

The Legacy of Culture History in the Southeastern United States

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Lyon, Edwin A. *A New Deal for Southeastern Archaeology*. Tuscaloosa: University of Alabama Press, 1996. xiv + 283 pp. including notes, bibliography and index. \$24.95 paper.

Griffin, Patricia C., ed. *Fifty Years of Southeastern Archaeology: Selected Works of John W. Griffin*. Gainesville: University Press of Florida, 1996. xx + 257 pp. including appendices and index. \$39.95 cloth.

McNutt, Charles, ed. *Prehistory of the Central Mississippi Valley*. Tuscaloosa: University of Alabama Press, 1996. xiii + 313 pp. including figures, tables, references and index. \$34.95 paper.

Culture history—literally, the writing of narratives that describe the historical development of cultures—was the first major paradigm in Americanist archaeology. Many of the analytical methods and techniques that archaeologists now routinely employ, as well as the general outlook that guides much modern research, were worked out first in the American Southwest, then in the Midwest and Southeast, between roughly 1914 and 1940 (Lyman et al. 1997).¹ Modern textbooks produced by American-trained archaeologists—regardless of whether the books concern how to do archaeology or the results of archaeological work—are largely organized around interpretive elements developed by culture historians.

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Given its centrality in the discipline, archaeologists need to understand where culture history came from and what its major tenets are.

Each book reviewed here provides a window on the culture-history period. Charles McNutt's *Prehistory of the Central Mississippi Valley* allows one to evaluate the successes and failures of culture history in what was perhaps the most important proving ground for the paradigm. Culture history might have been born in the Southwest, but it was in the Mississippi Alluvial Valley—that great expanse that stretches roughly from the juncture of the Ohio and Mississippi rivers to the Gulf of Mexico—where the paradigm reached its zenith. Edwin Lyon's *A New Deal for Southeastern Archaeology* captures some of the spirit of the emergence of culture history not only in the Mississippi Valley but in the rest of the Southeast, and Patricia Griffin's *Fifty Years of Southeastern Archaeology: Selected Works of John W. Griffin* provides insight into how the paradigm influenced the work of a single individual who spent most of his professional life working in the Southeast.

The books were not intended to be introductions to culture history. McNutt's purpose lay in assembling the work of nine authors, including himself, each of whom was asked to summarize the prehistoric archaeological record in select localities in the central Mississippi Valley. Thus, the essays focus on the results of archaeology instead of on the intellectual history behind that work, but they provide an excellent vehicle for examining the legacy of decades of culture-historical analysis in one of the truly remarkable archaeological areas of the United States. Lyon's task was to discuss the important role of federal funding in the development of southeastern archaeology. As a historian, he is interested less in the archaeological record per se and more in how various federal programs at once both infused large amounts of money into archaeology and imposed a series of bureaucratic obstacles with which archaeologists were ill-prepared to deal. Griffin presents a thoughtful introduction to a collection of her late husband's papers that address select aspects of archaeology and the archaeological record of Florida.

The three books have been reviewed previously in various venues (e.g., Ford 1997; Wesler 1997), and it is not my intention to duplicate those reviews. Rather, I focus on aspects of each book that when strung together create a picture of what archaeology in the Southeast was like during the heyday of culture history and highlight the legacy of that first paradigm in Americanist archaeology. Because the books are not overviews of culture history, I place selected aspects of each within a larger discussion of culture history in the Southeast. This necessitates producing a different kind of review than normally is encountered, in that I discuss several other works that contributed heavily to the intellectual climate of the period.

THE RISE OF CULTURE HISTORY

It was the culture-history paradigm with which many archaeologists of the 1960s became dissatisfied—so much so that they argued for its replacement by one based on the search for cultural processes that underlay the creation of the archaeological record. Under the guiding hand of Lewis Binford (e.g., 1962, 1965), this new approach, known as processualism, became the dominant paradigm in Americanist archaeology (Spencer 1997). Yet, the fundamental principles of culture history were so ingrained in Americanist thought that archaeologists, whether they realized it or not, carried many of them over to the new paradigm (Meltzer 1979). Although writings of the 1960s and 1970s suggest otherwise, archaeologists' efforts to be recognized as scientists by the academic community did not originate with processual archaeology. Early twentieth-century Americanists expressed a similar desire, and how they attempted to fulfill it is an interesting part of the discipline's history. Always in the middle of culture historians' efforts to make archaeology scientific was the notion of culture change and how best to examine and explain it. At the center of any discussion of change is the issue of time and how to measure it.

The genesis of culture history can be traced to the American Southwest and the work of A. V. Kidder, Nels Nelson, and A. L. Kroeber beginning around 1914 (Lyman et al. 1997). Within a span of three years, their collective work demonstrated that it was possible to order archaeological phenomena chronologically using changes in how pottery was made and decorated. The coalescence of seriation, superposition and stratigraphy, cross dating, and the direct historical approach into an integrated body of methods that were devised to track temporal and spatial variation in the North American archaeological record resulted in the appearance of a new tack for Americanist archaeology—culture history. Some processualists of the 1960s would later claim that culture history was nothing more than keeping track of time and space and creating endless lists of culture traits from various geographic localities. This, they would argue, was why the older archaeology had to be swept aside and replaced by one sensitive to understanding the cultural processes that led to the creation, use, and discard of materials found in the archaeological record.

The processualists might have thought that they brought about the decline in popularity of culture history—exploiting what they saw as natural weaknesses in the approach—but their critiques of the paradigm were late comers. The first real frontal assault came in the 1940s in the form of a dissertation written by an until-then-unheard-of Harvard graduate student, Walter Taylor (1948). In large part at the urging of Clyde Kluckhohn, Taylor excoriated archaeologists, several of them by

name, for spending too much time on the minutiae of chronology and not on, as Robert Braidwood (1959:79) would later put it, the "Indian behind the artifact." The problem was how to make the static archaeological record look more like something that would interest ethnologists, who by the 1950s had begun to grow tired of reading about chronological orderings and had begun to see archaeology as "the tail on the ethnological kite," as Julian Steward (1942:341) had earlier put it. This resulted in archaeology being reduced in the eyes of some to little more than the stepchild of ethnology. The efforts first of Taylor and then of Binford were aimed at proving that archaeologists *could* be anthropologists.

But in actuality, this had been the goal of culture historians from the start—a point missed entirely by most contemporary histories of Americanist archaeology. True, culture historians might have spent considerable time on the minutiae of chronology, as Taylor put it, but the goal they had in mind was to bring the static archaeological record to life. In short, they wanted to do paleoethnology. Culture historians were, after all, trained in departments of anthropology—for example, Nelson at the University of California, Kidder at Harvard, and Kroeber at Columbia—and to claim they were ignorant when it came to things anthropological is the height of arrogance. They *did* understand what cultural processes were; they just were not sure how best to go about isolating and explaining the processes.

