# CHAPTER 6: CLASSIFICATION IN PREHISTORY

In order to draw directly upon the propositions explicated in Part I, it is necessary to be able to treat prehistory as a special case of science, as a distinctive restriction of the general field. The definition of prehistory provided in Chapter 5 permits this kind of derivation by stipulating the kind of restrictions required to convert science, an abstract notion, into the science of artifacts, one of the” several special sciences. Drawing upon that definition, prehistory can be viewed as science restricted to the explanation of artifacts in cultural terms. Systematics, ~E means of formulating units and the subject of discussion in Part I, is held in common by all the special sciences; however, the specific form of the units employed and the kinds of choices and decisions made in their formulation are distinctive for each science. It is the particular kinds of arrangement of phenomena, governed in form by the theory of the special science, that provide the basic material for the science and its operations. Phenomena categorized for use by a specific science are customarily called data, and the term data will hereafter be restricted to such categorized phenomena. Phenomena will be retained for things and events without such categorization. In the widest sense, the data of prehistory are artifacts.

Since the means of segregating artifacts from other phenomena was necessarily discussed in defining prehistory, the problem to be considered now is how these data are structured for explanation by prehistory. Given our restriction to formal theory, the logical transition from science in general to a science of artifacts is a matter of the derivation of cultural arrangement from arrangement in general.

Systematics is necessarily part of any scientific endeavor, though it is rarely the focus of that endeavor. The means by which the units used have come into being and how they are identified in the phenomenological world are usually implicit, the investigator having learned implicitly what has been traditionally employed. Evaluation of those units is even less common than an explicit presentation of them. Prehistory represents no exception. This deficiency is of far more import for prehistory than other physical :sciences because, as has been indicated, the subject matter of prehistory cannot be viewed as something external to the investigator. The investigator is part of it, and so is his work. The temptation for him to use his own cultural background as theory for creating and manipulating units, rather than treating this background as subject matter, is great and deleterious. Explicit systematics, however, enables the prehistorian to separate his cultural background analytically from the theory employed in his investigations, as well as to make poorly expressed or unexpressed theory explicit.

There are, of course, important exceptions to this malady of implicitness in the literature of prehistory, works which thoughtfully consider systematics in relation to both phenomena and problems. For a variety of reasons they have not, however, been overtly and systematically employed either by the substantively-oriented majority of prehistorians or the increasingly large body of statistically-oriented tacticians. In spite of the paucity of overt use, Irving Rouse's Prehistory of Haiti: A Study in Method and J. O. Brew's The Archaeology of Alkalai Ridge, along with two articles, Alex D. Krieger's “~rhe Typological Concept” and Albert C. Spaulding's “Statistical Techniques for the Discovery of Artifact Types,” form the implicit basis of almost all of the archaeological literature that might be called prehistory. It is difficult to assess whether these works have actually, in an historical sense, been the derivation of the units employed in the literature, or whether they are simply overt expressions of a pre-existing but implicit approach by Prehistorians. Regardless of this point, the traditionally employed arrangements of prehistory are understandable in terms of the notions advanced in these studies. Perhaps the most remarkable element is that none of them has led to any measurable increase in the explicitness of systematics in the discipline. Primary among suspect causes of this condition is the fact that none presents a holistic scheme completely free of substantive connections. Further, each, to a greater or lesser extent, is unnecessarily infused with inferential properties. It has been these inferential aspects that have received elaboration by the authors and subsequent students, and these same aspects which have suffered severe and justified criticism and rejection. The article by Spaulding is concerned both with grouping of the first kind discussed in Chapter 4 and with classification, while the studies of Brew, Krieger, and Rouse are primarily within the field of classification. By necessity, these latter three studies and their subsequent elaboration form the basis of the examination of classification in prehistory undertaken here, as the Spaulding reference forms the basis for grouping in the succeeding chapter.

Within those arrangements distinctive of prehistory, classification plays the crucial role in the transition from science in general to a science of artifacts, for, as has been shown, classification is the only means of creating the intensionally defined units necessary for science. These units, as in other kinds of science, become the data in that they subsume all the relevant attributes of the phenomena for the particular kind of inquiry represented by prehistory. Further, they provide the terms by which the data can be discussed and manipulated. It is useful then, to lay out the specifications that all classification must meet for prehistory in general terms before treating more specific forms.

Recalling the earlier considerations of classification, it is necessary that the field for the classifications be defined, along with the problem to which the classification is directed and the attributes to be used in creating classes. At this most general level of concern, the field is that encompassed by the concept artifact, objects which owe some of their attributes to human activity. The problem similarly is to provide categories for these data that are cultural, for the ultimate purpose of explaining the products of human behavior and with them the behavior that created them in terms of ideas held in common by the makers and users. It should be re-emphasized here that location n the three-dimensional world is an attribute of an object as much as its color. Quite obviously some additional assumptive elements are required beyond those necessary for the construction of classification in general in order to derive classifications which meet these special stipulations. Indeed, the soundness of the formal basis of prehistory, and thus of prehistory as a branch of scientific inquiry, can be assessed from (and is a function of) the number of additional assumptions that must be made.

The additional assumptions are introduced in specifying the general characteristics that features used to create classes must display. The general field from which definitive attributes may be drawn is implicit in the notion of artifact. Only those attributes which can be assumed to be the result of human activity are useful. The identification of such attributes is a product of comparative study similar in all respects save scale to the identification of artifacts themselves. Stipulation of the appropriate field of attributes insures that objects identified as products of human activity will be further structured as products of human activity. For example it is quite possible to use a set of artificial attributes, either intuitively or overtly, to identify an object as an artifact, but then to further categorize the object as to kind in terms of natural attributes present on the object only incidental to its nature as an artifact. A clam shell as an element of a coastal shell midden can be readily identified as an artifact and this is generally done, though not necessarily under the term “artifact.” It is quite possible, however, that the total sample of clam shells might be categorized in terms of color, resulting in brown mussels, white mussels, and brown-and-white mussels. If the differences in color are due to differential preservation of the outer horny layer, the use of color as a dimension of features is clearly erroneous in a cultural classification. These kinds of errors are avoided by an explicit statement of the general requirements that must be met by an attribute for the purposes of prehistory.

It must be emphasized that the suitability of any set of attributes must be determined for each particular case as a product of a comparative study. No absolute list of attributes can be drawn up and labeled “relevant” or “cultural.” The attributes which can be shown to be relevant will differ from case to case. The material from which artifacts are made provides an excellent case in point. Within the realm of stone artifacts, the chemical composition is unmodified--only the shape is changed. Nonetheless, the chemical composition is frequently cultural, being the product of selection manifest as artificial locations. Often the relevance of chemical composition goes no further than its effects on whether the material will chip or crumble upon impact, a simple two-feature distinction. Only with detailed comparisons with the environment, however, will one be able to ascertain whether this simple set of features is adequate or whether a more complicated set involving color, texture, hardness, etc., are involved in the selection. The occurrence of only sandstone rocks as heating elements in earth ovens, when both limestone and sandstone are available, indicates that one was given preference and that the set of features used in creating a classification for this material must differentiate the two. Anyone familiar with the characteristics of these materials when heated will readily appreciate the reasons behind such a preference. Again, the point is that no absolute set of features can be set forth as universally relevant. Much in the same manner that a linguist must convert his phonetic record of speech into a phonemic record which is cultural, the prehistorian must demonstrate by comparison the relevance of the features to be used.

The use of the term “cultural” to mean relevant for explanation m terms o~ ~he concept culture is premature at this juncture for the definition of culture not only stipulates the element of human involvement (ideas) but restricts this general field to that set of ideas which can be assumed to be shared. This is a most crucial point for it is here that the articulation of phenomena With concepts Is made. This connection necessarily must be made by means of assumption. There are no articulations between the abstract and the real which are observable or demonstrable. Clearly, the assumption made is the formal foundation for all of prehistory, constituting the means by which science becomes the science of artifacts and serving to differentiate prehistory from other sciences. While there is no overt consideration of this point in the archaeological literature, it is implicitly considered m many works and the nature of the assumption is quite clear. Prehistory assumes that attributes which are the products of human activity and which recur over a series of artifacts (termed features) can be treated as manifestations of ideas held in common by makers and users of those artifacts.

Thus the link is made between the phenomenological and the ideational. In spite of its simplistic appearance this assumption has several ramifications which require exploration. Because it is the basis of all prehistory, the reasonableness of the assumption must be questioned. The importance of restricting the possible set of attributes to those which are demonstrably products of human behavior is evident. If the attributes considered are only those which are the products of human endeavor, it follows that any explanation of those attributes is necessarily done in human rather than natural terms. If their distinctiveness lies in their humanness, then so does their explanation. Further, given our assumptions about the uniqueness of the phenomenological world, recurrence or sharing necessitates an ideational element in the explanation.

