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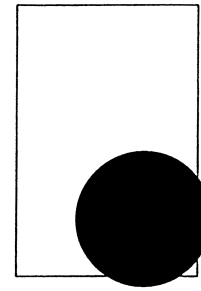

The epistemological nature of archaeological units

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Abstract

Understanding the epistemological nature of archaeological units, or types, is critical to archaeological research. Two aspects of units underpin the issue. First, ideational units must not be conflated with empirical units. Ideational units are units of measurement; empirical units are the things being measured. Ideational units can be either theoretical or descriptional. Theoretical units, as their name implies, are derived from a theoretical basis, and their utility for a specific analytical purpose must be tested. Descriptional units are not derived from theory and are not attached to an analytical purpose, although they may serve the purpose of communication. Second, theoretical units must be defined intensionally, through explicit listing of their diagnostic attributes. Most extant archaeological units have been extensionally derived from the specimens included in a unit, and often comprise descriptive as well as definitive attributes, without distinction between the two. The conflation of theoretical and empirical units remains a serious problem in archaeology.

Key Words

empirical units • extensional definition • historical types • intensional definition • theoretical units

INTRODUCTION

Artifact categorization is a routine exercise carried out by an archaeologist. The number and kinds of categorization systems that have been used in archaeology are numerous, as are the individual categories, or units, contained in them. Within this overwhelming array, in the Americanist literature of the 20th century, the categories themselves fall into several general kinds, the descriptors of which tell us something about how the individual units were used analytically. For example, Julian Steward (1954) distinguished among 'morphological,' 'historical-index,' and 'functional' types. The first comprised units that were 'elementary' and 'descriptive'; the second had 'chronological, not cultural, significance', and were 'time-markers' that were 'used to distinguish chronological

and [spatial] differences'; and the third were 'based on cultural use or role' (Steward, 1954: 54–5).

Archaeologists have long placed archaeological phenomena in ethnological categories, in an effort to monitor and explain change in how human groups arrange themselves socially and politically. Thus archaeological categorization is not solely in terms of empirical phenomena. Projectile points, pots, structures, and the like are empirical, but chiefdoms, states, and the like are not. The people who together make up a social unit are empirical, but the social unit itself is merely a category. Failure to keep separate empirical and nonempirical units has crippled archaeology and anthropology (O'Brien and Lyman, 2000), especially with respect to understanding how and why cultural phenomena change. This is not meant to suggest that nonempirical units are not useful; otherwise, for example, our system of keeping track of time – seconds, minutes, hours – is without merit. Different kinds of units are involved in categorization, but the division is not made along the line of empirical–nonempirical. Rather, it is made at a deeper, epistemological level – one that goes to the question of reality. Archaeologists and anthropologists are not the only ones who face epistemological problems over reality and units; biologists have for decades wrestled with the problem, relative to what a species is, with minimal success (O'Brien and Lyman, 2000).

Regardless of the kinds of categories that exist and of the immediate purposes to which units are put, scientists categorize things for one reason: to reduce variation. The human mind can cope with limited variation. Modern computers are of no help because unless they are told how to partition the data, all they *can* do is arrange it. In our view, archaeology is a science and thus must be theory driven. The linkage between theory and explanation – the ultimate goal of science – requires a logical means of creating units for a specified purpose; the units used to partition variation are derived from the theory one brings to the analysis (O'Brien and Lyman, 2000). First, an appropriate kind of unit must be selected. Second, a system for constructing the units must be selected. Logic dictates which system and units to use, but selection is conditioned by the understanding one has both of systematics and of the attendant epistemology. Here we highlight some important epistemological issues related to the creation and use of units. Although we focus specifically on Americanist archaeology, our discussion should be of interest to anthropologists and Old World archaeologists. The epistemological issues that underlie the creation and use of units transcend time, space, and specific categories of phenomena.

CATEGORIZATION IN AMERICANIST ARCHAEOLOGY

Prior to 1915, most Americanist archaeologists were searching for differences in sets of culture traits of a magnitude that suggested major qualitative differences in cultures. These cultures could then be construed as occupying different temporal positions, in such a manner as to align with a progressive evolutionary model of cultural development, like the one proposed by Lewis Henry Morgan (1877). The cultural differences found in the American archaeological record should be like those then being reported in Europe; anything of less magnitude was insignificant, but not invalid nor improbable (Kroeber, 1909). To produce implications that could be tested empirically, prehistorians had to alter how they measured culture change. The previously held view of minimal cultural development would shift between 1914 and 1916 when, through the efforts of

A.V. Kidder (1916, 1917; Kidder and Kidder, 1917), A.L. Kroeber (1916), Nels Nelson (1916), and Leslie Spier (1917) in the American Southwest, archaeologists modified their scale of observation, from that of the presence/absence of cultural traits to that of the frequencies of trait variants (Lyman and O'Brien, 1999, 2000; Lyman et al., 1997; O'Brien and Lyman, 1999a, 2000).

These efforts were grounded in the *direct historical approach* (Steward, 1942; Wedel, 1938). To anchor a relative chronology of culture traits, one began with the traits that dated to the historical period and then worked backward in time, focusing on traits that occurred in multiple assemblages (Lyman and O'Brien, 2000). This approach was used with great success by William Duncan Strong (1935, 1940), Waldo Wedel (1938, 1940), and researchers connected with the Bureau of American Ethnology and the US National Museum (O'Brien and Lyman, 1999b). Kidder, Kroeber, Nelson, and Spier not only inspected the presence/absence of cultural traits and their overlapping occurrences, but focused on the relative frequencies of trait variants – pottery-type units. This allowed them to measure time as a continuous variable, an entirely new way to perceive the passage of time in the archaeological record. It was, however, not wholly new in anthropology. Kroeber (1909: 5) had characterized fluctuations in frequencies of trait variants as 'passing changes of fashion'. Clark Wissler (1916: 195–6) described these as 'stylistic pulsations', but it was Nelson (1916: 167) who formalized this notion, when he wrote that 'normal frequency curves [of pottery styles] came slowly into vogue, attained a maximum and began a gradual decline'. This axiom – the 'popularity principle' (Lyman et al., 1997: 43) – was simply a common-sense explanation for perceived phenomena, but it came to serve as a central tenet of culture history.

