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CLASSIFICATION OF THE POTTERY

TYPOLOGY

SINCE practically everything in this report depends on the mass of potsherds collected at the expense of so much bending of backs, it becomes necessary to describe with candor the methods employed in their classification. Archaeology has not reached that stage of development in which there is only one correct way to do things, and, it is hoped, never will. What follows, therefore, is in no way intended as a treatise on the proper way to classify pottery, but merely a description of what was done by us and why — especially why. To say that the choice of methods of classification is governed by the nature of the material to be classified is a truism. But it is no less governed by the predilections and general attitudes of the classifier, and particularly by the ends which the classifier has in view. The extent to which classification may be a creative activity is perhaps not sufficiently recognized. Before embarking on a description of the actual methods of classification employed in the present study, we must therefore furnish a brief statement of our position in regard to the subject of cultural typology in general and pottery typology in particular.

The Concept of Type

In the study of archaeological materials there are, among others, two basically divergent interests: (1) interest in objects as expressions of the ideas and behavior of the people who made and used them; and (2) interest in objects as fossils for the determination of time and space relations. It cannot be maintained, of course, that these two interests are mutually exclusive, but it is an important fact that one's approach to problems of classification will depend very largely on which of them is being served. The first interest we may call, following Taylor,¹ cultural, as opposed to the second which is empirical, in the sense that the classifier is interested chiefly

¹ Taylor, 1948, p. 114.

in what he hopes to get out of it. He is content to work with fragmentary materials such as potsherds — in fact, prefers them because of their susceptibility to statistical treatment — since he is concerned primarily with the distribution of "cultures" in time and space, and only secondarily with the cultures so revealed. The resulting apparent indifference to "culture-context" has been characterized by Taylor as little short of criminal, and there is no question that studies rigorously conducted along these lines make unconscionably dull reading. Nevertheless, the most casual glance at the history of archaeology in any part of the world where it has progressed beyond the stage of antiquarianism will show their utility. Indeed, one might go further in suggesting that at certain stages in the development of archaeological knowledge they are indispensable. Until a certain amount of order has been achieved in respect to time-space relations on a regional scale, it may be questioned whether satisfactory cultural inferences can be drawn from any archaeological materials. We are in such an early stage of development in the Lower Mississippi Valley. The classification which is to be described in the following pages, is, in consequence, the outcome of a frankly, if not fanatically, empirical attitude toward the material.

It has become practically mandatory in putting typological studies into print to declare at the outset that classification is regarded therein purely as a "tool," fashioned to suit the material in hand and the kind of information one hopes to get out of it. Unfortunately, the phenomenon of inter-changeability of ends and means is not confined to political science. Also, there is magic in names. Once let a hatful of miserable fragments of fourth-rate pottery be dignified by a "Name," and there will follow inevitably the tendency for the name to become an entity, particularly in the mind of him who gives it. Go a step further and publish a description and the type embarks on

an independent existence of its own. At that point the classification ceases to be a "tool," and the archaeologist becomes one. This fate we shall endeavor, probably not successfully, to avoid in the pages to follow.

The "tool" that best seems to fit the present undertaking is the system of classification formally introduced into Southeastern archaeology at the Field Conference held at Birmingham, Alabama, in 1938. It is adopted here, not because of a belief in the necessity for a single standardized pottery classification in the Southeast — a classification with a capital C — but because it was the outgrowth of work on material similar to ours by students with the same general point of view as our own. This system is essentially the binomial classification of the Southwest, without any phylogenetic implications, and with important modifications arising from differences in the pottery to be classified. So far as we are aware, the underlying concepts and assumptions of this classification system, as applied to the Southeast, have not been explicitly stated, although the methodology has been described by Krieger.² As interpreted by the present writers, they run somewhat as follows:

It is first of all assumed that, after the introduction of pottery-making into the Southeast, a gradual shift of all ceramic features took place.³ Techniques of manufacture, surface finish, shape, and decoration constantly, but slowly, changed. Such changes were no doubt partly the result of new ideas from outside the area — whatever mechanisms were responsible for the introduction of pottery-making in the first place may have brought increments to the original stock of ideas from time to time — but in the main, changes are assumed to have been internal, resulting from play on the potentialities of current forms and styles. Revolutionary inventions, in the popular sense of the word, probably occurred rarely if at all. In general, therefore, pottery styles at any given time and place may be assumed to have derived from those that went before; if not in that particular spot, then in another region with which there was contact.

² Krieger, 1944.

³ The word "introduction" is used here in its widest possible sense. It is not intended to imply the assumption that there was only one introduction of pottery

Each community that had reached a certain level of sophistication in pottery-making will be found to have been maintaining side by side several different vessel styles. These are normally closely related, particularly in the matter of construction, paste, and surface finish, and seem to mark vessels made for different purposes or vessel ideas derived from different sources. If any one of these particular styles is examined at a single place and a single point in time, it will be seen that, while each vessel varies in minor detail, such variations tend to cluster about a norm. This norm represents the consensus of community opinion as to the correct features for this particular kind of vessel. Variations from the norm reflect the individual potter's interpretation of the prevailing styles, and the degree of variation tolerated is also culturally controlled. With the convenient hindsight of the archaeologist, we can divide such variations into two classes: those which were not followed by the rest of the community, and those that were. The latter are, of course, significant as the means by which ceramic development was accomplished.

In areas where the distribution of population was relatively stable, as seems to have been the case in most of the Southeast for long periods, ceramic development was general and in a crude sense surprisingly uniform. There was always a tendency, however, for particularly vigorous centers to impress their ideas on less enterprising neighbors. When a cross section of a large area is viewed at a given point in time, popularity centers will be seen for certain styles. Between these centers, styles vary and trend toward those of other centers in rough proportion to the distances involved, subject of course to ethnic distributions and geographic factors.

Thus, we have in mind the concept of a continuously evolving regional pottery tradition, showing a more or less parallel development in and around a number of centers, each of which employs a number of distinct but related styles, each style in turn being in process of change both areally and temporally.

into the Southeast, or even to rule out the possibility of independent invention. We can produce as good an evolutionary series from stone pots to Arkansas head vases as has been done anywhere else.

With this remarkably unstable material, we set out to fashion a key to the prehistory of the region. Faced with this three-dimensional flow, which seldom if ever exhibits "natural" segregation, and being obliged to reduce it to some sort of manageable form, we arbitrarily cut it into units. Such *created units of the ceramic continuum* are called *pottery types*.

The importance of this concept of type in the present study justifies a certain amount of repetition. It can be illustrated in a crude

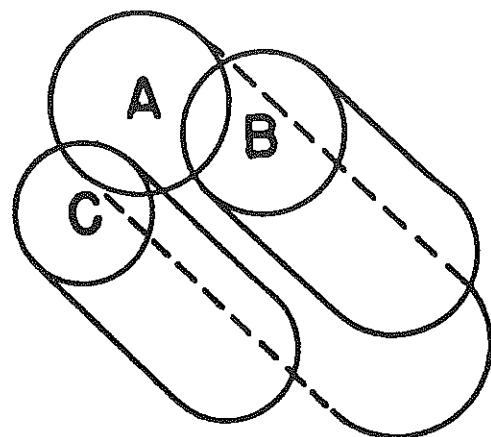


FIG. 5. Diagrammatic representation of the concept of pottery type.

diagrammatic form (fig. 5). Let the letter *A* represent a type and the circle around it, in purely abstract fashion, its limit of variability. That is to say, if an individual specimen does not fall within the range of variation represented by the circle, it does not conform to the type. We have already seen that variation and geographical range are closely related, so our circle represents, albeit crudely, the geographical range of the type as well. The circle thus describes the type two-dimensionally. To this, a third dimension, time, may be added by which our figure becomes a cylinder. The length of the cylinder represents the range of variability resulting from changes taking place in time. Several closely related and overlapping types—the most carefully defined types always overlap—can therefore be represented as above.

The length of the cylinders is shown as unequal because of the inevitable variation in

persistence of the peculiarities we have used to define the types. Actually, of course, these cylinders are a tremendous over-simplification. If it were possible to show it diagrammatically, they would be irregular in shape, expanding, contracting, branching, and coming together, so that no two horizontal cross sections would be the same. The figure is a hopelessly crude approximation to the facts, but it serves to illustrate the point of interest here, to wit, the arbitrariness of the whole typological procedure. The drawing of the circles is arbitrary and highly subjective—no two classifiers will draw precisely the same circle—the cutting-off of the cylinders is equally so. It is scarcely necessary to draw attention to the additional difficulty of determining whether a given variation is the result of spatial or temporal factors. If this is anything like a true picture of what a pottery type is, can anyone seriously ask whether it corresponds to a "natural" or cultural reality?

Whether, among the primitive communities of the Mississippi Alluvial Valley, groups or classes of pottery were recognized, and whether such entities, if we may speak of them as such, were conceptualized in terms anything like those with which we define a pottery type, are intriguing and by no means unimportant questions. We need not go into them here, however, because by reason of the very nature of the material to be classified the possibility that our and their conception of a given type might coincide is so remote as to be negligible. These are questions that can scarcely arise in the initial stages of a comprehensive pottery survey. Before leaving the subject, however, let us make it clear that, although the empirical typology here described—"working" typology as Krieger perhaps would call it—cannot be expected to show any strong relationship to cultural "reality," it does not follow that such relationship is precluded now and forever. To a certain extent, the characters we select as criteria for type definition, however dictated by expediency, not to say necessity, are bound to correspond to characters that might have served to distinguish one sort of pottery from another in the minds of the people who made and used it. We should, of course, make every possible effort to increase this correspondence. In course of time, with increased information

in respect to vessel shapes and over-all patterns of design—let us not forget that we are classifying vessels, though we have for most types only sherds to do it with—our types will be redefined in ever closer approximation to cultural "realities." In short, an eventual synthesis is possible between the seemingly antithetical attitudes loosely characterized above as "empirical" and "cultural," in which the product of classification, the pottery type, will finally achieve cultural meaning. The limits of the variability of the type will then no longer be wholly arbitrary decisions of the classifier, as is now the case, but will bear some correspondence to ethnographic distributions in time and space. On this hopeful note, let us conclude this brief introduction to the problems that beset those who set out to measure and relate, by means of wretched bits of fired clay, cultural phenomena of which there are as yet no other records.

The Concepts of "Pottery Complex" and "Series"

As already remarked, in cultures of the level reached by the pottery-making Indians of the Southeast, a given site, or level in a site, will show several discrete pottery types, each of which may have a separate history. It is convenient to refer to such a group of types as a "complex." This pottery complex in a given situation is usually described in terms of percentages of the several component types and, where so described, gives a very useful measure for comparison with other cultural situations. A further step is sometimes made in which all or several of the component types of a complex are given the same site designation and the group referred to as a "series." Thus, the types Alexander Incised, Alexander Pinched, and Alexander Dentate Stamped are members, along with O'Neal Plain and Smithsonian Zoned Stamped, of the well-known "Alexander Series" in northern Alabama. However tempting from the standpoint of simplicity, we are not using the device in the present work. As Krieger has pointed out,

* Krieger, 1944, fig. 25.

"Ford objects to this usage of the term 'series' on the grounds of poor English. 'i. A number of things or events standing or succeeding in order, and

such linkage of types is a further step in the typological process.⁴ Furthermore, when becomes clear that related types have the same distribution in time and space, it may be preferable to lump them together as a single type whereupon the concept of "series" become superfluous. Until such a time, their separate identities have to be maintained, and it is perhaps less confusing in the long run to keep the names distinct as well. It is hardly necessary to add that, in our view, neither "complex" nor "series" are classificatory terms in the sense that they represent an order, more inclusive than type, in a taxonomic system. The fact that they do tend to acquire such a meaning is an additional reason, in our view, for avoiding their use, except in a context that leaves no room for ambiguity. When, therefore, in the following pages, we find it convenient to speak of a pottery complex it will be understood to refer only to a given archaeological situation, and the term "series" will be, so far as possible, avoided.^{4a}

The Binomial System of Type Nomenclature

The above-described methodological freedom, however, does not by any means eliminate any of the difficulties or responsibilities of the classifier. As already pointed out, the norm of style, which we measure by means of pottery types, shifts both areally and chronologically in a manner so gradual that we are hard put to say at what point in either dimension one type leaves off and another begins. But this is not all. The separate characters of paste, surface, form, and decoration change also in time and space, and not all at the same rate. Each separate character has its own history and each history will provide a more or less sensitive register for the history of the culture as a whole. Now it would be unreasonable to hold that one character is unfailingly more important than another, or to insist that all pottery be classified on the basis of uniformly selected characters. It does, however, lessen the initial confusion to select

connected by a like relation; sequence; order; counsel; a succession of things; as, a *series* of calamities or triumphs." (Webster's New International Dictionary.)

the most sensitive — and at the same time most recognizable — characters as guides or “constants” in the process of classification. In Southeastern pottery generally, these are features of surface treatment and decoration, and thus it has come about that what may for convenience be called the Southeastern classification employs a binomial system of nomenclature in which the second term or “constant” is descriptive of surface treatment or decoration, as in *Mulberry Creek Cord-marked* or *Indian Bay Stamped*.

It may be asked why characters of shape are not regarded as equally sensitive and equally suggestive of culture change. The answer is found in our original point of departure and its paramount interest in sherds rather than vessels, and more objectively, perhaps, in the fact that except in the very latest periods the Southeastern potters had not reached an advanced stage of refinement in vessel form. Features of shape, particularly rim and lip modifications, have sometimes proved useful in sorting the present material, but it has not been found expedient to use them as constants in classification. All types set up by the Survey, therefore, are defined in the present work according to the Southeastern procedure.

