

proper roles as means of stating and correlating the occurrence and distribution of *denotata* of otherwise defined classes. Archaeology's predilection for borrowing from other disciplines does not bode well in this respect, for particularly in the biological sciences have grouping devices gained some currency. The temptation to employ these mechanically lucid devices to create units is deceptively enhanced by their explicit accounts of what is already known. Where they fail is less obvious; they cannot be employed heuristically, and they are not testable in any meaningful sense beyond their mechanics.

part ii

SYSTEMATICS IN PREHISTORY

5

PREHISTORY

*A*s ought to be apparent at this juncture, the considerations undertaken as Part I of this volume do not constitute a fully developed, coherent theoretical system. Not only is such available elsewhere in various forms, but a treatment of this nature would far exceed the requirements for introducing some coherence into the formal operations and units of prehistory. Our goal is a coherent theoretical system for the formal aspects of prehistory, a much more limited goal than a general system. Thus the general considerations have been restricted to the explication, in terms adapted to current prehistory, of key notions—key in the sense that they are or ought to be issues within prehistory.

The initial problem to be faced in constructing a theoretical system for prehistory in formal aspects, and the one attended in this chapter, is that of defining the field for consideration. There are, of course, many possible ways of accomplishing this, and the choice of means as well as the end result have important consequences for all further operations and require deliberation. Indeed, one source of confusion in the literature of prehistory has been attributed to the vague notion of what prehistory is and what it is or ought to be doing. This vagueness undoubtedly reflects the unstructured manner in which prehistory has developed primarily from Old World antiquarianism—sometimes in conjunction with the natural sciences; sometimes, as in this country, in close conjunction with sociocultural anthropology;

and sometimes, at least effectively, in isolation. As long ago as 1953, it was possible for an eminent American prehistorian, A. C. Spaulding, to summarize prehistory as being that which prehistorians like to do—and nothing or little more. In many if not most quarters today this characterization is still accurate, the only important difference being that some prehistorians like to do things which their colleagues of twenty years ago had not thought of doing. There is, of course, nothing wrong with prehistorians' enjoying what they do; this is healthy, a requirement of a viable discipline. However, serious difficulties arise when this kind of characterization is the only accurate means of defining prehistory.

Prehistory has been defined many times and in various ways, this fact itself contributing in no small measure to the vagueness surrounding the meaning. Universal acceptance has not been accorded any definition, at least in part because all the definitions are more or less substantive, tied to a given area or problem. Boundaries around the field are drawn in terms of time and space (e.g., using the literal meaning of the label prehistory), or definition is in terms of specific goals such as "cultural reconstruction." Even if one or another definition of these sorts should gain currency, the vagueness attending the field of prehistory would have been merely shuffled under the academic rug. Insofar as the definitions are substantive, either definitions in terms of subject matter limited in time and space or definitions in terms of results, they do not specify how the field operates. One can do anything with a given subject matter, yet not all treatments of the preliterate past would be considered prehistory even by those who employ a history-prehistory distinction in the definition of the field. Likewise, not all "reconstructions" of the past would be admitted as prehistory, especially those frankly based on speculation, by those workers "defining" prehistory as reconstruction.

As was pointed out on several occasions in earlier chapters, neither the subject matter nor even the results serve to adequately separate the various academic disciplines. Rather it is theory, the manner in which a particular discipline views phenomena, that distinguishes the various disciplines and sciences. A particular view of the world will always be more relevant to some kinds of things than others, a feature which lies at the root of the subject matter approach to definition. Likewise, a

particular view conditions the kinds of results possible. Especially with today's trend toward multidisciplinary study, the relevance of a particular discipline to a particular subject matter is continually expanding. There is, even in the space of a few years, ample demonstration of the independence of discipline and subject matter. Likewise, unanticipated results are not infrequently obtained, sometimes completely reorienting disciplines in terms of the "thing to do." These arguments do not mean that subject matter is unimportant for the definition of a discipline, but rather that the form of the subject matter, the way in which it is conceived, and not what it is, must be used, and, further, that subject matter alone or in combination with results is insufficient. If prehistory is to be an academic discipline and a science it must be *a kind of study*, not solely *the study of a kind of thing*.

One way in which prehistory could be defined consistently with the above discussion is to first develop a formal theory of prehistory and then define the field as that in which this theory is operative. While consistent and certainly accurate, this circular approach does not convey much information and would only contribute to the vagueness surrounding the meaning of prehistory instead of providing a basis for departure. The definition to be presented here, along with the explication of the terms used in it, is hopefully informative while at the same time consistent with the requirements of such a definition.

Given that prehistory has grown like Topsy, any definition, save that prehistory is what people who call themselves prehistorians do, is bound to exclude some things done under that label and perhaps include others not usually conceived of as prehistory. The definition to be presented has the advantage of including much of what is done under the label, and, further, the substantive definitions can be viewed as special cases of adaptation to a specific area, specific data, or a specific problem which holds the interest of a given investigator. Substantive definitions are not "wrong," but they are limited to the problem or data they are designed to serve. A general definition provides not only a means of discussing prehistory in theoretical terms but at the same time it provides a means of deriving the substantive definitions and enables one to link these definitions to one another rather than treating them as competitive, contradictory, and inconsistent.

In spite of an attempt to define prehistory in such a manner as to include much of what is done under this label, some kinds of activity and some specific studies are, of course, excluded. These exclusions result primarily from a failure of the activity or study to meet the requirements of science rather than on other grounds. Their exclusion here does not mean that they are not worthwhile, profitable, interesting, or entertaining. It simply means that they are different in important dimensions from the other activities considered and cannot be judged by the same yardstick. It is not asserted, for example, that "amateur archaeology" is not worthwhile or that theologically-based speculation on man's past is not interesting. They cannot, however, be evaluated by the same means as used here.

With these points considered, *prehistory* is defined herein as *the science of artifacts and relations between artifacts conducted in terms of the concept culture*. This definition stipulates: (1) the kind of study—science; (2) the main concept with which explanation is undertaken—culture; and (3) the manner in which phenomena must be conceived—artifact. Insofar as any given work conceives its data as artifacts and uses scientific means to achieve explanations framed in cultural terms, it is prehistory and within the realm of our examination. The remaining portions of this chapter will first explicate each of the three key notions involved in the definition and then examine the implications of this definition of prehistory for the relationships to other sciences and non-sciences closely linked to prehistory.

Science

In view of earlier discussion there is little need to further belabor this notion. Insofar as a given discipline has a theoretical structure which is employed to systematically organize phenomena for the purposes of explanation of these phenomena in a manner capable of testing, it may be considered a science. Employing this criterion excludes: (a) intuitive, non-rigorous approaches by virtue of a lack of overt theory and testability; (b) approaches which focus upon ideas rather than upon phenomena (e.g., philosophy); and (3) "descriptive" approaches which do not have explanation in the sense of prediction and/or control as an end product or a possible end product. A casual

survey of literature bearing the label prehistory might suggest that it generally fails to meet these criteria, particularly in a lack of theoretical structure and testable conclusions. It is the contention here that this impression is more apparent than real, that at least as far as formal theory, systematics, is concerned, most of what has already been done in prehistory meets this criterion, but implicitly rather than explicitly. Further, while most of its conclusions are untested, they are testable.

Artifact

Unfortunately, there is no generally accepted definition of the subject matter of prehistory, again because of the substantive preoccupation of the discipline. The many definitions in the literature reflect the requirements of particular problems, kinds of problems, and areas, and thus are not suited for theoretical use or, for that matter, practical application beyond the particular problem or area for which they are developed. This lack of unity has been customarily dealt with by ignoring it—apparently no thought accorded to the non-comparability and contradiction that such fundamental disagreement introduces into the product of different investigators' work.

The concept artifact must be treated as a kind of theoretical template which segregates those phenomena of interest and amenable to scientific study by means of the concept culture and thus imposes a particular view upon the phenomena so segregated. The term *artifact* will herein be understood to mean *anything which exhibits any physical attributes that can be assumed to be the results of human activity*. First it should be remembered that "anything" could be rewritten as any "thing" or "event" since these are considered interchangeable; however, most past work in the field involves a "thing" conception and terms, and the thing kind of terminology is retained. One notable exception to this traditional view is chronological studies which must conceive data as events for obvious reasons. You cannot date an object before you, since it is still in existence, but, rather, must date some event or events (e.g., the event of manufacture, breakage, deposition). The second aspect of the definition which might require explication is the use of "attribute." Attribute must be understood both as "thingness" and "eventness." Not only is attribute intended to refer to qualities in the

ordinary sense of quality, but also to position or location in the three-dimensional world. Human activity is manifest not only in changes of form but also changes or reorganization of locations, and, indeed, is usually a matter of both. One need think only of the importation of raw materials to have numerous examples of artifacts by virtue of location alone. The final aspect of the definition requiring some additional consideration is the "can be assumed to be" phrase. Unless one sees something being modified in form or moved, one must always *assume* the agent of human activity. Since prehistory is most often concerned with the past rather than the present, this becomes an important aspect of artifact and is the reason for the insertion of "assumed" in the definition. It is assumed that a given object or event is a product of human activity if its location or any other of its attributes cannot be accounted for by known natural processes. Thus the identification of artifacts is a problem of comparison with the known products of natural processes. It is important to recognize that individual attributes of objects are not in and of themselves distinctive of human activity until that point in history in which man begins to chemically alter the natural environment. Rather it is *pattern*—on an object, over a series of objects, or through space—that is distinctive. Prior to the advent of constructed materials the only means for shaping stone, for example, were pecking, grinding, and chipping, all of which occur naturally. Much prehistoric literature to the contrary, the removal of a flake is not the basis for assuming that an object is an artifact; however, the pattern of flakes removed from an object or the patterned occurrence of the objects through space may provide such a basis. For example, a chip on a finely-worked Danish Neolithic dagger is not distinctive of human modification. Each flake individually considered could well be the product of natural processes; however, the patterned occurrence of several hundred flakes resulting in the dagger form is distinctive especially in view of the large number of such objects known to occur and the context in which they are found including other objects most easily explained as the products of human activity. Only in those cases in which too little information is available to make appropriate comparisons is there any difficulty in deciding whether or not a given object can be assumed to be the product of human activity.

In this context it might be pointed out that science in-

evitably sacrifices completeness for accuracy. In viewing the identification of artifacts as a comparative problem, it is important only that everything identified as an artifact be indeed an artifact. Undoubtedly many things will be excluded that should be included, but this is not of pragmatic consequence. One of the normal kinds of progress within a science, and certainly here within prehistory, is the continual expansion of its sufficiency.

It is well to digress at this point to consider the utility, the necessity of theoretical definitions such as that presented for artifact. The several definitions of artifact in the archaeological literature can be viewed as special cases, restrictions for one or another reasons of this theoretical definition, and can be logically derived from it. If two definitions can be derived from the same general proposition, then the relationship between the two can be stated. Special definitions are often adaptations to the contingencies of executing a piece of research. Some definitions specify the scale of the object to be considered an artifact as portable discrete objects. This kind of definition is useful for the recovery and recording of data in the field, for obviously the size and coherence of an object have important bearing on techniques to be used. In this case, other larger or less coherent objects are given other designations such as "features" or "structures." Non-discrete units based upon proximity and association such as "squatting places" and other identifiable loci of specific activities are gaining currency as artifacts. Because of their lack of discreteness, a function of scale, these units must be analytically constructed and thus are terminologically differentiated from the more usual discrete objects. Such "features" and units are artifacts in the same sense as those items given the label "artifact," and they will be treated the same in any system of explanation. The differentiation is simply a recognition of the effect of scale and coherence on recovering and recording data.

