

Zeitschrift: Jahrbuch des Bernischen Historischen Museums
Band: 63-64 (1983-1984)

Artikel: Typology and interpretation
Autor: West, Frederick Hadleigh
DOI: <https://doi.org/10.5169/seals-1043498>

Nutzungsbedingungen

Die ETH-Bibliothek ist die Anbieterin der digitalisierten Zeitschriften. Sie besitzt keine Urheberrechte an den Zeitschriften und ist nicht verantwortlich für deren Inhalte. Die Rechte liegen in der Regel bei den Herausgebern beziehungsweise den externen Rechteinhabern. [Siehe Rechtliche Hinweise.](#)

Conditions d'utilisation

L'ETH Library est le fournisseur des revues numérisées. Elle ne détient aucun droit d'auteur sur les revues et n'est pas responsable de leur contenu. En règle générale, les droits sont détenus par les éditeurs ou les détenteurs de droits externes. [Voir Informations légales.](#)

Terms of use

The ETH Library is the provider of the digitised journals. It does not own any copyrights to the journals and is not responsible for their content. The rights usually lie with the publishers or the external rights holders. [See Legal notice.](#)

Download PDF: 18.10.2024

ETH-Bibliothek Zürich, E-Periodica, <https://www.e-periodica.ch>

Typology and interpretation

Frederick Hadleigh West

Contents

1. Introduction
2. Kinds of types
 - a) Formal
 - b) Functional
3. Inference and probability
 - a) Analogies with modern tools and functions
 - b) Ethnographic analogies
 - c) Use wear and consequent alteration
 - d) Levels of probability
 - e) Attenuation of reconstructive capacity
4. The type and the species
5. The dichotomy between culture and biology
6. The type in the systematic hierarchy
7. Archaeology in anthropology

“The study of the past therefore begins with the present”
(M. R. COHEN and E. NAGEL 1934, 324).

Introduction

It is to state the obvious to say that the task of archaeology is the delineation and interpretation of human history that is beyond the range of conventional record. The statement is obvious; perhaps trite even. It is also, of course, so imprecise as to be virtually specious. While we may *aspire* to delineate and interpret the past neither of these objectives is ever attained. This is an immutable fact that must be accepted as a first principle of archaeological science: *the unrecorded past is unknowable*.

The archaeological past comes to us as a remarkably variable spatial series of physical objects and traces of objects that are assumed to derive from the activities of past peoples. By means, or methodologies, likewise variable, the attempt is made (1) to discover or impose order on these series which procedure, when carried to what is judged its fullest feasible extent, will then (2) yield a (not, *the*) “narrative” of those particular past activities.

My purpose here will be to examine the kinds of narrative produced in archaeology especially through the *devices* which themselves appear fundamental to the construction of narrative. Were it not possible to discover some kind of regularity, some degree of repetitive patterning in archaeological evidence there could be no science of archaeology. Put another way, it is the recognition of repetitive patterning that allows for the ordering of data. That ordering, in turn, is requisite for the *systematic* presentation which by convenience we term “description and interpretation”.

Archaeological evidence may be said to provide a basis for two, and only two, kinds of narrative. *Formal narrative* treats of the characteristics of the physical evidence (principally objects) and attempts, by the discrimination of repetitive morphological patterns, or forms, to construct a network of relationships. *Functional narrative* seeks, by means of the physical evidence, to reconstruct uses and interrelationships of those evidences in order to reconstruct all, or facets of, the former functioning society.

These two approaches, while in no sense antithetical, are nonetheless sufficiently dissimilar that the prosecution of one may involve little reference to the other.

Regardless of the type of narrative its basis must, however, reside in the physical evidence. In largest measure, therefore, the narrative will consist of a structure composed of typologies. Without typology there can be no narrative. The type is the one absolutely essential element for all subsequent ordering of archaeological data. As will be suggested below the type in archaeology finds an analogy in the *species* of biology.

Despite the fact that it has to do preeminently with *ordering*, the subject of typology itself is one which may be said to have a certain mercurial quality about it. Perhaps it is for that reason that discussions of typology, in my view, very often seem to originate not at the fore part of the problem, but rather somewhere in the middle. That is to say, most discussions of *typology* deal almost exclusively with *technique*, with the means by which types are to be established. Very likely there is no harm done by this approach; it may in fact be the course of greatest utility. Still, it does seem that a protracted consideration of objectives might help to

avoid some of the fruitless discussion that sometimes accompanies those typological disquisitions which, whether by design or otherwise, deal exclusively with technique while seeming actually to want to address the more fundamental question.

