

# Introduction

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The publication in 1958 of *Method and Theory in American Archaeology* by Gordon R. Willey (Figure 1) and Philip Phillips (Figure 2) marked a turning point in Americanist archaeology. After half a century of being studied in its many and varied manifestations, the archaeological record of the Americas was sufficiently well known to permit the synthesis of hemispheric prehistory outlined in the book. Further, that same half century had witnessed the emergence and perfection of what came to be known as the culture-history paradigm (Lyman et al. 1997)—a set of methods and techniques for studying and analyzing the archaeological record and writing the history of cultural lineages. Willey and Phillips's book is a synopsis of their views on how to write that history and in some respects is a post hoc summation of the programmatic portion of the paradigm. But it is also a significant departure from what came before it in terms of the manner in which the authors chose to synthesize and make sense of the record. Although it is seldom acknowledged, that model came to influence virtually all subsequent efforts in Americanist archaeology.

Several questions come to mind as one reads this classic volume on culture history. What caused Willey and Phillips to produce the book in the first place? What prompted them to try a new framework for synthesizing the archaeological record of the Americas? Where did that framework come from? How did the



Figure 1. Gordon R. Willey, 1946, in the Viru Valley. Photograph courtesy of Gordon R. Willey.



Figure 2. Philip Phillips, circa late 1940s, in the field in the southeastern United States. Photograph courtesy of Gordon R. Willey.

discipline come to the point where, in Willey and Phillips's view, such a book was not only possible but in some sense necessary? Did the book really have a significant influence on the growth of the discipline, and if so, how? It is with the intent of answering these questions that we offer this introduction.

#### PRELUDE

In 1947 Americanist archaeology saw the publication of Paul S. Martin, George I. Quimby, and Donald Collier's book entitled *Indians before Columbus: Twenty Thousand Years of North American History Revealed by Archeology*. Although their estimate of the temporal remoteness of the first human occupation of the continent indicated in the subtitle has yet to be demonstrated, the book marked a significant point in the history of American archaeology. Sufficient data from many areas were available to allow a sort of grand summation, if not a synthesis, of continental prehistory. This volume had been preceded by various summaries of what was known about the prehistory of several regions of North America (e.g., Ford and Willey 1941; Griffin 1946, 1952; Kidder 1924), but it was the first book-length effort focused on the entire continent. Why was such a synthesis possible? How could it be fine-tuned and perfected? How, to borrow part of Martin, Quimby, and Collier's subtitle, could archaeology be made to "reveal history"?

Although they did not intend their book to be a text on archaeological methods, Martin, Quimby, and Collier (1947:3-13) addressed the last question by outlining the basics of those methods. They began by noting that archaeology was "a part of [anthropological] science" (p. 3)—a notion later repeated by Willey and Phillips (1958). They defined *culture* "in an anthropological sense [as embracing] the sum total of human behavior and activities which are handed on by precept, imitation, and social heritage. This includes all customs, habits, usages, attitudes, beliefs, religious and political ideas, and material products, such as methods of building houses, of manufacturing all kinds of artifacts (weapons, pottery, ornaments, baskets, cloth), of planting and har-

vesting” (p. 5). In describing archaeological excavation, they noted that it consists of “peeling or stripping down the site, layer by layer. . . . down to the bottom” (p. 7). A site should be excavated “like removing the layers of a cake” (p. 7), because “study of the position and order of sequence of the layers in which prehistoric remains are found “allows the construction of a cultural chronology, the presumption being that “the lowest or bottom layer of a deposit is the oldest and the top layer the most recent. It follows that the tools and other remains found in the bottom layer are older than those from the middle layer and that the latter are older than objects from the top layer” (p. 8). Of course, the last part of the statement is not necessarily true—hence the term *reversed stratigraphy* coined by archaeologists a decade earlier (Hawley 1937)—but it captured the general belief within the discipline that deeper recovery contexts meant older artifacts (Lyman and O’Brien 1999; O’Brien and Lyman 1999a).

Martin, Quimby, and Collier (1947) indicated that recovered materials must be classified: “The archeologist sorts his materials, placing like with like, and then makes comparisons with other similar or identical materials from near-by sites” (p. 8). After classification, “the archeologist can then determine whether one or more cultures are represented . . . and whether some materials are older than others” (pp. 8–9). But how does one integrate data from multiple sites? In particular, how does an archaeologist build a regional chronology spanning a long duration of time from several shorter, partial chronologies, each from a different site within the region? Martin, Quimby, and Collier were writing on the eve of the advent of radiocarbon dating, so they could not simply align several site-specific chronologies on the basis of temporal similarities. They were clear on the data-integration procedure and their opinion of it:

The chronological correlation of several prehistoric sites or of two or more stratigraphic sequences in different re-

gions is called *cross-dating*. The least satisfactory method of cross-dating is by means of typological comparison. In this comparison it is assumed if two prehistoric peoples within a given area shared a number of highly distinctive traits, such as forms of tools and styles of decoration, they lived at about the same period. . . .

Two stratigraphic sequences may be tied together if a single cultural period or phase is found to be common to both. For example, if Period A, the youngest in a sequence of four periods, is found at the bottom of another sequence of three periods, then the two sequences may be combined to give a complete sequence of six periods. (Martin et al. 1947:9–10)

The authors provided an accurate if incomplete and terse synopsis of archaeological methods. They did not explore the epistemology underpinning the methods, but that was not their purpose. They also summarized what was known of continental prehistory region by region, but they provided almost no explanation for why regional chronologies appeared the way they did. Instead, they arranged prehistoric cultures in a chart showing their spatio-temporal distributions (Martin et al. 1947:513–520). Within a decade these deficiencies would be resoundingly dealt with by Willey and Phillips, first in a set of three papers and then in a reprinting under one cover of these same but revised papers. In our view the publication of those papers in general and that volume in particular were significant events in the history of Americanist archaeology because their appearance in the literature marked a turning point in the discipline presaged by Martin, Quimby, and Collier's summary. That summary denoted a shift from a focused effort on the building of cultural chronology—sometimes referred to as “culture history”—to doing something anthropological with the chronologies available. How did Americanist archaeology come to this position?

## HISTORICAL BACKGROUND

The history of Americanist archaeology (Lyman et al. 1997; O'Brien 1996; O'Brien and Lyman 1998; Willey and Sabloff 1993) was such that archaeologists were—and still are—trained as anthropologists. This was in large part because the archaeological record was initially viewed as a temporally brief extension of the ethnographic record of the Americas into the past. One result of this view was the use of what came to be known as the direct historical approach (Steward 1942; Wedel 1938). Temporal sequences comprising a cultural lineage were built up by beginning with a list of the cultural traits of a historically documented culture and then working back ever deeper into the past by looking for archaeologically represented cultures that shared traits with the historically documented culture in the same geographic area. The assumptions were that (1) the number of traits shared with the recent culture became progressively fewer as the traits in successively older cultures in a lineage were polled, and (2) the overlapping of traits across several temporally contiguous cultures in a sequence linked them together to form the evolutionary lineage (Lyman and O'Brien 2000).

The direct historical approach had been used in Americanist archaeology for decades, perhaps evident most clearly in the work of Cyrus Thomas's (1894) Division of Mound Exploration within the Bureau of (American) Ethnology (O'Brien and Lyman 1999b). Constructing cultural lineages using the method did not require extensive time depth, and early on it was comforting for many (but not all) Americanist archaeologists to believe the North American record did not have much time depth (Meltzer 1983), because greater depth increased the chances of evolutionary convergence (Steward 1929). When the bottom dropped out of the perceived shallow time depth with the 1927 discovery of the Folsom site in northeastern New Mexico (Figgins 1927), the direct historical approach began to be overshadowed in some quarters by other methods of measuring time, several of which had been around for more than a decade.

Shortly after 1910 artifacts began to be classified in ways that allowed the measurement of time's passage (Lyman and O'Brien 1999; O'Brien and Lyman 1999a), either through the use of what came to be known as frequency seriation (e.g., Kroeber 1916) or through stratigraphic excavation (Kidder and Kidder 1917; Nelson 1916; Spier 1917). Various techniques for presenting and graphing such data were developed (Lyman et al. 1998), and numerous cultural chronologies for various areas were described between 1920 and 1950. These are what formed the heart of Martin, Quimby, and Collier's (1947) book. Always lurking in the background, however, was a concern for the cultural and anthropological implications of the culture histories being constructed by archaeologists (e.g., Strong 1936). These, it was thought, could be pursued after, but apparently *only* after, cultural sequences were constructed. As Julian Steward (1949:2) noted, "the conviction is widely held that the discovery of cultural laws is an ultimate goal of anthropology, to be attained when fact-collecting and detailed analyses of particular cultures *and sequences* are sufficiently advanced" (emphasis added).

Given the amount of time consumed in constructing cultural sequences, which left little time for the pursuit of anthropological issues, archaeologists might have felt as if their chosen field were "relegate[d] to the position of the tail on an ethnological kite," although perhaps "an extraordinarily long tail" (Steward 1942:341), which we suspect meant that archaeologists served an important function for anthropology by providing time depth to ethnological research. Archaeologists were always aware of their allegiance to and alliance with anthropology. As Roland Dixon (1913:558) noted early in the history of Americanist archaeology, "archeology is but prehistoric ethnology and ethnography." Several years later George Vaillant (1935:304) put it this way: "Unless archaeology is going definitely to shift from a branch of anthropology to an obscure type of mathematics, an effort must be made to relate the rhythms of cultural development with the pulsations of an evolving human society." And two years later Fay-Cooper Cole

and Thorne Deuel (1937:1) stated, "The avowed aim of archaeology is to make the past live again."

But given that archaeologists seemed to focus more on chronological issues than on anthropological ones, anthropologists felt compelled to remind archaeologists of their larger goal and to suggest ways to attain it. Steward and Frank Setzler (1938), along with John Bennett (1943), argued that archaeologists should concern themselves more with the functional significance of artifacts. This comprised determining not just the purpose(s) particular artifacts served but also their roles within a culture as a dynamic, operational entity. As Steward and Setzler (1938:6-7) phrased it, archaeology "can shed light not only on the chronological and spatial arrangements and associations of [cultural] elements [or traits], but on conditions underlying their origin, development, diffusion, acceptance, and interaction with one another. These are problems of cultural process, problems that archaeology and ethnology should have in common."

But how were archaeologists supposed to address such issues? Simply put, through the use of ethnographic analogy. As Paul Martin (1939:467) so eloquently put it:

Everyone is aware of the fact that it is impossible to explain and to give absolute meaning to all the discoveries which are made while digging ancient villages. All we can do is to interpret what we find in the light of our knowledge of modern Pueblo (Southwestern) Indians. In this way, it is possible to moderate our conjectures, and piece them together by means of reasonable imagination. Thus, the cold, unrelated and often dull archaeological facts are vivified and the reader may have some sort of reconstruction in his mind's eye of what the Basket Maker Indians were like and how they lived.

A few years later James Griffin (1943:340) was more terse but also more explicit when he remarked that the "interpretations made by archaeologists are inferences based upon similar materials used in an analogous but not identical cultural group."



We suspect the perceived utility of ethnographic analogy was reinforced by the use of the direct historical approach. For either usage—as a chronological tool or as an interpretive tool—the ethnographic record provided a source of information on dynamic cultures rather than simply a list of static culture traits (e.g., Bullen 1947). Thus Fred Eggan (1952:38) remarked that for “certain recent archeological cultures the direct historical method, once valid connections are established, offers an avenue by which late manifestations may be enlarged through inferences from ethnological horizons.” And Thomas Kehoe (1958) explicitly referred to his method as the “‘direct ethnological’ approach” when he used ethnographic data from the Great Plains as an analog to determine the function of rings of stones commonly found in the northern Great Plains as “tipi rings.”