Some kind of classification is required as the vehicle of explanation. If several objects hold features in common, and those features are of human origin, there is but a single plausible account: Intentionally or unintentionally, consciously or unconsciously, the objects were made to look alike by people who can be treated as possessing similar ideas about them and who have the same categories of features and ways of articulating the features into whole artifacts. In short, the objects can be treated as expressions of the same mental template. Now obviously this connection can be challenged in any given case by special explanations utilizing natural processes and chance; however, given the large series of cases represented by artifacts, infinite for all practical purposes, such challenges are trivial. No other single account is capable of subsuming all of the cases at hand..

Nonetheless, given both the language available for stating the assumption and the discussions presented in archaeological literature, important potentiality for misunderstanding the assumption exists. Three aspects need to be made abundantly clear in order to avoid any serious misunderstanding: (I) the locus of shared ideas; (2) the means by which they are shared; and (3) the scale at which they are shared. Each of these is treated briefly below.

1. LOCUS. While it is common to impute, at least for literary convenience, the sharing of ideas to the makers and users of artifacts, clearly this cannot profitably be demonstrated or held to be true. Ideas are 'not observable-only behavior and its results are. There is no way to know what, if anything, goes

on inside a living person's head, let alone a dead one's. The “sharing” element lies in the process of converting unique attributes into features which can recur, a process done by the prehistorian as the intuitive first step in analysis. What is important is that the recurrence of features over a series of objects can be treated as if there were such a force. As long as the units are systematically tested against phenomena, there is no point to querying whether or not the makers used the same categories as the investigator did, for the testing insures that the same end product is reached regardless of the route taken to get there. It is immaterial, for example, whether in learning to identify plants in some exotic language you use the same criteria as do the native speakers, so long as whatever criteria you do use produce the same assignments. There is no way to demonstrate that your criteria are the same as those of a native speaker or that the natives even share among themselves a single set of criteria. One thing that this discussion does indicate quite clearly is that “culture” is implicitly used by prehistorians, at least in the initial stages of classification, as other explanatory concepts are used in the physical sciences.

Since there has been some attempt to link the classifications of prehistory with the “folk classifications” of subject peoples (principally in cultural reconstruction approaches), some consideration of this specific aspect seems warranted. Above it was argued that this kind of linkage is unnecessary. Further, because it can never be a matter of demonstration, to make this a criterion of “good” classification is to base prehistory upon an unprovable and untenable proposition. The only utility in asserting that the locus of sharing is in the classification instead of the subject matter of the prehistorian is to eliminate this undemonstrable proposition; otherwise, and for practical purposes, the question of locus of sharing is trivial. It is further useful to indicate not only that linking “cultural classification” to “folk classification” is unnecessary and unparsimonious, but also that it is detrimental to the purposes of prehistory. Folk classifications, when such are obtainable, constitute subject matter like any other artifact or behavior instead of units of analysis and synthesis. To use folk units as the units of study is not terribly unlike a taxonomist asking a frog to what species he belongs. If an attempt is to be made to understand frogs to a greater degree and in a different manner than frogs understand themselves, the and not as a scientific unit. The source of this latent tendency in prehistory to regard as an ideal a congruence between cultural classification and folk classification is undoubtedly sociocultural anthropology, where many “analytic” units such as the named social units are elicited from the people themselves. The potential problems that can arise from such an equation become obvious if the temporal dimension is considered. How can one study change through time-say of projectile points-using a folk classification for projectile points current in A.D. 1, when that classification can hardly take into account projectile points made in the following 2000 years? Further, the classification, as a cultural phenomenon, changes through time as well as the phenomena it serves to order. The definitions of the classes will gradually change in meaning, introducing the very ambiguity that analytic classifications are intended to eliminate. The flat temporal perspective of sociocultural anthropology admits this kind of error more readily than does the context of prehistory. When time is meaningfully introduced, the equation between “folk classification” and “good cultural classification” is negated. The nature of folk classifications as grouping becomes apparent. As groups, such devices are restricted to a finite realm of time and space and to the particular view of that realm taken by the persons using it. The common sense categories of English are exactly the same. Attempts to categorize data with such “rubber yardsticks” can hardly be expected to yield meaningful units in any scientific sense. The rejection of grouping in general and folk classification in particular as a means of creating units for prehistory is not intended to exclude the latter from study. As a means of study they are useless, even deceptive; as a subject of study they may offer a great deal.

2. MEANS. The assumption posited as the basis of cultural classification does not stipulate the means by which ideas come to be shared. Indeed, whether or not ideas are actually shared is a trivial point. The sharing or recurrence of features is a function of classification and thus is purely formal. Many of the considerations in the literature are crippled by inferring the means of sharing, thus forcing the foundation of cultural classification to rely upon inference. These inferences are usually focused on distinguishing functional resemblances (that is, those features which are common to sets of artifacts because they were used for the same thing), from stylistic or historical resemblances (that is, features held in common as the result of historical connection either contemporaneously by diffusion or traditionally by persistence of style). Both of these assessments are obviously inferred from the observation of a feature's distribution over a series of objects, sharing in a purely formal sense. No doubt there are components of both functional and historical resemblance in the configuration of almost any object, so that, further, the inference is one of degree. Sharing as used in these discussions is formal, implying neither historical nor functional means of sharing. The means of sharing has to be referred from the number, pattern, and distribution of the shared features; it is a problem to which some attention has been directed but is not part of formal prehistoric theory.

3. SCALE. he third feature of the assumption is that no scale is specified for recurrence or sharing, The terminology used perhaps implies recurrence at the level of attributes of discrete objects; however, this is but the most commonly employed scale in prehistory. The units which share features need only be readily bounded in the phenomenological world. Thus the units may be communities, with the features, as house types; the units, houses, with the features as constructional elements of houses; the units, house floors, with the features as elements of house floors; the units, hearths, with the features as parts of hearths; the units, hearth lips, with the features as elements of hearth lips, etc. Only a relationship of scale between units (which must be bounded phenomena) and features (which must be classes of attributes of those phenomena) is stipulated. While the practical problems of discovery, recovery, and recording certainly do vary with the scale, the logical properties do not and thus have no role in theory.

This consideration of scale in relation to sharing brings into focus the contrived nature of the cultural/idiosyncratic contrast briefly noted in the preceding chapter. First, sharing is purely formal and inheres in the classification, and is not an intrinsic quality of phenomena. Adding to this the lack of intrinsic scale, one can easily appreciate that the question of whether or not two objects share features is a direct function of the definition of the features and the scale at which they are conceived. Two objects share or do not share features dependent only upon the discriminations made by the investigator. For example, two houses may be different in structure, one being built on piles, the other being built on the ground; one being small, the other large, etc. They may be regarded as different on these bases, and, if the pile-house is the only example of such a structure in a sample consisting otherwise of ground-level houses, it might be called idiosyncratic. It is idiosyncratic only in terms of the features used in the judgment. A different set of features, such as construction materials, function, etc., can be used to group the two structures together as the same thing. The two houses may be different as houses, but identical as parts of houses; that is, they differ at the scale of “house,” but are the same at the scale of “part of house.” Each house is made up of different arrangements of the identical features or parts. Any two objects which do not share features may be made to share features by reducing the scale of the comparison to parts of the objects. To call one object idiosyncratic because at a different scale, usually unspecified, a particular feature or set of features is not held in common with some other specified set of objects, is a failure to grasp the problem or the potentiality of classification. The relationship obtaining between two objects can be precisely specified by a statement of the nature and number of features held in common at a given scale. That at a given scale a specific set of features is not shared is perfectly evident, and the “idiosyncratic” object clearly differentiated, but not as something apart from a cultural system and unamenable to further inquiry utilizing cultural theory. There is a strong tendency, not only with the idiosyncratic/cultural dichotomy, to “freeze” scales and treat scale not as customary, but as absolute. The reasons for this are simple. The terminology is a product of such customary investigations, and each term is linked to either features or units at a given scale. Theoretical terms are lacking. While the terms “unit” and “feature” may lack appeal as “jargon,” they do permit one to discuss sharing and the units shared, as well as the vehicles of recurrence. The basic assumption does not and need not specify any scale. This needs to be specified for particular techniques and methods, but except as a concept scale does not enter into theory.

In summary, then, the assumption made by prehistory equates recurrent features of human origin with shared ideas of the makers and users of artifacts which display such features.

