CONTENTS

PART I

INTRODUCTION, BACKGROUND, AND OVERVIEW

CHAPTER ONE

THEN AND NOW

W. W. Taylor and American Archaeology

Allan L. Maca

> Petrified puddle ducks, Taylor said they were, the revered Alfred V. Kidder, Emil Haury, Frank H.H. Roberts Jr., William Webb, William Ritchie, James B. Griffin. Page after page, he tears apart their reports to argue disjunctions between avowed goal and actual performance. Neither before nor since has there been such a merciless exposure of cant, braggadocio, formulistic pronouncements, and naïve or unthinking procedures. Blood flowed in torrents from a host of gored oxen, and their bellowing could be heard throughout the land.
>
> ALICE KEHOE (1998: 97)

American archaeology was formally launched in 1935 with the creation of the Society for American Archaeology and its flagship journal, *American Antiquity*. Dissatisfaction with the status quo, however, was already in the air and grew significantly in the 1930s (e.g., Strong 1936; Steward and Setzler 1938). Then in 1940, Clyde Kluckhohn, a professor of anthropology at Harvard, raised the commentary to an assault level: he published a short, sharp critique of Mesoamerican—particularly Maya—archaeology, exposing the shortcomings of one of the more prestigious research programs in Americanist archaeology (Kluckhohn 1940). A few years later, Kluckhohn's friend and student, Walter W.

Taylor, built upon his mentor's assessments when he submitted his 1943 Harvard Ph.D. dissertation, titled "The Study of Archaeology: A Dialectic, Practical, and Critical Discussion with Special Reference to American Archaeology and the Conjunctive Approach." Several years later, having returned from the war, Taylor dramatically transformed his dissertation into the most stinging dissection of Americanist archaeology ever published, issued as Memoir 69 of the American Anthropological Association and titled simply *A Study of Archeology* (Taylor 1948). To this day, his book remains archaeology's greatest example of dissension in the ranks. It launched a new era in American archaeology, but it closed another and its author paid the consequences.

Taylor's monograph-length study provided a number of firsts: the first history (and historiography) of Americanist archaeology; the first complex examination of the concept of culture in archaeology; the first in-depth discussion of a theory of typology; the first substantial recommendations for a coherent program of Americanist method and theory; and the first major critiques of American archaeology, Maya archaeology, and the "pan-scientific" program of the Carnegie Institution. Many leaders in the field and their students saw the critiques as an affront (e.g., Burgh 1950; Woodbury 1954). They responded personally to Taylor's pronouncements and ridiculed him openly and furtively until the final decade of his life (Sabloff 2004; Longacre, this volume). Walter Taylor died in 1997.

This chapter provides background to what we might call the "case" of Walter W. Taylor. It places his dissension in the context of the last sixty years in American archaeology and serves as a general introduction to the volume as a whole.

INTRODUCTION

"Americanist," or "American," archaeology in the 1940s centered on archaeology in the Western Hemisphere, was largely based in the United States, but included archaeological research undertaken far and wide by those trained in the Americanist framework. This framework, or tradition for archaeological practice, was at that time based on the pursuit of a widely accepted, even standardized, program known as "culture history." It explored temporal sequences in archaeological data to ascertain the chronological depth and history of various societies in the New World and, to a lesser extent, the Old World. Through description and taxonomy of artifact assemblages, especially ceramics (e.g., Kidder 1927; McKern 1939), culture history worked to create localized cultural classifications for purposes of regional comparisons and integrations of data. Theory was not basic to research at this time. The pursuit of conceptual orientations and theory had negative connotations; it was considered speculation and discouraged (Kluckhohn 1939b: 333; 1940: 44; Willey and Sabloff 1993: 147). Writing about this situation in the 1930s, Kluckhohn (1939b: 333) noted, "To suggest that something is 'theoretical' is to suggest that it is slightly indecent." This was the intellectual climate in which Taylor's (fundamentally theoretical) work emerged. Moreover, the community was small and the social climate compressed.

After World War II, the field of American archaeology consisted not of the many thousands of practitioners we see today, but of many hundreds, most of whom were men and nearly all of whom were acquainted. Virtually everyone practicing archaeology at that time picked up Taylor's book (Woodbury 1954); they read his criticisms of then-current research and many tried—some unsuccessfully, others selectively—to comprehend the book as a whole. Readers were struck by the force of his critique and by the provocative and abstruse program for archaeological theory and method laid out in his "conjunctive approach," an ethnographic approach to archaeology that focuses on the construction of cultural contexts and the relationships and meanings deduced from analyses of diverse data sets. Although few young scholars dared to engage and build upon Taylor's approach directly, many took his formula to heart: some began to adopt many of Taylor's ideas while others experienced what might be called a change in conscience and orientation. The literature citing, discussing, and providing evidence for these trends is extensive and includes striking commentaries by dozens of archaeologists, including many of the field's leaders (e.g., Daniel 1950: 325; Willey 1953a; Mayer-Oakes 1963: 57; Dozier 1964: 80–81; Trigger 1968b: 532; Willey 1968: 51–52; Bayard 1969: 376; Trigger 1971: 323–324; Watson, LeBlanc, and Redman 1971: 21; Binford 1972: 1–14; Deetz 1972: 110; Schiffer 1972: 157; Flannery 1973: 48; Woodbury 1973b: 311; Willey and Sabloff 1974; Klejn 1977: 4, 9; Gumerman and Phillips 1978: 185; Thomas 1978: 231; Trigger 1980: 670; Watson, LeBlanc, and Redman 1984: 275; Ritchie 1985: 413; Spaulding 1985: 306–307; Deetz 1988; Willey and Sabloff 1993; Woodbury 1993: 148; Willey in Freidel 1994; Straus 1999: 295; Longacre 2000: 291–293; Binford 2001: 670; Quilter 2003: viii; Trigger 2006).

Present-day authors of textbooks and histories of American archaeology highlight Taylor's impact on what became the dominant scientific model in the 1960s and beyond, the so-called "New Archaeology." Centered on hypothesis testing and the use of evolutionary and ecological systems models, the New Archaeology made its greatest strides establishing archaeological methodologies that could link data to explanatory laws of culture change. As such, this program saw itself as a type of social revolution because it expected to be able to explicate universal human behavior—to derive, test, and prove cultural laws.

Taylor's program certainly set the stage for—some would say "inspired"—the New Archaeology, something I discuss at length toward the end of this chapter. The whole of Taylor's approach, however, never actually saw its full expression in the New Archaeology; rather, his proposals were adopted piecemeal, in subsets, or opportunistically by scholars over decades. Taylor's (1948) proposals emphasized theory (e.g., of reality) and social philosophy as much as methodology and method, and were explicitly anti-positivist. In this regard his basic epistemology simply differed from the positivism encouraged by the New Archaeology (as Watson notes, this volume). In the 1950s and the early 1960s there was a general silence regarding Taylor's proposals. After about 1968, however, there was a shift in terms of disciplinary recognition. It was at this point that the New Archaeology had taken root and several prominent advocates, secure in tenure or emeriti, began to admit more openly the impact of Taylor on their own influential work (e.g., Willey 1968, 1988; Binford 1972, 1983c; Binford in P. Sabloff 1998; Spaulding 1985; Deetz 1988; Willey in Freidel 1994; Longacre 2000). These discussions and dozens of others (cited above) help us to understand the research interests that Taylor's colleagues saw as basic to his conjunctive approach—interests, for example, in hypotheses testing, the concept of culture, a theory of typology, and the use of statistics, spatial analysis, environmental data, and non-artifactual data. These texts also clarify which of these interests were most attractive to the New Archaeologists and why and how they were borrowed. Other scholars writing at this time, attempting to move archaeology beyond the twenty-five year domination of the New Archaeology, acknowledged that Taylor developed innovations and ideas that are still worth considering and/or applying (e.g., Hodder 1986; Deetz 1988; Hodder and Hutson 2003). Combined, both the borrowed and still-emerging concepts demonstrate that Taylor's conjunctive approach has had unusual endurance and continuing influence.

A third more recent trend, found among those oft slandered Mayanists, also begs our attention and makes the timing and content of the present volume quite appropriate. Two distinct "schools" in Maya archaeology have adopted versions of Taylor's conjunctive approach as guides for and validations of archaeological practice. One of these focuses on the Postclassic period highland Quiche Maya (e.g., Carmack and Weeks 1981; Fox 1987) and has never taken to citing Taylor. The other, which I discuss in another chapter for this volume, is centered on the study of the Classic period lowland Maya (Fash 1994) and enjoys a special base of operations at Copan in Honduras (Fash and Sharer 1991). Beginning in the mid-1990s (i.e., Marcus 1995), this school began to cite their conjunctive research as the brainchild of Walter Taylor (e.g., Maca 2001, 2002; Canuto, Sharer, and Bell 2004; Canuto and Fash 2004; Golden and Borgstede 2004a; Sabloff 2004; Sharer and Golden 2004).

The visceral memories of Taylor's critique have died with many of the scholars who were alive when Taylor rattled the field. Yet as the Maya case demonstrates, Taylor's ideas remain current and gradually we are witnessing "conjunctive" research models traveling to other areas of the Americanist field, especially those centered on the study of complex societies (e.g., Joyce et al. 2004; Millaire 2004). As Mayanists struggle with their rationale for adopting Taylor, as well as with what he seems to have been telling us, other archaeologists and anthropologists continue to grapple with the vestiges of Taylor's message and where

American archaeology has journeyed since 1948 (e.g., Bennett 1998; Wylie 2002; Lyman and O'Brien 2004; O'Brien, Lyman, and Schiffer 2005; Trigger 2006; Hudson 2008). My coeditors and I present our book as a way for all interested readers to better acquaint themselves with the foregoing issues and phenomena, and to become more familiar not just with Walter Taylor, his work, and idiosyncrasies, but also with post–World War II American archaeology and a major case study in scientific dissension.

Walter Taylor's book remains in print after sixty years. This is exceptional for books on archaeology and another sign that Taylor's approach may yet find its full expression—or at least a warmer welcome. Nevertheless, our discipline remains at a crossroads: archaeology, now more than ever, is a fickle, negotiated ground for understanding who we are, where we have been, where we are going, and who has the right to decide. It is possible that the renewed interest in Taylor and the conjunctive approach is only resurgent and ephemeral. Whether we are seeing fleeting interest or a new dawn in conjunctive studies, our book looks forward to unprecedented and renewed discussions regarding history and theory in American archaeology and the diversity of perspectives we ought to expect and cultivate.

This chapter is a general introduction to Walter Taylor's famous book, *A Study of Archeology* (hereafter referred to as *ASOA*). Like the volume as a whole, this chapter addresses the reasons for, significance, character, context, and implications of dissension. The following sections provide a brief look at the tradition of "culture history" in archaeology, a discussion of Taylor's influences and mentors, and a substantial consideration of Taylor's (1948) book, its critique, and his conjunctive approach. I then examine Taylor's impact on the New Archaeology as well as the other waves of influence generated by Taylor's ideas, opinions, and research. I also include a "lessons" section, based on Taylor's example, provided for colleagues and students in the social sciences and, especially, for those pondering major critiques or reorientations of archaeological theory and practice.

AMERICAN ARCHAEOLOGY BEFORE WORLD WAR II

The Society of American Archaeology was founded in 1935 during the Depression-era "New Deal" administration of U.S. president Franklin Roosevelt (Griffin 1985). Many New Deal programs focused on building infrastructure and putting people back to work, and some of these required significant assistance from public archaeology—very much akin to the cultural resource management and salvage archaeology we see today. These included programs like the Works Progress Administration (WPA), Tennessee Valley Authority (TVA), and Civilian Conservation Corps (CCC) (Dunnell 1986: 23; Jennings 1986: 56; Willey 1988: 27–48; Willey and Sabloff 1993: 148; Kehoe 1998: 100). Dozens of young archaeologists cut their teeth on these excavations and benefited from the training provided by project directors such as Arthur R. Kelly and William S. Webb, the former an academic archaeologist, the latter an academy physicist who practiced archaeology and held joint appointments at the University of Kentucky beginning in the 1920s. Among the young archaeologists fresh out of college were future diehards like Gordon Willey and Walter Taylor, both of whom worked for Kelly in Georgia before going on to graduate school (Willey 1988: 27–48; 1994: 38). The American archaeology fraternity, as Dunnell (1986: 24) calls it, was indeed small at that time, and relatively few institutions provided professional training in archaeology. It was, however, a time of major changes during which amateurs took a backseat and large, well-funded institutions, such as the Carnegie Institution of Washington, came to dominate the field.

A general method for archaeology in the Americas developed around the turn of the century, largely as a result of advances in world archaeology tied to stratigraphy (the study of the superimposition of stratified deposits: e.g., Uhle 1903; and see Reyman 1989) and seriation (the study of changes in artifact styles and traits through time; e.g., Petrie 1899). This was something of a revolution—the original "new archaeology" (Wissler 1917)—and drove a standardization of goals and approaches, as well as comparability of results (Dunnell 1986: 26–27). By the 1920s and 1930s, these practices characterized Americanist archaeology and provided the baseline for work conducted by A. V. Kidder at Pecos, beginning in 1915–1916 (Kidder 1924), and continued onward through George Vaillant's fieldwork in the Valley of Mexico (1930), J. A. Ford's in the Southeast (Ford 1936, 1938; Ford and Willey 1940), W. C. Bennett's in South America (1934), and H. B. Collins's in the Arctic (1937). Ultimately, this led to a standard means for the definition of "type," a marker among artifact categories that allowed the study of spatial and temporal distributions (Krieger 1944). After 1929, where dendrochronology, or "tree-ring dating," was possible, such as in the American Southwest, types were more tightly controlled and narrowly defined. Elsewhere, seriation remained the central means for determining temporal distributions and the construction of chronologies. This ability to order the chronology of archaeological materials and to define types and their distributions became the mainstay of what is referred to as "culture history," an approach that became so prevalent that it has come to define an entire era of American archaeology (variously referred to as the "natural-history stage" [Caldwell 1959: 303]; "Descriptive-Historic" period [Willey 1968]; the "classificatory-chronological" period [Trigger 1980: 670]; and the early "Classificatory-Historical" period [Willey and Sabloff 1993: 96–151]).

The goal of building chronology was the centerpiece of pre–World War II practices and was embodied in the culture historical approach. Its resolution was aided by the introduction and ultimately widespread use of arbitrary Cartesian grids for survey and excavation and, on the New Deal projects, standardized field forms for measurements and observations. The formal practice of American archaeology at this time took root around a set of goals and methods that were consensual and the field entered what some (e.g., Dunnell 1986: 29) refer to as a highly productive "normal science" phase (sensu Kuhn 1962). Watson (1986: 450) notes that this was an era wherein the archaeological record was viewed as a direct reflection—reconstruction—of the past. Thus, a certain optimism emerged, shaped by the earlier introduction from Europe of positivism (Patterson 1986: 12), a philosophy of knowledge based on the scientific method and principles of verification (ibid.; Preucel 1991: 18–19). With the coalescence of the field around these principles and methods, archaeologists could begin to make testable statements, at least with respect to chronology (Dunnell 1986: 29).

The growth of culture history reduced the diversity of methods and procedures as this program bore verifiable and comparable results. The definition of types among archaeological units was almost wholly based on stylistic traits, the recording of which mainly reflected archaeologists' interests in discerning similarities and shared features of archaeological assemblages (as opposed to variations within and among them). Thus, American archaeology at that time centered on averaging traits to arrive at cultural norms (the so-called "normative" approach); the study of their distribution was then linked to processes that could explain shared aspects of material culture: for example, diffusion, trade, persistence, and migration (Dunnell 1986: 31). This further supported culture history as a coherent and consistent program organized around the study of the distribution of normative traits. Many authors commonly refer to the culture history period as the pursuit of "time-space systematics," that is, "mere chronicle, working out the geographical and temporal distributions of archaeological material and explaining changes by attributing them to external factors grouped under the headings of diffusion and migration" (Trigger 1989: 276).

Culture history was an effective program, tightly defined, that achieved what it set out to do. It has been so effective, in fact, that it is still the first step in research for much of American archaeology. Nevertheless, its results were limited and the range of questions that could be asked of the material record was quite narrow. For example, because the methods and methodologies were standardized and self-affirming, there was a lack of interest in theory construction and in concepts that could validate the approach in terms of larger, more abstract social, cultural, and/or historical goals. Although the practice of culture history endured, critiques appeared almost immediately after the 1935 creation of the Society for American Archaeology and the formal emergence of the discipline.

