# Prophet, Pariah, and Pioneer

## Walter W. Taylor and Dissension in American Archaeology

EDITED BY

Allan Maca, Jonathan Reyman, and William Folan

FOREWORD BY

Linda Cordell

PROPHET, PARIAH, AND PIONEER

In case of dissension, never dare to judge until you've heard the other side.

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*HERACLEIDAE*, CA. 428 BC

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For the mavericks, synthesizers, and dissenters

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# FOREWORD

*Linda Cordell*

The contributors to *Prophet, Pariah, and Pioneer: Walter W. Taylor and Dissension in American Archaeology* explore Taylor's life and work in archaeology. This is not a festschrift volume. Festschrifts are often thematically disparate statements by former students and colleagues. This book focuses on Taylor as a teacher and colleague and reviews his substantive research in the archaeology of the American Southwest and Mesoamerica. Most important, the chapters herein explore Taylor's detailed critique of Americanist archaeology (research undertaken by archaeologists trained in America, wherever they may work) and his formulation of what he called the "conjunctive approach," which offered direction for improving the field. As the editors indicate in their preface, some of the chapters in this book are critical of Taylor and his work and so depart from the generally celebratory nature of festschrift volumes. This book is not simply an exploration of an interesting personality in American archaeology. Many of the chapters are written by scholars who are known for their contributions to archaeological method and theory, and the volume as a whole should stimulate new dialogues in those areas and reflection on the nature of archaeological discourse.

Walter Willard Taylor (1913–1997), was educated at Yale, as an undergraduate, and Harvard, where he earned his Ph.D. in anthropology in 1943. He was a veteran (and POW) of World War II, and professor and chair of the Department of Anthropology at Southern Illinois University at Carbondale. In his doctoral dissertation, revised and subsequently published in 1948 as *A Study of Archeology*, Taylor provided a detailed critique of historical particularist archaeology, preoccupied with the systematics of time and space, that was prevalent in American archaeology in the first half of the twentieth century. His conjunctive approach was offered as a strategy for revitalizing the field (Watson 1983; Willey and Sabloff 1993: 96–152). *A Study of Archeology* became required reading in many graduate seminars in archaeological method and theory taught in universities in the later decades of the twentieth century, and the book is still in print (Taylor 1983). Taylor made enemies and had difficulty implementing his research agenda for reasons the contributors to this volume explore in detail, but the fact is that the shortcomings of early twentieth-century approaches continue to haunt archaeology. Many perspectives that are seen as innovative today (see Hodder 1991; Pauketat 2000; Hegmon 2003) owe an intellectual debt to Taylor. Here I explore briefly two facets of Taylor's work that are prominent in his legacy: the nature of his critique of Americanist archaeology and the strategy he used to deliver his ideas to his colleagues.

As Taylor (1983: 43) pointed out, archaeology "*per se* is no more than a method and a set of specialized techniques for the gathering of cultural information" or "the production of cultural information" (ibid., 44). Absent contemporary records, the data, observations, and stuff of archaeology are only "(1) spatial relationships, (2) quantity, and (3) chemico-physical specifications" (Taylor 1983: 145). Archaeology requires theory derived from another discipline (or disciplines) to interpret and make its data comprehensible or useful. The tools of archaeology may be used in the context of classical or biblical studies, architecture, or other disciplines. In the Americas, archaeology is usually offered in departments of anthropology where the intellectual goal is to understand culture at all times and places and the ways in which it develops and changes over time. Most Americanist archaeologists consider themselves anthropologists, whose mission it is to contribute to understanding the workings of culture in general.

In outlining his conjunctive approach, Taylor (1983: 153–154) argued that archaeology proceeds through different levels of analysis. Archaeological study may present the temporal sequence of data and contexts, producing local chronology, what he called "chronicle." For example, this might include a sequence of pottery types and house styles in a given area. Interpretation and synthesis of data and data contexts would produce ethnography (of a past society for archaeology) or in Taylor's terms, historiography. This would be a basic description of the past society comparable to a descriptive ethnography of a living group, such as a tribe or community. Taylor viewed the comparative study and interpretation of archaeological data and contexts as comparable to ethnology, which is the comparative study of living societies. Such comparison might be in chronological or cultural terms. A chronological example might be a study of the development of Pueblo Indian culture over time. A cultural ethnology might compare societies of hunter-gatherers in different environments. Only when archaeological data, data contexts, and interpretation addressed "culture, its nature and workings" (Taylor 1983: 53) would it be considered anthropology.

By the time Taylor wrote, archaeologists using the tools of stratigraphic analysis, and in the American Southwest dendrochronology (tree-ring dating), had gone beyond antiquarian collecting and had begun to write chronicle. Yet by neglecting the associations among artifacts, quantities (and ratios of artifacts and types of artifacts), and contexts (i.e., geological, biological, meteorological, and so forth), most archaeological reports, Taylor argued (1983: 45–94), failed to provide good chronicle. Taylor's analysis and critique of the problems in Americanist archaeology were thoughtful and thorough. He argued that the only way archaeology could get beyond basic chronicle was to develop method and theory, not simply refine field or laboratory techniques. He presented detailed critiques of the intellectual tools of archaeology—techniques of excavation and recording, classification, taxonomy, and quantification. He discussed the differences in goals of writing history (or historiography) and contributing to anthropology (the systematic study of culture). Although the topics Taylor discussed had been noted by others before him or at about the same time that he wrote (e.g., Cole and Deuel 1937; Kluckhohn 1940; Bennett 1943), Taylor's study was an in-depth analysis of issues that were and to this day are central to archaeology.

It is because Taylor's analysis was so penetrating and accurate that his work became a starting place for many scholars who have contributed greatly to archaeological method and theory, although not adopting most of Taylor's approach. For example, often beginning with Taylor's critique, there continues to be debate over whether archaeology is history or science (Spaulding 1968; Watson 1983; Binford 1989; Watson and Fotiadis 1990; Hodder 2001; O'Brien, Lyman, and Schiffer 2005). Taylor's work also resonates in ongoing discussion of whether anthropology is science and in debates about whether and how culture is, or is not, manifest in objects available for empirical archaeological study. Do archaeologists study culture directly through material objects, their quantities, and associations, or is culture purely ideational, nonmaterial, and therefore inferential (e.g., Hodder 1991)? As the essays in this volume explore, Taylor took positions on each of these issues, yet his notions are not necessarily the same as those of later writers who cite him. For example, although Binford (1972, 1983a) acknowledges the importance of *A Study of Archeology* in the development of his thinking, he (Binford 1983a: 61) distances himself from Taylor's (1983: 143) assertion that culture resides in the mind (Watson 1983). Because Taylor's analyses were so astute, even those who disagreed with his conclusions generally acknowledge having been guided by his perceptions. For these reasons, Taylor features in the citations of both Binford (1972, 1983a) and Hodder (1991), scholars who otherwise disagree on some basic principles of archaeological thought.

Among the chapters in the present volume, I found that Dark's discussion of the anthropology class Taylor taught to his fellow prisoners of war and both Reyman's and Weigand's remembrances of Taylor as a professor most helpful in understanding how Taylor likely refined and developed his own ideas and critical thinking. All of the chapters in Part IV of this volume, but especially those by Joyce and Maca, are helpful in elucidating the scholarly contexts within which Taylor developed his perspectives. Part IV also provides an excellent survey of topics relevant to current surveys of method and theory in Americanist archaeology.

In the infamous Chapter 3 of *A Study of Archeology*, Taylor analyzed the archeological programs of leading figures of his day (e.g., James B. Griffin, Emil W. Haury, Alfred V. Kidder, William A. Ritchie, Frank H.H. Roberts Jr., and William S. Webb). His task was "to analyze what the archaeologists say they have been doing and what they have actually done, and then to see how these two bodies of fact compare" (1983: 45). Taylor originally included disclaimers that his critiques were not personal (e.g., 1983: 45) and later stated that "contrary to what has apparently been the widespread view, that chapter is not a 'polemic.' I [Taylor] 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" (1983: 2). His colleagues, however, thought otherwise (Woodbury 1954; Longacre, this volume; Watson, this volume) and Taylor was ostracized by many in his profession.

As Maca explains in the introduction (and see Willey and Sabloff 1993: 154–155), Taylor's analysis and critique followed one that his professor and mentor, Clyde Kluckhohn (1940), leveled at Mesoamerican archaeology, including Kidder, the greatly respected, acknowledged dean of American archaeology. Kluckhohn, despite supervising archaeological research (Willey and Sabloff 1993: 155; Fowler, this volume), was a senior sociocultural anthropologist, and his remarks were published obscurely in a festschrift volume (Hay et al. 1940) for one of his Harvard colleagues, Alfred M. Tozzer. Taylor's analysis, in contrast, was a revision of his doctoral dissertation published at the beginning of his career, as a memoir of the American Anthropological Association. His book was therefore guaranteed a broad discipline-wide readership. As noted above, Taylor did not publicly, or for all I know privately, acknowledge a lack of judgment in disseminating his critique, although it cost him collegial goodwill. Taylor was certainly not the last to make his point by being critical of more senior scholars in his field, and some attacks are legendary (e.g., Binford 1972: 3–5; Flannery 1982). In fact the recent history of Americanist archaeology is traced through examination of who fought with whom over what, and what strategies were employed to recruit followers to promote ideas (O'Brien, Lyman, and Schiffer 2005).

Here I would make two points. First, even if colleagues marginalized Taylor, his book was widely incorporated in graduate archaeology seminars and his ideas are prominently acknowledged in histories of archaeological thought (such as Willey and Sabloff 1993). Second, although Taylor's ostracism is legendary (Longacre, this volume; Leone, this volume), he wrote at a time when scholars, sometimes critical of one another's work in print, continued to maintain personally cordial relationships. Watson (this volume) mentions "the original and obscure A. V. Kidder award" (Gumerman 2003), which is currently and temporarily in my possession. The "award" is a painting of a Navajo man producing a sand painting. On the obverse are the dated inscriptions transferring the painting, in acknowledgement of scholarly contributions, from Charles A. Amsden to A. V. Kidder, from Kidder to Clyde Kluckhohn, from Florence Kluckhohn (Clyde's widow) to Walter W. Taylor in memory of Clyde, from Taylor to Bob Euler, from Euler to George Gumerman (III), and from Gumerman to Linda Cordell. Gumerman and I are, and I am quite sure our predecessors were, deeply honored to have received this "award." That the painting was transferred from Kidder to Kluckhohn and Kluckhohn to Taylor points to respect that outweighed critique.

Finally, in 1983, on the occasion of the seventh printing of *A Study of Archeology*, Taylor (1983: 1) expressed his pleasure in knowing that "[a]rcheology in the United States today is a remarkably different discipline from what it was in 1948, and from my [Taylor's] view point, the outlook for the future is tremendously encouraging and exciting." More than twenty-five years later, Americanist archaeology has continued to grow, often in directions Taylor anticipated (see Hegmon 2003). The value of the current volume is that it captures a multifaceted individual from a variety of perspectives and places him in a time that was one of disciplinary change. Those who are interested in archaeology, who are students of the history of science, the philosophy of science, and the politics of academia, will find this an exceptionally useful book.

Linda S. Cordell
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# PREFACE

*Allan L. Maca, Jonathan E. Reyman, and William J. Folan*

In American archaeology, Walter Willard Taylor, the scholar and the man, has been misunderstood, misread, and mythologized; disparaged, vilified, and hailed as a founding father; ignored, glorified, snubbed, and treated at turns with contempt and compassion. How could one person elicit such a range of feelings and reactions? This book attempts to answer these questions, directly and obliquely, and to do so from a primarily professional point of view. We know about Taylor's personal life: at times he dealt coarsely with students and colleagues; he loved his dogs; he liked to hunt and brew beer; he built homes in Mexico and New Mexico; he lost his wife to cancer; as a marine and OSS spy he was wounded and captured by the Germans in World War II; and he taught anthropology to fellow prisoners before escaping. These composite images come through in various chapters. Indeed, sketches of Walt Taylor are offered here by many of the volume's authors—there are remembrances and characterizations and these aid our comprehension. However, we wish to note at the outset that, although parts of this book are, broadly speaking, ethnographic and sociological, even psychological, the book does not significantly focus on Taylor's personal life. Our goal has been to showcase Taylor's contribution to the history of the field of American archaeology, not (or at least not solely) to present the complex history of Walter Taylor.

Taylor was a scholar saddled with numerous contradictory myths and perceptions, most of which derive from responses to his famous book. At age thirty-four, he published a pathbreaking treatise that stunned his senior colleagues, damaged his career, and endured in print and influence well beyond his death in 1997. *A Study of Archeology* (1948) was issued by the American Anthropological Association and was intended to close the distance between archaeology and anthropology. The text was popular for the countless aspersions it cast but some found its theory and idiom impenetrable.

If Taylor's book and objectives have been difficult for many to interpret, it has proven more difficult still to securely identify Taylor's role in American archaeology, the sources of his ideas, the meaning and orientation of his magnum opus, and his influence on the field. We and the other volume contributors attempt to resolve some but certainly not all of these issues and to answer a select number of questions that are complicated or common or both. At the very least we want this book to breathe some life and analysis into the mesh of seeming contradictions and inconsistencies that characterize notions of Taylor's place in American archaeology. Many contradictions are plainly irresolvable; in fact, several of the volume authors contradict one another. But this should not be a surprise: our book is a beginning, in many ways an initial survey and excavation of a monument that will both elude and attract visitors for many years to come.

This book project formally began in 2003 with a forum at the Society for American Archaeology (SAA) meetings in Milwaukee, organized by Allan Maca and called "Walter W. Taylor: A Critical Appreciation." Maca had first read Taylor's book in a graduate seminar at Harvard University taught by Robert Preucel in the early 1990s. At that time he discovered that Taylor's book is complex and poorly understood and that it was a central source of controversy. Between 1995 and 2001, Maca observed that a form of Walter Taylor's "conjunctive approach" was being adopted and encouraged by senior scholars in Maya archaeology. Because of the unusual absence in the Mayanist literature of a discussion of Taylor's book, as well as a general lack of attribution to Taylor at that time, Maca wrote a dissertation chapter that addressed Taylor's apparent influence on the present-day archaeology of Copan in Honduras. An advisor, the epigrapher David Stuart, suggested that Maca contact William Folan, a Mayanist working in Mexico, and that they discuss pursuing the topic in more depth. Folan, with Jonathan Reyman, had tried in 1988 to assemble a festschrift volume for Taylor; Folan had been a student and friend and Reyman a Ph.D. student of Taylor. Because of lasting tensions in the field, however, they found few scholars willing to comment in print and the project was abandoned a year after it began. Folan and Reyman agreed to re-engage with the topic when Maca asked for their expertise and assistance in thinking about the structure of the SAA forum and whom to invite.

The forum gauged the significance of Taylor's work in American archaeology and what living scholars saw as the uses and applicability of his conjunctive approach. We chose participants who are critical still of Taylor as well as those who appreciate his contributions. Audience participation was crucial, and the several hours of discussion centered on short presentations given by William Folan, Don Fowler, George Gumerman, Rosemary Joyce, Mark Leone, William Longacre, Allan Maca, and Patty Jo Watson. Jonathan Reyman sent a paper since he could not attend. Many intriguing issues surfaced during discussions, ranging from matters affecting Taylor's work—the sociopolitics of postwar American archaeology, Taylor's chairmanship of the Department of Anthropology at SIU-C, his exposure to semiotics and uses of logic and philosophies of knowledge—to questions of Taylor's intentions—the possibility that Taylor sought the directorship of the Carnegie and the nature of his academic relationship with Clyde Kluckhohn. The level of post-SAA enthusiasm among participants and others remained significant, and so Maca decided to pursue an edited volume. Folan and Reyman were invited to be coeditors and together we requested contributions from additional scholars.

In his discussion of the response to Taylor's book (i.e., Taylor as persona non grata), Andrew Christenson (1989: 164–165) notes that Taylor suffered ostracism because he was young and lacked a power base; that had he been more established he would have suffered less; and that his professional fate was sealed by the elders in the field, many of whom were on the receiving end of his critique. He goes on to add (ibid., 165), "The writing and reaction to *A Study of Archeology* deserves careful examination. . . . [U]nfortunately, for the reasons discussed above, such a consideration will probably have to wait until the principal people involved die." We believe we have been able to produce the careful examination Christenson (and, e.g., Leone 1972c: 2) calls for and would like to acknowledge our appreciation for three men who were initially involved in this project but who sadly died before its completion: Gordon Willey, John Bennett, and Philip Dark. They are missed. Professor Dark fortunately was able to submit a final draft of his paper before his death.

This book is unique; no other substantial consideration of Taylor exists. It includes contributions by the only three students to complete their doctorates under Taylor, by the only anthropologist to share POW imprisonment with Taylor, and by two colleagues who worked with Taylor at the establishment of the SIU-C Department of Anthropology. The chapters include textual analyses of work published by Taylor and others, explanations of his courses and teaching, analyses of the culture of twentieth-century American archaeology, and commentaries on Taylor's interactions with colleagues, students, POWs, and others. Taylor's controversial 1948 monograph remains in print after sixty years, a rare phenomenon in archaeology. As interest in Taylor's work shows no signs of waning and appears to be growing, we offer our book as a means of constructing a fuller context by which to understand this maverick and his place in American archaeology. The title of the volume, taken partly from the provocative title of Folan's chapter, reflects our more abstract sense of Taylor's status in the history of our field and also our understanding of why this status has become so contradictory. Acts of dissension in academia can truly have mixed and extreme results.