Note, for example, how Patricia Griffin, in *Fifty Years of Southeastern Archaeology*, characterized her late husband's training at the University of Chicago, where he was influenced not only by archaeologist Fay-Cooper Cole but also by ethnologists A. R. Radcliffe-Brown, Robert Redfield, and Fred Eggan:

The University of Chicago gave Griffin a broad viewpoint and a centering that he never lost. The particular blend of humanism and science that he acquired there informed his professional career as it did that of many of his fellow students. Early on, Griffin internalized the philosophy of the four fields of anthropology, an icon at Chicago. While he did not go so far as to say, as others have, that "archaeology is anthropology or it is nothing" (Willey and Phillips 1958, 2),² through the years he generally corrected those who saw archaeology as totally separate from the field of anthropology. (p. 4)

In John Griffin's own words, which come from an article entitled "Missions and Mills: The Making of a Historic Archaeologist (reprinted in *Fifty Years of Southeastern Archaeology*), he noted that "Some recent writers of archaeological history seem to believe that we Midwesterners of that era, and particularly those of us at Chicago, were subjected to nothing but field methods, trait lists, and the McKern taxonomic system.³ They need to be reminded that the influence of A. R. Radcliffe-Brown was still strong, and that we were constantly confronted with the ideal of a science of society" (p. 16). These passages make it sound as if at least those

archaeologists trained at the University of Chicago never lost sight of the Indian behind the artifact. In truth, it was not simply Chicago dogma with which Griffin was imbued; archaeology as paleoethnology was widespread in university curricula by the 1930s.

CULTURE HISTORY COMES TO THE SOUTHEAST

Nowhere was the desire to make archaeology anthropological more in evidence than in the southeastern United States. The region had lagged behind the Southwest in terms of fieldwork conducted by trained archaeologists, and to remedy the situation the National Research Council's Committee on State Archaeological Surveys organized the Conference on Southern Pre-History, which was held in Birmingham, Alabama, in December 1932. This was the second of three regional archaeological conferences organized by the committee for the express purpose of bringing a 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 (O'Brien and Lyman 1999). Professional anthropologists and archaeologists from northern institutions, including the Bureau of American Ethnology (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. In *A New Deal for Southeastern Archaeology*, Lyon notes simply that the “meetings allowed a useful exchange of information among many of the archaeologists who would play major roles in federal archaeology in the depression” (p. 54). This is a tremendous understatement. Information undoubtedly was exchanged among participants, but the lasting value of the meeting was that it laid out a simple plan of action that could be followed in archaeological pursuits. And it *was* followed, almost to the letter.

A key item on the agenda was developing a chronological ordering of archaeological remains, which was not a mean feat 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 over 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, more traditionally anthropological aspects of prehistory. As their knowledge of the 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.

Southern archaeologists were told by their northern counterparts at the conference that there was a simple procedure that would assist their efforts not only in establishing chronological ordering but also in writing culture history. This procedure, which slightly later became known as the direct historical approach (Wedel 1938), was the legacy of the Birmingham Conference. The procedure had been around since the founding of the BAE in 1879 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, who as Lyon points out was characterized by Kroeber (1940:9) as an "advisor and godfather" to eastern archaeologists.

No one can legitimately argue with the logic of the direct historical approach: First, document similarities in cultural materials between those evident from ethnographic and ethnohistorical research and those evident archaeologically. Second, assume similar materials are temporally and phyletically related, and construct a continuous thread, or cultural lineage, from the past to the present. In his *An Introduction to Nebraska Archeology*, William Duncan Strong (1935; see also Strong 1936) noted the importance of the 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. Wherever the approach has been from the known historic into the unknown prehistoric, the results have more than justified the method, as the present superior status of archeology in the Iroquoian and Pueblo areas amply demonstrates. It seems surprising, therefore, that even today there are archeologists 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)

John Griffin, in his article "Missions and Mills: The Making of a Historic Archaeologist" (reprinted in *Fifty Years of Southeastern Archaeology*), also pointed out the importance of the direct historical approach: "It should also be remembered that culture change and acculturation, both viewed as processes, were popular research orientations [in the 1930s]; and, in archaeology, the direct historical approach advocated by Wedel, Strong, and others was new and exciting. This latter, I believe, is underestimated as a conditioning factor in some of us who later turned to the sites of

Western European culture" (p. 16). Without a chronological anchor, sequences could not be established, and assemblages of artifacts had the unsavory characteristic of floating in time, touching down who knew where and thus being of minimal utility in determining the development of historically documented cultures (Lyman and O'Brien 2000).

The timing of the Birmingham Conference was not entirely accidental. By 1932 the country was deep in the throes of a depression, and it was obvious that some sizable relief effort by the federal government was in the offing. Franklin Roosevelt had campaigned on the promise of putting people back to work, and his election in November 1932 sent a clear signal that assistance was on the way. The South was one of the hardest-hit regions, and it was reasonable to assume that it would receive immediate attention. One of the most efficient methods of getting people back to work was through massive labor-intensive programs such as building dams and reservoirs, which meant that archaeological resources would be destroyed. As Lyon clearly points out in *A New Deal for Southeastern Archaeology*, numerous archaeologists had been working for years to get federal funds earmarked for archaeological work. With the prospect of relief efforts on the horizon, it seemed possible to funnel some of the funding into archaeology. One potential problem, though, was the shortage of trained personnel in the South to lead the hoped-for fieldwork and analysis. Hence, the Birmingham Conference was held.

In the late summer and fall of 1933, using funds provided by the Federal Emergency Relief Administration, Frank M. Setzler of the U.S. National Museum and James A. Ford, then a student at Louisiana State University, demonstrated at the Marksville site in east-central Louisiana that one could effectively use crews of several tens of men to do archaeology (Quimby 1979:111). With initiation of the work at Marksville, what became known as New Deal archaeology had begun. Some archaeologists, primarily those who played no role in it, characterized New Deal archaeology as an inductive or unguided salvage operation, but as Lyon points out, it had a very explicit goal (in addition to assisting thousands of out-of-work Americans)—namely, to arrange the archaeological record in terms of time and space and to present the results of analysis in a timely fashion. This meant that careful attention had to be paid to similarities and differences among artifact assemblages.

In his remarks that opened the second day of the Birmingham Conference, Ralph 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 region 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.

Matthew Stirling, chief of the BAE, specified the procedure for answering the questions Linton posed:

It is significant that any attempt to develop a systematic procedure for archaeology in the Southeast must begin with our knowledge of the locations and movements of tribes in historic times. . . .

The method of procedure of the archaeologist should be, of course, to work from the known to the unknown. There exist many early aboriginal sites which can be definitely located and the dates of occupancy of which are known. Some of these sites have more than one period of occupancy by one or several tribes. 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. (Stirling 1932:20–21)

This procedure was, of course, the direct historical approach.

