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Archaeology at the Lyon’s Bluff site, A Mississippian and Protohistoric settlement in Oktibbeha County, Mississippi

Terry Lolley

Abstract

The first recorded excavation at the Lyon’s Bluff site was by Moreau Chambers in 1934–35. The site received further attention during the 1960s and 1970s as volunteer and student groups assisted in archaeological investigations under the direction of Richard Marshall. While several papers have appeared concerning various archaeological aspects of Lyon’s Bluff, the lack of a detailed, comprehensive report of investigations has hindered the dissemination of data recovered from the site. This paper will attempt once again to draw attention to the site’s importance and to present for the first time a complete, measured topographic map of the main site area.

Introduction

During the spring of 1993, with the help of volunteers from Mississippi State University’s Cobb Institute of Archaeology, topographic mapping of the Lyon’s Bluff site was undertaken. Lyon’s Bluff (22-Ok-520; the site has variously been designated in the past as 22-Ok-1, 22-Ok-501, and 22-Ok-520, but the latter site number officially supersedes the previous two: Keith Baca, personal communication) is located in northeastern Oktibbeha County, Mississippi, adjacent to Line Creek and about 20 km up the Tibbee Creek drainage from the Tibbee Creek site, 22-Lo-600 (Figure 1). It occupies an area of gently rolling hills which is today mostly pastureland with some hardwood and pine vegetation. Isolated chalk outcrops appear in some areas of the site.

Recorded archaeological investigations at the site extend back to Moreau Chambers in the 1930s. Chambers conducted a short-term excavation, uncovering burials and artifacts, but the results of his work were never published (see Galloway, this issue). The majority of the work conducted at the site, however, was led by now-retired Mississippi State University (MSU) Professor Richard Marshall in the late 1960s and early 1970s, and included
two joint field schools between MSU and the University of Mississippi. Numerous burials and artifacts were recovered in three seasons of fieldwork. A synthesis of this work has not appeared, but Marshall has reported some findings in several articles and papers referenced here.

Marshall used sketch maps and renderings from aerial photographs to describe the site (e.g. Marshall 1986a:Figure 6.1) and guide the placement of excavation units during the period of active field work. His descriptions concerning the number of "house mounds" and other landscape features at Lyon's Bluff are difficult to interpret without a more formal map. It was this lack of an overall topographic site map that led to the undertaking outlined in this paper. Volunteers and equipment from the Cobb Institute of Archaeology made the mapping of the site possible.

Site Mapping

The topographic map presented here was produced with the aid of a transit and stadia rods. Several datum points were used across the site, since the area was so large. The initial datum point was set at the southwest cor-

ner of the Daughters of the American Revolution granite monument atop the platform mound. Elevation and distance readings were recorded along transects in a 360° radius from each datum point. Certain limitations are noteworthy with regard to the current map. Time and manpower requirements limited the mapping of the site to that which lay in open pasture up to the edge of the heavily wooded area that surrounds it. There is also an open area farther to the west through a treeline that was not mapped. According to Marshall (personal communication) the site extends into the treeline north of the pasture some 100 meters and into the unmapped area to the west of the present topographic map's western boundary.

Marshall (1986a) believed that the site could be divided into two halves, with the eastern portion representing a Mississippian occupation overlaid by later Protohistoric and early Historic occupations and the western portion representing a later Protohistoric component. In addition, Marshall has suggested that the eastern portion of the site was palisaded, based on an "abrupt discontinuity in the surface distribution of cultural material" (Marshall 1986a:82).

Marshall (1986a:82) suggested that the eastern area around the platform mound and possibly the open plaza west of the mound had been "artificially leveled." The topographic map (Figure 2) indicates a raised area, extending west to east and encompassing the platform mound, that is presumably what Marshall was observing. He also suggested that some ten distinct house mounds were present around the platform mound and plaza area, with four more north of the mound "immediately overlooking the creek" (Marshall 1986a:82). Only three of these small mounds are apparent on the topographic map, two to the northeast and one to the southwest of the three-meter high platform mound (Figure 2). These mounds may correspond to the eastern house mounds 1, 6, and 14 on Marshall's sketch map (Figure 3). Marshall also indicates on his sketch map a Protohistoric house mound (14) overlying a small Mississippian house mound (9). No evidence of either of these features is apparent on the topographic map.

During Marshall's investigations, he also noted the presence of at least 15 small mounds, or swells, in the western site area that he attributed to a Protohistoric component. These were interpreted as house mounds. Surface collecting has been the extent of fieldwork on the mounds in the western site area, where Marshall (1986a:84) noted a high density of artifacts.
similar to those recovered from the Protohistoric complex around Starkville, Mississippi, by Atkinson (1979).

An examination of the current topographic map shows a small number of "swells" in the western portion of the site. Presumably these are locations of some of the house mounds Marshall observed in the field. A comparison of the topographic and sketch maps (Figure 4) allows some correlation between the mapped features and those observed by Marshall. The most prominent matches include mounds 1, 2, 3, 5, and 7. These mounds are visible as elevated features along an east-west ridge on the topographic map. The approximate locations of mounds 4, 6, 8, 15, and 16 are on rather level terrain among the mostly rolling pastureland. Estimated locations for mounds 9, 10, 11, and 17 were plotted directly from Marshall's sketch map, since mapping was not performed in the wooded area.

Mound locations 1, 2, 3, 5, and 7 were visible in the field in 1993. Attempts were made to locate visually the remaining mounds in the grass covered field. It was difficult to distinguish between the present landscape of rolling hills and any other "mounds" that may have been present. Marshall had the advantage during his fieldwork of a "plow-disturbed surface"
(Marshall 1986a:86) that afforded better opportunities to locate these mounds. It is important then to note that some of these “swells” were designated as house mounds based on artifact distributions and therefore may not be reflected topographically (Marshall, personal communication). It seems probable that the Protohistoric houses were built upon these rolling ridges, and some of the “swells” that were observed in the field were not necessarily the result of years of deposition. The lack of visual evidence for the house mounds may also be attributed to agricultural impacts to the site over the years. Marshall comments that some of the mounds are only elevated some 0.6 to 0.8 m (Marshall 1986a). Some of the mounds may have been of such a small elevation that they are not discernable at the 50 cm contour interval used in Figure 2.

Marshall’s field notes from the 1967 season provide some details on the work that was performed. In the northeast corner of the pasture, on the south bank of Line Creek, Marshall noted just over one meter of buried deposits, with numerous living surfaces and house floors. Several burials were recorded in approximately 27 square meters of excavation. This location correlates with the elevated area depicted on the topographic map on the bank of Line Creek and house mound 1 on the sketch map (Figures 3 and 4).

Excavations in 1967, south of the platform mound, included a small test pit that revealed over one meter of deposits. Several burials and house floors were found, which prompted the enlargement of the excavation area to some 22 square meters. One discovery west of the mound and east-southeast of a large pecan tree was the remains of a semi-flexed male with burial goods all placed in a log- and mat-lined grave beneath a house floor. Goods consisted of a cooking pot, grinding palette, mussel shell spoons, and two pestles. At the head of the individual were more shell spoons, a large piece of cut mica, a small pot, an incised shallow bowl, and a ground and polished slate pipe.

A third area investigated in 1967 was west-northwest of a pecan tree and consisted of approximately 36 square meters of excavation. Marshall noted that this elevated area may have been the remains of a house mound (9 and 14). Several burials were recorded, including the remains of ten small children and infants. The final portion of the site to be studied in 1967 was 54 square meters simply recorded as being to the west of the platform mound.
A possible circular house pattern was recorded in this area, while all other house patterns at the site were rectangular.

**Site Context**

The alluvial valleys of the Tombigbee River and its major tributaries in the Black Belt portions of the west-central Tombigbee drainage area contain the remnants of an extensive Mississippian occupation. Information on Lyon's Bluff, one of the largest Mississippian sites in this region, has been presented in a series of descriptive papers (Marshall 1977, 1986a, 1986b). Two phase sequences have been defined for the Mississippian-Protostrophic periods along the central Tombigbee River: the Tibbee Creek-Lyon's Bluff-Sorrels phase sequence based on Marshall's unpublished excavations at Lyon's Bluff, and Summerville I-IV, based on excavations at Lubub Creek (Pebley 1983). Due to the fact that the Lyon's Bluff excavations have not been completely reported, the majority of the archaeological literature related to this area discusses the Mississippian and Protohistoric periods in terms of the Summerville sequence. Marshall (1986a) and more recently Blitz (1993) have suggested that continued research may reveal important distinctions between the two phase sequences, particularly for the Protohistoric period.

The earliest Mississippian occupation at Lyon's Bluff may have been around A.D. 1130±65, based on a charcoal sample from a post mold submitted for dating by Marshall (1986a). This (uncalibrated) date places the site within the Early Mississippian, Tibbee Creek or Summerville I phase. The settlement system characterizing this period along the central Tombigbee is represented by mound sites and small outlying habitation sites. All but one of the six known platform mound sites on the upper and central Tombigbee River proper are located south of Columbus, Mississippi. A few other mound sites are scattered throughout the Tombigbee drainage along secondary streams, and the Lyon's Bluff site is the only one of these extensively investigated to date. This settlement system apparently consists of a local center with a platform mound and habitation area, like Lyon's Bluff, and dispersed small settlements or farmsteads (Blitz 1993). Blitz (1993) discusses the six other local mound centers along the central Tombigbee, the Butler, Chowder Springs, Coleman, Lubub Creek, Hiltman, and Brasfield sites. Lyon's Bluff and Lubub Creek are thus far the only ones from which data on occupational sequences have been obtained.

An additional radiocarbon date of 1210±65 from Lyon's Bluff is near the end of the Tibbee Creek phase (Marshall 1977). The structural remains from Lyon's Bluff that Marshall assigned to the Tibbee Creek phase represent wall trench houses with narrow, deep trenches, often crossing at the corners (Marshall 1986b). The Tibbee Creek site itself is considered to date to the end of the Summerville I phase, using Pebley's (1983) sequence. The single structure excavated at Tibbee Creek was also of a rectangular, wall trench construction (O'Hear et al. 1981).

The Middle Mississippian phase at the site is the Lyon's Bluff or Summerville II-III phase. Only a preliminary definition and dating of the Lyon's Bluff phase has been published (Marshall 1977, 1985). Lyon's Bluff phase houses at the site were larger than the previous Tibbee Creek phase (Marshall 1986b). Marshall (1986b) reports that during this phase there is an increased presence of Moundville-type special goods at Lyon's Bluff. The Coleman mound south of Columbus appears to have been abandoned by this time (Blitz 1993). Lyon's Bluff was possibly palisaded according to Marshall, while Lubub Creek was unfortified for part or all of the phase. According to Blitz (1993), site abandonment, cycles of fortification, and the adoption of Moundville Engraved pottery suggest an intense period of social interaction between local Tombigbee centers, presumably caused by the rise of Moundville to the east.

The Late Mississippian/Protohistoric phases at Lyon's Bluff were identified by Marshall (1986b) as the Sorrels and Mhoon phases, roughly equivalent to Summerville IV. A radiocarbon sample obtained from charred wood and charred corn from a pit feature had a corrected date of A.D. 1557±65 (Marshall 1977). Marshall (1986a) makes some comparison between the Sorrels phase and the Late Mississippian Moundville III phase, but others have correlated Sorrels with the Protohistoric Burial Urn culture, which would be closer to Moundville IV (Sheldon and Jenkins 1986; Solis and Walling 1982).

Marshall (1986a) suggests that the Mhoon phase at Lyon's Bluff is largely represented by the western village complex, which consisted of a series of scattered houses on ridge spurs. As noted above, he suggests that this phase is associated with the Protohistoric Rolling Hills complex in Starkville, six miles west of Lyon's Bluff (Atkinson 1979). Glass beads, brass bells, iron implements, and a Nodena Red and White bottle were identified by Atkinson.
at Rolling Hills. Refinements are still needed for the Protohistoric ceramic chronology in the central Tombigbee region.

Future Research

The mapping of the Lyon's Bluff site is a small beginning to a much larger task. The topographic map has indicated that the house mounds observed during the excavations in the 1960s and 1970s are less discernable on the ground today, both topographically and by artifact remains. Few artifacts were observed on the ground surface during the mapping survey in 1993, partly due to ground vegetation.

Future research at Lyon's Bluff would ideally include a re-analysis of previously recovered material and field records, plus new field investigations. A brief examination of some of the archived material from the site indicated the difficulty one would face in trying to work with the original material. Field provenience information is confusing and hard to correlate with plan maps and artifact collections (see Sam Brookes, this issue). Other records from the fieldwork are equally disorganized and hard to interpret. Analyzing and recording the cultural material for descriptive purposes may be the only option for most of the Lyon's Bluff artifacts. Exceptions to these statements may be seen for the burials and associated goods. The burial records have better provenience information and were cataloged more thoroughly than regular unit levels.

Primarily due to the state of the Lyon's Bluff records, new field investigations at the site may prove to be the most beneficial endeavor. Several options, done singularly or in combination, are available for future field work. A systematic survey of the site area involving shovel testing has never been performed. This type of testing would provide a good picture of artifact distributions across the site. Since Marshall plotted the house mound locations primarily based on surface artifact distributions, the testing would further enhance those location estimates and hopefully lead to the discovery of other house sites. At a minimum, the proposed house mound locations could be shovel tested to determine if they represented occupational deposition or natural topographic features.

Another option may consist of further excavations at the site. During this process a re-excavation of Marshall's (and possibly Chambers's) test units could be performed to rerecord the stratigraphy. This approach could provide information that would aid in clearing up some of the confusion with provenience records, unit profiles, and plan maps. New excavations could be focused on some of the western house mounds that are more definite in location. Data concerning house construction, chronology, and material remains for the Protohistoric occupation at Lyon's Bluff has been based on surface collections at the site. The excavation of these Protohistoric house locations would provide more useful data than the previous surface investigations.

Another consideration for field investigations may include remote sensing techniques. Ground-penetrating radar (GPR) could be used to aid in the location of subsurface features at these house mound locations. GPR would be a non-invasive technique to provide data on whether or not some of these swells in the western site area are actual house locations. Remote sensing may also be a tool for examining the possible location of the palisade that Marshall believes surrounded the eastern portion of the site.

In addition to further excavation and testing, the wooded portions of the site that were not mapped in 1993 need to be mapped. Marshall had indicated on his sketch map several house mound locations that are now within the treeline north of the western site area (Figure 4). Further mapping will determine whether these mounds are visible on the ground or are other examples of Lyon's Bluff house mounds that are represented only by artifact distributions.

One of the primary topics of discussion emerging from studies of mound sites like Lyon's Bluff involves socio-political characteristics of the sites and their inhabitants. In the present case, it is the socio-political relationships between the inhabitants of Lyon's Bluff, the other small mound centers south of Columbus, Mississippi, and the surrounding farmsteads that are of interest. Lubbbub Creek is currently seen as a local center with perceived associations with the Tibbee Creek, Kellogg, and Yarborough sites in the Tombigbee River Valley in Mississippi. These sites were in closer proximity to Lyon's Bluff than Lubbbub Creek. What type of relation, or competition, might there have been between Lyon's Bluff and Lubbbub Creek?

Blitz (1993) presents a discussion of prestige goods within a two-tiered Summerville settlement system, in this case between Lubbbub Creek and outlying farmsteads. Presuming that Lyon's Bluff was contemporary with Lubbbub Creek and occupied a similar position in a two-tiered settlement system, certain similarities should be evident between the two local centers, and indeed this seems to be the case. Both sites contain artifacts that have been considered to be "superordinate" at Moundville, even though it ap-
pears at Lubbub Creek that these goods were available to less elite people than at Moundville. As with the Summerville mound and farmstead sites, it appears that the occupants of Lyon's Bluff were also able to possess certain material objects that archaeologists have used to define rank at Moundville. Also similar are the small occurrences of non-local ceramics at Lyon's Bluff and Lubbub Creek.

As with other local centers and associated farmsteads, Lyon's Bluff was probably economically self-sufficient, with localized interests. The question still remains as to what the effect and extent of Moundville's influence may have been at the site. Hally (1987) and Blitz (1993) both discuss the possibility that, due to the distance between the two sites, political influence from Moundville may not have been a significant factor in the daily life of the Lubbub Creek site inhabitants. This same argument presumably would hold true for Lyon's Bluff as well. The economic role Lyon's Bluff may have played as a local center is also of interest. Recent bioarchaeological investigations have revealed differences in mortality profiles and carbon isotope ratios between Lyon's Bluff and the few local farmstead sites that have been investigated (Hogue, n.d.; Hogue and Peacock 1995; Hogue et al. 1996), and these differences in health and diet may well reflect socioeconomic differences that existed between the mound center and outlying farmsteads.

These and other questions concerning the role that Lyon's Bluff played in the Mississippian and Protohistoric environments in the Tombigbee River valley cannot be fully addressed until further testing of the site is performed. The accumulation of new data from the site is required to understand more completely the occupation of the site and the relationships that may have existed between it and other communities.

Conclusion

Although this paper began as the simple presentation of a topographic map, it evolved into more of a discussion of what we do and do not know about Lyon's Bluff. Incorporation of the site into a regional framework has been hampered by the lack of available source material. A large amount of cultural material still remains to be analyzed, and a synthesis of work already conducted at the site is still needed. The rich archaeological record at Lyon's Bluff can help to address the lack of data on local mound centers in the Tombigbee River valley. This single mound center on a secondary stream offers an opportunity to tackle many current research questions related to small-scale Mississippian communities in the region.

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Archaeology from Memory: Lyon's Bluff, 1968

Samuel O. Brookes

Abstract

For many archaeology students of the 1960s and 1970s, Lyon's Bluff was a rite of passage and an experience in participant observation. The following reminiscence revives the spirit of the times and explains some very obscure references for future archaeologists attempting to decipher students' notebooks from Lyon's Bluff.

Entry into the Field

The summer of 1968 stands out in my mind for a plethora of reasons. I had recently completed Anthropology 101 and 102 and could now take some upper level courses in archaeology. I was also excited, and not a little scared, because I had been selected to go on the "dig," the Ole Miss Summer Field School. This was going to be an adventure because as a poor student with no car, my travels in Mississippi were limited to walking around Oxford and one trip each to Memphis and New Albany. We were going to be based at Mississippi State in Starkville and dig at Lyon's Bluff. My experience in archaeology was minimal, as I had been a not-so-knowledgeable amateur for years, collecting Archaic tools all over Halifax County, VA. I did have two days' field experience digging with the Archaeological Society of Virginia. Tom Koehler, the professor at Ole Miss, had accepted me and a number of others for this expedition. We were given a list of required equipment, so I set out to comply with Koehler's request while I was at home on break before summer school started.

The first was easy enough: one trip to Wilborn's Hardware on Main St. and I had a Stanley steel tape measure (six foot length) and a Stanley line level. I also purchased a pith helmet, as I thought it was rather sporty. Then came the hard part. At that time I had a real phobia about needles (the hypodermic kind) and I had to get a tetanus shot. There was no recourse but to go see Hannah Hite, our beloved family nurse, and get the dreaded
shot. I can still remember that she did not hurt me with the shot. I wish they could all be like Hannah, now retired and writing a column for the local paper. Having accomplished that, I headed to Cluster Springs for a barbeque at Wilborn’s Snack Bar before flying back to Mississippi the next day.

Arriving in Oxford, I had a dorm room for a day or two, though the details are foggy to say the least. We met in the Ole Miss Anthropology Lab in the basement of the Education Department (John Connaway lived there when he was in graduate school, but that is another story for another time). This was the first time that all of us on the dig got together, and it was a motley crew, with fraternities and sororities represented as well as independents, hippies, archaeologists-to-be, and one philosophy major.

Trying to seem somewhat aloof while checking out the ladies and the competition, I sized up the group. All in all they seemed pleasant enough, with several cute girls. Mr. Koehler was talking about how we would all get to Starkville the next day. I somehow became friends with a guy named J. D. Caldwell from Jackson. He had a VW Beetle and we decided to room together in Starkville and drive down the following day. The next morning, J. D. and I drove through the Black Prairie area of Mississippi. I recall its looking dark and foreboding. On the way down, I talked a lot about Heller’s book, Catch-22, that I had read the semester before. I brought it along and J. D. read it, and being enthused by the book and the dig, he decided it would be cool not to unpack his suitcase the entire six weeks we were in Hamblin Hall, and so he didn’t. Catch-22 became sort of a cult classic for us that summer, and there were many references to it in both our conversations and field notes. This should cause some perplexity on the part of whomever tries to decipher those notes in future years. References to Lyon’s Bluff burning, Yossarian, etc. should be attributed to the rather flippant attitude that prevailed for much of the summer.

At any rate, we arrived in Starkville and found that much to our delight, the MSU Student Union was far superior to anything on the Ole Miss campus. The food that summer was out of this world. The Union became a major hangout when we weren’t at the famed Crossroads. The Roads sold beer and was much closer than the joints out from Oxford. It seemed to have an ambiance lacking in the dives around Holly Springs. We met with the MSU students at Montgomery Hall, which was quite a beautiful build-

ing and is now on the National Register of Historic Places, I believe. There I first met Dick Marshall, the archaeologist at MSU. Dick was a nice fellow and the MSU students seemed pleasant enough. The one MSU student who really stood out was a guy in khakis who was wisecracking Irish jokes and puns and quoting some guy named Ford. His name was Bill Hony. I remember we were all greatly impressed by the drying racks and lab setup at MSU. It was far ahead of Ole Miss. We also journeyed to the MSU bookstore to pick up the final piece of our equipment, an engineering notebook. Those were great. Sections were marked off in units of ten so it would be easy to plot our ten foot squares, which was the major unit we were using.