CRITIQUES OF THE CULTURE HISTORY APPROACH

Walter Taylor's (1948) book dealt a blow—arguably the fatal blow—to prewar American archaeology and its pursuit and production of strict culture history.

Simply skimming the chapters of *A Study of Archeology*, we gain a sense of the length and complexity of Taylor's contribution and can immediately understand, viscerally even, the weight of his diagnoses and prescriptions; subsequent sections of this chapter take us through this in some depth. Although the force and character of *ASOA* are unique in archaeology's history, however, it is important to note that much of the book's content and spirit did not appear ex nihilo. Rather, as if on the "shoulders of giants," Taylor drew from the theories, methodologies, and/or dissatisfactions of many senior and contemporary scholars—philosophers, ethnologists, archaeologists, and historians among them. In American archaeology immediately before the war, for example, there appeared several sharp article-length critiques of the field. Although these are all brief statements, we can find in them many threads that are later woven into Taylor's work. These include (but are not limited to), on the one hand, dissatisfactions with mere chronology and taxonomy and with the legacy of antiquarianism (i.e., dilettantism) and, on the other hand, recommendations for pursuing theory and holism in general and, more specifically, functionalism, context, culture process (or culture change), and human ecology. These short critiques were penned largely by prominent scholars of archaeology and ethnology working in the United States and effectively characterized the tensions emerging in prewar Americanist archaeology and anthropology.

The first of these critics is William Duncan Strong, the well-known archaeologist from Columbia University and one of the principal mentors of Gordon Willey. Strong offered what many regard as the earliest call for a reappraisal of then-current practices (Strong 1936; see Bennett 1943: 208n3; Willey and Sabloff 1993: 154). A proponent and teacher of the culture history approach, Strong nevertheless had sincere interest in matters of a theoretical nature (see Willey 1988: 84). His 1936 paper encouraged archaeology's relationship with anthropology, not least by suggesting that archaeologists draw from ethnology's interests in culture change. This places Strong among the early processualists and highlights for us one of the important emerging issues at that time. Perhaps of even greater significance in Strong's article, however, especially given the tenor of Taylor's later critique, is the following statement: "Middle America, the cradle of New World civilization, is at present a dark jungle of ignorance lit up at long intervals by tiny match-flares of scientific knowledge" (1936: 367).

Attention to the shortcomings of Middle American archaeology is central to Taylor's (1948) book, as well as to Kluckhohn's (1940) critique. Strong, however, was not himself a Middle Americanist and saw fit to cast gentle aspersions on numerous regions of archaeological inquiry. His sentiment regarding Middle American archaeology was nevertheless shared and discussed in a 1937 article by Alfred Tozzer, one of the leading Middle Americanists of the day and a professor and dissertation advisor to Walter Taylor. Tozzer's paper offered many complaints common during this period of time regarding, for example, an overabundance of facts in American archaeology and an absence of explanations (e.g., 1937: 159). However, Tozzer also focused specifically on Mayanists, noting that they have not come close to achieving a "social history" in any area of the Maya region. Following this, and perhaps regretting his impolite words, he went on to say, "May I be forgiven by my colleagues for exposing our ignorance" (ibid., 157). Strong's and Tozzer's formal complaints about Middle American archaeology show clearly that well before Kluckhohn and Taylor, there was dissatisfaction with the Middle American and, in particular, the Maya fields. The fact that Tozzer was voicing these should help us to better understand his relationship with (and influence on) Taylor. Other authors who were critical of American archaeology at this time also had crucial ideas and proposals, but theirs differed somewhat from those of Strong and Tozzer by focusing more, for example, on issues of functionalism, context, and human-environmental interactions.

A frequently cited example of early dissatisfaction with culture history is a seven-page article, published in *American Antiquity* in 1938, by Julian Steward, an ethnologist, and Frank Setzler, an archaeologist. Their pairing exemplified the importance for archaeology of an anthropological perspective and their proposals encouraged archaeology "to complete the cultural picture" (Steward and Setzler 1938: 8), that is, to cover much of the terrain standard to ethnologists: cultural-environmental interactions, settlement contexts, subsistence and carrying capacities, and, of course, culture change. They were explicit in calling for methodologies geared toward more than mere chronology and taxonomy, noting, for example, that "[c]andid introspection might suggest that our motivation is more akin to that of the collector than we should like to admit" (ibid., 6). Setting the tone for an important theme in Taylor's famous critique (1948: Chapter 3), Steward and Setzler (1938: 5) wrote, "We believe that it is unfortunate for several reasons that attempts to state broad objectives which are basic to all cultural anthropology and to interpret data in terms of them should be relegated to a future time of greater leisure and fullness of data" (cf. Woodbury 1954; Willey and Sabloff 1993: 164, 209n15). In other words, they argued that problem orientation and a change in practice were needed immediately.

Other important articles were published by Aarne M. Tallgren (1937), an archaeologist at the University of Helsinki, Finland, and John W. Bennett (1943), an American archaeologist and ethnologist, in *Antiquity* and *American Antiquity*, respectively. Both papers sought explicitly to encourage a more functionalist approach in archaeology (something Taylor also attempted to do, not least by drawing on the work of Ralph Linton, discussed below). Tallgren and Bennett also were aware of the importance of an ethnological approach in archaeology (e.g., Bennett 1943: 219) and of developing more appropriate theoretical perspectives in general. Tallgren, for example, wrote, "One must be bold enough to cast doubt both upon the theories of others and upon one's own, and even upon the foundations of one's own science and its method, if one is to achieve a criticism that is not barren but alive" (1937: 154). This certainly anticipates Taylor's later recommendations and, as we might expect, Taylor's (1948) monograph cites Tallgren, as well as Bennett, Steward and Setzler, and Strong. Taylor was indeed a product of the archaeology of his time and of his graduate studies and training in anthropology and archaeology at Harvard. Although some have used this fact to belittle Taylor's innovations (e.g., Willey and Phillips 1958; see Chapter 16, this volume), it is clear that his larger vision owed an even greater debt to ethnologists of his day and, especially, to the powerful critique presented by one ethnologist in particular, Clyde Kluckhohn.

Criticisms leveled at archaeology from within, including the above-mentioned five papers, did not have much impact in terms of modifying in any significant or clearly identifiable way the nature of Americanist archaeological practice: these were more polite commentaries and pleas than outright critiques; and at that time the culture historical approach did what it did so well that relatively few saw any point in changing. In 1940, however, in a paper titled, "The Conceptual Structure in Middle American Studies," Clyde Kluckhohn stepped up the intensity of criticisms by taking aim directly at "Middle American" (or today "Mesoamerican") archaeology, focusing largely on research conducted in the Maya area. The main theme of his paper was expressed a year earlier in "The Place of Theory in Anthropological Studies" (Kluckhohn 1939b), but the 1940 paper received more attention because of the specificity of its selected targets and it remains to this day a widely read and cited paper in American archaeology (e.g., Willey and Sabloff 1993: 155–156; Longacre 2000; Golden and Borgstede 2004b; Trigger 2006: 367, 401; Leventhal and Cornavaca 2007; reprinted in Leone 1972a: 28–33). Kluckhohn focused his attention directly on archaeology and openly criticized not just the field of Maya archaeology as a whole but specific individuals and institutions. The paper was not particularly cutting or caustic (as Taylor [1973a] demonstrates), but it repeatedly made the point that Americanist research utterly neglected theory. Kluckhohn also included pithy, biting phrases (similar to those we would later see from Taylor), such as "[f]actual richness and conceptual poverty are a poor pair of hosts at an intellectual banquet" (Kluckhohn 1940: 51).

Although Kluckhohn's paper is today considered a landmark or a landmine among prewar critiques of archaeology, its impact was limited at the time. The reasons for this are partly because of Kluckhohn's position outside of American archaeology—he had done archaeology but was considered an ethnologist—and because of the paper's short length and relatively obscure context (Hay et al. 1940). Kluckhohn clearly had in mind a broader critique of American archaeology, well beyond Middle America, but the shot at Maya archaeology was certainly too narrow to be as influential as he had hoped. His paper was read by many and is remembered and reexamined cyclically; however, its greatest impact was not directly on the field of archaeology but on his precocious student and friend, Walter Taylor.

WALTER WILLARD TAYLOR: GENERAL INFLUENCES

Walter Taylor entered the Ph.D. program in anthropology at Harvard University in 1938, concentrating in archaeology. He studied with an array of faculty, including Alfred Tozzer and John Otis Brew, but his primary mentor was unquestionably Clyde Kluckhohn. Taylor (1973a: 29) writes, "For twenty-four years, by osmosis and slow filtration, his influence seeped in and sometimes out, and what is Clyde Kluckhohn and what is myself today I cannot say." Kluckhohn allowed Taylor to sit in on his classes but only permitted Taylor to actually register for them twice as an auditor (Taylor 1973a: 24). This arrangement undoubtedly owed to their close friendship and the fact that, when not in the field, Kluckhohn was all business.

The two met in New Haven in the mid-thirties. Taylor was an undergraduate at Yale University and Kluckhohn had gone there to work with Edward Sapir, the structural linguist (ibid., 23). Even quite early in his career Kluckhohn was known as a theorist and critic, something that often left his colleagues irritated and nervous and was burdensome to him. He advised his young friend to follow a different path, but Taylor admired the "edge" that Kluckhohn possessed and so, not surprisingly, adopted the same orientation to academia (see Kennedy, this volume).

Taylor entered Harvard at Kluckhohn's urging and spent the summers from 1938 to 1940 working with Kluckhohn (and others) in the Southwest. It was during this time that their "tutor-friend" relationship was cemented and that Taylor became increasingly adept at discussing and arguing anthropological theory. In the Southwest, ruined kivas and late nights served as backdrops to their conversations (Taylor 1973a). The camaraderie continued in Cambridge, albeit much narrowed because of busy schedules, and was expressed at post-work gatherings each Saturday evening (ibid., 25). These Boston and Cambridge outings, usually enjoyed by several couples, were formative for Taylor, not least because he was typically the only archaeologist present.

Direct influences on Taylor during the pre–World War II period are not known in any complete way; for example, Taylor's book briefly cites prominent British archaeologists Vere Gordon Childe and Grahame Clark (see Dark, this volume), but the extent to which these men's ideas influenced Taylor is uncertain.[1] Beyond Kluckhohn, there are several pivotal figures whose mentorship Taylor cites (Taylor 1948: 9–10) and/or whose influence is traceable. At Yale, he was instructed by the archaeologist Cornelius Osgood and derived many of his ideas for a "conjunctive" archaeology via discussions with him between 1931 and 1936 (see Kehoe, this volume). It was also at Yale that Taylor met Leslie Spier (Euler 1997), a Boasian anthropologist from whom he learned much about the culture history approach in archaeology. Taylor held Spier in high regard and, with SIU colleague Carroll Riley, he edited a book dedicated to Spier (Riley and Taylor 1967) and wrote one of the chapters.[2] He also clearly learned much from Alfred Tozzer, whose course on the Maya was an inspiration for Taylor's extraordinary 1941 article on the Maya Ceremonial Bar (see Joyce, this volume). Although Taylor and Tozzer may have disagreed on aspects of how to approach archaeology, it seems they had a cordial and supportive relationship (Taylor 1948: 9; and see note 3, this chapter). Tozzer was one of Taylor's dissertation committee members, and it is an intriguing fact that Tozzer is not once cited in Taylor's 1948 book. This is such a glaring omission that we may assume it was intentional, to avoid implicating Tozzer in the criticism of his fellow Mayanists. Benedetto Croce was another of Taylor's important influences. He was an Italian philosopher of history and one of the leading social theorists in the world before World War II; where and how Taylor discovered his work is unknown. Also, Lyndon Hargrave, the Southwestern archaeologist, imparted to Taylor many of his ideas on the archaeology of northern Arizona (Taylor and Euler 1980; Euler 1997; Kennedy and Fowler chapters, this volume). None of these mentors and scholars, however, had the influence of Kluckhohn.

Clyde Kluckhohn was a complete anthropologist and exposed Taylor to the full range of anthropological thought, as well as to philosophy and psychology and, especially, the writings of Ralph Linton. Kluckhohn's specific contributions to Taylor's thinking are discussed in several other chapters in this volume (e.g., Kennedy, Joyce, and Maca), but it is worth focusing briefly here on a few of Kluckhohn's penetrating ideas, particularly as they pertain to Taylor's preparation of *A Study of Archeology*. He shaped Taylor's thinking both through ideas that Kluckhohn himself had been developing and through exposure to the writings and ideas of others. During the prewar period, he was one of the important scholars involved in trying to define and apply a concept of culture for anthropology (Kluckhohn and Kelly 1945; Kroeber and Kluckhohn 1952; see also Watson 1995; Bennett 1998; cf. White 1959a). For Kluckhohn (and later for Taylor) culture was *the* primary goal, guide, and consideration of anthropology and his ideas on this subject were heavily influenced by his exposure to psychoanalysis while a student in Vienna from 1931 to 1932. This developed into sincere interests and research in clinical psychology later in his career and influenced Taylor's thinking on the mentalist (or ideational) basis of culture (Taylor 1948: 97–112; and see below).

Kluckhohn also imparted to Taylor ideas regarding the importance of theory and conceptual structures for guiding research. Taylor (1973a: 18) explicitly mentions the significance for him of Kluckhohn's premier paper on this subject (Kluckhohn 1939b), a paper that Taylor does not cite in *ASOA* and that is often overlooked by archaeologists because of the stir caused by the later Maya paper (Kluckhohn 1940). Kluckhohn's 1939 piece stated and then supported with illustrations his impression that "American anthropologists . . . are still devoting an overwhelming proportion of their energies to the accumulation of facts" (1939b: 329). This explains for Kluckhohn his equally important observation that "not until 1933 did a book by an American anthropologist include the word 'theory' in its title" (ibid., 328). The development of a theoretical structure for American archaeology is so central to Taylor's 1948 book that the first page of his introductory section spends a paragraph broadcasting and setting up the problem of the absence of theory. On the whole, his treatise is a sincere exploration of workable theory for the field, and this owes in great part to Kluckhohn's influence. Taylor also employed more concrete elements of Kluckhohn's thinking, seen, for example, in his wholesale borrowing of Kluckhohn's definitions for the terms "theory," "method," and "technique" (Kluckhohn 1940: 43–44, cited in Taylor 1948: 8).[3] Taylor thus adopted and developed the vision and mission of Kluckhohn, as well as the language to pursue them.

Thanks to the exchange of information among scholars that has accompanied the production of this volume (see Reyman, Table 11.1, this volume; Joyce, this volume), we now know that Kluckhohn exposed Taylor to the Harvard philosophers, Alfred N. Whitehead, Willard V.O. Quine, and Charles S. Peirce.[4] This knowledge makes it much easier to comprehend several of the analytical strategies of Taylor's thinking (e.g., 1941a, 1948), including especially his interests in language and logic. While at Yale, Taylor would have been exposed to the work of Kluckhohn's friend Edward Sapir. Sapir developed an anthropological approach to the structural linguistics of semiologist Ferdinand de Saussure (1857–1913) and remains known for the Sapir-Whorf hypothesis regarding the relationship between language and culture. A major challenge for historians of archaeology will be to assess Taylor's knowledge and use of research in structuralism and semiotics conducted by Peirce, Saussure, Sapir, and others. Taylor had an acute sense of the media (language and writing) through which archaeologists communicate, and it is intriguing that, outside of archaeology, the term "conjunctive" is best known in linguistics and philosophy (associated with grammar and logic, respectively).

Taylor's interest in language also may explain why much of what he proposed flew right over the heads of many scholars of the day. His prose requires multiple readings, not unlike some of the more intransigent work of French postmodern philosophers.[5] It is fascinating that some of Taylor's ideas foreshadow aspects of postprocessualism, a facet of postmodernism in archaeology thirty-five years ahead of its emergence. Reyman (this volume) suggests that Taylor's teaching philosophy and methodology in the 1960s paralleled the "deconstructionist" approach of Jacques Derrida and others. Indeed, the conjunctive approach and some recent theories included under the heading of postprocessualism may derive from related schools of philosophical thought, albeit at different moments in the twentieth century.

