The “Old” Archaeology

So little work has been done in American archaeology on the explanatory level that it is difficult to find a name for it. The term “functional interpretation” has gained a certain amount of currency in American studies but it is not entirely satisfactory, since it implies that the functional is the only explanatory principle involved. We have substituted here the broader “processual interpretation,” which might conceivably cover any explanatory principle that might be invoked.... 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... we are no longer asking merely what but also how and even why.

—GORDON WILLEY AND PHILIP PHILLIPS, Method and Theory in American Archaeology

When Gordon Willey and Philip Phillips published Method and Theory in American Archaeology in 1958, they could not have known that their remarks, as well as their term “processual interpretation,” anticipated the birth of what would become the dominant approach in American archaeology—one that its chief architect, Lewis Binford (Figure 1.1), would label “processual archaeology” (Binford 1968a). It, too, would “attempt to discover regularities” in cultural processes as evidenced by the archaeological record, and to answer questions of “how and even why.” Proponents of that approach would begin to refer to it as the “new archaeology” because to them it represented a break with traditional archaeology, or what became widely known in the 1960s as “culture history.”

By the mid-1960s the term culture history was being employed derisively to bolster some processualists’ belief that earlier archaeologists had little interest in anything other than the most basic questions of what, where, and when (Binford 1968a, Flannery 1967; Martin 1971). This was, in many respects, a straw man. Culture historians did routinely furnish explanations for variation and change in the archaeological record—explanations informed, if only implicitly, by the theories coming out of the American historical-ethnology school of cultural anthropology (Harris 1968b). These explanations invoked as mechanisms “diffusion,” “migration,” “independent invention,” and other culture-historical processes visible ethnographically. Binford (1963, 1965, 1968a) railed against the underlying theory, which he said modeled culture as a stream of ideas or norms that significantly influence what people do. He referred to the theory as the “aquatic view” of culture. Binford argued that accounts stemming from this perspective were not “scientific”
explanations and that culture history would have to be replaced.

Culture history was never replaced. Processualists rejected some of the basic explanatory mechanisms of traditional culture history—diffusion, migration, and the like—together with its allegedly descriptive and inductive epistemology, but they did not cease to build cultural chronologies or to do the things archaeologists had always done. What, where, and when—converted into form, space, and time (Spaulding 1966b; Willey 1953a)—are foundational to archaeology, regardless of the brand. It is inconceivable it could ever be otherwise, else archaeology would become so metaphysical and esoteric that no one would pay much attention to it. Processualism represented a change in how the archaeological record was viewed, as well as the purported causes of cultural variation, but it did not replace culture history.

To understand the role played in American archaeology not only by processual archaeology but by other approaches that either grew out of it or were a reaction to it, we need to understand something of the pre-1960 period. Such understanding will, as Paul Bohannan and Mark Glazer (1988:xv) put it, “save [us] a good deal of unnecessary originality.” This dictum was ignored by more than a few processualists in the 1960s and ’70s, who either forgot or never knew that their forebears also had an interest in cultural processes. That interest was not expressed by culture historians at the same level of intensity as processualists eventually would, and not in the same philosophical terms, but it was there. Nevertheless, it took second seat to the more pressing business at hand: the construction of cultural chronologies.

By the time Willey and Phillips were writing, the archaeological record of the Western Hemisphere was reasonably well known, much of it admittedly in outline form, and several syntheses of large pieces of that record had recently appeared (e.g., Griffin 1952; Martin, Quimby, and Collier 1947). Willey and Phillips were writing at the dawn of radiocarbon dating, an event often heralded as the beginning of the chronological revolution in archaeology. Radiocarbon was a boon to establishing chronological control, but despite what some archaeologists claimed (e.g., Johnson 1967), it did not signal a revolution in American archaeology (Lyman 2000) (though perhaps it did for post-Neolithic Europe [Renfrew 1973a]). After Willard Libby and his students at the University of Chicago’s Institute for Nuclear Studies showed the archaeological usefulness of an unstable carbon isotope in 1947 (Taylor 1985, 2000), it was used more to refine existing New World chronologies than to create new ones.

The real chronological revolution in American archaeology occurred several decades earlier, when prehistorians working in the Southwest concluded that human occupation of North America was more ancient than previously thought. During the first two decades of the twentieth century it was commonly (although not universally) believed that the tenure of humans in North America did not exceed a couple thousand years and that during that short time little significant cultural change had, or could have, taken place. But then archaeologists were confronted with evidence that did not conform to a shallow past and began to change their minds. The revolution started in the prehistoric pueblos of north-central New Mexico around 1914 (Kidder 1915; Nelson 1913, 1916; Wissler 1915) and ended in a bison-bone bed in northeastern New Mexico just over a decade later (Figgins 1927).

Knowing that humans’ tenure in North America extends back some 11,000 years or more is important, but without the ability to
produce a temporal ordering of artifacts and the deposits in which they occur, archaeology is reduced to a jumble of materials that might as well date to a single point in time. This is why Berthold Laufer (1913:577), an expert on Chinese art and artifacts at the Field Museum in Chicago (Bronson 2003), considered chronology "the nerve electrifying the dead body of history." Maya prehistorian Alfred Tozzer (1926:283) put it only slightly differently: "[A]rchaeological data have an inert quality, a certain spinelessness when unaccompanied by a more or less definite chronological background." Chronological ordering requires the construction of units that measure the passage of time; in archaeology these units typically are artifact types.

One complaint from the processualists (e.g., Plog 1973a, 1973b, 1974) was that the units created by culture historians were less than ideal for studying culture change, although many processualists continued to use them. For several reasons, then, it is important to look back, if briefly, at those units to examine how and why they came into existence and why they've stayed around so long. Answers to questions of how and why suggest some of the reasons why a different kind of archaeology began to emerge in the 1950s and why it became influential in the 1960s. Examining those questions allows us to explore one of the central tenets of this book: that the intellectual position of any archaeologist is at least partially a result of those who came before, whether they advised and instructed the individual in question or merely did the fieldwork on which someone's knowledge of a portion of the past rests.

In this chapter we first outline how culture history originated by emphasizing that of the three dimensions of variation expressed by the archaeological record—time, space, and form—time is invisible. But if one is to write a prehistory of human artifacts, then the age of those artifacts must somehow be made visible; that is, inferred. Archaeologists working in the first half of the twentieth century took that as their top priority. Second, we show how and why the goals of archaeological research began in the 1950s to shift away from chronology and history and toward something else. That "something else" was processualism.

MEASURING TIME WITH ARTIFACTS

There has long been a tendency in American archaeology to state that a "stratigraphic revolution" occurred during the second decade of the twentieth century (e.g., Browman and Givens 1996; Strong 1952; Willey and Sabloff 1993). Recent research, however, has shown that many archaeologists prior to 1910 were both excavating in a manner that is readily considered stratigraphic and segregating artifact assemblages by stratum (Browman 2002; Lyman and O'Brien 1999). Still, it was difficult to determine how much time was represented in a given stratigraphic section. Some archaeologists, principally those aligned with Harvard's Frederic Ward Putnam (Figure 1.2), thought considerable time was represented in the archaeological record of North America (Meltzer 1983, 1985), similar to what had been found in Europe (Grayson 1983). Others, principally those aligned with the Bureau of Amer-

Figure 1.2. Frederic Ward Putnam, second curator of the Peabody Museum of American Archaeology and Ethnology at Harvard, 1909. (Courtesy Peabody Museum, Harvard University.)
RTIFACTS in American prehistoric and protohistoric contexts, evidence for a relatively recent origin of the human populations of the New World is found. By the mid-19th century, archaeologists had begun to place the origins of Native American cultures in the Pleistocene era, a time period that had been thought of as the prehistoric period. By the turn of the 20th century, this view had begun to change, with the discovery of artifacts that suggested a much more recent origin of human populations in the Americas. However, the evidence for the presence of humans in the Americas for the past 10,000 years is still debated, with some scholars arguing for a much earlier origin.

Across the eastern United States (Thomas 1884, 1891, 1894), instead, the builders of the mounds were the ancestors of Native American peoples whom early European explorers and settlers had found residing in the region. To Thomas, Holmes, and others, the archaeological record did not produce the kinds of differences in tools that were thought to signify great time depth. Thus Franz Boas (Figure 1.4) could announce at the turn of the century that “it seems probable that the remains found in most of the archaeological sites of America were left by a people similar in culture to the present Indians” (Boas 1902:1).

For Boas, “similar in culture” meant more or less similar in time. One of Boas's students, Alfred Kroeber (1909), used this reasoning in commenting on Max Uhle’s (1907) stratigraphic sequence of tools from shell middens along San Francisco Bay. Kroeber did not discount the value of Uhle’s method of stratigraphic excavation; rather, he questioned the significance of the chronological indications of cultural change Uhle documented. Kroeber noted that not only had Uhle found few artifacts but also the few he had recovered showed no marked cultural changes through the sequence. This meant no new “types” of artifacts had been introduced. Kroeber, like most other Americanists, was searching for differences in culture traits of a magnitude that would suggest major qualitative differences among cultures on an order similar to that represented by...
Europe's Paleolithic–Neolithic–Bronze Age–Iron Age sequence. Thus the relatively minor change Uhle documented was merely "a passing change of fashion in manufacture or in manipulation of the same process" and not "to be compared even for a moment with a transition as fundamental as that from paleolothic to neolithic" (Kroeber 1909:16).

