

Reply to Ford

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there categories of correct ways to dress, dance, talk, and solve the mother-in-law problem, but also there are proper ways to manufacture food vessels, water containers, cooking pots, etc. This is the patterning that is found imposed on the sherds in any time level in a village dump. The degree to which cultures allow variation in patterning varies widely from one culture to another; at different times; and from one aspect of the culture to another. The flexibility of the styles in Weeden Island is in contrast to the rather rigid patterning of Caddoan or Mississippian ceramics.

Spaulding's suggestion that statistical analysis of the patterning to be found in a collection from a village site will establish pottery types useful in study of culture history is amazingly naive. It will reveal the relative degree to which the people conformed to their set of ceramic styles at one time and place, but that is all it will do. Whether this information about ceramics is worth the work, I hesitate to say. However, it should be pointed out that Spaulding is advising the use of data in which variation due to the degree of conformance to standards is welded to variation due to style change with time. Such studies could be better made after the chronology is controlled.

The search for the natural units in culture history, which still haunts the work of archaeologists, is directly analogous to the early 19th century biologist's faith in immutable species following one another in orderly procession down the misty corridors of geological time. Surely it is time we progressed beyond cataclysmic archaeology where deposits representing each period are separated by layers of clean white sand. We now have techniques by which cultural development can be studied.

Patterning is not the central problem of typology, rather it is the framework in which the problem of setting up measures of time-change and geographical space-change of each unit of the pattern have to be solved.

To try to make this clear, I will discuss an actual situation. It is well known that a ceramic association consisting of a grit-tempered ware with a range of conoidal-base shapes and cordmarked decoration forms a fairly stable unit of the patterning that is found at a number of village sites in the northeastern United States. Let us make the entirely unwarranted assumption that we can view the distribution of this association of features at the year A.D. 700. As we cross geographical space to the southward, it will be seen that change took place in the "mean" — or we might say "ideal" — about which the actual specimens cluster. In Kentucky and Tennessee, grit is replaced with sand tempering; clay tempering appears in northern Alabama, and becomes the rule in the lower Mississippi Valley. Form changes from conoidal to rounded to flat base. Similar minor changes can be seen in the application of the surface finish.

Change of these associated traits tends to be gradual as space is crossed, and there is a good reason — which

need not be detailed here — why this should be so. There are no inevitable, necessary breaks which will force the classifier to cut this ceramic distribution into segments. However, diffusion does not operate unaffected by other factors. When enough information is available, it will doubtless be found that rate of change in this pattern unit across geography was speeded up by competition with other cultural forms or by natural, political, or linguistic boundaries. Also, change was probably slowed by movements of people, or routes of easy communication.

After chronology is well under control, it may be possible occasionally to associate recognizable units of ceramic pattern with tribes as Ritchie and MacNeish have recently done. However, there is no inherent reason why such divisions must coincide.

Similar change can be seen in the cordmarking tradition as it is viewed through time. For example, there is a drift from large to fine cords. Here, too, there were doubtless periods of acceleration and deceleration in change due to a variety of possible factors. However, there are no natural inevitable factors operating that will establish neat segments in this change. Replacements of populations will cause sudden breaks in the culture history of Fulton County, Illinois, but that is another matter.

To set up historically useful type units in a tradition such as is represented by cordmarked pottery, I can see no way to avoid detailed comparisons made site to site and through time. Also necessary is a wary awareness that it is the date and geographical position of the site which you chance to dig that give the association of features that look so significant. Had your site been a hundred miles to the north and a hundred years earlier, "Klankenburg Cordmarked" would have been slightly different — a category into which one could place only about half the sherds now called by that name.

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REPLY TO FORD

Ford's objections to the ideas advanced in "Statistical Techniques for the Discovery of Artifact Types" appear to revolve around (1) the notion that use of such techniques somehow constitutes a denial of continuous variation of culture in time and space and (2) certain implicit definitions of such terms as "artifact type" and "historical usefulness" which in effect make their use the exclusive prerogative of the archaeologist engaged in inferring chronology by ranking sites or components of sites in order of likeness as judged by relative frequency of attribute combinations. I shall attempt to show that the first objection is a gratuitous error and that the second is no more than a semantic quagmire.

