

BULLETIN NO. 11

SOUTHEASTERN ARCHAEOLOGICAL CONFERENCE

Proceedings of the
Twenty-Sixth
Southeastern Archaeological Conference

Edited by
Bettye J. Broyles
Morgantown, West Virginia

1969

EDITOR'S NOTE:

The Twenty-Sixth Southeastern Archaeological Conference was held at the Ambassador Motel in Macon, Georgia, on November 14-15, 1969, with John W. Griffin as Chairman.

Both of the Friday sessions were devoted to Contributed Papers on a variety of subjects, as was the Saturday morning session. The Saturday afternoon session consisted of a Symposium, chaired by William G. Haag, on Salvage Archaeology.

A few of the papers from the meeting are not being published in this Bulletin for a number of reasons. First, the paper by Sherwood Gagliano and Clarence H. Webb on "Archaic-Poverty Point Transition on the Pearl River Estuary, Mississippi" is to be included in a separate Bulletin (No. 12) concerned with the Poverty Point Culture, along with two papers on the same subject from the 1970 meeting.

The remaining papers could not be transcribed from the tapes and copies were not available for publication.

The papers which were transcribed from the tapes by your Editor were not sent to the speakers for corrections and additions for the simple reason that it took so long to get the papers back from the 1968 meeting so that Bulletin No. 9 could be published, it was felt that these papers could be published in their present form. They were re-written to some extent because the speakers were using slides, but, hopefully, nothing has been omitted or mis-interpreted.

The Symposium was transcribed almost in its entirety, with only small portions being omitted because the voices on the tape were obliterated by background noise and could not be understood.

Bettye J. Broyles
Editor/Treasurer SEAC
West Virginia Geological Survey
Morgantown, West Virginia

Members who Attended

The Twenty-Sixth Meeting of the

SOUTHEASTERN ARCHAEOLOGICAL CONFERENCE

Stanley Ahler	Patrick H. Garrow	Robert S. Neitzel
Betty Ann Anderson	Anne Gatewood	Robert W. Neuman
William S. Ayres	Marjorie Gay	Carey B. Oakley, Jr.
Steven G. Baker	Paul F. Gleeson	Danny Olinger
John S. Belmont	Don Gordy	John Pennington
Gerald Berry	J. B. Graham	Mrs. John Pennington
Susan M. Bodie	J. Bennett Graham	George W. Percy
Jeffrey P. Brain	Joseph E. Granger	Gregory Perino
Elsie Bratcher	John W. Griffin	Drexel A. Peterson, Jr.
Wesley Breedlove, Jr.	Mrs. M. Travis Grubbs	Richard R. Polhemus
Elenor Brock	Lee H. Hanson, Jr.	Brenda Pouncey
Oscar W. Brock	William G. Haag	Alden Redfield
Paul E. Brockingham	David J. Hally	Norman N. Ritchie
Sam Brookes	James W. Hatch	Angela Rodrigue
Bettye J. Broyles	Edward F. Heite	Jack R. Rudy
Marla Buckmaster	E. Thomas Hemmings	Thomas M. Ryan
Ralph H. Bunn, Jr.	Lynne Herman	Frank T. Schnell
Joseph R. Caldwell	John A. Hesse	Gail D. Schroeder
Lloyd N. Chapman	Charles A. Hoffman, Jr.	Stuart C. Schwartz
Phyllis L. Chase	W. A. Hohlweg	William F. Schweri, II
David W. Chase	William Hony	Donalf F. Smith
Ruth I. Christensen	Charles M. Hubbert	Gerald P. Smith
R. Burle Clay	Paul R. Huey	Richard L. Smith
Joffre L. Coe	Harold A. Huscher	Samuel D. Smith
Gerald Collins	Ned J. Jenkins	Stanley South
John M. Connaway	Calvin Jones	Robert L. Stephenson
John M. Corbett	James Josey	Gerry Wheeler Stone
Randy Cottier	Bennie C. Keel	Noel R. Stowe
John W. Cotter	James R. Keller	Bob Strickland
Don Crusoe	A. R. Kelly	Prentice Thomas
Hester A. Davis	Duane H. King	Robert M. Thorne
Clemens deBaillou	Thomas H. Koehler	Christy T. Trowell
David L. DeJarnette	Yulee Lazarus	Jack Tyler
Chester B. DePratter	R. Barry Lewis	John W. Walker
Stephen Deutschle	Shelia D. Lewis	Kitty L. Wallace
John H. Dickerson	Howard A. MacCord, Jr.	D. E. Ward
Ann Dilworth	Harry Madison	Sue Ward
Edward I. Dittmar	Marilyn Manos	Deborah Watts
Roger Dittmar	William Marquardt	Malcom C. Webb
John T. Dorwin	Richard A. Marshall	Jean M. Wheedon
Jerry G. Dukes	Patrick E. Martin	Donald Whitcomb
Charles H. Fairbanks	Pat McCoy	Mark Williams
Charles H. Faulkner	Samuel O. McGahey	Paul E. Williams
Richard D. Faust	Charles H. McNutt	Ray Williams
Jason Fenwick	Lawrence Meier	Robert E. Williams
Annette Ferguson	Gordon M. Midgette	Stephen Williams
Leland G. Ferguson	Rebecca M. Midgette	William D. Wood, Sr.
Janet Ford	Gerald Milanich	Jean B. Yarnell
Frank B. Fryman, Jr.	Joseph A. Murciak	Ridhard A. Yarnell
	Jerry J. Neilsen	

TABLE OF CONTENTS

MEMBERS WHO ATTENDED MEETING	ii
LIST OF ILLUSTRATIONS	iv
PROGRAM, TWENTY-SIXTH SOUTHEASTERN ARCHAEOLOGICAL CONFERENCE	v
ARCHAEOLOGICAL THEORY AND METHOD: SOME INFERENCES AND SPECULATIONS	
Robert M. Thorne	1
AN UNUSUAL HOUSE CONSTRUCTION IN MISSISSIPPI	
Samuel I. McGahey	6
A CEREMONIAL CENTER AT THE CHARLES TOWNE SITE	
Stanley South	9
BAKED CLAY OBJECTS FROM THE SITE OF THE 1670 SETTLEMENT AT CHARLES TOWNE, SOUTH CAROLINA	
Stanley South	13
A PRE-AGRICULTURAL VILLAGE SITE IN FULTON COUNTY, GEORGIA	
Arthur R. Kelly and Larry Meier	26
COMMENTS ON THE CONSTRUCTION OF MOUND A, OCMULGEE NATIONAL MONUMENT	
John W. Walker	30
DISTRIBUTION OF SOUTHEASTERN ARCHAIC PROJECTILE POINTS IN THE OHIO VALLEY	
Bettye J. Broyles	31
THE INTENSITY AND NATURE OF THE OCCUPATION OF RUSSELL CAVE, ALABAMA	
John W. Griffin	36
EXCAVATION AND ANALYSIS OF LIVING FLOORS IN ALLUVIAL SITES	
E. Thomas Hemmings	41
THERMAL TREATMENT OF FLINT	
Charles H. Fairbanks	46
WHITEOAK, A MIDDLE WOODLAND PHASE IN SOUTHWESTERN ALABAMA	
David W. Chase and Lynne L. Herman	47
SALVAGE EXCAVATIONS TO OBTAIN A HOPEWELL POPULATION	
Gregory Perino	49
RESERVOIR RESURVEY: A RELATIVELY UNEXPLORED POTENTIAL	
Frank T. Schnell	55
THE NATURE OF SOUTHERN HOPEWELL	
Jon L. Gibson	58
SYMPOSIUM: WHITHER SALVAGE ARCHAEOLOGY?	67

LIST OF ILLUSTRATIONS

House No. 1 at the Wilford Site, Coahoma County, Mississippi	8
Houses 2, 3, and 4 at the Wilford Site	8
Plan of the Major Architectural Features at 1670 Site of Charles Towne	10
Baked Clay Objects from the Charles Towne Site	12
Fragments of the Perforated Grooved Melon-shaped Form from Charles Towne ..	14
Biscuit-shaped Baked Clay Objects from the Charles Towne Site	16
Incised, Punctated, and Simple Stamped Baked Clay Objects from Charles Towne	18
Baked Clay Objects from Daws Island in Port Royal Sound, Bufort County, South Carolina, Apparently Relating to the Bilbo Site in Georgia ..	20
Artifact categories by layer in graph form from Russell Cave, Alabama	39
Sample of Whiteoak Fingernail Decorated types from Alabama	48
Waterways Network in the Southeast	54
Walter F. George Reservoir	54
Marital and Inheritance Possibilities of the Model for Hopewellian Social Organization	62

PROGRAM

THE TWENTY-SIXTH SOUTHEASTERN ARCHAEOLOGICAL CONFERENCE

SESSION I (Friday Morning) CONTRIBUTED PAPERS

Chairman: Stephen Williams, Harvard University

"Archaeological Theory and Method-- Some Inferences and Speculations"
Robert M. Thorne, University of Missouri

"An Unusual House Construction in Mississippi"
Samuel I. McGahey, Mississippi Archaeological Survey

"A Kiawah Ceremonial Center at Charles Towne, South Carolina"
Stanley South, University of South Carolina

"9 Fu 14, A Pre-Agricultural Village Site in Fulton County, Georgia"
A. R. Kelly and Larry Meier, University of Georgia

"Comments on the Construction of Mound A, Ocmulgee National Monument"
John W. Walker, National Park Service

SESSION II (Friday Afternoon) CONTRIBUTED PAPERS

Chairman: Joffre L. Coe, University of North Carolina

"Distribution of Southeastern Archaic Projectile Points in the Ohio Valley"
Bettye J. Broyles, West Virginia Geological Survey

"The Intensity and Nature of the Occupation of Russell Cave, Alabama"
John W. Griffin, National Park Service

"Excavation and Analysis of Living Floors in Alluvial Sites"
E. Thomas Hemmings, University of South Carolina

"Comments on the Thermal Treatment of Flint"
Charles H. Fairbanks, University of Florida

"Methods of Removing Soil Profiles"
Stanley South, University of South Carolina

"Coles Creek Culture in Southeastern Prehistory"
John S. Belmont, Southern Illinois University

"Whiteoak, A Middle Woodland Phase in Southwestern Alabama"
David W. Chase and Lynne L. Herman, Montgomery Museum of Fine Arts

"Salvage Excavations to Obtain a Hopewell Population"
Gregory Perino, Gilcrease Institute

SESSION IV

(Saturday Morning)

CONTRIBUTED PAPERS

Chairman: Joseph R. Caldwell, University of Georgia

"Archaic-Poverty Point Transition on the Pearl River Estuary, Mississippi"
Sherwood M. Gagliano and Clarence H. Webb, Louisiana State University
and Shreveport, Louisiana (Read by Robert Neuman)

"Reservoir Resurvey-- A Relatively Unexplored Potential"
Frank T. Schnell, Columbus Museum of Arts and Crafts

"Current Archaeological Program in South Carolina"
Robert L. Stephenson, University of South Carolina

"Two, Four, Six, Eight-- Let's All Help to Legislate"
Hester A. Davis, University of Arkansas

"The Nature of Southern Hopewell"
Jon L. Gibson, University of Southwestern Louisiana (Read by R. Neuman)

SESSION IV

(Saturday afternoon)

SYMPOSIUM: WHITHER SALVAGE ARCHAEOLOGY?

Chairman: William G. Haag, Louisiana State University

SESSION I

CONTRIBUTED PAPERS

ARCHAEOLOGICAL THEORY AND METHOD: SOME INFERENCES AND SPECULATIONS

Robert M. Thorne

University of Missouri

The paper which follows is the result of the 1969 University of Missouri Field School in which a series of test situations was set up. Theoretical ideas and aims which resulted are not particularly those of a single staff member, but evolved through the interaction between Dr. Richard A. Krause of the University of Missouri-Columbia, Dr. Nikolass J. van der Merwe of S.U.N.Y. at Binghamton, Mr. Robert T. Bray, Resident Archaeologist, and myself.

As one of the sub-fields of anthropology, American archaeological field methods and techniques in the past have continually reflected the theoretical approaches and interests of our sister sub-disciplines. During the 19th Century, for example, ethnologists oriented their research toward the demonstration of the events of social and technological evolution. Archaeologists took the same approach, but for them the artifact was of primary importance and was taken as the major indicator of cultural process. Then on the basis of artifactual data, broad configurational levels or stages of social development were formulated. Even though the artifact was the major focus of analytic interest, the methodology for its recovery and the subsequent analysis of the artifact-bearing contexts was at very best, rudimentary.

With the advent of Boasian anthropology, the formerly established underpinnings of cultural evolution were rapidly modified as the interests of American anthropologists emphasized the intensive study of individual cultures. Archaeological interests shifted also and many of the researchers of that period turned their attention to systematic attempts toward the ordering of local and regional artifact complexes in relation to both time and space. This led to the establishment of culture, focus, and phase as the basic conceptual units. These were in turn ordered in such a manner as to show the relationships involved in the development and spread of artifacts and artifact complexes. At the same time, data recovery techniques were revised and improved but not specifically in an attempt to bring archaeological research into line with ethnographic research. Archaeological method and theory was beginning to come into its own, resulting in an additional attempt to demonstrate the usefulness and integrity of the variously defined analytical units which were gaining in popularity.

More recently, our profession has witnessed the advent of what is called the "new archaeology". While some researchers feel that something truly new is happening in the profession in terms of method and theory, others feel that this phase is nothing more than the revitalization and re-emphasis of previously used methods and techniques with new techniques and methodology being added as they are developed. While the argument of "new" versus "old" has its lighter moments, the serious side of the picture portrays the use of archaeologically derived data for drawing inferences about the structure of extinct social systems.

Researchers who attest to this theoretical approach, view culture as being systemic and therefore composed of sub-systems with human behavior acting as the articulating force between the various sub-systems. Demonstrable variations in human behavior are considered to be both a product of sub-system restructuring and the means for establishing systemic harmony at a different level or plane. There is also an emphasis in this approach on process in culture change which is achieved through variation in one or more of the sub-systems. This may be viewed as growth, displacement or the reinforcement of one sub-system by another as the systemic balance is challenged by social, economic, political, or environmental forces. The prime analytic aim is the isolation of each sub-system and its subsequent study as a separate variable within the matrix of forces to which it is exposed. The ultimate goal, of course, is to construct an archaeologically testable model to explain the variations in prehistoric human behavior.

While the systemic approach has been debated as regards both its origin and aims and goals, it has generated considerable interest among a growing number of followers. It therefore seems appropriate to examine the implications of the systemic view of culture from the perspective of archaeological field techniques and methods. It should be emphasized that archaeologists who follow this approach must still derive the major portion of their data from the analysis of artifact-bearing contexts, which makes such an examination seem still more appropriate. The principal aim of this paper is to undertake such an examination, to make suggestions as to how this approach may be improved and to substantiate these suggestions with specific examples from the field. As previously mentioned, many of the ideas presented here grew out of the 1969 University of Missouri Field School, located at the Utz Site in north-central Missouri, and specific examples will be drawn from that research.

As with any theoretical approach, the systemic view of the nature of archaeological remains is particularistic, as has been stated by Martin and Longacre who say, "All of the material remains in an archaeological site are highly patterned or structured directly as a result of the ways in which the extinct society was organized and the patterned ways in which the people behaved." Now it might be that all of the material in an archaeological site is structured or highly patterned but it appears to be theoretical folly to assume that all archaeologically derivable patterning can be directly attributed to the behavior of the site's prehistoric inhabitants.

It therefore seems that there are two distinct but inter-related problems which the field worker must resolve: (1) the identification of those instances of behavioral patterning which reflect the inhabitant's activities as opposed to those which reflect the work of the researcher, and (2) the demarcation of kind and degree of relatedness among separate but analytically demonstrable instances of patterning which do reflect human behavior. This means that to those who follow the systemic approach, adequate field research should include productive statements about the relevant relationships among non-arbitrarily defined archaeological contexts and the prehistoric matrices in which they may occur.

In proposing fieldwork of this kind, the emphasis will, by necessity, be placed on the interpretation, evaluation, and selection of a set of alternative statements about a particular grouping of activities within a specific range of demonstrable social contexts. The criteria for evaluation of these statements and their adequacy should include: (1) productivity which should be stated in terms of the appropriate anticipation if not the actual prediction of archaeologically derivable events; (2) replicability or testability (Can the state patterning be tested and shown to re-occur?); and (3) economy (Does the statement

or statements produce the most information within a reasonable research framework?). The selection of such statements should in turn lead to a critical in-the-field examination of the analytically derived models of intra-cultural relationships which are being tested. This should be true regardless of whether the structural description of such relationships is based on prior analysis of particular and generalized occurrences in the archaeological record, or appear as ethnographically derived hypotheses. When the researcher is in the field, recording activities, analytic operations, and procedures for the evaluation of recovered data should be combined, thus allowing the constant adjustment and improvement of field recording techniques.

As a part of the past summer's program, the techniques of recording were continually discussed and revised to provide a more advantageous analytic situation in the laboratory. As a result of this rethinking, a series of problems emerged which are relevant to the suggestions of this paper and to the type of research suggested. The first of these is the establishment of the Criterion of Relevance, i.e., how can the behavioral patterns and relations developed by this method be shown to be culturally significant? Are the descriptions which we ultimately employ derived only from a prior notion of the occurrence which we expect to find on a prearranged mental grid? In the ideal situation, as prehistorians we should be attempting to describe what the significant behavioral patterns and relationships were and not what they should have been. The commitment to imputed definitions, either past or present, can never serve as testable criteria of relevance.

A number of our colleagues doubt the applicability of linguistic anthropological techniques to archaeological situations; the fact remains that some linguists feel their particular sub-discipline to be the most empirically oriented of the four in anthropology. While the merits of this idea are not pertinent to the present discussion, the two questions previously posed may be partially answered by the re-application of certain linguistic concepts. These will, in turn, provide some idea of the way in which the data recovered from the Utz Site were judged in terms of its relevance.

I would like to emphasize here that the examples which will be cited are site bound. This implies that researchers in other areas who wish to utilize the techniques and methods described here may have to alter them to suit specific situations in other areas.

In attempting to evaluate the relevance of systemic research techniques used at the Utz Site, repetitiveness of behavioral patterns which were demonstrable through archaeological recovery was found to be a suitable assessment entity. In this instance, repetitiveness was analytically defined as being context bound. By context bound behavior, we were considering the behavioral patterning, for example, exhibited in post setting as opposed to the behavior centered around storage pit preparation. In this manner, it was possible to show that culturally significant behavior derivable by archaeological means would not occur in free variation.

To cite an example, the excavations at the Utz Site were planned in such a manner as to establish three contrastive areas. The first of these was in an area which was plowed the last time in 1955 and only once during its agricultural history with mechanically drawn equipment. The second unit was located in an area reputed to have never been plowed, while the third was placed in an area which is reported to have been regularly farmed to the present. In this third area, excavations showed that the aboriginal cultural material was deposited to a depth of

three feet and mixed with 20th century debris. In this instance, and in contrast to the other two areas, the aboriginal material is considered to be in a state of free variation and not significant in the attempted interpretation of prehistoric behavioral patterns.

A second criterion of relevance may be thought of as the linearity of cultural events. This is somewhat akin to the processes of taxonomic phonemics in which a series of speech events are dismantled and reassembled thus providing a means by which behavioral rules may be written.

In the context of archaeology, a similar methodological approach may be applied to the sequence of events leading to the construction of a house or the making of a ceramic vessel. In the latter instance a number of events such as acquisition of clay and tempering material, their preparation, addition of the temper to the clay, manufacture, decoration, and firing will occur. While in all instances this will be a linear sequence of events, there will be options and alternatives which the investigator must isolate as repetitious actions so that he may write significant behavioral rules.

To make reasonably accurate constructions of significant prehistoric behavioral patterns, it is necessary to establish boundaries to delimit units of contrast. Again, criteria must be proposed to justify specific boundary formation.

One such criterion which was applicable at the Utz Site was the formation of sets of content contrasts. In the gross sense, it is possible to set up an in-the-field quantitative analytic framework to help in differentiating behavioral units. For example, the differential artifact count within a house as opposed to that which occurs outside may be used as an activity indicator. Therefore, one might hypothesize: (1) that the occurrence of ceramic pieces would be greater in and around a cooking area within a house; (2) that a similar count would not be high in a comparably sized general area outside of the structure; but (3) it would be higher in the trash pit or midden and in pottery manufacture areas.

Qualitatively, a similar situation can be set up for detailed laboratory analysis. In this case, however, more specific contrast sets should be established.

Another criterion of boundary formation which we found to be applicable in our research was the intensity at which some activity proceeds. These were defined as contextually specific contrastive units. The example just given concerning qualitative pottery counts is again applicable. In this instance, however, analytically derived data would be used to make interpretations of a different order. Another demonstrable example would be the number of posts per square foot within the provenience of a house as opposed to an equal area outside of the house. It can then be hypothesized that post setting involved in house construction will proceed with greater intensity than that which is associated with the construction of drying racks or storage platforms. Data so derived will form contrastive sets of behavioral clusters, e.g. house posts set by digging holes as opposed to those set by jamming the post into the ground.

In the attempt to demonstrate such an hypothesis, the traditional techniques of coring or circumferential excavation of pits and post molds was abandoned and in its stead, all potential pits and posts were cross-sectioned. In this manner, more data were made immediately available from which additional hypotheses could be derived and tested. For example, after cross-sectioning approximately a dozen potential posts, we were able to hypothesize that: (1) posts would be either rounded on the end or would be pointed with the point off-set to

one side and (2) post impressions would be approximately three times as deep as their diameter. In addition, it was possible to determine the diameter of the post, how it was set-- jammed into the ground vs. a dug post hole-- and the angle at which it was set. It was also possible to more accurately differentiate between posts and rodent burrows.

In applying the cross-section method to pit excavation, it was possible to hypothesize single or multiple prehistoric pit excavation, and to then demonstrate singularity or multiplicity of use and in some instances, the actual prehistoric order of excavation. In this manner also, it was possible to derive both quantitative and qualitative data from the pit fill proper.

Contrast sets may be used to provide a third criterion for formation of behavioral boundaries. In this instance, these are set up in terms of the use of spacial dimensions, and should include both horizontal and vertical space. For example, in the horizontal dimension, one might oppose intrastructure fire basins into extra-structure fire basins or intra-structure cache pits to extra-structure cache pits.

In vertical space, a somewhat more quantitative approach may be required. In this case, one might contrast the amount of vertical space required for pits as opposed to a comparable area used for above-ground storage facilities. In a more qualitative sense, stratigraphic sets may also be derived for activity delineation.

To this point, comparisons both in terms of relevance and boundary formation have been founded principally on a statistical basis. This, however, does not necessarily have to be the case and an additional means of separation may be derived through other sorts of data or those previously mentioned may be further substantiated.

An additional means of derivation, particularly in terms of boundary formation and even more specifically in terms of space contrasts was tested also. This included chemical soils analysis and contrastive photography through the use of infra-red, ultraviolet, standard black-and-white, and color media.

In the case of the chemical analysis of soil samples, the results which are currently available are preliminary, but even so, some meaningful information is available. On the basis of these data, it was impossible to make in-the-field differentiations between rodent burrows and potential post stains with about 85% accuracy. Even more importantly, chemical analyses proved to be quite valuable in the vertical delineation of human activity within the site.

Through stratigraphic inspection and the relative placement of such occurrences as hearths, post stains, and trash pits and storage pits, we hypothesized at least three distinct vertical levels of human activity. Soil samples were taken every one-half inch through the depths of the cultural deposit and two distinct chemical breaks were noted. These corresponded closely to the visible stratigraphic breaks.

The experimentation with different photographic media was somewhat less rewarding. A part of our difficulty may lie in yet undeveloped interpretation techniques.

An attempt was made to establish contrastive sets in all special photography situations. This proved to have some drawbacks in some instances, particularly with the ultraviolet medium. Ultraviolet requires the exclusion of all

visible light, i.e. it is principally night work, and as a result, the contrastive standard photographs will have to be made by flash. The major success of ultraviolet versus normal range photography and vision came in the definition of the vertical profile of a pit. The limits of the pit were not visible to the naked eye or on black-and-white film but were sensitive to ultraviolet light. Once the ultraviolet print was made, and the outline of the pit became visible, it was also possible to visually delineate it under normal light.

Infra-red produced two results. First, we found that slow-speed black-and-white infra-red sensitive film produced better tonal contrasts than standard wave length film, allowing us to hypothesize earlier in the excavation sequence where pits might occur. Secondly, infra-red spectrum photography indicated a "stain" in one unit which was quite similar to those of pits present in other units. This "stain" was not readily visible under the normal visual range nor did further excavation show the presence of concentrated cultural debris. In this case, the clue seems to be present but its significance in structuring excavations are still forthcoming.