Americanists would begin to reconsider how time, and hence culture change, could be measured, thanks in part to Clark Wissler, curator of anthropology at the American Museum of Natural History in New York and a former student of Boas's at Columbia. Wissler sent Nels Nelson (Figure 1.5) to the Rio Grande River valley of north-central New Mexico in 1912 specifically to construct a chronology of Puebloan cultural development and to determine how much time depth that chronology represented. Nelson had trained under Kroeber at the University of California and had tried his own hand at stratigraphically excavating shell middens along San Francisco Bay (Nelson 1906, 1910). In New Mexico, he immediately suspected that evidence of considerable time depth might be found along the Rio Grande and its tributaries; there were far too many sites to have all been occupied simultaneously. Also, the physical environment was too harsh to have supported the number of people indicated by all those sites. Therefore, they must have been occupied at different times, but how was he going to demonstrate this?

Nelson (1913, 1916) excavated several sites, including San Cristobal (Figure 1.6), the one for which he is best known. After collecting pottery sherds from arbitrarily defined vertical proveniences (Lyman, O'Brien, and Dunnell 1997; Lyman and O'Brien 1999; O'Brien 2003), he created types based on painted pottery designs and examined how the absolute frequencies of the types varied across proveniences. Nelson found that the frequencies fluctuated smoothly and gradually over time, displaying basically unimodal distributions. This provided him with a chronological ordering of pottery types. Then, by means of typological cross matching, Nelson determined the relative ages of other sites in the region. Leslie Spier, a colleague of Nelson's at the American Museum of Natural History.
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Museum, stated that Nelson’s use of changes in type frequencies was “the first exposition of a refined method for determining exactly the time sequence of archaeological materials in a primitive area” (Spier 1932:275). Nelson’s work signaled the beginning of the chronological revolution in American archaeology, as later archaeologists would characterize it.

Harvard-trained archaeologist Alfred Kidd er was another Southwesternist who detected temporal change in artifacts, and in 1915 he began to address questions of chronology by excavating at Pecos Pueblo, just to the north and east of where Nelson was working (Kidder 1916). Kidder, like Nelson, saw change in how the pottery was decorated. He sorted the painted designs into five types and put them in order—a seriation—based on his ideas of how the designs had evolved (Kidder 1917). This technique, termed “phyletic seriation” (Lyman, O’Brien, and Dunnell 1997), had been used in Great Britain to order gold coins (Evans 1850) and in Egypt to order predynastic graves (Petrie 1899, 1901). Kidder was aware of these studies. He confirmed by means of stratigraphic excavation that his ordering was chronological.

While working at Pecos Pueblo, Kidder and his wife, Madeleine, mimicked Nelson’s technique—later referred to as “ceramic stratigraphy” or “percentage stratigraphy” (Lyman, Wolverton, and O’Brien 1998)—but used natural rather than arbitrary vertical units, and relative rather than absolute frequencies of pottery types (Kidder and Kidder 1917). The Kidders found that their ceramic types, like Nelson’s, tended to display unimodal frequency distributions over stratigraphically defined time. They reasoned that the types were good temporal markers because they occupied different positions in the temporal continuum.

While on leave from the University of California in 1915 to conduct ethnographic fieldwork for the American Museum, Kroeber visited Zuni Pueblo, New Mexico, to the west of where Nelson and Kidder were working. During his walks across the countryside he noticed great differences in the designs on pottery scattered on prehistoric sites as well as on sites that, based on the condition of houses and other architectural features, the Zuni had abandoned during more recent times. Kroeber also observed that some decorations showed up only on pottery from prehistoric sites, whereas other designs occurred on pottery from what he suspected were more recent ruins. Were the kinds of pottery really of different ages? Like Nelson and Kidder, Kroeber had identified a chronological problem, and to solve it he invented the technique known as frequency seriation (Kroeber 1916). He created several pottery types and used them to order his surface collections to reflect the continuous and gradual passage of time, as if one type slowly faded away while a younger type gradually increased in frequency (Lyman and Harpole 2002). To Kroeber, this image of time characterized how change usually operated—as a slow, continuous phenomenon. This is how he had described what Max Uhle had found in the California shell middens, and how for several more years he would view the entire North American archaeological record (e.g., Kroeber 1923).

Kroeber concluded that the temporal implications of the arrangement of his surface-collected assemblages could be confirmed through excavation. At Wissler’s direction, Spier (1917) continued Kroeber’s archaeological work near Zuni the next year. He seriated surface-collected assemblages based on the relative frequencies of pottery types and used percentage stratigraphy to test the chronological significance of his and Kroeber’s seriations. He also inferred that the spatial distributions of various pottery types reflected the movement of peoples across the landscape.

The notion that time was a continuum may have grown imperceptibly out of what became known as the “direct historical approach” (Steward 1942), the method Cyrus Thomas had used so successfully to debunk the moundbuilder myth in the 1880s (O’Brien and Lyman 1999b). It later was used by Duncan Strong (1935) and Waldo Wedel (1938) on the Great Plains, not only as a means of tracking the passage of time but also for identifying the
ethnicity of the people responsible for artifact assemblages (Lyman and O’Brien 2004a). The analytical protocol of the direct historical approach was simple: to construct a relative chronology of artifacts, one began with the most recent, or historically known, end of the chronological continuum and then simply worked backward in time, using similarity in artifact form or function as the basis for putting assemblages closer together or farther apart in time. The direct historical approach worked well in some cases for connecting the artifacts of ethnographical groups with those of late prehistory, but it broke down with increasing age.

It is not surprising that American archaeologists turned to the ethnographic record for help, trained as they were (and still are) in anthropology departments. During the first several decades of the twentieth century, few archaeologists were, in fact, specialists, but many anthropologists happened to do some archaeology. Kroeber is a prime example; Clark Wissler, Clyde Kluckhohn, and Julian Steward are others. More importantly, the training these individuals received meant that they were steeped in the ethnological theories of the moment. At that time there was a particular approach to doing anthropological, including archaeological, research. Anthropologists held to the notion that culture was transmitted over time and across space, and they came to refer to the unit of transmission as a culture trait or culture element (Lyman and O’Brien 2003a). This notion—we hesitate to refer to it as a theory—allowed Boas (1896) to reject the universal cultural evolutionism of Herbert Spencer, Edward B. Tylor, Lewis Henry Morgan, and others because he could show that different histories of migration, diffusion, and invention could produce the same constellation of culture traits that made up a particular evolutionary stage. This “induced investigators to trace the distribution and history of customs and beliefs with care so as to ascertain empirically whether they are spontaneous creations or whether they are borrowed and adapted” (Boas 1904:522).

The research program Boas initiated was originally known as “historical ethnology” (Goldenweiser 1925) and later as “historical particularism” (Harris 1968b). With few exceptions, Boas’s students and other advocates of historical ethnology took on virtually any competitor, as Robert Lowie (1912, 1918) did against the vestiges of universal cultural evolutionism and Alexander Goldenweiser (1916) did against diffusionism. It didn’t take long before problems with the basic method of historical ethnology were recognized (Radin 1933), though many anthropologists continued their attempts to work out the history of the particular culture(s) they studied and to improve various analytical techniques (e.g., Clements, Schenck, and Brown 1926; Steward 1929). It was within historical ethnology that the triumvirate of explanatory mechanisms—diffusion, migration, and invention (not to ignore other mechanisms such as trade)—became deeply ingrained in ethnologists as well as archaeologists. One tracked the spatiotemporal distributions of culture traits, whether behaviors or artifacts, to get at the history of a culture, which itself was little more than a constellation of traits. This was the conceptual baggage bequeathed to archaeologists: they were to write culture history and explain that history in terms of the movement of traits underpinned by ideas in the minds of the culture bearers (Lyman and O’Brien 2004b). The popularity principle stated by Nelson and adopted by culture historians was merely an explicit statement of the notion that culture was a stream of ideas transmitted over time and across space—what Binford (1965) would later call the “aquatic view” of culture change.

To return to archaeology, the true analytical revolution early in the twentieth century resided in the recognition that time could be measured using artifacts if those items were classified in particular ways, such as Nelson, Kidder, and Kroeber had done when they built pottery types based on changes in decoration. Stratigraphy played an important role in chronology building, but initially for confirmation rather than discovery. The principle of superposition as represented by stratified archaeological deposits was assumed to provide a valid chronometer
that could be used to check whether traits such as pottery decorations displayed unimodal frequency distributions across superposed strata. Inferences that these distributions measured the passage of time could be tested and empirically confirmed or refuted—actions later said by processualists to characterize a science. But stratigraphy did not long remain the test medium for chronology. When stratigraphy shifted to a medium of discovering time based on the principle of superposition, other aspects of archaeological epistemology shifted as well.

MEASURING TIME WITH UNITS OF DEPOSITION

With stratigraphic confirmation of Kidder’s, Nelson’s, and Kroeber’s seriated pottery sequences in hand, Clark Wissler (1917) proclaimed that a “new archaeology” had emerged—a term that would be used on more than one occasion over the next fifty years. It is unclear whether by new archaeology Wissler meant that time now could be measured continuously by means of changing artifact frequencies, or that superposed strata could be used as the basis for measuring the passage of time. We think he meant the latter, even though the use of superposition was anything but “new” in 1917. Despite the inventiveness of his own colleagues at the American Museum, Wissler himself never shook the view that distinct strata denoted discontinuous occupations by multiple successive cultures. Maybe the differences from one stratum to the next weren’t on an epochal scale similar to the cultural stages evident in Europe, but they were still distinct enough to signal that different cultures were represented.