More important than his words on procedure was Stirling's (1932:22) reminder that "the inter-relationship of cultures [is] a flow rather than a series of static jumps." The significance of that reminder was lost not only on archaeologists working in the Southeast but on those working in the Americas generally. That culture history and evolutionary lineages of cultures were "series of static jumps" was at the heart of everyone's epistemology. This is a most critical point, and one that has been overlooked in reviews of the culture-historical period. In brief, culture historians early on developed two methods—frequency seriation and percentage stratigraphy—that measured time continuously. Kroeber, working in the Zuni region of New Mexico in the middle of the second decade of the twentieth century (Kroeber 1916), demonstrated the efficacy of frequency seriation for producing a continuous ordering of pottery types. Nelson (1916) and Kidder (1917; Kidder and Kidder 1917), working in north-central New Mexico, demonstrated how relative frequencies of pottery types from stratigraphic excavation—later termed *percentage stratigraphy* (Willey 1939)—could also be used to mark the passage of time.

Thus by 1917 there were two independent means of measuring time, with one serving as a check on the other. Importantly, these techniques marked the passage of time *continuously* as opposed to marking it in terms of discontinuous chunks. This strategy, however, was abandoned in the 1920s as archaeologists turned their pottery types into marker types, similar to how geologists use index fossils as time markers (O'Brien et al. 2000). Before long, culture historians began to see stratified deposits as representing layer cakes of cultures, each of which was identifiable by specific

marker types. At that point, culture historians became ensnared in a paradox: They saw *culture* as something that evolves continuously—it had in the past and does today—but, forgetting Stirling's caution, they were measuring it in terms of discontinuous chunks called cultures.

As a result, the literature became filled with interpretive statements to address seeming unconformities in the archaeological record—statements based on old anthropological standbys such as diffusion, acculturation, migration, and the like. For example, in one of his papers reprinted in *Fifty Years of Southeastern Archaeology*, "History and Archaeology in Florida," Griffin noted that

From the point of view of the archaeologist, one thing must be constantly borne in mind. The identification of the culture of one documented village does not prove that all such materials are attributable to the tribe that occupied the village; nor does it prove that all of the villages of that tribe share that culture. A single tribe may change its culture through time, may share a culture with other tribes, or may, in its various territorial divisions, possess divergent cultures. This is merely a statement of two cultural truisms: that tribe and culture are not inseparable concepts, and that culture change is a constant feature of human society. (p. 43)

Griffin's use of terms such as tribe and village was consonant with that of his contemporaries, all of whom derived the terms from common sense and not from any theory. The terms followed directly from nineteenth-century ethnology, as did any use of evolutionary terminology. One finds occasional reference to Darwinian evolution in the culture-history literature, as well as a few earnest if naive attempts to explain the record in Darwinian terms (Lyman and O'Brien 1997), but by and large such use was metaphorical. When culture historians talked about the evolution of cultures, or tribes, or any similar unit, they were basing their discussions on the progressive, stagelike models of Lewis Henry Morgan and Edward B. Tylor that had become popular in the nineteenth century. Few archaeologists questioned the usefulness of such models because it was axiomatic that culture evolved. As a result, not only the models but also the terms, or units, employed in the models became cast in stone. This made them real. If units such as tribe, village, and the like were real, then so too must be the archaeological units—pottery types, phases, and periods—used to pigeon-hole the massive variation seen in the record. What most culture historians forgot was that the units they were using to impose spatial and temporal control over the record were *not* real. The artifacts were real, but the units were not. Further, no one questioned the validity of borrowing sociocultural units—tribes, bands, and so on—from ethnology. This, of course, was a predictable occurrence, given wholesale reliance on the direct historical approach to provide chronological anchor points. On the one hand, there was nothing wrong with starting in the

present and working back in time, but on the other hand, there was no warrant for using nineteenth- and twentieth-century observations as proxies for the deep past.

Speaking at the Birmingham Conference, Henry Collins (1932:37) of the U.S. National Museum indicated 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 1932:40). This was, in short, a way of saying that 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 reasoning, flawed though it was, became part and parcel of culture history.

AFTER THE BIRMINGHAM CONFERENCE

Within a few years of the Birmingham Conference, the Southeast was awash in archaeological fieldwork brought about by the various New Deal programs. State universities began training students in archaeology, and all institutions wanted a piece of the federal money that was beginning to pour in. Lyon paints an excellent picture of the period, pointing out that whereas southern archaeologists were concerned with putting into practice what they had learned from their northern colleagues, the federal government was interested primarily in getting Americans back to work. Projects were organized on a state-by-state basis, often with little coordination between even adjoining states. As Lyon points out, "The archaeologists of the depression continually suffered from constraints of the relief bureaucracy.... WPA officials did not understand artistic or scientific work, particularly at the state level. Archaeologists, as well as artists, writers, and other professionals, felt new controls on their professional freedom. Adjustment by archaeologists to the growing and changing federal bureaucracy of the depression was difficult" (p. 64).

Lyon also is able to interweave purely historical information—when various federal programs began, who directed them, and so forth—with information as to why certain archaeological programs were conducted in the particular manner they were. Thus I find it difficult to believe that anyone interested in southeastern archaeology between roughly 1930 and 1940 would not read Lyon's book and come away the wiser for it.

That having been said, what one will *not* find in the book is much discussion of contemporary epistemological concerns that formed a fabric of interconnected threads throughout the period of Lyon's concern. In other words, there is little discussion of the intellectual engines that drove culture history. Lyon, being a historian, cannot be faulted for this omission, but it is *the* critical issue to understanding New Deal archaeology—not the day-to-day operations but, rather, why archaeologists came to the conclusions they did. One has to dig into the intellectual history of the period, but beneath the surface, past all the records pertaining to fieldwork and the like, is the interesting story of how one went about doing culture history.

One has to dig even deeper to get at the epistemological underpinnings of culture history. What emerges from the effort, however, is the discovery of a fascinating paradox (Dunnell 1995; Lyman et al. 1997; O'Brien and Lyman 1998)—namely, that culture historians were attempting to measure the continuous flow of time using a series of discontinuous units. This is what Matthew Stirling had cautioned archaeologists not to do, but for the most part they continued to view the archaeological record as a series of discrete chunks, even though they freely admitted that time itself was a continuum. And always at the base of efforts to order those chunks correctly was the direct historical approach.

The approach underlay the work of Ford, who conducted several projects on his own before being appointed project director of the Works Progress Administration (WPA) archaeology program in Louisiana. Lyon adequately details the early career of this archaeologist who almost single-handedly developed the cultural chronology for the lower Mississippi Valley—a chronology that then became the basis for chronological orderings over much of the South. Ford's first monograph was based on excavations at a small site in north-central Louisiana (Ford 1935), and he followed it the next year with a discussion of various surface collections made in Mississippi and Louisiana (Ford 1936). Thus by the time he assumed the reins of the WPA program in Louisiana, he was no novice to archaeology. He took a leave from the project to receive his master's degree from the University of Michigan in 1938, and while there he worked closely with James B. Griffin, who at the time was beginning to attract national attention as an expert on eastern United States pottery. It was through the combined efforts of Ford and Griffin that a pottery conference was held in Ann Arbor in May 1938 to standardize the formulation of pottery types. Out of that conference sprang the type system that is still used today—one based primarily on the kinds of designs carried on ceramic vessels (Ford and Griffin 1938).