Another of Kluckhohn's important influences on Taylor was his interest in the writings of Ralph Linton, the cultural anthropologist. Joyce's chapter in this volume mentions that Taylor was exposed to Linton through a class with Kluckhohn at Harvard. Other chapters by Clay and Schoenwetter (in this volume) discuss Taylor's own teaching of Linton's book *The Study of Man* (1936); for example, it was a core text in his introductory classes for undergraduates as well as his graduate seminars on method and theory. Moreover, Taylor cites Linton extensively in his 1948 book and even a quick perusal of the sections mentioning Linton demonstrates the deep intellectual debt Taylor owed him.

Ralph Linton (b. 1893) and Clyde Kluckhohn (b. 1905) each had significant archaeological experience early in their careers before leaving archaeology to pursue ethnographic research. In the early 1900s, the connection between archaeological and ethnographic investigations, in terms of goals and practices, was more pronounced and many anthropologists found themselves doing both. Kluckhohn's archaeological fieldwork was based in the Southwest, but he ultimately became known for his pathbreaking ethnological studies of the Ramah Navajo. Linton's archaeological background included the Southwest, in addition to New England, but after the mid-1920s he devoted himself to ethnography in the Pacific Islands, Madagascar, and southern Africa. The ability of these two scholars to understand archaeology, such that their writings reflected the problems inherent in pursuing culture through objects and material patterns, was paramount, if implicit, in Taylor's appreciation and use of their work. This was especially the case with Linton, whose ideas on function and use were sufficiently attractive to Taylor that some (e.g., Willey and Sabloff 1993: 160–164; cf. Trigger 2006) consider him among the functionalists of his day. "Functionalism" is generally tied to theories of integrated social systems and cultural holism that assume unified and bounded social or cultural units. Component parts of the system operate purposefully and/or meaningfully in relation to others, as in a synergism. In his book, for example, Taylor (1948: 117) cites the passage from Linton (1936: 404) that Clay recalls from his graduate studies:

> The use of any culture element is an expression of its relation to things external to the sociocultural configuration; its function is an expression of its relation to things within the configuration. Thus an axe has a use or uses with respect to the natural environment of the group, i.e., to chop wood. It has functions with respect both to the needs of the group and the operation of other elements within the culture configuration. It helps to satisfy the need for wood and makes possible a whole series of woodworking problems.

This relatively straightforward premise is used both concretely, as in Taylor's (1948) discussion of typology and classification, and as a structuring principle for his larger ideas regarding the conjunctive approach and the concept of culture.

Well before Kluckhohn, Linton was concerned with the concept of culture and, like Kluckhohn, explored this through reference to psychology and the development of the human mind. Thus, in Taylor's discussion regarding the concept of culture, we see frequent references to the ideas of Linton, and these support his arguments regarding culture and mental constructs. In considering the implications of the prolonged infancy of humans, Taylor writes (1948: 100),

> Linton has said (1936b, p. 72), the importance of this long period of parental dependency is that it permits and ensures learning on the part of the infant. That is to say, it facilitates the acquisition of mental constructs. Its value for culture most certainly does not lie in the acquisition of material objects or the accumulation of behavioral acts divorced from their mental residue.

Taylor defined his ideas regarding the concept of culture (e.g., 1948: 97–112; and explained in greater depth below) by reference to culture in its partitive sense, with a lowercase *c* (culture), and in its holistic sense, with a capital *C*. These were significant contributions not just to archaeology but also to anthropology as a whole (Bennett 1998). The famous log line "Archaeology is anthropology or it is nothing" was promulgated by Willey and Phillips (1958) and later used by Lewis Binford (1962) as the essential motto for the New Archaeology. Until Taylor's 1948 book, however, no archaeologist had seriously explored the basis, implications, and importance of the relationship between the disciplines, and no one had worked as hard to forge this relationship in substantial, coherent explanations of theory and method and with a culture concept as a guiding goal and principle. The work of Franz Boas was also a vital influence on Taylor in this regard.

Many have discounted Taylor's ideas because of personal reasons or because his book makes heavy demands on the reader (Watson 1983). Still others have neglected his book because of its supposed alignment with the "historical particularism" of Franz Boas, a movement in anthropology that countered nineteenth-century cultural evolutionism by advancing a relativist and humanist concern for the histories and culture of specific societies. Historical particularists argued that individual cultures or societies could best be understood in terms of their own inherent logic and historical trajectory, something that went against the generalizing theories of (unilinear) cultural evolutionism based on laws of human behavior and development. Kluckhohn and Linton both were strongly influenced by Boas and his intellectual contributions, although each diverged significantly from Boas's thinking in later years. Linton was especially familiar with Boas, studying with him at Columbia University in 1916–1917 and later succeeding him (controversially) as the Department Chair of Anthropology at that institution (1938–1945).

The lessons of Kluckhohn and Linton—and others, like Spier—no doubt led Taylor to an abiding respect for Franz Boas and his work. This is certainly obvious in aspects of Taylor's 1948 book, and particularly with respect to his elaboration of "culture" in its partitive sense (e.g., 1948: 98). However, Taylor's concept of culture was more nuanced and complex than this and included a notion of "Culture" in a more general or holistic sense as well. In actual fact, Taylor's development of the holistic concept of culture partly owes to his more careful reading of Boas (e.g., 1896) than most other postwar anthropologists undertook. Taylor notes that Boas encouraged the pursuit of larger questions that pertain to all of humankind, including the study of cultural process and general laws of culture change and cultural stasis. In this way, Boas sought, for a while at least, the same goals as the evolutionists, but with different sets of analytical preconditions. Boas (1896, cited in Taylor 1948: 38) writes:

> When we have cleared up the history of a single culture and understand the effects of the environment and the psychological conditions that are reflected in it we have made a step forward, as we can then investigate in how far the causes or other causes were at work in the development of other cultures. Thus by comparing histories of growth[,] general laws may be found.

Influenced by Boas and others, Taylor attempted to move beyond the principles of mere "historical particularism"—a basic influence on culture history—into a more integrated, yet nevertheless humanistic, science of culture, something he considered to be the rightful place of anthropology. His notion of Culture in the holistic sense reflects this (see Table 1.1): it is the highest level procedure of the conjunctive approach, titled "Cultural Anthropology," and focuses explicitly on the comparative study of cultures in order to explore the nature, processes, and development[6] of Culture.

Taylor stood on the shoulders of giants in building his program for American archaeology. He borrowed heavily from accumulated knowledge to produce his magnum opus and it is nearly impossible to begin to comprehend Taylor's message without recognizing his scholarly debts to his colleagues, mentors, and predecessors. However, it was Taylor's ability to integrate complex, and at times competing, models into a coherent whole and then to innovate still further beyond this amalgam that made his book cutting-edge, difficult, controversial, and masterful. By assessing intellectual trends and offering sincere proposals for interdisciplinarity, Taylor, to borrow from Barthes (below), created a "new object" that belonged to no one field but that could negotiate and be adapted to several at once or one alone. Thus, although it may be useful, if commonplace, to speak of Taylor's dissension in terms of the attacks he made on leaders in the field, it is probably more accurate and productive for the long term to consider this dissension in terms of his new and flexible, even alternative, recommendations for conceptualizing and practicing archaeology.

A STUDY OF ARCHEOLOGY (TAYLOR 1948)

> Interdisciplinary work, so much discussed these days, is not about confronting already constituted disciplines (none of which, in fact, is willing to let itself go). To do something interdisciplinary, it's not enough to choose a "subject" (a theme) and gather around it two or three sciences. Interdisciplinarity consists in creating a new object that belongs to none.
>
> R. BARTHES (1984: 100)

A year after the United States entered World War II, Walter Taylor enlisted in the Marine Corps. Before leaving for boot camp, he successfully defended his doctoral dissertation, titled "The Study of Archaeology: A Dialectic, Practical, and Critical Discussion with Special Reference to American Archaeology and the Conjunctive Approach" (1943). Many beyond Taylor's committee read his dissertation, an uncommon practice in most cases then and today. The interest in his text and ideas was sufficiently great that not long after returning from the war, he was given a Fellowship in the Humanities from the Rockefeller Foundation to craft a revision. He was then invited to publish this through the Memoir series of the Anthropological Association of America (AAA), the leading professional organization for anthropologists in the Americas.

We know that Taylor continued reading widely while on active duty; as a prisoner of war, for example, Taylor kept his mind sharp by teaching anthropology to fellow inmates (see Dark, this volume). Whatever may have transpired during the war years with respect to Taylor's thinking, once back home he reconfigured his ideas and altered substantially the tenor, contents, and structure of his manuscript (see Watson, this volume). The result is the book we all know today as *A Study of Archeology* (*ASOA*). One notes that, for publication, Taylor not only shortened the title, but removed the second 'a' from archaeology, an act that aligned him firmly with the Anthropological Association of America (AAA), as this was the spelling used officially by that organization; in fact, this was very likely the mandate of the AAA (P. Watson, personal communication, 2008). Taylor sought to reform, redirect, and recontextualize the entire tradition of American archaeology in order to bring it closer to anthropology. Clearly, the devil was in the details and publishing through the AAA would send a powerful message.

As most of the authors in this volume note, and as is well attested in countless commentaries on the history of method and theory in archaeology, Taylor was censured and marginalized after his book's publication. It is possible that *ASOA* would have had a more direct, immediate, and clearly identifiable impact on the field had he not chosen to criticize renowned members of the profession. He obviously believed, however, that this was necessary in order to make his point: he needed first to strip down and dissect current practices in order to present a new model in the form of his conjunctive approach. This maneuver may seem bold to many of us today, but given the size of the profession at that time, it was an outrageous, as well as self-destructive, decision.

ASOA consists of two parts, each divided into an introduction and three chapters. The table of contents is as follows:

The text of the book runs to 222 pages, including the 20 pages of endnotes. It underwent a major reprinting in 1968, complete with a new foreword, and was reissued as a new edition in 1983, this time with a foreword by Patty Jo Watson. Here I provide a brief chapter-by-chapter overview of the book, focusing on what I see as the two main themes represented by the two-part division: (1) assessment and critique of American archaeology; and (2) model for a reorientation of American archaeology. Taylor (1948: 6) says, "While Part I is to an appreciable extent destructive criticism, Part II is designed to be constructive." For each of the two parts, the third chapter is the climax, Chapter 3 being the (in)famous dissection of leading research and Chapter 6 constituting the formal explication of his "conjunctive approach." No one has ever analyzed Taylor's book or its structure and intentions as a whole (see Taylor 1972c). The present volume encourages colleagues, their students, and all interested readers to study and digest *ASOA* for themselves and I offer the following exegesis as a prompt.

ASOA Part I

The introduction to Part I provides a brief summary of the book's structure, a clarification of terms (adopted from Kluckhohn), a comment on notes and the bibliography, and an informative acknowledgments section. More importantly, Taylor imparts his overarching goal for the book as a whole: to offer American archaeology a conceptual scheme and to resolve "conflicts of a theoretical order" (1948: 5–6). Chapter 1 then leads the charge by outlining the "development of archeology as a field of study for the purpose of providing a *context* and in order to bring out some of the causes contributing to what I believe to be its unhealthy state" (ibid., 6; italics mine). Taylor's mention of "context" here should not be overlooked, for he seeks to establish—or construct—for archaeology the sort of sociocultural context that he later argues should be a central goal of archaeological practice. Thus, he opens the book by providing an example of the force and importance of the historiographic method he encourages: the writing of history with attention to the cultural milieu, past and present, which shapes that history (and its writing). This is an artful opening for it drives home his points, expressed in later chapters, regarding construction versus reconstruction.

ASOA Part I, Chapter 1: The Development of American Archeology

A Study of Archeology arrived thirteen years after what is recognized as the formal founding of the field of American archaeology. Thomas Patterson (1986: 7) notes that Chapter 1 represents the very first history of the field ever written; were this the only focus of his book, Taylor would have made a significant, trailblazing contribution. Later histories (e.g., Willey and Sabloff 1974; Trigger 1989) appear at intervals that indicate they were written to validate or explore new approaches or movements in Americanist archaeology. This "coincidence," between histories of the field and when they appear, suggests that we should expect Taylor's first chapter to pave the way for a larger goal and to establish a disciplinary context to validate it. Taylor (1948: 11) writes,

> I propose to give a brief outline of the chronological development of archeological research, whereby both the historical and theoretical import of this intra-disciplinary distinction will be clarified. To begin our study in this fashion has the added advantage of leading easily and logically into the major topic: the theoretical framework of Americanist archeology in the United States.

Taylor begins his discussion with the Middle Ages in Europe and then transitions into the more recent history of Americanist practices. In so doing, he pays special attention to the variety of archaeologies and related pursuits (e.g., geology, paleontology, art history, classics, and philology) and the ways in which they are geared toward the epistemologies and goals of either anthropology or history. Taylor notes that the "point upon which the archeological stream is observed to split is the literacy, the 'primitiveness,' and perhaps the artistic quality of the subject cultures" (1948: 24). He demonstrates that, because the field is so diverse and its roots and influences so poorly understood, it is difficult to discern a coherent "theoretical framework." Through this he sets up the direction of (and need for) his study: "[I]f . . . the splitting of the current has muddied the intellectual waters of the archeological stream, then we have cause for concern rather than complacency" (ibid.).

ASOA Part I, Chapter 2: Archeology: History or Anthropology?

Chapter 2 is a complicated discussion that asks a rather simple question: with respect to American archaeology, what is the relationship between anthropology and history? In other words, with which discipline should the field align? Taylor notes that American archaeology in the 1940s is overwhelmingly designated as a branch of cultural anthropology (which in the day meant "anthropology"), alongside ethnology, the study of living or present-day cultures and peoples. The goals of then-leading archaeologists, however, centered on the reconstruction of history (Watson 1986: 450). Taylor refers to this discrepancy—being anthropologists yet practicing history—as an "ambivalence" (Taylor 1948: 27) and asserts that it is necessary to define history and anthropology more clearly and to explore what they actually have to do with one another in terms that are relevant to archaeology.

In exploring the definition of history, Taylor focuses especially on the significance of "historiography." He cites and employs the ideas of the Italian philosopher of history Benedetto Croce, the "radical historicist" and anti-positivist (H. White 1973; Roberts 2007). Croce (1866–1952) was a major influence on Antonio Gramsci (the proponent of hegemony theory) and one of the world's leading social theorists of the early twentieth century. Following the approach of Croce, Taylor defines historiography, penning one of the most important lines of his *ASOA*, as "contemporary thought about past actuality and particularly this thought set down in writing or somehow projected in words. It denotes an abstraction or a set of abstractions from actuality, not that actuality itself" (ibid., 31). This point is key for Taylor's subsequent discussions regarding construction versus reconstruction as he (ibid.) explains that "[a]ny segment of past actuality which is verbalized, in writing or orally, is not that segment itself but merely an abstraction filtered through the mind of the verbalizer." Taylor's adoption of concepts basic to historiography becomes vital to his prescriptions for archaeology, specifically by identifying language as a constructed tool.

In working to understand these concepts, one can begin to see why Taylor's work was truly cutting-edge and why relatively few scholars of that era could comprehend it: he took pains to go beyond the mentalist proclivities of various American anthropologists in order to explore social theory deriving from European philosophies of history. Similar considerations do not emerge again in American archaeology until the 1970s and 1980s (Trigger 2006: 455–456). Taylor is an exemplar of the avant-garde when he (1948: 31) writes, "The written or spoken record of past actuality is, then, 'contemporary thought' about actuality." Thus, any history pursued through language, although focused on the past, derives wholly from the present. Taylor understood this point to be fundamental to a philosophical basis of archaeological research. Because this stems from what was explicitly anti-positivist thought (e.g., Croce), I believe it is difficult to argue, as Patterson (1986: 12) has, that Taylor participated in the adoption of the logical positivism common among archaeologists of his era, including the positivism that shaped much of the work and recommendations of Boas. Taylor's stance in this regard and the influence of Croce help us to understand why the expression of Taylor's work in the New Archaeology, a framework strongly tied to positivist philosophy, was incomplete at best.

