MISSISSIPPI ARCHAEOLOGY

Volume 26 December, 1991 Number 2

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Editorial office:
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Jackson, Mississippi 39205-0571

Typesetting and layout by Altamese Wash

ISSN 0738-775X

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Jackson, Mississippi

Julius Augustus Davies, M.D.,
An Early Contributor to Mississippi Archaeology

Timothy Pugh and Charles H. McNutt

Of the early amateur collections of Indian artifacts made in the Mid-South, the Davies Collection at the University of Mississippi, Oxford, ranks among the most important of those which are still intact. Most scholars in the Mississippi Valley who are concerned with the late prehistoric period are aware of the University's collection of material from the Walls locality. Few, however, are familiar with the man who donated the collection to the University. The following notes provide background information on this very important contributor to the archaeology of Mississippi.

Julius Augustus Davies was the son of James Baxter and Almeda Little Davies. He was born October 21, 1855, on the family plantation just northeast of Memphis, near Brunswick, in Shelby County, Tennessee. His birthplace, Davies Manor, was a two-story log structure built prior to 1807 and is regarded by many as the oldest standing structure in Shelby County. Davies Manor was placed on the National Register of Historic Places in 1975 (Davies-Rodgers 1990:10-11, 22).

"Gus" and his younger brother, William Little Davies, attended the Morning Sun School near the old Stagecoach Road (now Highway 64) and subsequently

Figure 1. Julius Augustus Davies, M.D.
Pleasant Hill School and the Brunswick Academy of Science. With a background and interest in medicine, the Davies brothers enrolled in the University of Nashville in 1874. That same year, the University of Nashville changed its name to Vanderbilt University; students already enrolled were given a choice of having either institutional designation placed on their diplomas. Julius Augustus chose “University of Nashville” (1879), while William chose “Vanderbilt University” (1878).

Subsequently, the Davies brothers did post-graduate work at New York University. William then returned home to establish a practice in Brunswick, while Julius Augustus, with a degree in ophthalmology, established an office in Memphis. Business in Memphis was slow, and soon Julius Davies moved a short distance south to Walls, Mississippi, where opportunities existed to practice medicine and also to grow cotton. He was to pursue both vocations successfully in the Walls community until a few years before his death on December 21, 1924.

Dr. Davies also invested in a number of pear trees, which he planted on several acres near his home in Walls. When Highway 61 was built through Walls, it ran through Dr. Davies’ orchard. Many of the remaining trees were destroyed by subsequent developments, but some remain behind the present-day Post Office.

Dr. Davies’ interest in prehistory was kindled by material he had excavated from a small mound that was literally in his front yard at Davies Manor. After moving to Walls, he soon began collecting material from the Walls site (22-Ds-500). He was encouraged in his pursuit by a fellow collector and student of Mississippi prehistory, Dr. Calvin S. Brown, Jr., from the University of Mississippi. Brown’s classic *Archaeology of Mississippi* (1926) was to rely heavily on the Davies material in its discussion of northwest Mississippi (cf. Ford 1987). We have no information regarding Davies’ possible acquaintance with other collectors, such as Fred Schubach of Memphis, who also collected quite a bit of material from Walls. There is no evidence that he had contacts with, or influence upon, the somewhat younger Dr. James K. Hampson, whose career had striking parallels to that of Davies (born in Memphis, an early interest in archaeology, medical school in Tennessee, post-graduate work in New York, followed by medical work and archaeological collecting in and near his family plantation [Williams 1973]).

Very little specific information is available regarding the sites from which individual artifacts were obtained; Davies did not keep records nor did he catalog his collection. Certainly the great majority of the collection (if not all of it) came from the Walls site, referred to as the “Walls burial fields” by Brown (1926:288). This was apparently the opinion of Griffin and Phillips, who studied and photographed the collection during the Lower Mississippi Valley survey (Griffin 1952:Figure 127; Phillips, Ford, and Griffin 1951:Figure 102, k-m).

The typical excavation techniques employed by Davies involved the apparently skillful use of a probe and one or two men to do the actual digging (Brown 1985). More often than not, his “field crew” consisted of his long-time servant, Mose Frazer. As Dr. Davies began his morning rounds, he would generally visit the site area and locate likely places to dig. He would then leave his excavator(s), make his rounds, and finally return to examine the day’s discoveries. The probe and pit technique hardly constitutes “ideal data recovery” by today’s standards, but it does produce a large number of artifacts, which was the widely accepted data recovery goal of his time. Use of the probe seems to have been regarded as an appropriate technique by serious collectors in the Delta at the turn of the century (e.g., Brown 1926:119). The intent behind Dr. Davies’ probing was in no sense one of financial gain, although commercial looting was already a major problem in the Delta during this period (Morse and Morse 1983). It is also worth recalling that the first even marginally professional excavations in northern Mississippi were those of Charles Peabody in 1901-02 near Clarksdale (Peabody 1904).

An interesting eyewitness account of Dr. Davies’ methods was provided to Janet Ford by the late Calvin S. Brown, III, in a letter dated August 11, 1985. With Dr. Ford’s permission, this account is quoted in its entirety.

We also once went out, with my father and Dr. Davies, on a dig, and it may be worth recording how Dr. Davies went about making his rich finds. He had devised a method perfectly suited to the alluvial deposits in which the burials at Walls lay. He had had a machine-shop in Memphis make him a probe out of a round bar of tool steel about 3/16 of an inch in diameter and some four and a half feet long. One end of this was tapered, over a distance of about two inches, to a very sharp point, and at the other end it was bent into a circle that would fit loosely around a broom-stick. Using a short stick passed through this circle as a handle, he would go about the fields probing until he hit something. He probed carefully, and had become very expert in determining, by feel and sound, what sort of thing he had hit. When he found something he would probe around within a few feet of it until he could mark out the edges of the burial on the ground. Then two of his field hands whom he had trained to do the job carefully would dig it up, as I remember usually from a depth of a couple of feet or so. Then he would take home anything that he wanted to keep, and have his men fill the hole. (His house was a very simple frame house of about four rooms, though he was a wealthy man. All the rooms were full of pots on the floors, with narrow pathways between them for essential routes. He did not clean up his finds, and
apparently never looked at them again once he found a place for them on the floor.)

In point of fact, it should be noted that the reason the finds were placed on the floor was that Dr. Davies had already filled all available shelves in his house with artifacts. Mr. Brown’s opinion, admirably placed in parentheses, that Dr. Davies “never looked at [his finds] once he had found a place for them on the floor” may well be correct, but it would make Davies a most unusual collector.

Another, very different, comment on Dr. Davies’ excavation methods is contained in a letter dated October 3, 1956, from the late Kenneth Beaudoin to Robert Rands, then at the University of Mississippi. Beaudoin relayed information that L.P. Wulf of Memphis,

who was a friend of Dr. Calvin Brown and Dr. Julius Davies, told me many times before his [Wulf’s] death that it was the practice of Dr. Davies to use a scoop and two mules and run it through the richest burial area, tearing up the burials and leaving them for the Spring rains to wash out the vessels and other grave associations. Many of the beautiful vessels in the Davies collection were without doubt uncovered in this fashion which though crude was effective.

It is certainly conceivable that Dr. Davies might have used a scoop to remove plowzone or the uppermost levels of the site, but the idea of tearing up burials and letting pots wash out in the spring rains leaves something to be desired. Even in the very unlikely event that the scoop would leave any pottery intact, it seems quite probable that other collectors in the area might not wait for the spring rains to do their work. Such an excavation method would indeed be crude, but of questionable effectiveness.

Davies evidently received generous offers from the Smithsonian Institution and other major museums to purchase his collection (Davies-Rodgers 1990). These offers were declined, and shortly before his death he donated his collection to the University of Mississippi. He shared Calvin Brown’s conviction that material found in Mississippi should stay in Mississippi.

The majority of the collection consists of some 400 vessels, and the “Davies Collection” frequently conjures a picture of a large number of pots and little else. In actuality, the collection contains numerous effigy sherds, bone tools, shell ear-plugs, stone bowls, discoidal gorgets, hoes, mortars, pipes, spades, “spuds,” celts, knives, and projectile points (Brown 1926:125-347). Davies’ interests obviously extended to all artifact classes of the ancient inhabitants of the Walls locality. Calvin Brown’s Archeology of Mississippi (1926) provided basic documentation of many of these artifacts for subsequent early investigations of the culture history of this locality (e.g., Phillips, Ford, and Griffin 1951; Griffin 1952; Rands 1956).

Dr. Julius Augustus Davies’ place in the history of Mid-South archaeology is undeniable. His fame, unfortunately, is not commensurate with his contribution. This is largely due to the fact that Davies did not publish descriptions of his material, whereas Calvin Brown did. Brown provides meticulous credit to Davies in his illustrations; later works, such as those cited above and Phillips (1970:170, 936), understandably refer to Brown’s publication but rarely to Davies himself. The collection, housed at the Department of Sociology and Anthropology at the University of Mississippi, continues to be the subject of scholarly interest. Much of this material was displayed in the “Towns and Temples Along the Mississippi: Art of the Mississippian Peoples” gallery exhibit organized by David Dye at Memphis State University in 1985. The collection is currently being studied, classified into the type-variety system, and photographed by Dye.

Davies and at least some of the other early collectors left another important legacy to professional archaeology: the nature of their interest. From experience with modern relic hunters, it is all too easy to infer that greed was the motivation of all early collectors. The fact that Dr. Davies never sold any of his artifacts would contravene this. A more likely source of his motivation is simple but intense interest. Without interest in the past and its artifacts, professional archaeology would never have taken root and many of us would be forced to seek employment elsewhere.

Acknowledgements

We have drawn extensively on conversations with, and writings of, Ellen Davies-Rodgers, Shelby County Historian, whose father was Dr. Davies’ first cousin. Without her generous assistance, this article would not have been possible. Dr. Gerald Smith, Director of the C.H. Nash Museum, Chucalissa, has contributed his expertise to reproduce the original photograph of Dr. Davies, provided by Ellen Davies-Rodgers. Final and sincere thanks are given to our most helpful anonymous reviewer who, amongst other things, reminded us that the existence of the collection at Mississippi was indeed known to many scholars, but the name of Dr. Davies was not.

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Adventures in Mississippi Archaeology: A Learning Experience

Jesse D. Jennings

The keynote speech for the 1991 Southeastern Archaeological Conference in Jackson, Mississippi offers reflections on archaeology in general and early archaeology in Mississippi in particular.

Perhaps I should offer an apology. I never have kept a diary or journal, so I am relying entirely on memory as I speak tonight. I find that I only remember the good things; like a sundial, my memory only counts the sunshine hours.

I should also say something about regional conferences such as this one. In my view they are now, and have always been, the most effective level of professional communication and exchange of ideas available. Less crowded and chaotic than SAA annual meetings, less remote than correspondence, they offer optimum opportunity for face-to-face exchange and widened perspective. I've been intimately involved with three: the SEAC (I attended the second, third, and several later ones), the PAC (attended the 5th, 6th, 7th, and others), and finally I organized the GBAC to accelerate my learning there when I moved to Utah. The GBAC and the PAC are now both anthropological conferences. Only SEAC remained pure—it retains its archaeological focus, although it now, like the PAC, has a formal structure, charges dues, and publishes a journal. The GBAC remains informal, without structure.

When I was honored by Sam Brookes' invitation to speak to the Southeastern Archaeological Conference in a plenary session, I immediately leaped at the opportunity to revisit Mississippi, the scene of many of my earlier crimes. But as I began to think about what to say that would be worthwhile—or even interesting, never mind the worthwhile—I became quite confused. What would be of interest to a group of this size of young and aging archaeologists? There would be no need to review Southeastern archaeology, because all of you know that already. Nor would an hour of reminiscences, which I could readily fill, be of particular interest to anyone but me and, perhaps, my wife. Because what happened as I began thinking about the Mississippi days was that my mind was flooded with memories, largely pleasant.
I have memories of hot dry summers and soggy winters; driving endless miles of back roads, either in suffocating dust or rutted quagmires. I enjoyed both. My thoughts then turned to my many engaging colleagues in the National Park Service, by which I was then employed. I remember, too, the little house north of Tupelo that my wife, Jane, and I enjoyed so much for so many years. I thought of friends around the state that I had made, including William McCain and Charlotte Capers, successively the directors of the Mississippi Department of Archives and History, where I spent many rewarding and sometimes exciting hours. I mention especially that to Charlotte Capers we owe the past and present strength of archaeology in Mississippi.

I finally decided that the main thing I remembered about Mississippi and my labors was that it was an almost unbroken period of daily learning. I learned more about my craft. I learned to put together, in a more coherent and larger framework, what I already knew about the Southeast. I should perhaps say that I have been doing archaeology as a professional since 1933, six years at least before I ever entered the state of Mississippi. My previous experience in archeology had involved the Hopewellian and Mississippian cultures in Southern Illinois, including two seasons at Kincaid; Mississippian and Lamar in North Carolina; St. John's in Florida; then again in the Chickamauga dam north of Chattanooga, where we dealt entirely with Mississippian materials. Shortly after that, with time out for work in Guatemala and a job as park ranger in Arizona, I spent a little more than a year as Acting Superintendent of Ocmulgee National Monument in central Georgia at Macon. There my exposure to field work was limited, although I did detect the correct Mississippian to Lamar sequence, Lamar of course being the later.

As for my qualifications for doing archaeological work, I'm afraid they were limited, although I did bring certain assets to archaeological work. I had grown up on a farm. I therefore knew black dirt from red. I knew that rodents burrowed and pushed things around in the ground. Since this experience was derived in New Mexico I had also observed a great deal of erosion. And I learned that soils, once ruptured, never heal. That's about what I knew when I began archaeology. I was, however, young and strong and eager, more or less observant, very curious, and I think my most important asset as I began archaeological work was how ignorant I was, and how aware I was of it. I regard ignorance as crucial to good field work—an attitude of ignorance toward any site, since each is unique and its contents cannot be anticipated.

My early work, as noted above, was with cultures familiar to all of you. I was, however, very handicapped. I knew nothing of palynology, taphonomy, paleoenvironment, phytoliths, optimal foraging theory, process, or sequential change. I didn't even know I was doing culture history. I knew nothing of sampling theory, C14 had not yet been thought of, and we were in the grip of the "Southwest is the oldest" syndrome. I'm embarrassed to say it, but I hadn't yet had the advantage of the enlightened guidance provided by Lewis Binford. That I was able to survive is, I think, a tribute to my endurance and, as I have emphasized above, my basic ignorance; ignorance I have consciously and deliberately maintained. Additionally I should note that my basic archaeological creed is that my conclusions have always been guided by what I saw. I was not burdened with theory or models or anything else I wanted to prove.

By the time I left the Trace in late 1946, I had come to understand a few of the things I listed above, but more importantly, I realized that the Southeast was uniform; the same generalized cultural events had occurred all over the South. The same sequence of cultures, albeit locally distinguishable from each other, had followed in the same sequence and at more or less the same times. And the Southeast was, one might say, blessed in having a deep, clear connection to the historic tribes.

I was transferred to the Natchez Trace in Mississippi in 1938 from Ocmulgee to investigate the archeology of the locally famous Battle of Ackia, where the Chickasaw had administered a sound thrashing to a French punitive force which had come up from Mobile in 1736. Please realize that I was not searching for Ackia. The location had already been established, beyond debate, by National Park Service historians out of Washington. I was merely a technician, or a handmaiden, for the historians. All I was to do was to determine what was there—what remained of the village. As the work progressed, I attempted to correlate the historical data compiled by the historians with the archaeological data. In so doing, I became increasingly aware that the village was not Ackia. The topography was wrong, and I couldn't make what I was finding fit the description of the area. I reported that the village was not Ackia. Eventually Ackia was quietly and efficiently removed from the list of national areas; perhaps I should say declassified. However, I was sure, based on the historical records, that the village I had excavated was part of the historic refuge of the displaced remnant of the Natchez tribe that fled the Fatherland site south of Natchez in 1729 after the slaughter of the French garrison there.

As I've indicated, the report of the Ackia work was not published, although I would have liked it. I think the reason the Park Service
wouldn't publish it was that the Service was actually embarrassed by the report. I had gone as fully as I could into additional historical documents and maps and discussed in as much detail as I could the errors in the historical interpretation of those maps and the fact that wherever Ackia was, it wasn't where I had been digging. I then described what I had seen at the site and what I thought about it and made the point that it was almost certainly the village, or one of the villages, at which the Natchez who fled their homeland had found refuge. In any case, the report wasn't printed. I can find no copy of it, nor can I suggest where one might be.* I would like to have consulted it before I attempted to talk to you about it. In any case James Atkinson later found Ackia; it lies about 400 yards from the hospital where my eldest son was born 46 years ago. An historic spot to be sure!

The research for the Ackia report introduced me to the U.S. War Department documents, the Letter Books and other documents in the Department of Archives, and much else of the fascinating, if saddening history of the southeastern tribes. Especially I enjoyed and admired the hardy, independent, even maverick Chickasaw, who refused to yield to the intruding Frenchmen. Lucky for us they liked the British.

