceptually isolable elements—i.e., traits and trait complexes—and some kinds of these change form, diffuse, and appear or disappear at different rates from others; that is, they are differentially responsive to particular kinds of conditions (Service 1964:365).

Accordingly, these traits are placed in three categories: (1) "Certain characteristics of art styles and rituals ... seem to persist among a particular people through long periods in their history, even under greatly changed conditions of life." (2) Some other features, on the other hand, are "rapidly borrowed or traded from one people to another." (3) Still other aspects of culture are "likely to respond functionally or adaptively to varying circumstances" (p. 356). This tripartite scheme is thus essential for the "discovery of phylogenetic relationship, diffusional continuum, or independent parallelism" (p. 375), for "there are aspects or parts of culture that in fact are best seen as diffusible shreds and patches, others as historically stable, and others as functionally adaptive" (p. 374). But Service admits that "we do not know enough about this yet, certainly not enough to take one or the other side so firmly as we conventionally do," and therefore, suggests that "one of the most appropriate tasks for cultural anthropologists today should be to conduct some studies designed to reveal more clearly and precisely how culture actually does behave" (p. 374).

In a similar attempt, as yet unpublished (Chang 1960), I have called cultural elements "stylistic" when they are historically stable and culturally idiosyncratic and "ecosocial" when they are adaptive and recurrent. Historical relationships based upon comparisons of stylistic elements are considered to be more reliable than those based upon comparisons of ecosocial elements.

Such categorizations of cultural elements according to their stable, diffusible, and adaptive nature for comparative purposes are apparently valid on a general level, but they have their limitations in archaeological application. It is doubtful, for instance, that cultural elements are inherently classifiable in these terms so that the archaeologists can make clear-cut distinctions in each case. Cultures, either as abstractions or as realities of collective behavior, do not behave; it is people who behave, who make choices among the available alternatives. In other words, the tripartite classification of archaeological material is not inherent in the traits or complexes themselves, but is determined by the roles they play in the sociocultural system to which they belong. In order to determine the comparability of traits and complexes and to specify their nature with reference to historic relationships, reconstructions of sociocultural systems must take priority to provide a functional context.

POSTSCRIPT

How does one recognize an archaeologist as an archaeologist? What are the symbols of his trade? In the field he uses trowels, spades, transits, and plane-tables. He is surrounded in his laboratory by glues, brushes, shelves full of sherds, stones, and bones, microscopes, light-tables, and tracing papers. He holds conferences with geologists, biologists, and "archaeometrists," who supply him with "hard" scientific data. He writes articles, books, and monographs with solid titles. Thus, he can be distinguished at a glance from his ethnologist colleague, with whom he sometimes exchanges ideas and information about type, analogy, social reconstruction, and perhaps evolution.

Despite his scientific disguise, the archaeologist is a humanist. In his classic essay, Sir Mortimer Wheeler reminds us that "as archaeological scientists, our subject is *Man* ... We dig up mere things, forgetful that our proper aim is to dig up *people*" (1950:122, 129). Rouse (1965:2) has recently reiterated that "we now dig sites not only to obtain artifacts but to learn all we can about the peoples who lived in the sites." In short, archaeology is a learning about peoples, namely, *ethnology*. It is "a method of reconstructing, from scant remains, the ethnology of a people now gone, and of whom we can learn only from such of their

remains as have endured the test of time" (Smith 1899:1).

From this viewpoint, the title of this article is tautological. But, as stated in the beginning, the identification of these two fields of learning is one of strategy, not of tactics. A cultural whole may be characterized as peoples and things in interactional association; ethnologists usually start from the people, whereas archaeologists must start from the things. This tactical difference calls for separate methodological systems, and to affirm their logical interrelationship or to identify simplistically the present with the past is only to reiterate the obvious.

Archaeologists and ethnologists will have to continue on their separate ways, even though a complete understanding of man and his culture must be a summation of their respective findings. But a methodological system based on the people provides useful experience for the study of the things, and vice versa. To say that ethnology and archaeology depend on each other may be extreme; but it is clear that neither can profit from extreme specialization.

Abstract

Anthropologists in the United States are faced with the issue of specialization *vs.* generalization; simply to say that the archaeologist and the ethnologist are both anthropologists no longer suffices to convince either of them that he has a lot to learn from, and contribute to, the other. Recognizing this practical fact, this paper nevertheless attempts to draw attention to some major areas of presumably common interest: typology, analogy, the formulation of sociocultural systems, and process, and the comparative method.

Comments

by LEWIS R. BINFORD☆

Los Angeles, Calif., U.S.A. 9 IX 66

In a short comment like this, it is impossible to analyze in detail the arguments set forth by Chang. Since I am in basic disagreement with the propositions on which his discussion is predicated, I shall address myself to these.

Chang states that the taxonomies with which we work should agree in their formal characteristics with the cognitive systems of the producers of the cultural elements under study. His argument can be summarized as follows: (1) Classification "*must* single out modes" (italics mine), the latter being "any standard, concept, or custom which governs the behavior of the artisans of a community." The role of taxonomy in archaeology is seen as the expression of identified "modes," or norms and values, held by extinct people. (2) Modes, or norms and values, can be abstracted from the patterned and repetitive occurrence of attributes in a population of artifacts. Chang's argument is in line with Herskovits' view of culture (1955: 354): "The very definition of what is normal or abnormal is relative to the cultural frame of reference." Therefore, Chang appears to be arguing that our taxonomies should be compatible with the cognitive frame of reference of the people under study; then and only then are our taxa meaningful. (3) A typology is judged successful if it works. How do we know it works? It works if with it we are able to identify modes. How do we identify modes? In the recognition of patterning in the archaeological record. Therefore, all workable classifications are "cognitively meaningful," although their workability is always relative.

Chang's position is an archaeological version of extreme cultural relativism, which, if carried to its logical conclusion, would deny to archaeology the possibility of becoming an objective, comparative science. As Bidney has pointed out (1953:425): "... the fact of cultural variations in historic cultures does not imply the absolute value of cultural differences and the obligation to respect them."

I maintain that given the theoretical tools available to us we may: (1) ask certain questions about the past or about the operation of cultural systems generally; (2) develop classifactory criteria which inform on variables believed relevant to the questions being asked; (3) investigate the archaeological record in terms of these criteria and draw valid conclusions, irrespective of the degree of conformity between our criteria and the cognitive systems of the manufacturers of the artifacts we study. It is, in fact, quite unlikely that the cognitive systems of extinct peoples would be in any way adequate to, or relevant for, modern scientific investigation of the processes responsible for observed differences and similarities between cultural systems.

I further question the utility of the general normative frame of reference in which Chang's arguments are cast (see Aberle 1960 and L. R. Binford 1965 for general criticisms of normative theory). If we were to attempt to work within the frame of questions Chang seeks to answer, we would be forced to explain cultural differences and similarities in the archaeological record in terms of different modes (norms) held by extinct peoples. The value of Chang's position would lie in explanation of the past in psychological terms. In this case, we would be palaeopsychologists, and our training equips us poorly for this role. If explanation is sought in cultural or ecological terms, direct linkages can be made between the relevant variables without translating them into ideational terms. Ideas are cultural and should therefore vary in functional congruence with other cultural elements. Since they are cultural elements, they can never be cited as the independent variables bringing about change in a system of which they are a part, unless one is willing to say that basic biological differences determine variability in ideas independently of their cultural setting. This position was rejected long ago in considering the archaeological remains of anatomically modern man.

I disagree with Chang's view of the aims of archaeology:

> Is it possible and fruitful to reconstruct culture and history by classifying artifacts (p. 227).
>
> The more such elements are available [material referents in ethnographic description] the greater number of unobservables can be restored, and the more specific and realistic characterizations ... can be given (p. 229).
>
> Indeed ... archaeological reconstruction is analogy (p. 231).
>
> The importance of ... the reconstruction of the sociocultural system cannot be overstated (p. 230).
>
> I propose a methodological procedure to identify and characterize the social groups of archaeological cultures (p. 231).
>
> It [archaeology] is a method of reconstructing ... the ethnology of a people now gone (p. 233).

