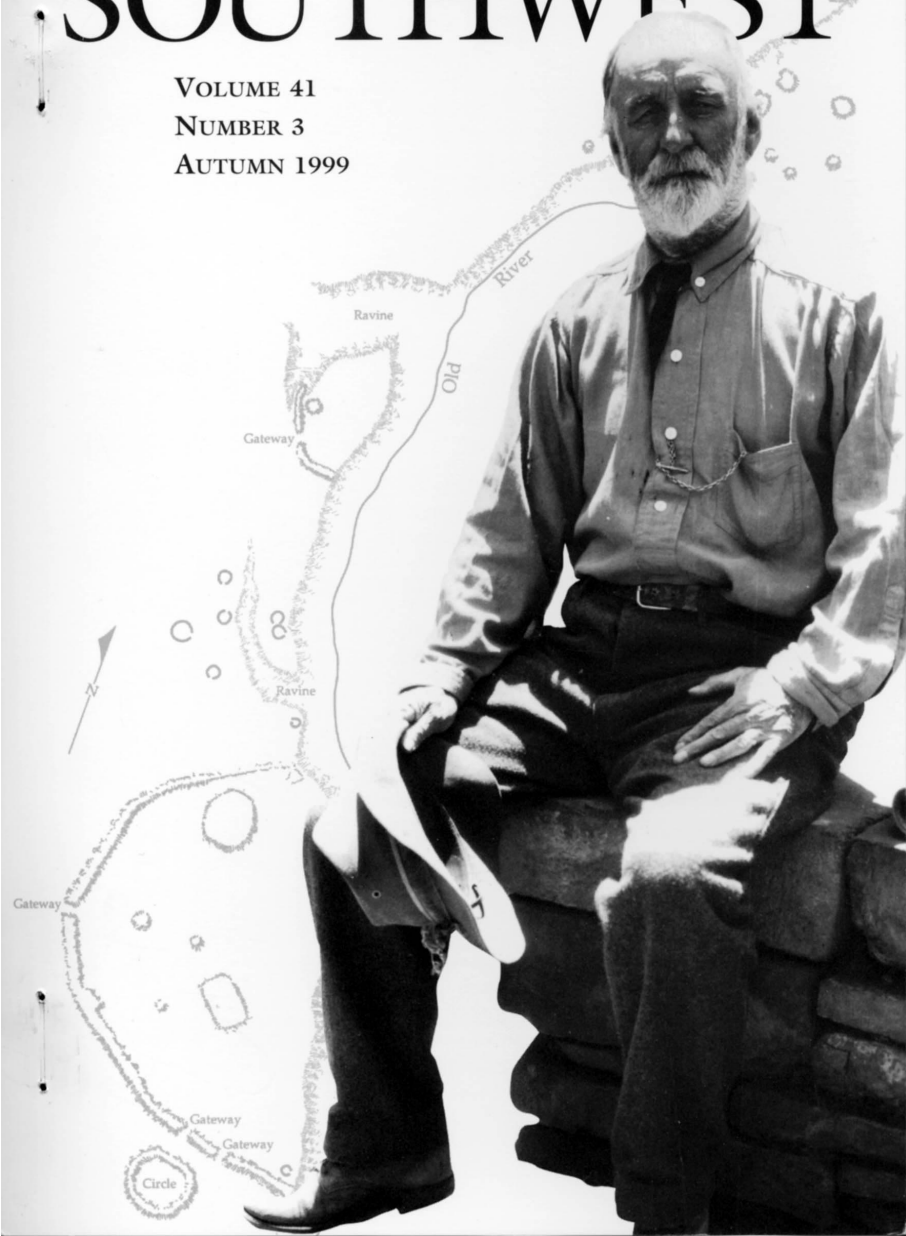


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# *The Bureau of American Ethnology and Its Legacy to Southeastern Archaeology*

MICHAEL J. O'BRIEN and R. LEE LYMAN

The Bureau of American Ethnology (BAE) enjoys a unique position in the archaeology of the Southeastern United States. Some of the more important figures connected with the bureau—Henry B. Collins, Winslow Walker, Gordon R. Willey, J. Walter Fewkes, and Matthew W. Stirling, three of whom were bureau chiefs—conducted research in the Southeast that laid a foundation for much of our modern understanding of the prehistory of the region. One can add to that list the myriad archaeologists whose work was sponsored officially or otherwise by the Bureau and whose findings were published through its outlets. In the 1920s, the Bureau, which had long been connected with archaeology and ethnology in the American Southwest, began to turn its attention to the Southeast. The latter region was poorly known archaeologically relative to the Southwest, though intermittent attention from northern institutions, including on occasion the Bureau, had documented an exceedingly rich record of prehistoric and ethnohistorical occupation (see Stoltzman [1973] for an overview).

Detailing the extensive role played by the BAE in Southeastern archaeology is well beyond the scope of this paper. Instead, we focus on the work of the Bureau during more or less the first five decades of its existence—the period from its founding in 1879 to December 1932, when Bureau personnel participated in the Conference on Southern Pre-History, the second of three regional archaeological conferences organized by the National Research Council's Committee on State Archaeological Surveys for the purpose of bringing some degree of order to the way in which prehistory was pursued in the eastern United States. That meeting, which became known as the Birmingham Conference, signaled an important turning point in the archaeology of the Southeast. Professional anthropologists and archaeologists from north-

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ern institutions, including the BAE and the U.S. National Museum, laid out a research agenda that both professionals and nonprofessionals, the latter of whom had dominated prehistoric studies in the Southeast (Fagette 1996: 13–16), could follow.

A key item on that agenda was developing a chronological ordering of archaeological remains—no mean feat, it was thought, in an area that lacked standing prehistoric architecture, perishable artifacts, and thick, stratified deposits. Chronological control was viewed as being so central to the pursuit of archaeology in the Southeast that all other studies were held to be of secondary importance. In the Southwest, where there had been concerted archaeological effort for well more than two decades, chronology was almost a thing of the past. To be sure, new pieces were constantly being hung on the chronological framework, but by 1932 Southwesternists were satisfied that the ordering was more or less complete (e.g., Kidder 1924, 1927), and they began to pursue other aspects of prehistory (Lyman, O'Brien, and Dunnell 1997a, 1997b). As their knowledge of the prehistoric Southwest grew, prehistorians began to look increasingly to the East for comparative data, but all they saw were personnel from local museums and amateur societies out collecting artifacts. Collins, at the time an assistant curator in the ethnology division of the National Museum, summed up the situation well in his Birmingham Conference paper. He was speaking specifically of one state, but his remarks were applicable to the Southeast as a whole: "Although Mississippi is rich in aboriginal remains and a considerable number of these have been investigated, it cannot be said that the work has clarified to any great extent the archaeological problems involved. The early investigators, in accordance with the unfortunate tendency of the time, too often proceeded on the assumption that the accumulation of specimens was an end in itself rather than a means toward the elucidation of archaeological problems" (Collins 1932b: 38).