Even Nels Nelson may have succumbed to the temptation of this view. Years after he worked in New Mexico, Nelson (1932:105) stated that the uninterrupted flow of the stream of culture could be monitored by observing “a few cross-sections of the flow taken at strategic points.” Note the words “strategic points.” Where were these? Were they so obvious—grand disjunctions, perhaps—that there could be no doubt where to monitor the flow, or were they selected by the archaeologist to best fit analytical needs? We think Nelson might have meant the latter, but we’re not sure. The reason for the uncertainty is that by the mid-1920s almost all archaeologists were coming to view those cross sections not as arbitrary boundary markers of the chronological continuum but as boundaries of real cultural units, often thought of as “occupations.”

The roots of this kind of thinking can be seen in the work of Manuel Gamio (1913) in the Valley of Mexico. Boas (1913) had Gamio excavate stratigraphically in order to test a suspected chronology of cultures as reflected in pottery (Figure 1.7). Gamio identified what amounted to “index markers”: pottery types diagnostic of particular cultures, each of which occupied a discrete stratigraphic position. Armed with marker types, one didn’t need to calculate relative percentages of artifact types to measure the flow of time. Now time could be carved up into separate periods, each defined by its own types. (This was very much in the tradition of French Paleolithic archaeology, which grew out of paleontology rather than anthropology [Chazan 1995; Sackett 1981; van Riper 1993].)

The next step was obvious: If marker types could be used as chronological indicators, couldn’t they also be used as cultural indicators? The answer, it seemed, was yes, and by the early 1930s the use of index markers, pottery or otherwise, as cultural identifiers was widespread in American archaeology. One of the methodological leaders in this effort was George Vaillant, whose work in the Valley of Mexico on Gamio’s “cultures” was sponsored by the American Museum (e.g., Vaillant 1935, 1937).

Regional syntheses, such as those provided for the Southwest by Alfred Kidder (1924, 1927) and Harold Colton (1939), focused on precisely this kind of dual unit that simultaneously allowed the measurement of time and the identification of distinct cultural units. This was problematic. On the one hand, that artifact types could mark the passage of time was testable initially with stratigraphic data and,
Figure 1.7. Manuel Gamio’s stratigraphic profile at Atzcapotzalco in Mexico City. Note the varied thicknesses of the arbitrary levels and the vertical distribution of different artifact forms (index markers), which Gamio assigned to one of three cultures (inverted U, V, and diamond symbols). (After Gamio 1913.)
shortly thereafter (in the Southwest), with dendrochronological data (Nash 1999). That artifact types might measure some sort of ethnicity, on the other hand, was not testable except in the present or very near-term past (e.g., Collins 1927). Why would archaeologists, who ostensibly wanted to test their ideas with empirical data, abandon their one criterion of science—testability? The answer to this question involves artifacts and how they are classified.

THE ARTIFACT CLASSIFICATION PROBLEM

Nobody would question that tools are made according to some conception of the final product, however vague and incomplete the concept in the artisan’s mind. If this were not the case, shapeless blobs of matter would result from human efforts to make tools. This holds true whether we are talking about inexpensive mass-produced shovels today or projectile points manufactured 5,000 years ago. The question that many archaeologists ask is, can we figure out what someone was thinking when he or she made a tool? Whether we can answer that question has long been debated in American archaeology, and we will come back to it at various points throughout the book. Here our interest is in how the question was handled in the pre-1960 period.

Early on, several archaeologists, including James Ford (1936) and Irving Rouse (1939), suggested that it might be possible to understand something of the mind of a prehistoric toolmaker, but they did not pursue the matter in any rigorous fashion, nor did anyone else. For starters, no one was sure how to test such a possibility. Ford (1954b) expanded on his contemporaries’ view of the unclear relationship between an artifact and the underlying conception of that artifact in the artisan’s mind by noting that even the artisan was not too clear about the relationship. Therefore, identifying prehistoric “mental templates,” as they came to be known thanks to James Deetz (1967), seemed less important to culture historians than other tasks, such as building cultural chronologies.

The possibility that mental templates underpinned artifact types, however, created a marked confusion over whether artifact types were analytical (etic) units or were somehow ethnologically real (emic) units (Dunnell 1980, 1986). The confusion is apparent in Alex Krieger’s (1944) discussion of typology. Krieger, like virtually all his contemporaries, was circumspect about how closely an archaeologist’s artifact types matched the artisan’s conceptions or cultural norms. His major contribution was to argue that a good type must pass the historical—significance test—it must have a continuous distribution in time and space. Nelson’s, Kidder’s, and Kroeber’s decoration-based types from New Mexico had that feature. To Krieger and other culture historians, spatiotemporal contiguity denoted the flow of ideas between and among people who were in contact with each other.

The critical issue for archaeologists was how prehistoric cultures were to be recognized. In general, these were defined as recurrent assemblages of types thought to represent culture traits (Colton 1939; Ford 1938; Gladwin and Gladwin 1930; McKern 1939). The fact that they were recurrent reinforced the notion that a set of shared ideas comprised a distinctive culture despite the fact that similarity was at least in part a result of how items were classified. The implication of recurrence was nonetheless a profound theoretical notion that again reflected the aquatic view. It locked prehistorians into a particular set of limited explanations (diffusion, migration, invention), and it suggested how artifacts were to be sorted and studied in order to explain the composition and distribution of cultures (Colton and Hargrave 1937; Ford and Griffin 1938; Gladwin and Gladwin 1934; McKern 1937).

One means of recognizing prehistoric cultures was by stratigraphic positioning, but remains were not always stratified. Index markers—artifacts that existed for only a brief time—were another means. They could be used across wide areas to identify artifact assemblages that belonged to the same culture. Index markers that closely resembled each other must
represent contiguity of the ideas that underlay their manufacture, such as when two flowing cultural streams of ideas had intersected. Similar types represented some sort of common ancestry, or homologous similarity. Or did they? Kroeber (1931) had noted the importance to paleontologists of distinguishing between homologous and analogous similarity, the latter denoting functional convergence rather than common ancestry. Archaeologists, however, failed to develop analytical means of testing empirical instances of this critically important distinction (Binford 1968a), despite a few efforts by ethnologists to do so (Lyman 2001).

Failure to develop a means to distinguish between analogous and homologous similarity resided in the lack of an explicit, well-developed theory of how cultures evolve that was applicable to archaeological materials. Early attempts to derive such evolutionary histories from the archaeological record used wording such as “one artifact type descended from another,” but any notion of relatedness in a genetic-like sense was largely metaphorical. When the wording became more literal and less metaphorical—as it did under the guidance of Harold Colton (1939; Colton and Hargrave 1937) and Harold Gladwin (e.g., Gladwin and Gladwin 1930, 1934), neither of whom had anthropological training—the reaction within the discipline was decidedly negative.

Gladwin used terms for his cultural units—roots, stems, branches—that implied a phylogenetic tree, but in the face of criticism he quickly abandoned many of the evolutionary connotations of such wording (Gladwin 1936). J. O. Brew (1946) published the death notice for such unabashedly evolutionary notions with his pithy insight that inanimate objects do not breed. Brew pointedly suggested restricting the term evolution to the transmission of genetic material. The result was a continued reliance on the notion that culture was best viewed as a braided, constantly flowing stream of ideas in which various cultural rivulets come together, break apart, and converge again in an endless, open cycle, thus demanding that mechanisms of culture change concern diffusion, migration, invention, and the like.

CONTINUING STRUGGLES

Whether time and space figured into archaeological classification was an issue separate from how classificatory units were actually used. With few exceptions—primarily the work in New Mexico by Nelson, Kidder, and Kroeber—almost all archaeological units, no matter the purpose for which they were created, were used as cultural identifiers of one sort or another. This was the tradition that went back to Gamio’s work in the Valley of Mexico and continued up through the classifications in the Southwest by Kidder (1936) and Colton (1939) and in the Southeast by Ford (1936, 1938). Ford was somewhat of an enigma when it came to cultural identification. Although he used artifacts (pottery in particular) as cultural identifiers, he typically stressed the arbitrariness of both the artifact types and the “cultures” archaeologists identified. This set him against other leading archaeologists, including James Griffin and Philip Phillips (O’Brien and Lyman 1998). Ford’s reasoning was simple: If the artifact types were artificial, and most archaeologists believed they were, then how could the “cultures” be anything but artificial?

Not everyone agreed that artifact types were artificial. Albert Spaulding (1953b), for one, suggested that statistical techniques could be used to discover types. Note that Spaulding did not say to “create” types but to “discover” them. In Spaulding’s view, if artifact types were discoverable, they must have had some reality in the minds of the prehistoric artisans. If so, then perhaps one could identify prehistoric societies. Further, because it was equivalent to a mental template, a type’s unimodal frequency distribution over time was readily (and in a way, theoretically) explained by the popularity principle. We suspect that Spaulding held the view that artifact types were more or less equivalent to culture traits, and culture traits reflected concepts in the minds of the culture bearer (Lyman and O’Brien 2003a)—an explicit
statement of normative theory as later described by Binford (1965).

Ford responded that this was nonsense (Ford 1954a), Spaulding (1954b) replied in kind, and Ford (1974c) produced a final statement on building types. Unfortunately, this statement conflated properties of types as real in an emic sense and properties of types as analytical constructs. Spaulding’s clear, concise statistical method found more favor than Ford’s among culture historians, particularly among prosen- sional archaeologists of the 1960s and ’70s. The latter is not surprising because Spaulding was one of Binford’s mentors at the University of Michigan (Chapters 2 and 3). Ford had no explanatory theory to undergird his position, and his discussions were often muddled. Also, his types for measuring time were built by trial and error. Spaulding couldn’t abide by such haphazard procedures and preferred statistical objectivity that could be warranted through reference to the fact that distinct cultures and ethnicities, however defined, were ethnographically visible (O’Brien and Lyman 1998). If they were ethnographically visible, then perhaps they were archaeologically visible as well. This caught the attention of anthropologically oriented archaeologists and contributed to what became known as “ceramic sociology” (Chapter 3).