If the reconstruction of the past were the major aim of archaeological investigation, then archaeology would be doomed to be a particularistic, nongeneralizing field. Our taxonomies would be as numerous as the different historical entities identified, and our

analytical tools would be geared to the explication of those aspects of prehistoric life which our current values and biases deemed meaningful to contemporary audiences. This is not to say that reconstruction and characterization of the past do not have their role in the general education of the public; they may also serve to make significant contributions to the intellectual climate of today, as suggested recently by Clark (1966*b*:99). I maintain, however, that they are not the ultimate aims of archaeology. I have elsewhere stated (1962:217) what I believe these aims are: the explication and explanation of cultural differences and similarities. Chang is asking for explication of the past; I would claim that archaeology is capable of more—explanation. Spaulding (1966:4) has argued that the reconstruction of past events does not constitute explanation, that there "is no such thing as 'historical' explanation, only the explanation of historical events." We must demand of ourselves concepts and methods which go beyond the mere reconstruction of the past.

I also disagree with Chang on the roles of analogy and of ethnographic data in archaeology. On analogy, he says:

> Analogy is the principal theoretical apparatus by which an archaeologist benefits from ethnological knowledge (p. 229).
>
> In short, "archaeology is the ethnography and culture history of past peoples" (Kluckhohn 1957:46) and its cornerstone is analogy (p. 230).
>
> Since each archaeological object and situation is unique, every archaeological reconstruction is analogy based upon a number of . . . presumptions and assumptions (p. 230).

In his discussion Chang makes no distinction between theory, generalizations of abstract qualities, arguments by enumeration, and arguments from analogy. This is one of the old confusions of anthropology (see Buettner-Janusch 1957:320-21 on Boas). Even if we were to admit that archaeological arguments always include an analogical component (and I am not convinced of this), this does not make these arguments analogies (L. R. Binford 1966*a*). The basic form of archaeological argument, or of any argument which seeks to formulate general propositions, should be logico-deductive. From a set of premises, we can frame testable hypotheses whose confirmation will lend support to the postulates and assumptions (premises) on which the hypotheses are based. It is in the testing of hypotheses as to the relationship between two or more variables that we can raise our hypotheses to the level of general laws of culture.

With regard to the role of ethnographic data in archaeological investigation, Chang writes:

> The ethnological recourse does not make analogy possible; it only renders its results probable or even scientifically true (p. 228).
>
> Can information additional to what is already available in living societies be thus obtained when the model of archaeological social grouping necessarily depends on knowledge provided by existing societies? The answer must perhaps be negative until archaeological techniques are better and more self-contained (p. 232).

A behavioral correlate for an archaeological fact may be postulated on the basis of ethnographically known conditions; but recourse to ethnography could never render such an argument probable or true, except in the form of an argument by enumeration. (This is not the same as an argument by analogy; see Stebbing 1950:243-56 for a full statement of this distinction.) In discussing analogy Childe writes: (1956:49):

> Ethnographic parallels in fact afford only clues in what direction to look for an explanation in the archaeological record itself.

I have argued elsewhere (1966*a*) that analogical arguments are more probable or true only when subsidiary hypotheses, drawn from the postulate made possible by the analogy, have been tested against other archaeological data.

There have been several recent statements on the role of ethnographic data in archaeological reasoning (Freeman 1966, S. R. Binford 1966, L. R. Binford 1966*b*); in all of these, arguments are given against the proposition that our knowledge of the past is limited by our knowledge of the present. We have available today both the techniques and sufficient self-containment to formulate testable hypotheses to explain archaeological observations.

The limitations imposed by Chang's approach are further exemplified by his discussion of "activity systems." Chang endorses the suggestion that these should be the basic units with which archaeologists deal. He goes so far as to suggest that "archaeologists . . . structure their types around a series of activity systems such as subsistence, domestic, technological, and other behavioral categories . . ." (p. 230). He further suggests that we might classify our materials according to an activity paradigm such as that provided by Murdock's *Outline of Cultural Materials*. The crucial question to be asked here is: What new information could possibly be gained about variations in the activity systems of the past by simply fitting archaeological remains into types which are ordered in terms of our preconceptions of what those activities were? Our task as archaeologists is to devise analytical means of *discovering* what past activities were, not to fit artifacts into activity classifications arrived at arbitrarily. If all of this has a familiar ring, it is, I fear, because it is just another facet of the old argument about arbitrary vs. discovered types (Ford 1954*a*, *b* vs. Spaulding 1953, 1954). Techniques for discovering activities in the past are available and have already been fruitfully employed (Hill 1965, Binford and Binford 1966).

If we take an analytical, rather than a descriptive, approach to the past, the limits on our generalizations are set only by the analytical techniques available, not by our substantive knowledge of the present. The issues raised by Chang have been debated for more than 20 years and are important ones. Chang's views are shared by many of our colleagues. In stating this I am not accusing Chang of being a traditionalist; the originality and usefulness of his work argues against this. I have expressed my views here in the belief that discussion of our basic ideas is essential to progress.

by BERNHARD BOCK☆

Braunschweig, Germany. 15 VII 66

As I strongly advocate cooperation among the various sciences of man, I have read Chang's paper with particular attention. It will certainly be welcomed by everyone who is interested in the whole of anthropology and its fundamental aspects, for it is a fine exeample of the interdependence of the various sciences of man from the point of view of their common methods as well as their common object.

Chang writes: "Intercommunication does take place, but it may become even rarer and more inconsequential as the trend of specialization continues and intensifies." I don't see why this should be true. On the contrary, I feel sure that the growing specialization of the sciences will *force* scholars to cooperate and to communicate. Specialists will often study the same subject from their respective points of view, and they will have to learn the plans and results in each other's specialties as early and as comprehensively as possible in order to coordinate their studies.

In this sense, Chang's readers are encouraged to extend the principle of "interrelationship" to other sciences of man beyond archaeology and ethnology. The aspects he mentions (typology, analogy, reconstruction,

evolution, comparison) are common to other areas of anthropological studies as well (e.g., prehistory, linguistics, social and physical anthropology), though of course their application is slightly different in each of the various fields. In this connection, another aspect applicable to all fields of anthropology should not be forgotten: statistics.

by ALOIS CLOSS☆

Graz, Austria. 18 VII 66

Chang's article is written from the point of view of an American archaeologist, but its selection of questions and their answers is so stimulating that it can be considered illuminating for the Old World as well. The fact that it was in America that connections between prehistoric groups and surviving primitive tribes were first discovered has been an important factor in the tendency toward integration of archaeology and ethnology in that country from their beginnings. If I am not mistaken, Holmes (1914) and Nelson (1919) were pioneers in this effort. A comparison of their work with the present situation as Chang describes it would have been useful.

Certainly the integration of archaeology and ethnohistory (see Baerreis 1961) is part of this present situation, and it was preceded by an attempt (Closs 1956) to show that the fields of "special-historical" ethnology (the study of peoples and their migrations) and general ethnology (the systematic presentation and comparative investigation of the basic forms, particularly among non-literate peoples, of material, social, and symbolic culture) have methods in common. This latter essay contains an extensive review of literature relevant to Chang's theme. The most important of these works are the following:

On typology, Schwantes (1952) and Angeli (1958); on the reconstruction of sociocultural systems, Gjessing (1962, 1963), Narr (1962), and Hančar (1955). In Italy Pigorini established "Paletnologia" as a discipline and founded a journal of the same name; in the beginning, it was more concerned with known prehistoric peoples, but since Laviosa Zambotti (1950; see Koppers 1952:46-57 for a critique) it has been directed more toward so-called culture-historical ethnology. Historical data on this school are given by Barocelli (1940). In Germany, the study of the archaeology of settlements was called "tribal research" and had definite nationalistic tendencies (see Kossina 1911). For the theory, see Jahn (1952) and also Menghin (1950); for an opposite view, see Wahle (1940-41) and Eggers (1950). In Vienna, the integration of ethnology and prehistory served the purposes of universal history and was thus in the field of general ethnology; see Closs (1956) for references to the works of Koppers, Schmidt, Haekel, Jettmar, Heine-Geldern, and, for a critique, Pittioni, and, on the other hand, see Otto (1953).