Thus, the most important topic at the Birmingham Conference in 1932 was time. The key question concerned the temporal and evolutionary relations of various archaeological manifestations termed "cultures." By the 1920s, it was clear that there was tremendous variation in the archaeological record of the Southeast, and it was equally clear that some of that variation must be due to temporal difference (Stoltman 1973: 135). Time had not always been an important consideration for Bureau personnel. John Wesley Powell, for example, was utterly unconcerned with archaeological time, as were the ethnologists he hired

to staff the Bureau, a result of the perception that the past did not differ fundamentally from the present. Prehistorians working under Powell were interested in time depth only if it was manifest in the archaeological record as difference on the order of that between the European Paleolithic and Neolithic periods (see Meltzer [1985] for a discussion of this issue). Ironically, the Bureau's greatest contribution to the Southeast—its legacy—was in outlining a method for measuring time and writing culture history. That legacy, which became known in the late 1930s as the direct historical approach (Wedel 1938), had been around since the Bureau's founding and had become part of its stock in trade, but it reached its greatest expression in the Southeast as a result of the landmark work of one of the Bureau's most accomplished ethnologists, John R. Swanton. To understand the roots of that legacy, we need to examine the beginning of the BAE's involvement in the Southeast.

#### EARTHEN MOUNDS AND THE QUESTION OF AUTHORSHIP

The BAE's involvement in the Southeast dates to the formation of the Division of Mound Exploration within the Bureau in 1881 and the mandate that Powell received from Congress: Answer decisively the question of which group or groups constructed the thousands of earthen mounds so evident across the eastern United States. By the time he was appointed to head the division, Thomas was convinced of the equation of the mound builders with the American Indians. For example, in 1884 he asked and then answered the question, "Who were the mound-builders? We answer unhesitatingly, Indians—the ancestors of some, perhaps of several of the tribes of modern or historic times" (Thomas 1884: 90). Thomas published the "Report on the Mound Explorations of the Bureau of Ethnology" in the *Twelfth Annual Report of the Bureau of Ethnology, 1890–1891* (Thomas 1894), and in it he discussed in great detail the mound explorations carried out by members of his crews as they worked their way over two dozen eastern states, including all the southern states with the exception of Texas and Virginia (figure 1).

Jennings (1974: 39) stated that the publication of Thomas' report can be thought of as "marking the birth of modern American archeology," though we consider this to be a bit of an overstatement, as there were many elements that had to crystallize before archaeology was

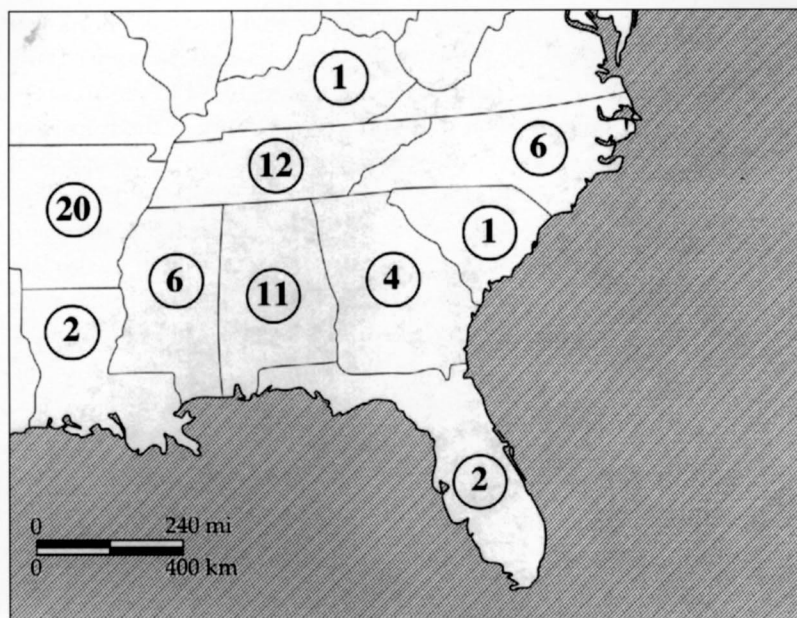


Figure 1. Southern states examined by the Division of Mound Exploration. Circled numbers represent the number of counties examined in each state.

practiced as it was in, say, the 1950s or even the 1920s (Lyman, O'Brien, and Dunnell 1997a). This does not mean that Thomas' report did not contribute an important part of that crystallization process, for it surely did. On the one hand, prior to the founding of the Division of Mound Exploration, archaeology was primarily an antiquarian activity, meaning that interest was placed on artifacts and earthen monuments themselves rather than on using such things as a means to other ends. The work summarized by Thomas (1894), on the other hand, had been done to solve a particular problem and demanded rigor in how materials and information were gathered. But, as Meltzer (1983: 39) notes, within the Powell-led BAE, past and present were not perceived as being qualitatively distinct. This was, in rough form, a warrant for using the direct historical approach, as it denoted a direct and historical connection between the visible historical period and the invisible prehistoric period.

Thomas thus referred continually to historical records of the sixteenth through eighteenth centuries, where it was documented that

the post-Columbian Indians were sufficiently culturally advanced (being sedentary agriculturalists) to have built the mounds and in some cases had actually been observed building mounds. Documenting typological similarity of artifacts from the historical and prehistoric periods merely completed the evolutionary, ethnic, and cultural linkages on which the direct historical approach was founded. To make his case that the mound builders and the American Indians were of the same historical lineage, Thomas developed a final list of eleven “theses” that were set forth in the final report, up from an earlier eight that had appeared in the *Fifth Annual Report of the Bureau of Ethnology* (Thomas 1887a) and down from the thirteen theses that had appeared in the fourth *Bureau of Ethnology Bulletin* (Thomas 1887b).

It took him 730 pages, but Thomas effectively laid to rest most of the major speculations regarding the nature of the earthworks and the groups responsible for erecting them. He claimed the evidence was there if anyone but examined it critically, especially the fact that earthworks with European items in them were similar to those from earlier periods. And importantly, Thomas noted there was no logical reason to suspect that the mound builders were of Mexican origin or that Indian groups had pushed the mound builders south. In other words, the archaeological record demonstrated to Thomas’ satisfaction that a high degree of cultural continuity had existed for an untold number of millennia and that such threads of continuity showed no major disruptions. Change, to be sure, had occurred—this much was indicated in the myriad forms of earthworks recorded and the different kinds of artifacts found within them—but this type of change was an orderly, continuous progression as opposed to a punctuated, disruptive progression of cultural epochs such as was evident in the European Paleolithic-Neolithic sequence. To Thomas, continuity had ruled throughout human tenure in the East, and it is clear that he favored tribal differences to explain the immense variation evident in the archaeological record.