Stratigraphic excavation was initially viewed as a confirmational strategy for testing the chronological significance of seriations, but by the 1920s it had become a discovery strategy. Sets of artifacts in archaeological strata were equated with individual cultures. The notion of time as a continuous dimension, evident in the early work of Kroeber, Spier, Kidder, and Nelson, was subverted into one where time was viewed as a series of bounded, discontinuous chunks (Lyman and O'Brien, 1999). These chunks were referred to variously as periods or cultures. Historical-index types could be used to measure the passage of time, but archaeologists also assumed, with no theoretical basis for so doing, that they could be correlated with ethnographic units (Lyman et al., 1997; O'Brien et al., 2000). Unclear at the time was that these units were products of two opposite ontologies (Lyman et al., 1997).

It stood to reason – an intuitive warrant in the absence of theory – that artifacts, being the products of human manufacture, must have been made according to intentional plans. Archaeologists such as James Ford (1935, 1936) and Irving Rouse (1939) suggested that artifacts probably reflected ideas in the heads of the artifact makers, although neither individual pursued this possibility in any rigorous analytical fashion. The result of this suggestion, however, was confusion over whether types were *analytical* units or *real* units, as is apparent in Alex Krieger's (1944) paper on typology. Krieger's major contribution was to point out that a good type must pass the historical-significance test and display a continuous distribution in time and space. Such temporal–spatial contiguity denoted the flow of ideas – a common-sense warrant for the distribution, but one fully in line with the way in which culture history was viewed by Americanist archaeologists trained as anthropologists.

Cultures were evident in the ethnological record, and their traits displayed unique and continuous distributions that could be accounted for with the culture-area concept and the age-area concept (e.g. Kroeber, 1931). Artifacts were products of cultures, so it was assumed that archaeological phenomena could be accounted for in similar terms. Wissler (1919) and Nelson (1919) showed that the culture-area and age-area notions were, as ethnological concepts, fully applicable to and even confirmed by the archaeological record. The critical issues were (a) how artifacts were to be sorted and studied, so that such explanatory tools could be called on, and (b) how cultures as larger-scale units were to be recognized and/or constructed (Lyman et al., 1997).

Given the assumptions that cultures are real, as opposed to units created by ethnologists, and that archaeological cultures can be recognized in the material record, artifact types became dual-purpose units. First, they were constructed to measure the passage of time. If collections were seriated or the frequencies of types plotted against vertical-recovery provenience and they displayed normal frequency distributions, then they must reflect the waxing and waning of a trait variant's popularity through time – and thus they could serve as a measure of time. As a corollary, if types in two different time and/or space positions were similar, then they must represent similarity of the ideas that underlay them as well. Thus, types could serve as measures of such things as diffusion or social interaction. Second, types served as cultural signatures. If a type's member specimens were perceived as being homogeneous and distributed over limited time and space, specimens of the type became index fossils that signified a particular culture.

This second purpose was one for which most types were ill suited, largely because their ethnological implications were untestable. Nevertheless, the final effort to refine classification systems within the confines of culture history was directed specifically toward perfecting the second purpose. This effort took the form of designating varieties of types. A variety was meant to have a more restricted distribution in time and/or space than the type of which it was a part (Wheat et al., 1958), and hence it could serve as a measure of those two dimensions at finer scales. It was also thought to serve as a finer-scale marker of individual cultures (e.g. Gifford, 1960; Phillips, 1958; Sabloff and Smith, 1969; Smith et al., 1960). The problems with such an approach were outlined by Robert Dunnell (1971), and echoed earlier remarks by Rouse (1939, 1960). Varieties, like types, were originally constructed as analytical tools for measuring time and space. They *might* have socio-cultural meaning, but such was not testable in a non-tautological manner. The failure to make clear how such units were constructed and to enumerate their definitive criteria exacerbated the conflation of units and interpretations of them.

In many respects, the situation in Americanist archaeology is not much different today. Why might this be the case? To answer this, the appropriate place to begin is with the most fundamental dichotomy, that between ideational and empirical units. All other discussion hinges on the ability to separate those two kinds of units.

IDEATIONAL AND EMPIRICAL UNITS

Theory, because it forms our ideas on cause, *always* has to be the final arbiter of which units are applicable for which kinds of analytical jobs. Not only does theory specify appropriate *kinds* of units, it also specifies *how* the units are to be used. If we were interested in functional variation in ceramic tableware, we would select as our analytical

dimensions rim form, lip angle, kind and location of use-wear marks, and other dimensions that theoretically are causally related to the property of interest. Vessel color is not selected for study. We are not saying color is unrelated to function; rather, our theory of function precludes color from consideration.

Dimensions and the attribute states under each dimension are *ideational* units; they are not real in the sense that they can be seen or picked up and held (Dunnell, 1986; O'Brien and Lyman, 2000). A bowl is an *empirical*, or *phenomenological*, unit – we can see and feel it – and it has properties that can be measured using ideational units. Ideational units are tools used to measure or characterize empirical things. A lip angle on a bowl is measured, using a goniometer, in ideational units known as degrees. Centimeters and grams, just as with degrees, do not exist in any empirical sense, but they *do* exist in an ideational sense. As such, they are useful for measuring properties *of* empirical units. Lip angle is an ideational unit with different empirical *manifestations.* Thus, a 60-degree lip angle on a specific bowl is an empirical unit. Ideational units are not interchangeable – centimeters, for example, measure length and not weight – but experience rarely allows for confusion here.