The terminological distinction between archaeoethnology and ethnoarchaeology raises the important question as to where this special integrative discipline really belongs. We here on the continent generally prefer to speak of prehistory rather than archaeology, since the term "archaeology" is restricted to the study of finds *with inscriptions*. A closer connection between ethnology and prehistory seems generally better founded; but ethnoarchaeology would also constitute a special field within "special-historical" ethnology insofar as it is concerned with prehistoric peoples and their migrations (see Closs 1956:176). To identify peoples and migrations from prehistoric evidence is to bring into play a specifically ethnological aspect. We should, I think, pay special attention to the historical value of typology, i.e., the extent to which a correct typology contributes to the understanding of the historical moment (compare Burgmann 1964).

The great question is whether a general cultural-historical ethnology could exist at all without considering "archaeology."

by GEORGE L. COWGILL☆

Waltham, Mass., U.S.A. 28 VII 66

Chang's paper, in most ways very fine, begins with what I think is yet another of the many unsatisfactory discussions of classification in archaeological literature. I will briefly note some major disagreements.

1) I share Chang's belief that many of the anthropologists doing "ethnoscience" are not just using new terms for old methods and ideas, but are in fact making important advances in techniques for eliciting, verifying, and representing alien systems of cognition. Nevertheless, this is one of the most difficult (though probably not totally impossible) aspects of human behavior to get at through archaeological techniques, and I agree with Gardin's (1965) discussion of the problems involved. It is a mistake to insist too strongly on resemblances between the archaeologist's categories and native categories, because one cannot do so without glossing over the difficulties, and because it diverts attention from other reasons for classification. Concerning the difficulties, Chang says:

> Even though cognitive systems are culturally determined, they do have absolute, however qualified, physical foundations. *Therefore,* in theory, it is always possible to recognize a cognitive system through observable physical differences by recognizing the meaningful hierarchies and contrasts (p. 228; italics mine).

I do not understand this argument; I cannot see that the "Therefore, in theory . . ." is a logical consequence of what precedes it, nor can I see how, even if it were so in theory, the cognitive systems could be recognized in practice. Chang, if I understand him rightly, proposes that cognitively significant attributes will be the ones that are relatively stable "in the long run" over space and time. Here I am not merely unconvinced; I definitely reject stability as a trustworthy criterion for cognitive significance. While it is important and interesting to investigate variations in the stability of different attributes, we must be open to a great many possible interpretations of observed variations. Using stability as an indicator of cognitive importance, one would attempt (if no native speakers could be interviewed) to discover the structure of English by looking for common features in a number of texts written over several centuries and in various regions. Surely a linguist would say that "in the long run" there has been quite a bit of change in English and some of the cognitively significant features of any specific dialect are shared with few or no other dialects. One could even argue that features which show the greatest variability between communities may have the greatest saliency for the natives (especially when they are regarded as markers of ethnic or class identity) and may be most emphasized in native terminology. I do not mean to propose variability as an alternative criterion, but rather to stress that it is not at all clear what criteria archaeologists can use for inferring native systems of terminology. Of course, since clarification can only come through relevant ethnographic work, my objection actually strengthens my agreement with Chang's more basic point about the importance of ethnology for archaeology.

2) Chang says (p. 228) that an archaeologist's classification is "significant" and "meaningful" when it "*works*—that is, when and only if he can interpret his material, with reference to his knowledge of the larger context of his site and culture, more consistently by means of one classification than another." To my mind, this only replaces one ambiguity with another, for it remains unclear what is meant by "consistent" in this context. A substantial paper could well be written on this topic alone. Archaeologists have often had very limited objectives in their interpretations of data, and classifications which have

served them well may be very unsatisfactory for other purposes. Also, in practice major criteria of workability have been that the scheme, without too much ambiguity, provides one and only one place for almost all objects encountered, and that no obvious violence is done to any of the worker's notions about the nature of man, society, and culture. There is a great need to examine various widely used notions in terms of the extent to which they are wrong, doubtful, or so vague that almost no data could contradict them. Also, there is danger of logical circularity, certain notions being taken as axiomatic and classifications being accepted or rejected accordingly as they fit or do not fit the original notions, without any real use of data to *test* the notions. Furthermore, Chang glosses over a real difficulty by not mentioning that there are cases of persistent controversy in which each party claims that his classification "works," yet the schemes differ enough to have importantly different implications for culture theory.

3) I suggest that a useful way to approach archaeological classification is to look for methods which will require the weakest possible a priori assumptions about what may be important in the data. Obviously we do not really observe or record very much in the first place unless we have some idea that the data may conceivably prove meaningful, nor can we begin to explore every logically conceivable relationship between different items of data. But, given a choice of alternative techniques, we should prefer that which (within the limits of feasibility), reguires the fewest a priori decisions about what is profitable or unprofitable to emphasize in the data. Classification is in the first instance a body of operations relevant to data storage and retrieval, and, while it is never independent of synthesis and interpretation, it ought not to be confused with them. We want to be as free as possible to explore any body of archaeological data for *anything* systematic about the distribution of *any* features, with regard to time, space, or one another; and anything systematic we find, if not readily attributable to chance, is an "interesting" archaeological fact which requires explanation and which may have implications for anthropological theory. Cognitive systems theory is only one of many frames of reference within which a given finding may be meaningful. Today, for example, the most fruitful archaeological interpretations are in culture-ecological frames of reference. Highly relevant ecological data may not always be reflected in native terminology, but terminology must handle such data nevertheless.

I have no arguments of consequence with the remainder of Chang's paper, which is the greater part, and, for most people, the more important part. He has done a splendid job of outlining kinds of ethnographic information needed for better archaeological interpretation and, ultimately, valuable feedback of ethnological theory. What we need most now are people and money to do work which will fill the gaps in data pointed to by Chang and the other archaeologists he quotes.

by SAMIR GHOSH☆

Berlin, Germany. 20 VII 66

It is important to know the Past in order to intepret the Present; on the other hand, without a proper analysis of the Present it is impossible to understand the Past. Here lies the meeting-point of archaeology and ethnology. Chang's sober, timely, and very welcome paper seeks close collaboration between two distinct groups of practitioners of anthropology, archaeologists and ethnologists. The areas of common interest are (according to Chang) typology, analogy, the reconstruction of sociocultural systems, and process and the comparative method. I would add one more: culture-trait universals.

While ethnologists (to use Chang's term) will agree to (and indeed do) collaborate with archaeologists, I doubt that archaeologists, with their solely "taxonomic" approach, will be able to cope with Chang's proposals. Moreover, the extent to which cultural behavior can be recognized and quantified from artifacts is a moot point. The archaeologist, posing as a "scientist" and claiming to be digging up the past, in fact often measures his own footsteps; by the same token, the ethnologist, often ego-centered, may see the culture he is trying to interpret as merely a storehouse of concatenated or conglomerated elements. There is danger in both these attitudes. It is, of course, sensible to ask ourselves as social scientists whether we are interested in a model-directed or a data-oriented discussion. Both types have good and bad points; but unless a social scientist has a theory, a philosophy, a *weltanschauung* of his own, his efforts, even with the best of intentions, will come to a dead-end. Unfortunately, Chang has no comments on this.

I am surprised to find that, well-informed as he is, Chang gives no cross-reference to another sister-discipline, linguistics. The publication of Chomsky's *Syntactic Structures* (1957; see also 1965) established a new vigorous, well-rounded philosophical school of linguistics with pragmatic aims and sound thinking. Modern archaeologists and ethnologists cannot ignore transformational linguistics, and they will greatly benefit by getting acquainted with it. I may also be permitted to draw Chang's attention to the papers of the international conference on Universals of Language at MIT, especially those by Casagrande and Greenberg (1966). The mental and cultural world of a community is too complicated for pragmatic firsthand study, but its deep structure has few "kernel" types, and the use of (completely or partially) ordered transformational rules makes it possible to interpret and understand the surface structure of a culture, diachronic (or historical-comparative), in the case of the archaeologist and synchronic (or descriptive-structural) in the case of the ethnologist. I very much agree with Chang that a "rethinking in archaeology" is necessary; ethnology and archaeology do not really depend on each other, but they do have a major common area of interest and knowledge, if both are interested in "Man."

by GUTORM GJESSING☆

Oslo, Norway. 7 IX 66

In spite of its originality, Chang's excellent article in many respects reflects an important trend in modern, particularly American, archaeology, viz., the attempt at epistemological and theoretical analysis of methods and results, the first fumbling steps toward integrating the field with the family of theoretical sciences. From a European point of view, the reasons for this American attitude are at least two-fold: (1) The American insistence on keeping anthropology a single field of study and the temporal continuity between archaeology and ethnology make for a closer interrelationship between the two disciplines. As one of my Southwestern friends once put it, "You know, when we are faced with a difficult problem, we can just step into the next pueblo and ask!" (2) While in Europe archaeology is still generally considered pre*history*—that is, part of the national history—it cannot, for obvious reasons, be so considered in the Americas, and thus American archaeology is freed from the fetters of a one-sided, ethnocentric, historical point of view.