The following brief excursion into this somewhat murky area will aim at elucidating some of the tacit assumptions upon which most of us seem to operate as we set about archaeological analysis. At the same time I shall try to say something about the intrinsic constraints operative in typological analysis which appear not always to be recognized, but which systematically circumscribe the successful attainment of our objectives.

Kinds of types

Perhaps it is necessary at this juncture to render homage to the rather pious assertion, often heard, that there is no limit to the number of types that can be derived from the same material. It would be convenient to simply dismiss this as a kind of archaeological one-liner aimed at causing consternation and wonder among the workaday folk. However, at bottom there is to be found here the old controversy as to whether types are made or discovered. Since it appears the answer to both those questions is yes, something further must be said.

If by this assertion it is meant that, of all possible discrete observations (and/or, attributes), the number of possible combinations is restricted only by the agility of the analyst, then, frail though it is, the logic holds and one must grant it verity. If beyond this it is held that any one combination is, or may be, just as meaningful as any other, then I must say the small amount of interest I might have had in this aspect of the subject wanes. It seems a line of thought destined only for blind alleys. As scientists we do not seek to impute order into the universe, but rather to discover it. If that seems at variance with suggesting that types are both made and found, it would seem to me that the answer to that dilemma is apparent. We *seek* to discover those regularities of prehistoric behavior which we must assume existed and to characterize these *modes* as types (though also on other levels of organization as well). Since one of the most evident difficulties under which we labor is the inability to observe directly these behaviors, there is introduced at once one of the intrinsic guarantors that our types will not correspond to what were, by their makers, construed as disparate categories of objects.

There are, obviously, other factors which constrain to produce variances in typologies—on the same time level, among the same people, and, indeed, by the same person.

To recognize these constraints, some of which are considered below, does not necessitate subscribing to the infinite number of typologies idea nor to its corollary that all our actions as typologists are thus completely arbitrary.

No matter where they might stand on the questions just touched upon, most archaeologists in practice are concerned with the organization of their data in the most usable form and set about it in ways approximate at least to a process of discovery.

The most obtrusive property of an artifact is its form. *Form* may be defined as a summation of all those physical attributes the interplay of which has produced the object as found. The process of producing the artifact may have commenced with the idea as to its ultimate appearance held by its fabricator (which broad category may in fact constitute the highest number of variables) and would proceed through a series of others such as material employed, the kind of workmanship, etc. In lithic specimens form may be further altered to greater or lesser extent by attrition, often discernible as patterns of beveling, of accidental minute fracturing, etc., to be found generally on cutting, piercing, or other types of working (or vulnerable) edges. Usually ascribed to use, there is produced by it only moderate alteration to form.

Every archaeological manifestation has a form, which is simply to say that it can be visually perceived. (On certain wet and frozen sites there are, of course, archaeological manifestations that are perceived by other senses.) The forms which repeat, which are recurrent, will be classed as formal types. In most of archaeological literature, “type” refers to the formal type as here defined. The formal type has an invaluable but unfortunately not invariable property: it had a fixed (and determinable) locus in time and space. A formal type is the product, neither of whimsy nor of idiosyncrasy; instead it reflects collective behavior norms of its place and time. The great utility of the formal type derives from (1) the fact that when combined with all other similar orderings, there is allowed a systematic, more or less objective, description of the data (site or assemblage) and (2) from the conviction that the regularities observed by the archaeologist are some reflection of the normative behavior—or culture—of the demised artisans. On these bases are established our putative histories. The study of the dispersal of forms through time and space is a continuing concern of the discipline.

The analysis of function in archaeology is, if not altogether new, at least an approach that has received a great deal of recent attention. Unfortunately, some of this attention has been of a sort, perhaps a little naive, that has sought to claim a methodological superiority for it that is quite unjustified on any grounds.