This assumption is implicit in the literature of prehistory as a general proposition, though corollaries derived from it as statements at specific levels and for specific purposes are sometimes explicit. The assumption utilizes shared ideas as an explanatory device--it is not necessary or even desirable to hold that shared ideas, culture, are actual constituents of the phenomenological world, any more than insisting that gravity is a force in the physical universe instead of a concept for the explanation of the motion of bodies. While it has been necessary to consider the issues of the locus, means, and scale of sharing, an explicit statement of the underlying assumption as a general proposition avoids the errors made in these areas. Sharing is a formal device and a function of classification. Some kind of sharing or recurrence is necessary for any classification or arrangement and the assumption simply specifies the rifles for insuring that the resultant units are useful for cultural theory. Recognition that the means of sharing, functional convergence or historical contact, is an inference based upon, not a part of, observable formal recurrence patterns, eliminates the second area of concern. Finally, a recognition that what is cultural, that is, what is shared, is a function of the scale of comparison as well as the features and units themselves, and thus relative, eliminates arguments based upon absolute statements of what is cultural, such as involved in the idiosyncratic/cultural dichotomy. The assumption posited as the formal basis of prehistory functions to derive cultural classification from classification in general; it provides the means for insuring that the units created are useful for manipulations in terms of the concept culture. It is the link between the scientific systems of prehistory and the phenomenological realm. Utilizing this general background to cultural classification, it is possible to see how cultural classification is actually realized in the discipline, first in terms of the kinds of classification employed, and then in terms of the scales at which it is customarily practiced.

## Kinds of Classification

Save in those studies which have arrangement as a goal for its own sake, it is obvious that a kind or kinds of classification are widely employed in prehistory. Both explicit statements outlining procedures and emphasizing the importance of units over the objects grouped in them and the characteristics of archaeological units generally (e.g., their ability to recur through time and space) make this clear. Differentiation of groups of artifacts from classes for artifacts is in evidence in the literature of the nineteenth century and has had overt expression in American prehistory at least since 1939, when Rouse clearly makes this distinction in theoretical terms in *Prehistory of Haiti*.

Identification of the kind or kinds of classification employed in the literature is not an easy matter. Far more frequently than not, classification as a process is implicit, the reader being privy only to the results. Further, it would seem, the process has not been explicit in the minds of many writers, for there are frequent errors of consistency and form. By far the most common and distressing error from a reader's point of view is a failure to differentiate classes from *denotata* of classes. Definitions, as necessary and sufficient conditions for membership in a class, are not presented separately from descriptions of a particular set of *denotata*. This combines into a single undifferentiated mass the features which objects must display to belong to a given unit and the features which the objects assigned to the unit happen to display in various frequencies. The results of using a classification to identify objects is presented, but the classification used is not. For example, the often-encountered “type description” usually consists of a list of dimensions (e.g., in the case of pottery, temper, paste, surface treatment, decoration, etc.) which have been filled in with specific features (shell or limestone temper, regular paste, plain surface, incised decoration, etc.) for each “type.” There is no way to differentiate those features and dimensions which an object assigned to a given type must display from those features and dimensions which an object may display. The use of the term “or” as in “shell or limestone temper” is a certain clue to the identification of that dimension as non-definitive. More difficulties are presented with the use of “usually” or “commonly” in deciding whether the features in question are distinctive of a type or not. Comparison with other “type descriptions” in the same set may further enable one to identify dimensions of features which are definitive and descriptive respectively. The lack of consistency resulting from an intuitive approach to classification leads to non-comparability of features used in “type descriptions” such that the dimension of decoration, for example, may be rendered as “incised decoration” in one instance but as “geometric decoration” in another, completely frustrating an attempt to reconstruct the classification that has been used. The “type descriptions” are in reality unstructured description of groups of artifacts which have already been identified with classes in a classification which has not been presented. Much of the non-replicability associated with the use classification and classes in prehistory stems directly from this problem-no classification has been presented even though one has obviously been employed. Unless one is willing to practice ethnoscience on the literatu1·e of prehistory to reconstruct classifications from unstructured descriptions of sets of *denotata*, the utilization of such “type descriptions” becomes an esoteric and mystical art. This condition is hardly desirable when the only justifiable purpose to classification is the creation of units with explicit, unambiguous meaning.

The obvious, though frequently inconsistent and poorly explicated, use of dimensions, and a lack of overt weighting of one dimension over others, are convincing evidence that paradigmatic classification lies behind most of the units employed in prehistory. Almost all of the kinds of classification labeled “typology” (not all things labeled typology are classification) in prehistory are paradigmatic classification. Regardless of whether the aim is actually achieved or not, a casual survey of any amount of archaeological literature shows that writers intend classes to be identifiable by reference to a set of distinctive features, thus indicating that classification, and not grouping, is being used, and that the features are unordered in terms of identification, thus demonstrating that the classification is paradigmatic. Additionally, classification rather than grouping is indicated by the fact that most archaeological units have distributions rather than locations. Because of poor explication and inconsistencies, this intention is often realizable only to an author and not his reader. The best explicit statements, both in principle and in example, are presented by A. C. Spaulding in “Statistical Techniques for the Discovery of Artifact Types” and by James Sackett's 1966 elaboration of this work in “Quantitative Analysis of Upper Paleolithic Stone Tools.” This is somewhat paradoxical in view of the fact that in neither case is paradigmatic classification the focus of attention. This kind of classification is so frequent that it is more feasible to examine those instances where paradigmatic classification as the underlying classificatory device is not assumed to be a sufficient account. This means, given our two-fold division of classification, an examination of taxonomy.

The term “taxonomy” is frequently used to cover a variety of things: a synonym for classification including paradigmatic classification, to distinguish it from analysis; a synonym for what is herein labeled “numerical taxonomy,” presumably because of this device's hierarchic structuring; and a label for taxonomic classification. Insofar as it is recognizable, the first kind of usage is unimportant; the second is considered under grouping devices in the succeeding chapter. The only real concern here, then, is the use of taxonomy as taxonomic classification. While the term has been borrowed from the biological sciences, most prehistorians readily agree that prehistory does not have a taxonomy comparable to the Linnaean Hierarchy, nor does it approach its subject matter in the same fashion. The oft-cited reason is that cultural processes are not unidirectional and thus are more complicated than those of genetics and inheritance. While one may allow this as true, it does not have any bearing upon the use of taxonomy; indeed, one might argue that taxonomy ought to be used for these very reasons. The use of taxonomic classification is and has been on the wane in prehistory for some time, largely as a result of Krieger's convincing arguments in “The Typological Concept” against the weighting of features. His arguments are phrased in terms of the practical difficulties encountered in making the required decisions, difficulties that are inherent in the unparsimonious form of taxonomy. In recent literature, taxonomy has played no important role. Some “type descriptions” which are inconsistent in the application of dimensions (the “incised decoration”/'geometric decoration” instance) might be viewed as taxonomies in which only the lowest level taxons are explicit; however, this is probably more a function of an analysis of the sets of “type description” than it is of the classification used by the original writer.

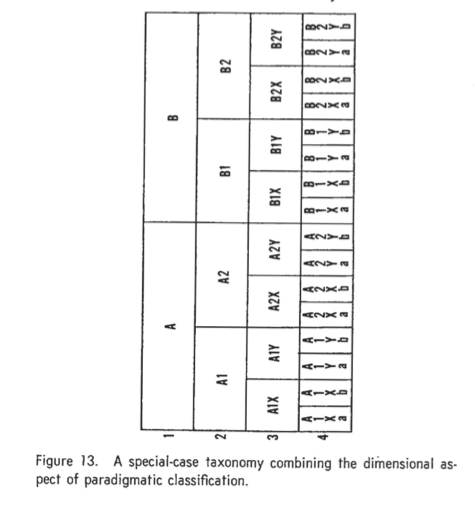


Figure 13. A special-case taxonomy combining the dimensional aspect of paradigmatic classification.

Otherwise, only simplistic sorts of taxonomy are used. The most common form is a kind of additional process in which one begins with an index or set of Classes created by the intersection of two dimensions of features. Subsequently, one or more dimensions of features, either singly or in sets, are added, effectively “sub-dividing'' the initial set of classes. In practice, of course, one could start with the most complicated level and successively remove sets of dimensions-essentially the reverse of the first situation. In prehistory the “ware” and “type” classifications for pottery, frequently used but infrequently explicated, and the “type-variety” classificatory schemes, are of this sort. Figure I3 illustrates the basic design of such a program in which the highest-level classes constitute an index, the second level of classes is created by adding a second dimension of features, the third level is created by the addition of still another dimension, and the fourth level of classes is created by the addition of a final dimension of features. In order to keep the illustration simple, each dimension is divided into two features, but obviously this is not necessary and certainly not usual. Further, as just indicated, this same figure could be described starting from the lowest level and talking about the others as successive subtractions of dimensions. In either approach to description, Class AI is a kind of A, as AIX is a kind of both AI and A, and so on. Upon close examination, not only do the classes included under the same superclass at the same level constitute a paradigm, but each entire level is a paradigm. If, for example, one is concerned with only the lowest-level classes, the entire classification can be treated as a paradigm. Clearly, then, this sort of taxonomy is a special case within the general field of taxonomy. If any given level in such a device is of concern to the exclusion of others, it is not necessary to treat the various dimensions as ordered or the classes as taxonomic. Class AIXa can be derived regardless of whether the X-Y dimension is employed before or after the A-B dimension. While ordered, the order is not necessary to derive the classes at any given level.