Initially, few culture historians made extensive use of the ethnographic record as a tool for interpreting in anthropological terms the artifacts they recovered. Instead, most of them focused closely on building chronological sequences of cultures and worried little about what those sequences might signify in terms of what would become known in the 1950s as cultural processes and patterns. Martin, Quimby, and Collier's (1947) volume signified not only that by the mid-1940s sufficient chronological information was available to permit a continent-wide summary but that the time was ripe to forge ahead to the loftier goals of ethnology. The next year Walter Taylor (1948) published his scathing criticism of culture history, pointing out that archaeology was not being sufficiently anthropological. To put Taylor's comments in perspective, we need to back up a few years and read what his adviser at Harvard, Clyde Kluckhohn, had to say about Americanist archaeology:

I should like to record an overwhelming impression that many students [of prehistory] are but slightly reformed antiquarians. To one who is a layman in these highly specialized realms there seems a great deal of obsessive wallowing in detail of and for itself. No one can feel more urgently than

the writer the imperative obligation of anthropologists [particularly archaeologists] to set their descriptions in such a rich context of detail that they can properly be used for comparative purposes. Yet proliferation of minutiae is not its own justification. (Kluckhohn 1940:42)

In Kluckhohn's (1940:49) view, "the antiquarian interest . . . makes the collection of artifacts an end in itself." His comments were explicitly directed toward an apparent deep concern by archaeologists with artifacts at the expense of the epistemology underlying their typologies such that "pottery classification in various areas in the New World [was] a welter of confusion" (Kluckhohn 1940:47)—something acknowledged even by archaeologists (e.g., Roberts 1935), although for different reasons. Kluckhohn (1940:43) predicted that were Americanist archaeology to continue on its present course, its practitioners would "find themselves classed with Aldous Huxley's figure who devoted his life to writing a history of the three-pronged fork." Kluckhohn (1940:51) cited with favor Steward and Setzler's (1938) recent paper "Function and Configuration in Archaeology" and argued that archaeologists should be searching for "uniformit[ies] in human behavior," for "generalities of human action," and for "processual integrations" of these (Kluckhohn 1940:50), but he did not indicate in an explicit manner how these lofty goals were to be attained.

Taylor completed his doctoral dissertation in 1943 and published a revised version as *A Study of Archeology* in 1948. It was privately printed again in 1964 and subsequently reprinted in 1967, 1968 (with a new foreword by Taylor), 1971, and 1973 by Southern Illinois University Press. It was reprinted yet again in 1983 with a new foreword by Patty Jo Watson. The volume was, as Watson (1983:x) noted, an attempt by Taylor to "dislodge Americanist archeologists of the 1940s from their preoccupation with time-space systematics, and to encourage them to make more and better use of their data. . . . [In Taylor's view] archeology should

be a means of recovering *cultural contexts* that are as full-bodied as possible." Taylor had echoed the earlier lament of his adviser (Kluckhohn 1940) and expressed the same desire for Americanist archaeology to become more anthropological. But unlike Kluckhohn, Taylor suggested a way to attain such a goal—what he termed the "conjunctive approach." Reviewers of Taylor's book tended either to focus on the derogatory and inflammatory language he used to characterize such luminaries as A. V. Kidder (e.g., Woodbury 1954; but see Daniel 1951), or to outline what they perceived to be contradictions internal to his suggested method for attaining anthropological goals (e.g., Burgh 1950). Taylor (1950) responded to the latter by noting that archaeologists were mere technicians who could use their data to do either history, which they commonly did, or anthropology, which they seldom did.

In our view, one thing the reviewers did not sufficiently emphasize was that Taylor's plea for an explicitly anthropological archaeology had been made before and on more than one occasion (e.g., Bennett 1943; Bullen 1947; Steward and Setzler 1938). They did not remark on the fact that in some respects Taylor's proposed "conjunctive approach" was little more than dressed-up ethnographic analogy. Nor did they mention that there was no indication in Taylor's discussion of how the archaeological record was to be *explained* other than to use the old standby cultural processes observable in modern societies—diffusion, migration, innovation, and the like. These facts, in combination with Taylor's failure to illustrate by example how the conjunctive approach was supposed to work, resulted in minimal positive feelings regarding his efforts until the 1960s (Taylor 1972). But even then, Taylor's characterization of an archaeologist as a mere technician was repeated. As Elman Service (1964:364) noted, "The day-to-day work of the archaeologist (as an *archaeologist*, not as an archaeologist turned general anthropologist or philosopher) is to dig up remains of peoples and their cultures, to map, measure, describe, count, and so on, and in his report to make an interpretation of what life was

like 'then.'" An archaeologist was still perceived by "general anthropologists" (to borrow Service's words) as a technician who served anthropology's needs, though as an ethnographer of the past. In looking back to the middle of the twentieth century, Willey (1984:10) observed that his "ethnological and social anthropological professors and colleagues . . . were the people who controlled the core of theory, and, unwittingly or not, they let us feel that archaeology was something second rate. . . . [A]rchaeologists did not have a high intellectual rating on the American scene." In our view, there was no effort on the part of archaeologists of the 1950s to argue for an archaeology for archaeology's sake; in fact, the role of archaeology as the handmaid of anthropology was reiterated by archaeologists (e.g., Strong 1952).

Some archaeologists did, however, grasp the larger message in Taylor's and Kluckhohn's discussions. Two of them—Gordon R. Willey and Philip Phillips—are the subject of this volume. They, like many of their contemporaries, were in a way preadapted to what Taylor was saying, and both had been doing anthropology long before Taylor came along. Archaeology was a part of anthropology, not the other way around, and archaeologists had been working toward the kind of goals Taylor, Kluckhohn, and others thought were important. But again, most archaeologists believed that attainment of those goals had to be preceded by the construction of what were variously termed cultural sequences or chronologies (see Chang [1958] and Meggers [1956] for early statements, and Sabloff and Willey [1967] and Woodbury [1972] for later statements of this perspective). That such chronologies could be constructed, tested, and refined or rejected placed the construction of cultural chronologies well within the realm of scientific inquiry—postulated sequences could be shown empirically to be incorrect. In short, they were testable. This contributed to the attractiveness of what came to be known as the culture-history paradigm, despite some characterizing it as being of an "essentially dull and uninteresting character" (Caldwell 1959:304). Taylor, Kluckhohn, and others of like mind did not make it clear how

one was to test the validity of the apparent *anthropological* significance of a prehistoric culture or cultural lineage, nor did they indicate how one might explain why a lineage had the particular form that it did. It was with these (and other) thoughts in mind that Willey and Phillips began to work on their landmark volume reprinted here. To understand how they came to the point of view they held in writing the book, we briefly examine the backgrounds of the two authors.

## THE AUTHORS

Philip Phillips (1900–1994) had an early interest in American colonial history, and when the stock market crashed in 1929 he turned from a career in architecture, having earned a master's degree from Harvard University in that subject in 1927, to anthropology and archaeology (Willey 1996). Interest in the latter topics had probably been nurtured during Phillips's graduate student days by close friendship with Singleton Moorehead, son of archaeologist Warren K. Moorehead at the R. S. Peabody Foundation in Andover, Massachusetts. One of Singleton's friends was Mesoamerican archaeologist George C. Vaillant, who became a friend of Phillips. It was at Vaillant's urgings that Phillips returned to Harvard to work under Vaillant's former professor Alfred M. Tozzer. Despite Tozzer's efforts to interest him in Mesoamerican archaeology, Phillips focused on the southeastern United States in order to stay near his wife and children (Willey 1996). Roland Dixon served until his death in 1934 as Phillips's "first mentor" (S. Williams, personal communication).

Phillips (Figure 3) received an appointment to Harvard's Peabody Museum in 1937, where he served in various capacities until his death. After completing his doctoral dissertation on the archaeology of the lower Mississippi Valley (Phillips 1939; the doctoral degree was awarded in 1940), he, along with James Ford and James Griffin, began a decade-long survey program that resulted in the landmark volume *Archaeological Survey in the Lower Mississippi Alluvial Valley, 1940–1947* (Phillips et al. 1951). That

his thoughts on archaeological method and culture change differed from Ford's were spelled out at some length in this monograph and in a follow-up volume (Phillips 1970), where he attempted to set the record straight on the differences of opinion (see O'Brien and Lyman [1998] for our view of these differences).

Phillips held the view that one should build a chronology of "cultures"—archaeology was, after all, anthropology (Phillips 1955)—and to build cultural chronologies required stratigraphic excavation. To Phillips, culture change was not a more-or-less smooth continuum of changes in the relative frequencies of artifact types (Ford's view), but rather a series of periods of stasis marked by relatively abrupt events of change from one suite of artifact types to another that marked the temporal boundaries of distinct cultures. Temporally successive cultures—equivalent to evolutionarily stable states—were stacked one on another on the basis of stratigraphic superposition to construct a chronological sequence of cultures. This view permeated his efforts (aided by Stephen Williams [Phillips 1970:32, fn 3]) at pottery typology, as evidenced in his important two-volume tome, *Archaeological Survey in the Lower Yazoo Basin, Mississippi, 1949–1955* (Phillips 1970), wherein he devoted considerable effort to show that culture change as manifest in artifact change was discontinuous and often abrupt.

Gordon Willey (1913– ), having "decided upon archaeology as a career at a very early age" (Willey 1984:6), completed his master's thesis (Willey 1936) at the University of Arizona under the guidance of Byron Cummings. In it Willey (Figure 4) summarized Americanist archaeological method, reasoning, he later indicated, that by better method—particularly, "how to dig" (Willey 1984:6)—many of archaeology's "principles" would be revealed. He then worked for a time with Arthur R. Kelly in Georgia, focusing on dendrochronology (Willey 1938), and later with Ford (Ford and Willey 1940, 1941) in Louisiana. He enrolled at Columbia in 1939, choosing William Duncan Strong as his major professor, and completed his doctoral dissertation in 1943. Willey did considerable work in Georgia and especially Florida, producing several notable articles (e.g., Willey 1939; Willey and



Figure 3. Philip Phillips, circa 1960. Photograph of a portrait by Phil's son Bradley Phillips, courtesy of Gordon R. Willey.

Woodbury 1942) as well as two major monographs (Willey 1949a, 1949b). He also worked in South America during the 1940s, which provided data for his dissertation as well as material for related publications (e.g., Willey 1945, 1948), several of which anticipate the results of his later collaborations with Phillips.

Based on his work in South America (Willey 1945), Willey introduced the concepts "horizon style" and "cultural tradition" to North America archaeology, both of which would play major roles in his and Phillips's writings in the 1950s. Further, Willey was interested not only in cultural chronology, as might be surmised from even a quick reading of his early monographs (Willey 1949a, 1949b), but also in the anthropological aspects of the archaeological record, as indicated by his and Woodbury's (1942:236) comment with respect to a "time chart" they constructed for north-western Florida: "It is hoped that the 'periods' will prove to repre-



Figure 4. Gordon R. Willey, circa 1990.  
Photograph courtesy of Gordon R. Willey.

sent distinct cultures when the bare skeleton of a ceramic chronology has been given flesh and body in the form of a full and 'functional' culture description."

Willey worked for a time in the 1940s with Julian Steward at the Bureau of American Ethnology, helping him prepare the *Handbook of South American Indians* for publication. Willey's remarks on his interaction with Steward, recorded some forty years later, are worth noting:

[Steward] had a strong culture evolutionary orientation, and in his attempts to achieve an overview grasp of the multitudinous South American *Handbook* data, both ethnographic



and archaeological, he leaned in this direction ever more strongly. At the same time, he was uncomfortable with evolutionary thinking in the abstract. He wanted to explore and compare the diverse lines of culture history. It was in my many long discussions with Steward that he turned me toward settlement archaeology and, specifically, to the settlement pattern study in the Viru Valley. It should be made clear, however, that the study, as it eventually appeared (Willey 1953b), was entirely my own designing. This is not said to detract from Steward's influence, but after the gathering of the field data in 1946, under his very general guidelines that archaeologists should stop being so single-site oriented, and should try to see man's adaptations to natural and social environments over wider landscapes, I was left on my own. (Willey 1984:10–11)

Willey's collaboration with Steward provided him with a model—cultural evolution—that would contribute in a major way to the significance of *Method and Theory in American Archaeology*.