Although the amount of material excavated in Louisiana by WPA project personnel precluded immediate publication of all but one report

(Ford and Willey 1940), Ford by 1940 recognized six ceramic periods in the greater Mississippi Valley. Without becoming too bogged down in chronological details, work at the Tchefuncte site and at contemporary sites in southern Louisiana demonstrated a "complex" of pottery that occurred stratigraphically below sherds that had become marker types of what Ford termed the Marksville period. Based on such findings, Ford created the Tchefuncte period. Work at the Greenhouse site, located just north of Marksville, though not published for over a decade (Ford 1951), provided materials that Ford used to create a new period, called Troyville, which he placed between the end of the Marksville period and the beginning of the Coles Creek period. Based on the stratigraphic position of material at several other sites, Ford created the Plaquemine period and wedged it in between the earlier Coles Creek period and the later historical (Natchez) period.

Tchefuncte as a period was accepted immediately by archaeologists working in the lower Mississippi Valley, because by 1940 it was generally recognized that there were sherds from the region that on purely stylistic grounds looked ancestral to the stamped and incised materials that were assigned to the slightly later Marksville period. It did not hurt Ford's case that those sherds were stratigraphically lower than typical Marksville materials at several sites in Louisiana. However, Troyville and Plaquemine at first were not generally accepted as "valid" periods. Jon Gibson (1982:271) noted that both were

transitional units. . . . carved out of ceramic complexes that had formerly been classified as something else. This confounded opponents who simply could not see how some cultural types could be Marksville or Coles Creek one day and Troyville or Plaquemine the next. These individuals apparently did not share Ford's view of culture as a gradually changing flow of ideas, with any one archaeological site encapsulating those elements which comprised a limited span of an unbroken continuum.

Many culture historians of the time subscribed, often implicitly, to the flowing-stream model of culture change, but the source of disagreement no doubt resided in misunderstandings over how to *measure* the continuum of cultural development. Ford was using analytical units of the *ideational* sort, meaning that he did not necessarily attach any reality to them. The pottery types *might* correlate with particular ideas of the original pottery makers, but then again, they might not. In other words, the types were not "real" in an empirical sense. Therefore, where one began or ended periods was meaningless. Many other culture historians, however, believed, or at least implied, that the time periods coincided with something real culturally. Thus, confusion over the fact that Ford's marker types in various combinations were definitional of a time period and not necessarily of a cultural unit contributed to a general lack of understanding. Ford did not help matters

with his division of the cultural continuum into arbitrary periods that he discussed as if they were real units or cultures.

Confusion of temporal units—periods and the like—with cultural units was typical of the culture-history period. Note what John Griffin, in a previously unpublished manuscript titled “Some Highlights in the History of Florida Archaeology” (reprinted in *Fifty Years of Southeastern Archaeology*) had to say about units: “One of the hallmarks of the earliest years of the Southeastern Archaeological Conference . . . was the definition of and discussion of pottery types in what came to be known, informally at least, as the Southeast Binomial System. We began to take our potsherds seriously. And pottery types and pottery series were soon joined by named cultural units (periods, phases, foci, assemblages, cultures—whatever)” (pp. 120–121). Whatever indeed; here Griffin illustrated the total conflation of various kinds of analytical units that was so prevalent throughout the cultural-historical period. As we will see, it still occurs in Americanist archaeology.

An additional example of unit conflation is found in another section of Griffin’s above-cited paper, in which he stated (p. 121) that his candidate for “the ‘first’ modern, or scientific contribution to the archaeology of Florida [would be] the *American Antiquity* paper by [Richard] Woodbury and [Gordon] Willey, ‘A Chronological Outline for the Northwest Florida Coast’” (Willey and Woodbury 1942). In seeking to construct a chronology of pottery types for the northwest coast of Florida, Willey and Woodbury (1942:236) wrote that

Any pottery type is based on a number of stylistic features found in combination, but changes occur over time, and transitions are often so gradual as to prevent sharp distinctions. However, the periods into which the pottery types have been grouped are each based on one or more “key” or “marker” types, which have been found to be sufficiently restricted in range and distinctive in appearance to allow their occurrence to be quite precisely determined. It is also hoped that the “periods” will prove to represent distinct cultures when the bare skeleton of ceramic chronology has been given flesh and body in the form of a full and “functional” culture description.

Analytical, or ideational, units (pottery types) construed as index fossils—the manifestations of which occurred in the archaeological record—could be used to measure the passage of time, but how could a time period be correlated with an ethnographic unit such as a “culture?” Even though such a correlation was untestable, it served as an ad hoc warrant for dividing the continuum into what otherwise were clearly arbitrary chunks.

The bottom line is that periods are temporal units created solely by the archaeologist to order variation. Thus there is nothing real about them. Ford realized this, but most of his contemporaries did not. However, simply because he knew the difference did not mean that Ford at times

was above conflating such things as periods and cultures. Somewhere along the way he dropped his earlier disclaimer (Ford 1938) that pottery complexes did not necessarily represent distinct cultures, and as a result his periods eventually took on essentialist properties and were referred to by him as "cultures." Thus we read statements such as "While the [A]dena-like culture was spreading through the upper Mississippi drainage and developing areal peculiarities, changes were going on in the Lower Mississippi Valley transforming the Tchefuncte to the next recognized cultural period, the Marksville" (Ford and Willey 1941:338).

As important as population movement was to Ford as a mechanism of culture change, there was one "new idea" for which he did not invoke it as a mechanism—an "idea" that would later figure prominently in a classic dialog about the Mississippi River valley and which is still very much alive today. That "idea" was shell-tempered pottery, which became a hallmark of Ford and Willey's (1941) Middle Mississippi *period*, which they divided into two segments, early and late. I italicize the word "period," as confusion has always existed in the literature because of the myriad ways in which the term *Mississippi*, or *Mississippian*, has been used. For example, William Henry Holmes (1886, 1903), who coined the term, used it as a geographic designation. James B. Griffin (1952) used the term *Mississippi* to refer to a period as well as to a culture type, and Willey (1966), in his summary volume on the archaeology of North America and Mesoamerica, used the designation *Mississippian tradition*.

Confusion over terminology was fostered by Ford and Willey's (1941) use of key terms such as "period" and "culture" without explicit definitions. The potential for conflation of these units was enhanced when they spoke metaphorically of "stages of a culture," such as Coles Creek culture or Tchefuncte culture, when they merely meant later sections of the time span occupied by the cultural units. With reference to the early Middle Mississippian period in the upper half of the lower Mississippi Valley, Ford and Willey (1941:348) noted that it "succeeds the Late Baytown in eastern Arkansas and western Mississippi. . . . It should be emphasized that the changes do not suggest a complete replacement of cultural features, but rather a development and an intrusion of new ideas." This was not so different from typical contemporary statements, which were little more than blends of diffusion and in-situ development:

Shell tempering and the use of handles on pottery vessels are the most marked changes in ceramics. Clay-tempered polished vessels are gradually replaced by vessels of similar shapes tempered with finely ground shell. Red slipped bowls, ears on bowl rims, types of incised decorations, wide-mouthed bottle forms, round bottomed bowls, flat bottomed bowls with flaring sides, beakers, and many other ceramic features change but little. Rectangular mounds in plaza arrangement, small thin projectile points, elbow pipes, and pottery trowels all come from the Late Baytown of the same region. (Ford and Willey 1941:348).