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## CHAPTER SUMMARIES

The following chapters present an overview of Walter Taylor's work and life and then move to discussions by Taylor's colleagues at Southern Illinois University. These are followed by contributions from several of Taylor's students at SIU, including the only three students to receive Ph.D.s under Taylor. The final section centers on critical analyses of Taylor's research and influence by a number of scholars, men and women alike, who work across the geographical and theoretical spectrum in American archaeology. One of the things that any careful reader of this volume will note is that many of the authors offer opinions and perspectives that contradict those expressed by others (herein and elsewhere). As editors, we have granted freedom to all views and by and large do not identify our differences or agreements with these authors. It is hoped that the array of perspectives will be weighed one against the other and ultimately draw the reader back to the work of Taylor and to other important research of the period.

Because the contributors have been allowed to share openly their personal and professional views, many of the chapters say at least as much about the authors as they do about Taylor. This is not surprising given the fact that Taylor had an uncanny ability to, on the one hand, alienate students and colleagues alike and, on the other hand, make them believe he wanted and valued a personal relationship with them. Taylor therefore is shown to be a highly complex individual. The complicated task of "reading" Taylor the man extends to reading Taylor's 1948 book and understanding its reception. For this reason, some of the volume authors discuss the conjunctive approach as inductive and others as deductive; some speak of Taylor's criticisms as personal and others cite them as professional. We appreciate the divergent views generated by decades of emotions, interpretations and misinterpretations, and personal and professional inclinations. It remains for a future generation to look at these in a somewhat more objective manner, but it is hoped that these chapters provide ready access to the broad spectrum of views that will always characterize Walter Taylor's place in American archaeology.

### *Part I: Introduction, Background and Overview*

Allan L. Maca provides the introductory chapter for the volume, placing Walter Taylor and his famous book in social, historical, and intellectual context. Maca examines American archaeology before World War II and the substantial scholarly influences on Taylor's thinking and discusses the still controversial relationship between the conjunctive approach and the New Archaeology. Maca's chapter is perhaps most significant for providing the first in-depth overview and analysis of Taylor's 1948 book ever published. The chapter closes with a lessons section for those pondering major critiques, in which are included fascinating passages about Taylor from an obscure book about OSS agents.

Jonathan E. Reyman offers a concise and informative biographical sketch of Taylor's life. It serves as a substantial complement to Brenda Kennedy's chapter for it excludes detailed discussions of scholarship yet fills in several gaps that Kennedy could not address. At the end of Reyman's chapter he includes a full bibliography of Taylor's published works.

Brenda V. Kennedy wrote her 1984 University of Calgary master's thesis in anthropology on Walter Taylor. This chapter is an updated and refined version of this manuscript. It is a biographical narrative, ranging from his youth to his later life, that benefits from personal documents provided by Taylor himself. In addressing Taylor's research, Kennedy considers his published work and provides a careful look at Taylor's impact on the theoretical and methodological movement known as the "New Archaeology." She also includes her own professional assessments of Taylor's ideas and on the whole provides the most thorough overview of Taylor's publications ever written.

### *Southern Illinois University: Colleagues' Perspectives*

Southern Illinois University at Carbondale was an important new node in the United States during the period of anthropology's expansion after World War II. When a large-scale project was put into effect (ca. 1958) to build the SIU Department of Anthropology for serious graduate study and the granting of Ph.D.s, Walter Taylor was hired as chair. Taylor, having had difficulty getting a job, had been living in Mexico with his family. When his wife became ill, he chose to return to the United States and began his tenure at SIU-C. The papers in this section provide context for this period and for Taylor's role and leadership in the department. The chapter by Philip Dark, a later addition to the SIU-C faculty, begins the section because its discussion starts with Taylor's experience as a POW during World War II.

Aside from J. Charles Kelley, Philip J.C. Dark knew Taylor longer and more intimately than any other contributor to this volume. Dark, a former British naval officer, provides many of the heretofore unpublished details of the time they spent together in the Marlag Nord German naval prisoner-of-war camp. Dark's paper recounts the conditions at Marlag and, using his own class notes, describes the Introduction to Anthropology class that Taylor provided for the prisoners. Dark, an accomplished artist, went on to doctoral studies at Yale in cultural anthropology and later became an SIU colleague of Taylor and succeeded him as chair of the department. The chapter discusses the whole of his professional relationship with Taylor. Dark's closing is of special interest because it provides a perspective on the reception of Taylor's work by archaeologists who were not Americanists.

The late J. Charles Kelley, a Harvard classmate and fieldwork colleague of Taylor in the Southwest, oversaw the founding of the museum and anthropology department at SIU and was instrumental in bringing Taylor to the university as the first department chair. Kelley recounts the early years of the department, beginning with a search for a chair and continuing through the development of the program under Taylor's leadership. The essay was originally written in the late 1980s in response to a request by Folan and Reyman for a chapter in their attempted volume on Taylor. The request was for a balanced overview on Professor Taylor as chairman of the SIU Department of Anthropology.

Carroll L. Riley, like Kelley, provides us with a glimpse of the early development of the SIU Department of Anthropology. However, Riley moves beyond Kelley's comments and carries his discussion through to Taylor's retirement in 1974. Riley covers his relationship with Taylor on both personal and professional levels; describes some of the internal departmental issues that he, Taylor, and others dealt with during Taylor's time at SIU; and ends with comments on the ascent and descent of Taylor's meteoric career.

### *Southern Illinois University: Students' Perspectives*

Taylor's relationships with his students were complex and varied; this becomes evident when we compare the papers by Clay, Schoenwetter, Folan, Weigand, and Reyman. In general, this section further characterizes the early years of the Department of Anthropology at SIU, especially as it existed under Taylor's guidance. We learn of the program structure, the means of qualifying for the Ph.D., and of the culture of the graduate program. Taylor graduated only three Ph.D. students in his sixteen years at SIU: Clay, Schoenwetter, and Reyman. (Folan had taken a class in Mexico with Taylor, was an M.A. student at SIU, but, like Weigand, received his Ph.D. under Professor Carroll Riley.) This section also provides general and specific insights into Taylor's courses, including archaeological theory, introductory anthropology, and European prehistory.

Following his studies at SIU, R. Berle Clay was at Tulane and later was state archaeologist in Kentucky. He begins talking about his teaching assistantship for Taylor's Introduction to Anthropology class and then goes on to the lessons of Taylor's graduate courses, including his strategy of teaching students to trace references backward in a sort of bibliographic historiography. Clay also mentions Taylor's language skills, particularly in Spanish. The great bulk of this chapter, and arguably its greatest significance, is Clay's discussion of how Taylor's ideas outstripped both the technology of the day and, in particular, Taylor's capacity to employ statistical analyses.

James Schoenwetter's paper proves that Taylor's relationships with his students were at times rocky. Schoenwetter expresses what he felt was an attitude of hazing by Taylor during the course of his graduate studies. He cites what he believes were deficiencies in the content and methods of Taylor's pedagogy but nonetheless explains some of the valuable lessons learned from Taylor that helped shape his successful career as a palynologist. Some of these are expressed more as "don'ts" than as "do's," but overall it is clear that Taylor's teachings provided a positive structure for Schoenwetter's dealings with his own students.

William J. Folan presents a tripartite view of Taylor as a family friend, mentor, and teacher. We see the complexity of a relationship that evolves over decades and crosses the lines between friendship and mentorship. Through this chapter we learn of Taylor's work and life in Mexico and later of the tensions in the SIU Department of Anthropology in the 1960s; this latter issue is a focus of Schoenwetter's paper as well. We also get a discussion of Taylor's life and thoughts after his retirement through Taylor's letters to Folan. At the request of Maca, the latter half of the chapter describes Folan's applications of the conjunctive approach during the course of his work in Mesoamerican archaeology and elsewhere.

Phil C. Weigand provides unique details on Taylor's course in archaeological theory, its direction, and how it was taught. Weigand goes beyond the context of the classroom to a description of Taylor's social dealings with students and the parties held at Taylor's house. He also characterizes Taylor's political views at the time and cites the internal problems at SIU, partly instigated by Taylor, that created difficulties for the shaping of Weigand's graduate research. Weigand closes with a discussion of structural-functionalism and its importance to Taylor.

Jonathan E. Reyman was the last of Taylor's three Ph.D. students. He expands a 1999 paper on Taylor published in Tim Murray's edited two-volume work, *The Great Archaeologists*. As the student with the greatest scholarly contact with Taylor, he characterizes the history and origins of the Department of Anthropology at SIU and, as such, complements the papers by Carroll Riley and J. Charles Kelley. Having set the scene for the departmental structure, Reyman's paper provides the deepest look we have into many areas of Taylor's teaching and life, from his class Themes in Southwestern Archaeology to a graduate research fellowship with Taylor at his library in Santa Fe and postdoctoral work with Taylor on the write-up of the Coahuila monograph, planned as the grand example of the conjunctive approach. Before closing, Reyman provides examples from his own work of the effect of Taylor's conjunctive approach.

### *Analyses of Taylor's Work and Influence*

The chapters in this section take us beyond Taylor's institutional base at SIU by addressing and analyzing the impact and implications of Taylor's work. The topics and theoretical orientations expressed here are fairly wide-ranging and are offered by scholars working in Mesoamerica, North America, and elsewhere.

William A. Longacre is one of the five contributors to this volume who was also a participant in a 1974 symposium honoring Taylor on the occasion of his retirement. Longacre considers the impact that Taylor's *ASOA* had on him during his graduate years and beyond. He places the book in the context of the structural-functionalism of the day and closes with a first-person account of the lambasting of Taylor during a special session at the golden anniversary of the Society for American Archaeology in 1985. Longacre makes a forceful point that the animosity toward Taylor continued for too long and that it is and was undeserved.

Patty Jo Watson is author of the foreword to the 1983 edition of Taylor's *A Study of Archeology*. She also is one of the five contributors to this volume who was a participant in a 1974 symposium honoring Taylor on the occasion of his retirement. Moreover, she is one of the three editors of the recently published *Sandals from Coahuila Cave*, by Walter W. Taylor (2003, Dumbarton Oaks Press). Watson's chapter includes discussion of Taylor's dissertation version of *A Study of Archeology* in comparison with the published version. She considers changes made from the dissertation to the published monograph, demonstrating, for example, how much longer and more pointed is his published critique of A. V. Kidder in the monograph. In her conclusion, Watson argues that after publication of *A Study of Archeology* and Taylor's explication of the conjunctive approach, he "walked away" from his conceptual scheme; he did not promote it in either his own work or that of his students and colleagues. Watson closes by identifying the best current example of the type of research Taylor would have encouraged.

Alice B. Kehoe focuses on the life and research orientation of the archaeologist and ethnologist Cornelius Osgood. Taylor studied with Osgood as an undergraduate at Yale and credits his mentor with having contributed deeply and fundamentally to his thinking. Taylor (1948: 10) wrote, for example, "Cornelius Osgood is responsible for much of the manner in which I look upon archeology." Kehoe's chapter identifies conjunctive-type trends in the scholarship and teaching of Osgood, which she argues must have been the basis of his impact on Taylor. She also briefly considers some crosscurrents of that time, citing the work of Rouse (also influenced by Osgood), Spier, Sapir, and others.

Rosemary A. Joyce wrote the original version of her chapter in 1988 for the volume that Folan and Reyman planned. It specifically addressed Taylor's 1941 *American Antiquity* paper, "The Ceremonial Bar and Associated Features of Maya Ornamental Art." That project was abandoned and the paper lay dormant for fifteen years. It was revised in light of new insights into Taylor's interest in semiotics. Joyce demonstrates that Taylor's 1941 paper is remarkable not only for the sophistication of its analysis but also because it was forty years ahead of its time. Among the many questions that Joyce's chapter encourages us to ask are, why did Taylor not pursue further issues of Maya ornamental art? And, perhaps most importantly, why this paper, which in many ways goes to the heart of the conjunctive approach, was not cited by Taylor himself in support of his arguments in *A Study of Archeology*?

Allan L. Maca is a Mesoamerican archaeologist working in the Maya area of western Honduras. His chapter focuses on Taylor's conjunctive approach, paying special attention to issues of attribution—who supported Taylor's ideas, who ignored them, and why. Archaeologists Gordon Willey, Lewis Binford, Joyce Marcus, and others play key roles in this regard. Maca explains that Taylor's initial arguments were, in the years after the 1948 publication, at best misunderstood and distorted and at worst ignored, but that after 1968 there was a resurgence of interest in Taylor linked to conscience cleansing, hindsight regarding the origins of the New Archaeology, and the tide of postprocessual thought. Today, a variation of the conjunctive approach emerges in Maya archaeology. The latter part of this chapter traces and evaluates this vestige in light of patterns of attribution.

Don D. Fowler gives us a broad consideration of Taylor's contributions to Southwestern archaeology. Many chapters in this volume discuss Taylor's *A Study of Archeology*. Fowler, however, chooses to focus on Taylor's other Southwestern work: The Pueblo Ecology Study, his history of Southwestern archaeology paper, his genetic model, and other works. It is for Fowler a mixed record in which successes are counterbalanced by failures, the history and nature of which may be better understood by granting larger recognition to the contributions of William Y. Adams and Lyndon L. Hargrave.

Mark P. Leone was editor of the 1972 book *Contemporary Archaeology*, to which Walter Taylor contributed his well-known rejoinder "Old Wine, New Skins." In his chapter for the present volume, Leone begins with an analysis of the anger directed at Taylor, which he sees as a matter of projection. Archaeologists, at the time and since, were aware that what they planned to do or said they did, did not match the results of their research. When Taylor specifically pointed this out in *A Study of Archeology*, they projected their preexisting frustrations and anger toward Taylor. In a shift to Marxist and post-colonial interpretation, Leone suggests why this was so. At least part of the anger, he says, came from the realization by archaeologists that they have been instruments in the colonial oppression of indigenous peoples. He also argues that critiques such as Taylor's occur periodically as part of the self-examination that goes on in any intellectual field. Rather than condemning Taylor in perpetuity, Leone argues that because such self-criticism is needed, we might look to Taylor as a model for what we should expect and seek.

The final chapter is a discussion of the volume as a whole, provided by Quetzil E. Castañeda, a sociocultural anthropologist and ethnographer of Maya archaeology in Mexico. He focuses on the importance of studying networks in academia—especially the social and political contexts of archaeology—both as a means for interpreting the significance of Taylor's work (and this volume) and as a direction for future research in American archaeology. Castañeda is one of the two contributors who are not archaeologists. He opens avenues for many novel investigations and we imagine that Taylor would have appreciated these remarks from the arena of cultural anthropology.

ALM
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# PROPHET, PARIAH, AND PIONEER

# 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:

Part I

Introduction

1. The Development of American Archeology
2. Archeology: History or Anthropology?
3. An Analysis of Americanist Archeology in the United States

Part II

Introduction

4. A Concept of Culture for Archeology
5. The Nature of Archeological Data: Typology and Classification
6. An Outline of Procedures for the Conjunctive Approach

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 nonmaterial 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?*

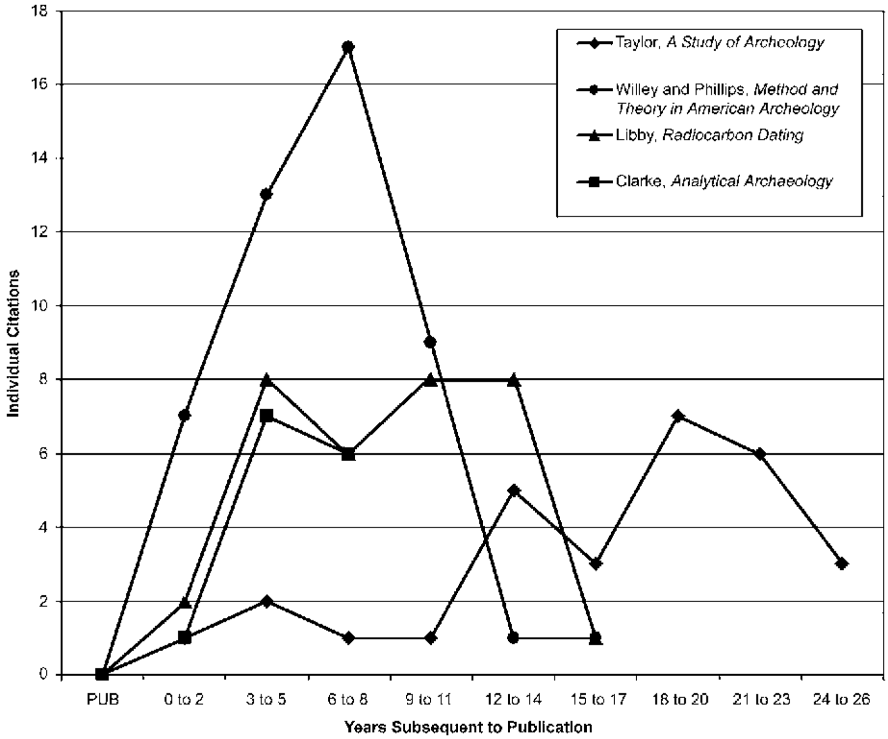
An article on Walter Taylor was recently published by Corey Hudson (2008) in the *Journal of Anthropological Archaeology* (*JAA*). The gist of Hudson's argument is clearly stated: "[T]here is no reason to believe that [Taylor] was a 'precursor of the major theoretical advances of the 1960s' (Fagan 2005: 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 war-torn 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 shoot-out. 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.

## ACKNOWLEDGMENTS

Special thanks to my coeditors, Jonathan Reyman and William Folan, for their assistance with this chapter. They suggested several wise additions and deletions to this chapter as a whole. I also thank Linda Cordell, Kristin Landau, Lee Lyman, Kevin McLeod, and Patty Jo Watson for their feedback on earlier drafts of this chapter. Any errors or omissions, however, are entirely my own and I am solely responsible for the content and tenor of this chapter.