We may speak then of dichotomous systems of analysis: the formal and the functional. The functional analysis of tools is one which would attempt to determine the purpose for which the tool was designed and the use to which it was put. Interpretation of implement function in the context of one assemblage, combined with distributions of all other such observations, could give rise to a functional interpretation of the site (assemblage) that would yield a totally different characterization of the site than would the formal interpretation. Spatial relationships of implements, of domestic features, characterization of structures, to each other and of sites to each other allow, it is held, reconstruction of non-tangible aspects of the culture. The functional interpretation could proceed without reference to time, without any reference to formal types present in the assemblage, and reach a series of conclusions and generalizations which would be quite meaningful within a system of functional archaeological analysis but be independent from and totally without reference to the formal system. (Ecological interpretation is understood as a special case of the functional.) Here emerges, however, a problem: *these are different systems of analysis*. There appears to be no way that their results can either be melded or meaningfully compared. Carrying, in this system, the interpretive weight of the type, *knife* is a useful functional artifact grouping. On the formal level, lacking formal typological standing or definition (e.g., Cody knife), the designation “knife” is one cut above “stone artifact” as far as its interpretational value is concerned. Formal types, contributing to a formal hierarchy of archaeological manifestations, may allow the construction of a network of temporal and spatial relations which could result in an historic or evolutionary interpretation of a culture form, of a region, etc. A parallel kind of network could be constructed functionally which theoretically could lead to the reconstruction of the societies and their internal and external workings. In either case, while the operations are similar, the objectives and the results will be different.

The distinction between these two systems is a logical one and appears to admit of no transgression. Obviously, an investigator working in one assemblage could make both kinds of analyses but the contribution of one to the other must be small and it would appear epistemologically wiser that the distinction between the two kinds of interpretation always be clearly drawn. Failure to maintain that distinction gives rise to a great deal of pointless discussion on the matter of archaeological interpretation. Better a world in which oranges remain oranges and apples remain apples.

Inference and probability

Ours is not an observational science. This simple fact imposes upon archaeology its most severe methodological constraint. It might be said, in fact, that it defines the very nature of the discipline. Archaeology, like palaeontology, like historical geology, is a reconstructive science. Though there has been some tendency to denigrate the relationship as being un-anthropological, the continuity of archaeology with earth sciences is always manifest. In consequence, the greater part of the field and subsequent procedures of archaeology relate, not to any area of cultural anthropology but rather to geology, palaeontology, and related sciences. On the highest interpretive level, obviously, recourse must be had to the parent discipline, ethnology or anthropology—but only in part. Geological proveniences bulk inordinately largely in archaeological interpretation and find no corollary in ethnographic literature. The same may be said for the painstaking description of usually “simple” forms characteristic of lithic artifacts. Rarely does this kind of fare find a congenial audience among cultural anthropologists. Other earth historians, if not enthralled, are at least sympathetic since similar undertakings are their task as well. The point here (a digressive one at that) is not to suggest that archaeology should reaffiliate but rather to emphasize that there are some clear and abiding disjunctures between archaeology and the rest of anthropology. Further, the fact that they exist should serve constantly to remind us that our interpretations cannot ever be at one with those anthropologists who work with living peoples. We operate instead under the same limitations and constraints on our knowledge as characterize the efforts of geologists and palaeontologists. *We infer relationships*—whether the aim is formal or functional interpretation. With inference comes a dependence upon probability theory. We may as well be forthright: (apart from autobiographical information) virtually everything an archaeologist says is probabilistic in nature. As auditor our task is to try to assess the level of probability which attends the archaeologist’s various pronouncements. As archaeologist it is to make that degree totally clear. That it is now possible to perform the necessary demonstrations so much more elegantly than in the past (see J. E. DORAN and F. R. HODSON 1975) in no way relieves us of the necessity of recalling that we are dealing in inference and nothing else. Acknowledgement of this fact is fundamental to our integrity as scientists.

One of the principal means by which the function of prehistoric tools is inferred is that of analogy. Although it is seldom formally invoked the analogy with modern (or near-modern) implements is evident at virtually all stages of archaeological discourse. Thus the labels “spearpoint”,

“harpoon head”, “scraper”, “burin”, etc. In a measure, of course, these names are applied simply as terms of convenience; it is exceedingly difficult to describe an artifact or to discuss it in any larger context without having a name for the item in question and for those to which it is being compared. It is my conviction that, recognizing their indispensability, these common names—always recognized as terminology of convenience—should be used as freely as the subject matter warrants. It does not seem efficient to try to develop a battery of names which are functionally neutral; the result is the same kind of impediment to thought that results from formal typological designations such as “Type 2 b”.

In fact, of course, whether they are designated terms of convenience or not, it is certain that in most cases when an elongate, sharp-edged stone implement is called a “knife” it is because, viscerally, that is what it is considered to have been. The archaeologist may relieve himself of the stigma of having made an unverifiable functional identification by the formula of overtly labeling it as speculation. The fully developed functional account of a site should require some demonstration of the probability that the functional names assigned to tools, domestic arrangements, etc., are reasonable. The value of the interpretation must be closely linked to the careful construction of a hierarchy of probabilities: tool forms as to functional types, site areas as to use or activity types, and so on up the line.