Although there is certainly a long and very stony road ahead before archaeology can be considered a deductive, theoretical science, any valid step in that direction should be considered progress, and European prehistorians still have much to learn from their American colleagues and hence from

Chang's important article. I will here be primarily concerned with the section "Reconstruction of sociocultural systems."

In many European countries, both East and West, the scope of prehistoric research has been greatly widened, not only in terms of socio-archaeological studies (Gjessing 1957), but also in terms of general culturological studies (Clark 1966*a*, *b*). In fact, the interrelationship between archaeology and ethnology is of long standing in practical European prehistoric and protohistoric studies (Brögger 1925, Childe 1940, Clark 1951, Gjessing 1955). Thus, Binford's not too kind characterization of archaeologists (quoted by Chang) has many and valuable exceptions. As a counterweight it may be well to remember Childe's (1951) words that

> archaeologists today have realized that they are dealing with concrete remains of societies, and that these societies, albeit illiterate, have left concrete embodiments not only of their material equipment, but also of their social institutions, superstitions, and behavior, fragmentary and ambiguous though these undoubtedly be.

Chang quotes Willey on the importance of ecology but he himself neglects to deal with the ecological aspect, which is of basic importance to the relationship between archaeology and ethnology. Indeed, it is the common ground of the two, not least for its definition of, and insight into, social groupings and their organization. Chang does say, I admit

> Instead of reviewing each of these areas [listed by Willey] and indicating what has been achieved and what has not, I would like to take an over-all view of the methodological problem with reference to sociocultural reconstruction as a whole.

But if one is to recover entire settlements of past populations, as Wolf (quoted by Chang) suggests, one must attempt "to grasp the archaeological equivalent of the ecologist's group and the social anthropologists' organization-bearing unit." This is so important, both theoretically and methodologically, that it should not be ignored, the less so as ecological viewpoints often extremely naive have nearly always been at the bottom of culture-historical archaeological research.

The interconnectedness between archaeology and ecology is implied in the premise that Man always has had to live in ordered societies organized in such a way that he was able to utilize certain culturally selected niches of the resources present. This necessarily must influence both theory and methodology of any reconstruction of sociocultural systems. "Two threads in such a study must be interwoven—the historical and the functional—as either one by itself will not do" (Haury 1956). It is, on the whole, regrettable that Chang has failed to take into consideration Haury's beautiful example of what can be achieved by combining ecological, historical, and functional methods. Nor does Chang seem to have exploited Julian Steward's pioneering work on the correlation between ecology, economy, and social systems, or Eggan's paper, "The Ethnological Cultures and their Archaeological Background" (1952), to mention only a few American examples. From the European quarter I miss particularly a reference to Clark's important work (1952) on the prehistoric economic basis of Europe. As I have myself tried to demonstrate the relevancy of ecology and a balanced use of analogies to ethnology (1955, 1963, 1964), I shall not recapitulate the arguments here. However, it seems at any rate obvious that conversion of energy and the energy pyramid make for the possibility of denser populations in agricultural communities than in those based on hunting. It is also obvious that bilateral kinship systems are more functional in small hunting societies, in which transfer of property and status are more important. The implications in terms of authority, political organization, etc., are also rather evident.

by SHIRLEY GORENSTEIN☆

New York, N.Y., U.S.A. 20 VII 66

While it is useful to discuss from a theoretical point of view, the several aspects of the interrelationship of archaeology and ethnology, in practice a close reciprocal relationship does not in fact exist. There has been very little working together on common problems and even less of one discipline's undertaking to solve the problems of the other. What we have instead is an occasional and tangential meeting. In general, the theoretical constructs in ethnology which archaeologists draw upon are formulated by ethnologists without any reference to archaeological theory or substance.

Chang, having quite rightly pointed out that recognition of the character of cognitive systems may help the archaeologist in developing a theory of typology, wonders why archaeologists and ethnologists have not worked together on this aspect of categorization. Perhaps it is because ethnologists are not particularly interested in typology. Typology is a matter of concern to those who deal with tangible material whose attributes can be measured or specifically described. Studies involving such material (for example, technology or settlement patterns) have been infrequent in contemporary ethnology (though very recently there has been an increasing trend in this direction). Ethnologists who do deal with this material, sometimes come up with something of use to archaeologists. On the whole, however, the ethnologist chooses subject matter that does not need to be typed, and therefore he is not forced into formulating a theory of typology. While many archaeologists are vitally interested in drawing on the work of ethnologists, ethnologists are seldom aware of archaeologists' needs if they are different from their own and do not orient their fieldwork or analyses toward the solution of archaeological problems. It is more common for the archaeologist himself to undertake an ethnographic study in order to solve an archaeological problem.

Chang has given us a useful synthesis of what should be, at least in part, the interrelationship of archaeology and ethnology. We should not overlook, however, the interrelationships of archaeology and other disciplines. Ethnologists can best help archaeologists who deal with the kind of societies traditionally studied by ethnologists, namely those with primitive and folk culture. For archaeologists dealing with very early culture (unlike anything extant) or with civilization, however, other disciplines have as much to offer as ethnology. Palaeolithic archaeology, for example, has a close relationship with geology and geography because they provide chronological data and because of the critical importance of the environment in the development and life of early man. High-culture archaeologists turn to the work of historians and political scientists because they must interpret written records and describe complex political systems. As archaeologists we are interested in understanding the culture of the past. From this point of view the work of ethnologists is of interest to us all; but some of us can learn as much or more from the natural sciences or from the more humanistic disciplines.

by KARL J. NARR☆

Münster, Germany. 20 VII 66

The interrelationship of archaeology and ethnology, especially the problems of analogy and cooperation in reconstructing sociocultural systems and historical units and processes, has been thoroughly discussed by many anthropologists of the Old World. K. C. Chang seems to regard these questions as almost entirely an American affair. CURRENT ANTHROPOLOGY is a "world journal." If Chang's aim was to give scholars of other parts of the world an example of a specific American approach to the problems in question, the article should have been presented

accordingly. As it stands, however, it seems to me merely one more example of the deplorable tendency of many American anthropologists to be contented with the works and thoughts of their own circle.

by CARROLL L. RILEY☆

Carbondale, Ill., U.S.A. 25 VII 66

Chang begins his interesting and provocative article by posing two questions that seem to me to warrant consideration. The first of these is whether it is "possible and fruitful to *reconstruct culture* [italics mine] and history by classifying artifacts without recognizing . . . cultural behavior?" It is, of course, possible to classify artifacts by form only, but since culture is at least in part a vast bundle of behavior, I fail to see how culture itself could be reconstructed by such means. The second question is whether there is a meaningful correlation between archaeological artifacts and the behavioral and cognitive systems of their makers. If I understand this question rightly, it is both central to the issue and irrelevant; for if the answer is "no" archaeology becomes impossible, and archaeologists turn into mere art collectors.

Only if archaeology is the "ethnology of the past" and only if human behavior, including the production of artifacts, is essentially patterned and repetitive regardless of time and space does archaeology have value, at least in any kind of anthropological context. Chang does, in fact, seem to assume that there are meaningful regularities in human behavior, and the body of his paper provides an excellent statement of some of the pitfalls and complexities of archaeological interpretation.