## 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).

CHAPTER TWO

# WALTER WILLARD TAYLOR JR.

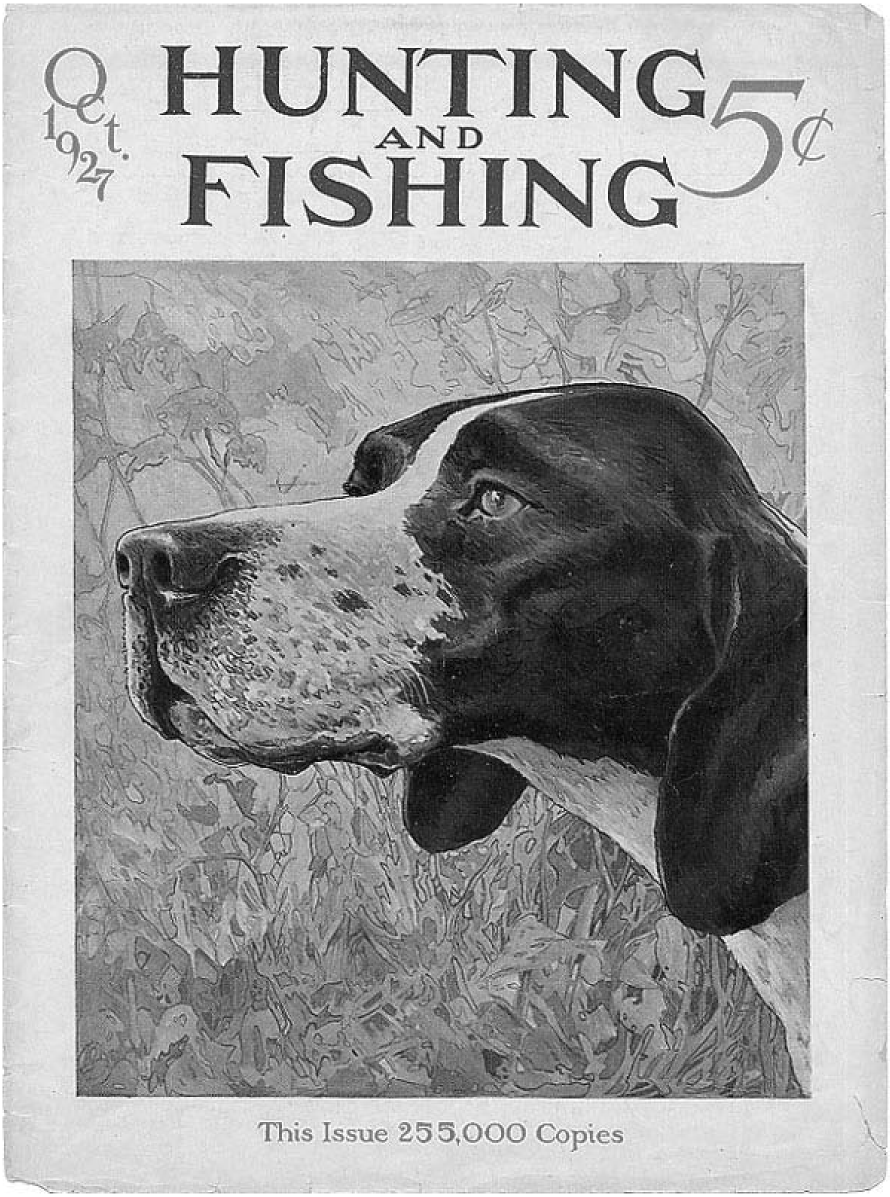
## A Biographical Sketch and Bibliography

*Jonathan E. Reyman*

Walter Willard Taylor Jr. was born to Walter Sr. and Marjorie Wells Taylor in Chicago, Illinois, on October 17, 1913, amidst a three-day spell of unseasonably warm weather.[1] Record high temperatures were set on both October 17 (86°F) and 18 (87°F). Those who would look for omens or portents in the weather at the time of Taylor's birth might view these high temperatures as indicators of the heat to come following the 1948 publication of *A Study of Archeology*. But on October 17, the howls of distress came only from Walter Jr., perhaps not unlike the reactions from those he criticized some thirty-five years later.

Taylor was seven or eight years old when his family moved to 10 Deer Park Court, Greenwich, Connecticut, from which Walter Sr. had an easy commute to Wall Street via the New York, New Haven, and Hartford Railroad. Taylor's father, a bond broker, relocated his office in 1932 to one of New York City's premier Art Deco skyscrapers—the Irving Trust Company Building, as it was then known, at One Wall Street.

Taylor's parents enrolled him at the Hotchkiss School in Lakeville, Connecticut—about eighty-five miles north of Greenwich—from which he graduated in 1931. Shortly after his matriculation in the fall of 1927, Walt published his first article—"Lucky Thirteen" in *Hunting and Fishing Magazine* (Fig. 2.1).




**2.1** Cover page of the October 1927 issue of *Hunting and Fishing Magazine*, in which Taylor published his first article, "Lucky Thirteen."

His account of an early, perhaps his first, hunting experience, "Lucky Thirteen" was always listed in his curriculum vitae. It is also the only publication for which Jr. appears after his name. All his professional publications were as Walter W. Taylor. As Kennedy (this volume) notes, Taylor was immersed at Hotchkiss in a wide variety of extracurricular activities that took time away from his academic studies. Some, such as music and drama, remained important through much of his life: Taylor learned to play the Spanish guitar and became expert at it, and he was heavily involved with theater in Santa Fe and Mexico. Hunting and fishing were also important until late in his life when he became physically unable to pursue them. Taylor especially enjoyed hunting ducks and quail, loved fly-fishing, and excelled at all of them. Unlike many hunters and fishermen among his contemporaries, Walt was not a trophy seeker: he ate what he hunted and caught, and he often invited guests to share the food he meticulously prepared.

Cooking was another longtime interest, as were fine wines. Walt was an excellent cook and an oenophile with an outstanding, well-stocked cellar. He enjoyed beer and brewed his own during his years in Santa Fe. One prominent memory remains from the fall of 1968: shortly after arriving in Santa Fe to study with Taylor in his library, I walked into the house one morning with Tom Holien (his graduate research assistant) and Barbara Peckham (his secretary). We were greeted by air redolent with the odor of stale beer. Hundreds of bottles of newly capped home brew had exploded the night before, shattering glass and spewing liquid throughout the brewing room where they were resting. Apparently, the fermenting process continued after bottling, and they blew up from the pressure. One almost became intoxicated from the fumes that lingered. Another beer-related recollection is that in 1971–1972, Taylor had fresh oysters flown in from the East Coast and for several days running feasted on raw oysters and beer for breakfast.

As a sportsman, Taylor was a participant, not a spectator. His father took him fly-fishing for salmon in Scandinavia and taught him to hunt, triggering lifetime passions. At Hotchkiss, Taylor may have learned to play lacrosse and probably began to play squash. The latter became a lifelong interest; he was a fine squash player who remained active until well into his sixties when an Achilles tendon problem ended his play.

Probably shortly after graduation from Hotchkiss, Taylor, by his own account, rode the rails at age seventeen—"hoboing"—to the Southwest. It was never clear to me whether this was an adventure, an act of teenage rebellion, or both, but upon his return from the Southwest, he entered Yale University. Walt's goal was Harvard, but his father, a Yale graduate, "persuaded" Walt to enroll at Yale where he earned departmental honors along with his A.B. in geology (1935). He also continued his involvement with sports, winning his class's middleweight boxing championship and the impressive cup that signified his achievement. Years later, as chair of the Department of Anthropology at SIU-C, an angry Taylor challenged Charles Kaut, a fellow anthropology faculty member, to settle their differences with their fists. Kaut wisely refused the challenge.

Edward Sapir and Cornelius Osgood were among Taylor's professors at Yale. Both significantly influenced his thinking about anthropology and archaeology, Osgood especially so in archaeology (Taylor 1948: 10). Taylor remained at Yale working on a master's degree and met Clyde Kluckhohn there in 1935–1936 (Taylor 1973a: 23). Kluckhohn quickly became Taylor's intellectual mentor and close friend.

Lyda Averill Paz was one of Taylor's graduate classmates at Yale. They married in September 1937; worked together in the field, notably during the 1940–1941 Coahuila Project; and had three children: Peter, Gordon ("Natch"), and Ann ("Miss Annie"). None followed their parents into anthropology, but Natch achieved notable success as a dancer and as one of the founders of Les Ballets Trockadero de Monte Carlo.

The summer of 1935 found Taylor at work on archaeological projects for the Museum of Northern Arizona, where he encountered Lyndon L. Hargrave, another important archaeological mentor. In 1936, Taylor excavated in Georgia with A. R. Kelly and then returned to the Southwest and Mexico. He did archaeological fieldwork in Arizona (1937, 1938); in Coahuila, Mexico (1937, 1939); and at Chaco Canyon, New Mexico, with Clyde Kluckhohn (1938–1940). Then, in Taylor's words, "[i]n the fall of 1938, on Clyde's urging, I went to Harvard. The relationship did not change. He [Clyde] merely added the role of patron to those of friend and tutor" (Taylor 1973a: 24). E. Wyllys Andrews IV, Taylor's cousin, also entered Harvard's graduate anthropology program in 1938.

As background and context for the intellectual milieu of Taylor's graduate student years, Kennedy (this volume) mentions many of his classmates at Yale, New Mexico, and Harvard; the faculty with whom he studied; and the faculty and students present during his summer field sessions. American archaeology was a smaller world then, and the names are almost a Who's Who of American Southwestern archaeology at the time. One of Taylor's Chaco Canyon supervisors, Frank H.H. Roberts Jr. was one of the six archaeologists singled out for criticism in *A Study of Archeology*.

Such critiques might have been begun at Chaco Canyon during late-night discussions with Clyde Kluckhohn. Taylor (1973a: 24, 29) writes that in 1938,

> [i]n early August . . . we [Taylor and Kluckhohn] moved to Chaco Canyon as staff members at the University of New Mexico summer field school. It was at that time that he introduced me to the Tower Kiva in Chetro Ketl, where then and later we sat in the dirt, leaning against the ancient walls, and talked for hours and hours in an isolation and rapport all but impossible elsewhere. For three consecutive summers, while I worked at Chaco and he was in the Southwest, we went to the Tower Kiva on those occasions when he visited the summer school. There, in the midst of an anthropological world, both of us

> full of ideas and problems, we could become so immersed in talk that many times it was morning light when we walked back to camp and to work . . .
>
> I can still hear the Tower Kiva echoing with invective as dark and blue as the starlit night, all because I had begged a question or assumed a premise—but I cannot for the life of me recall what question or premise it might have been.

Taylor and Kluckhohn also spent time attending Navajo dances. Kluckhohn was conducting a multiyear study of the Ramah Navajo, and Taylor accompanied him, furthering his education and direct field experience in cultural anthropology. It was while visiting the Navajo that Taylor acquired the handsome, wide, stamped silver Navajo bracelet that he wore thereafter. This bracelet and Taylor's paratrooper ring were the only pieces of jewelry I ever saw him wear (see photo section of this volume). His wristwatch—stainless steel and utilitarian—was hardly a piece of jewelry.

Chaco is conducive to discussion and argument, and Taylor may have further honed his critical skills around the evening campfires there. Walt and J. Charles Kelley both recounted their regular participation in these. One memorable campfire discussion involved a number of Chaco fieldworkers including Taylor, Kelley, and Frank Hibben (all now deceased). As the night wore on, each time someone left to go to bed, those remaining would severely criticize his deficiencies as an archaeologist (and perhaps other things as well). Finally, only Taylor, Kelley, and Hibben remained, at which point one of them (Taylor and Kelley's accounts differed in this detail) said something to the effect of "Well I'm not leaving so the two of you can cut *me* to shreds." Someone else said much the same thing, so all three agreed to leave together and to return to their respective tents, presumably with their dignity and reputations intact.

I have noted previously (Reyman 1992: 75; 1999: 689) that Taylor, like most of the male archaeologists of his day, did not welcome women into archaeology, either in the field or in the classroom. Walt's experience at Chaco taught him many things, but it did not teach him to appreciate women as colleagues. This seems curious given that during Walt's seasons at Chaco, both Florence Hawley and Bertha Dutton were there in supervisory positions and did fine archaeological work. Lyda did accompany Walt to Coahuila during the 1940–1941 fieldwork, but most of the time she did not live in the field camp but in the town of Cuatro Cienegas where she cataloged the excavated materials and analyzed the botanical specimens, her specialty (L.A.P. Taylor 1940).

Reading Taylor's (1973a) discussion of his relationship with Kluckhohn and his descriptions of Kluckhohn's analytical and critical skills and of Kluckhohn's style of teaching and mentoring, one can understand the influence he had on Taylor's professional career, both in terms of research and publication and in teaching. One interesting note that I do not believe was previously published is Taylor's (1973a: 25–26) assertion that Kluckhohn did not want him to write the dissertation that eventually became *A Study of Archeology*. Kluckhohn finally acquiesced: "Okay, Walt; go ahead and write it" (Taylor 1973a: 26), which strikes me, perhaps, as a belated reply to those critics of the monograph who claimed that Taylor was fronting for Kluckhohn (see Reyman 1999: 683). This point is reinforced by the letters between Taylor and Kluckhohn during Taylor's early World War II military service and after the war (e.g., Taylor to Kluckhohn February 2, 1943; February 21, 1943; December 1, 1946; and May 20, 1947).

Taylor's World War II experience is discussed by Euler (1997), Reyman (1997, 1999), and especially by Dark (this volume; see also Chapter 1). Suffice it to say, he served with distinction. After mustering out, a 1946 Rockefeller Foundation Fellowship in Humanities supported his work to revise and expand his dissertation into a publishable manuscript. It appeared in 1948 as *A Study of Archeology*, Memoir 69 of the American Anthropological Association, by which time Walt, Lyda, and their elder son, Peter (born while Taylor was a POW), had moved to Santa Fe.

## THE AFTERMATH OF *A STUDY OF ARCHEOLOGY*

The reaction to the publication of *A Study of Archeology* is well-known: some archaeologists read and damned it, and some read and praised it. Jennings (1986: 58) notes, "A third event [in 1948], ignored by many and therefore largely futile, was W. W. Taylor's (1948) essay" (see also Watson, this volume). Yet, "[a]lthough ignored for a time, its essence was reissued serially by many authors in the 1960s as the 'new archaeology'" (ibid.).

Several points must be made. First, the late J. Alden Mason, a well-respected archaeologist and former president of the SAA (1944), was editor of the *American Anthropologist* (1945–1948) when the manuscript was submitted for publication. Presumably, he would have read it, and it certainly went out for peer review. Neither Mason nor the reviewers apparently thought it so negative in tone or the critique so harsh or unfair that they raised objections to the style sufficient to reject the manuscript. Nor is there any extant evidence of which we are aware that Mason urged Taylor to modify or tone down what has sometimes been referred to as his ad hominem style of critique. There is no question about the harsh reactions by some following publication, but it is curious that there is no evidence to suggest such criticism *before* publication.

Second, it is true that Taylor had difficulty finding a permanent position at an American university (he did hold positions in Mexico [Kennedy and Folan, this volume]) once *A Study of Archeology* appeared in print. He had visiting positions at the University of Washington in 1949 (as assistant professor) and again in 1953 (as professor) and in the International Seminars program of the Friends Service Committee during 1948–1953 (as lecturer). However, the situation was, in some measure, a matter of Walt's choice. He had the financial resources to be independent, and as Euler (1997: 23) notes, "Walt led a somewhat peripatetic but rewarding life." James B. Griffin, one of the six archaeologists singled out for Taylor's criticism, was known to remark on several occasions to the effect that Taylor had too much money and did not need a job. However, this was not always true, especially by the early 1970s.

The third and final point is that although Taylor had difficulty finding a position he wanted until he came to SIU-C in 1958 (perhaps out of necessity; see Reyman 1999: 688–690), he was not as marginalized within American archaeology as might appear to be the case. The post-1948 evidence for this point includes Taylor's publication record, his record of professional service, and his honors from scientific societies.

An example of the first is Taylor's invited contribution (Taylor 1954) to the special Southwest Issue of *American Anthropologist*, edited by Emil W. Haury, one of the six archaeologists whose work Taylor singled out for criticism in *A Study of Archeology* (1948: 68–71). Another is Taylor's invited contribution, again with Haury, to the coauthored paper for the Seminars in Archaeology, 1955 (Haury et al. 1956). Invited book reviews for both *American Antiquity* and *American Anthropologist* are further evidence.

As for professional service, at J. Charles Kelley's invitation more than a year before Taylor was invited to chair the Department of Anthropology at SIU-C, he participated in a major conference under the auspices of the National Academy of Sciences–National Research Council, aimed at establishing the National Clearinghouse for the Identification of Non-Artifactual Archaeological Materials. Taylor edited the volume produced by the conference (Taylor 1957b), to which Emil Haury was once more a contributor, and the speakers at the conference included not only Haury but also James B. Griffin, another of the six archaeologists whose work Taylor critiqued in *A Study of Archeology*. Although Haury and Griffin did not agree with Taylor's analysis and especially with his review of their work, such did not preclude them from working with Taylor on archaeological issues.

Taylor's professional service also included two stints as program chair for the Pecos Conference (1958 and 1961), founded by Kidder in 1927. Taylor served as director of the program in cultural ecology for the National Research Council (1959) and was program chair for the United States for the thirty-fifth Annual Meeting of the SAA in Mexico City (1970).