Another kind of operating restriction is the division of artifacts into "incidental objects" or "non-cultural debris" or "food remains" and "artifacts." In this case the restriction serves to segregate artifacts into categories requiring different kinds of academic specialists for identification—bones to the zoologist, plants to the botanist, and tools to the prehistorian. Again, all the categories have the same logical properties. The differentiation reflects only the structuring of academic disciplines, not some difference in kind in the data.

Special definitions are likewise employed for particular kinds of problems. For example, an investigator interested in stylistic change might advantageously restrict artifact to intentionally manufactured items. This kind of definition is not at all uncommon in archaeological writing, for style has been an important area of inquiry. An investigator interested in technology may restrict artifact to manufactured items, the by-products of manufacture, and the raw materials. In similar fashion one finds that artifact is frequently restricted to modified forms in studies dealing with early man where the presence of man and his activities is problematic.

All of these special definitions and many more are best treated as part of method, and not matters of theory. All can be derived from the general theoretical definition and related to one another explicitly. If, in the construction of a program of research, the investigator starts with a theoretical definition and adapts it overtly to the problem at hand, the frequently encountered problem of utilizing concepts inappropriately defined for the particular purpose to which they are put is eliminated. Further, a precise statement of the comparability of different studies is possible, and the perspective gained from employing this procedure in developing tactical concepts also aids in recovery procedures. It is unfortunately true that in some parts of the United States many kinds of tools have not been collected in excavation and surface reconnaissance because the investigators were implicitly using a restricted definition of artifact which had been developed in stylistic studies; this has quite effectively rendered the data useless for any other kinds of studies. Most of the argument about what is to be called artifact and what is not is an argument about words, for argument is usually focused on two or more special tactical definitions designed for different purposes. The single most important benefit obtaining with frankly theoretical definitions is that theory—the concepts themselves apart from a particular problem—can be discussed. Indeed, there cannot be theory without such definitions, and with them arguments at cross-purpose can be avoided. Further, laws are impossible achievements until the terms in which they are phrased are theoretical.

Returning to the concept artifact itself, there is one final point that cannot be emphasized too strongly. Defined as it has been here, artifact is the *only* subject matter of prehistory. Pre-

historians do not study "culture" or past "societies" or "man's past." Culture and society are anthropological concepts, and man's past, a metaphor. The only tangible phenomenon which can serve as data, with which prehistorians actually work and which is capable of explanation, is that encompassed by artifact. Confusing the means of explanation (culture, society, and so on) with the phenomena that are to be explained (artifacts) only results in further confusion, inconsistency, and untestable conclusions. This, of course, does not mean that one cannot study concepts, or any other words, for that matter; it only means that such study is not prehistory, but rather philosophy or linguistics, depending upon the approach.

Culture

Culture is the most overworked word in the anthropological jargon. It would sometimes seem that every initiate to the anthropological disciplines must invent a definition for it to gain admittance to the profession. In 1952, Clyde Kluckhohn and A. L. Kroeber recorded some published definitions and concluded their treatment with one of their own, summarizing the salient features of previous definitions. Their definition constitutes a generalization, for not all of the features they include occur in any definition. The lack of a generally accepted meaning for the term which prompted the Kluckhohn and Kroeber endeavor appears in retrospect to have been aggravated if not generated by the insistence upon using substantively-bound, special-purpose definitions. The Kluckhohn and Kroeber definition did not rectify the problem. Indeed, this definition probably has less currency than many of the definitions it summarizes. As a generalization it still is restricted to the problems that were covered by the summarized definitions, and is too unwieldy for practical use. The disagreement, inconsistent usage, and outright contradictory content of many of the various definitions has been further complicated by a penchant for including as part of the definition various inferential elements that pertain to why the concept may be useful.

Herein the concept *culture* is to be understood as meaning *shared ideas*—and nothing more. The various special-case definitions may be derived from this by:

- (a) restricting the coverage to some special set or sets of shared ideas, in the fashion that restrictions can be imposed on the theoretical definition of artifact;
- (b) inferring or speculating how the ideas come to be shared (e.g., those stipulating learning);
- (c) inferring why the ideas are shared (e.g., those which view it as an adaptive system, etc.).

These tactical definitions have their place in methods (e.g., a special definition for the problems and views of economic anthropology) and in techniques (e.g., a definition adapted to the particular data being studied). However, they cannot provide an adequate basis for theoretical considerations.

Quite apart from this notion of culture as an explanatory concept, there is the use of culture in the partitive sense in both sociocultural and archaeological literature. In speaking of "a culture" sociocultural anthropologists are denoting a set of people who to a greater or lesser degree share a number of ideas which are not shared by people outside that set. In an archaeological context, "a culture" is even more vague, denoting either a given set of assemblages of artifacts or a set of abstract units such as phases or components, which hold in common a relatively large number of features or "traits." This usage of the term culture, in spite of the label, bears little relation to culture as an explanatory concept and is nowhere employed herein.

There are some important implications, however, of even the simplistic definition of culture used. First, culture is a concept, an idea. It has no objective existence itself and is not subject to study or explanation in any scientific fashion. It is a means of explanation. Further, its referent, shared ideas, does not have any objective existence. Ideas cannot be observed, but are always inferred from behavior, linguistic or otherwise, or products of behavior. A simplistic parallel can perhaps be usefully drawn between culture as an explanatory concept and the concept of gravity in the physical sciences. Gravity is a concept used in the explanation of the motion of bodies. There is no gravity in the phenomenological world; no one has ever seen it, and no amount of generalization will ever lead to gravity. Gravity is a posited concept which permits the prediction of the motion of bodies in fully calculable terms. What is observed is the motion of bodies; what is explained is the motion of bodies, and it is done in terms of the concept gravity. As in the case of cul-

ture, the referent for gravity is not observable; that is, forces cannot be seen or measured apart from the motion of bodies. It is in this manner that the concept culture can be and implicitly is employed by prehistory. Arguments as to whether artifacts and/or behavior are "culture" are just as nonsensical as arguments about whether the moon or its motion is gravity.

The character of the concept is imparted both by the stipulation of ideas as a referent and that the ideas must be shared. There is little doubt that explanation of artifacts and behavior can be usefully attempted in terms of the ideas held by the people involved. Perhaps the point of confusion in this respect revolves around *which* ideas of the people are considered. It is obvious that the "ideas" that are solicited from living people under study are not the means of explanation, but are part of what is to be explained. The ideas which serve as the referent for culture are imputed to the people to provide the mechanism for explanation, much in the manner as the force called gravity is imputed to nature for the explanation of motion of bodies. It is unimportant and indeed unknowable if either the forces or the ideas actually obtain in nature. What is of importance is whether or not such concepts permit the development of explanations, for explanations as predictions and means of control are testable. Nothing could be gained from a demonstration that the ideas called culture exist beyond the mind of the anthropologist or prehistorian.

While there has been some criticism of the "sharing" stipulation (see bibliography), this stipulation derives from and is a requirement of a science. "Sharing" implies, or rather is a rewording of, repetition or recurrence through time and/or space of some form. Without repetition explanation is impossible because nothing recurs to be explained. Without repetition both systematics and science are impossible. To conceive of data as unique or "idiosyncratic" is to abandon any attempt at explanation (not infrequently when these terms are used, they are tendered as explanations and employed as a warrant to consider no further the data so labeled). From the outset phenomena are assumed to be unique, and the problem is to categorize them so that they are no longer unique and thus are capable of explanation. Science cannot predict when a given molecule of water will leave the surface of a pan of water and at what temperature, but it can predict, and quite accurately, when the pan of water

will boil and how to bring about that condition. In short, the stipulation that culture, as a scientifically useful concept requires a component of sharing, permits the possibility of ahistorical laws for human activity. The concept culture, then, provides the means by which prehistorians explain the products of human activity. Quite obviously, this does not exhaust the possibilities of explaining those products; it does so only for them as artifacts. Physicists, geologists, biologists, theologians, and farmers can also explain the same objects, each with different results. One might insist in the first three cases that the results are trivial because they do not account for the human aspects or because they yield more interesting results for other phenomena, and in the latter two cases one might object that the results are not scientific and do not explain in the sense used here. But certainly none of these are wrong.

In summary, the definition of prehistory tendered earlier in the chapter can be more fully explicated. Prehistory is a kind of study, a science, sharing with other sciences the aim of explanation of phenomena utilizing a theoretical structure. Prehistory is distinguished from the other special sciences in that it employs the concept of culture as the basis for explanation of phenomena conceived of as artifacts.

The relationship obtaining between prehistory and non-sciences is but a special case of those obtaining between science in general and non-sciences which have been already considered in Chapter 1. Thus a treatment here of the relationships between prehistory and humanistic studies in general would be redundant; however, some detailed consideration of the relationship of prehistory as defined here and history and sociocultural anthropology is warranted by the close connection attributed these three fields in some archaeological writing. History, as was asserted earlier, can be distinguished from science, and thus from prehistory, on two fundamental grounds: (1) history does not produce or attempt explanation in the sense of prediction and control; and (2) the organization of history's data is assumed to be chronological. Thus the only "theory" history need employ is a common cultural background of writer and reader. History does not closely articulate with prehistory except in the sound of the name.

That history produces chronicles is not distinctive, for science likewise produces chronicles. However, the chronicles of

science must be rendered in terms of classes derived from theory, whereas the historical chronicle consists of chronologically-linked, unique events. The scientific chronicle, much a part of prehistory, is easily confused with historical chronicle, especially in prehistory where the theory employed in constructing a scientific chronicle has been left implicit. To further complicate matters, historical rather than scientific chronicle often appears under the title of prehistory, usually called "culture history." Indeed, most of the things called "culture history" are examples of the non-explanatory "descriptive" approach specifically excluded in defining prehistory as a science. Because the subject matter is usually preliterate man, "culture history" is usually an *inferential* historical chronicle, both the chronology and the events being inferred. In the view taken here, this kind of culture history is properly the practice of history on preliterate data, a kind of ancient history. History and prehistory are not complementary studies in terms of their subject matter. Each is applicable to the results of human activity regardless of the presence or absence of written records, though this feature profoundly affects the techniques of data collection.

Likewise, prehistory is applicable to contemporary results of human activity. The results of this application are less interesting to most people than those produced by history or other humanistic and scientific studies. For this reason, perhaps coupled with a feeling that one is not an "archaeologist" unless one deals with very ancient data, prehistory has seen comparatively little application to contemporary or modern data.

Importantly, history and prehistory have little in common, being quite different kinds of study with discrepant aims and potentials and overlapping fields of application. The general feeling that they are similar stems first from the fact that both make use of the chronicle, though each uses the chronicle differently, and, indeed, the chronicles themselves are different; and, secondly, because most prehistorians are also historians, that is, most people who practice prehistory also at one time or another construct "culture histories." Given their radically different nature, a separation of the two is absolutely necessary if progress is to be made in either.

