MISSISSIPPI ARCHAEOLOGY

Mississippi Archaeology is published semiannually by the Mississippi Department of Archives and History in cooperation with the Mississippi Archaeological Association to present information of a basically technical nature on field work, artifact analysis, and archaeological theory, and to serve as the journal of record for archaeological activity in Mississippi. Contributions treating the archaeology of Mississippi or the Southeastern region are solicited for publication. Preparation of manuscripts should follow the style used in this issue; arrangements for electronic transfer of manuscripts can be made after acceptance of a submission, but submission should be made in hard copy form.

Editorial office:
Mississippi Department of Archives and History
PO. Box 571
Jackson, MS 39205-0571

EDITOR
Patricia Galloway, Department of Archives and History

EDITORIAL BOARD
Aan Early, University of Arkansas-Fayetteville
Janet Rafferty, Mississippi State University
Kenneth E. Sassaman, University of Florida
Marvin T. Smith, Valdosta State University
Amy Young, University of Southern Mississippi

Typesetting and layout by Julie Bullock
Cover art by Cavett Taff

ISSN 0738-775X

Copyright 1999
Mississippi Department of Archives and History
Jackson, Mississippi

MISSISSIPPI ARCHAEOLOGY

Volume 34  Summer 1999  Number 1

CONTENTS

The Harry Osborn Collection: An Early Nineteenth-Century Artifact Assemblage from Memphis, Tennessee
Mary Evelyn Starr and Robert C. Mainfort, Jr. 1

The Potential for African-American Archaeology at Mount Locust (22-Je-522), Natchez Trace Parkway, Jefferson County, Mississippi
Amy L. Young 15

Book Reviews

From Morality (and "Sociology") to Politics in Smithsonian Anthropology and Museology
Marvin D. Jeter 41

O’Brien and Dunnell (eds.): Changing Perspectives on the Archaeology of the Central Mississippi Valley
Charles H. McNutt 77

Schambach: Pre-Caddoan Cultures in the Trans-Mississippi South: A Beginning Sequence, La Verna: The Caddo Chiefdoms: Caddo Economim and Politics, 700-1835
Timothy K. Perttula 102

Daniel: Hardaway Revisited: Early Archaic Settlement in the Southeast
Samuel O. Brookes 109

Maryland Historical Trust: GIS and Archaeology: A Demonstration [CD-ROM]
Douglas C. Sims 112
The Harry Osborn Collection: An Early Nineteenth Century Artifact Assemblage from Memphis, Tennessee

Mary Evelyn Starr and Robert C. Mainfort, Jr.

Abstract

A collection of early nineteenth-century aboriginal and Euroamerican artifacts from a site in Memphis was donated to the University of Arkansas Museum in 1967. Descriptions of these objects are presented, as is a brief discussion of the historical context of the material.

Introduction

One of the things that makes research of any kind an exciting profession or pastime is the sudden appearance of the unexpected. Here we discuss an intriguing collection that was "discovered" by the junior author while conducting research on the large corpus of artifacts from late period sites in northeastern Arkansas curated by the University of Arkansas Museum (e.g., Mainfort and Carroll 1996; Fisher-Carroll 1997).

During the fall of 1966, Mr. Harry Osborn, a resident of Memphis and a member of the Arkansas Archeological Society, visited the university museum and showed Dr. Charles McGimsey (Director) an assemblage of aboriginal and early nineteenth century Euroamerican artifacts that he had recovered from a site in Memphis, Tennessee. The circumstances of discovery were briefly reported in Field Notes 23 (November 1966) as follows:

Harry Osborn and his son, of Memphis, appeared at the Museum a few weeks ago with an extremely interesting batch of archeological material, and an interesting story. Last summer near the Osborn's [sic] home in suburban Memphis, a bulldozer was clearing and scraping for a housing development. Harry wandered over to the disturbed area to see if there might be anything interesting turning up, for that whole section in which he lived had been a huge plantation in the early days of settlement in Memphis. As he walked across the cleared area he quickly saw a large black circle of earth, about six feet in diameter. Thinking this was
probably an old cistern or well associated with the plantation, he stuck his probe in the ground thinking to hit brick—but he hit nothing. He then dropped to his knees and began scraping with a trowel. And what he turned up in the foot or so of black earth was both European and Indian material. This black earth must have been the bottom of a pit of some kind—animal bones and charcoal were also scattered through the dirt and at the bottom of the stained area.

Shortly thereafter the collection was transported to Fayetteville by Mr. Osborn’s son, Audrey, and it was accessioned by the museum on January 31, 1967 (Harry Osborn to Charles R. McGimsey, 1/23/67; temporary receipt for accession number 67-19, University of Arkansas Museum). Despite a request from State Archeologist Hester Davis (Hester Davis to Harry Osborn, 1/29/73), Mr. Osborn apparently provided no further details about his discovery. As a result, we know only that the site was located near Mr. Osborn’s home and that by September 1972: “The spot where I found this pit is now under the center of the living room of a house” (Harry Osborn to Miss [Hester] Davis, 9/9/72). From his letterhead stationary, we know that Mr. Osborn lived near the present-day intersection of Interstate 240 and Interstate 55 South (Figure 1).

![Figure 1. Location of the site.](image)

**Historical Background**

Published documentation on the Memphis area in the late eighteenth and early nineteenth centuries is fairly sparse, and a lengthy recapitulation is beyond the scope of this paper. A few brief vignettes will suffice to establish the context for Indian-Euroamerican relationships during this period.

Stopping at the bluffs forming the future site of Memphis in March 1791, British scientist John Pope noted that: “Here the Chickasaws once had a small Pottery” (1979:24). Pope also observed that: “At the upper End of this Bluff [i.e., near the mouth of the Wolf River—authors] is an old Blockhouse, built by a Captain Befereur’s Company, who had the Convoy of military Stores for the Chickasaws, which they deposited therein...” (Pope 1979:24). The “old Blockhouse” was not a trading post, but rather a facility in which goods were stored temporarily before being transported inland to the Chickasaw towns.

By 1795 some Chickasaws had established permanent residence at the Chickasaw Bluffs. In that year, they ceded a site in what is now the downtown core of Memphis for construction of Fort San Fernando de las Barrancas, with the Spanish-leaning Chickasaw chief Ulathapaye or Ugalalacabe (also known as William or Guillermo Glover and Wolf’s Friend) as signatory delegate (Arnold 1985:21). The British-financed firm Panton and Leslie had a store at Fort San Fernando (Gibson 1971:88; Roper 1970:7), and the Dutch merchant family of Benjamin Foy had conducted trade at the Chickasaw Bluffs since the late 1780s or early 1790s. With the cession of the eastern bank to the United States, Foy removed to Spanish Esperanza on the west bank and continued there as merchant and public official until after the Louisiana Purchase (Arnold 1985:158; Davis 1873:163–164; Goodspeed 1887:778).

S.C. Williams (1928:389–390) provides the 1797 account of Francis Baily’s visit to the area:

At Chickasaw Bluffs there are about five or six families settled, who may be called half-Indians; that is, they are persons who, in habit and manners, are nearly allied to them, and have generally married into the Indian families.

This tract of country belongs to the tribe of Chickasaw Indians, a warlike race, and one that preserves a good understanding with America, which the latter is obliged to keep up by presents sent annually to them.
Several of them appeared on the banks... They soon offered us the pipe of peace, of which each of us having smoked a whiff or two, our introduction to them was completed, and they began to trade with us, and showed us every thing worthy of observation in this place.

In 1802, the United States government established the Chickasaw Bluffs Trading House near the small military post on the bluffs (Fort Adams, later known as Fort Pike and Fort Pickering). In 1809, the value of skins bartered at this factory ($12,070) was the highest amount realized at any of the 14 government trading houses (Gibson 1971:95), almost all of which represented deer hides. The post was moved to the Arkansas River in the fall of 1818 (Plaisance 1952:49–51).

Private traders also operated along the Chickasaw Bluffs during the early 1800s, and it is impossible to attribute the Euroamerican artifacts in the Osborn collection to one or more specific sources. What is clear, however, is that Euroamerican goods were readily obtainable in the Memphis area during the time span suggested by the artifacts in the collection (see below). In the historical sources, there are many references to mixed-ethnicity households and "half-breeds," often with British names indicative of the century-long association of the Chickasaw and English nations. It seems likely that the Osborn collection represents one of these frontier families.

Aboriginal Ceramics

A minimum of six ceramic vessels of aboriginal manufacture are represented in the Osborn collection. The Chickashae Combed vessel fragment is a portion of a casserole, or incurving bowl form, that is very typical of this ceramic type (Figure 2, bottom). The fine, compact paste includes the unidentified white temper characteristic of the ware. Orifice diameter is about 22 cm, and the height of the sherd is 6 cm. The lip is round to slightly flattened. Thickness is quite uniform, ranging from 5.4 mm near the lip to 5.6 mm near the base. The exterior surface is well-polished, and the interior exhibits scrape marks around the circumference. The Chickashae Combed motif consists of slanting polygonal tracts bounded by a 4.7 mm wide brushed zone of wide, shallow, slightly overhanging scratches. In one instance the lines meet at the ends of the figure, but they do not on the other. The decorated zone occurs entirely on and above the vessel shoulder. Chickashae Combed was produced by the Choctaw in southern Missis-

ippi and Oklahoma during the late eighteenth and nineteenth centuries (Blitz 1985, 1995; Mooney 1997; Voss 1995; Voss and Blitz 1988).

Slightly more than one half of a small, globular Wilson Plain jar is represented in the Osborn collection. Dimensions of this reddish, fossil shell tempered vessel are as follows: maximum diameter, 9 cm; height, 6.5 cm; orifice diameter, 7 cm. A strap handle is attached to the recurving rim, and the flaring lip is thinned. The exterior is sooted. Surfaces are roughly-finished, and the vessel appears to have been mass-modeled (i.e., a "pinch pot"). Wilson Plain is strongly associated with historic Chickasaw sites (e.g., Atkinson 1987).

With an orifice diameter of 12 cm, the fragmentary Bell Plain vessel was a wide-necked jar or deep flaring bowl. The orifice is restricted to a diameter of 7 cm about 5 cm below the lip of this single, large rim sherd. Both surfaces are well-polished, and the sharply flaring lip is rounded.

Both sandy textured red-filmed sherds in the collection are slipped on both surfaces and probably represent fragments of a bowl. Gently sloping

![Figure 2. Aboriginal ceramics in the Osborn collection. Top left, Wilson Plain; top right, Bell Plain; bottom, Chickashae Combed.](image-url)
vessel inflection points observed on two sandy textured plain sherds suggests that these probably derive from jars.

Also included in the box that houses that collection is the basal portion of a coarse shell-tempered jar identified as Woodward Plain (Jerry Hilliard, personal communication). This type, typified by flat-based vessels, is associated with earlier Mississippian occupations in the Arkansas Ozarks, and an occurrence in a historic context in Memphis would be very incongruous. Reconstructed from 12 sherds, the preserved portion of the vessel has a maximum diameter of 18 cm at 10.5 cm height. The flat, straight-sided base is 7 cm in diameter and 1.4 cm thick. Wall thickness near the base is 7.7 mm. This vessel lacks accession numbers, which appear on all other aboriginal ceramics in the collection. The unlikely association of the vessel with the other artifacts in the collection, coupled with the lack of accession numbers, causes us to doubt that this vessel was found with the other material reported here, and we suspect that the vessel was misplaced sometime during the 30 years since the Osborn collection was donated.

Daub

The probable functions of three burned clay fragments could be ascertained. A 4 cm thick fragment was applied to riven wood while very wet, resulting in a lumpy exterior surface; the specimen is silty and untempered. Split wood grain impressions occur on Mississippian daub specimens from wattle-and-daub structures, but also are consistent with the mud-and-stick chimneys of post-contact Native American and Euroamerican log cabins.

A second fragment appears to be chinking, in that it was not completely liquefied before application and retains a granular ped structure. The exterior surface of this untempered specimen was smoothed. The final specimen is compact and somewhat lamellar in structure; it may derive from the interior of a hearth or a surface fire.

European Ceramics

The Osborn collection includes fragments of two creamware vessels, at least one of which was completely undecorated. With a rim diameter of about 16 cm, the bowl stood about 6 cm tall above the base, which has a tapering foot ring 8 cm in diameter and 9 mm in height (Figure 3). The material adhering to the surface has not been identified, but it does not react to dilute hydrochloric acid. A plate or soup plate is represented by a base and a rim sherd. Vessel size cannot be estimated from the sherds; the small preserved portion of the foot ring is about 1.5 mm tall.

A base sherd from a pearlware cup exhibits a thin, tapering foot ring that is consistent with a relatively early date of manufacture. The foot ring is approximately 5 cm in diameter, with a height of 7.5 mm. A narrow brown annular ring on the exterior of the ring suggests that the body may have been decorated in a "mocha" style (Figure 4, right).

Blue transfer print designs decorate both surfaces of a pearlware bowl, of which a single rim sherd was recovered. The exterior motif portrays an African male blowing a large horn on the shore of, or overlooking, a lake, while the border print on the interior includes a vignette of a rectangular structure with a chimney surrounded by two kinds of geometric filler (Figure 4, left). The lip is covered with a yellow-brown glaze— an odd choice for a blue and white vessel. The horn-player motif appears as an element on two documented early Staffordshire patterns, "Ethiopian Horn Player" and "Hunting with Cheetahs," both of which are unattributed and dated to circa 1810-1820 (Roberts 1998:58). The border of the latter pattern is very similar to the example in the Osborn collection.

Figure 3. Creamware bowl fragments.
Metal Artifacts

There are three badly corroded iron objects in the collection. Measuring 16 cm in length, the case knife has a rounded handle on which most of the wood has deteriorated. Corrosion obscures any markings that may be present on the blade. A portion of an iron serving spoon is 17.5 cm long, with a bowl 7 cm long; an unknown amount of the handle is missing. The preserved part of an iron hasp—probably for use with a chest, rather than a door—measures 17 cm long; the maximum width is 3.5 cm. The single hand-wrought nail is 12.7 cm long.

A pair of oval brass cufflinks, measuring 1.55 cm long and 1.0 cm wide, is useful for narrowing the age range of the assemblage. The face of the better-preserved specimen is almost completely covered with the relief bust of a colonial gentleman, who is identified immediately below as "JO ADAMS" (Figure 5). John Adams was the second President of the United States (1797–1801) and served two terms as Vice President under George Washington. Presumably these decorative items were produced during Adams’s popular presidency. A similar cufflink was found during excavations at the Tellico Blockhouse, which was occupied between 1794 and 1807 (Polhemus 1979: Plate 30M).

One arm is missing from the small brass or German silver cross (Figure 6, left). The complete specimen consisted of 10 joined, circular sections and a suspension ring at the top. Five sections and one common section form the length, while four sections and the common section form the arms. Length of the long axis, excluding the ring, is 2.88 cm.

There are three German silver ribbon brooches in the Osborn collection (Figure 6). Two are 2.9 cm in diameter, the other, 1.18 cm. All are convex/concave in cross-section. A thin, circular brass disk, 1.96 cm in diameter, exhibits beveling on one surface. In the center is a rectangular opening, one end of which has been damaged (Figure 6). This object is probably the back of a compound button. A small, conical brass tinker measuring 1.9 cm in length was also collected (Figure 6, right).

Faunal Remains

A total of 58 faunal specimens is preserved in the collection (Table 1). All were analyzed by Dr. Emanuel Breithburg and are summarized below in...
tabular form. The assemblage represents a mixture of domestic and wild mammals (domestic swine, white-tailed deer, raccoon, and possibly domestic cattle), birds (domestic chicken, wild turkey), and shellfish (fat mucket). All cut marks appear to have been made by a metal knife. One large mammal element is highly polished and exhibits a finely incised angular zigzag pattern.

Concluding Remarks

The Harry Osborn collection contains an interesting variety of Native American and Euroamerican artifacts. The cufflinks presumably postdate John Adams’s election as president (1797), while the European ceramics almost certainly are of pre-1820 age. Although the age of these artifacts can be established with some confidence, it should go without saying that the date of deposition is unknown.

The range of materials suggests a domestic context and, accepting the three pieces of daub as evidence of a chimney or chinking, that the “pit” in which the artifacts were discovered was associated with a log cabin—perhaps as a shallow cellar. With its mixture of Euroamerican and aboriginal objects, as well as domestic and wild animals, the artifact assemblage raises the possibility that the Osborn collection reflects circa 1800–1820 occupation by a “half-Indian” family like those described by Francis Baily in 1797 (S. C. Williams 1928:389–390). At least one member of the household probably was a Native American woman who produced traditional pottery, although the considerable variety in the Osborn collection suggests that some of the vessels represented may not be of local manufacture. Indeed, the pastes of the Wilson Plain and Chickachae Combed vessels are macroscopically typical of their respective north and south Mississippi centers of distribution. Parenthetically, the co-occurrence of Chickasaw (Wilson Plain) and Chocotaw (Chickachae Combed) ceramics is noteworthy, although not surprising, insofar as a mixed village of Chickasaws and Chocotaws was present in Chickasaw County, Mississippi, in 1805 (Atkinson 1987:35). Finally, it should be noted that the Osborn collection provides very good documentation of the persisting use of aboriginal ceramics into the first quarter of the nineteenth century.

No artifact assemblages comparable to the Osborn collection are documented from western Tennessee. It seems likely that similar sites were present

Table 1. Faunal remains.

<table>
<thead>
<tr>
<th>Taxa</th>
<th>Number</th>
<th>MNI</th>
<th>Burned</th>
<th>Cut</th>
<th>Modified</th>
</tr>
</thead>
<tbody>
<tr>
<td>Bos taurus (1), Domestic cattle</td>
<td>2</td>
<td>1</td>
<td>—</td>
<td>—</td>
<td>—</td>
</tr>
<tr>
<td>Odocoileus virginianus, White-tailed deer</td>
<td>9</td>
<td>1</td>
<td>2</td>
<td>3</td>
<td>—</td>
</tr>
<tr>
<td>Sus scrofa, Domestic swine</td>
<td>3</td>
<td>1</td>
<td>—</td>
<td>1</td>
<td>—</td>
</tr>
<tr>
<td>Mammal (pig/deer-sized)</td>
<td>22</td>
<td>—</td>
<td>—</td>
<td>3</td>
<td>—</td>
</tr>
<tr>
<td>Procyon lotor, Raccoon</td>
<td>3</td>
<td>1</td>
<td>—</td>
<td>1</td>
<td>—</td>
</tr>
<tr>
<td>Mammal</td>
<td>2</td>
<td>—</td>
<td>1</td>
<td>—</td>
<td>1</td>
</tr>
<tr>
<td>Meleagris gallopavo, Wild turkey</td>
<td>13</td>
<td>5</td>
<td>1</td>
<td>2</td>
<td>—</td>
</tr>
<tr>
<td>Gallus gallus, Domestic chicken</td>
<td>1</td>
<td>1</td>
<td>—</td>
<td>—</td>
<td>—</td>
</tr>
<tr>
<td>Bird</td>
<td>2</td>
<td>—</td>
<td>—</td>
<td>—</td>
<td>—</td>
</tr>
<tr>
<td>Lampsis radiata lutula, Fat mucket</td>
<td>1</td>
<td>1</td>
<td>—</td>
<td>—</td>
<td>—</td>
</tr>
</tbody>
</table>

TOTAL: 58  11  4  10  1
in the greater Memphis area, but have been destroyed by urban expansion. Indeed, without the initiative of Harry Osborn, an amateur archaeologist who happened to live near an area being cleared for development, the artifacts reported here would have remained unknown.

Acknowledgments

Michael Hoffman and Mary Suter (University of Arkansas Museum) generously allowed us to examine and photograph the Osborn collection. Emanuel Breitburg examined the faunal material. John R. Eatman provided useful references on the early history of Memphis. The photographs were taken by Jared Pelworth; Mike Evans produced Figure 1. Others who helped us in various ways include Rita Carroll, David Jeane, Mary Kwas, Charles McNutt, and Skip Stewart-Abernathy.

Mary Evelyn Starr is a Station Assistant with the Arkansas Archeological Survey. Robert Mainfort is Sponsored Research Administrator, Arkansas Archeological Survey, and Associate Professor of Anthropology, University of Arkansas.

References

Arnold, Morris S.

Arkinson, James R.

Blitz, John H.

Davis, James D.
1873 The history of the city of Memphis. Hix, Crumpson & Kelly, Memphis.

Fisher-Carroll, Rita
1997 Sociopolitical organization at Upper Nodena (3MS4) from a mortuary perspective. M.A. thesis, Department of Anthropology, University of Arkansas.

Gibson, Arrell M.

Goodspeed Publishing Company

Mainfort, Robert C., and Rita Carroll

Mooney, Timothy

Plaisance, Aloysius

Polhemus, Richard

Pope, John

Roberts, Gaye Blake

Roper, James

Voss, Jerome A.
Voss, Jerome A., and John H. Blitz

Williams, Samuel Cole

---

The Potential for African-American Archaeology at Mount Locust (22-Je-522), Natchez Trace Parkway, Jefferson County, Mississippi

Amy L. Young

Abstract

Mount Locust (22-Je-522) is known as an inn on the Natchez Trace in Jefferson County, Mississippi. When steamboats on the Mississippi River replaced the Natchez Trace as a major transportation route, the importance of Mount Locust as an inn declined and slave labor transformed it into a prosperous cotton plantation. Archaeological reconnaissance was conducted at Mount Locust to locate the position of the slave houses, and to answer some basic questions and build a database concerning African-American lifeways in the Natchez District of Mississippi.

Introduction

In 1998, an archaeological survey conducted by the summer field school of The University of Southern Mississippi was begun at Mount Locust in Jefferson County, Mississippi. The site of Mount Locust was originally established as an inn on the Natchez Trace in the late eighteenth century, but as the Trace dwindled in importance as a transportation route, the property was transformed through enslaved labor into a prosperous cotton plantation. The objective of the 1998 survey was to discover the location of the slave houses and to assess the potential of intensive investigations for uncovering information about African Americans associated with the inn and plantation. Because the property is part of the Natchez Trace Parkway, more information concerning the African-American heritage is actively being sought for interpretation for visitors (Thomas 1998), and the current archaeology at Mount Locust is part of that larger effort. The 1998 reconnaissance consisted of shovel test pits (approximately 30x30x30 cm) excavated at five and ten meter intervals in areas where it was believed slave cabins once stood. This field method
allowed for a large area to be systematically explored with a minimum of disturbance. Materials from the shovel tests were identified and quantified, entered into a database and exported into software to generate maps showing areal concentrations. Three areas with heavy quantities of materials were detected and recommended for more intensive testing. Although the area where the slave cabins likely stood was plowed in the past, concentrations of brick and other domestic debris suggest that clusters of artifacts representing individual houses can be fruitfully investigated in the future.

Mount Locust is located in the Natchez District, an important region in the archaeology of slavery and African-American life. The Natchez District, found along the Mississippi River in the southwest part of the state (Figure 1), has a long history of slavery, beginning in the early 1700s with French settlement, but slavery was not firmly established in the Natchez District until the 1770s, when the region was under British rule. British colonists brought numerous slaves with them when they settled the area. The Spanish ruled the area from 1779 until 1798, during which a plantation economy based on the cultivation of tobacco emerged. The Spanish government subsidized tobacco production in the Natchez District by purchasing the tobacco at prices above market value (Moore 1988). This spurred a growth in plantations and the importation of numerous enslaved Africans and African-Americans. A 1784 census indicates that 1121 whites and 498 slaves were living in the Natchez District (Singleton 1991:23). In 1798, when the Spanish left Mississippi, the estimated population of the Natchez District (town and surrounding countryside) was around 6000: 2400 slaves and 3600 whites (Singleton 1991:23).

While tobacco cultivation was important in the early development of Natchez plantations, cotton became the dominant crop during the nineteenth century. When the Natchez planters were no longer receiving help from the Spanish, they were forced to experiment with indigo and upland cotton. The arrival of the cotton gin in the Natchez District signaled the beginning of the cotton boom. According to Singleton (1991), the Natchez District attracted or created some of the South’s richest planter elite. In 1860, approximately 70% of the farmers in Adams County, in the Natchez District, owned over 50 slaves, and it was not

Figure 1. Location of Mount Locust in Natchez District.
uncommon for planters to own hundreds of slaves and several plantations (Davis 1994:20–22; Moore 1988; Singleton 1991:23). The size of the slaveholdings in the Natchez District is extraordinary, especially since in most other areas of the South only the wealthiest planters owned 50 or more slaves, and such planters comprised a minority rather than the average.

Because of the wealth and numerous plantations located in the Natchez District, archaeological investigations here should yield important information about slavery and African-American culture. Despite the number of plantations and the importance of the slave society, the only other work on a cotton plantation in the Natchez District aimed specifically at gaining information about slavery was conducted at Saragossa Plantation beginning in 1997 (Young 1998a,b; Tuma 1998). Data from Mount Locust therefore provides valuable comparative data in the investigation of the lives of enslaved African Americans in this region.

Historical Background

Mount Locust was established by William Ferguson around 1783, when he married Paulina Burch. Today Ferguson’s house, much altered from its original form, is part of the Natchez Trace Historical Park and is open to the public. Oral histories conducted in the 1930s and 1940s reveal that Mount Locust operated as an inn on the Natchez Trace. The property remained in the hands of Ferguson/Burch descendants until 1937, when it was acquired by the National Park Service (Phelps 1941, 1947).