In summary and in conclusion, the systemic approach can be of great value in field research. It strengthens Ford's idea that no archaeologist should go to the field without a problem in mind. This does not necessarily mean that the researcher must go into the field with specific sets of contrasts to be tested. He may instead, have a general problem in mind and on the basis of excavation, derive hypotheses and test them as the excavation progresses.

Field work is then oriented to a specific set of patterns which will be hypothesized, tested, and subsequently demonstrated to exist or they will be refuted. In addition, it removes the priori or intuitive approach to research and brings about the adherence to technique method and theory specific research. Finally, by following an excavation program such as the one previously suggested, archaeological research must become a scientifically conscious process.

#

AN UNUSUAL HOUSE CONSTRUCTION IN MISSISSIPPI

Samuel I. McGahey

Mississippi Archaeological Survey

During the summer of 1969 the Mississippi Archaeological Survey and a team from the University of Mississippi joined in the salvage excavation of the Winford Site, a Parchman Phase site in the Upper Sunflower area of the Yazoo Basin. This site is number 15-0-10 according to the Phillips, Ford, and Griffin system. The site had been plowed to a depth of about 18 inches over a period of several years and not much was left above that level. Therefore, heavy machinery was used to remove the first 18 inches of soil. What was revealed was, to us, most unusual.

We would expect to find the wall trenches, which were 37 feet in length plus or minus a few inches, but what was contained within the post mold pattern was completely unexpected. We have a network of very large posts up to a foot in diameter. There were 12 in each direction making a total of 144. These were

almost perfectly aligned to the direction of the trenches. In the center was a large post which measured 3 feet across and $8\frac{1}{2}$ feet deep from the surface. Connected with this was a long narrow pit about 10 feet long which sloped towards the post mold. Intrusive into the pattern were two more of these bathtub-shaped pits with what were apparently deep large post molds in the deep end. These were not nearly as deep as the center posts of what we call House # 1. Possibly these were connected with a similar pattern which has been plowed away over the years.

There must have been a raised floor in this house because there just is not room for any kind of activity-- the posts are too close together. We think that the pits were dug to facilitate raising of the center post of House # 1 and two houses of a later date.

Outside of the house on all four sides, there was a pattern of post molds which are quite similar to the ones found inside of the house. They were up to a foot in diameter. On the south side there is evidence of possible supports for an entrance way of some kind.

We have only begun to research the literature on this type of structure, and so far we have only been able to find one account which might be pertinent. In the account of the DeSoto expedition by Garcilasco de la Vega (Florida of the Inca, translated by John and Jeanette Warner, 1951, University of Texas, Austin, p. 554) he states that as the expedition was returning to the river on the west side to go downstream, they encountered a group who were taking refuge from a flood in structures from 18 to 24 feet off the ground. These structures were surrounded on four sides by corridors in which food and provisions were stored. So far, this fits rather nicely the structure at the Wilford Site. There is evidence of corridors on all four sides and apparently a raised floor. There was not too much detail in the de la Vega account and no mention was made of center posts or stairways, but this just might not have been mentioned.

A few feet to the southwest three more houses were uncovered. These are two reconstructions on approximately the same spot. The houses were oriented in the same direction as the larger house. The dimensions of the larger house in this area (House # 2) measured 28 feet on a side. In the approximate center of all three houses there was a large bathtub-shaped pit which sloped from one end to the other and had a large post mold in the deep end which was 18 inches in diameter and perfectly round. It was 7.38 feet below a section of burned floor which was found in one area. Unfortunately, we were not able to tie the burned floor to either one of the houses. This was apparently preserved because a historic house was over this spot until a fairly recent date and the edge of the road is relatively close.

The pattern uncovered in this second area is not as neat as the pattern present in House # 1, but the similarity is striking. The post molds were up to a foot in diameter and there is, again, the possibility for rows of support posts. One large post mold in the deep end of one of the pits could have been associated with either of the three superimposed houses, but it was exactly in the center of the largest house (# 2) and very close to the center of House # 3.

It was impossible to enlarge the excavation to determine exactly what was on this site because of a nearby cotton-patch.

In the vicinity of the house sites there is a small rectangular mound situated about 60 feet away. Mention was made in the 1951 survey report of a

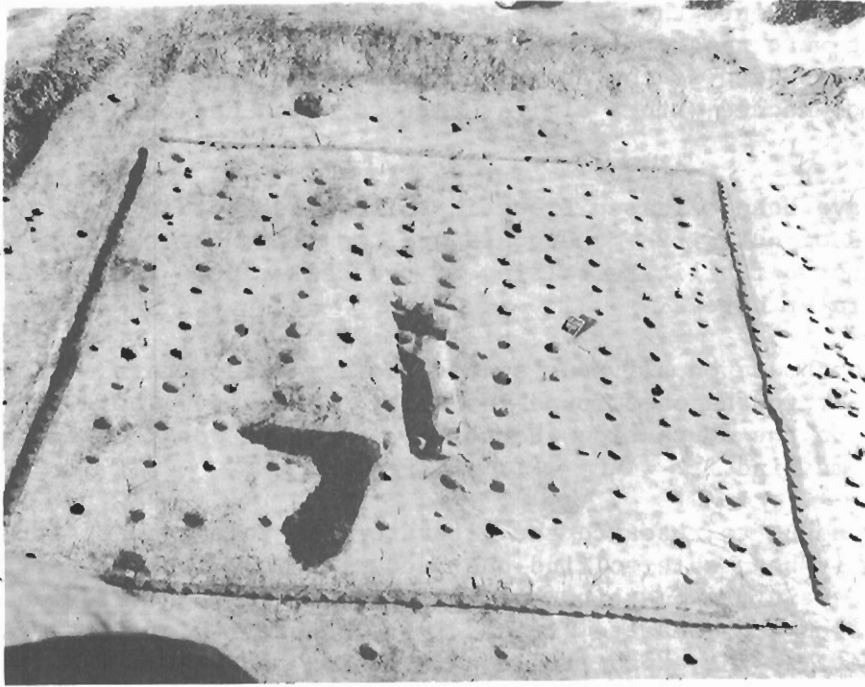


FIGURE 1- House 1 at the Wilford Site, Coahoma County, Mississippi, excavated by the Mississippi Archaeological Survey.

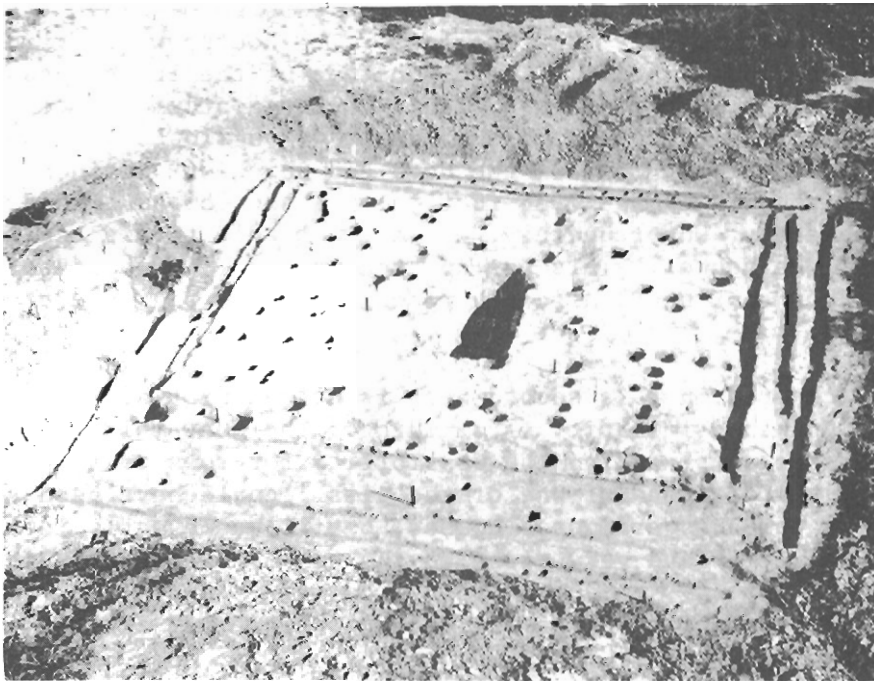


FIGURE 2- Houses 2, 3, and 4 at the Wilford Site.

smaller mound, but we found no traces of it.

In connection with the Wilford Site house pattern, the Hays Site should also be mentioned. The latter site is about 20 miles due south of the Wilford Site, both being in Coahoma County, Mississippi. We have not finished the pottery analysis, but most of it is shell-tempered; therefore the Hays Site is of the Mississippian Period. At Hays there were an apparently earlier wall trench house and a later house with only the post molds in evidence. The post mold house was also square. In the approximate center of both of these houses, but more nearly in the center of the later post mold house, there was a large hole which measured two feet across and 5.1 feet in depth.

When we excavated this site and did the drawings of the house patterns, we were not aware of the Wilford Site patterns. We included everything that we thought might be pertinent to the house. There was a center post and a pit about 10 feet long which was intruded into by the post. This pit was also sloping in the direction of the center post. There were also apparently what were support posts inside the house, although they were not in as neat a grid pattern as those at the Wilford Site, but very suggestive nevertheless.

We have taken wood samples wherever we found them to be used in identification of species. The center post at the Hays and two center posts from the Wilford Site were bald cypress. The posts from the wall trenches were ash, red oak, white oak, elm, hickory, and pecan. We also found bald cypress from the interior support posts at the Wilford Site.

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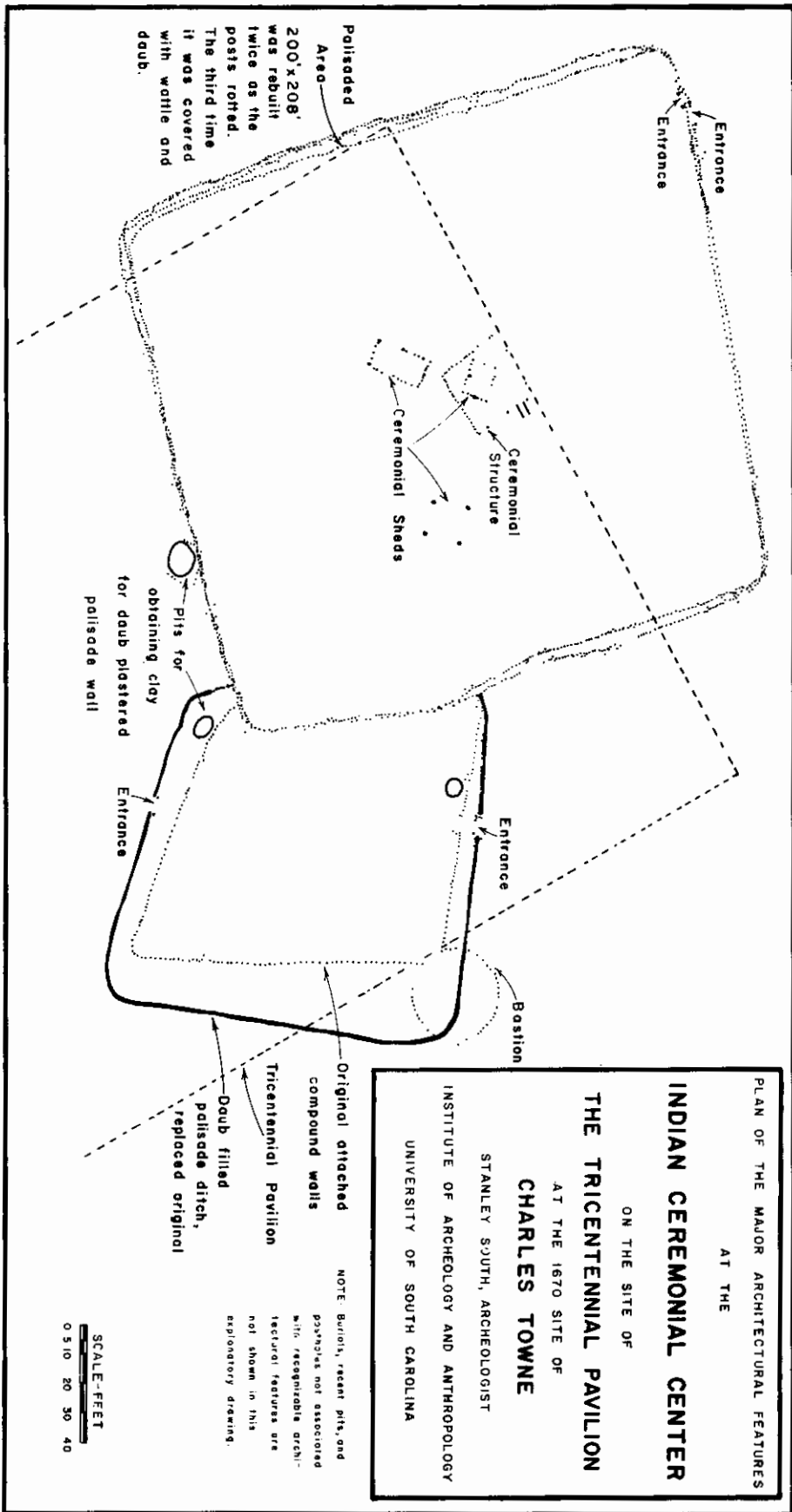
A CEREMONIAL CENTER AT THE CHARLES TOWNE SITE

Stanley South

University of South Carolina

During the excavation of the site of the 1670 settlement of Charles Towne on the south side of the Ashley River in Charleston, South Carolina, a request was made by the Tricentennial Commission for a survey of the site chosen for location of the exhibit pavilion. This project was expected to take two days of exploratory trenching in order to determine if any features were present that should not be destroyed by the construction of the pavilion. The two day project revealed a Pee Dee Complicated Stamped vessel, an incised vessel, one burial, and a line of postholes for a palisade. Due to these discoveries it was three months before the project was brought to an end by the construction of the Tricentennial Pavilion on the site.

This report is a general summary of the features found during this salvage project. The palisade enclosed an area generally two hundred feet square, and was found to have been rebuilt twice after the original construction. The first two palisades were not plastered, but the third had been covered with red clay daub, fragments of which were found in the postholes. Near the center of this compound, postmolds for a structure with entranceway trenches were found. At the time of the first rebuilding of this compound an additional compound was constructed adjoining the north corner of the original enclosure. This enclosure is of particular interest in that it had a circular row of postholes at the



PLAN OF THE MAJOR ARCHITECTURAL FEATURES
AT THE
INDIAN CEREMONIAL CENTER
ON THE SITE OF
THE TRICENTENNIAL PAVILION
AT THE 1670 SITE OF
CHARLES TOWNE
STANLEY SOUTH, ARCHEOLOGIST
INSTITUTE OF ARCHEOLOGY AND ANTHROPOLOGY
UNIVERSITY OF SOUTH CAROLINA

SCALE--FEET
0 5 10 20 30 40

west corner, measuring 32 feet across, which represented a bastion for protection of the entranceway found between this bastion and the original two hundred foot square compound wall. Destruction of the site by the pavilion construction prevented an examination of the features inside this smaller bastioned enclosure.

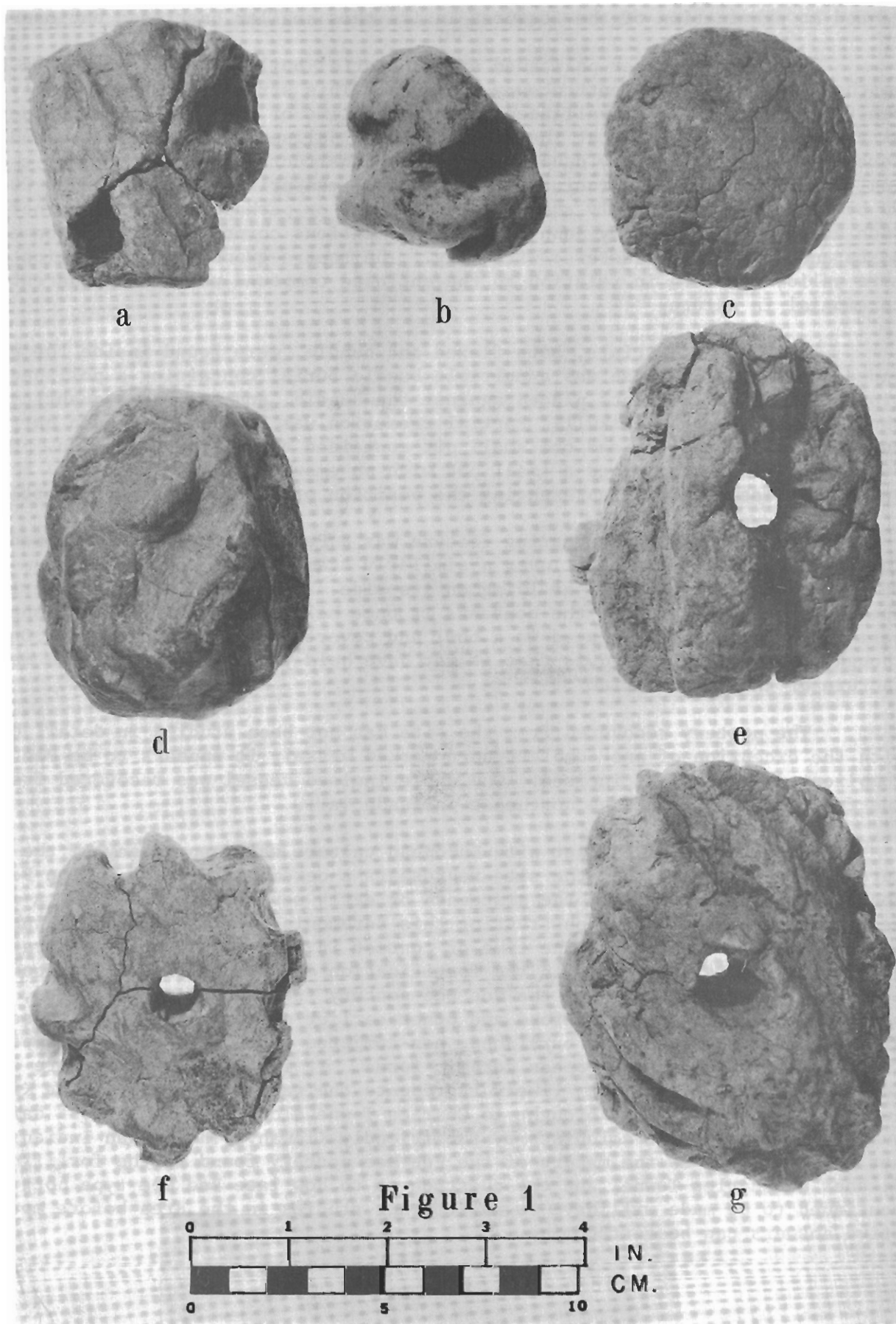
Inside the larger palisaded area a central building was represented by postholes and a pair of entranceway trenches. This structure was apparently the central focus within the palisaded area at the time of its original construction. At a later time, probably at the time the third palisade was constructed and plastered with clay daub, clay daubed sheds were constructed over the site of the original centrally located structure. These sheds, measuring 13 by 13, 10 by 20, and 10 by 12 feet were possibly square-ground sheds, or perhaps, communal corncribs such as those described by John Lawson. He mentions corncribs standing on eight posts and completely plastered with clay. The square-ground interpretation may be more valid, however, in relation to the enclosed compound which was apparently ceremonial in nature.

A number of burials, both flexed and bundle, were found inside the palisaded area. One of these was a multiple bundle burial containing a polished stone disc, mica fragments placed against the bundle, and one had a quantity of shell beads beneath the skull. Partial cremation was seen to have been carried out on several bundles, and one small pit contained a complete cremation. One bundle burial pit contained a small pot with an embryo, beside which were seven quartz pebbles probably representing the position of a wooden or gourd rattle.

A particularly interesting feature was a Pee Dee Complicated Stamped urn that had been placed in a pit over a similarly stamped bowl containing a smooth-surfaced, constricted-neck jar lying on its side. The layer of humus material in the jar gave no clue as to the contents it once held, and we are left to speculate why a jar would be placed in a bowl and covered by an urn in a pit. One thought that comes to mind is that it may have held yaupon leaves for the manufacture of the "black drink".

The pottery recovered from the site has not been studied in detail but, with the exception of one complete cord marked vessel, it appears to be Pee Dee Complicated Stamped and incised. One whole incised vessel was recovered from one feature just inside the northeast palisade wall.

No evidence of contact with European culture was seen in any of the features, and it appears that the site represents a ceremonial center of the Pee Dee Focus, probably dating from the sixteenth or early seventeenth century. A connection between the group constructing this ceremonial center and the Kiawah Indians occupying the area at the time of contact in 1670 cannot be demonstrated. However, some distance away, in the fortification ditches of the 1670 colonists at Charles Towne, a number of the Pee Dee pottery fragments were recovered, as were large quantities of Deptford and other earlier pottery types. In these ditches were fragments of a burnished, non-tempered ware, some of which appear to indicate influence from European ceramic forms. It is this burnished ware of the Colono-Indian type that very likely represents the ware being made by the Kiawah Indians of the 1670's and brought to Charles Towne along with the almost daily supplies furnished the colonists by the friendly Kiawah. The Kiawah Indians cannot be identified as the makers of the Pee Dee pottery found in the fortification ditches at Charles Towne, and the question as to how late Pee Dee type pottery continued to be made will have to await other studies in the area before more positive answers can be forthcoming.



BAKED CLAY OBJECTS FROM THE SITE OF THE 1670 SETTLEMENT
AT CHARLES TOWNE, SOUTH CAROLINA

Stanley South

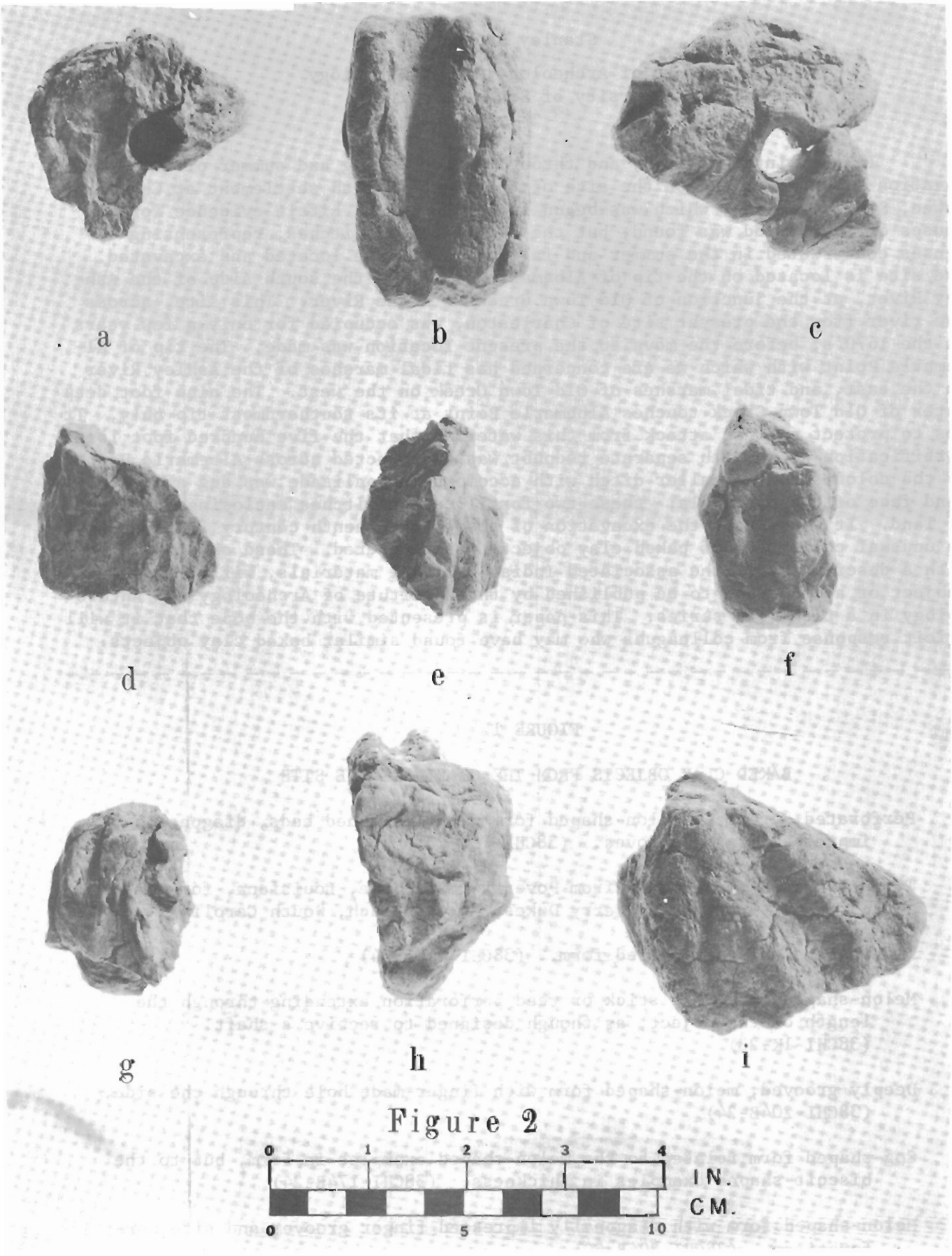
Institute of Archeology and Anthropology
University of South Carolina

In the winter of 1968, and throughout the spring and summer of 1969, excavation was carried out on the site of the first English settlement at Charles Towne, South Carolina, which was begun in April, 1670. Little evidence for the houses of the period was found, but the fortification ditches, representing defenses constructed in the summer and fall of 1670, were located and excavated. The site is located on the tip of Albemarle Point, on the south side of the Ashley River, at the junction of Old Town Creek with the River. This site, across the river from the present site of Charleston, was occupied for only a few years in the 1670's, before the move to the present location was made. The tip of Albemarle Point with which we are concerned has tidal marshes of the Ashley River on the east, and tidal marshes of Old Town Creek on the west. The nine foot deep water of Old Town Creek touches Albemarle Point at its southernmost tip only. It was to protect against attack from this waterway that the five hundred foot long fortification ditch with separate redoubt was constructed across Albemarle Point by the colonists. A smaller ditch with accompanying palisade was dug along the land face of the peninsula; these two fortification ditches enclosing ten acres of land. It was during the excavation of these seventeenth century features that an unusual collection of baked clay objects was recovered. These objects, along with a description of the associated Indian cultural materials, will form the subject of a monograph to be published by the Institute of Archeology and Anthropology in a technical series. This paper is presented with the hope that it will elicit response from colleagues who may have found similar baked clay objects.