Despite efforts to keep them straight, empirical units often become confused with ideational units. Philip Phillips, James A. Ford, and James B. Griffin (1951) recognized this problem when they noted that language required that we think and talk about pottery types such as 'Baytown Plain' as if they were real things. They cautioned that what was truly meant was that a particular sherd (pottery fragment) was classified as what '*for the time being* we have elected to call Baytown Plain' (Phillips et al., 1951: 66). The sherd is Baytown Plain *only* because it resides in a category we created and decided to call Baytown Plain. There is no essential property of a sherd that makes it Baytown Plain; it simply has the characteristics specified for that unit. Tomorrow we might reclassify the sherd as something else.

Two kinds of ideational units

Ideational units can be *descriptional*, used merely to characterize a thing, or they can be *theoretical*, created for specific analytical purposes. We could choose to characterize something on the basis of the dimension color and to specify several different attribute states of color, thereby constructing descriptive units. Color need have nothing to do with the particular focus of analysis, but it does describe empirical units, and hence it qualifies as an ideational unit. Alternatively, in light of a theoretical (causal) relation between, say, function and various lip angles of ceramic bowls, edge-angle units such as 1–30 degrees, 31–60 degrees, and 61–90 degrees would be theoretical units. A theoretical unit is an ideational unit that has explanatory significance *because of and only because of its theoretical relevance.*

Pottery types that culture historians created, while typically including a host of descriptive information that was ancillary to the main analytical focus – keeping track of time – were also theoretical in that they were formed for a specific purpose, their utility was tested, and then they were used *for* that purpose. Failure to indicate which attributes were definitive of a type and which were merely descriptive resulted in problems. Was a newly discovered specimen a member of Type A, or did it simply look like Type A? Why did a type sometimes provide a good measure of time's passage but other times not? With respect to the latter question, we suspect that it was often because

specimens were included in the type on the basis of descriptive, rather than definitive, criteria. Archaeologists got themselves into this predicament largely as a result of how they constructed types, despite the fact that those types were usually ideational units.

Extensional versus intensional derivation of units

Ideational units can be derived either intensionally or extensionally. An *intensional* definition comprises the necessary and sufficient conditions for membership in a unit; it explicitly lists the distinctive attributes that a phenomenon must display to be identified as a member of the unit. The definitive attributes of the unit are derived from theory; there is no necessary reference to real, empirical specimens when the unit is constructed. The three classes of lip angle mentioned earlier, 1–30 degrees, 31–60 degrees, and 61–90 degrees, derive from a theory of vessel function that indicates some lip angles are necessary for some functions, whereas other lip angles are necessary for other functions. We may never find specimens with lip angles of 31–60 degrees, but this is something to determine analytically. The fact that something might *not* exist has no bearing on unit construction.

An *extensional* unit is derived by enumerating selected attributes shared by the unit's members; the criteria comprising the unit are based on observed attributes of the actual members placed in a unit. The characteristics of extensionally derived units are not theoretically informed in an explicit manner. Most units that have been traditionally employed in archaeology are extensionally derived; they were formed as the result of an analyst subdividing a pile of artifacts into smaller piles, based on perceived similarities and differences among the objects. The same applies to sociopolitical units: examine an array of human groups and sort them into piles, based on perceived differences and similarities in such things as how descent is reckoned, how one refers to his or her mother's brother, where a newly married couple resides, and so on.

Extensionally derived units are dependent on the specimens examined, resulting in at least two problems. First, the distinction between definitive attributes and descriptive attributes does not necessarily follow from the derivation. We suspect most archaeologists working in North America would claim to 'know' a Clovis point when they saw one, but we have yet to find a written statement that explicitly specifies this unit's definitive criteria. Second, the fact that a limited set of phenomena can be sorted into more or less distinct piles of similar specimens, and that a definition or description of an average specimen in each pile can be derived, reinforces the notion that the piles are somehow real rather than analytical products. As a result, we read much about 'the question of whether Clovis is one entity or several' (West, 1996: 553). If the unit we term 'Clovis point' were an analytical construct, there would be no such question (O'Brien et al., 2001). That the question arises underscores the fact that, although the unit serves well as a time marker, it is perceived not only as descriptive but also in some (anthropological) sense real.

GROUPS AND CLASSES

In discussing the differences between theoretical and empirical units, Dunnell stated that this was 'the basis of the distinction that I (Dunnell, 1971) earlier characterized clumsily as the contrast between groups and classes, the former designating empirical entities and the latter encompassing the "theoretical" units' (Dunnell, 1986: 151). Dunnell took two

terms that have enjoyed common usage in archaeology, and attempted to show that each has a distinct meaning. Dunnell's later distinction between ideational units and empirical units heightened the earlier distinction. Classes are ideational units and *only* ideational units. As such, they can be either theoretical units or descriptive units. As ideational units, classes can be defined intensionally or extensionally. Similarly, groups are empirical units and *only* empirical units. They also can be constructed either extensionally or intensionally.

Groups as empirical units

Empirical things can occur singly, whereby the thing in question is unique in terms of particular properties, or they can occur in groups, where multiple things all share particular properties. Groups are collections of empirical things that are conceptually associated as a unit. The properties that things share might be of analytical interest, but there is no stipulation that they must be. Because they contain specimens, groups, like the specimens themselves, are empirical, or phenomenological, units. Groups are produced through any number of methods, from simple visual inspection to complex algorithms (O'Brien and Lyman, 2000). Such groups are extensionally derived from the pile of artifacts one actually examines, and hence they are idiosyncratic.