Taylor's post-1948 honors included, among others, a Guggenheim Fellowship (1950), election as a Fellow of the American Association for the Advancement of Science (1954), a Leo Kaplan Research Award from Sigma Xi (1973), and selection for *Who's Who in America* and *American Men of Science*. Although Taylor may have had difficulty finding a regular university position during the decade following the publication of *A Study of Archeology*, he was not so marginalized that he ceased to be a factor in American archaeology (cf. Watson, this volume).

Indeed, one can argue that not only did he continue to be a factor in American archaeology but also he was important in the larger discipline of American anthropology through his involvement in the founding and development at SIU-C in the late 1950s through the 1960s of one of the better new anthropology departments in the United States.

Chapter 4 in *A Study of Archeology* is titled "A Concept of Culture for Archeology." It is a subject that preoccupied Taylor not only for archaeology but also for anthropology in general. His concern with culture surely reflects the influence of both Ralph Linton (1936, 1955) and Clyde Kluckhohn and is evident in his teaching and writing. In the classes I took, Taylor devoted considerable time discussing his concept of culture, especially Culture in the holistic sense versus culture in the partitive sense (see Taylor 1948: 98–110), cultural versus culture, and the differences among empirical, cultural, and culture categories (see Taylor 1948: table 1). He wanted to ensure that his students understood what he (and almost every other anthropologist) considered the central concept of anthropology (Taylor 1948: 37). The drill was frequent and consistent using the Socratic Method (see Reyman, this volume), so much so that several of us wanted to say, "We get your point." This focus on the concept of culture was also reflected in his use of Linton's *The Study of Man* as a text (Clay and Schoenwetter, this volume) and his use of Linton's *The Tree of Culture*—completed by Linton's wife after his death in 1953 and published in 1955—as a supplemental reading.

The importance of the concept of culture is also apparent in Taylor's writing. Perusal of his full bibliography at the end of this chapter indicates he specifically discussed the issue in no fewer than six published papers and included discussion of it in several others. Taylor coedited *Culture and Life*, the volume dedicated to Kluckhohn, and his own essay (Taylor 1973a) focuses, in part, on Kluckhohn's view of culture and how it influenced Taylor. It was never clear to me, even in conversations with Taylor, exactly why he thought he was either incompletely understood or misunderstood about his concept of culture, but he believed that he was. Perhaps it was the emphasis by many New Archaeologists on behavior instead of what Taylor saw as the underlying mental template of culture (very much a Platonic archetype); perhaps it was their emphasis on nonnormative thought; perhaps they rejected his position that "[b]oth behavior and the results of behavior, if they stem from ideas, pertain to culture. They are not culture, but they are 'cultural'" (Taylor 1948: 102, see also 95). Material culture did not and could not exist for Taylor; culture was of the mind. Or finally, perhaps it was Taylor's insistence, at a time when almost all archaeologists talked and wrote of reconstructing the past, that cultural (not culture) history was a construction, *not* a reconstruction, and that reconstruction was not possible (Taylor 1948: 35–36, and passim). Whatever the reason or reasons, Taylor found it necessary to return to the concept of culture over the course of some twenty-five years following publication of the monograph. Had he continued to publish after his retirement in 1974 (see his bibliography, below), I have no doubt he would have continued to write on this topic.

## CARBONDALE AND BEYOND

Watson (this volume) does a masterful job of discussing the historical context for *A Study of Archeology* and a number of issues surrounding the lack of acceptance of Taylor's ideas. I previously covered some other issues affecting this (Reyman 1997; 1999), but a bit more discussion might be helpful.

Taylor had a difficult time, personally, after Lyda Taylor's death in May 1960, and this lapped over into his professional career. Colleagues at SIU-C told me that Taylor seemed to lose much of his professional motivation. (Charles H. Lange told me several times that Taylor had remarked to him that with Lyda's death, he [Taylor] had lost his "anchor" and "his compass.") This seems to be reflected in Taylor's meager publication record between 1960 and 1964, although two papers that he delivered at the IXth Mesa Redonda in Mexico City in 1962 and revised for publication—"La Posicion Cultural de la Comarca Lagunera en el Norte de Mexico" and "Las Excavaciones en la Cueva Tetavejo, Sonora"—were not published because the proceedings from the conference were never published (nor were the proceedings from the Xth Mesa Redonda). A search for the titles of the two papers does not show them in the UNAM library (Paul Schmidt, personal communication, October 1, 2007) nor are they listed in American library holdings or among Taylor's papers at the National Anthropological Archives. Taylor seems to have held out hope that they eventually would be published because they appear on his curriculum vitae as *in press* as late as 1983, the last curriculum vitae we have.

As noted previously (Reyman 1999) and also in another chapter in this volume and as can be seen in Taylor's complete bibliography below, Taylor's publication record is a modest one: three monographs (including two—1988 and 2003—of essentially the same report on the Coahuila work); three edited or coedited books; twelve chapters in edited books; ten journal articles; thirteen reviews; five commentaries; eight technical reports; and a handful of miscellaneous other papers. He might have published more, but his two subsequent marriages, disruptive in different ways, distracted Taylor from work, especially from completing the Coahuila report.

In 1962 he married Nancy Thompson Bergh (like Taylor, an OSS member in World War II), and although they built a magnificent house in Santa Fe, lived well, and traveled frequently, it was not a happy marriage. It lasted about eight years, and divorce proceedings began shortly after I arrived at Santa Fe in 1968 to study with Taylor. As discussed (Reyman 1999; see also this volume) Taylor assigned me research tasks and problems for study and would look at and discuss the results with me. But he was often absent from Santa Fe, and weeks could pass without significant interaction; it was very much a "learn on your own" experience, albeit in an almost perfect library setting. However, during my last weeks in his library, even when Taylor was there, he was preoccupied with designing his post-divorce home (he was an excellent architectural draftsman) on Camino Corrales. He also continued his hunting, fishing, squash playing, and other activities, which further reduced the time he spent preparing me for my Special Examination to be administered after I returned to Carbondale.

By the time I moved back to Santa Fe in 1971 as Taylor's postdoctoral researcher to help write the Coahuila report for publication, Taylor had married his third wife, Mary Henderson Swank, and was living in the Camino Corrales house. Again, he was frequently gone—hunting, fishing, traveling, playing squash several times a week, spending time at his cabin on the Pecos River east of Santa Fe—all of which took him away from the work at hand. It was frustrating. When I left Santa Fe in late August 1972 to take a teaching post at Illinois State University, my work was almost complete: some 1,200 pages of typescript on the various categories of fiber artifacts from the Coahuila caves, excluding sandals and sandal ties (Taylor worked on these) and basketry (to be written by James Adovasio). Taylor eventually published a report on the sandals and sandal ties and a short history of the Coahuila Project (Taylor 1988), which he immediately tried to suppress (Euler 1997). Taylor revised the acknowledgments in 1993 and cooperated with Nicholas J. Demerath, Mary C. Kennedy, and Patty Jo Watson in the editing of the manuscript. It was published posthumously (Taylor 2003).

Like his marriage to Nancy Thompson Bergh, Taylor's marriage to Mary Henderson was ultimately unhappy and ended in divorce. Yet Taylor's productivity in terms of publication increased somewhat between 1964 and 1968, during his second marriage, and also during the early years of his marriage to Mary—until his retirement from SIU-C in 1974. Then he effectively was through with archaeology. Nevertheless, his overall publication record from 1964 to 1973 is neither large nor impressive, although there are a few significant publications (e.g., Taylor 1964, 1966a, and 1973a).

This section complements Watson's argument (this volume) that Taylor essentially walked away from advocacy of his conjunctive approach, although, as she notes, he did manage to carry "out a conjunctive study of 958 sandals and 750 sandal ties." I agree with much of her analysis and reasoning regarding Taylor's motives and behavior. I suggest, however, that he did not just walk away but rather that he walked toward—he pursued—the "good life" he had always enjoyed.[2] But he did so, it seemed, with greater urgency as a consequence of his unhappiness with his second and third marriages,[3] and perhaps, because he realized that as archaeology became more statistically oriented, he had less to contribute. So he left Santa Fe and divided his time between Arizona and Alamos, Sonora, where he purchased a house and extensive property on the plaza, put in a large garden, and sold his produce at the market. Eventually, he and Mary divorced, and Taylor later moved with his "loving companion, Virginia Cotton" (Euler 1997) to Rockaway Beach, Oregon, where he died on April 14, 1997, of complications from Alzheimer's disease.

## NOTES

1. Material for this biographical sketch comes from several sources: first and foremost are my conversations with Taylor and my notes during the period from 1968 to 1986. Other sources include conversations with Taylor's SIU-C colleagues J. Charles Kelley, Ellen Abbott Kelley, Charles H. Lange, Elizabeth M. Lange, and Carroll L. Riley and SIU-C students R. Berle Clay, Thomas E. Holien, Joseph B. Mountjoy, Richard Pailes, and Phil C. Weigand, both while I was a graduate student at SIU-C and later. Taylor's field diaries and field notes, to which he gave me access during my two years working with him at his library in Santa Fe (1968–1969, 1971–1972), provided important information, as did his archived papers. Conversations with Taylor's friends and colleagues such as George Gumerman and the late Robert C. Euler, and with my coeditor William J. Folan also provided information and insights, and Euler's 1997 obituary of Taylor yielded useful details. I have tried not to repeat the biographical information contained within the introduction and several chapters of this volume, especially Brenda Kennedy's, in my earlier obituary (Reyman 1997) and biographical essay (Reyman 1999), and in the late Robert C. Euler's (1997) obituary of Taylor, but some overlap is inevitable. I used no anecdotes that could not be verified by at least two independent sources.

2. An example of Taylor's focus on the "good life" and on himself concerns the addition of an index to the 1967 Arcturus Books edition of *A Study of Archeology*. Taylor hired Elizabeth M. Lange to compile the index of names and subjects; she was the wife of Charles H. Lange, then-chair of the Department of Anthropology at SIU-C. He agreed to pay her $600 for the work, which she completed, but Taylor begged off from paying her. His excuse was that he had spent the money to purchase Gunner, his new black Labrador Retriever.

3. One reflection of this is that in the acknowledgments section of the first Coahuila Project report, Taylor (1988: xxi) had written: "I would like to mention particularly my former wife, Mary Henderson Taylor, for dedicated and expert help in the production of the original manuscript of this publication, especially for her performance of wearisome work with diligence and nicety, with enterprise, with grace and willingness. I thank her sincerely." By 1993, Taylor was divorced from Mary and living in Rockaway, Oregon. Mary was by then deceased, and in the acknowledgments of the later report (Taylor 2003: xii–xiv), dated September 30, 1993, Taylor makes no mention of her.

## WALTER W. TAYLOR: BIBLIOGRAPHY


1927 Lucky Thirteen. *Hunting and Fishing Magazine* 4(10).

1935 Quantitative Analysis in Connecticut Archaeology. *Connecticut Archaeological Society*, Bulletin 2:2–23.

1937 Report of an Archaeological Survey of Coahuila, Mexico. *New Mexico Anthropologist* 2(2):45–46.

1938 Preliminary Report on Sites in Coahuila, Mexico. Summary of paper read at April meeting of the American Association for the Advancement of Science Southwestern Division. *New Mexico Anthropologist* 2(4–5):84.

1941a The Ceremonial Bar and Associated Features of Maya Ornamental Art. *American Antiquity* 7(1):48–63.

1941b Review of *The Elements of Mazatec Witchcraft*, by Jean B. Johnson. *Journal of American Folklore* 54(211/212):111–112.

1943 Reviews of *A Reconstruction of Uto-Aztekan History* and *Report on Archaeology of Southern Chihuahua*, by Robert M. Zingg. *American Antiquity* 8(3):307–310.

1943 (with W. C. Boyd) Blood Groups of the Pre-historic Indians of Coahuila by Serological Tests of Their Mummified Remains. *Yearbook of the American Philosophical Society*, 178–180. American Philosophical Society, Philadelphia.

1946 Reviews of *Racial Prehistory in the Southwest and the Hawikuh Zunis*, by Carl Seltzer, and *The Excavations of Los Muertos and Neighboring Ruins of the Salt River Valley, Southern Arizona*, by Emil W. Haury. *New Mexico Quarterly* 16(3):378.

1947 *Summary Report of the Archaeology Reconnaissance of Coahuila Mexico*. Informe en el archivo técnico de la Dirección de Monumentos Prehispanicos, INAH. México, mecanoscrito (8 cuartillas y 27 fotografías).

1948 *A Study of Archeology*. American Anthropological Association, Memoir 69. American Anthropological Association, Menasha, WI.

1950 Rejoinder to Comments on Taylor's *A Study of Archeology*, by Robert F. Burgh. *American Anthropologist* 52(1):117–119.

1951 Review of *Prehistoric Southwesterners from Basketmaker to Pueblo*, by Charles A. Amsden. *American Antiquity* 16(3):273–274.

1953 Review of *Excavations in Big Hawk Valley, Wupatki National Monument Arizona*, by Watson Smith. *American Antiquity* 18(4):399.

1954a An Analysis of Some Salt Samples from the Southwest. *Plateau* 27(2):1–7.

1954b An Early Slab House Near Kayenta, Arizona, *Plateau* 26(4):109–123.

1954c Review of *Anthropology Today: An Encyclopedic Inventory*, by Alfred L. Kroeber, chairman, and *An Appraisal of Anthropology Today*, ed. Sol Tax et al. *American Anthropologist* 56(3):480–483.

1954d Southwestern Archaeology: Its History and Theory. *American Anthropologist* (special Southwest Issue, ed. Emil W. Haury) 56(4):561–575.

1955 Review of *Basket Maker II Sites near Durango, Colorado,* by Earl H. Morris and Robert F. Burgh. *American Journal of Archaeology* 59(3):262–264.

1956a (Emil W. Haury, Robert L. Rands, Albert C. Spaulding, Walter W. Taylor, Raymond H. Thompson, and Robert Wauchope). An Archaeological Approach to the Study of Cultural Stability. In *Seminars in Archaeology: 1955*, ed. Robert Wauchope, 31–57. Society for American Archaeology, Memoir 11. Society for American Archaeology, Salt Lake City.

1956b Review of *Prehistoric Stone Implements of Northeastern Arizona*, by Richard B. Woodbury. *American Anthropologist* 58(3):582–584.

1956c Some Implications of the Carbon-14 Dates from a Cave in Coahuila, Mexico. *Texas Archaeological Society*, Bulletin 27:215–234.

1957a A Clearing House or Central Agency. In *The Identification of Non-Artifactual Archaeological Materials*, ed. Walter W. Taylor, 61–62. Publication 565. National Academy of Sciences, —National Research Council, Washington, DC.

1957b (editor) *The Identification of Non-Artifactual Archaeological Materials*. Publication 565. National Academy of Sciences, National Research Council, Washington, DC.

1957c What the Archaeologist Needs from the Specialist. In *The Identification of Non-Artifactual Archaeological Materials*, ed. Walter W. Taylor, 11–13. Publication 565. National Academy of Sciences, National Research Council, Washington, DC.

1957d Editor's Foreword. In *The Identification of Non-Artifactual Archaeological Materials*, ed. Walter W. Taylor, 5. Publication 565. National Academy of Sciences, National Research Council, Washington, DC.

1957e Editor's Summary. In *The Identification of Non-Artifactual Archaeological Materials*, ed. Walter W. Taylor, 63–64. Publication 565. National Academy of Sciences, National Research Council, Washington, DC.

1957f Review of *Aspects of Culture*, by Harry Shapiro. *American Anthropologist* 59(5):900.

1957g Review of *Mogollon Culture Prior to A.D. 1000*, by Joe Ben Wheat. *American Antiquity* 22(2):205–206.

1958a Archaeological Survey of the Mexican Part of Diablo Reservoir. In *Appraisal of the Archaeological Resources of Diablo Reservoir, Val Verde County, Texas*, ed. John A. Graham and William A. Davis, 87. Archaeological Salvage Program Field Office, Austin, Texas.

1958b *Two Archaeological Studies in Northern Arizona, The Pueblo Ecology Study: Hail and Farewell*, and *A Brief Survey through the Grand Canyon of the Colorado River*. Museum of Northern Arizona, Bulletin 30. Museum of Northern Arizona, Flagstaff.

1960a Reply to MacNeish, *American Antiquity* 26(2):263–266.

1960b Review of *Preliminary Archaeological Investigations in the Sierra de Tamaulipas, Mexico*, by Richard S. MacNeish. *American Antiquity* 25(3):434–436.

1960c (and Francisco Gonzalez Rul). Archaeological Reconnaissance behind the Diablo Dam, Coahuila, Mexico. *Texas Archaeological Society*, Bulletin 31:153–165.

1961a Archaeology and Language in Western North America. *American Antiquity* 27(1):71–81.

1961b CA* Comment on Neolithic Diffusion Rates by Munro S. Edmonson. *Current Anthropology* 2(2):98.

1963a Leslie Spier, 1893–1961 [Obituary]. *American Antiquity* 28(3):379–381.

1963b The Role of Anthropology in Educational Planning, Appendix E. In Developing Institutional Resources to Assist with Educational Planning, by Robert Jacobs, G. C. Wiegand, and F. G. McComber. Southern Illinois University, Carbondale. Mimeo.

1964a *A Study of Archeology*. Reprint of 1948 publication. Southern Illinois University Press, Carbondale.

1964b Tethered Nomadism and Water Territoriality: An Hypothesis. *Actas y Memorias*, XXXV International Congress of Americanists, Mexico City, 1962, 2:197–203.