In the United States, but not universally, prehistory is academically considered part of sociocultural anthropology. While the close connection and in many respects profitable association

between the two is not to be denied, it is difficult to conceive of prehistory as a science and also a part of or kind of sociocultural anthropology. Most kinds of sociocultural anthropology have little about their nature to suggest the field is a science, though good cases can be made for particular branches (such as ethnoscience) and particular studies being so. Currently, the main part of sociocultural anthropology is more like a flat history of mainly non-western peoples. This does not mean that sociocultural anthropology is incapable of being the science of man, but simply that most of it is not that and is not developing in that direction. There are, however, important connections between prehistory and sociocultural anthropology, far more so than obtain between prehistory and history. The primary point of articulation is in the concept culture, a concept developed by sociocultural anthropology. Sharing such a fundamental concept has naturally resulted in a great many correlative commonalities. In many respects the terminology used to manipulate data is the same. Further, sociocultural anthropology's broad interests in all kinds of human activity have been adopted into prehistory, along with the perspective that comes from familiarity with non-western lifeways. Thus many of the distinctive and essential elements and directions of prehistory are held in common with sociocultural anthropology; however, these are articulated into two different kinds of study. In the case of prehistory the concepts are part of an over-all theoretical system aimed at explanation of human activity, whereas the kind of articulation these same concepts receive in sociocultural anthropology is less systematic, more various, and, at least from the outside, less indicative of a purpose. While one can be appreciative of the important contribution made by sociocultural anthropology to prehistory, there is nonetheless a very stringent limit to the interdependence of the two given their different structures, potentials, and aims.

With the "cultural reconstruction" approach, generally acknowledged to have been given its modern impetus by Walter W. Taylor's *A Study of Archaeology*, there is an attempt to do sociocultural anthropology in the past. Not an insignificant amount of modern endeavor represents a technically more sophisticated and less ambitious version of this general approach. To a limited extent all prehistorians engage in some kind of reconstruction, or, rather, construction; however, as an approach, "cultural reconstruction" has all of the non-explanatory,

tory, descriptive limitations of old-time cultural anthropology, complicated by far inferior data. The interests, not the methods, of sociocultural anthropology and its ancient analog "cultural reconstruction" makes a valuable contribution to prehistory. Again the point is not to criticize "cultural reconstruction" as such, but simply to note its exclusion from the realm of science and to differentiate it from prehistory.

In discussing the relationships of prehistory to history and sociocultural anthropology, alternative approaches to the study of artifacts have been indicated. History and sociocultural anthropology are not, of course, the only alternative studies of man's activities. There are many well-developed fields, mainly within what has been called humanistic studies, which attend a more restricted segment of man's activity. Many kinds of study and inquiry have something to offer about the remains of man's past. Prehistory is but one such study, the science specifically directed toward these remains.

The problem to be pursued in the remaining chapters is simply a delineation of how one gets from science in general to a science of artifacts—essentially a substitution of prehistory as defined here for science in the general scheme presented in Part I. The definition of prehistory provides all the necessary elements for making the logical step from science to a science of artifacts. Given the earlier scheme, this is phrased largely in terms of a shift from the arrangement of things, to the cultural arrangement of things. No attempt is made to develop a new means of making this step, but rather the aim is to make explicit the implicit manifestation of this step in the literature of prehistory.

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, the 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

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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 "The 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 wholistic 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

in 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. Any-one 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 in terms of the 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 in 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: (1) 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

frog's answer is going to be treated as an instance of highly unusual behavior 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 inferred 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. The 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/idiomatic 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 rules 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

noncomparability 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 literature 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 hierachic 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 Linnean 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.

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, effec-

		A		B		B1		B2		B2Y		B2Y-a					
		A1		A2		A2X		A2Y		B1X		B1Y		B2X		B2Y	
		1	2	3	4	5	6	7	8	9	10	11	12	13	14	15	
A1X	A1Y	A1X	A1Y	A2X	A2Y	A2X	A2Y	A2X	A2Y	B1X	B1Y	B1X	B1Y	B2X	B2Y	B2Y-a	
A1		A1		A2		A2		A2		B1		B1		B2		B2Y-a	
		A		A		A		A		B		B		B		B2Y-a	
		1		2		3		4		5		6		7		8	
		X		Y		X		Y		X		Y		X		Y	
		a		b		a		b		a		b		a		b	

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

tively "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 13 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 A1 is a kind of A, as A1X is a kind of both A1 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 A1Xa 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 as 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 13 the occurrence of Classes A1-B2 is a function of applying the 1-2 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 A1-B2 would not occur in the new classification as Aa-Bb do not occur in Figure 13. 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 linkage 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 inter-

changeably. It is necessary, however, to differentiate two notions of ranking or inclusiveness treated under the labels of level and scale 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.

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 (group) which display the same degree of inclusiveness or rank. Scale is the stipulation of the size of 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 of 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 phe-

nomena). Level alone is sufficient to discuss classification as a process both in a general sense 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.

In these terms the concept artifact designates the synthetic level of cultural phenomena. As defined, artifacts have no scale.

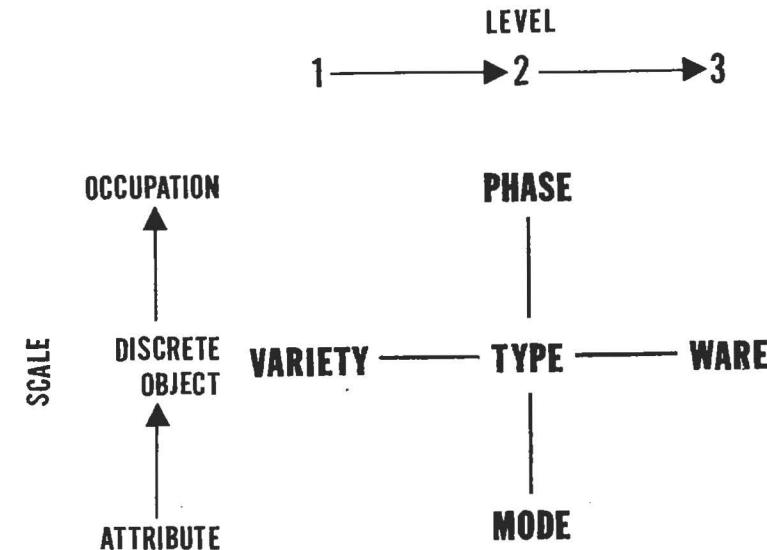


Figure 14. Relationship of level and scale.

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 the 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 minor 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 a result as a 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 conveying 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 litera-

ture, 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 in-

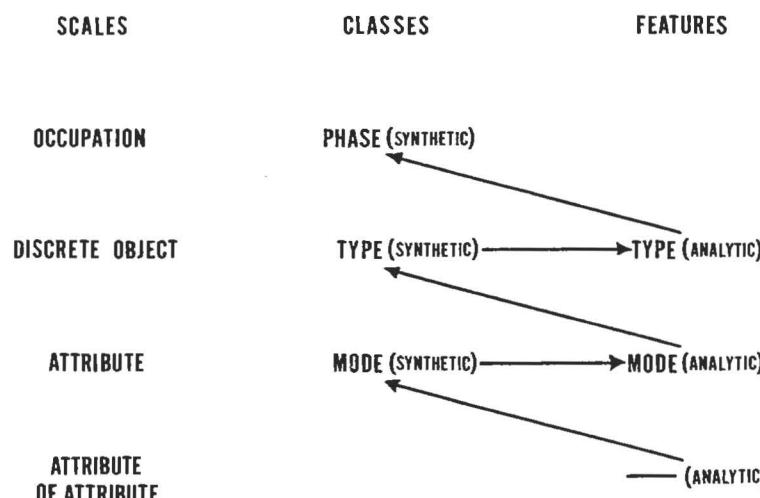


Figure 15. Scales of classification customarily employed in prehistory.

volved. 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.

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 criteria.

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, and the names serve to designate this change in level. Series and stages are

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 in time and space, and they serve to link other classifications through coordinate *denotata*. Series are usually defined upon stylistic features; stages are usually defined upon 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 continental summaries and literature intended for lay consumption or introductory texts.

Various combinations of these larger units occur in 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 di-

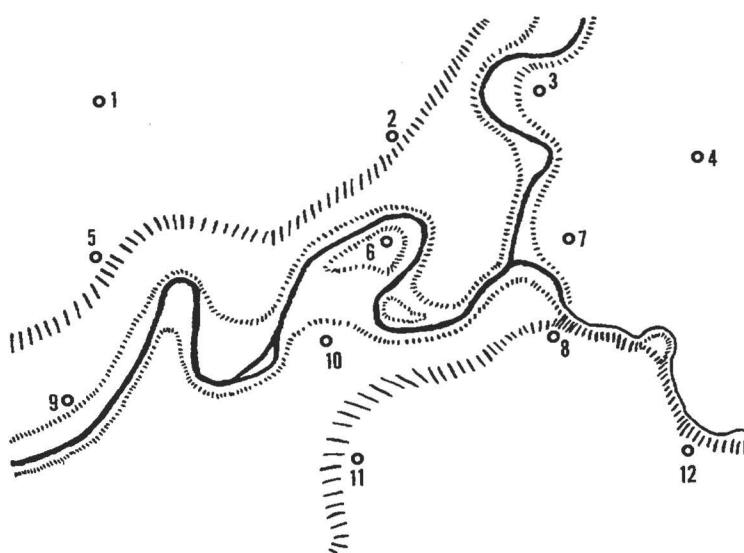


Figure 16. A hypothetical region showing the location of sites and distribution of modes. The modes occur as listed below and are abbreviated: cordmarked, cm; plain, pl; decorated, d; undecorated, **d**; shell tempering, s; limestone tempering, l; and other stone tempering, o.

1. cm, pl, d, **d**, s, l. 2. cm, **d**, o. 3. cm, pl, **d**, l, o. 4. cm, pl, d, d, s, l. 5. cm, pl, d, **d**, s. 6. cm, **d**, o. 7. cm, **d**, l, o. 8. cm, **d**, l, o. 9. cm, pl, d, **d**, l. 10. cm, pl, **d**, l, o. 11. cm, pl, d, **d**, s, l. 12. cm, pl, d, **d**, s.

mensions (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 no-

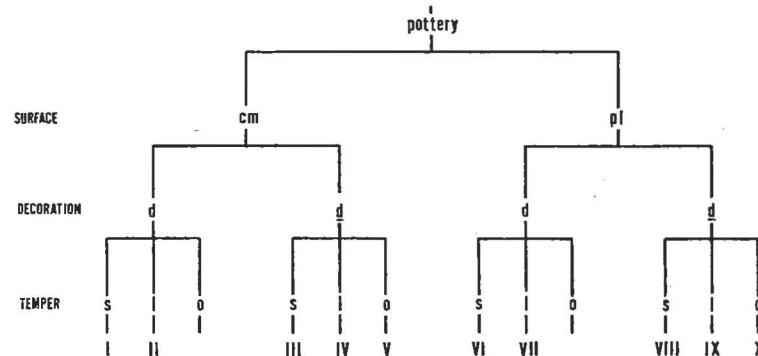


Figure 17. A paradigmatic classification utilizing the three dimensions of modes plotted in Figure 16. Abbreviations for modes are those used in Figure 16.

ticed 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.