Documentary evidence of the African-American slaves at Mount Locust is infuriatingly scant. The estate inventory of William Ferguson, who died around 1800, included real and personal property, but no slaves (Phelps 1941:24). The transformation into a cotton plantation began after William Ferguson died, by which time the family was a significant slaveholder. His widow, Mrs. P. Ferguson, appears in the 1820 federal census of Jefferson County, Mississippi (United States Bureau of Census [USBC] 1820) with 26 slaves, and in 1830 with 42 slaves. Unfortunately, the 1840 census of Jefferson County microfilm in Cook Library at the University of Southern Mississippi was illegible and Mrs. Ferguson was not found.

Mrs. Paulina Ferguson died in 1849. Mr. T. J. (Thomas Jefferson) Chamberlain, the next owner of Mount Locust, appears in the 1850 census of Jefferson County. At this time, he is listed on the population schedule (USBC 1850) as 35 years of age, living with M. Chamberlain, a female aged 25, Paulina Chamberlain, a female aged four, Duncan Chamberlain, aged two months, Thomas E. Graves, aged 30, an overseer, Jane Graves, aged 26, and S.C. Graves, a female aged four months. The slave schedule for 1850 shows Mr. Chamberlain with 35 slaves; 22 are male, 13 are female. Incidentally, Mr. Graves is also listed on the same page of the slave schedule as having two slaves, a five-year-old male and a 25-year-old female.

The 1860 slave schedule for Jefferson County shows Mr. T. Chamberlain with 72 slaves; 34 are male, 38 are female (USBC 1860). This census also indicates that there were 16 slave houses, which means that each slave house held, on average, four or five slaves. While the size of the slave holding at Mount Locust is above the average for the area, the number of slaves per house is somewhat low (Young 1998a).

These data corroborate the oral historical study completed in the 1930s and 1940s. Interviews with elderly residents (descendants of the Chamberlains) suggested that there were between 12 and 16 slave houses at Mount Locust. Evidently by the late antebellum period Mount Locust was a flourishing cotton plantation.

Previous Archaeological Investigations at Mount Locust

Archaeological investigations at Mount Locust were conducted in 1940 and 1941 by A. C. Spaulding under the supervision of J. D. Jennings (Phelps 1941:33–48). Concurrent with the archaeological and architectural investigations were interviews with the Chamberlains concerning what they remembered of the property. The major purpose of the archaeological investigations was to map the locations of the slave houses, the travelers’ sleeping house popularly called “Sleepy Hollow,” the overseer’s house, the kitchen, and other buildings associated with the property. The testing was also carried out to determine if an early Spanish fort with stockade and moat had once sat at the location (as local legend suggested), and to determine if the main house was situated on an Indian mound. These investigations identified a number of archaeo-
logical features that pertain to the occupation of Mount Locust between 1783 and the early twentieth century. It was determined that the house sat on a natural knoll, and no traces of Spanish occupation were found.

Testing consisted of trenches (3–5 feet in width) excavated where informants recalled that buildings stood. The kitchen was identified as a concentration of brick rubble located about 70 feet west of the main house (Phepols 1941:37). Not far north of the kitchen rubble, a paved brick floor was uncovered. This floor measured approximately 15 by 10 feet. The function was not determined.

Another concentration of brick rubble was tentatively identified as the guest house. The overseer’s house was also located and identified as a brick concentrations, including a possible brick pier. A number of brick walls were uncovered around the main house. A brick kiln was identified approximately 540 feet southwest of the main house. It was described in the report as a brick floor overlain with considerable brick rubble.

According to Chamberlain family oral histories from the 1940s, a slave nursery, where children were tended while parents worked in the fields, was located between the overseer’s house and the slave quarters. The area was tested, but no evidence of this building was located. Over 16,000 square feet of trenches were placed north of the main house in an attempt to locate remains of the slave quarters. No direct evidence of the houses was recognized, but a light scatter of artifacts was recorded.

A comparison of the maps of the archaeological trenches with the maps of where the Chamberlains remember the slave houses stood suggests that the trenches were placed too far west. Two features, Feature 4 and Feature 5, on the eastern edge of the area tested by Spaulding, may relate to the westernmost slave houses. These features were not described in the report (Phepols 1941), but were likely brick concentrations.

Unfortunately, the report of the 1941 archaeological dig at Mount Locust does not provide the detail necessary to understand the site fully. No artifacts are described. In fact, the only artifact mentioned by name in the report is brick. Also, the curation location of any artifacts recovered in the dig is now unknown. The data do indicate, however, that archaeological evidence of the slave houses at Mount Locust may still exist.

Archaeological Testing at Mount Locust: The 1998 Field Season

A primary datum point was driven into the ground at the northwest corner of the main house at Mount Locust. This datum point was designated N2000, E2000. Because buildings in the rear yard (and presumably the slave cabins and other outbuildings) would be aligned with the main house, the grid was aligned with grid north 49 degrees east of magnetic north (Figure 2).

The field behind the main house was plowed in the twentieth century. During the 1998 field season, it was divided into eight sections, and shovel tests were excavated at every 10 meter interval. In parts of two sections, additional shovel tests were excavated at five meter intervals, because the 10 meter testing yielded numerous nineteenth-century domestic artifacts. All shovel test pits were dug with a shovel and the soil was screened through quarter-inch mesh. All artifacts, rocks, and brick fragments were collected.

In addition to the 327 shovel tests excavated at Mount Locust, six one-meter units were placed in areas where nineteenth-century domestic debris was heaviest and where the maps associated with the early archaeology at Mount Locust indicated the possible location of the slave houses. Due to time constraints, only four of these unit excavations were completed. The units were excavated in arbitrary 5 cm levels, and all artifacts were collected and bagged according to unit level. It was hoped that some evidence of sub-plowzone deposits associated with the slave quarters may remain intact. While no intact sub-plowzone deposits were identified, these units did only cover a limited area, and the tests do not rule out the possibility that such less-disturbed deposits may yet be located. Unfortunately, evidence of the 1940s archaeological investigations was also not identified in the field and therefore it is not possible to locate precisely the position of the earlier trenches.

Artifacts from Shovel Test Pits

Brick from each shovel test pit was weighed and discarded at the site. Of the 327 shovel test pits excavated at Mount Locust, 225 yielded a total of 3908.5 grams of brick and 905 artifacts, ecofacts, or by-products. For purposes of counts, brick was either counted as present (n=1)
or absent (n=0). Artifact frequencies in the shovel test pits ranged from zero to 35.

The remainder of the artifacts were transferred to the Historical Archaeology Laboratory at USM, where they were washed, identified, quantified, and analyzed. Window glass thickness was measured with digital calipers. Excluding brick fragments, slag, coal, and charcoal, all materials were assigned a size category using a bullseye chart. Size category 1 artifacts measure less than 2 mm. Size category 2 artifacts measure less than 4 mm. Size category 3 artifacts measure less than 6 mm, etc. Additionally, where possible, artifacts were assigned to a functional group, following a modified version of South's classification (1977). The groups include unidentified, architecture, kitchen, personal, tobacco, arms, activities, prehistoric, and clothing. The frequencies within the functional groups are presented in Table 1.

Frequencies of individual artifacts recovered in the shovel test pits appear in Table 2. The most common artifact by frequency was brick, followed by nails and nail fragments. Charcoal, slag, ceramics, unidentified iron, container glass, prehistoric pottery, and chert flakes were also found in fairly high frequencies.

The primary objective of the shovel testing at Mount Locust was to locate evidence of possible structures, especially remains associated with the slave quarters that once stood behind the main house. The distribution of brick and other artifacts may help locate concentrations that correspond to brick chimney foundations or piers, and activities that took place in and around the slave cabins. Brick, nails, unidentified iron, ceramics, container glass, and slag were found in sufficient quantities to generate distribution maps.

Brick, the most common artifact recovered, was fairly ubiquitous, but several major clusters (Figure 3) appear at N2070 E1934, N2180 E1964,

Table 1. Frequencies of Mount Locust shovel test pit artifacts in functional groups.

<table>
<thead>
<tr>
<th>Group</th>
<th>Frequency</th>
<th>Percentage</th>
</tr>
</thead>
<tbody>
<tr>
<td>Personal</td>
<td>1</td>
<td>0.1</td>
</tr>
<tr>
<td>Clothing</td>
<td>4</td>
<td>0.4</td>
</tr>
<tr>
<td>Kitchen</td>
<td>89</td>
<td>8.0</td>
</tr>
<tr>
<td>Prehistoric</td>
<td>95</td>
<td>8.6</td>
</tr>
<tr>
<td>Activities</td>
<td>150</td>
<td>13.6</td>
</tr>
<tr>
<td>Unidentified</td>
<td>348</td>
<td>31.4</td>
</tr>
<tr>
<td>Architecture</td>
<td>419</td>
<td>37.9</td>
</tr>
<tr>
<td>TOTAL</td>
<td>1106</td>
<td>100</td>
</tr>
</tbody>
</table>
Table 2. Artifacts recovered from shovel test pits at Mount Locust.

<table>
<thead>
<tr>
<th>Object</th>
<th>Frequency</th>
</tr>
</thead>
<tbody>
<tr>
<td>Brick</td>
<td>201</td>
</tr>
<tr>
<td>Cut nail</td>
<td>194</td>
</tr>
<tr>
<td>Charcoal</td>
<td>161</td>
</tr>
<tr>
<td>Slag</td>
<td>88</td>
</tr>
<tr>
<td>Whiteware</td>
<td>88</td>
</tr>
<tr>
<td>Unidentified iron</td>
<td>81</td>
</tr>
<tr>
<td>Unidentified container glass</td>
<td>51</td>
</tr>
<tr>
<td>Pottery</td>
<td>45</td>
</tr>
<tr>
<td>Chert flake</td>
<td>43</td>
</tr>
<tr>
<td>Olive glass</td>
<td>37</td>
</tr>
<tr>
<td>Pearware</td>
<td>23</td>
</tr>
<tr>
<td>Unidentified glass</td>
<td>10</td>
</tr>
<tr>
<td>Window glass</td>
<td>10</td>
</tr>
<tr>
<td>Stoneware</td>
<td>9</td>
</tr>
<tr>
<td>Wire nail</td>
<td>8</td>
</tr>
<tr>
<td>Chert</td>
<td>6</td>
</tr>
<tr>
<td>Button</td>
<td>4</td>
</tr>
<tr>
<td>Coal</td>
<td>4</td>
</tr>
<tr>
<td>Porcelain</td>
<td>4</td>
</tr>
<tr>
<td>Bone</td>
<td>3</td>
</tr>
<tr>
<td>Ironstone</td>
<td>3</td>
</tr>
<tr>
<td>Tumbler</td>
<td>3</td>
</tr>
<tr>
<td><strong>TOTAL:</strong></td>
<td><strong>1106</strong></td>
</tr>
</tbody>
</table>

<table>
<thead>
<tr>
<th>Object</th>
<th>Frequency</th>
</tr>
</thead>
<tbody>
<tr>
<td>Unidentified refined ceramic</td>
<td>3</td>
</tr>
<tr>
<td>Unidentified nail</td>
<td>3</td>
</tr>
<tr>
<td>Yellow ware</td>
<td>3</td>
</tr>
<tr>
<td>Fence staple</td>
<td>2</td>
</tr>
<tr>
<td>Mortar</td>
<td>2</td>
</tr>
<tr>
<td>Wire</td>
<td>2</td>
</tr>
<tr>
<td>Bottle</td>
<td>1</td>
</tr>
<tr>
<td>Charcoal pencil</td>
<td>1</td>
</tr>
<tr>
<td>Chert shatter</td>
<td>1</td>
</tr>
<tr>
<td>Early stage biface</td>
<td>1</td>
</tr>
<tr>
<td>Flask</td>
<td>1</td>
</tr>
<tr>
<td>Iron ring</td>
<td>1</td>
</tr>
<tr>
<td>Iron spring</td>
<td>1</td>
</tr>
<tr>
<td>Jar</td>
<td>1</td>
</tr>
<tr>
<td>Lime</td>
<td>1</td>
</tr>
<tr>
<td>Paper</td>
<td>1</td>
</tr>
<tr>
<td>Pewter spoon</td>
<td>1</td>
</tr>
<tr>
<td>Redware</td>
<td>1</td>
</tr>
<tr>
<td>Rubber</td>
<td>1</td>
</tr>
<tr>
<td>Spike</td>
<td>1</td>
</tr>
<tr>
<td>Unidentified metal</td>
<td>1</td>
</tr>
</tbody>
</table>

N2100 E1964, N2050 E1954, N1990 E1964, and N2060 E1914. The first and largest concentration (N270 E1934) and the smallest concentration (N2060 E1914) are in suspected slave house locations, since they coincide with the location of slave houses on the "memory map" of Mount Locust based on Chamberlain recollections. The small concentrations at N2050 E1954 and N1990 E1964 may correspond to either "Sleepy Hollow" or the overseer's house.

A total of 205 nails and nail fragments was recovered at Mount Locust. No hand wrought nails were identified, but 194 cut nails manufactured primarily between 1790 and 1890 (Loveday 1983), eight wire nails manufactured after 1890, and three unidentified nails were identified. If any of the buildings dating to the antebellum period were in use in the late nineteenth or early twentieth centuries, then wire nails should comprise a small part of the total nail assemblage (Young 1994).

Three major concentrations of nails were found in the shovel test pits (Figure 4). One is centered at N2070 E1934, the same location as the largest brick concentration and possibly reflecting the location of slave houses. Another nail concentration is located at N2000 E1944, which corresponds to the location of "Sleepy Hollow" or the overseer's house. The third concentration of nails is found at N2100 E2004.

A total of 81 pieces of unidentified iron were also recovered in shovel tests (Figure 5). Most clustered around N2100 E1994. A much lighter
concentration is located at N2060 E1964. The iron concentrations do not coincide with any suspected slave house locations. It is likely that the iron and brick found in this area is related to a structure of another function.

Slag was also found in fairly high frequencies in the shovel test pits (n=88). Figure 6 presents the distribution of slag that appears to concentrate at N2090 E2004. The coincidence of iron and slag in this vicinity is a likely indicator of the presence of a blacksmith shop.

Domestic debris such as container glass and ceramics (tablewares and ceramics used for food storage and preparation) can also help pinpoint the location of dwellings. Most of the container glass consists of unidentified pieces, but also includes bottles and flasks, especially olive glass commonly associated with spirits like wine. A total of 94 sherds of container glass (Table 3) comprises three major clusters (Figure 7). One of the large concentrations appears at N2060 E1914, at the location of possible slave houses. Another is found at N1980 E1964 ("Sleepy Hollow"). The third is located at N2180 E1974.
Table 3. Container glass frequencies and percentages from shovel test pits at Mount Locust.

<table>
<thead>
<tr>
<th>Container Type</th>
<th>Frequency</th>
<th>Percentage</th>
</tr>
</thead>
<tbody>
<tr>
<td>Unidentified</td>
<td>51</td>
<td>54.2</td>
</tr>
<tr>
<td>Olive Glass</td>
<td>37</td>
<td>39.4</td>
</tr>
<tr>
<td>Tumbler</td>
<td>3</td>
<td>3.2</td>
</tr>
<tr>
<td>Bottle</td>
<td>1</td>
<td>1.1</td>
</tr>
<tr>
<td>Flask</td>
<td>1</td>
<td>1.1</td>
</tr>
<tr>
<td>Jar</td>
<td>1</td>
<td>1.1</td>
</tr>
<tr>
<td><strong>TOTAL</strong></td>
<td><strong>94</strong></td>
<td><strong>100.1</strong></td>
</tr>
</tbody>
</table>

A total of 134 sherds of ceramics was recovered from shovel test pits at Mount Locust. Table 4 presents the frequencies by ware-types. There are no very heavy concentrations, but light scatters at N2080 E1920, N2080 E1934, N2070 E1934 and N2030 E1934 may represent broken dishes found around one or more slave houses (Figure 8). The distribution of ceramics in Figure 8 does not reflect three important finds from a shovel test pit at the suspected location of slave houses: a nearly complete plate base with a backmark and a recessed footring along with a sherd of green shell-edge. Such complete artifacts are usually found in protected contexts, such as beneath structures; these finds offer hope that some undisturbed (unplowed) deposits relating to the slave houses may remain intact. Another small concentration of ceramics is found at N1980 E1944 and may represent “Sleepy Hollow” or the kitchen. There is also a small scatter of ceramics located at N2080 E1994.

Artifacts—One-by-One-Meter Units

Because of plowing, the six one-meter units were excavated in 5 cm arbitrary levels. There were no obvious soil color changes. The loess soils...
were very dry and powdery. The soil color gradually changed from a medium brown (10YR4/3) to a paler brown (10YR6/3), to yellowish brown (10YR5/6) as sterile soil was reached.

All artifacts from the units were processed, catalogued, and analyzed in the same manner as those from shovel test pits. Table 5 presents a breakdown of artifact categories from each of the units at Mount Locust.

Unit 1 was excavated to a depth of 25 cm, when sterile soil was reached, and contained 128 artifacts (including 1057 grams of brick). The artifacts were fairly evenly spread throughout the five levels. The heavy quantity of brick suggests that a remnant of a brick chimney foundation or a brick pier foundation may be nearby.

Unit 2 was also excavated to sterile soil at 25 cm. It contained 212 artifacts (including 1041 grams of brick). The assemblage from Unit 2 was dominated by architectural artifacts (see Table 5), consisting mostly of cut nails and cut nail fragments. The heavy quantity of brick from Unit 2 may indicate a nearby brick chimney or pier foundation.

Unit 3 was excavated to a depth of 26 cm in six levels. Level 6 was approximately one centimeter deep as sterile soil was reached. The entire unit yielded 102 artifacts (including 516 grams of brick).

Unit 4 was excavated to a depth of 20 cm in four levels. It yielded 115 artifacts (including 346 grams of brick). The bone was in very poor condition. Less brick was recovered from Units 4, 5, and 6 than the previously described units, suggesting that these units were further from brick piers and fireplaces.

Unit 5 was excavated to a depth of 10 cm in two levels. This unit was not completely excavated. The entire unit yielded 49 artifacts (including 344 grams of brick).

Unit 6 was also excavated to a depth of 10 cm in two levels. This unit was not completed. The unit contained 73 artifacts (including 238 grams of brick). The relatively high frequency of artifacts from this incomplete unit indicates that further work in this immediate vicinity should take place in the future.

The variation in the quantity of brick (over 1000 grams in two of the units, double the quantity in any of the others) and the numbers of artifacts recovered in the excavation units suggest that clusters of artifacts associated with individual slave houses may yet exist at Mount Locust, even though the field has been plowed. This may also indicate that undisturbed deposits (below plowzone) may also exist, although none were discovered during this reconnaissance.

Many of the ceramics recovered in the excavation of the six units, like many of the ceramics from the shovel test pits, indicate a late-eighteenththrough mid-nineteenth-century manufacturing date. No creamware was located, and only a single, very small piece of white saltglaze (1720–1770) was found, possibly a currated vessel. Whiteware (manufactured after 1830) dominated the ceramic assemblage, although pearlware (1780–1830) was also recovered. The most common decorative motif was shell edge (blue and green) found on whiteware and pearlware. Transfer-printed and painted vessels were also excavated and identified. Plates outnumber other identified vessel forms (Table 6). The frequencies of the different decorative motifs are shown in Table 7.

Most of the decorated ceramics date to the antebellum period. The paucity of porcelain and the relatively high frequencies of edge decorated and other minimally decorated vessels fits with most interpretations of material culture of enslaved families (Adams and Boling 1989). The few more expensive pieces, such as transfer printed plates and the tea wares, may indicate hand-me-downs from the main house. Additionally, the identifiable vessel forms show that plates and platters are the most com-

<table>
<thead>
<tr>
<th>Category</th>
<th>Unit 1</th>
<th>Unit 2</th>
<th>Unit 3</th>
<th>Unit 4</th>
<th>Unit 5</th>
<th>Unit 6</th>
<th>Total</th>
</tr>
</thead>
<tbody>
<tr>
<td>Unident</td>
<td>61</td>
<td>56</td>
<td>47</td>
<td>55</td>
<td>33</td>
<td>55</td>
<td>304</td>
</tr>
<tr>
<td>Activities</td>
<td>6</td>
<td>6</td>
<td>1</td>
<td>1</td>
<td>0</td>
<td>0</td>
<td>14</td>
</tr>
<tr>
<td>Architect (brick)</td>
<td>48(1057g)</td>
<td>126(1041g)</td>
<td>42(516g)</td>
<td>37(946g)</td>
<td>113(444g)</td>
<td>160(283g)</td>
<td>280(3542g)</td>
</tr>
<tr>
<td>Clothing</td>
<td>1</td>
<td>0</td>
<td>0</td>
<td>1</td>
<td>0</td>
<td>0</td>
<td>2</td>
</tr>
<tr>
<td>Kitchen</td>
<td>6</td>
<td>15</td>
<td>7</td>
<td>8</td>
<td>2</td>
<td>4</td>
<td>42</td>
</tr>
<tr>
<td>Plush</td>
<td>6</td>
<td>7</td>
<td>5</td>
<td>13</td>
<td>2</td>
<td>1</td>
<td>34</td>
</tr>
<tr>
<td>Tobacco</td>
<td>0</td>
<td>2</td>
<td>0</td>
<td>0</td>
<td>1</td>
<td>0</td>
<td>3</td>
</tr>
<tr>
<td><strong>TOTAL:</strong></td>
<td>128</td>
<td>212</td>
<td>102</td>
<td>115</td>
<td>49</td>
<td>73</td>
<td>679</td>
</tr>
</tbody>
</table>
Table 6. Ceramics from unit excavations at Mount Locust.

<table>
<thead>
<tr>
<th>Object</th>
<th>Freq</th>
<th>Form</th>
<th>Part</th>
<th>Interior Dec</th>
<th>Exterior Dec</th>
</tr>
</thead>
<tbody>
<tr>
<td>Ironstone</td>
<td>1</td>
<td>plate</td>
<td>marley</td>
<td>blue tp*</td>
<td></td>
</tr>
<tr>
<td>Pearlware</td>
<td>1</td>
<td>hollow</td>
<td>footing</td>
<td></td>
<td></td>
</tr>
<tr>
<td>Pearlware</td>
<td>1</td>
<td>plate</td>
<td>footing</td>
<td></td>
<td></td>
</tr>
<tr>
<td>Pearlware</td>
<td>1</td>
<td>plate</td>
<td>rim</td>
<td>blue shell</td>
<td></td>
</tr>
<tr>
<td>Pearlware</td>
<td>1</td>
<td>plate</td>
<td>rim</td>
<td>green shell</td>
<td></td>
</tr>
<tr>
<td>Pearlware</td>
<td>1</td>
<td>saucer</td>
<td>rim</td>
<td>blue</td>
<td></td>
</tr>
<tr>
<td>Pearlware</td>
<td>2</td>
<td>bottle</td>
<td>body</td>
<td>slip/salt</td>
<td></td>
</tr>
<tr>
<td>Stoneware</td>
<td>1</td>
<td>hollow</td>
<td>body</td>
<td>orange salt</td>
<td></td>
</tr>
<tr>
<td>White saltglaze</td>
<td>1</td>
<td></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>Whiteware</td>
<td>1</td>
<td></td>
<td></td>
<td>black tp</td>
<td></td>
</tr>
<tr>
<td>Whiteware</td>
<td>1</td>
<td></td>
<td></td>
<td>poly pt*</td>
<td></td>
</tr>
<tr>
<td>Whiteware</td>
<td>1</td>
<td></td>
<td></td>
<td>black tp</td>
<td></td>
</tr>
<tr>
<td>Whiteware</td>
<td>1</td>
<td></td>
<td></td>
<td>blue pt</td>
<td></td>
</tr>
<tr>
<td>Whiteware</td>
<td>1</td>
<td></td>
<td></td>
<td>blue tp</td>
<td></td>
</tr>
<tr>
<td>Whiteware</td>
<td>1</td>
<td></td>
<td></td>
<td>brown tp</td>
<td></td>
</tr>
<tr>
<td>Whiteware</td>
<td>1</td>
<td></td>
<td></td>
<td>poly pt</td>
<td></td>
</tr>
<tr>
<td>Whiteware</td>
<td>1</td>
<td></td>
<td></td>
<td>purple tp</td>
<td></td>
</tr>
<tr>
<td>Whiteware</td>
<td>1</td>
<td></td>
<td></td>
<td>body</td>
<td>blue pt</td>
</tr>
<tr>
<td>Whiteware</td>
<td>1</td>
<td>footing</td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>Whiteware</td>
<td>1</td>
<td>rim</td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>Whiteware</td>
<td>1</td>
<td>rim</td>
<td>black tp</td>
<td></td>
<td></td>
</tr>
<tr>
<td>Whiteware</td>
<td>1</td>
<td>cup</td>
<td>body</td>
<td>brown tp</td>
<td>brown tp</td>
</tr>
</tbody>
</table>

* transfer print # painted

Table 6, continued. Ceramics from unit excavations at Mount Locust.