By so doing, we shall have lessened but by no means removed the main typological difficulty. Type “A” let us suppose has been defined on the basis of what appears to be the “norm” at what appears to be a “center.” We may as well admit that these “centers” are often only such because they first attracted our attention, or because of our ignorance of the intervening spaces. As we go away from that center in space — the same thing happens in time, but we may leave that unpleasant fact aside for the moment — the characters that we have selected as determinants for the type gradually shift, the all-too familiar phenomenon of “creep,” until at some point we can stretch our original type definition no further and have to consider whether material “X” more closely resembles Type “B,” already established at another center, or whether it is not sufficiently like either “A” or “B” and must be given an independent status as Type “C.” These wretched hair-line decisions beset

the classifier at every step. The only helpful principle the writers have been able to find is one of the sheerest empiricism — ability to sort. Rigorous application of this principle results in some rather startling distributions. For example, we were unable to distinguish certain clay-tempered, cord-marked sherds found in our area from the type *Mulberry Creek Cord-marked*, originally set up in the *Pickwick Basin* on the Tennessee River several hundred miles away. Candor compels the admission that we could not sort it from what was then called *Deasonville Cord-marked* in southern Mississippi and Louisiana either. If *Mulberry Creek Cord-marked* and *Deasonville Cord-marked* could be separated, as implied in the fact that they were set up as distinct types, our material obviously could not be both, so an arbitrary decision had to be made. We first encountered the pottery in the northern part of the Survey Area where it more closely approximates *Mulberry Creek Cord-marked*. If we had encountered it first in the south, we undoubtedly would have been struck by its resemblance to *Deasonville Cord-marked*. Our solution was to call it *Mulberry Creek* and quietly liquidate *Deasonville*. From this sort of dilemma there is no escape, but its effects can be minimized by more knowledge of the pottery and sharper definition of types.

It may be surmised from the above that much depends on the sort of constants by which types are defined. The “creep” of types in time or space is by no means uniform. A generalized surface treatment like cord-marking, for example, will extend farther without sortable change than a type based on a specialized scheme of decoration; a plain undecorated type may go farther than either. So far as possible, an attempt should be made to reduce these inequalities by careful choice and definition of constants. On the other hand, bearing in mind that classification is only a means to an end, the classifier will choose the sort of constants that he thinks are likely to turn out to be culturally and historically significant. Finally, let it not be overlooked in the glow of these fine thoughts that in the initial stages of classification one seizes upon *any* feature that will serve to distinguish one group of sherds from another.

Having, so to speak, walked around the pottery type as understood here, and examined some of the more obvious aspects of its anatomy, we may conclude by admitting that the practical necessities of the situation have virtually destroyed it as a concept in any *a priori* sense. We are left with — and this is as much of a definition as will be found in the present work — a named abstraction, representing a combination of selected characters, susceptible of recognition from sherds alone. An artificial, albeit useful, creation of the classifier, the pottery type has at this stage of being little, if any, correspondent cultural “reality,” present or past. Exigencies of language require us to think and talk about pottery types as though they had some sort of independent existence. “This sherd is *Baytown Plain*.” Upon sufficient repetition of this statement, the concept *Baytown Plain* takes on a massive solidity. The time comes when we are ready to fight for dear old *Baytown*. What we have to try to remember is that the statement really means something like this: “This sherd sufficiently resembles material which *for the time being* we have elected to call *Baytown Plain*.” Frequent repetition of this and similar exorcisms we have found to be extremely salutary during the classificatory activities described below.

Methods of Sorting

The 1940 season’s material, consisting of surface collections only, from 149 sites, mainly in the Lower Arkansas River Lowland and St. Francis River Basin, was shipped to Baton Rouge for cataloguing, and there sorted by the writers at the conclusion of the field season. This preliminary sorting was done in the time-honored way, that is, by piling the whole mass of sherds into one terrifying heap, and sorting them as many ways as possible. The experience in other Southeastern areas prompted the use of tempering differences as a primary breakdown,³ and the result was that we ended up with 47 types loosely grouped into three temper groups: shell, clay, and sand. Fiber-tempering did not occur in the first season’s collections. The “constants” selected were in the categories of surface finish

³ So far as we are aware, the first use of tempering differences as a primary ceramic breakdown in the

and decoration. Under the latter, technique of decoration was relied on more than design, as is necessary when sorting surface material, on account of the small size of the sherds. It should be pointed out that these 47 types were not all “new.” Eight of them had been previously named and described in the Lower Mississippi Valley, and two were among the types set up in the *Pickwick Basin* in northern Alabama. After sorting, the material was broken up into the original site collections, counted, and tabulated. Some preliminary graphic analysis, of the sort described in a later section, was carried on by Ford, so that in going back into the field in the following spring, we had a fair idea of the probable chronological relationships of our provisional types, which was a great help, particularly in the selection of sites for stratigraphic testing.

The second and third season’s material, mainly from the Yazoo Basin, was handled differently. Site collections and stratigraphic material were kept intact. By this time, we felt we had a classification and that it was therefore unnecessary to throw all the pottery together, as was done the previous year. Now, this raises a question pertinent to the discussion of typological theory just completed. There is no question that, had we sorted this Yazoo Basin material afresh, forgetting as far as possible, our existing typology, we would have come out with a classification quite different from that presented here. Not that the list of types would have differed so much, but the types themselves would have been differently defined. The norms in the Yazoo Basin are not the same as in the St. Francis Basin. Throughout all our description and discussion of types, we are constantly being reminded of the “set” that was given to our typology by the fact that it was largely determined by the first season’s work. In spite of all efforts, our type definitions in many cases really apply to the St. Francis Basin pottery. We have not been able to rid our minds entirely of the notion that corresponding type material in the Yazoo Basin is deviant if not atypical. Another interesting result is that we tend to see a culture divide or “frontier” about the latitude of

Southeast was in Griffin’s study of the pottery of the Wheeler Basin in Webb, 1939.

the mouth of the Arkansas River, because it is here that the type definitions set up in the St. Francis begin to reach their limit of applicability; the types begin to lose their outlines. The fact is very neatly expressed in the increased percentages in the "unclassified" column of site tabulations south of this point. We have to be on guard against such appearances. Getting back to the point at issue, it seems now in retrospect that it would have been worth the extra effort to have piled all three season's collections together, and all subsequent material for that matter, and made a final grand sorting. However, our present purpose is to describe what we did, not what might have been done.

Sorting and analysis of the 1941 collection was interrupted by the war. When the work was resumed in 1946 the situation was as follows: The 1940 collections had been broken up. Approximately a third of the material was still in Baton Rouge. The remainder had been sorted at least once by one or more of the present writers, but it was felt that a new sorting by one of us was required not to eliminate the personal equation, which cannot be done, but to standardize the errors resulting therefrom, and accordingly all the material, to which was subsequently added the collections of 1946 and 1947, was assembled at the Peabody Museum. This was another opportunity to throw it together, but it was sorted by sites as before. In the process, a number of changes were made in the previous classification, several new types being set up to take care of the material in the southern part of the Yazoo Basin. At the same time a number of earlier types were abandoned. As each site collection was sorted, the types were segregated in small boxes and filed away by sites in cupboards where they were readily available for further study. Body sherds of the more common types were counted and put away in dead storage, and the counts entered in a dead-storage file. The material was thus in excellent shape for final analysis. This was done by type in the following manner. All the rims and bases (in types that have bases) — or in the case of the numerically smaller types, all the sherds — were spread out on long tables in geographical order from north to south. Data sheets

listing the occurrence by site of all measurable and observable features were prepared. Direct observation, backed up by these data sheets gave an excellent idea of the nature and extent of the variations of the type in its geographical range. "Centers" for the type, and for particular features within the type, were readily apparent. The gradual shift of the type away from these centers was clearly seen again and again. In some cases, this shift became so great as to require an arbitrary decision — thus far and no farther — and the creation of a new type. While the material was in this convenient form, quantitative distribution maps were made, which proved to be very useful in further delimiting the types.

At this point, it may be well to make another confession. What Hooton used to call the "fierce typologist" does not compromise with space or time. If two things look alike, he puts them together regardless. So, in theory, do we. But it sometimes happens otherwise in practice. When confronted with one of the innumerable hair-line decisions one has to make in sorting, it may be that the mere location of the site from which the sherd comes, or its chronological position, if it comes from an excavation, is sufficient to throw it one way or another. We have not scrupled to use such adventitious aids to sorting from time to time.

With the material still spread out on the tables and the data sheets as a control, a preliminary type description was written on the spot. Now to write a close description of a type represented by collections from several hundred sites covering a range of 200 to 300 miles, to say nothing of a considerable stretch of time, is not easy. There is an irresistible tendency to orient your description around what seems to be the "central" material of the type, and to describe the variations in terms of this assumed "center." It is hardly necessary to point out again that a center may be nothing more than the place where one happened to get the most material. It is also very apt to coincide with the place where one first set up the type. Pottery descriptions have always to be read with this in mind. Ours are only exceptional in that we admit it.

The last act before putting away one type and getting out another was to select a num-

ber of specimens for the type collection, and these were also inventoried by site in a card-index file. A further distillation from these was made for the photographs in this report.

Upon completion of the analysis, there remained a considerable residue of unclassified material. This was also spread out on the tables and sorted carefully for possible new types. Actually, none were found, which was reassuring in a way, as an indication that our classification was sufficiently thorough, for the present at least. Several tentative types were set up, and these are described in this section under the heading "Provisional Types." It was felt that these would no doubt attain the status of types with more material, so they are useful in pointing to areas for further investigation.

There remained only the final unexciting task of counting and tabulating on printed site inventory sheets to which were added the counts from the dead-storage and type-collection files. Totals and percentages were then calculated, and we were finally in a position to begin to get some returns on our investment.

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Assumptions

NOW the 346,099 sherds from 383 sites, collected by the Lower Mississippi Survey and duly classified as described in Section III, could be stored away in cabinets and forgotten for the time being. The data was safely on paper and time would heal our wounded consciences and dim our suspicions that at several points our classification was less than perfect. During the winter of 1947 Phillips turned to the problems of physiography, and the identification of historic sites; Griffin began the description of pottery types; and Ford started work on analysis, assisted and checked at every point by his somewhat fearful colleagues.

The basic assumptions which served as a foundation for the analytical procedure need to be stated in some detail. They will help to explain the procedure followed and it is hoped will prevent the reader from accepting the conclusions in an any more "positive" sense than the writers intend. We consider these assumptions as a set of probabilities which lead to conclusions that are our best guesses. Not that we intend to apologize for this admission. This we think is the real method of science. We are trying to expose our limitations and are not setting out to *prove* anything beyond all doubt.

A. In the portion of the Mississippi Valley which was surveyed and for the greater part of the span of history which is being studied, the aboriginal people were presumably agriculturists. The population was rather numerous, as will be shown later, and was collected in small villages. For these reasons it seems reasonable to think that there was comparative stability of peoples. These Indians did not wander as did the historic Indians of the Plains and, from the archaeological evidence, there seems to have been little or none of the frantic shifting of tribes that marks the post-contact history of the Eastern Indians. We are assuming then until the evidence indicates the contrary that

the people who carried the cultural traits we are studying were probably relatively stable geographically and that for the most part population changes were slow gradual ones.

B. While the prehistoric populations were comparatively stable in the larger geographic sense, this does not appear to have been true of the great majority of village sites. Some sites were inhabited throughout the time span which is being studied. Most, however, were occupied for a short time in proportion to the entire chronology. This assumption was based on archaeological experience in other parts of the Southeast and on a preliminary glance over the collections gathered in this Survey. The condition seems to be due to the limitations of the agricultural methods and equipment of the Indians. After a field had been cleared and used for crops for a few years, the grass and weeds probably moved in and took over. With the inefficient tools which the Southeastern Indians had, control of this vegetation very likely became so difficult after a few years that it was easier to ring and burn trees for a new field than it was to continue planting in the old one. In the course of a few decades, when all the desirable agricultural land in the vicinity of a village had been opened up to weeds in this fashion, the village would have to be moved to a new location.¹ This was the practice of the Southeastern tribes in the early Historic Period before they acquired plows, and such names as "Chickasaw Old Fields" and "Tuckabachee Old Fields" undoubtedly refer to such weed-grown abandoned land.

The securing of short time-span collections is essential if the method of seriating of surface collections is to be successfully applied. For this reason, careful attention was paid to the combinations of sherd material which were gathered from various parts of each village site. In the course of field work, where it was evident that one portion of a site yielded a different complex from that found on another part, two or more separate collections

were made. These were labeled "A," "B," etc., and were treated all through the course of analysis as though they came from different sites. A cross section of the ceramic styles in vogue at these different sites at one instant in time would have been the ideal material for seriation purposes, but that, of course, is an unattainable goal.

C. The third assumption has already been stated in the foregoing section on ceramic classification. Until the evidence suggests differently, we are assuming that in any large area cultural continuity in both time and space is to be expected as the normal state of affairs. A gradual change of feature with the passage of time and across the area, when it is viewed on any one time horizon, was our very idealized concept of the cultural history with which we are dealing. This does not mean that we did not anticipate the possibility of finding evidence of (1) the replacement of one population bearing a certain variety of culture by another population having entirely different customs; (2) the replacement of cultural features through acculturation from sources outside the region in which we are working; or (3) the specializing of cultural complexes in certain regions due to their being protected from the prevailing patterns of the area as a whole by such factors as geographical isolation, peculiarities of population distribution, linguistic barriers, or political groupings. These conditions were some of the things of which we expected to get hints from our study.

So we did not begin our analysis with any assumption that changes in ceramics, such as the shift from clay- to shell-tempering, necessarily indicate any abrupt cultural or population replacement. If the refuse deposits of the two time periods really should have a layer of clean white sand separating them after the classic model of stratigraphy, we wanted to be shown by the evidence.

D. Our fourth basic assumption has also been stated in the discussion of ceramic typology. We are assuming that each of our pottery types is a more or less sensitive instrument for measuring cultural change with the passage of time and distribution over space. We are a little placid about this assumption and

¹ Spier, 1917.

feel that we are on fairly certain grounds because we went to great pains to set up and adapt each type for exactly those purposes. Rearranging, merging, and splitting of type groups were guided by preliminary analysis and the resultant information about chronological relations.