Chang points out some shortcomings of the classic ethnological approach and suggests that perhaps we should have a group of *ethnoarchaeologists* who know what kind of ethnological information is most applicable to archaeology. I agree that this is needed but suggest that what both fields need even more is a flourishing branch which we might call *archaeoethnology* and which would approach archaeology with a firm grounding in ethnological research. Operating to some degree in both fields, I am constantly disturbed, not only by the scant attention paid by archaeologists to ethnological theory and method, but even more by the general indifference of practitioners of one field toward the other. This is bad for the ethnologist, for he becomes stranded in that never-never-land of the ethnological present. For the archaeologist, it can be fatal, because, working with the artifactual bones of human culture, he is ignorant of the complex and significant patterning that will allow him to reconstruct some fragments of the non-material culture. In the final analysis, every interpretation in archaeology is constructed, more or less, by use of ethnological (or historical) analogy.

by PHILIP E. L. SMITH☆

Montreal, Canada. 25 VII 66

A great deal of what Chang discusses and proposes falls within the area of general agreement; at least, I think that few anthropological archaeologists on this side of the North Atlantic will disagree with his central theme that archaeologists have something to learn from ethnologists and vice versa. The specializations he mentions in his first few paragraphs and the decline of the "general anthropologist" in North America are, of course, phenomena which occurred in Europe a good many years ago, early in this century. Some famous journals which had their roots in the more unspecialized era (e.g., *L'Anthropologie, Journal of the Royal Anthropological Institute, Zeitschrift für Ethnologie*) have continued to attempt to cover all the subfields, although (as is increasingly the case in North America) the individual contributions are virtually all written by specialists. This apparent separatism, whatever its disadvantages, was inevitable considering the spurt in growth and the adoption of new methodologies (as well as the abandonment of some old ones) in both subfields in the first few decades of this century. However, there have recently been some signs of renewed interest in a more active relationship between archaeology and ethnology in some countries (e.g., in the work of such individuals as Leroi-Gourhan in France and J. D. Clark in Britain, not to mention that of various Soviet and African workers who have never relinquised some forms of this approach; there are also some cases of increased cooperation between certain university departments of prehistory and ethnology). I am optimistic that a similar sense of enlightened self-interest will prevent the trend toward specialization in the New World from reaching an extreme.

Individuals will, and probably must, specialize, but as long as they continue to operate within some kind of system in which they and their ideas are constantly interacting archaeologists and ethnologists will continue to give some thought to their common aims, problems, and methods. To paraphrase Nicholas Murray Butler's remark that he was not worried about unbalanced books as long as there were balanced libraries, I see more cause for concern in overspecialization in university departments of anthropology (a rather serious problem in Canada), which may nearly or totally exclude either ethnology, or more frequently, archaeology.

by JULIAN H. STEWARD☆

Urbana, Ill., U.S.A. 15 VII 66

Chang's four foci of mutual interest to archaeology and ethnology—typology, analogy, sociocultural systems, and processes—are real and important, but I cannot overemphasize my conviction that the kind of research now done in these areas has resulted from fundamental changes in the problem orientation of cultural studies in general.

Only 30 years ago, archaeology retained the natural-history and humanistic orientation developed during its many years as a museum discipline. Taxonomy was constructed for its own sake, and in many monographs the major categories were based on materials—"objects of stone," "of wood," etc.—with the presumed use and the style of the objects secondary. Materials thus classified were arranged in time sequences and distributions. Ethnology had developed its own culture area taxonomy (which was later applied with some difficulties to prehistory), and its major objective was descriptive rather than explanatory analysis and it ascribed little importance to history and process.

A fundamentally new problem orientation was begun during the 1930's, but it did not achieve full impact until after World War II and has only in the past decade paid off in a substantial number of publications. The new goal of achieving causal explanations of culturual similarities and differences and it has permeated both archaeology and ethnology. It has necessarily introduced an interest in processes of change and in the factors that initiated these processes. This has led to comparisons of sociocultural systems wherein similarities as well as differences are accorded importance. It has also entailed extraordinarily difficult problems of social taxonomy, especially since different forms may serve similar purposes and vice versa.

The search for causal factors and processes that produced the great variety of sociocultural systems has required new concepts that give relevance to phenomena previously ignored by both archaeology and ethnology. The concept of cultural ecology is now generally accepted as a conceptualization of adaptive reactions of a society to its natural and social

environment that create internal processes of change. Recent detailed analysis of microvariations in environmental niches is disclosing local and seasonal sociocultural variations within societies—bands or tribes—which according to the older taxonomy-based implements and elements lists, had nearly identical cultural inventories. The ecological approach while not the explanation, has given relevance to aspects of the natural environment, technologies, and social systems that were wholly lacking in the so-called ideal ethnologies written before 1930. Employed by archaeology, it is throwing new light on early hunters and food collectors and creating new understandings of the processes involved in developments following the agricultural revolution.

Another concept that has developed from the recent orientation is that of levels of sociocultural organization which are created by new processes in the course of change. This, together with the many varieties of cultural-ecological adaptations, negates the early evolutionary assumption that universal principles can explain culture change in all periods and places. If substantive explanations of particular cultures are to be achieved, a thoroughly empirical approach is required.

Certain differences in field methods and techniques must, of course, distinguish archaeology and ethnology, and individuals may continue to be interested in archaeological materials for their own sake, in the uniqueness of each ethnology, or in any of the innumerable specializations into which cultural studies are splintering. The interest in explanatory or causal formulations, however, affords a common ground for archaeology and ethnology, and if its effects have not yet culminated perhaps it is simply too recent. Of Chang's 52 references, all but 7 were published after 1950. Only 30 years ago, the intellectual climate was strongly antagonistic to speculation about causes and to essays in cultural ecology. My comparative study of hunting bands (Steward 1936) was ignored for two decades, and my ecological study of the relationship of prehistoric settlement patterns to Western Pueblo social organization (Steward 1937) was refused by the *American Anthropologist*.

Today, detailed comparative studies that use concepts of ecology, social structure, and processes as tools for causal analysis are constantly interrelating the data of archaeology and ethnology. They are also requiring drastic modifications of earlier hypotheses. It should be stressed, however, that the new approach still consists of the exploration of a new methodology—a task for imaginative scientists. Until the basic problems that link archaeology and ethnology become clarified, there is litlle place for stereotyped procedures that can be applied by any technician.

The new problem orientation, however, is increasingly evident in substantive applications. The recent Ottawa Conference on Bands and the Chicago Symposium on Hunters have utilized the concept of ecology along with cultural-historical factors to place analysis of prehistoric and historic food hunters and collectors in fundamentally new perspectives. The interest in process and changing structures has led to research on the origins of the primary state structures, and this has required attention to ecological changes that accompanied the agricultural revolution as well as to the changing functional roles of theocratic, militaristic, commercial, and other institutions. A particularly impressive product of the new problem orientation is Robert Adams' (1966) analysis of evolutionary processes in Mexico and Mesopotamia from the early farm communities to the stratified states. Both areas involved ethnology, as inferred from early documents, and plain dirt archaeology, and the analysis includes phenomena that would have been ignored two decades ago.

Reply

by K. C. Chang

I shall confine my brief remarks to the few apparent disagreements between my views and those of George Cowgill and Lewis Binford on some of the archaeological issues discussed in my paper. Comments by the other scholars in the main supplement and complement my paper and have helped fill many of its gaps. I am particularly appreciative of Alois Closs's list of works by continental scholars on the archaeology-ethnology interrelationship, for it is an example of the international exchange of information for which CURRENT ANTHROPOLOGY is a forum. I am therefore puzzled by Karl Narr's criticism that my paper does not have world-wide coverage. Since this involves the nature of review articles in this journal, I should like to pass on to the Editor the onus of making any reply. For my part, I wish merely to underline what I have said in the third paragraph of the paper.

Cowgill "cannot see that the 'therefore, in theory, . . .' is a logical consequence of what precedes it." I am afraid that my penchant for economy in words is not matched by my effectiveness in their use. What I meant was simply this: Cognitive systems include systems of units, and when the units in question involve physical objects (such as archaeological artifacts) they are units of physical variations. The culturally determined part of a cognitive system like this has primarily to do with the level of categorization and hierarchy of the physical variations but not at all to do with the level of the physical properties of the variations themselves. If there are 100 and only 100 grades of red color, Culture A may call 1-50 pink and 51-100 purple, and Culture B may call them all red, but both of these cultures necessarily mean the same 100 color gradations. This being so, any mathematical ordering of these 100 grades of color would include both of the cognitively significant classifications.