One hallmark of the 1950s was the creation of a systematic means of classifying the ar- chaeological record on a large scale. The prime architects of the system were Philip Phillips and Gordon Willey (1953). The basic elements of the classification were two units—components and phases—that had been around in various guises since the mid-1930s (Lyman and O’Brien 2003b). Components were stratigraphically delimited aggregates of artifacts that were more or less equivalent to occupations, or communities. Phases were sets of virtually identical components and thus were the archaeological equivalents of societies. Operationally, the definitive attributes of components and phases were derived empirically from archaeological materials, not imposed on them on the basis of some theoretical model. As a result, the defini-

Because Phillips and Willey were concerned explicitly with culture history, they employed the by then typically used axiom that “typo-
logical similarity [denotes] cultural relatedness [which in turn] carries with it implications of a common or similar history” (Willey 1953a:363–364). They also discussed in depth two units that had been around informally for a long time and that Willey (1945) had defined formally. Each unit had clear implications for the flow of cultural ideas across time and space. Horizons were archaeological manifestations that had extensive distributions in space but limited distributions in time. Traditions were manifestations that had extensive distributions in time but limited distributions in space. Together they were the integrative units of choice that would provide the warp and weft to the tapestry of culture history, to use Rouse’s (1954) terms. Horizons suggested diffusion across space, and traditions indicated either cultural stability and persistence, or diffusion through time.

Willey and Phillips did not address how or why artifact styles should diffuse across space or through time, but no one else at the time did either. Incongruously, given their interests in what they called the “historical relatedness” of cultural phenomena, most archaeologists either did not believe or did not discuss the fact that theories of cultural transmission—the process that produces homologous similarity between artifacts and human behaviors through descent with modification—had much to offer. They ignored explicit statements by ethnolo-

ists regarding the influence of cultural transmission on culture change (e.g., Bruner 1956). Instead, the aquatic view served as a sort of commonsensical explanation for what were seen as formal similarities among archaeologi-

ical materials.
THE EVOLUTION OF CULTURE (AND OF CULTURES)

By the 1950s an alternative emerged, and it was not constrained by the need for theories of cultural transmission. This was cultural evolutionism, with two brands from which to choose: Leslie White’s (1943, 1959) and Julian Steward’s (1955). The choice depended on whether one was interested in the evolution of culture in general or that of specific cultures. If the former, White’s brand was better; if the latter, Steward’s brand was the obvious choice. Similarities between the two approaches were more apparent than real (Sahlins and Service 1960), although many anthropologists and archaeologists tended to misunderstand this key point. The works of both men would be highly influential in setting the agendas of the new archaeology of the 1960s, and thus we give them more than casual treatment.

White’s evolutionism was, and still is, the more difficult of the two to comprehend. For one thing, White was not a clear writer. For another, he was interested in several things at once, and in people’s minds these interests tended to blur. Thus White (Figure 1.8) often is remembered strictly as an evolutionist even though most of his theoretical writings had little to do with evolution. They had to do with culture and how to study it—what White (1949, 1959) labeled “culturology.” White first coined this term in 1947 and defined it as “the scientific study of the distinctive feature of man known as culture” (White 1947:210). Robert Carneiro (1981), Richard Barrett (1989), and others have argued persuasively that there is a disconnect between White’s cultural theory—captured best in his comment that “culture must be explained in terms of culture” (White 1949:141)—and his materialist-based evolutionary theory. It appears as if two different people were writing: one a culturologist interested only in the inner workings of culture, and the other an evolutionist interested only in culture as a progressive process. Recent examination of White’s ethnographic monographs and his field notes indicates an overlap between his ethnographies and his theory, but it was minimal (Whiteley 2003).

White’s followers tended to view his evolutionism as a new and radical way of explaining change in culture (with a big C), not changes in specific cultures (with a little c). White, however, did not see his evolutionism as anything particularly new. He always claimed that it did not “differ one whit in principle from that expressed in [Edward B.] Tylor’s Anthropology in 1881” (White 1959:ix). White could have easily included Lewis Henry Morgan (1877) and Herbert Spencer (1851, 1876). White was interested in cultural evolution as a progressive process, the key to which lay in how humans captured and used energy. To White, culture evolves as people harness more energy per capita per unit of time, or as they increase the efficiency of how the energy is put to work (White 1943). White (1949:390–391) called this his “basic law of evolution,” and he used it to explain every aspect of culture: “Culture thus becomes primarily a mechanism for harnessing energy and of putting it to work in the service of man, and, secondarily, of chan-
these interests were often evolutionary even in their simplicity and conduct of work—what White called the "feature of man." White first noted it as "the feature of man," and that there is a cultural theory—that "culture" (White 1920) and the cultural evolutionary spirit after taking graduate courses from White in the 1930s. Although Ford never really understood what White was advocating (O'Brien and Lyman 1998), he continually cited him when needing to define culture or explain why culture changed. As we shall see shortly, Ford would come under some heavy criticism for his Whitean notions.

Ford might have been impressed with White's evolutionism, but Julian Steward certainly was not. Steward (Figure 1.9) did his graduate work under the guidance of Alfred Kroeber and Robert Lowie at Berkeley, and it is likely that Kroeber, who focused in part on the relationship between natural environmental zones and cultural types (e.g., Kroeber 1939), reinforced Steward's interest in the ecology of cultures (Clemmer and Myers 1999; Kerns 2003; Murphy 1970). Exposure to Kroeber and Lowie also would have reinforced any latent notions Steward had regarding the importance of particularistic history. (We review Steward's cultural ecology more fully in Chapter 3.)

Ignoring the point that White's evolutionism would have fit comfortably with that of Tylor, Morgan, and Spencer (whom Steward labeled "unilinear" evolutionists), Steward assigned the label "universal" evolutionism to White's work. This was Steward's way of keeping it both distinct and distant from his own brand of evolutionism, which he labeled "multilinear." Steward (1955:19) defined *multilinear evolutionism* as being interested "in particular cultures, but instead of finding local variations and diversity troublesome facts which force the frame of reference from the particular to the general, it deals only with those limited parallels of form, function, and sequence which have empirical validity." For Steward, the "local variations and diversity" were what mattered in evolution. Identifying them was the evolutionist's first step, followed closely by matching them to the physical environment in which they occurred.

Steward expected to find concordances between such things as technology and environment, irrespective of geographic region, but he also expected to find nonconcordances. These he viewed as multiple, more or less distinct solutions to similar adaptive problems—hence the term "multilinear evolution." It was the differences that needed explaining, and in Steward's view such explanations were beyond White's evolutionism. Steward (1960) was particularly clear about this in his review of White's (1935) *The Evolution of Culture*. Steward (1937:101) was not shy about admitting that many of his own ideas rested on notions of "economic determinism" and thus that history mattered tremendously. But he also sought what he called "cultural regularities," or laws, and spelled out three requirements for identifying
regularities (Steward 1949:3): “(1) There must be a typology of cultures, patterns, and institutions... (2) Causal interrelationships of types must be established in sequential or synchronic terms, or both... [and] (3) The formulation of the independent recurrence of synchronic and/or sequential interrelationships of cultural phenomena is a scientific statement of cause and effect, regularities, or laws.”

In Steward’s view, laws of cultural processes, or simply cultural regularities, were detectable, but echoing his teachers, he said they should be built inductively. To illustrate what he meant, Steward (1949) offered a “trial formulation” of the development of civilizations in different areas of the world. To underscore that his formulation was based on multilinear evolutionaryism, he identified what he called “eras” and stated that these units “are not ‘stages,’ which in a world evolutionary scheme would apply equally to [all environments]. In these other kinds of areas, the functional interrelationship of subsistence patterns, population, settlements, social structure, cooperative work, warfare, and religion had distinctive forms and requires special formulations” (Steward 1949:23).

Thus, his “eras” were decidedly different than Whitean cultural stages, at least in Steward’s view. Eras were rank-ordered units that represented the process of cultural change and allowed cross-cultural comparison. In our view Steward’s eras were not much different than White’s stages.

Not surprisingly, given his emphasis on technology and the physical environment, Steward developed a strong following among archaeologists (e.g., Willey 1961; see Chapter 3). Unlike White, Steward spoke the archaeologist’s language. His articles and monographs—especially Basin-Plateau Aboriginal Sociopolitical Groups (Steward 1938), which he finished while working for the Bureau of American Ethnology—were well known in archaeological circles before Steward moved to Columbia University in 1946 (Murphy 1977). Knowing more than a little about archaeology, Steward would have been sympathetic to the archaeologist’s inferential needs, such as having relatively easy empirical access to technological and ecological aspects of culture (e.g., MacWhite 1956).

Linking technology via ecology with society and ideology allowed one to climb up the inferential ladder (Chang 1967a) with some security—from technology, the most straightforward and closely tied to the material record; to subsistence and economy; to social and political organization; and finally to ideology, the most tenuous and farthest removed from the empirical record. By the time Steward moved to the University of Illinois in 1952, his name was synonymous with the Shoshoni and Northern Paiute Indians—a synonymy that in the succeeding decade would make Steward a significant figure in American archaeology. Toward the end of his career Steward began to have serious doubts about multilinear evolutionary theory with respect to its “analytic or explanatory value” (Manners 1973:886).