With the Ackia assignment over with as far as I was concerned, I then began work on the Natchez Trace Parkway itself. The area was little known, almost blank on the archaeological map. My assignment was exactly like any CRM project of today. It was to inventory and possibly understand the archaeology of a 450 mile strip of country from the Mississippi River at Natchez to the Cumberland River at Nashville, Tennessee. The Natchez Trace Parkway, except in one or two places, followed the original Trace quite faithfully. But the Parkway was established for historical reasons. There was no thought given to archaeological processes or problems, but the people who located the original Natchez Trace and marked it for the construction of the Parkway became aware, albeit slowly, of several archaeological sites which the Trace passed. Fortunately, in the light of the injunction given to the Park Service by law to preserve and interpret national areas, there was need for some archaeological knowledge of the area. And interestingly, the situation called for what has become a very well-accepted method of learning a new country or a new area: the transect. The transect, of course, is an idea borrowed from the botanists, who use it in studying the vegetation of large or small areas. The Trace thus provided ideal conditions for learning in that it was a transect crossing at least three, and possibly four, historic Indian tribal territories, as well as a prehistoric culture sequence that extended from Early Archaic through Woodland to Mississippian and ultimately to the historic tribes.

Fortunately, except for the broad injunction to learn the archaeology of the Trace, I was operating with a free hand. I had no archaeological supervision of any sort. The superintendent of the Parkway was Malcolm Gardner, an historian with no interest in matters prehistoric. He simply gave me a map and told me to go to work. I was also issued a nearly new Ford station wagon; today they're called estate wagons. It was wooden and rickety and durable.

I therefore began a preliminary get-acquainted tour of the old Indian trail itself and wandered back and forth across a vaguely-defined ten-mile strip using what is now called an intuitive survey technique. This proved very valuable because it reinforced my conception of where to look for archaeological sites. One didn't roam the ridges where the old Trace normally ran, but the creeks, the bottoms, the major streambeds. In short, I looked where water and resources were likely to have existed in prehistoric times. I assumed, of course, that the historic and prehistoric distribution of resources would have been more or less similar for the last three or four thousand years.

Within a few months I felt more or less secure in my general knowledge of the Trace. Encouraged by the local WPA people of Lee County and Superintendent Gardner, I set up a WPA excavation program which was approved some time in the summer of 1940—two years after the Ackia project. Albert A. Spaulding joined me to operate the project. He was to be employed full-time. I, as his supervisor, planned to participate intermittently. The obvious place to begin was in the vicinity of Tupelo, where headquarters were and where the Ackia work had given us a toe-hold on the historic level. I had by that time discovered or been directed to a series of scattered village sites and the partially-destroyed Miller mound group. The WPA project as envisioned was to continue sampling the Chickasaw material as well as to search for stratification so we could push the record back in time.

I'm afraid I may have given the impression that I was the first and only individual interested in Mississippi prehistory. That is not the case. There was useful literature that I consulted in preparing myself to look for sites. Some of the literature is known to all of you. There was the extensive work of Clarence B. Moore; Thruston's work in Tennessee was then available and well known; as was the equally famous twelfth Bureau of American Ethnology Annual Report. For Mississippi itself, Calvin S. Brown's pioneer work all over the state was to hand. James Ford's intensive study of the pottery of Mississippi and Louisiana was by then available, and Collins' earlier work at Deasonville. For Alabama there was the work of Webb and DeJarnette...
in the Pickwick basin. And finally, by 1941, the WPA-derived Ford and Willey article was available. I emphasize the importance of reviewing previous work as a crucial part of a learning process.

During the months of extensive survey up and down the entire Parkway I had visited and marveled at the Selsertown [Emerald], Anna, Bynum, Pharr, and Shiloh mound sites, as well as a number of stone grave sites on the Duck River and the Cumberland. Also, I took one or two side trips to the Delta to visit famous sites, including Lake George and Winterville. I prepared and submitted to the Park Service three short reports, on Anna north of Natchez, Fatherland south of Natchez, and Lake George out near Holly Bluffs in the Delta, urging their inclusion in the NPS system or designation as historic sites of national importance. So far in the intervening fifty years no such action has been taken except at Fatherland. In connection with the Lake George site I did make a very careful plane table map of the layout, the mounds, the embankment and the ditch outside, and so on.

Steve Williams of Harvard borrowed the map a few years later and mentioned his surprise at how good the map was. He later had Spaulding, who had also visited Lake George with me on a later visit, make another map which turned out to be very similar, if not identical, to the one I had made earlier. Why was he surprised, I wonder?

I should mention in passing that I think the greatest thrill of all my years in southeastern archaeology came when I first saw Ann. The spectacular location on the bluffs overlooking the broad Mississippi Valley, the well-preserved mounds with the access or stairway ramps still so clearly visible, impressed me as no other site, including Cahokia and Moundville, ever had.

I don't recall all the many details about the actual operation of that brief project as Spaulding and I attempted to learn the sequence in the Lee County area. I do recall that WPA labor was not available five days a week. I think the people were allowed fifteen or twenty hours of work per week, so we would work two or three days and then all bets were off until the following week. You will recall that the WPA was a social welfare program, and the intent was not to make the unemployed wealthy; it was to keep them from starving to death, so work was not continuous. Actually, I think a great deal was accomplished in the approximately eight months the project lasted. When I could I would visit Spaulding at the site where he was working. Then we would meet in the late afternoon and discuss what had happened that day. Byron Long, the Chancery Clerk of Lee County, had had an interest in what I was doing from the beginning of the Ackia work. He provided an office in the courthouse with all the proper furnishings. Both he and Tom McCarthy, the county surveyor who knew every inch of the county by virtue of his employment, were very supportive. Particularly, I consulted them on matters political and social. I realized then (and even more so now) how carefully and well they advised me in my dealings with the people of the county.

As I recall it, I kept records and continued to investigate leads and make short surveying runs up and down the Trace during the operation of the project, although I did take small crews myself and sample several sites which lay to the north as I've already said. Also I recall that I had to take over from Spaulding at the Miller site because we had reluctantly transferred him to the Ferguson plantation down near Natchez where the historians wanted an archaeological evaluation of the original house and plantation structures. I began writing the report of our Lee County work after Spaulding went to Natchez. Although I had done some preliminary analysis of the artifacts and the overall plan of the structures uncovered, I did not do all of the analysis. Periodically I would drive to Macon to Ocmulgee National Monument, where the lab was functioning. The lab people continued the analysis following the instructions I gave them after my preliminary work. I was very grateful to that group for their skill and care in working on the material. When the analysis was completed, I thereupon began a report of our activity. Spaulding, when he wasn't out at the site, had, as requested, worked up excellent excavator's reports, i.e., the details of what he found at each site, and some very good maps.

After the project was terminated (I think it was February, 1941), I wrote the report which was eventually published by the Department of Archives and History in the Journal of Mississippi History.

After Spaulding left I continued the survey, working in the vicinity of Natchez and at Nashville during that period of time. After Pearl Harbor and after some soul-searching (I being a confirmed pacifist), I volunteered for service and devoted three years to winning the war, a most uninteresting time from an archaeological point of view, although I did see some very interesting places. After World War II there was no further field research other than reconnaissance and survey. I spent most of the time in ethnographical research and preparation of interpretive statements. I also remember spending a lot of time developing plans and ideas for two or three interpretive centers, one at each end of the Trace and perhaps one north of Jackson.

The most important thing I did from an archaeological point of view was to prepare a proposal for a two- or three-year excavation program. I have no idea now where the money was to come from, but it was there, and we did get the program approved. I had planned to learn about the Bynum site and Pharr Mounds; I wanted to sample Anna; to see if I could learn more about the construction history of Selsertown, since
it appeared to be in large measure a natural feature with two mounds sitting upon it; and there may have been a couple of other things. As I say, this program was approved and in the summer of 1946 I was beginning to pull together a crew to get serious about three years of field work and analysis. I was quite unexpectedly transferred to Omaha, where I entered a new world of Plains archaeology, a new world which I again set out to learn. John Cotter took over from me at the Trace. He spent a great deal of time at Bynum and some time at Selsertown and sampled some other site down there—I don’t remember what it was. I was pleased with the work, although I thought that had I been doing it, more would have been accomplished. Cotter was able to establish a local laboratory at the Tupelo headquarters, and John Corbett joined him as an assistant. Their work resulted primarily in the reporting of the Bynum site, Corbett soon being transferred to Washington. Cotter produced two or three papers of his work near Natchez. Ironically, they were published in *American Antiquity*, of which I was then editor.

As far as I can tell, my sole contributions to understanding Mississippi archaeology consist of the report of the Tupelo work, published by McCain, and a very short summary of the entire Trace published in *American Antiquity*. Those reports do not by any means convey the amount I learned and the fun I had learning it.

In quite another vein, I have been asked to comment on the changes I have seen in how archaeology is done since 1933. I’ve already mentioned some modern conceptual tools you folks have that I didn’t, but I’d like to list some fundamental changes that have come since my days in Mississippi.

First and most dramatic and possibly the most important in the overturning of old ideas was the drastic overhaul of our time scale that came in 1948 (published in 1949) from Carbon 14, a tool now taken for granted. As we adjusted to that expansion of time, I would say the next most important single concept is that of ecology-cultural ecology, that is the notion that humans are part of the total natural environment affected by, and affecting, the actions of other natural forces. Archaeologists had always been aware of environment as the arena where human life is played, but this interest was strengthened and focused—particularly I mention focused—by the emphasis on an ecological stance in interpreting archaeological data. Then by the mid-1950s the importance of settlement patterning was discovered—the idea that archaeological sites were not randomly scattered across the land but were purposefully situated. Again, archaeologists had long known that sites occurred in a patterned way; any experienced surveyor could predict within broad limits were sites would be found.

But its interpretive value, in providing clues to resource exploitation or evidence of social systems or social ordering or even politics, clues to trade networks or religious hierarchy, had not been truly perceived.

Then by 1960 in most of North America, as a result of the River Basin studies of the 1930s, much intensified in the ’40s and ’50s, the regional cultural sequences were well outlined so that there were few blank areas on the archaeological map of the continent. Therefore, in the 1960s with the cultural map largely filled in, archaeological attention turned to questions of why, when, and how, and students began to examine those questions with a much finer focus. Out of the ferment a concern with cultural process developed: why and how do the changes we see in the prehistoric cultural record come about.

Subsistence study became important; here both palynology and micro-and macro-fossil (both faunal and floral) studies have made key contributions not only to an understanding of subsistence but to paleoclimatological knowledge as well. Lithic sourcing—where did the people get their stones for tools and did the sources change through time—has revealed much about exchange and trade or task force deployment or both. Also important in understanding the exploitation of subsistence resources is the application of optimal foraging strategy (or as it is sometimes called, evolutionary biology), where caloric gain is weighed against effort expanded in procuring, transporting, and preparing dietary items.

There are many other new avenues of research that I can merely list. There is notably a very wide use of computers in doing statistical manipulation of data, and for recording and retrieving data. Taphonomy, or what happens to bones after the death of an animal and how this happens, has engaged the attention of some. Ethnoarchaeology and ethnohistory have proved rewarding. Experimental flint knapping and use of flint tools in order to determine what exactly the flint tools were used for has taught us a great deal about primitive technological survival. Sampling theory, I am sorry to say, was very important at one time in establishing site inventories, on the erroneous assumption that sites were randomly distributed rather than at locations rationally selected for cultural reasons. There are fewer sampling crimes being committed today, I am happy to note. Spatial analysis—how prehistoric people allocated space for domestic and village activities—has also interested some. The exploration of wetlands has become more important worldwide, but especially in the Great Basin and Florida.

The many research avenues I have listed are guided by a larger, more general theoretical concern with hunter-gatherer-forager behavior, which in itself is drawing wide attention.
In closing I mention a fundamental shift in the organization and direction in American archaeology. This is the Cultural Resource Management concept familiar to all of you. Less than two decades old, it has completely reshaped the discipline in many ways, far more than I can enumerate here. And I predict that the evolution of archaeological direction is still not over so far as CRM is concerned. Of course, the basis of CRM, that knowledge of the past is beyond price, providing background and perspective while inspiring national pride and inspiration for the future, is not new. It is as old as Western civilization and is but one part of the Western idea that knowledge, whether practical or esoteric, is valuable in and of itself. As such, it is certainly not new in archaeology. Witness the salvage programs in archaeology extending back into the 19th century. But through CRM, preservation, not the increase in knowledge, has become a goal in itself. The increments to knowledge that have resulted from CRM are in most cases serendipitous because CRM mitigation projects are not initiated with archaeological research or archaeological concerns in mind, but to protect and preserve any archaeological materials from despoliation or total loss. They are seen as part of our national heritage. The differences in objective, then, are obvious and wide apart. CRM projects are, in fact, concerned with modern, not prehistoric problems. And with CRM has come a great shift in emphasis. In fact, it is now possible to say that for a large segment of the CRM archaeological work force the field has shifted from collecting knowledge to disseminating it—a shift from process, postprocess, and potsherds to propaganda for the past.

Although I have listed at length the additions to the arsenal of concepts and methods and techniques and resources today's archaeologists have at their command and take for granted, I would now like to catalogue some of the increments to the Southeastern database you also draw upon and perhaps also take for granted. Each increment has come since my days in the Southeast. I list some of those best known to me. There are many more which I'm sure I should know about. I list these increments in no particular order. I would not presume to rank them in importance.

First, of course, I mention the Lower Mississippi Valley Survey (the LMS) started by Phillips, Ford, and Griffin: a survey that operated for 50 years. The LMS actually began while I was still in Mississippi; Phil visited me in Tupelo on his first trip to the Delta. Thanks to that survey, we probably now know the Delta better than any other segment of the South. Steve Williams and Jeff Brain have worked long in the Delta. Brain's work with the Tunica (among other things) is one of the best blends of archaeology with ethnology and history I know about. Griffin, of course, has contributed continuously to our knowledge of the Mississippian culture since the mid-1930s. In fact, I have the impression that Griffin attends any archaeological meeting, large or small, that's held east of Denver. There he always gives a paper which often gets published.

One of the most useful publications was Bruce Smith's analysis of the Mississippian adaptation to the riverine environment. The idea of the "externally-powered ecosystem" explains so much so simply and elegantly.

The continuing concern by many—Brown, Galloway, Muller, and others—with the Southern Cult interests me as well, especially the fact that the objects differ over both time and space. I guess Kneberg noticed it first, as did Phillips and Brown soon thereafter. The study of chieftoms and the recent restudy of the de Soto route by Hudson, Smith, Hally, DePratter, and others are particularly interesting. I've twice been involved with de Soto. I was sent to dig Peachtree in 1933 because Swanton, whom I met several times, was sure it was Guasili. It could have been; at least it was a Lamar site so it could be at the right age. In Lee County when I worked with the Chickasaw, I also checked on a site in Oktibbeha County to the southeast of Tupelo. Swanton, who told me about it, thought it was one de Soto had visited. It was a Marksville site and therefore far too old. But you can't blame Swanton. He was a linguist and didn't know Marksville pottery, but neither did anyone else in the early 1930s when Swanton was tramping the South. While Hudson and associates may be wrong, their route for de Soto is undoubtedly more accurate than Swanton's. Their identifying the chieftom of Coosa and the other provinces with the Late Mississippian (Lamar) horizon is entirely convincing to me. I personally much welcome the work of Dobyns and the archaeological support of Dobyns' thesis by Ramenofsky, as to the role of European diseases in altering aboriginal history over the Southeast as well as the entire Western Hemisphere. At one stroke they show first that disease, not European weapons, conquered the two continents, and second, they thereby explain the feeble and pauperized American cultures of the past five centuries and the empty areas of the continent.

I note the work of Peebles, Steponaitis, and others at Moundville as very valuable. The detailed findings of the Interstate mitigation work at Cahokia and the contributions of the Tombigbee Basin research represent two serendipitous but valuable CRM contributions toward completing our knowledge of the Southeast. The work of Hally, Williams, and Shapiro and others in defining and delimiting the Lamar culture is equally valuable. In my day, Lamar was a pottery type of uncertain age, nothing more. Thanks to Sears and Gilliland,
we know that the Belle Glade of Florida has space and time dimensions instead of being merely a poorly-dug site in the swamps of south-central Florida. I could mention other things, but I feel I have made my point. The Southeast is better understood in the 1990s than it was in the late 1940s.

Now, as a conclusion, I revert briefly to CRM and the fact that many of the more aggressive outreach programs are spearheaded and directed by women. That statement leads me to the final, and perhaps most important, change in our discipline. The change I note across the nation is the increasing presence of women in the work force. There are those who view this trend with alarm, considerable alarm. Although I cheerfully confess to more than a little male chauvinism, whatever objectivity I possess welcomes the females to the club. Early in my teaching career I noted that women are more persistent, smarter on average, and more realistic than men. They are today offering a new, intense level of competition for jobs and recognition—a strong and healthy stimulant to the discipline. I can even visualize a future where on some digs or field schools the cast will be entirely female, males specifically excluded, probably because they are such slow learners. Even the thought disturbs the males as they grow less complacent and feel the erosion of their presumed traditional superiority in matters intellectual.