1965 Report of Work Conducted under Project E: Work in the Gran Chichimeca Paralleling the Mesoamerican Frontier. National Science Foundation Grant #18586, Studies of the North-central Frontier of Mesoamerica, Carbondale, IL. Mimeo.

1966a Archaic Cultures Adjacent to the Northeastern Frontiers of Mesoamerica. In *Handbook of Middle American Indians*, vol. 4, ed. Gordon F. Ekholm and Gordon R. Willey, 59–94. University of Texas Press, Austin.

1966b The Concept of Culture and the Analysis of Difference. In *Actas y Memorias*, XXXVI Congreso Internacional de Americanistas, 1964, vol. 3:89–94. Sevilla.

1966c (and Robert C. Euler). Additional Archaeological Data from Upper Grand Canyon: Nankoweap to Unkar Revisited. *Plateau* 39(1):26–45.

1967a The "Sharing Criterion" and the Concept of Culture. In *American Historical Anthropology*, ed. Carroll L. Riley and Walter W. Taylor, 221–230. Southern Illinois University Press, Carbondale.

1967b *A Study of Archeology*. Reprint of 1948 publication. Arcturus Books, Southern Illinois University Press, Carbondale.

1967c (Carroll L. Riley and Walter W. Taylor, editors). *American Historical Anthropology: Essays in Honor of Leslie Spier*. Southern Illinois University Press, Carbondale.

1968a A Burial Bundle from Coahuila, Mexico. In *Collected Papers in Honor of Lyndon Lane Hargrave*, ed. Albert H. Schroeder, 23–56. Papers of the Archaeological Society of New Mexico 1. Museum of New Mexico Press, Santa Fe.

1968b *A Study of Archeology*. Reprint of 1948 publication. Arcturus Books, Southern Illinois University Press, Carbondale.

1968c Foreword to the 1968 printing of *A Study of Archeology*, 1–2. Arcturus Books, Southern Illinois University Press, Carbondale.

1969 Review of *New Perspectives in Archeology*, by Sally R. Binford and Lewis R. Binford. *Science* 165:382–384.

1972a Emic Attributes and Normative Theory in Archaeology. *Actas y Memorias,* XL International Congress of Americanists, Rome, 67–69.

1972b The Hunter-Gatherer Nomads of Northern Mexico: A Comparison of the Archival and Archaeological Records. In *World Archaeology*, ed. Barry Cunliffe, 4(2):167–178. Routledge and Kegan Paul, London.

1972c Old Wine and New Skins: A Contemporary Parable. In *Contemporary Archaeology*, ed. Mark Leone, 28–33. Southern Illinois University Press, Carbondale.

1973a Clyde Kluckhohn and American Archaeology. In *Culture and Life: Essays in Memory of Clyde Kluckhohn*, ed. Walter W. Taylor, John L. Fischer, and Evon Z. Vogt, 14–29. Southern Illinois University Press, Carbondale.

1973b The Nature and Nurture of Archeology: A Prospect. In *Research and Theory in Current Archeology*, ed. Charles L. Redman, 281–285. John Wiley and Sons, New York.

1973c Storage and the Neolithic Revolution. In *Estudios Dedicados a Prof. Dr. Luis Pericot*. Publicaciones Eventuales 23: 193–197. Instituto de Arqueologia y Prehistoria, Universidad de Barcelona, Barcelona.

1973d (Walter W. Taylor, John L. Fischer, and Evon Z. Vogt, editors). *Culture and Life: Essays in Memory of Clyde Kluckhohn*. Southern Illinois University Press, Carbondale.

1973e (Walter W. Taylor, John L. Fischer, and Evon Z. Vogt). Foreword. In *Culture and Life: Essays in Memory of Clyde Kluckhohn,* ed. Walter W. Taylor, John L. Fischer, and Evon Z. Vogt, vii–ix. Southern Illinois University Press, Carbondale.

1974 Review of *Cerámica incisa en Mallorca*, by Catalina Cantarelles Camps. *American Anthropologist* 76(4):963–964.

1980 (with Robert C. Euler). Lyndon Lane Hargrave, 1896–1978. *American Antiquity* 45(3):477–482.

1983 *A Study of Archeology* (reprint of the 1948 edition, with a foreword by Patty Jo Watson). Center for Archaeological Investigations, Southern Illinois University Press, Carbondale.

1988 *Contributions to Coahuila Archaeology, with an Introduction to the Coahuila Project.* Center for Archaeological Investigations, Research Paper No. 52, Southern Illinois University Press, Carbondale.

2003 *Sandals from Coahuila Caves*, *with an Introduction to the Coahuila Project, Coahuila, Mexico: 1937–1941, 1947*, ed. Nicholas J. Demerath, Mary C. Kennedy, and Patty Jo Watson. Studies in Pre-Columbian Art and Archaeology, No. 35. Dumbarton Research Library and Collection, Washington, DC.

N.d.a La Posicion Cultural de la Comarca Lagunera en el Norte de Mexico. IXth Mesa Redonda, Sociedad Mexicana de Antropologia, Mexico City.

N.d.b Las Excavaciones en la Cueva Tetavejo, Sonora. IXth Mesa Redonda, Sociedad Mexicana de Antropologia, Mexico City. (Taylor read these two papers at the IXth Mesa Redonda, revised them for publication, but the proceedings from the conference were never published.)

CHAPTER THREE

# NO MAN IS AN ISLAND

## The Scholarship of Walter W. Taylor

*Brenda V. Kennedy*

> It can easily be shown that most theories are intimately related to the purely personal experiences and "personalities" of their devisors and also to the prevailing pattern of thought. . . . Such a view does help us to view theories relativistically rather than absolutistically.
>
> CLYDE KLUCKHOHN (1939B: 342)

## INTRODUCTION

When I began this essay as a graduate student project in 1984, I knew little about the history of "American archaeology"[1] and nothing about the life and work of Walter W. Taylor. Hence, the task of assessing the significance of Taylor's theoretical and methodological contributions to the discipline has not been an easy one. The final product of my research is essentially a biographical narrative. The data on Taylor's life are drawn largely from a reply he made to my request for a copy of his curriculum vitae. Noting the limitations of his curriculum vitae "as to the context(s), motivations and impingements that have influenced both life and work," he kindly provided a ten-page account of the more personal aspects of his life. For others who may make better use of it, I have attempted to include much of it in the following pages. I also include my assessment of Taylor's ideas and opinions as drawn from his writings. My personal judgments are clearly identified as such.

Most attempts to describe the history of American archaeology concentrate on select themes or trends that dominated, and hence defined, particular periods of archaeological research and thinking (e.g., Strong 1952; Belmont and Williams 1965; Willey 1968; Willey and Sabloff 1993). These schemes chart major shifts in emphases throughout the development of the discipline but suffer from the inadequacies that plague all classification systems—they are designed to find patterns in a maze of variability and are perforce simplifications.

To more fully comprehend the complex themes in American archaeology, consideration must be given to other methods of inquiry. One alternative is to focus on the accomplishments of individuals who have played significant roles in the development of the discipline, much as Gordon Willey (1988) has done. Such an approach sheds light on the source of an individual's theoretical and methodological contributions and leads to a fuller understanding of same. It also enables the interested researcher to determine how life experiences and the prevailing pattern of thought in American archaeology influenced an individual's work and can elucidate the dynamic and influential relationships between and among individuals who were active in research.

This chapter is such a study. It is devoted to the particular contributions of Walter W. Taylor to the development of American archaeology. I take a biographical approach and chart the development of Taylor and his ideas by exploring the people and events that played a vital role in his career and by assessing the impact of his work on the "New Archaeology." The information is arranged to reflect the major episodes in Taylor's life and the importance of his most influential work, *A Study of Archeology* (1948). The story opens with a section titled The Formative Years, dealing with Taylor's life and career before 1948. This is followed by a section titled A Study of Archeology, dealing specifically with this book, its precursors, and the reactions it engendered. A third section, The Lull after the Storm, focuses on Taylor's career after 1948. Thereafter follows a discussion of Taylor's influence on the New Archaeology and his general position in American archaeology.

## THE FORMATIVE YEARS

### *When I Grow Up*

Walter Willard Taylor Jr. was born in Chicago, Illinois, on October 17, 1913, the son of Walter Willard and Marjorie Wells Taylor. During the early years of his life, his family moved first to Geneva, Illinois; then east to Douglaston, Long Island; and finally to Greenwich, Connecticut, in 1920 or 1921. It was at this point in his life that Taylor acquired his avid interest in the outdoors, the importance of which is illustrated by his comment:

> During the first or second summer we were in the east, my grandparents came from Chicago and took me in their car, via Niagara Falls, back to Chicago, and later my grandfather and my uncle went with me on a canoeing-camping-fishing trip for about two weeks in the woods of Wisconsin. I have been a camper-fisherman-hunter ever since, a status that has strongly influenced my life and my choice of profession. (Taylor, personal communication, 1983)

Taylor's early academic accomplishments were modest, something he attributes (Taylor, personal communication, 1983) to his ongoing interest in extracurricular activities, including sports, drama, music, an assortment of clubs, editing *Literary Monthly*, hunting, fishing, and camping. His future career aspirations were inclined toward ornithology, but by his senior year at Hotchkiss his interests had shifted to anthropology, and he had his sights set on Harvard.

### *Yale University*

Taylor's ambition to attend Harvard was not immediately realized. His father, a bond broker and graduate of Yale, invoked the powers of paternal persuasion and suggested that Taylor attend his alma mater. Taylor bowed to the pressure and enrolled at Yale in fall 1931. Over the next four years his academic interests assumed a growing role in his life, despite his continued interest in other activities. He graduated in spring 1935 with an A.B. in geology and departmental honors.

In summer 1935, Taylor participated in his first archaeological excavations, working for the Museum of Northern Arizona in Flagstaff on a crew that included J. Lawrence Angel, Marshall T. Newman, and Richard Wheeler under the direction of John C. McGregor. He returned to Yale in the fall of 1935 to enter graduate school where he took courses from Raymond Kelley, George Peter Murdock, Cornelius Osgood, Edward Sapir, Leslie Spier, and Clark Wissler. His fellow students included W. W. Hill, Lyda Averill Paz (who married Taylor), and B. Irving Rouse. These names help us understand the community to which he was exposed during the formative years of his career.

On a number of occasions, Taylor expressed an appreciation of the influence of professional associations (1948, 1963, 1973a; personal communication, 1983) and said, "I always had my students . . . search out these associations and see what they can infer from them" (Taylor, personal communication, 1983). I select for comment some of those relationships that I feel had a noticeable impact on Taylor's career or thinking, briefly indicating the nature of the influence. For special mention, I single out Cornelius Osgood, Edward Sapir, and Leslie Spier, although the influence of others also will be cited below.

Of Cornelius Osgood, Taylor later writes, “I feel, although I cannot be explicit, that [he] is responsible for much of the manner in which I look upon archeology; the discussions, not to say arguments, in which we engaged during the years from 1931 to 1936 keep coming back in many forms and in many contexts” (1948: 10). Although Taylor is not specific, it seems reasonable to suggest that a large part of Osgood’s influence lay in his effort to examine archaeological data from a cultural point of view (see Osgood 1943; Kehoe, this volume).

Spier and Sapir were both students of Franz Boas. In an obituary published in *American Antiquity*, Taylor (1963: 379–381) describes Spier as “one of the most Boasian of Boas’s students” in that he consistently noted the value of a broad, integrated view of culture and the discipline of anthropology. This view clearly influenced Taylor’s personal anthropological perspective. Taylor describes Spier’s early approach to both archaeological and ethnographic research as strongly culture historical, once again following the lead of Boas. However, he acknowledges that Spier became disillusioned with the approach, expressing doubts as to “the rigor, the precision, and the breadth of the distribution studies which were the foundation of a large portion of the culture-historical inferences” (Taylor 1963: 379–380). These doubts were expressed in Taylor’s 1948 review of American archaeology. Sapir’s influence is more difficult to pinpoint, but Taylor’s comments lead one to believe it reflects a different aspect of Boasian thought—“an interest in culture itself, the study of the nature, the processes, and the development of culture” (1948: 39). This influence is most apparent in Taylor’s emphasis on archaeological research as a means to achieve a better understanding of culture.

Taylor’s first archaeological publication appeared the same year he entered graduate school at Yale. “Quantitative Analysis in Connecticut Archaeology” (Taylor 1935) reports on his analysis of the Connecticut collection at Yale’s Peabody Museum and provides an interesting contrast to his later writings on archaeological theory and method. Taylor divides Connecticut into three sections, using its three main river systems with slight modifications to allow these to conform to areas occupied by recent Indian groups. Utilizing a list of artifact types common to eastern North American sites, he compares their occurrences in these three areas. The results of his analysis indicate differences in the artifact distributions throughout the state.

Taylor’s approach reflects the prevailing culture-area concept of the 1920s and 1930s as a means of explaining cultural differences and similarities. Granted, he worked on a small scale, but the procedure is the same: cultures are related to geographically delineated aspects of the environment with comparisons made on the basis of trait lists. Clark Wissler was deeply involved in such studies and possibly influenced Taylor in this regard. Wissler (1917) had tried to overcome difficulties in the approach by proposing the concept of a “culture center” from which trait assemblages diffused outward. A major problem with culture-area studies, of course, is the absence of a temporal element in the comparisons and, true to the model, Taylor makes no attempt to consider the temporal contexts of the materials being compared. Other problems revolve around the use of trait lists to facilitate such comparisons. Taylor goes beyond recording simple presence/absence and attempts to quantify trait distributions from the three areas; his approach utilizes “typological tags” but does not include descriptions of artifacts that might indicate the range of variability or consider associations of artifacts. These are two inadequacies Taylor (1948) criticizes in studies by other American archaeologists.

It is also notable that the types found on Taylor’s trait list are classified under headings he later describes as “empirical” rather than “cultural” categories. The former include such rubrics as *Stone, Bone, Objects of Copper, Environment*; the latter include *Food, Dress, Hunting, Textile Industry, Utilization of Environment, Containers, Transportation* (Taylor 1948: 114; emphasis in original). In his monograph, Taylor (1948: 124) suggests that the use of empirical types in comparative studies may produce misleading results.

Taylor’s analysis of Connecticut archaeology seems to reflect an emphasis on “objective” (or what he was later to call “empirical”) methods of analysis incorporated within the culture-area framework. Analysis seems to be for its own sake since Taylor never attempts to explain what the “objective” or “empirical” differences mean or how they can be explained. The reader is left to ask, so what? There is no clue that Taylor is destined to become one of the most well-known twentieth-century American archaeologists.

### *Setting a Course for the Future*

In the summer of 1936, Taylor was awarded a Laboratory of Anthropology Fellowship to study field methods in archaeology. He traveled to the Macon Plateau, Georgia, where, under the supervision of Arthur R. Kelly, he worked with J. Lawrence Angel and Gordon R. Willey, among others. Later in 1936 he headed west looking for employment. He said that he traveled “first to the Gila Pueblo, Globe, Arizona, where there was no job but Emil Haury gave me a leg up. Then to Flagstaff and the Museum of Northern Arizona where also there was no job, but Lyndon Hargrave and I had a number of days to get acquainted and start a life-long friendship” (Taylor, personal communication, 1983). From Flagstaff, Taylor went to Albuquerque and graduate school at the University of New Mexico, where he wanted to study under Donald Brand and Florence Hawley. Leslie Spier had planted this idea in his mind by emphasizing “the virgin research of Northern Mexico.” Taylor spent one year in New Mexico taking courses from Brand and Hawley, as well as from Wesley Bliss. Among his fellow students were several with whom Taylor developed long-term professional relationships and friendships, most notably J. Charles Kelley.

In the summer of 1937, he and J. C. Kelley conducted a survey in the Big Bend region of Texas, looking for sites comparable to those found across the Rio Grande in Coahuila, Mexico. Taylor followed this with a survey in Coahuila, and thus became involved in a research project that spanned his entire career. His first report relating to the Coahuila Project appeared in *New Mexico Anthropologist* (1937) and was followed by a number of others (Taylor 1938, 1956, 1958a, 1964, 1968a, 1972b; Taylor and Boyd 1943; Taylor and Rul 1960).

On September 6, 1937, Walter Taylor married Lyda Averill Paz, a fellow graduate student at Yale. They immediately moved to Flagstaff, where Taylor accepted a one-year appointment at the Arizona State Teachers College, substituting for John McGregor as instructor in anthropology, geology, and zoology (the benefits of a broad education!). The year that followed was one of intense work, but it also had an important influence on Taylor's future, for it was during this time that he became involved in two very important relationships, one with Hargrave and the other with Clyde Kluckhohn.

I have already noted that Taylor met Hargrave in the fall of 1936. However, the two did not become well acquainted until Taylor moved to Flagstaff, where Hargrave was working at the Museum of Northern Arizona. No doubt, a large part of their friendship was based on their common, penetrating interest in the world around them: Hargrave published in ornithology and archaeology; Taylor early on had wanted to become an ornithologist but later decided on archaeology.

In 1980, Taylor and Robert C. Euler published Hargrave's obituary in *American Antiquity*. They write of Hargrave educating them in "the way of the wholes": "He studied events both large and small, the obvious and the least apparent, the chains and interrelationships that together constituted for him the natural and cultural context" (Taylor and Euler 1980: 477). They recognize their indebtedness to Hargrave for teaching them that "nothing supplants academic integrity, objective, down-to-earth thinking and reasoning, dedication to the goal, dynamic and innovative ideas, and scrupulous, disciplined honesty" (Taylor and Euler 1980: 480). The emphasis on objective critical evaluation and honest reporting of the "facts" as one sees them is clearly apparent in Taylor's 1948 analysis of the status of American archaeology, as well as in his numerous articles and reviews.