A legitimate question then arises as to why this kind of device should be regarded as taxonomy rather than paradigmatic classification. The answer is that while any level of classes can be regarded a,s a paradigm, the entire structure does not present all possible permutations of the features and dimensions, and thus the occurrence of specific classes is conditioned by the ordered addition or subtraction of dimensions. For example, in Figure I3 the occurrence of Classes AI-B2 is a function of applying the I-2 dimension before the X-Y dimension or a-b dimension. Had the a-b dimension been the second employed in this example the second level of classes would be defined as Aa, Ba, and Bb, and Classes AI-B2 would not occur in the new classification as Aa-Bb do not occur in Figure I3. Clearly, dimensions are ranked in terms of importance, but the features within the dimensions are equally relevant for all previous distinctions. This special-case taxonomy, differentiated from other taxonomies in the consistent and exhaustive application of features through a given level, thus eliminating the assumptions of position required in other taxonomies, is potentially a powerful means of unit creation if rigorously executed. Potentially, however, is the key word. While the number of assumptions or weighting required is reduced by the consistent and exhaustive application of each dimension of features, assumptions of importance are still required to order the application of dimensions relative to each other. Unfortunately, the rationale for such decisions is inferential as is the case with the “type-variety” classificatory scheme, and thus the definition of units used to make the inferences depends upon the inferences, a kind of circularity characteristic of taxonomy. It is necessary to be able to answer why dimension A-B is applied first, 1-2 second, and so forth, in terms of observed fact, in order that the taxonomy be sufficiently parsimonious as to be useful for some specified purpose.

The “ware-type” and similar two-or three-level taxonomies, when constructed for a specific rather than descriptive purpose and when the relevance of the features employed in definition is demonstrable (requirements of all kinds of classification), meet this test. Utilizing the lower two levels of Figure 13 as a model, types in the “ware-type” scheme are equated with Level 4 and wares with Level 3. The larger the number of definitive features required of each class, the smaller the distribution the *denotata* of the class will be. Thus, for many kinds of problems, the Level 4 classes are optimal; however, the utility of any set of classes must be weighed against the data being manipulated. As is usually the case with wares and types, the wares represent the fabric of the ceramic (Features A-B, 1-2, and X-Y represent hardness, texture, and temper), and the types include the additional dimension of surface treatment. In practical terms, the fabric of the ceramic is almost invariably recovered with any sherd, whereas surface treatment may often be missing through the agency of erosion. An investigator using a “ware-type” scheme of this sort then has two alternatives available to him, wares or types. If his data are well preserved, he will probably employ types. If his material is poorly preserved he may choose wares for this will effectively increase the size of his sample and the reliability of its distribution. In short, the taxonomy provides alternative sets of classes, one which makes a maximum number of discriminations but requires optimal circumstances, and another which makes fewer discriminations under less than optimal circumstances. This special-case taxonomy functions, then, to adapt theoretical devices to actual bodies of data, and is really a part of technique rather than theory. The linkages between levels are observational: Surface finishes occur on pastes. The order is likewise observational: Surface treatments are destroyed before the paste disintegrates. Many similar examples of this kind of taxonomy functioning in this specific role may be found in the archaeological literature. There is no reason why more complicated structures cannot be employed for more complicated technical problems.

Further, this special-case taxonomy can be employed in adapting classificatory units to the requirements of particular methods utilizing this same feature of variable numbers of coordinate features employed in the several levels. The fewer criteria required for membership, the larger the number of objects which will fulfill the conditions of membership. Thus, using the type-variety method as an example, the level of wares will have greater utility in comparisons through larger amounts of time and space than will types or varieties, and generally are used for such purposes. Varieties, on the other hand, with a larger number of necessary features will be restricted to smaller amounts of time and 'space and thus are employed in inter-site comparison.

The important point, however, is this: The utility of this special-case taxonomy comes from its characteristics of linked paradigmatic classifications rather than its taxonomic features. Indeed, in the case of the type-variety system, the llnkage is observational, and it is this feature, minimizing the taxonomic element, which makes it useful. True taxonomies play no role in prehistoric theory, for to make them parsimonious they must be articulated with the phenomenological realm, and the articulations must be tested as hypotheses. For this same reason taxonomic classifications do function in the realm of technique which attends the articulation of classification and phenomena. The use of paradigmatic classifications linked together with a taxonomic structure is an excellent solution, so long as the taxonomic linkages are not inferential. Those few taxonomic classifications which are based upon inferential notions of “relatedness” or which base the ordering of levels upon inferences about the social groups making the ceramics require the demonstration of such inferences, and such demonstration is presumably the purpose for which the classification is created.

## Scale and Classification

Up to this point and in the archaeological literature generally, the terms “level” and “scale” have been used almost interchangeably. It is necessary, however, to differentiate two notions of ranking or inclusiveness treated under the labels to further specify the nature of classification as employed in prehistory and the particular kinds of classes that are customary. Implied in the use of both level and scale is a relative degree of inclusiveness or rank. Hereafter, level will be employed to denote inclusiveness in theoretical units, essentially the number of definitive features in a *significatum*. *A level is a set of units (classes) which display the same or comparable degree of inclusiveness or rank.* All the classes in paradigms are of the same level since all are mutually exclusive alternatives with equivalent definitive features in each *significatum*. On the other hand, taxonomies and the special-case taxonomy illustrated in Figure 13 consist of several levels. In taxonomies the level is determined by the number of oppositions and thus the number of definitive features in the definition of a taxon. Being ideational in nature, specific values cannot be assigned to levels apart from other levels. It is thus useful to employ the notion only when two or more sets of units or concepts of differing degree of inclusiveness are being employed , as in taxonomy. Further, the notion of level is applicable only when the various sets of classes constitute alternative classifications for the same phenomena. Types, wares, and varieties are best discussed as classes at different levels, since they differ in the size of the classes produced (inclusiveness from large to small) and since they are alternative classifications for potsherds or other discrete objects.

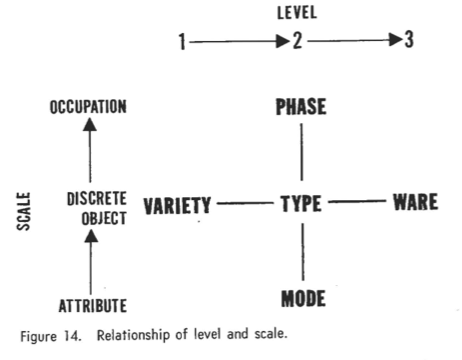
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Figure 14. Relationship of level and scale.

*Scale,* on the other hand, will be used to designate inclusiveness or ranking in the phenomenological realm, and thus is defined as a set of objects (groups) which display the same degree of inclusiveness or rank. Scale is the stipulation of the size of the phenomena being considered. One can construct classes for aggregates of objects, discrete objects, or parts of such objects. Although it is not so done, one could construct wares, types, and varieties of all the various scales just listed. Figure 14 illustrates the scale and level relationships among a series of units to be discussed in later sections. Here the vertical axis indicates scale and thus the relationship between mode, type, and phase is one of scale (they are classes for different scales of phenomena), whereas the horizontal axis represents level and thus the relationship between variety, type, and ware is one of level (they are alternative increasingly inclusive classes of the same phenomena). Level alone is sufficient to discuss classification as a process in both a general and within the confines of prehistory. Scale is necessary to specify particular classifications and kinds of units employed in prehistory, and, because it is phenomenological, scale can be specified in absolute terms.

Scale is specified by the investigator; it is not inherent in phenomena. A choice, which ultimately must be justifiable, is made. In prehistory, the scale of phenomena considered is traditional or customary. This is a most important point. The consideration that follows treats the *customary* scales considered in prehistory. It does not mean, nor should it be construed to mean, that three scales identified are the only ones possible or that they are the most profitable. There are, however, persuasive arguments in favor of at last a portion of the scales traditionally used.