FIGURE 1

BAKED CLAY OBJECTS FROM THE CHARLES TOWNE SITE

- a. Perforated, grooved, melon-shaped form with flattened ends, diagonally impressed finger grooves. (38CHI-154-23)
- b. Typical cross-grooved form from Poverty Point Site, Louisiana, for comparison. (Donated by Jerry Dukes, Myrtle Beach, South Carolina)
- c. Imperforate, biscuit-shaped form. (38CHI-230B-34)
- d. Melon-shaped form with stick or reed perforation extending through the length of the object, as though designed to receive a shaft. (38CHI-1K-23)
- e. Deeply grooved, melon-shaped form with finger-made hole through the side. (38CHI-204B-24)
- f. Cog-shaped form related to the melon-shaped examples in form, but to the biscuit-shaped examples in thickness. (38CHI-174B-24)
- g. Melon-shaped form with diagonally impressed finger grooves and side perforations. (38CHI-204B-60)



The Charles Towne baked clay objects are unusual in that they are not like those described from the Poverty Point Site (Ford and Webb 1956) or the Jaketown Site (Ford, Phillips, and Haag 1955), yet have an apparent relationship to those objects. The Charles Towne baked clay objects are different in that they are almost invariably furnished with holes, either longitudinally, or laterally. The holes are sometimes made with the finger, sometimes with a smooth round cane or stick, and sometimes with a small tapered stick, in which case there are numerous holes either going completely through the object or stopping before reaching the opposite side (Figs. 1-4).

The Perforated Grooved Melon-shaped Baked Clay Objects from Charles Towne

The form of the Charles Towne objects can be described as massive in comparison with the typical Poverty Point objects, some of the barrel shaped, or cylindrical forms being four inches long and three inches thick. There are two basic forms represented at Charles Towne; the perforated grooved melon-shaped and the perforated biscuit. Forty melon-shaped fragments and fifty-three biscuit fragments were found. The perforated grooved melon-shaped form is related to Poverty Point Type F, Melon-shaped (Ford and Webb 1956:40-41), but is longer, more cylindrical, and flatter, and more massive in appearance, but can perhaps, still fall within the basic Type F of Poverty Point, except for perforation through the end or the side. Another example, from Jaketown, and included in the Cross-grooved type from that site, is seen from the illustration (Ford, Phillips, and Haag 1955: Fig. 12b-d) to be closely related to the Charles Towne melon-shaped type. The finger impressed grooves at various angles over the surface of the Charles Towne objects are deeply impressed by small fingers in many cases, and only slightly indented in others. The holes through these melon-shaped baked clay objects were made by small fingers, usually one-half inch across. One exception is a smooth-sided hole apparently made with a reed or cane, or smooth stick, as though it were intended to be used on a shaft (Fig. 1d). Another exception, but included in the melon-shaped type is shorter than it is wide, being three inches wide and only half that in length, the finger grooves giving the appearance of a cog-wheel (Fig. 1f). Such an example is illustrated in the Jaketown report (ibid.: 44, Fig. 12d). The hole in this example is different in that it is cut from opposite sides using a flat cutting tool, such as a flat sliver from a cane.

The clay from which the objects are made is not tempered, though in some examples there are a number of holes made by a fibrous material that seems to have been incidentally included with the clay. Some examples of the perforated

FIGURE 2

FRAGMENTS OF THE PERFORATED
GROOVED MELON-SHAPED FORM FROM CHARLES TOWNE

- a. Top view of perforated, grooved, melon-shaped fragment. (38CHI-154B2-1)
- b. Side view of perforated, grooved, melon-shaped fragment. (38CHI-232B-24)
- c. Top view of melon-shaped fragment with finger perforation. (38CHI-204B-61)
- d-i. Perforated, grooved, melon-shaped fragments. (38CHI-34A-23; 38CHI-2-4B-55; 38CHI-154B7-24; 38CHI-204B-56; 38CHI-204B-50; 38CHI-204-59)

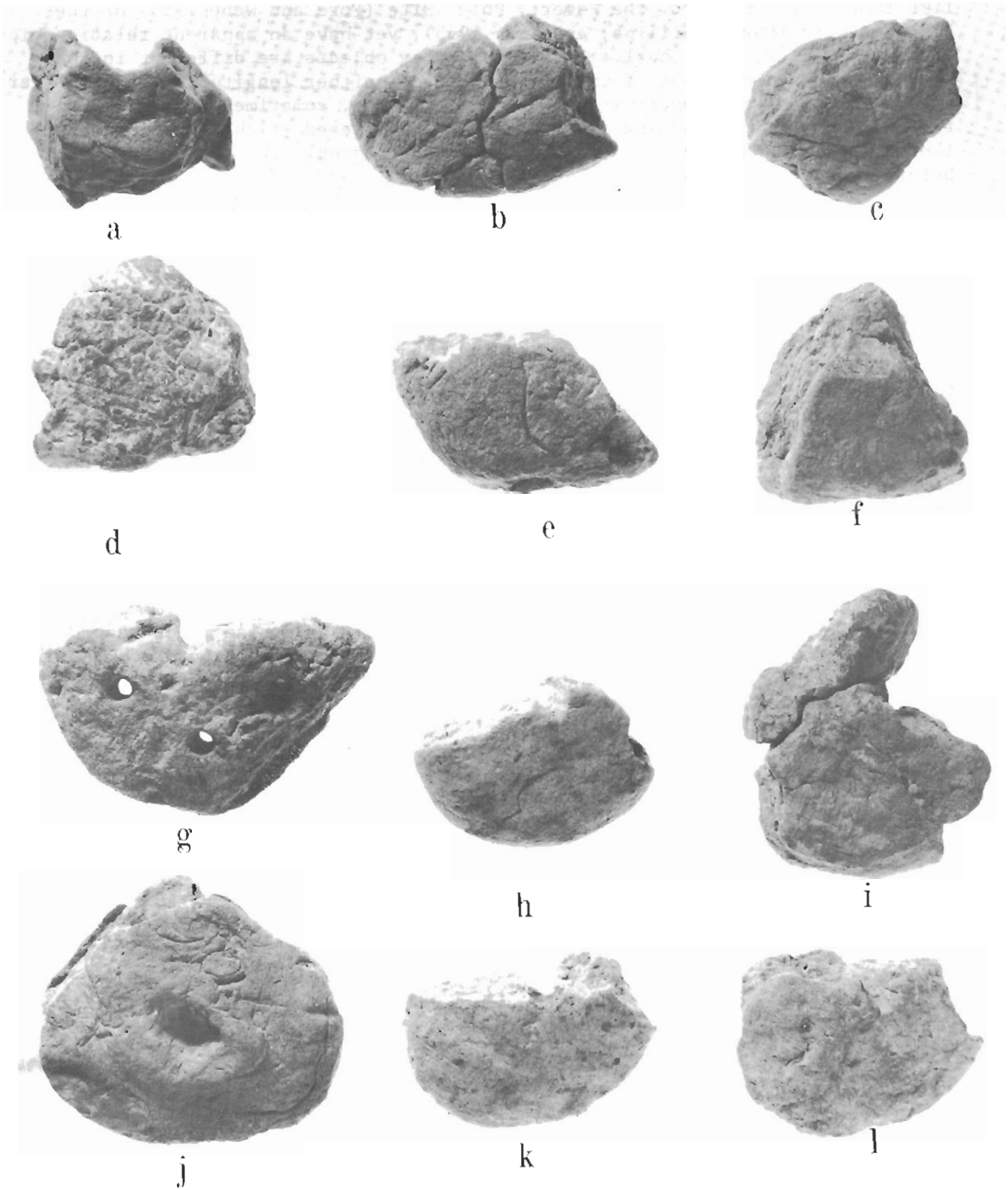
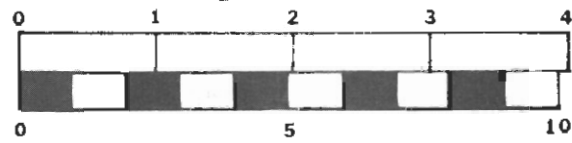


Figure 3



1 IN.
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grooved melon-shaped type are flattened on the end, and two such examples are incised with a series of parallel and zig-zag lines as a decorative treatment (Fig. 4h,i).

The Perforated Biscuit Form of Baked Clay Object from Charles Towne

The biscuit form from Charles Towne resembles a hand shaped biscuit, thicker in the middle, and round to irregularly round approaching triangular in shape (Fig. 3). A biscuit form is described from Poverty Point as coming from only one fire pit, where 45 examples were present (Ford and Webb 1956:42,44). The biscuit form was not illustrated in the Poverty Point report, but it is assumed that the Charles Towne examples are perhaps quite similar, with the exception of the holes in the Charles Towne examples, of course. One example from Charles Towne did not have either a central hole or the smaller holes, and may be close in form to the biscuit type from the Poverty Point pit (Fig. 1c).

The perforated biscuit form from Charles Towne most frequently has a central hole made with a small tapered dowel one-quarter inch in diameter (Fig. 3g-1). One example had a central finger hole, similar to the perforated melon-shaped type (Fig. 3a). Some fragments reveal a number of one-quarter inch holes, some of which barely break through the surface on the opposite side of the biscuit, and some that do not extend through at all (Fig. 3g, 4k,1). Only one example was not perforated (Fig. 1c). One fragment has numerous small punctations apparently applied as a design motif (Fig. 4d). Several examples are incised with parallel or cross-hatched lines (Fig. 4b,c,e-g), similar to an example from the Poverty Point Site (*ibid.*: 42-43, Fig. 14j). Two fragments are of particular interest in that they are simple stamped, apparently with a carved paddle (Fig. 4a).

Some decorated examples of baked clay objects were found at Poverty Point, and in terms of the artificial cooking stone interpretation, the decorations do not neatly fit, and may, as Ford, Phillips, and Haag have pointed out, represent objects with a different function than cooking stones (Ford, Phillips, and Haag 1955: 55). The zig-zag, cross-hatched, parallel incised decorative lines, and punctations on the Charles Towne examples would also imply a function

 FIGURE 3

BISCUIT-SHAPED BAKED CLAY OBJECTS
 FROM THE CHARLES TOWNE SITE

- a. Finger perforated, biscuit-shaped fragment. (38CHI-154B3-23)
- b-f. Fragments of biscuit-shaped forms. (38CHI-154B4-23; 38CHI-154B5-23; 38CHI-207C-32; 38CHI-154B13-23; 38CHI-13-155)
- g. Perforated biscuit-shaped form with several holes. (38CHI-2AA-1)
- h,i,k,l Biscuit-shaped forms with central perforation. (38CHI-154B-23; 38CHI-154B4-23; 38CHI-2A-23; 38CHI1-2-43).
- j. Biscuit-shaped form with central perforation cut with sliver of reed or stick, similar to steatite forms from Stallings Island. (38CHI-154BI-23)

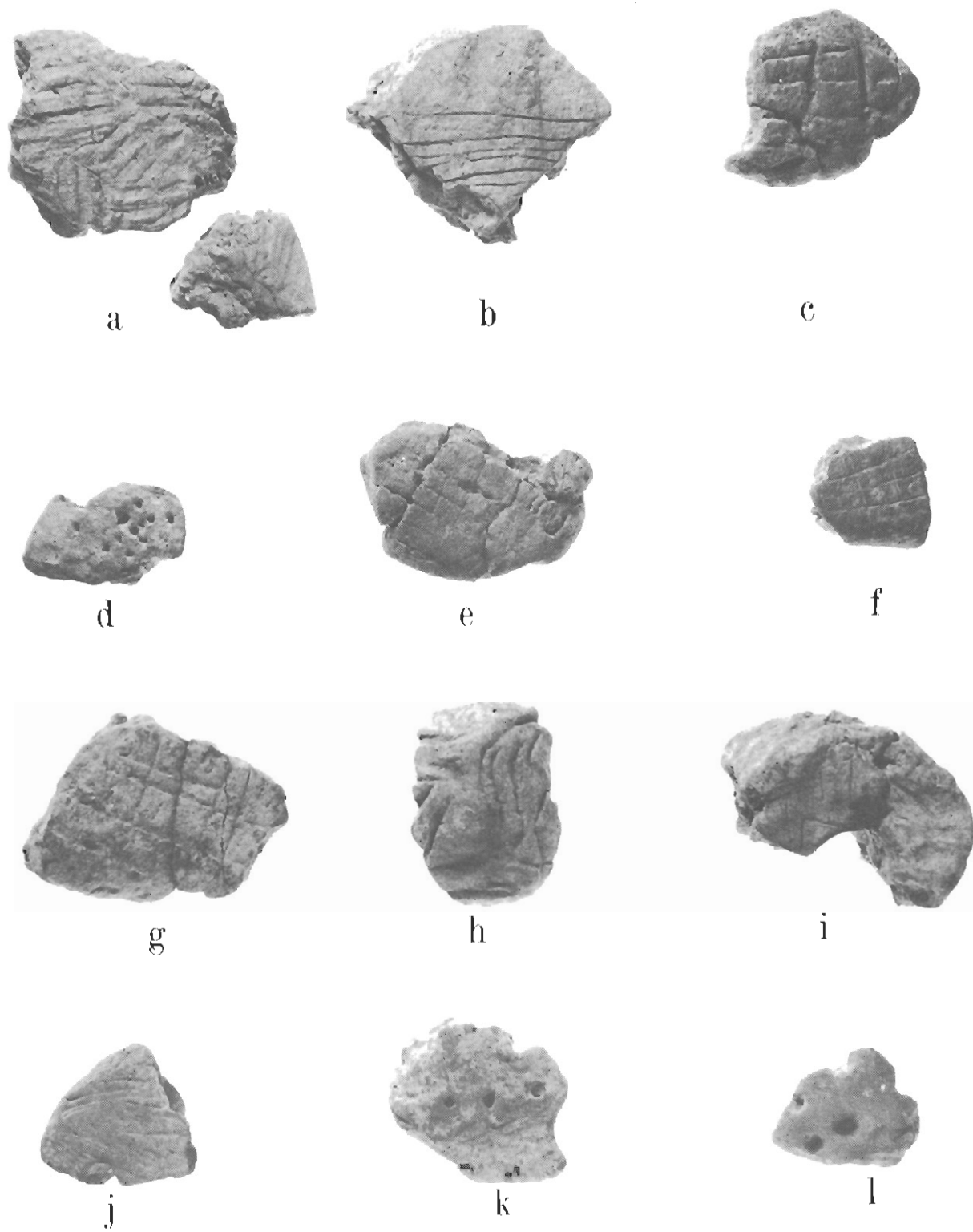
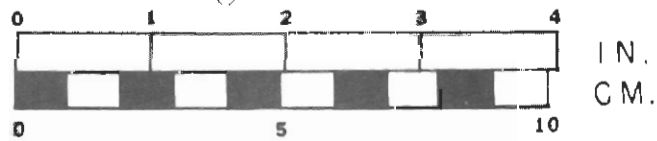


Figure 4



other than cooking stones, unless these were repeatedly reused without damage. The authors of the Jaketown report state that: "... we have problems of our own, but apparently decoration is not one of them" (*ibid*: 55). The decorated baked clay objects from Charles Towne present a problem not encountered at Jaketown.

The Provenience of the Baked Clay Objects from Charles Towne

In the process of stripping the plowed soil from above the area of the 1670 fortification ditches at Charles Towne, and above the area ten feet each side of the ditches, Indian pottery and baked clay objects were recovered. These lay beneath the plowed soil zone in the yellow sand containing no visible humus; the layer visually appearing to be undisturbed subsoil sand. After exploratory test trenches were cut by hand and the location of the 17th century ditches established, front loaders were brought into the area and the top plowed soil zone was removed from the main fortification ditch as well as the fortification ditch and palisade line along the land face of Albemarle Point. When this was done the shovel crew was used to schnitt off the exposed yellow sand layer lying just beneath the removed plowed soil zone. In the process of cleanly schnitting this layer for photographing and measuring the discolored ditch and other associated features, baked clay objects and Indian pottery were found. All objects found in the yellow sand layer in this manner were assigned a special provenience designation, and whenever the object was still laying *in situ*, photographs were made,

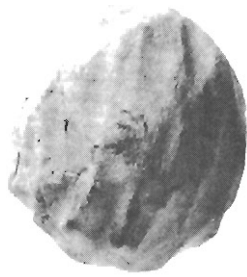
FIGURE 4

INCISED, PUNCTATED AND SIMPLE STAMPED
BAKED CLAY OBJECTS FROM CHARLES TOWNE

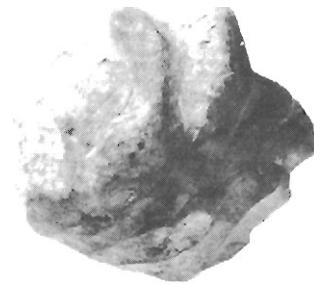
- a. Simple stamped, biscuit-shaped fragments, possibly indicating a later time period than previously known in the Southeast for baked clay objects. (38CHI-2-49; 38CHI-55A-24)
- b. Parallel line incised, biscuit-shaped fragment. (38CHI-200B-24)
- c. Cross-hatched, incised, biscuit-shaped fragment similar to examples from Poverty Point, Louisiana. (38CHI-121B-23)
- d. Shallow punctations applied as decoration on a baked clay object. (38CHI-200B-27)
- e-g. Cross-hatched, incised decorated, biscuit-shaped fragments. (38CHI-207A-36; 38CHI-55-23; 38CHI-73B-1)
- h. Incised decorated, flattened melon-shaped form; flattened end decoration in circle to right, body incising to left. (38CHI-200B-26)
- i. Top view of flattened end, melon-shaped form with zig-zag incised decorative lines around central, finger-perforated hole. (38CHI-200B-25)
- j. Scraped surface, biscuit-shaped fragment similar to plain sherds from the site. (38CHI-175B-26)
- k. Biscuit-shaped form with several small punctations extending through the disc. (38CHI-2N-37)
- l. Biscuit-shaped fragment with punctations that do not perforate the body. (38CHI-141-23)



a



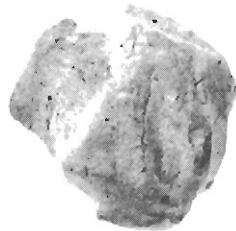
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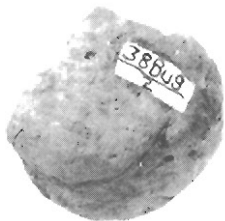
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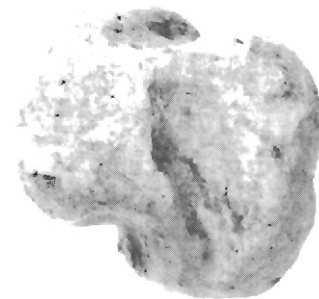
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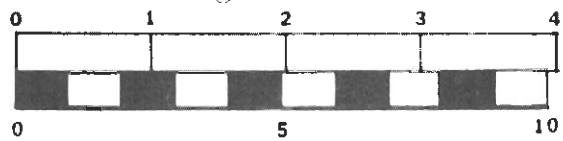


h



i

Figure 5



I N.
C M.

and the position of each clay object was recorded. Control squares were excavated in order to attempt to determine the depth of the artifact concentration within the yellow sand layer, and these revealed the same situation as seen elsewhere, i.e., the artifacts were apparently confined within three inches of the bottom of the plowed soil zone. In excavating the fortification ditches, fragments of baked clay objects were also recovered, having arrived there in the process of excavation and subsequent refilling of the fortification ditches. In fact, the majority of the pottery recovered from the 17th century fortification ditches was pre-17th century Indian in origin. In only a few instances could any sign of humus be seen to accompany the pottery and clay objects in the yellow sand layer, one of these being near the artillery redoubt near the tip of Albemarle Point. Here several baked clay objects were found lying together in a yellow sand matrix slightly stained with humus, but not presenting a definite pit outline that could be traced.

Similar instances of cultural material in sand layers, where the humus is no longer seen due to thousands of years of water penetration through the layer, are often seen on Archaic sites. This fact alone would tend to indicate a date of several thousand years for the clay objects on the Charles Towne Site. Fortunately better dating through association with pottery types also recovered in this yellow sand layer is available to us.

Cultural Material Associated in the Same Layer with the Baked Clay Objects

An analysis of the cultural materials from the yellow sand layer will be included in the more definitive report on the Charles Towne excavation, and only a listing of associated objects is presented here. Several steatite sherds were present, along with a Morrow Mountain projectile point, half of a hematite atlatl weight, and as yet unidentified mineralized vertebra of a large animal. Fiber-tempered plain pottery, a non-tempered plain type, a sandy plaste plain type, a non-tempered type with stamped impressions of what appears to be the rounded edge of a smooth paddle, Deptford Linear Check Stamped, Deptford Bold Check Stamped, and Thom's Creek Punctated (drag and jab mode) (Phelps 1968:20), were recovered

FIGURE 5

BAKED CLAY OBJECTS FROM DAWS ISLAND IN PORT ROYAL SOUND, BEAUFORT COUNTY, SOUTH CAROLINA (38BU9), APPARENTLY RELATING TO THE BILBO SITE IN GEORGIA

- a-c. Melon-shaped, baked clay objects nearer in form and size to the Poverty Point examples than are the Charles Towne specimens. (38BU9-21; 38BU9-27; 38BU9-26)
- d. Spherical-shaped form with flat paddle edge impressions forming a pattern over the surface. (38BU9-23)
- e. Melon-shaped fragment. (38BU9-24)
- f,h. Spherical-shaped form with slight finger impressions. (38BU9-25; 38BU9-2)
- g. Spherical-shaped form. (39BU9-22)
- i. Cross-grooved form from Poverty Point, included for scale and comparison. (Donated by Jerry Dukes, Myrtle Beach, South Carolina)

from the yellow sand layer (Griffin 1943: 155-68; Waddell 1963). A study of the clay of these types, comparing the presence of fine sand with the relationship of sand to clay found in the Ashley River, clay used in firing bricks for a number of early plantation houses in the Charleston area, and bricks found in the fortification ditches at Charles Towne indicates that the sand seen in these Indian types is presented as natural inclusions in the clay found in the area. Also present in the local clay, and seen in both the early Indian types and bricks made locally, are organic inclusions which, when fired, leave black pockets or holes in the Indian sherd or Colonial brick. The final report on the early cultural material from the Charles Towne Site will include the results of this study of local clays now being undertaken. The results seen so far indicate that the percentages of sand to clay existing in some Indian sherd types may well not be the result of intentional sand tempering formulas utilized by the Indians, but merely a mixture found in natural clay deposits available to them. An examination by archaeologists of locally made, non-tempered bricks may reveal surprisingly similar "sand temper" to that seen in Indian sherds of the area under study. Caution would therefore seem to be warranted before a sherd is identified as sand tempered or non-tempered.