<table>
<thead>
<tr>
<th>Object</th>
<th>Freq</th>
<th>Form</th>
<th>Part</th>
<th>Interior Dec</th>
<th>Exterior Dec</th>
</tr>
</thead>
<tbody>
<tr>
<td>Whiteware</td>
<td>1</td>
<td>cup</td>
<td>rim</td>
<td>green tp</td>
<td>green tp</td>
</tr>
<tr>
<td>Whiteware</td>
<td>1</td>
<td>flat</td>
<td>body</td>
<td></td>
<td></td>
</tr>
<tr>
<td>Whiteware</td>
<td>1</td>
<td>flat</td>
<td>body</td>
<td>blue tp</td>
<td></td>
</tr>
<tr>
<td>Whiteware</td>
<td>1</td>
<td>hollow</td>
<td>body</td>
<td>annular</td>
<td></td>
</tr>
<tr>
<td>Whiteware</td>
<td>1</td>
<td>hollow</td>
<td>body</td>
<td>banded</td>
<td></td>
</tr>
<tr>
<td>Whiteware</td>
<td>1</td>
<td>hollow</td>
<td>lid</td>
<td>poly pt</td>
<td></td>
</tr>
<tr>
<td>Whiteware</td>
<td>1</td>
<td>plate</td>
<td>body</td>
<td>black tp</td>
<td></td>
</tr>
<tr>
<td>Whiteware</td>
<td>1</td>
<td>plate</td>
<td>body</td>
<td>blue tp</td>
<td></td>
</tr>
<tr>
<td>Whiteware</td>
<td>1</td>
<td>plate</td>
<td>footing</td>
<td></td>
<td></td>
</tr>
<tr>
<td>Whiteware</td>
<td>1</td>
<td>plate</td>
<td>footing</td>
<td>poly pt</td>
<td></td>
</tr>
<tr>
<td>Whiteware</td>
<td>1</td>
<td>plate</td>
<td>marley</td>
<td>grn sponge</td>
<td></td>
</tr>
<tr>
<td>Whiteware</td>
<td>1</td>
<td>plate</td>
<td>rim</td>
<td>black tp</td>
<td></td>
</tr>
<tr>
<td>Whiteware</td>
<td>1</td>
<td>plate</td>
<td>rim</td>
<td>blue shell</td>
<td></td>
</tr>
<tr>
<td>Whiteware</td>
<td>1</td>
<td>plate</td>
<td>rim</td>
<td>red tp</td>
<td></td>
</tr>
<tr>
<td>Whiteware</td>
<td>1</td>
<td>platter</td>
<td>body</td>
<td></td>
<td></td>
</tr>
<tr>
<td>Whiteware</td>
<td>1</td>
<td>saucer</td>
<td>body</td>
<td>blue pt</td>
<td></td>
</tr>
<tr>
<td>Whiteware</td>
<td>1</td>
<td>saucer</td>
<td>body</td>
<td>poly pt</td>
<td></td>
</tr>
<tr>
<td>Whiteware</td>
<td>1</td>
<td>saucer</td>
<td>rim</td>
<td>blue pt</td>
<td></td>
</tr>
<tr>
<td>Whiteware</td>
<td>11</td>
<td></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>Yellow ware</td>
<td>1</td>
<td></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>Yellow ware</td>
<td>1</td>
<td></td>
<td></td>
<td>hollow body</td>
<td></td>
</tr>
</tbody>
</table>

Common vessel type, followed by unidentified hollow wares and saucers. Although similar ratios of flat tablewares to tea wares have been recorded for other slave sites, this may be somewhat unusual (Young 1997a). Typically, a preponderance of hollow vessels like bowls is associated with slow-cooked meals like soups and stews (using less costly cuts of meat)
that could be left cooking while slaves worked all day in the fields, whereas plates and platters are associated with planter meals consisting of roasted or baked meats (Otto 1984; Adams and Bolings 1989). Since the excavations at Mount Locust are as yet limited, these expected patterns may emerge with additional investigations.

Unfortunately, the shovel tests and the unit excavations show that the preservation of animal bone at Mount Locust is poor. Therefore it will be impossible to reconstruct the diet of the enslaved African Americans there through archaeological data obtained from the site. Comparison with other sites, such as Saragossa where bone preservation is good to excellent, could be used to make inferences about Mount Locust.

The heavy concentrations of brick, both in unit excavations and the shovel test pits, as well as the high frequencies of cut nails and the paucity of window glass, support the interpretation that the slave houses at Mount Locust were log or wood frame, built on brick piers with brick chimneys and unglazed windows, as indicated by oral histories. If the houses at Mount Locust were typical for the period, then they should have been single-room houses, approximately 15 by 15 feet in size, a single story with no glazed windows (Young 1998b). Probably the most remarkable aspect of the housing at Mount Locust is the presence of brick chimneys. Stick and mud chimneys appear to have been the most common type for slave houses in Mississippi (Young 1998b).

Conclusions

Research concerning life for enslaved African Americans at Mount Locust is underway. The initial phase of the research consisted of relocating the slave houses, and assessing the potential of this site to yield information about specific questions in African-American archaeology.

Current research concerning the African diaspora is often focused on such topics as cultural identity and how material culture is used to express and alter identity, religious beliefs and ideology, and the power relations between oppressor and oppressor (McKee 1998; Otser 1994, 1998; Samford 1996; Singleton 1995; Young 1997b). Some of the most interesting questions involve the relationship between the enslaved and enslaver and the contributions of Africans and African Americans to Southern and the larger American culture. Other questions are more basic and involve reconstructing the fundamental aspects of everyday life such as diet and housing (Singleton 1995).

While the potential for reconstructing diet and food procurement is limited at Mount Locust because of the poor preservation of animal bone, it appears possible to reconstruct what the houses were like and some of the materials that enslaved African Americans had inside their houses. Before the more complex questions like power relations and identity can be addressed, the more basic questions must be answered. Furthermore, work at other sites in the area must be conducted to search for repeating patterns and to put Mount Locust in a regional context.

The Natchez District was built largely by African-American slaves, yet the extent of the impact of enslaved people on the history and culture of the region remains poorly understood. Data from Mount Locust slave houses has the potential to begin to rectify this critical gap in our knowledge.

Archaeological reconnaissance in the area behind the main house discovered clusters of artifacts that are interpreted as relating to the slave quarters. Even if further archaeological work fails to identify undisturbed foundations, clusters of artifacts associated with particular houses (and families) is still likely.
Three areas that were included in the shovel testing showed significant clusters of artifacts that should receive additional archaeological work (indicated by circles on Figure 9). Further work in the area of N2070 E1934 is expected to yield material related to the slave quarters. This area should be more systematically tested. The cluster of artifacts around N2100 E1994, which may be the location of a blacksmith shop or agricultural building such as a barn, should also be targeted for further work. The third major cluster around N2000 E1944, which may be the location of "Sleepy Hollow," would provide comparative material for the slave quarters.

Because of the many visitors to Mount Locust, this site is ideal for conveying to the general public findings about the everyday lives of African Americans who lived and labored there. The information yet to be uncovered and analyzed in the archaeological, documentary, and oral historical records at Mount Locust becomes even more significant for this reason.

Acknowledgments

I would like to thank the 1998 USM field school participants for their hard work and good cheer, even in the hot sun. As always, I thank Philip Carr, Shana Walton, Misty Jaffe, Carolyn Ware, Amy Chasteen, and Ann-Marie Kiannel, our USM writers' group, for reading and offering comments. I am so very grateful to Tobie Jackson for his support in my research at Mount Locust. Thanks also go to Thelma Williams and the Southern Cross summer program that excavated with us at Mount Locust. I also wish to thank Ser Seshab Hetar, Ipirana Kwan, Ozelle Fisher, Ron and Mimi Miller, Andrew Robinson, Eric Chamberlain, Bennie Keel, and many others who helped in this research. The work was conducted under ARPA Permit NATR-98-002. The research presented was funded by The University of Southern Mississippi.

Amy L. Young is an assistant professor in the Department of Anthropology and Sociology at the University of Southern Mississippi in Hattiesburg.

Figure 9. Areas for future work at Mount Locust.
References

Adams, William H., and Sarah Jane Boling

Davis, Ronald L. F.

Loveday, Amos

McKee, Larry
1998 Some thoughts on the past, present, and future of the archaeology of the African diaspora. Paper presented at the plenary session “Where Are We and Where Do We Need to Go” at the annual meeting of the Society for Historical Archaeology, January 1998, Atlanta, GA.

Moore, John Hebron

Over, Charles E., Jr.


Orto, John Solomon

Phelps, Dawson A.


Samford, Patricia

Singleton, Theresa


South, Stan

Thomas, Lynnell

Tuma, Michael W.

Young, Amy L.


United States Bureau of Census
1820 Population schedule, Jefferson County, Mississippi. Microfilm, Cook Library, University of Southern Mississippi, Hattiesburg.

1830 Population schedule, Jefferson County, Mississippi. Microfilm, Cook Library, University of Southern Mississippi, Hattiesburg.

1850 Population schedule, Jefferson County, Mississippi. Microfilm, Cook Library, University of Southern Mississippi, Hattiesburg.

1850 Slave schedule, Jefferson County, Mississippi. Microfilm, Cook Library, University of Southern Mississippi, Hattiesburg.
From Morality (and "Sociology") to Politics in Smithsc ...an Anthropology and Museology

Marvin D. Jeter

This began as a regular book-review assignment. But (not completely by chance), the editor of Mississippi Archaeology had sent a book on Victorian anthropology and archaeology to a fanatic on certain aspects of these subjects with a few axes to grind, and one thing led to another. The primary topic at hand, and point of departure in some other directions, is:


This is a paperback reprint of a book originally published in 1981 in hardback as Savages and Scientists, priced at $19.95 (the equivalent of about $35 today; it is out of print). Except for the covers and a brief new preface (the longer original one has been deleted), only the title has been changed.

A few minor mistakes were carried over from the 1981 printing of Savages and Scientists, e.g., "Eastern" for Easter Island in a caption on p. 93 and "Frederick" for Frederic Ward Putnam on pp. 112, 250 (twice), 270, and 318 (the latter error also occurs in Hinsley 1985). Hinsley's historian-style citation system, often using more than 100 superscripts per chapter to include both references and "endnote" remarks after each chapter, appears very unwieldy by comparison with our comprehensive parenthetical citation system. Similarly, his bibliography is broken up into five separate categories. Worse yet, by some inexplicable historian's logic, multiple entries under the same author are in alphabetical order by titles, resulting in completely haphazard chronological order! The index is scary and inadequate.

In the process, Hinsley's (1981:10) rather provocative statement, "The history of anthropology, I am convinced, should not be left to anthropologists" has also been deleted, as has his more cautious follow-up, "Still, the historian who presumes to impose his judgment in this field is well advised to read circumspectly and respectfully, and needs patient friends to overcome disciplinary myopia." The former statement does appear somewhat inconsistent with Hinsley's later criticisms of anthropologists (e.g., Powell and his followers who imposed their own judgments on Indians and his obvious sympathies with those (e.g., Cushing and Mooney) who showed some empathy with the Indians they studied. Hinsley's "dismissal
to protect the guilty; namely, the author and the publisher, who originally
called it Savages and Scientists: the Smithsonian Institution and the Development
of American Anthropology, 1846–1910. That first word in the old title has
become horrendously politically incorrect in these latter days (and the third
word isn't looking all that great, in the wake of postmodern "science studies”).

A cynic might also conjecture that this reprint was quite opportunisti-
cally retitled, with an eye toward potential sales at the Smithsonian's long
forthcoming National Museum of the American Indian (NMAI). Its "George
Gustav Heye Center" opened in 1994, more or less coinciding with this
book's reappearance, at the old (1907) Alexander Hamilton U.S. Custom
House in New York City's Battery district. That venue, however, features
collections rescued and transferred (by a 1989 act of Congress) from the
financially-troubled Heye Foundation's decrepit Museum of the American
Indian in the blighted Bronx borough—not the ethnological and archaeo-
logical materials and information that were collected for the Smithsonian's
own National Museum of Natural History (NMNH), under the auspices,
personalities, and philosophies summarized here by Hinsley.

Turning away from the political and institutional complexities for a
moment, I should note that I found this book very informative when I first read
(or more accurately, used) it in the 1980s and greatly enjoyed revisiting it in
its new wrapper. My original selective readings were in the context of re-
searches on Edward Palmer, a widely-traveled multi-purpose specimen collec-
tor and archaeological pioneer who had been a field assistant for the Mound
Exploration Division of the Institution's Bureau of Ethnology in the early
1880s (Jeter 1990a, 1990b). In addition to a few perceptive impressions of
Palmer himself, Hinsley provided fairly abundant information on the
Smithsonian's second "secretary" (director) Spencer F. Baird (Palmer's long-
time patron in "natural history" collecting and the founder of the NMNH),
and on Bureau director John Wesley Powell, but surprisingly little on Division
director Cyrus Thomas, whose 1894 final report on the great "Mound Survey" debunked the myth of a lost race of "Mound-Builders" who supposed-
edly had not been the ancestors of Eastern U.S. Indians.

With my use of this book as a secondary reference source receding into
the past, I was free to read it thoroughly this time—and from another angle,
suggested by the new subtitle: that of a "moral anthropology in Victorian
America." A broader comparative perspective (plus some possibly surpris-
ding down-home relevance for Mississippi readers) was provided by a chance
find, in a remaindered-book bin, of a history of the development of American
sociology and social thought (Vidich and Lyman 1985), mainly con-
cerned with roughly the same period. As the authors of the latter work
remark, "in the nineteenth and early twentieth centuries, the social sciences
were not specialized in their present form: sociology, psychology, political
science, anthropology, and economics had not been separated from phil-
osophy and were not professionalized" (1985:xi–xii). I found their discus-
sions particularly enlightening because, as they note, "By the middle of the
twentieth century...sociology had created an autonomous image, and both
its origins in and its confrontations with religion were all but forgotten"
(1985:22; emphasis added). Having had a single, non-historical, under-
graduate survey course in sociology back in 1959, focused on mid-twenti-
eth-century social problems, I was overdue for some enlightenment about
that discipline's roots. Other readings in the histories of related fields have
provided still further enlarged contexts.

In focusing on American anthropology and the second half of the nine-
teenth century, Hinsley does not embed his story in its deeper and broader
contexts. Indeed, as the philosopher Roger Scruton (1995:102 ff) has re-
marked, the "idea of a moral science" arose with the triumphs of natural
science in seventeenth-century Europe and Great Britain, specifically first
expressed in certain mid-century works by Descartes and Spinoza. In the
eighteenth century, the earliest "social and behavioral sciences," now known
as economics and political science (then combined more holistically as "pol-
itical economy"), were effectively founded by Adam Smith, who as every-
one knows, wrote The Wealth of Nations, published in 1776. Less well known
are the facts that Smith held the Chair of Moral Philosophy at the Univer-
sity of Glasgow and that successive editions of his The Theory of Moral Sen-
timents, first published in 1759, led more or less directly to the Wealth

Nineteenth-century social thought was indeed in ferment, following upon
the rise of the physical and biological sciences, the theories of Enlighten-
ment thinkers about human nature and society, and the empirical consequences of the American and French revolutionary experiments. The mid-

century in America saw increasing westward expansion, with destruction or dispossession and removal of Indian groups, plus the intensifying debate over slavery and the trauma of the Civil War and growing concern over the implications of Darwin's demonstration of biological evolution. The later

decades and turn of the century brought the culmination of discovery, exploration, and conquest in the West, and beyond to foreign lands, complicated by a rising tide of foreign immigration; the consolidation of governmental, academic, and private institutions, and lurking in the background, the spectre of un-American, anti-Christian doctrines such as those of Marx and Nietzsche. The twentieth century, of course, has seen even more horrible wars and atrocities, but perhaps we have become so numbed by them, and by the accelerating pace and scope of change, that change no longer threatens us quite as it did our Victorian forebears. ("The threat is worse than its execution" is an old adage of the chessboard.) In this century the subject matter of science has become virtually divorced from moral considerations, although the practitioners of science are still wrestling with questions of professional ethics, and there are still plenty of rear-guard attempts to draw moral implications from more or less "scientific" findings.

At the Smithsonian: Joseph Henry Setting the Moral Tone

Part I of Hinsley's book provides background information on the earlier phase of Smithsonian anthropology, from the Institution's 1846 inception to 1880. Chapter I ("Magnificent Intentions") begins with views of the "indolent" incipient city of Washington, as seen in the 1840s by civilized observers from northern climes, including Henry Adams, Charles Dickens, and Anthony Trollope, and reviews the founding of the Smithsonian. Hinsley notes some early influences on views of Indians, including Dr. Samuel Morton's 1839 *Crania Americana*, which divided the Indian race into two families ("Toltecans" and "Barbarous"), and Lewis Henry Morgan's 1851 *Ancient Society* and later writings, which set forth his allegedly "unilinear" cultural-evolutionary scheme.

According to Hinsley (p. 23), "American investigators [of Indians] at mid-century followed three distinct routes: archaeology, philology, and physical anthropology." It seems odd that he does not mention here the most

basic of the traditional "four fields" of anthropology, variously known as cultural or social anthropology, or ethnography, or ethnology, especially since his immediately preceding discussion was about British and American ethnologists.

Chapter II summarizes the early Smithsonian's efforts at "Promoting Popular Science" under the leadership of its first secretary, the eminent physicist Joseph Henry. According to Hinsley (p. 38), Henry was an elitist who saw science as "a moral enterprise" for stimulating the intellect and spirituality of a remarkably unperceptive American populace. He thought of anthropology as, at best, "a kind of middle ground between literature and science" (p. 35).

As this chapter illustrates in some detail, Henry promoted the scientific study of Indian languages. Eventually (under Powell, see below), linguistics would become a major theme in Smithsonian anthropology. However, Henry "generally excluded...discussion of physical anthropology as politically explosive and morally repugnant" (p. 22). This avoidance must have intensified after the publication of Darwin's *Origin of Species* in 1859. (Later, Powell followed Henry's lead.)

Henry also supported publications in archaeology, wielding a heavy editorial hand on the speculative tendencies of Squier and Davis in their *Ancient Earthworks of the Mississippi Valley*, published in 1848 as the first volume in the "Smithsonian Contributions to Knowledge" series. A later volume, *Archaeology of the United States*, by Samuel F. Haven, countered Squier and Davis's "lost race" notions. Shortly before Henry's death in 1878, he remarked that anthropology had become the most popular branch of Smithsonian science.

But, as Chapter III ("An 'Omnium Gatherum'") points out, Henry was definitely not a museum man—or more precisely, not an advocate of mas-

3 As Hinsley points out, Henry was influenced by pioneering ethnologist Henry Rowe Schoolcraft, and worked with Yale linguistics professor William Dwight Whitney and field linguist George Gibbs IV. The fields of philology and comparative Indo-European linguistics had recently attained triumphs in Europe and England (Renfrew 1987:9–14; Mallory 1989:9–14), so they would have been highly respectable in Henry's intellectual and academic circles. Also, these fields did not involve the kinds of storing of bulky specimen collections and commitment to museology that Henry wanted to avoid.
sive collections of curiosities. He clearly saw the value of small, systematic collections for comparative research. He also wanted to keep the Smithsonian relatively free from political pressures and oversight, "the harassing questions of coarse and incompetent men" (p. 66), that would come with the need for funding to build a museum building, bring in numerous specimens, and exhibit them. Henry was certainly prescient in that regard: as later sections of Hinsley's book and more recent developments make abundantly clear, political pressures were to impinge significantly on Smithsonian anthropology and museology.

Meanwhile, in Sociology...

Meanwhile, American sociology had been born as "a moral and intellectual response" to the country's problems (especially those of race, class, and economics), according to Vidich and Lyman (1985:1). These authors (1985:9ff) point to a rather unlikely birthplace: Port Gibson, Mississippi, home of Henry Hughes (1829–1862), who as a 25-year-old lawyer published A Treatise on Sociology, Theoretical and Practical, in 1854.

Hughes had been heavily influenced by British and European thinkers, especially Auguste Comte, and struggled (rather unsuccessfully) to reconcile the conflicting forces of Protestant religious belief and morality, scientific methodology, and socioeconomic inequality (including a utopian, "benevolent" or "warranted" form of slavery). He died during the Civil War, and elitist sociology virtually died with him. Indeed, American sociology seems to have lacked major figures to lead it in new directions until the 1880s (see below).

Back at the Smithsonian: Spencer Baird's National Museum

While illuminating Henry's growing tolerance for Baird's collections and the prospect of a National Museum building, Hinsley's Chapter III tends to

understate the tensions between the two men's aims. Baird's "uncontrollable" and "insatiable" passion for obtaining specimens, by the boxcar load if possible, has been described better elsewhere (Bruce 1987:49, 198–99, 324–25; see also Jeter 1990a:12–13, 18–19, 80–84; n.d.). Here, Hinsley instead briefly summarizes the exemplary contributions of two of Baird's principal aides, Palmer the all-purpose field collector and specimen preparator and Edward Foreman the meticulous cataloger, in turning the Smithsonian toward Henry's dreaded "omnium gatherum" function (earning its eventual nickname, "the nation's attic").

In Part II, which represents over two-thirds of his book, Hinsley moves on to "Anthropology as government science" from 1879 (the year when Baird finally got his National Museum approved, and the Bureau was created; a turning point which might have made a neater ending for Part I) to 1910. Chapter IV, "From culture history to culture areas," summarizes the practice of Smithsonian anthropology from 1881 to 1908. The rationale for this starting date is not clear; although it coincides with the inception of the "Mound Survey" under Baird and Powell, that project is mentioned only in passing.

Otis T. Mason

Instead, this chapter (the second-longest in the book) is basically about the significance of the career of Otis Tufton Mason. Born in 1838 and devoutly religious, Mason stated that he had been "born again" under Baird's influence in 1869. He started working at the Smithsonian in 1873 as an unpaid volunteer under Baird's tutelage, became the National Museum's first curator of anthropology in 1884, and continued in that position (part-time after a stroke in 1898) until his death in 1908. Later portions of this chapter introduce some of Mason's colleagues, including the artist-geologist-archaeologist William Henry Holmes (see below) and George Brown Goode (another Baird protege, who became a museum administrator).

Hinsley sees Mason as caught up in a much larger milieu, in a time of social, political, intellectual, and theological crises, with multiple threats of chaotic or even revolutionary change. Victorian museums and other institutions were seen by the Establishment as instruments for imposing order, via classifications of artifacts and cultures, with nineteenth-century Euro-American civilization at the apex of cultural evolution. Above all,
discontinuities were to be avoided, and anthropology spanned the critical gap between natural science and moral philosophy. Mason's own specialty was to classify artifacts as material expressions of mental progress through invention—an "evolutionary" typological approach derived from the elitist British ethnological collector, General Augustus Henry Lane Fox, a.k.a. Pitt Rivers (Chapman 1985).

Franz Boas vs. Mason

Also appearing here is a much more important figure: Franz Boas (1858–1942), who was to become the "father" of American academic anthropology and a major opponent of the Smithsonian men. As a recent immigrant from Germany in the mid-1880s, Boas immediately criticized Mason's exhibits of cross-cultural comparisons and evolutionary progress, arguing instead for cultural relativism and non-comparability: individual tribes must be understood in light of their own unique culture-historical situations. Eventually, perhaps influenced by Boas's arguments as well as Powell's 1891 linguistic map of North America, Mason moved toward the perspective of individual tribes by coining the "culture area" concept and designing exhibits around it. In 1893, Mason and Holmes introduced (borrowed from European models; Jacknis 1985:81) "life group" displays, showing what "lay figures" of Indians using artifacts in appropriate surroundings. The history of competing approaches to museum displays, and the theoretical orientations behind them, is a fascinating subject in itself (cf. Stocking 1985).

Boas also argued (eventually with success) for the independence of language and culture from racial factors, in opposition to the Smithsonian men's racial determinism. Despite his "historical" emphasis, though, Boas was mainly concerned with cultural discontinuities between spatially distinct groups, seen via the shallow time depth of the "ethnographic present" and with little input from the incipient discipline of archaeology.3

3The issue of cultural continuity vs. discontinuity later became more important in American anthropology in another guise, with more emphasis on change through time, as archaeologists developed their relative and absolute methods of chronology-building. In the history of Lower Mississippi Valley archaeology, this is best exemplified by a fundamental difference of opinion between Phillip Phillips of Harvard and the home-grown "father of Lower Valley archaeology" James A. Ford (born in Water Valley, Mississippi), with various colleagues such as James B. Griffin leaning one way or another, or caught in the middle.

In the great report on the Lower Valley survey of 1940–47 (Phillips, Ford and Griffin 1951), Ford was responsible for the siezation of surface collections, while Phillips dealt with the stratigraphic test excavations (1951:vi). The major "problem orientation" that permeated the survey was the question of describing, if not explaining, the culture-historical trajectory that had led from one apparent "climax", that of the Hopewell/Marksville (Middle Woodland or "Mound Builder") period to that of the obviously later, and indeed temporally separated, Mississippian (or "Temple Mound") period. Ford's assumption of continuity was explicitly stated at the beginning of the siezation chapter (1951:220). But, at the conclusion of the chapter on stratigraphy (which, it should be noted, often lends itself to the identification of discontinuities), Phillips brought up the alternative possibility of significant discontinuities (1951:292). The trio briefly discussed the issue, but did not resolve it, in the concluding chapter (1951:927–28), with Phillips and Griffin voting for the likelihood of at least small discontinuities, and Ford holding out for a general continuity.

The strain of being caught between these two positions, if not his ultimate personal leaning, may well have been reflected in the title and content of Griffin's own slightly earlier syntheses, "Cultural change and continuity in Eastern United States archaeology" (Griffin 1946). Change can occur gradually and continuously, or abruptly and even discontinuously, but Griffin did not choose to highlight the latter possibility.