Perhaps the most forceful of Taylor's specific points on the subject of history is the issue of reconstruction (see also Taylor 1972c). This distinguished him from both the culture historians of the day as well as later New Archaeologists, and in considering later and more recent literature in archaeology, it is the main way to assess whether or not an author, archaeologist, or historian of archaeology has actually read or comprehended Taylor's book. Taylor (1948: 35) notes that the term "reconstruction" implies "a re-building to exact former specifications which . . . are not verifiable and, hence, not knowable." He goes on to say (ibid., 35–36),

> [T]he work of all historical disciplines really leads to construction and synthesis, not reconstruction and resynthesis. From this, it is further apparent that the real task of the students in historical disciplines settles down to seeing how sound, how plausible, and how acceptable their constructions can be made. Neither the anthropologist nor the historian should use the term *reconstruction* and thus make himself feel inadequate because he knows that his research will never permit him actually to reconstruct the life of past times with certainty and completeness. Rather, he should realize that even the contexts written from the best and fullest archives are constructions and the differences lie in the nature of the respective data, not in the procedures of basic theoretical factors.

Martin (1971: 4) and Leone (1972b: 25) discuss the gap between what archaeologists *want* to do (reconstruct) and what they *are able* to do (construct and approximate); Leone (this volume) even considers the anxiety this causes. These considerations were stimulated by Taylor (1948) and by his explicit remarks about the obvious limitations of archaeology. It is odd, therefore, even shocking, to see that virtually every mention of Taylor's work (and there are hundreds), from Woodbury's (1954) candid review to widely read modern texts (e.g., Hodder 1986; Willey and Sabloff 1993; Sharer and Ashmore 2002; Trigger 2006), cites Taylor's interest in "reconstructing" the past (cf. Trigger 1968a). This is solid evidence that the vast majority of scholars simply have not been able to manage its complex language and content (giving up before arriving at this central point [Taylor 1948: 35]).

Chapter 2 goes on to explain how history may be distinguished from anthropology or, in Taylor's terms, "historiography" from "cultural anthropology." The answer ultimately becomes the central organizing principle for Taylor's "conjunctive approach." He writes (1948: 41), "The purpose of historiography has been shown to be the construction of cultural contexts, while that of cultural anthropology is the comparative study of the nature and workings of culture."

As noted previously, Taylor (1948: 38–41) draws on Boasian concepts to emphasize the latter as the terrain of anthropology, that is, "the comparative study of the statics and dynamics of culture, its formal, functional, and developmental aspects" (ibid., 39). He claims, and I emphasize again, that Boas was misunderstood by many anthropologists who, reacting to historical particularism, saw all of his goals and ideas as fundamentally counter-evolutionary, whereas in reality Boas encouraged the same overarching goal as evolutionists: "an understanding of the nature, processes, and the development of culture" (ibid., 38). If some see here the basis of interests in processualism, or culture change, this should not be surprising; I take up this issue in somewhat greater depth later in this chapter.

Historiography, Taylor notes, is an analytical procedure that must precede and support "cultural anthropology," which is, again, one of the ultimate goals for archaeology, geared toward the "nature and workings of culture" (see Table 1.1). His emphasis on historiography reflects a recognition that the culture historical approach requires modification and a means of integrating it into a grander mission; it is thus a critical retooling of the then-conventional (culture historical) means of doing archaeology (see Chapter 16, this volume). In this way, the two disciplines in question (history and anthropology), when properly defined, engaged, and contextualized, contribute to the same task: practicing archaeology as a historical—or, better yet, historiographic—discipline under the guidance and in the service of anthropology. In this context, anthropology, owing in part to historiography, is as malleable, adaptable, and constructible as human society, human culture, and historical writing about these. It is with this understanding that Taylor (1948: 43) inks his famous lines: "Archeology *per se* is no more than a method and a set of specialized techniques for the gathering of cultural information. The archeologist, as archeologist, is really nothing but a technician." Archaeology, therefore, ceases to be merely archaeology and accedes to greater capacities when it integrates concepts from other disciplines (sensu Barthes, above). Thus, Taylor concludes that archaeology is neither history nor anthropology, but that as a set of methods and techniques it can be either one or something else entirely. The goal of archaeology is the "production" (not re-production) of cultural information (ibid., 44). Employing historiographic methodologies and theory, archaeology can approach the larger goals of anthropology, should it care to, and that, in large part, is what his book is fundamentally all about. The end of *ASOA*—the "climax" of Part II—lays this out in considerably more detail, where Chapter 6 explains the "conjunctive approach." I address this in turn below.

ASOA Part I, Chapter 3: An Analysis of Americanist Archeology in the United States

Chapter 3 is the "climax" of Part I and is considered by many to be the most famous chapter of *ASOA*. Certainly, it has been the most widely read. In it, Taylor repeatedly attacks leading archaeologists for their shortcomings: for failing to do anthropology by not providing syntheses of the nature and workings of culture; for failing to provide reports with details on provenience, materials, dimensions, and associations; for providing mere trait lists to describe time and space relationships; and for being too descriptive overall and failing to make meaningful interpretations (and the list goes on and on; see Taylor 1948: 45–94; Woodbury 1954: 293–294).

For the bulk of his critique, he singles out Alfred Kidder, the leader of the Division of Historical Research at the Carnegie Institution of Washington, DC. (see my other chapter for this volume). Taylor (1948: 46) notes that Kidder's influence upon archaeological research in the Americas "has been, and is now, of the greatest proportions. It is not too much to suggest that he is the most influential exponent of the discipline active in the Western Hemisphere today." Taylor also targets five other leaders in the field: Emil Haury (working in the U.S. Southwest), Frank Roberts (SW), William Webb (SE), William Ritchie (NE), and James Griffin (SE).[7] They endure nowhere near the criticism aimed at Kidder, however. Because of this targeting, we should not be surprised that the longest (at five pages) and most critical review of *ASOA* was written by Kidder's friend, colleague, and biographer, Richard Woodbury (1954 [review]; 1973a and 1993 [biographical discussions of Kidder]).

Taylor's criticisms of Kidder and others, although perhaps vitriolic to an unnecessary degree, have emerged as valid; he gave voice to the long-standing discontent of many who were too fearful or polite to act. His statements hit the mark hard and stimulated considerable behind-the-scenes discussion and discomfort. For example, Woodbury (1954: 292) notes, "[I]t is in verbal, and generally informal, comments that archaeologists have been most out-spoken concerning *A Study of Archeology*, and it is my impression that such comments have been preponderantly disapproving and rarely favorable." It is a truism, discussed in countless textbooks, that Taylor's invective penetrated the culture of American archaeology deeply, much more so than Taylor expected. In spite of the book's merits, the furor that followed publication led to an array of protracted personal and professional reprisals lasting nearly fifty years. At the 1985 Society for American Archaeology (SAA) meeting, for example, in a session celebrating the fiftieth anniversary of the SAA, anger and tension spilled out regarding Taylor's forty-year-old book (Sabloff 2004; Longacre, this volume). Taylor's ideas and innovations have been misunderstood and marginalized in many contexts or, frequently, appropriated without attribution. This issue of his status as pariah gains additional weight when we consider that, until he accepted a position at Southern Illinois University at Carbondale in 1958, Taylor had difficulty finding steady work and that, afterward, his SIU students often were seen as tainted goods (Reyman 1999).

Taylor never intended his attacks to be taken quite so personally or to have had such personal repercussions for him, a point he makes in the original edition and in a later printing. In 1948 (p. 45), he writes, "It is not to be thought that, in the following pages, the men selected for analysis are being criticized on a personal basis. Both the analysis and criticism will be of published results." Obviously, this had little effect since most readers saw the attack as fundamentally personal. Their response was so vituperative that in a new foreword to the 1968 printing of his monograph, Taylor (1968b: 2; cf. Reyman 1999: 682–683) states:

> Contrary to what has apparently been the widespread view, that chapter [3] is not a "polemic." I have always regarded it as an objective analysis from an explicitly stated point of view, a critique as detailed and comprehensive and fair as I could make it of archeological theory and practice, not of men. Therefore, until my opinions change in regard to archaeological research—and they have not—the chapter may be allowed to stand as a series of illustrative, essentially impersonal, and thus timeless examples.

As Folan notes in his chapter for this volume, the 1983 printing included yet another new "statement" in this regard: an index with the names of archaeologists mentioned in the text and the notation "commended." This serves to draw attention to Taylor's insistence that his book had not solely been geared toward critique but that it had offered praise in numerous instances. In this way, he wants us to see a balance between the criticisms and the extensive laudatory passages that cite the good research done by many. Taylor (e.g., 1948: 90–94) did in fact have kind words for all of the following: Walter Wedel, John Bennett, George Vaillant, Wendel Bennett, Harlan Smith, Fay-Cooper Cole, Thorne Deuel, Charles Fairbanks, Frank Setzler, Jesse Jennings, Ralph Beals, George Brainerd, Robert Smith, Cornelius Osgood, and especially Thomas Lewis and Madeline Kneberg and their (1946) monograph *Hiwassee Island*, "possibly the best archaeological report I have had the pleasure of reading" (Taylor 1948: 9).

Sixty years ago, it appears that praise for research gains far less attention than does criticism. Taylor did criticize, it is true, but this was certainly not the sole, nor perhaps even the central, feature of his book. Moreover, his lengthy and now infamous criticism leveled at Alfred Kidder was not the first, only, or last statement regarding the shortcomings of the Carnegie research program (see Bolles 1932; Kluckhohn 1940; Becker 1979; Hinsley 1989; Kubler 1990: 195; Castañeda 1996; Patterson 2001).

ASOA: *Part II*

Part II of *ASOA* represents the explicitly constructive segment of Taylor's magnum opus. It begins with an introduction (1948: 95) that sums up the problems with American archaeology that Taylor identified in Part I: "the building of chronological sequences and culture classifications with purely taxonomic inherencies . . . the writing of cultural chronicles . . . placing the resultant finds in one or another of the taxonomic pigeonholes . . . seldom [being concerned] with the cultural integration or implications of the data themselves." These shortcomings inhere in what he refers to as the comparative or taxonomic approach, or what we today recognize as "culture history." This strategy "applies itself mainly, if not wholly, to those phenomena which have comparative significance *outside* of the site or component. It neglects much of the local cultural 'corpus.' It is narrow and therefore wasteful of the potentialities of the archaeological data" (ibid.). In place of the lone taxonomic approach, Taylor (ibid., 95–96) offers his conjunctive approach, which has as its underlying goal

> [t]he elucidation of cultural conjunctives, the associations and relationships, the "affinities," within the manifestation under investigation. It aims at drawing the completest possible picture of past human life in terms of its human and geographic environment. It is chiefly interested in the relation of item to item, trait to trait, complex to complex (to use Linton's concepts) within the culture-unit represented and only subsequently in the taxonomic relation of these phenomena to similar ones outside of it.

He goes on to summarize (ibid.): "This attitude, the conjunctive approach, considers a site to be a discrete entity with a career and cultural expression(s) of its own. It is no longer just one more unit in a spatial and temporal range of comparable units." This issue of *within* and *outside*, of "discrete entity" versus "spatial and temporal range," is critical in understanding the significance and goals of the conjunctive approach, especially in the context of the period of culture history.

ASOA *Part II: Chapter 4: A Concept of Culture for Archeology*

The distinction between *within* and *outside* goes a long way toward helping us to comprehend the topic of Chapter 4 on the concept of culture, which solidifies the philosophical and anthropological basis of his book as a whole. It is worth noting that, as his chapter title suggests, he does not limit the implications of Chapter 4 to American archaeology alone. Perhaps this is part of the reason that this chapter, a substantial postwar addition to his dissertation, has gained positive recognition since its publication, both in archaeology (Deetz 1988; Watson 1995) and in social anthropology (Bennett 1998).

In fact, excepting White (1959a) and Binford (1965), very few authors since 1948 have criticized Taylor's explanation of the culture concept in *ASOA*; rather, many have praised his efforts. Two early reviewers criticized this chapter; Robert Burgh's (1950) review called it "dessicated" and Woodbury argued that the concepts were taken from the work of others. But even Woodbury (1954: 294) admitted that Taylor's definitions "reflect a serious attempt to grapple with a problem that is central to all archaeological work but which has often been slighted or entirely ignored" and that "Taylor is correct in saying that most of us have been far too imprecise about this crucial matter."

In retrospect, we can say that one of the greatest single contributions of Taylor's 1948 book to both archaeology and anthropology is his discussion of the concept of culture. His ideas were shaped by Tylor (1871), Boas (1896), Linton (1936), and Kluckhohn (see Kluckhohn and Kelly 1945); are related to those of Kroeber (1948; Kroeber and Kluckhohn 1952; see Watson 1995: 685; cf. White 1959a; White and Dillingham 1973: 23); and have been influential in the post–World War II period (as noted above). Bennett (1998: 304–305), for example, argues that the exploration of the culture concept became fundamental in archaeology only beginning in the 1950s, something that can be attributed to Taylor.

As I mentioned previously, Taylor (1948: 109–110) considered "Culture" in its holistic sense and its partitive sense, "culture." "Culture" with an uppercase *C* is a descriptive or explanatory concept for the mental constructs that are learned or created by individuals: all humans engage in this brand of "Culture," which can be either shared or idiosyncratic (Taylor 1948: 109). In lowercase, "culture" is a "historically derived system of culture traits . . . that tend to be shared by all or by specially designated individuals of a group or society" (ibid., 110). The partitive aspect of culture also is based on mental constructs. It is an especially important part of Taylor's conjunctive approach because to address a "historically derived system" through archaeology one must emphasize site-level research, that is, working to access localized culture and temporal and spatial contexts at the scale of the site or community. Thus, in Taylor's research program, culture, history, and site-level research are inextricably tied, centered on the above-mentioned importance of studying associations that are *inside* and *within* and that address a "discrete entity." Deetz (1988) recognized the importance of Taylor's partitive concept for considering history and historiography and the construction of specific cultural contexts. This led Deetz to consider the influence of contemporary thought in such constructions, an advance for which he credits Taylor.

Previously, I discussed Boas's influence on Taylor's notion of Culture. There is no need to repeat that discussion here, although it is important to recall that Taylor, like Boas and others, conceived of flexible levels of procedure; this is abundantly clear in the outline of the "conjunctive approach" (see Table 1.1). Culture (culture) in its partitive sense is the goal of historiography, which seeks to study manifestations *within* a localized context or site. Once localized contexts have been studied and interpreted as fully as possible, the archaeologist can carry on to the next level of the procedure, which involves further integrations by making comparisons between localized contexts. When the archaeologist does this, he or she is doing anthropology and is working to derive the nature and workings of human Culture in general, or "Culture" in the holistic sense. This effort might also include the study of the development of culture and culture change or cultural process. Taylor (1948) never discusses cultural evolutionary theory except to cite its peculiar interest in placing Western European civilization at its zenith (ibid., 20–21). However, Taylor's notion of Culture allowed, in fact encouraged, a consideration of Culture process and change. It seems merely that he wanted to stop short (much as Willey and Phillips [1958] did) of supporting evolutionary ideas as they were framed at that time lest he wind up associated with unilinear cultural evolutionists and social Darwinists, and thus risk compromising his stance on "contemporary projections" and their relation to past actualities.

The final important point regarding Taylor's view of culture, mentioned elsewhere but worth reiterating here, is his argument for culture as a mental construct consisting of ideas. This is the basis, for example, of his denial that archaeologists should speak of "material culture." He argues instead that when discussing artifacts and their traits we speak of the "objectifications of culture," not of culture itself. Objects, he contends, are not ideas; they can be interpreted variously and take on multiple meanings, depending on any of an array of contextual factors (time, space, culture, etc.). One may notice the similarities between this perspective and Taylor's view of archaeology as mere technique unless guided by a conceptual structure. This so-called mentalist view of culture also recalls his conclusions regarding history as projected thought. Without question there is a consistency, a "structural" coherence,[8] to the way in which and degree to which Taylor weaves together his ideas, points, and premises. This is less immediately evident in Chapter 5, owing to its complexity, but we can see a similar structure in his discussion of "empirical" versus "cultural" categories, the central feature of this chapter.

ASOA *Part II: Chapter 5: The Nature of Archeological Data: Typology and Classification*

Chapter 5 is a fascinating discussion that has had a deep influence on the field. Nevertheless, Woodbury, the defender of Kidder, regarded it as one of the least successful chapters, largely because of its length (nearly forty pages) and density. He (1954: 294) writes that it unjustly criticizes J. O. Brew's (1946) Alkali Ridge report and that Taylor claims as original his critique of the McKern Classification System, whereas others before him had made similar points (see also Kehoe 1998: 100–105). Although it is true that Chapter 5 is complicated and detailed, it represents another instance where Taylor addresses a topic that was largely neglected: the theoretical and methodological justification for the types, classes, and categories that were at the heart of the culture historical or taxonomic approach. Other archaeologists had explored issues and problems regarding taxonomy and trait lists, but Taylor was the first to lay out this matter as part of a larger critique and prescription and to do so within a theoretical framework.