Fieldwork

Our first trip to the site was memorable. Lyon’s Bluff was the first Indian mound I had ever seen in the flesh. It had a DAR marker on top which I believe proclaimed it to be Chicasa, where Hernando DeSoto & Co. fought the Chickasaw. Marshall and Koehler informed us that this was probably not the case. The site had sherds on it, and these were some of the first sherds I had ever found. There was also a lot of limestone around, and most of the crew could not tell limestone from sherds. Several of the crew—Sheila Lewis, Tom Birchet, Jason Fenwick, Bill Hony, and I—had some experience, so we could tell the difference. This ability earned me the title of “graduate assistant” along with Sheila Lewis, and I set out to instruct some of the students on the difference between ceramics and limestone. We found two types of ceramics, Bell Plain and Neely’s Ferry Plain. Dick Marshall taught me to sort the types. One had fine paste and it was hard to even see the shell (Bell) and the second was thick pottery with coarse shell (Neely’s Ferry). There was also another item we were introduced to that summer, daub. Daub was made of course the clay that had formed the walls of Indian houses, and had been fired when the houses burned. Daub was like brick, and it covered much of the area where we dug. It certainly covered the square that I worked in. We were issued mattocks as well as shovels, and without them we would never have chipped through the daub. But I am getting ahead of myself here.

We stood around in a circle and Marshall and Koehler talked about the site and what we were expected to do. Field techniques were discussed: we were to dig in six inch levels and record features etc. as we went down. This
lecture was interrupted by anguished cries from Bruce Gray. In an attempt to hear Marshall and Koehler better, he had stood on a large dirt pile. This was his, and our, first introduction to fire ants, and Bruce was in terrible pain as he struggled to pull off his ant-filled boots. We dug two students to a ten foot square. I chose Phyllis Anderson to be my partner as I had designs on that young lady, all to no avail I might add. We looked at the open pits from an earlier field school held the previous year. In fact we got down in the trenches and had a lesson in how to scrape down profiles. This resulted in several charcoal lenses as well as two Black Widow spiders being uncovered. Our units were placed next to these earlier ones.

Anyway, we began to chop through the first six inches of dirt and daub. It appeared to me to be mostly daub. We then troweled down the square. This was nearly impossible because of the aforementioned cursed daub. I do not remember our finding much, maybe some small sherds if anything. We were then to draw a horizontal plan of our square. Easy enough, I figured. The square is ten feet on each side and the engineering forms are in multiples of ten; piece of cake. Then I realized that a foot is comprised of twelve inches. We had units of ten on our form. Holy Moly, now I had to convert twelfths to tenths! This was in the day before hand-held calculators, computers, etc. Engineering students wore leather holsters that held slide rules. The easiest way was to roughly estimate the distance on the plan, close enough using inches we figured, and put the measurements in your notes or write them on the plan. I did not have the time or temperament to sit there and do conversions, and I suspect my fellow diggers were of similar mettle.

We then proceeded to the second level, six to twelve inches. This was done mainly by chopping out the daub. In today’s archaeology the daub is considered extremely important: check out Connaway’s use of it in the Wilford report (MDAH Archaeological Report 14) to establish construction techniques, and Peacock’s use of it to reconstruct the environment surrounding sites (Southeastern Archaeology 12, 2 [1998]:148-54). Below the daub was soil, which was a welcome relief, as my knuckles were severely scraped from the hard daub. All dirt was loaded into wheelbarrows and dumped in the dirt pile against the woods on the west side of the site. We did not screen the dirt. In modern times we would not only screen it but take many soil samples and retain all dirt from features for pollen analysis. Today soil would also be used for dating purposes. There is no doubt that much in the way of plant and animal remains was missed, but such were the field methods of the day. Additionally, several of us were smokers. While this habit is today frowned upon for reasons of health, it can also have a deleterious effect on C-14 samples. I further imagine a great deal of data was lost due to careless note taking and recording. I would not like to have my notes read at a meeting of professionals today. I was young and very green.

Marshall and Koehler had their hands full with two dozen green kids, chopping away at the site and all of us full of hormones. Luckily (for them) this was just prior to the sexual revolution. Anyhow, Lyon’s Bluff began to rain human burials on us. It seems as if everyone had at least two human skeletons in their pits, save for me and Phyllis. Alas, Koehler pulled Phyllis out of our square to help uncover burials. For this task, people used cane knives to scrape away soil from the soft moist bones without cutting them. Today burials are considered serious business, what with NAGPRA and Native American concerns. No such considerations existed in 1968, and nearly every square contained the remains of at least one human.

I continued to work alone and finally cleaned my square and recorded it. Koehler told me to keep it clean and to straighten the sides, as Dr. James B. Griffin was coming to the site. I had heard of Griffin and had read most of his Big Green Bible, Archeology of Eastern United States (1952). Koehler wanted him to meet me, as I wanted to become a professional. The idea was for me to quit work in my square and assist others. So I worked and ran wheelbarrows, attempting mainly to get near Phyllis, while everyone else finished level three, twelve to eighteen inches, and proceeded into level four. The great day arrived as did Dr. Griffin. I sat alone in my shallow square waiting for Griffin to acknowledge how clean my excavation was. Griffin listened while Koehler told him about my abilities. I was quite flattered right up until Griffin remarked, “Well he sure is a slow digger, everyone else is down several more levels.” Koehler bellowed, “Damnit Sam, get busy!” and my first meeting with Dr. Griffin was history.

Griffin wore a great hat for that visit—I have a picture somewhere—and he remarked that he had watched Tiny Tim on television the night before. Tiny Tim had a hit song with “Tiptoe Through the Tulips” and was famous for his quirky ways. As Griffin was driven off the site, he hung completely out of the back window and blew kisses to everyone, à la Tiny Tim! What a character!
I eventually got down to level three, and when I did, I hit a burial. Or at least at the time I thought it was a burial. The feature consisted of several vertebrae (three?) and a bone awl and maybe a couple more bones. It was a compact cluster of bones and I pedestalated and recorded it. After this I picked it up and took it over to the shade to remove the bones from the surrounding soil. At the time I thought the vertebrae were human, but they could have been deer or other large mammal. Other folks had better burials, large complete skeletons, some with triangular points in them. To the best of my recollection we found no complete ceramic vessels, though they had been found on the site in earlier years. We did not have any problem with vandalism, though some crows walked across a burial one night and smashed the teeth in a couple of skeletons.

As I said, just about everywhere we dug that summer we ran into burials. Bill Hony went back in the woods to the north to dig a garbage pit and twice hit burials. We used to surface hunt at lunchtime and occasionally found broken tools. I remember Tom Koechler found a couple of those shell "buttons," that are really part of a necklace, and are markers for the Alabama River Phase. Of course at the time I had no idea what they were, but was impressed with the workmanship nevertheless. Some of us tried to flintknapp, but none of us were very good. Marshall told us not to do that on an archaeological site. Oops! So some of the flakes in the southeast part of the site may not be aboriginal.

We covered the squares with sheets of plastic weighted at the corners with big chunks of daub. At the end of the dig, Dick Marshall gave me permission to take one big chunk of daub back to Virginia to show to my buddies in the ASV. The piece had a beautiful woven cane design that illustrated how the house walls were constructed. A couple of years later when I was at home I showed it to someone and for the first time turned it over. There on the back was a complete impression of an ear of corn! I later returned this to Dick as it is such a fine example; a botanist could glean much information from it.

Return from the Field

After six fun-filled weeks, the dig was over for most of us. I went back to Ole Miss to take French. A few folks stayed a couple of additional weeks to finish removing the burials. I wanted to join them but could not. I do not know if our excavations were backfilled that summer or not. I do remember it was a fun experience and was my introduction to archaeology. I think we all learned a lot, and I hope one day that site is written up because it is important, even if it is not Chicana. Several of the 1968 field crew went on to become professionals in archaeology and historic preservation. Sheila Lewis and Tom Birchett worked for the Corps of Engineers, Bruce Gray is still with MDOT, Jason Fenwick is in preservation work in Washington D.C., Bill Hony was an active archaeologist for years, Phyllis Anderson is an archaeologist in Sweden, and I am still working in the profession. One student, Gentry Yeatman, who later went into medicine I believe, attained fame when he X-rayed the mummy in the Old Capitol Museum and found it to be a hoax. This feat made Esquire magazine’s Dubious Achievement Awards in 1969. The students on the 1968 dig also made the same list in 1968 (in a roundabout way) because the day we took our field trip to Mountville, AL. was the rainiest day of the year. Columbus got over 10 inches in one day as we drove through.

All this made for great memories, and it was an experience that I will never forget. I think we all learned that archaeological work was important. The keeping of detailed notes and records can never be stressed enough. Even the tiniest thing can have great importance. Today people use the dirt as much as the artifacts to tell about past lifeways. Also, rather than washing artifacts, people today are looking for micro artifacts. As we move into the future, with greater use of computers and scientific data, our efforts will seem more and more outdated to future researchers. To aid those folks, we must keep as detailed a record of what we did and why as is possible.

Nineteen sixty-eight was an exciting year in another respect. That year John M. Connaway and Samuel O. McGahey were hired as archaeologists for the Mississippi Archaeological Survey. Dick Marshall was the director, and the budget if I remember was a whopping $20,000 for a fifteen-month period. MSU paid part of Marshall’s salary and loaned a vehicle. This doubled the number of archaeologists in the state to four. Stu Neitzel had recently left his position at MDAH, due in part to the advice of his doctor (a trumpet player in the New Bourbon Street Jazz Society). There were no Federal archaeologists in the state. The Natchez Trace Parkway had several who surveyed or excavated sites on an irregular basis, but did not put a full-time archaeologist on the Trace until the late 1970s. The National Forests had no
archaeologists, nor did the BLM. No archaeologists were employed by the Highway Department or any state agency, and of course in the days before SHPOs there were no private firms. Just Marshall at MSU, Koehler at Ole Miss, and Connaway and McGahey at MDAH. It must be remembered also that MDAH’s was a fledgling program and we were not at all sure it would survive. It did, though the program has never attained the level we had hoped: in 1968 we were talking about a Gulf Coast field office, and we still are, 32 years down the road. Nevertheless, we must look back fondly and see how many great things have occurred and how much good work has been accomplished since that mythic summer of 1968.

Postscriptum: Since I wrote this in January, I went home to South Boston, VA and actually found my field notebook from the dig. I have given this to MSU, along with some photos of the excavation that Harold (Bunker) Hill gave me. I also regret to report that our colleague and Lyon’s Bluff digger, Jason McCool Fenwick, died on Monday, March 13 in Washington D.C. where he was employed by the National Park Service. Finally, even as I write, the infamous Mississippi mummy is back on display in the Old Capitol Museum along with the letter from Gentry Yeatman informing Elbert Hilliard that it is a fake.

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Archaeology From the Archives: The Chambers Excavations at Lyon’s Bluff, 1934-35

Patricia K. Galloway

Abstract

_In 1934 and 1935 Moreau Chambers excavated at the Lyon’s Bluff site in an early attempt to clarify the archaeology of different regions of Mississippi. Though never reported formally, the excavations were recorded in field notes, drawings, and photographs. With the additional help of oral history evidence, this paper is an attempt to set the excavation in its historical context and to present its findings to serve as input to future work at the site._

Introduction

With the amount of development involving land alteration now taking place not only in Mississippi but throughout the country, and considering the fact that there is always a shortage of time, money, and expertise to carry out excavation to mitigate the effects of this land alteration, it may seem superfluous to try to reconstruct an excavation that took place almost fifty years ago. The work carried out at Lyon’s Bluff for the Mississippi Department of Archives and History (MDAH) by Moreau Chambers in 1934 and 1935, although not at all extensive in comparison with the work later led by Richard Marshall in the 1970s, and indeed although carried out without the more thorough understanding of the site’s extent and cultural affiliation that guided the later excavations, still did much toward beginning to define the nature of the site. In addition, Chambers’s work yielded several very interesting findings which deserve as full an exposition as is now possible with the aid of the sources at hand, since the evidence was no longer there for Marshall to explore when he did his work on the site. This evidence has never been formally reported, although it is available to archaeological researchers in the form of notebooks, drawings, and photographs at MDAH.

It is not unusual for excavations to be reconstructed on the basis of archival materials, but usually it is possible to refer to the material remains uncovered by the excavations to make up for missing information in the written record. This
is not possible in the present instance, but here we did have the opportunity to refer to the living memories of the two men who carried out the excavations, Moreau Chambers and Slater Gordon, when the original work on this paper was done in the 1980s. In several instances their oral testimonies have been able to dispel some confusion in the notes.

The intention of this report is to present data that may be used by others to pursue the history of the site. In particular, I hope that this report may serve as a tantalizing glimpse of things to come for those who will pursue their interest in the site through analysis of Marshall's more extensive investigations and new excavation projects. In addition, however, this report will also reflect the limits of usage of different kinds of archaeological evidence, especially when that evidence is only available in represented rather than material form.

Historical Context

As schoolboy and then college student, in company with James A. Ford and his brother David, Moreau B. C. Chambers spent several years visiting mound sites and collecting artifacts from them for MDAH on a part-time basis beginning in the late 1920s. Chambers was hired by Dunbar Rowland in 1934 as the Department's first full-time archaeologist, and during the summer of that year, with the assistance of Slater Rhodes Gordon and Everett Richardson, he began work on the Lyon's Bluff site in Oktibbeha County because it was convenient to the Brogan Mounds site that they had already excavated at the request of the West Point DAR chapter. This report will describe the two seasons of work at Lyon's Bluff that ensued, but before presenting the data it is necessary to provide some background in the history of early archaeology in Mississippi, both to explain the purpose of the excavation and to reconstruct the manner in which it was carried out.

Moreau B. C. Chambers had come to the attention of Dunbar Rowland, first Director of MDAH, when he visited Rowland's office in 1926 as a schoolboy of seventeen to show Rowland the collections he and James Ford had made from the Pocahontas site. While waiting in an anteroom for Rowland to arrive, Chambers met Henry B. Collins and discussed with him his interest in archaeology. Collins, himself formerly an employee of MDAH but by then an anthropologist with the Smithsonian Institution carrying out investigations in Mississippi, encouraged the boy, as did Rowland. In the following year, during the summer holiday, Chambers and Ford were hired by the Department to carry out diggings on mound sites around the state. Gordon Willey would later describe this activity as "officially sponsored 'pot hunting,' traveling from site to site by team and wagon" (Willey 1969), although it was probably no worse than what was being done in the name of archaeology elsewhere in the late 1920s. This work continued through subsequent summers under the informal name of the "Mississippi Archaeological Survey."

The young men at first had little to guide them beyond their keen interest, their reading of every archaeological publication they could get hold of, and the pamphlets being produced by the Committee on State and Local Archaeological Surveys of the National Research Council (National Research Council 1930). But their work won Henry Collins's patronage and advice, and he was eventually responsible for sending both Ford and Chambers to Alaska for the Smithsonian Institution. His advice was also probably responsible for Chambers's participation in the field school run by the University of Chicago and taught by Thorne Deuel, which was then establishing many modern archaeological field methods.

It should be noted that although recent attention has focused for various reasons on Ford's early career to the near exclusion of Chambers from this story (see Marvin Jeter's review of the two recent books by O'Brien and Lyman, this issue), it was clear from the beginning that Chambers was by far the more engaging of the two, took the lead in first attracting the attention of Rowland and Collins, and persistently secured the permissions of landowners and the support of local personalities to carry out, with Ford and without him, the Mississippi fieldwork that others would use to make their reputations. Because he then turned first to historical archaeology and finally to history tout court as a profession, his contribution has been, in the opinion of many who have made use of it, vastly underrated.

Dunbar Rowland's interest in sponsoring archaeological work was to obtain artifacts for the State Historical Museum that he was organizing to cap his thirty-year career as first Director of the state archives. His apparent personal preference for mound excavations may have stemmed from the great interest in "Moundbuilders" contemporary with his own youth or simply from the political expedience of directing that sites of interest to the lay public be explored. Chambers and Ford were passionately interested in archaeology of any kind, and in addition to that both were able to help finance their educations at Mississippi College with the pay from their seasons of summer work. It was only
after graduation and a season of employment as an archaeologist at the Shiloh Mounds in Tennessee that Chambers was hired for full-time work.

The work that Chambers then proceeded to undertake was only marginally aided by the manpower provided by laborers from Federal relief programs, since his boss Dunbar Rowland was a staunch opponent of Roosevelt and the New Deal. The Lyon's Bluff excavations of 1934-35 were actually pre-WPA archaeology, and it was only when local organizations were able to enlist the help of local relief agencies (most importantly the FERA) that such aid was available at all. The Lyon's Bluff site was excavated entirely without such funds.

Chamber's own interest in the site, and the interest that he conveyed to Rowland in order to obtain permission to excavate it, basically concentrated upon a desire to define something of the culture sequence of the area and to secure comparative material for the Brogan Mounds site. The strong local support for the project was at least partially due to adherence to the legend that the site had been the locus of a great battle, in which the Chokhuyi Indians had been wiped out by the combined efforts of their enemies, the Chickasaw and the Choctaw (Claiborne 1880:504; Halbert 1903:303-305), just as support for the Brogan excavation had grown from a belief that the mound held the slain of a great Chickasaw-Choctaw battle. Chambers repeatedly stated, in journal, correspondence, and interview, that he never gave much credence to these legends, nor does Rowland seem to have been much impressed by either story. Both would have been well-informed about the truth of at least the latter, since Rowland had edited Volume 1 of the planned *Mississippi Provincial Archives: English Dominion* series, which approached the period of the supposed Chokhuyi massacre, many years before (Rowland 1912).

The sources for this report on the 1934-35 excavations are all archival materials held by MDAH. The artifacts recovered from the site by Chambers, Gordon, and Richardson were destroyed, in all probability, by a fire in the warehouse where they were stored during World War II. The well-preserved skeletal materials from the site, which were carefully saved by the excavators, were irretrievably lost when they were put on loan to a private individual for display near Natchez; when the enterprise failed, the archaeological materials on display were scattered through neglect and outright theft (Baca 1989:38). Thus all we have to go on in reconstructing the excavation are the records made by Chambers and Gordon, both living at the time this work was begun and both participants in oral history interviews carried out by the Department. The records that were used are listed below with their archival references at MDAH; they will be referred to more conventionally throughout the text and in the references.

**Sources for the Lyon’s Bluff excavations of 1934-1935:**

1. Chambers’s Mississippi Archaeological Survey journal, 1933-37 (MDAH, Historic Preservation Division, series 1327, Moreau B. Chambers Files, 1932-1939; formerly vol. 218); referred to as “Journal.” This is a large bound journal book that Chambers used for a running narrative of events in the field, written in ink.

2. Chambers’s Mississippi Archaeological Survey notebook, 1932; 1935 (MDAH, Historic Preservation Division, series 1311, Archaeological Records and Reports, in folder “Mississippi Archaeological Survey Excavation Notebook 1932; 1935”; formerly vol. 220); referred to as “Notebook.” This small stenographer-sized school notebook was used by Chambers for actual penciled field notes of excavation details and includes some of the smaller drawings printed as tracings in the present report.

3. Set of 15 drawings of plans, sections, and burials (MDAH, Historic Preservation Division, Series 1311, Archaeological Records and Reports, in folder “Mississippi Archaeological Survey, Excavations, Lyon’s Bluff”; formerly vol. 220). All of these drawings were executed in sometimes very pale and/or smeared pencil on graph paper, so they were traced by Galloway for presentation in this report. The grid is not included. Also in the present report, inconsistent shadings added to plan and section drawings have been omitted in most cases where the nature of soil layers is otherwise indicated.

4. Photographs (MDAH, Historic Preservation Division, series 1331, Archaeological Photographs 1925-1938, filed by county and each given a unique number in the series). All the original prints have captions, some of them quite detailed, written on the reverse side; these are included as the captions for the photographs published here. Only a few photographs of the many available have been used in the present report.

5. Chambers’s correspondence (MDAH, Z1527, Chambers Papers). The finding aid for this collection at MDAH includes a calendar of all items,
dated and with correspondents indicated. Some items are addressed to James A. Ford.

6. Rowland’s correspondence (MDAH, Administration Division, series 1253; formerly vols. 29 and 30). Rowland’s correspondence is preserved in letterbooks.

7. Chambers oral history interview (MDAH, OH 80-01). This interview is transcribed and open for public use.

8. Gordon oral history interview (MDAH, OHP 395, 396, 397). As of this writing, this interview though transcribed is not yet open for public use.

The level of detail available from the sum of these sources is not equal to that of the field notes from a modern excavation, and of course the fact that there are no surviving artifacts available for further study presents definite problems, but the above list represents a surprising quantity of material, both for the stratigraphy and for the features in the area of the site that was excavated. There is, in short, an adequate supply of information to reconstruct some of the major findings of the excavation.