However, as has been made clear in the type descriptions, all of the types are not equally well adapted for this purpose. Because of the practical difficulties of making distinctions, some of the types, especially the undecorated ones, include material that represents long spans of time and large amounts of area. In other examples we are aware that the original concepts have changed during the classifying so that the resultant categories are somewhat broader than would have been desired. Mazique Incised is an example of this latter kind of type weakness. Despite this, we feel that we are fairly aware of this factor and thus have it under reasonable control.

E. The next point to be considered is not a basic assumption but rather a logical derivative of the preceding discussions. It has to do with the relative popularity of types through time. If our pottery types are successful measuring units for a continuous stream of changing cultural ideas, it follows that when the relative popularity of these types is graphed through time, a more or less long, single-peak curve will usually result. Put in another way, a type will first appear in very small percentages, will gradually increase to its maximum popularity, and then, as it is replaced by its succeeding type, will gradually decrease and disappear.

This interesting phenomena can be illustrated by endless examples taken from any span of culture history. Consider the popularity curve of the "Charleston" dance fad in the United States. A specific political concept, a particular word, or any other carefully defined cultural type will show the same popularity curve that Spier found in the history of Zuni pottery.^{1a}

This is an interesting phenomenon but do not let us be misled. We have not discovered a natural law operating independently of our own humble efforts. This peculiar charac-

¹ Linton, 1940, pp. 37-40.

teristic of type popularity distribution through time is something we have helped to bring about through our own conceptualization of the pottery types that manifest said behavior. How the curves come out is partly controlled by how the types are defined.

F. The sixth assumption is also a derivative of the foregoing discussions. If a complex of cultural materials representing a space-time continuum of culture history is classified in a consistent manner, the popularity curves of the various constituent types will form a pattern. Each portion of this pattern will be peculiar to a particular time and area. This concept may best be illustrated from contemporary culture. Lacking accurate data, as this sort of information is usually ignored by historians, let us manufacture some for purposes of illustration.² Let us say that in the State of Ohio in the year 1920, the following were the relative popularities of the indicated types of travel for distances over 5 miles:

	PER CENT
Walking	5
Riding horses	5
Horse and buggy	15
Gasoline-powered boats	5
Steamboats	5
Automobiles	20
Airplanes	2
Railways	43

Here is a ratio of popularity of transportation types which will never be exactly repeated in Ohio or anywhere else.

Now let us take a look at a supposed history of the relative popularity of transportation types in Ohio for a period extending sometime before and after 1920. This we have graphed in figure 15.³ Not only is the pattern different for each ten-year interval, but the quantitative picture of this stretch of culture history is a unique thing. The pattern of the popularity peaks of the different transportation types have never been repeated. A simi-

²Historical statistical data about manufacturing, trade, etc., will give this kind of information. However, it is easier to make up our illustration than to dig it out of the census.

³Here we have used the type of diagram which will often reoccur in this study, so we might as well explain it now. The passage of time is always represented by proceeding from the bottom to the top of

lar graph for Texas would doubtless show larger popularity of horse-riding. There wouldn't have been any steamboat travel at all in Utah. Indiana would show the same type as Ohio but in differing quantities and temporal relations.

So long as we maintain our classifications strictly as they are, we may review any number of representative samples of Ohio transportation history, and the same frequency pattern will result. The only way in which the pattern might be changed would be to change the classification. This can be done in a number of different ways. Let us show a few:

1. Travel without vehicles
Vehicles that travel on land
Vehicles that travel on water
Vehicles that travel in air
2. Man-powered travel
Animal-powered travel
Steam-powered travel
Gasoline-powered travel
Electric-powered travel
3. Travel 0-5 miles an hour
Travel 5-10 miles on hour
etc.

Note that in each case where the classification is rearranged, the quantitative-historical picture would be completely different. This is not to say that it would be any more true or false than the scheme which we have illustrated in figure 15. All of these classifications will measure time change in a cultural feature. The point of interest to the classifier is that the first scheme with the finer type divisions will do the job a little more accurately than the others. Still finer divisions which will do even better jobs will occur to the reader.

While this fanciful illustration is set up, let us go a little farther and show how the dating and seriation techniques that will be discussed later will work. Suppose that we

the figure. Each cultural type is assigned a vertical "axis," or imaginary line, which is indicated at both top and bottom of the figure. The relative popularity of the type is shown by the length of the horizontal bars that center on the type axis. This may be measured by means of the percentage scale given in the figure. Try it for the year 1920 and see if the graph agrees with the tabulation given above.

have a sample of the transportation habits of the Ohioans for an unknown date which showed the following percentages of popularity:

	PER CENT
Walking	5
Riding horses	12
Horse and buggy	28
Paddling and rowing boats	1
Steamboats	12
Automobiles	0
Airplanes	0
Railway	40

with frequency data on the transportation customs of Ohio for a number of years. We do not know the dates of these samples and have no idea as to their chronological sequence. We can't get a complete history out of this data but we can do something. By rearranging our samples, we can find the type frequency pattern and the relative order of the samples. We will not know the calendrical dates of the samples, the relative lengths of time occupied by the various sections of the chronology, or even which end of the chronology is the most recent in time, but we can develop the quan-

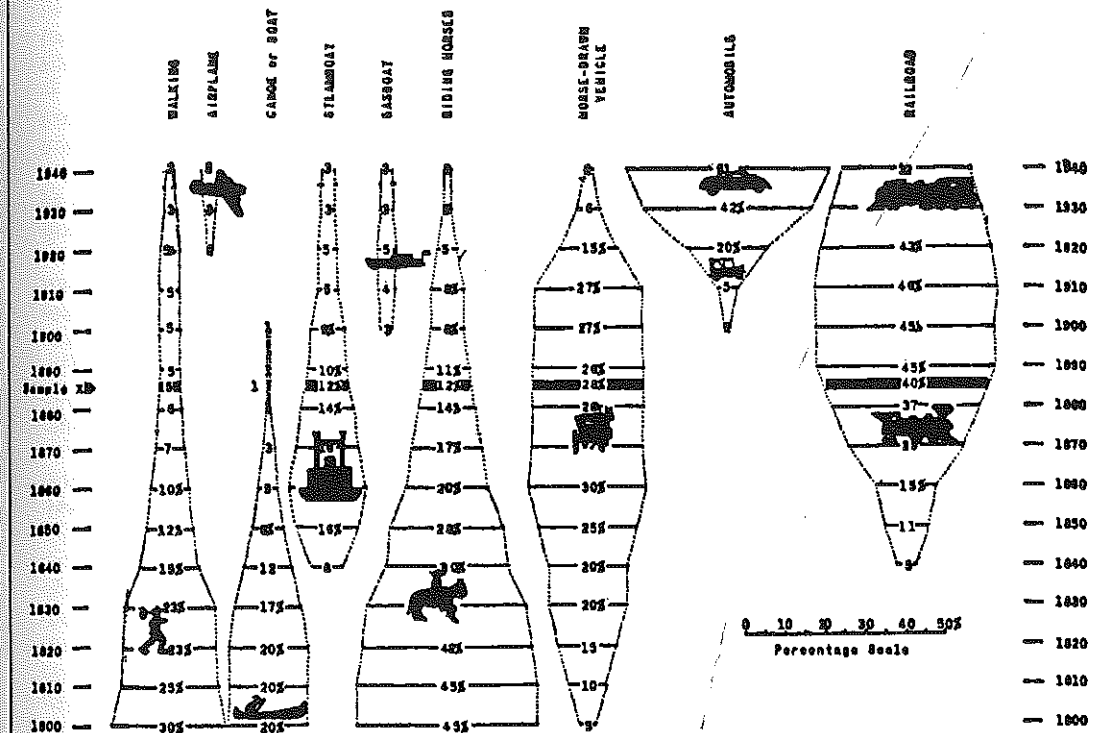


FIG. 15. Theoretical percentage frequency graph of transportation types in Ohio from A.D. 1800 to 1940.

When this information is graphed after the fashion used in figure 15, and the graph is placed on this chronology, it will be seen that the type frequencies of this sample, which we may as well call "X," will fit the chronology at only one point. As our figure shows, it dates about 1885.

Let us suppose again that we are faced with a situation in which we are merely provided

titative-historical pattern. This, in effect, is the seriation technique we have used.

This rather far-fetched bit of imaginary analysis is only worth-while if it brings out the point that systematic classification of cultural data representing a particular range of time creates in each case a characteristic quantitative pattern. We had this in mind as our sherds from the Mississippi Valley area were classified,

and the analytical procedure that will be described were the steps which were taken in search of these patterns.

G. Two more assumptions which we have made may be grouped together. We have assumed that our sampling of sites in each part of our Survey Area has been sufficiently thorough. We think that we have secured a sample of the pottery which was made during each stage of the chronologies which we will present so that no large time gaps remain unrepresented.

We are also guessing that a random sample of over fifty sherds is sufficient to indicate the proportionate type frequencies existing in the refuse from which the material was collected. A total of fifty is considered to be usable, but not particularly reliable. One hundred is much better and every sherd above one hundred is all to the good.⁴ It will be noted that some of our collections are quite large.

The foregoing assumptions which we made at the start of the analytical work, and which we intended to act upon until the evidence indicated that they were wrong, may be summarized as follows:

A. The distribution of prehistoric populations of the Survey Area was relatively stable.

B. The majority of the village sites were probably inhabited for a short time as compared to the entire time with which we are dealing.

C. The culture of the area in the main probably changed gradually rather than by means of mass migration from other areas.

D. If propositions A and C were true, the pottery types which we had defined would each show a single-peak popularity curve when measured through time, but the duration of such peaks, and the resulting curves, would vary from one type to another.

E. If D is true, then all the pottery-type frequency curves would be different in each part of the area on each time horizon, and a distinct pattern will appear when each part of the area is viewed through time.

F. Our sampling technique has been successful in getting samples representing continuous segments of time in all parts of the area

⁴For a brief discussion of quantitative reliability of collections, see Ford, 1936, pp. 13-14.

and also in securing enough material from the sites which we will treat to give a more or less reliable picture of the material available on the surface.

Analytical Procedure

The first step in our ceramic analysis was a simple and tedious one. On the sheets which recorded the classification of the material from each collection, the totals of these collections were run up on an adding machine, and the percentages of each type calculated by slide rule. The "Unclassified" sherds were included in these totals. This was done for all surface collections which contained more than fifty sherds, as well as for each level in the stratigraphic excavations.

Then a roll of graph paper marked with a centimeter-millimeter grid was secured. On a piece of this paper a "key" was prepared very carefully. This key indicated the position of the axis of each type from which bars showing the relative frequency of the types were to be drawn. The best spacing of the types along the key was something that had to be developed in the course of the analysis to prevent overlapping of the frequency bars. The arrangement was changed several times, and its final form is as given at the tops and bottoms of figures 17-21.

After the first key was worked out, the type frequency data for each collection was placed on a 5-centimeter-wide strip graph. This second step was also a routine mechanical matter and took some time to accomplish, particularly as this work several times pointed out defects in the positions of the types on the key. When the key was changed, all strip graphs made with the old key had to be discarded. Finally, however, all of the classification data was in this graphic form.

While this work was underway, the classification data was being analysed in another way by several student assistants⁵ at the American Museum. This was a distributional study of type frequencies. For each type a sheet of tracing cloth was placed over a map showing all site locations. Then, the percentage frequency of the type at each site, say

⁵Miss Margaret Rose, Miss Eileen Boecklen, and Mr. Gary Vesalius.

Mulberry Creek Cord-marked for example, was recorded in its proper geographical position on the traced map. Now, if the above-discussed assumptions are correct, that the average village site was inhabited for a relatively short period (see assumption B, above), and that our Survey work has gathered a sample of the material from sites representing each time period in all parts of the area (F, above), then in each part of the Survey Area there should be sites which show Mulberry Creek Cord-marked near or at its popularity peak. Other sites, which cover time ranges before or after the maximum popularity of the type, will, of course, show their occurrence in smaller percentages. With all of this in mind, the completed distribution maps of Mulberry Creek Cord-marked were inspected with particular attention to maximum occurrences. It was seen that it would be possible to draw lines which would enclose maximums in descending order, after the fashion of contour lines (see figs. 6-14). If we wished to coin a new word and help our science to become more profound, we might call these "Iso-ceramic Lines" — but let's not.

These distributional studies made plain something which we knew already from classifying the material: there would be both quantitative and qualitative variation at all time periods in the different parts of the Survey Area. They also showed something else which we had suspected would be true. Regional specialization tended to increase with the passage of time so that late complexes from the northern and southern ends of our Survey were more unlike than were the early. This is a common phenomenon for cultures at this stage of development and seems to be owing to factors such as decreased population mobility due to an increased dependence on agriculture; the establishment of more stable centers, such as ceremonial mound groups and towns; and an increase in the cohesion of political groupings made possible and necessitated by the improved food supply and consequent population increase; to which was added the increased availability of cultural ideas which could be combined to form "new" varieties.

With this data in hand, it was decided that the practical way in which to treat the chron-

⁶Brown, 1926, pp. 288-319.

ology of the Survey Area would be to divide it up into sub-areas based on the differences that could be observed in the material of the latest time horizons. A chronological column could then be worked out for each sub-area and comparisons between the areas could be made at the different time levels. We realized that the procedure which we were adopting was fully as arbitrary, and indeed was of the same kind of high-handed ruthlessness as were our decisions in regard to ceramic classification. We are again preparing to set up artificial boundaries, which this time are geographically defined, and draw the borderline cases back toward the selected concepts.