In other words, there was a gradual “evolution” of the Baytown period into the early Middle Mississippian period. There might have been a few new ideas that filtered in, but in terms of the pottery, mounds, projectile points, and so forth, Ford and Willey saw the basis of (descendent) early Middle Mississippian-period culture as being the preceding (ancestral) Baytown-period culture.

In 1941 the region between Vicksburg, Mississippi, to the south and the Ohio-Mississippi confluence to the north was not well known archaeologically. This is precisely the region to which Ford and Willey were referring in the above quote, and when they wrote their 1941 paper they had little reason to propose an in-situ development of Mississippian culture out of Baytown culture rather than a replacement of the latter by the former. I say this purely because the other geographic areas covered in the paper were, according to Ford and Willey, heavily influenced by population movements and thus reflected cultural replacements. But with respect to the central Mississippi Valley, they hedged. For several reasons, not the least of which was an intellectual stubbornness (O’Brien and Lyman 1998), Ford was convinced that the heartland of Mississippian culture in the Southeast was the central Mississippi Valley. The project that was geared toward providing the evidence for this scenario was perhaps the most significant one ever undertaken in the Southeast—a multiyear program that Ford, Griffin, and Philip Phillips devised in the fall of 1939 and put into effect the following spring. During the course of the nine-year project, crews spent a total of seven months in the field locating and mapping sites, making surface collections, and conducting test excavations. I use a brief discussion of that project to set the stage for examining the legacy of culture history in the central Mississippi Valley as exemplified in McNutt’s *Prehistory of the Central Mississippi Valley*.

THE LOWER MISSISSIPPI ALLUVIAL VALLEY SURVEY

Phillips, Ford, and Griffin published the results of their survey-and-excavation project under the title *Archaeological Survey in the Lower Mississippi Alluvial Valley, 1940–1947* (Phillips et al. 1951). The volume is, as Robert Dunnell (1985:299) put it, “a frank and penetrating discussion of state-of-the-art archaeology in the late 1940s.” To this I would add only that it undoubtedly is the most influential publication ever to appear on southeastern archaeology. The authors of the report were explicit about the objectives of the project: “The purpose of the Survey was to investigate the northern two-thirds of the alluvial valley of the Lower Mississippi River—roughly from the mouth of the Ohio to Vicksburg, Mississippi, an

area long regarded as one of the principal blind spots in the archaeology of the Southeast" (Phillips et al. 1951:v). In particular, the region was a blind spot relative to the origin of Mississippian culture:

There is general agreement among students of Southeastern archaeology that the climax of the late prehistoric cultures is the archaeological facies long recognized under the designation "Middle Mississippi." At a comparatively late date—A.D. 1400–1500 is probably not too late for its peak of development—this culture was firmly established over an immense area. . . . By 1939, when the present Survey was first discussed, an immense amount of data on Middle Mississippi had accumulated, but the problem of its origins and development appeared to be as far from resolution as ever. There was a general impression, shared by many students of Southeastern culture, that this was because the "central" Mississippi Valley, the assumed center of distribution of the culture, had not been sufficiently investigated. (Phillips et al. 1951:39)

Phillips et al. took pains to point out areas of disagreement among themselves, especially over how to interpret sites that had mixed pottery complexes—that is, sites that contained sherds from more than one period/culture. Ford viewed pottery as

developing in a continuum throughout its entire history in the Mississippi Valley, that whether new types evolve by modification of older ones or come in as new ideas from the outside, they take their place in an uninterrupted cultural flow. The logical consequence of such a view is that, in most cases a "mixed" pottery complex represents a single brief span of time on the continuum, an "instant" for all practical purposes, when both elements of the mixture were being made and used side by side. The importance of this postulation for the seriation method can hardly be exaggerated. Ford does not deny that mixed complexes sometimes do result from reoccupation of sites. Such collections he frankly banishes from his graphs and says so. . . .

Griffin and Phillips, on the other hand, while not rejecting the general theory of continuity, are inclined to feel . . . that there are more instances of mixture through reoccupation of sites than Ford has recognized. In particular . . . they have tended to see indications of at least one significant break in the otherwise placid stream of pottery continuity at the point where the tempering material shifts from clay to shell, in other words between the Baytown and Mississippi periods. They feel that, by including mixed collections on the graphs, Ford has effected a spurious transition that seems to prove his continuity hypothesis, but in reality leaves the question open. (Phillips et al. 1951:427)

Here Phillips and Griffin were drawing a line in the sand over the issue of continuity versus replacement. For them, Baytown culture was replaced by Mississippian culture—an interpretation based strictly on the change-over from clay- to shell-tempered pottery. Ford held fast to the notion of cultural continuity. Phillips and Griffin did not have a problem with contemporaneous pottery types *as long as* the types contained vessels tempered with the same material. However, when their excavation levels produced shell-tempered sherds alongside clay-tempered sherds, they immediately suspected the levels were mixed. Sites, if they contained unmixed deposits, should document a sudden changeover from clay to shell. Further, they

strongly suspected that the replacement was the result of ideas or groups from outside the Mississippi Valley. Ford, on the other hand, had little trouble with the notion that several different pottery "types" might coexist at any particular time, regardless of the material used as temper.

Most of the stratigraphic excavations made during the course of fieldwork were problematic for Phillips and Griffin because almost none of them showed the kind of sudden break they were looking for. Most cuts demonstrated that in general shell-tempered pottery replaced clay-tempered pottery, but the answer to one key question was unknown: "Did the pottery of the period we call 'Mississippi' develop locally out of the preceding Baytown Period complex in the Survey Area, or did it 'come in' from somewhere else?" (Phillips et al. 1951:247). Again, the authors set forth their own ideas on the matter:

Ford is inclined to favor the side of continuity. . . . He believes that most (not all) sites exhibiting such cases of Mississippi-Baytown superposition were continuously inhabited through a period of cultural change, represent[ing] in short a "transitional" phase in the cultural development of the area. Griffin, while not denying the role of internal development in the process, tends to look for the principal origins of Mississippi ceramics in sources outside the area, and in the main tends to regard the afore-said superposition of Mississippi over Baytown as evidence of cultural stratification in the ordinary acceptance of the term. Phillips, characteristically, cannot make up his mind. (Phillips et al. 1951:248)

Phillips and Griffin were forced to admit that the evidence from most sites favored the continuity proposition, nothing that "if there was a distinct and separable pre-Mississippi occupation of the site, our excavations do not show it" (Phillips et al. 1951:248). However, there was one site—the last one they tested—that *did* have the force and authority that Phillips and Griffin were looking for. That site was Rose Mound, located in Cross County, Arkansas. A single two-meter-square unit was excavated through the thick midden, producing a sizable collection of sherds. Like at other sites, clay-tempered sherds decreased through time, and shell-tempered sherds increased. And, like at all the other sites tested, there was considerable overlap as opposed to a clear separation of clay-tempered sherds and shell-tempered sherds. But here Phillips and Griffin had a ready answer for this overlap—namely that their horizontal excavation levels had crosscut angled depositional units, thereby inadvertently mixing clay-tempered Baytown sherds in with shell-tempered Mississippian sherds. Once "corrections" were made for sloped deposits by eliminating sherds from five excavation levels, the "real" nature of the depositional history of Rose Mound became clear: "late Baytown followed by early Mississippi with a rather definite break between" (Phillips et al. 1951:289).