We lump grouping methods under the generic term 'clustering approaches'. The objective of a clustering exercise is to produce groups – clusters – of things, each of which is more like the other things in that group than it is to things in other groups. To produce the clusters, things are taken one pair at a time and scored in terms of their similarity to each other. In archaeology these 'things' can be discrete objects, traits of objects, or sets of objects. Similarity coefficients are calculated in like manner for all pairs of things, and the coefficients are linked in descending order of similarity, producing the familiar dendrogram pattern of linkage. Clusters are identified either by visual inspection or by the use of conventional threshold values. This type of approach to clustering is termed numerical taxonomy, or phenetics (Mayr, 1981). It developed in modern form in biology (Sokal and Sneath, 1963), as an all-purpose method of constructing groups using a large number of morphological traits.

Statistically aided clustering approaches have been popular in recent archaeology (e.g. Cowgill, 1982; Doran and Hodson, 1975). Harold Driver and A.L. Kroeber (1932) may have been the first to argue for a more intuitive clustering approach in archaeological analysis, but it reached its fullest expression in the Midwestern Taxonomic Method (McKern, 1939). The purpose of that method was to produce a hierarchical set of nested units that simultaneously showed similarity and dissimilarity among archaeological units. It ignored time and space, concentrating solely on the presence or absence of extensionally derived traits to create the groups. In its pure form the method saw little application, because archaeologists found it nearly impossible to ignore time and space (O'Brien and Lyman, 2001). In altered form it was used widely, evolving into the phase-oriented systematics of Gordon R. Willey and Philip Phillips (1958; Phillips and Willey, 1953), that continues to underpin much Americanist archaeology. Many of the phase units created in the 1950s exist in essentially unaltered form today (Fox, 1998; O'Brien, 2000). Also unchanged is the desire to equate these phases with ethnological groups (O'Brien, 2000; O'Brien and Lyman, 2001).

The techniques available for clustering phenomena can serve as pattern-recognition

devices, but they are incapable of explaining anything. The problem is that, being the output of an inductive approach, the units created are void of theory. This is not to say that inductively derived observations are invalid, but it is illusory to deny the deductive component of observation (Dunnell, 1986: 189), which is what links observations to theory. It was largely by trial and error that the historical types formed by culture historians, during the first half of the 20th century, came to have temporal significance; they were not theoretically informed units.

Classes as ideational inits

Units that are nonphenomenological are classes – ideational units that have been constructed by explicit specification of their definitive attributes. A class *is* a definition – one that specifies the necessary and sufficient conditions, termed *significata* (Dunnell, 1971), that must be displayed by a specimen in order for it to be identified as a member of the class. Specifying the necessary and sufficient conditions for membership in a class does not automatically mean that there are specimens that meet those conditions. Specimens that do meet the conditions, and hence are identified as members of a class, constitute the *denotata* (Dunnell, 1971) of that class. A set of *denotata* is a group, but a group is not a set of *denotata* unless the included specimens have been *previously identified* as members of a class.

Because they are ideational units, classes of phenomena have *distributions* in time and space; because they are empirical units, made up of real things, groups have *locations* in time and space. This feature applies to groups produced by clustering methods, as well as to groups specified by classes. There is, however, a significant difference in the way we interpret locational information. On the one hand, the locations of groups formed by clustering methods are completely dependent on the specimens used to create the groups. Hence, we may have no way of ascertaining whether like specimens occur in other localities. On the other hand, groups specified by classes are not dependent on the specimens used to create them. Since classes are atemporal and aspatial, their *denotata* are free to vary in distribution across time and space; theoretically, specimens could occur anywhere. That they do *not* occur everywhere may be a significant finding.

We earlier noted there are two general procedures for creating groups, the first being to derive them extensionally from the set of specimens being examined. The second procedure involves two steps. First, specify the class definitions, or *significata*. Second, identify each specimen in a collection as a member of a particular class. This procedure demands that the class definitions be spelled out prior to placing specimens in groups. The difference between writing intensional definitions of classes and using clustering approaches is critical. Both procedures result in the creation of ideational units, but only the procedure using intensional definitions can produce *theoretical* units. How might theory inform the construction of the historical types of the culture historian?

Giving as an example the analytical goal of measuring time, we need units that occupy different positions in time. Additionally, each unit should occur only once along the temporal continuum. In the absence of independent evidence of time's passage (e.g. stratigraphy), attributes of artifacts comprise the only phenomena that can be measured. Theoretically, attributes – their particular expressions and their particular combinations – vary more or less continuously across both time and space. Historical types – referred to as 'styles' by culture historians – rendered as a particular attribute state or combination

of attribute states, ideally will have one of two spatio-temporal distributions. One distribution will involve a relatively brief span of time, but relatively much geographic space (Figure 1a); units with this sort of distribution are sometimes referred to as historical index types, or horizon styles. The other distribution will involve a relatively long span of time and relatively little geographic space (Figure 1b); units with this sort of distribution are sometimes referred to as temporal types, chronological types, or historical types. Both sorts of types tend to be 'selectively neutral' (Zink, 1996: 3) kinds; they do not have selective values (Dunnell, 1978; O'Brien and Holland 1990, 1992).