In conjunction with the use of the modern analogy there is frequently found—usually recognized and acknowledged—the use of parallels or similes drawn from observations among the world’s tribal peoples. These ethnographic analogies, when properly applied, may be assumed to confer a higher degree of probability on the functional identification than will be the case with the strictly modern analogy. Actually, of course, as observed by H. L. MOVIOUS et al. (1968) the kind of detailed ethnographic description the archaeologist would like to see simply does not exist. Arctic archaeologists have for a long time enjoyed the unusual circumstance of being able to have their artifact finds given Eskimo names *and* functional identifications by individuals working as excavators and guides. The standard categorization used in descriptions of Eskimo archaeology reflects this fortunate bias; all artifacts are placed into functional categories such as “men’s tools”, “women’s tools”, “seal hunting gear”, and the like. Even though the styles of decoration may have changed, the implement forms may retain a high degree of consistency and the resultant identification enjoys thus a similarly high degree of probability that is a correct one. However, inference and probability are involved here no less than in any other case—even if the identification is being suggested by a local Eskimo. The ma-

terial being excavated was not observed in use, much less collected from its user. The same end scraper form that recently functioned uniquely in working with the leg skins of caribou may have served as a woodworking tool in earlier times. The addition, thus, of use mark analysis to the identification serves to refine the identification (or increase the level of probability) but the fact remains that the identification is a probabilistic one.

I should like to add one other observation in the area of functional identification, the implications of which are somewhat tangential to what has been said before. As a tool concept *knife* is functional. Its requisites are clear. The same may be said for the tool concept *spear*. Each has clear and different functions that coincide only to the extent that each may possess long continuous cutting edges. On the one hand this characteristic is intended to sever internal organs, cause internal bleeding and death as it passes into the body of the prey; on the other that identical characteristic is intended to provide a long cutting edge which is generally concerned with more static and external matters over a wide range of materials. In an ethnographic film (not a commercial one and the title now forgotten) made by a Jesuit missionary in the former Belgian Congo there is a sequence in which the Pygmy hunters dispatch an elephant with their broad-bladed thrusting spears. Once that operation was performed they set about butchering the animal using the same spear now as a knife simply by grasping the spear shaft up high just below the cutting edge of the blade. (Interestingly, Joe Ben WHEAT [1975], in analyzing use wear on certain western American point forms, has suggested a similar kind of dual function was served.) I am not certain what moral is to be taken here unless it is as a small addition to that body of statutes called Archaeological Uncertainty Principles—to which we should all refer.

Certain reasonable estimations of function may be made from implement morphology as well as those which may be inferred from use wear. Actually, of course, in the latter case there should arise immediately the question whether or not the wear results from use at all or from some other causes. Thus, preceding the question what was the character of use, there is another more fundamental one: is it use wear or is it instead some accidental pattern which somehow resulted from the manufacturing process (perhaps from the way a blank may have been immobilized from shaping), from deliberate dulling (“backing”) to facilitate its handling in use, or hafting requirements at some stage in the tool’s manufacture or use or to other occurrences that might alter a rather fragile object? Once more the answer arrived at is not final but suppositional. (It would be good to derive some classificatory term to cover all these forms of subsequent alteration or wear that may be found on lithic arti-

facts. The differentia is simple: these marks comprise all those of a character not judged necessary to the manufacture of the basic finished form. Perhaps “consequent” or “accidental alteration” would serve.)

A great deal of formal and informal experimentation at replicating use wear has helped to provide sounder bases for reconstructing tool function. The minute, steep, and irregular retouch found frequently on the working edges of end scrapers is familiar to most. It is possible immediately to determine the direction at which the working end attacked the material that was worked. Whether it will ever be possible, however, to recognize with any high degree of probability whether wood and or bone was the substance worked or whether “hard materials” vs. “soft materials” will remain the finest level of discrimination available remains to be seen. “Probability” remains the operative term.

Tool morphology, considered in gross terms, obviously conveys something about the probable employment of the implement. An ellipsoidal pebble with battered ends is unlikely to have served as a knife even in the most primitive of eras. Accordingly, it will be called a “hammerstone”. But, perversely, we know it could also have functioned as an anvil stone.