Both Phillips (e.g., Ford et al. 1955; Phillips et al. 1951) and Willey (e.g., Ford and Willey 1941) had worked with Ford, and in fact Ford (1949) constructed the chronology on which Willey's (1953b) settlement-pattern study rested. Curiously, the processual archaeologists of the 1960s and 1970s applauded Willey's efforts in the direction of establishing settlement-pattern studies but did not mention Ford's contribution (e.g., Parsons 1972), despite Willey's (1953b:33–37) thorough acknowledgment of it. Both Willey and Phillips respected Ford while simultaneously disagreeing with him on various key points, and they dedicated their 1958 book to him “with sincere respect” (Phillips 1970:3), despite the fact that, as Phillips (1970:3) later noted, they expected “that [Ford] would disagree with everything it tried to say.” Ford, so far as we have been able to determine (O'Brien and Lyman 1998), never commented in the published record on the strengths or weaknesses of *Method and Theory in American Archaeology*. We suspect he actually would have found little to quibble with in terms of method—

what Willey and Phillips termed “horizon styles” were equivalent to what Ford called “marker types” (O’Brien et al. 2000)—although he probably would have disagreed with their view, held more strongly by Phillips than by Willey, that temporal sequences comprised empirically real cultures. In Ford’s view, time and culture change were continua, although the latter can vary in rate, and any chunks an archaeologist carved out of them were arbitrary rather than real (O’Brien and Lyman 1998, 1999a).

#### THE PAPERS

Willey and Phillips met in Georgia in 1937, and they collaborated on a short paper published in 1944 (Willey and Phillips 1944). Willey took a position at Harvard in 1950, and thus he and Phillips became professional neighbors. Phillips had just finished the Lower Mississippi Alluvial Valley volume (Phillips et al. 1951), and Willey had recently completed his Florida Gulf Coast volumes (Willey 1949a, 1949b) and was working on his Viru Valley materials (Willey 1953b). All of these monographs concerned placing sites in proper chronological sequence and explaining the culture-historical developments indicated. Looking back forty years later, Willey (1996:41) remarked that “although I can’t remember just how it all came about, [Phillips and I] must have talked over the ways in which American archaeologists, particularly North American archaeologists, had carried out archaeological classifications and systematics. As a result, we did two papers for the *American Anthropologist*, setting out our views about such matters.” Willey (1996:41) indicated the appearance of these two papers “prompted an invitation from a university press to combine these into a book, and *Method and Theory in American Archaeology* (1958) was the result.” Because the papers, in revised form, comprise the volume, it is important to know what they contain.

#### *An Operational Basis for Culture-Historical Integration (1953)*

In their first paper, “Method and Theory in American Archaeology: An Operational Basis for Culture-Historical Integration,”

Phillips and Willey (1953) discussed how to classify aggregates of artifacts—what they termed components—and how to link those units to form regional chronologies. It had long been axiomatic in Americanist archaeology that typological similarity was an indicator of cultural relatedness (e.g., Rouse 1939), and thus “a common or similar history” for units judged to be similar was automatically implied (Willey 1953a:363–364). Phillips and Willey (1953) would define two kinds of archaeological units—horizons and traditions—that explicitly held such phylogenetic implications.

Willey had elsewhere drawn a contrast between what he termed two archaeological theories:

[T]heories of culture change and continuity are fundamental to archeological studies. . . . [T]he treatment of archeological assemblages in any historicogenetic system [e.g., Colton 1939; Gladwin and Gladwin 1934] has a basis in theories of continuity and change. Even if space and time factors are not formally observed, principles of continuity and change are expressed in the degrees of trait likeness or unlikeness which are the mechanics for establishing the genetic lines binding the assemblages together. . . .

The processes by which, or through which, cultural continuity and change are maintained or accomplished have not received study and reflective thought commensurate with the way these concepts have been invoked by American archeologists. “Evolution” and “diffusion” have been tag names employed, but these are broad categories rather than specific explanations, and there have been few clear theoretical formulations along these lines. . . . [Concerning the Gladwin and Colton schemes] there is a rather simplistic evolutionary or genetic analogy at work here. To be sure, there is some universal basis for expressing the development of human culture in this fashion. . . . [But in some cases] it appears that evolutionary theory has been very naïvely applied. . . .

Diffusionist theory in American archeology has probably received more analysis, or analytical speculation, than has

evolutionist theory. It is at the core of most archeological interpretation. Trade, migration, gradual borrowing, and idea or stimulus diffusion have all been advanced in specific instances. . . . As with evolutionary hypotheses, theories of diffusion may be legitimately brought forward to explain various patternings in space-time distributions. (Willey 1953a:368–369)

Phillips and Willey (1953:617) suggested that “an archaeological culture is an arbitrary division of the space-time-cultural continuum” and followed their predecessors and contemporaries in conceiving of culture history as a constantly flowing yet braided stream. They began their discussion by noting that any archaeological unit is “the resultant combination of three unlike basic properties: space, time, and form” (Phillips and Willey 1953:617). This was followed by explicit definitions of three spatial units of different scales—*locality* (often a site), *region*, and *area*—the first two of which they aligned tentatively with a community (or local group) and a tribe (or society), respectively. Their archaeological units had, they hoped, some reality in a social or anthropological sense that could perhaps be discovered. Their formal units began, not surprisingly, with *components* and *phases*. The latter was chosen over W. C. McKern’s (1937, 1939) *focus* because Phillips and Willey preferred the “stronger temporal implication” of *phase*—a point Harold Gladwin (1936) had made earlier with respect to the use of systematics in the Southwest.

A component was approximately equivalent to the sociological units of band, neighborhood, and village, and a phase was approximately equivalent to a society. This alignment of archaeological units with sociocultural ones served as a commonsense warrant for the archaeological units. This means that the units had some objective and empirical reality. Following McKern (1937, 1939), Gladwin (1936), and Harold Colton (1939), Phillips and Willey (1953:619) defined a component as “the manifestation of a given [phase] at a specific site.” A phase was conceived of as being tem-

porally, spatially, and formally “sufficiently characteristic to distinguish it from all other units similarly conceived” (Phillips and Willey 1953:620).

Components could be “combined into phases because analysis reveals cultural uniformity amounting to practical identity” (Phillips and Willey 1953:630). Unlike in the *Midwestern Taxonomic Method* (McKern 1937, 1939) but following those working in the Southwest (Colton 1939; Gladwin 1936), a Phillips-and-Willey phase could be defined on the basis of a single component, although in practice this was apparently seldom the case (e.g., Phillips et al. 1951; Willey 1949a). The necessary and sufficient conditions for membership in a phase could then be modified on the basis of newly discovered components. For example, when several local sequences—where “a local sequence is a series of components found in vertical stratigraphic relationship in a single site” (Phillips and Willey 1953:623)—were correlated, the correlation was generally “accompanied by a progressive generalization of the definitions [of phases] until their original usefulness is impaired. . . . [I]n the process the original formulations [of local sequences] are retailored to fit the wider spatial and (perhaps) deeper temporal requirements” (Phillips and Willey 1953:623). Phases seem to be units that are largely dependent on the first component or set of components examined.

But how were sets of things to be identified so that their similarities could be measured? Phillips and Willey’s first requirement was to identify the boundaries of the sets. What helped here was the suggestion that “a site or a level within a site” could be a single component and thus represent a phase (Phillips and Willey 1953:620). Hence spatial boundaries, whether naturally or arbitrarily defined, dictated what went into a component and thus into a phase; all artifacts within the boundaries were included within the sets of things to be compared. In other words, phases were classes (conceptual units) of components, and components were empirical manifestations of phases. Thus one must have the phases in order to recognize or identify components, but one needs com-

ponents to write the phase definitions. The result is that a phase can be defined on the basis of one component. This is acceptable given Phillips and Willey's warrants for their units: Phases are real because they are at least approximately equivalent to real societies, as required by their notion "that you cannot hope to shed light on processes by means of abstractions that have no theoretically possible counterparts in *cultural and social 'reality'*" (Phillips and Willey 1953:629; emphasis added). They recognized that their "theory" was not theory in the usual sense but rather a hypothesis concerning a possible relation between an archaeological unit and an ethnological unit. And, as with the components and foci of the Midwestern Taxonomic Method, the only way Phillips and Willey's components could provide definitive criteria for their phases was if the continuum of culture change over time was somehow discontinuous. Otherwise, even a component of short duration might be a mixture of an ancestral and a descendent phase.<sup>1</sup>

What precisely, then, is a component? That a component is supposed to be equivalent to a band, neighborhood, or village provides a clue. It is, as in the Midwestern Taxonomic Method, the manifestation of a single occupation; thus it is a set of temporally, spatially, and behaviorally associated artifacts. As Willey (1953a:363–364) noted, "the concept of the assemblage is implicitly grounded in the historical validity of the artifact-feature complex as a *unit*. Such a unity, by the very nature of its internal associations, bespeaks spatial-temporal correlates. . . . [T]here is [therefore] covert historical theory in the assemblage concept. . . . The unity of the assemblage, if historical unity can be assumed, must lead to the conclusion that we are dealing with the remains of an integrated cultural complex in the case of the *component*." The locations of the temporal and spatial boundaries of an assemblage are dictated by stratigraphy; stratigraphic boundaries are empirical and therefore do not require theoretical specification. As Irving Rouse (1953:59) noted, stratigraphic excavation produces collections of artifacts that "are segregated by layers of refuse and subdivisions thereof, in order that the succession of *occupations* may

be determined" (emphasis added). This echoed earlier remarks made by Clark Wissler (1917), A. L. Kroeber (1925), and others that a single continuous occupation will occur in an unstratified deposit, whereas multiple, nonoverlapping occupations will each occur in a separate stratum. The creation of artificial boundaries, such as when one excavates in arbitrary levels, was to be avoided in order to "prevent contamination of one classified culture type by another" (Phillips 1942:200).

To Phillips and Willey, the apparent stability of phases was not totally an artifact of the geological boundaries of their member components. The "material traits [of a phase] can, under certain circumstances, be remarkably stable" (Phillips and Willey 1953:622). One could construct local sequences, regional sequences, and period and area chronologies that increase the spatial scale and "still maintain contact with the primary stratigraphic data" (Phillips and Willey 1953:624). This contact was maintained by choosing particular artifact types variously termed index fossils or marker types that were definitive of a phase and using the stratigraphically delimited beginnings and endings of these types' durations as phase boundaries. The braided stream of interacting cultural lineages no longer flows gradually and continuously; its rate of flow is generally slow but punctuated by temporally brief, fast stretches. This notion that there are periods of stasis in the flowing braided stream of cultural history is one significant legacy of the culture-history paradigm (Plog 1973).

*Historical-Developmental  
Interpretation (1955)*

Whereas the first paper (Phillips and Willey 1953) involved space-time (historical) integration, the second paper was originally supposed to involve "historical-developmental theory, the two subjects being considered as related but operationally distinct branches of archeological method and theory" (Willey and Phillips 1955:723). The second paper also was supposed to be shorter than the first, but in the end it became "longer" and "more empirical

than theoretical" (Willey and Phillips 1955:723). At ninety-five pages, the 1955 paper is probably the longest paper on archaeology ever published in *American Anthropologist*. It is surely one of the most significant papers ever published there or elsewhere on the subject of Americanist archaeology because it comprises *the* seminal attempt to synthesize the human prehistory of a hemisphere within a single framework.