As Smith (1990: 29–30) points out, Thomas’ field research was geared toward establishing a taxonomy of mound types, determining the geographical range of the various types, and studying the mode of construction and methods of burial in the ordinary conical tumuli (figure 2). Not surprisingly, Thomas did not describe the great majority of artifacts recovered from the mounds of the Mississippi Valley; some of this void was filled by his contemporary William Henry Holmes, who

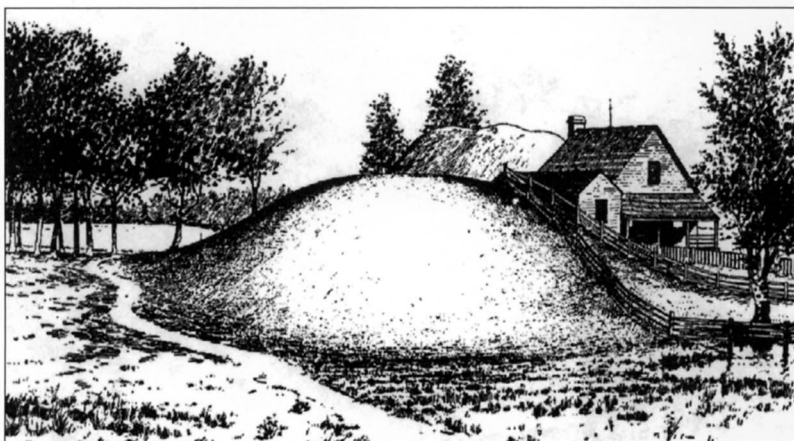
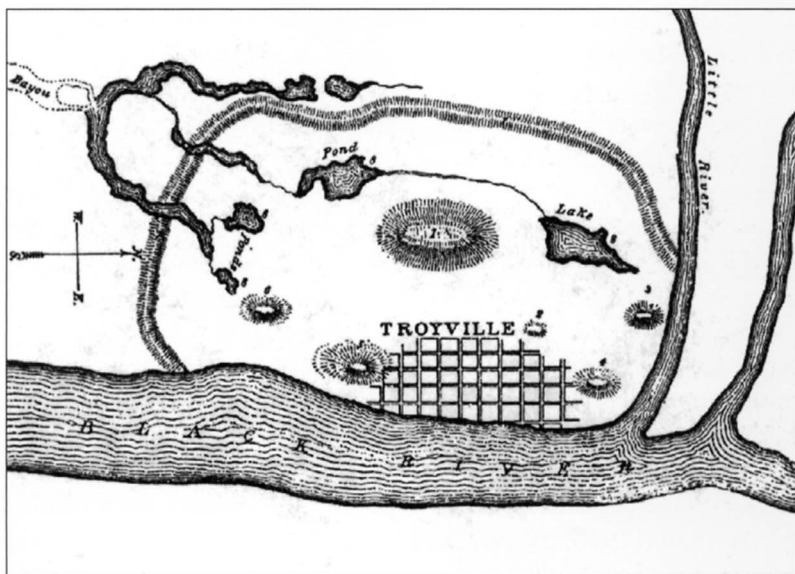


Figure 2. Sketch (top) and plan map (bottom) of the Troyville site, Catahoula Parish, Louisiana, made by a Division of Mound Exploration field party, 1880s (from Thomas 1894).



between 1884 and 1903 produced three treatises on pottery of the eastern United States (Holmes 1884, 1886, 1903). In his “Ancient Pottery of the Mississippi Valley” (1886), Holmes identified the key elements that one day would form the basis for constructing typologies of some of the pottery from the central Mississippi River valley.

THE SOUTHEAST IN THE  
EARLY TWENTIETH CENTURY

With the death of the mound-builder myth in the closing decade of the nineteenth century, Bureau archaeologists turned their full attention to other matters, some of which had been of considerable concern to them for some time. The one that has received the lion's share of attention from historians of archaeology (e.g., Meltzer 1983, 1985) was the great debate over the antiquity of humans in North America. Southeast prehistorians, with rare exceptions, did not figure into this debate, but they nonetheless were active, and their activities did not go unnoticed. Through time, both the BAE and the National Museum began turning their attention to the Southeast, as their interest became piqued by what local prehistorians were uncovering.

*Clarence B. Moore and Southern Waterways*

In contrast to Cyrus Thomas' hurried work in the Southeast, another Easterner, Clarence B. Moore, spent a quarter of a century, from roughly 1892 to 1917, exploring mounds along the major waterways of the southern states, in the process excavating several thousand skeletons and recovering countless ceramic vessels and other artifacts. Although his work was not sponsored by a federal agency, it was important for establishing a background for further research by Bureau archaeologists who came after him. He underwrote not only the costs of his projects but also the expenses of producing twenty reports dealing with the excavations, which appeared in the *Journal of the Academy of Natural Sciences of Philadelphia*. The reports are rather sketchy, but the accompanying field photographs and artifact illustrations are excellent.

Moore's work (e.g., 1905, 1908, 1909, 1910, 1911, 1912, 1913), and especially the artifacts it produced, spurred a resurgence of interest in the Southeast, especially by small state organizations and regional museums—precisely the groups at which the Birmingham Conference was aimed. In the 1920s, federal archaeologists became alarmed at the amount of organized “pothunting” that was going on in the Southeast, and they began to turn their attention to the problem. Pothunting, whether by museums or individuals, was not a recent phenomenon in the Southeast in the 1920s but rather an activity that had been well

documented for decades. In writing about commercially motivated looting along the St. Francis River in northeastern Arkansas, Moore charged that “the territory through which the river passes has been for years the headquarters for collectors and for persons wishing to make a livelihood or to increase their means by the sale of Indian pottery, and these individuals have worked for long periods and with indefatigable zeal” (Moore 1910: 259). The extent of such activities in the late nineteenth and early twentieth centuries reached far beyond northeastern Arkansas, as Thomas (1894: 183) had noted in his final report on the activities of the Division of Mound Exploration. Given these accounts, it is a wonder that anything was left of the archaeological record of the central Mississippi Valley by the beginning of the twentieth century.

### *The Committee on State Archaeological Surveys*

The National Research Council (NRC), through its Division of Anthropology and Psychology, which had been created in 1919, took up the effort against the destruction of archaeological resources. Two years later, that division formed the Committee on State Archaeological Surveys and named Roland B. Dixon of Harvard University as the first chairman. He was succeeded later that year by Clark Wissler of the American Museum of Natural History, who was followed by A. V. Kidder of Phillips Academy (Andover, Massachusetts) in 1924 and Carl Guthe of the University of Michigan in 1927. Early on, the committee realized that general knowledge of the Southeast lagged well behind that of the Southwest—a situation attributable to the large number of archaeologists who had been attracted to the latter region because of the architecture and pottery that occurred there and the demonstration by Kidder, Nels C. Nelson, A. L. Kroeber, and Leslie Spier that one could build cultural chronologies using those materials (Lyman, O’Brien, and Dunnell 1997a, 1997b). The committee also recognized that to channel the resurgence of interest in the Southeast that accompanied Moore’s discoveries—that is, to channel it in a way that was productive—would require the presence of professional archaeologists who knew the methods requisite to building chronologies and who would make long-term commitments to work in the region. Further, the committee also recognized that any professional archaeologist who made such a commitment had to be able to cross state boundaries, since, as Lyon (1996: 17) points out, “archaeological problems did not fit neatly within state boundaries.”