The conventional view of the burin being used as an engraving tool has been shown to be a sometime thing. F. BORDES (1965) has presented evidence that the function of at least some (technological) burins was of quite another sort. (Interestingly, this finding is paralleled by observations made on our Alaskan Denali complex burins in which the most frequent wear pattern is found medially along facet edges.) The terms of convenience used for artifact forms reflect “common sense” deductions from tool morphology. These undoubtedly have positive as well as negative aspects: were we members of some other order, the gross shape requirements of implements would be, accordingly, different. This is simply another way of suggesting that many of the conventional names used in archaeological parlance probably do bear some reasonable relation to reality. The difficulty comes in assessing the exact degree of proximity to the truth. Surely, had F. Bordes’s observations on the probable use of burins come up before implements of that class came to be termed “burins”, they would not have been called by that term at all. They might instead have been termed “shaves” or “drawknives”. While to all workers today dealing with these implements, the name “burin” connotes a technological class, in point of fact the name is, ineluctably, a functional term and an ill-fitting one at that. As such, it indicates the difficulties associated with an uncritical acceptance of terms deriving from common experience.

It may be noted that the greater part of the discussion above has revolved about the difficulties in assigning functional identities or functional type standing to artifact forms. It does not seem that difficulties as severe characterize the process of establishing formal types. Moreover, the difficulties that do exist here are of another sort. Here, at least as a first step, there is involved what J. E. DORAN and F. R. HODSON (1975) refer to as “pattern recognition”—the grouping of like with like. This may be carried out without any recourse whatever to second guessings as to how these groupings may have corresponded to the realities that pertained when the objects in question were made. The objectification of the groupings so established may be fairly simply accomplished and the assumption (corresponding to those existing on the functional level) is allowed that these groupings will correspond to the disparate categories of the original manufacturing processes (*not* to the typological concepts that may have been held). While some semblance of the estimated function of the tool usually is found in its formal designation, there is no requirement that it be there. Moreover, differentiating it from the functional type will be a nomenclatural element in the designation that speaks to its peculiar form. It is thus not a burin but a *burin de Noailles* or a *Folsom* projectile point. If well-characterized, well-established, the formal type will be readily distinguishable in any matrix of archaeological materials. In the primary sense, however, it must be strictly a *formal* identification. The meaning which attaches to it is not to be discerned in the form itself; it is rather a matter of definition.

To express the demand that the type must have “historical meaning” would appear to be gratuitous. The type *will* have historical meaning (i. e., given, again, that it is well-founded) because only in small measure is it a creation of the typologist; in far greater part it is the product of scientific discovery. To say that the formal type is in part a creation of the typologist is simply to recognize the inherent imperfections of any scheme of classification. Of far greater importance is the recognition that the type we seek to characterize is nothing more than an explicit expression of those regularities that characterize culture just as they characterize nature. Animal behavior is systematic. That which differentiates human behavior is its vast elaboration and with it the capacity—drive, perhaps—to produce the extra corporeal baggage that is our subject matter.

Beyond the form which characterizes the type, there are those properties of provenience. Because these forms are expressions of regular behavior they have fixed loci in time and space. It is, therefore, redundant to insist that the (formal) type have historical meaning; that is one of its invariable attributes.

Lest what has just been said sound more mechanistic than is intended, it is obviously not meant that particular formal artifact types somehow issue forth from the species; artifact forms arise in and issue from cultural systems having histories peculiar to themselves alone. Therein, once more, lies their reconstructive power. In similar corrective vein, it is not being said that the type should be conceived as rigid and invariable or capable of facile definition. These are problems beyond the scope of this essay.

Of the two lines of interpretation it would appear that the highest levels of probability and the greatest predictive value clearly inhere in that of formal analysis. The reasons seem to be basic, inherent, and systemic.

Whether the analogy used is modern or ethnographic (if I may make this distinction), it always is essential to archaeological interpretation. It is the principal tool of reconstruction. Unfortunately there is an attenuation of its reconstructive capacity with increasing antiquity of the archaeological manifestation in question and that attenuation is both more acute and more quickly reached in functional interpretation. Most often when this problem of attenuation is considered it is in terms of a kind of historical view, that simply too much unknown history intervenes between us and the events we would wish to interpret. There is another factor, however, of such enormous power in its own right that, when considered in conjunction with the sheer weight of unknown history, virtually guarantees that interpretations of most ancient prehistory will remain, at best, crippled: Our analogy is so built as to operate within the confines of one species. While we will fully recognize the play of history, we construe it, correctly to a point, as operating on our own kind and thus in ways that are at least recognizable to us. What seems often overlooked are the implications of applying our behavioral analogies across species and perhaps generic boundaries. It appears that the earliest records for completely modern man run back only on the order of 40,000 years. It would be folly to assume that the biological differences observable between *Homo sapiens sapiens* and *Homo sapiens neanderthalensis* did not carry with them behavioral consequences at least as marked. There are, in short, a number of major disjuncts, increasingly serious in nature, which demand consideration as prehistory is worked back. Acting in concert with these imponderables is an incalculably greater and greater burden of history.