In his published dissertation-derived summary of the Iacana complex of the Lower Yaoa Basin, Phillip's student Robert Greengo (1964:8–9, 11, 94, 122) repeatedly criticized Ford's "continuity" assumption. Phillips returned to the subject often in his valedictory magnum opus, based on the Lower Yaoa surveys and excavations he had directed from 1949 to 1955 but looking beyond to a synthesis of Lower Yaoa prehistory that remains highly influential to the present (Phillips 1970). In summary, he remarked, "the author has constantly sought for cultural discontinuities with an assiduity bordering on fanaticism...my rule seems to have been: when in doubt, separate" (Phillips 1970:973).

In a more recent Southeastern synthesis, however, Griffin's student Bruce D. Smith (a Smithsonian archaeologist himself), seems to have gone over toward the Fordian position, using the words "continuum" and "continuity" (or "continuities") repeatedly (Smith 1986). I have summarized other aspects of this extended debate elsewhere (Jeter 1989:63–64).
the-scenes politicking, and above all, visionary organizing (Darrah 1951; Stegner 1954; Jeter 1990a:13–20). Morgan and Herbert Spencer are seen as positive and negative influences on him.

From Morgan (1818–81), the upstate New York lawyer (cf. Hughes) who founded American anthropology with emphasis on kinship and social organization, Powell took the scheme of sociocultural evolution through stages (and sub-stages) of Savagery and Barbarism to Civilization. Interestingly (and characteristically), Powell proposed a fourth major stage, Enlightenment (Hinsley, p. 158). Morgan and Powell, unlike Spencer and his ilk, had both done extensive field work, and respected Indian lifeways. But on the theoretical side, although Morgan had a "system" and recognized interconnections among various aspects of culture, his version did not explain the interrelations enough for Powell, the master organizer. Morgan also had little interest in language or mythology, both of which were major concerns for Powell. A kind of "inter-related" synthesis, with emphasis on mental phenomena, became the basis of what Powell called "the New Ethnology." (This phrase may well have inspired Powell's subordinate Cyrus Thomas to refer to his great mound survey project as the "New Archaeology" in an 1887 letter; Brown 1990:26–27.)

Grandiose Schemes

Chapter VI, entitled "Toward an Anthropological Survey," deals with "the early years of the BAE [sic], 1879–1893." This is the longest chapter in the book, and the heart of it; fortunately, Hinsley does some of his very best writing here. The stage has been set, and Powell's "personal creation," the Bureau of Ethnology, swings into action.

For Lower Valley and Eastern U.S. archaeologists, the primary manifestation of the Bureau has always been its Mound Exploration Division, which started work in 1881 and came to its triumphant conclusion with Thomas's (1894) final report (Brown 1990; Jeter 1990; Smith 1990). But the mound survey was not Powell's main focus at all—far from it. Here, Hinsley ignores it and instead emphasizes Powell's organization of the Bureau in the service of his immediate and ultimate goals, concentrating on "salvage" work among the Indian groups threatened by Westward expansion.

Powell's main goal was an Indian linguistic classification scheme and map, not as an end in itself, but "for what [languages] revealed about man's mental growth" since he saw linguistics as "his clearest window into the mind of primitive man" (pp. 158, 161). A related project was a "synonymy" which would make sense of the conflicting mass of data on the names of tribes, and of groups of related tribes. Another was a history of treaties between the federal government and the various tribes. These latter projects were to be part of a program of, in effect, "applied anthropology" informing government policies which were to guide the destruction of doomed Indian culture(s) and the gradual, paternalistically humane absorption of the survivors (or their descendants) into Western civilization. Powell's "grandiose schemes of study" were them-

[6] In the Bureau's First Annual Report (for the 1879–80 fiscal year), after Powell's introductory "Report of the Director," the first two "accompanying papers" were his own "On the Evolution of Language" (Powell 1881a) and "Sketch of the Mythology of the North American Indians" (Powell 1881b).

[7] The "mound problem" was a non-problem for Powell, who had long since decided that the mounds had been built by Eastern North American Indians. I have suggested (Jeter 1990a:17–19; n.d.) that instead, the mound project's major original impetus was Baird's insatiable desire to obtain artifacts for his long-desired and newly-obtained National Museum. In behind-the-scenes maneuvering that would have been typical for both men, he channeled the project through Powell's Bureau in exchange for supporting Powell's succession to his own long-desired position, the directorship of the U.S. Geological Survey. Powell retained the Bureau directorship as well, and in a clever move, silenced a man who had criticized him for inactivity on the mound question, Wills de Haas, by hiring him to head the Bureau's new division. After a year, Powell replaced him with his own man, Thomas. Leaving the mound project to Thomas, Powell then turned his copious energies to his real interests, geology and Indians in the West.

[8] In a later section (pp. 236–37), Hinsley does take a retrospective look at the generally friendly and respectful relationship between Baird and Powell. He notes that Baird insisted as early as late 1879 that the Bureau should spend $5,000 of its $20,000 annual appropriation from Congress "as a collecting arm" for his National Museum, although Powell had not originally planned to do this sort of thing, being "largely interested in linguistics." But, Baird's invocation of Congressional pressures, and his superior position, carried the day. Once again, Hinsley does not specifically mention the "Mound Survey" but this appears to me as a clear precedent, with regard to both motive and method, for what I suspect as Baird's behind-the-scenes role in getting $5,000 of the Bureau's $25,000 Congressional appropriation of early 1881 earmarked for mound investigations.
selves doomed to incompleteness at best, but at least admirable in their "partial accomplishment" (Stegner 1954:264–65). Yet the publication of the linguistic classification and map (Powell 1891) may have marked a turning point after which "the Bureau began to lose direction and to drift" (p. 151).

Hinsley's account is peppered with references to contradictions, dilemmas, etc.; enough so to suggest a bias toward dialectical thinking on the part of the author. This may oversimplify the pressures and pulls on a multifaceted personality like Powell's, but it suffices to highlight some of the major strains and stresses he was under. According to Hinsley, Powell the organization man nevertheless gave priority, if not free rein, to his talented individual subordinates in making their field observations—but did the synthesizing and drawing of major conclusions himself. Hinsley sees this ultimately hierarchical structure as a key difference between Bureau anthropology and the later "professional" academic system established by Boas, wherein the graduate students were ostensibly free to set up their own projects and draw their own conclusions. However, historians of anthropology generally acknowledge the existence of a "Boasian milieu" (Harris 1968:290 ff) during and beyond the first half of the twentieth century, dominated by "Papa Franz" and his leading students, despite the lack of an officially designated hierarchy.

Powell's managerial style is illustrated by sketches of several subordinates and their work: James Pilling, clerk and detail man, compiler of an endless Indian bibliography; Col. Garrick Mallery, "soldier-scientist-litterateur" who studied Indian sign languages and rock art; the Rev. James O. Dorsey, who studied Siouans and their languages; and Albert Gatschet, another widely-traveled linguistic data-gatherer. The chapter includes several references (e.g., pp. 154–55, 157, 161, 170, 181) to Powell's (et al.) "romantic" vision of a grandiose final synthesis of human cultural development, a process which would eventually integrate the world's differentiated peoples into a civilized global community. This grand scheme reappears later in the book, as espoused by Powell's associates and successors (see below). It is also essentially the model of "acculturation leading to complete assimilation" which guided Federal legislation and agency policies (especially those of the Bureau of Indian Affairs) for many decades (Oswalt 1988:45–49).

Sociology Again

Lester Ward—and Powell

In the late nineteenth century, with Hughes's hopes for "slavocracy" obliterated, American sociology took a very different turn with Lester Frank Ward (1841–1913), the next major figure discussed by Vidich and Lyman (1985:20 ff). I had not heard of Ward before, but he soon appeared to me to have had a number of at least superficial resemblances to his virtual contemporary Powell. Both were self-made Midwesterners who were severely wounded during the Civil War. Like Powell, Ward was an optimistic positivist, and not above coining a civilized neologism or two to help out his burgeoning social science's vocabulary, e.g., "social karykinesis" for progressive social-evolutionary development, or "sociocracy" for a social system in which problems could be solved by techno-scientific means or social engineering (1985:28, 33). Ward's "faith in knowledge" or "intelligence" (1985:25–33) is perfectly compatible with Powell's emphasis on Mind.

The Vidich-Lyman book never mentions Powell, though. Hinsley (pp. 262–63) actually mentions Ward in the same context as Powell, but only in passing, as a "sociologist/paleobotanist" (an odd combination of specialties from our present viewpoint) who, like Powell, ignored disciplinary boundaries to address "fundamental issues of their day" with emphasis on the social implications of evolutionary doctrines. But there is no hint here of actual connections between the two men. A little research, however, soon revealed that in fact Ward had worked for 24 years in Washington for the U.S. Geological Survey (Dealey 1927:63), starting in 1881, the very year that Powell became the Survey's director!  

1 Or is it? After all, we have got used to pronouncements by the entomologist and ecologist Edward O. Wilson on sociobiology and human nature (Wilson 1975, 1978, 1998; Lumsden and Wilson 1981).

2 Hinsley also mentions "Ward" (last name only, not identified further) in passing on p. 255 (not cited under the entry for Ward in the Index), where he also names Powell and his protégé W. J. McGee. He refers to "the common beliefs of the trio" with regard to conservation of natural resources, but does not discuss philosophical or sociological issues.

3 In his biographical sketch of Ward, Dealey (1927) never mentions Powell, either. But in other researches into the Victorian scene (e.g., Jeter 1990a, n.d.), I have been repeatedly struck by the common occurrence of "networking" among the leading and lesser figures in archaeology, anthropology, and related disciplines.
Ward's major sociological works started appearing in the 1880s (Dealey 1927:70). They were not published by Powell's politically-sensitive geological agency (as were his paleobotanical studies), though, but as books by commercial publishers or articles in learned journals (Vidich and Lyman 1985:352). In the social arena, he went farther than Powell—even Marx, in some ways. With a "vigorously atheistic outlook," especially in his earlier writings, Ward attacked all religions and looked forward to the conversion of churches to "Halls of Science" (1985:20–21). He believed that reason, science, and education could free humanity, ignoring the material constraints that concerned Marx. Foreseeing the future of the social sciences, Ward viewed the university as the intellectual and cultural substitute for the church, and in 1906 "he was able to gratify a life-long ambition" (Dealey 1927:64) by becoming the first professor of sociology at Brown University, a post he held the rest of his life.

"Sociology" of a certain kind was also a major interest of Powell's. Near the end of his life, he published an 80-page essay (Powell 1903a) on the subject, which he defined as "the science of the control of human activities...by institutional devices," i.e., by "rules of conduct" (1903:65ff). He illustrated the evolution of such devices or rules by reviewing a societal sequence from savagery through barbarism and monarchy to "republicanism" (1903:72ff).

Although Powell "sought a single causal principle, a scientific God, to explain all phenomena" (p. 125), essentially like Ward's faith in Science, he stopped far short of Ward's active anti-religious stance. Slightly older and  

---

12 In 1870–71, Ward edited a journal called The Iconoclast (Dealey 1927:72).
13 The questions of Powell's own (evolving?) religious beliefs, and of mutual influences between him and Ward, are intriguing but beyond the scope of this essay. Here, I will merely note that although "Powell rarely cited Spencer or anyone else in his writings" (Hinsley, p. 120), in my own brief scanning of his rather garrulous works I did notice one possibly significant allusion to Ward. In an essay on "Sophilology" (which he defined as "the science of instruction"), written near the end of his life, Powell (1903:65ff) remarked, "The American philosopher...Mr Lester F. Ward...derives mind from force by evolution." (In this paper at least, Powell cited a number of ancient to modern authorities.)

Other indications of similar mindsets on the parts of Powell and Ward can be seen in their late-in-life attempts at massive syntheses: e.g., Powell's "grandiose vision" summarized above and his (1898) Truth and Error, and Ward's (1913–10) posthumous multi-volume

the son of a Methodist minister, Powell was more conservative. Like Henry, he was repelled by the ultimate implications of Darwinian biological evolution for human origins (and avoided physical anthropology); even more so by the so-called "Social Darwinism" espoused by "armchair" thinkers like Spencer. Instead, he saw humankind as unique, because of the human mind. "Powell intended to trace the development of mind from the most primitive savages to Washington scientists" (p. 126; the last words may have inspired this book's original title).

One of the major claims derived from Hinsley's search for "contradictions" is that Powell (and his follower McGee; see below) tried to combine two ultimately incompatible traditions in his brand of anthropology (pp. 152–54). One was the "natural history" exploring tradition of empirical "scientific" field research; the other was the "conjectural history" tradition derived from moral philosophy. Powell's attempted synthesis resulted in a hierarchical structure for his brand of government science, with himself at the top, drawing ultimate conclusions. As will be seen, not all his field men fit comfortably into this scheme.

Academic Sociology

Possibly due to its emphasis on contemporary American problems, sociology preceded the more exotic field of anthropology into this country's universities. Vidich and Lyman (1985:36ff) turn next to William Graham Sumner, who reportedly taught the country's very first college course in sociology at Yale around 1875. In Sumner's rather gloomy vision, some have seen a replacement of the stern God of Calvinist theology with equally stern social and economic laws; and a rejection of Marx's ultimate triumph of the proletariat, in favor of endless class struggles.

The remainder of the Vidich-Lyman book (1985:53ff) traces the development of academic sociology through the late nineteenth and early twentieth centuries, at institutions such as Harvard, Columbia, Midwestern univer—

Glimpses of the Cosmos. Ward "made his colleagues gasp" at Brown University by offering a course called "A Survey of All Knowledge" (Dealey 1927:61). I obtained a copy of Truth and Error via interlibrary loan, and was pleased (but not really surprised) to find that Powell had dedicated it to "Lester F. Ward, philosopher and friend."
The Decline of Powell’s Bureau

Nonconformists: Cushing and Mooney

Chapter IV, “Heroes and Homelessness,” presents lengthy sketches of the careers and personalities of two major field workers, Frank Hamilton Cushing and James Mooney, with sidelong glimpses of several others. Cushing (1857–99) was actually another protege of Baird’s, and had started his Smithsonian career in 1879 (that year again) on a Southwestern expedition to collect ethnological and archaeological materials for the National Museum, before foreign museums could grab them. An intuitive “young genius” who was regarded as brilliant but erratic, Cushing was soon residing as a “White Indian” at the pueblo of Zuni, beyond the effective control of Baird and Powell but especially fascinating to the latter for his alleged “ability to enter fully into savage thought” (p. 193).

But at Zuni, then on a privately-funded major archaeological expedition to excavate major Hohokam ruins in the vicinity of Phoenix, Arizona in the mid–late 1880s, then in a similar expedition to sites on the southwest Florida coast in the mid-1890s, the story was essentially the same. Cushing was repeatedly involved in remarkable discoveries and speculative explanations, embroiled in controversies, incapacitated by illnesses—and generally did not produce anything like adequate notes, let alone full final reports. Apparently, others had to pick up pieces of the pieces (Gilliland 1975, 1989; Haury 1945; Hinsley 1996). Even so, Hinsley (pp. 206–7) appears to see Cushing as a sympathetic character. But to some anthropologists and anthropologists, his career (or Southwestern–Southeastern career) appears a virtually unmitigated disaster.

Mooney (1861–1921), the son of Irish Catholic immigrants, grew up in Indiana with a sense of identification with oppressed minorities and a spirit of political activism. He first turned to journalism, but applied several times for work with the Bureau, beginning in 1882. Powell finally gave him a chance (but no salary) in 1885, and he soon caught on. He worked extensively with the Cherokees in North Carolina and later with Siouans and others on the Great Plains. He never became part of Powell’s clique, though, and attempted to interpret his data in his own way rather than following the Bureau’s cultural-evolutionary “party line.” A major clash occurred around his 1893 account of the Plains “Ghost Dance” movement, including a condemnation of the Wounded Knee massacre. His cross-cultural comparison of such Native revitalization movements with early Christianity, in a government publication, brought about a disclaimer from the politically sensitive Powell, who was already feeling strong pressures from Western development interests critical of his Geological Survey.

Hinsley sees Cushing and Mooney as different in their own ways from Powell’s kind of government anthropology in their attempts to go further in trying to understand Indian cultures. Yet, he insists that they fell short of being “professional” anthropologists, largely because they lacked the academic “institutional perspective” that Boas was soon to create.

Fortunately, Lyman seems far out of his home field here, willfully using a highly eccentric and idealistic concept of “civilization,” praising Cushing as a “social scientist” for “bold” speculations that turned out to be dead wrong, and uncritically citing the recent nonsensical works of the alleged “surrogate’s apprentice” Carlos Castaneda and the self-proclaimed “psychic archaeologist” Jeffrey Goodman (cf. De Mille 1990 and Williams 1991:297–304 for exposes of those quackeries). Lyman (1990:41) does accurately note that “only a selective portion” of Cushing’s approach has survived, in the “French structuralist” anthropology of Claude Levi-Strauss (a connection also made by Hinsley on p. 193), but some materialist anthropologists (e.g., Harris 1968:464 ff; 1979:165 ff) would argue that this is a survival of very questionable worth. From another angle, the decidedly non-materialist Boas suggested that “all [Cushing’s] work would have to be done over” (Hinsley, p. 193). I suspect that some of Hinsley’s (and Lyman’s) apparent empathy for Cushing also lies in the latter’s foreshadowing of post-structuralist, postmodern thought, via “his insight that reality...is a psychological construct” (Hinsley, p. 193).
McGee as Powell's Right Hand

In Chapter VII, "Fin-de-Siècle," Hinsley charts the 1893–1903 rise and fall of W. J. McGee, a rural Iowan who had joined Powell's Geological Survey in 1883. Ten years later, Powell was forced to resign as head of the Survey, and brought McGee along with him to the refuge of the Bureau. McGee's only field expedition, mainly in southern Arizona and adjacent Mexico during 1894 and 1895, was basically unsuccessful and nearly disastrous. Although he was an excellent observer, his linguistic ability and understanding were deficient, and he held the native Papago and Seri in low regard. His report and other writings were "pathetically derivative" (p. 240) of Powell's kind of cultural-evolutionary thinking, but he, too, had a talent for synthesizing which sufficed to bring him increasing status within the Bureau and the burgeoning "profession" of anthropology.

Powell's health had begun a "slow decline" (p. 236) around 1894, exacerbated by problems with his right arm (partially amputated during the Civil War) and he had already started pursuing his own theoretical interests "to an obsessive degree" (p. 238). After 1895, he spent almost all of his office time (p. 246) on the manuscript of a tome that was eventually published as Truth and Error or, the Science of Intellecution (Powell 1898). Hinsley merely mentions its primary title in passing, and indeed it seems to have been almost completely forgotten.

Meanwhile, McGee gradually took effective control of the Bureau's day-to-day operations and became a highly influential officer in anthropological organizations. Reaching his "zenith" between 1895 and 1900 (pp. 245–46). In turn-of-the-century publications (e.g., McGee 1899; 1901), he reiterated Powell's vision that humanity, though presently differentiated along lines of class, race, and culture, was inexorably moving toward "convergence in brain and blood" or "unity of blood and equality of culture" (p. 247). Hinsley concludes that in trying to synthesize their brands of science and philosophy (which they saw as virtual synonyms), McGee "extended Powell's system almost to the point of caricature" and "became an easy target" (p. 256).

Langley's Rise, Powell's Death, McGee's Fall

From the Smithsonian perspective, a secondary theme of this chapter, in the background but increasing in importance, is the rise of Samuel P. Langley, an astronomer who became the Institution's third secretary in 1887 after Baird's death. Hinsley devotes a fair amount of space to documenting the ensuing and ongoing jockeying for position. At least superficially, Powell got along well with Langley, McGee, and Holmes, and they with him; McGee and Holmes were originally congenial colleagues, too.

But Langley and McGee disliked each other almost from the outset. McGee's ambition was to succeed Powell as Bureau director, but Langley was under increasing political pressure from Powell's enemies, and even before Powell's death had decided to "purge the Bureau" (p. 248). In the event (in October 1902), Langley appointed Holmes, a weak administrator, but only as "chief" of the Bureau, eliminating its "director" title; and McGee "declared war on them both" (p. 250).

Boas Redux

Another major theme here is the reappearance of Franz Boas, mainly from outside the Smithsonian but sometimes partially within it, gradually consolidating his own position and with his own perspectives what the future of anthropology should be, at the Smithsonian and elsewhere. A major "break" for Boas had been an association, beginning in the early 1890s, with Frederic Ward Putnam, who had become the second curator of the Peabody Museum of American Archaeology and Ethnology at Harvard University in 1874 (Boas 1895:53), and had become involved in something of an institutional collecting rivalry with Baird (Jeter 1990a; n.d.). Putnam, who had a number of wide-ranging institutional and research connections, finally succeeded in gaining a professorship in anthropology at Harvard in 1887 (Hinsley 1985:61), and was able to integrate his museum and academic interests into a long-standing tradition (Jeter 1985:72; Jacknis 1985:108).

A crucial episode here was the Chicago World's Fair and Columbian Exposition of 1893. Working for Putnam, Boas had been involved in setting up exhibits of Northwest Coast Indian life for this event, but had no firm institutional base and had hoped to gain permanent employment with the Field Museum there after the Exposition closed. But, in behind-the-scenes maneuvering, he had been displaced by Holmes, who went to Chicago on a sort of temporary leave from the Smithsonian.

Boas had been "extremely bitter" (p. 251) about this situation, and especially toward Holmes. He had also become very disillusioned by his experi-
ences in trying to educate and uplift the public via his exhibits. He moved steadily away from the public aspect of museums, and toward their valuable function as repositories of research collections (Joseph Henry might well have found a congenial younger colleague in Boas, whose own first academic field was physics). With Putnam’s help, Boas eventually got appointments in New York, with the American Museum of Natural History in 1895 and—especially furtively for anthropology—with Columbia University shortly thereafter.16

Nevertheless, he did work extensively as a sort of contract-basis gatherer of linguistic data for the Bureau between 1893 and 1903, dealing primarily with McGee (p. 251). But more and more, Boas came to reject the “amateur element” in anthropology which had characterized Powell’s Bureau field workers, in favor of the kind of formal university-based professional training that he was gradually establishing.

The Inquisition

Things came to a head in 1903, when Langley expanded an investigation into forgery and embezzlement by a minor Bureau employee into a major inquiry into the Bureau’s recent administration under Powell (by then deceased) and especially, under Powell’s de facto “chief operating officer” McGee. The Powell-McGee emphasis on creative individualism rather than on the institution came under fire, marking “the political and historical limits of a tradition.” McGee’s position was abolished, and he resigned from the Bureau (pp. 253–56).

Hinsley’s final chapter, “Crisis and Aftermath,” outlines the decline of Smithsonian anthropology between 1902 and 1910 under Holmes (who actually stepped down in 1909). It begins with a summary of the increasing encroachments of politics on the Smithsonian, then returns to aspects of the 1903 investigation.

16 Anthropology courses had been taught at Columbia by others (from the psychology, philosophy, and social science departments) as early as 1893. Boas was appointed as a lecturer in anthropology in 1896, and as a professor of anthropology, in 1899, and the Department of Anthropology came into existence in 1901 (Stacking 1974:283–90).

Boas Contra Powell?

Here, the dominant character is Boas, who testified before the investigating committee and whose star was ascending through and well beyond this period. Although Hinsley makes some effort to contrast the Powell and Boas orientations, the most obvious differences are their institutional situations and Powell’s aversion to physical anthropology; Boas had a strong interest in it (p. 276), for his own sociopolitical reasons (Harris 1968:298, 300). But from my distance of roughly a century, they appear to have had a number of similarities.

Both Powell and Boas were strong administrators with a paternalistic bent (p. 273). Powell believed in “ever-greater accumulation of facts” which would eventually lead to inductive generalizations (p. 274) — essentially the approach of Boasian “particularism” (Harris 1968:286 ff). In their own ways, both were vitally interested in Indian linguistics, and both were ultimately most interested in psychological factors. And of course, both were moralists at heart.17 I am reminded of Marvin Harris’s (1968:321) remark that Boas and his followers “created an illusion” of debate but really represented “only a very small part of the spectrum” of possible opinion.

Much of the apparent contrast between Powell’s “government anthropology” and Boasian academic anthropology may have been derived from the differing institutional opportunities that were available when each was starting his own empire-building. More basically, much may also have derived from their differing individual situations: Powell was a mainstream Midwestern American who became a member of the Establishment and tended to look at Indians sympathetically but from the “superior” viewpoint of civilization studying savages with a view to assimilating them,

17 In the “Franz Boas” video from the 1980 season of the Odyssey television series, Hinsley made several brief appearances as a commentator; during one of these he referred to Boas as “a moralist of the nineteenth century” (he also refers to “Boas’s moral science” on p. 284 of the present volume). Boas himself was quoted as urging that museums be used to educate and uplift the public, to “counteract the influence of the saloon and the racetrack.” (A major point of the video, though, is Boas’s disillusionment after the Chicago 1893 experience.) Furthermore, he believed strongly that the social-behavioral sciences could help to solve social and political problems, with special emphasis on racism.
whereas Boas was a Jewish immigrant who wanted to criticize Euro-American institutions via anthropological contrasts and (somewhat like Cushing) empathized more with the viewpoints of the Indians. Here we have a classic early preview of the clash between the "scientific" distanced-observer's cultural-comparative "etic" viewpoint and the "humanistic" participant-observer's cultural-relativist "emic" point of view; but like Boas, the idealistic Powell lacked the cultural-materialistic perspective that has accompanied at least some modern "etic" orientations (cf. Harris 1968:568 ff; 1979:32 ff; Binford 1986:468–71).