From the beginning, the Lower St. Francis River area in Arkansas looked like a "natural" for a "Focal Grouping." Here are a number of highly similar sites, already known in archaeological literature (Parkin, Rose Mound, etc.), that seemed to stand off by themselves. This happy condition was improved by the fact that Survey work was not extended very far up the St. Francis River above these sites, so we were ignorant of any gradual transition toward any different-appearing complex in that direction. All the arbitrary decisions which would trouble us lay to the south and east. Ignorance and a classical tradition; it couldn't be better. We immediately set up a Lower St. Francis area and accepted the sites in quadrangles 11-N and 12-N as appropriate for starting chronological analysis.

The second area also looked good. Its literary background is provided by Calvin Brown's description of the material from the Walls Site⁶ near Memphis. The material from this and a number of closely related sites differed in a number of respects from the typical St. Francis area complex, as we have abundantly shown in Section IV. That this distinction proved to be partly due to difference in time does not lessen the initial lure of the situation. A *Memphis area* was defined and the sites included in quadrangles 13-O, 13-P, 14-O, and 14-P were taken as nuclear for starting the analysis.

We had a little more difficulty about the other three areas which were eventually set up. The literary background did not focus our attention so effectively, and we knew a

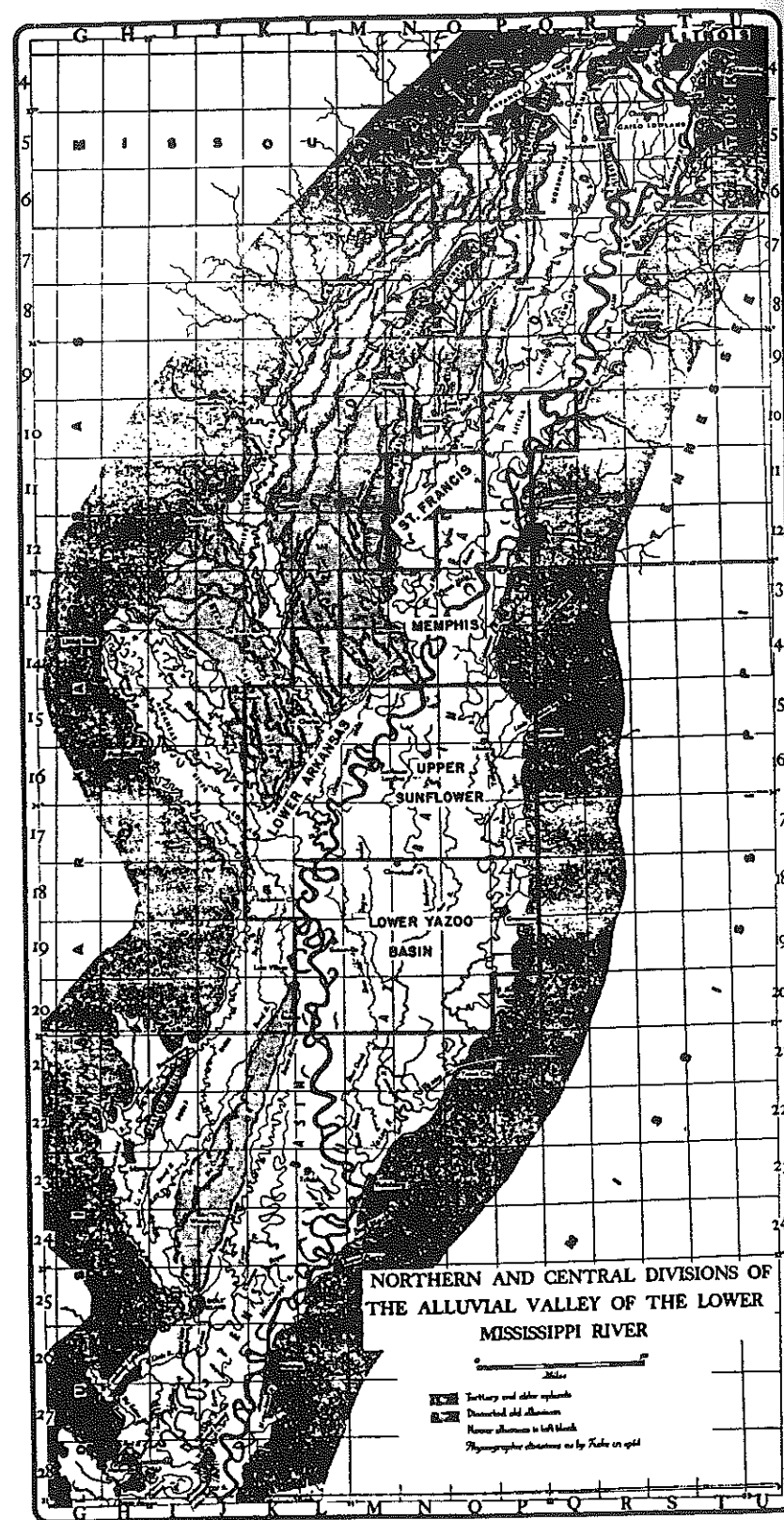


FIG. 16. Subdivision of Survey Area into analysis units for purposes of seriation.

little too much about "transitional" sites and material. After several false starts the following areas and beginning quadrangles were selected (fig. 16):

St. Francis Basin	11-N, 12-N
Memphis area	13-O, 13-P, 14-O, 14-P
Sunflower area	16-N, 16-O, 17-N, 17-O
Lower Yazoo Basin area	19-M, 19-N, 20-M, 20-N
Lower Arkansas River area	16-K, 16-L, 17-K, 17-L

It must be emphasized again that these areas have been set up solely for purposes of seriation and are therefore not to be confused with "foci" in the Midwestern taxonomic sense, or any other sort of cultural grouping.

It will be seen that the starting quadrangles for each area are geographically separated from the starting quadrangles of the other areas. This was intentional and was for the purpose of emphasizing the differences. The borderline cases were dealt with later as will be described.

By the time the study had reached this stage, we already had at hand considerable information as to the outlines of the ceramic chronologies in the region. One source of information was the sequences which had been worked out in the adjoining regions by Webb and his associates in northern Alabama;⁷ Jennings along the Natchez Trace Parkway in north-central Mississippi;⁸ Ford and his co-workers around the mouth of the Red River in Louisiana. A second very essential source of information were the stratigraphic excavations made by Phillips and Griffin, described in detail in a later section of this report. These revealed portions of the ceramic histories which could be used as partial backbone for the area chronologies. Our third source of information was the preliminary seriation analyses which we had made while classifying the site collections. So we had a rather good idea as to the relative time positions and distributions of many of the ceramic types. Despite this, the analytical procedures described here were followed out in detail, so far as possible, as though we had been completely innocent of such fore-knowledge.

Five sheets of heavy paper about 48 inches long and 20 inches wide were laid out on a large table side by side. The 20-inch width

⁷ Webb and DeJarnette, 1942.

of these sheets corresponded to the length of the strip graphs which recorded the type frequencies of each collection. Each of these sheets was headed with the name of one of the seriation areas, and they were placed on the table in the geographical relation of the areas from north to south. Then all of the strip graphs that represented collections from sites included in the quadrangles that served as the nucleus, or starting point, were separated out and placed on the appropriate sheet. The strips were laid horizontally across the sheets and were held in place at the edges by paper clips. As they were arranged and rearranged, particular care was taken to see that the type axes coincided.

We were now ready to begin the search for the quantitative patterning of pottery types, which for reasons that have been discussed in the foregoing, should exist in the area chronologies. This work was started with site collections of the Lower Yazoo Basin area (see fig. 17). These were relatively easy to seriate as two stratigraphic excavations were available to serve as guides for part of the history. The deepest of these excavations, Jaketown (20-O-1) Cut A, had fourteen levels and seemed to cover the greatest range of time. Accordingly, the strip graphs representing these levels were arranged on the sheet in the order in which they had come from the ground and immediately showed the frequency patterning for the time covered by the cut. The strips representing the second strata excavation, Shell Bluff (19-O-2) Cut A, were next put in place. The graph of the top level of this cut was slid along the sheet of paper until a point was found at which all its type frequencies best fitted the corresponding frequencies of the Jaketown cut. It was clear that the second level at Shell Bluff was older than the top level, but we could not know how much older it was in relation to the picture given by the Jaketown cut. Consequently, the second-level graph was placed below the first and slid downward until the best fit was secured.

Vertical arrangement of the material in the ground gave some control over the collections from the stratigraphic pits, and we knew that the collections from the lower levels had to

⁸ Jennings, 1941.

be older than those from the upper. However, for the surface collections we had no such guide. All we had was our assumption that the majority of these surface collections represented relatively short spans of time (see B, above) and the logic which led us to think that a quantitative patterning must be there.

The surface-collection graphs were taken one at a time and compared to the beginning that had been made with the stratigraphic material. If they fitted somewhere along the time represented by the excavations, the graph was fastened down to the backing sheet with paper clips. If percentages of such late types as Neeley's Ferry Plain and Bell Plain were too large, and proportions of such older types as Baytown Plain, Larto Red Filmed, and Mulberry Creek Cord-marked were too small, the collection was obviously later and the graph was placed above the excavations. These surface-collection graphs were shifted about in vertical relation to one another until patterning was developed as is shown in the upper part of figure 17.

The data from the starting quadrangles of the other four seriation areas were dealt with in a similar fashion, figures 17-21. Where stratigraphic information was available, it was used as a guide. Where there was none, the surface-collection graphs were shifted about to develop the best patterning that could be secured. In this way the five chronological columns were developed side by side.

The next phase of the analysis was to assign the sites in the intervening quadrangles to one or another of the five areas which had been set up. All of the site-collection graphs for each of these remaining quadrangles were seriated and then compared to the five area graphs. For example, the chronological patterning of quadrangle 18-M looked more like the chron-

ology begun for the Lower Yazoo area than any of the other sub-areas, so the collections from this quadrangle were fed into the Yazoo graph at the points where they fitted best.

Now, the area chronological graphs were virtually complete and good patterning of types could be seen. Apparently, our assumption that most of the surface collections represented relatively short lengths of time was correct. But while the majority did, some obviously did not. In a number of collections, early and late types were associated together in a fashion that showed either that the sites had been occupied for a long time, or there had been reoccupation. In order to clear up the patterning, the strips representing these collections were taken out. The numbers of these long time-span collections as compared to the shorter-lived sites that are used in the final graph are as follows:

AREA	NUMBER OF SHORT- TIME-SPAN SURFACE COLLS. USED IN	NUMBER OF LONG- TIME-SPAN COLLS. TAKEN OUT TO
	FINISHED GRAPH	CLARIFY GRAPH
Lower Yazoo Basin	48	1
Lower Arkansas River	19	1
Sunflower	81	9
Memphis	66	7
St. Francis	37	0
	Colls. used in graphs 251	Discarded 18*

Although eighteen surface collections with respectable sherd totals have been eliminated from the graphs because of the special requirements of this kind of analysis, this does not mean that the effort devoted to these sites has been lost. It may be expected that these are places where rather long spans of history may be examined in stratigraphic relation, if there is any depth to the deposits. So far,

16-O-14 (Stover)	110
16-O-17 (Longstreet)	160
17-O-11 (Cassidy)	249
16-O-1 (Dunn)	94
16-P-6 (Cox)	144
Memphis	
10-P-3 (Nettle Ridge)	477
10-Q-3 (Turnage)	328
14-N-6 (Helena Crossing)	80
13-P-4 (Dogwood Ridge)	354
13-P-10 (Irby)	1381
11-P-3 (Golightly Place)	241

*The full list of site collections excluded from the seriation graphs is as follows:

	SHERD TOTAL
Lower Yazoo	
20-O-1 (Jaketown)	4226
Lower Arkansas	
16-L-3 (Stovall)	218
Sunflower	
17-N-16 (Wilnot)	244
16-P-7 (Mitchell)	418
16-P-5 (Crosslyn)	127
16-P-1 (Charleston)	646

tests have been made in one of these sites, 20-O-1 (Jaketown), the results of which are discussed in the section on Stratigraphy (VI). It was quite evident why surface collections from this site were useless for seriation purposes; the occupation covered practically the full range of ceramic history in the area.

Handling of the Data from Stratigraphic Excavations

The incorporation of the data from the stratigraphic excavations into this analysis was done in a purely arbitrary fashion. Each level was treated as though it were a separate surface collection from a distinct site, except for the fact that care was taken to keep the levels in proper vertical order. The relation of stratigraphic levels to the soil profiles revealed by the walls of the excavations, which is discussed in detail in the next section of this report, was not worked out at the time this analysis was made, but had it been available would not have received consideration in this phase of the work. The seriation of the data in these five sub-areas was an attempt to discover the chronological patterning of the pottery types in each region and to reveal the consistency with which the types followed that pattern. In this handling of the data it was expected that such anomalies as the reoccupation of sites after they had been abandoned for any considerable length of time would be revealed by comparison with the evidence given by neighboring sites as to the chronological pattern of each sub-area.

There are some discrepancies between the interpretation given to the stratigraphic data in this section, written mainly by Ford, and the section on Stratigraphy which follows, written by Phillips. These disagreements are not basic differences as to the gross outlines of the chronology; there are no differences as to this. They have to do principally with the problem of whether the evidence indicates that there was a break in the deposition between the Baytown refuse characterized by clay-tempered pottery and the shell-tempered Mississippian deposits. In most cases this involves a question as to whether late Baytown (period D-C) or the early Mississippian Phase (period C-B) is missing in the strati-

graphic sequence. With the evidence which we have at present it does not seem possible to resolve these discrepancies to everyone's satisfaction, so we will allow them to stand. However, they can be explained by the fact that Phillips' judgments have been based on detailed examination of the internal evidence supplied by each strata cut while the guesses of Ford have attempted to reconcile the evidence given by both surface and excavated collections.