The type and the species

The *type* (the reference hereinafter is solely to the formal type) has an interesting parallel concept in the *species* of biology proper. There is close similarity in the character of argu-

mentation concerning definitions, tactics, implications, taxonomic vs. functional species and the like, and thus, one must suspect, much that may be learned here. Biological arguments on numerical taxonomy are a good fifteen years in advance of ours (viz., R. R. SOKAL and P. H. A. SNEATH 1963; V. H. HEYWOOD and J. MCNEILL 1964) and the debates that have been held in systematics over that period may be read with great profit by archaeologists who, if we are fortunate, stand on a threshold identical to that of biological taxonomists of fifteen to twenty years ago.

One of the parallels that is immediately apparent in this context is that the archaeological type must occupy the same pivotally important role in our systematics as does the species in biology. Arguments that the archaeological type must be historically relevant find exact correspondences in those demanding that the species, to be meaningful, etc., must be derived phylogenetically. The purely morphological type (i. e., divorced from any consideration other than its form) is parallel to the phenetic species of the biological systematist. At the very least the archaeologist, after trying to sort through these problems within his own discipline, can take some comfort in discovering colleagues of another field in identical straits (V. H. HEYWOOD and J. MCNEILL 1964).

Certainly the original classifications leading to typologies were established as devices of convenience—that of reducing a mass of unorganized data into manageable units. It is doubtful whether any such simple consideration has in the past 50 years motivated the archaeologist to classify his data and perhaps establish type keys. Instead, in virtually all cases, this has been carried out primarily in the wish to determine affinities. Thus, inadvertently perhaps, the archaeologist has been pursuing that which is, among biologists, often considered the higher aim, that of classifying into relationally meaningful categories.

How far and how fruitfully the analogy of type and species may be followed is not to be answered here. There is nothing scientifically or logically wrong with following analogical reasoning. It may indeed, be considered indispensable to systematic thought. The trick, one may suppose, is to determine how to dissociate the two problems at a point where maximum benefit has been gained from the available parallels but before reaching that at which the cases actually cease being reasonably analogous and the solution to the original problem is thus flawed. It is completely defensible, I think, to maintain that the analogy holds between the crucial importance of species and that of type. Each is the essential foundation upon which further hierarchies are to be built.

The dichotomy between culture and biology

The species-type analogue may be viewed as a specialized aspect of the general biology-culture analogy. Oftentimes the impression is given that this kind of approach is fundamentally wrong because it seeks to apply thought gained from what is seen as a mechanistic, law-driven system of nature, to an area where the machinery is far more delicate and whimsically subtle. Here the driver is most frequently not identifiable, directions are discernible only after the fact, and the application of schema from another discipline is seen as posing dangers to the fragile nuances of human history. Perhaps this is over-stated for it would appear to close the door forever on the possibility of a science of man. It is unlikely that many today would find it worthwhile even to discuss the counter-case much less to defend a position proved untenable in the 19th century. And yet, there still pervades these discussions in anthropology an attitude that harks back to the earlier one, that somehow if it is ultimately concluded that the behavior of man and his cultures is found amenable to description in terms of scientific laws, they will be of a different character from those of other sciences. The reason often expressed is that culture simply represents a different order of things. Two observations seem pertinent here: no reasonable definition of tool-use, that most essential characteristic of culture, is able to limit this form of behavior to man. There *is* no quantum difference between tool-use by certain other animals and that seen in the genus of man. Secondly, despite the accretions which, especially in more complex cultures, serve to mask the fact, culture may be seen as an adaptive device and only that. This is not to suggest that once set in train it does not, in a real sense, follow rules which arise within the system. Nor is it to say that there are no developments or excrescences in more complex cultures the adaptive values of which are either absent or difficult to determine. The reasons, again, are systemic. In any case, to insist upon a fundamental distinction between culture and biology is peculiarly unproductive for the sufficient reason that it seeks to rend the fabric of nature. Culture is, instead, best conceived in continuum with biology. The biological analogy then is something more than a mere convenience; it is a fruitful approach because it is appropriate. It was, after all, biological evolution in certain of the Hominidae that produced this particular behavioral tactic for survival. The regularity which characterizes culture is a continuation of, and of a piece with, the order which animates nature. The type is simply one manifestation of this order. It is no more an invention of the typologist than is the species a fabrication of the biological systematist (E. MAYR 1969; R. CRAWSHAY-

WILLIAMS in V. H. HEYWOOD and J. MCNEILL 1964, 81). To underestimate the importance of the type or, worse, to deny its objective existence is at once naive and destructive of the possibility of applying the method of science to the study of man.