Cultural evolutionism was on a lot of people’s minds in the 1950s (Carneiro 2003). Steward published Theory of Cultural Change: The Methodology of Multilinear Evolution in 1955, and White published The Evolution of Culture: The Development of Civilization to the Fall of Rome in 1959. Further, Marshall Sahlins and Elman Service edited Evolution and Culture (1960), which was geared toward reconciling the views of Steward and White, who were their mentors. Finally, the centennial celebration of Darwin’s (1859) On the Origin of Species was an event marked by anthropologists as well as biologists. That celebration yielded two works that had significant impacts on anthropology: Evolution and Anthropology: A Centennial Appraisal, edited by Betty Meggers and published by the Anthropological Society of Washington (Meggers 1959), and the three-volume Evolution after Darwin, edited by Sol Tax and published by the University of Chicago Press (Tax 1960).

The Tax volumes contain papers from across the disciplinary spectrum—biology, anthropology, sociology, and history. Not only do the papers form a historical statement of where evolutionism was a century after Darwin, they also are a reminder of the pluralistic nature of the
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term "evolution" (Carneiro 2003). Biologists have their meanings, anthropologists have their own, and so on. With one exception, the Meggers volume is limited to contributions by anthropologists. She assembled specialists from all four traditional subdisciplines, together with Ernst Mayr, an architect of the "modern synthesis" in biology that united paleontologists, geneticists, and naturalists under the Darwinian banner (Huxley 1942). The chapters in the Meggers volume reveal a surprising diversity of views on evolution. They show that despite the fact that anthropologists of all persuasions were interested in applying evolutionary principles in their work, there was a distinct lack of consensus as to what those principles were and how they could be applied. Equally clear is that anthropologists still embraced a clear separation between organic evolution and cultural evolution, just as they had in the post-Boasian era.

In the lone article by an anthropologist in the Meggers volume, William Haag took a conservative approach to evolutionism, noting that "it is only reasonable to assume that the field in anthropology that can most profitably use evolutionism to arrive at [a] new height is archaeology" (Haag 1959:105; see also Haag 1961). Haag had received his Ph.D. in ethnology at the University of Michigan, where he was greatly influenced by White. When Haag said that archaeology could profitably use evolutionism, he was referring to White's brand. Throughout the chapter Haag defended White against "errors" made by Steward in his criticism of Whitean evolution, pointing out that, in his opinion, Steward was more of a unilinear evolutionist than was any nineteenth-century evolutionist. Haag (1959:98) noted that Steward's brand of evolution was "quite adequate in its limited application and should be encouraged by emulation." There is nothing quite like damming with faint praise to make a point.

Another chapter in the Meggers volume is by Harvard ethnologist Clyde Kluckhohn. Based on the bulk of his writings, he is not usually considered an evolutionist, but it turns out that Kluckhohn had plenty to say about evolutionism. In fact, he was downright exuberant about its potential for advancing the goals of anthropology. He strongly advocated banning the "false biologic analogy," as Kroeber (1958:34) had put it, and "pressing parallels between different spheres" of inquiry (Kluckhohn 1959:154). He said that if there is "a certain orderliness in nature, it should not be surprising if some principles are found to prevail across the conventional boundaries of, say, biology, psychology, and culture. Indeed there is empirical evidence of still wider sweep. The same basic equation represents significant and similar relationships ranging from physics to economics...to linguistics...to biology...to culture" (Kluckhohn 1959:154).

It might appear as if Kluckhohn was suggesting that Darwinism is as appropriate a vehicle for examining language and culture as it is to examine the organic world. But that's not exactly what he was saying. The key words in the above quote are "similar relationships"; he did not say "identical relationships." A few paragraphs later, he states that "we should, on occasion, banish conceptual timidity and explore likenesses in process without regard for the traditional separation of disciplines" (Kluckhohn 1959:155). In using the terms "similar relationships" and "likenesses in process," Kluckhohn was saying that he viewed biological evolution and cultural evolution as parallel processes, not aspects of the same process. Cultural evolutionism was not much different in 1959 than it had been in the late nineteenth century.

This did not stop a few archaeologists from attempting to integrate one form or another of evolutionism into their analysis. Here we are not referring to instances of archaeologists paying lip service to evolutionism, but rather real efforts to use it as an explanatory framework. Betty Meggers (Figure 1.10) was one who throughout the 1950s and early 1960s showed a continuing interest in applying White's evolutionism (Meggers 1954, 1960, 1961). Her main interest was in trying to discover correlations between subsistence practice, especially
view of culture. Ford asserted that anthropologists study culture, not people. Cultural forms, he continued, are not created by individuals; new forms—whether of social customs, religion, or pottery—can come only from preceding forms (Ford 1962). This statement, which obviously followed White's (1949) views, echoed one Ford had made more than a decade earlier in the published version of his dissertation: "It is understood that individuals do not ‘create’ ideas. The concept of ‘free will’ seems to have no place in science. Individuals receive ideas from other humans, sometimes combine them, less frequently discover them in the natural world about them, and almost always pass them along to others" (Ford 1949:38).

Opler (1963:902) characterized Ford’s position as holding that “man has developed his enormous and intricate brain, his powers to remember and record the past, his abilities to probe the minute and the remote, his capacity for invention, communication, and planning, in order to remain a supermonor fit only to fetch and carry for Mother [Cultural] Evolution.” That was an interesting turn of words: Mother Evolution—that sounds similar to what Russians call their homeland, which is exactly the effect Opler wanted. Opler was, to coin a word, a Marxophobe. He saw the materialist hand of Karl Marx in everything White and his followers did.

In the same article in which Opler found Meggers’s use of White’s law strained, Opler tossed a hostile and vicious remark at her. As background, Meggers had defended White against Steward’s (1953:318) assertion that “White’s law of energy levels . . . can tell us nothing about the development of the characteristics of individual cultures.” Of course it can’t, replied Meggers, as White himself had noted: “the evolutionist’s formulas . . . are not applicable to the culture history of tribes and were not intended for this purpose” (White 1945:346; emphasis added). Anthropologists and archaeologists to this day still fail to realize, as did Steward, that White knew his brand of evolutionism had nothing to do with the “culture history of tribes,” as he put it. He was interested

Figure 1.10. Betty Meggers in her office at the National Museum of Natural History, 1972. (Courtesy Betty Meggers.)

agriculture, and social organization. In a volume honoring her former professor at Michigan, Meggers’s essay called on White’s law that energy capture (technology) exerts a strong influence on culture: “If agriculture is a significant culture-building force and environment is an important determinant of agricultural productivity, then it should be possible to find some correlation between the level of development that a particular culture has reached and the agricultural potential of the environment that it occupies” (Meggers 1966:306).

Cornell University ethnologist Morris Opler, in an article published in the Southwestern Journal of Anthropology, found Meggers’s effort strained because it “assumed that every series of variations has evolutionary significance” (Opler 1961:7). But then Opler found anything vaguely resembling evolutionism strained, especially if it was derived from the work of Leslie White. Meggers was not a unique target for Opler, who made it his business to comment on various uses of evolutionism by misguided anthropologists. In 1962 James Ford produced a short training manual aimed at South American archaeologists, and in it he expounded his
only in the evolution of culture, not specific cultures. As Eric Wolf (1960:150) put it when reviewing White’s 1949 book *The Science of Culture*, “in White’s terms, the study of the process of [natural] selection [working on individual cultures] would be history; the evolutionist would concern himself only with the construction of a genealogy of forms.”

Recognizing, as she phrased it, the “unanimity” of opinion between Steward and White over what White’s evolutionism could not do (and was not intended to do), Meggers (1960:302) had made the apparently innocent remark that “the effect of such unanimity has been to remove evolutionary theory from the practical tool kit of the field anthropologist to the high plane of philosophical discussion.” Here, we think, Meggers was concerned that Steward’s brand of evolutionism was eclipsing White’s brand for the very reason White had stated: his evolutionism had absolutely nothing to do with the “culture history of tribes.” This gave it an ethereal feel, whereas Steward’s multilinear evolutionism was something anyone could get their arms around and use. The purpose of Meggers’s paper was to rescue “evolutionary theory” (she meant White’s brand) from the high plane and to show how it could “be used as a guide in understanding the dynamics of individual cultures” (Meggers 1960:302).

This was clever phrasing on Meggers’s part, as she was attempting to show that White’s evolutionism had real-world, on-the-ground applications. Steward’s evolutionism, if we read between Meggers’s lines, was not a theory. White’s evolutionism, however, was a theory, and it could in fact be used to explain individual cases.

In commenting on her use of White’s law, Opler (1961:13) stated that “apparently the ‘practical tool kit’ Dr. Meggers urges upon the field of anthropologists is not quite so new as she represents, and its main contents seem to be a somewhat shopworn hammer and sickle” (Opler 1961:13). This was a particularly insensitive, even stupid remark to make, especially given the general mood in America at the time. Opler, however, had waited a long time to get back at Meggers for what he felt was a deliberate slight on her part fifteen years earlier when she failed to cite some of his work (Meggers 1946; see also Opler 1946 and especially Peace and Price 2003). Opler now saw a chance to put the knife in Meggers and White simultaneously.

Not even a decade earlier, much of the nation’s attention had been centered on the hunt for suspected Communists, first by the House Un-American Activities Committee, then by Joseph McCarthy’s Senate Permanent Subcommittee on Investigations. Anthropologists were not immune from the witch hunt (Nader 1997), and when Opler penned his remark in 1961, the wounds in anthropology were still raw from the outcome of the hearings. A number of anthropologists had come under fire for their alleged connections to the Communist Party, and several were fired by their home institutions as a result, including Morris Swadesh from City College of New York and Gene Weltfish from Columbia. Some of the rawness of the wounds was created by the fact that while this was going on, the American Anthropological Association sat on its hands and did nothing to support its members who were under suspicion (Price 1997).