Acknowledgement

The Southeastern Archaeological Conference is indebted to the Mississippi Humanities Council for sponsoring Dr. Jennings' presentation at its Jackson meeting on November 8, 1991.

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The Potential for African American Archaeology in Mississippi

Theresa A. Singleton

Although clearly the potential for African American archaeology in Mississippi is great, very few projects have addressed African American archaeology at all or have taken advantage of its suitability for addressing larger problems of historical archaeology and anthropology. In this paper these issues are taken up and suggestions for future projects are offered.

Introduction

Charles Sydor, author of Slavery in Mississippi, said of Mississippi, "few if any, southern states received as many slaves and exported so few" (1966:144). A steady stream of incoming slaves was an outstanding feature of slavery in Mississippi. If South Carolina was the "point of entry" for many slaves to the lower South, then Mississippi became the final stop for those slaves forced to relocate with their migratory owners or sold through the interstate slave trade. For this reason Mississippi represents, more than any other southern state, the place where diverse African American experiences came together to forge a common ground.

Scholars of many disciplines have investigated the African American experience in Mississippi, but archaeologists have been slow to follow their lead. This is unfortunate because Mississippi offers numerous possibilities for the archaeological study of African American life. Blacks have been an integral force in shaping the social and political life of the state since the introduction of slavery in the 1700s. When Mississippi achieved statehood in 1817, over 40% of the population was black, and by 1840 Mississippi had become a majority

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1 An earlier version of this paper was presented at a conference entitled "Digging the Afro-American Past: Archaeology and the Black Experience," held at the University of Mississippi in May 1989. I would like to thank the following persons for making available to me unpublished information and pointing me in the right direction: James Atkinson, Betty Boggess, Brad Chisolm, Jack Elliott, Mimi Miller, Stephen Miller, and David Peabody. I am especially grateful to Patricia Galloway, who urged me to submit the manuscript for consideration in Mississippi Archaeology.
black state, a demographic pattern which lasted for at least another century. Thus for most of Mississippi's history, African Americans numerically dominated the landscape and played critical roles in the construction of the state’s culture.

The purpose of this paper is to suggest to archaeologists that the systematic study of resources associated with African Americans can result in a more fruitful understanding of Mississippi's historical past. Archaeologists have investigated many African American sites in other states for the past two decades. This research interest is rapidly growing, and it has become a significant specialty within historical archaeology. Initially, archaeologists were only interested in recovering data on the material lives of African Americans and the extent to which an African heritage influenced those life-ways. While this is still an important emphasis, the field is slowly moving beyond this narrow concern to examine a diverse array of social relations, including class, gender, ethnicity, and power relationships.

Today, African American archaeology attempts to understand the distinctive experiences of African Americans through the study of processes of exchange and interaction among blacks, whites, and native peoples. It examines how this interaction was manifested on the landscape and preserved in material remains. Given its goals of examining cultural process, interaction, and social relations, African American archaeology should be of interest to all archaeologists.

This essay considers the research possibilities for conducting African American archaeology in Mississippi. The discussion is framed around three subareas of Mississippi, the Natchez district, the Northeastern Prairie and Ridge, and the Yazoo-Mississippi Delta (Figure 1). Each area provides a spatial and temporal context for understanding some of the major events and processes that had a profound effect on African American life in Mississippi, and some preliminary archaeological study of black occupied sites has already been undertaken in each area. Compared to most other southern states, and considering the numerous opportunities for research on this subject, however, the work done in Mississippi is infinitesimal, and it has been largely descriptive. Little attempt has been made to compare it with research undertaken in other places or to raise questions or problems upon which future work could be based. Land redevelopments poses an immediate threat to many sites; consequently, efforts to begin a

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2 My use of the term “culture” is in the traditional sense used by anthropologists, the total integrated complex of human activities. It is of course difficult to talk of a state’s culture. My point here is that African Americans contributed to the making of the cultural history of Mississippi from their earliest settlement in what became the state of Mississippi to the present.
research program in African American archaeology need to be implemented soon.

The Natchez District

African Americans first came to settle permanently in Mississippi as slaves. The earliest plantations in Mississippi were established in what became known as the Natchez District by the last quarter of the 18th century. This area roughly embraced the present-day counties of Claiborne, Jefferson, Adams, Franklin, Wilkinson, and portions of Amite and Warren counties. The French first claimed this area and founded plantations modelled on those they had established in the Caribbean. By 1720, there were 7,000 colonists living in all French controlled lands of the lower Mississippi River Valley, of which approximately 1500 were slaves (Gray 1941:65).

The best known site associated with French settlement in the Natchez district is Fort Rosalie. In 1716, the French constructed Fort Rosalie on bluffs overlooking the Mississippi River near the present location of the town of Natchez. This outpost protected French claims to the region and served as a nucleus for the town. The Natchez Indians destroyed the fort in 1729, but the French rebuilt it in 1730 after defeating the Natchez. The fort remained in French hands until 1763, when the area came under British rule as a consequence of the French defeat in the French and Indian War (Phelps 1966; Elliott 1989).

Although the national and regional significance of Fort Rosalie has long been recognized, the role of enslaved African Americans at this site has not received much attention. Slaves lived and worked at Fort Rosalie, as well as participated in the contest between the French and Natchez Indians. Slaves were captured by the Natchez Indians in the attack upon the fort and later recaptured by the French when the French defeated the Natchez. Archaeological investigations of the site of Fort Rosalie may shed light on aspects of the slaves’ life and their interaction with both the French and the Indians. Such research should attempt to examine the material evidence of African, French, and Native American contact.

Slavery was firmly established in the Natchez District under British rule with the arrival of British colonists in the 1770s. These settlers brought slaves with them and imported additional slaves as rapidly as possible (Moore 1988:73). The initial use of slaves was in subsistence farming. During the Spanish reign over southern Mississippi, 1779 to 1788, a Spanish subsidy on tobacco encouraged the emergence of a plantation economy based upon tobacco cultivation. At that time, the number of black and white residents greatly increased.

A census taken in 1784 indicated that 1,121 whites and 498 slaves were living in the Natchez District, and in 1798, the year the Spaniards left Mississippi, the population of the Natchez District was estimated to be 6,000, of whom 2400 were slaves (Moore 1988:76).

The tobacco plantations were short lived. The Spanish subsidy ended in 1790 and forced planters into bankruptcy. Planters briefly experimented with indigo and finally with upland cotton. Cotton cultivation became the mainstay of Natchez’s economy up to the time of the Civil War.

In time, the Natchez District, particularly Adams County where the town of Natchez is located, became the home of the South’s richest planter elite. Seventy percent of the farmers in Adams County owned over 50 slaves in 1860, while only 10 percent possessed no slaves. Moreover, it was not uncommon for planters in the hundred-slave category to own several plantations throughout the lower Mississippi Valley (Davis 1982:27). Absentee landownership was commonplace, as the planter elite preferred living in grand townhouses and suburban villas in and around the town of Natchez.

Very little is known of Mississippi’s eighteenth-century farms and plantations from either standing structures or archaeological remains. One of the few known structures is Mount Locust, located on the Natchez Trace in Jefferson County. This structure originated about 1780 as a one-room cabin and grew by successive additions until the time of the Civil War (Anonymous n.d.:1).

By the 1820s Mount Locust became known as Mound Plantation, with a guest house, overseer’s house, slave cabins, and other outbuildings. In 1941 the Park Service undertook archaeological investigations at the site of Mound Plantation to locate the plantation outbuildings (Phelps 1941). This early work is not only the first archaeological study of a plantation in Mississippi, but represents one of the first plantation studies to focus on the location of slave quarters and other outbuildings, not just the “big house” (Singleton 1990.71).

Numerous possibilities exist to investigate antebellum African American life on plantations in the former Natchez District. Many plantation homes are still standing, a few with surviving outbuildings. Although slave quarters tend to be the focus of investigations of African American life on plantations, a great deal of information on slaves can be recovered from outbuildings and other areas near the plantation mansion. In several recent excavations, objects presumably brought from Africa as well as objects made and used by the slaves have been recovered from the remains of plantation kitchens. More importantly, the investigation of plantation kitchens may indicate how
African Americans influenced the lifeways of the owners, particularly in the area of foodways (Singleton 1991:161). The plantation should not be thought of solely in terms of separate spaces for blacks and whites. Dell Upton, a student of vernacular architecture, has suggested that much of the plantation was under the slaves’ influence. “Although the planter might own a vast tract, his effective control was often limited to the immediate environs of his house. Even that was not assured” (Upton 1990:74). Slaves exerted their influence whenever and wherever they could, thus no area of the plantation should be ignored in the search for evidence of African American life.

Although a major interest of African American archaeology is the study of plantation life, it is certainly not the only interest. Numerous black-occupied sites in cities and towns have also been studied. Sites in and around the town of Natchez present opportunities for the examination of African American town life. A preliminary study of a free black household in Natchez has been conducted.

At the William T. Johnson house, home of a prominent free black of Natchez, limited archaeological testing was undertaken to assist in restoration of the house (Padgett 1978). Born a slave, Johnson became a barber and successful businessman. He kept a diary and recorded details of his own life and of many of the residents of Natchez, both black and white (Davis and Hogan 1954; Hogan and Davis 1968).

The archaeology at the Johnson house provided information on the evolution of the house and on some of the objects Johnson and his family acquired and used. The lack of comparative archaeological data from other antebellum Natchez sites, either black or white, makes it difficult to determine whether the archaeological record at the Johnson house reflects the lifestyle of an individual family or is indicative of the larger free black community. Future research at a variety of free black sites would provide a better context for the interpretation of the Johnson house site.

In addition to free blacks, other aspects of African American life could be studied through archaeology in Natchez. The slaves who serviced the town houses and villas of the planter elite could easily be studied, as they were often quartered in the kitchens and dairies that served these homes. Archaeological testing has been conducted at “Monmouth,” one of the better known villas of antebellum Natchez, to retrieve architectural information for recreation of slave dwellings (Boggess et al. n.d.). Slave pens, holding areas where slaves were kept prior to sale, present another possibility. After the War of 1812, Natchez became one of the busiest slave markets in the South. An excavation of a pen in Alexandria, Virginia, yielded material evidence of how slaves were treated prior to sale (Artemel et al. 1987). A third possibility is contraband barracks built for slaves escaping from plantations during the Union Army occupation of Natchez. During the Civil War, many slaves sought the protection of the Union Army and fought with Union soldiers against the Confederate Army. Similar contraband or refugee slave sites have been investigated elsewhere in the South (Singleton 1985; McBride and Sharp 1991).

The Natchez district has a longer history of African American settlement than any of the other regions in Mississippi. For this reason, it offers the greatest possibilities for archaeological research on African American life. Although some initial archaeological study has been conducted, most of it has been descriptive and not interpretative. And none of it has resulted from the development of research questions or problems which are essential to understanding these resources and placing them within a socio-historical context.

The Northeastern Prairie and Ridge

The removal of the Choctaw in 1830 and the Chickasaw in 1832 opened northeastern and central Mississippi to rapid agricultural expansion. Long before Indian removal, however, both the Choctaw and the Chickasaw made land concessions to white settlers: as early as 1801 in the Natchez district and in 1812 and 1816 in the northeastern prairie (Young 1961:185).

The northeastern prairie held a special attraction for cotton planters. This narrow belt of fertile soil running north to south through portions of Alcorn, Prentiss, Lee, Monroe, Clay, Lowndes, and Noxubee counties was excellent for cotton cultivation, while the ridge of highlands to the east was ideal for grazing animals.

An extension of the Alabama Black Prairie or Black Belt, the northeastern prairie formed the western portion of a plantation society known as the Alabama-Mississippi Black Belt. During its formative period as a plantation society, roughly between 1815 and 1840, the Black Belt was transformed from territories largely occupied by Indians to two new states that became major cotton producers (Miller 1992).

White settlers swarmed to the northeastern prairie to establish plantations on fresh lands, introduce slavery, and cultivate cotton on a large scale. Agricultural historian John H. Moore compares the settlement of northern Mississippi by cotton planters and farmers with the California gold rush of 1849. “With the aid of hordes of slaves brought in from the tobacco states and from the eastern cotton states, the new citizens of Mississippi placed the former Indian lands under
cotton cultivation in less than five years, enormously increasing the state's exports of the staple" (Moore 1988:16). By 1840 Mississippi emerged as the Old South's leading producer of cotton, outranking South Carolina and Georgia.

Of all the former plantation crop regions of the Old South, upland cotton plantations accounted for the vast majority. By 1850, 75 percent of the plantations in the South were engaged in cotton cultivation (Gray 1941:529). Yet, given the significance of antebellum cotton plantations, they remain virtually unexplored by archaeologists. Plantations of nearly every other staple have been excavated, including: rice, sea-island cotton, tobacco, sugar, even indigo. This is an area where Mississippi could take on a leading role, and the northeastern prairie would be an ideal location to examine the development of cotton plantations through time, from the formative period of 1815 to 1840 through to the postbellum reorganization.

Some archaeological work has been done on the latter period of cotton plantations. Construction of the Tennessee-Tombigbee Waterway generated two major studies of postbellum plantations in the Mississippi prairie and ridge at Waverly Plantation (Adams 1980) and Sharpley's Bottom (Kern et al. 1983). These are important baseline studies for understanding the material lives of tenant farmers. Although additional work is needed to embellish and reassess this work, the antebellum precursors of cotton plantations should not be ignored.

One research question that the study of antebellum cotton plantations should attempt to address: How did the lives of slaves from the tobacco, grain, and rice plantations change when they were brought to the Mississippi frontier? Labor historian Steven Miller suggests that work routines changed for the worse and that many of the concessions that slaveowners provided slaves in the East were not provided, at least initially, in the Black Belt (Miller 1992). Archaeological data could provide a material perspective on this question through the comparison of slave sites investigated in Virginia, South Carolina, Georgia, and in the prairie.

Studying antebellum slave life through archaeology in the prairie and perhaps most of Mississippi is difficult because many former slave settlements were reoccupied until recently or destroyed by modern agricultural methods. As suggested previously, material evidence of slave life can potentially be found anywhere on the plantation, therefore survey and testing of antebellum planter houses, kitchens, cotton gin houses, or other structures should consider this possibility.

Another avenue for the interpretation of antebellum plantation life is through the examination of plantation landscapes from the careful study of plats, historic photographs and views, standing structures, and verbal descriptions. Landscapes are consciously manipulated spaces within which a culture expresses itself, products of such ideas as social order and well-being. Folklorist John Vlach in a recent survey of plantations has shown that landscapes can reveal what the planter had in mind and how slaves may have found within his plans the means to create a landscape of their own (Vlach 1991:23).

To investigate antebellum African American life in the Black Belt may pose challenges to traditional archaeological field methods. It may require doing a fair amount of "above-ground" study of material culture along with systematic investigations of plowed fields. Regardless of the methods employed, the subject is an important one for understanding the formation and development of a major upland cotton belt.

The Yazoo-Mississippi River Delta

The Yazoo-Mississippi Delta is a large enclosed land basin between two rivers: the Mississippi and the Yazoo. Its western boundary is formed by the channel of the Mississippi River, which bends westward from Memphis, Tennessee and then turns slightly eastward near Vicksburg. The eastern boundary is formed by a line of bluffs (Langsford and Thibodeaux 1939:3). It includes the entire counties of Tunica, Coahoma, Quitman, Bolivar, Sunflower, LeFlore, Washington, Humphreys, Sharkey, Issaquena, and portions of Desoto, Panola, Tallahatchie, Grenada, Carroll, Holmes, Yazoo, and Warren counties.

The richest natural resource of the Delta is its soil. The soil is alluvial and subject to annual flooding naturally. These lands were largely unavailable to agriculture until they were cleared and levees constructed in the post-Civil War years. Some antebellum settlement did take place, but it was restricted to high bluffs bordering the two rivers. Construction of the levees along the Mississippi River permitted the cultivation of the abundant low swampy soils in between the rivers. Transportation of crops to market was greatly facilitated by the completion of the railway from Memphis to New Orleans in 1884. These conditions transformed the Delta from unused swamp into 4,000,000 acres of prime agricultural lands. By 1900 "the largest planters of the Delta were some of the wealthiest planters in the world" (Brandon 1967:viii).

After the Civil War, many black settlers migrated to the Delta from many parts of the South. They were enticed to the Delta by planters eager for labor, who filled them with ideas about the ease with which Delta lands could provide a living. Planters brought the new tenants
Mound Bayou has been extensively researched (cf. Hermann 1981), it has not been studied as a landscape. Such a study could examine the following questions: What cultural, environmental, or political factors influenced the town’s layout? How is social hierarchy expressed? What is the relationship between public and private places? Answers to these and other questions would provide a basis for understanding black towns contemporaneous with Mound Bayou that exist today solely as archaeological sites in Mississippi and elsewhere in the nation.