The non-tempered, sandy-clay sherds, and the fiber-tempered sandy-clay sherds are apparently made from the same clay source as the Deptford Linear Check Stamped sandy-clay sherds, i.e., water deposited sandy-clay. Other Deptford Stamped sherds have a greater quantity of slightly larger sand in the mixture, but this too may be present in the local clays, and caution as to the "sand tempered" character of these types is necessary before type descriptions are written.

Function of the Baked Clay Objects from Charles Towne

The interpretation for the classic Poverty Point baked clay objects is generally considered to be as substitute, or artificial, cooking stones (Ford and Webb 1956: 39). However, the Charles Towne perforated melon-shaped type, and the perforated biscuit type introduce an additional feature to be interpreted, that may or may not be satisfactorily explained by the artificial cooking stone interpretation. Perhaps the hole aided in handling the hot objects, but with the deeply impressed finger grooves on many examples, a hole through the side or end of the object would seem to offer but little additional advantage in this regard. One example (Fig. 1d), had a very smooth, parallel-sided hole that would appear to be designed to take a shaft, and the thought occurs that perhaps this particular example was designed as an artificial atlatl weight. One of the perforated biscuit forms (Fig. 3j) is very much like the perforated steatite objects found at Stallings Island (Claflin 1931: Plate 52), that have variously been suggested to have been net weights or steatite cooking stones. If the Stallings Island steatite objects were cooking stones, the Charles Towne example of similar form may be an artificial cooking stone of the Stallings Island type. This does not take care of the other objects with irregular finger formed holes, or those with small holes in the center and randomly over the body, and the only interpretation that might seem valid is that with the hole there may have been less fracturing of the clay during firing through better heat distribution. Admittedly this is a shaky suggestion, but one emphasizing that variation in form such as we see represented in these Charles Towne baked clay objects, requires an interpretation extending beyond that necessary at the Jaketown Site. In comparing the Poverty Point objects with similar examples from California, Ford, Phillips, and Haag state in regard to the many stone forms imitated in clay in that area:

These include earplugs, labrets, cup stones, sinkers, bolas, charm stones, etc. Whether these had the same function as their prototypes in stone or are simply clay "balls" of specialized form is for the California archaeologists to decide. It is as though we found bannerstones, boatstones, and plummets of baked clay in the Lower Mississippi similar in form to their stone counterparts in surrounding areas but similar to Poverty Point objects in composition and finish. Fortunately we do not, so we are not obliged to have an opinion on the question (Ford, Phillips, and Haag 1955: 54-55).

The variation in form between the Mississippi Valley baked clay objects and those found at Charles Towne (possibly pointing toward stone parallels) requires a special interpretation. Perhaps we will never be able to better the cautious conclusion of Ford, Phillips, and Haag when they said:

A judicious conclusion, on a continental basis, would be that only one explanation seems to answer most of the facts; that baked clay objects represent an invention, probably made more than once, in response to the household needs of a pottery-less people in a stone-less land (ibid.: 56).

The Time and Space Relationships of the Charles Towne Baked Clay Objects

From a comparison of the Charles Towne baked clay objects with those from Poverty Point and Jaketown, it appears that some of the "uncommon types" at Jaketown (ibid.: 44, Fig. 12), are close in form to the melon-shaped type from Charles Towne. The melon-shaped type from Poverty Point (Type F), and a biscuit-shaped form from one pit (Type I), are apparently equivalent parallels to the perforated melon and biscuit forms from Charles Towne (Figs. 1-4). Incised designs are also present at both sites (Ford and Webb 1956: 40-44, Figs. 13-14). It should be remembered, however, that the Charles Towne examples are perforated, an interesting difference from the Poverty Point and Jaketown examples. One cylindrical shaped form from Jaketown was illustrated as having a hole (or perhaps a finger impression), but no mention is made in the text of perforated forms (Ford, Phillips, and Haag 1955: 44, Fig. 12a). C. B. Moore does not indicate that perforated forms were present at Poverty Point (Moore 1913: 66-73), but in a recent study Clarence Webb has reported that five percent of the objects from the Poverty Point Site were typical or unusual, including perforated, miniature, and decorated forms (Webb 1968: 308-309, Figs. 2k-gg). This indicates the presence of perforated forms at Poverty Point, but apparently not like those from Charles Towne. Gagliano and Saucier (1963: 322, Fig. 2i) have reported perforated forms from the Linsley Site in southeastern Louisiana, and have illustrated one that appears to be a biscuit form with a central perforation, similar to some of the Charles Towne examples, which they refer to as an "unusual variety".

One of the most interesting parallels with the Charles Towne baked clay objects is reported by Webb from the Pearl River coastal Archaic shell midden sites examined by Gagliano (Webb 1968: 298, citing Gagliano 1963), located north of Lake Pontchartrain. Here were found "A few sandy baked clay objects, biscuit-shaped or pierced and grooved, and differing from the forms of Poverty Point objects..." (Webb 1968:298). This combination of pierced and grooved, and biscuit-shaped forms is that seen at Charles Towne.

Closer to the Charles Towne Site, on the Georgia coast, Antonio Waring, Jr., found a number of Poverty Point objects in the lower level of the Sapelo Is-

land Shell Ring in association with plain fiber-tempered pottery having a radiocarbon date of 1848 ± 250 B.C. (Ford, Phillips, and Haag 1955: 53; Williams 1968; Griffin 1952: 366). At the Dulany Site in Chatham County, Georgia, baked clay objects were also associated with fiber-tempered pottery, with a radiocarbon date of 1820 ± 200 B.C. (Williams 1968: 329). One cylindrical baked clay object was reported by Waring from the bottom of the shell deposit at the Bilbo Site in Chatham County, Georgia, and the radiocarbon dates are 1780 and 1870 ± 125 B.C. (Williams 1968: 330). The illustrated baked clay objects from the Sapelo Island Shell Ring are very similar in surface irregularities to the ones from Charles Towne, but apparently they were not perforated (Williams 1968: 276, Fig. 92c, 92d).

Closer yet to Charles Towne, on Daws Island, across the Broad River from Beaufort, South Carolina, a small shell mound or ring fragment (39Bu9) can be seen, washing away, and inundated at high tide. A collection from the area and the shell deposit was made by Tom Hemmings and Jim Michie (Hemmings 1969: 6). This included a number of baked clay objects with a sandy-clay paste, but fashioned in the spherical and melon-shaped forms more typical of Poverty Point than the Charles Town examples (see Fig. 5). Baked clay objects and plain fiber-tempered pottery were the only artifacts actually pulled from the eroding surface of the shell deposit. However, other artifacts were scattered along the beach, some of which, no doubt, came from the shell mound originally. Of particular interest in this latter group of objects is a small winged atlatl weight, a fragment of a smooth barrel-shaped atlatl weight, projectile points, a fragment of a steatite "net sinker" (or cooking stone), and a fragment of a finely engraved bone pin. All of these objects are identical with those illustrated from the Bilbo Site by Waring, promising an interesting parallel when the surviving remnant of the Daws Island Mound is archaeologically examined (Williams 1968: 168-178, Figs. 62-69).

A number of radiocarbon dates provide a good indication as to the likely dates for the Charles Towne baked clay objects. Gagliano and Saucier's date of 1740 ± 120 B.C. for the Linsley Site (Gagliano and Saucier 1963: 326), the Sapelo Island Shell Ring date of 1848 ± 250 B.C. (Ford, Phillips, and Haag 1955: 53; Williams 1968: 329; Griffin 1952: 366), the Bilbo dates of 1780 and 1870 ± 125 B.C. (Williams 1968: 330), and the Dulany Site date of 1820 ± 125 B.C. (*ibid.*: 330), all of which contained baked clay objects, clearly place a date approaching 2000 B.C. for the use of these objects. The association at the Bilbo Site, the Sapelo Island Shell Ring, and at the Dulany Site of clay objects with fiber-tempered pottery clearly points to the relationship of the fiber-tempered pottery from Charles Towne with the Charles Towne baked clay objects.

In his summary paper on the Poverty Point Culture Webb has said:

The picture emerges, therefore, of coastal peoples from Georgia /now South Carolina/ to Louisiana, between 2000 and 1000 B.C., who used baked clay balls, with or without stone or pottery vessels, in their cooking process (Webb 1968: 300).

Summary of the Baked Clay Objects from the Charles Towne Site

From a typological comparison with baked clay objects recovered from Louisiana to South Carolina in association with fiber-tempered pottery at a time period approaching 2000 B.C., we can see that the Charles Towne baked clay objects are very likely closely related in time and space to the Poverty Point objects. The fact that the majority of those at the Charles Towne Site are perforated presents an additional interpretive problem relating to function. With

the exploration of early sites on the South Carolina coast still in the infant stage, we can look forward to a time of more abundant data bearing on the baked clay objects, as more sites are examined. Perhaps then a more definitive explanation for the differences between the typical Poverty Point baked clay objects and the perforated forms from Charles Towne can be advanced.

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A PRE-AGRICULTURAL VILLAGE SITE IN FULTON COUNTY, GEORGIA

A. R. Kelly and Larry Meier

University of Georgia

The locale of 9 Fu 14 is a huge industrial park, some 3,500 acres in a bend of the Chattahoochee River in southwest Atlanta, which might be more familiar to some of you as the locale of the "Six Flags Over Georgia". The immediate sponsor, however, is a subsidiary major holding development company, The Great Southwest Development Company of Atlanta. Huge areas are being carved out of the terraces and hills in and around Atlanta, and this immediate development is only one of many. In addition to the 3,500 acres in which our site occurs, there are others along the industrial boulevard. I am told that when Apollo was streaming out from Cape Kennedy, one of the landmarks that could be seen in the southeast was this huge white, naked area of soil, and this was visible from several hundred miles out. I mention this as a little bit of dramatics to pinpoint the amount of destruction on major sites which must be going on every day all over Georgia and the southeast.

This site attracted a great deal of attention and an enormous amount of interest by the public in the early days. Larry Meier was largely instrumental in calling our attention to the site and its importance, and in getting the Great Southwest Development Company's cooperation in calling in the University of Georgia to conduct a reconnaissance. Later on the Company provided funds for an intensive survey. The whole thing mushroomed very quickly, and it was impossible to plan it. We went in with heavy machinery provided by the contractors and began peeling away the midden soil immediately overlying the basic clay sub-soil. The three and one-half to four acres covered by the site is to be the locale of an industrial area which is to be constructed in the next year. We have been working intensely since the first of April up to the present November. We are just beginning to understand the site and hope that the industrial expansion will not completely cover or destroy the site so that we might be able to go back and pick up a few more excavation units.

In planing off the top of this terrace, we uncovered the remains of about twenty-five structures. These are houses or domestic units belonging to Early-Middle or Middle Woodland and are largely denominated by simple stamped pottery, with smaller minorities of a type of check stamped which Joe Caldwell found all

through northeast Georgia. He called this Cartersville; and the relations and associations, in temper at least, are with the Deptford material on the south Atlantic coast. There is also a very small percentage of fabric impressed and, in some parts of the site, a little of the Swift Creek Complicated Stamped material. All of these are denominators of the Woodland as it has been defined over a good portion of the southeast.

The beauty of this site situation is that we are relatively uncomplicated by the intrusion of Mississippian overlay. In twenty-five years of Georgia survey we have, except for a few rare instances, been unable to find what you would call a pure Woodland site. The Woodland sites are always overlain and swamped by heavy Mississippian occupation so that your house patterns, your pits, all of your definitive features, are torn up and disrupted. You have a real problem in trying to reconstruct pictures or focus on a particular time or part of the total cultural sequence. Here at 9 Fu 14, we apparently have a Woodland site that is free from this later Mississippian Culture overlay. There are Mississippian sites about 100 yards away, but miraculously there was no interference. This was probably one of the factors which has made it possible to uncover a larger number of house sites or units than has been possible in the last twenty-odd years of southeast archaeology.

In the surveys conducted by the Smithsonian and the University of Georgia, for example, going back into the 40's and 50's, starting with Clark Hill and going through Hartwell, Buford, and into the Allatoona Reservoir, some 200 to 250 miles, from near Augusta to near the Tennessee-Alabama line, we worked on any number of good Woodland sites. Many of these should have produced patterns; but yet, in that entire span, Joe Caldwell tells me that he found only one recognizable full house pattern, at the Kellogg Site in the Allatoona Survey. This was also true along the entire span of the Chattahoochee, from the forks of the Flint and Chattahoochee to the upper Chattahoochee. We found patterns of houses in these surveys, but usually there was an overlay of Mississippian. Sometimes there was a rash, an "epidemic", of postmolds and it was impossible to reconstruct the house patterns. This was true at the Mandeville Site, under the mound, where we found a good village dominated by what you might call Deptford or Cartersville, good Early to Middle Woodland diagnostics. The postholes there looked as if they had been shot into the ground with a shotgun, and it was impossible to get any patterns out of them. So, you either have a richness of postmolds or you have such a faint dim scattered surface preservation that we have never been able to find house patterns in the southeast, and, I think, you could almost say in the whole eastern United States.

From the air, looking down on 9 Fu 14, you can see a pattern of what might easily be called a village layout. So, for the first time we can talk, I think, realistically and concretely about what might have been an example of an early village. This, as we all know, is an extremely important interval in North American prehistory-- this transition from a long early pottery interval which we call Woodland, which moved slowly out of the Archaic Culture and gradually culminates itself into the various little subregional patterns. The Indians learned to exploit the local environment and, as Caldwell and others have shown, they became very skillful and even developed a certain amount of efficiency in their hunting and gathering patterns. So much so that it has been suspected that they were capable of developing some sort of settled existence-- sedentary, more or less permanent residences-- without the necessity of being so nomadic or moving about so much as we find, for example, in the Plains or in the great West. I mention that as one of the primary significant things that comes from studying a type of site situation such as we have at 9 Fu 14, where

you can actually find twenty to twenty-five house patterns with indications that perhaps you actually have a ground plan of a little village. The cooking facilities or cooking pits were in place in the middle of the house, represented by clusters of fire cracked rock, with the black charcoal stains and what must at one time have been rather heavy bone and organic content. This has, of course, entirely vanished in the heavy acid Georgia soil.

There are three different kinds or modes of architecture represented on the site. All of the structures are circular, ranging from 12 to 20 feet in diameter, and have central hearths or cooking areas. Some of these seem to have flat floors with the posts or vertical supports around the outer edge and the hearth in the middle. They did not have a prepared hearth, but the accumulation of fire cracked rocks resulted from continuous cooking on the same spot. The midden debris was piled up around it. There seems to be about 10 or 12 of these structures that are about 15 feet in diameter. You do not get the overlapping in them that you usually get in Mississippian, so the fact that they are not intruding into one another suggests that this might actually have been a "one set" village occupation.

If you get close to the tree line that runs across the site, you find a different kind of structure that is still round and about the same diameter, but has a depressed floor. They are saucers, the floor being depressed about a foot, and the floor slopes up to a shoulder or rim. The post molds are in the rim. There is a conglomeration of fire cracked rocks in the center which theoretically are cooking stones. The fire cracked rocks in this site have been brought in, literally, by the ton, because there is no natural rock on this terrace.

The tree line running across the site is a persistent landmark and is very important. It was the Lumpkin Treaty Line made in the early 19th century to protect and separate the little Latin community which is about a mile away, from the early settlers. Because of this tree line, only one-half of one of the saucer shaped houses could be excavated. This provided an excellent profile of the house and revealed four zones in the approximate two feet of depth. Number 4 was the basic sub-soil or clay; Number 3 was a rich midden soil about 6 inches thick which accumulated when the house was being lived in; Number 2 is a homogenous compact grainy soil; and Number 1 is a recent soil or forest loam which is full of all sorts of small roots, earthworm holes, and everything else. The big trees are sending their roots down to the sub-soil and they are following the shape of the saucer-- they just hug the contour of these structures-- because, after all, this is clay underneath and the capillary action brings the moisture out.

One of the saucers, Number 13, had a pile of fire-cracked rock in the center and, in addition, several small satellite piles up on the bench-like rim. It looks like the stones had been heated in the center and then carried a few feet away up onto the rim. We are playing with the idea that possibly some of these structures might be sweat houses.

Another type of house that is a little different might have some special significance. In the midst of the part of the site that we think might have been a one-period village, there was a somewhat larger structure, 20 to 23 feet in diameter, which had a prepared puddled clay fire pit or hearth in the center. This type of prepared hearth is rather rare, I think, in Woodland sites in the southeast, but is very prominent in later Mississippian sites, in council houses and ceremonial structures. This is one of two structures which are larger and have this type of prepared hearth in the center. There is not as much midden de-

bris on the floor; it was much cleaner than the other structures on the site. We think that this is some sort of ceremonial building in contrast to the smaller domestic units.

The third type of structure is 7 feet in diameter with a huge cooking pit and two lines of fire-cracked stone across it, about 2 feet deep, separated by a sterile zone. It might be significant, indicating some sort of break or going away from the site and coming back. Some of the stones are arranged as if they were for cooking, some arranged as if they had pots on them, and some as if they might have had the carcass of a small mammal on it. In one area there were many small pieces of calcined bone which were not big enough to be human-- this is not a cremation. We do not know what the burial situation is at this site, since we have not found the burials. This site is like the big site at Mandeville that we did in southwest Georgia, in that respect.

We did not find the array of religious and artistic types of artifacts that we found at the Mandeville Site. A figurine is one of the few exceptions. It is a pottery figurine of a very sophisticated looking character. A small figurine was also found which was rather different. The figurine has some resemblance to some of the figurines that are, presumably, related to Hopewell. This site has very few of the Mandeville traits, and we found no copper at 9 Fu 14 as we did at the Mandeville Site.

HOWARD MAC CORD- What is your evidence that this site is pre-agricultural?

KELLY- Because we find the simple stamped pottery and all the Early to Middle Woodland diagnostics. We have never found any agriculture on this type of site anywhere in Georgia and we have not found it here either. There is very little organic material. You do not get bone except when it is calcined. We are taking soil samples and one of our graduate students found some seeds. We are working with Dick Yarnell on that. We are also trying to do something with the pollen. We have another graduate student who is working on this and she has found some pollen. We are still in the early stage and we do not know what we are going to be able to make out of these studies.

MAC CORD- The reason I ask you that question is that we have evidence of corn agriculture as early as the 1st century A.D. in the Virginia area.

KELLY- This would be about that period, but they didn't have the copper and you do not get the full range of stuff that we got at Mandeville. I get the impression that these people are way back in the boondocks in middle Georgia, on the upper Chattahoochee, and away from everything. They are just not getting the influences which are being felt at Mandeville and other southeastern Woodland sites. Maybe they just wouldn't have agriculture or they wouldn't accept it if it was available.

BOB STEPHENSON- What are the ceramics associated with that little saucer shaped house?

KELLY- We do not know yet. We think we are getting more fabric-marked pottery with these depressed houses or saucers. That might mean that this part of the village to the north and the east would perhaps be the old village. Then maybe 150 or so years later these houses with flat floors and the cooking pits in the middle-- little domestic units for single families-- would be the later village. This we will not know until we can take all of these things out and make comparative analysis house by house this winter.

COMMENTS ON THE CONSTRUCTION OF MOUND A,
OCMULGEE NATIONAL MONUMENT

John W. Walker

National Park Service

I would like to briefly mention some of the results of the six months of testing that were carried out on Mound A or the Great Temple Mound. The work was undertaken in order to trace the final stages of construction so that reconstruction could be carried out. We thought that probably there had been a great deal of erosion on the mound, but we found that there was very little, as least as far as erosion of the clay cap was concerned.

Most of my comments will be concerned with the architecture of the mound. In cross section, we found that the mound was surrounded on the outer edge by a curb, somewhat like one of the stages at the Funeral Mound or Mound C. Roughly 20 feet in from the curb, there was a step of 1 to 1½ feet high which formed a platform. This platform extended on across, dropped down, and another curb occurred.

These curbs have been observed at Mound C, as mentioned previously, and at one of the mounds at Rood's Landing. A curb is also present as one of the stages of construction at Irene. I did not find the sand in Mound A that was present in Mound C, and the curbs in Mound A are somewhat taller-- perhaps a foot and a half tall. I thought that the curb might be the remnants of a wall which had been covered with clay; however, in tracing this out horizontally, I could not find any post molds. I do not know the explanation for the curb and it still puzzles me. I am also not familiar with the little platforms from other sites.

There was a ramp on the front of the mound which is still evident, on about the fourth layer in, and evidence of steps. There was also evidence for a ramp on the east side of the mound as well as steps.

In stripping the top of the mound, I found evidence for a fire pit which would line up with the ramp, and there was also some scanty evidence for construction. There were a few post molds found, but nothing that would tell us precisely what size or shape the buildings might have been.

My testing was very limited, but I did go back to some of the notes from the excavations of the 30's. Work on the mound was quite limited at that time also, since it was such a tremendous project and then, as today, financial difficulties make undertaking a project such as this just about impossible.

(THE REMAINDER OF THIS TALK WAS ACCOMPANIED BY SLIDES)

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SESSION II

CONTRIBUTED PAPERS

DISTRIBUTION OF SOUTHEASTERN ARCHAIC PROJECTILE POINTS IN THE OHIO VALLEY

Bettye J. Broyles

West Virginia Geological Survey

The purpose of this paper is to trace the northern distribution of many of the Archaic projectile point types that were originally described from sites in North Carolina, Tennessee, and Alabama, and to discuss some of the types that have recently been named from the St. Albans Site in West Virginia. Information on this distribution was gathered from published sources in Kentucky, Ohio, Pennsylvania, and West Virginia, as well as many unpublished sources and surface collections. Therefore, the picture of the distribution that emerges at the present time may only be a reflection of the current information on the area and not the true situation.

At present, based on the distribution of the Archaic types, it appears that the Kanawha-New River system served as a major migration route between the southeast and the Ohio Valley.

Several rivers flowing through North Carolina begin as small streams in or near the mountains of West Virginia. One large river, the New River, begins in North Carolina near Blowing Rock and flows northward through West Virginia (where it becomes the Kanawha River after joining the Gauley River), emptying into the Ohio River at Point Pleasant. The headwaters of the Yadkin and Catawba rivers are only a few miles from those of the New River and could have formed a convenient route of travel from the Piedmont area. The points of origin of both the Clinch and Holston rivers, tributaries of the Tennessee River, are also nearby, allowing relatively easy entrance from the west. The Greenbrier River, largest tributary of the New River in West Virginia, would have offered easy access from the headwaters of the Potomac and James rivers.

A number of Indian paths were located along the Kanawha-New River route which were still in use during Historic times. These same paths were probably also much used during Prehistoric times by game and man.

Four summers of excavation at the St. Albans Site in Kanawha County, West Virginia have produced a large sample of Early Archaic projectile points and provided a sequence of radiocarbon dates ranging from 7900 B.C. to 6210 B.C. Large surface collections from the Mill Pond Site in Monroe County and several mountain top sites in Boone County have provided other Archaic point types that were not found at St. Albans. All of these collections have made archaeologists in the Ohio Valley more aware of the northward extent or spread of these Archaic point types and their early dates.

The oldest of the types described by Joffre Coe from North Carolina was the Hardaway point, which is both side notched and fluted. No points of this type have been found in the Ohio Valley, although two projectile points found on

the river bank near the St. Albans Site (not in the excavation) may be transitional types between HARDAWAY SIDE NOTCHED and some of the un-fluted side notched types. Both of the points from the Kanawha Valley have ground bases, are side notched, and one is fluted on both faces.

Another side notched point found at St. Albans has been named KESSELL SIDE NOTCHED. The one point from the excavation (a second point was found on the river bank near the site) was on top of a hearth in Zone 36 along with two corner notched points (Charleston Corner Notched). The side notched point most likely belongs in a deeper (unexcavated) level, having been picked up by the inhabitants of Zone 36 from somewhere along the edge of the levee as it was eroding into the deeper zones. This would place a date of older than 8000 B.C. on the KESSELL SIDE NOTCHED point type. Many examples of this point have been found in Boone County, one was found in Putnam County, and two have been reported from Ohio. Thus far, no specimens have been recovered from the upper Kanawha or New River, or from other areas of West Virginia. Much more study is needed on the distribution and more specimens of the type need to be examined.

The side notched type present in the southern or upper portion of the Kanawha-New River drainage more closely resembles the BIG SANDY SIDE NOTCHED types described by Madeline Kneberg. Two variations of the Big Sandy point were identified from the Mill Pond Site collection. One is broad bladed with deep side notches, heavy basal grinding, and serrated edges. The second variation is a long, narrow bladed point with shallow side notches.