The problem that follows, one that Cowgill has raised, is how the cognitively significant classifications could be recognized in archaeological practice. He says I have proposed "that cognitively significant attributes will be the ones that are relatively stable 'in the long run' over space and time," and he definitely rejects stability as a trustworthy criterion for cognitive significance. Here there is no disagreement, for I also would reject stability as the sole criterion for cognitive significance. I cannot find the word stability in my paper. What I have said is

> Information on the hierarchical and contrastive meaningfulness of the variations . . . can be recognized in a context of change, i.e., in the context of history. . . . Therefore, cognitively significant attributes stand out in a patterned manner in the archaeological record *in the long run.*

The key phrase here is "in a patterned manner," which includes stability but cannot be equated with it.

Cowgill emphasizes that the "workability" of archaeological classifications is ambiguous and can be controversial. This essentially underlines my own statement that "the workability is always relative and subject to proof and the subjective elements can only be minimized by the continuous widening of the sphere of consideration."

Cowgill thinks that "a useful way to approach archaeological classification is to look for methods which will require the weakest possible a priori assumptions about what may be important in the data." The reason for this seems to be that "classification is

in the first instance a body of operations relevant to data storage and retrieval, and, while it is never independent of synthesis and interpretation, it ought not to be confused with them." But he has stated previously that "archaeologists have often had very limited objectives in their interpretations of data, and classifications which have served them well may be very unsatisfactory for other purposes." With this I agree completely. But unless one makes some—perhaps a large number and several bundles of—tentative a priori assumptions about "what may be important in the data," how can one proceed with one set of classifications as against another set? Here I am not sure that we are in any disagreement; he talks about "weak" assumptions, whereas I call them "tentative" ones. Classification is basically a tool to fulfil certain objectives in archaeological studies. The tool itself is mechanical, precise, and devoid of distortions; distortion results from improper matching of particular classification systems and particular objectives. To make a priori assumptions—either weak or strong—does not mean that the classification results must be forced to fit them. If they do not fit, perhaps the assumptions were wrong, or perhaps the wrong set of classifications was adopted.

Apparently more serious disagreements seem to obtain between Binford and myself over the following issues: (1) the relation between archaeological classifications (fulfilling a variety of objectives) and "relativistic" cognitive systems; (2) the aims of archaeology—analytic-comparative vs. descriptive; (3) a distinction between analogy and "logico-deductive generalization;" and (4) the extent of archaeological independence from ethnography.

These include some of the most important issues in archaeological theory, and I cannot agree more with Binford that "discussion of our basic ideas is essential to progress." I feel, however, that this is not the proper place for their detailed discussion. My views on these issues—from which Binford's own position as stated in his comments sometimes, though not always, differs—have been presented in some detail in a yet unpublished paper, "Toward a Science of Prehistoric Society," that I read at a conference in 1962 of the American Association for the Advancement of Science and in *Rethinking Archaeology* (1967). The paper at hand discusses not the basic issues of archaeology but the major aspects of archaeology-ethnology interrelationship, and, therefore, it stresses the mutual dependence of the disciplines. I tried not to overstate my case wherever basic "propositions on which [my] discussion is predicated" were involved, but I may on occasion have let expediency and enthusiasm gain the upper hand over basic convictions in principle. If so, I appreciate having it pointed out.

For instance, on archaeological typology and the ethnological study of cognitive systems, I have, for the purpose of the paper, stressed their close interrelationship, but I have been careful—though apparently not careful enough—not to make any rigid equations. I said "archaeologists classify in order to reveal relevant information about the life and history of ancient peoples," not "in order to reveal the life and history of ancient peoples." I said "a classification that agrees with the cognitive systems of the makers of the artifacts would, for a variety of purposes, invariably work better than one that does not," instead of "classifications must always for all purposes agree with the cognitive systems of the makers of the artifacts." I said "all classifications in archaeology that are workable in the realm of cultural and historical interpretation are cognitively meaningful," rather than "all meaningful archaeological classifications must be cognitively significant." Perhaps these distinctions were too fine to be noticed, but it should be clear that my intent in these passages was to emphasize that "archaeologists and ethnologists have a common area of interest in the study of, and approach to, cognitive systems." I had no intention whatever of proposing or reiterating any normative theory of archaeological types. Binford may have noticed that the only reference to "mode" was in a quotation from Rouse (1960), and the quotation was used because it was relevant in that context. Rouse, it must be noted, discusses mode there in connection with what he calls analytic types, and he has a separate concept of descriptive types for other archaeological objectives; he, too, must not be accused of advancing there a solely normative interpretation of archaeological types. (Incidentally, I am not at all sure that "mode," "norm," and "cognitive significance" are identical concepts, as Binford states, but this is beside the point.)

Similarly, in the paper I stressed the use of archaeology in the reconstruction of the past because I am convinced it is in fulfilling this objective above all that archaeology must draw insight and specific knowledge from ethnology. I was not discussing the aims of archaeology. I agree with Binford that both "explication and explanation of cultural differences and similarities" are among the principal archaeological aims. In the matter of explanation, archaeology-ethnology interplay is also highly significant, and I briefly touched upon it under the heading "Process and the Comparative Method." As Julian Steward has pointed out, this aspect was not given the attention it deserves, and I am thankful for both his and Binford's comments on it. I must say, however, that the "mere reconstruction of the past," which Binford holds to be my view of the "ultimate aims of archaeology," is not exactly a point of view that I care to be associated with, although my view on this is not necessarily relevant in the present context. Archaeology has many aims, and for each there is an area or areas of archaeology-ethnology interplay. That is all that is relevant. The "ultimate" aims of archaeology, on the other hand, vary from one archaeologist to another.

Much of Binford's argument about analogy and the role of ethnological data in archaeological investigations has presumably been presented in full by Binford and others in the as yet unpublished works that he cites. These have not been made available to me, and I can have no learned basis for deliberation. To Binford's question,

> What new information could possibly be gained about variations in the activity systems of the past by simply fitting archaeological remains into types which are considered in terms of our preconceptions of what those activities were?

—a question that is said to arise because of my alleged suggestion that "we might classify our materials according to an activity paradigm as provided by Murdock's *Outline of Cultural Materials*"—I can only reply by repeating what I said in the paper:

> While such works as *Notes and Queries* and *Outline of Culture Materials,* which purport to provide a universal categorization of culture materials for ethnographers, are indispensable to archaeologists for designing such activity systems, these designs must primarily be determined by the nature and preservation of their material in the field.

I regret that this reply has not contributed anything new to my paper, but has used much space to repeat and explicate (and explain) my positions as stated earlier. Some of what I said was perhaps not clear; too much may have been taken for granted. Apparently the basic concepts in archaeology are still so controversial that one cannot be too precise in making statements that might be misinterpreted by others holding different theoretical positions. I am grateful to both Cowgill and Binford for raising these problems so that my own position might, it is hoped, be clarified.

References Cited

ABERLE, D. R. 1960. "The influence of Linguistics on early personality and culture theory," in *Essays in the science of culture*. Edited by G. Dole and R. L. Carneiro. New York: Thomas Y. Crowell. [LRB☆]

ADAMS, ROBERT M. 1966. *The evolution of urban society: Early Mesopotamia and Prehistoric Mexico*. Chicago: Aldine. [JHS☆]

ANGELI, W. 1958. Typologie und typologische Methode. *Archaeologia Austriaca* 23. [AC☆]

ASCHER, ROBERT. 1961. Analogy in archaeological interpretation. *Southwestern Journal of Anthropology* 17:317–25.

BAERREIS, D. A. 1961. The ethnohistoric approach and archaeology. *Ethnohistory* 8. [AC☆]

BAROCELLI, P. 1940. L'ultimo trentanno di studi paletnologici in Italia. *Bullettino di Paletnologia Italiana* N.S. 2 [AC☆]

BIDNEY, D. 1953. *Theoretical anthropology*. New York: Columbia University Press. [LRB☆]

BINFORD, LEWIS R. 1962. Archaeology as anthropology. *American Antiquity* 28: 217–25.