A half century of subsequent excavation in the central Mississippi Valley has demonstrated that there was indeed a change from clay-tempered

pottery to shell-tempered pottery, but not an abrupt replacement on the order envisioned by Phillips and Griffin. The important point is that Ford viewed the situation correctly—that is, that there was continuity in the ceramic sequence, irrespective of differences in tempering materials. For example, there are obvious continuities in vessel forms between those made during the Late Baytown period and those of the Early Mississippian period; Ford and Willey had noted these continuities in 1941. As the papers in McNutt's *Prehistory of the Central Mississippi Valley* demonstrate, some modern archaeologists, like Phillips and Griffin before them, continue to equate change in temper with cultural discontinuity.

In retrospect, the Ford versus Griffin and Phillips split is the perfect example of the difference in perspective among culture historians relative to the issue of culture change. Whereas Ford in the late 1940s and early 1950s viewed culture change as gradual and steady—a “placid stream of pottery continuity” (Phillips et al. 1951:427)—Phillips and Griffin conceived of the flow as jerky. This latter view emanated in part from the inherent discontinuity of stratigraphic units (Phillips et al. 1951:428)—thus the breaks in the flow that Phillips and Griffin perceived tended to correspond to stratigraphic boundaries. But this is exactly as Griffin and Phillips had *expected* the archaeological record to be arranged—neat, orderly units that had highly visible discontinuities between them—and they were frustrated in not finding it, at least until they began discarding excavation levels from Rose Mound. Phillips and Griffin's notions of “abrupt cultural change” demanded, in the end, that they view many sites as stratigraphically “mixed” when they showed no such abrupt change (Phillips et al. 1951:291–292).

Ultimately, the “orderly” replacement view forces one away from monitoring the differential persistence of variant forms through time and toward comparing the contents of temporal units and explaining their differences in transformational—historical and ethnological—terms. This perspective sprang directly from the explanatory system implicitly held by culture historians—common sense—that used essentialist categories for analysis. It is, as I discuss below, the sense-making system from which Americanist archaeology has not yet extricated itself.

THE LEGACY OF CULTURE HISTORY IN THE CENTRAL MISSISSIPPI VALLEY

The eight papers included in McNutt's *Prehistory of the Central Mississippi Valley* provide an excellent opportunity to examine the legacy of Phillips, Ford, and Griffin, in addition to that of numerous other southeastern

culture historians. The volume grew out of a conference held in conjunction with the Southern Anthropological Society meeting in Memphis, Tennessee, in 1989. McNutt's purpose in producing the volume was twofold: (1) to update culture histories for various parts of the central Mississippi Valley—southeastern Missouri, western Kentucky, western Tennessee, northeastern and east-central Arkansas, and the upper Yazoo Basin of northwestern Mississippi and (2) to provide some coordination among various culture sequences that have been used to subdivide the archaeological record of the central valley. Lack of a single, unified series of chronological units for the Mississippi Valley has long been viewed as problematic. One of the major reasons Phillips, Ford, and Griffin undertook their survey of the central valley was to expand Ford's chronology for the lower valley northward. Not surprisingly, things north of the mouth of the Red River looked different from those between the Red River mouth and the Gulf of Mexico, and a separate chronology was developed (Figure 1). By 1970, two more chronologies had been developed—Griffin's (1967) chronology for the eastern United States as a whole and Phillips's (1970) chronology for the entire Mississippi Valley below the mouth of the Ohio River (Figure 1). The latter was an attempt to wed the chronologies north and south of the mouth of the Red River.

Most contributors to the McNutt volume, including R. Barry Lewis, comment on the lack of fit between chronologies:

There are few one-to-one correlations between the time slices emphasized in the Griffin and Phillips frameworks, and the archaeological literature of the region contains a certain amount of terminological chaos that reflects the conflicts between them. One finds, for example, confusion about whether the term *Baytown* in a given context refers to part or all of the Early, Middle, or Late Woodland periods; about whether Tchula is properly considered comparable to the Early Woodland, the Middle Woodland, or parts of both; about whether the Poverty Point period in the northern Lower Valley is appropriately considered Early Woodland, Late Archaic, or part of both; and about how one patches the Lower Valley sequence to accommodate the fact that the Coles Creek period, which has strong trait-based connotations in the Yazoo Basin, is meaningless in the Bootheel [of Missouri]. I suspect that everyone who works in this region has felt the frustration of trying to calibrate the two temporal frameworks with sufficient accuracy and precision so that they can get on with their research. (p. 49)

Apparently Robert H. Lafferty and James E. Price escaped frustration by importing the entire southern chronology to their study of southeastern Missouri. Not only did they use Coles Creek as a period, but they also imported Poverty Point and Marksville, along with the mainstays Tchula, Baytown, and Mississippi. They gave no reason for using those period names, though I assume it was because the post-Archaic-period record of southeastern Missouri contains certain elements that to Lafferty and Price point downriver as opposed to upriver. But is the occurrence,