Co-ordinating the Area Chronologies

We are now in possession of five quantitative graphs representing the ceramic history of the five selected areas. However, these are relative histories. There is no absolute chronological scale by which the appropriate amounts of vertical spacing, which represents time that should be given to the early, middle, or late portions of each can be measured. The best that can be done is to try to correlate them one with another. This was done in the following fashion. Six strings, spaced and running parallel, were stretched from end to end of the table on which the graphs lay. Then portions of the graphs were adjusted up or down until the same types showed comparable relative quantities under the appropriate string. Thus, the third string down from the top, which has become line C on the time scale used in the finished drawings (figs. 17-21), was made to mark the point in each graph where Baytown Plain and Neeley's Ferry Plain were about equal, Mulberry Creek Cord-marked had practically disappeared, Bell Plain was just getting a start, and Larto Red Filmed was almost gone. In each case this procedure was a compromise. If the upper portion of the Lower Arkansas graph had been slid downward until all the percentages of Bell Plain were equal to those in the Sunflower and Memphis areas along the C horizon line, then the Baytown-Neeley's Ferry relationship would have been all out of adjustment. All the type patterns were considered in this correlating process and the A to G time-scale arrangement given in the five final graphs is the end result of many compromises. So this scale is presented as a time framework for the chronologies. Time F in the Yazoo area, for

example, is supposed to be the same as F in the Lower St. Francis.

The necessity for compromises of this kind was not unexpected. As a matter of fact, they are an inherent part of this kind of cultural analysis. The groups of ideas to whose products have been tagged such names as Mazique Incised did not spring up simultaneously all over the area. They moved from one part to another, and that took time. For example, the ideas of red slipping on clay-tempered vessels (Larto Red Filmed) apparently was moving from south to north through the region, while cord-marking on clay-tempered pots (Mulberry Creek Cord-marked) was moving from northeast to south. Naturally, the former is earlier to the south and the latter to the north.

The student who is particularly interested in the history of this area, or of the procedure by which this balancing was done, may check it—if he has the time and patience—by placing the five area graphs (figs. 17–21) side by side and following across the relative time position of each type. This process has been a subjective weighing of the evidence provided by each type position and of course is always open to question. As a matter of fact, there has been considerable question as to certain aspects of this arrangement which should receive attention at this point. Griffin and Phillips are of the opinion that the late materials in the Arkansas area actually date somewhat later than they are represented in the graph of that area (fig. 18). They think that the pottery type Wallace Incised probably extends up to the time when the Quapaw were discovered by the French. This opinion is somewhat reinforced by the fact that the type is practically confined to the region in which the Quapaw were described and occurred in appreciable amount in the top levels of two cuts in the Menard Site (17-K-1), and on the surface of the near-by Wallace Site (17-K-3) which there is reason to believe may have been the site of the Quapaw

¹⁰ Moore, 1908a, figs. 8, 10, 19. Compare with Quimby, 1942.

¹¹ Griffin's reposit to this is simple. He thinks that the Yazoo and Sunflower columns also have their latest portions placed too early. More of the sites in those areas should fall after time B.

Phillips thinks that this is an instance where the

village of Osotouy (Uzutihi), first visited by the French in 1686 (see p. 414). As additional evidence, Clarence B. Moore excavated burials in the fields near the Menard Site that were accompanied by European material. Admitting that the cemetery excavated by Moore almost certainly is of Quapaw origin, Ford has hesitated to raise the upper part of the Arkansas graph for several reasons. First, to do so would also bring the types which accompany Wallace Incised up to a later date where their proportions would not be consistent with those of the same types in the neighboring areas. Second, Moore's illustrated material does not show any examples of the types Wallace Incised. However, this does not mean that he may not have found such vessels. The third and most convincing point (to Ford) is the fact that Moore does illustrate three vessels of the type Fatherland Incised, the pottery which the Natchez tribe farther down the Mississippi were making about A.D. 1700.¹⁰ In addition, he found "teapot vessels," another trait shared with the Natchez. Neither Fatherland Incised nor any of the late "Caddo" types with which it is normally associated appeared in the Survey collections from the Menard and near-by sites. While far from denying that this vicinity is the likely site of a historic Quapaw village from which Moore sampled the burials, it does not appear likely to Ford that the site collections and uppermost strata levels in our Arkansas area graph represent this historic occupation.¹¹

Comparison of the area graphs will show that the late collections in the Memphis area have been allowed to come up to the most recent times. This was practically forced by the large percentage of Bell Plain found on the surfaces of the late sites in that area. In contrast the other areas show much smaller percentages of this type as a very late feature. It is possible, as discussed in the next section, that a part of this Bell Plain is pot-hunter refuse or is burial ware which has been ripped

assumption of continuous distribution of a pottery type has played us false. Bell Plain, which carried the weight of identification of the late time, seems to have a discontinuous distribution in space. Therefore, according to this view, the near lack of Bell Plain in the top portions of the Lower Arkansas graph is not chronologically significant.

from graves by cultivation. However, the trends in accompanying types: decrease of Barton Incised, increase of Parkin Punctated, and the appearance of Rhodes Incised and Vernon Paul Appliqué, suggests that there is a certain consistency to this situation that makes the increase of Bell a significant marker of the passage of time in this area—whatever may be the factors involved.

It is thought that probably none of these columns extend to the beginning of reliable historic documentation about A.D. 1700. This is consistent with the fact that the French explorers of that period indicate that the population of the Mississippi flood-plain area between the mouth of the Yazoo River, where villages of Yazoo and Tunica were found, and the northern limits to which our Survey has extended was very scanty indeed. About the mouth of the Arkansas River were found the Quapaw or Arkansa, and those are the only people who can be placed with any certainty. In the upper drainage of the Yazoo were the Tiou,¹² Chakchiuma,¹³ and Ibitoupa.¹⁴ Swanton estimates that the total of this Upper Yazoo population was less than 1000 people.¹⁵

This is far from enough people to account for the number of sites which we have dated as occupied during the later Mississippian period, and, in fact, is markedly in contrast to the population picture given by the De Soto narratives for the year 1542 as will be shown in a later section.

Clarence B. Moore found burials accompanied by glass beads and other European material at several sites through the area we have surveyed.¹⁶ The pottery which he illustrates from the Rhodes and Bradley Places is clearly of late Memphis area types but, as Moore's report does not associate the illustrated materials with the burials that are described, it is impossible to state definitely that the European material was found with this complex. Even if it is associated with it, it should be noted that the possibilities for the aborigines acquiring glass beads probably go back some-

what before 1700 in this area, if not back to the period of De Soto's exploration in 1542.

There is some reason to expect that the ceramic complex which prevailed at least as far north as the Sunflower area in 1700 had a small percentage of incised pottery resembling in both decoration and shape the historic Natchez-type Fatherland Incised.¹⁷ It has already been pointed out that Moore found a small proportion of this type associated with European material near the Menard Site. Charles Peabody's excavations in the Oliver Site in our Sunflower area produced at least one vessel of this type.¹⁸ Again, the association with the European material which was found in some quantity cannot be determined from the report. However, the type did not appear in any of our late collections. Clearly, further search needs to be made for rare contact sites in the Survey Area with a view to determining the exact forms of the late ceramic complexes in the different parts of the region. Until this is done, it cannot be stated with certainty exactly when these columns end.

The finished area graphs are given as figures 17, 18, 19, 20, and 21. The collections are listed by site designations, 12-N-7, etc., down the left side of each graph. Collections which were made from restricted areas in certain sites are indicated as A, B, etc. (12-N-3A). The stratigraphic cuts made in certain sites are shown by staffs on the left side of the diagrams, and each level of such excavations is indicated with depth in centimeters. Each staff is shaded to aid in relating it to the corresponding type frequency bars given in the body of the charts.

The pottery types are represented by vertical "axes" which are labeled at both top and bottom of the diagrams. Equally spaced on either side of the appropriate axes are horizontal bars the length of which represents type percentages according to the scale given in the lower right-hand corner of the graph. It will be noted that only one-half of the full length of the frequency bars for the relatively

¹² Swanton, 1946, p. 194.

¹³ Swanton, 1946, p. 105.

¹⁴ Swanton, 1946, p. 140.

¹⁵ Swanton, 1946, p. 107.

¹⁶ See Moore, 1911, pp. 406, ff., Kent Place (our

13-N-4); pp. 413, ff., Rhodes Place; and pp. 427, ff., Bradley Place.

¹⁷ Quimby, 1942, pp. 263–64.

¹⁸ Peabody, 1904, pl. 14, line 4.

abundant types Neeley's Ferry Plain and Mulberry Creek Cord-marked has been shown. These types are arranged at the left and right-hand sides of the graphs, respectively, and this device has enabled us to decrease the over-all width of the illustrations.

On the right-hand side of each graph are listed the collection totals. These will indicate the amount of reliance that may be placed upon the samples. The time scale, A, B, C, etc., which relate the graphs to one another in the manner which has been described above, is on the right-hand side of each. These are the smallest time divisions which we have felt justified in making in the chronologies. The more comprehensive names which we are using Tchula, Baytown, and Mississippian are also given with the time range of each period indicated.

Explanations of complicated diagrams are tedious reading and frequently serve mainly to hide the essential simplicity of the scheme. The reader who is still confused at this point may be less so after comparing the following tabulation of types at Site 19-L-6 (Refuge) with the collection as graphed at the very top of the Lower Yazoo Basin area diagram (fig. 17).

19-L-6 (Refuge)

TYPE NAME	NO. SHERDS	PERCENTAGE
Neeley's Ferry Plain	304	.463 *
Baytown Plain	31	.047
Bell Plain	263	.400
Parkin Punctated	21	.032
Leland Incised	28	.043
Unclassified	9	.014 †

* Half of percentage shown in graph.

† Not graphed.

Discussion of the Seriation Technique

Such, then, was the analytical procedure followed in developing the area graphs, and some of the reasons why it was done so. The seriation of surface collections might have carried the full weight of the evidence for developing the chronological type patterning, but as some stratigraphic excavations were available in each area it did not have to. There is a tendency among some archaeologists to affect an attitude of suspicion and doubt in regard to the seriation technique, and it has

often been asserted that the results of such "juggling" cannot be accepted unless supported by vertical stratigraphy. It seems likely that such an attitude may arise from one or both of two sources: either a misconception of the phenomena of cultural change and the part that typology plays in measuring that change, or a lack of understanding of the seriation technique. As a matter of fact, both seriation and the vertical stratigraphic technique have certain advantages and defects under different conditions and must be applied to chronological problems with a careful regard for their limitations.

The chief limitation of seriation is the fact that it must work with degrees of probability which are often quite difficult to measure or even estimate. Usually, the measure has to be the pragmatic one of the results obtained. In our area, for example, any one or all of the probabilities stated at the beginning of this section may not have been true. The population may not have been relatively stable. There might have been sudden and frequent movements of populations so that the cultural change in any one locality would have had little semblance of order. Had this been true, we might expect either that the development of a sequence by this means would have been impossible, or that cultural periods would have been developed which were clearly delimited, one from the other.

It is also possible that a majority of the villages might have been inhabited for very long periods of time. If this had been true, it would have been impossible to separate early and late pottery features by surface collecting and seriation techniques. There is, of course, a degree of this kind of error in all of the samples which we have handled, and this is probably the principal defect of the technique. None of the collections are the instant cross section of the ceramic content of the culture at each site which would be the ideal situation. The fact that each of the surface collections does represent a time span of a certain length must, in theory, result in a certain "fogging" of the quantitative history. For example, if we assume that we have done a perfect job of sampling and classifying and have placed one of our strip graphs so that its vertical position cor-

rectly represents the mean date of the site occupation, then it is plain that this graph will represent the early types which were fading or perhaps disappeared soon after the site was first occupied, too high in the chronological scale. Conversely, the late types which belong to the latter part of the occupation are also pulled back to the mean position and show as too early.

Again, the occasional reoccupation of sites after a lapse of time might be a disruptive factor. It is even possible that there might have been at some periods the general custom of utilizing older sites. This also would result in our securing a mixture of old and new cultural materials and would invalidate our assumption for continuous occupation. Had this happened in a majority of cases, the odds are very much against there having been any consistent pattern to the selection of the earlier sites which would be utilized. Only in the event that a region had been cleared of a previous population by conquest, and the conquerors had moved in and begun to utilize the settlements and fields of the people whom they replaced, could there be any probability of a consistent sequence of types. In such a case the seriation technique would reveal the cultural chronology, but interpretations as to cultural and population continuity might be led astray. It is very probable, however, that there would be "pure" deposits of the late phase of the earlier occupation, and the early phase of the later, which would illustrate the break in cultural continuity.

We can also be certain that none of the collections show type frequencies to the exact percentage that would be found if every sherd at a site had been gathered and classified. For these reasons, we would like to say again that success in this type of work demands numerous collections, and the imperfections of the technique are such that the majority of the indications must be taken as evidence. Two or three sherds of a type that seems to be quite late in a surface collection from a site that by all other indications is rather early do not worry the seriator at all. There are too many ways in which such a chance mixture could have occurred. He is more concerned by the fact that the overwhelming majority of the sherds of this type take a late position, and that

the preponderance of the material from the site fits into the early ceramic pattern. Add to all this the uncertainties of classification which we have outlined in a foregoing section, and it is easy to see why we would like to stress the fact that success in this type of work demands a number of fairly sizable collections, and that only indications given by the majority of the situations must be accepted as evidence.

The Use of Stratigraphic Data in Seriation

The analysis of stratigraphic data as such will be discussed at length in the following section. Here we are concerned principally with the use of stratigraphic along with surface collections in the seriation technique and their limitations from this point of view.

Phillips and Griffin in the 1941, 1946, and 1947 field seasons made a total of seventeen stratigraphic excavations at nine different sites. All of these gave the anticipated results and showed evidence of change in type frequencies with the passage of time. Of these, fourteen were clear-cut enough to be incorporated in the area graphs and three could not be used for reasons that are explained below. This high degree of success in the effort to obtain this type of evidence was directly due to a careful selection of sites to excavate. Before beginning, each excavator had a fairly clear notion as to at least a part of the chronological patterning which the site would reveal.