Hinsley’s Stance

In these days, it is of some interest to know where a writer is “coming from” politically. Hinsley has rather consistently taken his stand on the politically-correct left wing in reviewing the histories of anthropology and archaeology. Reacting to another essay by Hinsley (1989), one rather reactionary reviewer (Lamberg-Karlovsky 1990:581) has accused him of telling “good fables” in which he “depicts the evils of the past and draws a moral [that word again!] lesson from them of what is to be avoided today: imperialism, colonialism, racism, sexism, and a tarnished capitalism.”

In the interest of balance, I should also note that an earlier review of Savages and Scientists (which I consulted only after virtually completing this essay) pronounced it a milestone in the history of American anthropology (Fenton 1982:650), and generally praised it, although noting (1982:652) that Hinsley (rather surprisingly, given his obvious sympathies) had ignored “early Indian collaborators who contributed solid publications based on fieldwork in their native languages” such as the Omaha, Francis La Flesche.

Twentieth Century Smithsonian Anthropology and Museology

The earlier reviewer of Savages and Scientists, William Fenton (1982:653), himself a mid-twentieth-century BAE man, concluded “It is a pity that Hinsley stopped where he did. The terminal 50 years of the BAE might prove equally interesting and important.” Well, the book is not merely about the new-defunct BAE, but the larger subject of Smithsonian anthropology, with strong overtones of its applications to museology. I will not attempt anything like a continuation of Hinsley’s work, but a few aspects of twentieth-century Smithsonian anthropology, with emphasis on the Southeast, archaeology, and museology, are worth mentioning here.

Hinsley at the Helm, Tackling with the Winds of Change

Hinsley summarises Holmes’s struggles to keep government anthropology and Powell’s legacy afloat, in contrast with the rise of Boasian academic anthropology at Columbia and elsewhere. Holmes attempted to expand the scope of Bureau anthropology, achieving broad area coverage in new field work which “lacked focus or depth” and was “theoretically sterile” (p. 281). He at last brought in physical anthropology by hiring Aleš Hrdlicka, who reinforced Holmes’s own ongoing battle against enthusiasts’ claims for “Paleolithic man” in America (pp. 281–82). And he saw a long-delayed two-volume Handbook of American Indians North of Mexico through to publication in 1907 and 1910.

At the end of the book, a sketch of the “Story of the Human Race as Completed” (p. 288), made by Holmes in 1920, schematically represents the old Powell vision of a sort of braided stream of humanity: differentiating from the ice age to the present, but with future prospects for “integration” into one main stream. The Bureau men foresaw “assimilation and... evolution to a world culture” (p. 287); the Boas vision was more like what is now called “multiculturalism.” The outcome is very much at issue today.
The best example of Bureau continuity from turn-of-century to midcentury, especially with reference to the Southeast, is the work of John R. Swanton (1873–1958). He was a student of both Boas and Putnam (p. 279), but worked for the Bureau and published on Southeastern Indian ethnography and linguistics in its Annual Reports and Bulletins for more than four decades. He is perhaps best known to archaeologists, though, for his (1939) report on the route of the Hernando de Soto entrada, recently reprinted by the Smithsonian, and more recently heavily criticized for lack of agreement with present-day archaeological knowledge (cf. papers in Young and Hoffman 1993) and native use of documents (Galloway 1997a:284–85; 1997b:420–21).

Between 1910 and World War II, there were sporadic forays by BAE archaeologists into the Southeast and Lower Mississippi Valley, with mixed results. Edwin Lyon (1996) has recently summarized their projects, and I will only mention a few of likely interest here. They included an early 1920s reconnaissance that touched on Mississippi, followed by Gerard Fowke’s inadequately reported work at Marksville and other sites in Louisiana (Lyon 1996:14; Toth 1974:20–21).

Henry Collins did mid-twenties to 1930 work in Louisiana and Mississippi, culminating in work at the Deasonville site which resulted in a brief report (Collins 1932) and the formal archaeological initiation of two young Mississippi men who later became eminent in the field, Moreau B. Chambers and James A. Ford (Blitz 1988). In 1931–32, Winslow Walker did salvage work at the remarkable Troyville site in eastern Louisiana, not far from Natchez (Walker 1936). Smithsonian archaeologist Frank Setzler, assisted by Ford, excavated intensively at Marksville in 1933, but never produced anything approaching an adequate report (Toth 1974:13, 21 ff).

After Setzler’s death, Ford felt a personal responsibility to complete the report, but Ford himself died in 1968 before he could finish the task (Brown 1978:34). For years, their Marksville non-report has been regarded as a “missing key” to that period in Lower Valley prehistory (Phillips 1970:896, 965), but a very recent discovery has revealed that Ford had gotten closer to completion of the final report than had been previously realized, and plans are afoot to publish a “nearly complete” report soon (Kuttruff et al. 1997).

The last hurrah for the BAE in archaeology was the massive “River Basin Survey” program, involving extensive and intensive excavations despite its title. It began in 1945–46 and finally wound down in the late 1960s. It is best known for salvage work in reservoir areas designated by dam construction projects in the Missouri River drainage basin, but it also became involved in several Southeastern states, though not in or near Mississippi (Lyon 1996:203).

So much for the BAE, but anthropologists and archaeologists based in the Smithsonian’s Department of Anthropology and/or the NMNH have continued to contribute to Southeastern and other U.S. Indian studies. They include William C. Sturtevant, long-time curator of North American ethnology, who has special interests in the Eastern U.S.; and Bruce D. Smith, since the 1970s curator of North American archaeology, with synthesizing interests mainly in and near the Southeast, evolving from an initial specialty in archaeozoology to a more recent emphasis on archaeobotany.

During the 1970s, the first volumes in the Institution’s long-planned “new” Handbook of North American Indians project began to appear, under Sturtevant’s general editorship. The complete set is projected to comprise an encyclopedic 20 volumes; at this writing, 11 have appeared, including two dealing with the Southwest (the only culture area so honored). Based on my rather desultory use of the volumes so far available, they appear to be solid, if somewhat stolid, compendia by mainly senior figures from both academic and governmental institutions, and (from my perspective as a former Southwesterner) perhaps a bit dated. Hinsley’s (p. 282) comment on the 1907–10 Handbook, “a model of nineteenth-century popular science—a generation later,” might easily be adapted to many of the volumes and articles in the new series, by clicking the century number up a notch, and changing “popular science” to “Establishment anthropology and archaeology.”

The “Southeast” Handbook volume has not appeared yet (though a renewed “push” toward its completion was initiated in late 1998), but the Smithsonian Press, perhaps inspired by Smith, has recently brought forth a number of publications of great interest to Southeasternists. I will not cite

21 See the next note for a somewhat similar “insider’s” evaluation of another recent Smithsonian series.
them formally or exhaustively here, but they include reprints of Cyrus Thomas’s “Mound Survey” report and Swanton’s Soto volume, both with new introductions by Southeasternists, and various new monographs on Southeastern archaeology and related topics, including the 1989–1990–1991 Columbian Consequences volumes edited by David Hurst Thomas, and most recently Dan Morse’s 1997 report on the Sloan site, a Dalton cemetery in northeast Arkansas.

With regard to Eastern and Southeastern archaeological museology, though, the Smithsonian’s Anthropology Department and NMNH appear to have kept a low profile in recent years, if not decades. This may have been merely a result of an emphasis on research, but may also have been a sort of adaptive response to the changing political climate of increasing Indian activism and the passing of the NAGPRA legislation with the possibility of massive repatriations of artifacts as well as skeletal materials. A related factor may have been the prospective creation of the Institution’s National Museum of the American Indian (NMAI; see below). In any event, the NMNH has so far been spared political firestorms like the one that erupted in 1995 around the Smithsonian’s National Air and Space Museum’s planned exhibit relating the story of the World War II bomber Enola Gay and the atomic bombing of Hiroshima and Nagasaki.

This low political profile may have been gained at the cost of stagnation in the archaeological exhibit department, though. Admittedly, I have been “out of the loop” for some time, but when I last saw them some time ago, a number of the NMNH’s “Indian” archaeological exhibits were virtually “museum pieces” in their own right (the same could be said of displays at a number of other large museums, including the Field in Chicago). The last major Smithsonian showing of Eastern/Southeastern archaeological materials that I can recall was the 1985 “Ancient Art of the American Woodland Indians” exhibit, which consisted predominantly of borrowed items (Brose et al. 1985), and its venue was the National Gallery of Art, not the NMNH.

32 The volume’s editor, David Hurst Thomas (1991:xxi) concluded, “... despite our best efforts to elicit an extended suite of opinion and perspective, the final result remains biased toward white, Anglo, male scholarship.”

Into the New Millennium with the NMAI

I will conclude with a brief look at aspects of ongoing plans for the Smithsonian’s latest venture into museology, at the NMAI. For more than 50 years, beginning in the late 1800s, George Heye and his successors at the Museum of the American Indian in the Bronx had amassed one of the world’s finest collections of New World ethnological and archaeological specimens—about a million items, all told. Part of the bailout deal, alluded to near the beginning of this essay, was an agreement to maintain a public facility in New York. Presently at least, its exhibits (over?) emphasize western North American materials.

Much of the Heye collection, though, was to be moved south, to another NMAI unit, a Cultural Resources Center in Suitland, Maryland (a Washington suburb), scheduled to open in 1998. Of major interest here is the fact that the Heye Foundation had acquired many of the archaeological materials collected from the Southeastern states by Clarence Bloomfield Moore for the Academy of Natural Sciences of Philadelphia, from 1891 to 1918. Hopefully, Moore’s and other relevant collections will become much more accessible to Southeasternists in this new facility.

The final unit, the main NMAI exhibit building, after a great deal of noisy wrangling about its design (by an Indian architect), is currently expected to open as part of the Smithsonian complex on the National Mall in Washington around the year 2002. Its exhibits will also feature Heye collection materials.

It is uncertain what the new museum’s relationship will be with the NMNH collections. At present, they remain under the administration of the Smithsonian’s Anthropology Department. By the 1970s, the old NMNH building was virtually bulging at the seams, with cases of collections lining the corridors and even in the balcony of the Rotunda, overlooking the famous stuffed elephant centerpiece. Starting in the early 1980s, most of these materials were moved a few miles out of town to a huge new curation facility in Maryland, but its high-security, limited-access situation does not provide exhibit space, and departmental budgets have been limited since the Reagan years.

In passing, it is interesting to note that it has long been, in effect, the nation’s official policy to classify Native American “Indians” and their artifacts within “natural history” along with plant, animal, and geological speci-
mens, rather than alongside the works of lighter-skinned "white" folks. The new NMAI will continue this tradition of cultural segregation, but will at least separate "Indian culture" from "nature." Both decisions are in keeping with dominant trends of Indian activism and separatism in the 1980s and 1990s; the first director of the NMAI, W. Richard West, Jr., is an attorney with Southern Cheyenne Indian ancestry.

American Indians, then, are to have their own National Museum, but its exhibits and emphases will probably shortchange Southeastern Indians and archaeology. The NMAI’s research staff is heavily weighted toward ethnologists with interests in Western North America (7 of 12 persons listed on the NMAI Web site in July 1998). There are no Southeasternists (nor Northeasternists, nor Mesoamericanists, for that matter), and none who even list an interest in archaeology except for an Andean specialist. West himself has remarked, "This museum will be alive, not full of mannequins. It will be a showcase of live bodies and living arts and artifacts that are still used….I want to be sure that the museum treats all parts of the Indian culture, especially the arts, such as dance and graphic art. The view of culture in most museums is narrow, seen through the eyes of ethnologists."

Not to mention archaeologists! West’s plan certainly seems appropriate if the overwhelming purpose of the NMAI is to broadcast the message that Indian peoples and their cultures (or is it “the Indian culture?”) are still alive. I will readily admit to having an archaeological bias, but even allowing for that, it seems “narrow” to me to de-emphasize arts and artifacts that are no longer used, and cultures that no longer exist, but that represent the triumphs and tribulations of more than 10,000 years of culture history—over 95% of Indians’ temporal existence in the New World.

Acknowledgments and Dedication

Many thanks are due to Pat Galloway, for commissioning this review and for her patience in bearing with me through the delays as it expanded into a rather lengthy essay. I am also grateful to the interlibrary loan staff of the Library at the University of Arkansas at Monticello, for obtaining several long-out-of-print volumes for me. And to my wife Charlotte Copeland for her constant support and companionship, particularly in this case during our enjoyable and productive visits to used book stores, sites, and museums around the greater Southeast.

This essay is dedicated to my daughter Amanda Ann Jeter, on the occasion of her graduation with multiple honors from Washington University in St. Louis, in the not totally unrelated field of Comparative Literature.

Marvin D. Jeter is Station Archaeologist with the Arkansas Archaeological Survey at the University of Arkansas in Monticello.

References Cited

Binford, Lewis R.

Blitz, John H.

Brose, David S., James A. Brown, and David W. Penney

Brown, Ian W.


Bruce, Robert V.

Chapman, William Ryan

Collins, Henry B., Jr.
Darrah, William C.

Dealey, James Q.

De Mille, Richard

Fenton, William N.

Galloway, Patricia K.


Gilliland, Marion Spur


Greengo, Robert E.

Griffin, James B.

Harris, Marvin

Harris, Marvin

Haury, Emil W.
1945 The excavation of Los Muertos and neighboring ruins in the Salt River Valley, Southern Arizona. Papers of the Peabody Museum, Harvard University 24(1).

Hinsley, Curtis M.


Jacknis, Ira

Jeter, Marvin D.


Kuttruff, Carl, Michael J. O'Brien, and R. Lee Lyman

Lamberg-Karlovsky, Carl C.

Lumsden, Charles J., and Edward O. Wilson

Lyman, Stanford M.

Lyon, Edwin A.

Mallory, J. P.

McGee, William John

Meltzer, David J., and Robert C. Dunnell

Oswalt, Wendell H.

Phillips, Philip

Phillips, Philip, James A. Ford, and James B. Griffin

Powell, John Wesley
1881a On the evolution of language, as exhibited in the specialization of the grammatical processes, the differentiation of the parts of speech, and the integration of the sentence; from a study of Indian languages. First Annual Report of the Bureau of Ethnology, 1–16. Smithsonian Institution, Washington, D.C.

1898 Truth and error: Or, the science of intellection. Open Court, Chicago.
1903b Sophiology, or the science of activities designed to give instruction. Twentieth Annual Report of the Bureau of American Ethnology, clx–cxcvii.

Raphael, David D., and Alec L. Macfie

Renfrew, Colin

Scruton, Roger

Smith, Bruce D.
Smith, Bruce D.  

Stegner, Wallace E.  

Stocking, George W., Jr.  


Thomas, Cyrus  

Thomas, David Hurst  

Toth, Alan  

Vidich, Arthur J., and Stanford M. Lyman  

Walker, Winslow M.  

Ward, Lester Frank  
1913–18 Glimpses of the cosmos, vols. 1–6, with editorial assistance by Emily Palmer Cape and Sarah Emma Simons. G. P. Putnam’s Sons, New York.

Williams, Stephen  

Wilson, Edward O.  


Young, Gloria A., and Michael P. Hoffman (eds.)  
Book Reviews


Reviewed by Charles H. McNutt

Unexpectedly, I find myself most dismayed by the title of this volume. Titles are very important; typically they are displayed prominently on the cover of a volume and indicate, to some extent, what the reader can expect to find inside. The editors describe the Central Mississippi River Valley as “between the Arkansas River...on the south and Thebes, Illinois on the north” (p. 1). The present volume contains 13 contributions on a variety of topics. The introductory chapter (Ch. 1) emphasizes southeast Missouri, but does comment at some length on Phillips et al. 1951. An additional six chapters deal exclusively with the bootheal area of Missouri (Chs. 2, 6, 7, 8, 9, 11), four chapters (3, 5, 10, 13) deal with areas clustered about the bootheal (within 100 km), in northeast Arkansas, southeast Missouri, western Kentucky, and west-central Tennessee. Twenhafel (Ch. 12) lies some 150 km to the north, but is also some 50 km north of what is usually considered the northern limit of the central Mississippi Valley. Only one contribution (Ch. 4) considers any kind of data from the entire Central Mississippi River Valley. This is not a book, then, about the Central Mississippi River Valley. The editors appear to be well aware of this fact (p. 11).

Inevitably, the contributions vary in quality, but I was immediately interested in finding out what the “changing perspectives” might be. I found some excellent but quite traditional archaeology, some not-so-excellent quite traditional archaeology, some interesting technical studies, a very puzzling misapplication of mathematics, and a distribution study. The only contribution that struck me as what may be a “changing [new?] perspective” is Telser’s contribution on “non-site archaeology.” Generally, this is not a book about changing perspectives, either, although the editors attempt to outline
the manner in which contributors to the volume "readdress" problems faced by earlier investigators (pp. 26–29).

It would have been much more accurate, but not as catchy, to entitle this volume something like "New Information on (or Contributions to) the Archaeology of the Missouri Bootheel and Adjacent Areas." The present title is both ostentatious and misleading.

As is traditional, I will consider the contributions in order. Chapter 1, appropriately by the editors, is an introduction to the central Mississippi River Valley. They note, as was detailed above, that their contributors have emphasized the area of southeast Missouri. For this reason, they feel justified in emphasizing this same limited area in their introduction. Recalling that these are invited contributions, there is an uncomfortable circularity in this justification. The editors set out "(1) to summarize the who, what, when, and where of various research efforts; (2) to mention some of the major issues with which investigators wrestled; and (3) to show how some of the work reported in some of the various chapters included here has redressed these issues" (p. 4).

A major section on the physical setting follows. It is difficult to tell which editor contributes most heavily here, but I have always felt that O'Brien is at his best in discussing local topography and ecology (e.g. his section in Cat. Monsters and Head Pits). The who, what, when, where section is interesting—of necessity a bit patchy. The strictly historical aspects can be (and are) presented chronologically. The surprising things in this section are (1) the statement that the Cache River survey produced a "data base that is unmatched at present" (p. 24); and (2) the presence of only one reference to Carl Chapman. The Cache River survey of 1975 was a timely but self-conscious attempt to show that CRM archeology could be "scientific" if properly planned, etc. The editors must have access to information not widely published to make their claim for the unmatched nature of the database produced. Excluding the Dalton material, there are absolutely no artifact counts of the surface collections made or of the six-day limited testing carried out at seven prehistoric sites. I do not wish to belittle the accomplishments of this early survey, but I feel the editors' comment is a bit enthusiastic. Insofar as Carl Chapman is concerned, regardless of whether one agrees with him entirely (I don't), his importance to Missouri archeology needs no justification—it extends far beyond a single, passing reference.

This section also serves as a forum to introduce the ominous "essentialist," a term that quite frankly has become somewhat of a drone in O'Brien's recent writings. What he is worried about is the uncritical acceptance of phases as real, rather than arbitrary, constructs. Lack of criticalness is a valid concern, but hardly a new one. Griffin's warnings in his review of Phillip's 1970 work are obvious examples. Phases should not be accepted uncritically of course, but they should be refined to approach some cultural reality—not banished from the literature (especially if nothing is offered to take their place). An arbitrary phase, sensu O'Brien, cannot be refined or improved, unless one grants that it can be made less arbitrary and hence more real. This is not the place to dwell upon this revival of a long established argument (arbitrary vs. real) in new philosophical clothing.

It is unfortunate that the many problems faced by the earlier investigators were not presented in a more organized manner. The problems may have been caused by "essentialism," as the editors imply, but a simple listing would improve this section's organization and perhaps allow the editors to make a stronger point about the damage done by essentialist philosophy and to offer something positive and presumably non-essentialist.

The third goal, to examine how some of the contributions contained in the volume readress some of the problems faced by earlier investigators, comprises commentaries on each chapter. These commentaries are best discussed in conjunction with the chapters to which they pertain.

Chapter 2 is a contribution by Gregory L. Fox, in which he attempts to examine Mississippian-period phases in southeast Missouri. He examines Stephen Williams's 1954 data from the Cresno excavations and also assesses "the similarity between and among the multiple phases and components defined by Williams (1954) and others" (e.g., Marshall 1965). He uses frequencies of standard types taken from "published and manuscript sources and from collections housed at the Museum of Anthropology, University of Missouri—Columbia" (p. 38).

Much of this analysis is taken from his 1992 dissertation (e.g., his Table 2:4 is the same as Table 8 in his dissertation). Unfortunately, he uses the Brainerd-Robinson (BR) Index of similarity to achieve his comparisons. The BR technique is a crude, virtually meaningless, and inappropriate method to compare groups of artifact assemblages. Crude because it is based on cumulative absolute differences between percentages of pottery types
with no regard whatsoever to the types involved; *virtually meaningless* because it is based on percentages (which must sum to 100) and you can compare two collections of 5000 sherds, one of 5000 sherds with one of 25, or two with 15 sherds apiece with equal facility (I observed this more than two decades ago in *American Antiquity*, vol. 28); inappropriate because it attempts to provide a one-dimensional solution for a multi-dimensional problem.

The BR technique has not been overly popular in recent years, not because of my earlier admonishments but because of the ease with which more sophisticated techniques—cluster analysis, k-means analysis, multi-dimensional scaling, principal components, correspondence analysis, etc. have become available in computer packages. Unfortunately, there seems to have been a revival of the BR technique by O'Brien and some of his students.

A few specific comments on the Crosmo analysis will suffice to point out the pitfalls of relying on BR. Although Fox does not give his basic data for Crosmo in this paper, he does provide it in his dissertation. His BR matrix for Crosmo in this paper includes Mississippi Plain and Bell Plain percentages. Because these two plainware types constitute 90% of the collections, his similarity matrices are not particularly discriminating—or interesting.

Eliminating Mississippi Plain and Bell Plain counts from Williams's 23 "analytical units" (AUs), only six have more than 100 sherds (Units 4, 10, 14, 17, 18, and 20); three additional units have more than 91 sherds (Units 2, 6, and 12). No AU has counts between 70 and 92, so the abovementioned nine units can be considered as the "most productive," and might conceivably be of some analytical value using BR. At the other extreme, eight AUs (again discounting Mississippi Plain and Bell Plain) have fewer than 30 sherds (AUs 5, 7, 9, 13, 15, 16, 19, 21, and 23). Their significance in a BR analysis is dubious, at best. Unfortunately, Fox hangs his hat on the very smallest unit—AU 23.

The least-similar coefficients [recall the inclusion of plain wares in Fox's calculations] result from comparisons made between other units and AU 23, the deepest unit (3.5 to 4.0 feet, in Excavation Section 3). This coefficient is somewhat surprising since AU 23 contains all the horizon markers of the Cairo Lowland phase as defined by Williams (1954)—Kimmswick Fabric Impressed, the Wickliffe series (Plain, Cordmarked, Thick, and Incised), and O'Byam Engraved—and does not contain any Crosmo/Cahokia Cordmarked sherds (a marker of the Early Mississippian Period occupation...). Interestingly, this deep AU is most similar to two units (AU 9 and AU 10) that were closest to the surface...suggesting that the excavated Crosmo assemblage represented in Williams's (1954) units is primarily a mixture of sherds from different periods and thus there is no basis for identifying distinct Early Mississippian-period and Late Mississippian-period components (pp. 41-42; my brackets).

This sweeping analysis is based on the misrepresentation of exactly 3 sherds. Of the 152 sherds in AU 23, 149 are either Mississippi Plain or Bell Plain. There are 3 other sherds. Two are classified as "Wickliffe Thick series." I do not understand how these can represent "Wickliffe Plain, Wickliffe Cordmarked, Wickliffe Thick, and Wickliffe Incised." The third sherd is Kimmswick Fabric Impressed. Contrary to Fox's claim, there is no O'Byam Engraved (or Incised) reported.

I will not follow the additional "statistical analyses" presented in this chapter. Fox is well aware of the problems caused by the overbearing nature of Mississippi Plain and Bell Plain, and does alternately include and delete them from his subsequent analyses, largely cluster analysis and multidimensional scaling of BR indices of site-pair assemblages. (Cluster analysis of points representing pairs of sites [BR indices] places unrealistic stress on phase definitions and is almost certainly doomed to failure.)

This brings me to a final point. In an attempt to evaluate the BR indices of Williams's excavation units at Crosmo (Table 2-4) I realized that *nowhere does Fox provide any pottery type counts in this chapter*—for Crosmo or for any other site collection. One must take these indices at face value, with no knowledge of the size or composition of the collections being manipulated. Because some of these data are from manuscripts and some are derived from unpublished collections, this omission is extremely unfortunate. Publishing raw data such as is represented in these collections would not take up too much space—I suspect that much of it is the same as appeared in *Cat Monsters* and in Fox's dissertation. These data would allow one to see the size of the collections—some quite small—and to note any erroneous calculations or unwarranted interpretations. I am quite surprised that the editors allowed this omission to take place.

In closing, I would observe that Fox has some interesting ideas, but he certainly has not demonstrated them here. There are indeed overlaps and
inconsistencies in phase definitions and contents—again I refer the reader
to Phillips’s 1970 graphs of three “Nodena Phase” sites, and the differences
between Upper Nodena on the one hand and Notgrass and Carson Lake on
the other (1970:931; Fig. 44Bb). It is not, as the editors claim, a “clear
demonstration that phenomena once treated as being the same are more
similar to phenomena treated as being different...that the phases in current
use in southeastern Missouri are neither classes nor groups, but inconsistent
sets of assemblages...” (p. 26-27). Much of the apparent confusion shown
in Fox’s Tables 2-5 and 2-7 is probably due to his use of the BR coefficient
on small collections rather than to fuzzy classification. I would urge him to
pursue application of cluster analysis (but not of BR indices), k-means analy-
sis, and MDS to more appropriate data, as has been advocated on recent
occasions by Bob Mainfort; to avoid the BR index like the plague unless he
feels he has large, representative samples representing short time spans; and
above all, to disclose his data so that his findings can be examined critically.