Certainly a major research focus of the Delta should be the investigation of postbellum tenant plantations from 1880 to the present. Many of these studies could be above ground studies of existing structures or of other aspects of landscapes. Although tenant sites are the most recent archaeological sites in the Delta, their future is most threatened because there has been little effort to identify, record, and document them for future research. Many sites are eventually destroyed in the Delta as the agricultural focus changes from cotton to rice, to catfish farms, which ultimately causes irreversible damage to all cultural resources.

Summary and Conclusions

The archaeological study of African American life is a rapidly developing research interest. Sites in Mississippi can provide the testing ground for investigating numerous questions and problems, including topics that have not been addressed in previous research, or where current information is too preliminary or site-specific, or to critique current approaches and interpretations. However, this can only happen if the archaeology conducted on African American life in Mississippi develops beyond its current direction of collecting data for architectural restoration or for salvaging sites, without research goals that move the field forward. Most African American archaeology undertaken in the United States today has resulted from contract archaeology. Those projects that have helped to set the direction of the field have done so because of the kinds of research problems that were investigated.

What also needs to change is the way in which archaeologists perceive historic sites. For example, if Fort Rosalie is thought of primarily as a French colonial site that would provide information only on French and Indian relations, then the possibility of understanding French and African interaction on the colonial frontier is lost. Similarly, if in the investigation of an antebellum plantation the plantation mansion is studied as a European American site and the slave quarters...
as the African American site, then the influence each had on the other may not be adequately appreciated.

The future of African American archaeology lies in finding ways of understanding not only how African Americans constructed their lives, but how their lives were influenced by others, and how they influenced the lives of others. In Mississippi, these complex interactions began taking shape at the time of the first permanent European settlement in the early 1700s. This process continued and was perhaps reinforced as blacks assumed the numerical majority. The challenge for archaeologists is to discover how these social relations, interactions, and exchanges are reflected in the material record.

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LATE ARCHAIC SETTLEMENT AND POVERTY POINT CONNECTIONS IN THE LOWLANDS OF SOUTHEAST ARKANSAS: AN INITIAL ASSESSMENT

H. Edwin Jackson and Marvin D. Jeter

This paper reviews evidence for Late Archaic settlement in the southeast Arkansas lowlands and its possible connection with or participation in a Poverty Point trading network.

Introduction

An important attribute of the "late Late Archaic" Poverty Point period is evidence for long distance movement of raw materials and goods. Between approximately 2000 and 700 BC a wide range of non-local raw materials and also finished items flowed into and through the Lower Mississippi Valley (Webb 1982). Many of the artifacts made from these non-local materials were deposited at the Poverty Point site, but some portion of the traded materials became part of the archaeological records of numerous other Poverty Point period sites of lesser magnitude. The presence of non-local materials, along with clay cooking balls (Poverty Point Objects, henceforth PPOs), "appropriate" projectile points, and several other artifact classes, provide markers for the effects of Poverty Point interaction from the Missouri bootheel to the Gulf of Mexico (Webb 1968, 1982).

Archaeological research in the Yazoo Basin, the Ouachita River Valley, central Louisiana, and the Louisiana and Mississippi Gulf Coast has documented site clusters that share evidence of intensive connections with the Poverty Point system. One region that has been neglected by Poverty Point research is the lowlands of southeast Arkansas (Figure 1). There is good reason to seek connections with the Poverty Point system here. These lowlands lie just on the periphery of the Poverty Point heartland, with the southern edge of the present study area less than 30 miles from the Poverty Point site (Ford and Webb 1956). They include the Bartholomew-Macon archaeological region and part of the Arkansas River Lowland region (Jeter 1982: Fig. 6-2). In this paper, we will attempt to remedy partially the neglect of
sas River occupied the Bartholomew meander belt, flowing along the western edge of the southeast Arkansas lowlands into northeast Louisiana and along the present course of the lower Ouachita River (Saucier and Kidder 1986).

The Poverty Point site, on Macon Ridge, dominated the divide between this Arkansas River course and the Lower Mississippi River. Southeast Arkansas is geographically intermediate between the Poverty Point core area and a number of seemingly important source areas, providing another reason for seeking to document Poverty Point influence in the area. Arkansas (its resources certainly, and its people perhaps) played a central role as the source of a number of materials that frequently found their way to Poverty Point and related sites. These materials include novaculite from the Ouachitas, Pitkin chert, a variety of other Ozark cherts, slate, and minerals such as magnetite, hematite, and quartz crystal (Jeter and Jackson 1991).

Evidence of Poverty Point Culture in the Southeastern Lowlands

Published information concerning Poverty Point-related components in the study area has been limited to two sites, Deep Bayou (3-Li-25) and Lloyd's Bayou (3-As-84). They are unique because they have produced trade goods, expectable projectile points, and also numerous PPOs, indicating not only interaction with Poverty Point peoples but use of distinctive technological—in this case cooking—traits.

The Deep Bayou site is near the northern end of the present study area, about 25 km from Pine Bluff. Webb (1968) mentioned the site based on his examination of amateur collections from it, and included it as the only site in southeast Arkansas on his first list of Poverty Point components (Webb 1982: Table 18). It covered about two acres (0.8 ha). Poverty Point artifacts from the site included cylindrical grooved, biconical, melon shaped-grooved, and twisted PPO varieties, steatite sherds, magnetite and hematite plummets, greenstone celts, slate and limonite gorget fragments, a red jasper bead, and quartz crystal. The chipped stone tool industry included “several hundred” projectile points (not typed), as well as microblade tools. A local avocational archaeologist reported finding about 30 Gary points and portions of three Motley-like points (AAS site records). Gibson (1983:322) has suggested that the strategic location of the Deep Bayou site, between the Bartholomew-Arkansas River and the headwaters of Bayou Macon, may have afforded its occupants some control over the transport of materials to Poverty Point. More recently, we have noted
that it was also near the abandoned Amos-Arkansas meander belt, leading toward the Jaketown site, a major Poverty Point center in the Lower Yazoo Basin of Mississippi (Jeter and Jackson 1991). Unfortunately, when the site was visited in 1985 by Jackson and John House, it had been completely destroyed by land-leveelling. Only a few novaculite flakes were collected from the surface. Direct testing of hypotheses about this intriguing site is no longer possible.

Lloyd’s Bayou, located on an oxbow of the Bayou Bartholomew meander belt in Ashley County, was mentioned briefly by Webb (1982:6,9). First reported by the University of Arkansas Museum, it was the subject of salvage excavation by the Arkansas Archeological Survey (AAS) in 1966, which documented primarily post-Archaic components. One eroded Poverty Point Object was found in a deep level (McClurkan 1968). While the evidence of Poverty Point ties is scant from Lloyd’s Bayou, it is more conclusive at another nearby site, 3-As-183, on the same oxbow lake (see below).

The Present Study

Archaeological research in the study area has concentrated on post-Archaic cultures, in particular those distributed along the Bayou Bartholomew floodplain (e.g. Jackson 1992; Jeter 1982, 1986; Rolingson 1976). Nonetheless, surveys, site visits by the authors and other Arkansas Archeological Survey archaeologists, and limited CRM activities have produced a number of Archaic collections. These in addition to documentation by the authors of several private collections provide the basis for this study. The collections described below in some detail are curated at the Arkansas Archeological Survey Station at the University of Arkansas, Monticello (UAM).

The study is limited in several ways. First, geographic boundaries were set by the UAM station’s area of responsibility and include parts of Lincoln, Desha, Drew, Chicot, and Ashley counties. Even within this restricted area, coverage is somewhat patchy, representing the activities of station archaeologists, contract surveys, and other researchers rather than a systematically designed and implemented regional survey. For instance there has been a concentration of archaeological activities along the Bayou Bartholomew meander belt, resulting in the neglect of a number of archaeologically unknown pockets, such as along Amos Bayou and between Bayou Bartholomew and Bayou Macon in Ashley and Chicot counties. Surveys of these localities will undoubtedly alter the results presented here. Finally, collection size varies greatly, from just a few flakes to several hundred pieces, and some significant portion of the intersite variability is a function of sample size differences.

Temporal Boundaries

The beginning and end points of the Late Archaic in this region are largely a matter of definition. Our study includes components thought to date between 3000 and 1000 or perhaps as late as 700 BC. Arkansas researchers (Schambach 1970; Schambach and Rolingson 1981:179; Kelley 1984:35-36) agree on a two-part division. Although Schambach has included the millennium between 3000 and 2000 BC with the Middle Archaic (e.g. Schambach 1979), it is more commonly considered part of the Late Archaic (e.g. House 1982; Schambach and Rolingson 1981:179) and is included here. West of the Mississippi Valley, this time range is associated with what Schambach (1970) has called the Big Creek culture, a complex associated with Evans and Bulverde points (e.g. Kelley 1984). Other “early Late Archaic” projectile point types include Edgewood, Ensor, and Yarborough.

The latter half of the Late Archaic is coincidental with the development of the Poverty Point culture, spanning the period from 2000 until 1000 BC or shortly thereafter. This is generally called the Poverty Point period. Corner-notched Late Archaic points (Marcos and Ellis types, for instance) may persist, but the time range is generally characterized by the prevalence of stemmed projectile points, primarily large and medium sized Gary points (Gary vars. Gary and Leflore; e.g. Schambach 1970), but also Kent and Pontchartrain varieties. Square stemmed points similar to Bulverdes continue to be made as well. In addition are points more exclusively associated with the Poverty Point culture, including Motley, Epps, Delhi, Macon, Carrollton, and Hale types (Webb, Ford, and Gagliano 1970; Webb 1982). There appears to be an increased use of Arkansas novaculite in some areas of southern Arkansas (House 1980, 1982; Kelley 1984; Schambach 1979); this may be a key attribute of the latter half of the period.

The Sample

An examination of site files produced at least 143 sites identified as potentially having Archaic components (Table 1). At least in the studied collections, there are no diagnostic artifacts indicative of pre-Late Archaic components. However, it should be noted that Rolingson (1974) reported two Middle Archaic Johnson points from separate sites in Ashley County, along Bayou Bartholomew (3-As-76 and 3-As-84), suggesting preservation of some older, pre-Late Archaic
alluvial surfaces. Both of these sites are recorded as having major occupations of the Botsford phase (Rolingson 1974) which Rolingson (1971) defined as essentially coeval with the Poverty Point period. Realizing the potential pitfalls, some of the sites in the sample were included if they lacked ceramics and if they included materials generally associated with the Late Archaic (naculite flakes, for instance), even though no diagnostic artifacts were noted or collected. A number of components along Bayou Bartholomew are also specifically noted in the site files as belonging to the Botsford phase. As far as we can tell, nearly all of these sites are known only from surface collections. Thus, there is the distinct possibility that pre-Late Archaic components may be buried, particularly along the earlier pre-Bartholomew stage Arkansas River courses (Jeter 1982:89-91).

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Table 1. Distribution of Archaic components in the study area by county.

Of the 143 sites identified as Archaic, fifty-two are represented in UAM station collections and appear in fact to be Archaic. This sample included sites represented by points or other diagnostics and those producing debitage or raw materials known to have been used during the Archaic. Among these raw materials is naculite, which as indicated above reached its peak use during the Late Archaic, as well as Boone, Pitkin, Big Fork, and Ozark mountain-derived cherts. Other materials considered to be Archaic, and Late Archaic in particular, are magnetite, slate, hematite, jasper, galena, and quartz crystal. Quartz crystal alone was not considered sufficient to identify an Archaic component, since a resurgence in its use is associated with the considerably later Plum Bayou culture (Rolingson 1982:89).

Site Distribution

Based only on those sites with clear documentation or available collections, more than 50% of Late Archaic settlements are concentrated along the Stage 6 Bayou Bartholomew meander, followed in descending order by Bayou Macon, Crooked Bayou, Big Bayou, and Chocctaw Bayou. Along Bayou Bartholomew, sites are located on the outside edge of oxbow lakes, or else in backswamp areas, often associated with smaller drainages. This pattern of site distribution contrasts markedly with those of later time periods, in which sites are well represented along the present Bartholomew channel (and elsewhere). While economic or sociopolitical factors may be at the base of this contrast, it is more likely that the Bartholomew meander belt still contained the active Arkansas course(s) during the Late Archaic period. Whether the oxbow lakes were active river channels or already cut off when occupied by Late Archaic peoples remains to be demonstrated.

In contrast to the Bartholomew pattern, nearly all of the sites along Bayou Macon are situated along the main channel, reflecting the fact that by Late Archaic times that bayou had long been an underfit stream occupying the former Arkansas Stage 4 channel. Similarly, sites along Crooked Bayou, which in Desha County drains the lowlying intermeander zone between the Stage 2 (occupied by Bayou Macon north of Lake Macon) and the Stage 5-6 (Bartholomew) meander belts, are found along its main channel.

Since we lack excavation data, we have little idea of site function(s). Impressionistically, there is considerable variability in size, suggesting both base camps and more limited-use locations, or else variably sized palimpsests resulting from repeated overlapping reoccupations. Site locations in oxbow settings are similar to those noted in northeastern Louisiana (Gregory et al. 1970; Jackson 1986, 1989), which may be an indication of similar adaptive strategies. However, the Louisiana sites often display considerable midden development, which does not seem to be the case in the study area. Lack of midden development may point to a significant difference in settlement function, limited to periodic occupational duration. One alternative suggestion (Jeter and Jackson 1991) is that Late Archaic populations alternated seasonally between upland and lowland settings. In this model, families retreated to high elevations during the warm months of the year to take advantage of cooler weather, upland game, and quarry sites. Fall to early winter migration to the lowlands would have taken advantage of the prime hunting and collecting opportunities of this time of the year. Such a model has been proposed for northeast Arkansas for roughly the same time range (Morse and Morse 1983:130-132).
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Table 2. Distribution of Archaic point types among Arkansas lowland sites, AAS-UAM Collections. Note: Local and non-local materials are differentiated in the table in the following manner: points of local material / points of non-local material; *large late Archaic varieties only included in the tabulation.*
**Projectile Points**

An examination of diagnostic projectile points in the UAM collections supports the general impression that the earliest (as presently documented) intensive use of the southeast Arkansas lowlands began in the early centuries of the Late Archaic. Of the 52 collections examined, 39 included projectile points assignable to defined Archaic types, a total of 209 points.

All of the site collections that include Late Archaic points are tabulated in Table 2. “Early Late Archaic” occupations (3000-2000 BC) are represented by Evans, Bulverde, Ensor, and Edgewood points. Bulverdes are the most common, occurring in 10 collections, with the other types represented at four or fewer sites. Of some interest is that Bulverdes and Evanses do not co-occur in any collections, in contrast to the Big Creek culture pattern to the west.

Since points similar to Ensor, Yarborough, and Edgewood types are reported from the Poverty Point site and other Poverty Point components (Webb 1982; Webb, Ford, and Gagliano 1970), their use may have extended into the latter half of the Late Archaic. Except for one case in the collections examined (3-As-178, which is represented by a single point), they occur with types associated with the succeeding Poverty Point period, leaving open the possibility that they were produced after the early Late Archaic.

All of the remaining projectile types identified in the collections are considered to be late Late Archaic in age and have been found at the Poverty Point site or at other Poverty Point components. In the sample are examples of each of the points that Webb (1982:46-49) included in his list of types considered a part of the Poverty Point material culture: Gary, Pontchartrain, Ellis, Evans, Kent, Delhi, Motley, Marcos, Carrollton, Hale, Epps, Macon, and Marshall types. The single Langtry point, similar to a Gary, should fall within the time span as well.

The most common late Late Archaic point type is Gary, including the large and medium sized varieties established by Schambach (1970, 1982). Relatively large numbers of three other stemmed varieties (Kent, Pontchartrain, and Carrollton) and two corner-notched types (Williams and a provisional type “A” similar to a Calf Creek point) are also in the collections. In terms of ubiquity, Gary points again top the list, occurring in 20 collections, followed by Bulverde and Kent (9 collections each), Williams and Carrollton (5 each), Pontchartrain, Delhi, and Provisional Type “A” (4 each), Ellis and Motley (3 each), and Hale (2). The remainder are represented in single collections.

Projectile points were produced from local brown, tan, and white pebble cherts and from materials obtainable from distant sources either via direct procurement or exchange. Slightly over half (53.5%) of the projectile points recorded in this study were produced from non-local materials. This impressive figure is a consequence of the number of Gary points in the collections (34.5% of the total number of points), of which 87% were produced from non-local material, exclusively novaculite.

Excluding Garys, two-thirds of the the recorded points were made from local materials. Only Evans, Epps, Edgewood, and Macon (the latter three represented by only one example each) were made exclusively from local cherts, but with the exception of the Motley and Webb points, the remaining types were made more often from local material. Novaculite was by far the most common material, with other materials such as Big Fork, Boone, Piklin, and White Ozark (Searcy County region) cherts occurring rarely. No projectile points of midwestern gray or Tennessee Valley Fort Payne cherts were noted, although these were represented in other artifact classes at 3-Dr-63 (see below).