The principal defect, from the point of view of seriation, in the information provided by stratigraphic excavations is a result of what might be termed migration, particularly upward migration of material in midden deposits. This is most pronounced in middens in which refuse and soil was accumulated very slowly. Apparently, the activities of the Indians who lived on such sites, the digging of post-holes and pits, and overturning the soil in other ways, has tended to bring old pottery and other refuse to higher levels in the growing deposit. This is particularly true of the later Mississippian horizons. Analysis of stratigraphic studies in such deposits make the older type appear to have lasted much longer than really was the case. This factor is doubtless always present in the analysis of all midden deposits. Usually, how-

ever, the distortion of the graphs is so small that it falls well within the limits of the variations that have to be allowed in this kind of analysis.

The control which we have over this accidental upward weighting of midden-deposit evidence is the comparison of such unusually slow-growing cuts with the results of other excavations in the same area. A still better check is the comparison of these cuts with seriated short time-span surface collections.

The most pronounced example of upward migration which we have encountered in this study are the two strata cuts that were made at Lake Cormorant (13-P-8). These are described on pages 249-52. The site is located in the Memphis area and the excavations revealed about 120 cm. of refuse deposit, the material from which, when analyzed, proved to represent the entire ceramic chronology for the area from time G to A. All of the types found in the area are well represented, for the collections from each level were substantial. The popularity peaks of the types form a pattern which is in perfect agreement with the seriation graph of the Memphis area as a whole as can be seen by comparing the stratigraphic and seriation graphs (figs. 25 and 27 with fig. 20). However, if we were to accept the evidence offered by the Lake Cormorant Site we would have to believe that the types Withers Fabric-impressed and Baytown Plain were still being made in time B to A. All the other sites collected from the Memphis area by both the surface and stratigraphic techniques show that this was not so. We conclude then that these older types in the Lake Cormorant Site have been brought up to the surface of the midden by overturning of the soil. For this reason, it has not been possible to incorporate the Lake Cormorant data in the Memphis area graph.

The second phenomenon found in strata-cut tests is that at times they misrepresent the history of the site being studied by completely skipping or being deficient in the material that represents certain spans of time. The reason for this is not difficult to find. While a village was occupied, the midden material accumulated at any one spot only so long as it was being actively deposited at that place. In the Southwest, where intentional dumps were utilized

or in Peru where substantial buildings of stone and adobe were occupied uninterruptedly, there was little reason to change the locales of garbage disposal. However, in the eastern United States the houses were impermanent structures of wood, and from the excavation of numerous sites it is clear that considerable shifting of house locations was done in rebuilding. Thus, it may happen that one of our strata pits was put down at a spot where a house stood for the first third of the time the village lasted; was rather far from any dwellings during the second third; and was again near a house during the last third. A graph of the type frequencies will — if it is clear enough — show a definite shift in percentage frequencies at the level where deposition paused. The same thing will result if the pit chanced to pass through a house floor or a courtyard which was intentionally kept clean of debris.

The Question of Population and Cultural Continuity

One of the most interesting questions raised in the interpretation of the data which we have to present is whether there are indications of cultural and, by inference, population discontinuity between the Baytown and Mississippian periods. This has an important bearing on the matter of how and where did the Mississippian cultures develop, the major current mystery of Eastern archaeology. Did the Mississippian culture come into the Survey Area from outside, carried by a new population in such a way that there was a distinct break in the cultural sequence, or was there a period of gradual but possibly rapid cultural change at the beginning of this period when new cultural ideas (carried perhaps by some intruding people) came into the area and merged with the Baytown. We cannot pretend to settle this question, for our data are confined to ceramics. However, the ceramic histories and the villages that have been investigated give enough evidence to permit some discussion. This discussion centers about the more specific question of whether reoccupation has occurred on these sites where the shell-tempered Mississippian pottery complex is mixed with the clay-tempered Baytown ware. A glance at the five area

graphs will show that there are a substantial number of such sites in each sub-area, most of them represented by surface collections and a few by stratigraphic excavations. Do all of these sites represent reoccupation?

There can be little doubt that reoccupation is represented by some of these collections. These sites where an early Baytown complex is mixed with shell-tempered pottery, such as 14-O-1 and 14-O-2 near the bottom of the Memphis area graph (fig. 20), seem to have a thin Mississippian occupation mixed with early Baytown, with material of the intervening periods missing. Some of the surface collections excluded from seriation may also be interpreted in this way. Also, there may be some examples of reoccupation where the time during which the site was unoccupied was so short that it is impossible to measure it in cultural terms. The real question is whether the majority of mixed sites represent reoccupation. If site reoccupation were the explanation for this mixture, it might be expected that late Mississippian material would be mixed with early Baytown pottery about as often as occupations of the early part of the Mississippian chanced to be placed over late Baytown refuse. The early Baytown sites are in just as favored geographical locations as the late, and there is little reason why these spots should have been avoided by the later invaders. In this event, little or no patterning would appear in either the attempts at seriation or in the strata excavations. However, there is also the possibility that the later people conquered the territory and settled down to use the cleared fields and villages of those whom they had displaced. The techniques applied here would not be able to clearly detect such an event. Even if this somewhat unlikely kind of population replacement had occurred, it is probable that there would be some early Mississippian villages which were established in new, unoccupied spots which would not have the late Baytown mixture, and conversely some of the conquered late Baytown villages which were not reoccupied, and thus did not show the early Mississippian mixture. There are several sites which may be interpreted in this way such as Collins (13-O-9), of the late Baytown in the Memphis area graph (fig. 20), but the number

is small. The patterning revealed by the majority of the site collections indicates to one of the present writers at least (Ford) that there was essential continuity of the ceramic complex and, by inference, of the majority of the population.

Another and parallel approach to this question of continuity lies in an examination of the possibility of certain ceramic decorations which are found on clay-tempered pottery being directly ancestral to similar decorations on the shell-tempered wares. This will be treated elsewhere, and it is sufficient to say here that this evidence does not suggest that there has been a cultural break.

Relative Dating of Village Sites

The foregoing was the analytical procedure which was directed toward the development of the five area chronological columns (figs. 17-21). Now, we call attention to the fact that in the analysis process we have also provided relative dates for the collections studied. The vertical positions in which the collection graphs have been arranged in the five chronological columns show the relative mean dating of these collections. However, it must be emphasized that this is a *mean* or *average* date. As has been mentioned above, each of these collections represents refuse which was in the process of deposition for a shorter or longer period of time — 10, 25, 50, or 100 years, we do not know. There is no external evidence which can be used to resolve this uncertainty. We are aware that what has been done is to "flatten out" the cultural evidence which accumulated during the occupation span that each collection represents and treat the collection as though it were a cross section of the cultural content at one moment in time. If our analytical operations were perfect, we might expect that the time at which the collection best fitted in the chronology would be about the mid-point of the period through which the refuse was accumulating. This is the reason for the term "Mean Date" which will be applied to the graphed time position of the collections.

Frequently, there is in the collections some evidence on which a judgment of the relative time span represented may be based. The presence of types which are chronologically

earlier or later than the mean date may indicate approximately how far the time span of a site extended from its mean date. This evidence has served as a basis for the judgments of the time spans of site collections listed under the heading "Range" in table 1. The majority of collections, it will be noted, are listed as falling within one of our lettered subdivisions. These are collections which show no evidence of any long period of occupation and which seem to be about as homogeneous in content as is the usual 10-centimeter level of a stratigraphic cut in this part of the Mississippi Valley.

The above discussion has reference, it will be noted, to the dating of collections. The question as to whether a collection completely and fully dates a site is another matter. There is always the possibility that either (1) only the top and latest refuse is on the surface of the site, or (2) earlier refuse is on the surface but at some point which was not investigated. There can be little doubt that we have made this error in the dating of some sites, but we suspect that the proportion will be quite small. The principal reason for thinking so is that refuse deposits that extend below the plow zone are not common. Numerically, there are more of these deposits than has generally been supposed in the Mississippi Valley, but the proportion of deep to superficial sites is undoubtedly small. The second reason is that this possibility was kept in mind during the course of the field work, and as far as possible all sites were examined to see if areal differentiation of material could be detected. In these cases localized collections were made. Thus, while we cannot say with complete confidence that site "X" is fully dated by its surface collection, we are fairly well satisfied that the great majority of the mean dates do not suffer from serious error of this kind.

An interesting comparison can be made between the graphed positions of surface col-

²⁰ Note that the graphs of these two surface collections show mixture of both early and late types, a condition that is clearly explained by the length of time represented in the deposits as shown by the stratigraphic excavations. Their lessened value for giving a clear seriation is obvious, and possibly they should have been excluded from the graphs as were the 18 long time-span surface collections described

lections from certain sites and the later stratigraphic excavations in these same sites. Although Ford insists that at the time these collections were being seriated he paid not the slightest attention to site designations but concentrated on type frequencies, the reader had best judge the appropriateness of each position for himself.

On the area graphs, we make the following comparisons:

- 19-O-2, general surface collection with 19-O-2 strata cut (fig. 17);
- 17-K-1, a general surface collection with the two strata cuts made on the site, A and B (fig. 18);
- 17-L-1B, a localized surface collection with strata Cut A, made in same part of the site (fig. 18);
- 17-L-1C, a localized surface collection with strata Cut B, made in the same part of the site (fig. 18);
- 16-N-2,²⁰ a general surface collection with the two strata cuts A and B made in old and younger parts of the site (fig. 19);
- 16-N-2B, a localized surface collection with strata Cut B, made in the same part of the site (fig. 19);
- 16-N-6,²⁰ a general surface collection with the three strata cuts made in this site, A, B, and C (fig. 19).

The Walls Site (13-P-1) and the Rose Site (12-N-3) are the only cases where such collections fit in the graphs at the upper end of the time span indicated by excavations in the same sites (cf. figs. 20, 21). When the fact is recalled that the sites enumerated were selected for excavation partly on the basis of their showing a depth of midden deposit, and that these depths ranging from 75 to 240 cm. are exceptional rather than the rule on sites in this region, it can be seen that the chances are rather good that we have secured samples representing the full time range of most sites. The problem of buried strata can virtually be ignored so long as we are considering the *majority* situation.

However, this slight degree of doubt which

above. However, they are included here both to illustrate this effect and to point out the tendency of these surface collections to take a position intermediate of the time range of the site. The surface collection from site 20-O-1 (fig. 17), another long time-range site, would have illustrated the same condition, but was not included, as explained above.

must be admitted for the fullness of the site-dating shown by any particular surface collection has no bearing at all on the validity of the quantitative-chronological patterning which derives from the seriation of these

collections. The probabilities are still in favor of each collection representing a continuous segment of time, whether this segment be only the latter portion of the length of time any one site has been occupied or not.

Seriation and Stratigraphy

BEFORE embarking on a detailed cut-by-cut analysis of the stratigraphic excavations conducted by the Lower Mississippi Archaeological Survey, it will be well to clarify further the relationship, from the standpoint of method, between seriation and stratigraphy as used in the present study. In a sense we have to regard them as independent methods of analysis. This is perhaps less a result of choice than necessity, owing to the circumstances that the seriation analysis was done in New York by Ford in 1947 and the stratigraphic analysis by Phillips in Cambridge in 1948, both assisted by long-range advice and criticism from Griffin in Ann Arbor. Ford made significant use of stratigraphic data in the seriation by interpolating strata with surface collections as explained on page 228 and shown in figures 17-21. Wherever practicable, the strip graphs representing individual levels in strata cuts were laid down first as a guide to the ordering of surface collections, so it might be more correct to say in such cases that the surface data were interpolated into a stratigraphic framework. The results in our opinion fully justified this unorthodox combination of two distinct kinds of data.

However, it might be pointed out that this is not stratigraphic analysis *per se* and cannot take its place entirely. It was done, in fact more or less has to be done, without regard for what may be called the ground context, i.e., without consideration for the sometimes complex relationships of pottery-type frequencies to special conditions on the site. It is the latter "tied-down" sort of interpretation that will be described in the present section. That the results will always tally with the broader patterning produced by the seriation technique is hardly to be expected, and in fact they do not. Instances of disagreement will be noted as we come to them, and a general discussion of their over-all significance will be given in the concluding remarks at the end of the section. It may be asked why we do

not resolve — "hide" would be a better word — such disagreements by adjusting the discrepancies on the seriation charts. Such a procedure would carry the implication that stratigraphic analysis is a more "accurate" and sure method of dating than seriation, which is by no means certain. We must not lose sight of the fact that interpretation of a stratigraphic cut is accurate, assuming that it is accurate, for that cut only. In attempting to extend that interpretation to the site as a whole, or beyond, we have to make use of the same concepts of patterning upon which the seriation technique is based. No, we may as well admit that, in most cases of disagreement, we simply do not know which method has produced the correct interpretation and let both stand — for the present. Fortunately, the discrepancies are not serious. Given the conditions of physical separation under which the two analyses were carried out, and the not identical points of view of those who carried them out, the extent of agreement is a matter for self-congratulation.

The Chronological Framework

It cannot be too strongly emphasized that we have not yet reached the stage of having, or even requiring, a cultural typology in the Survey Area. In fact, we have not yet taken the first step toward such a typology, the determination of significant cultural units or "foci," if you prefer the term. The nomenclature used in this and all other sections of the present report is derived solely from the seriation analysis described in the preceding section. As may be readily seen by reference to any of the charts in that section, the scheme consists of six time divisions set off by the letters A to G, which are in turn grouped into three periods, Mississippi, Baytown, and Tchula. Thus we have two different ways of designating time position, by the use of letters or by the use of names. Both are subject to the ambiguity that attaches to most archaeological designations. They refer primarily to time but have cultural, or better say, since we

are dealing with pottery alone, typological implications. As an example of such limited typological implication, it will be recalled that C on the seriation time scale, representing the division point between the Mississippi and Baytown periods, was arbitrarily fixed at the point where Baytown and Neeley's Ferry Plain were approximately equal in percentage frequency; in other words, at the mid-point of a theoretical transition from the use of clay- to shell-tempering in the area. It would be vain to assert, therefore, that the distinction between Baytown and Mississippi is wholly chronological and in no sense typological.