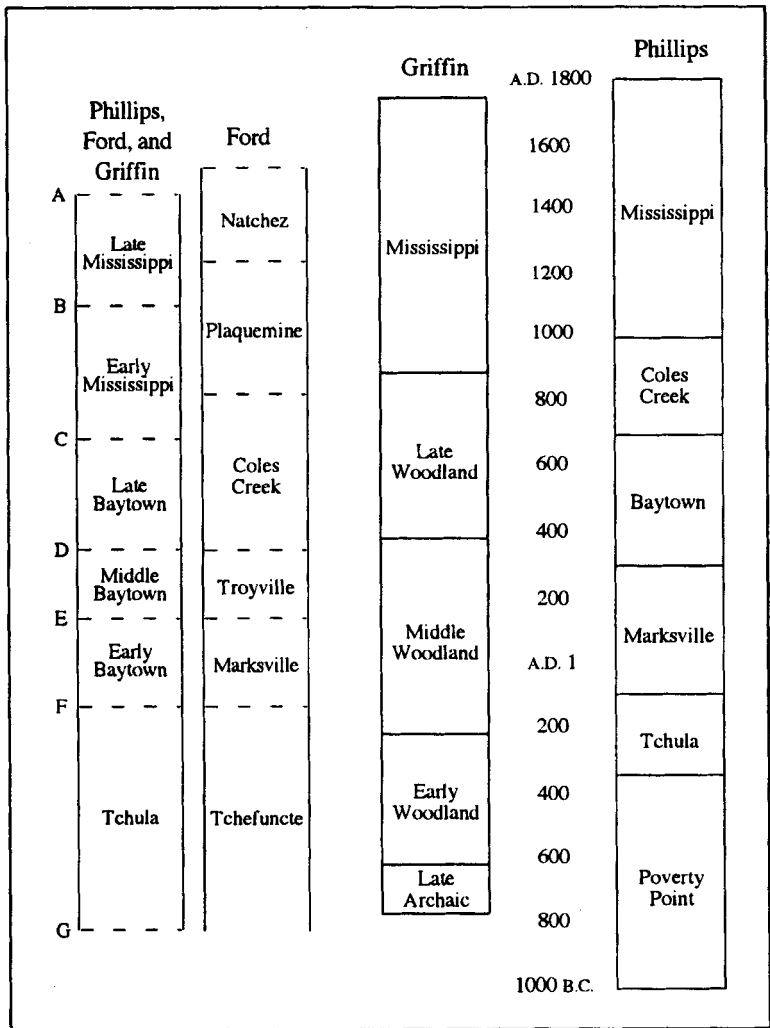


Figure 1. Correlation of four archaeological chronologies that have been used in the Mississippi River valley. The chronology on the far left was based on the survey of portions of the central valley by Philip Phillips, James A. Ford, and James B. Griffin in 1950 (Phillips et al. 1951). They aligned that chronology with a composite based on the work of Ford and others in the lower Mississippi Valley (second column from left). The third column from the left shows the chronological scheme for the eastern United States developed by Griffin (1967), and the column on the far right is the composite master chronology developed by Phillips (1970) for the central and lower Mississippi Valley.

for example, of Poverty Point fired-clay objects at several sites in southeastern Missouri enough of a reason to import a period designation from several hundred miles to the south? Most archaeologists working in the central Mississippi Valley are not going to buy such an import, just as they will not buy the rest of the chronology. The important point here is not that Lafferty and Price assigned names to periods—it does not matter what one calls a particular span of time—but that they arbitrarily used loaded terms from the lower Mississippi Valley without a shred of explanation of why they were doing so.

The trouble has always been that the units within existing chronologies have been treated as more than what they really are—namely, nothing more than *temporal* units. As we saw earlier, culture historians in the Southeast have long played free and loose with terms. The conflation of periods, traditions, and the like crops up in most if not all of the papers in the McNutt volume. Note, for example, what Gerald P. Smith says in his paper on western Tennessee: “Late Woodland and Baytown are here regarded as distinct cultural traditions following [the] Middle Woodland [period] and preceding the appearance of local Mississippian culture. . . . The Late Woodland tradition . . . is regarded as an extension of Midwestern Late Woodland, whereas Baytown is regarded as an extension of the Gulf Coastal tradition” (p. 109). Smith is caught here in a no-man’s land in his use of terms such as Woodland as both a period and a cultural unit (tradition). When he discusses the Middle Woodland, he uses it as a period designation, but when he discusses Late Woodland, he uses it as a tradition designation. One feels a certain amount of sympathy for Smith, because his study area is complex archaeologically. For the period roughly A.D. 1–400, the Middle Woodland period, the archaeological record is relatively homogeneous across the area, but after A.D. 400, things become complicated. One finds pottery that looks very much like contemporary materials to the north—his Late Woodland tradition—but one also finds pottery that looks like contemporary materials to the south—his Baytown tradition. What does one do in such a case? Whatever option one selects, I would think that doing what Smith did—take what traditionally have been period designations and turn them into traditions—would be the least desirable.

Adding to the terminological confusion—not to mention the epistemological confusion—is the phase concept made fashionable by Willey and Phillips in the 1950s (Phillips and Willey 1953; Willey and Phillips 1955, 1958) through their reformulation of the older Midwestern Taxonomic Method unit (McKern 1939). Phases were set up by Willey and Phillips (1958) as temporal-spatial units used to keep track of variation in the archaeological record. Components, from which phases were constructed, were vertically bounded excavation units that represented a phase at a

particular site. The important point is that phases and components were viewed solely as archaeological constructs. Although Phillips and Willey (1953:617) expressed the opinion that "archaeo-sociological correlations may eventually be possible," they suggested that "the archaeologist is on firmer footing... with the conception of an archaeological culture as an *arbitrarily defined unit or segment* of the total continuum" (emphasis added). But even Willey and Phillips, despite issuing such a caveat, forged ahead and made correlations between archaeological and sociocultural phenomena. Components were more or less equivalent to occupations or communities, and phases were "time-space-culture units" equivalent to societies.

The definitive attributes of components and phases were derived from archaeological materials, not imposed on them, and thus those definitive criteria could be modified in the light of new evidence. Of course, this presupposes that modifications would be made. They rarely have been, perhaps because archaeologists working in the central Mississippi Valley believe, like Phillips and Willey (1953:622), that their phases are "remarkably stable." In short, the phases are real cultural units in the sense of an ethnographer's cultures (Lyman et al. 1997). Once they have been identified, why change them?

It would appear that phases, as conceived by Willey and Phillips (1958), can take one of three forms. First, they conceivably could be classes, meaning that there exists a concise set of criteria that define a phase so rigidly that components placed within that phase could be placed in no other phase. Second, phases could be groups, meaning that components placed in a particular phase are more like other components in that phase than they are to components placed in any other phase. And third, phases could be accidents of history, meaning that components are assigned to a phase on some ad hoc, casual basis. When one looks at the dozens of phases that have been concocted for partitioning the archaeological record of the Mississippi Valley, one is forced to conclude that the vast majority, if not all, of them are historical accidents, often constructed on a whim and based on one or a few components.

Lewis, apologetically, states that

In my first attempt to sort out the southeast Missouri chronological muddle, I could do little more than describe the disarray of published phase descriptions (Lewis 1972:38-40). I later learned that this affliction—the desire to save the chronological sequence from the legacy of researchers with long experience in the region—is common among new researchers in the Bootheel. In my case I wrote arrogantly, "These phases are in general poorly described and frequently based on little more than surface collections or perhaps one or two lightly tested components. Most, if not all of the phases which have been formulated for Southeast Missouri have been erected on a rather shaky archaeological foundation of crossdating [and]... a few radiocarbon dates scattered here and there. (p. 75)

Lewis has nothing to apologize for; his words are as true today as they were in 1972: Archaeological phases in southeastern Missouri, as they are up and down the Mississippi Valley, are not even particularly good heuristic devices (Fox 1998; O'Brien 1995). As the papers in *Prehistory of the Central Mississippi Valley* make clear, however, phases are alive and well in the region. They also make clear that there still is a tendency to interchange terms such as period—a temporal unit—and phase—a time-space unit. Lewis, for example, states that his western-Kentucky phases are constantly being referred to as periods by some of his colleagues. Isn't it rather ironic that after all this time archaeologists are still arguing over whether a particular unit is a period or a phase? Willey and Phillips made the distinction clear in the 1950s.