Field Methods and Techniques of Excavation

To a certain extent it would be possible to reconstruct many of the field methods used in the excavation from the bare records of the excavation alone. But fortunately we have more than this to go on, in that Chamber’s education in field methods is fairly well documented both by his own correspondence and a knowledge of the previous archaeological fieldwork that he had done. I quote in extenso from an outline of his career that he submitted in support of an application for graduate work to the anthropology program at Harvard University in 1934, showing references in square brackets to subsequent publication of materials recovered through his participation:

Here, briefly, is my record:—From the season of 1927 through the 1930 season I was Field Archaeologist on the staff of the Mississippi Department of Archives and History, engaged in locating and excavating Indian mounds of the Big Black culture in Central Mississippi for the State Museum [Ford 1936]. In 1930 I was in charge of the field work of the Department; our most important project was the excavation of a rich burial mound near Natchez, Miss., built by the Natchez tribe after their contact with the French [Neitzel 1965]. At various times I have been associated with Mr. Henry B. Collins, Jr., of the U.S. National Museum, particularly during village site excavations in the winters of 1929 and 1930, at Deasonville, Yazoo Co., Miss., reported on by Collins in “Excavations at a Prehistoric Indian Village in Mississippi,” Smithsonian Publ. No. 2898 [Collins 1932a].

From May to October 1931 I was a member of the U.S. National Museum Expedition to the Bering Sea region, where after collecting skeletal material near Unalaska and at Unalakleet, I spent three months on St. Lawrence Island, continuing the stratigraphic excavation of a group of ancient Eskimo village sites begun the previous season by Collins [Collins 1935].

During the 1932 and 1933 field seasons I was in charge of the Archaeological Survey of Mississippi, sponsored jointly by the Miss. Dept. of Archives and History and the Smithsonian Institution. Potsherd collections were made on the surface of a number of village sites throughout Mississippi; one result of this work has been the determining of the pottery characteristics of several historic tribes and the relation of certain prehistoric tribes to these historic groups. In this work I had a part [Ford 1935; Collins 1932b].

I might suggest that the report of the National Research Council’s Conference on Southern Pre-History, of which I was a member, contains notices of my work (pp. 39-41, 68, 70) [National Research Council 1932].

This past season, after carrying on the archaeological survey work in Mississippi as long as funds permitted, I participated in the work of the University of Chicago Field Party excavating a group of mounds and a village site in Fulton Co., Illinois, under the guidance of Mr. Thorne Deuel [Cole and Deuel 1937].


This curriculum vitae reads like a roll call of the important contemporary developments in archaeological method, from the work on stratigraphic excavations carried out by Collins in the Arctic and Cole and Deuel in the Illinois valley to the extensive surface collection work that Chambers had done with Ford and without him, which was to contribute so importantly through the work of Ford and Collins to the ceramic chronology of the Southeast and to the implementation of the Direct Historic Approach in
that area. Reference to the published works cited in brackets above gives a fairly complete picture of Chambers’s exposure to the archaeological techniques of the period. Knowing this we can see in his work at Lyon’s Bluff the application of a sort of smorgasbord of techniques called forth both by conditions at the site and by a chronic shortage of time, manpower, and money. But this perspective also gives us insight into what Chambers was trying to do in the most extensive part of the excavation, and thus permits us to reflect upon the reliability of the evidence that survives.

Reconstructions of the Chambers-Ford excavations on mound sites along the Big Black by Vin Steponaitis and John Shafter have shown, through careful examination of the field notes from those excavations, that the work was usually carried out by striking a trench through the mound at some point, most frequently the middle, and continuing to take various slices of the mound out until nothing was left (Steponaitis personal communication). The recording technique used, as recommended by the National Research Council’s pamphlets, was that of triangulation to record the positions of artifacts and burials both horizontally and vertically within the mound (National Research Council 1930). Little effort was made to record the stratigraphy of the mounds, but excavation of the burials seems to have been fairly careful and recording was by and large systematic and painstaking as far as the objects and bones were concerned. The same seems to have been true of Chamber’s work on Mound C at the Fatherland site. Neitzel, in his reconstruction of those excavations, observed:

The two triangulation points used by Chambers were not identifiable at the site. Apparently he drove nails into two selected trees south of Mound C and recorded measurements of the position of skulls and other finds by using a tape measure. This practice was followed by the Mississippi Department of Archives and History field parties at the time. Levels were measured by sighting along a carpenter’s level mounted on a homemade tripod. Occasionally a depth below the surface was recorded (Neitzel 1965:40).

Chamber’s work with Ford and Collins on the Deasonville site in 1929 and 1930 exposed him to more complex horizontal recording techniques in excavating three house rings. The three excavators had only a total of ten days’ work on the site, so they utilized a mule team and scraper to remove the plow zone once they had located the house middens by surface scatter and shovel testing. The observations recorded by Collins regarding the fine distinctions observed between materials and soils in the various wall trenches indicates that these trenches, their postmolds, and accompanying features were carefully excavated as features once they were exposed by the scraper. It is evident both from Collins’ report of the excavations (1932a) and from correspondence with Collins reflecting Ford’s speculations on the house structures that there had been insufficient stratigraphic control to determine definitively if the circular wall trenches were in fact contemporary or represented sequent rebuildings of the same structure (cf. Collins to Ford, February 8, 1930 and March 6, 1930).

Chambers mentions the “stratigraphic” excavation of Eskimo village sites, but both Collins’s publication regarding those excavations (1935) and Chamber’s correspondence with Otto Geist show that the only “natural levels” in which the trenches he excavated could have been worked were the depths to which the surface thawed between workings: “As you know, about three or four inches is the average thickness of each section, since atmospheric thawing extends only to that depth when the cuts are being worked every other day, in rotation” (Chambers to Geist, November 14, 1933). Hence in Alaska, while the layer-peeling technique might have been known, it was not necessarily used, and arbitrary levels seem to have been the rule. Certainly the stratigraphy as it showed up in the profile was recorded, and probably in a more detailed manner than Chambers had done before.

Another technique with which Chambers was thoroughly familiar was that of surface collecting from archaeological sites. He and Ford had made extensive surface collections, probably under the influence of Collins, who had early realized how much could be done with regard to cultural distributions using surface collections (Collins 1927, 1932b). It was in 1933, however, when Chambers was sending a collection of this kind to Carl Guthe at the University of Michigan, that he explained the technique that he and Ford had used in making these collections:

In making surface collections, when the material is plentiful, as at NX-12 [22-No-503, the Mrs. S.C. Monk Place], it has been our policy to pick up all rim sherds, as many decorated sherds as possible (or all that show decoration, if such are scarce), a representative amount of plain ware, and all sherds seen that show unusual features in decoration, finish, or tempering: naturally all stone artifacts are saved, and a few typical rejects. Where material is scarce, everything seen is saved (Chambers to Guthe, November 3, 1933).

Guthe did not hesitate to inform Chambers by return mail that while the method he had used would give a “fairly good sampling” of the pottery, it was completely invalid where statistical treatment of the resulting data was anticipated.
Instead, he recommended following the method of controlled surface collection then being developed by Kidder in the Southwest:

A given area, say ten feet square, was marked off upon the surface of the ground, and then every sherd larger than a thumb nail was saved. Such a collection clearly would indicate the relative proportion of the different types of wares and the relation of rim to body sherds (Guthrie to Chambers, November 11, 1933).

As will be seen, Chambers may not have followed this model for the surface collections he made at Lyon's Bluff, but his awareness of Guthrie's comments must be taken into account when evaluating the intention behind these collections. The few sherds that were drawn in the notebooks were decorated.

Since Chambers's correspondence with Guthrie on this point dates from after he had spent the summer with the University of Chicago field school on excavations at the Crable site in Fulton County, Illinois under the direction of Thorne Deuel, we must conclude that in this field school there was not much mention of surface collection. The major influence to which Chambers was here exposed was the stratigraphic excavation technique ("section and peel," as Neitzel, trained in the same field school the next year, was to do for the rest of his professional life [Brown and Brain 1981]), a technique which had been brought to such a fine state of development by Cole and Deuel at Chicago (Cole and Deuel 1937; also cf. Webb and DeJarnette 1942:95-104). According to Chambers's account of his work at the Crable Mississippian site, he did not excavate primarily in the mounds, but in the village area (Chambers to Lindflors, July 25, 1933). This would mean that here he learned an elaboration of the field techniques of feature excavation that he had employed at Deasonville, together with the method of excavating by natural levels that was not used at Deasonville because of the shallowness of the deposit and the speed with which that excavation had to be carried out. A comparison between the example field notes published in Cole and Deuel's methodology chapter (Chapt. II, Figure 4) and Chambers's Notebook of 1935 shows that his recording techniques in the area excavated that year were nearly identical to those used at Crable, although the only features assigned numbers at Lyon's Bluff were trenches and burials.

A similar situation to that at Deasonville obtained at the Shiloh Mounds excavation that Chambers worked on in 1934 under the direction of Frank Roberts. Examination of Chambers's notes from this excavation shows that the work on the mounds was carried out by trenching in arbitrary levels, while the work on the thin single-component village deposit concentrated upon the recording of structures as at Deasonville; again, as had been the case from Chambers's first field work, all artifacts were carefully plotted both horizontally and vertically. According to Bruce Smith, referring to the original Shiloh field notes in the collections of the Smithsonian (personal communication 1982), the apparent speed of the original mound construction and the thinness of the village deposit (approximately six inches) justified both techniques, as did the fact that artifact evidence argues strongly for a single component on the site. The only new technique to be noted in this work was the use of a carefully laid out site grid which provided the reference points for measurements (cf. plates in Webb and DeJarnette 1942).

It is evident, then, that Chambers had been exposed to the best archaeological field technique then in use, in the company of other young men who were to transform the discipline of archaeology. When we come to judge how these techniques were used at Lyon's Bluff, however, we must take the practicalities of the situation into account. In this work he had no large crew, trained or otherwise, to work with. By the standards of the parties with which he had worked elsewhere, the outfitting of the MDAH party, constrained as it was by the tiniest of budgets, was primitive. Again because of monetary considerations, the 1934 season suffered particularly in lack of time. Not least, although in 1934 the site was relatively untouched, when Chambers and Gordon returned in 1935 they found that the whole field up to the base of the mound had been cultivated and put into cotton (compare Plates 1 and 2). Further, they had to limit their excavation to the area of damaged cotton they could afford to pay for—which meant that spoil had to be piled dangerously near the edge of the trench—and were diligently watched by the tenant to see that they did not exceed this area. Finally, the excavators were plagued with having to handle the public relations aspect of the project themselves, which meant that they had to entertain frequent, sometimes daily, influxes of visitors from nearby Starville and Columbus. It is not surprising, then, that whatever the youthful idealism with which the excavators may have approached the site initially, they had to adapt themselves to the situation they found as best they might.

The 1934 site map was not fully completed until 1935, nor does the site ever seem to have been gridded except in the area excavated during the latter season. The initial season's work consisted of a series of test pits in and beside
the mound and along the northern bluff-edge border of the site. In 1934 these pits were apparently dug in arbitrary levels until a feature was encountered, and if this feature was a burial, then it was carefully exposed and slightly pedestal for photographing. Profiles for the test pits in the mound were drawn, but this was not done for the bluff-edge pits, which were assumed to have been dug into an unstratified midden deposit. The portion of the trench west of the mound that was excavated in 1934 was not profiled in that year, and no measured horizontal plan was drawn later, since the trench had to be filled in at the end of the 1934 season; according to the Notebook, the 1934 trench was dug somewhat at random as burials were uncovered. In 1935, with excavation focusing on an extension of this single trench (a small test pit was also put into a “house mound” near the excavators’ camp), records were less haphazard. Detailed plans and profiles were drawn of the 1935 workings in the trench, with part of the 1934 workings indicated. In both years burials were drawn and their relationships with one another carefully indicated. The mapping of the site was essentially a rough plan-view, carried out with tape, compass, and the kind of improvised leveling device remarked on by Neitzel at the Fatherland site (Journal 8/9/1934). There is no record of anything but relative levels defined by a few datum points. The mapping of the site itself simply used several prominent trees as reference points, but with reference to these and using a compass, baselines for the excavation were laid out in 1935 (Notebook 6/25/1935).

The rationale for this overview of technique will become clear as the reconstruction of the excavation proceeds. Several of the problems encountered with this reconstruction stem directly from the choice of techniques and the inconsistent application of those techniques. In closing this section something also needs to be said of the method to be followed in the reconstruction. First it should be stressed that one aim of this report is to present the bulk of the data just as it was recorded by Chambers, and to this end a selection of the site photographs and all of the site drawings are reproduced essentially as they were found in the archives, with the intention of providing a baseline of reference for the reconstruction itself. Most of the textual matter, contained in the Journal and Notebook, will be included either as verbatim citation or as paraphrase; some of it, however, especially that which represents verbal description and tabulation of profile measurements that is also represented graphically on the profile drawings, will not be included except where it is found to be at variance with the profile drawings or photographs.
Since I am using several kinds of evidence—written description, photographs, drawings, and later recorded oral remembrance—some specific declarations should be made to define the relative value to be accorded to each of these kinds of evidence when two or more sources are in conflict. Obviously the literal visual record is to be preferred, when photographs are of a quality and content to address any such problem. Drawings should agree with photographs where both reproduce the same data, and often add more information than the contrast values of the photographs can portray. Where written description disagrees with both of these, the pictorial evidence is to be preferred; where it disagrees with the drawing but not with the photograph, derivative drawings were made to rectify an assumed error. In general the oral interview materials have last priority when they cover the same ground as either written or pictorial evidence, since an intervening forty-odd years is likely to have dimmed precise memory, but for some matters connected with particularly striking observations about the excavation and especially for matters that are not covered at all elsewhere, the oral testimony was given greater weight than this.

Account of the Excavations

The 1934 Season at Lyon's Bluff

In June of 1934 Moreau Chambers was sent by Dunbar Rowland to excavate the Brogan Mounds site near West Point, Mississippi, at the request of the local DAR chapter. The mound was excavated in stepped trenches and at least seven burials were found in it; then it was restored for public viewing by FERA laborers secured for the work by the West Point DAR (Chambers to Rowland, 6/30/1934 and 7/16/1934). Chambers then suggested that since there was no further work to be done on that site, an economy could be realized if the party, which consisted of Chambers, Slater Gordon, and Everett Richardson, could continue excavations at another nearby site. The site selected by Chambers was Lyon's Bluff, shown to him by Mr. John H. Wellborn on July 12. Chambers' initial impressions of the site were recorded in a letter to Rowland explaining why it had been chosen for excavation:

It lies in a very inaccessible place on Line Creek, about two miles, airline, from Muldown, a station on the I.C.R.R....Upon the surface we found potsherds tempered with bits of shell, and bearing crude designs of a sort unlike the material that we had been encountering near West Point and

along the Tombigbee. There is a fairly large mound a few yards from the bluff, and the appearance of the spot agrees with the description of the Chocchumia village, in which the remnant of the tribe was besieged and exterminated by the Choctaws and Chickasaws. Mr. Wellborn said that an old settler here had told him years ago that this was the site of their village, and certainly the pottery is quite different from that of surrounding sites; in fact, it more nearly resembles what we found in the drainage of the Yazoo River in Holmes Co. or in Yazoo Co. I am quite ready to believe that it represents a tribe alien to the Choctaw and Chickasaw (Chambers to Rowland, 7/16/1934).

He recorded further detail in the Journal:

The creek bluff is at its highest here, rising nearly fifteen feet above the ordinary water level...The limestone outcropping comes very near to the surface on the Okibiche side of Line Creek, and in places unwise cultivation of the land many years ago, completely uncovered great sheets of the shaly rock. A bend in the creek was selected by the Indians for the site of their village. Potsherds lie scattered over the ground several hundred yards along the creek bluff, both east and west of the point near the center of the village site where the Starville-West Point high-powered electric line crosses the creek. A mound from which a gnarled cedar tree rises is near the center of this bend in the creek. At the nearest part of the bank, a midden deposit has raised the soil level nearly two feet. This spot safely above high water mark, was evidently the place chosen for some of the more important houses. Either because of the difficulty of placing this strip of land under cultivation or because of its innate desirability as a building site, our findings at this part of the site revealed more signs of wattle and daub houses and midden refuse than elsewhere on the village site (Journal, 7/12/1934).

The obvious richness of the area along the northern bluff edge (see Figure 1) accounts for Chambers's strategy in his initial approach to the excavation of the site. As is the case even today, human bone could be seen eroding out of this bluff edge on the first visit to the site, and this amounted to the most obviously and easily retrievable material. It offered an inviting beginning point for the excavation (Gordon 1983). The apparent first step was to strip this bluff edge at the upper surface and then to dig down through what seems to have been perceived as an unstratified midden deposit, probably by arbitrary levels. During the early clearing stages along the bluff top, but not specifically given a location in the notes, "sherds decorated with
thumb-nail imprints and having notched rims* were found (Journal, 7/14/1934). The pits numbered 1-4 on the plan-view in Figure 1 included two burial pits recognized as such, dug into the midden:

Pit #1: Burial located about 15 inches deep in the midden. Flexed burial of an infant laid on its left side or back, knees to chest, forearm across breast; skull was removed during excavation and many parts saved (Journal, 7/16/1934 and 7/19/1934. Burnt daub found below burial.)

Pit #2: Test pit back from bluff edge. Apparently nothing particularly remarkable found; sole observation is that burnt daub was found in this pit, as it was in Pits 1 and 4 (Journal, 7/27/1934. Pottery scatter probably similar to that in Pit 4).

Pit #3: Burial washing out of edge of bluff slope; only upper portion of skeleton present (Journal, 7/16/1934).

Pit #4: Most easterly test pit at bluff edge. There is more description of the findings in this pit than of those in the others, which permits the reconstruction of a rough stratigraphy as the sequence of days’ diggings is followed (Table 1).

There is no indication in any of the notes that profiles were ever drawn of these test pits, nor do any such drawings survive.

The plan-view of the site shows two unnumbered rectangular test pits which were put into the central portion of the mound during the same
Figure 2. Profiles through E-W trench in mound (Chambers Drawing).

Figure 3. Profiles through N-S trench in mound (Chambers Drawing).
time, both of them actually short trenches. Doubtless Chambers would have liked to place one of these pits across the center of the mound, but the presence of a cedar tree growing out of the mound’s summit prevented this and probably accounts for the distance from one another of the two tests. Profile drawings of these two pits were made (Figures 2 and 3), but the text in the notebooks reveals additional detail. The initial layer encountered as the trenches were begun consisted of “masses of bones” (Journal, 7/17/1934); it is likely that these bones were identified as food refuse (possibly even of recent origin), or they would have been excavated more carefully. Potsherds were found mixed with the mound fill as deep as eleven feet (Journal, 7/25/1934), and one of the trenches was continued to a depth of 13 feet, doubtless in an effort to reach undisturbed soil. The dates on the drawings indicate that the profile of the E-W trench was not drawn by Chambers (although it is labeled in his handwriting), since Chambers spent July 21 in West Point. Hence the profile of this trench was probably measured and drawn by Gordon or Richardson and later copied by Chambers (Figure 2).

This observation makes the explanation of the inconsistencies between the two drawings easier. Figure 4 shows the profiles of both pits redrawn to a reduced scale and shown with their E-W faces in correct relation to one another, separated by the 6 feet occupied by the cedar trees’ root system. From this composite drawing it is easy to see that although the two trench profiles belonging to each trench are internally consistent, there are definite problems in harmonizing the stratigraphy across the gap between the two trenches. Quite clearly, the profile that we know was drawn by Chambers (Figure 3), probably reflecting the greater experience and sophistication of observation that he had, demonstrates a great deal more complexity than the other. It is quite likely that the single coarse sand layer in the E-W trench, recorded as being delimited by “hard burnt floors” and divided horizontally by a thin layer of clay, can be related across the hiatus to the two layers of sand separated by lenses of black gumbo and sandy clay on the other side, and that the observation of a continuous layer of black waxy clay from near the top to the bottom of the E-W trench may be related more exactly to the black gumbo observed in the upper third of the N-S trench and the nearly continuous reddish brown gumbo observed in almost the whole of its lower two-thirds. The sketch included on the drawing of the N-S trench profiles to show the location of the two trenches indicates that the
E-W trench could easily have picked up evidence of structures standing on the western side of the mound top (hence facing east) that would not show up in the N-S trench, just as the N-S trench seems to have picked up a similar burnt layer at a deeper level that would indicate a more centrally placed structure. The two burnt layers found near the bottom of the N-S trench are perhaps even more interesting, since they, taken with the other three structure evidences already cited, make a total of five structure floors found on five different levels in the mound, suggesting five building phases.

That Chambers's placement of the two trenches in the mound was a wise one is demonstrated by the disturbances already known to be present and their evidence as it shows up in the profiles. Chambers had been informed that the mound had already been potholed, and a stone axe had been dug from it (Journal, 7/12/1934; Chambers to Rowland, 7/24/1934). This digging is doubtless represented by the disturbed area indicated on the profile drawings for the N-S trench; probably this was one reason that Chambers so sited the trench. It also seems that at least part of the E-W trench at its eastern end shows disturbance by roots from the cedar tree, which would additionally account for the stratigraphic hiatus already discussed.