Such being the case, we have to guard against confusion between our period designations and typological concepts already in use. Fortunately, the archaeology of the earlier periods, Tchula and Baytown, has not been sufficiently dealt with in this area to have become seriously involved in concepts of culture type. That of the Mississippi Period, however, has long been identified with the generic concept "Middle Mississippi," which, in our understanding, is primarily a typological concept, not without chronological implications to be sure, but fundamentally a concept of culture type. As indicated above, our studies have not reached the stage of total (archaeological) culture analysis leading to the delineation of "cultures" or culture types. Furthermore, the pottery of the Mississippi Period in our area, while included within the general definition of Middle Mississippi pottery, does not appear to exhibit its full typochronological range. Except as faintly foreshadowed in one or two stratigraphic components to be described presently, we have not found anything typologically comparable to early Middle Mississippi pottery as exemplified elsewhere in Hiwassee, Old Village, and related foci. We have, therefore, deemed it advisable to substitute for Middle Mississippi the more general term Mississippi, carrying (we hope) a minimum of cultural and typological implication. This involves us in certain terminological difficulties, which must be made as explicit as possible. Like Middle Mississippi our Mississippi Period is also di-

vided into two parts, early and late, C-B and B-A, but this is an arbitrary and strictly chronological division, as devoid of typological significance as can be, being based on differences in percentage frequencies rather than differences in constituent types. If, in the interest of clear expression, it becomes necessary to refer to the "early portion of the Mississippi Period" or simply "early Mississippi" it is not to be understood that this means that the local early Mississippi material dated between C and B is typologically correspondent to what has been referred to as early Middle Mississippi in other parts of the Southeast.¹ If this point has been made sufficiently clear, we should be able to proceed without undue misunderstanding and without raising the sort of questions with which we are not yet prepared to deal.

Definitions: Stratification and Stratigraphy

As outlined in the introductory section of this report, the original over-all Survey program contemplated three successive stages of investigation: (1) preliminary site survey and analysis of surface collections; (2) stratigraphic testing; and (3) site excavation. To date, only a slight beginning has been made on stage (2). To what follows in this section, therefore, the word "preliminary" is more than usually applicable. The results, however, justify a rather detailed presentation, not only for their bearing on the immediate problems, but from the broader point of view of archaeological method in the Southeast.

The use of stratigraphic methods in the eastern United States has not yet developed to an extent comparable with their use in other areas of American archaeology. This is mainly due to an earlier impression on the part of Eastern archaeologists that the method was not applicable, owing to the paucity of deep deposits yielding long cultural sequences. It is also partly due, perhaps, to a misconception regarding the stratigraphic method. To many archaeologists, stratigraphy necessarily involves a situation in which materials can be segregated on the basis of distinct and separable soil zones. Such is fortunately not the

¹ Ford and Willey, 1941; Griffin, 1946, fig. 6.

case. It frequently happens, as we shall show, that a homogeneous deposit, without observable soil stratification, may be made to yield a stratigraphic record of the utmost value. Obviously, such an unstratified deposit will have to be excavated by arbitrary levels, to which method the term "metrical stratigraphy" has sometimes been applied in derogation,² as opposed to "natural stratigraphy" obtained by peeling stratified layers. If we were to regard "natural" stratigraphy as the only valid method, the discouraging outlook referred to above would be justified. On the other hand, unstratified or weakly stratified midden deposits of sufficient depth for excavation by "metrical" analysis are not rare. An example of successful exploitation of such deposits is to be seen in the excavations of Willey and Woodbury on the Gulf Coast of Florida in 1940.³

There is no need for injecting this terminology into the present discussion, since our stratigraphy—so far at least—is all of the metrical variety. The distinction, however, between "stratification," the description of the actual ground situation, and "stratigraphy," as applied to the chronological interpretation of the ground situation, whether by "natural" or "metrical" methods, is a useful one and will be maintained here. Under the heading "stratification," we shall refer to soil zones as revealed by trench profiles; under "stratigraphy," the analysis of the excavated material and interpretation of the results. The one is what you find, the other is what you do with it. The separation will serve to bring out the fact that it is possible to have stratigraphy without stratification and *vice versa*. In line with this distinction, the terms "stratum," "zone," "deposit," etc., will be hereinafter used to refer to the ground stratification, the term "level" being reserved for the arbitrarily excavated unit of "metrical" stratigraphy.

Methods of Excavation

The simple methods of stratigraphic testing used by the Lower Mississippi Archaeological Survey may be described very briefly. Vil-

²In the Southwest. Information by J. O. Brew.

lage site deposits in the Alluvial Valley rarely exceed 1 to 2 meters in total depth. Ten centimeters was therefore chosen as a unit of depth, convenient for seriating, without presenting serious difficulties in excavating. The first cut (Walls A) was dug 3 meters square, but on finding that a sufficient yield of sherds could have been obtained from a smaller area, subsequent cuts were made only 2 meters square. Ideally, cuts should be dimensioned to get an adequate sherd sample per level from the smallest possible space, but we could never agree as to just what constituted an adequate sample, and therefore adhered to the convenient 2-meter square throughout. In only one instance (Rose A) was the yield per level below what we should have liked.

Field procedures were of the most elementary description. Cuts were laid out with a compass, corner stakes leveled with a carpenter's level lashed to a two-by-four and used as datum for all subsequent leveling. Plans at successive levels and profiles upon completion of cut were drawn to scale. Cuts were located by reference to a "permanent" bench mark, consisting of an iron pipe with brass cap stamped with symbols guaranteed to mystify all future visitors to the site. Cuts were designated by the site number and a capital letter A, B, C, etc., in the order made. Levels were numbered from top to bottom. Finds catalogued 16-N-6/A-9 would therefore read "Oliver Site (16-N-6), Cut A, Level 9."

Excavated material was put through ½-inch screens. It was found that one shovel hand could keep two to four men busy at the screens, but constant supervision was required to maintain accuracy of level. With such a crew, two or three working days were sufficient for the completion of a cut. As far as practicable, sherds were washed, catalogued, and sorted on the spot to avoid useless labor on unsatisfactory cuts.

In all, 17 cuts were made in 9 sites, not including several which were abandoned for one reason or another, usually lack of depth. These cuts varied from 50 to 240 cm. in depth, the average being 106 cm. or between

³Willey and Woodbury, 1942; Willey, 1949.

10 and 11 levels. Not one of the 17 failed to yield some stratigraphic information of value, several of them revealed pottery sequences covering almost the entire known span of pottery-making in the Lower Mississippi Valley. No other answer is needed for those who may still be skeptical about the practicability of stratigraphic studies in the Southeast.

Method of Analysis

The crucial operation in the interpretation of stratigraphic data is the correlation of pottery distributions with soil stratification, if the latter is present. In a homogeneous deposit without observable profiles you have to take the pottery distribution at face value so to speak, assuming that the changes or lack of changes from level to level mean just what they say. Where stratification has been recorded, however, an opportunity is given to evaluate such changes a little more realistically. It is hardly necessary to point out that the effect of local conditions and events, as revealed by the profiles, may be such as to materially affect the distribution of pottery in the ground. Thus, two distinct phases of interpretation are involved: (1) interpretation of profiles for what they may reveal of events on the site, or that particular portion of it; and (2) interpretation of pottery distributions in the light of such events. No attempt is made here to minimize the possibilities for error in such a complicated interplay of guesswork. Limitations of excavation technique guarantee that the chances of 100-per cent successful interpretation are nonexistent. One simply does the best one can with the available information.

Assuming for the moment that there is a complete record of the four profiles of a given cut, the specific mechanical difficulty is how to effect a graphic comparison of that record with the pottery picture. After considerable experimentation, we have found the most satisfactory method is to construct the pottery graph by the usual manner of seriating bar graphs for each level, in the same vertical scale as the profile drawing, and superimpose one upon the other. Unfortunately, it happens rarely that stratification is so congruent that the profile of one wall of the cut may be allowed to stand for the other three. One

way around this difficulty is to construct an ideal profile by averaging the stratification of all four walls of the cut. This method is sometimes sufficient, but has to be used with caution, because it gives a misleading effect of conformity. In most cases we have found it more satisfactory to use an alternative method involving a composite profile drawing, a sort of palimpsest of all four profiles such as may be seen in figure 23. This, of course, has the opposite effect of exaggerating incongruities, but the resulting errors of interpretation are more likely to be on the side of safety.

* * * * *

SUMMARY OF STRATIGRAPHY

The results, if such they may be called, of the small-scale excavations described above may be summarized under three headings: (1) the actual stratigraphic results; (2) the light they throw on the stratigraphic and, indirectly, on the seriation methods; (3) the Baytown-Mississippi problem.

(1) Stratigraphic Results

These were, in general, most encouraging. Out of a total of seventeen cuts, all but two gave positive stratigraphic results. In both cases the failure was due to the fact that the cuts had been put down into low mounds. In one of these (Alligator A) stratigraphy, as

between mound and sub-mound, was indicated but was masked statistically by the large percentages of early types in the mound fill. The other (Massey B) was completely negative. Of the fifteen successful cuts, four gave information on relationships within the Mississippi Period (Walls A, B, Menard A, Rose A); six on relationships within the Baytown Period (Lake Cormorant A, B, Oliver B, C, Jaketown A, Shell Bluff A); all but one (Walls A) gave evidence of the relationship of the Baytown and Mississippi periods; one (Jaketown A) covered all three periods, Tchula, Baytown, and Mississippi; and two (Jaketown A and B) furnished a glimpse of

a possible pre-pottery period underlying the Tchula. In relation to the time and expense involved, these excavations paid off extremely well.

Not only were they individually successful, but the story they tell is completely consistent, so far as it goes. In no case was there conflict or uncertainty so far as the position of individual types or complexes is concerned. They are always in the same — hence we can call it the right — order. The uncertainties are on a higher level of interpretation, but before considering these, it will be well to summarize the results of this study under the second heading outlined above.

(2) The Stratigraphic Method

First, let it be recalled that any remarks made here on the subject of method apply solely to "metrical" stratigraphy. Most if not all of our difficulties of interpretation might have been avoided if we had been able to "peel" stratified deposits instead of digging them in arbitrary horizontal levels. The first generalization, then, is that "metrical" stratigraphy is not good enough. On the other hand, it is, in most cases, the only practicable method. Of the seventeen cuts described here, only four (Oliver A, C, Menard A, and Rose A) were in deposits that might possibly have been peeled by means of the block. The others, among them some of the most successful cuts from the point of view of pottery stratigraphy, could only have been dug by arbitrary levels. We did not consider using the block technique, for reasons of economy, but will in the future be on the look-out for opportunities to do so. Comparison of results of the two methods applied side by side might be very revealing.

While on the subject of excavation technique, another detail we learned the hard way is that great care should be taken to eliminate material from pits and post-holes in the course of digging. We were deliberately careless in this regard on the theory that such material would not be in sufficient amount to upset the over-all statistical results. As a matter of fact, it rarely does so, but in questions having to do with continuity or discontinuity of deposits, in other words, the presence or absence of a "transitional" phase, out-of-place material

in pits and post-holes may be important if not decisive. A great deal of tedious discussion in this section might have been avoided if we had not such material to account for.

Of first importance, on the side of interpretation, is a repetition of the warning, several times repeated in the foregoing pages, that interpretations based on one cut are valid for that cut only; insofar as they are extended to apply to the site as a whole, or a larger area, they lose their force in direct ratio to such extension. We have pointed out several instances in which the interpretation of an individual cut would have been very different but for additional information derived from other cuts on the same site. This leads to the recommendation that, except under special circumstances, more than one cut should be made on a site. This seems so obvious it may be wondered why we mention it. However, it is often overlooked. In our own private disagreements, the existence of which the careful reader may detect here and there, we have often found ourselves using what we *think* happened in 4 square meters of deposit as proof of cultural events on an extensive regional scale.

(3) The Baytown-Mississippi Problem

The most difficult problem raised by these excavations is the nature of the relationship between the Baytown and Mississippi complexes in the area. This problem was repeatedly brought into sharp focus in the course of the interpretations of the several pits. It could only be dimly perceived in the course of the seriation analysis, because that method assumes that the collections used represent continuous occupation. Collections that could be seen to be from stratified or reoccupied sites were not used in the analysis. Collections containing a mixture of Baytown with Mississippi types that were used were assumed to include a transitional phase between these two periods in a continuum and were placed on the graphs in that position. There are certain mechanical factors involved in this that must not be overlooked. Depending on the number of such mixed sites, the resulting pattern in each area was one of a gradual replacement of Baytown by Mississippi types. The precise shape of the individual type frequency patterns, i.e.,

the degree to which they show a sharp or gradual increase or diminution, is to a certain extent influenced by the mere number of these mixed collections, since space for them has to be found on the graph. In other words, the seriation technique, without more careful controls than we were able to bring to bear, tends to draw out the patterns vertically, with the result that changes and replacements tend to look more gradual than they are in fact. The net result is to reinforce the assumption of continuity upon which the method is based. The effect of stratigraphic diagramming is just the reverse, particularly in a refuse deposit of slow accumulation. Compare the generally abrupt pattern of the first appearance of Neeley's Ferry Plain on the stratigraphic charts with its slow, gradual increase on the seriation charts. There is no point in discussing which is right. Such terms are meaningless until you have absolute chronology by which to scale the charts. But the difference is important in terms of interpretation and general thinking. The slowly expanding figures on the seriation charts fortify the original assumption of gradual change in a cultural continuum. The rapidly expanding figures of the stratigraphic charts lead to an impression of abrupt cultural change, an impression not a little influenced by the fact that sites were selected for excavation on the expectation that they would show cultural change. In short, the results of both methods, as used here, are "rigged" to a certain extent

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in favor of one side or the other of this important question.