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

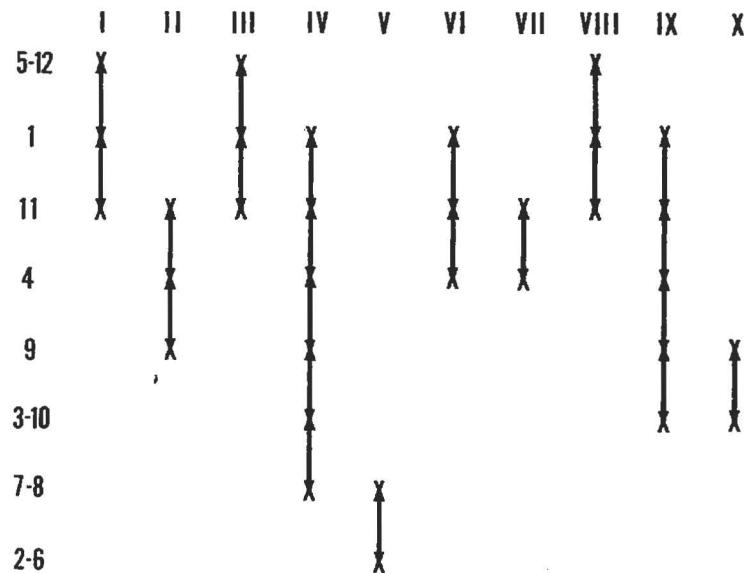


Figure 18. An occurrence seriation of the data in Figures 16 and 17. Localities 5 and 12, 3 and 10, 7 and 8, and 2 and 6 cannot be differentiated on the basis of presence and absence alone.

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.

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 groups 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 will be as many 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.

7

GROUPING IN PREHISTORY

Groups, aggregates of phenomena, are the focus of scientific study, for it is phenomena that science seeks to explain. However, it has been argued that, as the device for construction of groups, grouping is entirely inappropriate to scientific endeavor, and that the only groups profitably employed are the *denotata* of classes, especially the *denotata* of paradigmatic classes. To briefly review the rationale for the exclusion of grouping as a device for scientific unit construction: (1) groups constructed by means of grouping devices can have only extensional definitions consisting of a list of members; and (2) thus such units cannot recur through time and space (a requirement of prediction and control) or be shared (the special requirement of the notion culture). Because groups so constructed consist *only* of their members, they are applicable only to the members originally included and cannot incorporate new information. Such groups are history-bound, inappropriate to, and indeed impossible to use for measuring change in either time or space.

The cursory considerations undertaken in this chapter, then, are germane to our purpose only insofar as grouping has been used or suggested in prehistory to create units. The major aim is to identify grouping so that it can be avoided. The identification of groups which are the products of grouping as

opposed to groups which are the *denotata* of classes would be an easy matter were it not for a penchant of prehistory's literature to present a description of *denotata* without presenting the classification by means of which the *denotata* were assembled. This procedural error makes it difficult to distinguish grouping and classification in the literature, for most of the readily usable criteria are not presented (e.g., explicit definitions). Adding to this difficulty is the lack of problem in many studies. When units are constructed for their own sake or when the "problem" is "description," the units are not used beyond their names, eliminating any possibility of identifying the nature of the unit from its characteristics of use. This is most unfortunate, for if a unit is employed for some purpose, the use will suffice to distinguish between those units which are the product of grouping and those which are the product of classification. These circumstances are sufficiently common that most units used by prehistorians are amenable to interpretation as either groups or classes. Only when the entire discipline is considered is it possible to assess the nature of the units commonly employed.

Admitting the difficulty of distinguishing grouping from classification in prehistory as a function of the sloppy treatment accorded systematics, all that can be done is point out some of the more frank uses of grouping and the problems which result from these attempts and proposed procedures. From the outset it should be evident that any method, irrespective of its pragmatic utility, can be constructed on paper. The only requirements it must meet are those of logical consistency. Thus it is possible, and, in fact, occurs, that the rationale for some specific study's units may be presented as one or another grouping device, even when the actual procedure has been paradigmatic or taxonomic classification and when the device offered as the rationale could not conceivably have produced the units attributed to it.

Insofar as I am aware, grouping devices have been used as the rationale or proposed as the means of unit construction only at the scales of discrete object and occupation. Both numerical taxonomy and statistical clustering are in evidence for discrete objects, while only numerical taxonomy has been used for occupations. In all cases the units have been labeled with terms used to designate classes so that "type" in the litera-

ture can mean either units which are *denotata* of paradigmatic classes or the products of grouping. The remainder of this chapter will attempt to show how grouping has been used, what the characteristics of its use are, and the problems which result.

Statistical Clustering

In terms of method, there is nothing which can be added beyond what has already been presented in Part I, since that discussion is based largely upon the use of statistical clustering in prehistory. The primary advocate of statistical clustering in prehistory has been A. C. Spaulding, who first detailed the approach in his 1953 "Statistical Techniques for the Discovery of Artifact Types." The approach begins with a paradigmatic classification. Indeed, Spaulding presents the clearest statement of paradigmatic classification that can be found in the prehistoric literature, being particularly noteworthy in the clear recognition of the dimensional character of the defining modes. The frequency of the definitive modes is tabulated for the collection being considered, and an expected frequency of combinations of the modes in discrete objects assuming a random association of the modes is calculated. Essentially this is a statement or prediction of the number of combinations that will be found strictly as a function of the frequency of the modes. The next step is the tabulation of the actual combinations of modes found in the collection, and the results of this tabulation are compared with the expected frequencies. The outcome of this comparison, which takes into account the size of the sample considered, is the isolation of combinations of modes which cannot be accounted for as the result of random association and vagaries of sample size. There are, of course, two possible kinds of clusters: negative ones, combinations which do not occur or which occur much less frequently than would be predicted on the basis of random association; and positive ones, clusters which occur more frequently than could be predicted on the basis of frequency of the individual modes. The ability to detail what combinations are actually realized out of those combinations that are logically possible is one of the distinct advantages of explicit paradigmatic classification over other kinds of arrangement. It provides immediate feedback in

the form of a non-random distribution that the attributes chosen are the products of patterned behavior. Should the distribution be random, it is reasonable to assume that the attributes chosen are not culturally significant in the form in which they have been conceived. The isolation of positive clusters is taken to be a discovery of genuine tendencies on the part of the makers to combine sets of attributes, and the positive clusters labeled types, or rather, potential types. They are potential only, because if two or more significant clusters differ in a few modes (i.e., are closely "similar"), they will be grouped together as a single type of two varieties.

Up to this point in the procedures, there are no serious difficulties. Two sets of classes are in evidence, the modes used to characterize the material and their combinations into paradigmatic classes (Spaulding's attribute combinations). The comparison of the frequency of the modes with the frequency of their combination indicates that the choices of modes are culturally significant. The difficulty arises when those combinations which are heavily represented are singled out as "types," something quite different from the sense in which type is usually employed, for here the types are directly linked through the counts made of attributes and combinations to a particular body of artifacts. Further, not all the objects in the collection need fall in positive clusters, and those which are infrequently represented are not recognized as types but relegated to the status of "abnormal" combinations of modes. It is likewise entirely possible that no clusters, either positive or negative, might be found, and thus the collection be regarded as having no types or as being all of one type.

What has been done is clear, as is the nature of the units which result from this approach. The *denotata* of paradigmatic classes (termed in the approach "attribute combinations") at a given location in time and space have been counted and this tabulation compared with a tabulation of the *denotata* of the definitive modes (termed in the approach "attributes"). The comparison of these two sets of *denotata* differing in scale is then used to create units called "types." The "types" are quite obviously groups of real objects. Any kind of counting requires phenomena, and any kind of units based on count in any fashion are phenomenological, that is, groups. This situation could be treated as a particular case of the general confusion of

classes with their *denotata* in prehistory, the name "type" simply being applied to the objects assigned to the type at a given locality, were it not for the lumping of closely "similar" clusters into the same unit as varieties.

Further difficulties arise when the infrequently noted combinations are regarded as abnormal combinations of modes. A combination of modes which is infrequently represented at one locality and point in time, and thus an "abnormal combination," will usually be in some other locality and time frequently represented and thus at that locality a "type." Popularity varies through time and space, and units based upon popularity necessarily vary as well. The peculiar consequence of employing statistical clustering is the creation of sets of units unique to each sample location—giving rise to a "rubber yardstick." Being bound to the occurrence of attribute combinations at specific localities, the meaning of the units will change with the frequency of representation. Types so constructed cannot provide means of either comparing localities with one another or measuring formal change. In short, the units are descriptive and are not capable of providing the terms for explanation. Nor, in the absence of problem, are they testable. Figure 19 presents a comparison of the distribution of *denotata* of paradigmatic classes labeled Type A through Type D with statistical clusters labeled "Type a-Type f." In this simplified hypothetical case, the vertical axis of the diagram represents time, the width of the curves the frequency of occurrence of *denotata* of paradigmatic classes. The paradigmatic classes do not change through time but rather the frequency of occurrence or the presence and absence of their *denotata* change. All of the localities represented by the bars in the diagram can thus be compared with each other, the paradigmatic classes providing the basis of comparison. The statistical clusters, it will be immediately noted, are restricted to specific localities, being actual groups of artifacts, and thus these units themselves change through time and provide no basis for comparing the various localities. In situations requiring larger numbers of types, the contrast between clustering techniques for unit construction and paradigmatic classes would be even more dramatic, though more complicated in its portrayal. The addition of new localities, new data, will result in a proliferation of the number of clusters but will not affect the number of paradigmatic types.

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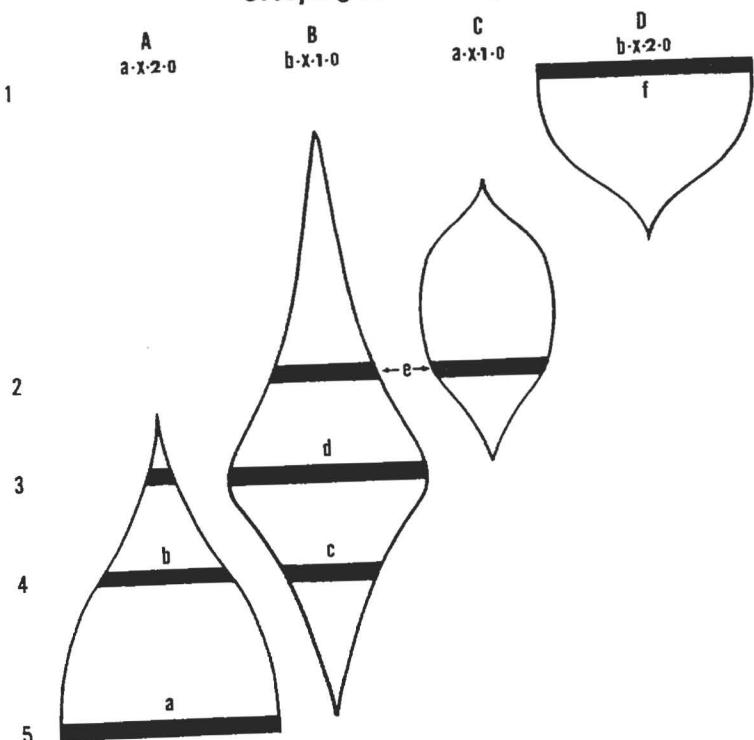


Figure 19. A comparison of statistical clusters (a-f) and the distribution of paradigmatic classes (A-D). Localities (1-5) are represented by horizontal sets of bars; the frequency of occurrence of *denotata* is represented by horizontal sets of bars; the frequency of occurrence of *denotata* is represented by the width of the curve. The definition of each paradigmatic class is shown immediately beneath the type designation.