Implicit in the wording of the preceding discussion and in much of the archaeological literature is a scale best designated as that of portable discrete object, identifiable in that when moved, its component parts remain in the same spatial relationship to each other. A hammer, a coffee cup, and a dog are all examples of this scale. The strongest arguments in favor of using this manipulatory criterion as the starting point in reckoning scales is the ease with which it is identified and the untested but rather reasonable assumption that manipulation of objects is a relevant factor to all living things. Some problems do inhere in the fact that discreteness, like any other quality, changes through time. To take a pronounced example, a dog can be observed to become several discrete objects after death occurs and chemical decomposition begins. After decay eliminates all of the soft parts, discreteness again becomes fairly stable since decomposition affects the bones more slowly. Discreteness and chemical decay are obvious concerns of any investigations directed toward the past. Chemical decay is but one readily observable and familiar form of changing discreteness. As a result of this difficulty, a choice has to be made in distinguishing discrete objects in prehistory: Are discrete objects those objects currently meeting the criterion of manipulatory discreteness, or should discrete objects be considered only those objects or sets of objects which met this criterion at the time at which they were made or used? Fortunately, the former position seems to have been almost universally settled upon by prehistorians (excepting some similar areas of fuzziness), though not without some nagging concern about the changing nature of discreteness. The answer can be weighed as fortunate, for this position permits scale to be determined observationally rather than inferentially. Former discrete units are subjects for inference, but one which is made upon the observed discrete objects remaining to us.

One set of superficial exceptions might be noted, usually presented in the literature under the term “features” or “structures.” These units, while differentiated by their label from portable discrete objects, are not treated differently in any essential fashion. The only point of difference lies in the pragmatic realm of recovery; the discrete objects called “features” are not portable but are usually represented by characteristics of soil which cannot be moved without destroying the discreteness of the object or which are simply too big to be conveniently moved. Houses, pits, and fire hearths are objects which usually fall into this category. While differently labeled and frequently described in separate sections of reports, they are usually treated as objects equivalent in scale to potsherds and projectile points.

The discrete object is the basis for reckoning the other two commonly-employed scales. Even the casual student of archaeological literature is aware of a scale larger than that of discrete object, if only because discrete objects are often treated as component pieces of larger things. One looks, however, in vain for an explicit statement of what the scale is or how it may be identified. There is, for example, no explicit statement of what “phases” are classes of, though their nature as classes is perfectly obvious. More frequently than not, tautology characterizes statements relating such classes to the phenomena which they purport to order, running something like “phases are classes of components” and then “components are manifestations of phases.” In this case one must have the classes to recognize the phenomena, and one must have the phenomena to construct the classes. While there are numerous classifications in evidence at this scale, there is nothing in the literature to suggest that the inventors of such classifications know in any precise way what they are classifications for. The scale of phenomena is simply not identified. This is, without any doubt, the most serious deficiency in the formal theory of prehistory today.

There are a number of contributing factors. Our own perception of phenomena dictates that any scale larger than discrete object will be seen as a group of objects, some kind of aggregate. In a real sense the phenomena are constructed, and thus the possibility of different aggregates' being constructed by different people exists to a degree not possible at the scale of discrete object. Further, and unlike the discrete object, the dimensions of time and space are apparent in aggregates. What is lacking in prehistory is a statement of how such aggregates are to be constructed. Lacking a common perception and lacking special rules to overcome it, prehistorians have created, largely by accident, a Pandora's box of phenomena, holding in common only the fact that they are aggregates of objects.

Another factor, sometimes explicit, is the use of sociocultural anthropology as a model. The main impetus for higher-scale units of phenomena appears to be a desire to have units comparable to the “community” and whose classification will result in units analogous to “societies,” “tribes,” “cultures,” or “peoples.” In spite of this, prehistorians have long recognized that the resulting classes, such as phases, are not directly comparable to units in sociocultural anthropology, even if they have not always stated why. The difficulty in using a notion such as “community” for the scale of phenomena lies in the fact that communities’ remains do not come in readily identifiable physical units. Communities must be inferred and thus cannot be the basis of distinguishing phenomena. The matter is further complicated because the objects which the prehistorian wishes to treat as an aggregate are situated in both time and space, rather than space alone, as is the case with most sociocultural units.

A final factor, perhaps as much as a result as cause, is that the devices used to create units at this scale are usually explicated as either grouping or taxonomic classification, neither of which lends itself to convey the means by which decisions are made by the investigator. The tautological relationship expressed between classes at this scale and the phenomena certainly is a characteristic of these devices. Regardless of the rationale provided for unit construction at this higher scale, it is apparent from actual practice that classification, not grouping, is the means by which units are formulated, since the units have distributions, new information can be identified with previously established units, and even, in some cases, the necessary and sufficient conditions for membership are stated (e.g., determinants).

Admitting the desirability of a scale of phenomena larger than discrete object and recognizing that such units must be by necessity aggregates not as readily identifiable as discrete objects, it becomes necessary to state the characteristics that units at such a larger scale should display. It is not the purpose of this treatise to write anew the formal theory of prehistory, but simply to provide a framework for using what has been written. Nonetheless, at least a name for the units at this higher scale is required to continue any discussion, even if the unit cannot be precisely defined. Notions such as site (the place where the archaeologist digs) or component (which presumes the classifications for identification) will not suffice. The actual unit employed is the “collection.” The object of classification is collections of discrete objects obtained in a spatially restricted area. How the space is restricted and the conditions its contents must meet is the focus of the problem. Judging from the literature, it is usually done intuitively. Yet there are clearly a set of goals which these collections, sometimes labeled assemblages, are intended to meet. First, it is evident that the objects making up the aggregate are intended to include only those made by the same set of people. Secondly, the set of objects is intended to represent those people at that place, that is, the collection or assemblage is to represent a sample of a spatial cluster. Thirdly, the set of objects is intended to represent a specifiable temporal segment, usually a period of continuous residence. In my own work the need for such units has arisen and the unit has been termed “occupation,” defined as a spatial cluster of discrete objects which can reasonably be assumed to be the product of a single group of people over that period of time during which they were in continuous residence at that particular locality.

Quite obviously, the occupation is a tactical unit, not a theoretical one, and adapted to a specific body of data, in this case seasonal settlements. It is not generally useful. One need, for example, only consider the remains left by civilized peoples who may be in “continuous residence” at a given locality for a thousand years to appreciate the limitations. A tactical definition such as this does point toward a solution. The terms of the definition must be discrete objects--these are phenomenological and identifiable. The spatial boundaries will necessarily be based on proximity of discrete objects, again recognizable in phenomena. The spatial clusters of objects must be accountable as the products of a single group of people and deposited over a finite, specifiable time. A more workable definition might be constructed by treating the temporal element in terms of comparability and defining occupation as a spatial cluster of discrete objects which can reasonably be assumed to be the product of a single group of people at that particular locality deposited over a period of continuous residence comparable to other such units in the same study. This too is a tactical definition, not a theoretical one, but it does offer a more general solution than does the first, and effectively compresses the dimensions of time and space from the unit so that it is comparable to discrete objects. This kind of unit definition suffers from the principal disabilities of most archaeological notions; so defined, the units of one study are not comparable to those of another. Be this difficulty as it may, the term “occupation” can be used for the scale of phenomena above that of “discrete object” if cognizance is taken of the fact that the label only suffices to continue the discussion and does not constitute the resolution of this serious problem.

One thing ought to be clear. Whatever set of rules may be developed to distinguish the phenomena being treated as occupations, only a portion of remains treated as discrete objects can be classified at a higher scale, perhaps only a modest portion. Because the occupation, however defined, will always be an aggregate of objects lacking physical discreteness, it will be subject to alteration through time by simple mechanical motion greatly reducing the number of clusters which can be reasonably assumed to be the product of a single group of people or any other specified condition. This reduction in sufficiency is to be expected as a consequence of the greater precision and information required. It will always be the case that more archaeological remains can be accounted for and explained as discrete objects than as occupations or any other kind of aggregate.

Less inclusive scales than that of discrete objects present fewer difficulties than does the more inclusive scale, primarily because they are less frequently used and because they are component rather than composite elements and thus can make use of manipulatory discreteness for their identification. Less inclusive scales are always “pieces” or features of discrete objects-the problem of identification is simply a matter of conveying the manner in which discrete objects are to be divided. While not a common level at which paradigmatic classes are formed for the purpose of making hypotheses, the scale of “part artifact” or attribute is very familiar in the literature, for it is at this scale that features which are both the elements used in definition of classes and description of their *denotata* are formed. These are intuitive when used as the analytic units for classification at the scale of discrete objects; however, paradigmatic classes have been usefully formed at the scale of part-artifact.

Proceeding from the least inclusive or smallest scale to the most inclusive, those scales customarily used in prehistory are the “attribute” (of discrete object), “discrete object” (including both portable and non-portable objects), and “occupation” (aggregate of discrete objects). These scales constitute the three “sizes” of artifacts ordinarily treated by classification in the discipline. All three have the same properties of human involvement, and all are treated as things. They differ in physical size and the manner in which they are perceived, differences which profoundly affect their recovery as data but differences which do not enter into their properties as alternative units of classification. Obviously the inferences made about artifacts at each of the scales are widely different, and this is the reason for employing several rather than a single scale.