On August 7 Chambers recorded the profiles of the N-S trench, and on the 9th the sketch map of the site was completed and the mound trenches refilled. On this date the first mention of excavation in Pit 5 to the west of the mound is made, but it is not clear whether this trench had been started previously. Why it was located where it was has been explained by Gordon: the disparity between the height of the mound (about 10 feet) and the depth of the trench where sterile soil was reached (13 feet) led the excavators to the conclusion that the mound's base extended farther than originally supposed (Gordon 1983). Again in the account of Pit 5 there is mention of digging in midden refuse, and Gordon's explanation is confirmed by the fact that Chambers terms the location the "western approach to the mound" (Journal, 8/9/1934). For the remaining three days of the excavation digging continued in this pit alone. Five burials were exposed, excavated, photographed, drawn, and removed in the space of those three days (Figures 5 and 6), all of them in an excellent state of preservation. Chambers's notes remark on the "great quantities of broken animal bones and sherds" found in the midden in which the burials were placed (Journal, 8/11/1934), but no such materials survive and no profile of this trench was drawn; indeed, Chambers himself felt that

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Figure 5. Burials 1-5, Pit 5 (Chambers Drawing).
it was inaccurate to describe it as a trench, for as he remarks, the pit was “enlarged irregularly as skeleton after skeleton was discovered” (Notebook, 6/19/1935). Since there is so little evidence apart from the horizontal distribution of the burials to go on, I will include further discussion of these materials under the full description of Pit 5.

A final apparently last-minute trench, Pit 6, was placed across the eastern tail of the mound, probably to define its edge, but very little evidence of it remains. It is shown on the site plan (Figure 1), and a tiny cross-sectional sketch dated 8/11/1934 was drawn on the back of the site plan (Figure 7), together with notes that indicate that it was 12 feet long and 30” deep. The trench showed a sloping white sand layer over charcoal-bearing black gumbo clay, while the notes say that sherds (of unspecified type) were found in the sand layer.

Pit 5: 1934 and 1935

The excavations in Pit 5 in 1934 were summarized succinctly by Chambers at the beginning of the 1935 season:

Near the end of the 1934 season, a pit, #5, was started at the outer edge of the mound nearest the power line to the WNW. In this pit, enlarged irregularly as skeleton after skeleton was discovered, a midden deposit rich in sherds and animal bones was found to extend from the surface to an approximate depth of 2’6”, depending upon the side measured, as the E side encroached on the mound slope. Five burials, excellently preserved, were found at an approx. depth of two feet in this debris, each buried on its right side, the knees flexed, four of them with heads pointing to the south, the fifth, a female, buried with head to the East. With the exception of a shell spoon behind the head of B#5 and a fossil horse tooth touching the feet of B#3 and an abrading stone near its tibia, there were no accompanying finds. The patella of a sixth burial were discovered, but not removed, in the bank south of Burials 4 & 5 (Notebook, 6/19/35).

Using this information together with the horizontal plan of burial locations that can be reconstructed from the drawings of the skeletons, it is possible to create something of the plan of the 1934 Pit 5 and to locate the burials in a hypothesized profile drawn along a line that transects the area of the five excavated burials (Figures 8 and 9; compare Plate 3).

Tracings of Chambers’ original drawings of Burials 1-5 are shown in Figures 5 and 6. Because the relative locations of these burials were measured rather carefully, even in the absence of a grid location they can be laid out with approximate accuracy using an extension of the N-S baseline laid out in 1935 to yield the plan shown in Figure 8. Examining the profiles of the 1935 Pit 5 and its western extension, and noting where the evidence of the 1934 pit was seen to impinge upon these profiles, an approximate southern boundary for the 1934 pit can be established. Referring to the photographic record (especially Plate 3), confirmation of the horizontal extent of the 1934 pit in the south can be made, and it is also possible to trace a rough boundary for the east and north sides of this pit in the same way. Several portions of the boundary, especially on the eastern side and in the area between N20 and N17, cannot be defined with any confidence at all.
Figure 8. Composite plan: Burials 1-10 in 1934 Pit 5, 1935 Pit 5, 1935 Pit 5 extension.

Plate 3. Lyon's Bluff. Pit #5, Burials #1 to 4, facing NW. Slater Rhodes Gordon. Steel tape by Burial #1 is extended to 3 feet. August 11, 1934. [MDAH 1331-438]
The western extension of the 1934 Pit 5, which is substantiated by the profile of the north face of the Pit 5 western extension made in 1935 (see Figure 21), was apparently dug out in search of further burials to the west but abandoned when Burial 5 was found to the east—at any rate, we know nothing further about it but that it had been dug to a depth of 2'6" along the N-S baseline and was only about 1' deep where it intersected the north profile of the Pit 5 extension, though this latter fact would seem to confirm the suggested experimental nature of this spur dug west from the existing pit.

Looking at the distribution of burials in the 1934 Pit 5 as reconstructed in Figure 8, we can use the depths that Chambers recorded for the burials to draw a very crude hypothesized profile through E 27° from N19 to N32 (Figure 9). This does no more than to display graphically Chambers's assertion of the estimated uniform depth of the midden deposit in the extract quoted above. There was, however, no apparent surface of any kind on which the burials were placed, nor any clear evidence of burial pits.

At this point it makes sense to treat the rest of the burials recorded from Pit 5 in 1935 (excluding Burial 11, which will be accorded separate treatment) by quoting Chambers's descriptions of the burials, which he wrote in the margins of the drawings, accompanied by the remainder of the skeleton drawings (Figures 10-12). It will be noticed that Chambers attempted to sex most of the burials, probably with the advice of several local doctors who were interested in the excavation, and some attempt was made to judge the age of the individuals also, chiefly depending upon the state of the epiphyses of the skeleton and on the development and wear of the teeth.

Descriptions of burials from Lyon's Bluff transcribed verbatim from drawings:

Burial #1, Pit #5 [Figure 5]
8-11-34
Male, head to S., flexed on right side, heels to hips, left humerus parallel with vertebra, left wrist beneath left knee; right humerus broken, right radius & ulna nearly parallel w. left femur, hand beneath leg bones. Left foot is above right foot. Skull cracked. Epiphyses [sic] indicate immaturity. Two feet deep in midden refuse.

Burials #2 & 3 are together. [Figure 5]
#2 is probably an infant, lying beneath #3. B#3 had separate epiphyses [sic] on long bones, & was buried in much the same position as the other burials. Position of B#2 could not be determined.

Burial #4, Pit #5 [Figure 5]
Lyon's Bluff v.s.
Ok-1 8-10-34
Adult male, head to the south, flexed on right side, facing East, heels near hips, left humerus parallel with vertebra, left wrist beneath knees, right arm tightly flexed at side, hand lying beneath face. Skull is but slightly higher than rest of skeleton; cranium badly crushed in; facial portion in good condition. Bones are in excellent condition. Several curious teeth; some missing when unearthed. Found at 1 1/2 to 2 ft. deep in refuse heap at foot of Lyon's Bluff Mound, West side. Sketched & photographed.

Burial #5, Pit #5 [Figure 6]
8-12-34
Lyon's Bluff
Oktibbeha Co., Miss.
Female, flexed on right side, head to East, left arm extended beneath legs, right arm tightly flexed, hand at chin. Left femur at rt. angle to vertebra, both feet together near pelvis. //Infant skull fragments occurred immediately above bones of #5 at position indicated.// Shell spoon formed from worked mussel shell lay at back of skull of #5. The skull was badly crushed. All bones in fair condition. Epiphyses [sic] on bones of #5 indicate that this was not a fully matured individual. Depth, 2 1/2 ft. Near B.#4.

June 22, 1935
Burial #6: [Figure 10]
Adolescent; sex, probably male; knees flexed, heels to hips; head to south; left arm slightly bent, left hand at right hip; left shoulder displaced to right side; frontal portion of skull crushed, and decayed, head faces east; skull vertical; right forearm...
vertical; right hand fallen above ribs near left humerus. Burial in fairly good condition. (The patellae of this burial were encountered at close of 1934 season in the bank S of Burial #5.) The epiphyses [sic] on many of the bones, such as the vertebrae, pelvis, long bones, &c., were detached—an indication of immaturity. Depth, approximately 20".

Skull position: N15°9’; E3’8”.
Lyon’s Bluff v.s., Pit #5
Burial #7; [Figure 11]
Male (?), flexed face down, feet above and to the left of pelvis; right arm slightly flexed, right hand beneath right pelvic bone; left humerus extends parallel to vertebral column, left forearm passes beneath breast. Skull, face downward, is toward the South. Skull is on slightly higher level than knees. Epiphyses [sic] not entirely knit. Position of top of skull: - E3’6”; N10’6”; depth 19’.

Ok-1 Lyon’s Bluff, Pit #5
Burial #8; [Figure 11] Female (?), skull to SE, flexed on left side, heels to hips, left knee to breast; right forearm crosses vertebra, resting against left femur. Left arm flexed, hand against shoulder. Epiphyses [sic] unattached. Skull rests in vertical position, facing West. Skull is whole, and shows distinct posterior flattening. Upper teeth missing in front and to right side. Fingers of right hand slightly clenched. Bones are in fairly good condition. Top of skull lay beneath approximately 9” of soil. Photographed, sketched, and excavated, 6-27-35. Position of back of skull: - N6’7”; E3’0”. Position of left toes: - N8’0”; E0’6”. Length overall, 35”.

Figure 10. Burial 6, Pit 5 (Chambers Drawing).

Figure 11. Burials 7 and 8, Pit 5 (Chambers Drawing; composite of two drawings).

Lyon’s Bluff, Ok-1
July 13, 1935
Burial #9;—[Figure 12]
Immature, sex not determined. Head to East, flexed on left side, right shoulder out of position and immediately below chin, which—with skull—faces South. Right femur extended perpendicular to vertebra; right foot lies above left ankle. Heels 8” or 9” from pelvis. Skull in poor condition, although facial portion is well preserved. Right scapula over ribs below mandible. Right humerus makes 45 1/2 angle from
From the close proximity and similar stratigraphic location of Burials 1-6 as described by Chambers, it would not be unreasonable to assume that these burials and possibly Burials 7, 9, and 10 might be taken as contemporaneous with one another. But to confirm any such statement it would be necessary to examine the record in more detail. The stratigraphy of the 1935 Pit 5 and extension fortunately was recorded in profile (see Figures 16, 17, and 21). The grid upon which these drawings were based was explained by Chambers in the Notebook:

Taking the N-S line along the W side of this season’s addition to Pit #5, & still designated as such, as the BASELINE, mag. North and South, a point in the cotton field 20 ft. S. of the NW corner of this rectangular pit was set up, and a line East and West was laid off by compass, passing 67 ft. E through a point on the summit of the mound 6 feet due North of this line, intersecting the N-S Baseline at the point in the cotton, forms the East-West BASE LINE (Notebook, 6/25/1935).

This description was accompanied by a sketch shown as Figure 13. Although we do have the profile drawings for this trench, the horizontal plans of Pit 5 and its extension present a confusing composite of levels (Figures 14 and 15), showing different areas at different levels and not showing the locations of features at other levels. This makes it very difficult indeed to

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Figure 12. Burials 9 and 10, Pit 5 (Chambers Drawing).


Burial #10:—[Figure 12]

Aged male, head to East, flexed on back, heels to hips, knees to right side. Skull tilts upward to west, in good condition, sutures almost entirely closed. Left arm extended along left side, right arm extended, slightly flexed, hand above pelvis. Skull excellent; other bones in good condition. Skull (at top back) N16°3′ W6°3′ depth 27′ below Base Line. Photographed and sketched 7-13-35.

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Figure 13. Location of baselines, 1935 (Chambers Notebook).
Figure 14. Plan of Pit 5, 1935 (Chambers Drawing).

Figure 15. Plan of part of Pit 5 and Pit 5 extension, 1935 (Chambers Drawing).
connect the undifferentiated “midden deposit” of the burials with stratigraphic evidence that may have been recorded many feet away.

Excavation activities in 1935, carried out by Chambers and Gordon with occasional and fleeting unpaid assistance from local amateurs, were preceded by a general surface collection made in the newly-plowed cotton field, which the excavators found to be “covered with sherds and animal bones, a clear indication of the importance of this settlement in Indian times” (Journal, 6/14/1935). According to Slater Gordon, surface collections in 1935 yielded a relatively high proportion of sherds resembling what we now classify as Moundville Black-filmed Engraved (about 25% as opposed to the fractional percentage now to be found on the site: Gordon 1984 and Marshall, personal communication). At any rate, the location of the filled-in 1934 Pit 5 was covered with standing cotton, and permission had to be secured from the landowner to remove some of the cotton in order to extend the pit. The area they planned to excavate was “an area probably 18 ft. x 24 ft. approximately 50 square yards, on the edge of the field next to the uncultivated mound” (Journal, 6/17/1935), and there was apparently some wrangling over the amount of reimbursement for the damage to the standing cotton, but eventually permission was secured by the new Oktibbeha County Historical Commission to excavate an area of a quarter acre, and the Commission financed the reimbursement.

Excavation began on June 19, when work was resumed at the southeast end of the 1934 Pit 5, extending south over the area where the excavators expected to find the remainder of Burial 6, whose knees had been exposed in the earlier excavation. This part of the excavation seems to have been carried out by peeling off layers of deposit until the skeleton was reached at a depth of approximately 20” from the surface. Chambers describes the procedure that was followed in the Journal (6/21/1935):

By digging in from one corner of our square pit along two side walls in narrow trenches much deeper than the position of the skeleton which we shall uncover near the center of the excavation, then by carefully troweling off the dirt above the probable location of the skeleton in thin layers of approximately three inches in thickness until other evidence points to the nearness of the bones; when shaving with a trowel becomes necessary, we gradually remove the dirt from around the edges of the place where our work proves the skeletons to lie, and it gradually appears as we follow along the bones, cleaning off the dirt above them.

Digging continued below the level of the skeleton, since the occurrence of charred corn-cobs in a hearth area near a depth of two feet was noted. In the process of digging, a small piece of red ocher and fragments of an elbow clay pipe (red-slipped and found about a foot from Burial 6 [Notebook, after 6/19/1935]), as well as “various decorated sherds,” were also found (Journal, 6/19/1935), and when the burial was being removed, “a nicely worked antler tip” was found under the left fibula (Journal, 6/11/1935). The Notebook (after 6/19/1935) notes the location of the hearth feature: 40° S from NW corner, 54° E, 20” NNE of Burial 6 skull. It was therefore found under Burial 6.

On June 20 the work was interrupted while the excavators made an additional intensive surface collection in the vicinity of the mound as the tenant cultivated the cotton in that area:

It was found expedient to go ahead of him up and down the rows collecting the best of the surface material that would soon be covered by plow. Our systematic search assembled an excellent assortment of decorated sherds, a stone bead, projectile points, a segment of a stone palette, hammerstones, etc., and getting these objects we were merely selecting the best from the quantities of sherds, bones, wall plaster and stones strewn over the ridge at the bend of Line Creek (Journal, 6/20/1935).

That Chambers adhered to his former surface collection practice with Ford and did not follow Guthri’s advice in surface sampling was doubtless due to several factors: with the cultivating going on, he and Gordon had been unable to lay out a grid for a controlled sampling and were undoubtly rushed for time, and Chambers did not know at the outset of the excavation that the work would not be continued on a more lavish scale at a later point in the season; he was hoping that laborers on relief employment could be secured for the work (Journal, 7/8/1935). Completing their surface collection in the area to be cultivated, Chambers and Gordon returned to stripping the soil of the plowzone from the surface of the Pit 5 area they had staked out for excavation, finding a great deal of midden material in the process.

For the period from June 19 to August 2 the entries in the Journal were made on a daily basis, so to a good extent it is possible to follow the excavation of the northern end of the 1935 Pit 5 in fair detail. According to the Journal, a test trench was apparently dug diagonally across the northwest corner of the new Pit 5 area, doubtless deepening that part of the area that had been dug out
in 1934 and including a good part of the area under Burial 6. The purpose of this trench was to create a profile face to guide further stratigraphic digging, and the effort was being made to reach undisturbed subsoil. The two sources differ slightly on the description of the layers encountered, but in general they agree about the sequence and consistency of the soils:

<table>
<thead>
<tr>
<th></th>
<th></th>
</tr>
</thead>
<tbody>
<tr>
<td>midden deposit</td>
<td>refuse layer</td>
</tr>
<tr>
<td>about 30&quot; from surface, barren, limey-clay subsoil a foot thick</td>
<td>yellowish clay yielding charcoal and burnt bones</td>
</tr>
</tbody>
</table>
| black, ashy midden deposit, deposit cut by yellow clay feature 6" below beginning of black midden, containing straight row of post holes 2'-3" in diameter and in a line 10" north of EW | dark midden earth at 3'-4", yellow clay feature containing row of post holes "at nearly four feet below the surface"

Comparing this verbal information with the information represented by the profile drawings (Figures 16 and 17) is not straightforward; the area to be concerned with here is that of the northwest corner of the 1935 Pit 5, and on both the north and west face profiles of the trench the 1934 Pit 5 is clearly delineated. Beside that disturbed area on the north and west faces of that trench, we have the following sequences:

<table>
<thead>
<tr>
<th>West Face (Figure 17)</th>
<th>North Face (Figure 16)</th>
</tr>
</thead>
<tbody>
<tr>
<td>Lighter colored midden earth</td>
<td>Upper midden</td>
</tr>
<tr>
<td>? (several disturbances on north end)</td>
<td>Black midden</td>
</tr>
<tr>
<td>Yellow clay mixed with black midden</td>
<td>Black midden</td>
</tr>
<tr>
<td></td>
<td>Undisturbed yellow clay [NE corner]</td>
</tr>
</tbody>
</table>

There is no problem with the "midden," "refuse," or "upper midden" layer mentioned or portrayed in all cases, but it appears that in the case of the next layer down, the mixed clay/midden layer portrayed on the west face profile
served as the source of the identification in the Journal, while the black midden layer inferred on the north face, well to the east of the west face at East 6', may only indicate that the character of the midden had changed across the gap of the 1934 pit as the edge of the mound was reached. The "barren, limey-clay subsoil" mentioned in the Notebook thus is likely to be the same as the yellow clay containing some midden materials described in the Journal and shown on the west face profile, as otherwise it remains unexplained.

It was on June 24 that the wall trench containing a row of postholes was first encountered in the northwestern corner of Pit 5 as a distinctive strip of yellow clay making a sharp discontinuity with the lower black midden layer. At first this yellow clay was apparently thought to represent undisturbed subsoil (Notebook, after 6/19/1935), but Chambers soon determined on the basis of the regularly-spaced postholes that he was dealing with some sort of wall construction (Journal, 6/24/1935) that should be investigated more thoroughly. This yellow clay was found at a depth of 4' beneath the surface and 6" beneath the beginning of the black midden layer (see Plate 4). It is therefore doubtful that

Plate 4. Profile of north end of west wall of Pit #5. Note plumb-bob at N15' and the outline of 1934 Pit #5, also the two floors of the house. The point of the trowel indicates the fire pit dipping sharply under the lower lime-clay floor, and the bent and is stuck up against the pit profile where the house wall passes through the west wall of the pit. July, 1935 [sic, the yellow wall trench is not yet excavated here, so the date must be closer to June 24, when it was first found]. [MDAH 1331-456]
any evidence of the post molds at a higher level
within the black midden soil would have been ob-
served had they existed (they were described as
“dark circular spots of black ashy earth” [Journal,
6/24/1935]), although it is clear that the burned
hearth area found beneath Burial 6, if the location
given in the Notebook is accurate, overfled this wall
construction and was not associated with it (see Fig-
ure 17). Further investigation of the wall construc-
tion on June 25 revealed that the wall trench repre-
sented by the yellow clay was bordered on both
north and south sides by the black midden material
and that it extended only about 4 1/2 feet eastward into
Pit 5 (see Plate 5). Chambers speculated:

The finding of this narrow wall of yellow
clay studded with post holes at regular
intervals suggests two
possibilities connected with its construction: either a narrow trench was
dug down from a higher level into the dark midden earth to afford a firm
support for the vertical posts that were emplaced in it, or the wall

antedates the midden refuse, which has accumulated since the construc-
tion of the feature, which may have been one of the first structures in this
part of the village (Journal, 6/25/1935).

An attempt was made to explore further north to find some trace of another
wall meeting the one already found, on the reasoning that its abrupt end
might only be a gap or entrance, but none was found, although the digging
in that area of the pit was hindered by the accumulation of the spoil pile at
the east side of the pit.

As an extension of the pit was made toward the south in search of an-
other length of wall, another burial, number 7, was encountered at the same
20 feet depth as Burial 6. This burial was excavated, drawn, and sketched, and
the same procedure was carried out with Burial 8, whose foot had been
discovered on June 25 during the same search, but which was buried at a
depth of only one foot. Both were removed by the end of the 27th, and the
only remarkable evidence noted in connection with either was that Burial 7
seemed to have been buried in some sort of fire pit, since it was surrounded
by ashes and lumps of charcoal. Also at a one-foot distance from Burial 7 a
fragment of the bowl of a red-slipped pottery pipe was found.

Both Chambers and Gordon were suffering from the heat working several
feet deep in Pit 5, and for a few days work in that pit was abandoned in favor
of a test pit in which Chambers had gradually recognized as a house mound “about
eighteen steps NNW of our tent door” (Journal, 6/28/1935). In this pit, whose
location can be vaguely guessed at on the basis of the location of the 1935 camp
in Figure 1, large undecorated potsherds, charcoal, daub, and animal bone were
found during the first week, while later, half of a miniature pot, sherds with red
and white painted decoration, and a human effigy from a pot were found. No
further information about these artifacts is given. At a depth of seven inches
Chambers noted a layer of yellow sand, which he reckoned to be the house
floor, but although after this test Chambers realized that the surrounding area
was studded with similar small mounds, no further testing was done (Journal,
6/28/1935; 7/11/1935). One day was, however, spent in making additional sur-
face collections in the vicinity of the farm road, and the finds cited from this
effort included:

a nice stone discoidal, some sherd discoidals, a medallion-like human face
from the side of a pot, some zoomorphic effigy heads from rims of bowls,
and several nicely decorated sherds, one bearing an interlocking scroll design (Journal, 6/29/1935).