The lack of standardization in archaeological practice—in short, the lack of methodological sophistication—was of some concern to the committee, as expressed by Guthe (1930) in his report to the NRC on the committee's activities. He noted that developing a “standardization of technique, as has occurred in the Pueblo region, will lead to a greater mutual understanding of problems and to a wholesale cooperation between students” (Guthe 1930: 57). That standardization was founded on the direct historical approach (e.g., Kidder 1916; Spier 1917) and would be provided to Southeasternists in 1932 by archaeologists associated with the BAE and the National Museum.

### *Gerard B. Fowke and the Marksville Site*

Some of those archaeologists would leave a lasting methodological impression on the Southeast, but others would not. This does not imply that they had no impact on the emerging picture of Southeastern prehistory, but the work they conducted often was not particularly careful, and it certainly was not carried out as part of a long-term research program. Rather, justification for fieldwork was provided by the suspected presence of archaeological loot. Such efforts, of course, might be justifiable in instances where archaeological remains were threatened with destruction, but oftentimes the cure was worse than the disease. Perhaps the best example of this search-and-seizure mentality is found in the work of Gerard B. Fowke—a peripatetic journeyman associated with the BAE who had worked for Cyrus Thomas in 1887, just as the mound-exploration project was winding down. Fowke worked over broad areas of the Midwest and Southeast (Fowke 1894, 1896, 1905, 1910, 1912, 1922, 1927, 1928), sometimes on his own, other times as a hired hand, and although his methods left much to be desired (O'Brien 1996), some of his observations were useful to later investigators.

The centerpiece of Fowke's work in the Southeast was his excavations at the Marksville site in Avoyelles Parish, Louisiana. Marksville was impressive, containing at least twenty mounds and three linear earthworks spread out in the bottomland and on the bluff along Old River, a former channel of the Red River (figure 3). In 1926, as part of his survey of the Red River valley, Fowke spent three months excavating mounds in the Marksville vicinity and constructing a map of the earthworks (figure 4). Fowke knew what he was looking for in his investigations at Marksville. He bypassed several flat-topped mounds,

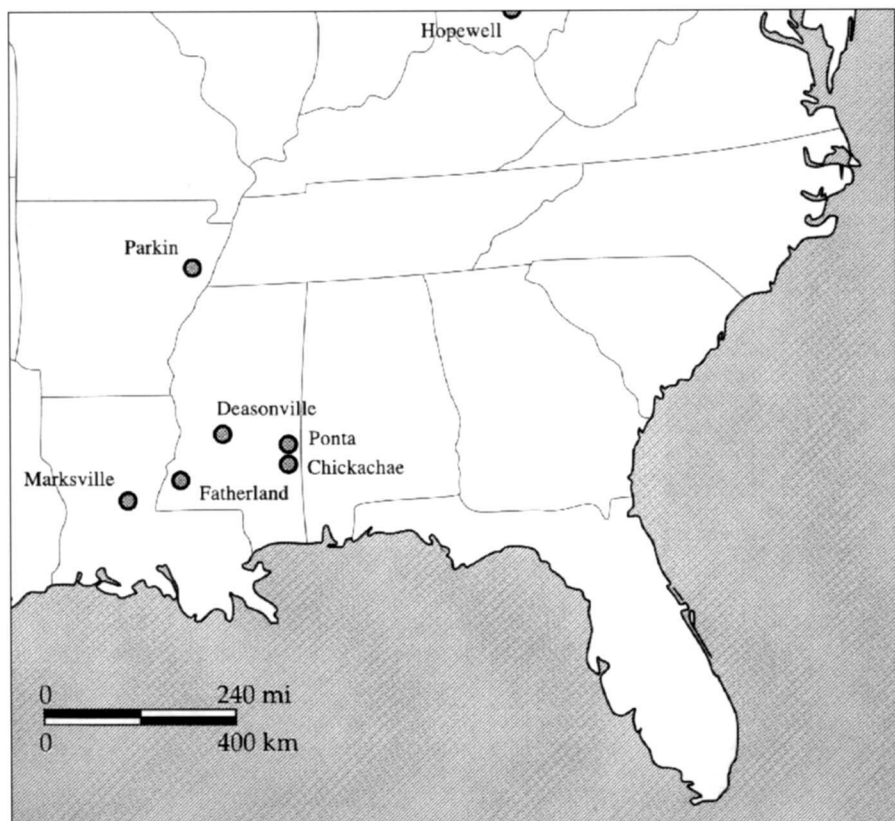


Figure 3. Locations of archaeological sites mentioned in the text.

which, aside from the enclosures, were the most prominent features at the site, and concentrated on several conical mounds, which he assumed contained burials. Of much more interest than human remains were the pottery vessels that came from mounds 4 and 8. They bore designs that were strikingly similar to those on vessels from the Scioto, Muskingam, and neighboring drainages of southern Ohio, a region that by 1933 was widely regarded as the heartland of so-called Hopewell culture.

Journeyman excavators such as Fowke were on one side of the federal-archaeology house in the 1920s. They were holdovers from the earlier days when the policy, especially in the National Museum, was to haul in as many artifacts as possible to stock the shelves of the museum.



purchase or discover artifacts for the National Museum (Hinsley 1981: 236). On the other side of the house were archaeologists and ethnologists who still fit the mold of Powell's original Bureau—careful field workers who viewed their mission as reporters to the American people on Indian affairs.

*Henry B. Collins and the Archaeology of the Mississippi Valley*

One such field worker was Henry B. Collins (figure 5), an Alabaman who had graduated from Millsaps College in Mississippi and who had undertaken extensive fieldwork in the Arctic and was fast establishing himself as one of the leading authorities on the prehistory of that region. In 1925, he was invited by the director of the Mississippi Department of Archives and History (MDAH), Dunbar Rowland, to carry out archaeological work in the Choctaw region of western Mississippi. Later that year, with the assistance of H. H. Knoblock of MDAH, Collins began looking for archaeological sites in east-central Mississippi and initiated an anthropometric study of Choctaws living in Newton County (Collins 1926). With funding from the American Association for the Advancement of Science, Collins returned the following spring, first

*Figure 5. Photograph of Henry B. Collins, assistant curator of ethnology, U.S. National Museum, Smithsonian Institution, ca. 1930 (negative no. 11,033-A; photograph courtesy of National Anthropological Archives, Smithsonian Institution).*



conducting a brief archaeological reconnaissance in southern Louisiana and Mississippi (Collins 1927a, 1927b) and then continuing the Choc-taw project in Newton County. This was exactly the kind of applied work that Powell had envisioned for Washington-based anthropology.

Out of all the federal archaeologists working in the Southeast, Collins would have the most significant and lasting impact, though his seminal role often is overlooked. His work in Louisiana and Mississippi during the 1920s is of particular interest because of the impact it had on succeeding generations of archaeologists—an intellectual genealogy that can be traced from Collins through James A. Ford, who from the late 1930s to the middle 1950s was the dominant force in Southeastern archaeology. Collins trained Ford in the late 1920s when the latter was still a high-school student, and Ford later used what he learned while working in western Mississippi as he set about the arduous task of carving up prehistoric time in the lower Mississippi River valley—a task that was largely complete by 1940 (O'Brien and Lyman 1998, 1999a).