Points resembling BIG SANDY SIDE NOTCHED are fairly common in Ohio and are illustrated in the Ohio Archaeologist from Muskingum and Tuscarawas counties. None of the side notched points illustrated from Archaic sites in Pennsylvania resemble the Big Sandy type. In Kentucky, Big Sandy-like points were found in the lowest levels of the shell midden sites such as Read. They also occurred at the Parrish Village Site.

The earliest of the corner notched types defined by Joffre Coe, PALMER CORNER NOTCHED, has been found in Tennessee, Georgia, the Carolinas, along the Atlantic Coast, and as far north as New York State, but does not appear to have spread northward to the Ohio Valley. Similar points found in West Virginia, Ohio, and Pennsylvania do not exhibit the heavy basal grinding that distinguishes the Palmer point, therefore cannot be classified as that type. They are, however, probably descendents of the Palmer type, or related to it in some way.

Two newly defined corner notched types, CHARLESTON CORNER NOTCHED and AMOS CORNER NOTCHED, probably belong in the corner notched sequence between the Palmer and Kirk types. The CHARLESTON CORNER NOTCHED point was found on top of a hearth in Zone 36 at the St. Albans Site, which has been radiocarbon dated at 7900 B.C. Two additional Charleston points were recovered from Zone 24 during the 1968 excavation, but the largest sample of this type has come from the mountain top sites in Boone County. The CHARLESTON CORNER NOTCHED type is distinguished by the oblique flaking and the medial ridge running down the face (a result of the chipping technique). The AMOS CORNER NOTCHED point also has a limited distribution, having been found thus far only in Putnam and Mason counties, West Virginia, although a sample of several hundred points was recovered from these two sites. The type has not been identified from Kentucky, Ohio, or Pennsylvania, but this is probably due to the fact that the type description has not been published. The AMOS CORNER NOTCHED point differs from the Charleston and Kirk types in that the base is never as wide as the blade, and it is longer and narrower. The serrations on the blade seem to be deeper and more pronounced on the Amos type.

The KIRK CORNER NOTCHED type, also defined by Coe, appears to have a wider distribution than any of the earlier corner notched types, being found on several sites in West Virginia, Pennsylvania, Ohio, and Kentucky. The type was found in the excavation at the St. Albans Site in Zones 16, 18, and 20 (dated at 6850 B.C., 6900 B.C., and 6980 B.C., respectively). The points from Zones 16 and 18 are all between 2 and 2½ inches long, while those from Zone 20 rarely exceed 1½ inches in length. These points have serrated edges and bases as wide or wider than the blade on most specimens. A few of the bases are smoothed, but never heavily ground. Other sites in the Kanawha-New River drainage that have yielded large numbers of KIRK CORNER NOTCHED points (mostly the large variety) are located in Monroe (Mill Pond Site), Summers, Boone, and Mason counties, although there are instances of scattered finds in other parts of the State, extending as far east as the Cheat River and Potomac River valleys.

In Pennsylvania, KIRK CORNER NOTCHED points have been found on a site near Johnstown and several of the points illustrated by Mayer-Oakes in Prehistory of the Upper Ohio Valley from the Allegheny drainage are probably Kirk points. Several corner notched serrated edged points from Ohio that are probably the Kirk type are illustrated by Robert Converse in his article "Ohio Flint Types" in the Ohio Archaeologist. Among the Kentucky sites which produced KIRK CORNER NOTCHED points (at least the illustrations seem to be that type) were two in McLean County, the Barrett and Butterfield sites, and possibly the Roach Site. The illustrations of the three types of projectile points found at Indian Knoll contain many points that resemble KIRK CORNER NOTCHED and KIRK STEMMED, although the Indian Knoll Site is usually considered to be late Archaic. Due to dubious associations it is difficult to properly appraise their significance.

According to Joffre Coe, the KIRK STEMMED and KIRK SERRATED, in that order, occur slightly later than KIRK CORNER NOTCHED. The types do not seem to have as wide a distribution as the corner notched type, or at least they have not been recognized as often in some areas. They are not as numerous in collections from West Virginia as the corner notched type, although they were present in the Mill Pond Site collections and have been found in the excavation of the St. Albans Site (but not in a zone all by itself). Based on the radiocarbon dates from St. Albans for KIRK CORNER NOTCHED, the KIRK STEMMED and KIRK SERRATED types should date around 6800 B.C.

No examples of the KIRK STEMMED or KIRK SERRATED have been reported from Ohio or Pennsylvania, but the KIRK SERRATED type was illustrated by Rolinson and Schwartz from the Roach, Morris, and Parrish sites in Kentucky. In his Guide to the Identification of Certain American Indian Projectile Points, Robert Bell states that examples of Kirk points (Serrated) have been found in Trigg, Muhlenberg, Lyon, and Hopkins counties, Kentucky. These counties are all in the western portion of the State.

Another previously un-named type of point was found in the excavation at St. Albans in the zone just above those containing the Kirk Corner Notched type. It has since been named MAC CORKLE STEMMED even though the sample from the site is very small. A number of specimens have been found in the mountain top sites in Boone County, but additional information on the distribution of the type is unknown at present. No date has been received for this zone, but it should occur at about 6800 B.C. or slightly later if the Kirk Stemmed and Kirk Serrated types are to fit into the sequence just above the corner notched variety. MAC CORKLE STEMMED points are almost as large as the KIRK CORNER NOTCHED point, but the blade tends to be wider and is not always serrated. The stems are small and always concave. This is a good transitional type between the larger corner notched

types and the smaller bifurcated and stemmed types.

In the two zones above the one containing the MacCorkle type at St. Albans, another previously un-named type occurred, ST. ALBANS SIDE NOTCHED, VARIETY A and VARIETY B. Variety A was found in Zone 12 (dated at 6880 B.C) and Variety B in Zone 11 (dated at 6870 B.C.). The only distinguishing characteristics are better defined shoulders, serrated edges, and basal grinding on Variety A. On the whole, this variety tends to be shorter and wider than Variety B. It would be almost impossible to separate these two varieties in a surface collection; therefore, they cannot be given separate type status. Based on the position of this type in the St. Albans Site, the dates received thus far seem about 300 years too early (the plus or minus range was 500 and 700 years) and they would better fit a date of about 6500 B.C. Another date of 6290 B.C. \pm 300 years was received for Zone 12, but this is about 200 years too late. The plus and minus range of the two dates overlap for about 400 years (6200 to 6600 B.C.) and would, again, indicate a date closer to 6500 B.C.

Both varieties of ST. ALBANS SIDE NOTCHED have been found in the mountain top sites of Boone County, but only from a few other sites in the Kanawha-New River Basin. There have been a few reported finds from Pennsylvania, but otherwise their distribution seems to be limited to the western portion of West Virginia.

The LE CROY BIFURCATED BASED type of projectile point probably has the widest distribution of any Archaic point type found in the eastern United States, being found as far north as New York and Michigan, or at least they are the most easily recognized. The type was first described by Madeline Kneberg from surface material collected at the LeCroy Site on the Tennessee River above Chattanooga, Tennessee. The LeCroy point has been found in Zones 6 and 8 at the St. Albans Site and dated at 6300 B.C. (Zone 6). This type of point was also common in the Boone County collections, as well as the Mill Pond Site in Monroe County. It has been found on a large number of sites along the Greenbrier River and its tributaries in both Greenbrier and Pocahontas counties, and also on other sites in Monroe County along Indian Creek. The LeCroy point appears to have its heaviest concentration along the Kanawha-New River and Ohio River valleys (sites near Huntington and Wheeling in the Ohio Valley), although several specimens have been reported from sites in the northern and eastern portion of the State in the Cheat, Buchannon, and Monongahela river drainages. They also occur in the eastern panhandle in the Potomac drainage.

A number of LE CROY BIFURCATED BASED points have been reported from the Chartiers Valley of Pennsylvania, as well as a number of other sites in the western part of the State. In Ohio, according to Robert Converse, the bifurcated based points are common in the northern part of the State but are found in varying frequency of occurrence in other parts of the State.

Five bifurcated based points were reported from the Henderson, Roach, Morris, and Parrish sites, and a few are illustrated from the Peter Village in Fayette County, Kentucky.

A fifth un-named point type found at the St. Albans Site in Zones 2 and 4 has been named KANAWHA STEMMED. This type of point has been found in North Carolina and Tennessee but was never named or described. Identical points were present in the collection from the LeCroy Site, and seem to have almost as wide a distribution as that point type. They are found on sites in West Virginia that contained LeCroy points and many sites which did not. The type occurs on sites throughout the Kanawha New River drainage, both along the main river channel and

its tributaries, and in the northern portion of the State in the Cheat and Monongahela drainages. A radiocarbon date from St. Albans places this point at 6210 B.C.

KANAWHA STEMMED points are present on sites in the Chartiers Valley of southwestern Pennsylvania and the Siggins Site in the Allegheny Valley of northwestern Pennsylvania. In Ohio, this type of point was illustrated from Washington and Turcaroras counties. Several of the bifurcated based points illustrated from Michigan are apparently Kanawha points rather than LeCroy. Thus far, none have been reported from Kentucky.

The projectile point in the North Carolina sequence above the Kirk points was called STANLY STEMMED by Coe. This type of point occurs on a few sites in West Virginia, but has not been reported from Pennsylvania, Ohio, or Kentucky. One specimen was among the Mill Pond Site collections and two have recently been found in a collection from Greenbrier County, both sites being in the New River drainage area. The third site which produced two STANLY STEMMED points is in the Tygart River Valley near Elkins, West Virginia. The Tygart River heads near the source of the Greenbrier River in southern Randolph County and flows northward into the Monongahela River. This type of point should date at about 6000 B.C.

The EVA BASAL NOTCHED point has been dated at 5200 B.C. at the Eva Site in West Tennessee. This point does not have a very wide distribution, occurring mainly in the Tennessee Valley area of northern Alabama and western Tennessee. None have been reported from West Virginia or Pennsylvania, and only a few from Ohio and Kentucky. A few have been found on sites of the Faulkner Focus of Illinois. The Eva point appears to be restricted to the Tennessee and lower Ohio valleys.

Another of Coe's types, MORROW MOUNTAIN STEMMED, seems to be limited in distribution and does not occur in Pennsylvania or Ohio. A few points resembling this type have been found in West Virginia and a few were illustrated from the Carlson Annis Mound in Kentucky. According to Coe, they have been found in Virginia and New Jersey, indicating a concentration in the Mid-Atlantic area rather than west of the mountains.

The GUILFORD LANCEOLATE, also defined by Coe, dates at about 4000 B.C. and has been found on several sites in the Kanawha-New River Basin in Monroe, Summers, Nicholas, and Putnam counties and along the Greenbrier river in Greenbrier and Pocahontas counties. Putnam County, West Virginia, seems to be the northern limit of the type since none have been reported or illustrated from Ohio or Pennsylvania. A few have been reported from the northern and eastern portions of the State in Preston County (Cheat River) and Randolph County (Tygart River Valley).

No specimens of Coe's HALIFAX SIDE NOTCHED type, dated at about 3500 B.C., have been identified from either West Virginia, Pennsylvania, Ohio, or Kentucky.

The late Archaic projectile point type that has the widest distribution over the eastern United States is called SAVANNAH RIVER STEMMED, although it is known by other names in some areas (Appalachian Stemmed, Benton Stemmed, and Kays Stemmed in Tennessee, for example). They are found on sites in Virginia, New Jersey, and New York, but do not seem to be very common in Ohio or Pennsylvania. These points usually date around 2000 B.C. wherever they are found, or what name is used for them.

In West Virginia, SAVANNAH RIVER STEMMED points were found on the Mill Pond Site and other sites in Monroe, Summers, and Greenbrier counties, as well as a few sites in Grant County near Petersburg, which is in the eastern portion of the State and in the Potomac drainage. One point found on the surface at the St. Albans Site has a Savannah River-like base, and the blade had been worked down from a much larger point. It is quite possible that a zone containing Savannah River points was in the six feet removed for construction of U.S. Route 60.

In summary, although negative evidence is not always reliable and frequently only represents the lack of work in certain areas, it appears at present that the concentration of the various Archaic projectile point types named and described from North Carolina and Tennessee is in the Kanawha-New River Basin and Ohio Valley, with less frequent occurrences along the Allegheny, Monongahela, and Cheat rivers in southwestern Pennsylvania and northern West Virginia.

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THE INTENSITY AND NATURE OF THE OCCUPATION OF
RUSSELL CAVE, ALABAMA

John W. Griffin

National Park Service

The present remarks are drawn from a larger report now being edited for publication, dealing with the National Park Service excavations at Russell Cave National Monument in 1962. Details of artifact content are not given here; rather, several approaches to the data are explored to give some idea of the way in which the cave was used over the years.

Analysis was in terms of physical layers which were given letter designations. The layers, their general cultural affinities, and the time spans as indicated by a series of radiocarbon determinations are as follows:

Layer A	Superficial	
Layer B	Mississippian and Woodland	A.D. 800-1400
Layer C	Woodland	A.D. 100-800
Layer D	Early Woodland	A.D. 100-500 B.C.
Layer E	Late Archaic	500-3500 B.C.
Layer F	Middle Archaic	3500-5000 B.C.
Layer G	Early Archaic	5000-7000 B.C.

Rate of Accumulation and Intensity of Occupation

Table 1 contains information which enables us to make a very rough assessment of the average rate of accumulation of the deposits and of the intensity of

TABLE 1
RATE OF ACCUMULATION AND INTENSITY OF OCCUPATION

Layer	Duration (Years)	Average Thickness	Average Accumulation (Per Year)	Number of Artifacts	Artifacts Per Year Sample Area x 30	Number of Potsherds	Potsherds Per Year Sample Area x 30
B	600	9 in.	.015 in.	79	.132	2,202	3.67
C	700	12 in.	.017 in.	202	.288	2,728	3.70
D	600	9 in.	.015 in.	87	.128	1,168	1.95
E	3,000	12 in.	.004 in.	169	.057		
F	1,500	18 in.	.012 in.	169	.113		
G	2,000	18 in.	.009 in.	247	.123		

occupation of the site. The first three columns measure the rate of accumulation, which in no instance rises even to the figure of two one-hundredths of an inch per year. In most cases it is considerably less. The figures for Layers G and F are admittedly crude because of the variation in the thickness of the layers. But even doubling the depth for Layer G would still place the figure below two one-hundredths of an inch per year. In these two layers most of the accumulation is presumed to derive from flooding and ponding activities before the occupational floor rose above flood levels in the adjacent sinkhole.

Layer E discloses the lowest rate of accumulation, fitting in very well with geological observations of Dr. John T. Hack which will be presented in the full report on the site.

Layers B and C represent a combination of roof dusting, organic accumulation incident to occupation, and outside dirt introduced by the inhabitants. However, the rate of accumulation would not seem to support any interpretation that the inhabitants brought in dirt to create clean living floors.

The major point demonstrated is that the average yearly accumulation in Russell Cave was very slight. The implication is clear that occupational levels representing true components are difficult or impossible to isolate in such a situation, and that physical mixture can lead to confusion. For this reason the layers at Russell Cave are regarded as time blocks rather than cultural units.

The remaining columns of Table 1 provide a measurement of the intensity of use. The number of artifacts and sherds from the excavated area is given, and this in turn is presented as an average per year. To gain some impression of the total site, it is assumed that approximately one-thirtieth of the area (and hence the material) is represented by our excavation. The resulting averages per year are still very small. The implications are that (1) the cave was not occupied every year, (2) the annual time of occupancy was short, or (3) the groups were indeed quite small. These are not alternate explanations; the true picture probably represents a combination of the factors.

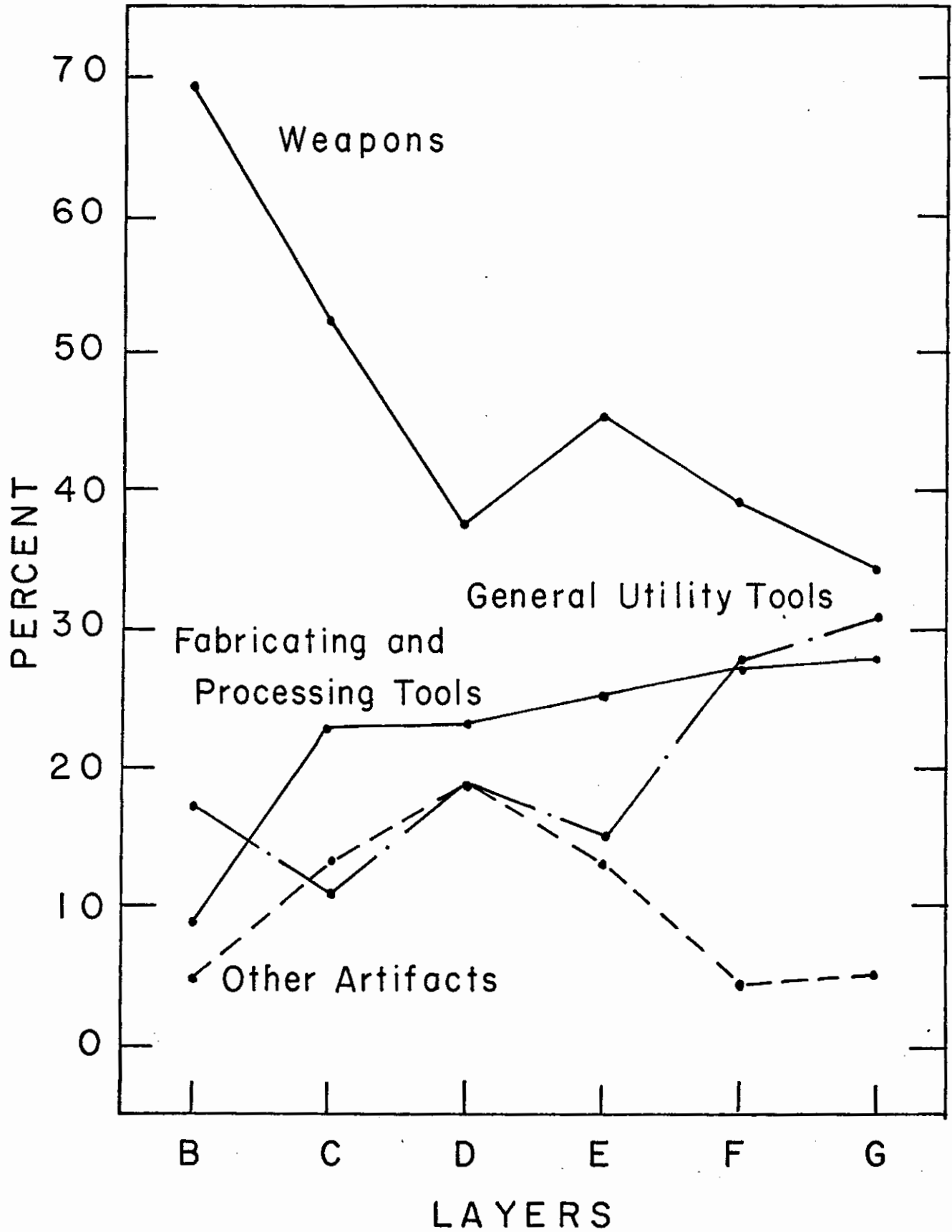
It seems apparent that the use of Russell Cave cannot be characterized as intensive. This exercise proved very instructive to the author who developed an impression in the field of a site "loaded" with material. I would urge others to develop more refined techniques for measuring this sort of data; the figures presented here are very crude and should not be used for direct comparison with other sites.

Type of Occupation

There is some evidence, which I will not present here, suggesting that Russell Cave was occupied seasonally. Winter seems to be the basic period of use, but this might stretch from late fall to early spring. We are, therefore, dealing with only a portion of the annual cycle of the various groups involved. The question arises, can we make any reasonable statements about the way in which the cave was used?

To explore this possibility the artifacts were grouped into functional categories patterned after those used by Howard Winters in his work on the Wabash River. Pottery was excluded in our analysis since the container category was not represented in the preceramic layers. Our typology was not designed with this end in mind, and this, coupled with the pitfalls of functional attribution, ren-

FIGURE 1



ders our attempt considerably less than perfect. But I think it is at least suggestive.

Figure 1 presents the artifact categories by layer in graph form. The most obvious trend is the relatively steady increase in weapons in relation to total artifacts through time. Layer D is the most divergent in this and other respects.

Before discussing the implications of this graph, I want to digress slightly and consider the Systemic Index as defined by Winters (1963: 35). This index attempts to measure the relationship between hunting activities and the more sedentary patterns characterized by the processing of a wide range of raw materials and the manufacture of basic implements of production. The index is obtained by dividing the sum of the fabricating and processing tools and domestic implements by weapons. Categories such as general utility tools and ornaments are omitted in this calculation. Winters stresses that this is at present an illustrative device and not an analytical tool.

As applied to Russell Cave, all layers fall in or near the figures which Winters believes represent hunting camps, and display a trend which would indicate a greater emphasis through time on hunting in relation to the total activity pattern represented in the cave. I regard this as a close approximation of the facts of the case.

Returning to the categories graphed in Figure 1, we note that weapons (primarily projectile points) constitute the highest percentage group in all layers, and account for over half of the artifact content in the top two analyzed layers. The importance of hunting in all layers is unquestionable, but there are indications of variation in the extent, but primarily in the intensity, of other activities carried on at the site.

In Early and Middle Archaic times (Layers G and F) somewhat of a balance is struck between the major categories. While seasonality was already present, the total cultural inventory was relatively simple, the phases of the seasonal round were not yet highly specialized, and a greater range of the total processes were carried on than was later to be the case.

In the Late Archaic Period we find our minimum intensity of use, and along with this increase of hunting in relation to other activities pursued in the winter camp. This is the time range within which we find larger sites with an expanded cultural inventory in the East in general. This is the time which represents the realization of Caldwell's "Primary Forest Efficiency". We may have at Russell Cave an indication of the more specialized nature of the seasonal camps which seem to characterize the Late Archaic. The drop in total intensity of use may be related to the relative paucity of large riverine Archaic sites in the Gunter'sville Basin. The total adaptive pattern of the times may not have "fit" as well in this particular area as elsewhere.

The Early Woodland of Layer D is poorly understood here. The intensity of site use seems only a little greater than in Early and Middle Archaic times, and the pattern of use is also somewhat similar to these earlier periods. This is obviously an intrusive culture into this part of the Tennessee Valley, perhaps bringing with it an adaptive pattern established elsewhere.

By the time of the Woodland in Layer C we are dealing with cultural groups which are presumably practicing horticulture, although not, of course, at

their winter camp in Russell Cave. The weapon category rises sharply, indicating that the use of the cave was rather strictly as a winter hunting camp.

With Layer B we are dealing with the later manifestations of the same Woodland cultures, and the bulk of what little Mississippian material is present in the cave. Here, even more clearly, the emphasis is on the use of the cave as a hunting camp. By Mississippian times, indeed, it probably served only as a stopping place for a completely male party engaged specifically in the hunt.

In summary, Russell Cave was occupied on an intermittent and seasonal basis for more than 8,000 years. Used always as a winter camp, there was a trend over the years for hunting to dominate more and more the activities carried out at the site. This restriction of activity is the opposite side of the coin from the overall developmental picture of Southeastern prehistory.

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EXCAVATION AND ANALYSIS OF LIVING FLOORS IN ALLUVIAL SITES

E. Thomas Hennings

Institute of Archeology and Anthropology
University of South Carolina

The word "floor", used to describe a particular kind of occurrence of human artifacts, has a long history in British archaeology. In 1884 this title appeared in the Journal of the Royal Anthropological Institute: "A Palaeolithic Floor at North-East London", and no doubt earlier examples could be found (Smith 1884).

A famous Paleolithic floor, discovered in 1879 at Crayford, Kent, in the lower Thames Valley, gives some idea of the information that can be obtained from buried sites of this type. F.C.J. Spurrell (1880) recovered, among other material, a Levallois core broken in two pieces and more than sixty flakes which could be replaced on the core to rebuild the parent flint nodule. This find should be familiar to American archaeologists because it is described in Oakley's (1949) publication, "Man the Tool-Maker".

But floors, excluding the architectural type, are apparently not familiar to us. There is not a word about them in a field manual or introductory text, not are they described under another name. In America, interest in living floors has been confined to archaeologists trained or working in the Old World, but there is evidence that this situation is changing. More than ten years of published reports dealing with open air sites in Africa, Europe, and the Middle East demonstrate the special importance of living floors for understanding the cultural life of Early Man.