Aside from the inappropriateness of the remark, Opler knew that it didn’t apply to what White, much less Meggers, was advocating. In the article baiting Meggers, Opler went on at length to show that in tracing their evolutionary genealogy to Tylor and Morgan, White and his followers conveniently skipped over several generations of Marxist thinkers, including Marx and Engels. Tellingly, on Opler’s list of Marxist thinkers were Vladimir Lenin and Joseph Stalin, two of the most feared individuals of the twentieth century. It did not trouble Opler that this made no sense intellectually. The “tenor” of White’s theory might have been Marxist (Layton 1997), and he might have been a member of the Socialist Labor Party (Peace and Price 2001; Shankman and Dino 2001), but White wasn’t a Marxist. There actually was very little of Karl Marx in anything White ever wrote. White talked about the three tiers of...
technology, society, and ideology, but he never discussed, at least in any incisive manner, the control of labor, unequal access to resources, or a dialectic between environment and the three tiers of culture. And if there was no dialectical materialism, then how could it legitimately be called Marxist? Opler, who had been a student of White's at the University of Buffalo in the late 1920s, was, as Marvin Harris (1968b:637) put it, "one of the relatively few figures in anthropology who may be reckoned as well-acquainted with Marxist theory." Opler thus knew that there was not a trace of dialectical materialism in White's work. He was simply red-baiting.

As long as one lumped White in with Lenin and Stalin, why not go further and add Nikolai Bukharin to the mix? Opler did. But putting Bukharin on the same list with Marx, Engels, and Lenin made about as much sense as putting White on it. Bukharin was a man whom Lenin (in his December 1922 bedside "testament") called an important theorist, but someone who never fully understood the dialectic. Opler undoubtedly knew of Lenin's remark, so why would he list Bukharin? Either Opler disagreed with Lenin, which we doubt, or he wanted to hang yet another red ornament on Leslie White's genealogical tree, with no regard for whether it actually belonged there.

Opler's remarks were met with outrage by some, and Meggers received numerous supportive comments from her colleagues, several of whom also wrote Leslie Spier, the editor of the Southwestern Journal of Anthropology, to register their complaint about the article's tone (Peace and Price 2003). Meggers responded to Opler, but she took the high road, again pointing out that the sole reason for writing the article was "because it seemed to me that too much attention is paid to talking about theory, defining terms, and tracing the history of ideas, and too little is devoted to seeing if or how theories work" (Meggers 1961:333). This more or less ended the Opler-Meggers affair, although Opler continued his jabs at White (e.g., Opler 1962), even labeling him as anti-Semitic (Peace and Price 2003). Meggers emerged from the affair unblemished. She would go on to more controversies, but these would be over strictly archaeological matters, such as the hypothesized movement of prehistoric people and pottery from Japan to Ecuador (Meggers and Evans 1962; Meggers, Evans, and Estrada 1965). Professionally, White fared well, too. The generally favorable reception that his ideas on culture and cultural evolution received during the 1950s presaged their reception in archaeology during the 1960s and '70s.

There is a footnote to the White-Meggers-Opler affair. The first issue of the American Anthropologist for 1962 listed as "president-elect" of the American Anthropological Association "Morris E. Opler, Cornell University, Ithaca, N.Y." We wondered if perhaps Opler's 1961 article had come out after members had voted on officers, or at least during the voting period, but it didn't happen that way. The article came out in the first issue of the year, being in fact the lead article in that issue. Balloting for society officers took place later in the year, just prior to the November meeting. By that time members would have read or heard about the controversy. Our guess is that many of them undoubtedly supported Opler, at least in private, or they didn't care one way or the other.

But that's only half the footnote. In the first issue of the American Anthropologist for 1963, in the line directly beneath the one listing Opler as president of the AAA, was the name of the new president-elect: "Leslie A. White, University of Michigan, Ann Arbor, Mich." In retrospect, it seems ironic that the leading anthropological society in the United States would be led, in back-to-back years, by one of the most right-leaning anthropologists in the country and then by one of the most left-leaning. Or maybe this was not as ironic as it seems. Perhaps it was just a classic example, albeit on a tiny scale, of the dialectic that so fascinated Marx and Engels. Regardless, the tug of war between right and left being felt in the AAA would, in a few years, extend to almost every corner of American culture.

Two people who did not join the movement toward evolutionism in anthropology during
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the 1950s were Gordon Willey and Philip Phillips, who made no mention of Leslie White in Method and Theory in American Archaeology. They did mention Julian Steward but focused strictly on the classification scheme he had developed a decade earlier for Middle America and the Andes (Steward 1948). Steward defined six "periods," which Willey and Phillips correctly pointed out were better thought of as "developmental stages," despite Steward's (1949) protestations to the contrary. They then worked out a similar classification for the entire Western Hemisphere, naming their "stages" Early Lithic, Archaic, Formative, Classic, and Postclassic. Willey and Phillips stated that theirs was not an effort to explain the archaeological record but rather an attempt to structure what was known about it. They also were careful to point out that their classification was in no sense an evolutionary one, although they could see where the reader might think it mirrored Steward's multilinear evolutionism.

Evolution, whether biological or cultural, entails change, and to measure it one needs very specific kinds of units. Willey and Phillips knew this, and they also knew that Americanists had not done a particularly good job of creating units of change, although it wasn't for lack of trying. Their own system provided a means of classifying archaeological phenomena, but it did not enable the measurement of culture change in any way other than the traditional one of stacking up in columns and arranging in rows prehistoric cultures called "phases." This procedure created units that were internally homogeneous, thus implying cultural stability within the spatiotemporal coordinates of the units, and forcing virtually all change into the borders defining the units (Plog 1973a, 1973b, 1974). Change was no longer continuous, as Jim Ford had envisioned it; rather, it was discontinuous and occurred only between periods of stasis of varying duration.

This concept sounded like what Kroeber and others had espoused so many decades earlier. In sum, the cultural units archaeologists created may have reflected actual periods of stasis and events of change, but given how they were created, it was virtually impossible to test if that was the case or if change had been more or less continuous. Whatever the case, it was the construction of spatiotemporal frameworks—what processualists would later somewhat inaccurately refer to as "descriptive" models—more than attempting to understand why those frameworks looked the way they did that occupied the energies of culture historians. Many of them were content with such work, but virtually from the beginning some were not.

THE SEEDS OF DISCONTENT

Units that allowed the segregation of formal variation in artifacts across the spatiotemporal dimensions had been designed and used by culture historians since the second decade of the twentieth century. Willey and Phillips wrote Method and Theory in American Archaeology to reduce the number of units and to specify the analytical utility of the few that remained. But the overarching reason was to nudge archaeologists out of their singular focus on time, space, and form into an emphasis on the processes that produced the archaeological record. The cultural processes Willey and Phillips had in mind were none other than the "diffusion-migration-invention" ones of their predecessors. But they also wanted more: they wanted archaeologists to consider the "cultural and social aspects" of the archaeological record (Willey and Phillips 1958:6). Thus, for Willey and Phillips, it was time to move beyond the description of artifact forms and distributions and to try to discern their anthropological implications.

Was this nudging on the part of Willey and Phillips something new? Hardly. Although the bulk of archaeological effort was directed toward answering the what, where, and when questions, the literature makes it clear that these were only the most basic goals held by many archaeologists working between 1915 and 1950. American archaeologists fully subscribed to ethnologist Roland B. Dixon's dictum that "archeology is but prehistoric ethnology and ethnography" (Dixon 1913; 153), but their efforts often fell short. Julian Steward and Frank
Setzler (1938) noted the general failure of archaeologists to interpret their data in terms of problems that were broad enough to be useful to the student of culture. Not surprisingly, given Steward’s interests in human interaction with the physical environment, they hoped for more discussion of subsistence and the relationship of the archaeologically represented culture to the physical environment. They also hoped for interpretations of artifacts in terms of their functional roles in the configuration of human behavior and activity, arguing that “surely we can shed some light not only on chronological and spatial arrangements and associations of [cultural] elements, but on conditions underlying their origin, development, diffusion, acceptance and interaction with one another. These are problems of cultural process” (Steward and Setzler 1938:7).

Similar pleas were made a few years later by John Bennett, who started out professionally as an archaeologist but switched to ethnology. Bennett (1943) believed, incorrectly we think, that efforts to reconstruct cultural dynamics from artifacts were not favorably received by many archaeologists. He believed that if archaeologists would pay as much attention to an artifact’s function as they did to its date of manufacture, they would be aligning their discipline more closely with ethnology. A few years later, Bennett suggested that one reason archaeologists avoided making inferences about cultural dynamics was that archaeology, “of all ‘sciences,’ must guard against speculative theory and imaginative hypotheses, since archaeology is the most profoundly ‘historical’ science of all, the discipline in which conclusion is most closely bound to the actual perceptible, touchable, countable evidence” (Bennett 1946:198). This, we think, is a more accurate statement than what Bennett had expressed a few years earlier. Archaeologists were hesitant to make inferences about cultural dynamics, not hostile to the idea. A similar sentiment was present in paleontology, a sister discipline of archaeology that is also—in Bennett’s terms—a “profoundly historical science” (Jepsen, Mayr, and Simpson 1949). Why should these two historical disciplines allegedly be so hesitant to write theory and test hypotheses?