#### THE CONFERENCE ON SOUTHERN PRE-HISTORY

It was into the intellectually shallow waters of Southeastern archaeology that the National Research Council's Committee on State Archaeological Surveys conference waded in 1932, when it hosted its second of three regional meetings designed to facilitate communication among archaeologists. The first had been held in St. Louis in 1929 and had attracted wide attendance by Midwestern archaeologists, and the committee was eager to replicate the success in Birmingham, Alabama. Organizers, led by Guthe, were careful not to give the impression that a group of outsiders, all from the North, was telling Southerners not only how to do archaeology but also how to organize a meeting. Neil Judd, curator of archaeology in the National Museum, expressed this concern to Guthe in a letter written in September 1932: "As you well know, the South is most conservative and sectional in its attitude; in general it resents northern advice and aid however altruistic" (cited in Lyon 1996: 54).

The conference, which was, as Gibson (1982: 258) pointed out, "without doubt one of the most influential professional meetings ever held on Southeastern archaeology," convened at the Hotel Tutwiler in Birmingham on December 18, 1932, and lasted three days. The report that was issued after the meeting carried the text of the papers pre-

sented, along with comments made by session chairmen. That report makes it obvious that Guthe took Judd's concern seriously when he drew up the program. The major papers were by nationally recognized archaeologists and anthropologists from Northern institutions. In addition to Wissler, Guthe, and Judd, presenters were Ralph Linton of the University of Wisconsin; Fay-Cooper Cole of the University of Chicago; Warren K. Moorehead of Phillips Academy; and John R. Swanton, Matthew W. Stirling, and William Duncan Strong of the BAE. These papers were arranged around summaries of the archaeological records of individual states, presented for the most part by Southern prehistorians familiar with those records. P. A. Brannon of the Alabama Anthropological Society chaired this session, Samuel C. Dellinger of the University of Arkansas spoke on Arkansas, Walter B. Jones of the Alabama Museum of Natural History spoke on Moundville cultures, Charles K. Peacock of the East Tennessee Archaeological Society spoke on Tennessee, and James E. Pearce of the University of Texas spoke on eastern Texas. In addition, Winslow Walker of the BAE spoke on Louisiana, and Collins spoke on Mississippi.

Although it carried no by-line, the introduction to the conference volume must have been written by Guthe. In it he stated clearly the purpose of the conference:

The Conference on Southern Pre-History . . . was called for the purposes of reviewing the available information on the pre-history of the southeastern states, discussing the best methods of approach to archaeology in this region, and to its general problems, and the developing of closer cooperation through the personal contacts of the members of the conference. During the past few years, the interest in Indian pre-history of the lower Mississippi Valley and the southern Atlantic states has been increasing steadily, and a number of institutions have undertaken research work in this field. Developments from studies of the same period in the northern part of the Mississippi Valley and from work on certain Southwestern problems indicate that as the knowledge of the pre-historic cultures of the southeast increases, the problems of the neighboring areas will be more clearly understood. It was for the purpose of fostering more rapid increase of this knowledge that this conference of experts in the study of pre-history from all over the United States was called to meet with interested students of the South. (Guthe 1932: 1)

Guthe selected his words carefully because he really was saying that nowhere in the Southeast were approaches that routinely were employed in the Southwest being incorporated into fieldwork and analysis. Part of the problem lay in the attraction that the Southwest had long held for prehistorians—it literally drained archaeological brainpower into that region at the expense of other regions—and part of it lay in the fact that Southern universities were not producing students trained in archaeology. In some states, such as Alabama, the majority of work was undertaken by museums, oftentimes in conjunction with local archaeological societies. In other states, amateur-based societies were left to their own devices. In some cases, the quality of work was credible, at least for the time period, but in others it was deplorable. To ensure that everyone was on the same page, the Committee on State Archaeological Surveys had launched the regional meetings.

Ensuring that everyone was on the same page meant that the regional experts—the ones actually doing much of the work in the Southeast—either would be trained in proper procedure or, failing that, at least be made aware of what proper procedure was. To that end, the last day of the conference was dedicated to three topics—“exploration and excavation,” “laboratory and museum work,” and “comparative research and publication”—with the morning devoted to presentations by Cole, Judd, and Wissler and the afternoon to roundtable discussions led by Moorehead, Strong, and William S. Webb, a physicist-turned-archaeologist from the University of Kentucky who was soon to head much of the federal-relief archaeology that took place in the South. In essence, the sessions were geared primarily toward imparting some degree of information on the proper methods of excavating a site, of analyzing the artifacts that came out of the ground, of preserving the artifacts, and of presenting the results of the work. It is unclear, however, how much of an effect these presentations actually had on Southeastern archaeology. The same cannot be said, however, of some of the papers presented in the other sessions, especially those by Walker on Louisiana and Collins on Mississippi.

### *Winslow Walker and Louisiana Prehistory*

Walker’s paper in particular was well thought out, and, as Gibson (1982: 259) notes, what Walker had to say about the promise of Louisiana’s archaeological record and the future directions that should be

taken in an effort to understand that record apparently had a profound effect on two young men in attendance—James A. Ford and Fred B. Kniffen, the latter a newly appointed faculty member at Louisiana State University who had trained under geographer Carl Sauer and anthropologist A. L. Kroeber at the University of California, Berkeley. Both Ford and Kniffen immediately began orienting their work in some of the directions in which Walker was pointing (O'Brien and Lyman 1998, 1999a).

One issue that Walker touched on in his presentation was the correlation between archaeological site location and river channels—more precisely, using the history of river channels to date archaeological sites. Kniffen had already begun exploring the relation between site location and geomorphic features in southern Louisiana, especially relative to land subsidence, as part of Richard Russell's coastal-environments program at Louisiana State University, but he would soon develop several other innovative techniques (Kniffen 1936, 1938), in part because of Walker's influence (O'Brien and Lyman 1998, 1999a).

Everything else that an archaeologist wanted to do necessarily hinged on the ability to order remains chronologically. In the Southwest, seriation was a commonly used method for ordering assemblages (O'Brien and Lyman 1999b), and although Kniffen (1936, 1938) introduced it into the Southeast (without referring to it by name), it was not until the late 1940s that it became used there in any kind of routine fashion (O'Brien and Lyman 1998, 1999b). Walker (1932: 48) had a different strategy in mind when he noted that "it is futile to attempt a classification of pre-historic mound cultures in the lower Mississippi Valley until we know more definitely whether or not they have any connection with the principal [historical] tribes found there. . . . Some of these Indians we know were builders of mounds, but just which ones, and through what stages of development they may have passed, are problems requiring further attention."