Excavation then continued in Pit 5 as the whole area of the proposed investigation was dug into. As the work progressed, no further burials were found, but various finds and features were noted in some detail. By this time the Notebook reflects Chambers's conviction that the black midden layer portrayed as the lowest midden layer, the one into which the yellow clay-filled wall trench had been dug, was beneath the base of the mound itself (e.g. Notebook, 7/2/1935), which he had decided was "defined" by a sandy stratum visible in the section drawing of the pit's east face (see Figure 16 and Plate 6) and still visible as far west as E6'. In the black midden layer, as we have seen, many interesting features and artifacts were found, most reflecting domestic activities and evidence of cultivated and gathered food resources (see Figure 18). At a depth of four inches in this midden layer was a bed or heap of mussel shells in a pile about 10" in diameter, located at N12'6", E6'. In this same midden deposit, but 2" deeper, a fine projectile point, made of dark gray stone and roughly triangular in shape, was found also, at N12'6", E7'6". Another interesting find, particularly since it is one of only two ceramic sherds drawn by Chambers, was a rim sherd:

Plate 6. Profile of south end of East wall of Pit #5, showing sand strata at base of mound—on level with "step" in floor of the pit. July 6, 1935. [MDAH 1331-457]

Figure 18. Horizontal plot of artifacts and features, Pit 5, 1935.
Small rim sherd of bowl, notched projecting rim, scroll design accompanied by hachured area...Color light tan to dark brown, shell tempered hard, reddish-brown paste, approx. 4 mm. thick below thickened rim; smooth outer surface, rougher inner surface (Notebook, 7/2/1935; see Figure 19A).

This sherd was found at a depth of two feet below the surface, thus in the midden deposit, and came from the area below and slightly south of Burial 7. Barely a foot away to the west and at the same depth, a small red triangular projectile point was found, further to the southeast the charred fragments of a tree stump were uncovered, beginning at the top of the black midden which at this point dipped to 2'6".

Digging once more in the north end of the trench, Chambers located a bed of large, undamaged mussel shells six inches below the top of the midden deposit and 4'6" deep (Notebook, 7/3/1935; see Figure 20).

Nearby, at a depth of 4'2" and in an ashly deposit below a yellow clay lens and above the black midden deposit, a mass of charred corn cobs appeared heaped together. At a similar depth but located south of the yellow clay wall trench was another heap of charred matter, possibly "some kind of grain," according to Chambers (Notebook, 7/5/1935).

With Chambers working in the north end, Gordon continued deepening the trench's south end. On the 6th of July he was rewarded with the finding of another decorated potsherd six inches below the sand stratum in that part of the trench, 2'6" deep in the midden:

shed with handle bearing herring-bone design of parallel trailed lines, combined w. punctations shell tempered, dark gray color, thickness 4-5 mm. (Notebook, 7/6/1935; see Figure 19B)

Near the same area and at a depth of about 3', two burned features, one red and the other red with a white ash center, appeared. To the east side of the south end of the trench, at a 50" depth, a mica sheet was uncovered (dimensions not given—Notebook, 7/6/1935).

At the same time Chambers was at work on the yellow clay wall trench, finding another posthole and deepening the trench enough to determine that the wall trench ran to nearly a foot deep (Journal, 7/6/1935). Another possible posthole was discovered some two feet east of the end of the wall trench and buried down in the midden. Chambers sketched the horizontal plan of the wall and its postholes in the Notebook (Figure 20). The two excavators also drew the east and north trench wall profiles (Figure 16 and Plate 7; all measurements for the drawings are given in the Notebook, dated 7/6/1935).

During Sunday and Monday Chambers was apparently occupied with public relations activities, but Gordon seems to have taken up work again on the Monday (7/8/1935), for on the following day Chambers, apparently after having found another posthole near the wall trench (Notebook, 7/11/1935), turned his attentions to Gordon's findings. The feature that Gordon had discovered ran near the western wall of Pit 5 between 11° and 13° N and consisted of a 1' thick floor, at 3'6" deep, of whitish packed rotten limestone clay, surfaced with yellow sand, covered on that surface by an accumulation of 2" of black gumbo clay that was apparently "in all respects
Figure 20. Pit 5, plot of selected features, 1935 (Chambers Notebook).

Plate 8. South end of profile of west wall of Pit #5, showing white clay house floor, partly covered with sanded gumba layer, and some post holes showing in the floor. String marks base level. Burlap shelter sags down from overhang. Ca. July 10, 1935. [MDAH 1331-460]

Plate 9. Detail of west wall of Pit #5, showing hard lime-clay floor, thin black surfacing over it, well sanded, and the posthole passing through this floor from a higher level. String indicates surface of the ground. Probably July 17, 1935. [MDAH 1331-462; note shadow of burlap awning]
identical with the raw gumbo extending beneath the house floor” (Journal, 7/9/1935). This presumed floor was pierced by postholes, but no artifacts appeared on it, so Chambers speculated that it had likely been abandoned intentionally. Further postholes were found on the following day, but no clear alignment appeared (Notebook, n.d. but following 7/10/1935). Chambers then drew the west wall profile (Figure 17, measurements in Notebook, 7/10/1935; details in Plates 8 and 9), which does not show the new “floor,” and the only complete horizontal plan of Pit 5, which does take the new house floor into account (Figure 14) apparently does not record all the postholes. For some reason no measurements of layers were taken and no profile of the south wall of the trench was drawn, though there is a rather dark photograph of Chambers showing a nicely cleaned section (Plate 10).

The following day a westward extension to Pit 5 was started in an attempt to follow the yellow clay-filled wall trench westward to further features and to clarify the relationship between the wall trench and the mound edge. Beginning at the surface, the excavators removed midden earth “extremely prolific in sherds, animal bones, charred corn-cobs, etc.” (Journal, 7/11/1935). At a depth of 6” (or 1’ Notebook, n.d., after 7/10/1935) a reddish burned area with several postholes was discovered (but not shown on any plan), with further midden soil below it. Caches of charred corn cobs were found in the midden soil. “Near the surface” a fine perforated greenstone object was found (no better located or described; Journal 7/12/1935), and two additional burials, 9 and 10, were encountered (see Figure 12 and Notebook for 7/13/1935). One of the corn cob caches was 1’6” from the skull of Burial 9 and 2’” deep, like both burials.

On July 13, in spite of the attentions of five carloads of visitors, the excavators uncovered, pedestaled, photographed, sketched, and removed Burials 9 and 10. When work resumed on July 15, an attempt to take the northeast corner of the original Pit 5 down to an undisturbed level to trace the end of a posthole resulted in the discovery of rotten limestone bedrock at 5’9”, 6’9” below the midden. A small area was dug deeper by a foot, upon which ground water was reached (Notebook, 7/15/1935; see Figures 14 and 16, to which later drawing this part was apparently added after the original draft) and the hole was refilled.

At the same time it was apparently Gordon who proceeded with excavation of the Pit 5 extension, as a photograph shows him digging beyond a temporary baulk to the west of the existing pit (Plate 11); according to the Journal (7/15/1935), “discolored areas that showed up in the floor of the
extension of the pit westward, wherein we have found Burials #9 and 10, led to the belief that we have found another burned wall." This was soon seen to be a burned floor, situated south of the extension of the wall trench
seen in original Pit 5 and level with its top at a level of 3'. An intrusive pit,
dug down from the 2 3/16" level in the area where Burial 9 had been found,
cut through this floor. Apparently within this pit, at a depth of 38", Burial
11, consisting of a crushed skull, the bones of both hands, and a few ribs
and vertebrae, was found, along with an alligator skull and turtle carapace
and plastron fragments which lay between the burial and the location of the
wall extension. The turtle shells apparently formed a bed for the alligator
skull and lined the north side of the intrusive pit. On the day these finds
were made, Chambers was unwilling to postulate any link between Burial
11 and the faunal remains or the wall and floor (see Notebook 7/15/1935
and Journal 7/15/1935-7/17/1935). Professor Ross Hutchins of the Missis-
sippi State Zoology faculty positively identified the alligator skull as such
two days later.

On July 18 the profiles of the Pit 5 extension were drawn (Notebook, 7/18/1935; see Figure 21) and Burial 11 was removed; at this point Chambers
seems to have changed his mind about the relation of the faunal remains to
the intrusive pit, which he believed ended 6" above where they lay. He con-
cluded that

the fact that [the] shellcovered area extends slightly beneath this north
limit of the [intrusive] pit toward the wall on a rising plane links this
deposit with the wall of the house, & lends color to the assumption that
this shell covered level is actually the floor of the house. In that case, B
#11 lies in its dismembered state in a pit that almost grazed the house wall
after cutting through a higher burned floor...Probably, then, the alligator
skull & turtle shell floor belong to the earlier occupancy of this house,
and the nearby B #11 is unrelated (Notebook, 7/18/1935; see detail
drawings of the human and faunal remains in Figure 22 and photographs
in Plates 12 and 13).

On the 19th the alligator skull and turtle shells were removed to reveal a
layer of mussel shells beneath, and several new hypotheses were advanced by
Chambers: 1) that the deposit of faunal remains was in the intrusive pit
where it just grazed the burnt floor on its north edge; 2) that the wall trench
seen in the Pit 5 extension, which contrasted with that in Pit 5 proper by
Plate 12. Burial #11, the alligator skull and the turtle shells, and the excavated house wall to the right. July 17, 1935. [MDAH 1331-483]

Plate 13. A view of the addition to Pit #5 consisting of a pit extending ten feet west from the N12-N17" line. This gives a good view of B#11, the alligator skull, the house wall, and shows the intrusive pit marked by string at its traceable margins. July 18, 1935. [MDAH 1331-480]

Figure 22. Details of Burial 11 and faunal remains, Pit 5 extension, 1935 (Chambers Notebook). Top: General sketch of entire pit configuration; Bottom: Sketch showing alligator skull in relation to heap of turtle shells. This outer line represents the possible outlines of the intrusive pit. Alligator skull at depth of approximately 44". This area of turtle shells is 27" in length, and in the widest part, 1' across. Turtle A was found to have been buried whole.
looking burnt, was probably a later rebuilding of the Pit 5 wall, and that traces of a structure contemporaneous with the Pit 5 wall trench could be seen running under the burnt wall trench and possibly linked to the white clay floor found at a depth of 4' 1".

This is the evidence we have: everything comes to a screeching halt in the notes with this final day's work, with many questions unanswered. The Pit 5 and Pit 5 Extension trenches had to be backfilled and the excavation terminated in the following few days, leaving Chambers no opportunity to investigate his hypotheses. Chambers did not go back to the site; Rowland died in 1936; and archaeological fieldwork was curtailed under his successor.

Analysis of the Data

As has been pointed out in the foregoing description of the excavations, the analysis of the evidence was not part of the work required of Chambers by MDAH. Rowland's interest was in artifacts for the museum, and he doubtless felt that with the retention of the artifacts and the written record of the excavation, an adequate record of the site had been provided. We must regret this quite understandable assumption, particularly in view of the loss of the artifacts, since no reconstruction at whatever short distance can ever hope to match the original insight of the excavator with his work freshly in mind. But a new awareness of the importance of the site, fostered by the work done by Marshall and several interim reports on that work, increases the potential interest in the original excavations on the site.

The Bluff-edge Midden

There is little evidence for the definition of multiple chronological components in the minor excavations carried out along the bluff edge, although the pottery descriptions suggest more than one. Marshall's excavations, which were much more extensive in this area, will eventually serve as the context for the few observations that can be made here.

First it is clear, as I have mentioned above, that the excavations here were essentially opportunistic, removing eroding burials and testing the area of the bluff edge in the hope of discovering the structural evidence suggested by the fragments of daub that were found on the surface. The four excavated pits were placed at intervals to cover most of the length of the northern edge of the bluff. The three layers indicated in the field descriptions of the excavation were ten inches of "midden," in which a rich variety of ceramics and some daub with grain imprints was found (daub in pit 4); five inches of burnt daub with cane imprints (found in pits 1, 2, and 4); and a layer underneath that, in which very little cultural material was found, but what was found was not significantly different from the material that overlies in the mound. Chambers does not indicate how deeply this layer was excavated, but from his earliest observation of the profile offered by the eroding bluff-edge, he indicated that the area showed a midden accumulation of "nearly two feet" (Journal, 7/12/1934). The presence of the generous quantities of daub along with the midden has always pointed clearly to residential structures on this part of the site.

There is no profile for any of the four pits that were dug at the bluff edge, but the sequence described in the notes seems to suggest that this area of the site had been at first lightly utilized by its inhabitants (though not kept cleared and clean) and then had houses built upon it. When a house was destroyed by fire, the area might have been reused for another or the site of the house might be shifted. Pit 4 was placed in an area that seems to coincide with that of the MAA excavations at the northeast corner of the mound area, shown as the northeasternmost pit excavated by Marshall in 1967 on Lolley's Figure 4 (this issue) and identified by Marshall as the site of a house mound. Its stratigraphy as observed by Chambers showed two daub concentrations separated by midden fill, suggesting the usual sequence of destruction and construction by which such a mound was built up. Later excavations in the same area uncovered what Marshall has tentatively described as "seven or eight consecutive house cores" (Gordon 1982).

The burials had probably been made into the floor of the structure or structures represented by the burnt daub layer, the dominant practice among the Chickasaws later in the Lee County region. The infant skeleton in Pit 1 was found "about 15 inches deep in the midden" (Journal, 7/16/1934), which would indicate that it had been found below the burnt daub. It was a flesh burial, arranged flexed on its left side with its forearm across its chest. The second, partial skeleton of an adult was found an unspecified distance vertically down the eroding bluff-edge slope. Probably this burial had also been made in the flesh, but parts of it had already eroded into the creek.
The descriptions Chambers gives of the potsherds suffer from their very immediacy; the pottery was unfortunately not washed or grouped for analysis at the time. Yet the descriptions are quite suggestive in spite of these handicaps; attributes distinctive to at least three types are mentioned. The "polished black ware bearing faintly trailed scroll design" (Journal 7/23/1934) suggests Moundville Black-filmed Engraved (Gordon 1982). The "rim sherd of shell-tempered ware with everted lip, bearing on the inner surface the trailed outline of a hand" (Journal 7/25/1934) sounds very much like the Southeastern Ceremonial Complex hand motif found on flared-rim bowls of Alabama River types. Finally, the "sherd of a thick globular vessel bearing a red painted design" (Journal 7/25/1934) may be representative of a Nodena-like type, although it sounds too coarse for Nodena.

Such an assemblage of types is somewhat confusing, if it is assumed that they are contemporaneous. All three come from the upper midden layer above the layer of cane-pressed burnt daub, yet the three may represent more than one phase. As defined by Marshall (1977:56-57), the black-filmed incised ware would probably belong to the Middle Mississippi Lyon's Bluff phase, while the red-painted and incised-hand decorated sherds probably belong to the later Sorrells phase. The apparent mixing of the two periods in this area of the site is probably a result of the erosional process or of construction or burial disturbance, and that fact makes it uncertain whether one or the other or both phases are to be connected with the burials and the occupation on this part of the site. The possible connections with the Mississippi Valley to the west suggested by the red-slipped ware would not be strange given the apparent role played by simple chieftains on the Tombigbee as middlemen between the Mississippi Valley chieftains and the Moundville hegemony during Middle Mississippi times.

The Mound

Already in 1934 there was clear evidence of looting on the top of the mound, and a large cedar tree had taken root and prospered in the center of its summit. The two trenches placed in the mound by Chambers and Gordon in 1934 remain the only controlled excavation of the Lyon's Bluff mound that has ever taken place, so the information to be gleaned from it is especially important. Two trenches, one running east and west (Figure 2) and one north and south (Figure 3), were placed in the summit of the mound far enough apart to miss the cedar tree. I have discussed previously the problems with reconciling the profiles of the two trenches across the tree-root hiatus. Figure 4 shows the profiles of the two trenches laid out in a composite for analytical purposes.

We would expect that a mound of this period and cultural affiliation was built in stages, and that at each stage it was surmounted by a structure of some public significance, usually burned or otherwise dismantled at the end of its usefulness and covered over with a new layer, sometimes of an unusual soil type, thus raising the mound by stages and preserving within it the evidence of its function and evolution. This kind of sequence is familiar from nearby and comparable mounds like that at Lubbbub Creek (see Blitz 1993 and Lolley, this issue). Further, on the basis of European colonial accounts, especially of the Natchez, Mississippian substructural mounds have been seen as the site for three types of structure: the chief’s house, the Summary of the site of the village/chiefdom, or the temple. Each is identified on the basis of characteristic material evidence. Evidence of food preparation and other domestic activities—including possibly the feasting identified in several recent studies—is identified with the house of a chief or elite. A structure free of ordinary refuse but having burials and unrelated human skeletal fragments is construed as a chiefdom house. And a structure characterized by a hearth of more than ordinary size and perhaps a wall screening part of the interior is thought to be a temple. These three may also be mixed to some extent: the temple, according to most ethnographic evidence, also served as chief house for certain members of the elite, while the chief was in some cases buried at least temporarily in the floor of his house. Ethnographic evidence suggests that only the chief’s house and the temple would be subject to periodic razing by fire and rebuilding on a new surface.

As discussed above, multiple layers of burning and construction debris were found in the two mound trenches, but evidence on east and west sides of the summit seem to differ somewhat. At least three rebuilding episodes are clearly indicated on the east side of the mound by layers of white sand and confirmed by the slight evidence of the white sand layer in Pit 6 on the mound’s eastern edge (compare the sand/clay alternation and the concentric horizontal arrangement of layers at Lubbbub in Blitz 1993:74-75), but there is no evidence there of structure floors except for a possible "fire place" on the western edge of the south profile. On the western side of the summit, on the other hand, at least three burnt floors appear to be present in the upper levels. This is what would be expected if structures were placed on the western side of the summit facing
east. At deeper levels, on the other hand, clear evidence of “hard burned floor” appears in the deep pit created in the eastern trench to reach undisturbed levels, but since no such deep pit was dug on the western side of the mound we lack any kind of evidence that would tell whether such structures stretched into that area. On both sides there is a deep layer of “reddish brown gumbo” or “black waxy clay” (these two are likely the same) that apparently remained undifferentiated to the perception of the excavators for nearly a third of the mound’s 14” height, and the description on the western trench drawing, indicating that the clay was “interspersed with lime nodules, sherds and animal bones,” would seem to suggest a considerable single effort of building by basket loading of midden dirt. Thus the evidence from the mound suggests two structure sequences, possibly representing “temples” in both instances, separated temporarily by a major addition to the mound incorporating midden dirt. If we compare it to the evidence from Lubbock, it is possible that the very low-level floors uncovered in the eastern trench may represent an earlier “emergent” or “premound” Mississippian phase at the site, followed by an initial significant mound-construction effort, followed by subsequent reworkings of both the structure on the mound and the mound itself, through three or more major leadership transitions. Such a premound phase would explain the dissonance between the depth from the top of the mound to undisturbed soil and that from the mound summit to the sand layer in the east profile of Pit 5.

Pit 5: Mound-edge Burials

The most spectacular findings of the 1934-35 excavations at Lyon’s Bluff certainly came from Pit 5. Here we have burials and features in plenty, more than amply making up for the lack of artifacts. It is this very embarrassment of riches, coupled with the relatively large area that was uncovered, that makes the analysis of the excavation on this part of the site so difficult.

Clearly Chambers’s initial efforts in Pit 5 in 1934 were no more than random searches for burial artifacts. Rather than excavating according to any sort of stratigraphic control, the excavators merely followed the lure of burial finds, as the random opening of an area to the west at the level of N17-N20, that yielded no burials, suggests (see Plate 3). Chambers seems to have been thinking in terms of an undifferentiated midden, this time associated with the western edge of the mound in some way (Notebook, 7/2/1935), unless the northwestern mound slope evident on Lolley’s contour map (Figures 2 and 4, this issue) is in fact an artifact of the backfilling of Chambers’s excavation. Chambers never referred to any point to a discernable burial pit outline at a higher level in the midden fill, so I am assuming that no such pits cut the overlying midden, and have accordingly treated the overlying midden, burials, and underlying midden as three separate events. Chambers was not much concerned in 1934 or indeed at the beginning of 1935 with a search for structures, since he did not expect to find them, but since structures on the site have proved to be so consistently characterized by the presence of puddled clay floors and daub, it is unlikely that he would have missed such structures here.

The excavation record suggests that Chambers thought that Burials 1-10 west of the mound were roughly contemporaneous: they were all buried at roughly the same depth and none of the burials was intercut by intrusive burials. All were flesh burials, adults, subadults, and infants, and all were flexed in their graves. Few were accompanied with any grave goods at all, but all seem to have been laid out carefully. Six (1, 2, 3, 4, 6, and 7) were oriented with their heads to the south, while four (5, 8, 9, 10) were laid with their heads to the east (the position of the infant that apparently accompanied Burial 5 was undetermined). Burial 7 was apparently buried prone, and Chambers felt that it had been placed in some sort of concentration of ash and charcoal.