Taylor worked for Hargrave on an excavation in the area between Williams and Grand Canyon, Arizona. He comments: "It is unfortunate but true that that field season resulted in considerable disagreement between Hargrave and myself, particularly in the manner in which field records were kept and the program developed—or not developed. . . . I did learn a great deal from Hargrave in many aspects of archaeological theory (but *not* method)" (Taylor, personal communication, 1983).

It was also during Taylor's year at Flagstaff that he became better acquainted with Clyde Kluckhohn. The two had first met in 1935–1936 when Kluckhohn went to Yale to work with Edward Sapir, and Taylor and Kluckhohn had corresponded in 1937. When Taylor moved to Flagstaff, Kluckhohn was working on the Hopi reservation at Moencopi but came to Flagstaff on several occasions, and the two had the opportunity to talk. Then in August 1938, both men traveled to Chaco Canyon to work at the University of New Mexico Field School. Arthur R. Kelly was field supervisor, Taylor was foreman, and Kluckhohn, Ernst Antevs, Donald Brand, Florence Hawley, J. Charles Kelley, Stuart Northrop, Leslie Spier, and Leland Wyman were faculty members.

Taylor's relationship with Kluckhohn took shape during the field season. "It was at that time that he introduced me to the Tower Kiva in Chetro Ketl, where then and later we sat in the dirt, leaning against the ancient walls, and talked for hours and hours in an isolation and rapport all but impossible elsewhere" (Taylor 1973a: 24).

Over the next two summers that Taylor spent in Chaco, as well as on numerous other occasions, these opportunities were to present themselves again and again. Through these talks, Taylor says, there developed "the most influential anthropological/professional relationships that I was ever to have" (Taylor, personal communication, 1983). Its aspects are warmly described in Taylor's (1973a) article "Clyde Kluckhohn and American Archaeology" and may be summarized as follows. First, Kluckhohn (like Spier and Hargrave) stressed the importance of a broad integrated approach to anthropological research. He maintained that one should be an anthropologist first and then an archaeologist or ethnologist. Second, although Kluckhohn's archaeological publications are limited in number (there are only four), they are important contributions. Taylor (1973a: 27) says these include "the explicit examination and rigorous application of basic concepts and his definition of those concepts in terms, and in a context, applicable to archaeology. In each publication, his obvious aim was to urge American archaeology . . . to extend itself beyond mere time/space considerations and to write culture history to the fullest extent of the data." (It is just these issues that Taylor takes up in his *A Study of Archeology*.) Third, Kluckhohn was not impressed by established authority and was not averse to heaping criticism where criticism was due. He recognized, however, that the role of the critic was not an enviable one. To Kluckhohn's dismay, I am sure, Taylor was influenced by this approach and followed in these footsteps (see Reyman 1999: 682–683; Reyman, Chapter 11, and Maca, Chapter 1, this volume).

### *Harvard University*

It was Clyde Kluckhohn's urging that brought Taylor to Harvard in the fall of 1938, realizing Taylor's childhood ambition. However, Harvard's academic program proved to be somewhat disappointing.

> In comparison to what I considered the mature, stimulating and productive manner in which courses were conducted at Yale, . . . the courses at Harvard seemed to be frustratingly "undergraduate," pitched to a level built on the passing on of "facts" *ex cathedra* with the expectation that they be memorized and regurgitated by the students when called upon, in classroom or examination hall [a teaching style that Taylor emulated at Southern Illinois University, as discussed in the chapters by Folan and Schoenwetter in this volume]. With some exceptions, the Yale courses were true seminars based on student discussions, argument, elaborations and modifications based on library research, based on faculty suggestions rather than "assigned readings" and giving the student opportunity to develop his own research and points of view. (Taylor, personal communication, 1983)

Nevertheless, Taylor acknowledges several compensations for the classroom flaws at Harvard, including the Peabody Museum library and the broad range of professional help. It was up to the student to avail himself of these opportunities. (The latter is something that Taylor insisted on at SIU.)

During his two years at Harvard, Taylor took for credit the equivalent of four full courses, receiving A's in all but one half course in which he received a B+ "under rather strange circumstances." He took courses from Earnest Hooton, Alfred Tozzer, and Lauriston Ward and audited others, including two from Kluckhohn, who would not allow him to take his courses for credit.

During his two years at Harvard, Taylor continued to spend his summers working at the New Mexico Field School. During the first of these (1939), his immediate supervisor was Frank Setzler, and during the second it was Frank Roberts. The faculty was generally the same as in 1938 with the addition of W. W. Hill and Paul Reiter. Following each session, he went to Coahuila to do further survey and to prepare for intensive excavations.

The Coahuila excavations began in the winter of 1940 and lasted for ten months. His crew included Albert Schroeder, a Mexican cook and two or three laborers, and his wife, Lyda. Lyda stayed mostly in Cuatro Cienegas, where she cleaned and cataloged materials recovered during the excavations. She also identified botanical specimens. Taylor writes: "We set up week-end quarters and a lab and storage facilities in a private house in Cuatro Cienegas. We excavated four sites, two completely, one about half, and the fourth tested and found wanting so abandoned" (Taylor, personal communication, 1983).

Upon returning to Cambridge, Taylor worked on his Coahuila material using lab space provided at the Peabody Museum. At about the same time, he became a "collaborator in anthropology" with the U.S. National Museum, Smithsonian Institution, and a director of the Northern Mexico Archaeological Fund of the Smithsonian (a position he held from 1940 to 1949 and from 1957 to the late 1980s).

A Hemenway Fellowship (1941) enabled Taylor to spend much more time working on his dissertation. His originally accepted dissertation topic was to be a report on his Coahuila research. However, “the pressures of wartime” made this seem impossible; there was too much material to analyze to allow completion of the work before enlisting in the Marines. For this reason, Taylor decided to write what Kluckhohn referred to as “an armchair thesis,” but here he met with some resistance.

> I wanted to use the Mexican data and the ideas which I had developed while digging and studying them to make an analysis of archaeological theory and to demonstrate a new approach to archaeology. But [Kluckhohn] refused to approve the change—and for several reasons, one of which was that my dissertation would be theoretically and critically oriented as his had been, and he feared that a burdensome image might become attached to me as it had to him. (Taylor 1973a: 26)

Ah, the wisdom of experience! Nevertheless, at the end of the spring semester of 1942, Taylor presented a series of lectures to Kluckhohn’s advanced class in archaeological theory, using this opportunity to advance his own ideas of the past and future course of American archaeology. These lectures convinced Kluckhohn of the significance of Taylor’s ideas, and he agreed to the change.

Meanwhile, Taylor’s career was progressing. In 1941 he published his first article in *American Antiquity*, “The Ceremonial Bar and Associated Features of Maya Ornamental Art” (1941a; and see Joyce, this volume). This essay clearly demonstrates Kluckhohn’s influence, especially his emphasis on archaeological anthropology. In essence, the paper examines the origin and relationships of three artistic motifs found in Maya art: the ceremonial bar, the bar pendant, and the frieze-mask.[2] Challenging Spinden’s (1913) assertion that the three were separate elements, Taylor proposes they form a single complex with the bar and pendant as conceptual equivalents and closely linked to the frieze-mask. He examines the origin and development of these forms, their typological similarities, associations, and chronological relationships and goes beyond this to ask, “Do they have meaning for the interpretation of Maya concepts and ideas or only for the understanding of artistic elaboration?” (Taylor 1941a: 52). It is this last step that is truly significant for two reasons. First, the question leads one to explore the ideas—the culture and resultant cultural patterning that produce the ceremonial bar (and other decorated objects) and are reflected in them. And second, this question foreshadows Taylor’s developing, deep interest in mental constructs (mental templates), a normative view of culture, and the cultural products that are the outcomes, all of which are essential to the conjunctive approach and the overall theoretical structure in *A Study of Archeology*. Taylor also published his first critical review (of a book on Mazatec witchcraft) in 1941 (Taylor 1941b), the first of a series of reviews written by Taylor during his lifetime.

In the fall of 1942, while completing his doctoral dissertation, Taylor accepted a temporary teaching position at the University of Texas in Austin. There he taught

Introductory Anthropology, Southwestern Ethnology, and Physical Anthropology, reaffirming his broad abilities in anthropology. In December of the same year he enlisted in the United States Marine Corps and was called up for duty shortly thereafter, but not before successfully defending his thesis in January 1943. The thesis was titled "The Study of Archaeology: A Dialectic, Practical and Critical Discussion with Special Reference to American Archaeology and the Conjunctive Approach."

### *World War II*

Taylor served in World War II with the Office of Strategic Services in Algeria, Corsica, Italy, and France. He initially tried to avoid thinking about anthropology but changed his mind about this when he was taken as a prisoner of war in 1944. He spent time in two German camps, Stalag VII-A and Marlag Nord, where his thoughts turned increasingly to his dissertation and the revisions that would be required before it could be published. Taylor also attempted to make the best of a bad situation by offering courses in anthropology and geology in the POW schools (see Dark, this volume). This he did

> to give me something to do and to relieve the boredom of my fellow Kriegsgefangener, American (few), British (very many, one of whom [Philip J.C. Dark] took his first anthropology, in prison, from me and went on to the Chairmanship of Anthropology at Southern Illinois University), French (few), Greek (few), and a few other "odd bods" as my British colleagues say. (Taylor, personal communication, 1983)

In 1945, Taylor and three other prisoners escaped from the camp and he was sent home early. For his service he was awarded the Bronze Star with a citation and the Purple Heart.

After spending several months in a naval hospital being treated for hepatitis, Taylor returned to Cambridge and Harvard to edit and expand his thesis for publication. His wife and older son (born while Taylor was a POW) went on to Santa Fe, where Taylor joined them later in 1945.[3] A Rockefeller Fellowship in the Humanities financed the work on his dissertation through 1946 and on into 1947.[4] The revised thesis was published in 1948 as Memoir 69 of the American Anthropological Association under the title *A Study of Archeology*. It was to ensure Taylor a definite but controversial place in the history of American archaeology.

## *A STUDY OF ARCHEOLOGY*

### *Content*

When Taylor wrote *A Study of Archeology* (1948), he was reacting to what he and a few others, notably Kluckhohn (1939b, 1940), perceived to be a number of problems in American archaeology as of the early 1940s. He approached this issue in a well-organized fashion, dividing the study into two major parts. The first presents Taylor's personal views on the history and status of what he refers to as "Americanist archeology," outlining the specific nature, and often the source, of the problems in archaeological research and thinking that he feels deserve attention. The second proposes the adoption of a new approach to archaeological research that he feels will resolve these problems, the conjunctive approach.

In Part I of *A Study of Archeology*, Taylor examines issues in American archaeology as of the late 1930s and early 1940s, beginning with the dichotomy between archaeology as history or anthropology (Taylor 1948: 34–39), and equates written history with historiography, an issue that was debated until relatively recently and remains current (e.g., Deetz 1988).[5]

Taylor suggests that archaeological research may have as its final end product historiography or cultural anthropology (although he definitely favors the latter) and states, in one of the most controversial passages in the book, that "archeology *per se* is no more than a method and set of specialized techniques for the gathering of cultural information. The archeologist, as archeologist, is really nothing but a technician" (1948: 43). The archaeologist, as archaeologist, is concerned with the production of data; his approach to the analysis of the data produced and the goals he envisions will determine the disciplinary affiliation of his work (Taylor 1948: 44).

It is on the basis of this discussion that Taylor presents his assessment of the status of "Americanist archeology" in the United States and selects for critical examination the works of a number of prominent American archaeologists: A. V. Kidder, F.H.H. Roberts Jr., E. W. Haury, W. S. Webb, W. A. Ritchie, and J. B. Griffin. His evaluation of their work is unsparing and highly critical (see also chapters by Maca, this volume).

In Part II, Taylor proposes a new approach to archaeological research. His "conjunctive approach" has as its aim the most complete possible description of the cultures of human groups and is primarily interested in the interrelationships that exist within a culture—for example, between the group and its environment and among the cultural institutions, social structure, and social organization including kinship, religion, political organization, and economics—and how these are manifested in and understood from the archaeological record. To this end, one should ask questions of the data such as, What goes with what? It is significant where objects and cultural debris are found in a site, for example, fiber "quids" in association with fire-cracked rock areas in Frightful Cave (Taylor 1966a: 73, 81), and it might be significant where they are not found. This approach is designed to encourage the construction of more complete "cultural contexts," which Taylor considers the minimum that archaeologists should strive for, to provide materials for the study of culture itself.[6]

The conjunctive approach, outlined in the final chapter of *A Study of Archeology*, is a means to this end. Its primary goal is

> the 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. (1948: 95–96)

Taylor notes that important and distinctive features of the conjunctive approach lie in "the mental attitude and broad objectives with which the archaeologist attacks his research" (1948: 152). The conjunctive approach is not a set of procedures but a way of looking at archaeological problems; he argues that exact knowledge or absolute truth is unreachable (see discussions of Taylor's distinction between "construction" and "re-construction" in Maca's chapters, this volume).

There are various other suggestions scattered through the final chapter of Taylor's book that are not directly related to the conjunctive approach but are worthy of mention. For example, he encourages that sites be excavated according to depositional units (i.e., natural layers) and not arbitrary units (of standard measure); that less extensive/more intensive work for shorter periods of time would save money and leave more time for the kind of detailed analysis required by the conjunctive approach; that a system of archaeological apprenticeship be established that would give students valuable field experience and college credits (while again saving money on the project); that archaeologists make more use of specialists in other disciplines and that a clearinghouse or central agency be established to coordinate such efforts.

Taylor recognized that many of the ideas he proposed in *A Study of Archeology* were not new. Indeed, in the decade prior to Taylor's dissertation defense the trend of archaeological research and thinking was heading in some of the directions he proposed (e.g., Strong 1936; Steward and Setzler 1938; Kluckhohn 1939b, 1940 [Taylor adopted Kluckhohn's concepts of theory, method, and technique in *A Study of Archeology*]; and Bennett 1943).

Because many of Taylor's ideas were adopted from others, often with modifications, some argue that Taylor's contribution is overrated and hardly revolutionary. For example, Woodbury (1954: 295) suggests the conjunctive approach is "merely a reflection and elaboration of a general trend in mid-twentieth-century archaeology, namely, a dissatisfaction with the mere accumulation of data, and the desire to use data ultimately for meaningful syntheses and interpretations." Woodbury, however, failed to see that *A Study of Archeology* was the first comprehensive statement of this important trend in American archaeology and had a far greater impact than any of the earlier statements.

### *Reactions*

The immediate response to *A Study of Archeology* was not favorable. The remarks made in print tend to manifest a certain ambivalence. There were some positive comments. James Watson (1949: 55) refers to it as "a thorough statement of some basic questions in modern archaeology." Robert Burgh (1950: 114) states that "it is the first comprehensive and systematic attempt that has been made to formulate a discipline for the practice of archaeology in North America." And Glyn Daniel (1951: 83) describes it as "a very important book which should be read carefully, and pondered over." However, there were many more negative comments than positive ones.

Many believe that Taylor's comments on well-known American archaeologists such as Kidder, Roberts, and Ritchie are in the nature of personal attacks and have a "strangely patronizing air" (Woodbury 1954: 293). Supporters of this view, however, clearly did not read or chose to ignore or disbelieve the remarks with which Taylor (1948: 45) opened his discussion of American archaeology. Here he clearly states: "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 [and] . . . the extent to which the final results of empirical research measure up, or do not measure up, to the aims stated or implied by the various researchers themselves." These critics also ignore the many positive statements that Taylor scatters throughout his discussion. Taylor did not set out to criticize men per se but rather the status of American archaeology in terms of its theory and practice; and in this respect his criticisms were valid. I suspect there was no way he could have voiced these criticisms in a manner that the establishment would have found acceptable. Unfortunately, it is these comments that many people choose to remember while forgetting the more valuable arguments Taylor made. *A Study of Archeology* clearly established Taylor's reputation as a critic (just as Kluckhohn had warned).

Another criticism was that Taylor suggests nothing more than cultural historic reconstruction. James Ford (1952: 314) writes in reference to Taylor's work, "If a clear and complete reconstruction of all possible details of man's unrecorded history in all parts of the world is the primary goal of modern archaeology, then we have merely refined the ancient curio and fact-collecting activities of our predecessors and still can only beg that our studies be tolerated for esthetic purposes." It is obvious from this comment that Ford misses one of Taylor's major points: that although archaeology may stop at the level of historiography (as Ford describes), it should strive to attain the level of cultural anthropology and consider the nature and workings of culture. Moreover, Taylor recognized archaeological interpretations as constructions; he did not seek or propose to "reconstruct." This is another major point that Ford misses.

Yet an additional criticism was that Taylor sets out his conjunctive approach in opposition to the comparative or taxonomic approach but makes much use of comparative studies. This criticism by Burgh (1950) also indicates a less-than-careful reading of the text. Taylor clearly states that the use of comparative studies within the framework of the conjunctive approach reflects the difference between a means and an end. He suggests that the taxonomic approach uses cross-cultural comparisons to achieve its goal, "pigeonholing," whereas the conjunctive approach uses cross-cultural comparisons as a means to interpret better the culture of a particular group of people who occupied a specific site.

Taylor also has been criticized for saying in several places that an archaeologist is merely a technician but in others noting that the archaeologist does historiography and cultural anthropology (e.g., Burgh 1950; Walker 1978). Iain Walker (1978: 209) has written, "An archaeologist is no more merely a man who digs than is a historian merely a man who reads medieval manuscripts; an archaeologist is a man who interprets what he finds as a result of his excavation, and a historian is a man who interprets events in the light of his documentary research."