Chapter 3, by Paul Kreisa, examines the effect of spatial variation on
competing chronologies in the Ohio-Mississippi confluence region, spe-
cifically those of Lewis and Wesler. These are radically different kinds of
chronologies. Lewis divides the post-A.D. 900 period (i.e. Mississippian)
into four 200-year segments. These are regarded as phases (James Bayou,
A.D. 900–1100; Dorena, A.D. 1100–1300; Medley, A.D. 1300–1500; and
Jackson, post-A.D. 1500). Lewis then seems to ascribe proper cultural con-
tent to these phases. He is quite emphatic in stating that his “phases” are not
time periods; Kreisa gives a good account of Lewis’s justification for this
procedure. It cannot be denied, however, that the primary basis for this
scheme is chronological—not cultural. Kreisa also provides an excellent and
concise summary of the cultural content ascribed to the first three phases,
which are the only ones that overlap with Wesler’s chronology.

Wesler developed his chronology on the basis of his excavations at
Wickliffe and proceeded in a more traditional manner, isolating his phases
based on cultural content and subsequently ascribing chronological param-
eters to his scheme. He proposed a tripartite scheme: Early Wickliffe (A.D.
1100–1200); Middle Wickliffe (A.D. 1200–1250); and Late Wickliffe (A.D.
1250–1350). As Kreisa notes, this scheme has been extended from Wickliffe
to other parts of the confluence area. Wesler’s chronology overlaps Lewis’s
Dorena phase and the first 50 years of the Medley phase. (I am not comfort-
able with Lewis’s time-based “phases.” Having made this caveat, I will dis-
continue the tedious use of quotation marks around the word.)

Kreisa next notes a number of conflicts between the cultural content
ascribed to these various phases. Differences do indeed exist, but they are
differences in degree, not in kind. To examine the possible causes of these
differences, Kreisa uses ceramic assemblages from four sites excavated by
the University of Illinois project in western Kentucky: from north to south,
Rowlands, Twin Mounds, Turk, and Adams. These are described as form-
ing a north–south transect. A better description of this transect, in my mind,
would be (from south to north) Adams, Turk, [Wickliffe], and Twin Mounds,
with Rowlands well to the east, up the Ohio and near Kincade. This is a
minor point, but possibly of interest to those not possessing Kreisa’s article.
(Because so much has been made of the Crosno site in the previous chapter,
it may be worth noting that Crosno lies across the river, approximately
midway between Adams and Turk.)

Kreisa uses sherds and radiocarbon dates from his four sites spanning an
interval (calibrated) of ca. A.D. 1150–1450. He provides both his sherd counts
and radiocarbon dates. He creates three time intervals from his radiocarbon
dates in a very peculiar way. He observes that “One set of calibrated dates
has ranges between roughly A.D. 1200 and 1300, and another set clusters
between A.D. 1300 and 1400.” If this were correct, he would have two clus-
ters—early and late. But he then proceeds: “Taking the centerpoints of these
ranges, A.D. 1250 and A.D. 1350, respectively, results in a three-fold tempo-
ral division: early, or pre-A.D. 1250; middle, or A.D. 1250–1350; and late,
or post-A.D. 1350” (p. 68). His “middle” now consists of equal parts of his
early and late clusters.

Fortunately, Kreisa provides data (Fig. 3-3) indicating that he really has
three clusters of dates!: Early dates at Adams-5 and Turk-3, with calibrated
ages (+1 standard deviation) of A.D. 1030, 1165, 1225 and 1030, 1160,
1220, respectively. Another group of five dates has (middle) intercepts closely
clustered at calibrated A.D. 1280–1295. Five additional dates are clustered
at cal. A.D. 1340–1355. Two dates are post-A.D. 1400 and one date (Adams-
3) has calibrated ranges of A.D. 1170, 1230, 1280. This latter date is diffi-
cult to place in any cluster—it overlaps the upper end of the 1 s.d. range of
the early pair of dates and the lower end of the 1 s.d. range of the second
group.
All of this manipulation has a happy outcome—a dividing line between the early dates and the cluster of five dates at A.D. 1250 is not unreasonable; it might be a bit earlier. Likewise, a division between the middle and later groups at A.D. 1350 is acceptable but should probably be 25 years earlier—at A.D. 1325. We can, however, proceed to examine Kreisa’s analysis roughly in terms of his early, middle, and late periods.

Kreisa takes what may indeed be a new perspective—he accepts his dates, or at least his date ranges—and examines the temporal implications for fluctuations in the appearance and decline of ceramic assemblages. Many of us tend to explain differences in radiocarbon dates away and to ignore minor differences in ceramic frequencies.

Kreisa’s article is carefully crafted, and he suggests that certain ceramic types are earlier in the northern part of his (quite limited) region, while others seem to appear earlier in the south. He has provided testable hypotheses regarding the variation of proportions of ceramic types used to define phases in different areas and has shown that the proportions used in these definitions may well be rather arbitrary breaks in a temporal continuum.

It may be noted in conclusion that the anomalous behavior of his “Early Twin Mounds” sample in his cluster analysis (Figure 3-4) is due to the fact that this is the smallest sized sample he has and, consequently, very small aberrations can have disproportionate impact. In this particular case, his 12 sherd from Kimmswick are the culprits—if eight or nine of them had been Mississippi Plain, things would have worked out much better. I only make this point to emphasize that radical variation in sample size (and Kreisa’s samples vary from 151 to 3943 sherd) requires great caution. I can do no better than quote Kreisa, who notes that “To enhance the accuracy of chronology, archaeologists must continue to control for sample size, use a number of assemblages, adequately date those assemblages and, perhaps most important, identify the geographic limits of accuracy for the proposed chronology” (p. 79, emphasis in original).

Chapter 4, by David H. Dye, examines the distribution of Walls Engraved pottery in the Central Mississippi Valley, and is the only paper in the entire volume that considers basic data from the Central Mississippi River Valley as it is commonly perceived. Distribution studies are hardly a novel approach in Central Valley archaeology, but, as Dye shows, they can be extremely fruitful.

The author first describes the variability and history of reports of engraved pottery in the Central Mississippi River Valley. The two varieties of particular concern were originally given formal definitions in the Lower Alluvial Valley Report (PFG 1951) under the type names of Walls Engraved and Hull Engraved. The ambiguous relationship of these classes is again discussed. Finally, Dye discusses the distribution of varieties of a single decorative motif—the scroll. In passing, he also provides a very useful classification of relevant vessel forms.

Phillips and Williams, in developing their type-variety system, designated Walls and Hull as varieties of Walls Engraved—var. Walls and Hull. Apparently engraved vessels with Rhodes scrolls were also subsumed under Walls Engraved, var. Walls. Because both Hull and Rhodes are distinctive and unique designs, the wisdom of regarding them as varieties of Walls Engraved—just because they are engraved and from the Central Valley—is a matter for future discussion, in the opinion of this reviewer. Admittedly, not a major matter.

Dye’s presentation of data on var. Walls and Hull is excellent. He lists the sites from which he has whole vessels and sherds, noting the paucity of examples from each site. (One might wish that Dye had followed Fox’s example and labeled his sites.) He gives the distribution of vessels and sherds of both varieties. As noted above, he categorizes eleven major vessel forms on which these varieties occur, and uses these in his distributional discussions, listing six areas from which the majority of his specimens derive. Dye notes that subglobular bottles (by far his most common form) extend from southeastern Missouri to just south of Memphis. He observes that subglobular bottles with short necks and wide orifices (again, his most common bottle form by far) occur with higher frequency “in the Horseshoe Lake and Walls areas than in other areas” (p. 95). In point of fact, they occur in equal numbers in his Wilson area sites (Upper Nodena, Turnage, and Pecan Point). He also observes that “subglobular bottles with tall necks and narrow orifices occur throughout the region except in the Horseshoe Lake and Walls locales.” Because Dye is examining six major areas and only has four such bottles, the significance of this observation remains to be evaluated.

Dye follows this with a very interesting observation—interesting because it is a completely testable hypothesis: “I propose that the distribution of neck forms associated with Walls Engraved vessels reflects the general
distribution of those neck forms independent of whether they occur on engraved vessels." He also postulates that the high shouldered jar has a distribution localized to the Horseshoe Lake area and is rare in other cases (p. 95).

One final point on Dye's distribution study. His maps show the distribution of whole vessels and (separately) sherds. His examination demonstrates a point that seems intuitively obvious (especially after someone else has pointed it out). Distribution studies, particularly those concerned with relatively rare types, are best conducted at the level of sherds rather than whole vessels.

Dye concludes his study with an examination of the distribution of a single family of designs—various types of scrolls found on Walls Engraved vessels. Whereas he is able to see tendencies for some of his scroll variants to be geographically concentrated and to be associated with specific vessel shapes, these associations are not as neat as one may like. This may be exactly what we should follow up on, so that we may some day utilize the terms "trade sherd/vessel" and "intrusive" in Central Mississippi River Valley archeological terminology.

It is poor taste to criticize someone for not doing something they never intended to do, and I won't start now. I must comment, however, that Dye obviously has a tremendous database for Walls Engraved sherds and vessels. It would be interesting if either he or one of his students would use this information to test and perhaps refine the various varieties of Walls Engraved proposed by Terry Childs in the Arkansas Archeologist (vol. 32, 1993).

Chapter 5, by Robert Mainfort and Michael Moore, describes a Phase II project at the very interesting Graves Lake site (40-La-92), located in the Lower Hatchie National Wildlife Refuge, Lauderdale County, Tennessee. A ceramic collection had been obtained from the northeast sector of the site in 1987. The 1990 investigations described in this chapter were occasioned when a maintenance worker made several passes with a road grader along the eastern boundary of the site, exposing daub and other cultural material.

The site was mapped, a controlled surface collection was made, test units were made in what appear to be three house areas, other tests were made to determine the extent and depth of the site, and seven radiocarbon dates were obtained.

Discussions, analyses, and distributions of materials from the 1990 surface collection are provided, as are descriptions and maps of portions of three apparent houses. Sherd tabulations from the 1987 collections and from Richardson's Landing (n=1067) are also provided. The 1987 (n=471) and 1990 (n=2278) collections from Graves Lake are rather different. Mainfort notes (p. 109) that "the higher frequency in the 1990 collection of sherds of the type Bell Plain and of sherds of several decorated types...might be attributable to the reported occurrence of numerous graves in the 1987 collection area. Radiocarbon determinations presented below suggest that use of the 1987 collection area might postdate major use of other portions of the site."

I suspect that his reference to the high frequency of Bell Plain in the 1990 collection (quite correct if he equates "frequency" with "count," [1987 n=222; 1990 n=776]) is essentially a slip, and he is referring to the higher proportion of Bell Plain (47%) in the 1987 collections (vs. 34% in the 1990 collections). It is dangerous to put words in a colleague's mouth (especially Mainfort's), but this would be in keeping with the fairly established idea of high proportions of Bell Plain in burial areas. This is also in keeping with his observation that of the six appliqued sherds recovered (characteristic of post-A.D. 1540 assemblages in adjacent areas), all came from the 1987 collection in the southeast part of the site (p. 111).

Two radiocarbon dates were obtained for each of the partial house patterns exposed and a seventh was obtained from the 1987 collection area. As the authors note, the radiocarbon dates could represent two occupational periods, although all of the dates overlap at ca. cal. A.D. 1450 at the two sigma range. House 1, located in the western portion of the site, had an average calibrated date of A.D. 1410 (1425) 1435 (1 sigma). House 3, near the top of the northeastern slope of the site, produced an average calibrated date of A.D. 1495 (1530, 1555, 1635) 1650 (1 sigma). Using a slightly newer version of Calib (3.0.3) than was available to the authors (which produces non-significant differences of ca. 6 years for these dates), House 1 dates average cal. A.D. 1411 (1431) 1442 (1 sigma), with a 100% likelihood that the actual date falls between A.D. 1412 and 1442. House 3 dates average cal. A.D. A.D. 1515 (1636) 1648 (1 sigma) with a 75% likelihood that the actual date lies between A.D. 1517 and 1587. Certainly a case can be made for House 3 postdating House 1.

The two dates from House 2 appear to present a dilemma: one date overlaps nicely with House 1 and the second date overlaps nicely with House
3. The average cal. date for House 2 is A.D. 1431 (1445) 1476 (1 sigma) with a 96% probability that the actual date falls within the interval A.D. 1418 and 1490. House 2 could be regarded as intermediary between Houses 1 and 3, but there seems little in the surface collections in the vicinity of House 2 to differentiate it from House 1 or to ally it with House 3.

The seventh radiocarbon date (TX-6079) came from the area of the 1987 collections and produced a date of cal. A.D. 1515 (1640) 1660 (1 sigma). This date, plus those from House 3, leads me to agree with the authors' statement (previously cited) that “Radiocarbon determinations presented below suggest the use of the 1987 collection area might postdate major use of other portions of the site” (p. 109). I would go further, and say that the radiocarbon dates, the ceramics, and the vessels from House 3 all indicate the strong probability that use of the 1987 collection area postdates that of the 1990 collection area. One can postulate an early occupation ca. A.D. 1420-1460 and a late occupation ca. A.D. 1525-1600.

If my task is to review, why spend so much time on analysis? Simply because I feel there is more significance to this article than meets the eye. In the words of Kit Wesser, I would say that the authors have a good date for the introduction of vertical applique forms in this area—sometime within the A.D. 1525–1600 (late) interval. Of course, they already felt this to be the case—“characteristic of post-A.D. 1540 assemblages in (adjacent areas)” (p. 111). But I think that Graves Lake provides the first validation of this date as far south as Lauderdale County, Tennessee.

The authors next provide a discussion of interiorly beveled rims (House's "Memphis Rim") as a characteristic of post-A.D. 1450 occupations. House found this to be the case in the Kent phase area. The authors classify collections from 15 sites in terms of “sharp,” “moderate,” “slight,” and “no” bevel. Site collections range in size from 15 to 473 rim sherds. The reader will be interested to know that both the Sweat (40-La-26) and Dry Arm (40-La-19) sites are included. A detailed analysis is not presented, but a strong case is made that “late period sites in the Central Mississippi Valley can be chronologically ordered on the frequency of rim beveling.” (p. 121). This hypothesis is applied to two groups of sites, providing site orderings that can be tested in the future by radiocarbon or other determinations.

In closing, supplemental data is appended from 1989 investigations at the Richardson's Landing site (40-Tp-2). Two radiocarbon determinations from a house had a cal. average of A.D. 1410 (1425) 1435. Charcoal from a sub-plowzone pit provided a calibrated date of A.D. 1410 (1435) 1465. These dates, by the way, correspond quite well with the early occupation postulated above for Graves Lake.

I regard this chapter as exquisite—the authors have presented a tremendous amount of data in a very few pages. They have provided us with testable hypotheses for the future, and have added greatly to our knowledge of western Tennessee.

Chapter 6, by Robert Lafferty, deals with change and settlement location in the Cairo Lowland—specifically in the New Madrid Floodway. After providing a brief history of major investigations in the area, Lafferty notes that his data come from a few, but thoroughly surveyed areas, as opposed to the more usual survey of large numbers of smaller blocks scattered randomly about the survey area. Actually, Lafferty deals with two areas—one in the northern portion of the New Madrid Floodway (Birds Point and O'Bryan Ridge) and one in the southern portion of the floodway (Barnes and Sugar Tree Ridges).

Lafferty outlines his goals explicitly: (1) to test Niemczycki's 1987 predictive model for site location, (2) to examine the dynamic development of the complex landscape in the northern portion of the survey area and the relation of archaeological sites to specific landforms, and (3) to examine location of sites on two relatively stable landforms—Barnes Ridge and Sugar Tree Ridge—in the southern portion of his area.

He traces the developments in the northern portion of his area using the Fisk channels, which appear to be correct in their relative sequencing (but not chronological estimates) in this area. Lafferty uses radiocarbon dates from pollen cores and archaeological sites to date the evolution of the landscape in the northern portion of his area and is able to correlate sites of various areas to certain landforms/channels.

With regard to the predictive model, based largely on soil types, Lafferty found no significant difference (p=0.10) in numbers of sites in predicted high probability areas and predicted low probability areas. He did find that elevation was a very important factor—by transferring some 74 hectares of high elevation land from the "low probability" area to the "high probability" area, he found a significant difference in site association at the p=0.01 level.
In discussing the southern portion of his area, Lafferty is able to associate sites of broad time periods (Late Archaic, Woodland, Mississippian) with various Fisk channels and portions of Barnes and Sugar Tree ridges.

Finally, the author discusses settlement location in both the northern and southern areas through time. He observes that his sample is quite probably skewed in favor of later sites, due to destruction and/or burial of early deposits by the meandering Mississippi River. Lafferty takes Poverty Point fired-clay objects (PPO) as temporal markers for the Late Archaic period—30 such sites were found, some with very deep deposits. In one such site (Rinaud, 23-Mi-621), deposits were widespread and deep, and the author observes that “No temporally diagnostic artifacts from any other period were found, so we assume the deposit dates to the Late Archaic period.” (p. 144).

Next, Lafferty observes that forty-four sites in the project area contained sand-tempered pottery, and that there is a significant correlation (p<0.05) between this pottery and sites that produced PPOs. He continues “Some sand-tempered sherds may be contemporary with the Poverty Point objects; others probably date much later, pointing out that Woodland-period groups used the same landforms that their predecessors had” (p. 144). It should be observed that Lafferty appears to be regarding sand-tempered pottery as Early Woodland. This statement is of relevance to previous discussions by west Tennessee archaeologists (primarily Robert Mainfort and Gerald Smith) regarding the significance of PPOs. It echoes Smith’s statement in a book edited by the reviewer, who begins his discussion of Early Woodland with “Stability of the groupings established during the Poverty Point period is indicated by a general continuity of the same local areas into the Early Woodland period” (1996:104).

In the following paragraph, Lafferty notes that clay-tempered Baytown Plain and Mulberry Creek Cordmarked (found at 104 sites) are used as markers for Woodland-period occupations. Apparently both the sand tempered (Barnes series) pottery and clay-tempered Baytown series are being used as markers for Woodland—a fairly well established practice in the area. The probable existence of mounds at the Woodland period sites of Weems, Burkett, and Nally is noted.

For the Mississippian period sites, Lafferty uses shell-tempered and shell-and-clay-tempered sherds as markers. Evidence of Mississippian period occupation was best in the southern portion of his area (but evidence in the northern part of the survey area appears to be deeply buried). Lafferty concludes with the observation that in the southern portion of his survey area, radiocarbon dates suggest that clay temper was in use well into the thirteenth century and perhaps even later.

Although the reconstruction of the landscape, the testing of the predictive model, and the association of occupation periods with the Fisk channels and landforms was quite successful (and hardly claimed to be a new perspective), Lafferty raises some problems that deserve more attention. The temporal duration of the Barnes series bears discussion, and the problem of the late clay-tempered pottery also deserves amplification. In particular, if clay-tempered pottery is indicative of Woodland, who was making it in the thirteenth century and perhaps later? These are old and persistent problems in southeast Missouri. It is a bit much to expect the author to provide definitive answers, but his informed ruminations would have been appreciated.

Chapter 7, by Patrice Teltsner, is the only paper in the volume which I feel might represent a “changing”—i.e. new—perspective. Teltsner’s concern is Williams’s “Vacant Quarter” hypothesis. It may be wise, in order to evaluate this contribution, to repeat briefly segments of Williams’s most recent statement of his Vacant Quarter Hypothesis (in Dye’s Towns and Temples).

The Vacant Quarter Hypothesis raises the question of the general abandonment of the major Mississippian ceremonial centers in an area centering on the mouth of the Ohio River, on the Mississippi River from Cahokia...to New Madrid, up the Ohio River to Evanston, Indiana, and up the Cumberland River to the Nashville Basin. The desertion of these large mound centers and their supporting villages apparently took place in the period A.D. 1450–1550. Population relocation...is posited as part of a likely explanation....The term ‘vacant’ is used; however it should not be understood to suggest that the area was completely devoid of native American peoples who hunted there....The year-round settled villages are no longer there. (1990:173).

Teltsner does not deny the above outline of abandonment of major population centers, but suggests that an alternate to population relocation to other areas (as described by Williams) would be population dispersion. As she notes, “This would have created a late prehistoric record that appears very different and far less visible....Consequently, I suggest we approach
the late prehistoric record in terms of demographic reorganization. In this context, abandonment is only one of a variety of possibilities" (p. 150).

Teltscher chose three fields south of the Sandy Woods site and subjected each to very detailed surveys—essentially running transects spaced on 5 meter centers. Individual artifacts were flagged, two or more artifacts within 10 meters were designated "clusters," then the extent of each cluster was determined and collections were made. Individual artifacts were plotted and collected as well.

As Teltscher points out, "...the clusters...are observational units and have no behavioral significance. Because the surfaces examined are quite ancient, determining whether any spatial aggregate of artifacts represents an assemblage of associated artifacts is a chronological question, answered on the basis of analytical decisions very different from spatial association" (p. 155). I must return to this point later.

The author describes her collection areas and the clusters within each. It is difficult to associate her clusters with the artifact symbols she provides on the maps. Densities, ratios of ceramics to lithics, and other data are provided for her clusters. In her analysis the author also refers quite frequently to the "offsite record." I am sure this term is defined somewhere in the paper, but I have been unable to locate it.

As I understand Teltscher's study, based on artifact density only one (or possibly two) of her 16 clusters in the three fields would probably have ended up in the site files—an obvious underestimation of the amount of material, or at least number of clusters, in the three fields. Fourteen clusters produced pottery; all of these produced sand-tempered pottery (called Woodland) and 12 produced sherds containing, inter alia, shell tempering, used grudgingly as an indicator of Mississippian. It must also be noted that 12 of her clusters produced less than 0.15 artifacts/4-meter square.

Teltscher observes that, in the absence of well defined typologies for sand-tempered and shell-tempered pottery, she can say little more about chronology. Given the fact that she has found some very minimal scatters and quite a few "find spots," all undated, she certainly can't say much about "demographic reorganization."

In short, whereas this is a new approach—and one which may ultimately be of some value—I'm not sure where we can go with Teltscher's small, mixed, undatable collections at present. To demonstrate her proposed post-

A.D. 1550 demographic reorganization model (which does contain a temporal element), Teltscher needs to state her hypothesis quite clearly (she has), indicate how she proposes to test this hypothesis (this she has not done), describe how she proposes to operationalize this test (which she certainly has not done), and then proceed. I should think that comparably intensive surveys for what might qualify as post-A.D. 1550 Mississippian villages or hamlets (based on sherd collections) would be more relevant to the problem at hand.

Chapter 8, by Timothy Perttula, is an excellent (and traditional) report and analysis of materials found at Powers Fort, the civic and ceremonial center of the Powers Phase occupation in the Western Lowlands of South Missouri. The site is located on a major sand ridge near the Ozark Escarpment and Little Black River drainage ecotone.

After providing an excellent description of the site's setting, Perttula provides a thorough history of more than a century of work at Powers Fort. Indeed, approximately one-half of the paper is devoted to these sections. Much of this, however, consists of previously unpublished work by the 1969 Powers Phase Project.

The agent responsible for the "subsequent [i.e. post-Powers Phase Project] investigations" is not identified as firmly as one might wish, although it is apparently James Price, who "subdivided the site...for purposes of provenience control in further surface-collection activities..." (p. 185).

A tentative settlement plan is offered for the site, including a major mound, subsidiary mounds, a plaza, perhaps six rows of houses with some 24 houses per row, the location of a possible community house or specialized residence, and possibly a perimeter ditch (shown on earlier maps).

Material culture descriptions are brief and limited to ceramics and lithics. Apparently the Powers Fort pottery is almost all Mississippi Plain; a few sherds of Old Town Red are present along with a few examples of incised and punctuated sherds. Arrow points are similar to those reported from Turner and Snodgrass, although the proportions differ.

Probably the most interesting aspect of the Powers Fort site is that it appears to have maintained a sizeable population throughout the Powers Phase (A.D. 1250–1350), whereas other sites, even larger ones like Turner and Snodgrass, were occupied for a very brief time—perhaps five to ten years, then abandoned and possibly burned by their inhabitants. This situation should
provide an unparalleled opportunity to dissect the long-term Powers Fort assemblage into discrete units, based on the material from Turner and Snodgrass. Purtula is to be congratulated for this major contribution to archaeological knowledge of southeast Missouri, the Powers phase, and the Central Mississippi Valley.

Chapter 9, by Robert Dunnell, discusses his recent work at the Langdon site in Dunklin County, Missouri. Langdon is a large, fortified Mississippian site on the Malden Plain. Although known for more than a century, the earliest map of the site is a sketch-map made by Stephen Williams and appearing in his 1954 dissertation. Dunnell has some problems with Williams’s map (width of the state highway, etc.) but never comments on the accuracy or lack of accuracy of mound locations. (Williams completed his dissertation 45 years ago and persistent critiques of it strike me as a bit odd. At least Teltser is commenting on something more recent.)

Only a very small amount of work is reported in this contribution: “...an average of two and a half days with four assistants...in each of five years” (p. 205). In other words, two weeks with a 5 person crew. Still, a large amount of information was obtained—a topographic map was made, a controlled surface collection (transects) obtained, a magnetometer survey was conducted, and several thermoluminescence (TL) dates were obtained. Site structure was obtained rather satisfactorily with the aid of aerial photographs. In a manner of speaking, this chapter attests to the amount of information that can be obtained in a relatively short time.