In theory, selectively neutral kinds result from transmission and denote homologous similarity or common ancestry (Lyman, 2001; O'Brien and Lyman, 2000). Because styles are not subject to the selective processes of the environment in which they are found, they are free to vary in frequency, and this makes them particularly useful for measuring time (O'Brien and Lyman, 1999a). Theory informs us as to the kinds of

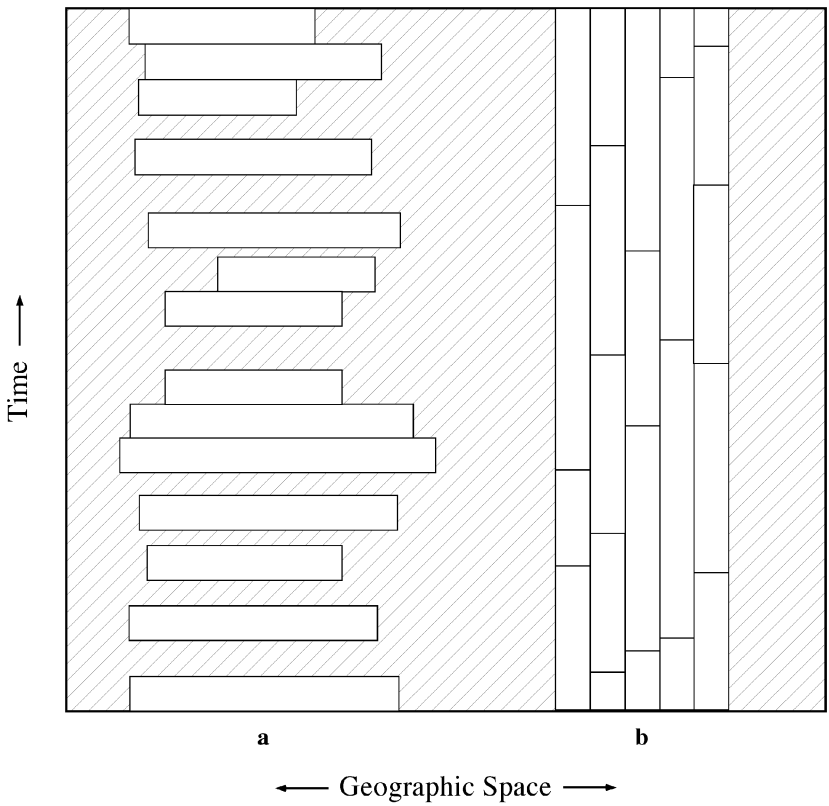

Figure 1. A model of two kinds of units used by archaeologists to measure time. Artifact form varies continuously along both axes, but there is no absolute scale on either axis. Each polygon represents a unit used to measure variation; shaded areas represent formal variation not measured by those units: (a) analytical units include relatively large geographic areas but brief time spans, and they do not overlap in time; (b) analytical units include relatively small geographic areas but long time spans, and they overlap through time.

attributes that should be used to build (intensionally) our ideational units, or classes. Equally important, theory also provides us with test implications with respect to the kinds of distributions historical types will have across time and space. If types do not display such distributions, they will not serve their analytical purpose well, if at all.

Classes are ideational units used to measure variation. The *significata* not only define the class, but they *are* the class. A class might have *denotata*, but it also might not. The fact that there could exist a class with no *denotata* – empirical specimens – indicates that as a measurement unit, a class cannot have an essence. There is no *essential property* that forces a phenomenon to be classified the way it is. Rather, the analyst specifies the *properties of interest* that cause an object or event to be so classified.

SYSTEMS OF CLASSIFICATION

There are numerous systems of classification – the method that structures attributes and thus influences class definitions. We discuss two of them here. One is 'taxonomic classification', the other is 'paradigmatic classification'. The former is a hierarchical structure comprising an ordered set of oppositions, that creates a division of the field of phenomena into classes, subclasses, and so on (Dunnell, 1971: 76). Figure 2 illustrates the relations between and among different levels of units within a simple taxonomy. At any level the units could be termed classes. For example, in Figure 2 there is a class E, but if lower levels of exclusivity are used, the classes become E1 and E2. At an even lower level of exclusivity, the classes are E1a, E1b, E2c, and E2d, and at the lowest level the classes are E1aI, E1aII . . . E2dVIII.

In taxonomic classification, the *significata* of a class reflect an order that is constructed by the analyst. For example, note in Figure 2 that attributes a and b are relevant only for 'superclass' E1. This does not mean that phenomena assigned to, say, class E2cV do not display attribute a or b; it means *only* that because they display the feature that makes them assignable to class E2, attributes a and b are not considered. The particular attribute that makes the phenomena assignable to class E2 is more important in the judgment of the analyst than is the attribute that results in phenomena being assigned to E1. Attributes a and b are irrelevant to further sorting of phenomena in E2, and attributes c and d are irrelevant to further sorting of phenomena in E1. Thus the features that comprise *significata* are said to be 'weighted'.

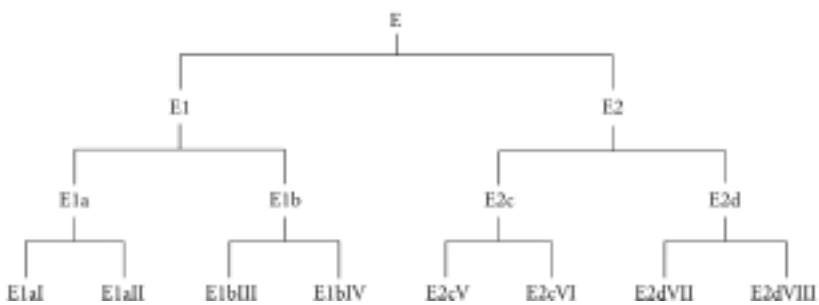

Figure 2. A taxonomy of classes at increasing levels of exclusivity.

Although the various levels of the Linnean taxonomy house empirical referents – organisms – it is usually the case that only the lowest levels – species – are treated that way (Ereshefsky, 1991). We do not, for example, categorize plants and animals by family but by species. It is only because species are members of genera, genera are members of families, and so on, that we can identify a particular organism as representing a particular genus or family. This method of ordering is used to reflect the amount of similarity among the taxa being categorized (e.g. Hull, 1970; Mayr, 1981, 1995). The Linnaean system as conceived of today illustrates not only similarity but *also* ancestry (Ereshefsky, 1994). This does not mean that we cannot be wrong in our assignments of organisms to particular taxonomic units, and hence be wrong in our assessment of ancestor–descendant relationships. We still have to distinguish between similar features that result from common ancestry – homologs – and those that result from functional convergence – analogs.