These difficulties were recognized by Spaulding in proposing the device, for he clearly states that the "types" are restricted groups of objects, and that, further, the "types" are restricted to single localities and single occupations of these localities, the essential identifying characteristics of grouping devices. However, the importance of these difficulties, seeing them as difficulties rather than characteristics, is not appreciated, for the

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"problem" for which the statistical clusters were to provide an order was "description." Thus, in the absence of any testable, definable goal, there was no means to judge the utility of the results. Any kind of units which provide a means of naming will suffice "description." No complicated devices are required, nor do they have any demonstrable advantages, save maybe intellectual satisfaction, over any other means of naming.

The publication of this device for unit construction led to a long and rather involved argument in the literature with James Ford, who was using types in the sense used herein (paradigmatic classes). The major components of this argument are listed in the bibliography, and make most useful reading, clearly demonstrating the problems presented by using the term "type" for widely different kinds of units. A careful reading of this argument will also demonstrate the utility of making a distinction between groups and classes in attempting to understand the archaeological literature. As in all cases in which argument is more about words (in this case "type") than substance, the argument slowly dies instead of being concluded decisively. It is worth pointing out, however, that the statistical cluster has not seen use in prehistory for any operations beyond those kinds of studies in which creating units for their own sake—description—is the goal. Spaulding's clear exposition, some of the finest in the archaeological literature, is often cited as the rationale for "type" even when what is actually done is paradigmatic classification such as Ford argued for, though hardly as succinctly.

The discussion of clustering leads rather directly to a larger problem, the quest for "folk classifications." Presumably this quest is a motivation behind statistical clustering as a means of unit formation, given that one of its stated aims is the discovery of genuine tendencies on the part of the makers to combine modes. Aside from the fact that there is no way to know whether or not the modes initially used were recognized in some cognitive sense by the makers, the irrelevance, indeed the detriment of such "folk classifications" to scientific investigation, has already been argued. It must be emphasized that employing paradigmatic classes in no way prohibits a statement of these combinational tendencies. These variable representations of combinations are, however, statements about the distribution of *denotata*, not characteristics of the classes.

For example, in Figure 19 one may by inspection or by the methods outlined as statistical clustering characterize the time and space represented at Locality 5 by the tendency for a single combination of modes (a-X-2-o), Locality 2 by the tendency for two combinations (a-X-1-o and b-X-1-o) which differ in a single mode, and so on, without binding the analytic units to the circumstances that obtain at any one of these localities. One might further speculate that the people involved in Locality 5 recognized but one type; that those at Locality 4, one type of two varieties; and so forth. Insofar as there are no means available to test these statements they must remain speculations. Folk classes constitute interesting data, artifacts, when and if they can be recovered. They are to be explained; they are not an explanation. The aim of making analytic categories coincident with folk categories quite obviously will always result in the units being groups, since the categories themselves are phenomena. This particular goal evidenced in some archaeological studies is an excellent case in point with regard to the inappropriateness of sociocultural anthropology, from which the notion derives, as a model for prehistoric investigation.

Numerical Taxonomy

Numerical taxonomy has been proposed as a device for creating units at the scale of discrete objects; however, this is not yet widely practiced. Numerical taxonomy produces groups, and thus the units have the same characteristics as statistical clusters insofar as their utility in scientific endeavor is concerned. They are contingency-bound, undefined and undefinable, and restricted to the material from which they are derived. They cannot serve as the basis for comparison, nor can they incorporate new data without changing the structure of the units. Such groups bear the same relationship to the distribution of paradigmatic classes as do the statistical clusters in Figure 19. The advocacy of numerical taxonomy as a means of unit construction at the scale of discrete objects has followed the systematic exposition of this device in the biological sciences and incorporates the statistical sophistication characteristic of these disciplines.

Far more important than the proposed use of numerical

taxonomy at the scale of discrete objects is the widespread use, or at least advocacy, of numerical taxonomy to construct units for aggregates of discrete objects, the scale of phenomena herein called occupations. This use of numerical taxonomy long antedates the appearance of this device in the biological sciences and appears during the 1930's in a non-statistical form. In fact, it is almost the only device explicated in the prehistoric literature for unit construction at the scale of occupation, and this in spite of the fact that the units actually employed are, when identifiable, almost invariably paradigmatic classes.

The general approach is best stated in its early form by William McKern, one of the inventors of the device, in "The Midwestern Taxonomic Method as an Aid to Archaeological Culture Study," published in 1939. There it is proposed that aggregates of discrete objects, collections which are termed components, be compared with one another in terms of "traits" in order to assess the degree of similarity exhibited between collections. No formal coefficient of similarity or agreement is employed, but, rather, the expression of similarity takes the form of a list of linked (shared) traits and diagnostic (unshared) traits. The linked traits, of course, are the ones used to create the units while the diagnostic ones are to serve the purpose of identification. It is apparent even from the outset that grouping, in this case numerical taxonomy, and classification are undifferentiated in the system, the linked traits clearly belonging to a grouping device, while the diagnostic traits suggest that the groups are to be employed as classes. The lowest-level unit is the component which is considered empirical, that is, part of the phenomenological realm and the referent for the other units in the system. These components are successively grouped on the basis of similarity into foci, aspects, phases, patterns, and bases, with foci being the most similar units, the bases the least similar. It is further observed that styles are linked traits between foci and that as one goes to higher levels the linked traits progressively become first more technological and then more functional. This generalization, which amounts to saying that styles have smaller distributions than do technologies or functions, admits the possibility of viewing the Midwestern system as a series of classifications, each level being defined by different kinds of criteria. This impression is,

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however, most superficial. Higher-level units effectively group lower-level units; the components assigned to Focus 1 will not be split among two or three aspects but will belong to the same aspect. The only way in which this coordination of units at different levels may be achieved is the inclusion of all the criteria at the lowest level (focus) and reducing the number to

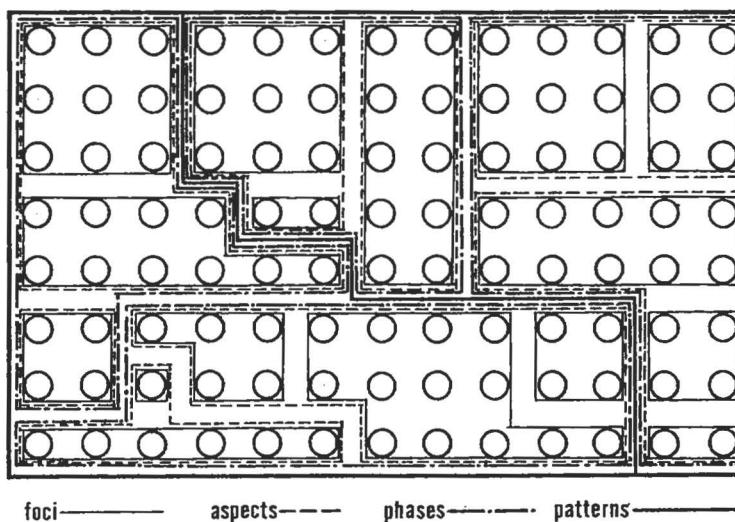


Figure 20. A schematic diagram showing the coordination of various levels within the Midwestern Taxonomic System. Components are represented by small circles and the various groupings by rectangular boxes. All of the boxes include other boxes and none of them intersect or cross-cut boxes at another level.

derive the next level, and so forth. Not only are styles linked traits at the level of focus, but so are all the other traits which are linked at higher levels. Thus the difference, for example, between foci and patterns is not in the kind of criteria, but in the *number* of criteria, which are held in common. This characteristic of coordination of various levels in the Midwestern Taxonomic System is illustrated in Figure 20, in which components are represented by small circles and the various groupings by rectangular boxes. All of the boxes include other boxes and none of them intersect or cross-cut boxes at another level. Figure 21 illustrates the Midwestern system employing a

Numerical Taxonomy

smaller number of components and showing the hierarchic relationships between the various levels of units. The similarity of this figure to the dendrogram in Chapter 4 is apparent, this latter construction being the general structure of numerical taxonomies.

The use of the "trait list," especially in subsequent studies employing the Midwestern Taxonomic System, to characterize all of the contents of components and which then serves as the basis for comparing components to state the similarity between

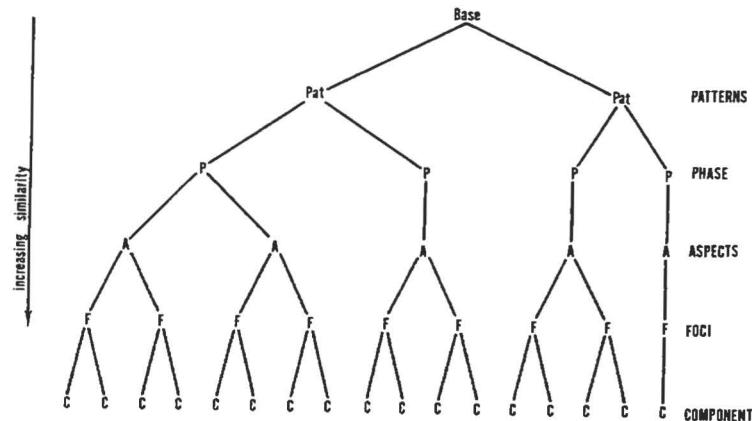


Figure 21. A schematic illustration of the hierarchic relationships within the Midwestern Taxonomic System.

them, presages the polythetic character of modern numerical taxonomy. Importantly, in the 1939 statement of the method McKern emphasizes the phenetic character of the units so formulated. They do not imply "relatedness" or distribution in time and space, but simple formal similarity. This is, of course, a function of choosing number of traits (similarity) over kind of traits as the means of constructing the units. Because there is no control over the kind of criteria used, the resulting units do not have any specifiable meaning. This is important to note, for Figure 21 could be viewed as a taxonomic classification rather than a numerical taxonomy if it were not clear that similarity, not identity, is the basis for construction. Further, as is clear not only from the early formulations of this system

but also in its subsequent use, the units consist not of sets of criteria (such is impossible since they can vary from case to case), but of groups of empirical entities, the components.

It has been advantageous to give the Midwestern Taxonomic System detailed and specific consideration because this method is the basis, at least technologically, of all the modern units constructed at the scale of occupation. In subsequent use the higher levels, from aspect upward to base, have gradually been abandoned—generally because chronology and developmental constructions were required, and, one may speculate, because there is no directly analogous unit in sociocultural anthropology above the focus which is equated in a general way with society or "culture." The only major change has been the replacement of the term "focus" with the term "phase" (see Willey and Phillips in bibliography). The notion of "settlement," introduced in recent years by K. C. Chang, the only unit at this scale which departs from the previous formulations, contains strong elements of the Midwestern scheme. Settlement employed as an empirical unit is almost analogous to component and when employed as a class or when the community concerned is not localized in space analogous to focus or phase.

Regardless of the particular terms used in the statement of the system and the number of units retained, there is one important and rather obvious inconsistency—the definitions of phase (focus) and component and the relationship between the two basic units. Components, it is insisted, are empirical units. Yet they are not. Component, regardless of the names used, is a manifestation of a phase or focus at a given locality. This is, of course, a possible way to state the relationship between a class and its *denotata* at a given point in time and space; however, this statement is not a definition by any standards, for if a component is a manifestation of a phase, then one must have phases before one can have components to be able to identify and bound them. On the other hand, phases are said to be groups of components. One must have the components before one can have the phases. This is an interesting circularity, and one which is entirely predictable. As has been pointed out herein in numerous contexts, the "definitions" of groups are always extensional and thus may always be reduced to a statement that "the group is because the group is." The simple fact of the matter is that if one follows the published method, one

cannot construct either phases or components; yet, of course, components and phases are constructed. The question thus becomes how.