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<th>Galena</th>
<th>Petrified Wood</th>
<th>Jasper Bead</th>
<th>Slate</th>
<th>Magnetc Plummets</th>
<th>Polished Granite</th>
<th>Steatite</th>
<th>Plummets Others</th>
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Table 3. Possible Poverty Point trade items in UAM-AAS collections.
Another material used in projectile point manufacture during the Poverty Point period here is Tallahatta quartzite, which outcrops in southwestern Alabama and immediately adjacent Mississippi and was transported into the Lower Mississippi Valley during the Poverty Point period (e.g. Conn 1976; Lehmann 1982:10). In a study of private collections, Jeter (1991) has identified 7 points or point fragments produced from this material. The sites include 3-Dr-111 near Dermott, 3-De-87 near Tillar, 3-As-335 on Bayou Bartholomew, 3-As-119 (an upland site, west of the study area), 3-As-219, and 3-As-183 on Grampus Lake. This last site, rather than Lloyd's Bayou, may be the source of Poverty Point materials along this oxbow. The seventh point was unprovenienced, but was apparently collected along Bayou Macon in southern Desha or northern Chicot county.

Trade Goods and Raw Materials on Late Archaic Sites

A number of the Archaic collections examined included trade items indicative of participation in the Late Archaic, possibly Poverty Point-centered exchange system. No particular pattern is apparent in the distribution of items, although sample size is admittedly quite small. Trade items recorded, in order of frequency of occurrence, included: quartz crystals, polished stone (greenstone, granite) artifacts, magnetite and hematite plummets and plummets fragments, pieces of petrified wood, slate, steatite bowl sherds, a red chert or jasper bead, a single galena bead, and a fragment of micaceous schist (Table 3). Of these, the galena most clearly represents a non-Arkansas source.

In addition to sites represented in UAM collections, AAS files mention relevant trade items from at least four sites. The Crooked Bayou site (3-Dr-11) produced two jasper beads, as well as the Gary, Yarborough, and Pontchartrain points included in the study sample (Table 2). A “stone bead,” which may or may not be of Poverty Point vintage, is reported from 3-As-177, and steatite (bowl?) fragments are reported from 3-As-144 and 3-Li-6.

Small sample size prevents decisive generalizations, but what little evidence there is suggests that proximity to the Poverty Point core area in northeastern Louisiana offered little advantage in terms of access to items that most likely derived from the Poverty Point exchange network (steatite, galena).

Non-Local Raw Materials in the Southeastern Lowlands

A third objective of the examination of survey collections was to characterize the nature and degree of reliance there might have been on access to non-local stone for chipped stone tool production. Table 4 summarizes the distribution of material types used by Late Archaic people, broken down by site. Quantitative intersite comparisons are not meaningful, since samples vary greatly in size, and some collections consist only of tools (either donated or selectively recovered from the site), while others include substantial samples of waste materials. However, the collections summary does permit several observations. First, it is apparent that based on frequency of occurrence, in-state materials—novaculite, Ozark chert, Boone chert, and Pitkin chert—are the prevailing non-local materials. Novaculite consistently occurs, recorded in 41 of the 52 site collections (79%); local cherts are represented in 47 (90%). The remaining Arkansas materials occur sporadically: Pitkin chert on 11 of the sites examined (23%), White Ozark and Boone chert on 6 sites (12%), and Big Fork chert on 3 sites (6%). Of these, only Big Fork was not represented by debitage; the remainder likely arrived at the study area as unmodified nodules or more likely as blanks. Only 3-Dr-63 (represented by a large collection) produced the wider range of material types indicative of access to the Poverty Point exchange system, with tools of northern gray, Fort Payne, and Pickwick cherts. These are not represented by debitage, suggesting trade of finished artifacts.

House (1980) has already pointed out the importance of novaculite during the Late Archaic; his observation was based on Arkansas River lowland sites just north of those being considered here. At these sites novaculite is often equal to or in some cases actually exceeds local chert representation. Although difficult to demonstrate with these particular collections, it appears that there is a north-to-south decrease in the representation of novaculite in collections, with the greatest amounts in Lincoln County and the least amounts in Ashley and Chicot counties. We have decent debitage collections from six sites in Lincoln and Desha counties (in the northern end of the study area). Of these local chert debitage exceeds novaculite in only two assemblages: 3-Li-85, which has a significant later component, and 3-De-47. Ratios of novaculite to local chert are as high as 17 to 1 at sites in northern Lincoln County. In Drew County the ratio at 3-Dr-35 is 9 to 1. In contrast, at the southern end of the study area the ratio at two Chicot County sites, 3-Ch-40 and 3-Ch-41, is 5 to 1 and 0.125 to 1, respectively. Three sites in Chicot County, represented by admittedly small collections, had no novaculite pieces. It appears that access to novaculite was to the northwest, presumably via the Arkansas River valley; reliance on a redistribution mechanism centered at the Poverty Point site is unlikely.
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<th>Big Fork</th>
<th>Quartzite</th>
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Table 4. Presence of material types in AAS-UAM collections. T= Tool; D=Debitage.
Other Poverty Point Evidence

Several sites have produced additional evidence of participation in the Poverty Point sphere. One site is 3-Dr-63 (Woods Place No. 3), northwest of Tillar, Arkansas, which appears to represent an example of a Poverty Point-related habitation. The roster of projectile point types includes a comparatively wide range of Poverty Point-related ones (Table 2). Site occupants appear to have had access to a wider range of stone for flintknapping than did their neighbors and ancestors, including several varieties assumed only to have made their way to the Lower Mississippi Valley during the Poverty Point period. Several flaked stone “hoes” and roughly two dozen baked clay balls, mainly biconical and biscuit-shaped, also demonstrate rather direct interaction with Poverty Point peoples. Finally, Poverty Point contact is indicated by the variety of trade materials recovered from surface collections (Table 3). A fragment of an unfinished magnetite plummet suggests that the site’s inhabitants had been involved in at least some trade good manufacture. Also included in the collection is a rather atypical spherical plummet made from siltstone.

Another of the sites represented in the UAM collections, 3-De-45 (Harris Place), produced a collection of bifacially flaked drills or borers which may be evidence of production for trade. While these tools are present in certain other collections, they are never particularly numerous, yet from 3-De-45 there are eight in a relatively small collection. All but one are made from novaculite. The single Motley point in the UAM collections was found at 3-De-45. Several other sites represented in private collections may have Poverty Point components, or at least evidence of interaction with the Poverty Point exchange network. One that is particularly interesting is the Stark site (3-Dr-111) near Dermott. It is situated on a remnant natural levee associated with an abandoned Arkansas River segment along the Bayou Bartholomew meander belt. The site has produced an array of quite incredible magnetite plummets, not at all unlike some of the very finest collected from the Poverty Point site itself. In addition to finished pieces are semi-fabricated and broken examples, suggesting that at least some steps in manufacture occurred at the Stark site. Other Late Archaic artifacts collected from the site (it is multicomponent) include several projectile point types associated with the Poverty Point culture, including one made from Tallahatta quartzite, as mentioned above.

A second site represented in private collections is 3-As-183 (In Between site) on Grampus Lake, near the Llloyd’s Bayou site. In addition to a number of Late Archaic points and novaculite debitage, are polished hematite plummets, quartz, and a dart point tip of Tallahatta quartzite. A third site, Reid (3-As-219) located on an old oxbow west of Wilmot, produced novaculite debitage, Late Archaic points, a Tallahatta quartzite point, and two magnetite plummets.

Finally, test excavation of a recently recorded small mound (3-As-379) located on an oxbow lake on the Bartholomew meander belt, a few miles north of the Louisiana state line, has produced cross-grooved, melon grooved, and biconical PPOs as well as novaculite debitage. Information is not yet available regarding site size, presence of habitation, or other artifacts. However, if the mound is indeed of Poverty Point age, it will certainly cause a redrawing of the Poverty Point core area boundaries.

Discussion

In general, despite the relative proximity to the center of the Poverty Point universe and the direct articulation via several watercourses, southeastern Arkansas peoples, with only a few notable exceptions, appear to have reaped few direct material benefits from participation in the Poverty Point system. Mundane needs, in particular stone for flintknapping, were provided either by regional exchange networks or direct procurement, but clearly without any external assistance from Poverty Point brokers. In general, raw materials from sites demonstrate dependence on south-central and northern Arkansas sources. In the southeastern Arkansas lowlands, distribution suggests a fall-off from north to south, suggesting that whatever provisioning mechanism existed, it operated down the Arkansas River. The evidence suggests that a few traders and their families acquired a wider array of materials and artifacts and even took on some of the Poverty Point cultural trappings, but their neighbors maintained a distinctively local Late Archaic lifeway, benefiting only from the occasional trinket obtained by local trade.

A couple of factors related to the coarse-scale chronology we have used may mediate the impression of an overall low level of interaction, since Poverty Point period sites are treated here as more or less contemporary. The first is developmental; if interaction was limited mainly to the latter half of the second millennium BC when Poverty Point interaction appears to have reached its climax, then earlier sites would lack non-local goods. Second, even within a more limited time frame, we might expect considerable fluctuation in interaction, yet there would be a constant rate of creation of new sites. Both of these factors would contribute to a greater representation of sites that lacked Poverty Point diagnostics.
Conceptual and Perceptual Issues in Poverty Point Archaeology

Many aspects of the Poverty Point culture continue to baffle archaeologists. For instance, given the widespread, yet discontinuous, distribution of Poverty Point traits from the Missouri bootheel to the Gulf Coast, just what mechanisms were responsible for binding these communities together? While intensity of interaction may be indicated by representation of artifact types or raw materials, the rules that determined inclusion or exclusion remain unidentified.

Another set of questions concerns the Poverty Point site itself. Without a doubt, at 1000 BC the Poverty Point site was unsurpassed by any site in the Eastern Woodlands, in terms of size, architectural complexity, and sheer number of at least periodic occupants. What were the functions of the Poverty Point site within the context of the local settlement system, especially as it had to do with local political integration? Specifically, does the site represent the administrative center of a chiefdom-level polity (e.g. Gibson 1974, 1987)? If so, what was the domain of this political elite? A convenient answer would be to include Poverty Point components located in northeast Louisiana. However, even here evidence suggests that participation in the Poverty Point system was uneven. Kidder’s (1986) discussion of Bœuf River Poverty Point components is an example of how difficult establishing political boundaries can be.

A second unresolved issue is that of the role of the Poverty Point site and its residents in orchestrating interregional interaction across a much broader area, as evidenced by the archaeological distribution of non-local goods and materials throughout the Lower Mississippi Valley and to a lesser extent elsewhere. Recently one of us (Jackson 1991) has argued that the latter function may offer a more reasonable explanatory model for the evolution and operation of the Poverty Point site. Using a model derived from a cross-cultural ethnographic examination of hunter-gatherer trade fairs, Jackson suggested that Poverty Point’s primary purpose was to provide individuals or groups from unrelated societies the opportunity and necessary neutral ground to participate in the Poverty Point exchange system. Arguably more important for most participants than the acquisition of material goods was the opportunity to establish and maintain a multi-societal network of alliances that could be called upon in times of need. A relatively small resident population could have acted as overseers, similar to the function served by inhabitants of another strategic location, the Dalles region on the Columbia Plateau (e.g. Spier and Sapir 1930). Importantly, such a model requires no assumption of a chiefdom level system centered at Poverty Point.

A model based on trading may be useful for accounting for the interregional distribution of Poverty Point goods, but in and of itself, it is insufficient to account for the archaeological record of the Poverty Point site. Whatever was going on during the Poverty Point period went on to a great extent at Poverty Point. In addition to impressive communal effort in monumental construction, the people who lived at or visited the site manufactured, used, and lost or perhaps purposefully disposed of a large number and wide range of artifacts made from exotic materials, in particular chipped and polished stone artifacts.

The intensity and periodicity with which these artifacts entered the archaeological record is at present uncertain, since chronology and context are still only grossly understood. Nonetheless, whatever might be argued regarding the Poverty Point site and its permanent or temporary occupants as redistributors of goods, the site served as a repository of the majority of materials that must have moved about the system.

This brings us back to the question of accounting for the Poverty Point evidence in southeast Arkansas. Turning back to ethnographically documented fair-centered exchange systems, social groups were bonded through exchange and ritual obligations perpetuated differentially by individuals (Jackson 1991). The degree of participation was variable among different members of a society and probably also from year to year. If social bonds rather than material acquisitions were the goals of participants, it is likely that only small, comparatively minor amounts of goods were transported back home at the completion of the fair. Beyond the borders of the Poverty Point society in the northeast Louisiana core area (regardless of the degree of social complexity, local residents there benefited from the influx of raw materials and goods into northeast Louisiana), diagnostic Poverty Point goods and materials probably had rather unsystematic distribution, reflecting the variable degree of participation in the Poverty Point trade fair and the effects of secondary trading at the local level. Back home, those individuals most involved in the system would have been in a position to enhance their local prestige through exchange. They may also have symbolically marked themselves as participants by assimilating some of the more mundane characteristics of Poverty Point culture, such as Poverty Point earth oven cookery using clay balls (not unlike the propensity for Southwestern anthropologists to wear turquoise jewelry). This likely accounts for the evidence of Poverty Point culture at Lloyd's Bayou, Deep Bayou, and the few other sites with significant evidence for Poverty Point ties. Otherwise, the introduction of Poverty
Point artifacts and materials was intermittent and of minor consequence. It is unlikely that, overall, the Poverty Point interaction sphere had much of an impact on the populations living in the Arkansas lowlands, since the vast majority of the non-local materials that do show up derive from Arkansas sources. Access to these materials, possibly as they were moved through southeast Arkansas toward the Poverty Point site, appears to have been of much greater significance to local residents.

Acknowledgements

The collections research on which this paper is based was conducted while Jackson was station archeologist for the Arkansas Archeological Survey at the UAM station. Both authors acknowledge the support and assistance of the Survey’s staff. Thanks to Susan Scott who read drafts of this paper and also to Patricia Galloway for her editorial assistance.

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Lost and Found: The Sixth Ridge at Poverty Point

Jon L. Gibson

The ridges in the north sector of the Poverty Point enclosure were built over a filled-in depression. Recent excavations have shown that the depression was probably a gully, similar to the small bayou which presently flows through the same area, and was not a cove of an ancient now extinct lake as was presupposed. The depression was covered up and ridges were built over the ravine. Six ridges were erected, just as in other sections of the enclosure. This paper presents the dirty facts on which these findings were based.

What's the Problem

Back in 1983, I decided there might have been a lake lying alongside the Poverty Point in 1100 BC. There is no lake there now, only a small picturesque bayou and some beanfields on the cleared swampland on the other side. There were certain compelling incongruities that led me to think that Poverty Point might have been lakeside property. First, there was a layer of mud visible in the bluff bank of Bayou Macon, a layer we came to call "Deep Six." Glen Greene excavated part of that layer in 1983 (Greene 1986). The unusual thing about "Deep Six" was that it looked, smelled, and felt like the kind of mud that occurs around the edges of slow-moving bayous, swamps, and lakes. Now the mud in itself was not unusual; the unusual thing was that the layer seemed to be at too high an elevation to have formed under the base level conditions which exist in the swamp today. In other words, given the present-day water levels at which fine-grained sediments drop out, it did not seem that mud could have been deposited at that elevation.

A clue to what might have happened finally surfaced when we noted an old natural levee ridge abutting the Macon Ridge bluff about a mile below the site. That ridge branch off the meander belt ridge now occupied by Joe's Bayou about three miles to the east. Arville Tuchet and Thurman Allen, our soil science associates, told me that that ridge could have acted like a dam and caused water to pond behind it. That would have raised water levels above stream levels, and since the elevations on the lowest part of that ridge were well above the level of "Deep Six," the lake idea offered a perfect solution for our dilemma. Roger Sauzier and Glen Greene, however, had doubts about such a scenario.

One of the problems with explaining the nature of Poverty Point, particularly for those who felt the site was home to a large resident population, was how to feed all those people without farm produce. Squash was the only domestic plant known from Poverty Point contexts in the Macon Ridge area (i.e., the Copes site, see Jackson 1982, 1986), but practically everyone agrees that Poverty Point subsistence was not based on farming but on hunting and gathering. Although archaeologists are now beginning to acknowledge that sociopolitical complexity can and has occurred in hunting and gathering contexts, it is still necessary to offer explicit evidence for how those populations were supporting themselves, especially the kind of large populations that some of us imagine for the Poverty Point site. This problem is not nearly so acute if permanent populations were small and could be provisioned by virtually any reasonable system of seasonal exploitation geared to the natural bounty of the land. Opinions on the population of Poverty Point during its heyday around 1100 BC range all the way from zero (no permanent residents at all, only occasional visitors) to several thousand year-round occupants.