Gross artifact frequencies are displayed in terms of both counts and weights—a trend that is thankfully gaining in popularity. Unfortunately, this treatment is not extended to more specific categories—i.e. “shell tempered” counts/weights are shown, but not various type counts/weights.

The advantages of TL dating are described in what I would term begrudging detail, and the range of dates for Langdon is given; a list of individual dates would have been preferable. After some comments regarding correction factors for TL dates, the occupation of Langdon is estimated by Dunnell at between A.D. 1100 and A.D. 1500. These dates are approximately what one would assume from the pottery, although the presence of Campbell Applique and Wallace Incised may argue for a slightly later duration.

Although Dunnell discusses his ceramics and lithics, he provides no counts. Even though this is justified on the basis of the non-representative-ness of transect samples, these samples are what he—and his readers—have to work with. At least, we are informed that only 3% of his sherds show Bell paste, the remainder coarse-shell types. A wide variety of forms—salt pan, hooded bottles, jars, long and short necked bottles, etc.—is represented. In discussing his lithic assemblage, he notes that both Madison and Nodena forms are present (but again, no counts) and attributes this to a late Mississippian assemblage. There is no mention of end scrapers, pipe drills, large triangular points, or the late Armorer Focus.

In concluding, the author discusses what little is known of a settlement pattern in the vicinity of Langdon—there has been very little survey work in the area. A three-tiered system seems to be present but remains to be validated.

The author concludes, that early Langdon seems fairly closely related to sites in the Cairo Lowland, but that it (and sites such as Vancil) become distinctive in their own right, representing a consolidation of populations. This is in keeping with what has been thought regarding the Bootheel for some time. His stated need to place Langdon more firmly in a regional context goes without saying—it applies to a tremendous number of other sites in the area. Well dated and well defined phases may ultimately help.

Chapter 10, by David Benn, discusses his excavations at the Moon site in Poinsett County, Arkansas. This contribution is certainly one of the gems of this volume. Benn’s work at the Middle Mississippian Moon site and the nearby Emergent Mississippian Priestly site constitutes, in my opinion, the major contribution to Central Mississippi Valley archaeology since the Powers Phase project in the 1970s. What makes this work so important, particularly at Moon, is that Benn has exposed an entire site, much in the manner of Price’s work at Snodgrass and the massive FAI-270 projects in the American Bottom.

After a brief summary of work in the area, Benn describes his field methods—a total controlled surface collection, followed by machine stripping of the entire site area and hand skimming of features. The approach is certainly novel, but in this case some 35 structures and associated features were disclosed. Ultimately, after reporting his surface collected material in some detail, he produces an excellent site plan, showing houses clustered about two plaza areas, surrounded by a palisade. It is against this site plan that Benn is able to plot the total distribution of various key artifacts.

The ceramics at the site can only be described as weirdly utilitarian. Of almost 25,000 excavated shell-tempered sherds, 99.9% were classified as
Mississippi Plain, 0.1% Bell Plain. Twenty-eight decorated vessels, as I read the author, are represented by 32 sherds. What may be a high-status (or established status) area is isolated about the plaza in the southwestern corner of the site on the basis of relatively dense concentrations of decorated pottery, Bell Plain, and other unusual items.

Probably the most important section of Benn’s contribution is his discussion of the development of community and settlement patterns. Drawing upon his 1990 discussions in the Priestly and Moon reports, he sees the Early Mississippian settlement at Priestly as fairly typical. This site consisted of about a half dozen square houses along a sand ridge next to a water source, with an unusually large public structure (evidently a chancel house) that probably housed the dispersed communities around Priestly. Other characteristics of the Early Mississippian sites in the area include building structures with standard orientations, grouping structures around a central plaza or courtyard, and the construction of public facilities. He also notes that Early Mississippian and Late Woodland settlements occupied the same alluvial environments and probably had similar relations to their environment—hunters, gatherers, and simple horticulturalists. Subsequent developments include nucleated villages and, after about A.D. 1000, the construction of planned communities and ceremonial centers with conspicuous mounds. Benn sees strong parallels to the pre-A.D. 1000 sequence in the central and southern parts of the American Bottom, during the Patrick, Dohah, Range, and George Reeves phases. To the extent that this is true, one might think that this same settlement sequence should be found in the Bootheel of southeastern Missouri. Benn is to be congratulated for his major contributions at Moon, Priestly, and in the present volume.

Chapter 11, by Patrick McCutcheon and Robert Dunnell, is a study in the variability of Crowly’s Ridge gravel. Two broad questions are addressed: (1) are prehistoric lithic assemblages random samples of Crowly’s Ridge gravel and (2) if gravel exploitation is not random, what are the lithic properties responsible for the non-random pattern.

The authors describe Crowly’s Ridge as an erosional remnant. As I understand it, there also exists the possibility that the ridge is the result of tectonic uplift—this is a matter for geologists to resolve (cf. Van Arsdale et al. in Bull. Seismological Soc. of America 85, 4:963–985; 1995) and not specifically germane to the present contribution. But they also state that “throughout prehistory the [Crowly’s Ridge] gravels were the major source of raw material for groups living in much of the central Mississippi Valley.” This seems a bit strong, and again reflects the unfortunately localized focus of this volume. Rock can be (and was) also obtained from the bluffs and uplands along the eastern side of the Mississippi River.

Three collections of Crowly’s Ridge gravel were made. A complex set of dimensions and attributes is developed, involving variables of groundmass, solid inclusions (present/absent), void inclusions (ditto), distribution of solid inclusions, and distribution of random inclusions. The authors’ first concern was to determine whether or not rock physical properties correlate with gravel size. A large number of t-tests was run; the null hypothesis (no significant difference, p<0.05) was rarely rejected. The only conclusion reached is that “rocks with voids are larger in terms of mean weight and length in all three gravel collections. The consistency of this pattern suggests that the presence/absence of void inclusions is interacting with gravel size and therefore that gravel size is dependent on gravel composition” (p. 271).

As a result of the above, the authors felt it best not to pool all three samples. Presumably this means that the three samples vary geographically in composition even though standard sizes were used, but I am unable to see that this is demonstrated.

The authors next compare five archeological assemblages with the nearest gravel sample, using the previously mentioned dimensions. The Crowly’s Ridge gravel samples were used to generate a set of expected values against which the archeological assemblages could be compared using chi-square tests. The presence/absence of solid inclusions does not depart significantly from the composition of the nearest gravel source in any case. All other dimensions (with one exception) depart significantly from the composition of the nearest gravels. Obviously, gravel was rejected on the basis of groundmass, inclusion distribution, and the presence of voids. In order to determine the magnitude of difference (presumably strength of association) a set of Brainerd Robinson coefficients was generated. I have already commented at length on BR indices—their use as a measure of either the magnitude of differences or strength of association in a chi-square test is certainly novel. Not surprisingly, “the highest scores should be between the nearest source-assemblage pair. This is generally not the case, nor was it the
case for the solid inclusion dimension, in which no apparent selection is present" (p. 273). The test for association was regarded as inconclusive, suggesting that either the sources were not the ones exploited by the local inhabitants or that multiple sources were used. In view of the many significant chi-square values, some indication of strength of association (Cramer's V, phi, tau, etc.) would have been interesting.

The next tests were for fracture toughness. It was determined that orthoquartzites have "lower mean fracture loads" (break easier) than non-granular cherts. Seventeen prepared slabs were tested, but weight loss resulting from these experiments was not collected systematically. It is suspected that orthoquartzites tend to crumble.

A great deal of effort seems to have gone into this paper. In concluding, I can only quote the authors: "The rocks comprising Crowley's Ridge gravels vary in size and composition at all locations sampled. As a result, much more detailed analysis of this important lithic resource is required before its archeological use can be understood" (p. 279).

Chapter 12, by Carol Morrow, discusses blade technology and non-local cherts at the Tewenhafel site in southern Illinois. She notes that Midwestern archeologists traditionally view evidence of prismatic blade technology and non-local cherts—particularly blue-gray cherts—as being Middle Woodland (ca. 250 B.C.-A.D. 400) in age and Hopewellian in terms of cultural manifestation. She examines these assumptions using data from the Tewenhafel site, occupied during the Middle Woodland and early Late Woodland periods, ca. 200 B.C.-A.D. 600. The specific question addressed in her chapter is whether these two traits, blades and non-local cherts, correspond precisely with the appearance of Havana Hopewell pottery at Tewenhafel or whether they preceded and/or postdated that appearance (p. 281).

A discussion of the history of the traditional view, cited above, is given. The association of blades and blue gray chert, particularly Cobden/Dongola chert from the vicinity of Tewenhafel, seems well documented. Indeed, Tewenhafel has been considered the primary site that traded Cobden/Dongola chert (primarily in the form of disks) northward into the Havana area and as far to the south as the Upper Sunflower in Mississippi. Havana groups were apparently making disks of their own from local sources, including the raspberry-colored Grimes Hill chert. This latter is of interest in that it is found in small amounts at Tewenhafel.

Morrow's material was recovered from village area midden, apparently largely from pits. She employed Braun's 5 sub-period scheme for Middle and Late Woodland, ranging from early Middle Woodland (150 B.C.-A.D. 50) through late Late Woodland, and describes the ceramic content of associated pits and the size of the lithic assemblages. An excellent discussion of her terminology is followed by a statement of her analytical goals: to trace the presence or absence of conical-core blade manufacture, biface production, and amorphous core reduction.

Results of her analysis show that both blade and biface technologies were at their peak in the early Middle Woodland period, (presumably 150 B.C.-A.D. 50) and declined through late Late Woodland times (post-A.D. 600). One peculiar thing becomes apparent in examining chert types from which blades are manufactured: local Cobden/Dongola and non-local Crescent and Grimes Hill cherts are used. But during the middle Middle Woodland and early Late Woodland (which produced most of the blades in the sample) blades of non-local cherts are almost as common as Cobden/Dongola blades. Analysis suggests that Crescent chert was probably imported to Tewenhafel in the form of prepared blade-cores and that the Grimes Hill material was imported in the form of finished blades. A very convincing argument for mining earlier deposits ("recycling") by late Late Woodland occupants is adduced. (This consideration may have some relevance to the previous chapter.)

The author's conclusions bear some examination. She feels she has tested the assumption that blade technology and use of non-local cherts were coeval with other Hopewellian traits by examining "whether those two traits were restricted to the period A.D. 1-200. The analysis here demonstrates that the appearance and use of blades and nonlocal cherts at Tewenhafel occurred earlier and lasted much longer than that time span" (p. 297). This seems like a very restrictive date range to test Hopewell associations. She has shown maximal use of blades at Tewenhafel during the early Middle Woodland period, but the associated pit contained "a few sherds of Havana ware (Havana Plain and Havana Zoned) ... but Hopewell/Baehr materials were absent" (p. 286).

At the Holding site to the north, in American Bottom, blades are common during the roughly contemporary Holding phase (50 B.C.-A.D. 150). Good relationships to Tewenhafel are shown in the presence of Cobden/
Dongola cherts. Of the 870 blades and blade fragments at Holding, 11.3% are of Grimes Hill chert. A single blade core of Grimes Hill (here identified as a northern Illinois type) was also recovered.

I would be more comfortable to see pre-Hopewell demonstrated in a pre-50 B.C. context—say at least before the local Cement Hollow phase, which is regarded as the initial portion of the Middle Woodland Havana tradition. Yet at the Mund site, no blades were found in two deposits assigned to the Cement Hollow phase. The lithic sample was, it should be noted, very small and the absence of blades here may not be significant. But I do feel that demonstrating that blades and non-local cherts preceded the appearance of typical Hopewell traits may have to transcend Tewehafel or provide a tighter definition of "typical" Hopewell traits.

The final contribution, Chapter 13 by Diana Greenlee, is entitled "Prehistoric Diet in the Central Mississippi River Valley." It is actually a thorough discussion of stable carbon isotope analysis, the inferential evidence it provides for maize consumption, and the significance of this information in understanding late prehistoric cultural developments. As the author notes, stable carbon isotope ratios (13C/12C) have shown that the timing, degree, etc. at which maize was introduced into prehistoric subsistence systems varies from region to region and that dietary dependence was not a prerequisite for the appearance of either Hopewellian or Mississippian culture. "Thus, long-held commonsensical assumptions about the nature of prehistoric subsistence systems, dietary change, the relation between subsistence and settlement, and the relation between cultural elaboration and maize agriculture are all open to question" (p. 299).

Greenlee notes that bone chemistry remains our only potential source of direct information about prehistoric diet (here and elsewhere read prehistoric use of maize), and that care must be taken to distinguish between ante- and postmortem chemical changes in bone.

The history of stable carbon isotope ratios (hereafter SCIRs) and the single example of trace element analysis are discussed and ably evaluated. The first and largest study (19 individuals from 13 sites) led the authors to conclude that (1) populations did not consume significant amounts of maize until after "Emergent Mississippian" (i.e., until after ca. A.D. 1000); (2) following that date maize rapidly became a major dietary component; and (3) it so remained until historic times. A second study essentially confirmed the first, but, as the author notes, there is quite a bit of variation in the post-A.D. 1000 SCIRs.

There follows a long and excellent discussion of the problems in assessing the dietary record. Possible effects of preservation, "collagen" extraction techniques, differences in individual metabolism, behavioral variations, chronology of specimens, environment, settlement systems, and community composition (including the possibility of immigrants) are all discussed. Quite expectedly, as the author moves from specific effects of preservation conditions to such increasingly general (and poorly documented) things as settlement systems and community composition, her comments must also become increasingly general. For this we can hardly fault the author; she has called attention to problems we all must deal with.

The author next presents new data on 11 individuals, all from southeast Missouri. Four are from Campbell, 2 from Murphy, 1 from Pelt's, and 4 from Langdon. The author dates Langdon A.D. 1250–1550, but I believe Dunnell (Chapter 9) prefers a somewhat earlier date of A.D. 1100–1500 (p. 216). Her sample treatment procedures and regimen are described in detail, presumably to facilitate reliable replication of her work by other scholars.

Conclusions based on these new data are in line with other post-A.D. 1000 data, showing enriched 13C content, indicating significant maize consumption. She continues:

Although such a small sample precludes the establishment of any conclusive statements with regard to maize consumption by southeastern-Missouri Mississippian populations, three patterns merit further investigation. First, the data support the observation that [SCIRs] of the central Mississippi River Valley are, in general, less enriched and more variable than...in many other areas of the Eastern Woodlands. Second, significant interpopulational differences exist...Third, these data suggest that the environment may indeed have significantly influenced prehistoric diet (320–23).

The paper concludes with suggestions for future directions in dietary research. Better temporal control is required, with radiocarbon analyses of specimens tested for SCIRs. Although adults are typically chosen for study, there are reasons that argue for more attention to subadults. Efforts must be made to document the effects of environment. Finally, the effect of immigration (suspected at Campbell and Kersey) on intra- and interpopulational differences should be considered.
This addition to our very limited knowledge of SCIRs and maize consumption, and the clear manner in which it has been presented is more than welcome. Apparently the author will continue this project and provide us with increasing amounts of data. Presumably she will submit her material for radiocarbon analysis, in keeping with her desire for better temporal control. It might also be useful to obtain samples from sites of limited occupation spans; her largest sample comes from Langdon, a site occupied for approximately four centuries. Yet on her time scale, all of the Langdon specimens are treated (or necessity) as contemporaneous (A.D. 1300). This is certainly a good mid-range date, but finer controls may indicate such things as environmental change, etc., at this single site.

Finally, it must be observed that the author's opening comments regarding "long-held commonsensical assumptions about the nature of prehistoric subsistence systems, etc. are all open to question," were never addressed, nor could they have been, given the nature of her data. Perhaps it would have been more appropriate to put these in the "future directions" section at the end of her paper.

I have finally come to the end of what my readers (if any remain) must regard as an overly long and tedious review. In closing, I can only repeat my initial observation—that the contributions are quite uneven and geographically limited to the Missouri Bootheel and adjacent areas. The better contributions certainly outweigh those of more questionable merit; the former definitely will make the volume an important addition to the libraries of local scholars.

Charles H. McNutt is Professor Emeritus of Anthropology at the University of Memphis.


Reviewed by Timothy K. Perttula

Pre-Caddoan Cultures in the Trans-Mississippi South is an important monograph on the Archaic and Woodland cultures living in the modern-day Oak-

Hickory Forest, portions of the Oak-Hickory Forest/Prairie Transition, and Southwestern Evergreen Forest west of the Mississippi River and south of the Missouri River. Schambach dubs this area the "Trans-Mississippi South (TMS)," and he considers it a "culturally valid natural area," one set "apart from the Plains and the Lower Mississippi Valley to the west and east of it, and from the Missouri, Illinois, and Ohio valleys to the north" (p. 9).

The monograph is ostensibly a report on the 1939 Works Progress Administration (WPA) excavations at the Means and Cooper sites in the Ouachita River basin of southwestern Arkansas, originally written as a 1970 Ph.D. dissertation (Harvard University). In the unreviewed text, now published along with artifact figures and maps thanks to monies provided by the Dellinger Memorial Fund of the Arkansas Archaeological Society, Schambach employs rigorous artifact classifications of lithic and ceramic artifacts, along with analyses of available horizontal and vertical stratigraphic information from the extensive excavations at the two sites and wide-ranging comparisons to other sites in Arkansas, Missouri, Oklahoma, and Texas, to develop a sequence of cultures in the mid-Ouachita River basin and beyond. Because of these analyses, according to Schambach (p. xi), "the Cooper site could be considered one of the dozen or so most important multiple component Archaic sites in the Southeast." He goes on to state that the "Woodland period Fourche Maline components at Cooper and Means are collectively the most informative components of this culture excavated so far."

In perhaps the most interesting part of this Arkansas Archaeological Survey publication, Schambach adds a new introduction to the monograph to place his 1970 findings in a broader 1998 context. While debatable, badly, he concludes that "our knowledge of the Archaic and Woodland period cultures of the Trans-Mississippi South has not advanced significantly since 1969. Much of what I wrote then is still current and many of the concepts I formulated have yet to be tested" (p. xi).

The Cooper and Means sites are substantial midden deposits on knolls in the Ouachita River basin. The middens were upwards of 4 feet thick at Cooper, and 0.2–1 foot thick at Means, except for one area with more than 6 feet of overbank midden. WPA excavations recovered large quantities of lithic and stone artifacts in their broad excavations at the two sites (ca. 1100 m³ and 1700 m³), including 7,311 sherds, three vessels, clay balls, Poole pipes, and pottery discs from Cooper, and 10,469 sherds, 30 vessels, Poole
pipes, loop pipes, pottery discs, and "baked clay briquets" at Means; the latter resemble the fired-earthen bricks from Middle Archaic mound contexts in northern Louisiana (Saunders and Allen 1997). Schambach defines new plain wares (Cooper Boneware, Ouachita Plain, and Ouachita Ironware) from these collections, and provides an excellent description and discussion of the character of these wares, as well as the Williams Plain ceramics and small amounts of Marksville and Coles Creek affiliated sherds from the two sites. Post-A.D. 1200 Caddoan ceramics are also present, particularly at Means.

The lithic artifacts from Cooper and Means include projectile points (2,100 and 236, respectively) of a number of new and previously recognized types, varieties, and subgroupings, knives, drills, perforators, end scrapers, bifacial tools and blanks, unifacial flake and blade tools, polished and ground stone adzes and celts, boatstones from the Means site, double-bitted axes, and hammers and choppers. It is very informative to read Schambach's insightful discussion of the classificatory niceties of projectile point types, and he cuts and hacks his way through inadequate and apparently out-of-date typological formulations with a vengeance; in most cases his criticisms are solid, but not always. His dismissal of the differences between Darl and Pedernales points, for instance (p. 38) does not hold up based on more recent work in central Texas where the two different types are separated by more than 1000 years and occur in distinct stratigraphic contexts (see Collins 1995).

Schambach also presents the raw data on thickness and stem width of the contracting stem Gary dart point and varieties—by far the most common dart point form in most Trans-Mississippi South Archaic and Woodland sites, and crucial to estimating the temporal span of many sites in the absence of radiocarbon dates. This data, in combination with the available horizontal and vertical stratigraphic information, brought home to this reviewer just how weak the underpinning of our temporal and chronological assignments are in the Trans-Mississippi South, despite Schambach's painstaking analyses, because the vertical separation between varieties is dubious, and there is also considerable horizontal overlap between the different Gary varieties (see Figures 89 and 91 and Appendix A). Chronological assignments to Late Archaic and Woodland archeological components in the 30 years since then have proceeded as if the Gary varieties have distinct and well-demonstrated chronological differences, but the available data from the poorly stratified Cooper and Means sites are less than clearcut. Perhaps this is what Schambach meant when he said that "writing the archeology of a 30-year-old excavation is not altogether a satisfying experience" (p. 33).

Schambach discusses the shapes and sizes of the various tools, the raw materials used (primarily novaculite from the Ouachita Mountains), and regional comparisons from other Trans-Mississippi South sites with similar tools. Cooper also has a large bone and antler tool assemblage (n=223), dominated by splinter awls, utilized antler tines, and cut antler. The artifact descriptions of these various tool forms are well-done, with clear photographs. Overall, the ceramic, lithic, and bone artifacts serve as a valuable compendium of the material culture from Archaic and Woodland period contexts in the Trans-Mississippi South.

Features were uncommon, probably due to the relatively crude excavations employed by the WPA crews. There were 31 burials at Cooper (about 90% Archaic or Woodland in age), as well as a few postholes and two knife caches, while at Means the excavations uncovered a rectangular mid-Ouachita phase Caddoan structure, several pits and basins within the structure, and 13 Caddoan burials in the vicinity of the structure.

In Chapter 6, the monograph's concluding chapter, Schambach first discusses the cultural sequence at the Cooper and Means site by defining and reconstructing the different Archaic, Woodland, and Caddoan components and their diagnostic, probable, and possible artifact traits (see Tables 17 and 19), along with estimates of their age based on comparisons with other sites in the region; no radiocarbon dates have been obtained from Cooper and Means. Even in the 30 years since the Ph.D. dissertation was written, radiocarbon dates to tie down these Archaic and Woodland sequences in calibrated radiocarbon years are few and far between. In the Harvard tradition, Schambach then assigns phase names to each of the components, "even where it is impossible to point to related excavated ones, in the expectation that they will be found" (p. 109). The Lost Bayou, Oak Grove, and Duschman's Garden phases are grouped into a Fourche Maline Culture, which Schambach believes encompasses Woodland period groups living in "virtually all of the Trans-Mississippi South from the southern foothills of the Ozarks south to east Texas at least as far as the Sabine River" (p. 123). Not too surprisingly given the geographical distribution of Fourche Maline
components, Schambach concludes, and I believe rightly so, that the Fourche Maline Culture is ancestral to the post-A.D. 800/900 Caddoan cultures of the Trans-Mississippi South.

As a cornerstone of what is known and not known about Archaic and Woodland period cultures in the Trans-Mississippi South and the Caddo area, I strongly recommend Pre-Caddoan Cultures in the Trans-Mississippi South to professional and avocational archeologists interested in the region’s archeological record and Native American history. I hope that other archeologists will emulate Schambach’s ground-breaking work by taking on the analysis and publication of other early collections now reposing in facilities in Arkansas, Louisiana, Oklahoma, and Texas, and thus contribute to a new and broader understanding of Archaic, Woodland, and Caddoan cultural lifeways in the area.

Recently, several books have been published on the Caddo Indians of southwest Arkansas, eastern Oklahoma, northwest Louisiana, and eastern Texas that have provided valuable insights into the history and ethnography of the Caddo people, including Caddo Indians: Where We Come From (Carter 1995), The Caddo Indians: Tribes at the Convergence of Empires, 1542–1854 (Smith 1995), and The Caddos, The Wichitas, and the United States, 1846–1901 (Smith 1996). The publication of David La Vere’s book continues in this tradition with The Caddo Chiefdoms.

La Vere employs archival sources, oral historical information, and archeological evidence to examine both the development and elaboration of the Caddo chiefdoms (which he considers to be related to the Mississippian chiefdoms elsewhere in the Southeast) in prehistoric times, as well as the rise and decline in power of the Caddo chiefdoms after European contact. In considering the character and evolution of Caddoan societies following the colonization and settlement of the area by Spanish, French, and American groups, La Vere situates the introduction of epidemic diseases, the deerskin/horse/gun trade, Osage and Chickasaw slave raiding, and the different machinations of Europeans within an account of the political, social, economic, and religious forces that drove the activities and relationships of Caddoan chiefdoms, their important leaders, and their “magnificent history” (p. 9).

The core of the book consists of La Vere’s chapters on “The Horse, Gun, and Deerskin Trades”; “Challenges to the Chiefdoms,” in which he discusses the effects of European diseases on Caddoan polities, Caddo-European intermarriage, the effects of slave raiding, and the impact of mercantile capitalism on aboriginal economic strategies; and “Restructuring the Chiefdoms.” In them, he nicely describes how the political and economic traditions of the Caddo peoples “allowed them to take an active role in shaping European needs to their advantage” (p. 5). The Caddo chiefdoms did this through the development of kinship relationships and obligations, including what La Vere calls “fictive” kin relationships or kin relationships through ritual adoptions, that solidified gift exchange and trade between European leaders and Caddo chiefs.