The spatial cluster that constitutes an occupation is in some senses empirically discrete-through time, with additional activity both natural and cultural, this discreteness is lost to a greater or lesser extent. Today additional scales are being recognized, at least experimentally, that lie between the discrete object and the occupation, clusterings of objects within occupations which give them their patterned character. Such treatments are not yet routinized to the extent that a single or series of intermediate scales are widely recognized in the fashion of attribute-object-occupation and thus are not properly treated here. Simply noting such a direction in prehistoric researches serves two purposes: (1) it emphasizes the arbitrary and customary nature of the three-scale system, and (2) points up the possibility of extracting, currently by means of distributions and associations of objects within occupations, phenomena at scales not ordinarily perceived as such. All of us would see both objects and occupations and things; not many would perceive an activity locus as a thing, yet our “common sense” perception is no measure of utility, even though the three-scale system is just such “common-sense” perception.

At any given scale an infinite number of classifications is possible, with alternative classifications for the same objects. Different classifications may have different purposes and thus make use of different criteria. Such alternative classifications often differ in level. Taking again the type-variety system, “wares,” “types,” and “varieties” are alternative classifications of potsherds, three classifications differing in level but treating the same scale of phenomena. Such ranked constructions must not be confused with classifications of different scales such as the “mode,” “type,” and “phase” classification presented in the following pages. Further, in constructing classes two scales must always be used. The features employed as criteria will be drawn from a scale below that of the classes. To formulate classes of discrete objects, features must be drawn from the scale of attribute. Likewise, features defining classes of occupations will be drawn from the scale of discrete object or attributes of discrete object, or both.

The following section identifies the specific classifications currently employed in prehistory in terms of the framework just set forth. Perhaps as much as ninety per cent of all classification used in prehistory, when sufficient information is provided, can be treated as members of this system. This is, of course, in spite of divergent terminology in which different units are called by the same label (as is the case with type) and the same unit is labeled with different names (as is the case with mode), and in spite of a lack of a precise separation between the classes and their *denotata* and the inconsistencies introduced by this failure.

## Classification in Prehistory

Figure 15 presents the set of classificatory units widely employed today, using the most common terms for the units involved. It is important to note that at each of the customarily chosen scales two different kinds of units occur, one a synthetic or classificatory unit and the other an analytic unit. The unit mode, for example, is identical or can be identical in content when used in the definition of type or as a paradigmatic class in its own right. The distinction between analysis and synthesis is relative. If modes are used as features in the definition of types, they will be treated as if they are intuitive classes of attributes even if they themselves are the product of an explicit classification at the scale of attribute. This is so because paradigmatic classification presents only one set of definitions: the features used to phrase the definitions are defined outside that particular classification.

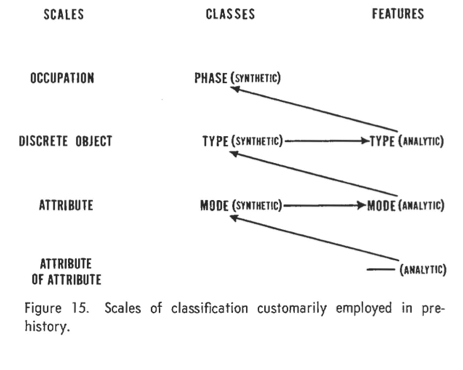


Figure 15. Scales of classification customarily employed in prehistory.

Beginning at the lowest scale of phenomena, Figure 15 indicates an unnamed analytic unit used to define modes, the units at the next highest scale. This unit is implicit in the literature, principally because modes are usually considered “indivisible units,” the smallest possible qualities, a view which obviates an explicit statement of definition. The inclusion of such a unit at a scale beneath those ordinarily employed serves mainly to allow for the definition of modes, not because it is frequently encountered. The unnamed unit in Figure 15 can be defined as an intuitive cultural class of attributes of attributes of discrete objects. Intuitive, in this and the following definitions, indicates that the unit so characterized is not the product of an explicit classification in the particular context employed. Cultural should be understood as meaning that the elements of the definition, be they intuitive or explicit, can be assumed to be the product of human activity, that is, artificial. Insofar. as I am aware, there is no synthetic or classificatory unit at the scale of attribute of attribute of discrete object.

“Mode” is the term applied to classes, both analytic and synthetic, at the scale of attribute of discrete object. This classificatory unit plays the crucial role in the system of classifications employed in prehistory. Some investigations are conducted at this scale (e.g., Rands cited in bibliography), and so modes sometimes are defined classificatory units. So employed, mode is defined as a cultural paradigmatic class of attributes of discrete objects. This, however, is not the most important or frequent use of this class. Its most important use has been as the analytic step providing definitions for classes at the scale of discrete object. Types are defined and described in terms of modes. This unit is probably the subject of more terminological abuse than any other. Modes have been and are called “features” (meaning characteristics), “attributes,” “factemes,” and “traits,” to name only a few. Employed in an analytic context, mode is defined as an intuitive cultural class of attributes of discrete objects. Since the mode is the smallest-scale unit commonly employed in prehistory, it bears the burden of converting classification in general into cultural classification for prehistory. It is usually here that the assumption that attributes which are the product of human activity and which recur or are shared may be treated as the product of shared ideas is injected into the work of the prehistorian. This assumption itself is sufficient for the creation of modes as analytic units. Unfortunately, modes, irrespective of what they are called in a given piece of literature, are frequently dealt with only incidentally. The comparative work required in assuming a given attribute or set of attributes to be the product of human activity is not frequently presented in explicit form. Attributes are not cultural; they are part of the natural world. To assume that a given set of attributes is the product of human activity requires a comparative study. Modes treated under the terms “trait” or “characteristic” seem particularly plagued with this lack of serious concern. What is cultural varies from place to place and from time to time. The mode functions in prehistory to isolate sets of attributes which are cultural in a particular context. Undoubtedly because of the sloppy treatment this matter has received in many cases, modes that would have been useful for the purposes of the given study have been left unused while other “attributes” or “traits” employed are not even cultural, let alone relevant to the problem considered. The importance of modes in prehistory cannot be overemphasized. They themselves are sometimes used to provide the basis of hypotheses and inferences about styles and technology as well as chronological problems, but their most important use is in the definition of all further cultural classes employed in prehistory. The term mode has been chosen from the plethora of terms because of its chronological priority in association with good definition (defined in Rouse, 1939). The majority of investigations in prehistory are conducted at the level of discrete objects if only because of the ease of identifying this scale. As is the case with mode, units at this scale are employed both as units of study and as means of defining units at still higher scales. In contrast with mode, however, the unit at this scale, type, is most frequently used as a unit of study rather than an element for definition. In this synthetic context, type is defined as a paradigmatic class of discrete objects defined by modes. It is possible here and at this scale to specify the units used for definition, in this case modes, and thus drop the cultural adjective for type. Types must be cultural if they are defined by modes. This also obviates any need to make the basic assumption more than once. Types are sometimes used to define units at the next higher scale. In this context of analysis, type is defined as an intuitive cultural class of discrete objects. While types so used are intuitive at the next scale of classification, in practice they almost never are, for they have been formulated as units of study defined in terms of modes and then used as elements of definition at a higher scale.

Like mode, type has seen considerable terminological abuse, more in the direction of different kinds of units being called types than in different names being used for the unit here called type. “Type,” especially when qualified as “descriptive,” is often used for intuitive groups which do not in any respect meet the criteria of classes and is thus employed as a synonym for English “kind.” Type is also applied to the products of grouping devices, particularly statistical clustering, and this is the most serious terminological problem, given the magnitude of the distinction between groups and classes. On the other side of the coin, the terms “variety,” “ware,” “style,” and “functional class” are but a few of the names occasionally applied to paradigmatic classes at the scale of discrete objects. Most of these terms reflect not the kind of unit, but the particular purpose for which the class has been constructed. Thus functional classes are usually types which are explicitly created for the purpose of inferring the function of discrete objects. The terms “ware,” “type,” and “variety” in the type-variety system name types that differ in level : “wares” being the types which are used for comparisons over large amounts of time and space, “types” being the types used for comparisons within small areas and limited amounts of time, and “varieties” being types used primarily for intra-site comparison. As has been pointed out, all three are paradigmatic classes, or can be, for discrete objects differing in level. The choice of definitive modes is predicated on the purpose to which the units are to be put.

To reiterate: Types are paradigmatic classes of discrete objects defined by modes. Types are not groups of objects, but classes whose *significata* consist of sets of modes stating the necessary and sufficient conditions of membership. Since these conditions are modes and modes are cultural, types are cultural.