Ed Jackson (1982, 1986), argues convincingly that a large permanent population at the Poverty Point site just could not have sustained itself, given the same kind of subsistence base he reconstructed for the nearby Copes site, unless the hunting and gathering territory exploited by the Poverty Point people was exceedingly large, larger in fact than the distances between it and its neighboring (and presumably contemporary) hamlets. He reasons, quite appropriately, that such a large hunting ground, one that not only impinged on but actually enveloped the hunting grounds of its neighbors, is simply not the usual state of affairs in ethnographically known hunting and gathering situations. So Jackson concluded that Poverty Point could not have had a large permanent population; he envisioned the Poverty Point site as a "fairground," where tribes from near and far met periodically to trade (Jackson 1991).

I saw the lake as offering another solution to the issue Jackson raised about the size of Poverty Point's hunting and gathering grounds. The lake would have been a veritable cornucopia, and it would literally have been at Poverty Point's doorstep. The biotic richness of Mississippi backwater lakes is well known; Jackson acknowledged the enormous biotic productivity of such environments. When a food supply is as abundant, as concentrated, and as continually
replenished as it would have been in one of these backwater lakes, a much smaller region can suffice to provide subsistence. So the idea of having a lake alongside Poverty Point and having a major portion of the subsistence derive from it was quite an appealing idea to me. The work reported here was thus an effort to find some independent evidence for or against the lake idea.

Besides the hypothetical lake, I had another problem. The Poverty Point ridged enclosure consists of six concentric artificial embankments, built in a partial elliptical configuration. Today, however, there is no obvious evidence for the sixth, or outermost, ridge in the northwestern and northern sectors of the enclosure. The enclosure is divided into sectors by the so-called "aisles," the architectural corridors which cut through the ridges of the enclosure at five spots (Figure 1). My problem was this: Did the builders simply not finish the northern extremity of the sixth ridge, or was the ridge destroyed by the gullying which created the small bayou in this section of the site? The last segment we could find of the sixth ridge was in the northwest sector, where it ran right up to the lip of the scarp marking the little bottom where the bayou flowed. Over in the north sector the sixth ridge was not apparent at all, but there were two places where the distance between the fifth ridge and the bayou bank was far enough for remnants of the sixth ridge to be found, if it had been built. I wanted to check these spots out to see if my faith in architectural symmetry could be restored.

These were the main problems to be investigated; the lake idea and the "lost" ridge question guided this research. The answers to our problems lay in the dirt, and the way the dirt helped us resolve these problems is the focus of this statement. The keys were soil textures, Atterberg Limits, and lowly concretions.

**By the Shores of Gitche Gumee**

Our excavations in 1983 had confirmed the presence of a depression beneath the ridges in the north sector of the enclosure (Gibson 1984). Glen Greene tapped this depression in his "Deep Six" excavations, and I had found it beneath N4. As we began to inquire after the nature of this depression, we realized that Bill Haag (1986) and Carl Kuttruff (1975) had also encountered this feature. Just how big was it? What was it? Could it possibly have been an arm, or cove, of the ancient lake we envisioned? The only way to find out was by more testing: A good way to confirm the lake hypothesis would be to discover floodplain clays deposited on the floor of the old depression. The reasoning was simple. If a lake had existed and if the depression had been connected...
to the lake, then lake bottom sediments should have accumulated there.

The 1989 excavations encountered clay (Gibson 1990), but it was not floodplain clay; it was not the buried Sharkey or Alligator clays, which should have been found if the depression had been connected with a big lake to the east. Instead the clays were part of the core of the Macon Ridge, an eroded Pleistocene terrace. The clays were capped by native loess in some places, while in others they were capped by artificial fill dirt. This stratigraphic relationship showed that the clays had been deposited before the Peoria loess accumulated, and the loess apparently accumulated around 10,500 B.P., well before there could have been a lake along the Macon Ridge front. So the lack of floodplain clays suggested two possibilities: one, that the depression was not part of the lake, as we had hoped; or two, that there had been no lake out there to start with. One thing our investigations did reveal was that the depression was only about 150 m across, a little small for a cove or arm of a lake and more compatible with the size of a large gully much like Harland Bayou, which presently cuts through the same area of the site today. Thus there was no independent confirmation of the lake we were seeking. Instead, we did know there was a big gully here, a gully filled in during ridge construction.

Red, White, and Brown: Artificial Fill, Clay and Loess

Our excavations affirmed Kuttruff’s (1975) and Haag’s (1986) discoveries that ridge construction in the north sector was carried out by simply piling up dirt at the constructionally active ends of the ridges. The same technique also seems to have been used in some parts of the northwest sector of the ridged enclosure (e.g., NW4), but not in others (e.g., NW6 midsection). Of interest is the use of both clay and loess as fill. These two materials have distinctly different properties, which would have affected building techniques, and almost certainly must have been selected because of those properties. The clays used in building were Macon Ridge basal clays, those that lie below the loess mantle on the old Pleistocene terrace proper. The builders didn’t need to dig a deep borrow pit to reach the clays, which would have been exposed on the surface in any deep gully or along the bluff which marks the eastern extent of the site.

The loess was used to form the bulk of the ridge masses, and the clay occurs in thin bands or lenses on top of the loess layers. Loess is characterized among other qualities by the capacity to turn easily from a solid into a liquid, called low liquid limits (Touche and Gibson 1990). Clay, on the other hand, has high liquid limits, which means that it takes an enormous amount of absorbed water (by weight) to turn clay into a liquid, far more water than it takes loess, which is a silt loam. Clay also has high plastic limits, which means that it takes large quantity of water to change clay from a solid into a plastic. Therefore on both counts clay would have made an excellent sealant, or protective coating, and covering the loess piles with thin layers of clay would have been a perfect solution for the melt-down problem, as it would have kept water from seeping into the silt loam inner mass of the ridges and causing the ridges to collapse. The limits, liquid and plastic, are called by solids experts the Atterberg Limits, and the ancient builders must have been acutely aware of these properties of fill dirt, selecting for them accordingly.

Clay and silt loam are quite easy to distinguish texturally. Problems arise in distinguishing the intermediate textural classes, such as silty clay loam and silty clay. Even the loess, which is a silt loam, has a textural silty clay loam horizon called the argillie, or subsoil. I have found that color is an excellent indicator of these textural classes at the Poverty Point site, and that, in turn, is a pretty good indicator of the source of the material used for fill dirt or of the nature of the ground you are examining. Red, white, and brown are indeed color keys to recognition. Reds in the 5YR and 7.5YR Munsell ranges, “whites” (really olives) in the 2.5Y and 5Y ranges, and even yellows in the 2.5YR range are nearly fail-safe markers of Macon Ridge basal clay, the material that was used for construction stabilization or which helped stabilize ridge masses even if it had not been deliberately chosen for that purpose. Browns, on the other hand, in the 10YR and less often in the 7.5YR ranges, are the colors of loess, including the silty clay loam and the silty clay which comprise the argillie horizons of loessal soils.

Besides textures and colors, there are two other helpful ways to distinguish artificial fill dirt and native undisturbed soil. The simplest way is to measure the thickness of the topsoil (combined A and E horizons) above the argillic horizon. The argillic may be treated like a marker bed. Even when colors and textures are gradational in a soil section, the argillic stands out because of the abrupt to clear boundary which can be easily felt with an experienced trowel, even if not the practiced eye. Once the top of the argillic horizon is located, the thickness of the material above it to the ground surface can be measured; if it exceeds the normal thickness of the native soil, it is relatively certain that dirt has been added somehow. It could be artificial fill, although colluvium, alluvium, and other potential sources of deposition need to be carefully considered first. Topsoil thickness can be found in various soil survey bulletins.
The other clue to telling artificially deposited and natural dirt apart lies in the vertical distribution of artifacts. These facts are not usually available until after analysis, so this method is not as helpful in the field. In natural unaltered soils, the number of artifacts usually shows a steady decline with depth or a steady decline followed by a jump at the top of the argillic horizon. This pattern is restricted to silt loam soils, as far as we know at Poverty Point. The jump in frequencies at the base of the topsoil top of the subsoil border is due to the low liquid limit factor discussed earlier. Artifacts, especially the heavy ones, simply sink down into the silt loam soil when the soil is very wet and stack up against the relatively impervious clay layer, which doesn’t liquefy readily (Touche and Gibson 1990). The “heavier artifacts drop lower than lighter ones” phenomenon has serious implications for chronological interpretations based simply on the principle of superposition.

On the other hand, if artifact numbers show no steady falloff or have two, three, or more peaks, none of which correspond to the top of the argillic, then fill dirt may be indicated. If the old pre-construction ground surface supported occupation, then there should be a frequency bump corresponding to it in the stratigraphic profile. If some of the fill layers were taken from occupation areas nearby, then those layers should also have more artifacts than layers which were comprised mainly of subsoil or of dirt taken from unoccupied portions of the site.

Did You Find the Lost Ridge?

With all these indicators to help us distinguish fill dirt from natural soil, we looked at our stratigraphic profiles and artifact inventories to see if we could come to some reasonable determination as to whether or not the Poverty Point builders had ever completed the northern part of the ridged enclosure, had erected the full complement of six concentric rings.

In the northwest sector, the outer three ridges (NW4, NW5, and NW6) ended abruptly against the little bluff that marks the Harland Bayou bottom, so we couldn’t tell whether they had ever continued on around or not. The north sector only bore five ridge segments. There was no apparent sixth ridge there, but we must remember that the bank of Harland Bayou ran right up against the fifth ridge in many places, raising the prospect that the other ridge had just caved into the bayou.

This was a real puzzle. The ridges weren’t there, and there were no apparent remnants out in the Harland Bayou bottom—no slump blocks, no residual artifacts, nothing that suggested that the ridges had once crossed this area where the gully now runs. How could we ever tell whether the ridges were there when we could find no vestiges of them? The lowly concretion gave us our best clue.

Concretions are hard nodules of concentrated mineral compounds or cemented soil grains which form naturally under wet soil conditions. These little things were superabundant in upper soil horizons in the northwest sector of the ridged enclosure (Gibson 1990), sometimes more than ten thousand concretions being recovered from a single 10 cm level in a one by one meter test unit using quarter inch screens. So many concretions means that a low wet spot was present on the landscape here. But today the same area is well-drained. The bluff allows runoff and ground-water to drain and seep quickly down into the Harland Bayou bottom, where it runs into the gully-like channel and is carried into the Bayou Macon, which flows at the foot of the Macon Ridge bluff about 400 m to the east. These contrasting situations, poorly drained when the ridges were built and well-drained now, mean that the creation of the low scarp which now facilitates drainage of this area occurred in recent times, or at the least, it occurred after the ridges were built. So I think there is a strong, albeit indirect, case that the full complement of six ridges was indeed built in this part of the site.

But what about the sixth ridge in the north, the one that initially prompted our research interests? There were relatively few concretions in this area, meaning that the original landscape was well-drained, a fact borne out by the absolute elevations on the natural ground surface, which show the land to have been nearly as high as some of the ridge segments that were constructed across the nearby depression. We punched several solid cores and found no basket-loaded dirt in two spots which were far enough away from the fifth ridge to have included at least the lower flank of the sixth ridge—if it had ever been built and if it had been spaced 43 m away from the fifth ridge, as seemed to be the standard spacing elsewhere in the enclosure. The outer ridge appeared not to have been finished in this part of the site.

Almost as an afterthought, Thurman Allen, my soil science partner, and I made a third test located down a sharp narrow slope leading to the junction of Harland Bayou and Bayou Macon, in an area where the coring truck could not go. I found a patch of dark dirt and some clay ball fragments; Thurman used a hand probe and punched a hole. The top part of the dirt in the probe looked like midden, probably washed downslope from the fifth ridge on the higher ground above us. The third barrel-full of dirt in the probe yielded a mixture of textures and colors, including some basal clays, evidence of basket loading that does—after all—point to the existence of the sixth ridge in the north.
Mississippian and Woodland Paste Varieties from the Vicinity of Starkville, Mississippi

Richard A. Marshall and Rex D. Poole, Jr.

Certain of the late Mississippian, Protohistoric, and early Historic Amerindian remains in and around Starkville, Mississippi were the products of a common set of developmental trajectories, here designated the Starkville Archaeological Complex. Understanding this complex will, however, require that the materials be ordered by using previously defined and newly created pottery types. This essay begins that task.

Introduction

Insofar as the late Mississippian, Protohistoric, and early Historic Amerindian remains in and around Starkville, Mississippi were the products of a common set of developmental trajectories, they are a part of a Starkville Archaeological Complex. Further, the inter- and intrasite content variability displayed by members of the complex should, if Swanton (1946) is correct, illustrate aspects of Chakchiuma ethnogenesis. To draw the necessary information from these remains will, however, require: (1) a comprehensive and systematic survey of the area and (2) some assurance that the temporal order assigned to the various kinds of remains is correct or very nearly so. The first prerequisite, a comprehensive and systematic survey, can be addressed by using the partial and ad hoc reconnaissance thus far conducted as a springboard for a more exhaustive, intensive, and systematic future effort. The second prerequisite, a believable temporal ordering, may be approached through the creation of “historical types”—types with specific chronological placement—from the ceramics now in hand. The first prerequisite will require funding for additional work. The second, i.e., the creation of historical ceramic types, is the subject of this essay.

Southeastern archaeologists commonly reduce the ceramic variability to be accounted for by using the idea of types and varieties. All such types and varieties are, however, descriptions in which the analyst identifies as significant some of the many properties of things or groups of things and uses these to the exclusion of all others. The properties chosen during this process and the reasons for choosing them are therefore of fundamental importance. The types and
This approach, namely using temper and manufacturing technique for broad temporal order and elements of vessel morphology and decoration for fine scale distinctions, seems to work reasonably well for most of the Southeast. Unfortunately the Starkville complex materials do not segregate so cleanly or clearly as the rest because many of them are undecorated, and crushed fossil shell, sand, and grog are all used as tempering agents in late Mississippian, Protohistoric, and early Historic pottery (Marshall 1990). This makes them hard if not impossible to separate from their Woodland predecessors by qualitative judgments about kind of clay body inclusion. It is our contention, however, that greater attention to the details of tempering as reflected in quantitatively different patterns of clay body inclusion can be used for creating the necessary fine scale time and space distributions. To this issue we now turn.

In keeping with previous archaeological practices we shall refer to a given pattern of clay body inclusions as “paste.” Paste variety is emphasized in the following discussion with the intention of “breaking down” paste kinds prior to recognition and/or identification of pottery types (Phillips 1970). Reference to pottery types herein will be minimal and it will be assumed that the definition of specific pottery types will ultimately assist in the delineation of cultural, chronological, and spatial differences within the Woodland and Mississippian complexes in and around Starkville. It will be further assumed that minor variations may assist chronological definition within single- or multisite artifact inventories. It should be noted too that the Mississippian stage paste descriptions offered here are based in part on data derived from unpublished papers that discuss the surface ceramics from the Lyon’s Bluff archaeological site (22-Ok-520; Marshall 1986).

Woodland Pastes

From a qualitative perspective Woodland pastes are tempered with grog, sand, or a mixture of sand and grog. Most previously proposed Woodland pottery types and their varieties include temper type in their definitions (Jenkins 1981:124-26). Those of import here are the Baldwin and Baytown plain types including the Baldwin varieties Blubber, Lubhub, Baldwin, and O’Neal (Jenkins 1981:126) and the Baytown varieties Roper, Tishomingo (Jenkins 1981), and Ridge (Stubbis 1982:54). But if we apply a quantitative approach to describing pastes, can we achieve a reasonably good fit between patterns of clay body inclusion and previously defined varieties? If we distinguish between fine, small, coarse, and large particles of grog or sand and sparse, modest, or abundant amounts of each in the clay body, will our

varieties formed should be justified by a real fit between the class or classes and sub-classes being described and the phenomena for whose study they were created. It is not enough to notice that a property is observable and hope that it might correspond with some issue of interest; one must know how the attribute reflects the phenomenon itself. In other words, one must not classify pottery sherds as things, but rather as the consequences of intentional human behavior (Krause 1990:711-26).

If, for example, we view potsherds as the consequences of intentional human behavior we may, by specifying a target phenomenon for each, create three broad but potentially useful kinds of types that for convenience we may call manufacturing, functional, and historical types. The target phenomena for manufacturing types would be regularities in manufacture, the target for functional types would be regularities in use, and the target for historical types would be time and space restricted regularities in manufacture or use. Hence in framing manufacturing types the analyst must specify the properties of his or her sample that have demonstrable significance in the making of pots and use these to the exclusion of others in defining the type. To create functional types from the same materials the analyst must specify those properties of manufacture that also have presumed or demonstrated functional significance and do so to the exclusion of all others. The same is true of historical types, with the proviso that only those properties of manufacture or use with time-space significance are of import (Krause 1981).