Walker as well as Burgh misunderstand the subtleties of Taylor's statements. In his rejoinder to Burgh, Taylor (1950) explains his point of view. Archaeologists, he says, are involved in many diverse projects in their work, with the technical angle constituting the least common denominator. For this reason, he argues, the archaeologist, as just an archaeologist, is a technician, but depending on his interpretation and synthesis of the data he assumes the role of anthropologist or historian. This view is echoed by Spaulding (1968: 38) in the following comment:

> I conclude that prehistoric archeology is indeed historical in the sense of having a primary interest in objects of that past, but that this historicism is a beneficent state that does not imply that archeology should be cast out from either science or anthropology. Archeology is scientific to the degree that it is anthropological, and it is anthropological to the degree that anthropology can provide cogent premises for inferences about archeological data. In fact, archeology as such is simply a technique (essentially digging holes in the ground or stooping over to pick up objects) which can be employed in the service of anthropology, history, or amusement.

Taylor certainly does not mean to denigrate archaeology in any sense by his comments but only to stress its interrelationship with other disciplines. I find his viewpoint acceptable but have trouble comprehending what an archaeologist is in Taylor's view when he or she is defining a problem and planning excavation.

Yet another criticism regards Taylor's suggestion that the archaeologist should attempt to collect and record all the data available from a site; this is seen as unreasonable and even physically impossible. Both Willey (1953a) and Walker (1978) argue it is impossible to collect data, except in relation to a particular problem. In response to this, I note there are many cases in which archaeological data are collected with no purpose or problem in mind, except excavation itself. Taylor's view merely reflects a concern with the very limited scope of archaeological questions in the first part of this century. Because there are limits on the foresight anyone can have in discerning what information will be of value to future workers, the archaeologist has an obligation to collect the maximum amount of data possible since the record is destroyed during excavation. Moreover, the state of the art in terms of techniques and technologies will always limit or dictate what can and will be collected (e.g., $C^{14}$ dating, flotation, remote sensing).

Taylor also has been criticized extensively for not giving us a practical example embodying his ideas (e.g., Martin 1954; Woodbury 1954); and some have argued that Taylor had no real amount of archaeological experience (e.g., Walker 1978). To the extent that these criticisms are valid, and it is debatable that they are, neither of these apparent weaknesses has effectively detracted from the acceptance of Taylor's book as a classic in American archaeological literature. Taylor himself was aware of his own scholarly shortcomings and openly referred to his failure to produce the final Coahuila report as the "albatross" around his neck. Moreover, as noted earlier, Taylor did have a variety of archaeological field experience. The problem was simply that he never managed to publish a monograph-length example of the conjunctive approach.

The conjunctive approach was designed to be a way of thinking, not simply to embody a particular set of methods or procedures. Any site report that Taylor produced would have been seen as *the* definitive way to apply this approach, and this would have led to a series of misconceptions or criticisms. Nevertheless, Taylor should have published, if only to still the carping about his failure to produce the Coahuila report, something that dogged him his entire life.

One other important criticism is that Taylor's approach involves too much time and money to make it practicable (Rouse 1953). I sympathize with this sentiment to an extent and think that Taylor recognizes the problem when he suggests several means of saving time and money in the field. However, I also think that many fail to realize that Taylor's approach could be applied on a limited scale to a particular problem. He gives several examples in his text of the application of the conjunctive approach, none of which involves beginning with the final definitive statement on the cultural contexts present.

I do not think that many of the criticisms of Taylor's book were justified or easily validated. No work is perfect. In *A Study of Archeology*, certain sections have been judged as weaker than others; Chapter 5, "The Nature of Archeological Data: Typology and Classification," is one example. There are also certain practical constraints that limit application of the conjunctive approach. Nevertheless, as a critical review of the state of "Americanist archeology" in the first half of the twentieth century and as a statement of a major theoretical reorientation, it remains a benchmark.

In the introduction to *A Study of Archeology*, Taylor (1948: 7) writes: "[I]n the long run, it will make very little difference whether the ideas to be put forward here turn out to be rallying posts or targets. If they become either, the study will have served its purpose. The really important thing is to focus the attention of the archeologists upon the nature of their objectives, their practices, and their conceptual tools." Judging by the reactions elicited, this work achieved new territory and synthesis and therefore achieved its purpose. *A Study of Archeology* was a success.

## THE LULL AFTER THE STORM

### *Spades and Flowers in Santa Fe*

The publication of *A Study of Archeology* had an immediate impact on Taylor's career as he could not secure a regular academic position. He therefore continued to live in Santa Fe where he did "precious little anthropology." "Mostly I hunted and fished around Santa Fe and enjoyed my friends, the land, and the scenery there. I began to dig in the ground once more—but this time for the growing of mostly vegetables, but some flowers; I also built several small greenhouses and started to grow orchids, first for ourselves and our friends and then commercially" (Taylor, personal communication, 1983).

Work on the Coahuila Project continued with a field season in 1950 and the completion of the lab work. Taylor accepted temporary teaching positions with the Quaker International Seminars in various places throughout the West (1948–1953) and at the University of Washington (1949, 1953). In 1949 he was made director of the Southwest Archaeological Fund of the Smithsonian, the money for which came from a wealthy friend of Taylor's and also from Taylor himself. He retained this position until 1957. Also during this time, he spent four field seasons working in Arizona (1949, 1951–1953).

Between 1948 and 1954, the year the Taylor family left Santa Fe, Taylor published three articles, four reviews, and one rejoinder. The articles reflect his interest in Southwestern archaeology and general anthropology, most notably "Southwestern Archaeology: Its History and Theory" (Taylor 1954). Here we see Taylor the synthesizer at work once more as he traces the principal stages in the evolution of Southwestern archaeology and compares them with contemporary developments in other parts of the United States.

Irving Rouse (1954) takes issue with several points in Taylor's synthesis, questioning the suggested disappearance of the one-culture concept at the end of the Cushing-Fewkes phase, proposing that the concept of cultural tradition may have been more important than culture area in the development of Southwest archaeology, and expressing doubt about the influence of easterners on archaeology in this area, saying the patterns of research may simply reflect the suitability of sites for spatial and temporal comparisons. Despite these disagreements, Taylor's paper remains widely read and cited.

### *Mexico Forever*

In 1954, Taylor was invited to teach at the Escuela Nacional de Antropología e Historia and moved his family to Mexico City with the intention of establishing permanent residence there. He spent one year as a visiting professor of anthropology at the Escuela Nacional and then moved to Mexico City College in 1955 (see Folan, this volume). In 1956 he directed excavations at Cueva Tetavejo, located about halfway between Hermosillo and Guaymas in Sonora province, and conducted a survey along the Sonora-Arizona border. For the time being, he shelved the possibility of writing the Coahuila report, overwhelmed by the volume of data and the realization that it would take a very long time to produce the type of report he was interested in (and perhaps was expected to produce).

Work on his Tetavejo material continued through to the spring of 1958, at which point Lyda Taylor was diagnosed with terminal cancer and wished to return to the United States before she died. Coincidentally, in 1957, Taylor had been offered the position of first chair of the Department of Anthropology at Southern Illinois University at Carbondale. He had turned down the offer, but now, through J. Charles Kelley, was extended a second invitation. Taylor accepted the post and the family moved to Illinois (see Reyman 1999: 688–689 and this volume).

During Taylor's four years in Mexico, he continued to publish, but most pieces cannot be classified as significant contributions except for the edited volume *The Identification of Non-Artifactual Archaeological Materials* (Taylor 1957b). This is a report on a conference held in Chicago (March 11–13, 1956) by the Committee on Archaeological Identification of the National Academy of Sciences. Taylor contributed two papers to the proceedings, both of which developed ideas first hinted at in *A Study of Archeology*. The first is titled "What the Archaeologist Needs from the Specialist" (1957c). In this article, Taylor (1957c: 11) states what he considers to be archaeology's *minimum* tasks.

> The first is to produce from their resting places both natural and cultural data to construct contexts as nearly as possible as they existed and as they were interrelated in the past: to define the human ecology. The second is to elucidate the temporal and cultural relationships of his material: the first of these yields comparative chronology or chronicle, the second establishes the cultural relations of his material with other cultural materials and includes cultural taxonomy. The third task is to provide some sort of absolute dating

> so that, among other things, studies of cultural change may be facilitated and comparisons made across areas over which no direct, one-to-one cultural comparisons are possible.

Given these minimum tasks, but recognizing the complexity of archaeological data required to fulfill them, Taylor says we must rely on specialists for certain information. He stresses the axiom that the accuracy of archaeological inference relies upon "the quality and quantity of [the archaeologist's] empirical data. . . . The more [quality] information obtained and utilized by the archaeologist in the construction of his cultural and natural contexts, the closer the approximation to past actuality he may be expected to attain" (Taylor 1957c: 12). However, one must remember that the nonspecialist may not recognize the significance of the material to be studied and the questions to be answered, and hence archaeologists must focus on communication, encouraging mutual education among diverse specialists.

Taylor's second paper in this volume, "A Clearing House or Central Agency" (1957a), relates to the first and echoes some of what he encouraged in *A Study of Archeology* (1948: 201). The proposed clearinghouse would coordinate inter- and intra-disciplinary services that are essential to "anthropologists of whatever stripe" but for which they lack the resources to undertake. It is described as a kind of "middleman between anthropologist and specialist . . . providing both communication and financial assistance" (1957a: 61). The National Science Foundation approached Taylor a short time later to submit a formal proposal for such an agency. Taylor did, but the referees rejected it for reasons that were never made totally clear (Taylor 1973b). Financial constraints may have been involved.

### *Southern Illinois University*

Taylor assumed his duties at Southern Illinois University in September 1958, faced with the task of building the department from scratch. Over the next five years, the faculty grew from three to sixteen, and a graduate program was developed with a doctoral program that Clyde Kluckhohn described as one of the top ten in the country (Taylor, personal communication, 1983). Lyda died in May 1960; on November 24, 1962, Taylor married Nancy Thompson Bergh.

Taylor resigned his position as chair in 1963 in accordance with department policy that the chair be rotated. Taylor did not have to resign, however; he chose to do so because he felt that he had accomplished what he could, because he was tired and the department was in a degree of turmoil, and because he wanted more time for other things. Granted a research position that required only two quarters of residence, he began to work again on the Coahuila material with the help of a series of graduate students. However, within a short period of time, he developed an interest in a new project, the "Bell Beakers" of the transitional

Neolithic/Bronze Age (see Clay, this volume). He explains the origin of this project as follows:

> The Beaker Project grew out of a seminar I held at SIU on the Neolithic cultures of Europe, a long-time interest of mine. It became apparent to us, during the course of the seminar, that there could be grave doubt that the famous Bell Beakers originated in Spain or even the Iberian Peninsula. It seemed . . . that here was a prime (glaring) case of what Kluckhohn called "received systems" and/or "crystallized sacrosanct dogmas." Bosch Gimpera had said it (a long time ago) and therefore it must be true and all subsequent data fitted into that system or pattern. It did not seem so to us. (Taylor, personal communication, 1983)

Taylor set out to test this hypothesis and so conducted fieldwork in Spain in 1963 with museum searches in France, England, and Scotland. Taylor describes the work as "fascinating" and a great diversion from the Coahuila material, which was becoming an ever-greater weight on his shoulders. He felt guilty for not having finished this work sooner, but the sheer volume of data with its "intricacy, complexity, and detail" induced him to put it off. He writes: "Procrastination was a bugbear and I could not shake it; the material was very exciting but I was deathly sick of it after all those years. However, I kept plodding along in it. . . . It was mental, and often actually physical, hell!" (Taylor, personal communication, 1983). Over the years, Taylor continually found diversions to take him away from working on the Coahuila report. For the two years that Reyman worked with him, the second on the report itself, Taylor played squash, hunted, fished, and went on vacation, among other things, to avoid sustained work on the report (Reyman, personal communication, 2008).

In 1964, Taylor, J. Charles Kelley, and Pedro Armillas received a National Science Foundation grant to work in the northern frontiers of Mexico. Taylor was allotted the northeast quadrant and excavated in Zacatecas with his younger son and two graduate students. There followed more fieldwork in Spain in 1967. Unfortunately, the work on the Beaker Project was to end here as Taylor's second marriage began to deteriorate at about this time, and his attention was diverted, never to be refocused.

In 1970, Taylor was divorced from Nancy and married Mary Henderson Swank. He was granted research leave for one year to work on the Coahuila material and then returned to his teaching duties until his retirement at the end of June 1974. Upon retirement, he was granted the status Professor Emeritus of Anthropology.

In the sixteen years Taylor was at SIU, he produced seventeen articles or reports and four reviews and edited two volumes. Several articles deal with Coahuila and will be considered later; others show his continuing interest in the general concerns of anthropology. In "Archaeology and Language in Western North America" (Taylor 1961), he attempts to explain the distribution of languages in

Desert Culture groups in terms of historic and cultural factors. "The Concept of Culture and the Analysis of Difference" (Taylor 1966b) develops an idea first presented in *A Study of Archeology*, and it was published in revised form as "The Sharing Criterion and the Concept of Culture" (Taylor 1967a). Here Taylor examines the distinction between phenomena categorized as "cultural" (shared by several persons) and those categorized as "idiosyncratic" (pertinent to one person alone). He maintains that these two categories are not mutually exclusive and that idiosyncratic behavior is, in fact, cultural behavior. Taylor describes the phenomena of the "real world" in terms of Kroeber's (1936) frames of reference: cultural, social, psychological, biological, and chemico-physical. On a second axis he considers the various levels of abstraction used to order these data: first the observational level, that is, individual impressions and abstractions; second the referential level where order is imposed on the primary abstractions by referring them to previously made abstractions on the basis of perceived "likeness"; and third the explanatory level consisting of explanatory or causal abstractions.

Within each level of the abstractions, phenomena may be categorized as idiosyncratic or normative. At the first level, all impressions are idiosyncratic, whereas on the second and third one finds the introduction of norms to indicate the sharing criterion. The discussion clearly indicates that the issue of being normative or idiosyncratic is independent of the frame of reference, including the cultural frame of reference. Taylor (1967a: 229) comments:

> [T]o insist upon the sharing criterion for any frame of reference, including the cultural, is to deny the significance, even the existence, of variation within that frame. Anthropologists do not deny the significance of variation for their studies within a cultural frame of reference, and thus, to be logically sound, they cannot insist upon the sharing criterion in their definitions of culture or the cultural frame of reference.

Taylor (1972a) made a further contribution to archaeological theory when he published a very short article, "Emic Attributes and Normative Theory in Archaeology." Once again, he chooses to build on an idea first mentioned in *A Study of Archeology*, in this case the distinction between empirical and cultural attributes. He suggests that these concepts may be subsumed under the concepts of *etic* and *emic*, respectively, as discussed by Kenneth Pike (1954) and Marvin Harris (1968). (Generally, etic refers to the perspective of someone outside of a culture or society; emic refers to the insider's perspective.) Empirical attributes are those distinctions judged appropriate by archaeologists, and cultural attributes are those that had meaning to bygone people. Rewording his earlier logic, Taylor (1972a) suggests that *emic* attributes can be identified only by inference from *etic* data, and such inferences are necessary if archaeologists are interested in typology and classification, cross-cultural and chronological relationships, or the nature and working of culture. He argues it is only if attribute similarities are not assumed to be fortuitous but based on consistent ideas that such interpretations may be considered valid.

A few brief comments should be made with respect to Taylor's piece on "Storage and the Neolithic Revolution" (Taylor 1973c). Here we see his general interest in anthropology combined with his specific interest in Neolithic cultures. Taylor proposes that Childe's "Neolithic Revolution" and Braidwood's "food-producing stage" might just as aptly be referred to as the "storage revolution" and sets out to examine the relationships among food production, food storage, and other aspects of culture from a cultural evolutionary point of view. He begins by noting that storage is necessary if a group is to benefit from increased food resources, in terms of both delayed consumption and preserving seeds for the next year's harvest. He draws attention to the fact that under favorable conditions, certain hunter-gatherer groups can have a relatively sedentary life, especially when they are able to store or conserve food. The origins of food production, Taylor suggests, may have been related to these circumstances. He proposes an evolutionary sequence of pre-Neolithic to Neolithic developments as follows (Taylor 1973c: 196):

- Storage of wild products (tethered nomadism to partial sedentariness, Paleolithic/Mesolithic).
- Animal husbandry (pastoral nomadism or partial sedentariness, proto-Neolithic).
- Incipient agriculture (sporadic nomadism, early Neolithic).
- Sedentariness and ceramics (village farming, full Neolithic).

### *Retirement*

Upon retirement in 1974, Taylor devoted a year and a half to completion of his Coahuila report. At about this time, financial considerations forced him to sell his library. He decided that without it, he could not keep up with his professional obligations. Never one to waffle, Taylor writes: "I did not want to become an old fuddy-duddy, out-of-date has-been—so I 'cut it off sharp.' I have done only casual reading in anthropology since that date; I have attended no professional meetings; I have not associated with my colleagues, except those with whom I have a personal, not merely or solely a professional, relation" (Taylor, personal communication, 1983).

## THE COAHUILA PROJECT

After the publication of *Contributions to Coahuila Archaeology, with an Introduction to the Coahuila Project* (Taylor 1988), Taylor attempted to suppress it from distribution because he was so dissatisfied with it (Euler 1997). The revised version appeared posthumously as *Sandals from Coahuila Caves* (Taylor 2003). Together, these comprise the last major statements of Taylor's archaeological career. Owing to this, it is logical to end my discussion with some remarks on the Coahuila Project.