Basic to Taylor's discussion is the question of how archaeologists designate type categories. He finds that then-current practices could be separated into those who employ "empirical" categories (determined by archaeologists) and those who use "cultural" categories (relating to the "world of bygone people" or specific to the ethnic or cultural groups who produced the objects in question) (Taylor 1948: 122; and see Watson 1983: xi). Empirical categories, Taylor (1948: 122) says, are based on "chemico-physical attributes"; cultural categories are based on "criteria pertaining to cultural or culture attributes, such as techniques of manufacture, use, function, meaning, and culture idea." He notes that empirical categories are the only ones that are directly observable; cultural categories, on the other hand, although perhaps based on observable data, require interpretation (i.e., construction) and the testing of hypotheses and are thus inferential. Cultural categories, Taylor (ibid.) writes:

> advance by inference from the empirical, and the results are to be viewed as hypotheses to be tested. For the archaeologist, the empirical or purely observational has only a mediate function, forming merely the basis, not the goal, of his studies. By definition, he is interested in cultural contexts or in culture itself, and the categories which obviously he should seek are those pertaining to those fields. Also, and for the same reasons, his interests lie, not in the phenomena of his own world, but in the world of the original makers, users, or possessors, individually or as groups. In other words, the pertinent question to be asked is, "What may be inferred today from present evidence as to those things that were relevant, significant, meaningful *to the bygone individuals and societies under investigation*?"

In certain respects the themes and ideas that Taylor addresses in this section are among the most difficult—and important—in the entire book. For example, he goes on to discuss empirical/inferential versus objective/subjective (ibid., 123) and why and how these sets of terms should or should not be used (he argues that the latter should be reserved for philosophical discussions). Overall, he attempts a sincere exploration of the theory of typology and classification, one of the most compelling aspects of which is his insistence that cultural categories be derived through the formation and testing of hypotheses—the basis of a deductive approach. This is not a minor point, especially when we consider that many authors, apparently never having read Taylor thoroughly, relegate him and his work to the purely inductivist age of culture history.

Some historians (e.g., Willey and Sabloff 1993) consider issues regarding culture change and evolution to be at the heart of the theoretical differences between the New Archaeology and the conjunctive approach. With respect to thoughts on methodological differences, hypothesis testing lies at the center. Many writers believe to this day that Taylor encouraged an approach that was strictly inductive, that is, one that sought to construct theories (generalizations) from data (particulars), and that hypotheses and their testing were not part of Taylor's conjunctive model. For this reason, Taylor has at times been lumped by some into a tradition with antiquarians and ceramic taxonomists, researchers who begin their work by garnering objects for study, without the guiding light of a problem or hypothesis.

Binford and the New Archaeologists claim themselves as champions of the hypothetico-deductive approach and have contributed to the perpetuation of the inductivist label for Walter Taylor. Consider, for example, the opinion of Binford (1983c: 5) on the subject: "The message that most archaeologists received from Taylor's appeal was that they ought to look harder and for more detail, because only new facts could expand their knowledge." Historians of archaeology are often culpable as well, including those, like Trigger (1989: 278), whose books are frequently used in teaching: "[Taylor] regarded defining the relations between parts and explaining change as problems that must be approached inductively."

Willey and Sabloff plainly recognize that Taylor espoused hypothesis testing at every level of his model. For example, they state (1993: 163): "Speculation, Taylor stoutly maintained, was not only justified in archaeology but required. It was the very life of the discipline, for, if archaeology was to investigate the non-material aspects of culture through its material ones, it must have recourse to hypotheses." Taylor discusses the importance of hypothesis testing with respect to building cultural contexts (1948: 111) and in developing useful typologies (cited above). But he is most explicit regarding the importance of deduction where he discusses problem orientation for research. He writes (1948: 157; see also Tallgren 1937: 154):

> Other disciplines are constantly reworking their hypotheses and formulating new ones upon which to proceed with further research. When these are found to demand modification and change these are altered. Why should archaeology assume the pretentious burden of infallibility? Why is it not possible to project hypotheses, specifically labeled as such, and then to go on from these toward testing and answering the questions thus raised? Why should every archaeological hypothesis have to stand and be correct for all time?

Taylor's discussion of typology and classification in Chapter 5 may be the least accessible segment of his tome, but this owes more to the general difficulty of the topic and to the care he takes in exploring it than to any hasty statements he makes, redundancies, obfuscations, or trivialities. Watson (1983: xi) notes that Taylor's chapter anticipated major discussions of typology during the era of the New Archaeology, such as those by Hill and Evans (1972) and Watson, LeBlanc, and Redman (1971: 126–134). In their history of American archaeology, Willey and Sabloff (1993: 164–165) cite Taylor's Chapter 5 as one of the important precursors to the Ford-Spaulding debate of the 1950s, known today as the formative discussion of typology.

Taylor's chapter provided the first in-depth consideration of typology and advanced considerably the discussion regarding empirical versus inferential categories, or as Willey and Sabloff (ibid., 142) say, "imposed vs. discovered" types (see also the distinction between "etic" and "emic" in, e.g., Pike 1954 and Taylor 1972a). As noted above, Taylor believed that imposed or empirical types were useful, at the very least as a starting point for analyses, but that archaeologists had to work to discover the categories known to the makers and users of the object in question. Albert Spaulding has taken the same position as Taylor with regard to empirical versus inferential categories, advocating the building and testing of hypotheses. Spaulding, moreover, has discussed his advocacy of inferential categories as a means to access the ideas of artifact makers (Spaulding 1960: 76) or what Watson and colleagues (1984: 208–209) refer to as "mental templates." Owing to statements by Spaulding, and to his interest in what we can call a Taylorean "mentalist" approach to culture, one might argue that Spaulding was sincerely influenced by Taylor's work on typology as expressed in *ASOA*. In fact, Spaulding's early work (1953: 306) cites a general debt to Taylor (1948: 113–130). This is later supported by his recognition of Taylor's role as a pioneering theorist in American archaeology (Spaulding 1985: 307).

Taylor certainly played a critical role in the development of ideas regarding typology in the 1950s, and it may be some time before this is more fully understood. Perhaps this can happen once the conjunctive approach is better and more thoroughly explored, something I hope my comments here can begin to do. To close my discussion of *ASOA*, the following provides a brief consideration of the conjunctive approach, as an aid to future considerations and readings. Other discussions of the conjunctive approach can be found in numerous chapters in this volume, including those by Folan, Reyman, and Maca.

ASOA *Part II: Chapter 6*

Taylor's crowning achievement is Chapter 6 on the "conjunctive approach." Table 1.1 provides an outline of the model, arranged as flexible steps of a procedure. It essentially states five goals that can be dealt with sequentially or as overlapping protocols. These are (1) to establish the importance of problem orientation for fieldwork, and in particular the testing and modifying of hypotheses; (2) to encourage the collection and study of as many lines of evidence as possible; (3) to build an analytical foundation through the synthesis of chronological and spatial contexts at the local or "site" level; (4) to integrate site-level studies into frameworks for comparative research of cultural development on regional or higher levels; and the final or overarching goal (5) to

Table 1.1. The conjunctive approach (after Taylor 1948: 153)

A. PROBLEM

B. DATA
- 1. Collection
  - a. Local cultural
    - 1) Artifacts
    - 2) Cultural refuse
    - 3) Deposits
  - b. Local human biological
  - c. Contemporaneous geographical
    - 1) Geological
    - 2) Meteorological
    - 3) Floral
    - 4) Faunal
  - d. Non-local human
    - 1) Contemporaneous
    - 2) Pre-local
    - 3) Post-local
  - e. Non-contemporaneous geographical
    - 1) Pre-local
    - 2) Post-local
- 2. Study
  - a. Criticism of validity of data
  - b. Analysis
  - c. Interpretation of data
  - d. Description
- 3. Presentation

C. LOCAL CHRONOLOGY (chronicle)

D. SYNTHESES AND CONTEXT (ethnography or historiography)

E. COMPARATIVE (ethnology)
- 1. Cultural
- 2. Chronological

F. STUDY OF CULTURE, ITS NATURE AND WORKINGS (anthropology)

develop research questions and contributions that serve the larger interests and goals of anthropology. For Taylor (1948: 41), "it is a false dichotomy that separates cultural anthropology from historiography . . . there is a common pool of source material from which they both may draw . . . which suits their special purposes. It is, therefore, in these special purposes that the differences between the two disciplines lie."

In Taylor's discussion and outline of the conjunctive approach, his model is presented as a set of sequential phases or steps. He explicitly states, however, that the different procedures would naturally be undertaken at different times, as opposed to a linear progression of archaeological practice. Alison Wylie's (2002) discussion of the conjunctive approach misses this point when she discounts his model as rigid and outdated. Taylor (1948: 152) writes,

> The studying of data may proceed together with its collection, rather than after, as for instance in the case of material which cannot be removed from the field or which is destroyed during excavation. . . . Nor are the headings mutually exclusive or segregated according to cultural criteria. They are inclusive and descriptive, representing merely a working scheme to suggest, not dictate, the mechanics of archaeological research.

Further proof that the model is meant to be flexible with regard to procedural steps is that Taylor organizes his discussion of the steps in an unusual way (see Table 1.1). He begins with the Problem (heading A) and then proceeds in reverse order (from heading F). He says that he does this because the type of data and means of collecting may vary depending on the goals for synthesis and study. His explanation of the reverse order makes sense in a rational way, but it also has the effect of demonstrating the adaptability of his model as practiced—that it can be used for diverse circumstances and research designs.

Taylor's Chapter 6 does not provide a clear statement or summary of what the conjunctive approach is, which is intriguing. Beyond the explanation of his book's direction in the introduction to Part I (Taylor 1948: 7), we only see Taylor defining the conjunctive approach in the introduction to Part II (ibid., 95–96), the third chapter of which is his formal outline of "the conjunctive approach." In other words, his chapter delineating the approach nowhere provides an overview of what it is. Because *ASOA* is carefully crafted, it is fair to assume that *all* of Part II is Taylor's presentation of the conjunctive approach. This means that Chapters 4 and 5 on the culture concept and typology, respectively, are fundamental to Taylor's conjunctive aims and, as such, each chapter may serve to support and elaborate specific procedures. This suggests one distinct way of approaching the book, that is, with the understanding that the entire tome, including Chapters 1 and 2, is a platform for the conjunctive approach—with Chapter 3 thrown in for good measure as a validation and to ensure an audience. As I have noted, many since 1948 have commented on Taylor's book even though they have not read it closely. But did anyone read his book carefully when it came out? Apparently, almost everyone interested in archaeology did or at least tried (Woodbury 1954, 1973). A decade later and beyond, it seems that fewer and fewer scholars attempted to tackle it and that a lot of stock interpretations were simply passed uncritically from one author to another—as with the examples of hypothesis testing and "reconstruction." As I mention at the beginning of this chapter, countless authors claim that Walter Taylor had an impact on the formation of the New Archaeology. Although some have borrowed his ideas without attribution, which creates certain obstacles to tracing *ASOA*'s effects, there is sufficient evidence that the impact of Taylor's ideas was substantial. The following section presents some of this evidence as a means to demonstrate the profound influence of Walter Taylor on the emergence of a New Archaeology in the late 1950s and early 1960s, and on American archaeology in general.

EFFECTS OF *ASOA* ON AMERICAN ARCHAEOLOGY

Assessments of Taylor and His Contributions

A number of books and articles discuss Walter Taylor's work and significance in depth. Some of these are histories or overviews of the field of archaeology (e.g., Daniel 1950; Watson, LeBlanc, and Redman 1971, 1984; Willey and Sabloff 1974, 1980, 1993; Trigger 1989, 2006; Kehoe 1998; Wylie 2002); others are analytical commentaries and/or retrospectives (e.g., Trigger 1971; Watson 1983; Deetz 1988; Watson 1995; Reyman 1999; Longacre 2000; Hudson 2008). In general, these tend to include both negative and positive assessments and, in some cases, misinterpretations of Taylor's work are apparent. Even among the "mixed bag," however, are some bold, broad, and positive statements that must be considered. For example, Willey and Sabloff (1993: 164) have written:

> In spite of the immediate negative reaction from a large part of the archaeological profession, Taylor's words were not forgotten. A decade and a half later, some of them were echoed in the New Archaeology. . . . More immediately, they helped keep alive the interest in context and function for some archaeologists in the 1950s. . . . Taylor's critique seemed unwarranted, and there was initial resentment; but, after this anger had died down, there was quiet acceptance of many of his ideas.

The degree to which this is recognized by others is neatly expressed by Watson, LeBlanc, and Redman in their important treatises (1971, 1984). The opening paragraph of Watson and colleagues (1984), for example, is devoted solely to a discussion of Walter Taylor; he is mentioned or his work considered repeatedly therein; and the book closes by naming him (along with Wheeler, Kidder, Spaulding, and Braidwood) as one of the founders of scientific archaeology: "Walter W. Taylor, who stressed the importance of the cultural context of archaeological materials" (ibid., 275). As mentioned in this chapter and elsewhere in this volume, many scholars have cited Taylor as a marker for the beginning of a new era and the end of an old (e.g., Guthe 1952; Caldwell 1959; Brew 1968; Willey 1968; Trigger 1971; Kehoe 1998; Longacre 2000). The following section explores how and why such views are or can be held, particularly with respect to Taylor's influence on the New Archaeology of the 1960s. Articles by Hudson (2008), Sterud (1978), Caldwell (1959), and Trigger (1971) serve as structuring mechanisms for my commentary and argumentation. I close the section by discussing how complicated—and perhaps unreasonable—it is to compare the conjunctive approach and the New Archaeology and provide a brief discussion of a seminar organized in honor of Taylor's retirement.

Taylor as Instrumental to the New Archaeology?

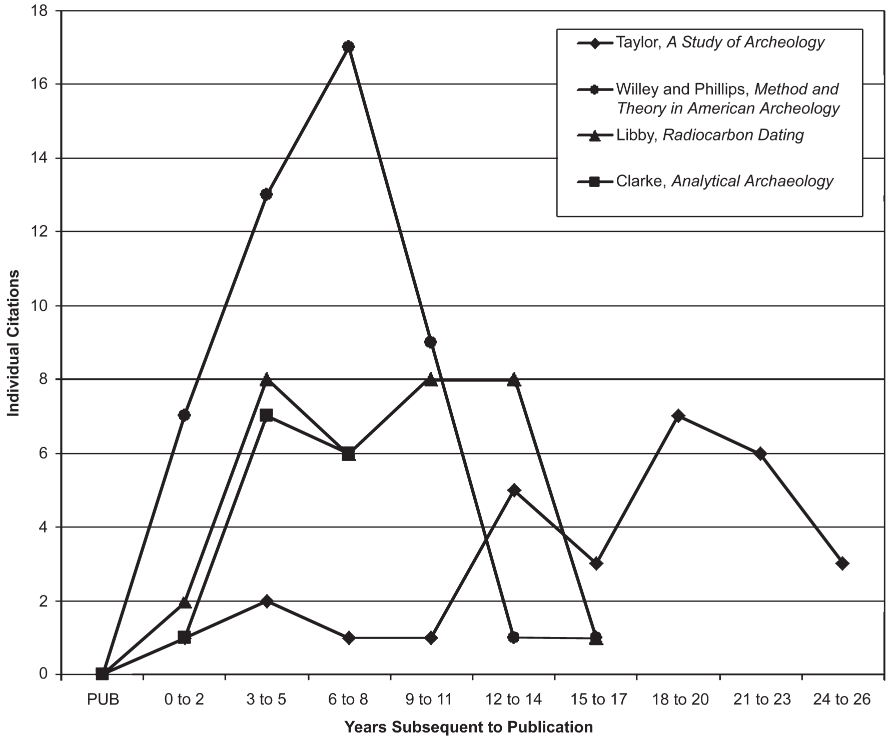
An article on Walter Taylor was recently published by Corey Hudson (2008) in the *Journal of Anthropological Archaeology* (*JAA*). The gist of Hudson's argument is clearly stated: "[T]here is no reason to believe that [Taylor] was a 'precursor of the major theoretical advances of the 1960s' (Fagan 2005: 177), or that the 'essence [of his work] was reissued serially by many authors of the 1960s as the 'new archaeology' (Jennings 1986: 58)" (Hudson 2008: 199). Hudson claims that such beliefs are merely "received wisdom" (ibid., 192), that Taylor is too often credited for what he did *not* do (i.e., inspire the New Archaeology) and ignored for what he did do (namely, provide good if brief examples of the conjunctive approach [ibid., 195–196]). I do not want to delve here into the details of Hudson's article, preferring to allow readers of this book and that journal to make up their own minds. I do wish, however, to point out two (related) problems with the article that indicate both a poor reading of Taylor's (1948) book and a degree of naïveté regarding how Taylor's book was or was not received in the 1950s and 1960s and why. The case of Hudson's article, I suggest, says as much about the field of American archaeology in general as it does about the ideas, agenda, and scholarship of individuals and institutions.