The fact that these burials were not associated with house floors (unless the “ash pit” of Burial 7 suggests a structure floor) suggests either that they differ from the burials of the bluff-edge midden in temporal placement or in status. Their location on the site so near to the mound would be unusual unless the mound had perhaps fallen into disuse and they represent a terminal episode on the site, which is quite possible. The sample is too small for assertions about burial orientation or age/sex distributions to be of much use, except as an addition to as yet unpublished evidence of numerous other burials on the site (cf. Brookes, this issue).

Pit 5: Structures

Beneath the midden layer containing the ten burials, Chambers and Gordon found a number of structures that were investigated rather inconsistently. An attempt was made, when structures appeared, to excavate them in natural units—i.e., to uncover and define the structures as structures. The inconsistent depth in the excavation of the original Pit 5 (see Figure 16) is probably explained by the fact that Chambers left what he believed to be the tail of the
mound to the east, signaled by the layer of sand in the Pit 5 eastern section, mostly alone once the burials had been removed (with the exception of the northeast corner of the trench, where an attempt was made to find an undisturbed level). Structures were found toward the western half of the trench—as Chambers apparently thought beyond the western edge of the mound—beneath the burials. Most notable of these were what Chambers referred to as a white lime-clay floor at a depth of over three feet (Figures 14 and 17), and of course the yellow clay wall trench defining a structure with which the alligator-skel skull burial was (perhaps fortuitously) associated (Figures 15 and 17). Chambers’s developing understanding of the site was drastically truncated by the termination of the season in 1935, but by the end of the excavation he seems to have decided that all these structures were interrelated, in that he came to think that perhaps the line of posts apparently extending the line in the yellow clay wall trench into the Pit 5 extension to the west may have represented a rebuilding of that structure, and the lime-clay floor may have been related to the “marly clay” with which the yellow clay wall trench was related. Since only the single wall was found, little more could be said, but it is possible that an eastern wall of a very large building lies undetected under the shallowly-excavated eastern part of the trench; or alternatively, a wall perpendicular to the east end of the wall in the area to the north of Pit 5, defining a building of unspecified size, may also exist. This is clearly a problem for future excavators.

Pit 5: Alligator-Skull Burial

It goes without saying that the individual who was found buried next to the yellow clay wall trench was important, but we know very little else about him—or her, since to Chambers’s great credit he did not even attempt to sex this partial skeleton consisting of skull, hands, a few ribs and vertebrae, and one patella. But he did firmly decide that the burial, with its accompanying pavement of turtle shells overlying a shell layer and with an alligator skull disposed upon the turtle-shell pavement, was made into the pit he delineated penetrating the floor associated with the westward extension of the yellow clay wall trench (see Figure 15 and Plate 13). This burial seems to represent a textbook case of the secondary process described by Du Pratz for Natchez elites: provisional burial for a specified period of time, followed by exhumation of long bones for honorific preservation in temple or above-ground ossuary; the main difference is that this individual was apparently not buried in a mound, but beside one. Probably the exhumation exaggerated the disturbance of the original burial pit and made it that much more noticeable.

Another difference from the normal secondary practice is more dramatic: most southeastern bundle burials, the ultimate form that such processed remains took, include a skull, so it is odd that there was a skull present with the remains of this burial—that it was not removed with the long bones, scapulae, and pelvis. But significant mound-associated burials in the Southeast do sometimes contain additional, assumed “trophy” skulls, and it may be worth considering here that the skull apparently “left behind” may not have belonged to the individual in the burial any more than the alligator skull did.

It is tempting to see in this burial evidence of belief and ceremonial practice. Alligators are still occasionally found on the Tombigbee or its tributaries, were doubtless present in greater numbers in the past, and would have been a source of food as well as danger. Turtles were clearly a significant source of food in the region. Both have been associated with matrilineal clans in the Southeast as well, the alligator with the Alabama, Chickasaw, and Creeks, and the turtle with the Creeks and Yuchi (Swanton 1946:658-60). Although this particular placement of faunal elements with a burial is unusual, the presence of turtle shells with burials, interpreted as ornaments or dance rattles (cf. Gahagan 1981), is known from the late Mississippian period and associated with female burials. It could also be argued that the alligator skull element could have been used as a mask for ceremonies, though without the skull to examine for purposeful alteration, this must remain a suggestion.

Stratigraphy

The most important issue that the Chambers data can still profitably address depend importantly upon the sequencing of the most significant findings of the excavation: “midden” burials, the alligator-skel burial, domiciliary structural evidence, and mound construction. This is, of course, just the trickiest part of using this evidence. Chambers established no conclusive identification among layers found on different parts of the site; most of his grasp of the site’s structure was dependent upon vague notions of historical sequence trapped in the pre-radiocarbon short chronology. When he found actual structural floors of puddled clay or distinct changes in soil character due to structural activities, he recognized and recorded them, but it is unrealistic to expect that more subtle aspects of stratigraphy would have been seen or recorded. Furthermore, since
we are now without the apparently considerable amount of pottery that was found in various of the features on the site, we cannot count sherds to yield a seriation. The only recourse is what must be a flawed attempt to reason through a hypothesized sequence of evidence. To that end I offer Figure 23, a diagram loosely based on the Harris Matrix format (Harris 1979), laid out roughly according to the site plan in Figure 8, with Pit 5 to the left and the mound to the right. It is based upon the few explicit statements of superposition that Chambers made and that have been cited already, the remainder being inferred from the plan and section drawings. Note that the "upper midden" has been subdivided to accommodate "burial events" 1-10, but these burials are considered coeval only in the sense that the locations of prior burials were known when others were placed.

In correlating structural relationships between the mound and Pit 5 I caution against the confusion introduced by the variance in Chamber's practice in profiling his trenches. The mound trenches are profiled facing south, while the Pit 5 trench entirely lacks a south profile, and all its plans and section drawings are oriented toward the north. But it seems clear that Chambers thought the "coarse sand" layer so prominent on the west end of his east-west mound trench (Figure 3) was represented by the sandy layer evident on the east face of Pit 5, while the "mixed clay" or "brownish loam" that lay over the top layer of sand may be related to the so-called "upper midden" of Pit 5, apparently rather lighter in color, in which Burials 1-10 were found. If this correlation is valid, then the midden burials can indeed be attributed to a period after some mounding episode; but we cannot be sure that it was the last such episode unless we can establish physical continuity between the two sand layers, and that is a task for the future.

The major midden layers on the site as observed in Pit 5 (specified here as upper midden, upper black midden, lower black midden) seem to suggest three periods, as Marshall has argued, but building activities in the Pit 5 area during the middle period (see Figures 17 and 21) wreaked such destruction on the stratigraphy that they blur distinctions below the upper midden at the level of the west profile (as implied by the plan as well—see Figure 14) and make it clear that these activities in this area of the site were later than the upper black midden layer (which they cut and disturb) but earlier than the upper midden. It should also be remembered that we have no means of knowing how much plow damage was done to the site before Chambers saw it, and accordingly
how many layers had been removed from both the mound and the rest of the site. Finally, it should be emphasized that the alligator-skull burial was found well below Burial 9 and the upper midden, so it appears to predate the final period on the site.

Although the material evidence of the findings of Chambers's excavations at Lyon's Bluff are now gone, the records he kept and the fact that no additional excavations have been carried out where he excavated on the site mean that it will be possible in the future to follow up on his findings. I hope that this examination of the available evidence will encourage such investigations, as well as the exploitation of the archival evidence of other excavations whose collections are now lost.

Acknowledgements

I wish to thank the following individuals for assistance with the original research on this paper in 1982: Alan Downer, Bruce Smith, Christopher S. Peebles, Richard Marshall, Moreau B. C. Chambers, and Slater Rhodes Gordon. I apologize particularly to the latter two gentlemen if I have misinterpreted their work.

Patricia Galloway is the editor of Mississippi Archaeology, and this is her last issue.

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Availability of Lithic Raw Materials and Curation at Two Mississippi Sites

Jeffrey Alvey

Abstract

Through their research archaeologists have attempted to understand the organization of lithic technology and how it relates to human behavior. Convincing arguments demonstrate a link between the effort of stone-tool production and prehistoric mobility. However, to conclude that prehistoric mobility is the only predictable link to stone-tool production technology is underestimating the complexity of human behavior. It is the purpose of my research to demonstrate that there is at least one other variable that contributes to the effort of stone-tool production. That variable is the availability of lithic raw materials.

Introduction

In the hope of understanding how the organization of lithic technology relates to prehistoric human behavior, archaeologists have expended a lot of time and energy studying the relationship between the effort of stone tool production and prehistoric mobility. Analyses by people such as Lewis Binford and Robin Torrence have shown a link between these aspects of culture (Binford 1979; Torrence 1983). However, some archaeological research has suggested that a second variable associated with technological organization may be just as important as settlement configuration. That variable is the availability of lithic raw material resources, and it is the effects of lithic raw material availability on prehistoric stone tool technologies that I am investigating. In other words I am simply trying to determine whether or not there is sufficient evidence to suggest that in cases where lithic raw material is not easily available, humans tend to recycle or rework their tools made from this material, and in cases where raw material is easily available, humans tend to discard used tools as soon as they become worn or broken. If there is evidence for these patterns in human behavior it could be said that this behavior explains why curation occurs. I will briefly discuss
two examples of previous research on the topic and then present the results from my own investigations at two prehistoric sites in northeast Mississippi.

Previous Research

There has been considerable research conducted in the past to explain why curation occurs. Curation is a theoretical construct used to label a type of technological organization. Stone tool technologies based on curation include a wide variety of tools that have undergone additional effort in production and/or maintenance. This extra effort is characterized by things such as several resharpening episodes. These tools are effective for a variety of tasks; they are manufactured in anticipation of use, maintained through a number of uses, transported from locality to locality for those uses, and recycled for other tasks when no longer useful for their primary purposes (Bamforth, 1986). On the other end of this production continuum are technologies based on expediency. Expedient tools are manufactured, used, and discarded according to the needs of the moment (Bamforth, 1986).

Lewis Binford links curation to overall subsistence-settlement organization (Binford 1979), while Robin Torrence links it to the problem of scheduling different activities around one another, which she calls "time-stress" (Torrence 1983). Other archaeologists have argued that no single factor can explain curation. Bamforth argues that the availability of lithic resources critically affects technological efficiency, and that the two aspects of stone tool curation known as maintenance and recycling are direct responses to raw material shortages (Bamforth 1986). Bamforth drew his conclusions based upon the analysis of a stone tool collection from a site in Lubbock County, Texas. That assemblage contained three basic types of raw materials. Based on the hypothesis that maintenance and recycling rates vary with access to raw material, and that distance to a quarry has an important effect on access to it, Bamforth showed that these three materials had differing rates of retouch and breakage depending on how far the site was from each of the materials' source areas (Bamforth 1986: 47). Based on this evidence, Bamforth argued that tool curation is a set of complex behaviors that cannot be explained by any single factor and that no aspect of technology, with curation as a specific example, can be predicted solely from an understanding of subsistence-settlement organization (Bamforth 1986: 48).

One other example of recent work that has been done to investigate the impact of raw material availability on stone tool technologies is by Tammy Stone (Stone 1994). She investigated the impact of raw material scarcity on ground-stone manufacture and use, based on an assemblage from the Classic-period Hohokam site of Pueblo Grande, Arizona. Ground stone tools were used predominantly to process foodstuffs through crushing and grinding. Stone argued that current models of ground stone tool technology are based on assumptions of energy efficiency and organization, but that these measures of efficiency have been limited to food-processing requirements. She further argues that the impact of raw material scarcity has not been considered systematically (Stone 1994: 680). As a result reconstructions of the subsistence economy, and shifts in it through time, may be inaccurate where raw material scarcity is a potential factor. To support her assertions she showed that the ground stone assemblages of the Classic-period Hohokam site that were made of non-local material exemplified a greater intensity of use. Metates made of non-local basalt were "smaller in size and showed more wear than those made from locally available material," and also were more likely to be "discarded in fragmented form" than metates of local material (Stone 1994: 690). She concluded that "the current model relating ground-stone assemblages to the subsistence economy needs to be modified in areas of raw material scarcity" (Stone 1994: 691).

Raw Material Availability and Stone Tool Curation in Northeast Mississippi

In order to understand how the availability of raw material affects the aspects of stone tool curation known as maintenance and resharpening or reworking, I examined the lithic assemblages from two sites in northeast Mississippi: 22-Mo-768, which is located in Monroe County, and 22-Le-701, which is located in Lee County. Site 22-Mo-768 is about 40 meters from the western bank of the Tombigbee River and 22-Le-701 is approximately 33 kilometers northwest from the river (See Figure 1). The proximity of these sites to the river is significant because the river served as source areas for lithic raw material—specifically gravel chert—for the people who occupied these two sites. Cretaceous gravels were exposed by river downcutting and redeposition during the Pleistocene and Holocene (Russell 1987). I am using these two sites in my investigation because it might be
expected that assemblages from functionally similar sites within a settlement pattern would show variability in chert tools due to differences in distance to the raw material source area. Both sites are considered to be general habitation sites reflective of a sedentary occupation (Rafferty 1980), and both are predominantly Gulf Formalional. Distance to source should thus be the primary variable affecting the lithic assemblages. If so then 22-Mo-768, which is close to the source area, should contain mostly tools that were relatively large in size and showed little sign of reworking or extensive resharpening, while 22-Le-701 should contain tools that were smaller in size, with extensive reworking and resharpening evident.

I used three different methods to determine whether or not there were differences in tool size and the extent of lithic reworking and resharpening between the two assemblages. The first method dealt with the question of whether or not there were differences in the individual sizes of the stone tools from these two sites. In order to determine this I used calipers to take general measurements of all the biface from these two sites. This included preforms and finished bifacial tools. The finished bifaces included projectile points and drills. The measurements I took were width, which was taken at the widest point of the biface, and length. I then used these data to compute the plan view area of each biface (See Table 1).

Measurements of weight were taken using gram scales. These data (Table 2) agree with the measurements of plan view area. They both show that the biface from 22-Le-701 are generally smaller in size than the bifaces from 22-Mo-768.

The second method of analysis involved determining the extent to which projectile points had been resharpened. Studies have shown that intensively resharpened points are characterized by steeper blade edge angles (Hoffman 1985). It is logical to assume that the flintknappers who made the tools that were found at 22-Mo-768 and 22-Le-701 employed resharpening techniques that minimized the risk of seriously damaging or breaking those tools. Because of its greater precision and control, pressure flaking was frequently used to achieve these goals. One of the effects of pressure flaking is that it causes reduction in blade width more rapidly than in thickness. This in turn causes blade edge angles to increase with successive resharpenings (Figure 2). Projectile points at the beginning of their use life should be char-
acterized by a larger blade size with acute blade edge angles, whereas more extensively resharpened points will have smaller blade sizes with more obtuse edge angles. In other words, by determining the blade edge angles of different projectile points, one can determine which points have been resharpened the most. Figure 3 shows how this measurement was taken. The results are shown in Table 3.

![Figure 2. The effects of pressure flaking on projectile points](image)

*Figure 2. The effects of pressure flaking on projectile points*

<table>
<thead>
<tr>
<th>Table 3. Blade Edge Angles. All values are in degrees.</th>
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<tbody>
<tr>
<td>Range</td>
</tr>
<tr>
<td>-------</td>
</tr>
<tr>
<td>22-Mo-768</td>
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<tr>
<td>22-Le-701</td>
</tr>
</tbody>
</table>

![Figure 3. Measurement of blade edge angle](image)

*Figure 3. Measurement of blade edge angle*

These data suggest that the projectile points from 22-Le-701 were more intensively resharpened than the ones at 22-Mo-768.

The third method employed deals with flake analysis, specifically that part of the flake known as the bulb of percussion. I chose this method based upon an article written by Andrew Pelcin (Pelcin 1997). His article presented the results of a controlled experiment that examined the relationship between core surface morphology and the flake attributes of length, thickness, and mass. The information gained from this experiment that was relevant to my research was the demonstration that a reduction of bulb size would be a major component of raw material conservation. Pelcin noted that “a large bulb of percussion indicates that the other flake dimensions will be smaller, and a small bulb of percussion indicates that the other flake dimensions will be larger” (Pelcin 1997: 754). Under conditions of limited raw material, there is selective pressure to reduce the raw material more efficiently into the largest number of flakes and greatest dimensions possible. There are several actions under the control of the flintknapper which affect the size of the bulb of percussion, including indenter velocity, indenter diameter, angle of blow, and indenter type (Pelcin 1997). Any or all of these factors would need to be controlled as part of the reduction process and should be reflected in the attributes of flakes from an assemblage produced with limited raw material. Based on this information, I investigated whether or not the flintknapping strategies of the people at 22-Mo-768 and 22-Le-701 were influenced by raw material availability. Since the reduction of bulb size is a major component of raw material conservation, I decided to take measurements of the bulbs from the flakes found at 22-Mo-768 and 22-Le-701, specifically the thickness of the bulbs. Using calipers, thickness measurements were taken of the bulbs of percussion of 1,000 randomly selected flakes, 500 from each site. No separation was made based on reduction stage. I created fourteen classes for these measurements by starting with .85 mm and adding .5 mm per category. Each flake was put into its appropriate class based on the thickness of its bulb of percussion. The data are shown in Table 4.

**Conclusion**

Analysis of stone tools has demonstrated that raw material availability was a factor in structuring stone tool technologies at two Gulf Formational sites in
Table 4. Flake Analysis – Baked of Percussion Thicknesses (mm)

<table>
<thead>
<tr>
<th></th>
<th>22-Mo-768</th>
<th>22-Le-741</th>
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<tbody>
<tr>
<td>0.85-1.35</td>
<td>3 (6%)</td>
<td>37 (7.4%)</td>
</tr>
<tr>
<td>1.36-1.85</td>
<td>35 (7%)</td>
<td>141 (28.2%)</td>
</tr>
<tr>
<td>1.86-2.35</td>
<td>57 (11.4%)</td>
<td>118 (23.6%)</td>
</tr>
<tr>
<td>2.36-2.85</td>
<td>63 (12.6%)</td>
<td>71 (14.2%)</td>
</tr>
<tr>
<td>2.86-3.35</td>
<td>76 (15.2%)</td>
<td>51 (10.2%)</td>
</tr>
<tr>
<td>3.36-3.85</td>
<td>91 (18.2%)</td>
<td>28 (5.6%)</td>
</tr>
<tr>
<td>3.86-4.35</td>
<td>45 (9%)</td>
<td>17 (3.4%)</td>
</tr>
<tr>
<td>4.36-4.85</td>
<td>36 (7.2%)</td>
<td>13 (2.6%)</td>
</tr>
<tr>
<td>4.86-5.35</td>
<td>33 (6.6%)</td>
<td>7 (1.4%)</td>
</tr>
<tr>
<td>5.36-5.85</td>
<td>27 (5.4%)</td>
<td>7 (1.4%)</td>
</tr>
<tr>
<td>5.86-6.35</td>
<td>23 (4.6%)</td>
<td>4 (8%)</td>
</tr>
<tr>
<td>6.36-6.85</td>
<td>5 (1%)</td>
<td>3 (0.6%)</td>
</tr>
<tr>
<td>6.86-7.35</td>
<td>3 (0.6%)</td>
<td>2 (0.4%)</td>
</tr>
<tr>
<td>7.36-7.85</td>
<td>3 (0.6%)</td>
<td>1 (0.2%)</td>
</tr>
</tbody>
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northeast Mississippi. This supports previous research, which suggests that explaining the variation in stone tool production technology is a multidimensional problem that must account for several potential sources of variation. I am not suggesting that Binford, who links stone tool technologies to subsistence-settlement organization, or Torrence, who links it to time-stress, are wrong. I am simply saying that every case has unique conditions that contribute to the final organization of technology, and that one of those general conditions that must be considered is lithic raw material availability.

Acknowledgements

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Jeffrey Alsey has a BA in anthropology and is a graduate student in computer visualization at Mississippi State University.

Book Reviews


Reviewed by Paul Welch

The society centered at Cahokia was (choose one):

A. An autonomous state (evidence: those mounds sure are big).
B. The core of a world-system that extracted bulk quantities of food, clothing, and raw materials from peripheries hundreds of miles away (evidence: a few Cahokia-style pots here and there).
C. A peripheral outpost of the Aztec world-system (evidence: some hemisphere-wide similarities in conceptualizing supernatural beings).
D. A Hobbesian Leviathan (evidence: lots of fancy stuff at Cahokia but not so much elsewhere).
E. A theocratic chiefdom run by an extensive hierarchy of priest-nobles who tried to control the daily lives of everyone everywhere (evidence: temple/shrines in even podunk little non-mound communities).
F. A complex chiefdom with an elite whose lives barely impinged on, and who were little concerned with, ordinary folks (evidence: ditto).
G. A series of simple chiefdoms whose leaders were tied by alliance, marriage, or descent to the paramount chief (the evidence is—dare I say it—the same evidence all the other options claim as their support).

Though simplistic and unwarrantably facetious, the foregoing pick-a-winner question summarizes what we know (or don't know) about Cahokia and the other Mississippian communities of the American Bottom. All these interpretations, and more, have appeared in print within the past decade. Which one should we believe? George Milner's moderately technical book, The Cahokia Chiefdom, espouses answer G. In fact, answer G is a direct quotation from the book, up to the beginning of the parenthesis. Among the cognoscenti, this is the viewpoint known as "little Cahokia," and Milner...
gives us perhaps the littlest of little Cahokias. The principal reason he believes Cahokia is littler than the other interpretations is simply that there were not as many people present as others would have us believe. And the reason there weren’t that many people is that the place was wet.