Certainly the most famous living floor site is FLK 1 at Olduvai Gorge in East Africa, where in 1959 the Leakeys discovered the Zinjanthropus skull associated with Oldowan tools, rubble, and animal food remains. FLK 1, dated by Potassium Argon at about one and three-fourths million years, is not the oldest floor in Bed 1. A stratigraphically lower floor, FLK NN1, was the site of the discovery of the Homo habilis juvenile.

However, from the standpoint of developing new techniques for excavation and analysis of Paleolithic living floor sites, the essential work has been carried out by J. Desmond Clark in East Africa (Kolambo Falls) and Syria, and by F. Clark Howell in East Africa (Isimial) and Spain. Mention should also be made of Leakey's work at Olorgesale in East Africa. Recent work on open air sites in Europe and the United States has been stimulated by the techniques and findings of these investigators. Clark and Howell describe their approach as Paleo-anthropology, and summarize the objective as follows:

The living places of pleistocene peoples are capable of revealing the same kind of evidence as to behavior and ecology as do those of much later times when the appropriate techniques of exposure and excavation are applied to their recovery. The systematic investigation of such occupation sites by a small but increasing number of pre-historians has introduced a new dimension into studies of the Paleolithic, whereas previously the emphasis was largely on establishing the succession of industries in the particular region in its relative chronological framework and on the study of the typological development of the material culture, the focus is now on the occupational occurrences, on the interrelationships of the various artifacts, food debris, and natural material on a site, both to each other and to the site as a whole. This permits a reconstruction of the pattern of settlement and so provides the basis for the interpretation of the behavior of the group of individuals that lived there. Such field studies have resulted in the application of more refined technique of Paleolithic sites than was previously the case ... (Clark and Howell 1966)

Living Floor Occurrences and Descriptive Terminology

Living floors occur in alluvial stratigraphic contexts which imply specific conditions of origin and history. Artifacts and cultural debris are concentrated both laterally and vertically as a result of (1) human activity and (2) burial prior to dispersion by natural processes. It is the degree of concentration that sets a living floor apart from other kinds of alluvial sites, where occupational assemblages have been dispersed or the occurrence of artifacts is observed to be diffuse in some degree (Isaac 1968: 255).

In a sense living floors are deposits of zero thickness. Under ideal, but actually occurring, field conditions, no accretion of natural materials and, generally speaking, sparse accumulation of cultural materials characterized the period of occupation. For this reason excavation by arbitrary levels is totally inappropriate, while the use of natural levels must be specially adapted. It is not natural strata which are lifted in order to obtain synchronous or near synchronous assemblages of cultural materials, but the cultural materials themselves, which occupy a contact between strata.

Such a contact can be described in geological terms as a paraconformity or non-sequence, a minor break in a depositional sequence, marked by changes in lithology or evidence of erosion, which is bounded by conformable alluvial stratigraphic units. It represents a local and relatively brief episode of nondeposition or erosion. The time interval, of assumed minor duration, can be described as a diastem. It is usually desirable to know the length of this interval.

The archaeologist recognizes the break as an old land surface which was the scene of human activity, hence the use of the term "living floor". I object to usage of this term in certain cases. It implies habitation, while a number of excavated floors record activities other than habitation, such as animal kills and associated processing activities. I recommend living floor continue to be applied to assemblages from buried land surfaces which reflect habitation or camp situations, while occupation floor or some more specific term be applied to assemblages in floor context reflecting other kinds of activities. As a final terminological recommendation, I suggest that the term occupation surface be used when describing the nature of the substrate where human occupation has occurred; for example, data on the composition of the substrate, the slope or relief of the surface, the occurrence of a weathering zone (paleosol), and so on, are relevant to interpretation of an occupation surface.

Principles of Interpretation

The great value of living floor or occupation floor assemblages lies in the preservation of context of artifacts, features, and cultural debris. By preservation of context I mean that the spatial array of these materials discovered by the archaeologist is the result of past human behavior. Moreover, an assemblage in occupation floor context can in many cases be shown to be an essentially synchronous assemblage. From a geological point of view materials located on a minor unconformity are stratigraphically contemporaneous. But I am speaking of a much finer control of chronology. Occupation floor assemblages provide a basis for inferring specific behavioral events which were localized in space and restricted in time (Kitts 1966). We refer to these events or series of events as activities, and we are interested in them not only as unique historical occurrences, but also as recurrent or patterned aspects of a total cultural system.

Archaeologically inferred events are not so uncommon. They include, for example, a burial, a cache, a burned house, or the worked Levallois core from Crayford. But the farther back one goes in the archaeological record, the fewer opportunities one has to observe the remains of an archaeological event, and therefore to make empirically based or specific statements about human behavior. Thus, the importance of preservation of context on living floors is apparent.

It follows that one of the requirements for interpreting living or occupation floors is to assess the degree of preservation of context. Howell and Clark (1963) distinguish between primary archaeological context and disturbed or partially disturbed context. The former case occurs when a living floor is "sealed in" by gently deposited, fine-grained sediments prior to displacement of its contents by natural processes. Normally, this can be determined by observing clusters or arrays of cultural materials which are logically the result of human activity. Negative evidence to support this observation includes the absence of post-occupational disturbance such as slopewash, truncation, intrusion, differential compaction, etc., on the occupation surface. In the case of disturbed context, obviously the nature and extent of disturbance must be evaluated before statements are made about human behavior. Furthermore, the same problem of loss of perishable materials is present, as in all archaeological interpretation.

A second requirement for interpreting living or occupation floors is that the spatial limits of occupation must be defined and the excavation must encompass the entire area (Clark 1960). In this way a settlement or activity area is isolated, with an opportunity to investigate internal structure or differences in activity within a site area as well as to make valid comparisons with other site areas.

Other related requirements are to recover all macroscopic cultural material (artifacts, faunal remains, lithic debris, hearth stones, etc.) for analysis, to observe and record all cultural or possible cultural features, and to maintain precise proveniences for all classes of data. Both of these requirements, extensive excavation and intensive, controlled recovery, are considerations of sampling adequacy. They have resulted in the recent emphasis on opening up large horizontal areas rather than relying on a pattern of trenches or test pits.

Analysis of living floor contexts depends on some principles of logic which are extensions of archaeological principles already in use. Basically, one is concerned with the nearness or distance of cultural items which we call association. A dense cluster of cultural material on a floor is attributed to a behavioral event, especially if it exhibits internal consistency, such as the Levallois core and replaceable flakes. In many living floor sites it has been found that more or less discrete clusters of cultural materials have resulted from the allocation of space for different kinds of activities (activity loci). When the residue of different activities is not discrete or is not separated by sterile occupation surface, but is intermingled, specific statements about human behavior are made difficult, because there is generally no observable superposition on living floors by which these residues can be separated and ordered.

As a final point I would suggest that, although known living floor sites in the New World are rare, alluvial-filled basins are not, and we may expect to encounter more such sites of various ages. It would seem that special techniques of excavation and analysis, which are only now being developed, should be applied to these sites.

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THERMAL TREATMENT OF FLINT

Charles H. Fairbanks

University of Florida

ABSTRACT

Barbara Purdy at the University of Florida has begun a study of the changes that take place during the thermal treatment of local Florida flint and silicified limestone. The objects of the research are two-fold:

1. To determine what heat treatment may have been applied to local flints, what effect this treatment had on the amorphous structure of the flint and
2. what changes treatment brought about in chipping technique or appearance of the chipped artifacts.

We have so far chipped samples of local flint and then subjected the other part of the same nodule to heat treatment. Changes in color to tans, reds, or browns are the most obvious changes. There is apparently no change in chemical structure as revealed by X-ray diffraction studies. We are now preparing thin sections and have a preliminary indication that minute cracks in the flint are "healed" by thermal treatment. Further samples need to be run to explore a wider range of materials and of heat processes.

Mrs. Purdy attended the chipping seminar this summer held at Idaho State University under the direction of Dan Crabtree.

We plan to excavate an Archaic site this spring where deep red chipped flint artifacts and flint scrap suggest that thermal treatment may have been performed. We hope to find a fire area with evidence of how the flint was treated. We are also systematically searching for flint outcrops used by the Archaic Indians.

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WHITEOAK, A MIDDLE WOODLAND PHASE
IN SOUTHWESTERN ALABAMA

David W. Chase and Lynne L. Herman
Montgomery Museum of Fine Arts

It would be ideal if some of the many pottery types recovered and published upon would be presented as a part of a tradition continuum reflecting a stage of stylistic development and change. Too frequently a pottery type is offered simply as an isolate, and relegated to the nebulous limbo of "Late Early Woodland" or "Middle Middle Woodland" or somesuch.

We contend that the archaeological exposure of a given pottery type, the elements of which appear to extend in time into both earlier and later periods, justifies efforts to correlate the type with the apparent continuum of the overall tradition. This has been done for check and complicated stamping, but only to a much lesser degree has it been done for finger-nail and finger decorating. We suspect that the reason for this is largely because the regions where this tradition is best known have not been as thoroughly investigated as other areas of the southeast.

Check stamping, for example, has a very long tradition which seems to have its earliest beginnings in Deptford in eastern Georgia. We are now prepared to show that a similar lineage exists for the various forms of finger decorated pottery in central and southwest Alabama. In 1951, Ford presented evidence for a finger-decorated tradition continuum in the Mississippi Valley. I quote from his illuminating Troyville-Coles Creek Report:

This extremely simple decorative tradition is an old one in the Mississippi Valley. The earliest examples are to be found in the types Alexander Pinched and Tammany Pinched of the Tennessee River and Lower Mississippi valleys. These types are dated on the Louisiana Tchefuncte time horizon. A clay tempered type, Evansville Punctated, carries this tradition through the Baytown periods of northwestern Mississippi and eastern Arkansas, but strangely enough there is no comparable finger-punctated treatment on pottery from the end of Tchefuncte to the latter part of the Coles Creek Period in Louisiana. As we have seen, this technique reappears here as Wilkinson Punctated. The earlier frequencies have been quite small, but towards the latter part of the chronology there is a trend towards much greater popularity for the treatment. As an example, the shell-tempered type of the Mississippian Period in the St. Francis Basin, Parkin Punctated, reaches a maximum of about 15 per cent. This and the "Caddoan Area" are undoubtedly the popularity center at a late date. The late occurrences in eastern Louisiana seem to be a reflection from this center (Ford 1951: 89).

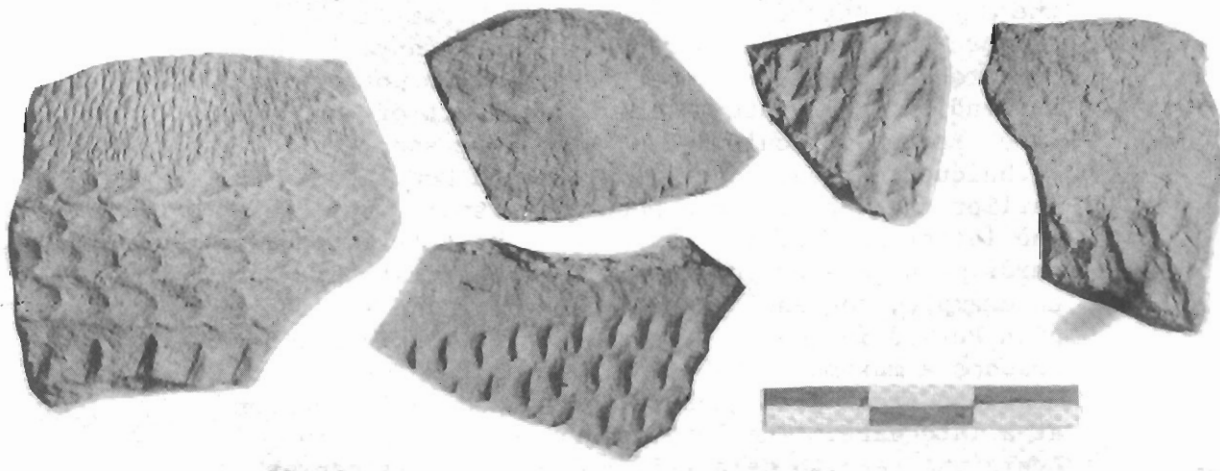
Ford's study of the finger decorating tradition shows Tucker Ridge Pinched to be the only descendent type east of the Mississippi with no subsequent types listed. In 1960, Wimberly presented Deas Pinched as a relative type for Tucker and on about the same time level. In 1964, the exposure of the Autauga pottery types at the upper end of the Alabama River Basin with its almost diagnostic finger nail punctating tradition together with a date of 920 A.D., indicated a probable continuum, distributional evidence for which existed, we suspected, somewhere between the Tchefuncte type site in Louisiana and the Montgomery area of central Alabama.

Such evidence was recovered last spring in Dallas County on a site at the mouth of the Whiteoak Creek; a very large Woodland component whose ceramics reflected a predominant use of fingers and finger nails in the decorating process. Pinching, punctating, stippling and pattern pinching were all present. (see Chase 1969: 17-20). Pottery was thick, grit tempered, and had an orange-red cast. Six distinct types were identified.

A second site, located near Big Cedar Creek some four miles to the north, yielded the same range of types. Certain of these seemed to be similar to finger nail punctated pottery seen in the Autauga in the Montgomery area except that Autauga had relatively little pinched and no stippled sherds or sherd showing combinations of finger applied designs-- these being common to the Whiteoak series.

Stylistically, the Whiteoak series looked earlier than the Autauga, but at the time, we had little to go on for making assignments in time except for the factor of the stratigraphic position of Whiteoak-- sandwiched between an overlying shell tempered Historic contact village and a sand tempered check stamped level below, the latter probably pertaining to some local Early Woodland form although diagnostic elements such as tetrapods were not recovered.

More recently, we discovered a shell midden in Wilcox County along the west bank of the Alabama River. Limited testing uncovered associated Whiteoak sherds and what appeared to be a sand tempered form of Mulberry Creek Cord Marked. Such an association implies a somewhat earlier position for Whiteoak than we were ready to guess. It would also imply a closer relationship to Deas Pinched on definitive grounds, but best of all, it demonstrated that we had finally intercepted the finger decorating tradition at an earlier time level than Autauga and certainly later than Tchefuncte.



Sample of Whiteoak Fingernail Decorated types from Alabama, Site Ds 53.

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SALVAGE EXCAVATIONS TO OBTAIN A HOPEWELL POPULATION

Gregory Perino

Gilcrease Institute

The project for the summer of 1969 was to obtain a Hopewell population from six mounds on the Illinois River near Kampsville, Illinois. Excavation of the Gibson Mounds was conducted as a joint effort of the Gilcrease Institute, the University of Chicago, Indiana University, and Northwestern University. A total of seven students was supplied by these institutions, and a few volunteers from the area helped. Jane Buikstra, University of Chicago, was in charge of the skeletal material and was instrumental, with the help of another graduate student, in removing the skeletons from the ground. She will work up the physical anthropology report for her doctoral dissertation, which should prove to be most interesting. All skeletal material was cleaned at the Northwestern University field laboratory which was nearby. I was in charge of the excavation, directing them so that we could finish in three months. We worked for 80 straight days, losing only two days to heavy rains, and working under a cover of plastic on lighter rainy days to expose the skeletons.

The Gibson Mounds consist of six Hopewell mounds, all of which had been pitted years before by collectors who were trying to locate the log tombs thought to contain artifacts. Most of the tombs were found, but a few were missed. Fortunately the collectors had reburied most of the bones they had located, and a total of 150 Hopewell skeletons was recovered from the six mounds.

Mound 4, the largest, was 100 feet across and about 11 feet high when we began our excavation. It had been excavated some 25 years ago by Mr. Lester Gibson. Several people had tried to get into the tomb in this mound and had failed, but Mr. Gibson spent a month excavating from the top to get into it. We have the artifacts that he found in the mound.

The project was conducted to recover a Hopewell population thought to be comparatively intact, despite the destruction of many of the log tombs. Our guess proved to be correct and we feel confident that all of the skeletal material was recovered except in two or three instances where the pot-hunters had not re-interred some of the bones, although parts of these skeletons were recovered.

We go into the field with no preconceived ideas and try to keep our eyes open for features as they develop during excavation. All Hopewell mounds have different mortuary disposal systems, so we must look for this system to develop as we excavate each mound. Most burials are made peripheral to the log tomb or in a cemetery plot to one side. As the denuded skeletons were removed from the log tomb, sometimes a hand, foot, or skull would become separated from the skeleton and remained in the tomb or was placed on or near the ramps. This is the soil that is backing up the log tomb on the outside. Later on, when the Hopewell were through with an area, they would build a secondary mound structure over the cemetery and the tomb. Sometimes skeletons were buried out further from the tomb and were not included under the secondary cap, or possibly they had been interred after the mound was completed. These were located with the probe.

The artifacts from Mound 1 included a set of wolf jaws that had been cut (they were actually in two pieces), a projectile point, a rubbed hematite stone, and a beaver tooth hafted in an antler. A slot had been cut that was squared off at the back end and the tooth was backed against this square end and tied. There were also some drilled gar scales that were apparently on some sort of a pouch, but we could not figure out what kind of a pouch. Burial 6 in Mound 1 had a large flint flake knife and three projectile points, the latter of which were beneath the back. Burial 5 had a Baehr Brushed vessel near the feet. This was about the worst of the burials because it was in the top soil which is very acid. The burials in the sub-soil were very well preserved because it is full of lime drip.

There were Archaic burials beneath Mound 1 which were sort of a surprise to us, although we would expect to find such things on these bluffs. What had happened here was that an Archaic cemetery had been established some 1,500 years before the Hopewell made use of the area. The burials were laid on top of the bluff and about a foot of top soil placed over them, creating one of the first mounds. In the interim, trees and other things grew on the bluff, breaking up the bones and scattering them. A lot of the area had eroded and we found only about six inches of top soil over the skeletons. Then the Hopewell built their mound on the area. There were two full-grooved axes from the Archaic level, as well as some rather unusual projectile points that have been difficult to identify. The majority of points are side notched or un-notched. The reason that we think that we had whole skeletons which were separated by trees and other things is the fact that some of the skeletal parts are articulated. We must remember that the forest grew over these things for 1,500 years with less than a foot of soil covering them before the Hopewell capped them. About four skulls are restorable so that we can have some study done on the Archaic skeletons.

Burials were found in Mound 1 in the mound fill, on the original ground surface, and in a sub-floor pit. The mound had two tombs, but not necessarily centrally located. The one on the edge was the latest of the two tombs and was entirely covered with limestone slabs and huge blocks of flint which are present in the side of the bluff to the east. One of the child burials was also capped with limestone slabs.

Mound 2 was a rather small mound but contained 50 skeletons, and a log tomb. Two of the burials were north of the tomb and the remainder were placed in a cemetery on the crest of the ridge or pressed against the edge of the bluff. We found that the limestone slabs that had capped the roof of the tomb had been thrown off by the Hopewell people and the tomb destroyed by them. All we found in the tomb were a few phalanges and a skull at one end. The only artifacts recovered were a small conch shell vessel and a shell ear spool or plug that was

tear-drop-shaped.

The first two burials that we found in Mound 2 were to the north of the tomb. One of them was buried face-down and the other face-up. We have found that we get at least one and sometimes two of these face-down burials in every mound, and that they are always females. Two of the child burials had copper necklaces made of all sizes of rolled copper. In some instances we found intrusions of burials on burials, and we got quite a mixture of skeletons, but the preservation was quite good. In one case, the skeleton had been laid on fill that had been placed on the edge of the bluff. The edge of the bluff goes down in this area, but this was extended out for four or five feet by the Hopewell and a lot of the burials were on or in this fill. The Hopewell people seemed to always want to bury in the same place so that they often disturbed their own skeletons. In this mound there were three skeletons with a row of rocks over some of them. This is different from the other mound.

In Mound 3 we were also getting these extended skeletons, but they were outside of the mound about five feet from the edge. Apparently, the Hopewell people had intended to build this mound to cover the skeletons but never got around to it. These burials were found with the probe. We probed every foot for a distance of at least 10 feet from the edge of the mound and got these chance burials. Two skeletons buried one on top of the other appeared as a "two-headed person" when uncovered. The lower portion of one of the skeletons had been removed and then thrown back into the pit in reverse order. One of the burials had what we thought might be a stone cover for the grave, but it turned out to be stones that were covering the skeleton of an infant. Once in a great while you get a flexed Hopewell burial, such as one that was found in this mound. The feet and some of the other bones were missing, and probably were left in the tomb when the skeleton was removed from it. Another flexed burial was disturbed by ground hogs.

We found two skeletons on the side of Mound 3 that Dr. George Neumann thought had some Archaic features. The skull certainly is different and does not have the high frontal-occipital deformation of the Hopewell skulls. These were skulls of people that were either there before the mound was constructed or they could have been late Woodland burials which were intruded into the edge of the mound-- they were down near the outer edge of the mound. So, this is going to be a problem for the physical anthropologist to determine which of these are Hopewell and which are not. We do not have too many intrusive burials, maybe not more than three or four. We did find a small group out at the edge of the knoll near this mound.

In Mound 3, at the very top, there was a huge pit that a pot-holer had dug many years ago. He smoked Prince Albert tobacco because his can was in the hole. He apparently ran across an intrusive burial. We got most of the skeleton because he put it back. He also removed some of the stones from the top of the roof of the tomb. This tomb is unusual. The logs were around the edge of the tomb originally, and they were chinked with green clay at the junction of the soil and the logs. There were rows of rocks over the top lengthwise that were on top of the logs of the roof. In this sub-floor log encircled tomb, which was about 16 inches deep, was a skeleton of an elderly man, the skeleton of a child, and the skeleton of a bird with teeth of cut mica near the feet. We knew that this was a wading bird because it had rather long legs. Dr. Paul W. Parmalee of the Illinois State Museum identified it as a Rosette Spoonbill, which are not native to Illinois and, I think, only one or two have been reported in that State. Yet, the Hopewell people at Snyders and other sites show the Spoonbill bird along with the raptorial birds on their pottery.

On one of the knolls some dirt had been added which did not appear like a mound to us, but I suppose we would have to call it a mound since it had quite a bit of soil added and there were a number of burials in it. The skeletons were not in too good a shape, but one had four flake knives with it and another four flake knives and four rocks which are probably pottery trowels. Some of the flint was native and some of it came from the Flint Ridge area of Ohio.

Mound 4 is the largest of the mounds and is the one that was excavated by Gibson. We took the mound off in five-foot wide strips. We were very disappointed in the skeletal material because we only got two skeletons and we had three people working on this mound for three months. The fact is that this is the largest of the mounds and probably was a ceremonial type structure. There were three individuals associated with the central tomb, according to Gibson. The tomb contained some of the most unusual material that I have seen. Of the two burials that we found, the child had a large number of beads around the neck and the adult had a clay ear spool in association. The second burial was found at the edge of the mound. We thought that there was a prepared floor in one area, although it was composed of yellow loess rather than the sand normally found. The ramps to the tomb were very long in this mound. We were surprised to find a sub-floor pit beneath the tomb. Gibson apparently located it but did not do too much digging since the basket loads of earth that show up in the profiles continue into it. There is no evidence that this sub-floor pit-- or original tomb-- was ever used. No bones or anything were found on the floor. The yellow soil that we thought might be a prepared floor was the dirt that had been thrown out of this pit and not a prepared floor. The pit was about 16 inches deep, which is about the average depth of most of the Illinois Hopewell sub-floor tombs that are surrounded by logs.

Some of the artifacts that were recovered by Gibson included the original Gibson points that are similar to the Snyders point but are more side notched than corner notched. Gibson also found copper ear spools covered with meteoric iron, one of the very few instances in which this was used, and bone ear spools that are similar to the clay ear spools. Construction of the bone and clay ear spools is interesting in that they have four drilled holes through the outer edge from the center perforation. In other words, each has eight holes coming in from the outside which means that they could sew four pearls, or other beads, onto the periphery of the ear spool which is channeled like a pulley on the outside. All of the clay ear spools are of this type. There were a great many drilled bear teeth found by Gibson, some of them cut in half and flat, and imitations of bear teeth made from the edge or lips of the conch shell. Gibson also found two of the conch shells with the lips cut off and made into vessels. Four pipes were also found in this particular tomb, probably making this one of the most unusual tombs. One was a raven pipe with pearl eyes, another was made of limestone and the other two of a material that may be serpentine. The raven pipe is pecking at a Hopewell feature on the stem. One of the serpentine pipes, which is black, has a very thin stem. A great many pearl beads were in this tomb. As Gibson describes it, there was an adult male on the floor of the tomb, with a child very much like the one that was in Mound 3 with the bird, and artifacts were piled on the floor all around. On top of the tomb was another individual, a male, who had the bone ear spool and apparently one or two other artifacts, and I think a Hopewell Zoned pot.