———. 1965. Archaeological systematics and the study of culture process. *American Antiquity* 31(2), Part 1:203–10. [LRB☆]

———. 1966*a*. Smudge pits and hide smoking: The use of analogy in archaeological reasoning. *American Antiquity*. In press. [LRB☆]

———. 1966*b*. Methodological considerations in the archaeological use of ethnographic data. Paper delivered at the Wenner-Gren Foundation Conference on Man the Hunter. University of Chicago, April 6–9. [LRB☆]

BINFORD, S. R. 1966. Ethnographic data and understanding the Pleistocene. Paper delivered at the Wenner-Gren Foundation Conference on Man the Hunter, University of Chicago, April 6–9. [LRB☆]

BINFORD, L. R., and S. R. BINFORD. 1966. "A preliminary analysis of functional variability in the Mousterian of Levallois facies," in *Recent studies in Paleoanthropology*. Edited by J. D. Clark and F. C. Howell. *American Anthropologist* 68(2), part 2. [LRB☆]

BRAIDWOOD, ROBERT J. 1960. "Levels in prehistory: A model for the consideration of the evidence," in *The Evolution of Man*, vol. 2 of *Evolution After Darwin*. Edited by Sol Tax, pp. 143–51. Chicago: University of Chicago Press.

BRÖGGER, A. W. 1926. *Kultuurgeschichte des norwegischen Altertums*. Instituttet for sammenlignende kulturforskning, Serie A, II. [GG☆]

BUETTNER-JANUSCH, J. 1957. Boas and Mason: Particularism versus generalization. *American Anthropologist* 59:318–24. [LRB☆]

BURGMANN, A. 1964. Das Anthropos-Institut: Zielsetzung und Aufgabe. *Kairos* 6: 49–50. [AC☆]

CASAGRANDE, J. B. 1966. "Language universals in anthropological perspective," in *Universals of language*. Edited by J. H. Greenberg. 2nd edition. Cambridge: M.I.T. Press. [SG☆]

CHANG, KWANG-CHIH. 1958. Study of the neolithic social grouping: Examples from the New World. *American Anthropologist* 60:298–334.

———. 1960. Prehistoric settlements in China: A study of archaeological method and theory. Unpublished Ph.D. dissertation, Harvard University, Cambridge, Mass.

———. 1962. Settlement and community patterns in some circumpolar societies. *Arctic Anthropology* 1:28–41.

———. 1966. *Rethinking archaeology*. New York: Random House. In press.

CHILDE, V. GORDON. 1936. "Changing methods and aims in prehistory." *Proceedings of the Prehistoric Society for 1936*, pp. 1–15. Cambridge: Cambridge University Press.

———. 1940. *Prehistoric communities of the British Isles*. London. [GG☆]

———. 1951. *Social evolution*. New York: Schuman.

———. 1956. *Piecing together the past: The interpretation of archaeological data*. New York: Frederick A. Praeger. [LRB☆]

CHOMSKY, NOAM. 1957. *Syntactic structures;* The Hague: Mouton. [SG☆]

———. 1965. *Aspects of the theory of syntax*. Cambridge: M.I.T. Press. [SG☆]

CLARK, GRAHAME. 1951. "Folk culture and the study of European prehistory," in *Aspects of archaeology in Great Britain and beyond: Essays presented to O.G.S. Crawford*. Edited by W. F. Grimes, pp. 49–65. London.

———. 1952. *Prehistoric Europe: the economic basis*. London: Methuen. [GG☆]

———. 1957. *Star Carr*. Cambridge: Cambridge University Press.

———. 1966*a*. Traffic in stone axe and adge blades, in *The Economic History Review*, 2nd Series 18:1–28. [GG☆]

———. 1966*b*. Prehistory and human behavior. *Proceedings of the American Philosophical Society* 110(2):91–99. [LRB, GG☆]

CLOSS, A. 1956. Abgrenzung und Aufriß einer speziellen historischen Ethnologie. *Zeitschrift für Ethnologie* 81:161–79. [AC☆]

COE, MICHAEL D. 1961. Social typology and the tropical forest civilizations. *Comparative Studies in Society and History* 4:65–85.

CONKLIN, HAROLD C. 1955. Hanunóo color categories. *Southwestern Journal of Anthropology* 11:339–44.

———. 1962*a*. Comment on: The ethnographic study of cognitive systems, by Charles Frake, in *Anthropology and human behavior*. Edited by Thomas Gladwin and W. C. Sturtevant, pp. 86–91. Washington, D.C.: Anthropological Society of Washington.

———. 1962*b*. Lexicographical treatment of folk taxonomies. *International Journal of American Linguistics* 28:119–41.

DEETZ, JAMES F. 1965. *The dynamics of stylistic change in Arikara ceramics*. Illinois Studies in Anthropology, no. 4.

DI PESO, CHARLES. 1956. *The Upper Pima of San Caijetano del Tumacacori*. Dragoon: The Amerind Foundation.

EGGAN, FRED. 1952. "The ethnological cultures and their archaeological background," in *Archaeology of the eastern United States*. Edited by J. B. Griffin, pp. 35–45. Chicago: The University of Chicago Press. [GG☆]

EGGERS, H. J. 1950. "Das Problem der ethnischen Deutung in der Frühgeschichte," *Festschrift für E. Wahle*, pp. 49–59, Heidelberg. [AC☆]

FORD, J. A. 1954*a*. Comment on A. C. Spaulding's "Statistical Techniques for the Discovery of Artifact Types." *American Antiquity* 19:390–91. [LRB☆]

———. 1954*b*. The type concept revisited. *American Anthropologist* 56:42–54. [LRB☆]

FOSTER, GEORGE M. 1965. "The sociology of pottery: Questions and hypotheses arising from contemporary Mexican work," in *Ceramics and Man*. Edited by F. R. Matson, pp. 43–61. Viking Fund Publications in Anthropology, no. 41.

FRAKE, CHARLES. 1961. The diagnosis of disease among the Subanun of Mindanao. *American Anthropologist* 63:113–32.

———. 1962. "The ethnographic study of cognitive systems," in *Anthropology and human behavior*. Edited by Thomas Gladwin and W. C. Sturtevant, pp. 72–85. Washington, D.C.: Anthropological Society of Washington.

FREEMAN, L. G. 1966. A theoretical framework for interpreting archaeological materials. Paper delivered at the Wenner-Gren Foundation Conference on Man the Hunter. University of Chicago, April 6–9. [LRB☆]

GARDIN, J.-C. 1965. On a possible interpretation of componential analysis in archaeology. *American Anthropologist* 67 (5, part 2):9–22. [GLC☆]

GJESSING, GUTORM. 1955. Prehistoric social groups in North Norway, *Proceedings of the Prehistoric Society*, 21:84–92. [GG☆]
(Also in The Bobbs-Merrill. Reprint Series in the Social Sciences, A. 298.)

———. 1957. "Trends in European prehistory," *Selected Papers of the Fifth Congress of Anthropological and Ethnological Sciences*, pp. 43–48. Philadelphia: University of Pennsylvania Press. [GG☆]

———. 1962. The relationship between ethnology and social anthropology. *Ethnos* 27:167–85. [AC☆]

———. 1963. Socio-archaeology. *Folk* 5:103–12. [AC, GG☆]

———. 1964. Adherence to group territory, *Folk* 6:55–62. [GG☆]

GREENBERG, J. H. 1966. "Some universals of grammar with particular reference to the order of meaningful elements," in *Universals of language*. Edited by J. H. Greenberg. 2nd edition. Cambridge: M.I.T. Press. [SG☆]

HANČAR, F. 1955. *Das Pferd in prähistorischer und frühgeschichtlicher Zeit*. Wiener Beiträge für Kulturgescichte und Linguistik 11, 83, 85, 87, 119, 173, 201. [AC☆]

HAURY, EMIL. 1956. "Speculations on prehistoric settlement patterns in the Southwest," in *Prehistoric settlement patterns in the New World*. Edited by Gordon R. Willey, pp. 3–10. Viking Fund Publications in Anthropology no. 23. [GG☆]

HERSKOVITS, M. J. 1955. *Cultural anthropology*. New York: Alfred A. Knopf. [LRB☆]

HILL, J. N. 1965. Broken K: A prehistoric society in eastern Arizona. Unpublished doctoral dissertation, University of Chicago. [LRB☆]

HOLE, FRANK, and ROBERT F. HEIZER. 1965. *An introduction to prehistoric archaeology*. New York: Holt, Rinehart and Winston.