The type in the systematic hierarchy

The methodical ordering of archaeological evidence aims at the reconstruction of human history and human evolution. If the word "cultural" is substituted for "human" the sense remains the same but, to me, the objective is unwontedly narrowed. However put, this seems, by common consent, to be the major aim of archaeology. A subsidiary one, then, is the functional reconstruction of archaeological evidence. The maintenance of this logical distinction is, however, essential.

The type gives characterization to a body of like artifacts. The summary of all types, computed as percentages of the whole, gives characterization to the artifact assemblage. The combination of all other data with that of the artifact assemblage will provide characterization of the component (or site if single occupation) which then may contribute to the delineation of the complex. An appropriate question is this: in terms recognizable to the ethnologist, what are the presumed equivalents of these rather antiseptic names? The type perhaps presents no great problem in conceptualization. The others do. On the formal level the question can be simply avoided; on the functional level it cannot. And, it must be admitted that, desirable as it is that we maintain clearly-drawn the distinction between the functional and the formal, there are instances where the interpretation on the one level becomes indispensable to the other. The Mousterian problem as seen at Combe Grenal seems a ready exemplar; the placement in higher levels of the classificatory hierarchy of those errant layers will depend upon whether those layers are proven to be simply *aspects* of each other or whether they are instead separable in the manner maintained by F. Bordes. In fact, if one were to refuse adamantly the crossing over of the two forms of interpretations the Combe Grenal controversy would evaporate; on strict classificatory grounds the layers are different and there, it would seem, would be an end to it.

To resume the quest for correspondences: "Locality" might be an ethnological and functional equivalent of the component—a place where people of one cultural grouping stayed sufficiently long to leave retrievable evidence of their former presence. The latter parts, of course, must be stipulated as they are not necessary parts of the understanding of locality. The term "complex", by general accord,

seems to be taken as an equivalent of the ethnographic culture—a population composed of several parts proximally disposed both temporally and spatially. “Tradition” in European usage seems to be cognate with “complex”. There are those who would equate culture as just used with tribal culture or “tribe”. It would appear that a large part of the difficulty in applying that concept archaeologically springs from its often nebulous character in the ethnographic context. The wiser course seems to avoid its use unless for some reason the evidence seems especially to warrant it. Speaking from the standpoint of some familiarity with living Northern Athabaskan Indians in America, I can attest that a certain amount of head-scratching accompanies the question whether the Netsi Kutchin are to be construed a tribe, or whether the Kutchin (of which there are nine such named groups) are the tribe. The decision, please note, is generally reached on linguistic grounds! I doubt seriously whether any archaeological perception could be drawn so sharply as to identify Kutchin as a demonstrably separable entity. “Northern Athabaskan” might more reasonably represent an equivalent to the archaeological culture. We could theorize about the meaning of the spatial and temporal entities contained within that category but they would probably remain third order speculations. How far the Northern Athabaskan case has applicability in this current context and how far it is peculiar to the North, I cannot judge. It might be remembered, however, that the political and societal laxity characteristic of Northern peoples which contributed so heavily to this particular cultural homogeneity, must also have obtained on earlier horizons everywhere. It will be unreasonable, therefore, to impute complex levels of organization to very early peoples when inference tells us they probably did not exist.

The attempt above has not been aimed at presenting either new terms or new ideas about the nature of these higher levels of organization. Accordingly the subject is left at the point of the archaeological culture or complex. These seem elemental constructs in formal interpretation. What is done thereafter may be, in part, a function of the interpreter’s particular objective.