At the root of the difficulties presented by component/phase lies the problem of identifying a phenomenological unit larger than discrete object. The solution offered in the literature, calling phases groups of components, is clearly rhetorical and nothing more. The phenomenological units cannot be components, for one cannot identify components without first having phases, and one is still left wondering what phases are units of. Something of a solution is presented in the Midwestern Taxonomic System itself. It was noted that the explication of the system overtly involves numerical taxonomy, but also implicitly, some kind of classification as well. The "traits" used in constructing the units of the system are categorized as linked when shared and as diagnostic when not shared. The diagnostic-linked categorization is exhaustive. A trait is either linked or diagnostic in a given context. There is, however, a third category of traits, a category which is clearly drawn from some system other than the explicated numerical taxonomy, namely determinants. Determinants constitute a set of traits which recur as a complex from component to component and which is distinctive of a focus. Clearly the determinants of a focus constitute a *post hoc* class *significatum*, something quite apart from the system as set forth as a kind of grouping and inconsistent with the system as a whole. Further, since there is no ranking or weighting of the determinant traits, it is reasonable to assume that the determinants of a focus constitute the *significatum* of a paradigmatic class. This is a primary reason that phase was defined as a paradigmatic class of occupations in the previous chapter.

What apparently is generally done by prehistorians, even though explicated in terms of a numerical taxonomy, is paradigmatic classification. This enables one to account for: (1) how it is possible to create phases when the published rationale is insufficient to create them; (2) why only the focus has been seriously retained from the Midwestern Taxonomic System; (3) how it is possible to identify new collections with previously established units; and (4) why determinants, inconsistent with the main theme of the Midwestern Taxonomic System, are nonetheless included in it. This eliminates the circularity of

the current treatment of component and phase. Component is used to designate the *denotata* of a class, the phase or focus, at a given locality. Because the *denotata* are real and because the actual units being classified lack discreteness, it has been easy to confuse the results of identification with the phenomena for

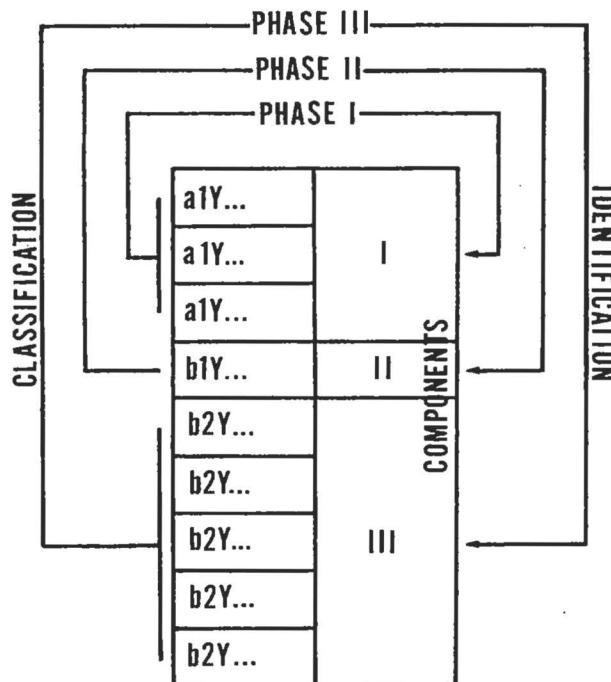


Figure 22. Pragmatic relationships between occupations, phases, and components.

which the classification has been constructed. The relationships between the notions of occupation, phase (focus), and component are illustrated schematically in Figure 22. In this diagram the two columns of boxes represent an ideal stratified site. Those labeled a₁y—b₂y are occupations, while those labeled I-III represent components. The phases are paradigmatic classes of occupations, the components the *denotata* of each phase. A locality may consist of several occupations all of

which belong to the same phase and thus the locality is a single-component site. Alternatively, there may be several occupations which belong to different phases, and thus several components will be recognized at the locality such as in Figure 22.

Viewing the Midwestern Taxonomic System as a numerical taxonomy employed as a rationale for paradigmatic classification eliminates most of the inconsistency in the literature about units at the scale of occupation. It does not, however, eliminate the difficulties inherent in the scale itself. While phases can be treated as paradigmatic classes, components as their *denotata* at a single location, there still is no general definition of what the phenomenological units are. Phases may be paradigmatic classes, but it is not possible to say, at least theoretically, what they are paradigmatic classes of. That such a glaring deficiency should be encountered is not surprising when one considers the dual role the term component has played—on the one hand as the phenomenological unit, and, on the other, the identified *denotata*. Pinpointing the circularity of the component/phase relationship is the crucial first step in correcting this conceptual deficiency.

Other kinds of arrangement, particularly keys, are used from time to time in prehistory. Their use has been rather straightforward, and there is little difficulty in recognizing keys. The only difficulty that inheres in their use is that ordinarily the classification for which the key has been made is not presented separately from the key so that the user is restricted to the classes of the key in his identifications. An excellent example of the key as used in prehistory is included in the appended reading list (see Schwartz, 1961).

Summary

Grouping devices both of the kind herein called statistical clustering and numerical taxonomy occur in the prehistoric literature, and, in fact, constitute some of the better theoretical exposition in the discipline. Both clustering and numerical taxonomy can be done with archaeological materials, but, in spite of lip-service to the contrary, neither has been widely employed in problem-solving for rather simple reasons. Their unit-products are groups, and groups cannot serve as the basis for either comparison or measurement. They are things to be com-

pared and measured. Further, lacking the feature of recurrence necessary for prediction and explanation, their future utility seems unlikely. In the literature, the major uses to which grouping devices have been put are to provide a rationale for paradigmatic classification (inappropriately) and to provide names for the units in "description." Given the inexplicit nature of much prehistoric literature, the identification of the device used to create a set of units is often difficult. In the case of grouping devices, their actual use seems restricted to "descriptive" studies. An ability to distinguish grouping from classification in this context is a moot point; any means of categorizing and naming will suffice, since these kinds of studies have no specifiable problem and thus are not testable. In those cases in which grouping is offered as a rationale for an underlying classification, the use to which the units are put will suffice to indicate the superficial nature of the grouping rationale.

In no fashion is the consideration here any rejection of the techniques of statistical clustering or, for that matter, numerical taxonomy, but only a rejection of their use as means of formulating units. They are exceedingly useful devices for the description of the characteristics of class *denotata* and their behavior in dimensions of variability. They cannot, however, provide useful analytic units for any science. Their appropriate role lies in the generation and testing of hypotheses about classes, not in the construction of the classes. The degree to which grouping can produce usable units is a direct function of the implicit classifications used (attributes and attribute combinations) by these devices. Treating the grouping techniques as the means of unit formation only further obscures the definition of the classes that they must employ.

8

SUMMARY

Today prehistory is rapidly becoming a science, a trend established nearly twenty years ago and one which has gained marked momentum in the last ten. In situations of rapid change there is a strong tendency for an old/new dichotomy to develop and such there is, at least to a moderate degree, in prehistory, represented by what has been called the "old archaeology" and the "new archaeology." The old archaeology, concerned primarily with objects and names for the objects, is giving way to explanatory methods and objectives of the new archaeology. The freedom permitted the workers of the old archaeology as an art is increasingly being constrained by the goals of the new, primarily by the scientific insistence that statements and constructs be testable. The greatest constraints and the greatest progress in reformulating prehistory as a science has been made in the realm of methods of explanation, much of which has been borrowed from and modified after similar procedures of the hard sciences.

The terminological confusion and conceptual imprecision of the old archaeology created few difficulties for the field as an art, for it really did not matter what the terms and concepts meant if they were not to be tested. With the insistence that statements and constructs be testable has come the requirement of knowing precisely what the units and statements mean, and the imprecision of the old archaeology has become a major and, in some respects, almost insurmountable liability. From the

point of view of the student of prehistory, the old archaeology could be acquired as an art—by intuitive assimilation. Today the discipline must be treated as a body of knowledge which can be learned. Yet the terminological morass presents serious barriers to such acquisition.

In establishing scientific goals and methods to achieve them, the new archaeology has adopted rather uncritically the units devised by the old archaeology. In many cases, the old units were not specifically designed for any specific purpose, let alone the newly conceived aims of the last two decades. Unfortunately, explanations can be no better than the units they employ and the data they attempt to explain, and the new archaeology has not seriously considered either of these. Indeed, the most serious criticisms of the new archaeology turn on its lack of formal sophistication.

In spite of the inadequacy of the old archaeology when measured by the criteria of science and in spite of the lack of any means of internal evaluation aside from a poll of majority opinion as to what is good or which prehistorian is good, explanations of limited scope and capable of independent evaluation have been made. The question is "how?" What makes the "good" prehistorian good? The old archaeology does not provide explicit answers.

It is in this milieu that we focused upon the construction of units in prehistory, units which have been largely devised by the old archaeology in an unsystematic fashion over a relatively long period of time. These units remain the formal foundation of both new and old archaeologies. Our considerations have been primarily of the old archaeology, which constitutes the bulk of the general literature and the literature on systematics, but with sympathy for the goals of the new. The purpose has been not so much to point out errors and inconsistencies (though this is an important aspect), but to isolate the good features, good, again, from a scientific perspective. Since the many problems faced by the student of prehistory, the confusing terminology, the inconsistencies of method, the lack of evaluative methods, and the lack of any kind of unity of discipline, are all interconnected, a single relatively simple solution was sought in setting forth a unitary system of unit construction including the assumptions on which it is founded and which satisfies the requirements of science. This system

was employed to organize and clarify the various units and terms employed by prehistory. Inevitably, much of that currently labeled archaeology was found inadequate as science and discarded from consideration. Much of the literature, however, displays a central theme amenable to interpretation as a scientifically useful system of formal theory. This is nowhere in its entirety spelled out in the archaeological literature. Parts are explicit here, others there, some of it nowhere explicit but implicit only in the operations of the discipline. That a large part of what is done is amenable to such interpretation is a commendation for prehistory. The central theme is essentially an answer to the question of how "good prehistory" and "good prehistorians" have been identified, and why "good prehistory" works. Too, application of the general model permits a unified set of terms, ones which mean the same thing every time they are used, to be employed, and provides a way to identify any unit, given sufficient information, regardless of the name given it in a particular study. A unified terminology was not possible in the old archaeology because of its substantive preoccupation. As long as the names and units were bound to specific studies and specific problems, there was no possibility of developing overt theory. The words for its construction were lacking. In an attempt to avoid this substantive tie and the traditional confusion between concepts and their referents, examples in concrete terms have been kept to a minimum. A unified terminology, measured itself against the goal of science for its utility, can prove of immense benefit in learning prehistory, how it works, its limitations both current and potential, and aid in the selection of units for specific kinds of problems. It is fair, I think, to say that the lack of progress of the old archaeology is not so much a function of the people practicing it or intrinsic failures of the conceptual tools, but of the inconsistent terminology effectively barring much communication and admitting much misunderstanding and error. The rather uncritical borrowing of units by the new archaeology for purposes never conceived when the units were created can be avoided. A general model, no matter how simplified it may be, offers the possibility of distinguishing the inadequacies of theory from the misuse, abuse, and misapplication of good theory. Rational evaluation is possible.