HOLMES, W. 1914. Areas of American Culture. *American Anthropologist* 16. [AC☆]

HOWARD, ALAN. 1963. Land, activity systems, and decision-making models in Rotuma. *Ethnology* 2:407–40.

JAHN, M. 1952. *Abgrenzung von Kulturgruppen und Völkern in der Vorgeschichte.* Berichte über die Verhandlungen der Sächs, Akademie der Leipzig, Philosopische-Historischen Klasse 99, 3. [AC☆]

KARLGREN, BERNHARD. 1937. New studies of Chinese bronzes. *Bulletin of the Museum of Far Eastern Antiquities* 9:1–112.

KLUCKHOHN, CLYDE. 1957. Premier Book Edition. *Mirror for Man.* New York: Fawcett.

———. 1960. "The use of typology in anthropological theory." *Selected Papers of the Fifth International Congress of Anthropological and Ethnological Sciences.* Edited by Anthony F. C. Wallace, pp. 134–40. Philadelphia: University of Pennsylvania Press.

KOPPERS, W. 1951. Ethnologie und Prähistorie in ihrem Verhältnis zur Geschichtswissenschaft. *Anzeiger der Österreicherin Akademie der Wissenschaften* 88:319 ff. [AC☆]

———. 1952. *Kultur und Sprach.* Wien. [AC☆]

———. 1953. Zusammenarbeit von Ethnologie und Prähistorie. *Anzeiger der Österreicherin Akademie der Wissenschaften* 91:67 ff. [AC☆]

———. 1957. Das Problem der Universalgeschichte im Lichte von Ethnologie und Prähistorie. *Anthropos* 52:369–89. [AC☆]

KOSSINA, GUSTAV. 1911. *Die Herkunft Germanen zur Methode der Siedlungsarchäologie.* Würzburg Kabitzsch. Mannus-Bibliothek 6. [AC☆]

LAVIOSA ZAMBOTTI, P. 1950. *Ursprung und Ausbreitung der Kultur.* Baden-Baden. [AC☆]

LONGACRE, WILLIAM A. 1964. Archaeology as anthropology: A case study. *Science* 144:1454–55.

LOTHROP, SAMUEL K. 1942. *Coclè, an archaeological study of Central Panama.* Memoirs of the Peabody Museum, Harvard University, vol. 8, part 2.

MENGHIN, O. 1936. Grundlinien einer Methode der indogermanischen Stammeskunde. *H. Arntz, Germanen und Indogermanen,* I Teil, 15 Band, S. 61–67. Indogermanische Bibliothek, Heidelberg. [AC☆]

MURDOCK, GEORGE P., C. FORD, A. E. HUDSON, R. KENNEDY, L. W. SIMMONS, and J. W. M. WHITING. 1961. *Outline of cultural materials.* New Haven: Human Relations Area Files.

NADEL, S. F. 1951. *The foundations of social anthropology.* Glencoe: The Free Press.

NARR, K. J. 1962. Approaches to the social life of earliest man. *Anthropos* 57:604–20. [AC☆]

NASH, JUNE. 1964. The structuring of social relations: An activity analysis. *Estudios de Cultura Maya* 4:335–59.

NELSON, N. C. 1919. The archaeology of the Southwest. *Proceedings of the National Academy* 5. [AC☆]

NICHOLSON, H. B. 1958. Settlement pattern analysis in contemporary American archaeology. *American Anthropologist* 60: 1189–91.

OLIVER, DOUGLAS L. 1958. An ethnographer's method for formulating descriptions of "social structure." *American Anthropologist* 60:801–26.

OTTO, K. H., with G. GUHE and H. GRÜNERT. 1962. Frühgeschichteforschung im Rahmen der Gesellschaftswissenschaften. *Zeitschrift für ethnologische und archäologische Forschungen* 3:13–58.

PARSONS, ELSIE CLEWS. 1940. Relations between ethnology and archaeology in the Southwest. *American Antiquity* 5:214–20.

PITTIONI, R. 1951. Prähistorie oder Urgeschichte? *Anzeiger der Österreicherin Akademie der Wissenschaften* 88:69–91. [AC☆]

ROYAL ANTHROPOLOGICAL INSTITUTE OF GREAT BRITAIN AND IRELAND. 1951. 6th edition. *Notes and queries on anthropology.* London: Routledge and Kegan Paul.

ROUSE, IRVING. 1960. The classification of artifacts in archaeology. *American Antiquity* 25:313–23.

———. 1964. "Archaeological approach to cultural evolution," in *Explorations in cultural anthropology.* Edited by W. Goodenough, pp. 455–68. New York: McGraw-Hill.

———. 1965. The place of "peoples" in prehistoric research. *Journal of the Royal Anthropological Institute* 95:1–15.

SCHWANTES, G. 1952. Vom Wesen der Typologie. *Offa* 10. [AC☆]

SERVICE, ELMAN R. 1964. "Archaeological theory and ethnological fact," in *Process and pattern in culture.* Edited by R. A. Manners, pp. 364–75. Chicago: Aldine.

SMITH, HARLAN I. 1899. "The ethnological arrangement of archaeological material." *Report of the Museum Association of the United Kingdom for 1898.* Glasgow and Edinburgh: W. Hodge.

SMITH, WATSON. 1962. Schools, pots, and potters. *American Anthropologist* 64: 1165–78.

SPAULDING, A. C. 1953. Review of: Measurement of some prehistoric design developments in the Southeastern States, by J. A. Ford (American Museum of Natural History Anthropological Papers no. 44). *American Anthropologist* 55:588–91. [LRB☆]

———. 1954. Reply to Ford. *American Antiquity* 19:391–93. [LRB☆]

———. 1966. "Explanation in archaeology," in *New perspectives in archaeology.* Edited by L. R. Binford and S. R. Binford. Chicago: Aldine. In press. [LRB☆]

STEBBING, L. S. 1950. 7th edition. *A modern introduction to logic.* New York: Harper. [LRB☆]

STEWARD, JULIAN H. 1936. "The economic and social basis of primitive bands," in *Essays on anthropology in honor of Alfred Louis Kroeber,* pp. 311–50. Berkeley: University of California Press. [JHS☆]

———. 1937. Ecological aspects of Southwestern society. *Anthropos,* 32:87–104. [JHS☆]

———. 1942. The direct historical approach in archaeology. *American Antiquity* 7: 337–43.

———. 1955. *Theory of culture change.* Urbana: University of Illinois Press.

STEWARD, JULIAN H., and FRANK M. SETZLER. 1938. Function and configuration in archaeology. *American Antiquity* 4:4–10.

TALLGREN, A. M. 1937. The method of prehistoric archaeology. *Antiquity* 11:152–61.

TAYLOR, WALTER W. 1948. *A study of archaeology.* Memoirs of the American Anthropological Association, no. 69.

THOMPSON, RAYMOND. 1958. *Modern Yucatecan Maya pottery: A study of the nature of the archaeological inference.* Memoirs of the Society for American Archaeology, no. 15.

WAHLE, E. 1940/41. Zur ethnischen Deutung frühgeschichtlicher Kulturprovinzen. Grenzen der frühgeschichtlichen Erkenntnis. Sitzungsberichte der Akademie der Wissenschafter Heidelberg, Philosophische-Historischen Klasse S. 5–147. [AC☆]

WHEELER, SIR MORTIMER. 1950. What *matters* in archaeology. *Antiquity* 24: 122–30.

WILLEY, GORDON R. 1953*a*. Comments, in *An appraisal of anthropology today.* Edited by Sol Tax, Loren Eisely, Irving Rouse, and Carl Voegelin, pp. 229–30. Chicago: University of Chicago Press.

———. 1953*b*. *Prehistoric settlement patterns in the Viru valley, Peru.* Bureau of American Ethnology Bulletin 155.

———. 1956. *Prehistoric settlement patterns in the New World.* Viking Fund Publications in Anthropology, no. 23.

———. 1962. The early great styles and the rise of the pre-Columbian civilizations. *American Anthropologist* 64:1–14.

WOLF, ERIC R. 1964. *Anthropology.* Englewood Cliffs: Prentice-Hall.