There are substantial difficulties in identifying the phenomenological units at the next highest scale, that of occupation, and thus it is not surprising that there is considerable confusion (both conceptual and terminological) about classification at that scale. The most commonly employed term for these classes is “phase”; however, the theoretical rationale for the construction of phases is usually phrased as a kind of numerical taxonomy. This particular rationale is considered in the following chapter. It is sufficient here to note that the units formulated have all the characteristics of paradigmatic classes (e.g., distributions in time and space, plus unranked or unweighted definitive criteria called determinants), and that they can be used to identify new data. Only classificatory or synthetic units appear to be constructed at this scale. Phases do not serve as analytic units for any higher scale of phenomena. In spite of divergent explanations for the phase, it is employed as a paradigmatic class of occupations defined by types and/or modes. Phases are identified as recurrent sets of types or, less frequently, modes. In the literature, “phase,” “focus,” and “culture” are often used interchangeably for paradigmatic classes of occupations. The terminological difficulties are increased by the use of such labels as “complex,” “industry,” and “assemblage” to refer to both the *denotata* and the *significata* of the classes. The term “component” has seen fairly consistent usage as a label for the *denotata* of a given phase at a given locality.

The construction of phases in the discipline has largely been directed toward the construction of classes which can be called “whole cultural,” that is, classes which link together the various remains of a single set of people. It has been customary to call paradigmatic classes of occupations other names when constructed for purposes other than “whole cultural” units. Many of the “larger units” considered later in this chapter are phases; that is, they are paradigmatic classes of occupations, but they are not necessarily “whole cultural” units. The definition of phase presented here is not restricted to classes for any particular problem. There may be, and indeed are, phases formulated on the basis of functional criteria as well as those formulated along the more customary lines with stylistic cri.

In summary, there are three fundamental scales at which paradigmatic classes are formed in prehistory: (1) attribute of discrete object, with the resulting classes termed modes; (2) discrete object, with the resulting classes termed types; and (3) occupations or aggregates of discrete objects, with the resulting classes termed phases. There is implicit a fourth scale, that of attribute of attribute of discrete object, at which the units are not named and which function only as the analysis for modes when such is attempted. Modes are basic to the system because it is here that classification usually begins and the assumption which makes classifications cultural is employed. Modes serve both analytic and synthetic functions with the analytic function dominating. Types are the most widely used classes, almost always serving as synthetic units which in turn are used as analytic units. Phases are the highest scale of classes commonly employed, and they function entirely as synthetic units. Since types are defined in terms of modes, their *significata* being' combinations of modes, types are cultural by definition. Phases can draw upon either modes or types for definition, and likewise are thus cultural.

## Some Still Larger Units in Prehistory

It is the contention here that there are but these three scales at which synthetic units are ordinarily formed and a fourth which currently serves only as an analytic step leading to the definition of modes. There are, however, a number of named units in the literature which superficially appear “larger.” Because of this quality of “largeness,” there is no confusion in the literature about their nature as classes. The *denotata* are simply too numerous and too extensive to be assembled into a group, effectively preventing the confusion of class and *denotata*. These 'larger” classes differ in no fundamental respect from those already discussed. They represent the very same classes (modes, types, and phases) but are defined for special purposes or at a level higher than that usually associated with classes labeled modes, types, and phases. Since there are a large number of such named units it is not possible or profitable to consider them all. The most widely used are tradition, horizon-style, horizon, series, and stage. The treatment of these notions here is brief, serving only as a pattern for how such classes may in general be regarded. Tradition, horizon, and horizon-style may be examined together since they are labels for “special cases” of the units just considered. These three units do not specify any particular scale, but rather are modes, types, and phases whose *denotata* display special temporal-spatial distributions.

Traditions are modes, types, or phases whose *denotata* display an extensive distribution through the dimension of time in conjunction with a limited distribution in space. The term tradition serves simply to name those modes, types, and phases with this kind of distribution. This particular distribution is the source of many inferences in prehistory concerning development, continuity, and “genetic relationship,” and thus the need for a term to designate classes appropriate to such operations. Further, many explanatory models operate only within the confines of such classes, providing another important reason for their delineation. Frequently, traditions and one or more sets of other classes will be superimposed to provide the basis for inferring complicated temporal-spatial relationships. Classes which have the distribution of tradition are often defined upon functionally relevant features since such features tend to change more slowly than, for example, features of style.

Horizon and horizon-style are parallel constructions which designate classes whose *denotata* have extensive distributions in space coupled with restricted distributions in time. Horizon style is most frequently applied at the scale of attribute, whereas horizon is the term used at larger scales. Again, the terms serve to designate classes with distributions of particular interest to many prehistorians, for the particular distribution labeled horizon or horizon-style serves as the basis for inferring such things as migration, diffusion, and contact.

Series and stages differ from tradition and horizon in that they do not serve to label classes of particular distributional characteristics. In the case of both stage and series, the level of classification is higher than ordinarily used, usually, but not necessarily, at the level of phase. In both cases the defining criteria are relatively few compared to usual classes with the result that their *denotata* are of wider occurrence ~ time and space, and they serve to link other classifications through coordinate *denotata*. Series are usually defined upon stylistic features; stages are usually defined technological features. Thus series tend to have coherent distributions in both time and space, whereas stages tend to have coherent distributions only in time. Because they involve few criteria the amount of information provided by such classifications is relatively limited, and their main use lies in conceptual summaries and literature intended for lay consumption or introductory texts.

Various combinations of these larger units occur m the literature or are possible, especially if they are employed at different scales. The area-cotradition is an example of both tradition and horizon distributions used together. The more criteria that are employed, however, the more restricted the use of the resulting units. The important thing to recognize is that these grand classes differ in level and purpose but not in scale from the units considered here. Traditions are classes for the same scales as modes, types, and phases and are best treated as special kinds of modes, types, and phases. Series and stages likewise are classes for occupations (primarily) and thus are best considered phases defined by a small number of specially selected features.

## Problem and Evaluation

The absence of an identifiable phenomenological unit above the scale of discrete object may be the most serious conceptual void in prehistory's formal theory, but by far the most serious operational difficulty is the chronic lack of problem and consequent lack of rational means of evaluating classifications. Thus, in turning to consider evaluation and problem, we are turning to classifications rather than the process itself. This difficulty is linked with, and perhaps in part is a result of, the confusion between the *denotata* of classes and the classes themselves, and concomitantly to the confusion of description (of *denotata*) with definition (of classes). A class “means” its definition or *significatum*. If, for example, we have a class defined red-rough-solid, the distribution of this class's *denotata* is that of only the objects as red-rough-solids and nothing else. Any hypothesis made to account for the distribution is an account of the objects as red-rough-solids. This class could not be used as the basis for inferences about shape, size, or any other characteristics of the objects identified as *denotata*, for these other characteristics are variable. Similar arguments could be made for association of *denotata* of different classes. The use to which a class may be put is a direct function of how it is defined. Problem and class definition are intimately linked.

As we have seen, definition of classes, regardless of the kind of classification, involves the selection of some classes of attributes as criteria. Thus the point at which problem enters classification is in the selection of definitive characteristics. A survey of archaeological literature shows three alternative treatments. Most commonly, the selection of criteria and the definition of problem is simply ignored. Classes are formulated by means unknown to the reader and perhaps to the formulator, and thus do not have an explicit *significatum*. The classes mean nothing and can legitimately be used for nothing. These cases may usually be recognized by the use of such terms as “descriptive,” “inherent,” “essence,” or “natural.” “Description” is usually proffered as the purpose. If, however, description is a purpose or problem, then any set of criteria will serve for all that is required is a set of words. There is no way to evaluate such constructions, nor do they have any meaning. They are natural, inherent, and represent the essence of the real world.

A second less frequently realized alternative is the explicit statement of the criteria chosen for the definition of classes but with no specified problem for which the classes are to serve. In this case it is possible to treat the Classes as meaningful and to make hypotheses about their distribution and association, but there is no way to evaluate their utility. The criteria, while explicit, are commonly drawn at random, and the classes are not a useful organization for any problem. Indeed, this alternative seems to be realized when the object is “description,” and the classes are not constructed for any use beyond a device to say what was found where and to provide terms for the ubiquitous “site-to-site comparisons.” All the comparisons mean, however, is that thus and such types are found in thus and such places, in spite of the speculation sometimes associated with such “comparison.”