With this in mind, we may summarize the stratigraphic evidence on the Baytown-Mississippi question. Of the fifteen cuts, that showed superposition, one (Shell Bluff A) appeared to show positive evidence in favor of continuity; two more (Walls B, Menard B) favored, but weakly, the same interpretation; three (Oliver A, Alligator B, Rose A) showed definite evidences of discontinuity; and three more (Oliver B, C, Massey A) were interpreted that way with somewhat less assurance; the remaining six gave no indications on this particular issue one way or the other.

It is important to note that certain cuts that gave the clearest evidence of a discontinuity (Oliver A, Alligator B, Rose A) showed up as transitional on the seriation charts. The final conclusion is that, while the assumption of general continuity between Baytown and Mississippi periods in this area is not disproved, there is sufficient evidence on the other side to call it into question. If this is correct, it follows that more vigorous controls have to be introduced into the seriation method to eliminate the use as "transitional" of mixed collections that are actually the result of reoccupation. In the meantime, if we have succeeded in exposing some of the difficulties inherent in both seriation and stratigraphy, the foregoing pages will not have been written in vain.

SUMMARY AND CONCLUSIONS

THE results of the first phase of a continuing survey program in the northern part of the Lower Mississippi Valley have been presented in the foregoing pages under various headings written by various hands not always animated by identical points of view. The effect has been perhaps to create the impression that we have settled many questions, whereas we have all too often only settled the same questions in several different ways. This cannot fail to have left the reader, provided he has not already left the report, in considerable confusion, and it now becomes our duty to straighten him — and ourselves — out. What is required at this point is a general summary of what the three authors can agree on, or failing that, at least to bring together their conflicting interpretations so the reader may compare and judge between them. Up to now, we have attempted to keep such disagreements in the background by writing more or less consistently in the first person plural, giving an

effect of praiseworthy unanimity. We have bribed each other's silence by promises of an opportunity to express dissenting opinions and prejudices in the concluding section. A change of style is therefore indicated if we are to give scope to these individual interpretations. From here on, the third person singular will frequently appear in order to make clear which author's point of view is being expressed.

The plan of this concluding section is as follows. We shall first review briefly what we have done and what we, each and severally, think of it. Following this, we shall present a summary of the archaeology of the area period by period, a sort of culture-historical reconstruction, in which we shall use any information that is available whether obtained by us or others. Finally, we shall conclude by attempting to fit our findings into the general picture of Eastern archaeology with a few observations in regard to their bearing on the over-all chronological problem.

AUTHORS' REVIEW

It may not be good form to forestall adverse criticism by offering a review of one's own work, but in the case of a report that makes no pretense of finality it is excusable. Nothing is more exasperating than being criticized for failure to succeed in something one has not tried to do. That is sure to happen to the Lower Mississippi Archaeological Survey. We have merely presented a progress report and are as dissatisfied with it as the most carping critic could possibly be. This is our opportunity for expressing that dissatisfaction, but, since there are three of us, it takes different forms and applies unequally to different sections of the work. A rather detailed review is required for which we beg, once more, the reader's indulgence.

In the long opening section, dealing with the geographical conditions of the Survey Area, a great deal of emphasis was placed on the physical differences between the various areal subdivisions and it was confidently pre-

dicted that these differences would be reflected in the archaeology. We now have to explain why this expectation has not been fulfilled, or at least why very little more was said about it. At the time when Phillips was writing this section, inspired by the enlightened environmentalism of Kroeber, Ford was carrying out the seriation analysis described in Section V, in which Mississippi River Commission quadrangles were used as convenient units of area and grouped into five subdivisions on a purely empirical basis. In other words, while Phillips was saying all those fine things about natural conditions and their effect on culture and the shape of cultural distributions, Ford was working out a grouping quite independent of natural subdivisions, in many cases crosscutting them, which nevertheless seemed to give satisfactory results, and eventually became the framework for most of our archaeological generalizations about the area. Phillips is, not unnaturally, unhappy over this and harbors a

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suspicion that an analysis that took these natural subdivisions more into account might have produced better results. However, it may be pointed out here that the work of the Survey has so far been confined almost entirely to the St. Francis and Yazoo basins which are both comprised in one major type of area, to wit, flood plain of Mississippi-Ohio derivation (see classification, p. 20). Very little work was done in flood-plain areas of "other than Ohio-Mississippi" derivation. A few sites were located in the lower part of the Arkansas River Lowland, but the Survey was not carried into the White-Black River Lowland nor the Boeuf Basin. Thus, no opportunity is yet afforded to make cultural comparisons between these two major categories of flood plain. From general information, however, it can be fairly confidently predicted that the flood-plain areas of other than Mississippi-Ohio derivation are not going to show as dense an occupation, particularly in the Mississippi Period, as the more fertile portions of the flood plain covered by the Survey. As for the older alluvial plains and upland remnants, we have produced no new comparative information at all. Thus, it cannot be said that the moderate environmentalist point of view in Section I has been refuted. It has remained unfulfilled simply in the sense that it has not been tested.

The pottery classification described in Section III is a joint responsibility. We all had a hand in it and are all equally dissatisfied with it. This is normal. The archaeologist who thinks he has achieved a final classification of anything is a rare and probably untrustworthy individual. Most of the shortcomings of our classification have been fully exposed in the type descriptions. Our guess is that very few of our types will stand up when more and better material is available. Many of them will break down into more specialized groups, a few (we may hope) will be combined into more general groups. It is not likely that the total number of types will be reduced. The outlook for the Southeast as a whole, so long as present typological methods remain in favor, is not pleasant to contemplate. Where we are now counting types in tens, they will be counted in hundreds. However, the proof of

the typing is in the using. Our classification cannot be too bad or it would not have produced the consistent patterning of types through time that is shown in the seriation and stratigraphic analyses of Sections V and VI. It seems to have been equal to the purposes for which it was devised, which is all that should be asked of any classification.

Section IV presents a series of distributional studies of certain characteristic pottery forms and designs that have a special interest from the wider point of view of relationships with Mesoamerica and the Southwest. These have been entirely the work of Griffin and have led him to several important conclusions, to wit, that in the Mississippi Period there were more or less direct contacts between the northern part of our area and the Southwest, that these contacts may have begun fairly early, as early as Developmental Pueblo, that the exchange of traits may not have been entirely in one direction, and that the route was not through Texas. It is Griffin's belief that in the recent emphasis of Southeastern students on connections between the Southeast and Mesoamerica, these important evidences of Southwestern connections have been overlooked. His co-authors heartily endorse this opinion. There are in fact many other ceramic traits not covered by Griffin, coming under the heading of miscellaneous small pottery objects, such as disks, ladles, trowels, etc., that point the same way. But to agree that this is a neglected phase of research is not to regard it as *the* answer to a long-standing problem. Griffin's findings in this section present one more evidence of the complexity of processes that underlies any focalization of traits that we refer to as "culture." There is enough of this kind of evidence already to make an expression such as "the origin of Mississippi culture" look faintly ridiculous.

The seriation analysis described in Section V is at once the most fruitful source of results in the report and conflict among the authors. Results, because the chronological framework upon which our findings are hung derives from it; conflict, because, although we all profess belief in the general validity of the method, no two of us have the same degree of confidence in its results. Before commenting fur-

ther on the nature of our disagreement, it may be well to point out wherein the seriation technique differs from other methods of analysis. The usual method of organizing archaeological data over a large area is to isolate culture groupings, "phases,"¹ "foci," or whatever one chooses to call them, by combining units, usually sites or levels within sites, and to rationalize on the basis of fairly complete trait inventories the inter-relations of these groupings in terms of space and time. This is an inadequate statement of a complicated procedure but the essential features are that it is a process of combination, of "building-up" from smaller to larger units, and that a good deal of detailed information is needed even to begin it. We have not yet made an attempt to organize our findings in this way; in fact, it may be doubted whether it is possible at the present time. If we have occasionally referred to groups of sites or localities by some convenient designation outside of the seriation framework, no classificatory significance has been implied. The grouping in question is valid only for the particular matter in hand.

The seriation method, on the other hand, makes an effort to grasp the main outlines of the prehistory before the details are known, by means of graphic delineation of the behavior of one trait complex, in this case pottery, through time. Having established the over-all pattern, the stream of time represented is cut into units at points where shifts in patterning occur. Thus, in a sense, the seriation method works from the general to the particular, the reverse of the conventional method, and is therefore well suited to an initial attack on an area. The contrast must not be over-stressed, however. The results are not as different as the methods. The "periods" derived from seriation are not as different conceptually from "foci" as one might think. They have typological as well as temporal implications, since the cuts are made at points where typological shifts affect the general pattern. Ford stands on the unshakable philosophical ground that none of these concepts have any cultural "reality" anyhow, all being arbitrary con-

structs of the archaeologist. If there are differences they are differences of degree not of kind. This is consistent with his view that the predominating characteristic of culture is continuity both in space and time. To the extent that such a view is correct, any divisions spatial or temporal, however derived, must of course be wholly arbitrary.

As applied specifically to pottery, the only thing we are in a position to generalize about, Ford's idea is that it was developing in a continuum throughout its entire history in the Mississippi Valley, that whether new types evolve by modification of older ones or come in as new ideas from outside, they take their place in an uninterrupted cultural flow. The logical consequence of such a view is that, in most cases a "mixed" pottery complex represents a single brief span of time on that continuum, an "instant" for all practical purposes, when both elements of the mixture were being made and used side by side. The importance of this postulation for the seriation method can hardly be exaggerated. Ford does not deny that mixed complexes sometimes do result from reoccupation of sites. Such collections he frankly banishes from his graphs and says so (p. 233).

Griffin and Phillips, on the other hand, while not rejecting the general theory of continuity, are inclined to feel (with emphasis in order named) that there are more instances of mixture through reoccupation of sites than Ford has recognized. In particular, as pointed out in the individual sections written by them, they have tended to see indications of at least one significant break in the otherwise placid stream of pottery continuity at the point where the tempering material shifts from clay to shell, in other words between the Baytown and Mississippi periods. They feel that, by including mixed collections on the graphs, Ford has effected a spurious transition that seems to prove his continuity hypothesis, but in reality leaves the question open. Another way of putting it would be that the seriation technique, being based on the assumption of continuity, is unable to cope with a "break"

¹In the sense in which this term is used in Mesoamerica and the Southwest.

of the nature described above, consequently, to establish whether such a break is really there or not, some other method of analysis is required. The upshot is that this compact majority of two, while perfectly willing to endorse and even use the results of the seriation analysis, do not regard the evidence it shows of such continuity as final.

Griffin would like to have it stated that he started with the opinion that there was a break between the earlier horizon and the Mississippi Period. Phillips only came to it gradually as a result of his independent analysis of stratigraphic collections in Section VI, where the reader may follow his painful lucubrations on the subject. One might have thought that stratigraphy would have settled the Baytown-Mississippi continuity question one way or the other. It did not. The only conclusion we are able to agree on is that the subjective element enters into the stratigraphic method just as surely as into the seriation method. That more stratigraphic cuts gave evidence of discontinuity than the reverse Ford attributes to a bias on the part of Phillips in favor of that solution. Phillips naturally protests his innocence, but the reader will have to decide between them. We have perhaps made too much of this issue, which, after all, involves only one point on the time scale. In general, the results of seriation and stratigraphy were in satisfactory agreement.

The experimental correlation of archaeological and geological time scales presented in Section VII is so tentative, and the various possibilities of error are so thoroughly exposed in that section, that nothing further need be said about it here. We are all agreed that the results are sufficiently promising to justify further work along these lines. We cannot predict that this is going to result in a new archaeological technique that will supersede those now in use in the area. What is more likely is that by using the channel sequence as a control in seriation and stratigraphic studies some of the present difficulties of interpretation that have been so fully confessed above will be eliminated.

More important from a long-range point of view is the possibility that correlation of archaeology and hydrography plus C^{14} dating

will result in an accurate absolute time scale for the entire Mississippi Alluvial Valley, which would be as welcome to the hydrographer as to the archaeologist.

The study of spatial and temporal distributions of sites as to type in Section VIII is another promising approach. In its present form it can be no better than the dating on which it is based. The difficulty of dating sites from surface collections whether by seriation or any other method has been repeatedly emphasized and the lack of agreement among the three authors as to the dating of individual sites freely exposed. The period date for the first appearance of new types of mounds and site plans, as set forth in Section VIII, cannot be regarded as final. Phillips and Griffin, for example, hold reservations concerning the appearance of small ceremonial centers with rectangular platform mounds as early as the middle Baytown Period (E-D). On the other hand, there can hardly be any question about the relative order of appearance of the various types of sites, and the population distributions during the several periods indicated by the maps (figs. 64 to 69) is probably in the main correct. With more and better data of the same sort, some very interesting demographic questions might be indicated.

The long Section IX dealing with the problems—but containing no solutions—of the identification of sites from documentary sources might, in its author's (Phillips) opinion, better have been published separately, if at all. It is principally due to Griffin that it has been included here. He feels that, notwithstanding the meager results, it is a valuable contribution as an object lesson for archaeologists who brashly enter the field of history. It also contains leads for further research. The search for Quapaw sites of the contact period was shown to be difficult but not quite hopeless. The Lower Mississippi Survey has not been back in the Lower Arkansas region since this study was made. In particular, further investigation of the Wallace Site (17-K-3) is indicated, but the entire area should be thoroughly searched as well. The importance of establishing the latest pottery typology in this area is sufficiently apparent in the uncertainties and disagreements that crop up whenever the

late Mississippi Period falls under discussion. In some respects this most recent period, which ought to be the best known, has been

the hardest to deal with chronologically. This will continue to be the case until we can fix its terminus by means of a few good contact sites.

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