The organization of Milner’s book is straightforward. He commences with a review of the area’s history and archaeology, setting the stage for the argument developed throughout the rest of the book. The second chapter argues convincingly that the current appearance of the American Bottom—soybean fields, subdivisions, and slums—radically misrepresents the character of the landscape before massive diking and drainage projects dried the place out. Milner has assembled a wonderful collection of early descriptions of the place; my favorite is from Charles Dickens, who didn’t like much in America and in particular did not like the “ill-favoured [...] unbroken slough of black mud and water [with] no variety but in depth [...] vast tracts of undrained swampy land [filled with] stagnant, slimy, rotten, filthy water.” While the wetlands produced large quantities of foods, such as fish and fowl, the wetlands also severely constrained the amount of the land surface suitable for housing, and for planting maize. Chapters 3 and 4 summarize information about pottery style distributions and subsistence remains from the Late Woodland through Mississippian periods. This brings us to the portion of the book likely to arouse strong reactions from other American Bottom archaeologists.

In Chapter 5 (and in part of Chapter 7), Milner argues that sites are pretty much alike in terms of production and consumption of food and artifacts, with the only exception being the uppermost level of the social hierarchy. This statement plainly challenges the interpretations of archaeologists who stress differences between levels of the settlement hierarchy, those who argue that there were full-time craft specialists, and those who infer the presence of part-time, elite-attached specialists at Cahokia. Milner makes a point of emphasizing that all interpretations—his and those of others—must make use of all the available evidence, rather than selecting only the data that serve to confirm a particular interpretation. Given this emphasis, I was surprised to find that while a great deal of quantitative information is presented in graphs, there are no tables, nor even listings of the sources of the data in most of those graphs. This impacts to many of Milner’s assertions an air of “trust me, I have all the data.” For example, no data are presented to support the claim that, “A monotonic decline in [Ramey

Incised] jar size with increasing distance from Cahokia described by Pauketat and Emerson would not be nearly as neat if they were not as selective about which sites to include in their exiguous sample.” Cries of “show me the data!” are likely to echo through the next chapter as well.

Chapter 6 presents Milner’s calculation of the population of Cahokia, and of the American Bottom as a whole, by phase. Or rather, we are told what variables he measured and used in equations to produce his results, though we are not given the raw data (again, no tables). These results put the “little” in “little Cahokia.” At its height in the Stirling phase, the population of the American Bottom is estimated to have been in the range of 15,000 to 50,000, with the upper number cast as a probably exaggerated upper limit. For Cahokia proper, Milner estimates between 3,000 and 8,000, with the upper number again described as a probably exaggerated upper limit. Some other researchers would have us believe that Cahokia alone had as many people as Milner says lived in the entire Bottom. Milner’s results, thus, imply a society of far smaller scale than that envisioned by archaeologists who interpret Cahokia as state capital, world-system core, imperial outpost, or market center.

In Chapter 7 we are given more results from calculations, this time calculations of how much labor, per phase, went into building mounds and erecting palisades. Dividing total labor hours per phase by the phase lengths, Milner obtains average annual labor requirements that are well within the capabilities of the small population size he calculated in the previous chapter. We are also presented with information about the social differences manifest within and between cemeteries, with an argument that neither marine shell beads nor arrowheads were made by full-time craft specialists, nor even by elite-attached specialists. Overall, this chapter’s message is that the highest elite at Cahokia had the largest numbers of fancy goods, but they did not maintain exclusive control over those fancy goods, did not preside over a differentiated economy, and did not control vast labor forces.

The concluding chapter assembles all the foregoing information in support of Milner’s notion of American Bottom Mississippian society as “a serially homologous structure with each part being not only structurally similar to the others but capable of supporting itself and going its own way under favorable circumstances.” The Cahokia site was big not because it was the center of a vast or complex society, but because it was where the best
farm land in the Bottom was, and because it had no significant rivals within hundreds of kilometers. In short, answer G in the multiple-guess question with which this review began.

So, should we accept Milner’s answer, or one of the other ones in the list? Milner’s answer is plausible, but so are several of the other interpretations. And this is precisely the problem in American Bottom archaeology: archaeologists are looking to see whether they can find data that appear to agree with their interpretations. If you approach the archaeological record asking the question “Can I find something that fits my preconceptions?” I guarantee that you will succeed; humans are adept at maintaining preconceptions. But if you think that archaeology has more to do with science than with advocacy journalism, you ought to ask the question, “What could I find in the archaeological record that would prove that my [or someone else’s] preconceptions are wrong?” Nobody is doing this in the American Bottom. Strikingly, in his penultimate paragraph Milner explicitly disavows a scientific approach: “It was never my intent to somehow prove that the various aspects of the mighty Cahokia scenario, either individually or collectively, are wrong. Instead, my objective has been to show that existing data are compatible with an alternative model for a structurally simpler society, one that was not as politically centralized, economically differentiated, heavily populated, or aggressively expansionistic as commonly thought.”

As a reviewer I can suggest that a different goal might have been preferable, but only the author gets to specify a book’s goal. Milner does indeed show, in considerable detail, that the existing data are compatible with a model less structurally complex than the world-system/state/market/urban models. The book is readable, occasionally amusing, and well illustrated. Tabulated data would have improved the usefulness of the book for other researchers, though probably at considerable cost in both dollars and readability. Because data from other Mississippian societies have forced me to accept smaller and less complex societies than my preconceptions posited, I suspect Milner may be closer to the truth of Cahokia than any of the “mighty Cahokia” models. If asked to recommend just one book about Cahokia or the American Bottom, this would be it.

Paul Welch is a professor of anthropology at Queens College, City University of New York.


Reviewed by H. Edwin Jackson

The earthen constructions of pre-Columbian Native Americans are wondrous testaments to the rich cultures of peoples inhabiting eastern North America during the last 6000 years. It was, in fact, a summer’s fieldwork in Kentucky excavating a modest Adena burial mound that piqued my interest in southeastern archaeology. Now as a professional archaeologist, research goals are somewhat abstract anthropological concerns: to better understand the broad patterns and organization of the once living cultures now represented by the archaeological record, and to contribute in some small way to theory about cultural dynamics. This way of thinking tends to direct one’s thoughts about earthwork-demarcated ceremonial and political centers toward their function in the societies that built them. Who constructed them and for what purpose? Do they represent the efforts of kin groups or multisocietal gatherings? Do they reflect the communal efforts of a people expressing social solidarity through collective labor? Or do they demonstrate the ability of an elite to coordinate and control social labor in a milieu of political competition among nations? How do location and relative size reflect the administrative efforts of chiefly societies? In the context of such questions, the sites become abstractions—ceremonial centers, chiefly residences, and local administrative hubs. These are good questions, and they serve as the glue that keeps archaeology part of anthropology. Nonetheless, from time to time it is good to make the pilgrimage to Poverty Point, visit the Winterville mound group, or stand at the base of Monk’s Mound at Cahokia, to rekindle the awe that such sites simply inspire in their own right.

Untethered by abstract anthropological questions, William Morgan, an award-winning architect, offers us a different view of the mounds and other earthworks of eastern North America. In his volume, Precolombian Architecture in Eastern North America, Morgan presents an architect’s interpretations of the earthworks constructed by Native Americans from the perspectives of form, geometry, symmetry, order, and siting—architectural concepts that focus our full attention on the sites themselves. These are the
constructs at work in the aesthetics of modern edifices, conscious applications by architects to produce an intended sense perception (though not necessarily recognized in such terms by inhabitants or visitors). Morgan argues that the pre Columbian architects also had such constructs in mind as they planned, laid out, and constructed their monumental sites. To demonstrate these principles in action, Morgan has translated archaeological maps, aerial photographs, and many site visits into prepared architectural renderings of nearly one hundred aboriginal earthworks.

The book begins with an introduction that provides a thumbnail sketch of prehistory and a discussion of architectural principles. Morgan divides the prehistoric record into periods based on architectural trends. Three periods are identified, corresponding to peaks in earthen construction. These peaks are separated by building lulls between 1000 and 500 BC and again between AD 500 and 800. Sites are organized by architectural period and geographic region. Each site is rendered in a scale plan drawing and short description, often augmented by aerial perspectives, and occasionally by photographs.

Morgan's acquaintance with the evolving archaeological understanding of Archaic mound-building is revealed in the section on Period I sites, which includes Watson Brake, Stelly Mounds, and Poverty Point, as well as Tick Island, Horn's Island, and Tomoka in Florida, and a number of South Atlantic shell rings.

Period II sites include 24 Adena and Hopewell sites and contemporary sites in Florida and the Lower Mississippi Valley. Not surprisingly, this section is dominated by Hopewellian geometric earthworks in Ohio. Two sites, Tchula Lake and Spanish Fort, represent Mississippi.

Sixty-one sites represent Period III. There are several Late Woodland constructions (Toltec and Greenhouse, for instance) but primarily the section documents and interprets mound centers of the Mississippian period. Morgan includes a considerable range of sites in terms of geography, complexity, and size (from Cahokia in Illinois to Irene in Georgia). Not surprisingly, Mississippi is well represented in this section, contributing roughly one fifth (12) of the Period III sites, including the well known Winterville, Lake George, Anna, Emerald, and Fatherland sites, as well as lesser known sites such as Perkins, Alligator, Kinlock, and Magee in the Mississippi Delta.

An appendix follows with scale maps of sites from around the world, both prehistoric (Pyramids of Giza, Teotihuacan, Stonehenge) and historic (St. Peter's Square, Vieux Carré, and the White House) to permit comparison. These maps underscore the impressive nature of the prehistoric earthworks included in the volume.

It is perhaps a little too easy to find limitations in Morgan's work from an archaeological standpoint. Chronological placement is somewhat vague, and the interpretive problems associated with multicomponent sites (whose ideas about layout are at work, for instance) are glossed over. The attempt in the introductory chapter to provide a culture history overview falls a bit short from an anthropological perspective, though admittedly this is not a book about prehistory, per se. My greatest concern is how one distinguishes documentation of the architectural conceptions of the makers from the imposition of structure, form, and balance in the mind of the analyst, in this case Morgan. In part, my suspicion arises from the scant information that often provides the basis for many of the site plans. Renderings are sometimes based on field sketches or surveyors' notes. Considerable interpretive reconstruction (p. 29) is needed to render site geometry, mound morphology, and symmetry. For instance, the St. Louis group reconstruction (best known to us by mid-nineteenth century photographs of dirt being removed by mule drawn wagons) is based on Maj. S. J. Long's 1819 survey containing data recorded in the field with only a hand compass and measuring tape when the site was densely overgrown (p. 123). More than a few of the more recent site plans that form the basis for Morgan's reconstruction are based on little more. Is the portrayed symmetry or organization suggested by field sketches sufficient to conclude the same about the intentions of the builders? There is no doubt that Native Americans had developed complex mathematical and astronomic principles to lay out sites (civil engineering and alignment studies have demonstrated at least some of these). However, the assumption of symmetry and organization revealed by the drawings may impose an order (our order) that may not necessarily have existed, and thus may obscure variability that in itself may prove to be enlightening.

These complaints aside, this volume is a useful one. Morgan has assembled an impressive cross-section of Native American monumental architecture. The site descriptions include interesting historical anecdotes, information from interviews with excavators, and information about public access. What I found to have had the greatest effect on my views about the sites included in the volume is that all are drawn at the same scale (with
the exception of a couple of megasites—Cahokia, Poverty Point), so that visual comparisons are possible. Moreover, Morgan has brought together a selection of sites covering a broad spectrum of time, space, construction style, organization, and function that is not available in other single sources. In doing so it captures in a unique way the architectural achievements of prehistoric Native Americans.

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Reviewed by David Anderson

This volume brings together four classic early reports by C. B. Moore: *Certain Aboriginal Mounds of the Georgia Coast* (originally published in 1897), *Certain Aboriginal Mounds of the Coast of South Carolina, Certain Aboriginal Mounds of the Savannah River*, and *Certain Aboriginal Mounds of the Altamaha River* (the latter three originally published in 1899). It is one of a series of volumes released by the University of Alabama Press documenting Moore's prodigious work across the Southeast in the late nineteenth and early twentieth centuries. As recounted in the introduction to this and the other volumes of this series, Moore, a wealthy northern industrialist, cruised the rivers of the Southeast for over two decades in his steamboat, aptly named the *Gopher of Philadelphia*, excavating at vast numbers of sites. Moore followed the pattern of some of the greatest archaeologists of his day, such as Flinders Petrie, in excavating in hot climes (i.e., the southeastern U.S. in Moore's case, Egypt in Petrie's) during the winter months and then spending the remainder of the year at home in more temperate latitudes (i.e., Philadelphia, England) preparing reports on their findings, which were then submitted for publication before they set out anew for the field.

Originally published in the *Journal of the Academy of Natural Sciences of Philadelphia*, these volumes, like the modern reprints, used an oversize format and were lavishly illustrated with black and white line drawings, as well as occasional photographs and (in the originals, but unfortunately not in the reprints) color tinted images. For many years now, C. B. Moore's archaeological reports have commanded high prices on the antiquarian book market. I did an Internet search, in fact, and found that to obtain copies of the volumes where the Georgia and South Carolina expedition reports appeared originally would cost approximately $250.00. (Younger generations do not need to hear that thirty years ago it was still possible to obtain complete sets of Moore's reports for a few dollars each, essentially the cost of postage, from the Academy of Natural Sciences in Philadelphia. I just missed this opportunity myself, but have heard about it for years from older colleagues. I thus particularly appreciate what the University of Alabama Press is doing, since I have never felt like spending the several thousand dollars necessary to acquire a complete set.) Such prices had, in fact, made Moore's work inaccessible to recent generations of professionals and avocationalists alike, unless they were either wealthy or fortunate enough to live near a very good library. The University of Alabama Press has thus done a great service to southeastern archaeology by making these volumes available in an attractive and affordable format.

So too has Lewis Larson. The long-time state archaeologist of Georgia, now recently retired, Larson spent much of his adult life working on and near the Georgia coast. It is no exaggeration to say that he probably knows more about the archaeology of the area than any person alive. He has shared this knowledge with us all in an important, lengthy, and highly detailed *Introduction*, which runs to 85 pages. In writing for one classic in southeastern archaeology, Lewis Larson has, in my opinion, created another one with his overview of archaeological research on the Georgia coast in the twentieth century.

Larson clearly did his historical research. References to Moore's original field notes, artifacts, and even contemporary newspaper articles abound, making the introduction enjoyable and informative reading. Larson works his way site by site through Moore's accounts, detailing what was done and what was found, not only by Moore, but also by subsequent generations of archaeologists revisiting these sites down through the years. In describing the artifacts or features that were found by Moore, Larson does a first class job of identifying them using modern terminology, as well as in offering suggestions about how these sites are now interpreted. Most importantly, Larson provides reference after reference to his own work at or visits to these sites over the past half century, giving us a personal account of his own long
involvement with and knowledge of local archaeology. What is presented about coastal Georgia archaeology in the Introduction is thus an extremely important and welcome addition to the literature. The essay is a fitting climax to the career of a man who has done so much to advance our understanding of coastal Georgia archaeology, and eloquent testimony to his writing skills.

In my only quibble with this volume, it is clear that Larson knew less about recent archaeological investigations in coastal South Carolina and along the Savannah River. His accounts of recent work in these areas are somewhat abbreviated compared to the wealth of data provided for the Georgia coast. Many key references to recent research in the South Carolina area, in fact, are ignored or noted only in passing. Readers interested in more detail about Moore's work at mound sites along the Savannah River, and how these sites are now interpreted, for example, are invited to look at my own book, The Savannah River Chiefdoms (Alabama 1994). In an appendix to that volume, in fact, I reproduced and annotated Moore's descriptions of the mound sites he examined along the Savannah, and in the text each site is discussed in some detail. To be fair, however, C. B. Moore's work and writings on the Georgia coast were far more extensive than his work along the South Carolina coast or up the Savannah River. By page count, in fact, Moore devoted over 150 pages to the former area and little more than 25 pages to the latter. South Carolina archaeology's treatment in this volume, while comparatively minimal, is thus not altogether out of place.

Larson's efforts, beyond his outstanding Introduction, also include a detailed appendix giving Moore's name for each site, its current state number (if it could be relocated or still existed), and a synopsis of the artifacts, burial types, and cultural period found at each. Larson also prepared a detailed and useful index, complementing the indices Moore himself prepared, which are also included. These are comprehensive and valuable reference materials.

This volume is about Moore's excavation activity, of course, and that is what this reprint edition ultimately celebrates. The printing of this volume (and all the volumes in this series) was heavily subsidized by a number of organizations, including private companies, state archaeological societies and professional councils, and the Southeastern Archaeological Conference. That these organizations have supported these publications so strongly is a testament to the importance of Moore's work. Moore visited and worked at a great many sites and, for those that have since been destroyed, his reports provide the only information we now have. For many other sites still extant, Moore's examination represents either the first scientific excavation undertaken at them, or some of the earliest work.

Moore's reports, this one included, are well written and illustrated, fully equal to the professional standards of the time. Moore brought in outside specialists to aid in the interpretation of artifacts. Dr. Milo Miller, a surgeon, who, among other things, helped with the excavation and description of human burials, usually accompanied Moore. His reports thus provide an early example of multidisciplinary archaeological research. But perhaps most importantly from the standpoint of modern archaeology is the fact that these accounts exist at all. That is, Moore actually wrote reports and saw to their publication. This is something many twentieth-century archaeologists have had trouble doing, at least prior to the rise of CRM archaeology, where reporting is a mandated part of the process.

C. B. Moore's productivity in combining extensive fieldwork with prompt publication still serves as a model for modern archaeologists. He was the outstanding southeastern archaeologist of his generation. It is only fitting that each year the Southeastern Archaeological Conference bestows an award “For excellence in archaeology by a young scholar in southeastern studies” named after him. Those interested in learning more about C. B. Moore's remarkable career, and about southeastern archaeology in general, would be well advised to acquire these volumes.

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Reviewed by Tony Boudreaux

Few topics have generated as much regional attention and controversy as that which has surrounded the archaeological and ethnohistorical investigation
of Hernando de Soto’s trek across the southeastern United States. Professional and lay researchers have spent untold hours poring over maps and accounts associated with the expedition in attempts to reconstruct its route. The lure of the study of de Soto’s journey is easy to understand because its accounts provide a window into the physical and cultural landscape of the sixteenth-century Southeast. Also, as the first period of extended contact between Native Americans and Europeans in the Southeast, it was a prelude to the more extensive interactions that would follow. Although much attention has been focused on reconstructing the route of de Soto’s army, only one archaeological site has been unequivocally associated with a town visited by the Spaniards. De Soto and his army spent the winter of 1539-40, the first of the expedition, occupying Anhaica, the principal town of the Apalachee chiefdom. The archaeological and ethnohistorical investigation of this site, the Governor Martin site in Tallahassee, Florida, is the focus of Charles Ewen and John Hann’s *Hernando De Soto among the Apalachee*.

Ewen and Hann’s book consists of five sections. The prologue (pp. 1-17, written by Ewen) places the 1539-40 winter encampment in a broader context by briefly discussing de Soto’s background and by presenting a culture history of the Apalachee. Rather than portray de Soto as a valiant knight or the devil incarnate, Ewen attempts to represent de Soto as a man with personal and career motivations by discussing his family history and previous military experience. The Apalachee encountered by de Soto were part of a chiefdom that had been centered around the Tallahassee area since about A.D. 1100. While the Apalachee could not prevent the occupation of Anhaica, they did not acquiesce but instead spent the winter harassing and killing Spaniards when possible. They even burned the settlement on two occasions. While the Apalachee endured beyond de Soto and were incorporated into the Franciscan mission system during the seventeenth century, they were enslaved and displaced in the early eighteenth century by the Carolina militia and its native allies.

In “The Search and Rescue Mission” (pp. 21-47), Ewen presents the story of the unsuccessful intentional search for Anhaica and the site’s ultimate serendipitous discovery. While attempts have been made since the eighteenth century to reconstruct de Soto’s route from the narratives, the systematic search for Anhaica specifically began in 1935 when John R. Swanton, based on distances and descriptions of terrain in the accounts, placed the town in the vicinity of Tallahassee. The next serious attempt to locate Anhaica came in 1980 when Louis Tesar used the accounts to establish archaeologically recognizable criteria for the settlement. He then used the distances in the accounts and the locations of early historic roads to establish several likely locations for Anhaica within the Tallahassee area. In light of these efforts, it is ironic that the ultimate discovery of the site was quite by chance. The physical discovery of Anhaica occurred when Calvin B. Jones, an archaeologist with the Florida Division of Archaeological Research, asked permission to examine a ridge top when he noticed that it was being commercially developed. The discovery of Spanish artifacts by Jones led to a volunteer crew salvaging part of what was initially thought to be a mission site. Jones’s recognition of the fact that the site predated the Mission period led to negotiations between the state and the developers as well as the piecing together of funding for a ten month excavation of portions of the site by a professional crew. Ewen’s account of this saga provides a good insight into the roles of private landowners, the state, and the public regarding archaeological resources and how the relationships between them were negotiated in this particular case. The part of this process that has stayed with me most has been how influential concerned individuals were at critical junctures in the process. For example, if it were not for the initiative of Jones and the cooperation of the contractor, the Governor Martin site as a tangible connection to an important historic event would have been scraped away and concreted over without notice.