The issues involved can be clarified by describing three levels of organization of artifacts with respect to the

attributes which they exhibit. I wish to point out in advance that the result of classification at each of these levels has been called an artifact type, and that the levels have an orderly logical relationship to each other. This relationship is lineal; each level represents an elaboration of that immediately preceding it by the addition of new concepts.

Level I is the primary organization of the empirical data, which are the artifacts or fragments of artifacts in an archaeological assemblage. Operating procedure at this level consists of observing and tallying the attribute combinations present. If, as is frequently the case, the researcher plans to conduct comparative investigations by means of these attribute combinations, the tallies are expressed as proportions in order to provide commensurate data. A type at this level is a group of artifacts linked by the possession of a specific attribute combination which someone chooses to call a type.

Level II is an elaboration of the data provided by the Level I classification; the combination counts of Level I are analyzed to provide the total frequencies of each attribute noted, and the relationship of these attribute frequencies to the combination counts is investigated to provide information on the amount and nature of attribute clustering present in the assemblage. A type at this level is a group of artifacts exhibiting a consistent and distinctive cluster of attributes.

Level III is in turn based on the data of Level II, but adds the attribute of function to the attribute clusters of Level II. A type at this level consists of a group of artifacts exhibiting a cluster of distinctive attributes and having a distinctive function or functions. In the case of archaeological data, the function in most situations must be inferred from the attribute clusters of Level II by means of attributes which are not physical characteristics of the artifact (provenience, for example) or by imputing function on the basis of ethnographic analogy.

In order to avoid confusion here, the classificatory entities of Level I will be called simply attribute combinations, those of Level II attribute clusters, and those of Level III functional types. The major purpose of my paper was to suggest suitable techniques for discovering the amount and nature of attribute clustering in any archaeological assemblage—in short, how to perform the characteristic operations of Level II. It was further suggested that the definite attribute clusters isolated by these techniques were artifact types, i.e., corresponded closely to a general idea of the signification of the word “type.” I supposed that an especially valuable feature of types so defined was the fact that they included inferences as to the behavior of the makers of the artifacts, in contrast to the boldly empirical attribute combinations so often called types (and even dignified by a “Binomial System of Nomenclature”). The attribute clusters are “natural” units in the sense that they represent a special effort to infer the behavior patterns of the makers, not the particular needs of an archaeologist working on a particular problem.

These preliminary remarks lead up to the semantic question mentioned in the first paragraph. Ford's comments, although never rising to the level of a coherent definition of what he means by an artifact type, indicate that in his view an artifact type is something which cannot be delineated on the basis of data from a single society over a restricted period of time, thus neatly excluding the possibility that our current knowledge will allow us to describe the artifact types of our own culture in 1953. Dismissing this result as nonsensical, I move on to infer that on the positive side a Fordian artifact type is “historically useful” and that an attribute cluster is not. Unfortunately, we are not favored with an intelligible statement of what is meant by historical usefulness, but it is plain that it has something to do with site to site comparisons extending through some undefined segment of time. Ford's default leaves a clear field for my own definition of historical usefulness: I would argue that any reasonably consistent and well defined social behavior pattern is historically useful, i.e., meaningful in assessing similarities and differences between any two components. The major purpose of my paper was to explore techniques for discovering consistent and well defined behavior patterns, and if the techniques actually do what they are supposed to do they cannot fail to yield historically useful units. The crucial point is adequacy of sample, not occurrence at 2 or 20 or 200 sites.

The alleged incompatibility of attribute clusters and a situation of continuous cultural development (disregard for “basic cultural theory” in Ford's terminology) can be dealt with simply. The methods I described are supposed to be an efficient process for discovering and describing the attribute clusters in any archaeological assemblage, and are nothing more than that. No suggestion was made that any statistical operations would disclose the ultimate significance of the clusters described; significance depends on the nature of the assemblage. The clusters may be the product of contemporaneous patterning, or of a systematic shift in styles over a considerable period of time, or the result of mixture of two more or less discrete cultural traditions, or combinations of these and other factors. Judgment of significance is primarily a matter of interpreting the nature of the archaeological deposit, which, even in the case where a single tradition is involved, can range from the group of projectile points imbedded in the Naco mammoth to the meters of continuous deposit found at some southeastern sites. But the attribute clusters are an excellent device for describing the outcome of such judgment in culturally meaningful terms. Formal recognition of transitional combinations (those differing by only one attribute from each of two distinct types) is particularly helpful in the exposition of continuous cultural change. I would like to make the malicious observation that the pottery attribute combinations used in site-to-site comparison by Ford fail to disclose ultimate significance in precisely the same fashion and to the same degree as do the attribute clusters, and to query Ford as to whether or not the

binomial southeastern pottery types should be abandoned forthwith on the ground that they disregard basic cultural theory.