Hudson (2008: 198) discusses Taylor's interest in "reconstructing" context and affinities and also, for support of his arguments, cites Binford's (1972: 8) notion that Taylor sought "behavioral reconstructions." As I point out earlier in this chapter (and see Kehoe 1998: 233), the suggestion or belief by an author that Taylor was interested in "*re*constructions" provides solid evidence that Taylor's book was never actually read by that author or that the author simply did not (or could not) understand the text (see Taylor 1948: 35–36). In fact, such a belief (i.e., in Taylor's focus on reconstruction) is a much better example of "received wisdom" than the one Hudson provides in his article (see my discussion on Chapter 2 of *ASOA*). Hudson's non-reading or misreading of one of the foundational tenets of Taylor's conjunctive approach disqualifies most of the rest of his arguments in the *JAA* article, resembles other misreadings and misinterpretations of Taylor on the part of Hudson's colleagues at Missouri (e.g., Lyman and O'Brien 2004: 377–378), and partly explains Hudson's admitted confusion regarding patterns of citation in the archaeological literature in the decades following the publication of Taylor's book. This last point signals the second problem I see in Hudson's paper.

Hudson (2008: 197) asks, "If Taylor was so instrumental in the development of New Archaeology why wasn't he recognized as such?" This question appears to refer to a lack of interest in Taylor's book post 1948. However, if Hudson is referring to the 1970s and 1980s, the era after which the New Archaeology had taken root, the author needs to do more research. If Hudson is referring to the 1950s and 1960s, the author ignores one of the most popular rules and mantras in modern archaeology: an absence of evidence is not evidence of absence. Rather, absences, delays, or biases in citations can indicate significant shifts, trends, and/or discriminations in the disciplinary sociopolitics of archaeology and/or changes in the theoretical or practical leanings in the field. Several important expositions demonstrate this: for example, a seminal article on citation patterns of *American Antiquity* published by Eugene Sterud (1978) and the more recent analytical work of Scott Hutson (2002, 2006) on gender and citation trends in leading journals. These papers demonstrate powerfully how and why rigorous citation analyses can provide more than just information on intellectual genealogies. These issues are discussed more deeply in my other chapter for this volume, but two additional points follow from the foregoing.

Sterud (1978: 300–301) shows that there were frequent citations of Taylor's book, but that these were delayed by ten to twelve years after its publication; in other words, the citations did not occur with any frequency until the late 1950s and early 1960s (see Figure 1.1). For approximately a decade after its publication, Taylor's book received little overt attention in American archaeology's leading journal. The evidence of citation patterns suggests that "when [Taylor] came to be regarded as the forerunner to the 'processual' [New Archaeology] developments of the 1960s . . . his 1948 work became more important" (ibid., 300). This is my first point and I return to it presently. The second point is that, given the more frequent references to Taylor in the 1960s, we might conclude that the much lower frequency in the 1950s reflects not so much an absence as a silence. Because of the offensive tone and the power of Taylor's criticisms of archaeology's leaders, no one wished to align himself or herself with Taylor lest this bring career reprisals. None but Woodbury (1954) chose to engage and his negative review of *ASOA* appeared fully six years after its publication, indicating that the tension and anger remained quite fresh at that time.

In the six to eight years following Woodbury's review, tensions in the field eased and ultimately a younger, larger, highly vocal group of scholars, led by Lewis Binford, read *ASOA* and the works of those who had read it (and who perhaps had never [in print] admitted to doing so). This group was granted more leeway in their dissension than their "older brother" had been (see Deetz 1989) and a really different practice of archaeology—that is, different from prewar approaches—was able to take hold. Did this younger program appear ex nihilo? To what extent did this new and different approach resemble Taylor's conjunctive approach? There were certain fundamental differences between the two approaches—their epistemologies being the most significant. Hudson (2008) recognizes others, such as (for the New Archaeology) the importance of intentionally sampling and extrapolating from just a representative segment of the data universe. Related to this, there also are apparent differences in terms of scale—Taylor suggest-



1.1 Number of individual citations in *American Antiquity* for four well-known books, in the years subsequent to publication (redrawn after Sterud 1978: 301).

ing larger and more intensive analytical efforts than most New Archaeologists saw as practical (e.g., Rouse 1954; Ritchie 1980). Nevertheless, many scholars in the years intervening between 1948 and the 1980s have employed Taylor's prescriptions, borrowed parts of them, and/or celebrated their utility and advances. Most of these scholars are so certain that Taylor inspired the changes reflected in the New Archaeology that they do not even bother to argue the point. In the cases where we do see discussions regarding how and why the New Archaeology grew out of Taylor's work, it is clear that many of the perceived discrepancies in approaches are largely because of differences in language, terminology, and the analytical tools and methodological models available during the respective time periods. Great cultural changes occurred in the United States between 1948 and the 1960s, and many of these derived from advances in scientific knowledge and technology as well as the acceptability of dissension (Deetz 1989; Lamberg-Karlovsky 1989: 4). The following discussion addresses more directly the underlying principles of the New Archaeology as well as the general and specific areas of the New Archaeology that many authors claim were gifts of Walter Taylor and his conjunctive approach. These authors range from important practitioners and founders of the New Archaeology to historians and theorists of archaeology.

The "New" Archaeology

The earliest significant mention of a "new" archaeology was by Clark Wissler in 1917, when he argued for the importance of the relatively novel stratigraphic method. This was a major advance for Americanist archaeology and became a linchpin of the culture history approach. The next significant (if disparaging) mention of a "new" archaeology was that of Richard Woodbury in his (1954) review of Walter Taylor's book. Shortly after this, we see what has become the most cited reference to a new archaeology, provided by Joseph Caldwell in *Science* (1959). The title of Caldwell's paper, "The New American Archaeology," reflects what was then a growing, and increasingly accepted, movement among archaeologists practicing in the United States in the 1950s. Caldwell's article is significant for several reasons, two of which stand out. The first is that he cites Walter Taylor (1948) as the main break with the old archaeology, namely, culture history. The second is that Caldwell provides the parameters of the new archaeology and these clearly—terminologically and conceptually—introduce the framework promoted and codified in the 1960s by Binford and his group (colleagues, mentors, and students). Caldwell (1959: 304–306) discusses all of the following essential lines of research: culture process, culture-environment connections and interrelations between humans and ecology, inference and hypothesis testing, and cultural evolution. In 1959, Binford was nowhere to be seen.

Thus, the major changes that occurred in the field and led to the New Archaeology (sensu Caldwell 1959 and sensu Binford 1962) occurred sometime between the end of the war and 1959, during those ten or so "silent" years after the publication of Taylor's *ASOA* (see Sterud 1978). The New Archaeology that was formally hatched by Binford has been visibly trendsetting and formed the structural foundation for much of later twentieth-century archaeology all over the world. Binford and others would like us to think, however, that their program arose of its own force and volition, that, in effect, there were no precedents and that it resembles only vaguely what went before it (Binford 1968b: 27). If we were only to consider the article by Caldwell, we would know this to be mere bravado and rhetoric. However, if we consider Taylor's 1948 work, accounting for differences in idiom, we might see this as patently wrong. To assess where the New Archaeology came from, not just its individual elements but also its bid for paradigmatic coherence, there arguably exist two main research loci to explore. One is the question of this silent decade before Binford came on the scene, that is, the years immediately after the appearance of *ASOA*. During this time, Taylor's ideas morphed into the goals and nomenclature of others, including particularly those whose careers, unlike Taylor's, were on the rise. These issues and the research bridging the gap between Taylor's and Caldwell's publications are addressed in my other chapter for this volume. Here I wish to more directly examine the second research area that informs the question of whether the New Archaeology owes its origins to Walter Taylor. The following looks at the similarities (and differences) between the conjunctive approach and the New Archaeology and at what scholars say about these during the period when the New Archaeology took root (i.e., post 1968). Taylor's (1969, 1972c) appraisals of Binford lead my discussion, but Trigger's (1971) article on archaeology and ecology is used as the basis for my analysis.

Systems, Statistics, Process, and Culture/Culturology

In a series of articles in the 1960s, beginning with a 1962 article titled "Archaeology as Anthropology," Lewis Binford synthesized the elements present in Caldwell's article (see Willey and Sabloff 1993: 223–224). In doing so, he stressed evolutionary and ecological thinking and employed, as a sort of glue, a systems perspective and hypothetico-deductive reasoning. The result was a paradigmatic program for archaeological research that has stimulated and guided forty years of work, branched in numerous directions, and provoked decades of rebuttals and alternative approaches. By the late 1960s, many scholars began reflecting on the origins or beginnings of the New Archaeology, and they felt not only comfortable but justified citing and discussing Walter Taylor's contributions (see Leone 1972a: x; 1972c: 2). One of these was Taylor himself, who engaged in a debate—albeit rather one-sided—with Binford (see Willey and Sabloff 1993: 222–223 for one interpretation of this). Taylor (1969: 383) challenged the Binfords (Lewis and his then-wife, Sally, also an archaeologist), arguing that a systems perspective and hypothesis testing were around long ago and were fundamental to his conjunctive approach (evidence Taylor 1948: 109–110). Taylor went on to say that his *ASOA* contains the majority of ideas and coherence that are claimed by and present in the New Archaeology. In a later paper (Taylor 1972c), he went into more detail and suggested that the borrowings were even more galling because Binford and others never noted their intellectual debt to him. Citing his own 1948 book, Taylor (ibid., 28–29) says that all of the following (and more) were pulled directly or indirectly from his conjunctive approach: ideas regarding the nature of culture (including its variability) and culture process; hypothesis testing and the importance of inferences; and a systemic view of cultural context. He credits Binford mainly with persistence and benefiting from the use of some new technologies. We could easily discount as bitterness Taylor's claims were it not for the fact that the writings of numerous other scholars support them.

In the following I focus on the primary glue of the New Archaeology, that is, the "systems" approach, frequently associated with integrative ecological systems models for human societies as well as contextual holism. Several scholars have acknowledged Taylor as the archaeologist who introduced this and paved the way for its acceptance; I mention just a few of them. In *Current Anthropology*, Leo Klejn (1977: 7) states that Binford's systems approach was encouraged by functionalist ideas borrowed from Taylor. Klejn (ibid.) writes, "Taylor had already abandoned the representation of culture as a mere list of traits which could be added up (i.e., the 'additive' understanding of culture) and had called for the study of functions and functional connections of objects in a context." Michael Schiffer (1972: 157), in an article in *American Antiquity* titled "Archaeological Context and Systemic Context," acknowledges first that the model he proposes was anticipated by Binford and Chang in the 1960s and then acknowledges a "general debt" to Walter Taylor's "seminal work."

Trigger (1971: 323–325), in an article called "Archaeology and Ecology" published in *World Archaeology*, provides one of the most thorough discussions of Taylor's influence on the New Archaeology, identifying his "systems" ideas as the basis of this impact. His section on the "American systemic approach" highlights Taylor's 1948 book and explains his contributions to modern systems ideas at considerable length. Several passages from this discussion are worth quoting directly; I also use his examples to follow out my analysis. For example, Trigger (ibid., 323) writes:

> The initial step in this direction was the publication of Walter Taylor's (1948) *A Study of Archeology*. This book was a much-deserved reaction against the prolonged survival in American archaeology of an interest in identifying culture-units, working out local chronologies and tracing external cultural connections, much in the spirit of the early diffusionists. Taylor attacked the neglect of the nonmaterial aspects of culture and the failure of archaeologists to consider artifacts in a functional context.

Trigger goes on to explain how Taylor's work influenced other scholars and who these are (e.g., Willey and Phillips [1958]; Binford [1962]) and then a page later provides substantially more detail in this regard (ibid., 324):

> On a programmatic level, Taylor's approach has had far-reaching impact. There is widespread agreement that artifacts must be studied as products, and therefore as reflections, of cultural systems. There is also growing interest in developing techniques to elicit new kinds of information from archaeological data; particularly concerning social (and to a lesser degree political) structures. . . . Much more attention is now being paid to the micro-distribution of artifacts within individual sites in the hope that these distributions will shed light on the social behavior of the people who made or used these artifacts (Hill 1966, 1968; Longacre 1968). Related to this is an increasing concern with settlement patterns, which are viewed as the fossilized stage on which social action has taken place (Chang 1958, 1962, 1968; Trigger 1965: 2). Multivariate analysis of stylistic variation, along the lines pioneered by James Deetz (1965), has helped to shed valuable light on prehistoric residence patterns. . . . Archaeologists

> have also been making forays into the ethnographic literature to search out detailed correlations between aspects of material and nonmaterial culture that can be used to interpret archaeological data (Chang 1958; Cook and Heizer 1968). Many of these studies require manipulating vast quantities of data and have been practicable only with the assistance of computers.

These comments by Trigger, as well as those by Klejn, Schiffer, and others, go a long way toward demonstrating that the importance of Taylor's book far exceeded that of his critique, that it opened wide the door for new discussions and research agendas, and culminated, whether intentionally or not, in major contributions to the New Archaeology. To show further the extent to which this is true, we can pursue some of the references Trigger makes to specific authors and their publications. Willey and Phillips's 1958 treatise, *Method and Theory in American Archaeology*, proposed a cultural historical and developmental (i.e., proto-evolutionary) model for the whole of the Americas. Willey (1988: 299) has noted more recently that Taylor motivated him and Phillips in the writing of their book, not least by his insistence on the need for theory in archaeology. *Method and Theory* became the most influential work of its day: it ushered in the era of comparative evolutionary approaches and served as a benchmark for Lewis Binford's formulation of the New Archaeology (see the introductions to Binford 1962 and 1965). Willey, in a 1994 interview with David Freidel, gives perhaps the greatest endorsement of Walter Taylor's work ever recorded: he cites Taylor's book as *the* most important development in archaeology during his lifetime. Elsewhere, Willey (1968: 52; Willey and Sabloff 1993: 209) has noted that Taylor influenced him in his early work on settlement patterns and, in a well-known book chapter titled "One Hundred Years of American Archaeology," published when the New Archaeology was taking hold, Willey cited Taylor as the first spokesmen for the modern period. He writes (1968: 50), "[T]he first strong statement of the new trends we are considering . . . was Walter Taylor's *A Study of Archaeology* [*sic*]."

Trigger (1971: 324) also cites the influence of Taylor's systems ideas on Hill, Longacre, and Deetz, all of whom are widely considered to have been leading proponents and exemplars of the New Archaeology. Taylor's interest in ideology and style, seen first in his 1941 article on the Maya ceremonial bar, was formalized in *A Study of Archeology*. This interest was surely one (among others) of Taylor's influences on the era of "Ceramic Sociology" (Longacre 2000: 293), the work by Deetz on Arikara ceramics (Deetz 1965), and the research by Longacre and Hill on style, kinship, and social structure at Carter Ranch (Longacre 1970) and Broken K Pueblo (Hill 1970), respectively. All these works include Taylor's book in their bibliographies.

I have mentioned Taylor's profound influence on the conceptual structure of Jim Deetz's (1988) research. Above, Trigger specifically mentions Deetz's use of multivariate analysis of stylistic traits, another development that followed on the heels of Taylor's research (as also noted by Willey 1966: 29). As Clay discusses (this volume), Taylor was hampered by the limited statistical tools of his era and might have had a much greater impact had there been computers available at that time. Nevertheless, as part of his conjunctive approach, Taylor advocated the statistical analysis of variables, associations, and affinities, and, in particular, the distribution of artifacts. This required tools for calculation and assessment of patterns that were mostly lacking at that time. As a result, he developed his Master Maximum Method (MMM), which, Clay tells us, Taylor called "the poor man's chi-square." Taylor (2003: 42) writes: "The MMM establishes parameters of expected frequency for categories (classes, types, sub-types, etc.) of specimens excavated from archaeological sites. It compares the actual frequencies and their deviations from expectancy within and between sites and excavation units of sites." The analyses were rendered in charts (Taylor 1948: 177; 2003: 43) and demonstrate Taylor's struggle with the relatively low technologies of his day (see Fig. 13.1) as well as his insistence that mathematical tools and instruments could be of enormous help to archaeologists.