The general model for unit construction in prehistory and

the assumptions it is founded upon have been detailed in the several chapters of Part II in piecemeal fashion in an attempt both to show their rationale in the general scheme presented in Part I and to tie them generally to practice in prehistory. The focus upon the original formulations of classificatory concepts in the discipline rather than more modern renamed and elaborated versions points up the basic lack of change in prehistoric systematics. The remainder of this final chapter attempts to present in brief form this general model, the formal theory of prehistory.

Part I presents the basic notions used throughout, the core of which is the distinction between ideational and phenomenological realms and the correlative distinctions between definition and description, classes and groups, and classification and grouping. The import of the distinctions is two-fold: (1) the means of evaluating ideational constructs and phenomena differ—logical proof in the former case and probability or plausibility in the latter; and (2) the characteristics of ideational constructs and phenomenological units differ in respects that affect their utility in scientific endeavor. Ideational constructs are ahistorical and capable of intensional definition, whereas phenomenological units are contingency-bound and capable of extensional definition at best. The ideational and phenomenological realms are articulated in the framework of science, in that science employs ideational constructs to explain phenomena. The distinction between ideational and phenomenological is entirely analytic, in the nature of the logic of justification. If only because men necessarily use language, the two are fused in reality. The utility of such distinctions lies in their ability to clarify what has been done, for both evaluation and communication, but it is not a program of operational procedures.

Explanation is taken to mean prediction and control. Differences in value are capable of explanation but differences in kind (a function of unit construction) are not. The role of formal theory in science is to provide the means of organizing phenomena so that their explanation is possible. The prime requisite for such organizations is that the units permit recurrence and it is recurrence that enables one to link the known (observed fact) with the unknown (prediction).

Means for unit construction were examined in terms of

the ideational/phenomenological distinction which results in the identification of two fundamentally different devices for creating arrangements—classification, which produces ahistorical, intensionally-defined, ideational units, termed classes; and grouping, which produces contingency-bound, extensionally-defined sets of phenomena called groups. Classes are articulated with phenomena by means of identification, isolating at given points in time and space those phenomena which display the necessary and sufficient conditions for membership as stipulated by the intensional definition. Identified phenomena constitute special groups called the *denotata* of the class. The only groups, aggregates of phenomena, which are capable of explanation in a scientific sense are the *denotata* of classes. Identification is absent with grouping devices since the units and the phenomena are coterminous.

An additional dimension, that of ranking or mutual relationship of units within a system of arrangement, was added to the first distinctions, and two kinds of classification and two kinds of grouping so distinguished: unranked classes all at the same level produced by paradigmatic classification, and hierarchically ranked classes at several levels produced by taxonomic classification; unranked groups all at the same level produced by statistical clustering, and potentially hierarchically ranked groups at several levels produced by numerical taxonomy. Ranking is deemed relevant because it affects the parsimony of the various devices, hierarchic arrangements being the least parsimonious, but most elegant.

A comparison of the four possible means of arrangement with the requirements of unit construction for science identifies classificatory devices as appropriate to this general aim and grouping as inappropriate. Classification produces units which have a defined meaning and the possibility of recurrence, whereas grouping produces units which have no specifiable meaning beyond their existence and which are localized at a given point in time and space. Classification provides a means of measuring changes in value and a basis for comparison. Grouping is not reconcilable to measuring change, and cannot provide the framework for comparison. Within classification, the differences between paradigmatic classification and taxonomic classification affect their utility, or rather their roles in scientific endeavor. Paradigmatic classification, by reason of its

relative parsimony, can function as the main tool for unit construction when the purpose is heuristic. Taxonomic classification, because of its lack of parsimony, is useful only in didactic situations in which elegant presentation of already established paradigmatic classifications is required. The characteristic lack of parsimony of taxonomies vitiates their use unless the organization can be shown to be appropriate beforehand.

Another aspect of classification emphasized is the role of problem in its use and evaluation. To construct a classification requires a series of decisions or choices. The field or root of the classification (determined by the discipline), the scale at which features are to be distinguished (determined by the field), and the sets of features to be employed as criteria (determined by the problem) all must be selected and defined. The selection of a feature as definitive represents a hypothesis about the nature of the feature and its relevance for a specific problem. If the hypothesis is assumed to be correct, then the manner in which its use will organize phenomena is predictable. The utility of the hypothesis is tested by comparing the expected distribution with the actual distribution of *denotata*. A given classification either will organize a corpus so that the problem is soluble, or it will not. If alternative classifications are available, the criteria of elegance, parsimony, and sufficiency provide a means of selecting the most appropriate one. Without a specified problem, however, there is no means of justifying the selection of definitive features or evaluating the utility of the classification. Unfortunately, far too often classifications are not accompanied by explicit statements of problem, and this is currently the single greatest operational deficiency in prehistoric systematics.

Grouping, since it produces extensionally defined units, is not amenable to testing. Because groups are applicable as units only to the material from which they are derived, they cannot be tested against new data. Only the mechanics of grouping are testable, not the groups themselves. Groups are thus usually proclaimed as natural or non-arbitrary in lieu of some specified meaning, and the problem for which they serve is that of "description." It was noted that any set of words will suffice the purpose of "description," and thus the means by which the words are invented is irrelevant.

To construct a model of prehistory's formal theory utilizing

these notions requires that the discipline be defined as a science in such terms as to delineate empirically the subject matter and to specify the general character of its potential explanations. Prehistory is thus the science of artifacts conducted in terms of the concept culture. Artifact, the concept delineating the subject matter of prehistory, is understood to mean anything which owes any of its attributes to human activity, and culture, the concept controlling the nature of its explanations, is defined as shared ideas. Prehistory is the science which isolates human products and seeks to explain them in human terms. The specification of the kind of study (science), the subject matter (artifact), and the kind of explanation (culture) provides the basic elements for the statement of prehistory's formal theory.

The definitions of both artifact and culture are theoretical and thus do not evidence any particular problem or any particular body of data. They are intended to subsume all of the tactical definitions found in the literature, treating these definitions as special cases of the theoretical definitions and derived by restricting the range of application for a particular problem or body of data. Use of the definitions in the archaeological literature is impossible in the present context because they either evidence some specific content or embody non-essential inferences.

Once the discipline has been defined and the notions of artifact and culture theoretically defined, the development of a model of prehistory's formal operations is relatively straightforward. The aim of the inquiry can be specified. The role of formal theory must be that of creating cultural classifications for artifacts. Making use of the general considerations in Part I, the goal is limited to showing how prehistory converts classification in general into cultural classifications of artifacts. This entailed the identification of the kinds of classification employed and the manner in which they are used. A necessary adjunct is the identification of grouping devices frequently encountered assuming classificatory functions in "descriptive" and "cultural reconstruction" approaches.

The identification of the devices used (classification or grouping) and the forms they take (paradigmatic or taxonomic classification and statistical clustering or numerical taxonomy) is not an easy matter, since the means by which they have been created is rarely explicit, the units customarily taken for

granted, and, more often than not, unsystematically named. The account provided for the formal operations of prehistory is thus sufficient only to account for what is done. Its parsimony cannot be assessed directly from the literary sources. The examination suggests that only two kinds of classification are employed: paradigmatic classification, which is by far the most common, and a special form of taxonomic classification which makes use of the dimensional features of paradigmatic classification. True taxonomy is not important; the few cases which are conformable to interpretation as true taxonomy are probably poorly explicated examples of the special-case taxonomy. Both statistical clustering and numerical taxonomy are restricted to the "descriptive" approach with the exception of the general use of numerical taxonomy to provide a rationale for the implicit paradigmatic procedures which lead to the construction of phases. Paradigmatic classes are commonly formed for three scales of phenomena: (1) attributes of discrete objects, with the classes termed "modes"; (2) discrete objects, with the classes termed "types"; and (3) occupations (aggregates of discrete objects), with the classes termed "phases." The special-case taxonomies link alternative paradigmatic classifications which differ from each other in level at all three scales. The number of paradigmatic classifications at any scale is infinitely large, not only by varying the level, but also by changing the criteria used—common means of creating classes to serve specific problems (e.g., functional types, historical types at the scale of discrete object; phases, foci, traditions, horizons, stages, and so forth, at the scale of occupation).

Prehistory has traditionally conceived phenomena in such a manner as to be amenable to scientific explanation. Prehistory further imposes the requirement that the units be cultural. The identities represented by classes and the "behavior" of these classes with respect to other classes and in other dimensions (time/space) must be viewed as the products of ideas held in common by the men who made, used, and deposited the artifacts concerned. How the cultural requirement is met can be treated separately.

Artifacts are identified by the criterion of human involvement and their identification serves to isolate those phenomena amenable to the interests of prehistory. The ease with which artifacts may be identified varies with scale and circumstance;

a certain number of human products may be excluded because of an inability to reasonably assume their artificial origin. This loss of data is a necessary sacrifice to accuracy. It is essential for the purposes of the discipline that no natural objects be included by its systematics, but it is not equally important that all artificial objects are included. The creation of categories of artifacts requires the stipulation of scale and the three scales enumerated above are nearly universal though not exhaustive in prehistory. Implicit in classification at the lowest scale is a fourth and still lower scale, attributes of attributes of discrete objects. Higher scales make use of classes formed at lower scales in analysis.

Using the concept artifact to segregate phenomena for which cultural classes can be constructed does not mean *ipso facto* that classes of artifacts are cultural. While this possibility is not generally recognized, noncultural classes are in practice generally avoided intuitively. To insure structurally that artifact classes are cultural, additional operations are involved.

First, the potential features themselves must be identified as products of human activity, a parallel procedure to the identification of artifacts and making use of a dichotomous index (artificial attribute/natural attribute). As with artifact, the identification of artificial attributes varies with circumstance, and a certain number of artificial attributes will be lost, since conditions may not reasonably permit the assumption of human involvement. Restricting the source of criteria to artificial attributes insures that artifacts will be treated as human products, eliminating the possibility of natural classes of artifacts.

Secondly, prehistory makes an assumption to convert classes defined on the basis of artificial attributes into cultural classes. This single, simple assumption is the pivotal operation in formal theory, the one upon which the discipline is based—creating features, usually modes, from artificial attributes and completing the articulation of the notion of artifact and that of culture. It is assumed that if a set of objects share the same feature, and that if that feature is artificial, then the objects share that feature because the people responsible shared the same idea. A simple equation is made between recurrent feature and shared idea. As was argued in Part II, this is the only plausible account for shared artificial features. While never explicitly stated in the literature, this is the most universal

operation in prehistory, the one which provides such coherence as the discipline has. A certain amount of confusion is possible, and does indeed occur, with regard to this equation: a concern with "intent," contact, and genetic relationships (analogues/homologs). These *post hoc* queries are irrelevant so far as formal theory is concerned. The equation stipulates only formal sharing, that the artifacts concerned may be treated (for a specified purpose) as products of the same mental template. The equation does not say how or why the sharing takes place. This latter concern is entirely inferential; it is an explanation of the distribution of class *denotata*, and it plays no role in their definition.

The testability of classifications is a requirement of science, but a requirement neglected by prehistory even though many of its classifications are testable. The terminological confusion, the vagueness of labeling, and the lack of explicit problems combine to deprive prehistoric classification of much of its potential. Perhaps it is too much to ask for testing and explicit statement of problem when all the preceding operations are implicit, haphazard, and *ad hoc* constructions in common practice. Nonetheless, when the operations are explicit the investigator is forced to make explicit statements of problem and thus creates potentially testable classifications. To construct classes, specific dimensions of features, again usually modes, must be selected as criteria, other dimensions being discarded from a definitive role in the context of a given classification. This decision, or set of decisions, must be justified on the basis of the relevance of the set of dimensions chosen to the problem for which the classes are being constructed. The justification should always have the form of an hypothesis about the nature and relevance of the dimension of modes to the problem. The testing of these devices follows the basic pattern for hypothesis evaluation: the decision, once made, either will create classes which have the necessary distribution characteristics or it will not. An affirmative result (e.g., a particular set of classes will function in seriation) shows that the classes are sufficient. Comparison with alternative classifications makes it possible to ascertain if the classification is the best available.