The link between peoples living during the prehistoric period and those occupying the region during historical times was what Walker referred to as the "proto-historic" period—a temporal unit about which, Walker (1932: 48) admitted, "we are completely in the dark archaeologically." How did one deal with the protohistoric period? Walker (1932: 48) had the answer—one that had long been apparent to Smithsonian archaeologists working in the Southeast: "The clue to this phase is the identification of sites visited by the Spaniards in 1542 and by the

French in 1682. Special investigations should be made of all relics purporting to date back to either of these periods of exploration.” Walker (1932: 48) also addressed the investigation of prehistoric remains: “Sites known to contain only prehistoric material should not, of course, be neglected, as there is much work to be done in determining the relationships of the northern and southern mound cultures. But it is more important to establish first the succession of historic and proto-historic cultures, before attempting to say positively just what cultures belonged strictly to prehistoric times.”

Walker was advocating what his Smithsonian colleague Waldo Wedel (1938) would refer to a few years later as the direct historical approach. No one can legitimately argue with the logic of the approach, which was not new in the 1930s but rather had been the strategy adopted by Powell and Thomas for the Division of Mound Exploration: First, document the similarities in cultural materials between those evident from ethnographic and ethnohistorical research and those evident archaeologically. Second, assume similar materials are temporally and phylogenetically related, and construct a continuous thread or cultural lineage from the past to the present. In “An Introduction to Nebraska Archeology,” BAE archaeologist William Duncan Strong (1935; see also Strong 1936) noted the importance of this approach:

It is the firm belief of the author that the possibilities of historic archeology in North America are not fully realized by the majority of anthropologists at the present time. . . . It seems surprising, therefore, that even today there are archaeologists more interested in segregating obscure early cultures of unknown periods and affiliations than they are in determining the historic cultures and sequences represented in the regions to be worked. Obviously, in such work the historic cultures need not be an end in themselves, but they do seem to represent the threads that give most promise of untangling the complex skein of prehistory. (Strong 1935: 296)

There are two critical aspects of the direct historical approach. First, it provided “a fixed datum point to which sequences may be tied” (Steward 1942: 337). That is, it gave one a chronological anchor in the historical period to which archaeological materials of otherwise unknown relative age could be tied. Second, the more similar prehistoric materials were to the historically documented materials, the more re-

cent they were; conversely, materials that were less similar to historically documented materials came from farther back in time. That is, the direct historic approach demanded the study of homologous similarity, a point unrecognized at the time. Without a chronological anchor, sequences could not be established, and assemblages of artifacts had the unsavory characteristic of floating in time and thus being of minimal utility in determining the development of historically documented cultures. This was one of the major weaknesses of the Midwestern Taxonomic Method (e.g., Steward 1942), developed at the same time that the Birmingham Conference was held and soon to be popularized in the North (McKern 1939). Archaeologists working in the Southeast did not adopt the Midwestern Taxonomic Method (Dunnell 1990: 19; Fagette 1996: 8) because of the clear methodological guidelines provided at the conference, the requirements of successfully doing New Deal Archaeology that appeared a year later (Fagette 1996; Lyon 1996; Quimby 1979), and the fact that the direct historic approach could be applied successfully in the Southeast.

*John R. Swanton and Southeastern Ethnology*

The success that Walker and other archaeologists working in the Southeast had in applying the direct historical approach was based in large part on the work of John R. Swanton, a Harvard-trained archaeologist-turned-ethnologist who spent his career with the BAE. Swanton's early work was on North American Indian languages, and although he continued to produce linguistic texts throughout his career (e.g., Dorsey and Swanton 1912; Gatschet and Swanton 1932; Swanton 1919, 1940; Swanton and Halbert 1915; Thomas and Swanton 1911), he became better known for his ethnological work, especially as it related to the route Hernán de Soto took during his Southeastern entrada. Swanton was an archaeologist's dream—someone who both spoke the language and was sympathetic to the goals of prehistory. Even more importantly, Swanton was someone who could place individual Indian groups in particular places at particular times. This was no small feat in the Southeast, where Indian tribes had experienced centuries of contact with first one then another white group—Spanish, French, British, and American—the result of which was the constant movement of aboriginal groups from one locality to another. It took someone like Swanton, who Kroeber (1940: 3) characterized as “ex-

hibit[ing] a streak of historical genius,” to sift through the myriad historical documents on the Southeast and to figure out where particular aboriginal groups were at different times in the past.

Swanton addressed the broad issue of Southeastern prehistory in two papers he presented at Birmingham, one titled “Southeastern Indians of History” and the other “The Relation of the Southeast to General Culture Problems of American Pre-History.” Neither was particularly earth-shaking but rather was a synopsis of what he had been advocating to archaeologists for years: Use the ethnohistorical record as a starting point—the chronological anchor—for the reconstruction of prehistory in the Southeast. Participants at the Birmingham Conference sprinkled their presentations with praise for Swanton and the work that he had done (Stirling 1932:21; Wissler 1932: 4).

*Henry B. Collins and the Birth of Southeastern Culture History*

Collins (1932b: 37) also paid homage to Swanton in his paper on historical-period sites in Mississippi: “Our knowledge of the ethnology of the Mississippi Indians is based almost entirely upon the work of Dr. John R. Swanton, whose careful researches have thrown much light on the linguistic and cultural affinities of the Muskogean and other southern stocks.” However, Collins (1932b: 37–38) also noted that “there yet remains the task of determining the limits of various groups in pre-historic times, their relations one to another and to other southeastern groups, an undertaking that as yet has been hardly begun.” Collins (1932b: 38) believed the most immediate problem facing Southeastern archaeologists was the lack of a “basis for chronology,” and he, like his fellow National Museum and BAE colleagues, advocated using the direct historical approach. Collins (1927a: 259–60) had reached a similar conclusion in an earlier paper on Choctaw village sites in Mississippi, in which he stressed how important it was for Southern archaeologists “to seize upon every available source of tribal identification of the cultures represented, and to accomplish this end there is probably no safer beginning than to locate the historic Indian village sites and to study their type of cultural remains for comparison with other sites of unknown age.”

By the time of the Birmingham Conference, Collins was convinced that, of all the “available source[s] of tribal identification,” pottery held the most hope for developing chronological ordering:

[P]otsherds are of decided value as chronological determinants and, if present in sufficient quantities to show the entire pottery range of the site, are of far more significance than a number of complete vessels which might not happen to show such a range. In fact, the obliterating effect of white civilization has reached such a point that at many aboriginal sites potsherds are the only really useful material that the archaeologist is able to salvage. The lowly potsherd thus seems destined to bear much of the weight of the chronology that we all hope may sometime be established for Southern archaeology. (Collins 1932b: 38)

As we discuss in detail elsewhere (O'Brien and Lyman 1998, 1999a), Collins' comment implies (1) a pottery type designates an ethnic group such as a tribe, (2) ethnic groups have histories, and (3) a pottery type designates a specific period in the history of an ethnic group. These were common assumptions among Southwesternists (Lyman, O'Brien, and Dunnell 1997a, 1997b), but they were novel thoughts for someone working in the Southeast. In short, they provided the epistemological warrant for application of the direct historical approach.