I have noted earlier that Taylor influenced Spaulding (e.g., 1953) in his work on typology; and Spaulding later notes (1985: 307) that the delay in acceptance of Taylor's concepts probably owed to a lack of methods, techniques, and technologies that are now standard. David Hurst Thomas, in an article about statistics in archaeology, cites Taylor as the first to encourage forcefully the use of statistics as a standard feature in archaeological practice. Thomas (1978: 231) writes: "In the mid-1940s, W. W. Taylor repeatedly urged his colleagues to extricate themselves from the morass of trait lists and get on with the business of studying people. Taylor (1948) quite rightly recognized the importance of quantitative methods in archaeology, and subsequent archaeologists have successfully elevated archaeological awareness above the trait list mentality of the 1940s." Statistics, of course, became a central analytical method for the New Archaeology (e.g., Heizer and Cook 1960; Thomas 1978; Watson, LeBlanc, and Redman 1984: 21–22).

Another one of the distinctive features of the New Archaeology is its emphasis on the study of culture change, also referred to as cultural or culture process. For this reason, the New Archaeology is frequently labeled as "Processual Archaeology." The processual interests of the New Archaeology reflect its ties to mid-twentieth-century cultural evolutionary theory (e.g., White 1949; Steward 1955). Taylor's 1948 book explores the importance of studying culture change and cultural "development." Some scholars (e.g., Sterud 1978) recognize that he was the leader of the processual movement long before it was identified as such, although Willey and Sabloff (1993: 222–223) disagree with this perspective on the basis of differences in terminologies and important technical issues. They agree with Binford[9] that the New Archaeology has no absolute precedent. Moreover, they contend that Taylor's versions of evolutionary ("developmental") and systems models were not linked to the mechanisms for culture change that are defining aspects of the New Archaeology, namely, internal stimuli for cultural change and systemic regulating mechanisms that allow for cultural adaptation (which must be understood by reference to laws of evolutionary potential and the requirement that systems, when destabilized internally or externally, must achieve equilibrium).

Still, a close look at Taylor's (1948: 156–170) writing proves that he builds both earlier and then-current interests in culture change into a coherent program, such that it must be granted that he is in fact the first face of a processualist agenda. Nevertheless, although the New Archaeology was fueled by Taylor's recommendations, its evolutionism did not follow the road Taylor constructed. This owes to several factors. First, the appearance of "a battery of new methods, techniques, and aids that were not available in 1948" (Willey and Sabloff 1993: 223) allowed for types of analyses that differed from much of what Taylor recommended. Second, these analyses were driven by questions that derived from very different orientations to the nature of reality and the ability of archaeology to access that reality (see Watson, this volume). Third, the basis of the differences (from Taylor) apparent in the orientation of the New Archaeology is tied to the assumption that past realities can be *re*constructed, typically from a mere subset of the data universe, and that culture—by definition—exists as humans' "extrasomatic" means of adaptation to changing conditions, especially environmental conditions. These differ from the definitions offered by Taylor.

Leslie White's (1959a) article on "culturology," titled "The Concept of Culture" and published in *American Anthropologist*, is one of the most extraordinary and unusual—almost esoteric and alchemical—papers ever published in relation to American archaeology. It is little wonder that it helped to spawn a veritable sect of archaeology. Building from segments of his earlier pathbreaking book, *The Science of Culture* (1949), White (1959a: 237–238; 1973: 23) stressed the extrasomatic basis of culture in his refutations of Taylor's (and others') notions of culture as mental and ideational. White argued that culture, as extra-somatic, is linked technically and conceptually to the somatic, that is, to that which is tangible and measurable empirically—artifacts, labor, and so forth (White 1959a). In this way, material objects shaped by human use are culture, not merely objectifications of culture as Taylor (1948) argued. Binford (e.g., 1972) adhered to his mentor's (White's) viewpoint and, as such, represents a fundamental difference in perspective from Taylor.

An even more dramatic, related difference regards Taylor's and Binford's (i.e., New Archaeology's) views on the overall aim and abilities of archaeology: construction versus *re*construction, respectively. Based on differences in concepts of culture and on epistemological differences related to views on the capacity of archaeology to model and represent past reality, there is no way to argue convincingly that Taylor's conjunctive approach was reborn or refashioned *from whole cloth* into the New Archaeology. At the same time, neither Taylor nor the vast majority of scholars who have discussed these issues have argued for a wholesale transmission of ideas and approaches. As Bennett (1998) notes, Taylor opened the door for discussions of the concept of culture in archaeology and therefore, regardless of the degree to which White, an ethnologist, diverges in his conceptualizations from Taylor, all archaeological roads (vis-à-vis the culture concept) lead back to *ASOA*.

Many of Taylor's ideas were either employed by later archaeologists or modified and adapted to specific problems. Some of these were borrowed as *sets* of protocols and ideas and this is why we see the adoption of combined contextual and functional interests that stress interdisciplinarity, site-level research, cultural systems, quantitative analysis, environmental factors, and nonmaterial aspects of culture (e.g., social and political organization). In the great majority of expressions of the New Archaeology, Taylor's interests in historiography and in history were cast off or simply ignored. As the New Archaeology adapted to changing needs and technologies in American society as well as to the demands of the fledgling National Science Foundation (b. 1950), we see that anthropological science, materialism, and culturology (sensu White 1959a) grew in importance as sustaining approaches and perspectives.

Synthesis for the Future

Taylor's model and recommendations for the practice and theory of American archaeology achieved an unusual synthesis of the empirical and ideational approaches that reflect much of the conflict in Euro-American intellectual history and that have anticipated recent and ongoing debates in Euro-American archaeology. This remains poorly studied, however, because Taylor has been labeled a strict normative theorist (Binford 1965; Hodder 1986; Lyman and O'Brien 2004; cf. Taylor 1967a) and because no one has yet explored his influence on cognitive archaeology or his interest in Benedetto Croce, semiotics, and structural linguistics. Taylor's work may properly be construed as a bridge between eras and paradigms. For decades now, various scholars have offered examples of or recommended theoretical compromises—syntheses and middle grounds—for the future of American archaeology (e.g., Earle and Preucel 1987; Renfrew 1989; McAnany 1995; Spencer-Wood 2000; Thomas 2000; Hegmon 2003; Trigger 2003; Watson, this volume). There is no doubt that Americanists and others will continue to seek reconciliations between the thriving processualist and postprocessualist agendas, and between these and the concerns of indigenous and other interested groups whose history and identity are at stake (cf. Flannery 2006). A return to Walter Taylor's book—as a roots resource and a guide—may serve as a constructive means of advancing such discussions and experiments, especially regarding the future of archaeology in any Americanist tradition.

I close this section of the chapter at the point where Taylor closed his academic career, with a brief presentation of a seminar organized in honor of Taylor on the eve of his retirement from Southern Illinois University at Carbondale (SIU-C). An understanding of the topics covered and contributors involved reinforces much of what I have addressed in my discussion of Walter Taylor and the New Archaeology. However, the seminar also demonstrates that some doors must be left open for future analysis. For example, only two scholars in the history of world archaeology have created “contextual” archaeologies—Taylor and Ian Hodder. To what extent are these approaches and their philosophical and theoretical foundations similar and/or different, and what can we learn by exploring such questions?

Upon Taylor’s retirement in 1974, George Gumerman, then an associate professor of anthropology at SIU-C, organized the conference or “seminar” in honor of Taylor on April 29 and 30.[10] The scholars arrived, the meetings were held, and there was a plan to publish the papers later as a kind of festschrift volume. In keeping with the seeming jinx on the Taylor legacy, however, the publication never appeared. Nonetheless, the suggested topics for the conference and the list of invitees are instructive with respect to the influence or impact of Walter Taylor on American archaeology. In the letter of invitation (February 21, 1974) to the conference participants, Gumerman offered several themes for discussion, based on areas in American archaeology where Taylor is seen to have been influential. These are (1) the concept of culture in archaeology; (2) the archaeologist’s utilization of non-artifactual materials or the method of study of such materials; and (3) the future of archaeology. The list of contributors helps us to gain a good understanding of the perspectives that were taken to address these topics. The participants were R. Berle Clay, Tulane University; the late Robert Euler, Fort Lewis College; George J. Gumerman, SIU-C; James N. Hill, UCLA; William A. Longacre, University of Arizona; Jon Muller, SIU-C; Charles Redman, New York University; Jonathan Reyman, Illinois University; Stuart Struever, Northwestern University; and Patty Jo Watson, Washington University.[11] Note that fully half of the participants were leaders in the New Archaeology movement and remain recognized as such to this day (two of whom contributed chapters to this volume); two others had been Taylor’s students (also contributors to this volume); another two were Taylor’s colleagues at SIU-C; and another, Euler, was a close friend and colleague in Southwestern archaeology. What are we to make of this assemblage of facts and affinities? Considering the relative youth of the New Archaeology luminaries at that time, it is clear that something about the future of archaeology, and about the extent of Taylor’s influence, was highlighted by this gathering.

LESSONS FROM THE CASE OF WALTER TAYLOR

Given Taylor’s impact on the field of archaeology, we must puzzle over why this has not been more widely explored. This book begins to help us to solve this puzzle. Along the way, we see that there are several lessons that can be taken from the "phenomenon" of Walter Taylor. I offer the following for colleagues and students in the social sciences and, especially, for those pondering major critiques or reorientations in archaeological theory and practice. The first two lessons are rather straightforward and I keep my discussion of them brief. The third is more complex and requires some elaboration in the form of a fourth. This last is certainly the main lesson as the first three are moderated by moral/ethical/behavioral issues that (for better or worse) attach to Taylor's legacy in this book and elsewhere.

The first lesson is that if one wishes to build a successful career, one should think twice about attacking one's elders (Christenson 1989: 164–165); this is particularly true in a field that remains as relatively intimate as American archaeology. Funding decisions, peer review selections, job networks, committee leadership in professional organizations, and journal editorships tend to be in the hands of accomplished senior scholars. It is perhaps an understatement to note that American archaeology and academia more broadly are as socially and politically situated as ever. Second, we all should be less quick to condemn those with seemingly radical or difficult ideas; rather, it would behoove us to treat them gently, to encourage departures as a sign of healthy and diverse discussion, and to refrain from everywhere and always linking the professional to the personal (see Leone, this volume).

The third lesson is a familiar one to academic archaeologists and to academics in general and can be summed up succinctly as "publish or suffer the consequences." In this regard, the debate in the pages of *American Antiquity* between Walter Taylor and Richard (Scotty) MacNeish is instructive. Known to this day as the "MacNeish-Taylor debate," it began with Taylor's (1960b) critical review of MacNeish's (1958) monograph on excavations at the Sierra de Tamaulipas caves in northern Mexico. Taylor criticized MacNeish for an array of perceived errors in procedure and interpretation, tied largely to methods for phase designations. MacNeish (1960) replied by restating his case, introducing new data, and greatly clarifying his explanation of his methods. In fact, the process of responding to Taylor led to a notable change thereafter in MacNeish's documentation of fieldwork. Flannery (2001: 152) writes, "Many of MacNeish's later reports took pains to outline his methods of establishing types, complexes, and phases, as if he felt that Taylor were still looking over his shoulder." In his autobiography, MacNeish (1978: 247) writes that Walter Taylor is "[o]ne of the few archaeologists who really took a hard look at our methods, theories, and techniques and who aggravated some of us, like me, to think more clearly about what we were doing and where we hoped to go."

Although MacNeish was grateful to Taylor and saw his influence on American archaeology as profound and obvious, he was never so cowed as to refrain from sharing his legendary honesty, as when he joked that "Taylor and I shared an interest in the conjunctive approach; he talked about it, I did it" (Flannery 2001: 152). In his obituary of MacNeish, Flannery (ibid.) emphasizes this last point when he notes that Taylor never produced a monograph on Frightful Cave: "Unfortunately, in *A Study of Archaeology* [*sic*] Taylor had proposed an interdisciplinary 'conjunctive approach' for which he himself never got around to providing a book-length demonstration. . . . If there is a lesson here for young archaeologists, it is this: The stairway to heaven is not paved with brilliant critiques of others' work but with good reports on your own sites." The point here is that if one advances a new idea, protocol, or paradigm with the hope that it will have a substantial impact, or if one wishes to make a statement by criticizing the work of others, one must subsequently provide examples for how to proceed, especially in the form of published articles and monographs. With respect to Taylor and his legacy, many of the chapters in this volume emphasize precisely this point and this judgment.

It is worth mentioning, however, that several scholars, including Taylor himself, have explicitly questioned this reasoning, that is, the notion that Taylor's work somehow failed or lost force by his inability to produce a *material* demonstration of the conjunctive approach. For example, Trigger (1968b: 532) writes:

> By viewing individual cultures not as collections of traits, but as systems, Taylor's approach has contributed significantly to the understanding of cultural processes that underlie and have produced the archaeological record. Compared to this, the fact that no one, including Taylor himself, has produced a site report that measures up to his ideal specifications is of no importance.

Many writers who discuss the conjunctive approach mention Taylor's failure to publish an example; Trigger's view of the situation therefore can be considered the first dissenting opinion on the topic. His comments are intriguing and give us another avenue to explore the lessons provided by the "case" of Walter W. Taylor. Even Richard Woodbury, with whom Taylor had a difficult relationship after the publication of *ASOA*,[12] declares that despite the absence of an example of the conjunctive approach, Taylor made major changes in the field: "Unfortunately, no one has yet made a convincing application of the approach that Taylor offered. But the direction in which he urged archaeology to move has been followed, that is, the incorporation of anthropological concepts and insights into archeological research" (Woodbury 1973b: 311).

Taylor's thoughts on the matter (1969; 1972c; 2003) echo Trigger's (1968b) viewpoint (see also Adovasio 2004: 609) but then at turns are heavy with guilt for not producing the example that MacNeish, Flannery, and many others have demanded. Taylor's (1972c) response to Binford made the case that his (1948) book and its ideas stimulated lasting changes in the field via their impact on the New Archaeology. Taylor claimed it was not necessary for him to publish examples of the conjunctive approach seeing as he had already "provided enough pertinent material for critics to chew on for quite a spell" (ibid., 30). David Hurst Thomas (1979: 49), however, reminds us that "American archaeologists since the time of Thomas Jefferson have acknowledged the necessity, in fact, the obligation, to publish their own findings. Taylor's critique suffered because of his failure to do so." Of course—and this represents yet another paradoxical moment in the case of Taylor—one of these American archaeologists insisting on the importance of publication was Taylor himself.

In Chapter 6 of *ASOA* (1948), on the conjunctive approach, Taylor writes, "[I]t is incumbent upon the archaeologist to publish the empirical bases for all his inferences in order that the reader may judge for himself their acceptability" (ibid., 156; and see Chang 1967: 133). Forty pages later, he reiterates this: "[T]he empirical bases for all published interpretations and inferences should be given to the reader" (ibid., 194). Although Taylor recognized that full publication of project data and interpretations requires considerable time, energy, and, especially, money, he nevertheless repeatedly emphasized the necessity of doing so. When obstacles or limitations are too great, he suggested *more focused* means of presenting research and results; for example, he noted that if a specialist readership is not anticipated and if one's interests lie in presenting the broad cultural picture, publication of the cultural context would be sufficient (ibid.). He believed that publication of research was an obligation, not least because the original record is destroyed through excavation.