At this point, it seems more profitable to abandon debating tactics and go to what I take to be the root of Ford's dissatisfaction with the attribute clusters (he has not challenged the validity of the techniques used to discover the clusters). This root is simply the fact that the attribute clusters are obviously not identical with the pottery types used by southeastern archaeologists and hence, in Ford's reasoning are certainly not pottery types, and moreover are probably not good for anything else. The southeastern types are attribute combinations which have been found to be useful in ranking components in order of likeness for the purpose of inferring relative chronology; usefulness here means occurring in varying proportions at several sites (Phillips, Ford, and Griffin, 1951, pp. 61-66). Although this point of view has been criticized above, the argument can be summarized by pointing out that (1) inferring relative chronology is not the sole objective of archaeology, the problem of description of assemblages in terms of patterns of human behavior being equally important; and (2) the attribute cluster with its explicit investigation of patterning has a somewhat better claim to the name "type" than does the attribute combination (the functional type has the best claim of all). By way of comment on statement (1), it might be asked whether anyone has shown that simple attribute counts would not discriminate sufficiently well between components to permit ranking; if they can discriminate, the Fordian type is both an inefficient tool for inferring chronology and an incomplete descriptive tool without any cogent reason for existence. The dispute over names mentioned in (2) is trivial, although it is certain that some confusion in classification has resulted from calling attribute combinations "types" owing to a more or less inchoate recognition that the word does imply patterning. If any semantic boundaries are to be set, clearly the functional type is entitled to the label, the attribute cluster probably is, and the attribute combination is probably out of bounds. I should add that I do not favor setting any boundaries by legislation; I am quite willing to let Ford have his types if he will let me have mine. The important thing is to be explicit about what kind of type one is talking about.

BIBLIOGRAPHY

- PHILLIPS, PHILIP, JAMES A. FORD, AND JAMES B. GRIFFIN
1951. Archaeological Survey in the Lower Mississippi Valley, 1940-1947. *Papers of the Peabody Museum of American Archaeology and Ethnology, Harvard University*, Vol. 25. Cambridge.

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AN UNUSUAL MASONRY WALL IN A KAYENTA ANASAZI CLIFF SITE

Located in deep, narrow limestone canyons draining into the Little Colorado River in the vicinity of Wupatki Basin in northern Arizona are numerous small masonry-walled cliff ruins. While these were apparently occupied at the same time as the large pueblos now included within the boundaries of Wupatki National Monument, they are little known and have not previously been reported upon except in the archaeological surveys of the Museum of Northern Arizona.

One of these sites, N.A.3940, situated with a southern exposure some fifty feet above the floor of Antelope Canyon, is of interest in that the masonry courses are laid in beds of grass rather than with the usual adobe mortar. The ruin itself is located in a shallow overhang in the Kaibab limestone cliff and contains but three small rooms. In no place do the walls remain standing over three feet, and along the front they have almost completely deteriorated. On the surface of the fill inside the rooms only a few corn cobs and four sherds were found. The latter, Moenkopi Corrugated, Flagstaff Black-on-white, Walnut Black-on-white, and Sosi Black-on-white, would indicate a short occupation of the site during Pueblo III times, some time between A.D. 1100 and 1200 (Colton and Hargrave, 1937). No trash accumulation was observed.

The masonry was composed wholly of blocks of Kaibab limestone and the grass, which was identified (by comparison with specimens in the Museum of Northern Arizona herbarium) as *Andropogon* sp. This grass was laid across the entire width of each course, rather than merely being "chinked" in the crevices (Fig. 112). In most instances, whole clumps were placed in parallel rows, forming a more or less plastic base upon which the blocks could be set. There were no indications of any use of mud in association.

One obvious reason for this use of grass instead of mud is the apparent absence of adobe in this limestone area. Even the floor of the canyon would have been covered, at the time of occupation, with a layer of



Fig. 112