The third alternative, the statement of both problem and definition of classes, is the least frequently realized. The statement of a problem for which the classification is to serve as the organizing device provides the rationale for making the choice, be it overt or covert, that must be made in defining classes. The utility of a classification then becomes testable. Either the classification will organize data for, say, a chronology, or it will not. The particular choices made can be weighed against other possible choices and those best suited to the problem selected. While implicit in many important respects, James Ford's pottery classifications for the Southeastern United States are some of the best examples of problem-oriented classification. His sole concern was classifications for ceramics which could be used in constructing chronologies with the seriation method. While it is not often possible to separate the *significata* of his types from the description of the material assigned to them, his own general statements indicate how the decisions were made: only those combinations of modes which had short distributions in time were suitable. His definitions are stylistic. Further, he admits the possibility of making wrong selections which will not prove useful for his purposes and which will have to be “reformulated.” While it is possible to recognize Ford's problem and to state generally how he employed classification, principally types, for its solution, his chronic failure to differentiate type definitions from the description of their *denotata* makes for difficulties in using his material as an example.

By way of summarizing this third alternative it is useful to introduce an example which begins with the selection of criteria for the definition of types and follows through to their evaluation. For these purposes the problem can be stated as chronology, the method for which the classes must function as seriation, thus closely following Ford's interests, and hopefully elucidating some of the operations which make it work. Let us say we have a series of pottery collections from the set of localities shown in Figure 16. Our immediate purpose will be the selection of a series of dimensions of modes suitable for seriation-modes whose primary variation in representation in the area of concern is through time rather than through other dimensions (e.g., space). Seriation orders groups by arranging them so that the distribution of the *denotata* of historical classes is continuous and if the frequency of occurrence is treated these frequencies take the form of a unimodal curve. For the purposes of illustration we need consider only the first model, that of continuous distribution, usually termed occurrence seriation. One might begin simply by combining all the collections and distinguishing various features of their construction, decoration, and the like, being careful to ascertain their artificial nature. Since styles are desired, certain kinds of attributes will intuitively be important from the beginning, such as decoration. Other kinds of attributes, such as shape, may have strong functional components; and still others, such as clay, spatial components. These problematic features will greatly outweigh those which can reasonably be assumed to be relevant. From these will have to be distinguished features useful for defining historical types. As with anything, initially one must guess as to which will be useful and which will not. The guesses will be phrased as hypotheses that x mode is historical in its distributional characteristics. Various means are available to enable one to make relatively good guesses. For example, having noticed what features occur at what locations, one could plot the spatial distribution of the modes as is done in Figure 17. Thus controlling one dimension of variation one can narrow the field of choices by reasoning:

1. Modes which occur at only one location are useless since they do not provide a means for comparing the various collections.
2. Modes which occur at all locations are not likely to be useful since they change too slowly to provide precise comparisons.
3. Modes that exhibit distributions closely linked to (a) geography or (b) environments are obviously variable in terms of space and/or function to a significant degree and are thus unsuitable.

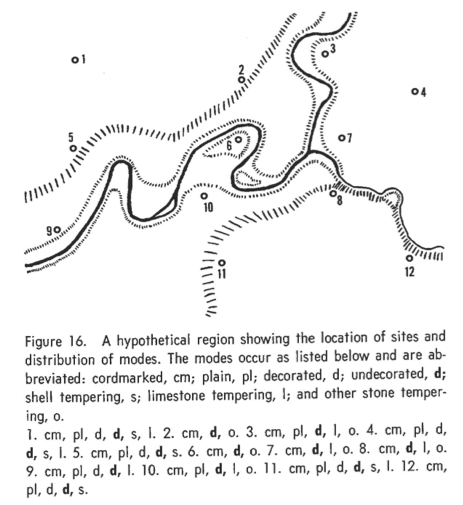


Figure 16. A hypothetical region showing the location of sites and distribution of modes. The modes occur as listed below and are abbreviated: cordmarked, cn; plain, pl; decorated, d; undecorated, **d**; shell tempering, s; limestone tempering, l; and other stone tempering, o.

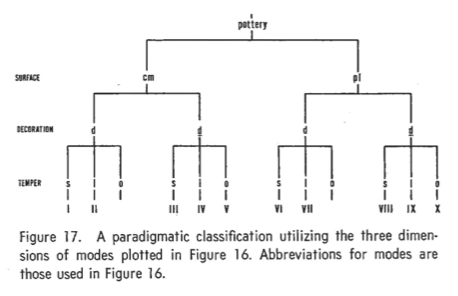


Figure 17. A paradigmatic classification utilizing the three dimensions of modes plotted in Figure 16. Abbreviations for modes are those used in Figure 16.

The search can be narrowed thus to modes which occur at several but not all locations and which do not exhibit any clear-cut patterning in space or correlation with environments. The justification for such choices could take the form: If cultural attributes have been chosen, they can be expected to have a patterned distribution. Features which display a random distribution in space must be variable in uncontrolled dimensions -among others, time. In Figure 16, modes in the dimensions of temper, decoration, and surface finish have the desired distribution, whereas the dimensions of shape and color as well as clay would appear to be patterned in space or correlated with environment. Limiting the initial choices in such a manner gives one reason to believe that types defined by these modes will be worth testing to see whether or not they are in fact historical. Noting this kind of distribution does not mean ipso facto that the unpatterned sets of modes will define useful historical types, for there are many other possible explanations for the lack of spatial pattern.

Figure 17 shows a paradigmatic classification utilizing three dimensions of modes: surface treatment divided into modes “cord-marked” and “plain”; decoration divided into modes “decorated” and “undecorated”; and temper divided into modes “shell,” 'limestone,” and “other stone.” Two of the twelve classes so generated have no *denotata*, that is, no sherds are cord-marked, decorated, and tempered with stone other than limestone; and no sherds are plain, decorated, and tempered with stone other than limestone. All the other classes are given names, Types 1-10. The next step will be identifying each location in terms of the types represented in its collection.

The final step is the seriation, the arranging of the Groups A-L so that the distribution of Types 1-10 is continuous. The seriation actually constitutes a test of the hypotheses made in selecting the definitive modes. If the groups can be arranged so that all of the types display continuous distributions (Figure 18), then the selection hypotheses can be considered correct. As anyone who has frequently employed seriation is aware, randomly devised classes will not closely approximate the required distribution. If the groups cannot be so arranged, with appropriate allowances made for the effects of sampling error upon the representation of the types, then the hypotheses made in the selection of definitive modes is shown to be incorrect and the types must be rejected. There may be a number of reasons why a set of groups cannot be seriated, aside from applying the technique to data for which it is inappropriate. The dimensions chosen may be appropriate (e.g., tempering is historical), but the divisions into modes incorrect (e.g., shell, stone, and sand rather than shell, limestone, and other stone); or the dimensions may vary significantly in dimensions other than time, thus tending to randomize temporal variation. There are means available to solve for these possibilities, but these are beyond the scope of this illustration.

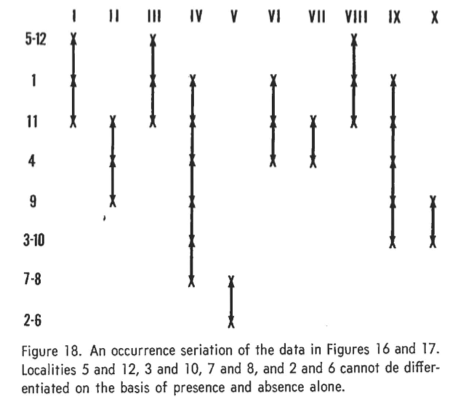
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Figure 18. An occurrence seriation of the data in Figures 16 and 17. Localities 5 and 12, 3 and 10, 7 and 8, and 2 and 6 cannot de differentiated on the basis of presence and absence alone.

To be certain that the order produced by a seriation is a chronology will require additional seriations of the same set of groups in terms of other materials (e.g., projectile point types, house types, burial types, etc.), and only that order which is repeated from one seriation to the next can be treated as a chronology. Insofar as testing the utility of a set of classes is concerned, however, the ability to seriate the group suffices.

Without a specifically stated problem there is no way, even if the definitive criteria are explicit, to justify the selection made. If the problem is specified and the *significatum* explicit, then: (1) the relevance of the criteria chosen to the problem is testable, that is, an assessment of utility is possible; and (2) given alternative classifications, the most sufficient and parsimonious can be chosen.

As it stands, however, most classifications are taken for granted. There is little or no concern with how the classes came to be and why. Classification is very often done for its own sake, and this requires no evaluation or concern. A prime contributor is the feeling that all of us have that a thing should have a name. The only problem is deciding what name to use. The implication from our discussion is that there classifications as there are problems. This is certainly not new, for just such an assertion is the crux of J. O. Brew's arguments cited earlier. His admonishments have not been generally heeded, because it is discomforting to the archaeologist to have one and the same set of artifacts belonging to ten different phases, or the same object assigned to ten different types. Nonetheless, the classes used must be a function of the problem if they are to mean anything if they are to be subject to testing and evaluation and if they are to be accepted because of utility rather than on faith.