Yet Taylor never managed to produce the Coahuila report. With Reyman, he worked on the enormous manuscript (1,200+ pages), but it was never published (Reyman 1999). He eventually pulled together one segment of the data (on sandals) in the late 1970s, published it (1988), but then quickly withdrew it (Euler 1997; Reyman 1999: 696; Taylor 2003: xv). Recently, however, Nicholas Demerath, Mary Kennedy, and Patty Jo Watson teamed as editors to publish another version, *Sandals from Coahuila Cave* (2003), the equivalent of a "more focused" presentation. Taylor's (2003) preface candidly discusses the reasons for his failure to publish the whole Coahuila report and thus his inability to provide a substantial example of his conjunctive approach. He says that nearly all of the analysis of the Coahuila materials had long been completed but that several other time-consuming tasks remained. Then he explains (ibid., xv), "The delay in completing these tasks can be attributed to many things: military service, changes of residence and work, the procrastinations of increasing age, plus a severe reaction to the professional reception of my monograph, *A Study of Archeology*." As there is no indication that these reasons are ranked in terms of importance, it appears that Taylor gave equal weight to each; but this may not be the case.[13]

Although we may never know what really blocked his efforts to remove the Coahuila "albatross around his neck,"[14] there are issues still worth considering and this is why I linger on this final lesson. Taylor's inability to publish the Coahuila report has generally only been seen in professional terms: either he could not muster the energy and intellect to demonstrate what he so self-righteously imposed on the profession or the lack of example serves as proof that his approach was wrong or misguided. Yet a mere glance at the above quotation from his preface tells us that the reasons Taylor himself provides have relatively little to do with professional considerations and address an array of mostly personal obstacles. Moreover, among these reasons we can detect notable silences: for example, some widely known personal setbacks (which are discussed in this volume) are not cited explicitly at all and one of the stated reasons—military service—is thrown in among the others but certainly may have carried greater weight. The following addresses these silences as a means of closing my discussion.

The case of Walter Taylor has much to teach us regarding whom to criticize, and whom not, and how each of us can avoid certain types of criticism, or at least career reprisals, by publishing what we excavate, analyze, and interpret. But there is also a fourth lesson: the case of Taylor teaches us that we are all more blood than ink; that is, there are typically substantial life issues that influence professional work, in both good and bad ways (see Kennedy and Leone, this volume). From this perspective, we learn something that is too often ignored in biographies and historiographies of academic disciplines: behind every scholar, disciplinary leader, savaged theorist, and public persona, there is a human being with personal obstacles, family commitments, neuroses, hang-ups, and experience of tragedy. The book my coeditors and I have assembled on Walter Taylor—the man, the scholar, the pariah, pioneer, prophet, dissenter, gadfly, upstart, pedant, and so forth—includes many anecdotes, personal remembrances, and characterizations of him as a human being. More often than not, however, these are offered unsympathetically, humorously, or as avenues for authors to prove they knew something of the *real* Walt. My coeditors and I have intentionally discouraged contributors from more deeply examining Taylor's personal life; thus, the present volume contains few to no detailed discussions regarding family finances (property, debt, alimonies, etc.), family relations, marriages and divorces, vacation locales, who his friends were, or even the extent of his hobbies (such as acting). There are three dimensions of his life, however, that surface in this book (or in other publications, e.g., Reyman 1999) and that are unelaborated or silent (through no editorial work on my part or that of my coeditors). I highlight them here as a means to provide a more human side to the weight of the albatross—a burden too frequently framed in purely professional terms.

First is Taylor's love of the outdoors. This emerges in several chapters in this volume and Taylor certainly alludes to his hunting, fishing, and canoeing when citing (2003: xv) "the procrastinations of increasing age." It is clear that he loved these recreational activities, but we might consider why he loved them more the older he became; an argument could be made that it was not merely the result of an interest in loafing through late middle age, retirement, and old age. If we consider a second silence, that regarding the death of his first wife, Lyda, we might gain more perspective on why his sporting endeavors took so much of his time: after her death, they were probably one of the few pastimes that brought him a measure of unrestrained joy. By all accounts, Lyda was his one true love and her relatively early death, of cancer in 1960,[15] left Taylor in a poor state and affected his life in ways that we will probably never understand (see chapters by Kennedy, Reyman [bio], Kelley, and Riley, this volume; Reyman 1999: 688). Taylor's *ASOA* is known by many to this day as the inspiration for the dictum "Archaeology is anthropology or it is nothing!" Moreover, Taylor's attention in his book to the tenets of cultural anthropology and his ties personally and intellectually to great ethnologists are also well-known. His closest connection to anthropology, however, doubtless came through his relationship with Lyda who was trained as a sociocultural anthropologist and apparently had a large influence on how he valued that field.[16] Although we as outsiders can only speculate, much of his energy for pursuing anthropology in archaeology and for vindicating his anthropological mission probably died with her.

The third silence is Taylor's military service (Euler 1997; Reyman 1999). Neither in this volume nor elsewhere do we find details about his parachuting behind enemy lines in Europe or how he was eventually captured, nor do we learn how as a Marine he became involved in the Office of Strategic Services (OSS) in the first place; fewer than a dozen Marines fought in Europe. The late Philip Dark (this volume) has supplied a rich array of information about their time together in prison camp, but we never have access to most of the facts regarding how Taylor arrived there. It is fascinating to learn that Taylor *himself* did not know many of these until a few years after Lyda's death, when he returned to France to resolve issues of guilt and hazy memory that had plagued him since his capture.

In his book, *The OSS in World War II* (1986), Edward Hymoff devotes several pages to Walter W. Taylor, the last Marine captured in the European theater. The account is based on letters written by Taylor as well as original U.S. government archival documents. These grant us insights into his experiences in wartorn Europe that in turn provide insights into what kinds of personal tragedies he lived through and how he came to explore these. I have chosen to include here all of the relevant passages from Hymoff's captivating book (ibid., 314–315):

> On August 21, five days after Ortiz and most of "Union II" Mission were captured, Second Lieutenant Walter W. Taylor was taken captive in a shootout. He was the last of four Marines captured in Europe, all of whom would survive the War upon liberation in April 1945. Taylor had been assigned to the OSS intelligence team attached to the 36th Infantry Division which landed with the U.S. Seventh Army in the invasion of Southern France at Cannes-Nice on August 15. As a line-crosser, Taylor and his section chief and a Marine sergeant attached to the team sneaked behind enemy lines in an effort to learn

whether the Wehrmacht would stand and fight or retreat. Along with an agent recruited from the local *Maquis*, Taylor headed for his target—the town of Grasse, fifteen miles inland and west of Nice.

[Taylor explains:] "I was to stay behind with the agent and Citroen (a car the two had "liberated"), accomplish the mission of taking him in and waiting and then taking him out; and then we were to get to the 36th as fast as we could. The agent had been leading the Resistance fight against the Germans ever since the landing and was absolutely exhausted, falling asleep time and time again while we were briefing him. . . . At dawn the next morning, the agent and I headed for the town of St. Cezaire, which was declared to be in the hands of the Resistance and where I was to let the agent down and wait for his return from Grasse. However, during the night, due to Allied pressure on Draguignan and Fayence, what evidently was a company of Germans had taken up positions in St. Cezaire. On approaching the dead-still town by the steep and winding road, we ran into a roadblock of land mines; we both thought it was the Resistance, and the agent took my carbine and jumped out of the car to walk toward the line of mines. He lasted just about 10 feet beyond the car and died with a bullet through his head. I still thought it was the trigger-happy Resistance but started to get out of there . . . even faster when I finally saw a German forage cap behind some bushes above the road. But the car jammed against the outer coping, and a German jumped down the road in front of me and threw a grenade under the car. I tried to get out of the right door and luckily did not, because I would have been completely exposed to the rifle fire from the high cliff on that side above the car. The grenade exploded and I was splashed unconscious on the road.* When I came to, I was surrounded."

During the ride to Grasse for interrogation, Allied aircraft continuously strafed the vehicle in which Taylor was traveling as POW. During the excitement of the attacks by friendly aircraft, the OSS Marine managed to stuff an incriminating document behind the seat cushion of the vehicle. Although suffering from painful grenade wounds, he was subjected to intensive interrogation which ended when he vomited on the uniform of his inquisitor. The next 20 days were spent traveling to Italy, and stopping at six different German and Italian hospitals for treatment of his wounds. At the end of November he was sent to the same POW camp as [OSS Major] Ortiz.

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*In a letter written to the Historical Branch at Marine Corps Headquarters on May 31, 1966, Taylor related how the hand grenade had shredded his left thumb and that some twelve fragments had struck his leg "6 of which at last count remain." He also wrote that for some years he felt guilt for the death of the French agent who was killed, adding: "It might be interesting to note that when I have thought about the incident of my capture I have always pictured us as coming down a long hill and seeing, across a wooded stream valley, the site of the road-block with men in uniform scurrying about and climbing the cliff-embankment. I have always blamed myself for thinking them to be Resistance and not recognizing them as Germans . . . and thus causing our trouble and the death of the agent. However, after years of trying, in 1963 I returned to the scene and found that the road did go down the opposite side of the valley, that there were no trees, that the actual site of the road-block is completely invisible from any part of the road until one is within about 20 yards, in other words that I could not possibly have seen men . . . scurrying or been aware of the block."

Watson (this volume) shows that, after returning home from the war,[17] Taylor intensified his attacks on American archaeology's leaders. For his (1948) *ASOA*, he made substantial changes to his 1943 dissertation that reflected years of reading, teaching, and thinking, as well as, we may imagine, life and death situations in which he probably learned a lot about honesty, integrity, fear, and consequences. Pondering Hymoff's account of Taylor's capture, we gain another perspective not simply on Taylor's travels and travails but on what kinds of experience and perspective he brought back from the war. Based on the archaeological literature, we might characterize these as a devil-may-care attitude, a fighting spirit, fearlessness, and more; but of course we might be misinterpreting or just plain wrong. Two things at least are certain: first, he held a long-standing (nineteen-year-old) guilt that he failed his mission and caused the death of a leader of the French Resistance; second, he only found time to return to that scene in the few years after Lyda's death.

It is challenging to draw meaningful conclusions from scattered events in Taylor's life and more challenging still to offer these as explanations—or excuses—for why he eased off from working on the Coahuila report. However, if we are to count and assess the lessons we learn from *ASOA*'s publication, the furor it caused, and the aggravation it brought its author, it may be worthwhile to consider the larger context and look beyond the more common explanations. Although Taylor's motivations and obstacles—his reality—will likely elude us indefinitely, we can at least learn to accept the possibility that not every *professional* judgment or interpretation of Taylor's actions will take us very far in understanding him, his book, or that volatile period in the history of American archaeology.

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NOTES

1. Willey and Sabloff (1993: 209n14) write in an endnote that "Taylor (1948: 170) was quite positively influenced by the British archaeologist Grahame Clark (1939, 1940)."

The reference is to a *very* brief mention in Chapter 6 of *ASOA*, where Taylor praises Clark's (1940) book as an "archaeological ethnography." Following this, Taylor quotes a few lines from another book by Clark (1939) that say that archaeology is not about antiquities but about people. These references are far too insubstantial to make any conclusions regarding influence. Trigger (2006: 371) suggests that Taylor expressed ideas in his book that parallel those of Clark (1939) although claims that Taylor failed to cite Childe and Clark. This claim is incorrect. See the brief ruminations on Clark offered by Dark (this volume). In his seminars, Taylor often praised Clark's work (Reyman, personal communication, 2008).

2. Taylor also coedited a book on Kluckhohn (Taylor, Fischer, and Vogt 1973) and contributed a chapter as well (Taylor 1973a).

3. While walking the aisles of the famous Powell's Books in Portland, Oregon (November 2007), I was stunned to discover Walter Taylor's personal copy of *The Maya and Their Neighbors* (Hay et al. 1940). It was a gift to Taylor from Alfred Tozzer who signed it "To my best research Assistant and Friend." Opposite the dedication is the stamp of Taylor's personal library. Jonathan Reyman was able to verify that the margin notes within the book were indeed written in Taylor's hand. One of the chapters with careful underlining and margin scribbling is Clyde Kluckhohn's well-known critique of Middle American archaeology. Taylor highlighted Kluckhohn's definitions of the terms—or, as Kluckhohn called them, the "hierarchy of abstractions" (1940: 43)—"methodology," "theory," "method," and "technique." He also highlighted Kluckhohn's discussions (ibid., 48) that explain the terms "assumption," "axiom," and "postulate." Among the other sections highlighted by Taylor are three that critique the Carnegie Institution. In one (p. 45) of these, Kluckhohn says that the CIW's multidisciplinary program is "but an extension of the received system, an improvement of method by intensification and intellectual cross-fertilization." In another, Kluckhohn (p. 50) writes "the light in which the members of the Carnegie staff view various specific questions reveals fairly consistent historical versus scientific interests." Taylor also highlighted Kluckhohn's (p. 46) discussion of the importance of theory, where he cites the resistance of the Carnegie to move beyond fact collecting: beyond the notion that "'theorizing' is what you do when you are too lazy, or too impatient, or too much of an armchair person to go out and get the facts."

4. Whitehead was at Harvard after 1924 (teaching logic, math, and the philosophy of science). Quine was Whitehead's student at Harvard, receiving his Ph.D. in 1932, and later taught logic and analytic philosophy there. Peirce, who preceded intellectually both Whitehead and Quine, studied at Harvard but never was hired there; his papers ultimately found a home at Harvard, however, and were published between 1931 and 1936. Taylor began graduate study at Harvard in 1938.

5. Burgh (1950: 117) refers to Taylor's "pretentious nomenclature," and Woodbury (1954: 292) to his "grandiose language."

6. "Development" was the term that both preceded the use of "evolution" and was used as a safe (apolitical) substitute for it in the proto-evolutionary era in American archaeology (e.g., see Willey and Phillips 1958).

7. For a concise overview of the standing and accomplishments of these five men, see Hudson (2008: 194): Haury headed the University of Arizona Department of Anthropology and the Arizona State Museum, an important funding agency in Southwest archaeology; Roberts led the River Basin Survey, was president of the SAA in 1950, and held lead-

ership positions with the AAA, American Association for the Advancement of Science, and more; Webb and Ritchie conducted major surveys and excavations and published widely; Griffin was head of the Ceramic Repository at the University of Michigan, wrote a major work on Eastern North American archaeology, and was president of the SAA in 1951.

8. Weigand and Longacre (this volume) discuss the importance in prewar anthropology of structural-functionalism and Weigand suggests that Taylor was interested in this approach after the war. The organization of Taylor's book may thus reflect an intentional design that works between the poles or in the interstices of idealism and empiricism. This attempt at creating an operational synthesis for theory and practice in archaeology is something I take up at the close of this chapter.

9. It is worth highlighting here the fact that although Willey came slowly to accept the New Archaeology, Sabloff and his wife, Paula, were ardent supporters of Binford and the New Archaeology (Sabloff 1990; P. Sabloff 1998). I mention this in part to inform any bias some readers may see in the Willey and Sabloff (1993) discussion.

10. Sincere thanks to Pat Watson for passing on to me her collection of materials (letters, papers, and announcements) associated with Taylor's retirement seminar. Reyman (personal communication, 2008) informed me that "[t]he seminar was not well attended, even by Taylor's SIU-C colleagues, many of whom were conspicuous in their absence. Students did not attend in large numbers even though there were major archaeologists—Hill, Longacre, Struever, Watson, etc.—as participants. I was told at one of the after-meeting gatherings that students were not encouraged to attend."

11. This is the list as written on the flyers for the event. Reyman (1999) notes, however, that James Brown was also included.

12. In an *American Antiquity* article celebrating the recently deceased Emil Haury, Ray Thompson (1995: 657) writes: "I remember being on the edge of a conversation between Emil and Walter Taylor at the Pecos conference in Flagstaff in 1953 [*sic*]. . . . Emil suggested that Walt might find it useful to consult with Woodbury on whatever it was they were discussing. Walt responded to Emil's suggestion by saying that he would never talk to Woodbury. Emil asked why and Walt explained that Woodbury had said some unkind things about him in that [1954] review. Emil's response was to chuckle and to point out that although Walt had said some unkind things about him [Haury] in his 1948 publication, those comments did not prevent him from talking to Walt." Reyman (1999) has also commented on Taylor's thin skin. When I phoned Woodbury in 2002, with an invitation to participate in this project, he responded curtly "no."

13. Reyman (personal communication, 2008) provided a ranking for me: (1) procrastination, because he always seemed to have something better to do: hunt, fish, travel, buy wine, and so forth; (2) a degree of fear that "they" (especially Jimmy B. [Griffin]) or their students were waiting for him coupled with the realization that he lacked the statistical tools and the useful production of data (his excavation units were not fine enough) to produce the full report he wanted and his critics demanded.

14. Taylor's words (see Reyman 1999: 684).

15. Clyde Kluckhohn also died (suddenly) in 1960. This must have been one of the worst years of Taylor's life.

16. Taylor's wife, Lyda, was also trained as a botanist.

17. Taylor "earned a Purple Heart and a Bronze Star and resigned as a captain in 1955" (Euler 1997).