Bennett (1946) blamed the reticence on an empirical tendency in archaeology, which he attributed to the interest in trait lists and trait-element studies of Kroeber and his associates at the University of California, Berkeley. Archaeology had been firmly entrenched in empiricism long before Kroeber and his students and colleagues began producing the culture-element distribution lists (e.g., Klimek 1935; Kroeber 1936). Further, archaeologists of the 1930s and 1940s were fascinated with trait lists, the construction of which was central to anthropological methods of historical ethnology (Lyman and O’Brien 2003a). Bennett suggested that any theory of historical continuity subscribed to, if only implicitly, by archaeologists using those methods was in no sense proved but was simply a good working hypothesis that allowed inferences about the past to be made. Similarly, any theory of culture involving the differences between folk and urban societies (e.g., Redfield 1947) “permits inferences about the meaning of archaeological remains because it assumes that culture objects have some structural relation to the culture and society producing them” (Bennett 1946:200). The gist of Bennett’s remarks would be echoed a few years later by Willey and Phillips, and slightly later by Binford and the processualists.

Bennett was not the only critic of archaeologists’ seeming preoccupation with trait lists and the like. Writing a few years before Bennett, Clyde Kluckhohn, another archaeologist-turned-anthropologist, similarly noted the reluctance of archaeologists to translate their data into something an ethnologist could appreciate. Kluckhohn (1939, 1940) suggested two reasons for this hesitancy. First, archaeologists feared that they would be shown to be wrong, and second, such conjectures were antithetical to the inductive historical approach that most archaeologists (and paleontologists) followed. This was, strictly speaking, an accurate assessment; archaeologists—like all historians and scientists—do work inductively at least some of the time (Chapter 4).
It seems to us that culture historians bought into this distinction because their anthropologist teachers had bought into it (Lyman and O’Brien 2004a).

Kluckhohn’s comments had minimal impact on American archaeology, but the same cannot be said of a lengthy critique made by one of his Harvard doctoral students, Walter Taylor (Figure 1.11). Taylor’s pointed remarks formed a large part of his dissertation, which appeared in 1943 and was later published jointly by the Society for American Archaeology and the American Anthropological Association as A Study of Archaeology (Taylor 1948). Taylor urged his colleagues to be more anthropological, but he departed from Kluckhohn in how he delivered his argument. Whereas Kluckhohn merely patronized archaeologists, Taylor lambasted them—at least some of them, including some of the most prominent. In so doing he did not make the logical error Kluckhohn had by asserting that archaeologists were not interested in ethnological issues. Rather, Taylor said that understanding culture was an important goal of archaeology—or so one would gather from what archaeologists said. Taylor concluded, however, that what archaeologists said and what they actually did were entirely different.

It was one thing for Taylor to take on his peers in print, but it was another to take on the likes of Alfred Kidder, who at the time probably was the most respected figure in American archaeology, and James Griffin (Figure 1.12), who had developed an outstanding reputation in eastern United States archaeology. Taylor wrote this about Griffin’s work:

[O]n a number of occasions [Griffin] has either stated or implied that the ultimate aim of archaeological investigation is to reconstruct the life of the past. But it is difficult to credit him with anything more than lip service to these ideals. Time and time again he has failed to make use of the data at hand, to synthesize into any sort of picture the information gathered from a site or area, to abstract from the material objects their meaning for the life of their former possessors, to
ARCHAEOLOGY AS A PROCESS

Despite such remarks by establishment archaeologists, the few who bothered to respond to Taylor (1948) (e.g., Burgh 1950) dismissed his arguments altogether. As Richard Woodbury (1954:293) put it in one of the few published reviews of *A Study of Archeology*, “it hardly seems a justifiable procedure to condemn a scholar, in archeology or in any other field, because his accomplishments fall short of his ambition.” And Taylor had certainly condemned Kidder, Griffin, Emil Haury, and several other prominent prehistorians. Those archaeologists who entered graduate school after World War II did not feel the sting of Taylor’s critique as had their mentors, and it seems that more than a few of the younger generation found Taylor to be inspirational. Lew Binford (1972b:2) referred to the used copy of Taylor’s book that he bought and that “still sits on my bookshelf—full of stratified marginal notes, reflecting something of the changes in my thinking since 1955.”

But *A Study of Archeology* was more than critique: it was Taylor’s (1948:93–94) introduction of the “conjunctive approach” as a means of turning archeology into prehistoric ethnology, defining the method as “the elucidation of cultural conjunctives, the associations and relationships, the ‘affinities,’ within the manifestation under study. It aims at drawing the completest possible picture of past life in terms of its human and geographical environment. It is chiefly interested in the relation of item to item, trait to trait, complex to complex…within the culture-unit represented and only subsequently in the taxonomic relation of these phenomena to similar ones outside of it.” In other words, the conjunctive approach looked primarily at the inner workings of a culture unit and only secondarily at how it might relate to other culture units in a taxonomic framework. It was an attempt to transfer the still-trendy British functionalism into archaeological practice, but Taylor offered little guidance, much less any concrete methods, for how to carry out the conjunctive approach.

interact in terms of broad categories such as subsistence and social groupings the information from archeological materials. He has consistently failed to follow up leads which might have led him to some significant information concerning the life of the people and has been content merely to investigate factors of taxonomic, i.e., comparative, significance. (Taylor 1948:81)

These were bold accusations coming from someone just out of graduate school, and as one can imagine, Taylor’s comments were not received warmly. Despite the valid parts of his critique, many archaeologists chose not to acknowledge them, at least in print. Perhaps most ironically, when published in 1948, Taylor’s remarks came at a time when some archaeologists were thinking about questions other than what, where, and when. Gordon Willey, for example, had earlier noted that once chronological matters had been dealt with, one should attempt to “give them flesh and body in the form of a full and ‘functional’ culture description” (Willey and Woodbury 1942:236).
Binford (1965) would later point out that Taylor’s approach rested on the aquatic theory of culture. In our view, Taylor gave that theory a much clearer expression than anyone else writing between 1920 and 1960. The important point is that the conjunctive approach rested on an ontology no different than traditional culture-historical archaeology. Perhaps for this reason, Woodbury and others made the obvious gambit of requesting a concrete example of how Taylor’s proposed puente would allow archaeology to attain the loftier goal of understanding the operation of extinct cultural systems. Taylor never produced one, although he intended to use his decade-long study of sites in northern Mexico as an example. Taylor’s failure to produce a case study was, in our view, hardly a reason to ignore his suggestions, many of which anticipated those of the processualists (Chapter 2).

As a footnote, the backlash to Taylor’s scathing and unmerciful critique of American archaeology (and archaeologists) did not preclude his obtaining a job. He was hired by Southern Illinois University at Carbondale in 1958 to develop a department of anthropology (Euler 1997). He had very few doctoral students over the years—five is the number we’ve heard—and in that respect he did not have much of an impact on archaeology. But he didn’t need it. No one else in the history of American archaeology has had, or likely will ever have, as big an impact with a single publication. In terms of the intellectual strategies we mentioned in the introduction, what Taylor did certainly worked, but it came at a very hefty price in terms of bitter feelings between Taylor and many of his colleagues.

Bitter memories sometimes die hard. In 1985, at the fiftieth annual meeting of the Society for American Archaeology in Denver, a panel of distinguished archaeologists reminisced in front of a packed house about how the discipline had changed over the years. Jerry Sabloff, the moderator, innocently asked if any panel members cared to comment on the furor that Taylor’s monograph had created forty years earlier. Several panelists made critical remarks, and Griffin, whom Taylor had berated so pointedly, snapped, “Harvard never should have given him a degree.” A few seconds later, Walter Taylor got up from his seat, which was toward the front of the ballroom, walked down the aisle, and left the room. He never again attended an SAA meeting. Taylor was seventy-two at the time, and Griffin, eighty.

Griffin undoubtedly would not have subscribed to Patty Jo Watson’s (1983:x) suggestion that Taylor’s motive in writing A Study of Archeology was “not to generate ill will but rather to stimulate examination...of aims, goals and purposes by American archaeologists.” Nor, we suspect, would Griffin have taken Taylor at his word when he stated in the foreword to the fourth printing of his book that he still believed that the critique was “of archaeological theory and practice, not of men” and was as “fair as I could make it” (Taylor 1968:2).

Regardless of Taylor’s motive, which we will never really know, his message was beginning to resonate. A Study of Archeology captured more attention than it might have otherwise because of its confrontational tone, but Taylor’s invitation to make archaeology more anthropological was not original. Nor would the invitation end with him. In one of the more quoted phrases from the preprocessual days, Philip Phillips, paraphrasing Roland Dixon’s (1913) earlier comment, remarked in 1955 that “New World archaeology is anthropology or it is nothing” (Phillips 1955:246–247). Willey and Phillips (1958:2) repeated that sentence in their book, and Binford (1962a) used it in the opening sentence of his seminal paper, “Archaeology as Anthropology.” This statement more than any other fuels our sense that by the early 1950s there existed a general restlessness in American archaeology. By that time extensive surveys had located thousands of sites, untold numbers of sites had been sampled, millions of artifacts had been placed into types, and numerous cultural chronologies had been built. Archaeologists, trained as anthropologists, were tired of the same old ends and saw anthropology as providing an avenue to something new.
To make archaeology anthropological, one had to study culture and its processes. Joseph Caldwell, of the Illinois State Museum, underscored that point in a *Science* paper, “The New American Archaeology,” published a year after Willey and Phillips’s book. Caldwell (1959:304) noted that the new archaeology was “tending to be more concerned with culture process and less concerned with the descriptive content of prehistoric cultures,” which he labeled “dull and uninteresting.” Caldwell’s message wasn’t news to most archaeologists. They had long believed that culture processes were important, but they typically used the traditional diffusionist sorts of cultural processes while erecting a basic time-space framework of cultural forms (e.g., Meggers 1955). At the time, these processes were still largely the ones many ethnologists discussed (e.g., Murdock 1956; Spindler and Spindler 1959). What, then, made Caldwell’s “new archaeology” so new?