An important consequence, not as widely recognized and practiced as could be hoped for, is that there are—indeed there must be—as many classifications as there are problems and

methods for their solution. Different classifications are created by changing levels (e.g., "ware-type-variety") and by changing criteria (e.g., functional types, ethnological types, historical types). Monolithic "right" classifications have no place in prehistory or any other science which lacks a singularity of inquiry. A single rock will have many names, a single occupation belong to many phases. Our common sense desire for a perfect class/object equation has no utility for phenomena beyond common experience and common interest.

Figure 23 presents in summary fashion the basic elements of formal theory as employed in prehistory. The diagram illustrates only the most usual program, that is, one which begins with modes as the analytic step for the classification of discrete objects. The figure represents the path taken to convert classification into cultural classification of artifacts. The end-product classes, (mode, type, and phase) denote the scale of phenomena for which they are constructed, their paradigmatic nature, and the cultural quality of their defining criteria. These are the only characteristics which can be included for the discipline as a whole in a problem-free context. The number of defining criteria and the particular criteria chosen are a function of the requirements of particular kinds of problems, and variations in these aspects produce the large number of modes, types, and phases, sometimes recognized under special labels such as tradition, horizon, ware, or functional type, and sometimes terminologically undifferentiated. However, prehistory's formal theory, in spite of the presentation afforded it in the literature, is rather simple—there being but three kinds of units employed and their formulation as cultural units of scientific utility being founded upon a small set of simple discriminations and a single, important, but simple assumption.

This view is, of course, not the only one possible. Given the terminological difficulties of prehistory's literature, many accounts are undoubtedly possible. The account presented does offer the advantages of consistency coupled with utility for the purpose of explanation. Also, this is an account of what has been done, not what might be done or could be done. Simply examining Figure 23 suggests that redundancy is involved in the repetition of the artificial/natural discrimination for both artifacts and attributes. This, however, appears to be the way it is done. The prehistorian recovers artifacts and then dis-

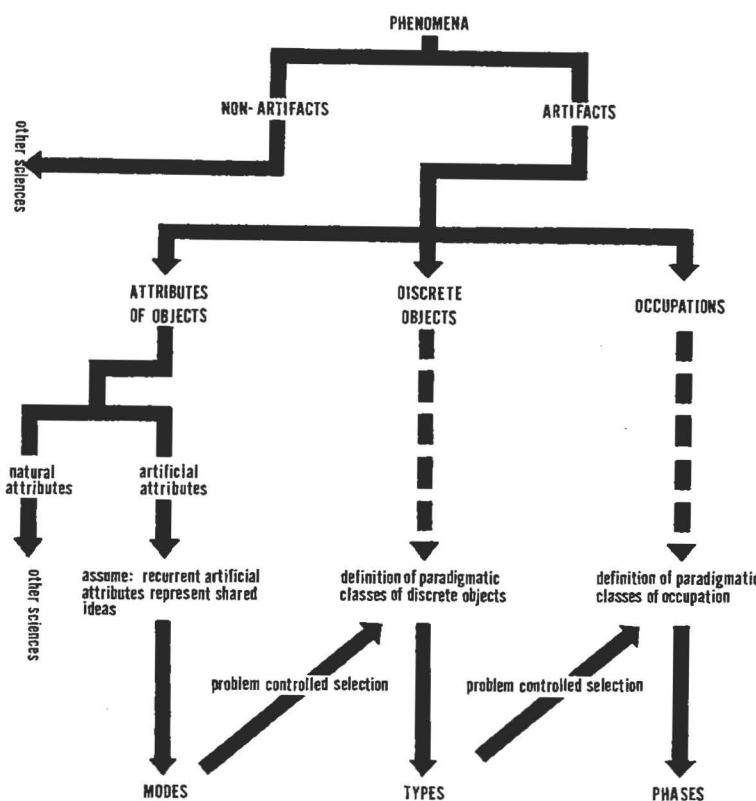


Figure 23. Elemental formal operations in prehistory.

criminated their attributes rather than recovering artificial attributes and then noting the objects in which they occur.

Larger Issues

Lest the approach taken be misunderstood, it should be clear that the qualitative program adhered to throughout is not a rejection of statistical-oriented approaches. It rather tries to show why grouping and counts are inappropriate to unit construction. The particular view espoused here is perfectly consistent with the goals and most of the methods of the new

archaeology, save those attempts to employ groups in the stead of classes. This latter procedure I have tried to show is both counter-productive and deceptive—counter-productive because it does not convey meaning precisely and is not capable of expansion; deceptive because, while it cannot predict, it does provide lucid and elegant accounts of what one already knows.

Statistical techniques have their appropriate role in the manipulation of data, not in its creation. These techniques are necessary to construct distributions and compare and correlate data in their variable aspects, and they will be all the more powerful if they constitute tests of hypotheses embedded in the systematics rather than a shuffling of data. This is possible only when units are properly and explicitly formulated, and it is in this that the "old archaeology" can make an important contribution to the new.

The abuse and inconsistency in the use of classification and its consequent failure in many circumstances certainly points to the need for improvement; however, improvement is not forthcoming from grouping devices. Bad practice has been confused with bad theory and method. The method—classification—is sound, indeed the only device possible, but its practice in prehistory has thwarted its potential. Certainly there are further ramifications. To a formalist, much of the concern with elaborate statistical devices appears as an attempt to correct with a calculus arithmetic errors which have been obscured by multitudinous manipulations. Many of the problems for which factor analysis is proposed, for example, would simply not exist if the formal units had been precisely constructed in the first place with a full knowledge of the wide range of units that can be and in fact have been employed. There may well be a role for factor analysis but it does not appear to be the imperfect correction of errors in systematics; if it has a role to play, this can be reasonably determined only after a reasonable systematic is in use. The qualitative and quantitative are not alternatives; they are necessary complements. The manipulation of the phenomenological world must always be, overtly or covertly, statistical in at least some minimal sense. What correlations mean, not their utility, is questioned. Statistical manipulation is not a means of discovery (this always has been and always will be "guessing"), but a means of testing the efficacy of qualitatively derived units. Regarding the units as givens rather than hy-

potheses is the principal sin of the old archaeology; currently, disregarding the role of qualitative operations is the sin of the new.

The old archaeology and the new archaeology are not competing approaches, for each has a distinctive and dependent relationship with the other. The old archaeology, basically qualitative, provides the units which the new archaeology can use for explanation. Both are necessary components of a science. The terminological and conceptual ambiguity of the old archaeology resulting in both misapplication of classification and lack of explanation is a function of the state of development of the discipline. There is no model for a science of man, though there are models for parts of it such as have been used here. Errors have been made, perhaps the principal one being the use of sociocultural anthropology as a model. The new archaeology has corrected some of these errors. It has provided the stimulation to correct others. It has incorporated others. The clarification of the old archaeology is a requirement for the success of the new.

DEFINITIONS

1 Preliminary Notions

- concept: an intensionally defined term specific to an academic discipline
- definition: the necessary and sufficient conditions for membership in a unit
- description: a compilation of the variable attributes of an individual case or set of cases
- extensional definition: the necessary and sufficient conditions for membership in a unit rendered by enumeration of the members or a statistical summary of same
- generalization: a statistical summary of the attributes of a given set of phenomena
- hypothesis: a proposed explanation for a specific set of things or events
- ideational: anything which does not have objective existence
- intensional definition: the necessary and sufficient conditions for membership rendered as a set of distinctive features which an object or event must display to be a member
- method: a sub-system of theory which is directed toward the solution of a particular class of problem
- phenomenological: anything which has an objective existence
- principle (law): a theoretical statement of a relationship held to obtain between two or more classes
- science: a systematic study deriving from a logical system which results in the ordering of phenomena to which it is applied

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in such a manner as to make them ahistorical and capable of explanation (obviously defined to emphasize the role of formal theory)
systematics: the procedures for the creation of sets of units derived from a logical system for a specified purpose
technique: the application of a particular method to a given set of phenomena
theory: a system of units (classes) and relationships (laws) between units that provides the basis for the explanation of phenomena

2 Classification

analysis (analytic step): the discrimination of attributes within a stipulated field and the selection of criteria from such attributes
arbitrary: not inherent in nature as a sole solution
arrangement: any activity which produces ordered sets of units
attribute: the smallest qualitatively distinct unit discriminated for a field of phenomena in a given investigation
class: an intensionally defined unit of meaning
classification: the creation of units of meaning by means of stipulating redundancies
denotatum: any actual instance (thing or event) assigned to a specific class; the means of indicating that an object has been designated as a member of a class
grouping: the creation of units of phenomena
identification: the process of sorting phenomena in terms of class *significata* with the purpose of assigning them to specific classes
significatum: the necessary and sufficient conditions for membership in a class, an intensional definition of a class

3 Kinds of Classification

dimension: a set of mutually exclusive alternative features
index: a unidimensional classification herein treated as a special-case paradigm
paradigmatic classification: dimensional classification in which classes are formed by intersection

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root: the field of paradigmatic classification expressed as a feature common to all classes in such a classification
taxonomic classification: non-dimensional classification in which classes are defined by means of intersection

4 Non-Classificatory Arrangement

coefficient of similarity: a numerical expression of the number of features in which two objects or events agree (no scale implied: object may be attribute, discrete object, and so on)
group: an aggregate of actual objects or events, either physically or conceptually associated as a unit (no scale implied)
grouping device: any method for the delineation of units which makes use of quantitative characteristics of a particular set of phenomena to produce units with the characteristics of groups
identification devices: any formal structure designed to assign events or objects to previously defined classes
numerical taxonomy: a grouping device which utilizes similarity of constituent pairs to delimit units
similarity: a quantitative assessment of the number of features shared by two or more objects or events (no scale implied)
statistical clustering: methods of grouping which employ the frequency of association to delimit units

5 Prehistory

artifact: *anything* which exhibits any physical attributes that can be assumed to be the results of human activity
culture: a concept referring to shared ideas used as an explanatory device
prehistory: the science of artifacts and relations between artifacts conducted in terms of culture

6 Classification in Prehistory

data: phenomena categorized for use by a specific science
horizon: a cultural class which displays an extensive distribution

- in space and a restricted distribution in time (*horizon-style* is applied when the classes are at the scale of attribute)
level: a set of units (classes) which display the same or comparable degree of inclusiveness or rank
mode: (analytic) an intuitive cultural class of attributes of discrete objects; (synthetic) a cultural paradigmatic class of attributes of discrete objects
occupation: a spatial cluster of discrete objects which can reasonably be assumed to be the product of a single group of people at a particular locality and deposited there over a period of continuous residence, comparable to other such units in the same study
phase: (synthetic) a paradigmatic class of occupations defined by types and/or modes
scale: a set of objects (group) which display the same degree of inclusiveness or rank
tradition: a cultural class which displays an extensive distribution in time and a limited distribution in space
type: (analytic) an intuitive cultural class of discrete objects; (synthetic) a paradigmatic class of discrete objects defined by modes

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