Collins was interested in the origin of the pottery: Had it "developed locally" (Collins 1927a: 263), or did it have its origins to the west? Was there an even earlier occupancy of Choctaw territory by some other tribe? To answer these questions, Collins (1927a: 263) noted that it "would be very desirable . . . to have additional collections of pottery from other known Choctaw village sites and from the little known mounds and unidentified sites of central and western Mississippi." One site that provided the needed information was Deasonville (see figure 3), which James A. Ford and Moreau Chambers had found during their survey of portions of Yazoo County, Mississippi, while employed by the Mississippi Department of Archives and History. Collins, Ford, and Chambers spent a week in December 1929 and three days the following December excavating a small part of the site. Collins' discussion of the pottery from Deasonville and its possible chronological placement are of considerable interest, and we discuss these two issues at length. If one were forced to pick a particular piece of work as the point at which culture history in the lower Mississippi Valley was born, Collins' (1932a) analysis of Deasonville could arguably serve that purpose.

The excavations at Deasonville, limited though they were, produced a large sample of sherds, but in order to gauge the "relative proportions

of the various types of ware represented,” Collins (1932a: 12) had his crew pick up “every sherd on and between three cotton rows for a distance of about 100 feet.” Sherds in both the excavated and surface-collected samples were described in the report, but Collins, probably realizing that numerous sherds had been missed during the hurried excavation, reported frequencies of only the surface-collected sherds. He recognized seven pottery types, or what he referred to as “kinds of wares” (table 1). The classification system was created out of the sherds he had available from the site, though the terms he used to create the categories were well established (e.g., Guthe 1928). In 1932, the binomial system of pottery categorization so familiar to modern students of Southeastern prehistory was still several years away from being introduced into the region (Lyman, O’Brien, and Dunnell 1997a, 1997b; O’Brien and Lyman 1998, 1999a), but with even a modicum of knowledge one can easily translate between several of Collins’ kinds of wares and later-named types. This should not be too surprising since he was using the same technological and decorative features to create his units that Ford and others would eventually use to create types.

The way in which Collins went about using the pottery from Deasonville to create a chronological ordering was innovative. He had already identified what he considered to be nineteenth-century Choctaw pottery, characterized by the presence of multiple 5–10-mm-wide trailed lines that had been made by a comb-like instrument dragged across the surface of a vessel (Collins 1927a). This pottery might also extend back before the nineteenth century—Collins as yet had no way of knowing this—but there was little doubt in his mind that it could be identified as Choctaw. By the time Collins was writing the Deasonville report, Ford and Chambers had succeeded in locating what appeared to be Natchez and Tunica sites in western Mississippi, including what became known as the Fatherland site, or the Grand Village of the Natchez, in Adams County, Mississippi (Neitzel 1965; figure 3). Collins (1932a: 18) thus was “able to make use of these additional data as comparative references” in the Deasonville report—data that would figure prominently in Collins’ chronological positioning of the site.

Collins began his discussion of chronological ordering by noting that “red and white painted ware was the most characteristic single type of decorated pottery found at the Deasonville site” (Collins 1932a: 18). He also noted that Moore (1911) had found similarly decorated pottery in Warren, Bolivar, and Tunica Counties in western Mississippi,

Table 1. Pottery Classification System Used by Henry B. Collins for Sherds from the Deasonville Site, Yazoo County, Mississippi

Kinds of Wares	Shape of Vessels	Paste		
		Texture	Color	Temper
Undecorated	Bowls and jars	Coarse	Gray, black, or reddish	Pulverized potsherds
Cordmarked	Jars	ditto	Buff, gray, or black	ditto
Painted	Conical and rounded bowls	Fine	Blue-gray	Pulverized mussel shells
Incised (rim only)	Bowls and jars	Coarse	Gray, black, or reddish	Pulverized potsherds
Incised (body of vessel)	—	Usually fine; some porous	Light brown or gray	Pulverized mussel shells (some vegetable fiber)
Punctated	—	ditto	ditto	ditto
Rouletted or stamped	—	Fine	Gray	Pulverized mussel shells

Data from Collins (1932a).

though it was more common in eastern Arkansas and northeastern Louisiana (Moore 1910). Collins also was quick to observe that “European material was found by Moore at several of the sites from which came the red and white painted pottery” (Collins 1932a: 18). Collins’ red-and-white-painted pottery corresponds to a type referred to since the late 1940s (Phillips, Ford, and Griffin 1951) as Nodena Red-and-White—a type that Philip Phillips (1970) later subdivided into several varieties, one of which, Ellison, was based specifically on the “well-known shell-tempered red and white ware found by Henry Collins at the Deasonville site in 1929” (Phillips 1970: 143).

Collins then turned his attention to the incised sherds from Deasonville, noting that “this is a style of decoration which Ford and

Surface			Rims	
Finish	Color	Decoration	Shape	Decoration
Usually smooth	Drab gray or light brown	—	Straight or slightly incurved	Occasionally a line on top
Cordmarked	Light brown to dark gray	Cord impressions	Straight or slightly incurved (seldom everted)	—
Smooth	Red, white, and gray	Alternating red and white bands and scallops	Enlarged; slightly overhanging on both sides	Usually painted red
Usually smooth	Gray or brown	Band of lines below rim	Straight or slightly incurving	Occasionally a line on top
Somewhat rough	Buff or cream	Straight and curved lines over body of vessel	Straight, incurved, and everted	Looped handles
ditto	ditto	Punctations, usually in bands	Usually everted	ditto
—	—	Rouletted or finely stamped area enclosed deep lines	—	—

Chambers have found to be characteristic of certain prehistoric sites in western Mississippi as distinguished from near-by historic sites of the Natchez and Tunica” (Collins 1932a: 19). Importantly, Ford and Chambers’ data demonstrated that sherds containing the overhanging incised lines—later termed Coles Creek (Ford 1935a: 10)—did not occur on sites known ethnographically to have been inhabited by the Natchez, whose pottery exhibits a “usually polished surface and scroll or meander design” (Collins 1932a: 19). Nor were there at Deasonville any Choctaw sherds, “characterized by straight or curving bands of very fine lines applied with a comb-like implement [Collins 1927a], or [any Tunica sherds], in which the decoration consists of somewhat enlarged rims bearing indentations or scallops together with a single encircling line along the top” (Collins 1932a: 19).

Given (1) the absence of pottery that had recently been found to be associated with known groups, (2) the absence of European items, and

(3) “the presence of another type [the one with horizontal, overhanging incised lines] which at other Mississippi sites appears just as definitely prehistoric,” Collins (1932a: 19) drew the obvious conclusion: Deasonville was a prehistoric site. But he was not sure how old the site was: “Study of the potsherds from Deasonville fails to reveal any clues which might be of value as showing the chronological position of the site beyond the mere fact that it is prehistoric” (Collins 1932a: 19). He commented further, “We must know . . . much more about the geographical range of the various types of Southeastern pottery and the relative position occupied by each, and especially we must know which types are found associated with European material and which types are never found in such association” (Collins 1932a: 20). For some reason it never occurred to Collins—or if it did, he abandoned the thought—that he had evidence right in front of him that would have given him a clue to the antiquity of the site. His failure to employ that evidence is all the more surprising in light of the fact that he had earlier cited it with regard to the age of the red-and-white pottery—the particular type of pottery that Moore had found on sites in northeastern Arkansas that also produced European goods.