**ON THE EVE OF PROCESSUAL ARCHAEOLOGY**

The 1950s witnessed an increasing number of efforts to understand the social systems represented by extinct cultures. Some of these were commonsensical, such as Albert Spaulding’s (1952:262) suggestion that the elaborately constructed Adena burial mounds in the Ohio Valley contained the remains of individuals of “great prestige,” whereas other individuals were simply cremated and buried in place. William Sears (1954, 1958), an expert on the southeastern United States, was more explicit about the potential social, ceremonial, and ritualistic meanings of the covariant associations of artifacts, elements of tomb construction, and earthen mounds at sites along the Gulf Coast. Other scholars sought different sorts of social information from the archaeological record. Paul Martin and John Rinaldo (1950) studied community-pattern data in the Southwest and drew up a developmental history of possible social structures based on change in such characteristics as room size, settlement size, and distributions of functional classes of artifacts.

Gordon Willey (Figure 1.13) himself had recently produced what can be considered the first systematic study of settlement patterns, noting that the Virú Valley, Peru, project involved “the study of human adaptation to the valley environment over a long period of time” (Willey 1953b:xvii). In a masterful stroke, Willey (1953b:1) made legitimate the study of how human settlements were distributed across the landscape when he argued that “settlement patterns are, to a large extent, directly shaped by widely held cultural needs, [and so] offer a strategic starting point for the functional interpretation of archeological cultures.”

A few years later, K. C. Chang (1958:298) sought to “work out an outline of objective procedure which will allow the archeological skeleton to regain its ethnological flesh and blood.” He advocated “world-wide cross-cultural survey” rather than simplistic ethnographic analogies, and was inspired by Willey’s settlement-pattern work to seek “some correlation between the settlement pattern of a dwelling site and the social grouping of its occupants” (Chang 1958:298). His goal was to identify prehistoric social units such as households and communities, and he urged that archaeologists do this “rather than identify archaeological regions and areas by time-spacing material traits, since cultural traits are mean-

![Figure 1.13. Gordon Willey, Virú Valley, Peru, 1946. (Courtesy Gordon Willey.)](image)
The “Old” Archaeology

In an article on social and religious systems, Sears (1961) made several points that anticipated nearly verbatim many statements that would appear later in the 1960s. First, he argued that different categories of evidence were required for different sorts of information on prehistoric social organization, and that collecting the various information would require different field techniques. Second, he noted that inferences and reconstructions derived from one class of data must be verified and correlated with those from other classes. In more modern terms, multiple lines of evidence and patterned covariation of variables would help confirm a hypothesis. Third, fieldwork should be guided by a problem so that the artifacts and contextual information collected are appropriate to the questions at hand. A feature such as a burial mound, for example, should be excavated and analyzed “as a fossilized ceremony rather than as a repository for pots, sherds, and bones” (Sears 1961:227). Finally, Sears stated that he believed the study of processes of change was archaeology’s most important goal. Similar statements would echo this rallying cry of processual archaeology.

Commentators on Sears’s article, as is often the case for articles given the unique treatment that Current Anthropology affords, were equally divided in the sense of favoring or finding fault with his arguments. Some suggested that to accomplish what Sears advocated required the use of what became known as ethnarchaeology (Chapters 5 and 7)—using modern ethnographic analogues to interpret the past. Several commentators noted that there appeared to be two goals of archaeological research. One, the historical goal, was readily identifiable; the other, the processual goal, was not so distinct. Sears was advocating the second goal, but he was simultaneously not advocating abandoning the first. This was a point with which all commentators seemed to agree, regardless of whether they thought the second goal was attainable. Sears again displayed remarkable prescience in his response to the commentators. His last sentence reads, in part, “there is no substitute for the use of the scientific method, applied through the theories and bodies of information which have been developed by cultural anthropology and through the techniques which are part of the stock in trade of a trained archaeologist” (Sears 1961:243).

Another sign that something new was afoot in American archaeology during the late 1950s was that Robert Ehrich found it necessary not only to inform his European colleagues, in a European journal (Ehrich 1961), of the uniquely American relationship between anthropology and archaeology but also to publish very similar remarks in a leading American journal (Ehrich 1963). In those two papers, Ehrich mirrors the comments of others who indicated that American archaeology minimally had two goals. The first was a historical one aimed at reconstructing the “actual history of cultural development both in general and in particular instances and to gain an understanding of the laws and processes which may be involved.” The second goal was a nonhistorical one aimed at studying the “actual functioning of individual cultures [and] the interrelationships of their parts” (Ehrich 1961:623, 1963:16). He referred to the latter as “paleoethnography” and suggested that one might compare sequences of historical development and paleoethnographies “to derive principles of culture change”—what he referred to as “paleoethnology” (Ehrich 1961:624, 1963:18). Paleoethnography and paleoethnology were done by means of ethnographic analogy. Ehrich cautioned against making too simplistic an analogy because, if anything, ethnographic data suggested there were no clear-cut, immutable correlations between, say, aspects of social organization and subsistence base.

Julian Steward had also advocated a cautious position when he discussed cultural variation, pointing out that sometimes there were concordances between cultures—distinct cultures arriving at similar solutions to similar ecological or adaptive problems—but other times there weren't. Had Ehrich pointed to a potential problem with Whitean cultural...
evolutionism? Maybe, but certainly not to White, who consistently maintained that he was interested in the evolution of culture, not in the evolution of cultures. Cultural variation from a historical perspective held no interest for White. Cultural variation of a functional sort, however, was of considerable interest to White and would become one of the phenomena many processualists would seek to study and understand (Chapter 2).

These cases are not just a few selected ones favored by a few individuals. The discipline as a whole seemed to be restless, as evidenced by the fact that the Society for American Archaeology sponsored, with substantial financial assistance from the Carnegie Corporation of New York, a series of seminars in 1955 on various topics such as culture contact, culture stability, and types of community patterning. As Robert Wauchope (1956:vi) noted in the preface to the volume resulting from the seminars, “I sometimes wonder whether the Corporation actually had confidence in the intellectual curiosity of archaeologists or whether they just wanted to see how stupid we really were, for over the past twenty years or so the stereotype of the American archaeologist has somehow come to be a pretty dull sort of clod, with most of his gray matter under his fingernails.” One reviewer of the volume, highly respected anthropologist Edmund Spicer (1957:186), noted that it reflected “the growing effort to utilize archaeological data in the general understanding of cultural processes.” Another remarked that all of the “participants have attempted to probe the limits of archaeological inference, as it can contribute to or extend culture theory, derived largely from ethnological sources” (Siegel 1957:925). To us, if not to some of Wauchope’s contemporaries, it appears that archaeologists in the 1950s also had gray matter between their ears.

Between about 1915 and 1950, most Americanists spent their time documenting formal, temporal, and spatial variation in the archaeological record. Although critics such as Walter Taylor argued that archaeologists had divorced themselves from anthropology, this was not entirely true. Archaeologists never surrendered the belief that their discipline was a special form of ethnology and thus could contribute to general anthropological theory. They wanted to be more than simply the tail on “an ethnological kite” (Steward 1942:342), but they were unsure of how to do that. Knowing that they had access to the entire time span of human cultures, they also appreciated that the materials they collected varied tremendously in age. Thus the first order of business was to gain control of the temporal dimension, after which they could attend to other issues such as social and political organization. Interest in such issues is evident in the early settlement-pattern studies strongly influenced by Gordon Willey’s work in Peru (Chapter 3).

Not surprisingly, many early attempts at “paleoethnography” (Ehrich 1961, 1963) were problematic. Some efforts rested on ethnographic analogy, which as a basic method in the archaeologist’s tool kit underwent some evolution and diversification itself (Lyman and O’Brien 2001c). The direct historical approach had been underpinned by what were termed “specific historical analogs”; cultures that were viewed as evolutionary descendants of the archaeological culture under scrutiny were used as the source of interpretive analogues. That practice changed in the 1950s when archaeologists added “general comparative analogues” to their tool kit (Chapters 3 and 7). These could be any ethnographically documented culture that occupied a similar environment and had a similar level of technology to the archaeological culture under study. Not by coincidence, Julian Steward’s multilinear evolutionism became a popular approach, and his study of the Shoshoni and Northern Paiute (Steward 1938) became the source of a widely used, general comparative analogue (Chapter 3). This loosening of the restrictions on the acceptability of interpretive sources produced not only a “new analogy” (Ascher 1961a) but also a growing interest in experimental archaeology (Chapter
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3). Both sources of interpretive models would come to play critical roles in the 1960s as processual archaeology arrived on the scene.

By 1960 the number of universities producing anthropologists specializing in archaeology was growing, up from the handful that had produced most of the Ph.D.s up to that point. It is difficult to say whether this contributed in any direct way to the growing pluralism in archaeology, but if nothing else that growth expanded the job market. This, in turn, meant

more professors in the classroom, which meant archaeologists came into contact with more students. This meant there were more minds to shape and more potential acolytes for anyone charismatic enough to take advantage of the situation. One such person was Lew Binford, who had some concrete ideas about how to save archaeology from itself. As we will see in Chapter 2, he would pick up plenty of help along the way.