Willey and Phillips (1955) introduced an explicit model to aid in the interpretation of cultural chronologies. Their model comprised what we have elsewhere termed "cultural evolutionism" (Lyman and O'Brien 1997). Since the second decade of the twentieth century Americanist archaeologists had focused on determining the chronological and developmental histories of indigenous cultural lineages (Lyman et al. 1997), but they also wanted to be able somehow to *explain* the histories of those lineages once they had been determined. They were writing history as chronicle, but they also wanted to understand, preferably in anthropological terms, why the history of a particular cultural lineage had the form that it did. This meant they had to have an explanatory algorithm that could be called on to account for the archaeological record. The unilineal and universal (later, "general" [Sahlins 1960]) cultural evolution of Lewis Henry Morgan and Edward B. Tylor had fallen from favor among anthropologists at the end of the nineteenth century, although it never completely disappeared from the discipline. Darwin's model of historical development and evolution had been criticized as inapplicable to human cultural development since early in the twentieth century, and by 1950 it no longer was seen as worthy of consideration (Lyman and O'Brien 1997). But Steward (e.g., 1948, 1949, 1953) and Leslie White (e.g., 1945, 1949) were actively resurrecting the unilinear model as Phillips and Willey began to write, and this was the model they opted for.

Recall that Willey had worked with Steward in the 1940s, and he later remarked that his and Phillips's "historical-developmental interpretation of New World prehistory . . . was clearly



influenced by Steward and also by an outline that had been propounded very briefly by A[lex] D. Krieger at a 1952 symposium" (Willey and Sabloff 1993:206–207). Krieger's outline (1953b) summarized the data for various areas of North America but "without any formal chronological or developmental structure" (Willey and Phillips 1958:68). It actually was in a related, brief comment that Krieger (1953a:247) outlined his four-stage developmental structure for the "cultural evolution of the New World" that Willey and Phillips (1955:726–727) attributed to him in their article. Krieger (1953a:247–248) began with definitions of "stage" and "period":

I will consider a [cultural evolutionary] "stage" a segment of a historical sequence in a given area, characterized by a dominating pattern of economic existence. The general economic life and outlines of social structure of past peoples can often be inferred from archaeological remains and can be related to similar phenomena, whether the dates are known or not. The term "period," on the other hand, might be considered to depend upon chronology. Thus a stage may be recognized by content alone, and in the event that accurate dates can be obtained for it in a given area, it could be said that the *stage* here existed during such-and-such *period*. Further, the same stage may be said to appear at different times or periods in different areas and also to end at different times. A stage may also include several locally distinctive culture complexes and minor time divisions.

Prior to this time, the terms *period* and *stage* had often been used synonymously (e.g., Ford and Willey 1941). Phillips and Willey (1953) had earlier implied that stages and periods were different kinds of units, but they did not define either as an analytical or conceptual unit. We return to these important points later.

Krieger (1953a) referred to a still-earlier formulation of cul-

tural evolutionary stages in the New World—one outlined by Steward (1949; see also Steward 1948)—as well as to how his stages aligned with those of the Old World. Alignment of Krieger's sequence of stages with other such sequences was difficult because different definitive criteria were used in each. Willey and Phillips (1955:726) mentioned Steward's efforts as well as those of others (e.g., Ford and Willey 1941; Griffin 1946), but they also pointed out that those earlier formulations focused on cultural developments within a particular culture area and were not "on an America-wide scale" (Willey and Phillips 1955:723). They also noted that in 1955 there was as yet "no device (except calendrical dating) by which cultural traditions can be temporally equated on a continental or hemispheric scale" (Willey and Phillips 1955:725). Their horizons and traditions would work within each individual culture area, but how were the units to be aligned? They indicated that for "larger syntheses another type of formulation must be resorted to, one that is free from strict limitations of space and time, yet has a general historical validity in the widest sense. The only possible kind of scheme that meets these requirements, so far as we can see, is a series of cultural stages in a historical-developmental sequence" (Willey and Phillips 1955:725). All one had to do was determine to which stage a cultural phase belonged, and the phases from multiple widely separated areas could be aligned with one another. The most significant problem, then, was in "establishing comparable standards of stage definition" (Willey and Phillips 1955:727).

Willey and Phillips (1955:725) cautioned that when deciding on "standards of stage definition" the archaeologist should remember that "cultural development has a time dimension, but this dimension is not necessarily uniform for all localities or regions of an area." In Area A the temporal boundary between Stage 1 and Stage 2 might fall at 6000 B.P., whereas in Area B that temporal boundary may fall at 4000 B.P. Such a result was possible because the definitive criteria of the historical-developmental stages should, in Willey and Phillips's (1955:727) view, not involve the temporal

dimension but instead comprise “functional or configurational” phenomena, where the latter consist of “cultural institutions: sociopolitical and socioeconomic forms, religion, art, technology, and intellectual achievement.” Such criteria were difficult to establish for the earliest stages because they were inferential; therefore, for these earliest stages, “simple technological, typological, or subsistence criteria” were used (Willey and Phillips 1955:727). In their initial formulation Willey and Phillips (1955:728–729) identified six stages: Early Lithic, Archaic, Preformative, Formative, Classic, and Postclassic. The first two stages were defined by technological and subsistence criteria, the last three by configurational criteria, and the third—Preformative—by a combination of both typological and configurational criteria.

Willey and Phillips (1955:729) indicated that their “theoretical attitudes and position” were such that to them “it is obvious that the processes of culture change through time and space are not analogous to those of biological evolution. Because of the unique qualities of convergence and assimilation (diffusion) the archeologist will never be in the position of the paleontologist who, from a single bone, can reconstruct the total organism.” These sentences contain two important points. The first point—that cultural and biological evolution are not “analogous”—had found earlier expression in the writings of Kroeber, Franz Boas, and others in the 1920s and 1930s and resided in the questionable view that a culture and a biological species are evolutionary units of equivalent scale (Lyman and O’Brien 1997; O’Brien and Lyman 2000). At this scale biological evolution is only branching and diversifying whereas cultural evolution is reticulate; cultural lineages not only diverge over time but also come back together because of diffusion, migration, acculturation, and the like. Many anthropologists and archaeologists reasoned that this difference in the operation of cultural and biological evolution negated the utility of Darwinian theory for explaining the history of a cultural lineage (Lyman and O’Brien 1997).

Although related to the first point, the second point shifts the

scale from cultures and species to units of transmission—genes for organisms and cultural traits for cultures—sometimes termed *replicators* (e.g., Lyman and O'Brien 1998; O'Brien and Lyman 2000; and references therein). The significance of the second point resides in Willey and Phillips's (1955:729–730) belief that “cultures are [*not*] necessarily transmitted as integrated wholes,” as is the genotype of an organism. Cultural evolution involves the transmission of replicators that can, potentially, be transmitted from person to person completely independent of one another; hence Robert Lowie's (1920:441) famous dictum that a culture is but a “thing of shreds and patches,” each piece originating (potentially) in a different source culture. Willey and Phillips believed biological transmission and genetic inheritance do not work the way that cultural transmission and inheritance do, the source of this belief residing in equating cultures and species and in how species are viewed. Although in biology every organism can be categorized as belonging to a particular species, every organism within a particular species does *not* have exactly the same genotype as every other member of that particular species. Therefore, a “total organism” can be “reconstruct[ed]” (Willey and Phillips 1955:729) from a single bone *only* if two things are known: which species the bone represents and how much individual (intraspecific) variation exists within that species, irrespective of whether the variation is genetically dictated or is merely phenotypic.

A species comprises a set of populations of organisms, the individual members of which can potentially interbreed and produce viable offspring. This makes for each individual organism being little more than a thing of genetic shreds and patches because each organism receives genes from its ancestors, which may or may not have transmitted genes to other organisms. Because it is a population that evolves, and a population does not necessarily comprise all members of a species, the equating of a culture and a species is inappropriate. Finally, no paleontologist can reconstruct a complete organism from a single bone without knowing something of the antecedent organisms. Similarly, an archaeologist cannot re-

construct, say, what might be called Clovis culture from a single Clovis point without similar kinds of knowledge. The point of Willey and Phillips's remark that biological and cultural evolution are not analogous processes was that their "area phylogenetic systems of culture classification . . . discussed in our previous article (Phillips and Willey 1953) suffer from this fundamental defect" (Willey and Phillips 1955:730). In the 1955 paper they wanted to avoid dealing with the reticulate nature of cultural evolution.

The problem of how best to handle the problem of evolving yet interacting culture lineages was dealt with in two ways. First, the term *historical-developmental sequence* was used rather than the term *evolution*. Second, Willey and Phillips (1955:730) indicated that "culture development cannot be considered profitably outside a framework of culture history." Thus their historical-developmental sequence was held to "not eschew history but is an attempt to provide for its own examination within a historical frame of reference. Such a historical frame of reference takes cognizance not only of sequence but also of the complex interrelationships among cultural phases and traditions (diffusion)" (Willey and Phillips 1955:730). They did not intend to "impose an evolutionary determinism on the data" (Willey and Phillips 1955:788) and instead sought first to organize "archeological data in terms of a real world" and second to "abstract . . . certain characteristics that seem to have significance from the point of view of the general development of New World culture" (Willey and Phillips 1955:789). In the end, Willey and Phillips (1955:790-791) found that although their sequence could be improved by "better stage definitions" and more data of a "configurational" sort, its contribution was in suggesting questions for future research to answer.

### *Responses to the Papers*

Several individuals commented on either or both of the papers, and Willey and Phillips took various comments into account when they rewrote the papers for publication in book form. Some commentators basically agreed with the authors, whereas others were

less than impressed; some commentators focused on the methods (Phillips and Willey 1953), others on the historical-developmental scheme (Willey and Phillips 1955). We summarize some of the comments in the next few paragraphs and reserve mention of others for the next section.

Eóin MacWhite (1956:6) underscored the importance of Phillips and Willey's (1953) discussion of the concept of an archaeological culture, noting that despite its imperfections, the concept had "become an almost unquestioned postulate of archeological thinking, [and] represents the most important heuristic advance in modern archeology." A parallel to such a view is found in paleontology, where *conceiving* of one or more sets of fossils as making up a biological species and *analyzing* those fossils as such resulted in the emergence of modern paleobiology (see Eldredge 1985, 1989; Gould and Eldredge 1993; and references therein). This shift in view—irrespective of the problem of being unable to *demonstrate* that a particular pile of fossils in fact represented a unit identical to an extant biological species—allowed paleontologists to rewrite Darwinian evolutionary theory in paleontological terms rather than the other way around<sup>2</sup>—reconstructing the paleontological record into a biota functioning within an ecosystem. But instead of following a similar path, MacWhite, Phillips, Willey, and their contemporaries and successors focused on the fact that they were anthropologists who happened to be doing their anthropology via study of the archaeological record. Thus having to contend with the fact that "the archeological culture is a specialized concept dealing with only fragments of the totality of culture" (MacWhite 1956:7) took precedence over any effort to write anthropological theory in purely archaeological terms (e.g., Binford 1977b; Schiffer 1976).

McKern (1956) commented on the historical-developmental sequence (Willey and Phillips 1955), arguing that ecological factors, ethnological data, and historical information had not been sufficiently considered. Specifically, he "refuse[d] to accept the arbitrary allocation of early Northwest Coast culture to the Ar-

chaic as evidence that other Archaic cultures ‘must have possessed’ [Willey and Phillips 1955:791] similarly complex cultural developments” (McKern 1956:360). More significantly, he pointed out that, contrary to Willey and Phillips’s position, an agricultural economy was unnecessary to a sedentary life. McKern concluded that in their cultural-stage “taxonomy”—the classification of entities into a set of discrete units based on particular criteria—Willey and Phillips had not done a very good job of setting up definitive criteria. Willey and Phillips (1958) would address this particular classification problem in their second formulation.