Now, since pots must be manufactured before we may assign temporal or spatial significance to them or assign meaning to their use, understanding how they were made is a first and central issue—an issue that must take precedence over the time and space distribution of the practices that formed them. In general pottery making will include the following steps: (1) obtain clay, (2) prepare clay, (3) shape the vessel, (4) decorate the vessel, (5) dry the vessel, and (6) fire the vessel. Southeastern archaeologists have traditionally used the content of the first three steps to form broad historic types. The use of mass modeled residual clays tempered with fiber is thought to have preceded the use of coiled and sand tempered clays. Grog and limestone tempers seem to have gradually grown in popularity at the expense of sand tempered wares, and these in turn were replaced by coiled pots made from shell tempered clays. The results of step 3 (i.e., vessel, rim, and lip shape) and the content of step 4 (i.e., forms of decoration) are typically used to create yet finer distinctions within the coarser-grained time-space order provided by using the content of steps 1, 2, and 3 (see Jenkins and Krause 1986).
description of clay body inclusion pattern match previously defined Baytown and Baldwin varieties? Since this was an empirical question we approached it by first defining our categories, then using them to analyze the patterns of clay body inclusions reported in the literature. Finally we matched our quantifications against the varieties as previously defined.

For our purposes we took “fine” to mean a particle size of less than .2 mm in diameter; “small” to mean particle sizes of circa .2 to .5 mm in diameter; and “coarse” to indicate particle sizes of circa .5 mm to 1 mm in diameter. We understood “sparse” to mean that the tempering agent is not common or is seen infrequently in the paste; “modest” to mean that it is not overly common in the paste but mixed throughout; and “abundant” to mean that the tempering agent is very common and appears to be visually dominant in the paste. Applying these criteria to the pattern of clay body inclusions in specimens previously defined as types and varieties yielded the following mapping.

**Woodland Types and Varieties**

As previously noted, there were four Baldwin Plain varieties of interest—Blubber, Lubbub, Baldwin, and O’Neal. In variety Blubber paste, the tempering was rounded small sand grains in modest amount. In variety Lubbub paste the tempering was also well rounded sand grains, but in small to coarse size and in abundant amount. There is occasional sparse small grog in this paste. In var. Baldwin paste, the tempering is fine to small sized sand grains in modest amount. Here there is a strong resemblance to Baytown paste var. Roper, the two distinguished by the presence or absence of grog tempering with the sand. Pottery type Baldwin Plain, var. O’Neal had the same paste as var. Baldwin (Jenkins 1981:126). The two could, however be easily and consistently separated by referencing non-paste attributes.

The three Baytown varieties of interest were Roper, Tishomingo (Jenkins 1981), and Ridge (Stubs 1982:54). Variety Roper paste has a broader range of variation than described (Jenkins 1981:89). Included are various aplastics in addition to the grog temper or a fine textured clay with no apparent aplelastic. The latter paste variation is often marked by the presence of fine mica. The grog temper is small in size and modest to abundant in amount. In var. Tishomingo paste, grog and sand tempering is approximately equal, small to coarse in size, and sparse to modest in amount. Thus in the Woodland pastes, we have two varieties with patterns of grog tempered clay body inclusions and three varieties with patterns of sand tempered clay body inclusions. Variety Ridge, with its mixture of minimal grog and sand tempering apparently occurring in both Woodland and late Mississippian deposits, has questionable chronological value.

**Mississippian Pastes**

A similar approach can be applied to Mississippian ceramics. Mississippian pastes are of two kinds, Mississippi and Bell (Phillips 1970:130). Mississippi paste, as used here, refers to crushed live freshwater bivalve shell, of small to large particle size, readily and easily seen on one or both surfaces of the pottery sherd. Bell paste refers to crushed live freshwater bivalve shell, of fine to very fine particle size, of abundant to very sparse occurrence in the paste, and easy to see to almost absent. By fine we shall mean pulverized shell, with the majority of particles .5 mm or less in diameter; by “small” we mean crushed shell with the majority of particles between .5 mm and 1.5 mm in diameter. We interpreted “sparse” to mean not common in the paste; “modest” to mean not overly common but evenly mixed throughout the paste; and “abundant” to mean very common throughout the paste.

**Mississippian Types and Varieties**

Mississippi Plain pottery has been defined in the literature (Phillips 1970:130) and is generally the standard for the pattern of clay body inclusions called Mississippi paste. Mississippi paste is tempered predominantly with crushed live bivalve shell, particle size and abundance being such that it is seen readily. It is consistently marked by the presence of large particles of shell, usually 3 mm or less, but rarely up to 5 mm. There are abundant smaller particles of shell as well. By using the amount and size of shell and other aplelastic several paste varieties may be recognized.

Plain Mississippian pottery with a Mississippi paste, Mississippi Plain var. Warrior, typically has a chalky or soapy feel. It is the dominant plain ware throughout the Tibbee Creek, Lyon’s Bluff, and Sorrels phases (Marshall 1986), which are comparable to the Moundville I through IV and Summerville I-IV (Peeples 1987:4) phases for the central Black Prairie region of east Mississippi. A Mississippi paste that appears intermediate between var. Warrior and the Bell paste pattern of clay body inclusions is Mississippi Plain var. Hale (Mann 1983:50-52, 62; not to be confused with Bell paste var. Hale [Phillips 1970:58]). Since the two pastes (Bell and Mississippi) merge one into the other, it is difficult to sort pottery only on particle size and abundance of temper. Tempering particles in sherds of Mississippi
Plain var. Hale are generally modest in size (smaller than var. Warrior) but typical Bell paste vessel characteristics often accompany the sherds so tempered. Steponaitis (1980, 1983) effectively used this fact in sorting the Moundville vessels. Mississippi Plain var. Hale’s greatest use appears to have been in the Tibbee Creek and Lyon's Bluff phases, although it remained important into the Sorrels phase. Mississippi Plain var. Hull Lake has a similar temporal distribution. It is, however, a shell tempered pottery type that also includes grog tempering in modest to abundant amounts.

We have named a shell tempered but fine, sandy Mississippi paste var. Copeland. Because the sand in var. Copeland is fine and not easily seen, our primary sorting criterion is the sherd’s sandy or gritty feel. Mississippi Plain var. Copeland is second only to var. Warrior as the commonest paste type at the Lyon’s Bluff site. Variety Copeland is primarily associated with the Lyon’s Bluff and Sorrels phases, but it appears also in the Mhoon phase. We have named an even sandier shell tempered Mississippi paste found at the Lyon’s Bluff site var. Mhoon. The sand particles in the type Mississippi Plain var. Mhoon are coarse, usually water worn, up to .5 mm or larger in diameter, but never greater than 1 mm. The sand is visible on the surface of all specimens in the form of abundant small dark spots, and in sherd cross-sections as small spherical light amber-colored globules. Variety Mhoon pottery is late, terminal Sorrels to post Sorrels phases and associated primarily with the Mhoon phase at Lyon’s Bluff, where it is fairly common at most house sites of the West Complex or Protohistoric unit (Marshall 1986). Variety Mhoon pottery is also very common at house sites belonging to the Starkville complex, where it dates to the Protohistoric and/or early Historic occupations thought to be Chokchiuma (Swanton 1946). Early Historic Chickasaw potteries have a similar paste and texture (Stubbins 1982; Atkinson 1987).

Phillips first defined Bell paste (Phillips 1970:58), whose regional standard, Bell Plain var. Hale, is tempered with fine shell particles which are sparse to moderately abundant. There is a chalky to soapy feel to this pottery, which was common at the Lyon’s Bluff site through the Tibbee Creek, Lyon’s Bluff, and Sorrels phases. This variety continued into the Mhoon phase at Lyon’s Bluff and is found sparingly in sites of the Starkville Complex. Bell Plain var. Big Sandy is a shell tempered pottery with grog pellet clay body inclusions that was found in modest abundance at Lyon’s Bluff in the Tibbee Creek, Lyon’s Bluff, and Sorrels phases.

At the Lyon’s Bluff site there are two varieties of Bell paste that contain a mixture of shell and sand, var. Willie and var. Ramsey. Variety Willie has sparse to modest fine shell and modest to abundant fine sand. Variety Ramsey has a sparse amount of coarse sand mixed with its moderately abundant crushed shell tempering. Variety Ramsey paste is obviously related to var. Willie, since the two grade one into the other.

The local use of crushed fossil shell as temper led us to define two paste types we have called Wilson Plain var. Wilson and Wilson Plain var. Oktibbeha. Variety Wilson, which has a coarse fossil shell temper, appears rarely at the Lyon’s Bluff site, but is much more common in the Starkville Complex and in some Chickasaw sites (Stubbins 1982; Atkinson 1987). Variety Oktibbeha (Stubbins 1982) is distinguished by sparse fine sand in addition to small fragments of fossil shell. Both vars. Wilson and Oktibbeha are found in the Mhoon phase at Lyon’s Bluff and appear in one or more of the Protohistoric and Historic phases of the Starkville Complex.

Summary and Conclusions

Of the two Woodland stage paste types, Baldwin and Baytown, Baldwin pastes are earlier and retained their popularity until the early years of the Christian era, after which Baytown pastes began a steady growth in popularity. Hence we would expect that those potteries with Baldwin pastes and its varieties, namely Blubber, Lubbub, Baldwin, and O’Neal, would indicate an earlier placement than potteries with Baytown pastes and its varieties Roper and Tishomingo. The Baytown variety Ridge, however, has an ambiguous temporal distribution and may not be used as an historical type without revision. The use of Baytown pastes continues into the early years of the Mississippian stage, and this Woodland paste may indeed have been the model for sand and grog inclusions in Mississippian stage pastes.

Those Mississippian stage pastes of Mississippian type, vars. Warrior, Hale, and Hull Lake, and those of Bell type, vars. Big Sandy and Hale, are found in the Tibbee Creek, Lyon’s Bluff, and Sorrels phases, while that of Bell type var. Hale continued in use during the subsequent Mhoon phase. Mississippi paste type var. Copeland is first popular in the Lyon’s Bluff phase and continues to be of import in the Sorrels and Mhoon phases. Mississippi paste type var. Mhoon is later still, being found in sites of the Sorrels and Mhoon phases and sites identified as historic Chickasaw and presumed Chokchiuma. The most recent wares have a Wilson paste of vars. Wilson and Oktibbeha. The use of fossil shell is the hallmark of this paste type, which is found in Mhoon phase sites and those of Chickasaw identity.
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Gregory A. Waselkov

“What then is the American, this new man?”
J. Hector St. John Crèvecoeur, Letters from an American Farmer, 1782

The creation of Americans began immediately after Columbus established his settlement at La Isabella in 1493. I use the term “American” in a broad sense (distinct, though, from its other general meaning of “Indian” or “Native American”), much as Crèvecoeur did, to refer to the societies that began to develop in the western hemisphere after that fateful day five hundred years ago. What links together all of the tremendous diversity of societies that exists in North and South America today is their common heritage of cultural amalgamation. The precise outcome, in any given country, depended on the particular relationships that arose between indigenous and intrusive peoples, each with its own cultural achievements and biases, as they competed for control of the local natural environment. Charles Ewen employs archaeological methods in an attempt to explore this process as it unfolded at Puerto Real, a Spanish colonial town site in modern Haiti, during the sixteenth century.

As Ewen notes, historians have long recognized the important “effects of the New World inhabitants and environment on the European colonists” (p. 1). To name just a few, Frederick Jackson Turner’s “frontier hypothesis,” Walter Prescott Webb’s studies of the Great Plains, and James Axtell’s more recent research on the colonial English have explored this line of inquiry, with revealing insights for United States history. But historical archaeologists, Ewen claims, have typically skirted the topic, preferring to view colonial Europeans as relatively unaffected by native peoples, who served mainly as foils to the newcomers. While Ewen probably overstates his case—some archaeologists studying English and French colonial sites have taken a keen interest in the impact of Indian subsistence, settlement, trade, and intermarriage with Europeans—he may accurately describe the situation for Spanish colonial sites. But dramatic changes are occurring in that arena too, as recent publications on several major research projects at Spanish mission sites in Florida attest.

As part of her work at St. Augustine during the last two decades, Kathleen Deagan devised a hypothesis that Ewen attempts to test using his data from Puerto Real. Deagan maintains that in colonial situations where most of the colonists were men, those men conservatively retained the European artifactual trappings associated with male activities, while permitting acculturation of Indian materials “in the less visible, female-dominated areas of daily activity” (p. 3). In this view, Indian women served as active agents of cultural change via their roles as wives, concubines, and slaves in colonial households dominated by European men.

Ewen derives five archaeological tests of Deagan’s hypothesis. (1) He predicts that food preparation, primarily a female activity in that era, would have involved the use of both European and locally-made ceramics. (2) High social status should be discernible almost entirely by the presence of European status-related artifacts. (3) Structures should follow European style and town plan, though construction material may be local in origin. (4) European and local food resources should both appear in the food remains. (5) Finally, a period of experimentation should eventually lead to a “crystallization,” when an effective “colonial pattern” of creole culture becomes generally accepted.

Puerto Real was founded in 1503. Little written documentation on life in the town has been discovered in the archives of Spain, but it is known that early in the century there were about 100 households. Only a few score Spaniards, including three Spanish women, controlled some 800 Indian slaves and serfs in 1514. After 1520, African slaves dominated the population. The town struggled to survive as a sort of colonial backwater of the Spanish empire in the Caribbean, relying mainly on cattle raising for hides and smuggling, until the occupants were forcibly relocated after 75 years of unexceptional existence.

In contrast to the rather humdrum history of Puerto Real, the archaeological site of the town is extraordinarily interesting. A Haitian antiquarian, Dr. William Hodges, discovered the site in 1974. Archaeologists from the University of Florida, under the direction first of Charles Fairbanks and later of Kathleen Deagan, surveyed the site and excavated several structures between 1979 and 1985. Locus 19, a large midden and associated stone wall, were dug by Charles Ewen during the last two years of the project, and this is the part of Puerto
Real discussed in greatest detail in this book. Excavation of the midden recovered over 49,000 artifacts, which are described and illustrated in Chapter 6 (undoubtedly the section readers will find most useful). Most came from the midden, which was excavated in 10 cm deep arbitrary levels, since natural strata were not observed. Through an analysis of artifact types found in the arbitrary levels, Ewen proposes a temporal division of the levels into “Early Period” (1503-1550) and “Late Period” (1550-1578) contexts. Immediately south of the midden was a stone wall, 22 m long, with two brick drains in the western end. Although he repeatedly refers to this wall as part of a house, later in the book Ewen more cautiously interprets this wall as “the facade of a residential structure with an attached walled courtyard or the back wall of an enclosed courtyard” (p. 107).

After describing his methods, the archaeological context, and the artifact assemblage, the author evaluates his tests of Deagan’s hypothesis in light of the evidence from Locus 19 at Puerto Real. At this point in Ewen’s book the critical reader will begin to note some apparent inconsistencies between the data and his interpretations.

Chronology seems to present the most serious problems. Deagan’s hypothesis pertains to the period of direct contact and interaction between Spaniards and native Taino Indians, a relatively brief period on Hispaniola because of a “decline in the native population” (in Ewen’s words, p. 26). (As an aside, it seems to me inappropriate to describe as a “decline” the catastrophic decimation of Hispaniola’s million Indian inhabitants that occurred between 1492 and 1520.) Despite attempts to augment the labor force with enslaved Bahamians, the Indian population of Hispaniola continued to fall precipitously until a smallpox epidemic in 1518-1519 essentially extirpated the remaining Tainos, although a few Indians still resisted Spanish domination in the 1530s (p. 29). Consequently, an archaeologist studying the impact of Indians on Europeans at Puerto Real must distinguish clearly between the pre-1520 era of Indian-European interaction and the post-1520 era, when Indians were a negligible force in culture change in Hispaniola. To his own question, “Is it possible to clearly distinguish between late and early occupation at Puerto Real?” Ewen responds, “the answer is yes” (p. 48). Yet he chooses the year 1550 as the dividing point between early and late periods, his “Early Period” lasts thirty years longer than did the Indian population!

Other problems arise from the author’s interpretation of the arbitrary levels excavated in the midden at Locus 19. Ewen states that period proveniences were established “using stratigraphy [by which he presumably means “superposition,” since there were no visible strata] and artifact terminus post quern” (p. 48). As one reviews the percentages of each artifact type attributed to the two periods presented in Appendix 1, however, the nearly identical nature of the two assemblages becomes readily apparent. As even Ewen concedes (p. 112), few statistically significant differences exist in artifact percentage distributions between the two periods. Looking more closely at these tables, other discrepancies appear. Montelupo Polychrome, which predates 1550, was recovered only from “Late Period” levels. Post-1550 Cologne Stoneware and Ming Porcelain did in fact occur, as expected, mainly in “Late Period” levels. But even though a high percentage of pre-1550 Melado Ware was found in appropriately “Early Period” contexts, more specimens came from “Late Period” levels. In sum, the distinctions Ewen draws between his two periods are unconvincing. Most of the artifacts date to the last half of the sixteenth century (as the author belatedly admits on p. 115).