Mound 5 was very very badly pitted and there is a pit large enough to put three cars into it. They literally lifted the log tomb out of this mound, however they only disturbed one or maybe two, and we got a lot of the bones. Out at the

edge of the mound where the burials are supposed to be we began to find a pattern. One of the skeletons had a wolf jaw ornament at his waist. These have been found at the waist in this exact position before, and they may not have been pendant-type things entirely. Another late Woodland, or possibly earlier, skeleton was found buried outside the mound at the edge, but not a great distance from the Hopewell burials. Some of the skeletons had been dumped into the grave pits peripheral to the tomb. Sometimes the skull was loose, and many bones were missing. Although we got less than one-half of this mound excavated, on paper the sub-floor burials began to make a pattern, showing up in a circle. Then we did not care much that the other tombs had been destroyed by the pot-hunters because we actually had an earlier Hopewell feature before this mound was constructed here. The Hopewell had used the top of this knoll as a mound. There were two central pits and a number of burials surrounding the pit in a circle. Later, the log tomb was constructed over this and then the mound was built. Another of the burials in this circle was face down. Usually the pits are covered with logs, but we could not prove it in this mound.

One of the central pits in Mound 5 contained a skeleton which had its legs still articulated, but the rest of the burial was strung out in the fill of the mound. On the sides of this pit was a very deep pit containing the remains of a very well preserved male skeleton, fully extended, with two cut elk teeth and a number of ancúlusa shell beads, two lumps of galena, a broken turtle shell ornament, and possibly other artifacts, but the ground hogs had taken this area where the artifacts had been and we had to recover a lot of them from the ground hog burrows. A Hopewell Zoned and Dentate Stamped vessel was removed from Mound 5 by Gibson before he excavated Mound 4.

Mound 6 was the last of the mounds to be excavated. A big hickory tree grew right in the middle of the structure, and it contained a big pit dug about 1890 by someone looking for the tomb. We found two tin cans in the bottom of the pit that had the flanges on the edge rather than the bottom, which indicated the age of the pit. We did not find a skeleton, and the only thing that had been in this mound was the one skeleton that was in the tomb, and we got most of the bones from it. Again, this must have been a special structure like Mound 4. The pot-hunter of the late 1800's had not excavated the entire tomb. There was a strip about a foot wide and about four feet long at the west end of the tomb that had not been removed, and when we removed this section we found a complete conch shell and a number of split turkey bone awls. This proves that you cannot leave even a few inches of a site unexcavated.

In summary, a total of 150 skeletons was recovered from the six Gibson Mounds. We feel that this constituted, as close as obtainable, the entire population of the mounds. It is our opinion that there are many sites worth salvaging in order to obtain a complete, or nearly complete, skeletal inventory for future population studies.

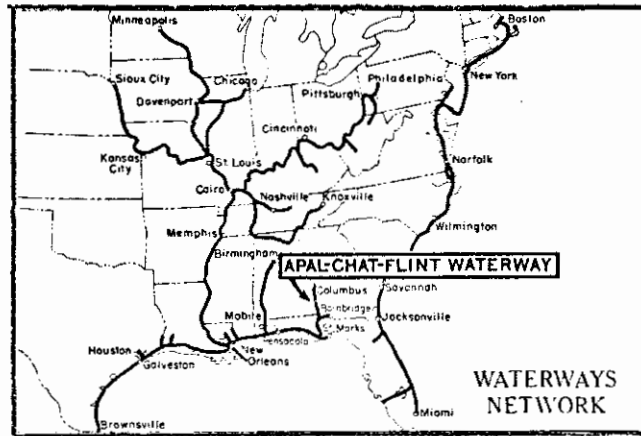


FIGURE 1

Waterways Network in the Southeast

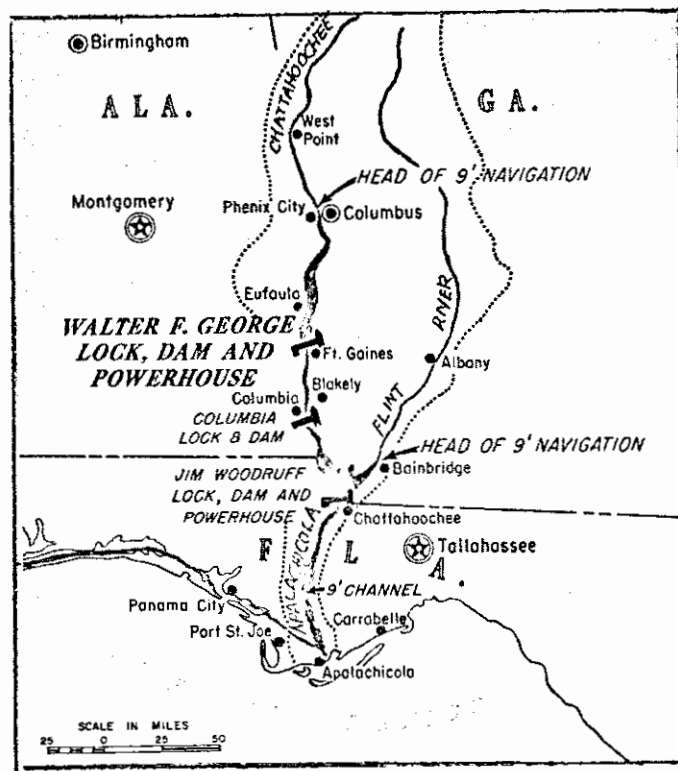


FIGURE 2

Walter F. George Reservoir

SESSION III

CONTRIBUTED PAPERS

ARCHAEOLOGICAL RESURVEY:

A RELATIVELY UNEXPLORED POTENTIAL

Frank T. Schnell

Columbus (Ga.) Museum of Arts and Crafts, Inc.

These few comments might be given a title suitable for many archaeological salvage discussions-- "Exercises in Frustration." This paper is, to a certain extent, a statement of a phenomenon so well known and so obvious that it is often forgotten by the professional. It is something, however, of which collectors are all too well aware. This phenomenon under consideration is the fact that whereas some sites may be gone but not forgotten, there are-- in reservoirs-- great numbers of sites which are forgotten but not gone. I might add that many of these, though not gone, are rapidly going.

Since the flooding of the Walter F. George Reservoir on the Chattahoochee River in 1962, constant reports have been made to the Columbus Museum of Arts and Crafts, Columbus, Georgia, of rapid exposure of archaeological material, primarily due to level fluctuation and wave action of the reservoir. Many of these exposures are so obvious that people who have never recognized an artifact or have never collected artifacts rapidly fall under the spell of "Indian head hunting." This exposure phenomenon has been reported in other reservoirs (Guthe 1967; McNutt and Graham 1967; Perryman 1967), but I personally had no concept as to the magnitude of the problem.

As to the magnitude of the problem, the following are a few figures concerning the Walter F. George Reservoir. The lake at normal summer pool is approximately 46,000 acres, whereas at normal winter pool it is 38,000 acres. These figures are based on the horizontal plane acreage of water and do not take into account even normal drought and flooding. This means that well over 10,000 acres of land are eroded and exposed by the reservoir in the course of an average year.

This exposure of land is not evenly distributed throughout the reservoir. The eighty channel miles of the Walter F. George Reservoir may be roughly divided into three physiographic sections. The first one-quarter of the reservoir (the lower section) is, to a large extent, flooded to what I call the second terrace above the river channel and the majority of the site component settlement types are camps or very small villages. These sites are relatively unproductive for the collectors, and because of the average beach slope of more than 30 degrees, are not as deeply or extensively eroded. The upper reservoir section of 37 miles is, with some important exceptions, still largely within the main river channel and many sites on its banks are not affected at all during normal times.

It is the middle, 23-mile-long section which is the area of greatest immediate importance. Here the normal pool level half floods most river bank

sites-- as is the case of the Rood Mounds Site-- the winter level almost exactly outlines the site occupation area. It is in this particular section that volumes of data can be retrieved with relative simplicity, often without great expense.

Let me make the following points:

- 1) Reservoirs can have widely varying shore line fluctuation conditions causing:
 - a) gradual sheet erosion dropping most material vertically,
 - b) rapid sheet erosion which carries away most but the heavier (more dense) artifacts,
 - c) silting, where sites may be simply encapsulated and invisible but protected,
 - d) bank erosion, where site strata are exposed and erosion is vertical rather than horizontal-- collectors constantly check these because they rapidly expose features with the possibility of exposing whole vessels or other kinds of artifacts, and
 - e) beach erosion-- this type of erosion on sloping banks usually reveals the highest amount of "surface" material. This is the situation at the Rood Site.

- 2) Reservoir resurvey can be a relatively inexpensive task with a possible high yield of new site component locations and previously recorded site component boundary information. Many sites on the first terrace have been previously covered by river silting and when erosion begins, these sites may be brought to light for the first time.

Generally speaking, there are a number of factors involved in the investigation of reservoir sites "after the flood." There are, as a matter of fact, a whole body of relatively distinctive methods and techniques involved in the resurvey of completed reservoirs. Practically unique problems are present in the horizontal and vertical location of sites and material. As Charles McNutt and Bennett Graham (1967) pointed out in a report on a problem oriented resurvey of Kentucky Lake, the visual appearance of localities are in a constant stage of change-- there is a rapid fluctuation of water level and consequent shoreline morphology. In addition there is the somewhat slower, but still relatively rapid changing of soil contours. All of this adds up to the point that, except under certain conditions, you have a considerable problem figuring out where a site is located geographically. The problem may not be on a magnitude of submarine archaeology, but it is far more difficult than "dry dirt" archaeology.

And let me emphasize that nostalgia about dry dirt. In the Walter F. George Reservoir, the winter drop in normal pool level (ignoring drought, etc.), a total area of over 8,000 acres of beaches and mud flats are exposed. This exposure brings out more than the normally expected dangers in this type of exploration, such as an increased population density of poisonous snakes. I thought, when working with David Chase on a survey of Fort Benning, Georgia, that learning to avoid dud shells and booby traps made life interesting, but it has nothing to compare with the all too frequent event in reservoir resurvey of stepping suddenly into quicksand and rapidly wondering how deep it is.

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THE NATURE OF SOUTHERN HOPEWELL

Jon L. Gibson

University of Southwestern Louisiana

American archaeology is experiencing a reorientation. This reorientation is not due to any new or improved technique for gathering data, although these are many and certainly desirable. Rather, the changing countenance of archaeology can be attributed to a shift in aims from reconstructing culture histories to explaining the variability in culture systems. To achieve this goal, archaeologists are utilizing far more data than before and are interpreting them in a scientific fashion.

One of the exciting advances brought about by this new orientation has been the elucidation of aspects of prehistoric social organization. Studies by Deetz (1965, 1968), Longacre (1964, 1968), and Hill (1966, 1968) have shown just how much information is recoverable on extinct kinship systems-- information that traditional archaeological measures has most often neglected.

This paper is another attempt at extracting a corpus of information on social organization, which has lain dormant in the functional, structural, and contextual poses of the archaeological record. The data selected for this illustration are from the Hopewellian remains in the Lower Mississippi Valley.

DEFINING ELEMENTS OF HOPEWELL

Caldwell (1964: 136-7) has called attention to the fact that Hopewell means many things to many people. There is, however, a certain body of material traits that is commonly recognized as constituting Hopewell no matter what region it happens to be in. Some of these distinctive features generally shared on a pan-Eastern basis include: a) interments in or on primary structures (earth platform or log crib) in multimantled conical mounds; b) a variety of pottery modes including zoned rocker stamping, cross-hatched cambered rims, and curvilinear (occasionally raptorial bird) motifs; c) use of copper and galena as media for several artifact classes, such as bicymbal earspools, rolled or shaped beads, reel-shaped gorgets, celts, and repousse plates; d) platform pipes of clay or stone, either plain or effigy forms; e) large pointed poll celts, usually of greenstone; and f) smaller objects, such as perforated canine teeth and pearl and shell beads.

MARKSVILLE: THE LOWER MISSISSIPPI VALLEY HOPEWELL

Hopewellian elements in the Lower Valley were first brought to our attention by Fowke, Setzler, and Ford from their work at Marksville and at other localities in the Red River Mouth region. Ford (1936) established a Marksville Period on this basis. The Crooks Mound in LaSalle Parish still remains the most adequately reported site in this region (Ford and Willey 1940). It is Crooks that furnishes the substantive data for this interpretative model which follows. The Helena Crossing Site (Ford 1963) and the Coral Snake Mound (Jensen 1969) have provided corroborative tests, increasing the model probability.

DERIVING THE MODEL

The Crooks burial record was reviewed for the purpose of explicating some of the differences which were apparent in the structural makeup of the mounds. At least six burial episodes were represented; five in the large conical structure (Mound A) and one in the smaller platform mound (Mound B).

Sequence of Burial Events:

- 1) A single bundle burial was placed on the original ground surface and was covered by a rectangular burial platform.
- 2) After the platform had stood for sometime, long enough for erosion to begin acting on it, a mass burial of 168 individuals was made on its surface, and a layer of soil was added to cover the remains.
- 3) The following burial episode involved the addition of a domed pile of earth and the interment of 214 individuals. This was separated from anterior and posterior events by lapses of time, sufficient for weathering profiles to develop in the soils (Ford and Willey 1940: 25-26). Even though events 2 and 3 are evidently separate and unique, it is felt that they are probably dependent and are reflective of a single burial ideal, simply separated in time. For this reason, they have been combined in Table 1. Justification for this intromission will be presently forthcoming.
- 4) Event 4 consisted of the raising of a dome-- the primary mound-- within which were placed some 270 interments, most lying horizontally suggesting placement during construction or in pits let down from the surface. This structure is probably quite complex, perhaps resulting from at least three separate accumulations and numerous individual burials. But concordant with the data, which is only suggestive, this is viewed as a simple event. In its completed form, the primary mound had a large post set in its top, a smaller one down the northwest slope, and a staircase of 15 logs on the east side.
- 5) The next-to-last event, which gave Mound A its completed appearance, was the erection of a large cap of earth (secondary mantle) which completely covered the earlier structures. During this constructional stage, 503 individuals were buried, most in shallow pits at different levels in the rising mound.
- 6) The sixth and last mortuary activity at Crooks consisted of the erection of a low rectangular platform, some 110 feet south of the large and now complete conical Mound A. Thirteen interments were made in this structure (Mound B) while it was being built.

Ranking the Burials:

Three readily separable mortuary events provide neatly packaged sets of data. It is the setting out of the differences within and among each of these sets that forms the basis of the following model.

The most obvious, and I might add, statistically meaningful differences are those relating to burial form or position. Ford and Willey (1940: 36-38) have isolated six classes of burial position including extended, semiflexed, flexed, partially disarticulated, bundle, and isolated skull. The nonclassifiable forms have been put in an indeterminate category and are omitted in these calculations. The working assumption here is that the differences observed in burial form are reflective of distinctions which were expressed among the individual members of the Crooks community.

The burial forms have been arranged on an ordinal (ranking) scale by the logical recognition that some of the forms show more post-mortem preparation than others. It has been necessary to combine Ford and Willey's six burial classes into three because of the inability to demonstrate even minor differences in the amount of devoted preparation on other than three levels. This new arrangement is as follows:

Class I, bundle and partially disarticulated;

Class II, flexed and semi-flexed;

Class III, isolated skull and extended.

Using the reasoning, Class I burials (bundle and partially disarticulated) would have received the maximum amount of mortuary preparation that involved saving, cleaning, placing of bones in containers, and subsequent interring in the mound. An independent supporting datum for the ranking of this class is the observation that more grave furniture was proportionally associated with bundle burials than with the numerically dominant isolated skull interments (Ford and Willey 1940: 40). In this connection, it is probably significant that the initial burial at Crooks was a bundle. Analogous treatment of high class individuals was practiced among the Natchez of protohistoric and historic times (Neitzel 1965). Class II interments, the flexed forms, would have been dedicated the next highest proportional amount of care. Minimally, this may have required no more than simple flexion of the legs and the addition of a wrapping to assure proper position prior to mound burial. As with the Class I burials noted above, the ratio of grave associations with Class II interments is higher than in Class III. The isolated skull and extended category ranks lowest on the scale. Very little attention, other than interment in the mound, seems to have dignified these forms. The five extended burials had no articles of association (Ford and Willey 1940: 44). It is probable in most cases that burial goods in apparent proximity to isolated skulls were actually intended for some other inhumation. The isolated skulls themselves may have only been a part of a set of funeral placements. The tendency for isolated skulls to cluster near the cranium or the innominate of articulated burials supports this possibility (ibid.: 38).

Aligning the Archaeological Record with Past Sociocultural Systems:

The percentages of burial classes have been calculated for each mortuary event (Table 1). These events were probably set in motion by the demise of an individual of high status or by the need to empty a charnel house. Interment sets thus contained within each stage of mound construction are considered to be indicative of certain demographic characteristics of the Crooks population. That is to say that the percentages of burials in each of the interment categories would reflect the proportions of members in each status position or social class.

TABLE 1
 PERCENTAGES OF CROOKS BURIALS
 RECALCULATED FROM FORD AND WILLEY (1940)

LOCATION	I	II	III	TOTAL
Mound B	8.3	83.3	8.3	12
Secondary Mantle	16.3	67.2	16.5	496
Primary Mound	27.1	59.2	13.7	218
Below 2 Combined	20.5	37.3	42.2	316
Mass Burial Area	23.6	24.1	52.3	196
Burial Platform	17.0	53.0	30.0	147

Thus, the shifts in class frequencies within the mound stages would record changes in social class membership over a period of time, probably less than 150 years.

The Model:

A particular trend of change is illustrated by these calculations (Table 1). It is expressed by the proportional expansion of Class II at the expense of the other classes, from 37.3 % for the initial combined mortuary event to 83.3% in the last event signified by the erection of Mound B. It is undoubtedly significant that only 13 interments were made in this latter (terminal) burial episode, compared with 503 for the immediately prior event (secondary mantle). The articulation of the archaeological record with the societal characteristics of the Crooks community seems to hinge on the explanation of this expansive trend.

A model of social organization based on an hierarchical class structure with certain obligatory marriage contracts and rules of class inheritance can be employed to explain these data. The model bears obvious similarities with the social organizational system of the Natchez Indians, as it was recorded by several 18th century observers (information collated by Swanton 1911). There are several points of difference, but for our purposes these need not be expounded. The specific features of the model are:

- 1) Class exogamy. Members of equal status (class) were forbidden to marry.
- 2) Selection of marriage partners. Members of upper classes (I and II) were required to select marital partners from the lower status group (Class III), whose own membership was dependent on the successful integration of nonstatus individuals from a general non-Hopewellian populus.

- 3) Matrilineal inheritance mobility. Statuses of children resulting from the parings were equated with the statuses of their mothers, when the mothers held upper class (I and II) positions. For example, the children of a union between a Class I mother and a Class III father become members of Class I.
- 4) Restricted patrilineal mobility. Opposingly, the marriage of an upper class male (I and II) and a Class III female could only result in children having Class II statuses.

The possibilities for marriage and inheritance are presented graphically in Figure 1.

As is readily apparent from the above listed criteria, Class III individuals were being constantly absorbed by the upper classes. This drain on the marriage pool required, for the system to have been operative for any length of time, the assimilation of people (non-Hopewellian) into the lower class. These additions could have resulted from the births of children of Class III wives and non-Hopewellian (classless) men and eventually from the propagation of a number of potentially class-mobile children born to Class III husbands and classless women.

In general terms, the eventual effects of a system of social organization, such as the one described here, would be a rapid growth of the intermediate class with concomitant depletion of the lower. This would be brought about by the fact that Class II was not only perpetuating itself through its own female members but was receiving the progeny of Class I and Class II males into its membership. Class I would have maintained itself as long as there were female members. If the system was allowed to run its course in a stable population, the depletion of Class III would be so acute after several generations that there would not be enough people to furnish husbands and wives for the upper classes. At Crooks, such overbalancing apparently occurred after five generations.

FIGURE 1

MARITAL AND INHERITANCE POSSIBILITIES OF THE
MODEL FOR HOPEWELLIAN SOCIAL ORGANIZATION

FATHER		MOTHER		OFFSPRING
I	+	III	→	II
II	+	III	→	II
III	+	No Class	→	No Class
III	+	I	→	I
III	+	II	→	II
No Class	+	III	→	III

TABLE 2
 PERCENTAGES OF BURIALS FROM
 HELENA CROSSING, MOUND C

LOCATION	BUNDLE	EXTENDED	DISARTICULATED	TOTAL
Primary Mound	0	92.8	7.1	14
Mound Base	23.1	46.1	30.8	13

TABLE 3
 PERCENTAGES OF BURIALS FROM
 CORAL SNAKE MOUND

LOCATION	BUNDLES	CREMATIONS	CONCENTRATIONS	TOTAL
Second Mantle	.20	.80	0	5
First Mantle	.15	.55	.30	40

Testing the Model:

The model has been applied to other Lower Mississippi Valley Hopewellian data from Helena Crossing (Ford 1963) and Coral Snake Mound (Jensen 1969). The results are given in Tables 2 and 3. Duplication of the Crooks trend in class membership is apparent at both sites, even though overbalancing occurred after only two generations. These corroborating comparisons have yielded additional or subsidiary hypotheses about the Hopewellian system. One is that Hopewellian ideology did not perscribe the form of burial, only that different forms be reserved exclusively for different classes. The rank of these burials is, however, still to be determined by hierarchically arranging the amount of preparation visited on each form. The second hypothesis is that the number of burial episodes (mound mantles) varies directly with the frequency of aboriginal population participating in the Hopewellian social system, i.e., the fewer the mound increments, the smaller the population.

The ability of the model to incorporate or predict independent data would seem to warrant a wider and more inclusive application, especially in the cases of Copena and Deptford/Santa Rosa-Swift Creek.

CONCLUSIONS

Space, unfortunately, does not permit the presentation of numerous data bearing on some of the conclusions to be made in this section. Regard them as speculative, if you wish, but they have been drawn with a large corpus of empirical evidence in their favor (Gibson 1969).

Lower Valley Hopewell (Marksville) is a social organizational phenomenon and is not a "culture" in any sense of the word. The Hopewellian "complex"-- mound burial, inhumation form, and characteristic artifacts-- are simply status insignias, referable to hierarchically ranked social classes. That this "complex" forms sort of an overlay on cultures which are basically Woodland or Baytown has been appreciated for many years (Griffin 1946: 62-63). Not all contemporary cultures were affected, and sometimes, only one or two sites of a particular culture participated in the Hopewellian system.

The model of Hopewellian social structure as described herein possesses certain characteristics, namely the disproportionate swelling of Class II, which makes it unstable over a period of time. This internal disrupting characteristic has received considerable attention in the Natchez analogue (Hart 1943: 374-386; Quimby 1946: 134-136; Fischer 1964: 53-65; Goldberg 1958: 239-241).

It is precisely this factor which led to the practically pan-Eastern distribution of Hopewell and contributed to its well-known cyclical (rise and decline) character. A corollary drawn from Quimby's (1946: 134) idea that the viability of the Natchez system could have been extended by assimilating other tribes can be used to explain this process. Simply, it is that the Hopewellian system moved from community to community, from culture to culture, along both spatial and temporal axes, in order to replenish the Class III marriage pool on which systemic maintenance depended. The necessity for abandonment of a site, or locality, or region was brought about when the overwhelming majority of the local incorporative populus had inherited Class II status. This would seem to account for the early, widespread popularity of the system and its subsequent decline when non-Hopewellian peoples became fewer and/or possibly more resistant. There appears to have been nothing catastrophic in the Hopewellian decline in individual localities.

The most obvious effect was a return to the regionalism that had existed prior to the establishment of the Hopewellian system. The large scale interactive networks, along which moved many of the Hopewellian esoteric trappings, were no longer maintained. The breakdown of the status regime is nowhere more apparent than in burial practices. Burials were placed in ossuaries. Mounds ceased to be constructed. Status-symbolizing packages of burial furniture were non-existent. Some stylistic carry-over is evident in ceramics, particularly in the persistence of zoned rocker stamping. However, the context of function shifted from status-recalling mortuary vessels to everyday utilitarian wares. In the Lower Valley, these subsequent developments are called Issaquena and Troyville.

The actual mechanism involved in the transferal of the Hopewellian system from locale to locale is unknown but is unimportant in terms of the explanation offered here. What is more important is the potential of the explanation in generating theory of Eastern cultural development. In advance of empirical demonstration, it is only suggested at this time that Hopewell, as a unique social system, persisted by dint of its special characteristics for some two millennia or more. It is called by different names, e.g., Alto Caddoan, Weeden Island, Natchezan, Southern Cult, but its longevity and subsequent evolution can be painstakingly traced from individual sites to more inclusive regions until its final and conclusive disruption at the hands of the Europeans.