In the book’s penultimate chapter, “The Chiefdoms Shatter,” La Vere reviews how the Caddo strategy of persuasion rather than force, kinship relationships and gift-giving, and alliance-building with Europeans and other Native American groups fared among the American settlers and government agents after the United States’ purchase of the province of Louisiana in 1803. The principal problem the Caddo chiefdoms faced “was the continual encroachment on their traditional lands by Americans” (p. 145). The American settlers, farmers, and merchants were interested in building relationships with the Caddo peoples only if they fostered ways for them to take the Caddo lands. They were not true relationships based on familial or kinship bonds, but empty ones: “The problem for the Caddos was not that they adopted strangers into their families. It was when strangers refused to become family. When family members did not uphold their obligations, things fell apart. And things did fall apart for the Caddos” (p. 152).

The book’s consideration of the Caddoan prehistoric archeological record is adequate in giving a sense of the power and nature of the ancient Caddo chiefdoms. These chiefdoms built earthen mounds for the religious and political elite in Caddoan societies, carried out long-distance trade in exotic items, lived in sedentary communities, hamlets, and villages, and depended upon the cultivation of maize and other tropical cultigens for their dietary needs.

Nevertheless, La Vere’s characterization of the Caddoan archeological record is fraught with misleading statements and/or interpretations that are not supported by the available archeological evidence on the prehistoric Caddo. For instance, La Vere concludes that the last mounds built by the
Caddo were in the fourteenth century, when in fact earthen mounds were built and used by the Caddo in the sixteenth and seventeenth centuries along the Red River basin and other major stream valleys in the Caddoan area. He also suggests that the cessation of mound building among the Caddo was the result of great droughts around A.D. 1350. The available paleoenvironmental data for the Caddoan area, albeit not particularly robust, provides no evidence of a drought at this time, nor an abandonment of the Spiro chiefdom then, as La Vere suggests. If anything, the warmest and driest years occurred in the fifteenth and sixteenth centuries, when very successful Caddoan agricultural communities flourished throughout much of the region occupied for several hundred years by Caddoan peoples. It is also not the case that by A.D. 700 Caddoan society depended upon maize for their survival, or that the development of a Caddoan “theocratic chiefdom” (p. 11) was fueled by a population explosion based on agriculture. Stable carbon isotope analyses of human remains, and the evidence from the flotation of feature deposits on numerous Caddoan archaeological sites, suggests instead that the Caddoan diet was not dependent upon maize until after ca. A.D. 1100, or that an intensification of maize production occurred much before A.D. 1300. There also is no archeological evidence to support the old saw trotted out by La Vere (pp. 27–28) that Mesoamerican “influences” or connections had anything to do with either the development of the Caddo chiefdoms, or with the spread of a Mississippian cultural tradition from the Caddoan area throughout the Southeast U.S.

Despite my difficulties with La Vere’s archeological interpretations of the ancient Caddo chiefdoms, I appreciate the soundness of the author’s basic premise that the prehistoric Caddo tradition of powerful chiefdoms contributed to the Caddo’s “sense of unity, destiny, and greatness” (p. 154) in historic times. Also compellingly laid out by La Vere is the apparent reliance of Caddoan communities in prehistoric and historic times on powerful chiefs, such as Tinhouen, Dehahuit, and Jose Maria in the eighteenth and nineteenth centuries, and the strength of reciprocal kinship obligations. The Caddo Chiefdoms is a fine addition to the “Indians of the Southeast” series published by the University of Nebraska Press, and a book that should be of considerable interest to historians, ethnologists, and archeologists alike interested in the history of the Caddo peoples.

Timothy K. Pertula is an archeologist with Archeological & Environmental Consultants in Austin, Texas.

References Cited

Carter, C. E.

Collins, M. B.

Saunders, J., and T. Allen

Smith, E. T.


Reviewed by Samuel O. Brookes

I jumped at a chance to review this book for several reasons. First, Joffre Coe's 1964 volume on his work in the Carolina Piedmont (including the first excavations at Hardaway) is considered a classic in Southeastern (and American) archaeology. Coe's work at Hardaway and other sites gave us what is sometimes referred to as the Coe axiom: that is, at any one period in time, only one style of projectile point was in use. Thus Coe is often credited with destroying what some refer to as the golf bag theory: an Indian carrying a huge bag full of different spears, one for deer, one for turkey, etc. Second, Randy Daniel is a friend of mine and we usually get together at SEAC to discuss Dalton and other early cultures. Finally, it was largely my reading of Coe in junior college that convinced me to become an archaeologist. Neither Joffre Coe nor Randy Daniel is to be blamed for that, however.
This work by Randy Daniel, actually a revised version of his dissertation, reports not only on Coe's 1948 and mid 1950s excavations, but on Ward's excavations in the 1970s.

Daniel divides the book into eight chapters. After the introduction, (chapter 1), he has a good chapter on the previous excavations at the site. This is important because Hardaway was at first thought to be just another upland site. However as other sites were excavated the significance of the site was noted by Coe. It is important to mention here that even though the site is an upland site, it is stratified. This is of great importance because many people believe that only alluvial sites have depositional sequences, while upland sites are eroded, and Hardaway proves that such is not always the case. Hardaway is not as nicely stratified as we would wish, but I will return to this point later. Chapter 2 gives a good account of prior excavations as well as the depositional sequence of the site.

Chapter 3 features an in-depth discussion of the raw materials present at the site. Daniel provides an excellent description of the geology of the region and its bearing on the stone available to prehistoric inhabitants. Skipping ahead to chapter 7, "Uwharrie Rhyolite and Early Archaic Settlement Range in the Carolina Piedmont," we find a thorough discussion of quarry sites in the area around Hardaway. Daniel is to be complemented for including this chapter, for it places the site in a larger context than is usually done. Daniel shows the distribution of raw materials and projectile point types for a major portion of both North and South Carolina. This chapter leads him to the conclusion that settlement mobility was not dictated by drainage basins, as has been suggested here in the Southeast, but rather was linked to stone outcrops, as Bill Gardner in Virginia and Al Goodyear in South Carolina have suggested. While I do not disagree with Daniel on this, I feel that the reliance on gravel in the Midsouth region (read lack of raw material that can be quarried) probably accounts for this difference in settlement strategies. It would be interesting to plot blade edge beveling here, as I would bet it would occur where settlement is defined by drainage basins, due to lack of suitable raw material (large blanks and preforms) and the need for highly curated artifacts.

Chapter 4, devoted to artifacts, is the longest in the book and rightfully so. Hardaway was chock full of artifacts. At this point the limited stratigraphy of the site comes into play. Coe's sequence remains essentially the same, but Daniel cannot specifically say that the side-notched Hardaway points are later than or the same age as the Hardaway Dalton. This is not the author's fault; as I stated earlier, the stratigraphy, while present, is not what we would like to see. It is satisfying to see the adze at the site (it was not described in Coe's earlier report), but Daniel is unable to place it with the Dalton component. While it resembles the Dalton adze described by Morse, it could be part of the later Kirk occupation. One example of a Welker knife was recovered, but it cannot be placed with a specific point type. I would bet that it is an artifact of the tail end of the Early Archaic (which happens to be the onset of the Hypsithermal), but I cannot say for sure. This is an excellent chapter and Daniel's coverage of the artifacts is quite thorough. I would like to see a few more photographs, and if they could not be actual size at least the scale could be consistent throughout. I realize I am picky about things of this nature, and while the photographs that are present in no way detract from the volume, a few more would certainly add to it.

The influence of Lewis Binford is very apparent in chapter 5, which deals with placing the lithic tools into functional and organizational types to determine site function. It is only proper that Binfordian theory should come into play here, for he is not only one of the foremost proponents of archaeological theory in America, but he was one of Joffie Coe's graduate students on the Hardaway site, along with a young fellow named Stanley South. This is my favorite chapter in the book. The categories supplied by Daniel are both logical and immediately applicable to data here in Mississippi. Here we are not held back by such obstacles as metavolcanic rock. At any rate curated and expedient technologies should be familiar to all who have read Jay Johnson's work here in the pages of Mississippi Archaeology. This chapter should be read by all who have an interest in learning about lithics and people. Those tools are just a means to learn about the people who made them, and here Daniel, with help from Binford, Johnson, and others, provides a recipe to do just that.

Chapter 6, "Intrasite Spatial Analysis" is a tad dry, but the data gleaned from this study suggest a base camp function for the site, which agrees with other data presented by the author. The final chapter on Early Archaic settlement is very good. While I feel Randy is basically correct in the area where he is working, a settlement system tied to stone quarry sites will not work in the Lower Mississippi Valley area where gravel deposits are the preferred
raw material source. If one can obtain Coe's 1964 volume and look at the size of the Hardaway Dalton points, one is struck by the huge size as compared to our Mississippi Dalton points. Recently at Camp Shelby, Larry Walters picked up a San Patrice point on a site. The point is identical to the Hardaway side notched type named by Coe. As I looked at it in the field and then later looked at the greatly reduced specimens in Randy's book, I realized that the specimen from Camp Shelby was less than half the size of the North Carolina specimens. Obviously different types of raw material cause this dichotomy, but still the size difference is astounding when one considers that the types are identical in form. The mental template is the same over a distance of 800 miles; the difference in size and sharpening is simply due to raw material differences. I am always interested in typology because this is not just happenstance, but is a product of the people who made the tools according to a tight mental template for a reason. When we can answer the question of why this particular template, we will understand even more about the inhabitants of the Hardaway site. These are minor points, however, and Randy Daniel has done an outstanding job of reevaluating the Hardaway site. Coe's 1964 report is a classic and so too is this volume.

Samuel O. Brookes is an archaeologist with the U.S. Forest Service, Jackson, Mississippi.


Reviewed by Douglas C. Sims

Archaeology often draws upon the theoretical, methodological, and technological advancements of other sciences to stimulate its development. A prominent example of this borrowing is the profession's increasing reliance on geographic information systems (GIS) to manage the voluminous amounts of data generated over the last thirty years of cultural resource management related work. Initially considered a geographer's tool, the applications for GIS have now spread to practically every profession using spatial data. A GIS is a computer application capable of assembling, storing, manipulating, and displaying geographically referenced information. A typical GIS may consist of a variety of data, such as hydrographic, topographic, geologic, soils, and transportation routes, provided that the elements of each layer or component possess known or fixed locations. Data queries of this information help users identify spatial relationships through a variety of graphic or tabular forms. Such a system can assist archaeologists not only in managing information, but in understanding distributional patterns among recorded data (e.g., sites, artifacts, materials, etc.).

Most GIS applications currently focus on demographics, emergency management, and natural resources, not on archaeological data. One, however, provides an extremely good model for archaeologists and preservationists alike. The Maryland Heritage Trust (MHT) is charged with numerous preservation-related activities, including "maintaining inventories that include prehistoric and historic archeological sites, buildings, districts, structures and objects of historic significance." MHT realized the need to manage and access this information more effectively, and therefore, with a grant from the National Endowment for the Humanities (NEH), developed a GIS. This GIS is available to the general public as a CD-ROM (Compact Disk–Read Only Memory) that can be accessed using a modestly configured PC (see specifications above).

MHT divides its CD-ROM into four main sections: project overview, NEH involvement, the actual GIS, and multiple demonstrations. The disk also includes two minor sections focusing on a glossary and project credits. The Project Overview and NEH sections are discussed together, followed by brief looks at the actual geographic information system and some of its capabilities as portrayed by the demonstrations. The result should provide users with an idea of what information the CD-ROM contains, as well as what it is capable of doing.

The Project Overview gives a concise summary of the origins of MHT's GIS. It takes the user from the GIS's conception to completion, as well as previews four demonstration projects using slides and other media that test and illustrate the utility of the system. Future research questions, possibly addressed through an upgraded GIS, are also proposed. If you like bells and whistles, this is your section. A word of warning, however: even the ardent harpsichord enthusiast will quickly tire of the cheesy Muzak played throughout the overview. I strongly suggest you turn up the stereo (or turn off your
PC's sound) while viewing this section. Also mentioned here is the NEH grant that allowed the digitization of the archaeological database containing archaeological site and cultural resource survey locations. The grant also assisted with the development of a search word index enabling more accurate database queries. More detailed information concerning the grant can be accessed at the NEH section.

While the Project Overview provides easy to understand highlights on the system’s capabilities, the GIS section provides insights on the system’s organization. The information presented here should help those interested in the technical aspects of a GIS, such as its development and contents, its limitations, and its potential as a future research tool. If you are not familiar with computer and GIS nomenclature, I strongly advise that you read the glossary section before venturing into this section.

The first step in developing the GIS required the collection of data in an electronic format. MHT digitized an entire set of 7.5’ USGS topographic quadrangle maps to serve as the base layer for the system. Upon establishing a base layer, MHT digitized the locational information on historic properties (buildings, districts, structures), archaeological sites, survey areas, National Register of Historic Places properties, and property easements. Metadata describing each recorded property (e.g., related data sets, type of data, type of source material, scale or accuracy, dates of source materials, methods of data collection, geographic area covered, datums, and conditions and restrictions) were also compiled. Although restrictions were deemed important and necessary for all data, it was critical to protect archaeological data. Like Mississippi, Maryland has an exemption clause in the state open records law allowing them to protect such sensitive information. The final developmental step required linking all the data layers together, thereby allowing researchers and managers alike to query information from all or selected layers of housed data.

While primary data, such as sites, property, and survey information, were loaded into the system, so were a variety of related data sets, such as SPOT Satellite Imagery, infrared digital and black and white orthophoto quads, wetlands areas, historic shorelines, and a property parcel database, enabling the user to conduct further examinations of the area of interest. Although the GIS is in its early stages, plans for additional growth are well underway. For example, MHT is currently creating a database for all recorded standing structures, as well as focusing on concerns about data distribution/confidentiality policies (i.e. archaeological sites), data maintenance, updating and improvement of the GIS. As one can see, a GIS requires a lot of work and long term commitments of time, money, and manpower to ensure its continued success.

The demonstration section reviews four projects created to test the capabilities and accuracies of the GIS. The projects include a fluted point/Paleoindian sites survey, regional surveys of artifacts, shell midden distribution, and historic map manipulation. Each demonstration is chock-full of graphics and text illustrating the utility and success of the system. For example, the Paleoindian site, shell midden site, and artifact surveys employ base maps with numerous data overlays emphasizing site and material distributions over time. Such queried data provides graphic representations of distributions, thereby allowing for quicker and easier spatial and temporal analyses.

The project with the best visual result, however, involved the digitization of several eighteenth-, nineteenth-, and twentieth-century maps denoting the presence of roads, buildings, and geographic features. Each digitized map was georeferenced with modern maps (e.g., 7.5’ USGS topos and orthophotoquads) to ensure an accurate match. The overlaying of modern maps with historic ones revealed the locations of many non-extant structures and geographic features long since gone or altered. Field verification at some of these sites proved that map manipulation was an accurate survey technique capable of helping locate and document everything from old house sites and forts to changes in the natural environment.

Continued residential, commercial, and industrial development will result in data generated by cultural resources management studies. This information will need to be housed in a system that effectively manages it, as well as provides accessibility to the researcher. The multifunctional capabilities of GIS, from displaying artifact distribution patterns to helping create predictive models, will not only assist academic and contact archaeologists in carrying out research and serving the client better, but also in managing the resource more efficiently. While geographic information systems should not be considered a panacea, they are useful management and research tools, and for the time being it appears that GIS is the best and most efficient way to accomplish these tasks. MHT’s CD-ROM, likewise, is the best tool cur-
recently available to explain the benefits of such a system to archaeologists and others concerned with cultural resource management and preservation. For more information about the CD-ROM and MHT, access their website at: http://ari.ari.net/mdshpo/xorstr.html, or simply use a search engine and type in Maryland Historical Trust.

Douglas C. Sims is an archaeologist with the Historic Preservation Division of the Mississippi Department of Archives and History.


Reviewed by Jeffrey M. Mitchem

Hernando de Soto was not a nice man. Although this would seem to be common knowledge, those of us primarily concerned with the archaeological remains of his activities in North America tend to push this fact into the back of our minds. Duncan's book decisively brings it back to the forefront in this balanced and eloquent work.

A paperbound reprint of the 1995 Crown Publishers hardback, this book is arguably the most thorough and unbiased biography of Hernando de Soto to date. While several previous works have presented biographical details about his life, the most widely read were written with the aim of glorifying the man and omitted mention of his more despicable exploits. In contrast, Duncan unflinchingly relates events and details that can be gleaned from historical documents, narratives, and other sources, then augments this with archaeological information and other recent research on the 1539–1543 expedition to North America. The result is more than just a biography. It is really three books in one: a succinct description of those episodes of the early sixteenth-century Spanish Conquest in which Soto was involved, a detailed consideration of his North American expedition, and a chronological biography of Hernando de Soto.

Although little is known of Soto's childhood (there is even disagreement about what year he was born), it appears that he headed for the New World at about age 14. Within a few years he had gained a reputation as a skilled horseman, an effective cavalry officer, and a cruel persecutor of the native people. He further developed these talents in expeditions in Panama, Colombia, Nicaragua, Honduras, El Salvador, and Peru. He also became quite wealthy, both from his share of booty (especially Inca gold from Peru) and from his participation in the (officially illegal) trade in Indian slaves.

The first 200 pages of Duncan's book recount Soto's activities in Central and South America until 1535, when he returned to Spain a wealthy and famous man. Although the details of these exploits are complicated, Duncan's well-written narrative makes for fascinating reading, while at the same time painting a clear picture of the ambitious Spaniard. The result is not very flattering. During these early decades of conquest, there were various intrigues and rivalries among the Spanish officers and soldiers, and Soto was intimately involved in some of these scandals. At least part of his wealth was amassed by deceit and betrayal of his superiors. Duncan's documented examples also demonstrate that any recorded cruelty to the natives during his North American expedition pales in comparison to the atrocities committed against the Central and South American native people by Soto and the soldiers under his command.

The second half of the book recounts his expedition to La Florida. Much of this will be familiar to those who have kept up with the expedition literature that has been published in the last decade or so. After describing Soto's activities as Governor of Cuba in 1538–1539, Duncan gives a detailed chronological account of the trek through Florida and the Southeast. Using information from the expedition narratives and archaeological research, he provides a very readable account. Duncan enlivens the narrative by including his impressions and vivid descriptions of some of the areas through which the expedition passed, many of which he visited during research for this book. As usual, many readers and scholars will quibble and complain about details, use/misuse of primary and secondary sources, and interpretation of archaeological evidence. This is now par for the course. To Duncan's credit, he readily admits that his account is "at best an informed conjecture" (p. 245). He also explains that he uses the Hudson route for most of his narrative. It should be noted, however, that he often digresses to discuss competing views and debates among scholars about particular sites or portions of the route.

Duncan relates the movements and events associated with the expedition as it traveled, ending with Soto's death in 1542. The text is followed by
94 pages of footnotes, providing not only citations in support of various statements, but also discussing archaeological and other evidence for the choice of locations. Duncan also critically examines statements from narratives and historical accounts, as well as discussing alternative possibilities.

This book was not written as a scholarly treatise, but as a popular account of Hernando de Soto's life and activities in the New World. It does include enough citations and references for interested students to search out more information, however. A glossary and chronicle summary of Soto's life will also be of help to the general reader. I noted a few errors in spelling of names and titles of some publications, but otherwise it is an impressive work. I do want to state for the record, however, that Nueva Cadiz beads were definitely not manufactured in the settlement of that name in Venezuela (as stated on pages 258 and 497).

David Duncan has written a readable work that is hard to put down. It will serve as a good source for people who want to learn about Hernando de Soto and his exploration of what is now the southern United States.

*Jeffrey M. Mitchein is an Associate Archeologist with the Arkasmas Archeological Survey.*


Reviewed by Mary Evelyn Starr

Based on a 1993 SAA symposium, the eight chapters of *Archaeology of the Moundville Chiefdom* detail very important changes in the interpretation of this major ceremonial center, which has seen extensive investigation since the days of C. B. Moore (whose Moundville reports have just been reprinted). This accessible volume suggests many problems that still need to be addressed, despite being one of the best-documented Mississippian sites. Throughout, there is an air of constant testing and refinement of hypotheses, but there is not a lot of controversy evident. With the exception of the chapters on biology, the contributors are in agreement with each other and refer to each other for support. The contents are, briefly:

1. "A New History of Moundville," about 23 pages of text by the editors. This chapter is a concise summarization of newly proposed stages: intensification of local production in the old West Jefferson phase, initial centralization around A.D. 1050-1200, regional consolidation, entrenched paramountcy around A.D. 1300-1450, and collapse and reorganization in the former Alabama River phase.

2. "Population Trends at Moundville," some 15 pages by Steponaitis, who argues that comparison of grave-lot seriation with midden ceramics requires significant revision of population estimates. The maximum population in the Moundville I phase was perhaps only about 1000 people; during the Moundville II and III use as a regional cemetery there were probably only a few hundred residents.

3. "Moundville as a Diagnostic Ceremonial Center," by Knight. At about 15 pages, this chapter, my favorite, seemed too short. Pure speculation, backed with nineteenth-century Chickasaw ethnology, provides evidence for moiety, clan ranking, ancestor cults, a paramount class, and elite management of esoteric knowledge. The site plan is presented as a sociogram of the elite worldview, and this monumental device "for stabilizing societal relationships over generations" (p. 60) is characterized as the massive embodiment of this worldview in terms of space and mass.

4. "Domestic Life on the Northwest Riverbank at Moundville," by C. M. Scarry, consists of about thirty pages of detailed descriptions of the mundane world of the palisaded, compactly-settled Moundville I settlement. The possibility of the production of primitive wealth items from Potts Ferry formation fine sandstone, Appalachian muscovite mica, and Hillabee formation "greenstone" schist remains possible but is far from demonstrated.

5. "Of Time and the River: Perspectives on Health during the Moundville Chiefdom," is about fifteen pages of text by M. L. Powell. After analysis of 144 of some 3200 folks dug up at Moundville, she finds that sex and class distinctions are not significant in terms of stature, health, diet, or pathology. Tuberculosis and treponemal syndromes are evident. The general good health of the people buried at Moundville is attributed to lack of enforced class distinctions and to a dispersed settlement system in phases II and III.

6. "Human Subsistence at Moundville: The Stable-Isotope Data," around eleven pages by M. J. Schoeninger and M. R. Schurr, has excellent discus-
sions of the physics methods and of the formulae used in computations of diet. Apparently "C plants (corn) provided 60%-80% of the calories consumed, and fish some 25% of the protein (pp. 127-28) until the protohistoric lessening of emphasis on farming.

7. "Outlying Sites Within the Moundville Chiefdom," some 27 pages of detailed description of secondary centers in the Black Warrior valley by P. P. Welch, shows the pressing problem of continuing site degradation. Several farmsteads that were not even recognized until the 1970s (p. 138) have been tested, but so much remains to be done!

8. "The Oliver Site and Early Moundville I Phase Economic Organization," eleven pages by L. M. Michaels, compares an early farmstead with a secondary center, showing little status differentiation but the potential that the use of nonlocal stone resources may have already begun to be centralized.

Peebles's foreword emphasizes the long-term teamwork that has contributed to this exceptional project in American archaeology. While presenting interpretations different from those first advanced nearly thirty years ago, the main point seems to be that "only a fraction of the collections generated by this fieldwork have ever been thoroughly analyzed..." (p. 2). While we have had the site plan since Moore's day, until recently it was not shown that it represents essentially one episode of planning. Without this knowledge, Knight's style of analysis of symbolism was not possible. The production of the earthworks was undoubtedly the outcome of careful planning, but soon after construction, the participants from the south end of the site—furthest from the river and the axial mound—opted out. We still don't know why this particular West Jefferson phase population concentration became a major center, nor why the center became a necropolis. Nor do we understand what the palisades—surely a wonder of the Mississippian world—were all about. Were they sacred enclosures, fortifications, or simply disastrous environmental mismanagement? Questions abound.

Other than plans and graphs, there are few illustrations, particularly of artifacts, and mercifully little discussion of ceramics. Except for Scarry's and Welch's chapters, there is little direct citation of radiocarbon dating to support contentions of the authors' having taken a more diachronic view. Overall, *Archaeology of the Moundville Chiefdom* is well integrated and well indexed, with many citations of SEAC papers and CRM reports. The book seems rather brief for the price, and is printed on low-quality paper, but then, as the editors note, "Our bet is that it will not be long before additional resynthesis is necessary" (p. xix). This is an essential volume for anyone who is undertaking research on the Moundville chiefdom to contribute to the next "resynthesis."

*Mary Evelyn Starr is a Station Assistant with the Arkansas Archeological Survey.*
MISSISSIPPI ARCHAEOLOGICAL ASSOCIATION

1999 Officers:

President: Terry McClung, Rural Route 1, Box 187, Troy, AL 36079

Northern Vice President: Mary Evelyn Starr, P.O. Box 39, Sledge, MS 38670-0039

Southern Vice President: Amy Young, Department of Sociology and Anthropology, University of Southern Mississippi, SS Box 5074, Hattiesburg, MS 39404-5074

Secretary-Treasurer: Patricia Galloway, Mississippi Department of Archives and History, PO. Box 571, Jackson, MS 39205-0571

The Mississippi Archaeological Association is an organization of professional archaeologists and lay people actively involved with archaeology and archaeological preservation, uniting in a common effort to understand the prehistory and history of Mississippi and the surrounding region. Anyone who has a sincere interest in the cultural heritage of the state and who can and will dedicate himself to the preservation and protection of that heritage for all to enjoy is eligible for membership. The Association has as one of its important objectives the mission of encouraging scientific archaeological investigations and supports the dissemination of information from these investigations in its publications, which are received by its members as a benefit of membership. 1999 dues for individuals are $15.00; families, $18.00; students, $10.00; institutions, $20.00. Individual life membership is $200.00.