Perhaps the most critical comments were provided by Albert Spaulding, who wrote a letter to Willey and Phillips, which they quote at length in the book (pp. 15–16), and published a review of the two *American Anthropologist* papers, in which he focused primarily on the 1953 paper (Spaulding 1957). Phillips and Willey (1957) responded almost immediately to the latter, noting that Spaulding had been notified of the differences between the 1953/55 version of the discussion and the upcoming 1958 version. They also asked interested scholars to withhold their comments until that revised version was available. In his review, Spaulding expressed concern over the fact that Phillips and Willey (1953) were conflating the measurements of three distinct dimensions—time, geographic space, and [cultural] form—with their phase, tradition, and horizon units (see also Spaulding 1960). Spaulding held up the Midwestern Taxonomic Method as exemplary of a classification system built explicitly to measure but one dimension—form—and pointed out that Phillips and Willey’s (1953) criticism of it was akin to “criticizing an automobile because it will not fly” (Spaulding 1957:86). Time, space, and form were, Spaulding emphasized, continua, just as Phillips and Willey had said. How, then, were these continua to be divided up into units that were not only manageable but also had some analytical utility and anthropological validity?

In Spaulding’s view, formal variation in artifacts exists across space and time, but it does so in an analytically useful way.

Specifically, it varies discontinuously. That is, there are natural junctures between artifact and cultural forms—junctures that occur at particular places along the time and space continua that result in what Spaulding termed “clusters” of forms created by “varying rates of culture change through time, shifts of culture type over space for ecological reasons, or conceivably even some remarkably bad luck of sampling” (Spaulding 1957:87). In his view, “clustering of this sort is generally characteristic of cultural data and the development of adequate scaling and analytical techniques for its study is the most important methodological problem confronting archaeologists at present” (Spaulding 1957:87).

Spaulding’s solution—and, following Brainerd (1951a, 1951b), he had already suggested some analytical techniques to attain it (Spaulding 1953)—was to find those natural junctures and natural clusters. Once found, those junctures would allow the archaeologist to divide space, time, and form into natural—what we would term “real”—clusters or units that had anthropological significance. This would, in Spaulding’s view, circumvent the problem of trying to measure all three dimensions simultaneously. Willey and Phillips (1958:16–17) for the most part later conceded that natural clusters and junctures in the continua may well exist, and the archaeologist should be watchful for them, but this seems to have been more Phillips’s idea than it was Willey’s, as many of the writings of the former make clear (e.g., Phillips 1970; Phillips et al. 1951). Spaulding’s suggestion was a clever solution and reinforced the notion that the archaeological record had to be constituted in anthropological terms in order to be explained by anthropological theory. No archaeological theory—at least no anthropological theory written in archaeological terms—was necessary (Lyman et al. 1997; O’Brien and Lyman 1998, 1999a, 2000).

#### THE BOOK

The first three paragraphs printed on the dust jacket of the original hardback edition of *Method and Theory in American Archaeology* provide an accurate abstract of the volume:



Archaeology is brought into relationship with the broader field of anthropology in this pioneering volume. Taking their work out of the narrow realm of antiquarian interests, the authors bring American archaeological studies into the general concern of social scientists by emphasizing the basic relationship between the artifacts and remains of ancient cultures and the social context in which they were created.

Equally important, the authors of this book have helped to solve a major problem in American archaeological studies—that of the academic and geographic isolation of the various areas of research. By organizing all the data and findings of New World prehistory into a general “historical-developmental” scheme, the authors have established a common terminology.

Here is a powerful and important study dealing with the theory, method, and content of American archaeology—a book that tries to bring order out of present-day confusion and that organizes the study of the prehistory of the Americas in such a way that the general nature of New World cultural growth can be clearly understood.

The book is divided into three parts: (1) a brief introduction that serves to establish the metaphysic the authors used to structure their work, (2) the methods they followed, and (3) the substantive results of applying their methods within a particular interpretive model of culture change. Given that each of these three was published earlier in slightly different form, it is of at least historical importance to describe some of the similarities and differences between the earlier formulations of Phillips and Willey's ideas and the later formulations that they included in the book.

### *The Introduction*

The introduction to *Method and Theory in American Archaeology* first appeared as an article written by Phillips, and in it he made the famous statement, “New World archaeology is anthropology

or it is nothing" (Phillips 1955:246–247). Other archaeologists were saying the same thing at this time, if in less well-phrased words. Recall, for example, Martin, Quimby, and Collier's (1947:3) statement quoted earlier that archaeology "is a part of [anthropological] science." Similarly, Fay-Cooper Cole (1956:2) was emphatic when he said, "Archaeology is more than a consideration of material objects and bare chronology. It deals with evolving human society." Anthropologists generally also were saying this. Clyde Kluckhohn (1949:52), for one, wrote, "In principle, archaeology is identical with the work of the anthropological describers who deal with living peoples. Archaeology is the ethnography and culture history of past peoples."

Phillips's famous sentence was rewritten in the book version as "American archaeology is anthropology or it is nothing." Most other word changes between the 1955 version of the paper and the 1958 version are of little significance. Two, however, warrant comment; but before turning to those changes in wording we think it important to devote some discussion to the derivation of Phillips's often-quoted phrase. Phillips (1955:246) noted that the phrase was a paraphrase of "Maitland's famous dictum," but he neither identified Maitland nor provided a reference to his dictum. Phillips seems to have merely been following his intellectual predecessors, because although the anthropological literature of the 1940s and 1950s refers to Maitland's statement, with one notable exception it contains minimal information on either the context of the statement or its author (e.g., Bidney 1953:183, 261; Evans-Pritchard 1950:123; Herskovits 1948:461; Radin 1949:523).

Frederic William Maitland (1850–1906) was a jurist who studied and wrote on English law, particularly its history. He first stated his "famous dictum" in a lecture entitled "The Body Politic" delivered in 1899 and published in 1936 in a collection of his essays (Maitland 1936). The full sentence containing his statement reads, "My own belief is that by and by anthropology will have the choice between being history and being nothing" (Maitland 1936:249). In a detailed analysis of the full context of this statement, David

Bidney (1953:261–264) concluded that Maitland was reacting negatively to the universal cultural evolutionism of Herbert Spencer, thereby (perhaps unknowingly) aligning himself with Franz Boas (e.g., 1896). We agree with Bidney's (1953:263–264) conclusion that the "point of Maitland's criticism and prophesy is, then, that the anthropologists and sociologists who are seeking for a universal natural history of society and culture as subject to natural law simply are not reckoning with the empirical facts of history. Anthropology is essentially, [Maitland] held, the study of social history and each society must be studied empirically without any preconceived notions as to what will be found."

Maitland (1936:256) worded the last sentence of "The Body Politic" as follows: "I do regret the suggestion that at the present time the student of history should hope for and aim at ever wider and wider generalizations." Throughout the paper he characterized as fallacious the construction of an empirical generalization inductively and then using it as a way either to predict the future or to understand the past. With respect to cultural evolution, he had earlier written the following rather telling statement:

Even had our anthropologists at their command materials that would justify them in prescribing a normal programme for the human race and in decreeing that every independent portion of mankind must, if it is so to move at all, move through one fated series of stages which may be designated as Stage *A*, Stage *B*, Stage *C* and so forth, we still should have to face the fact that the rapidly progressive groups have been just those which have not been independent, which have not worked out their own salvation, but have appropriated alien ideas and have thus been enabled, for anything that we can tell, to leap from Stage *A* to Stage *X* without passing through any intermediate stages. (Maitland 1897:345)

As Robert Lowie (1920:435) noted with respect to this statement, it was clear to Maitland (as it was to Lowie) that "historical

laws" concerning the development of cultural lineages did not exist.

We at first thought it strange that Phillips should use the "famous dictum" of someone who would deny the validity of the historical-developmental scheme he and Willey would describe, but on reading "The Body Politic" we began to suspect that Phillips may not have read the essay. In the paragraph containing his "famous dictum," Maitland (1936:249) stated that if sufficient historical data are examined, "each case begins to look very unique and a law which deduces . . . any sequence of 'states' begins to look exceedingly improbable. . . . [V]ery rarely indeed have we any direct evidence of the passage of a barbarous nation from one state to another." The last statement is followed directly by his famous dictum. Perhaps most revealing with respect to whether or not Phillips had read "The Body Politic," Maitland (1936:242) indicated that "I am including under the name of history what some people call archaeology; for to my mind an archaeology that is not history is somewhat less than nothing, and a Special Board for History and Archaeology is like a Special Board for Mathematics and the Rule of Three." Maitland was strongly advocating what anthropologists in North America would later term "historical particularism" and derogating the likes of what became Leslie White's and Julian Steward's versions of cultural evolution. Perhaps Phillips *did* read Maitland's "The Body Politic," but if so, he apparently ignored the larger message of that essay in his own essays with Willey.

As we noted, there are two significant changes in wording between Phillips's (1955) original paper and the introduction to *Method and Theory in American Archaeology*. First, the diagram on page 4 of the book has, at the "explanation" level, the words "processual interpretation," whereas in Phillips's (1955:248) original article those words were "functional interpretation." Willey and Phillips (1958:5) noted this change in the book. Second, Phillips (1955:249-250) indicated that his term *functional interpretation* "certainly implies an attempt to discover regularities in the

relationships given by the methods of descriptive integration. Whatever we call it, the important fact is that on this explanatory level of organization, archaeological formulations are viewed in both their cultural and social aspects. It is not possible to go about investigating the *how* of cultural facts without taking into account their *efficient* causes, which are social facts." These sentences were rewritten for the book to read as follows:

We have substituted here [for "functional interpretation"] the broader "processual interpretation," which might conceivably cover any explanatory principle that might be invoked. In the context of archaeology, processual interpretation is the study of the nature of what is vaguely referred to as the culture-historical process. Practically speaking, it implies an attempt to discover regularities in the relationships given by the methods of culture-historical integration. Whatever we choose to call it, the important consideration is that, on this explanatory level of organization where we are no longer asking merely what but also how and even why, our formulations must be viewed in both their cultural and their social aspects. It is not possible to go about investigating culture-historical processes and causality without reference to the efficient causes of cultural change, which are people or groups of people, and therefore lie in the social aspect of reality. (Willey and Phillips 1958:5-6)

In their introduction Willey and Phillips (1958:5) indicated that "Culture-historical integration is . . . comparable to ethnography with the time dimension added," but lamented that "little work has been done in American archaeology on the explanatory level." The integrative methods of culture historians involved constructing sequences of phases, identifying horizons and traditions, and using the axiom that typological similarity denoted historical relatedness. The archaeological units used had sociocultural connotations and thus allowed the analyst to invoke as mechanisms of

change the sociocultural processes observed in modern cultures by anthropologists and ethnographers (e.g., Meggers 1955). Archaeological units having sociocultural connotations were necessary because they were “intelligible in *both* the cultural and social aspects of the [human] behavior that is our subject matter” (Willey and Phillips 1958:49). How any sort of one-to-one correspondence between an archaeological unit and a unit derived from ethnology was supposed to be established would not be explicitly addressed until nearly a decade later. We return to this point briefly in the last section of this introduction.

*An Operational Basis for  
Culture-Historical Integration (1958)*

Part 1 of *Method and Theory in American Archaeology* is a revised version of the paper published in 1953, which was changed as a result of comments Willey and Phillips received from Albert Spaulding and Irving Rouse. We discussed Spaulding’s comments above and focus here on those of Rouse. In part 1 of the book, Willey and Phillips explicitly addressed the problem of determining the evolutionary relations between cultural phases and then explaining them. Phillips and Willey (1953) had originally suggested that incorporating the concepts of *traditions*, *horizons*, and *horizon styles* would reflect the braided stream of the evolution of cultures and allow one to correlate phases. As Rouse (1954:222) noted, a metaphor could be drawn between the use of horizons and traditions as integrative devices for archaeological materials distributed across an area and a rectangular piece of cloth, the side edges representing the geographical limits of the area and the top and bottom edges representing the temporal limits: “The warp threads of the cloth consist of a series of regional traditions running from the bottom towards the top of the cloth, while the weft is composed of a number of horizon styles which extend from one side of the cloth towards the other. The cloth is decorated with a series of irregularly arranged rectangles, each representing a single culture [read: *phase*], and these are so colored that they appear to

form a series of horizontal bands.” Our representation of what Rouse was talking about is shown in Figure 5.