In contrast to taxonomic classification, paradigmatic classification uses an unordered and thus unweighted structure of attributes. The investigator first specifies the dimensions and the attributes of each dimension that might be of analytical interest. There is no limit to the number of dimensions and attributes. Each specimen is then classified by noting the attributes of each dimension. Any attribute state belonging to a single dimension can combine with any attribute state belonging to any other dimension. Whether they actually *do* or not is a separate, empirical question. Figure 3 illustrates a three-dimensional paradigmatic classification. Dimension X, *height*, has two attribute states, dimension Y, *depth*, has three, and dimension Z, *width*, has two. The classes formed by the intersections of various attribute states are the three-dimensional boxes shown in the diagram. There are 12 of them (2 x 3 x 2) – 1IA, 1IIA, 2IB, and so on. The various intersections of the three dimensions form the class definitions.

Paradigmatic classes exhibit several important characteristics. First, all dimensions are equally important in formulating all classes. In Figure 3, dimension Z is just as important as dimension Y in defining a class. That attribute state B is written behind (to the right of) attribute state II in class definition 2IIB is simply procedural. We could just as validly have started with dimension Z and ended with dimension X. Second, paradigmatic classes are unambiguous, both in terms of internal structure and in terms of their application to the creation of groups. Since all attribute states of a dimension are mutually exclusive, there can be no internal contradiction. Things cannot be green and blue at the same time. If they are green, they are in one class; if they are blue, they are in another class. Third, paradigmatic classes are comparable with all other classes in the same classification because all classes entail the same dimensions. Fourth, any paradigmatic classification is infinitely expandable – attribute states can be added as needed. Similarly, deletion of a dimension or of an attribute found to be analytically useless or ambiguous does not require another examination of specimens (e.g. Beck and Jones, 1989).

Maintaining a distinction between groups and classes

The distinction between ideational and empirical units is not difficult to understand, but it is clear from the archaeological literature that a gulf exists between concept and practice (O'Brien and Lyman, 2000). We know that a class or type is not real, but the 'stuff' with which we are dealing *is* real. Archaeologists routinely deal with types, but even those who have reminded us that types are arbitrary constructs (Phillips et al., 1951) have been unable to keep the distinction clear – in the end treating types as things that,

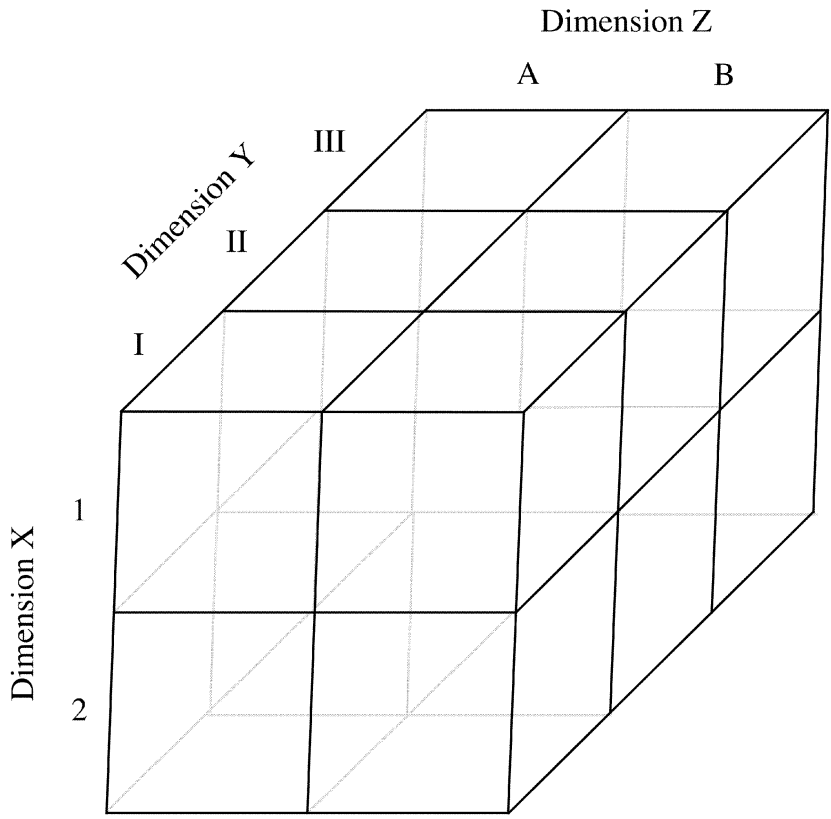

Figure 3. A three-dimensional paradigmatic classification system showing the intersection of the attribute states under each dimension. Twelve classes are shown: 1IA, 1IB, 1IIA . . . 2IIIB.

because of their supposed reality, reflect cultural norms. During the heyday of culture history, failure to appreciate the difference between ideational and empirical units left the door open for debate over how to construct types. This exchange, which began in 1953, came to be referred to as the 'Ford–Spaulding' debate, and in it one can see many of the problems inherent in trying to do archaeology without the theory to support the end product. Because the problems exist today in almost unaltered form, it is worth looking back at that debate, to see not only what it was about, but more importantly why it did not clear up the confusion between ideational and empirical units.

Until the early 1950s, most archaeologists were content to worry about the chronological placement of their artifact types. It was thought that such units *might* reflect the cultural norms or customs of the people who made the artifacts that were being placed in the types – the units might in some way be 'real' – but such suspicions were merely common-sense rationalizations for the units (Lyman et al., 1997). They were not empirically testable save in a tautological manner, as in George Brainerd's (1951a) notion that, because the popularity of cultural norms produced normal frequency distributions

of types through time, the empirical manifestations of such frequency distributions denoted norms or customs. In the absence of theory, common sense prevailed, and the possibility that types were somehow real units grew stronger (e.g. Taylor, 1948). The issue reached a head when Albert Spaulding (1953) published an article describing a technique for discovering real types, to which James Ford (1954a) responded. Spaulding (1954) replied in turn, and Ford (1954c) finally produced a statement concerning his views on types.