Archaeology in anthropology

Very little has been essayed explicitly as to how reasonable objectives of archaeological typology *cum* full-blown interpretation might square with the objectives of anthropology at large. As suggested above there are basic differences between the procedures used and data collected by the cultural anthropologist and the archaeologist. It is sometimes said that we deal in “different kinds of evidence” and yet that is

demonstrably not true, which gives rise to the obvious response that since the evidence in both cases is cultural it must be possible for it all to be thrown into a common interpretive matrix whereby the resultant interpretations can be made directly comparable. Unfortunately there is a fundamental difficulty: it is not the nature of the evidence but rather that distinction between observation and inference. It is, after all, true that we can only infer that our subject matter (i. e., tangible subject matter) is actually man-made. All sciences proceed on the basis of certain necessary assumptions; this happens to be the one underpinning archaeology, much as the assumption that there is an external world may be held fundamental to science at large. Of the two kinds of interpretation the functional comes nearest to commensurateness with cultural anthropology. Yet it would seem that precisely here are the powers of archaeological reasoning feeblest.

As mentioned previously, Eskimo archaeology has long enjoyed the special privilege of being integrated on the spot, as it were, with Eskimo ethnography. This has best been seen in the well-known St. Lawrence Island sequence. There, with considerable confidence, functional assignments may be traced back all the way to Old Bering Sea or approximately 2,000 years. This is owing, of course, to the remarkable persistence in basic forms throughout that long period. Most students of Eskimo prehistory appear convinced that the earliest recognizable roots of this remarkable Arctic culture are to be found in the Denbigh Flint Complex (DFC). Yet the attenuation of formal resemblances is so pronounced between DFC and readily-recognizable prehistoric Eskimo cultures that most authorities will admit that the presumed ancestral relationship is primarily based in geography alone (H.-G. BANDI 1969). Needless to say, with the formal resemblances, upon which were based the functional identifications, virtually totally absent, Denbigh does not enjoy the interpretive advantages of its presumed direct offspring.

No amount of urging of archaeological evidence will contribute *new* data on human kinship relationships. Instead, what has been learned of that subject from living peoples may be *applied* analogically in a functional interpretation of archaeological records. The results must then be appraised in the light of probabilities. The more complex the application, the lower those levels will be. Furthermore, the attenuation of the reconstructive capacity—for whatever reasons—as the analogy is applied to the interpretation of older materials, reduces the level of confidence to the vanishing point.

This condition has its bright side. In no manner may the study of history be combined with the ethnographic record to even suggest anything like what is now known of prehis-

tory. The data simply are not there and this is the obvious corollary to and brighter side of that discontinuity separating the two. The conclusion one must reach seems to be that there are some restricted cases where archaeological results may be made commensurate to cultural anthropology. Generally, archaeology is best considered complementary; it is to that extent, despite us all, an independent contributor to the broad subject of anthropology in a manner analogous, vis-à-vis the parent discipline, to biological anthropology. Each contributes new evidence, not obtainable by any other means, to a thus vastly amplified understanding of man. The overlap areas, expectably, will be cultivated; the development of functional interpretation should proceed, conscious, firstly of its limitations, then of its capabilities. It should be clear, however, that this is much the smaller part of the contribution to be made by archaeology. Clearly, it would be fundamentally wrong to try to narrow and restrict archaeological inquiry by forcing it into a mold it cannot possibly fit. Recognition of the capabilities and the limitations of archaeological interpretation is, in many ways, best exemplified by a consideration of the preeminent place of the type.

References cited

- BANDI, H.-G., *Eskimo Prehistory*. College 1969.
- BORDES, F., Utilisation possible des côtés des burins. *Fundberichte aus Schwaben*, N.F. Band 17. Stuttgart 1965, 3-4.
- COHEN, M. R. and NAGEL, E., *An Introduction to Logic and the Scientific Method*. Harcourt 1934.
- DORAN, J. E. and HODSON, F. R., *Mathematics and Computers in Archaeology*. Edinburgh 1975.
- HEYWOOD, V. H. and MCNEILL, J., *Phenetic and Phylogenetic Classification* (The Systematics Association, Publication No. 6). London 1964.
- MAYR, E., *Principles of Systematic Zoology*. New York 1969.
- MOVIUS, H. L., DAVID, N. C., BRICKER, H. M. and CLAY, R. B., *The Analysis of Certain Major Classes of Upper Palaeolithic Tools* (American School of Prehistoric Research, Peabody Museum, Harvard University, Bulletin No. 26). Cambridge 1968.
- SOKAL, R. R. and SNEATH, P. H. A., *Principles of Numerical Taxonomy*. San Francisco and London 1963.
- WHEAT, Joe Ben, (Paper on re-use of artifacts delivered at Society for American Archaeology Meeting, Dallas 1975).

Dr. Frederick Hadleigh West
 Curator of Archaeology
 Peabody Museum
 Salem, Massachusetts 01970/USA