Rouse (1955) saw as too simplistic the traditional dictum that typological similarity denoted cultural relatedness. He wanted to determine the phylogenetic relations among phases rather than merely track the spatio-temporal distributions of a few artifact types, which he perceived would be the result of using horizons and traditions. To illustrate this, he identified three ways that one could correlate phases. First, one might use a Midwestern Taxonomic Method-like procedure to group phases that shared traits. Why the traits were shared was a separate issue. Second, one could note similarities in the spatio-temporal distributions of phases. Identical or adjacent distributions of two or more phases “establishes contemporaneity and contiguity, or lack thereof, and nothing else” (Rouse 1955:717). To argue that contemporaneous phases were phylogenetically related “because they share a given horizon style . . . is on the genetic rather than the distributional level of interpretation, for it requires an assumption that the style has diffused from one phase to the others with little or no time lag” (Rouse 1955:718). Third, one might trace the “genetic” relations among phases by establishing that the phases had been in “contact” temporally and spatially by using horizons and traditions that comprised homologous types (Rouse 1955:719). One needed to distinguish between analogous and homologous similarity—the linchpin to this kind of comparison—to ensure the relations were, metaphorically, “genetic”—the result of transmission and common heritage. Kroeber (1931) had emphasized the distinction between analogous and homologous similarity for historical research, but Americanist archaeologists tended to ignore it, as Binford (1968a) pointed out some years later.

Wiley and Phillips (1958:31) argued that Rouse’s “genetic” relations could only “be revealed and expressed by means of integrative concepts that are culturally determined.” The phrase *culturally determined* was critical because horizons and horizon styles by definition reflected cultural transmission (diffusion) over space;

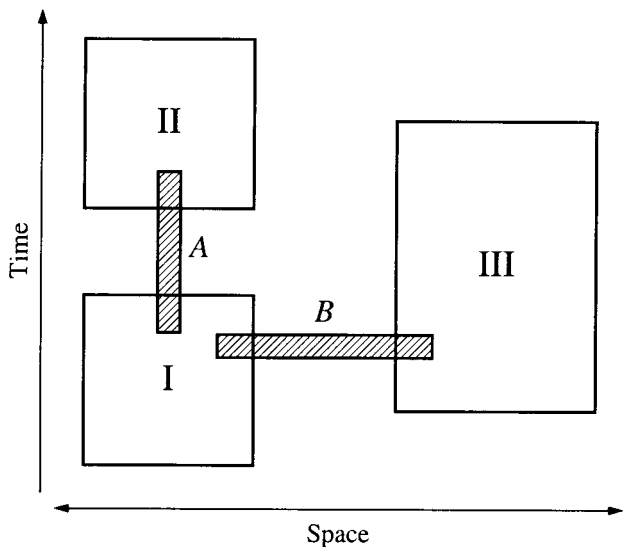


Figure 5. A model of the warp and weft threads of culture history as described by Irving Rouse (1954). Artifact form varies more or less continuously along both the horizontal and vertical axes. Each unshaded rectangle with a Roman numeral is a cultural phase. The shaded rectangle labeled *A* represents a tradition—one or more artifact types or cultural traits that link temporally successive phases I and II. The shaded rectangle labeled *B* represents a horizon—one (horizon style) or more artifact types or cultural traits that link phases II and III; the linkage denotes some contemporaneity as a result of diffusion. The addition of other phases, traditions, and horizons would produce the culture-history tapestry described by Rouse.

they thus were meant to reflect homologous similarity. A tradition was “a (primarily) temporal continuity represented by persistent configurations in single technologies or other systems of related forms” that, according to Willey and Phillips (1958:38), operated at Rouse’s (1955) “genetic level of interpretation” because they reflected cultural transmission across time. Horizons and traditions provided the empirical warrants for discussing the historical development of cultures. They were “integrative” units that



denoted “some form of historical contact,” but the “contact” they denoted did *not* signify “phylogeny” (Willey and Phillips 1958:30). Willey’s (1953a:368) suggestion that “principles of continuity and change are expressed in the degrees of trait likeness and unlikeness which are the mechanisms of establishing the *genetic* lines binding the assemblages together” (emphasis added) thus was metaphorical with respect to cultural transmission. But historical “relatedness,” when couched in a temporal framework aimed at studying the developmental lineages of cultures such as that envisioned by Willey and Phillips, cannot fail to be phylogenetic in the sense of Darwinian evolution’s “descent with modification.” The general abhorrence in archaeology of anything Darwinian (see Lyman and O’Brien [1997] for discussion) resulted in this contradiction internal to the Willey-and-Phillips scheme going unrecognized. Also, the supposed inapplicability of Darwinian evolution to understanding cultural development resulted in a marked favoring of diffusionist explanations couched within the multilinear cultural evolutionism of Steward.

Willey and Phillips (1958:16–17) modified their original wording to reflect Spaulding’s concerns for natural clusters and boundaries, indicating that they now preferred “the concept of an archaeological unit as a provisionally defined segment of the total continuum, whose ultimate validation will depend on the degree to which its internal spatial and temporal dimensions can be shown to coincide with significant variations in the nature and rate of cultural change in that continuum.” Archaeologists were to make every effort to “understand precisely what quantities of space and time are involved in the formulation” of such cultural units (Willey and Phillips 1958:17). This concern attended the notion of the *tempo* of culture change: Is it slow or fast? Is it gradual and continuous, or jerky and discontinuous? Is it some combination of both? And, if so, when is it one and when is it the other?

Given that archaeologists had only recently been provided with a nearly universally applicable absolute chronological method—radiocarbon dating—it is not surprising that questions concern-

ing the tempo of culture change could only now be seriously addressed. Dendrochronology had provided some earlier insight into such matters, but it was limited temporally and geographically. Radiocarbon dating had no such limitations, and it hastened the demise of culture history, because to most archaeologists radiocarbon made the methods developed for building cultural chronologies things of the past (O'Brien and Lyman 1999a). Now, whole sets of artifacts could be dated absolutely. The method advocated by Willey and Phillips, built on more than forty years of disciplinary tradition, ensured that archaeologists viewed those sets of artifacts as *cultures*.

*Historical-Developmental  
Interpretation (1958)*

Willey and Phillips (1958:61–72) began the second formulation of their historical-developmental sequence by reiterating many of the points they raised in the initial formulation (Willey and Phillips 1955). They characterized the building of cultural chronologies and the determining of historical relations of cultural phases and traditions as “descriptive” (Willey and Phillips 1958:61). They wanted to move beyond description and actually “explain” what previously had merely been described, but they found it difficult to do so. Thus the synthesis they presented—their “historical-developmental interpretation”—was at a level that was “sort of gray borderland between description and explanation” (Willey and Phillips 1958:61). Phases, traditions, and horizons were the nuts and bolts of their structure, but the blueprint comprised cultural stages arranged under patterns. Two basic economic patterns—hunting and gathering, and agriculture—were recognized in the American archaeological record. Chronologically earlier preagricultural stages were defined on the basis of technological traits, and later agricultural stages on the basis of “more complex data [pertaining] to social and political organization, religion, aesthetics” (Willey and Phillips 1958:73). This shift from relatively empirical criteria to inferential criteria went largely unremarked,

save for one thing: The new formulation comprised five rather than the original six stages used in the 1955 paper. The “Preformative” stage of the original formulation—the one with combined technological and cultural configurational criteria—was omitted. This required some changes “in definition and emphasis on criteria” among the remaining five stages, but the “archaeological content” of the remaining stages was “unchanged” (Willey and Phillips 1958:74).

In what we take to be a significant addition to their original operational basis, Willey and Phillips (1958:75, Figure 2) included a new figure and a discussion of it to indicate some of the complexities of using cultural stages to synthesize the prehistory of a hemisphere. They were concerned explicitly with classifying prehistoric cultures manifested archaeologically as belonging to a particular historical-developmental stage. How, they wondered, could this be accomplished, especially given the problem of “cultural lag” (Willey and Phillips 1958:74)? Their discussion was well reasoned, but it illuminated some of the difficulties in what they were proposing to do: using classification units—stages—that were written largely in anthropological and cultural terms. The definitive criteria of these stages were partly empirical and partly inferential. Further, each stage was characterized as an evolutionarily stable state comprising a suite of definitive and descriptive criteria, yet those two kinds of criteria were not always clearly distinguished. That is, a particular cultural trait might sometimes be definitive and other times descriptive. Finally, each cultural lineage evolved at a unique tempo and by a unique set of modes. Because each cultural stage was conceived of as a set of traits, sometimes a particular trait occurred in more than one stage. It is with respect to the latter that Willey and Phillips were concerned about cultural lag: How could they classify an archaeological manifestation that seemed to represent, say, the Lithic stage when that manifestation contained some traits that were definitive of the Archaic stage?

The source of this difficulty resided in the fact that Willey and Phillips’s stages were not rigidly *defined* theoretical and analytical

units; instead they were loosely *described* empirical units derived from ethnology, for which they sought matches in the archaeological record. That the units were at least partly empirical is indicated by Willey and Phillips (1958:74) noting that the five stages had temporal boundaries of varying rigidity. This probably resulted because, although they noted that others had confused the concepts of stage and period and that distinguishing between stages and periods was a "necessity" (Willey and Phillips 1958:66), Willey and Phillips themselves never explicitly defined either as a conceptual analytical unit and instead cited Krieger's (1953a) definition quoted earlier. In other words, they viewed each stage as a kind of unit that was to be determined empirically rather than defined explicitly as a conceptual unit to be used for analytical purposes. This procedure was typical of the time, however (Lyman et al. 1997). Douglas Byers (1959), for example, not only provided a cogent conceptual history of the unit known as the Archaic but also introduced five papers that sought to establish empirically the validity of that unit. Those papers sometimes treated the unit as a stage, sometimes as a period. A decade later, yet another series of papers (Irwin-Williams 1968) had the same task and sought to complete it in the same way with, not surprisingly, the same result.

These efforts underscore the difficulties involved in using cultural-evolutionary stages derived from empirical generalizations, which, in turn, are derived from ethnological data. As John Rowe (1962:51) noted a few years after Willey and Phillips's book appeared, the use of developmental stages was the source of the problem: "Because of the close association of stages with the theory of cultural evolution, virtually every archaeologist who uses stages to organize his data thereby builds into them certain assumptions about cultural development without being aware that he is doing so. Later, in making his cultural interpretations, he discovers the pattern of cultural development which was assumed in his system of organization [the stages] and thinks that he is deriving it empirically from the data."

Willey and Phillips's (1958:205) hoped-for "testing" of their historical-developmental model could not occur if by that term they meant an effort to assess whether or not the data they summarized invalidated the five-stage sequence. What we think they meant by "testing" was, "Can the data we have at hand be rendered to fit the model? If so, then the five-stage model is a reasonable one for summarizing the historical development of the prehistoric cultures of the Americas." In this respect their testing was successful. What did reviewers think about the methods and the model?

#### REVIEWS AND COMMENTS

One might have anticipated that a book like *Method and Theory in American Archaeology* would receive numerous reviews of varying opinions, and that in fact was the case (Hymes 1958; Kelley 1958; Meggers and Evans 1958; Roberts 1958). In addition, at least two items were published that were not formal reviews but rather brief articles that focused on perceived weaknesses in the book (e.g., Miller 1959; Swanson 1959). Comments and opinions were also rendered in the context of other discussions (e.g., Caldwell 1959).

Frank Roberts (1958:980) summarized in a mere three paragraphs the content of the book and offered no evaluation of its contents, although he did emphasize that the authors took the perspective that "the big problem for the archaeologist is the interpretation of his findings as social and cultural phenomena in the anthropological sense." Dell Hymes (1958:344A), an anthropological linguist, used five paragraphs to summarize the contents, noting that the discussion in the book had "its feet firm amid sherds and site reports, [yet] still lifts its eyes unto the hills of general anthropological theory." We think it important to note that Roberts (1958) used Willey and Phillips's term *development* in his review, whereas Hymes (1958:344A), while recognizing that Willey and Phillips shied away from "evolutionary or any other causal implications" that their historical-developmental sequence of five

stages might imply, found their efforts to be “a firm step toward a modern evolutionary approach.”