Given that he knew his red-and-white painted pottery from Deasonville was similar to what Moore had found at various sites, and given that he had already pointed out that Moore had recovered European items from those sites, why didn't Collins take the next obvious step and remark that perhaps his painted pottery type was a fairly late occurrence, though not as late as the pottery of the historic Natchez, Tunica, and Choctaw? We will never know the answer, though part of it might lie in how Collins—and he certainly was not alone—viewed chronology and site occupation. In brief, Collins believed that people inhabited sites for fairly short periods of time and then abandoned them. He thus dichotomized prehistoric and historic-period sites, with nothing in between. In this all-or-nothing game, a site either had sherds left by the historic Natchez, Choctaw, or Tunica, or it had sherds left by prehistoric groups. Perhaps it never occurred to Collins that sites could span both periods, as did some of Moore's Arkansas sites that contained both prehistoric painted pottery and European trade goods.

Alternatively, maybe he did not know how to measure culture change across the temporal continuum, but that seems doubtful. Rather, we suspect that he was so firmly wedded to the notion that a pottery type designated a particular ethnic group or tribe, and by implication a cer-

tain time period, that he perceived sites such as Moore's as representing temporally mixed occupations. This epistemological position does not preclude the possibility of multiple consecutive occupations but views each occupation as temporally distinct. In conjunction with the notion that there is a discrete and distinct prehistoric period followed by an equally discrete and distinct historical one, any assemblage containing artifacts from both periods must be mixed; that is, the natural discontinuity of the record demanded by this epistemology renders Walker's (1932) protohistoric period invisible.

Phillips (1970: 143) later commented with respect to Collins' work at Deasonville that "At this early time no one could have had sufficient knowledge to recognize that the site had more than one occupation." In reality, the notion of multi-occupation was not in the vocabulary of Southeastern archaeologists. For the most part, time was viewed as a continuum against which little discontinuous blips—occupations—could be placed. Unlike their colleagues working in the Southwest, archaeologists working in the Southeast had not yet begun to carve up time into ceramic periods or other, similar units. This situation, however, would soon change, and the chief architect would be Ford, assisted in several cases by his long-time friend and an archaeologist with the BAE, Gordon Willey (Ford and Willey 1940, 1941). But by then, the groundwork had been laid for measuring time. That groundwork, followed by Ford (at least in his early work) and his contemporaries, comprised the direct historical approach.

### CONCLUDING COMMENTS

In 1933, Frank Setzler of the National Museum and his field assistant, Ford, demonstrated that one could effectively use crews of several tens of men to do archaeology (Quimby 1979: 111), as what became known as New Deal Archaeology began. That research was not inductive or unguided salvage, as some have characterized it. Rather, it had a very explicit goal. In his remarks that opened the second day of the Birmingham Conference, Linton (1932: 3) stated explicitly the research questions that would soon guide much of the New Deal Archaeology:

The worker in any of the surrounding regions finds evidences not merely of diffusion, but of actual migrations coming into his particular area from the southeast, but until the history of that re-

gion is better known, it is impossible for him to tell when such migrants left the southeast, what part of it they came from, what their cultural or racial affiliations may have been, or how they are linked to other cultures marginal to the same area.

Stirling (1932: 20–21) reiterated the questions, thereby reinforcing them in the minds of those in attendance. He also specified the procedure for answering them: “The first problem in developing the archaeology of the given locality is to isolate the known historic cultures leaving a residue of unknown pre-historic, should such exist. Both vertical and horizontal stratigraphy can usually be applied. . . . From our knowledge of the pottery used by the historic tribes, many significant hints are offered regarding pre-historic movements of peoples.” The method comprised the direct historical approach, first used by Thomas in his resolution of the mound-builder controversy. Stirling (1932: 22) also offered the important caution that “the inter-relationship of cultures [is] a flow rather than a series of static jumps.” The significance of that caution was lost not only on archaeologists working in the Southeast but on those working in the Americas generally, which ultimately resulted in the fall from favor of the culture-history paradigm (Lyman, O’Brien, and Dunnell 1997a). That culture history and evolutionary lineages of cultures were “series of static jumps” was at the heart of Collins’, and everyone else’s, epistemology.

Collins (1932b: 37) indicated, for example, that one could determine “the limits of the various [ethnic or tribal] groups in pre-historic times.” He also stated that typological differences in pottery denoted “cultural differences” (Collins 1932b: 40). This was, in short, a way of saying that his understanding of the archaeological record was derived from ethnological theory and ethnographic data. Tribes were discrete chunks of humanity that bore distinct cultural traits and had particular locations in time and space. Assuming that it was possible to identify cultures in the archaeological record (usually on the basis of some typologically distinctive artifacts), when such an identification was made, each prehistoric culture must, it was thought, represent a discontinuous ethnic unit such as a tribe. With the benefit of hindsight, the ontological parallels between the concept of biological species and the concept of prehistoric cultures are remarkable (Lyman, O’Brien, and Dunnell 1997a; Lyman and O’Brien 1997). The analytical problem is one of identifying the historically antecedent species or cultures that were also

ancestral (in an evolutionary sense) to historically or ethnohistorically documented species or cultures, respectively. In other words, phyletic histories of cultures were determined between about 1910 and 1970 in precisely the same sense that prehistoric *Homo erectus* is today conceived to have evolved into *Homo sapiens*. The procedure for determining these phyletic histories was introduced in the Southeast so that culture history could be written there as it had been elsewhere in the Americas.

The procedure focused on *homologous* similarity, or similarity resulting from shared ancestry. Thus, for example, Setzler's (1934) work at the Marksville site in Louisiana resulted in the conclusion that the people who occupied that site were culturally and biologically related both in the sense of homology to people who deposited artifacts assigned to the Hopewell culture of Ohio (O'Brien and Lyman 1998). Collins and others affiliated with or influenced by the BAE used exactly the same reasoning. The diffusion from the BAE and the National Museum to Southeastern archaeologists of the general idea that typological similarity denoted homologous similarity was completed in grand fashion by the end of the Birmingham Conference of 1932. The idea made sense from the perspective of Swanton, Linton, Walker, and Collins, all major figures in the discipline at the time, and everyone in attendance adopted it. The take-home message was simple: Work from the known to the unknown so that you have (1) a chronological anchor for your temporal sequence of cultures and (2) the most recent evolutionary descendant of a cultural lineage to use as a comparative base for determining historically antecedent cultures. This was really not a new message, but Southeastern archaeologists adopted it wholeheartedly and took the direct historical approach to heights unparalleled in Americanist archaeology. ❖

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