Ford (e.g. 1935, 1936, 1952, 1954b) was always clear on the purpose of his types: 'Types should be classes of material which promise to be useful tools *in interpreting culture history*' (Ford and Griffin, 1938: 2, emphasis added). He figured that types could, in some way, reflect cultural ideas, but this was irrelevant to the greater problem at hand. Ford's historical types were 'classes' – ideational units formed extensionally and then tested to ensure that they measured time. But such trial-and-error-created types had an arbitrary appearance, a property Spaulding did not like.

Spaulding wanted a classification technique that 'expressed at one stroke the classifier's opinion of the cultural relationship and the chronological position of an assemblage' – as such a technique that would allow 'a combined presentation of [the] independent units of chronological position and cultural affinity' (Spaulding, 1949: 5). This was a lofty goal – the creation of units that marked not only time but also ethnicity – but the means of creating this kind of unit were unclear to Spaulding in 1949. That changed in 1951, when Brainerd (1951b: 118) indicated that types comprised 'combinations of similar or identical attributes', that such units 'must mirror the culturally established requirements met by the artisans', and that the 'attributes used in sorting artifacts into types should thus be objectively chosen as those which occur most often in combination in single artifacts' (Brainerd, 1951b: 118). Statistical analysis, Brainerd suggested, was the method of choice for discovering such attribute combinations, and he concluded that '[b]etter technique is the solution' to constructing types (Brainerd, 1951b: 124).

Spaulding (1953) thus chose statistics as a 'better technique' and argued further that types formed by clustering algorithms had socio-behavioral meaning. His definition of a type as 'a group of artifacts exhibiting a consistent assemblage of attributes whose combined properties give a characteristic pattern' (Spaulding, 1953: 305) was compatible with Brainerd's and earlier definitions (e.g. Rouse, 1939), because of its emphasis on the recurrence of attribute combinations. One significant difference was that Spaulding's technique for identifying recurrence was explicit rather than trial and error. His emphasis on attribute combinations meant that his types had class-like properties, but since the recurrence of attribute combinations was empirically determined, his types were extensionally derived and thus also had group-like properties. Statistical rigor did not solve the problem – no rules for choosing attributes were specified – and, instead, strengthened the belief that Spaulding's types had some reality.

Spaulding worked with sherds from one site at a time, tallying attribute combinations and creating groups based on recurrent patterning. Were the types represented at locations outside of the one that produced the sample that was analyzed? This question was impossible to answer because of the extensional derivation of the types. If a single sherd from outside the original sample had been added to it, the entire exercise would have had to be repeated. Despite their class-like properties, Spaulding's types were extensional groups with particular locations.

Ford's types were also extensionally formed from combinations of attributes, but the pottery samples were widely distributed (O'Brien and Lyman, 1998; O'Brien et al., 2000). The types had temporal and spatial distributions as opposed to locations. They were formed inductively, but they were theoretical units because they were tested deductively, to determine whether they measured time. If not, they were discarded or refined. Ford's orientation was narrow – types were nothing but analytical units that the archaeologist constructed, so as to permit chronological ordering of ceramic assemblages. But Ford never clearly distinguished between theoretical units and empirical units, and as a result his attempts to use those types to explain culture history were 'badly muddled' (Dunnell, 1986: 172).

Spaulding was up front with his position: types were inherent in the specimens; they were empirical units. Thus to Spaulding: 'Classification into types is a process of discovery of combinations of attributes *favored by the makers* of the artifacts, not an arbitrary procedure of the classifier' (1953: 305, emphasis added). Because types are inherent in the data, they must be discovered inductively, and statistical techniques provided the objective means of determining which attributes regularly, and more often than random chance allows, co-occur on specimen after specimen. Since artifacts are products of human behavior, discovery of recurring attribute combinations – types – is simultaneously discovery of that behavior.

Ford (1954a) protested that Spaulding's (1953) approach was 'amazingly naive' because it only suggested cultural norms; it did not help one *do* archaeology. Spaulding (1954: 393) responded that Ford had not 'challenged the validity of the techniques [Spaulding] used to discover [attribute] clusters', and underscored the procedural murkiness in Ford's constructions of 'attribute combinations'. Here, the debate was over method; Spaulding failed to see the difference between Ford's types as theoretical units – classes – and his own types as groups. The latter were obviously different kinds of units than Ford's, and they were built to do different work. Spaulding noted that his 'attribute clusters' included 'inferences as to the behavior of the makers of the artifacts'(1954: 392). Here, the debate was over the meaning of types. Faced with Ford's questioning of both the usefulness of his units and the lack of a non-tautological test of their interpretive meaning, Spaulding was forced to turn to his method for justification. The legitimacy of the claim that discovered attribute combinations reflected human behavior had to come from method, as there was no other place from which it *could* come. Such combinations existed as human creations and were sortable into recognizable, empirical sets; thus, they *had* to be real.

Cultural types existed, Ford thought, but he was not interested in discovering them. He wanted 'type groupings consciously selected [by the archaeologist to produce] a workable typology . . . designed for the reconstruction of culture history in time and space' (Ford, 1954c: 52). However, he never specified how such groupings were to be extracted from the flowing, constantly changing, cultural stream. To Ford, significant formal variation existed at any point in the time–space continuum, and although that variation might 'tend to cluster about a mean, which [the analyst] could visualize as the central theme of the type . . . [he or she] cannot rely upon the culture bearers to define this theme. They may or may not be aware of it . . . The [type], then, is an abstraction made by the [analyst] and derived from cultural activity' (Ford, 1954c: 45).