J. Charles Kelley (1958:457) found part 1 of the book to be “a relatively clear and uninspired discourse on generally accepted theoretical principles of American archaeology. Such discourses have a tendency to reflect accepted practices in the field rather than serve as vanguard pronouncements.” Part 2 was an entirely different matter. It comprised “a magnificent, daring, and intuitive synthesis of American prehistory. . . . [that] will have great utility for years to come for both professional anthropologists and students. There is nothing like it in print” (Kelley 1958:457). Kelley suspected that there was “carelessness with regard to detail” for some geographic areas, but he cited no examples (Kelley 1958:457); other reviewers would. What was of greater moment was the fact that the treatment in part 2 was “more interpretive and inferential than the authors admit. Application of hemisphere-wide stage diagnostics is not accomplished without stress” (Kelley 1958:457). Nevertheless, Kelley (1958:457) concluded that *Method and Theory in American Archaeology* was “the indispensable book of the decade or the half century for American archaeologists.”

In contrast to MacWhite's (1956) opinion of the notion of “archaeological culture” discussed earlier, Betty Meggers and Clifford Evans (1958:195) found Willey and Phillips's (1958) use of the term *culture* to be contradictory. It was used in multiple ways to signify multiple sorts of things, probably because Willey and Phillips provided no “clear-cut definition” of the term, making the “theoretical discussion difficult to follow” (Meggers and Evans 1958:195). In contrast to Kelley's (1958) perception of part 1, Meggers and Evans (1958:195) indicated that “archaeological method and theory is so unsystematized that almost everyone will probably be able to find statements, concepts, or assumptions in the book with which he disagrees.” They found the historical-developmental scheme of part 2 to be “deficient. Assignment of cultures to stages is often a question of emphasis, and is complicated by geographical location. . . . This problem arises from the fact that the stages were originally conceived in the context of the

high culture area of Andean South America, where their [definitive] criteria are relatively specific and a real historical-developmental sequence is involved" (Meggers and Evans 1958:195). In other words, the definitive criteria for identifying each stage were derived by polling some known cultural manifestations in a limited area of, say, South America; no wonder those criteria wouldn't necessarily appear in other geographic areas. This procedure of formulating units of any scale—artifact type, tool kit, cultural phase, culture—had plagued the culture-history paradigm since its inception (Lyman et al. 1997), so it is not surprising that it would affect Willey and Phillips's efforts at synthesis. Within a very few years, James Hester (1962) proposed a different set of stages that was supposed to avoid this problem, and although the framework was informed by Steward's (1955) cultural ecology and evolution, the definitive criteria of the example units Hester chose were derived from empirical data.

Finally, Meggers and Evans (1958:196) pointed out that the historical-developmental sequence "should be a tool for arriving at interpretation," implying that the organizational framework Willey and Phillips used might "be slanted" because it concealed "significant facts." That is, because the framework itself was something of an interpretation of what happened in American prehistory—a fact Willey and Phillips (1955, 1958) clearly acknowledged—it forced empirical data to be interpreted in a particular way (recall Rowe's [1962] comment cited above). This is merely another way of saying what has now become common knowledge among archaeologists: No observation is "objective" in the sense of being theory-free. Again, the significance of Meggers and Evans's insight has gone largely unremarked, perhaps because Willey and Phillips argued that their sequence was largely descriptive and also stated that "ours is not an evolutionary scheme" (Willey and Phillips 1958:70). Joseph Caldwell (1959:307), for one, indicated that he found unconvincing Willey and Phillips's (1958:70–71) assurances that their scheme was not evolutionary, although he did not elaborate on the point.

However, Earl Swanson (1959) *did* elaborate on the point. He

began his commentary by noting that Willey and Phillips's "pre-occupation with taxonomy" was perhaps "due to a search, among Americanists, for scientific respectability, for an obvious expression and extension of scientific method. The recurrent interest in systematics as a form of theory certainly suggests some preference for the archaeologist as scientist rather than as historian" (Swanson 1959:120). In Swanson's (1959:121) opinion, "No theory has been developed. What is *assumed* is an evolutionary theory about the nature of culture, though Willey and Phillips are shy about admitting this." To Swanson (1959:121), a "theory should propose to account for the relationships of events and processes of American prehistory. . . . Theory should be developed in the process of writing history, not as a straightforward exercise in systematics." Here Swanson was anticipating statements made later by philosophers of sciences who argued that theory construction and unit construction must occur simultaneously if, for example, biological evolution is to be understood and explained (e.g., Hull 1970; Lewontin 1974). We thus agree with Swanson's (1959:123) assessment that Willey and Phillips's historical-developmental sequence of five cultural stages did not provide a theory for "understanding . . . how and why" cultural lineages evolved the way they did. It was instead a model that *assumed* a particular evolutionary course.

In something of an elaboration of Swanson's (1959) comments, Tom Miller (1959:55) stated that "Willey and Phillips [1958], their own statements to the contrary, are basically unilinear cultural evolutionists in their outlook." Swanson (1959) argued that Willey and Phillips's cultural stages were arbitrary and interpretive and thus not explanatory, and Miller (1959:56) went further with his remark that by using those stages to categorize various archaeological manifestations, "Willey and Phillips are assuming what they ought to be trying to prove: the progress of New World cultures." In Miller's (1959) view, Swanson's (1959) complaints originated from a nonevolutionary point of view; Willey and Phillips's historical-developmental scheme was patently of the cultural evolutionary sort such as that advocated by Julian Steward. But like



Swanson, Miller (1959:56) felt that abandoning such a model “would bear far more fruit than attempts to stretch the data to fit the Willey and Phillips sequence.” History shows that Swanson’s and Miller’s (and Rowe’s) concerns were largely ignored. Instead, Americanist archaeologists adopted the modeled framework of developmental stages and applied it with increasing regularity to the materials they studied.

#### AFTER THE BOOK

The shift away from an earlier focus on establishing sequences of various artifact types to explaining in anthropological terms sequences of cultures represented by artifacts prompted Caldwell (1959:304) to proclaim that there was emerging in the 1950s a “New American archaeology” that was “tending to be more concerned with culture process and less concerned with the descriptive content of prehistoric cultures.” The culture processes were the ones witnessed by anthropologists—diffusion, migration, and the like (e.g., Meggers 1955). Archaeologists were, in the 1950s, focusing more closely on wringing anthropological meaning from the archaeological record than on writing anthropological theory in archaeological terms. Evidence of this is found in such things as the 1955 Seminars in Archaeology, sponsored by the Carnegie Corporation and carried out under the auspices of the Society for American Archaeology. The publication resulting from those seminars (Wauchope 1956) underscored, as one reviewer put it, “the growing effort to utilize archaeological data in the general understanding of cultural processes” (Spicer 1957:186). This was so because, as another reviewer noted, all “participants [in the seminars] have attempted . . . to probe the limits of archeological inference, as it can contribute to or extend culture theory, derived largely from ethnological sources” (Siegel 1957:925). Similarly, Willey’s (1953b) landmark study of settlement patterns was explicitly focused on function and adaptation in the form of land use—topics that at least one reviewer (e.g., Stigler 1955) recognized as patently ethnological.

Our point should be obvious: When Willey and Phillips first published their historical-developmental sequence in 1955, Americanist archaeology had begun to take a new direction. K. C. Chang's (1967b:6) observation that "American archaeology has never really lacked either the sentiment or rationale for inquiring into the behavioral aspects of artifacts" is true. And in the 1950s Americanist archaeology began to turn away from a narrow focus on the building of cultural chronologies and toward ethnological interpretation of those chronologies, with a concomitant increased interest in cultural processes. As Meggers (1955:116) noted in yet another edited collection of papers focusing on the ethnological aspects of the archaeological record (Meggers and Evans 1955), this new direction represented "the coming of age of American archaeology." As Willey and Phillips (1955) did in their initial attempt at historical-developmental synthesis, most authors whose papers were included in the Meggers and Evans (1955) volume used a cultural-evolutionary framework (e.g., Steward 1949) to help model and make sense of the archaeological record as it was then known in particular areas. That several centennial celebrations of the publication of Charles Darwin's *On the Origin of Species* took place before the end of the decade (e.g., Meggers 1959; Tax 1960) no doubt reinforced the feeling among Americanist archaeologists that they could make significant contributions to anthropological theory, particularly of the cultural-evolutionary sort (e.g., Rouse 1964). Unlike other anthropologists, archaeologists had access to the time depth of cultures. Americanist archaeologists had, as Willey and Phillips (1955, 1958) noted, been studying cultural development since at least the second decade of the twentieth century, and thus notions of evolution of some sort, though typically implicit, had underpinned all such efforts.

No doubt another contribution to the feeling among archaeologists that schemes such as Willey and Phillips's (1955, 1958) historical-developmental sequence illustrated that archaeology was more than Steward's "tail on an ethnological kite" was made when cultural anthropologists Marshall Sahlins and Elman Service (1960)

attempted to resolve the apparent differences between White's version of cultural evolution—termed “general evolution”—and Steward's version—termed “specific evolution.” In his review of Sahlins and Service's book, Willey (1961:442) found the discussion “useful and clarifying up to a point.” The specific evolution of Steward was “history plus explanation of process—the story of how a given culture, or culture continuum, changed through time by the processes of its adaptations to natural and superorganic environments” (Willey 1961:442). In Willey's (1961:442) view, such clarity did not exist for general evolution because “its processes are obscure.” Further, he disagreed with the central tenet of the book that “the technico-economic realm must always be given priority in explaining the processes of culture change” (Willey 1961:442), concluding that “until the processes of this general evolution are better understood, particularly as these pertain to the way in which the many streams of specific evolution feed in the main one, I cannot appreciate the difference between a general universal evolution of culture and a general universal culture history” (Willey 1961:443). Willey was, as he and Phillips had made clear (1955, 1958), well aware of the fact that each distinct cultural lineage did not necessarily have to pass through the same sequence of evolutionary stages—hence their hesitancy to categorize their historical-developmental sequence as evolutionary. This significant point was missed by various people who commented on their book (e.g., Miller 1959; Swanson 1959) and by many subsequent authors.

The point was not missed by Willey and Phillips's contemporary William Haag, perhaps because Haag had collaborated with Phillips a few years earlier (Ford et al. 1955). Haag (1959:101) perceptively noted that their “theory is not considered [by Willey and Phillips] another evolutionary scheme.” This is so, according to Haag (1959:101), because Willey and Phillips are “doubtful about the degree of correspondence of their theoretical position to Steward's [multilinear evolution].” In Haag's (1959:101) view, however, “there seems little reason to doubt that setting the Ameri-

can [record] in a series of sequential stages that describe culture change through time is applied multilinear evolution.” We think by the word *applied* Haag was addressing the fact that Willey and Phillips did not use the sequence of stages in any explanatory sense but rather merely as a data-ordering device or model. We think this because Haag (1959:101) goes on to state that Willey and Phillips “carefully avoid any contaminating *explanations* of culture change which presumably would take historical-developmental descriptions out of the realm of culture history and into the nether land of cultural evolution” (emphasis in original). Later archaeologists would not be so hesitant to append explanations to the data ordered by the model.

Importantly, Haag (1959:95, 101) notes in two places the fact that Willey and Phillips had general anthropological goals in mind. Given that, in Haag’s (1959:95) view, “archeology [is] now accepted as an integral part of cultural anthropology,” Willey and Phillips’s (1958:2) claim that archaeology “has for its ultimate purpose the discovery of regularities that are in a sense spaceless and timeless” was a reflection of the fact that for them archaeology was a science. For Haag (1959:96), too, archaeology’s “approach to problem is scientific.” Thus, “it is patently rather absurd to indulge in polemics as to whether archeology is history or science” (Haag 1959:95). Here, Haag was responding to statements such as Phillips’s (1970:3) later one that, in his view, the “main premise” of the Willey and Phillips (1958) volume was to make clear that “the ultimate aim of archaeology is science, but at the present stage the practice has more of the character of history.”