Since Ewen was, in fact, unable to discriminate the early Indian-European period at Locus 19, his attempt to apply Deagan’s hypothesis was bound to fail. Consider, for example, his analysis of locally-made ceramics. These include two Indian-made wares (Meillac and Carrier) and one ware (Christophe Plain) that he obliquely identifies as the product of African slaves, which occur in the following proportions:

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<th>Early</th>
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<tr>
<td>Meillac</td>
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<td>46</td>
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<td>%</td>
<td>1.30</td>
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<td>Carrier</td>
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<td>3</td>
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<td></td>
<td>%</td>
<td>0.09</td>
</tr>
<tr>
<td>Christophe Plain</td>
<td>#</td>
<td>1,319</td>
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<td></td>
<td>%</td>
<td>37.39</td>
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Obviously, these data (i.e., 159 sherds from a total of over 49,000 artifacts) do not suggest much Indian contact, early or late. The percentages also reflect the close statistical similarity of his two “periods,” a pattern found throughout the artifact inventory. From this ceramic data, though, Ewen detects “a shift in the nature of the locally manufactured ceramics through time. . . . The shifts in ceramic types mirror the demographic changes occurring in the labor force at Puerto Real. As the Indian population declined, it was replaced by imported African slaves” (p. 103). If the historical record was unavailable, could we discern these demographic changes from Ewen’s artifact distribution? Of course not.
Ewen has missed an opportunity to explore topics for which his data are much more appropriate: the origins of the Caribbean plantation economy and white-black social relations on Hispaniola. He has, however, provided an intriguing glimpse of a very important colonial site, about which we will hopefully receive further reports from other participants in the University of Florida project.

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Tristram R. Kidder

It is an increasingly rare phenomenon in today’s world of archeological publishing to witness not only a well written and highly commendable site report, but also to find one which makes a significant and in many ways groundbreaking contribution to the regional archaeological literature. Such is the case, though, with the slim monograph entitled “Coles Creek and Mississippi Period Foragers in the Felsenthal Region of the Lower Mississippi Valley,” edited by Frank Schambach. In this work Schambach and his collaborators report on a series of excavations at the Bangs Slough site (3-Ca-3) in the Felsenthal region of southeast Arkansas. This report is all the more important because the Bangs Slough site is one of those places which would never end up in the literature if it were not for the requirements of contract archeology. The editor and the Arkansas Archeological Survey have purposefully set out to “get the information on the Coles Creek and Mississippi period occupations ... out of the gray literature of contract archeology” (Schambach, p. 2). They have succeeded admirably, although the work is not without criticism. Because of this research we have a new and different perspective on the behavior of prehistoric Native Americans in an area which is rapidly becoming one of the best understood archaeological regions in the Lower Mississippi Valley.

The Bangs Slough site report consists of eight chapters, references cited, and an appendix. The first and last chapters, essentially an introduction and conclusion, are by Frank Schambach, who also served as editor and principal investigator for the research. Schambach also helped to write the chapter on excavations (with Randall Guendling) and also the chapter on pottery (with David Waddell). Other chapters cover the non-pottery artifacts (by Waddell and Kathryn King), archeobotany (by Frances King), faunal remains (by Mona Colburn and Bonnie Styles), and bioarcheology (by Murray Marks). Together these chapters form a well written, concise description of the Bangs Slough site and its material remains.

The introduction by Schambach provides a fast start to the volume. The excavations are identified as having resulted from an Army Corps of Engineers mitigation program undertaken due to proposed rises in the pool level due to construction on the Ouachita River. Schambach notes that excavations revealed five components: late Mississippi (or protohistoric) period Caney Bayou phase; Mississippi period Gran Marais phase; late Coles Creek-early Mississippi period Cypress Swamp phase; late Coles Creek Small Slough phase; and an unidentified late-to-middle Coles Creek phase of unidentified nature. Not mentioned in the introduction but touched on very briefly elsewhere are a Poverty Point Callion phase component, and a Late Archaic and early Ceramic period occupations as well (see pp. 2, 14, 113). These later components were almost nonexistent in the excavations reported here, but they are known to occur at the site based on surface collections, previous excavations, and from several artifacts recovered during work by Schambach and his crew.

Schambach points out that the Coles Creek and early Mississippi period components were the most important, due to both their stratigraphic integrity and extent, but also because of the information which could be gleaned from these deposits and which was otherwise not known or had not been previously discovered. “The Cypress Swamp phase component is the most important unit ... in terms of the size of the assemblage and also in terms of what it tells us about the cultural development in the Felsenthal region at the critical period from A.D. 900-A.D. 1000” (p. 1). Schambach notes that this is the time period in the Southeast when agriculture was becoming increasingly important and when sedentary village life was the primary mode of living. He goes on then to raise and emphasize the main point of the monograph, and the aspect of it which will be sure to encourage substantial rethinking of regional prehistory: “we found no evidence for horticulture or sedentism at Bangs Slough ... Instead we found ... that the Cypress Swamp people were living as semi-sedentary
hunters, fishermen, and gatherers, visiting the Bangs Slough site briefly in their seasonal rounds" (p. 1). To further demonstrate the thrust of this thinking, Schambach then extends his argument: "the broader question our research raises is did this pattern prevail throughout the Lower Mississippi Valley on the eve of the Mississippi period, as I suspect, or was the Felsenthal regional atypical" (p. 1). Although Schambach is unable to document his argument fully beyond the Bangs Slough site, he raises a provocative issue, and moreover, he tackles it head on in the research laid out in this monograph.

The evidence available to buttress the arguments so strongly put forth in the introduction are to be found in the succeeding chapters on excavations and the analysis of the material remains. The discussion of the excavations is refreshingly simple, yet open and honest. Schambach had initially observed that the excavations reported in the monograph were from a limited part of a large site. Notably, they consisted of test pits and trenches placed near the bank of Bangs Slough. Schambach, to his credit, meets this problem directly: "Thus, the question of the representativeness of our samples from the dumps [shell middens along Bangs Slough] is a major issue in this report ... Here we have had to consider it very carefully" (p. 2). While the reader may have cause to question this statement (see below), it is important to note that the authors were aware of the problem from the outset.

The chapter on the excavations consists primarily of two sections, one on methods and the other concerned with the description of the strata. The methods section was straightforward and revealed little to disagree with. I was interested to note, however, that the sampling procedures, which would turn out to be critical for further evaluation, were perhaps not as rigorous as would be expected. Initially all soil was screened through 1.5 mm (1/16th inch) mesh, but after two days this sampling was switched and 6 mm (1/4 inch) mesh was substituted. The decision to change screen sizes was practical and was the result of the all too familiar need to measure returns against cost in time and effort. However, one result was that sampling became biased and uneven, since they reverted "to 1.5 mm mesh only when we thought there was a need to check for very small specimens" (p. 8). As a "control on this procedure" five 30-by-20 cm column samples from the "deepest, richest looking parts of the midden" were excavated and screened through 1.5 mm mesh "and then subjected to flotation processing" (p. 8). While this sampling procedure is reasonable, and clearly outlined, it is somewhat disheartening since the report in several places trumpets the fact that they used "sophisticated collection techniques" (abstract, pp. 1, 113). Since much of the conclusion rests on the absence of specific data (notably the lack of archaeobotanical evidence for cultigens), the failure to implement a more comprehensive flotation procedure must be noted. It is also distressing to think that flotation procedures would be implemented only after the control samples had been screened through 1.5 mm mesh. Given the high clay content of the soils (p. 8) this method may well have resulted in the fragmentation of even relatively small seeds and seed fragments prior to their being floated. It would also be helpful for the reader to know the details of the flotation procedure.

The bulk of the section on excavations is given over to a discussion of the stratigraphy in the "east" and "west" middens, the two areas excavated during mitigation. Both excavation areas were begun by cleaning out previous test pits undertaken by Richard Weinstein and David Kelley (1984). These previous excavations provided a "key" for the excavation of natural stratigraphic units and allowed for a relatively simple stratification of top soil, shell midden (or middens), and "earth" middens. Due to a stroke of luck the local water table fell in the site area during excavations, allowing researchers to extend trenches beyond the normal water level of Bangs Slough. The interpretation of these two midden areas is that they constituted two discrete shell refuse "dumps," or over-the-bank middens. The excavations leave no doubt that these middens were not one-time events, but had accumulated over time. The shell dumps, however, were apparently the result of a relatively limited time period of accumulation. Through extensive ceramic analysis these dumps were shown to be essentially coeval, although there was no stratigraphic linkage between the two midden areas. The most important deposits from an archaeological point of view were analysis units 2 and 3, which consisted of almost all of the shell bearing strata in the east and west midden areas, respectively. One reason that these strata were so important is because they constituted the bulk of the evidence for the Cypress Swamp phase, and because they were well dated through an "excellent series of dates" (p. 18).

There are 11 radiocarbon dates altogether from the Bangs Slough site (p. 18, tables 1, 32). Eight dates were run on samples from Schambach's excavations, and three came from Weinstein and Kelley's earlier research (1984). This suite of determinations "make Cypress Swamp the most extensively dated phase in the Felsenthal sequence and probably the most accurately dated" (pp. 117-118). I would go so far as to say that these dates provide the strongest, most consistent set of dates in the central and southern Lower Mississippi Valley. These dates, however, are actually better than the authors realize (see tables 1, 32). The dates from Bangs Slough and reported by Schambach are presented in the text in uncorrected and corrected forms. The
correction factors used were from Damon et al. 1974, and have subsequently been modified and replaced by those of Stuiver and Pearson (1986). The revised corrections indicate that in all cases (not including a date run on carbonized cane [see p. 18]) the calibrated date is more recent than is shown in table 18 (see Kidder 1990:table 1). An examination of the dates at one standard deviation, however, suggests that they are actually statistically indistinguishable. In fact, a weighted average of all radiocarbon dates from Bangs Slough (excluding two dates, UGA-4728, and 83-606-85) indicates that there is a 100% probability that these are all dating the same time frame, which is a span from AD 1011-1024 (at one standard deviation; at two standard deviations the spread increases to AD 998-1030). This is an almost impossibly good date sequence, and argues that analysis units 2 and 3 are contemporary (to make an archaeological understatement).

It is slightly annoying to have to read these dates presented in this outdated format. The calibration curve has been available on computer disc since 1987, and in fact I published a list of the Bangs Slough calibrated dates in 1990 (Kidder 1990). Also the laboratory numbers are not given here, although Schambach kindly provided me with these data, which I published (Kidder 1990:table 1). Despite my complaints I am absolutely amazed by the Bangs Slough dates, which, when combined with the later Paw Paw dates (Kelley 1990; Kidder 1990:table 1), forms a remarkably unshakable suite of determinations for the period ca. AD 1000. Cypress Swamp does deserve consideration as the best dated phase in the Lower Mississippi Valley. These dates also confirm my feeling that the Felsenthal sequence, at least in its later end, is essentially better dated and more firmly supported by radiocarbon dates than is any other sequence in the valley.

The sections on analysis of material remains follow the discussion of excavations and stratigraphy. The ceramics chapter presents a “gold mine of information . . . in very small nuggets that no one except a regional specialist could be expected to spot, much less appreciate” (p. 19). There is indeed a wealth of information in this chapter. The description and analysis of pottery is structured around the now well known and very utilitarian “collegiate” system devised by Schambach for the Felsenthal. This descriptive system is paralleled, when possible, by type-variety classifications, often based on ceramic units defined in the Lower Valley proper. Schambach and Waddell have provided a highly useful and full analysis of the ceramics on a descriptive level. The ceramics are used in the latter part of the chapter to create analysis units and to compare the Bangs Slough collection with other regional assemblages, components, and sequences. Regional specialists will indeed find this to be one of the most significant elements of the Bangs Slough report. The only drawback is that the ceramic assemblage is basically limited in scope, both by nature of its temporal duration and by its apparent functional specialization. One minor point I would raise is that Schambach and Waddell often compare varieties in the Felsenthal to those in the Lower Valley, which is, I think, inappropriate. The sherds at Bangs Slough are, as the authors have pointed out, local variants, and as such deserve local names in conformity to the rules of continuity espoused by Phillips and Schambach. Thus, references to Lower Valley varieties of Coles Creek Incised such as Hardy, Mott, and Coles Creek should be avoided and if necessary generic connections should be noted. The authors should bear in mind that the Felsenthal sequence is in many ways more secure than that originally defined in the east. Perhaps we who work in the Lower Valley itself should be referring to the Felsenthal pottery for our comparisons, not vice versa.

The chapter on “Other Artifacts” focuses mainly on lithics. This is an important chapter because it not only describes the collections, but also interprets the lithic assemblage in terms of reduction sequence analysis. While the end result of the tool assemblage is a limited collection of finished materials, the debitage and reduction debris demonstrates a more complex pattern of resource procurement and utilization. Although most of the chert and novaculite were obtained from local gravel deposits, some may have been brought into the site from the Ouachita Mountains. Lithic reduction, tool production, and tool rejuvenation were all practiced at the site. The quantity of lithics was modest but indicates a significant industry. Clearly the lithic assemblage cannot be attributed to occasional and casual production, but rather a sustained utilization of stone for a multitude of tasks.

The chapter on archaeobotany is an important section of this report. To those who are familiar with the AAS reports this chapter will have a familiar feel to it. King begins by analyzing General Land Office inventories and correlating site location with tree types and soil cover. She observes that the Bangs Slough site is located on the southern end of the local distribution of soils suitable for agriculture (Fig. 73) and indicates that this could be taken as potential evidence that the site occupants might have been using domesticated plants in their diet (p. 90). The subsequent analysis of "36 samples" from Bangs Slough did not reveal any domesticated plant foods, and in fact the total archaeobotanical assemblage was small and of limited diversity. The presence of uncarbonized plant taxa in some of the assemblages is discussed honestly, and the absence of domesticated foods is incorporated into a model suggesting functionally specialized (possibly
seasonal?) activities and intensive exploitation of a “broad-spectrum” diet “based on wild plant resources” (p. 93).

The faunal remains are similarly analyzed, and from them it is concluded that the Bangs Slough site was a specialized camp with limited activities focused on the procurement of mussels and the initial butchering of animals, especially deer. The faunal analysis suggests that the site deposits “do not reflect a full range of subsistence activity” (p. 107). Seasonal analysi data are ambiguous, and can be used to support several scenarios from year-round occupation to temporary seasonal encampments. Faunal procurement does not seem to shift dramatically through time, although shellfish collecting was clearly an important activity in the early Mississippi period Cypress Swamp phase. The fauna at Bangs Slough are compared to other regional assemblages, and also to sites in Illinois to generate comparative analyses. These analyses confirm the suspicion that the Bangs Slough middens were generated by “primary butchering activities,” and do not reflect a “generalized midden assemblage” (p. 107). It is curious to note that the Bangs Slough site is compared briefly to Paw Paw, yet David Kelley’s recent thesis (1990) on the fauna from that site is not mentioned.

The last ‘data’ chapter is a four-page discussion of the single burial found at Bangs Slough. This is essentially a description of the burial “for the use of future researchers” (p. 109). The skeleton was “pathologically unremarkable,” and the dentition showed “moderate to severe attrition” and “extreme facturing or chipping” probably resulting from grit added to the diet as the result of food processing (p. 109-110). The wear and attrition on this skeleton are similar to Baytown and Fourche Maline populations elsewhere in the region.

The conclusion brings the results of this research together into a coherent picture which is used to argue emphatically the case that these are the remains of Coles Creek and early Mississippi period hunter-gatherers. In this chapter Schambach briefly reviews the Bangs Slough site sequence and discusses the site occupation in terms of regional culture history. This is a fine summary and an important contribution to the archaeology of the Lower Mississippi Valley. Schambach observes that because the middens investigated represent a full range of material remains, it is thus likely that the distribution of floral and faunal remains is due not to sampling error but to prehistoric behavior. The logical conclusion is that these middens were “from a series of sporadically occupied hunting, fishing, and shellfish collecting camps” (p. 114). The absence of cultigens is argued to be a true reflection of the lack of horticultural behavior.

The argument made by Schambach and convincingly presented in this volume falls on sympathetic ears. My own research in the Lower Mississippi Valley seems to be yielding similar results. However, I am a bit wary of making too much of the argument that cultivation was not practiced based on negative evidence, especially since our sampling is hardly representative across sites and regions. Schambach has not, by his own admission, been able to examine the entire site, and thus the specialized activities noted for the Bangs Slough middens may not be representative of the site function as a whole. The flotation processing at Bangs Slough may have been flawed by sampling bias and recovery procedures. Both King (p. 93) and Schambach (p. 120) indicate that they would expect corn to be found in the form of carbonized cobs. Our research in the Lower Valley shows that recovery of corn could not occur without an intensive flotation sampling procedure. And in no case have we found whole or partial cobs, yet we have found corn in a limited number of contexts. My point is not that Schambach and his collaborators are wrong, but that it is important to move slowly in creating our interpretive edifices.

The Bangs Slough volume is a remarkably productive and refreshingly well presented site report. The data and ideas contained within this slim work will be important parts of the regional prehistoric sequence for years to come. I must note with dismay the rather poor production quality and the terrible design and layout. This work should also have been updated, since it represents nothing more than a refinement of the original 1985 contract report. Ideas and references have changed since then, and it is a shame that these new ideas and new data were not incorporated into such an outstanding piece of research. Despite minor typographic errors and a poor layout this is a monograph which every archaeologist with an interest in Southeastern archaeology should have on her/his bookshelves.

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References