Despite these problems, McNutt's volume probably will stand for years to come as a well-used reference book on the central Mississippi Valley. It contains a wealth of information, idiosyncratic as some of it may be, that will be useful to hardcore researchers in the region. One great value of this book is in underscoring the point that for all the years of work in the region, the central Mississippi Valley is still a "blind spot," just as it was six decades ago when Phillips, Ford, and Griffin began their examination of the Baytown-Mississippian "transition." We certainly know more details about the archaeological record than were known then, but instead of documenting those details and attempting to derive explanations for why they are the way they are, we are forever attempting to cram those details into units that do not work anymore. They might have once served a purpose, but not now. The papers in this volume forcefully demonstrate that at issue is something far more complicated than simply patching the Phillips (1970) sequence for the lower valley to the Griffin (1967) sequence for the upper valley. Rather, they are telling us that the archaeological record is too complicated to be stuffed into decades-old essentialist units. If the last quarter century of archaeology in the lower Mississippi Valley has taught us anything, it is that we are no better off having carved up the record in supposedly finer and finer temporal-spatial units (Fox 1998; O'Brien 1995, 1996; O'Brien and Dunnell 1998).

SUMMARY

The southeastern United States provides an excellent laboratory in which to examine the history of Americanist culture history. It was there, at the hands of such eminent archaeologists as James A. Ford, Philip Phillips, James B. Griffin, and Gordon R. Willey, that culture history reached its full potential, and it was there where the internal contradictions of

culture history were the most noticeable. Culture history had a single underlying purpose—to document the history of development of prehistoric cultures in the Americas—and its central tenet was summed up best by Willey (1953:361): “The objectives of archaeology . . . are approached by the study and manipulation of three basic factors: form, space, and time.” Any field or analytical method that would bring archaeologists closer to studying and manipulating those three dimensions—seriation and percentage stratigraphy, for example—was quickly added to the culture-history arsenal.

In terms of field methods, the immediate roots of southeastern archaeology can be traced to the Midwest, especially to the University of Chicago and Fay-Cooper Cole's influence. As Lyon points out in *A New Deal for Southeastern Archaeology*, the number of students who took part in the University of Chicago field school at the Kincaid site in southern Illinois and who then went on to make important contributions to southeastern archaeology is impressive. Such students included David DeJarnette, James B. Griffin, Stuart Neitzel, and John W. Griffin, the latter of whom provides snippets of information on the Chicago field school in *Fifty Years of Southeastern Archaeology*. Without question, excavation techniques used in the Southeast were in part the result of midwestern influence, but as Lyon also points out, despite the introduction of stratigraphic techniques, the direct historical approach, the Midwestern Taxonomic Method, and other methods and techniques, “These approaches and methods alone would not have transformed southeastern archaeology during the 1930s. The engine of change that combined these elements and transformed southeastern archaeology was relief archaeology” (p. 62).

It is difficult to disagree with Lyon on this point, and yet more or less by the early 1940s the relief effort was mostly over but the intellectual tradition of culture history persisted. In many respects it exists today in the Southeast in unencumbered form, having escaped the radical changes in outlook ushered in by the processual archaeology of the 1960s and the postprocessual archaeology of the 1980s. For example, as so many papers in *Prehistory of the Central Mississippi Valley* make evident, we are still pigeon-holing shell-tempered pottery into one unit (be it temporal and/or cultural) and clay-tempered pottery into another, just as we are referring to pottery tempered with both materials as “transitional.” Lyon is cognizant of this conservative trajectory, and he references a key paper by Dunnell (1990) that examined this conservatism. “One can only speculate,” Dunnell (1990:19) wrote, “that the Southeast's strong association with culture history led it to assume a defensive posture when that paradigm became the brunt of the new archaeological polemic. Or perhaps it was simply an unwillingness to surrender as flawed what just

shortly before had been the crowning achievement of the discipline." Also, students who received their degrees from those who had a heavy hand in contributing to the growth of southeastern culture history tended to work in the Southeast after receiving their degrees, thus carrying on the tradition in which they had been reared. Also, perhaps there is in the Southeast a cultural quirk that has led to the continuance of traditional culture history—what Ian Brown (1994:73) calls "'southern hospitality,' 'joie de vivre,' or 'live and let live' ... [which] has resulted in a generally amiable atmosphere."

As Dunnell and I point out elsewhere (O'Brien and Dunnell 1998), in many respects the archaeology of the central Mississippi River valley has been the victim of its own successes. The efforts first of Ford and then of Ford acting in concert with Willey, Phillips, and Griffin laid the foundation for what became, in the hands of Phillips and Willey (1953; Willey and Phillips 1955, 1958), the method of doing culture history. The concept central to culture history, the archaeological phase, was applied in the valley first and most uniformly. Yet in the succeeding years, many of the innovative features in the Phillips et al. system were lost. That so much of the archaeology of such a large region came to be investigated within this paradigm makes it almost impossible to differentiate between the artificial constructs and the empirical reality they were intended to describe (Fox 1998). Progress became adding new bits to the same old structure instead of reexamining the structure itself. Beginning in the middle 1970s, cultural-resource management demanded a broadening of the archaeological purview, but even this redirection did not carry with it much of a critical examination of some of the underlying presumptions that carried over from the days of culture history. The methods that Phillips, Ford, and Griffin had used to measure time had later come undone at the hands of Phillips (1970) and his successors so that culture history of the 1960s and 1970s was a hodge-podge of ill-advised assumptions and approaches. The situation has not improved, and if anything the number of units such as pottery varieties and phases has increased considerably.

If the reader is interested in the fascinating period of Americanist archaeology during which culture history flourished, I recommend examining the three books reviewed here. *Fifty Years of Southeastern Archaeology* and *A New Deal for Southeastern Archaeology* are solid entry points to the literature of the period, and *Prehistory of the Central Mississippi Valley* will yield striking examples of the culture-history legacy. The critical reader probably will come away with the thought that there must be a better method of explaining the content and context of the archaeological record than by reducing it to a bunch of units based on similarity-dissimilarity or by holding it up as an ethnological analog.

NOTES

1. I gratefully acknowledge my intellectual partnership with R. L. Lyman on matters of Americanist culture history.
2. Actually, Philip Phillips first used the phrase "New World archaeology is anthropology or it is nothing" in an article titled "American Archaeology and General Anthropological Theory" (Phillips 1955:246-247). Phillips took the phrase from Frederic W. Maitland, an English jurist who stated, "My own belief is that by and by anthropology will have the choice between being history and being nothing" (Maitland 1936:249).
3. Properly termed the Midwestern Taxonomic Method, of which W. C. McKern (e.g., 1939) was the chief architect.

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