The truly seminal yet not previously acknowledged nature of Willey and Phillips’s book is found in one other statement by Haag. In his (Haag 1959:101) view, “without even changing the word regularities to ‘law’ it seems [Willey and Phillips’s claim] is an admirable statement of one of the tenets of [the cultural] evolutionism [of Steward and White].” These interwoven threads—cultural evolutionism, the search for laws or regularities, a commitment to a scientific problem solving—first laid out under a single cover by

Willey and Phillips (1958) would form the cloth that became known as processual archaeology in the late 1960s.

The so-called new, or processual, archaeology that emerged in the 1960s, largely at the urging of Binford (1962, 1964, 1965), followed many of the precedents set by Willey and Phillips (1955, 1958) and their contemporaries. Therefore, we agree with K. C. Chang's (1967b:14) remarks at the time: "I am not impressed by the phrase 'new archaeology' that one sometimes finds in current [1960s] literature. What is old today was new in its own time, and what is now new will become old tomorrow. To say an archaeology is new is to alienate it from the old, whereas one could more profitably absorb and reorganize the old." For example, claims that the new archaeology was generalizing and "nomothetic" because it sought laws or regularities of how cultures work (e.g., review in Sabloff et al. 1973:116–118) clearly had precedence in the 1950s. Americanist archaeology may have been historical, particularizing, and "idiographic" in the 1930s and 1940s, but that was not the ultimate goal of Phil Phillips, Gordon Willey, and a host of their contemporaries in the 1950s. Nor does it seem to us (Lyman and O'Brien 1998; O'Brien and Lyman 2000; see also Trigger 1973) that the dichotomy between nomothetic and idiographic archaeology often noted by processual archaeologists is so strong as they imply (e.g., Longacre 1973; Spaulding 1973).

Processual archaeologists sought, first, specific evolutionary histories of cultural lineages—couching these in functional and adaptational terms—and second, the alignment of those histories with models of cultural evolution founded on the bases established by White and Steward. Ever the borrowers, the new archaeologists used the discussions of their anthropologist brethren. Culture no longer was some socially transmitted phenomena "acquired by man," as Tylor (1871) had defined it, but rather humankind's "extrasomatic means of adaptation," as defined by White (1959)—a definition borrowed by Binford (1962) and adopted by the new archaeologists.

Although Willey and Phillips (1958:6) had used the wording

“culture-historical process,” the term *culture process* became one of the buzzwords of the “new archaeology” of the 1960s (e.g., Binford 1962; Flannery 1967) and was eventually modified to serve as the name of that new archaeology. As far as we can determine, the term *processual archaeology* was used first in 1968 (Binford 1968b). The term *process* was defined as “the dynamic relationships (causes and effects) operative among the components of a system or between systematic components and the environment” (Binford 1968b:269). The term *culture process* was in turn defined as “the dynamic relationships (causes and effects) operative among socio-cultural systems . . . those processes responsible for changes observed in the organization and/or content of the systems . . . the integration of new formal components into the system. . . [the] dynamic relationships operative among cultural systems” (Binford 1968a:14). This definition is precisely what Phillips (1955) and Willey and Phillips (1958) had in mind. This point was seldom acknowledged by the processual archaeologists of the 1960s and 1970s, probably because virtually all the writings of the culture historians—or “traditional archaeologists” as they sometimes were referred to—had been argued to be variously flawed, intellectually sterile, or narrowly focused on particularistic history rather than on the more laudable generalizing goals of anthropology (Binford 1968a; Deetz 1970; Flannery 1967; Watson 1973).

There was no effort on the part of archaeologists of the 1950s and 1960s to argue for an archaeology for archaeology’s sake. Instead, archaeologists perceived themselves, to borrow James Deetz’s (1970:123) wording, as “anthropologists who dig.” This meant that they first had to reconstitute the static archaeological record into one or more dynamic cultural systems before they could begin to explain that record with anthropologically worded theory. What had been known as “ethnographic analogy” became the preferred tool for the job—a tool with which culture historians had long been familiar (Lyman et al. 1997; O’Brien and Lyman 2001). Willey and Phillips (1955, 1958; Phillips and Willey 1953) had exacerbated the use of this tool when demanding that archaeological

units have some ethnological equivalent. A decade later, Chang (1967b:14), for example, paraphrased their words when he wrote that the "category selected for study must be a unit that is both meaningful in terms of socio-cultural behavior and practical for archaeological application."

The basic protocol for building an ethnographic analogy was spelled out by Raymond Thompson (1958:5):

The archaeologist who formulates an indicated conclusion is suggesting that there is a correlation between a certain set of archaeological material percepta and a particular range of sociocultural behavior. He must test this conclusion by demonstrating that an artifact-behavior correlation similar to the suggested one is a common occurrence in ethnographic reality. What actually happens is that he compares an artifact type which is derived from archaeological data with a similar artifact type in a known life situation. If the resemblance in the form of the 2 artifact types is reasonably close, he can infer that the archaeological type shares the technique, behavior, or other cultural activity which is usually associated with the ethnographic type.

Thus the archaeologist proceeds from basic or descriptive data to a contextual or interpretive inference by demonstrating the existence and validity of various degrees of relation of likeness. This similarity or parallelism of relations is called analogy. Archaeological inference, and particularly that related to the reconstruction of cultural (and ecological) contexts, is impossible without recourse to analogy.

By the 1950s ethnographic analogy had expanded from underpinning the direct historical approach to include two distinct kinds of applications. In the earliest statement we have found on the different applications of analogy an archaeologist might employ, Willey (1953c:229) distinguished between what he referred to as "general comparative analogy," in which we are interested in cul-

tures for comparisons, in cultures of the same general level of technological development, perhaps existing under similar environmental situations,” and what he termed “specific historical analogy [where] you have, in a single area, a presumed or reasonably demonstrated cultural continuity from prehistoric to historic times, on into a living ethnology in the same area.” Thompson (1958:5–6), citing Willey (1953c), reiterated the distinction a few years later, noting that in “the general comparative analogy the artifact-behavior correlation derives from a pattern of repeated occurrences in a large number of cultures. In contrast, the specific historical type [of analogy] depends upon the existence of a direct continuity in a single culture or area.”

Robert Ascher (1961:317), noting that analogy was the “most widely used of the tools of archaeological interpretation,” elaborated on the distinction several years later. He pointed out that what Willey termed “specific historical analogy” assumed “historical continuity,” or what we would term “evolutionary” or “heritable continuity” (Lyman and O’Brien 2000; O’Brien and Lyman 1999a, 2000), between the modern analog and the prehistoric phenomenon being interpreted (Ascher 1961:319). Ascher offered what he saw as a better method—the “new analogy”—that made no such assumption: “In effect, the new analogy consists of boundary conditions for the choice of suitable analogs.” Despite what Ascher said, those boundary conditions were basically those specified by Willey (see also Clark 1953). As Ascher (1961:319) summarized them, “the canon is: seek analogies in cultures which manipulate similar environments in similar ways.” Historical continuity was *not* required, as it clearly had been for the direct historical approach, although it was still felt that such a connection would strengthen the validity of the analogically based inference (e.g., Ascher 1961; Binford 1967; Chang 1967a). No one, however, said explicitly *why* it would strengthen it. As we know, it strengthens it because the similarity of phenomena—the basis of an analogical argument—can have two sources. It can be the result of similar ancestry, in which case the similarity is said to be homologous, or



it can be the result of convergence, in which case the similarity is said to be analogous. Of course, similarity may be of both kinds when transmission is capable of occurring between lineages, such as is the case with cultural lineages. In this case, one lineage may adopt via diffusion from another lineage, say an artifact form that solves some adaptive problem. That form as expressed in both lineages has a common ancestor but also comprises convergence—the creation of similar solutions to a common problem. An ethnographic analog that comprises both sorts of similarity is thought to be more valid, then, because it entails elements of both kinds of similarity. Willey and Phillips (1958:23) probably exacerbated the tempo of the shift from specific historical analogs to general comparative ones when they argued that their conception of an archaeological phase was “freed” of the “requirement” of “cultural continuity.”

Binford (1962:219) observed that “the study and establishment of correlations between types of social structure classified on the basis of behavioral attributes and structural types of material elements [is] one of the major areas of anthropological research yet to be developed. Once such correlations are established, archaeologists can attack the problems of evolutionary change in social systems.” Although not without precedent (e.g., Kleindienst and Watson 1956; Thompson 1958), ethnoarchaeology, formation-process studies, and middle-range-theory building emerged as hot topics of the late 1960s and the 1970s (e.g., Binford 1977a; Bonnicksen 1973; Donnan and Clewlow 1974; Gould 1968, 1971, 1978; Heider 1967; Kramer 1979; Longacre and Ayers 1968; Schiffer 1976). In the midst of this activity, Fred Plog (1973:189) remarked that archaeologists “have probably spent more time in attempting to infer and describe lifeways associated with a given [cultural] stage than in accounting for changes from stage to stage.” And a few years later Binford (1977b) observed that little effort had been made to construct explanatory or higher-level theory. In our view, the reason for this was simple: Archaeologists still viewed themselves as anthropologists first and archaeologists second (e.g.,

Woodbury 1963, 1972). In fact, we suggest that most Americanist archaeologists working today would agree with David Anderson's recent answer to the question, "Should archaeology remain within anthropology?" Anderson's (2000:141–142) response was, "You can indeed find employment in archaeology without any training in anthropology, but you can do archaeology better if you have been educated within an anthropological framework."

The anthropological theory that was, and still is, most often borrowed by archaeologists was foretold by Willey and Phillips (1955, 1958)—the cultural evolutionism of White and, particularly, Steward. That cultural evolutionism was more a model than a true theory—a point made by Willey and Phillips—went unrecognized by most archaeologists, although it was pointed out early in the development of processual archaeology that despite the numerous publications by anthropologists concerning the model and its application (e.g., Carneiro 1968, 1970, 1973; Cohen and Service 1978; Fried 1960, 1967; Hester 1962; Service 1962, 1975), the various formulations of cultural evolutionism had only minimal explanatory power (Erasmus 1969; see also Erasmus 1968). Yet it became the framework within which much processual archaeology was, and still is, accomplished (see selected references in Gregg [1991] and Spencer [1997]). The roots of that framework resided in archaeology, specifically culture history (Willey 1960), which is ironic in light of the disdain most processualists have shown that paradigm.

Herein resides what we take to be the major reason to reprint *Method and Theory in American Archaeology*: It comprises the first serious effort by archaeologists to employ White's and, particularly, Steward's reasoning to the archaeological record. Recall, for example, Willey's (Willey and Sabloff 1993:206–207) remarks quoted earlier regarding Steward's influence. All archaeologists, regardless of stripe, need to understand the history of their discipline, especially with reference to various evolutionary schemes that over the years have underpinned attempts to explain variation in the record. Certainly there were important antecedents, but Willey and Phillips's effort was hemispheric in scope, and it

grappled with making the results of applying basic archaeological method—what became known as culture history—amenable to anthropological understanding. Their discussion comprises the perspective of two major figures in Americanist archaeology on the fundamental methods of writing culture history as they were understood and practiced just as the radiocarbon method was becoming generally available (e.g., Broecker and Kulp 1956; Libby 1952). Close reading of the volume will still, more than four decades after it was first printed, pay major dividends.

#### NOTES

1. Phillips (e.g., 1942; 1970:523–524) grappled with this problem throughout his professional career.

2. Not everyone agrees with the argument that paleontologists have rewritten Darwinian evolutionary theory in paleontological terms (e.g., Ricklefs 1978), although the literature cited in the preceding sentence suggests such rewriting has taken place and has successfully allowed significant insights into the history of past life.

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