While the preceding section was more of a narrative in which Ewen’s personal attachment to the project was obvious, “The Archaeology of the Governor Martin Site” (pp. 51-113, by Ewen) reads more like a traditional site report. Although the Governor Martin site has been occupied intermittently for 2000 years, this section focuses on the materials associated with the Apalachee/de Soto component. The features associated with this occupation include two structures, a borrow pit, burials, and a cooking pit. The artifacts discussed in this section include Apalachee ceramics and items that would have been brought in by the Spanish, such as pieces of chain mail, a crossbow quarrel, coins, wrought nails, and glass beads. The chain mail and crossbow quarrel were critical to the association of the site with de Soto because both of these kinds of items were obsolete by the time the later missions were established. This section is useful as an introduction to Span-
ish colonial material culture for people like me who are unfamiliar with it. I especially enjoyed the photographs of items that were used by the expedition, as they satisfied my antiquarian desire to see objects that were associated with such a significant event.

When work began at the Governor Martin site in the late 1980s, it was felt that the translations of the de Soto expedition accounts in existence at that time held limited potential for scholarly research because of the liberties that had been taken in order to make them more readable. As a result, John Hann undertook a translation of the relevant portions of the documents "for the guidance of the archaeologists working on the Martin site project" (133). "The Documents Relevant to the Governor Martin Site" (pp. 117-213) presents Hann's translations of the portions of the four accounts that are concerned with the Apalachee. The Apalachee sections of the Elvas, Ranjel, and Biedma narratives are presented in their entirety, while space restrictions allowed only a part of the Incas' Apalachee account to be presented.

The final section of the book is an epilogue (pp. 214-218, by Ewen) that focuses on the state of Spanish colonial research in North America. In this section, Ewen laments the lack of public interest in the Columbian Quincentennial and prognosticates trends in Spanish colonial archaeology.

Overall, I was disappointed with this book. While it draws information from a number of sources (e.g., de Soto's biography, Spanish history, Apalachee archaeology, etc.), the book does not have a central thesis to provide continuity among the miscellaneous materials presented. Instead of synthesizing new and old data in an attempt to provide insights into the expedition or Apalachee society, the Governor Martin site is essentially added to the cultural chronology that already existed for the Tallahassee area. I think that the absence of a summary and conclusion section is indicative of the book's lack of focus. It ends with a generic epilogue that does not explicitly refer to anything from the preceding chapters.

Essentially, I was left wondering about the purpose of this book. While the research potential of the Governor Martin site is discussed, this book does not take any steps toward realizing that potential. While the Spanish artifacts were neat to see and read about, the report section is of limited utility to professionals because the data are not presented in any way that allows reanalysis. Parts of the book do seem to target a non-specialist audience. As such, it may be of interest to someone unfamiliar with archaeological field techniques, Spanish colonial artifacts, or the negotiations involved in the mitigation of archaeological resources. As best I can tell, the book's main contribution seems to be Ewen's account of the Governor Martin site's discovery and investigation. I suspect, though, that anyone who reads the book's title and expects some archaeological insights into the expedition or the sixteenth-century Apalachee chieftdom will be dissatisfied, as I was.

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Reviewed by Randolph J. Widmer

Archaeology is a fascinating paradox. One the one hand it is one of the most inherently interesting subjects there is. The general public has an enormous appetite for archaeology, witnessed by numerous news releases in the popular media as well as countless documentaries on television. To the general public, archaeology is adventurous, exciting, dynamic, and can in no way seem boring. However, as I have experienced with students in my classes over the years, archaeology makes for the driest, most tedious and boring reading there is. This can be attributed to the numerous, if not excessive, lists of dates, phases, and ceramic types, the lengthy tables of frequencies of artifacts, and charts and graphs showing the relationships of one set of variables with another. Or how about the exciting description of architecture? The detailed account of how each wall is constructed with the number of courses of stones in each wall and the number of stones in each course? Add to that the exciting description of each feature encountered in an excavation or all of the intricacies of the stratigraphic layering. Also, be sure not to leave out all of the footnotes or references required to justify the analysis. Then, when one is to summarize the behavioral implications of the analysis, the text is couched in extreme erudition or jargon. The only thing worse then reading such text, and I speak from experience, is writing it.
One can of course avoid all of the above "problems" or "awkwardness" in archaeological reporting by simply ignoring the exacting tabulated data, the precise and boring descriptive prose of an excavation, and simply provide the reader with a narrative reconstruction of what life was like at an archaeological site. However, more often than not such treatises are associated with less than authoritative speculation, unsubstantiated conclusions, or even worse, crass sensationalism. It can also be the case that such a work is so simplified and generalized as to be of little utility to the professional community and as such becomes relegated to the secondary school level.

It is therefore a refreshing surprise to come across a book that appeals to both professional and interested public alike and makes no apologies to either group for its content or style. *The Apalachee Indians and Mission San Luis* is a true treasure in archaeological publication. This work synthesizes the extensive archaeological and historical research that has been conducted over the last 16 years at the Mission San Luis, which is located within the city limits of Tallahassee, Florida. The results of these studies are presented in an extremely well-written book, wonderfully illustrated with 120 color graphics, including paintings, maps, and photographs. It is printed on glossy paper in a 7x10 format and is available in both a hardcover and paperback edition.

The book is actually two texts in one. The first is the narrative text which can stand alone as a history of the Mission and the colonial Spanish and native Apalachee inhabitants. Woven through this text is a visual narrative provided by the extensive illustrations, which are often labeled with thorough descriptions. On other occasions, primarily with the paintings, no labels are provided but are unnecessary since they are typically depictions of life in the community and are self-explanatory. What is impressive is the range of types of illustrations. These include sophisticated computer-generated contour maps of the site and its features, excavation plans, photographs of field work, and reproductions of original Spanish drawings and maps. But the real strengths of this work are the beautiful photographs of artifacts recovered from the excavations, and the wonderful paintings in a number of different and effective media styles, including watercolor, line and color and oil or acrylic styles. This in effect results in a "coffee table" product, but this is by no means a derogatory statement since it is one of the strengths of the book.

The book basically tells the story of the initial appearance of Spanish explorers in the Apalachee province, the effect it had on the indigenous Apalachee people that lived there, and the establishment of missions in Florida (more specifically the Mission of San Luis), and the ways of life of all of the different groups that resided in the Mission, including the Apalachee, the clergy, the military, and the Spanish residents. It also provides the political history of the mission system to provide a context for the changes occurring at San Luis.

Hanna and McEwan first start by discussing the geographical setting of Apalachee and the ways of life of its inhabitants. They then briefly discuss the de Soto entrada into Apalachee, its impact on the natives, and the location of the site where the first meeting took place. Next they discuss the traditional lifeways of the Apalachee, their political structure, religion, language, and social life. The second chapter deals with the historical context of the founding of the missions in the Apalachee province. This is done in historical sequence, starting with the initial mission efforts, followed by the political problems that developed within the mission system, ultimately resulting in revolts at various missions throughout Florida. The authors then discuss the distribution of the Apalachee missions in the mid-seventeenth century and the archaeological evidence for these missions.

Chapter 3 is the longest section of the book. In it the authors discuss the mission community at San Luis during its heyday. They begin their exposition with the Spanish residents, particularly the Florencio family, then discuss the Spanish garrison, and finally describe the Apalachee residents, particularly those of political importance. A genealogy of the original Florencio family is presented and a detailed genealogy of all of the family members is included in the appendix. The community organization at San Luis focuses on the identification and integration of all of the structures and plazas comprising the mission. Here, archaeological data are relied on to provide these findings. The various buildings, both European—such as the main plaza, the church, the friary, residences, and the fort—and Indian—the chief's house, the council house, storage sheds, and commoner houses—are described and illustrated with floor plans and artists' reconstructions. Other aspects of community life, like food production, acquisition, preparation, and serving; clothing and adornment; and finally weapons are also described from the archaeological evidence. The last section of the chapter focuses on religion and deals with the conversion of the Apalachee, their traditional religion, and the Apalachee ball game.
The next chapter discusses the numerous impacts that occurred to the mission between 1674 and 1704. This is the time when conversion was complete but the effects of slave raiding from the north began and population continued to decline. At the same time fleeing Indians migrated from the north while the English intensified the Indian slave trade. Also, internal Spanish demands on labor for agricultural production and land and labor for ranches taxed the Indian system, resulting in revolts and shortages of labor. Disease further disrupted the mission way of life. The fifth and final chapter presents the demise of the Apalachee after the attacks on the Spanish missions in Florida by the English. It documents the various movements and ultimate fate of the Apalachee and the missions in Florida.

In all this book creates an accurate, rich, and very realistic image of what life was like in the mission community, not only for the Apalachee, but also for Spanish clergy, military, and families. Most importantly it brings out what life was like from the various perspectives of the different groups. If I do have any quibbles with the book they are minor. For example the Apalachee are characterized as having a highly stratified social, political, and religious organization. This is not correct, since the Apalachee were never a stratified society, instead they were a ranked society. However, I understand the authors' reason for not discussing the concepts of chiefdoms and ranking because they are not readily understandable by the general public. They do, however, provide a clear and understandable discussion of the hereditary system of inheritance of political offices that is the basis of ranking and the chiefdom type of society, without using these esoteric terms.

Who should read this book? The book does not have footnotes or citations within the text. Some might argue that this automatically makes this a non scholarly work. I disagree. The lack of footnotes or references and the easy, clear, jargon-free writing in no way diminishes the academic merits of the book. A bibliography and index are provided, and the academic qualifications of the authors require no justification. I find this format extremely useful to the professional who does not actually work on the topic but wishes to be informed and learn about the topic in the easiest and most pleasurable way. This book does exactly that, and is a must reading for anyone interested in Spanish colonization, missionization, Native Americans, or the Mississippian or protohistoric archaeology of the southeastern United States. It is also a beautiful production, and I would recommend getting the hardbound copy because it is that kind of book. I only wish other books on archaeological topics could be produced in similar format.

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Reviewed by Marvin D. Jeter

James Alfred Ford, born in 1911 in Water Valley, did his first fieldwork in this state in the late 1920s. He soon established the analytical methods and cultural sequences for which he is regarded as the founding father of modern Lower Mississippi Valley (LMV) and Southeastern archaeology. He died in 1968, and is receding into the history of LMV archaeology, which diminishes problems in writing about such a strong personality, respected by many and liked heartily by some, but not all. A book-length biography is overdue, but has not been tried until now.

The producers of these volumes are not just two guys writing on a whim. Recently, they have produced a veritable avalanche of books and articles, touting "evolutionary archaeology" (EA) and generating a bit of controversy. EA crystallized around 1980, in works by Robert Dunnell, and most proponents are his former students or associates. His first major Southeastern paper (1990) outlined the rise and decline of the "culture historical paradigm," of which Ford was Alpha-male leader. Dunnell concluded that perhaps "the Southeast will rise again" theoretically.

EA emphasizes a "strictly Darwinian" approach, building "historical chronicles" and explaining them via models adapted from paleobiology. It
downplays human intention and anthropology, concentrating on variation and change in *artifacts*. Its encroachment into the Southeast has intensified: O’Brien and Dunnell edited a 1998 book on regional archaeology (reviewed in this journal, Summer 1999). In a “forum” at the 1998 SEAC meeting, O’Brien and Lyman accused some others of having “no theory.” They see Ford as a forerunner of their materialist approaches who fell into “essentialist” typological thinking, also for lack of theory.

*Measuring the flow of time (MFT)* reprints 11 early papers by Ford and colleagues. The cover photo shows him young, intense, lean-and-hungry. Pages and line drawings are clean and crisp; photos came out fairly well. The base map (p. 6) contains several errors: Fatherland is south of Natchez, not north; Troyville is on the Ouachita-Black River, not west of it; Crooks is southwest of Troyville, not west.

*MFT* opens with excerpts from Henry Collins’s 1932 report on the Deasonville site in Yazoo County, where Ford assisted. Two popular articles by Ford are followed by his first (1935) site report. Although brief, it established the stratigraphic basis for the LMV ceramic sequence. His first pottery types had designations such as 14d, which were merely nominal-level labels.

Most of *MFT* is occupied by Ford’s first (1936) major monograph. It featured an “analytical” ceramic classification system, using numbers to denote attribute combinations. The resultant “types” looked like “81;101/74;12” or worse, but this system worked. Ford expanded his sequence, but overemphasized ceramics and surface collections from sites he assumed were single-component. A glaring example is “Stone artifacts from Caddo [sites] illustrating 13 specimens; not one is even chronologically close to Caddo!” A 1938 article by Ford’s mentor Fred Kniffen used Ford’s types, but spared readers their complexities.

Next is a 1938 paper by Ford and James B. Griffin. It was tremendously significant, recording the first SEAC meeting, standardizing ceramic terminology, and introducing the “binomial” system (e.g., Marksville Stamped). A brief 1938 *American Antiquity* article by Ford cited Southwestern exemplars and his own monograph, as methods for chronological control in the Southeast.

We only get about half of the 1940 Crooks site report, by Ford and Gordon Willey. Omitted are discussions of excavations, non-ceramic artifacts, and the summary. A new reader would not learn that 1,135 human burials were excavated!

The volume concludes with the 1941 Ford-Willey Eastern U.S. synthesis. They introduced the “Burial Mound–Temple Mound” scheme, comparing cultural sequences of five “profiles” (mainly along major river valleys). However, their date guesstimates were much too conservative: they thought the Archaic lasted until 700 AD, put Marksville around 1000 AD, and Hopewell even later. As in the Crooks report, Ford took the lead in speculating about the generally northward spread of traits by diffusions and migrations.

These themes are amplified in the other book. Its cover shows the young Ford in Alaska, optimistic and determined. The epigraph quotes him on archaeology as (compatibly with EA) studying “culture” without regard for its human carriers. The authors review the development of stratigraphy, seriation, and ceramic classification in the Southwest and Southeast; interwoven are materialist-essentialist contrasts.

Chapters 2 and 3 cover Ford’s career to 1941, beginning with Collins’s strong early influences. They assumed that distinctive ceramic complexes characterized historic tribes and equivalent prehistoric “cultures.” Such discontinuous images were ultimately incompatible with Ford’s view of continuous ceramic-cultural change.

There followed work at Marksville and Macon, Georgia, Ford’s own first Louisiana projects, and enrollment at LSU. He married Ethel Campbell in 1934; excerpts from her letters illuminate those busy days and her contributions. While he worked on projects, she digested his course readings and wrote some papers for him. Ford hated sociology, but liked anthropology and geosciences, graduating in geology in 1936. Major diversions began in 1937; that crucial year included another visit to Macon, where he met Willey; a field school in Chaco Canyon, New Mexico; meeting Griffin; and starting M.A. work at Michigan. Soon, Ford also ran WPA projects in Louisiana.

Chapter 4 begins at Columbia University, where Ford was “thoroughly disgusted” by Osage anthropology. Archaeologically, Columbia led him to Colombia and Peru, eventually producing his 1949 dissertation. Meanwhile, Ford and Griffin had initiated the great Mississippi Valley survey, seeking correlations between the LMV and Midwest. Phillips, completing his LMV dissertation, asked to join.
The 1951 "PFQ" survey report almost bursts its seams between and within chapters written separately and together by these master culture-historians. Phillips was an almost fanatical seeker of discontinuities, an arch-essentialist. Griffin sided with Phillips on most issues. The authors remark that although everybody "knows" Ford emphasized seriation (and he used the term himself), he actually did not use pure seriation! Instead, he used "percentage stratigraphy" to set up graphs, then interpolated surface-collection percentage bars.

Ford's 1951 Greenhouse site report reiterated his views: types and periods were arbitrary, convenient means of subdividing continuous flows of time and culture. It didn't matter whether he started a period when a type appeared or peaked. To Phillips, the former was right, the latter wrong: periods should demarcate discontinuities.

In Chapter 5, 1952-56, the colleagues drift farther apart. Willey collaborated with Phillips on essentialist method and theory, stressing anthropological connections. Griffin had objected to one of Ford's survey chapter drafts, so Ford published it separately in 1952, setting off his famous journal debates with Albert Spaulding.

The authors emphasize Ford's innovative graphic techniques, but seem baffled as to how he came up with them. I think he borrowed them from subfields of geology. Louisiana geoscientists influenced him strongly. He and Willey said their profiles were "analogous to geological profiles" (p. 141). Ford thought a colleague "adapted [a] graph style from paleobiology" (p. 221). His stratigraphic/seriation graphics, using bars of varying widths to display a type's changing popularity, resemble the traditional "spindle diagrams" of paleontology. Ford used a complex graphic device (Figure 5.4) to illustrate diffusions of types among adjacent areas. I found in a stratigraphy text a closely analogous graphic, reprinted from a 1933 source, illustrating migrations of paleontological species. Descendant species were indicated by "prime" marks; in Ford's example, "ancestral" types got the marks.

This chapter also summarizes Ford's "conversion" to cultural discontinuities. The authors briefly mention Phillips's founding of the Lower Mississippi Survey (LMS). Work in the Yazoo and Tensas basins by LMS/ Harvard students chipped away at Ford's "continuity" assumption, but Ford's change was more directly triggered by his own work. He viewed the Baytown-

Mississippian transition at Jakerown as "invasion of new ideas ... possibly a new people" (p. 253). However, his colleagues (Phillips and William Haag) distanced themselves from his suggestion that Poverty Point culture, including early mounds, represented "an early southward thrust of . . . Hopewellian culture," reversing the direction from 1940-41. In the 1956 Poverty Point report, Ford elaborated this notion. In a review, Willey argued that the influences had probably come the other way, probably thinking of Olmec earthworks. As the authors note, that seems dubious too, but they fail to mention recent work by Joe Saunders, documenting the hemisphere's earliest known mounds—in northeastern Louisiana.

Chapter 6 deals with Ford's last years. He excavated two Arkansas sites, reporting hurriedly on Menard in 1958 and Helena in 1963. The authors speak of a "long-held assumption" that Menard was the Quapaw settlement of Ootouy, but omit mentioning that PFG had assigned that honor instead to the Wallace site. I suggest that Ford came to Menard seeking Ootouy, and "found" it. Equally interesting is what he was not seeking: nobody had yet extended the PFG revision of the Soto route beyond their northeast Arkansas study region; this was not done until 1985. If Charles Hudson is correct, Menard in 1542 was the scene of a dense population of Mississippians. I suggest that the Quapaw did not arrive until over a century later. Ford may have excavated protohistoric Mississippian remains without separating them from Quapaw materials (if any were present). Despite Helen's position between Marksville and Illinois Hopewell, Ford made no substantive regional comparisons. Instead, he proclaimed that Hopewell "must in some way be related to the Middle American 'Formative' and to the basic culture of the Andean region" (pp. 280-281).

"Absolute" chronologies were superseding Ford's relative sequences, but he thought his analytical tools could still be of use, tracking cultural connections—ideologies—across regions and areas, maybe even hemispheres and oceans. He became further inspired by claims that the earliest pottery in this hemisphere had been brought to Ecuador by Japanese seafarers. Early pottery had also been identified on the Georgia-Florida coasts. Ford wrote a 1966 article suggesting connections between Georgia-Florida and the South American "Early Formative."

Ford's final project was his larger monograph on the American Formative. His work became urgent when he learned that he had incurable cancer.
He died in early 1968, but friends got the volume published the next year. The present authors regard it as "the worst of his career. . . an unfitting testament [and] end to. . . the culture-historical period." (p. 6).

Yet, it seems ironic that we, who look askance at diffusionist-migrationist explanations in prehistory, search eagerly for "influences" on Ford and other historic figures, and retrace their wanderings. Do literate cultures have a monopoly on significant information exchanges? Or do we merely lack methods of detecting them prehistorically? Even so, migrations are making a comeback in Old World archaeology, assisted by genetics and linguistics. We hear strange scenarios of prehistoric Mesoamericans (cannibals) in charge at Chaco Canyon. In the eastern Southeast, Marvin Smith suggests short- and long-distance protohistoric migrations.

Ironically, Ford's attempts at LMV "direct historical" archaeology may have been led astray by his assumption of continuity (and superficial ethnohistorical studies), masking significant protohistoric migrations. Patricia Galloway has suggested that the Choctaw assembled from several different directions. Ford's "Tunica rim" in southwestern Mississippi is now regarded as late prehistoric Plaquemine-Natchezan; Tunicans are believed to have been well to the north then, and later migrated there. Ford and Phillips mistakenly noted northeast Louisiana as "Caddoan." Their "Quapaw phase" has been dropped. The Natchez, whom Ford regarded as recent arrivals in their historic homeland, could have significant time depth there, though other "northern Natchezans" may have moved southward after the entrada. One factor that makes such scenarios thinkable is our turning away from essentialist thinking.

Ford wanted to be "slightly in advance of the majority" of archaeologists (p.215), but was also ahead of available technology. With today's computers, his unwieldy "thumbs-and-paper-clips" seriation method (p. 325) could be replaced by forefinger mouse-clicks. His mind-numbing analytical classification system would be gobbled up by databases always ready for more, able to produce instant graphics or GIS-layered maps.

The authors' (pp. 330-331) concluding image, of Ford in a balloon, viewing the cultural stream from progressively lower altitudes, strikes me as inappropriate. I see instead a prescient Rocket Man, looking down from progressively higher altitudes at bigger pictures, inspired to higher flights of fancy—and yes, very much in need of a stabilizing system of Theory.

Dedication

This review is dedicated to the departing editor of *Mississippi Archaeology*, Patricia Galloway, in appreciation of her numerous contributions to archaeology and ethnohistory, and especially for her accomplishments in making this the finest of the state journals.

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