Ford proposed that discontinuities presented convenient points at which to insert *arbitrary* breaks in the continuum; if such discontinuities did not present themselves,

one could make the cuts at arbitrary points (e.g. Ford, 1952). Ford's critics (e.g. Phillips, 1970: 908–9) never understood how Ford could make temporal breaks when no natural divisions – either stratigraphic disruptions or the appearance of new cultural traits – presented themselves. He could do it because in his mind the flow of time, and hence of culture, was seamless, varying only in tempo (O'Brien and Lyman, 1998). Chunks had to be carved out of the continuum for analytical purposes, but there was nothing real about them. Types were accidents of the samples available: '[T]he particular locality where an archeological collection chances to be made will be one of the factors that determines the mean and range of variation that are demonstrated in any particular tradition in the culture that is being studied' (Ford, 1954c: 49). Samples of the continuum would provide discontinuous snapshots of that continuum; hence, types 'are easily separable and they look natural [that is, 'real']' (Ford, 1954c: 52).

Ford's strategy for refuting Spaulding's position failed because his allusions to customs and standards gave Spaulding's types a certain reality. But Ford's basic position stands in stark contrast to that of Spaulding, whose types 'presume that significant variation occurs as more or less discrete packages and that variation not assignable to such packages lacks explanatory significance' (Dunnell, 1986: 181). Comparison of Spaulding's 'real' types must be qualitative and must focus on differences between them. Ford's types stem from a conception of reality that holds that variation in form is continuous across space and through time (Lyman et al., 1997; O'Brien and Lyman, 1998). Division of that continuity into chunks through the use of artifact classes is a trial-and-error process, the successes of which Ford evaluated with the historical-significance criterion (Krieger, 1944).

The Ford–Spaulding debate did little to clear up the confusion over the nature of types, and in fact some archaeologists (e.g. Cowgill, 1963) saw no contradiction in the two positions. Given the general belief that types probably have some cultural meaning, and the repetition of statements such as 'New World archaeology is anthropology or it is nothing' (Phillips, 1955: 246–7; see for example Binford, 1962; Taylor, 1948; Willey and Phillips, 1958), it is not surprising that Americanist archaeologists took the identification of prehistoric social entities as archaeology's highest calling.

CONCLUDING REMARKS

The soundness of the decisions one makes with regard to systematics is predicated on understanding several epistemological issues, the most basic of which is the difference between ideational and empirical units. Two other issues – the difference between extensional and intensional definitions and the difference between groups and classes – are also important. Categorization serves two purposes – to structure observations, so that they can be explained, and to allow efficient communication. Many of the types Americanist archaeologists mention are adequate communication devices; we all have a good idea what is signified empirically by the term 'Clovis point'. That particular unit also works well as an analytical device around which some explanations can be built; although based on numerous tests, its value here seems increasingly limited to temporal implications.

Are most of the types that have been proposed in Americanist archaeology classes or groups? This is a significant question in deciding how best to employ the various types that have been proposed, particularly if we plan on using them analytically. Both Ford's and Spaulding's types were classes, although the latter's lost their class-like characteristics, after statistically significant combinations of attributes were argued to be real. Spaulding's

types were statistical products, the definitions of which depended on the sample of specimens to which the clustering procedure was applied. In Ford's system it didn't matter what the sample was, because he selected only those modes that were potentially useful for keeping track of time. The type was defined by the combination of modes. We prefer a Fordian-like system – specifically, paradigmatic classification – precisely because the units are constructed to perform a particular analytical task, and their utility for that task is testable. Such a system allows variation to be measured in a theoretically relevant manner, and we like this because it is variation upon which processes of change such as natural selection and drift operate. Change measured with theoretical units is explicable using a version of Darwinian evolutionary theory cast in archaeological terms (Lyman and O'Brien, 1998; O'Brien and Holland, 1990, 1992; O'Brien and Lyman, 2000).

Intensionally defined classes, whose definitions are imposed on, rather than drawn from, a set of phenomena, have played a small role in Americanist archaeology. Of far greater importance have been extensionally derived units, the derivations originating in the set of phenomena to be classified. Such units have been *described*, and when in great detail, the description may border on a definition, but more often it does not. By the middle of the second decade of the 20th century, archaeologists working in the American Southwest realized that pottery designs and surface treatments had changed over time, and hence sherds carrying traces of those designs and treatments could be used as temporal markers. Eventually, Southwesternists realized that there was no standardization in how pottery types were created and named. The Pecos Conference and the Globe Conference were founded in part to alleviate this problem, one result of which was a standardization in creating type names. The binomial naming system that appeared in the Southwest eventually made its way into the Southeast through the efforts of Ford and Griffin (1960 [1938]), who also sought to standardize the manner in which types were created. Despite such efforts, classifications were still inconsistent from region to region and from investigator to investigator (Dunnell, 1986).

Following the Ford–Spaulding debate, most archaeologists followed the Fordian tradition of creating types in ad hoc fashion, assuming that the types were useful for keeping track of time and space. Yet they also followed Spaulding when they assumed that their types were useful for getting at sociocultural issues, such as what kind of marriage pattern a particular community practiced (e.g. Deetz, 1965; Hill, 1970; Longacre, 1966). This perceived ability to access the past, discovered by the early processualists, was a legacy of the conflation of units that occurred during the heyday of culture history. In many respects the same applies today. We would do well to remember that not all units are created equal; more to the point, units created for one analytical purpose are not necessarily suited for other analytical purposes. Finally, it should be noted that the majority of archaeological types in existence might perform certain functions, but given the nontheoretical, extensional nature of their creation, any success is largely accidental. This applies equally to artifact types as well as to types applied to nonempirical phenomena – be it kinship systems, sociopolitical organization, or food-procurement systems.

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