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SESSION IV

SYMPOSIUM: WHITHER SALVAGE ARCHAEOLOGY?

William G. Haag, Chairman

This afternoon, as you are well aware, we would like to have some kind of an informal discussion group. As a consequence, the more people who might occupy some of these front chairs the better we might resolve this. We really are in one way or another in the southern United States, all engaged in some aspects of salvage archaeology. Probably many of us have now been involved long enough that we resent this being called "salvage" archaeology, for the simple fact that it is a kind of archaeological work that we hope never ends. Those of us who are old enough to have been involved in W.P.A. archaeology recognized the fact from the beginning that this was somewhat of a "stop-gap" operation and it was not likely to continue forever. Those of us who were exposed to this program long enough changed our minds. It was the W.P.A. workers who changed our minds. They began to look upon this as a career, and, in fact, to most of them it was the best job they had ever had in their lives. Many of them expected to end up as, well, National Park Service personnel, or something of that kind. As a matter of fact, many of them ended up as college professors teaching North American archaeology.

Certainly, salvage archaeology is something that we can recognize is never going to end, but probably is going to increase in importance in the future. As Hester Davis indicated this morning, this is something which is up to the individual states, to build a program to its fullest potential and it does have, as many of you know, a full potential. What we might talk about this afternoon would be the airing, perhaps, on the one hand, of the problems encountered in salvage archaeology, and secondly, what are the more concrete aims in salvage archaeology. I have not really given this an awful lot of thought, and I am hoping that there will be many of you who have given it a lot of thought and have first-hand questions to ask and second, answers to give, who would in the long run be able to say, this is the direction that we should be taking, these are the aims which we should be holding up, justified, and these are probabilities which we are not properly exploiting.

Thus, without any philosophical discussion on the nature of salvage archaeology and whether we should be thinking of this as, after all, ultimately, one of our goals. As Hester Davis stated in her little brochure on archaeology for the non-archaeologist, we are all stewards of the past. Each one of us, as a professional, has a certain obligation to this whole role and we must fulfill it to the hilt. Well, it is the "hilt" that we are here gathered to discuss. Thus, I would like to throw this open to any kind of volunteer expression and if I do not get any volunteer expressions I am going to ask somebody to volunteer.

JONES- I may be stating a problem here where no problem exists, but I would like to know your definition of salvage archaeology. What is the difference between "salvage" archaeology and any other kind of archaeology?

HAAG- Here is a point then, that many of us can respond to. In fact, I have already sort of implied that perhaps this is not a justified term, to talk about

"salvage" archaeology. Can we hear from someone who is or has been engaged in salvage archaeology or has found it necessary to speak to that point?

CORBETT- As an old ex-W.P.A.'er who ended up in the Park Service, the point here I would say, and I approach this subject slightly unconscious and probably biased point of view. I would say that salvage archaeology is that archaeology which normally you might not have been performing, not the normal routine type of work, but it suddenly becomes apparent to you that it is work that has to be done before something is to be destroyed. salvage archaeology is done in emergency situations. This emergency never stops. It has been stated that projects that are Federally oriented are the largest destroyers of archaeological remains. I do not think this is any longer true. There are other activities which are destroying more archaeology than the Federal Government. Airports, subdivisions, industrial plants, etc., all that kind of activity is continuing. I would like to hear a discussion of how archaeology, either salvage or problem oriented, can keep pace with this exploding situation which we have on hand today.

MARSHALL- I think John has put his finger on it, and this is our real problem. Shall we call this "emergency" archaeology instead of "salvage" archaeology? "Requisite" archaeology as opposed to "problem" or "Theoretical" archaeology?

GRIFFIN- Why not just call it archaeology?

MARSHALL- All right, I agree.

HUSCHER- Why not avoid the polarity and say they overlap, but on one polarity you put the emphasis on necessity, which must overlie then your freedom of choice. If you have time to plan and maybe work over a period of years with adequate finances, then you are not bound by these emergency necessities which are implicit in some of the crash jobs; and the two do overlap, they certainly overlap. The problem is, what can we get for our money in a given situation. I cannot see making a distinction between "salvage" archaeology and "problem" archaeology. I think they are both part of the same thing, and you can approach salvage archaeology by making it problem archaeology.

KELLY- I do feel, after being engaged in this program for a long number of years, that we need a term to describe this kind of archaeology, because, after all, this applies to perhaps 90% of the archaeology that has been and is being done in the southeast. It is undoubtedly a great majority. Most all of the archaeology that has been done in the last 20 years is what we call "salvage" archaeology. Indications are that, as time goes by, perhaps an even greater proportion will be archaeology of this kind. It seems to me that the important element in all of this so-called "emergency" or "salvage" archaeology is that it is "terminal" archaeology. If it isn't done, the site is going to be developed and its contents-- artifacts and materials-- will be terminated. It will be forever destroyed. Your archaeology for which you have a National Science Foundation Grant, in 90% of the cases, is on sites that are not in danger of being destroyed and that will remain there. It is not "terminal". The fact that you are saving it is not an element or consideration in setting up this piece of research. You do it simply because you are persuing a research objective. So, it seems to me that we might call all of this "terminal" archaeology. That means you are addressing yourself to a site not because you are attracted to it, because it is a specific research problem, but because it is an important site and you do not want to see it destroyed. It is "terminal" archaeology in that sense.

CLAY- I think it might be useful to look at emergency archaeology as primarily

of necessity in aerial and temporal sampling problems. Now, in all archaeology we have our sampling problem, but in emergency archaeology this is first and foremost.

HAAG- I believe that we can make a good case for the point that, despite the fact that we are not in control in choice of sites to excavate when we are concerned with this emergency archaeology, the results produced have been, in nearly every case, desirable, after all; that we have not done enough detailed archaeology in the southern part of the United States despite all this long period of salvage archaeology, so to speak, there still is a picture that every site fills in another little segment of it. Each one of them is even more revealing. We had that experience in Louisiana recently, where we sort of approached an archaeological site that was to be salvaged with the view that we knew exactly what we were going to get out of it, and we did not get anything like that at all. It was very very important from the standpoint of contributions to knowledge.

STEPHENSON- I would like to comment that I think Harold Huscher put his finger right on the point. I get increasingly annoyed at this dichotomy and polarity between "salvage" archaeology and so-called "problem oriented" archaeology. Salvage archaeology can be, should be, must be, usually is, problem oriented. It is problem oriented after the fact, after the time when you know that this is the site that you are going to have to dig, and you develop your problem, or if there are problems there you are able to see them and develop them, then you are doing "problem oriented" archaeology if you are doing anything good. I suggest that there is not this dichotomy between "salvage" archaeology and "problem oriented" archaeology. It has come to mean that salvage archaeology is something kind of dirty, it is doing something that nobody really wants to do, but he kind of is forced to do. This is not the crux of the matter at all. It is something that you have to do and it is something that is important and you do it right, and, I submit, usually has been done just as right as the problem oriented archaeology.

Now, Art put his finger on another point, and that is the main difference between salvage archaeology and other kinds of archaeology-- and as far as I am concerned there is no such thing as problem oriented archaeology, everything is problem oriented-- and that is that it is terminal. You can take your St. Albans Site and you can start digging and, if things develop, you could work 150 years if you wanted to,.... or 5 years. But you would not do that when they are building a dam or building a highway or an airport. This is terminal. You have a certain number of days, or months, or years, to get this job done and when that is done no one else can come back in and do anything at all because the site is gone, terminated. You have got to do it, so some of your field techniques are a little bit different, but they do not have to be inferior. I suggest that Harold was correct in saying that this dichotomy, this polarity, is not justified. We are doing archaeology, whether it be on a terminal site or on one that you can continue for as long as you want to.

CALDWELL- I agree with Bob Stephenson absolutely and there is one little by-way that I think is worth mentioning. We are not, in this generation, capable of deciding which are the most important sites to dig, yet sometimes-- and we have seen it happen with the Great Southwest Atlanta Corporation-- we are "willy-nilly" forced to dig a site that you don't want to, and all of a sudden you get all your ideas changed and you say this is something that I would have dug in the first place if I had just had sense enough to dig it. One time I was up at the Dixon Mound, I had a crew and I was running out of places to dig. Well, I

knew good-and-well that Indians do not live on the side of a hill, unless one leg is shorter than the other, so I sent them up the side of the hill, and, by gosh, there was a village site, and I would never have dug there in a hundred years unless I had to. Now I know that Indians do live on the side of the hill.

HAAG- Well, the very fact that the original Antiquities Act that Hester Davis spoke about this morning, was concerned only with reservoirs, indicates that at that time the best site archaeology was done in reservoirs. We have moved out of this category. Art brought out the fact that, as you fly from Atlanta to New York City, at night particularly, you can see that even these Georgia hills have bright lights everywhere. It is occupied everywhere. There is a tremendous change in the surface of the land just by this simple process of building houses and subdivisions. So, it is inevitable that we have got to turn our attention to other things besides big dams and industrial applications and construction, to an awareness that an awful lot of what we are losing is being lost by everyday commonplace "garden variety" of construction. Sometimes, of course, this is a boon to the archaeologist. There are areas which may have had forest cover or something of this nature and you could not find any archaeological remains, but here comes some stupid guy who cuts rectangular streets through there and aids you in your patterned collecting. So, perhaps we do have to turn more and more of our attention to that sphere.

CORBETT- I would like to make the point that there should not be this dichotomy between "salvage" and "problem oriented" archaeology, and the proof of the matter is that "salvage" archaeology has forced us into "problem oriented" archaeology. Frequently when a reservoir is being built in an area that has been investigated for 50 years or one where you have a year or two in which to gather information and sites, then you know what the problems are. You do not know what the problems are when you first move into a reservoir if you have not had time to do this preliminary work. It is problem oriented after you get in there, after you have been forced in there. The point that Art raised about getting involved in other segments I think is important. It is a salesmanship job.

HAAG- The last piece of archaeological work that James Ford did in the field-- I happened to be with him-- was a real emergency. A local industry in Baton Rouge was going to make an expansion to their plant and they proposed to reduce an area that included two large earth mounds. They gave us anything we needed, laborers, equipment, etc. I really believe that we have not looked into these private prospects as much as we should have. I know that this company in Baton Rouge was using this as a contribution to education and a tax write-off. Another aspect of this is that, as many of us have found out in deals like this, there is a tremendous personal interest on the part of the public in this sort of activity. You do not really have to apologize for doing archaeology as adults. I can remember being questioned while I was doing field work, you mean to tell me that a grown man goes around picking up stuff like this and doing this kind of work. Maybe we need to try to approach this in a way that will lead to some kind of fruitful results. Maybe we need to become a little more organized in our approach to the public. I think this endeavor that is the outgrowth of the Antiquities Act.... the amendment.... will have far-reaching effects because one of the aims is to get to the people who really do not know what archaeology is all about. They wouldn't know a potsherd if they found one in their corn-pone. So, perhaps we need to begin to work more and more in that area.

CORBETT- The Antiquities Act was passed in 1906. The act that is being amended is the Act of 1960 dealing with reservoirs.

HAAG- The Antiquities Act of 1906, if I recall (I could hardly read then), was concerned with the preservation of material found on public lands.

CORBETT- Lands controlled by the Federal Government.

HAAG- This amendment does look like it will close up a great many of the holes and enable an expansion in a direction that could prove very fruitful to us.

STEPHENSON- I think the real crux of our problem today in salvage, in archaeology, is the fact that we are being overwhelmed by an emergency salvage necessity that faces us today. As John pointed out, there are urban developments, airfields, highways, and reservoirs, everything you can think of. There is enough emergency in any one state today to take all the archaeologists time that are in all the other states. It seems to me that our basic problem now revolves around the matter of how we are going to handle all of these instant emergencies that are overwhelming us. We do not have enough archaeologists. Some of the sites are small and others are large, but we have an obligation to ourselves and to our profession to do this work. In South Carolina we have so many things that we can't even ask for money for because we do not have the people to do the work. I think this is the real crux of whether salvage archaeology today, is how are we going to handle the mass of jobs we have to do?

KELLY- I think that also we still have not grasped the rate at which sites are being destroyed. I think this has to do with the fact that we have not really grasped the archaeological richness of most of the total area of the southeast. At the present rate at which these areas are being destroyed, completely denuded, and torn down to a depth of several feet so that there is no possibility of any future work, you can calculate that within a relatively short period of time there are not going to be any archaeological sites at all except those that have been incorporated in National Parks or Monuments or some sort of public owned land. And even if they are incorporated in such public owned areas they are not really protected there, because we never know when a county or state is going to do something with their own land, even if they are only developing a recreation area. Because when you develop a recreational area you build ponds, lakes, golf courses, and various other things. So, unless it is controlled, they are going to destroy the sites that are left on these areas. The point I am trying to make is that I do not think that we realize the terrific impact, the acceleration, that is going on at a rate that within 25 or 50 years there will literally not be any archaeological sites left to salvage or to approach from any point of view, problematical or otherwise.

GRIFFIN- Given the immensity of this destruction and the shortage of people that Bob just talked about, granted that we could get all the money that we want, the shortage of people and the immensity of the problem comes down to the fact that somehow, either explicitly or implicitly, we are going to have to start exercising some priorities. Then the question arises, on what basis do we select these priorities? Is it the one that comes across your desk first, or will we try to ask ourselves what do we want to know? On what basis do we make priorities.

HAAG- This is the real impetus, this matter of priorities and immediate threat, for the formation of the Mississippi Valley Archaeological Survey-- not in the Phillips, Ford, and Griffin vintage, but more recently. It is because we stand in the face of a technological threat to these agricultural lands in the area that did not exist before. Here, all over the lower Mississippi Valley, one sees what is called a "land-plane", which is a big device with a blade on it

that they drag and take out every inch-high bump and end up with a piece of land that is as level as this floor. In the process of doing this, naturally, any archaeological material is just scattered all over the place. Well, this was something that we might cluck our tongues at a few years ago, but in Louisiana, as an example, something like 70,000 acres was so-done during 1968. In that same year, almost 3 million acres were land-leveled in Arkansas, and they have 15 million acres projected in the future. Our archaeological sites are literally obliterated. You find that instead of it being concentrated in an area, say 300 yards in diameter, now it is spread over a third of a mile. There is nothing left, so this is a genuine threat. We do have to think in terms of priorities.

STEPHENSON- There is one other matter in terms of priorities, still in another dimension, that must be inserted here, and that is the non-emergency sites that also have to be dug. St. Albans is, I think, a good example again. Here is a non-emergency site, a non-salvage site, that is crucial to a large area, a terrifically important site, that will help to answer some of our problems that make some of the jobs in salvage easier if we get this kind of a job done first. So, we have to insert some priorities too into the mass of things that we have to do in salvage. Further, these non-salvage areas or sites that are not in an emergency today, may be in a terrific emergency 5 years from now. If we can priority ourselves and our work, where we can get these things done a little bit ahead of time without knowing that we are going to have to, then we are in far better shape on the salvage. So, we have got to schedule some priorities for the non-emergency jobs too.

HAAG- In order to do this adequately, one needs to be an economist. As an example, this whole matter of land claiming in the lower Mississippi Valley is a direct outgrowth of the fact that they were beginning to put controls on cotton acreage, but there were no controls, and still are no controls, on soy-bean acreage, so that areas which were thought to be utterly useless for agricultural purposes are now being land-claimed, or leveled, and put into soy-beans. Mr. Neitzel has seen acre after acre in central Mississippi which he once cherished as the homeland of birds and other things to be shot and eaten that are now open soy-bean fields. Maybe the man only gets in one crop in seven years, but it is still worth his while to get that one crop in-- it is profitable. So, we could not anticipate this as something that was going to occupy our time, but we certainly can anticipate that similar things will happen, and it will not be confined to areicultural areas, it will be urban areas as well that we have to consider.

GRANGER- I think that another problem that has not been brought up is the fact that the initial destructions are themselves causing peripheral destruction as well. For instance, when a lake or reservoir is constructed, the land developers move in and grab up the land immediately around the lake so that we are faced with this peripheral problem. When we dug the reservoirs our contracts covered only the area that was to be directly effected by the reservoir, not those that are far above the high-water mark that the land developers are effecting around the reservoir. This is another source of destruction.

HUSCHER- this problem of emergency. In the different kinds of archaeological situations..... what we might call the "make time available". For instance, reservoir projects are usually outlined far in advance of actual construction. Now in other cases, in the commercial emergency crash jobs, you have no warning. They just move in with an end-loader and the next think you know they have half-a-square mile leveled..... longer range projects.... some of us old-timers should point out that it is not hopeless, but it does have to be done

SOUTH- I would like to comment in regard to assigning priorities to these sites. I think the questions we would like to answer are very significant, as to why we picked a particular site to be excavated; and if, in regard to a temple mound, for instance, no further information is desired other than just what kind of pottery was there, then let's take a bulldozer, split it in two and see. I think that these sites are not in that much need of that kind of salvage. I think we should think in terms of the fact that 50 years from now many of these reservoirs are going to be silted full and the temple mounds will still be there if they have not been bulldozed down in the name of salvage. We should take great care in the technique we use in assigning these priorities. We should not be so caloused in that we destroy evidence just because it is going to be under water for a few years.

HAAG- I think that is a very good point, in fact, archaeologists have always been aware of this, maybe sometimes in a vague sort of way, but when work was done in the T.V.A. program, as an example, a great deal of attention was given to an extremely accurate survey, the results of which were deposited in the National Archives, where 500 years from now if someone does find that you could walk all the way across Pickwich Reservoir (if you know where the stones are) then you will be able to dig down under 180 feet of silt and there is a priceless site, in fact 550 of them in the Pickwick Reservoir alone, so that is an important point.

JONES- assuming here that you could give priorities, presuming that you know which sites are going to be destroyed in time for you to work out a system but this would depend on you knowing all the sites in the area.

HAAG- Even a survey that you did to the best of your ability will not reveal all of the sites. This is very evident in areas where there is a very marked erosion going on, on the one hand, and sedimentation going on, on the other hand. In many of the coastal areas you have this sort of thing where the archaeological material is frequently only exposed by some sort of operation like construction of a canal or drainage ditch or something, it is 4 feet under silt or something.

LAZARUS- One small angle on assigning priorities occurred to me as "acts of God" you might say, where you have erosion We have sites we are losing in the bay from just plain weather. They have to be planned for excavation, too.

STEPHENSON- I would like to ask Harold Huscher to comment on a little experiment that he did several years ago along the Missouri River on a site that had been under water and was back out again.

HUSCHER- The soil in the southeast is different from the silt-soil in the Missouri valley. The reservoir was pulled down after the initial filling. A number of sites were exposed that we had not known about before, and one of them in particular had been found by the local collectors wading out into the water. We screened some of the site and found a few artifacts.....

STEPHENSON- This is exactly the point I was trying to make, that some of these sites in some of these areas where you have a worse situation or various kinds of siltings, when the site is silted up and the water is gone, 100-200 years later, and then when you go back there, you can gather goodies, but in effect you have nothing but a surface collection to work with. So often it is not possible to go back a 100 years later and dig up this site that has been covered by water.

HAAG- When you go back it is not well preserved. Does anyone have thoughts, maybe, about what or how this problem can be more systematically approached. Are we, because we are doing just the very best that we can and that generally means that we are asking for funds and getting about a fourth of that amount of money, are we doing everything that we can do? Would there be a more profitable approach to this?

CALDWELL- One thing that might help, Bill. I am new to academia, but I suspect that it will be easier in the long run to get money from Uncle Sam or from some corporation than it will be to get it from a Dean of Arts and Sciences. I would suggest that over the years we try to get as many archaeological positions set up within our universities as we can. In Georgia, for instance, every time we get a contract from the Park Service, there goes one professor down the drain, because there is nothing else that he can do except that until the contract is done, and if you have only three or four southeastern archaeologists, and some of us do not have that many, those men are just wasted until that contract is completed. So, a thorough-going approach to this, I think, is to enlarge university staffs if it is humanly possible, and I am not sure that it is.

HUSCHER- It is my own hunch (and because I am more interested in survey and particularly in the distribution) that the earlier studies in places where we really have time to do it, should be conducted more with an idea of a sampling program which permits us to write-off the non-essential sites. You do not waste your efforts and can then concentrate your work on those things that, either by virtue of uniqueness or promise of great return for the amount of time and money to be expended-- you then would have your priorities. That gives us another basis for differentiation. These are two separate problems, and I would much prefer to keep them separate. The idea that aerial distribution and sampling simply to identify, so that you can plot this distribution data, and then you have your detailed studies which are different problems and have to be handled in a different way. These distributions should be made much more consistently than they have been, a great deal of non-essential can be written-off and should be done as early as you possibly can.

HANSON- How are we going to gear-up to meet the need when you can't predict from one year to the next how much money you are going to have to work with?

CORBETT- Concerning the problem of how to get all of this work done, I think there is a relatively un-tapped resource and that is the amateur archaeologists and the amateur archaeological societies, like in Missouri, for example. Many of them are capable of doing professional work.

STEPHENSON- Those of us who are attached to universities in one way or another might develop night classes to instruct the amateurs in field techniques, etc. Night lectures to train them, work with them as we do the graduate and undergraduate students.

MARSHALL- What about getting individuals within each state to push for legislation on the level, laws patterned somewhat after this reservoir act of 1960, that any construction within the state on private or public lands of large proportion will have to be cleared through the state organization. I know, this is bureaucracy, but we are trying to experiment with something like this in Mississippi. It has not been in action long enough to evaluate the approach, but we are going to push it a little further before we see what happens.

HAAG- Does anyone have any experience in this?

CORBETT- This is a good idea, but pattern it after the preservation act of 1966.

HAAG- I know from my own experiences that this is an extremely difficult thing to get through the state legislature. Ordinarily what you can get through is so "toothless" and innocuous, I think that in the long run it does more damage than good, but it is desirable even though you get nothing that has any police action, any penalties, for not abiding by it. It does constantly bring this thing to public attention and eventually you will get enough education in the very idea, just as in Arkansas, which I think is a model for the results of a continued expenditure of energy. The first bill that they finally got through the legislative body was vetoed by the Governor simply because of the fact that he did not want to make it illegal or even undesirable to do archaeological work without some kind of clearance. Because he considered this a perfectly innocuous past-time for people. Well of course, it seems to me, as a neighboring state of Arkansas, if Arkansas can do it maybe even Louisiana might even come around to it sometime. I do believe that probably our best results are going to come by working through these interested amateurs and it is remarkable how many of them are perfectly scientific in their approach. Maybe it is true that some of them come into it because of a kind of antiquarianism, and it is true that some of them are simply forever beyond the pale, but nevertheless, you can easily bring most of them into that situation where they see something beyond just picking up pretty pieces.

BAKER- I would like to make a comment that may be pertinent. I view the fight we have as archaeologists as only part of a bigger fight and that is the environmental fight, not just historic sites or prehistoric sites, I think it is much more than that

HAAG- Perhaps we might have reached that point where I might summarize what we have said, in an inadequate sort of way. It is perfectly obvious that we do recognize the fact that we as a profession are faced with a problem, literally, how to accomplish what we, as professional archaeologists recognize as our duty, to salvage as much of these remains as possible, and secondly, how do we supply that kind of manpower. In a fluctuating market, the demands are not the same at all times, and by the same token, the amount of available funds are never the same at all times. We do not have any uniformity to this. Other aspects of this are that we do recognize that there are a number of inter related ways in which we can approach this. I think we are of the consensus that the exploitation of the amateur archaeologist is something that we have neglected and something which we can show produces frequent results.

So, I believe that we are not, perhaps, quite ready to talk about some sort of organized program for attacking the problem of salvage archaeology. We still have not even recognized all the aspects of this problem and in each year we learn more from someone else who is doing something different and we emulate better ways to the best of our ability. I think, largely, we have to play this by ear and that each time we point out one of these acts, it does warn us that this will recur again. We know, and as many of you have pointed out, that the situations that we did not anticipate a few years ago may certainly give rise to new situations. Construction and many other things of this sort all alter the face of the earth. We must be ready to meet this in every way, while we can take a broader, more idealistic approach or view of our problem, mainly trying to cooperate with every other agency, every other discipline that is concerned with conservation as it is concerned with man. Perhaps there isn't much more we can say at this point except that we are like mosquitos at a nudist camp: we know why we are there, but we just don't know where to begin.

CORBETT- I do not think that we have to appoint any committees and I do not think that we have to take any kind of positive actions..... I think these kinds of sessions are necessary. This is the finest form of archaeological group therapy that I know of.

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(END OF SATURDAY AFTERNOON SESSION AND 1969 MEETING)