As noted earlier, Taylor's first fieldwork in Coahuila was in the summer of 1937 and was followed up by sessions in 1939, 1940, 1941, and 1947 and brief sessions in 1950 and 1958. Over the years, this project came to be viewed by others as the work that would provide a clear example of the application of a conjunctive approach to archaeology. However, for various reasons—the immenseness of the task Taylor set for himself, other professional commitments, and perhaps the unfair expectations of others—publication of the final report was delayed and, in the end, it was never produced. Instead, through the years, the archaeological community has had to content itself with a collection of brief articles.

The earliest of these deal strictly with survey results (Taylor 1937, 1938) and are of no particular interest here. The next to appear, "Blood Groups of the Prehistoric Indians of Coahuila by Serological Tests of their Mummified Remains" (Taylor and Boyd 1943), is interesting in that it indicates Taylor's broad interests and the use of physical anthropological data to determine population affinities or origins. Although the results reported are only preliminary, they suggest "that the Coahuila culture represented an ethnic group not identical with all the modern inhabitants of the American Southwest and judging by their blood groups possibly allied to the Big Bend Basket-maker culture or possibly to certain early South American groups" (Taylor and Boyd 1943: 180). This is the kind of hypothesis that could also be tested using cultural data at the disposal of the archaeologist.

Next are the comments on Coahuila in *A Study of Archeology* and a report on radiocarbon dates from Frightful Cave (Taylor 1956) in which Taylor extends Jennings's concept of Desert Culture into Mexico. The next notable article referring to Coahuila is "Tethered Nomadism and Water Territoriality: An Hypothesis" (Taylor 1964) in which Taylor draws attention to the location of sites in the northern part of the state that show "a marked and long persistent selectivity in the choice of places for settlement" (1964: 197). Taylor (ibid.) asks, "What factors could have influenced site selection; what was the relationship between settlement patterns and other cultural, social and natural aspects of this eco-system?" He concludes that this type of nomadism—tethered nomadism—occurred when people were "tied" to a particular water source to which they had rights. He then points out that the boundaries of each group's nomadism were determined by the distance the group could safely travel from the water source and coins the phrase "water territoriality."

Both tethered nomadism and water territoriality have implications for other aspects of culture. They encourage cultural conservatism and exert a strong influence on the size and composition of the social groups. Infant mortality would likely have been high, not many people would have reached old age, and given the increased demands placed on females in nurturing young, the sex ratio would likely have favored males. Taylor notes that the pattern of life in Coahuila today is nearly identical to the one he proposes for past groups, even though new people have inhabited the area. He submits this may reflect environmental determinism or Goldenweiser's (1933) idea of the limitation of possibilities.

The concept of nomadism is given further treatment in "The Hunter-Gatherer Nomads of Northern Mexico: A Comparison of the Archival and Archaeological Records" (Taylor 1972b). Here Taylor defines three characteristics of nomadic cultures that influence archaeological investigations: (1) dwellings that are easily moved or abandoned; (2) small social units and a limited inventory of cultural objects; and (3) relatively lightweight, small, unbreakable, portable tools, utensils, and so forth. He notes the camps of nomadic people are poorly represented, sites are rarely stratified and often disturbed, and the only instances in which the remains are practical for archaeological study are those in which there has been a concentration of occupation in time and space. Taylor comments that in northeastern Mexico, this situation is found in caves and rock shelters, and hence, he has concentrated his efforts in these areas (Taylor 1937, 1966a; Taylor and Rul 1960). He admits this is a biased sample and suggests if open sites were examined, a different picture might emerge; however, he doubts the differences would be significant. Taylor compares data collected from archival and archaeological sources related to nomadic groups and finds agreement between the two sources.

The most extensive description of the Coahuila material to date is found in "Archaic Cultures Adjacent to the Northeastern Frontiers of Mesoamerica" (Taylor 1966a). Taylor states that he is dealing with a single cultural tradition spanning 10,000 years within which he identifies five complexes: Cienegas, Coahuila, Jora, Mayran, and Coastal Plain. He discusses the concepts of Desert Culture and tethered nomadism, makes comparisons with sites from Tamaulipas and Texas, as well as peripheral areas of Mexico, and offers comments on ethnohistorical and linguistic data.

The final statements on the Coahuila Project are the above-mentioned *Contributions to Coahuila Archaeology, with an Introduction to the Coahuila Project* (Taylor 1988) and *Sandals from Coahuila Caves* (Taylor 2003). Both are, in their own ways, significant contributions, but they did not remove the millstone that long hung around Taylor's neck.

## THE INFLUENCE OF WALTER W. TAYLOR

In the foregoing discussion I have addressed the ways in which Taylor's work was influenced by the general climate of anthropological thought and the particular opinions of certain key individuals. Before concluding this chapter, it is appropriate to consider the nature of the influence that Taylor himself exerted; this discussion is in many ways a complement to a similar section in the introductory chapter of this volume.

In her foreword to the 1983 edition of *A Study of Archeology*, Patty Jo Watson (1983: xi) comments:

> [In this work] Taylor anticipates nearly everything that has come to be expected in good archaeological reports: the importance of understanding and working in terms of the natural stratigraphy, the importance of exact proveniences; the importance of biological data, of the entire paleo-environment, and of investigating the natural resources potentially available to prehistoric human populations; the importance of prompt and full publications; and the need for archaeological field schools to train students in recovery techniques and in documentation procedures.

I would qualify Watson's remarks to say Taylor not only anticipates these developments but has had a primary role in instigating them. Taylor provoked American archaeologists to think: to think about theory and method, about explanation in archaeology, the nature of inference, the problems of data collection, and about hypothesis testing. He encouraged archaeologists to re-evaluate their positions within the broader discipline of anthropology, especially so as to reassess the nature and significance of the contributions they could make to the study of culture (particularly the problems of cultural process and the formulation of laws of cultural dynamics).

Taylor's impact is given differential assessment by the two major factions within American archaeology: the "neo-traditionalists" and the "new archaeologists." Taylor (1972c: 28) defines "neo-traditionalism" as "an archaeology having traditional goals but working with an expanded range of data and modern techniques which have evolved in response to a somewhat modified, but still recognizable traditional conceptual scheme." New Archaeology, on the other hand, divorces itself from the traditional approach and sees itself as a radical new wing of archaeology devoted to scientific pursuits. Most neo-traditionalists readily acknowledge Taylor's contribution to present-day archaeology (see Trigger 1978); however, most New Archaeologists tend to minimize, ignore, or deny it (see Binford 1983b).

Despite their clamoring (or maybe to encourage clamoring), Taylor (1972c: 30) wrote, "I allow myself the presumption of looking upon much of the New Archaeology as practical application of a basic conceptual scheme, the earliest more or less complete expression of which was the conjunctive approach." The validity of this statement, expressed also in an earlier article (1969), is determined by the level at which one chooses to approach the history of American archaeology. Schuyler (1971: 397) makes this point:

> [I]t is apparent that indeed the question of a most recent stage in American archaeology is a complex one. If, for example, we want to speak of a new, and last[,] stage in American archaeology simply on a theoretical level we could easily push the 1950 date back to 1948 and W. W. Taylor's *A Study of Archeology*, or even earlier to some of Kluckhohn's work. On the other hand, if we are speaking of a new approach not only on a theoretical but also on an operational level, we might . . . have to move the date well up to the early 1960s when Binford, Deetz, Hill, Longacre, Martin, Schwartz, and others attempted to put into operation in the field what had in the past only been discussed.

It is easy to sympathize with Schuyler's view. Although there is much continuity from Taylor through to the New Archaeology of the 1960s, there are significant differences as well. These revolve around the nature of problem solving and the application of the systems approach.

Taylor's levels of procedures for archaeological investigation include formulation of a problem, unbiased data collection, analysis, description, synthesis, and interpretation. Taylor knew culture was not static but would appear so at any momentary "slice" in time made by an archaeologist. His is essentially a normative view of culture in that he believes the form and variability of the material remains represent the norms to which past cultural behavior conformed in producing them (Wylie 1982: 56–60). The methods of interpretation utilized are largely inductive, relying, for example, on inference from empirical data and utilizing ethnographic analogy (although as Folan notes in this volume, Taylor was wary of ethnographic analogy and the degree to which it might be used). There is the explicit assumption that "absolute truth" is unattainable, but one can make ever-closer approximations to reality through the testing of hypotheses drawn from inferences.

The levels of procedure utilized by the New Archaeologists are essentially the same, but there are important differences in the manner in which they are approached. To begin with, data collection is more closely related to the particular problem being investigated. Also, the New Archaeology approach incorporates a dynamic systemic view in which culture is man's extrasomatic means of adapting to his environment. More attention is devoted to the manner in which cultural material is produced, with the recognition that "[a] whole range of factors and conditions (besides the normative) may affect the production, use and deposition of material culture" (Wylie 1982: 69). The methods of interpretation are largely deductive, relying on inference from a body of laws. And there is an overriding concern with the validity of the inferences drawn. "The significance and validity of his interpretations is the main justification which the archaeologist can offer in support of his profession" (Kleindienst and Watson 1956: 75).

In general, I think no one can deny Walter Taylor's impact on American archaeology. *A Study of Archeology* was the statement of an important new trend, synthesizing the advances of the late 1930s and early 1940s (see Watson, this volume, for an alternative assessment). Its suggestions were reiterated in some of Taylor's succeeding publications (e.g., Taylor 1966b, 1972a, 1972c), and the results are clearly seen in the recognized shift in archaeology beginning in the 1950s. However, there was another important shift in thinking associated with Lewis R. Binford (e.g., Binford 1962) and the New Archaeology. I do not think the latter shift was any more significant than that anticipated by Taylor; it built on what preceded it with proposed major modifications. My view of the history of American archaeology favors a "linear-continuum" (see Wylie 1982: chapter 1) or evolutionary model of development from Strong (1936) through Steward and Setzler (1938), Kluckhohn (1939b, 1940), Bennett (1943) to Taylor (1948) and then to Binford and his students. This is not characterized by gradual change, but by a series of "fits and starts" in which Taylor and Binford represent two of the most significant "starts." This is Taylor's legacy and his place in the development of Americanist archaeology.

## AFTERWORD

This chapter is an effort to maximize the collection of "empirical data" relating to Taylor's life and work and has conscientiously presented these data for the use of future scholars who may draw their own conclusions. I have tried to place Taylor's writings in a spatio-temporal framework and have sought out the interrelationships among thoughts, experiences, and associations.

Not one of us exists in a vacuum. Each of us is influenced by the environment in which we live and work and by our particular life experiences, and each of us has the capacity to influence others. Clearly, "[n]o man is an island, entire of itself; every man is a piece of the continent, a part of the main" (Donne 1959 [1624]: 108). Walter Taylor's life and the shape and influence of his work exemplify this, although in the initial reaction to *A Study of Archeology* and in subsequent years, Taylor may have thought that he was living on an island. His 1972 essay "Old Wine and New Skins: A Contemporary Parable" reflects his realization of this, as do his letters to Folan (this volume). It is only now, sixty years after the publication of *A Study of Archeology*, that Taylor's ideas, especially the conjunctive approach, are finding their way explicitly into American archaeology (see Chapters 1 and 16, this volume). Taylor neither is an island any longer nor consigned to one.

## ACKNOWLEDGMENTS

I take this opportunity to thank the late Dr. Walter W. Taylor for his interest in my project. Thanks are also extended to the late Dr. Richard Forbis, my friends,

and fellow students whose stimulating discussions have sparked not a few of the ideas found in this chapter.

## NOTES

1. The term “American archaeology” is used in this chapter to refer to archaeology as practiced in the New World.

2. “The Ceremonial Bar is a bar-like object, straight or curved, which is clasped to the breast of certain anthropomorphic representations in Maya art. . . . The Bar Pendant is a horizontal pendant which hangs from the neck of, or appears apparently unsupported on the breast of, many anthropomorphic and other figures. . . . The Frieze-mask is the large, full face ‘mask’ seen above the doors on certain buildings of Campeche and Yucatan.” (Taylor 1941a: 48n3).

3. Walter and Lyda Taylor had three children before she died in 1960: Peter Wells, Ann Averill, and Gordon McAuliffe (Natch Taylor).

4. The Rockefeller Fellowship was arranged for Taylor while he was overseas. Those involved included Henry Collins, Clyde Kluckhohn, and Frank Setzler.

5. The debate revolving around archaeology as history, anthropology, or science has its own long history. For a detailed account see Watson (1973).

6. Taylor uses the terms “construct” and “synthesize,” rather than “reconstruct” and “resynthesize” because he feels we can never be sure the pictures we create are accurate. He uses the term “cultural context” to refer to cultural behavior and its results, the associations and relations of elements, and the balance between them. The term “culture context” is used to refer to inferred ideas and is on the fifth level of Taylor’s conjunctive procedure.

# PART II

## SOUTHERN ILLINOIS UNIVERSITY: COLLEAGUES’ PERSPECTIVES

CHAPTER FOUR

# WALTER TAYLOR

## POW, Professor, and Colleague

*Philip J.C. Dark*

This chapter addresses Walter Taylor's experiences during World War II and provides some insight to his life during the short period he was a prisoner of war and to his interests in anthropology. It was in this period that we first met and subsequently developed a close relationship. I discuss this relationship as it extended to my family and also included a period of interaction as colleagues at Southern Illinois University at Carbondale.

There is a problem when casting one's mind back to past events, happenings, ideas, and so on and, as an anthropologist, one must always be aware that memory is subject to error as well as being fickle in invention. Further, the ethnographer is inevitably selective. In consequence, the parameters of context can get distorted, content added to or left out depending on selection from memory: "Observe, now, how history becomes defiled through lapse of time and the help of the bad memories of men" (Mark Twain in *Life on the Mississippi*, chapter 55). I have indeed found that I have ideas of what happened to me as a POW, as a naval officer, that I have misconstrued when I have checked them against an account of the facts recorded at the time of their happening. However, during the three and a quarter years of my incarceration, I kept a log, or journal, of all the books I read, comments on them, ideas they generated, and particularly my activities as a practicing artist—all relatively innocuous in case the Germans confiscated it. It is to this record that I have referred in the preparation of this chapter and from which I have abstracted the account that follows.

## MARLAG O, PRISONER OF WAR CAMP

I briefly recount something of the nature of POW life in order to convey to the reader an idea of the context into which Walter Taylor was plunged.[1] The POW camp was called Marlag O, small in comparison to many POW camps; it was for naval officers and a few Royal Marines. The Germans captured only some 300 officers from the Royal Navy (Fleet Air Arm officers usually were sent to camps for Royal Air Force personnel). The camp was run by the Kriegsmarine. Being small had its advantages, and sometimes disadvantages, with respect to the German navy's concern for its "few" prisoners.

Marlag O was built in 1942. Before that time, naval officers who were POWs—including some captured in 1939—were located in one or two other POW camps. With the creation of Marlag O, all naval officers were brought together into a single camp. It consisted of a compound surrounded by barbed wire, with watchtowers for guards with machine guns placed at strategic points.[2] In this compound were several wooden huts, each divided into a number of rooms for POWs. Initially, there were some eight prisoners to a room and one or two single rooms for senior officers. There were latrines, cold-water showers, a hut for messing, and a hut for recreation and staging shows and plays, which the Germans encouraged and liked to attend.

Arrangements were established early on between Germany and the United Kingdom and its allies with respect to sending and receiving mail, food parcels sent through the Red Cross, book and tobacco parcels, and a nine-pound personal parcel every six months. Not that all went smoothly; time took on its own dimension. Those who wanted to study, learn a language or some subject, were catered to by taking examinations from the University of London or the Royal Society of Arts. Examination papers were sent by mail. In Marlag, the extent of knowledge and skills—practical skills of engineers and other naval specialists—was considerable because of the command of various languages by regular naval officers and reservists; in addition, the civilian backgrounds of reservists provided a range of professions. These competencies were put to use: courses in a variety of subjects were given over the years to which Taylor added one on anthropology. There was a library in the camp with a remarkable range of books augmented by people's personal books, for it was permitted to send book parcels, even though not all dispatched books reached their destination or survived the censors. The Swedish Red Cross was a generous donor. Sources for anthropology were surprisingly present and included Kroeber's *Anthropology*, Margaret Mead's *Growing up in New Guinea* and *Coming of Age in Samoa*, and books by

Huxley, R. R. Marett, and others.[3] Art, too, was well covered and there were many fictional classics.

There were periods of quiet, and then the various escaping activities stirred things up and normal routines were disrupted. Quite a few people escaped only to be recaptured. However, Lieutenant Commander W. Stevens escaped and reached Switzerland. One enterprising officer who was fluent in German, Russian, and French and learned the vocabulary of a French veterinarian (which his false papers identified him as) escaped, but was recaptured 300 yards from the Swiss border. Perhaps the most enterprising cover was that of David James, using much of his ordinary uniform and false papers, which identified him as Ivan Bugarov, a Bulgarian naval officer. He managed to hide in the hold of a ship going from Lubeck to Sweden and was flown back to England in a clandestine plane. But in all that I have sketched above, our predominant concern was more basic: it was food.

Although one learned to live with being hungry, it was only by being engaged in some activity could it be kept at bay. Living off German rations, I lost more than fifty pounds in the first months of captivity; this is but an example of what we all underwent. Without the Red Cross food parcels from the United Kingdom, Canada, and New Zealand, I guess we would have faded away. German rations were meager, to say the least. Red jam, for example, was sometimes issued. If one heated it, it became a sort of pulp. We understood it was made from coal, a substance the Germans used most skillfully to produce a variety of products, ranging from honey to blankets. I recall someone washing one of these blankets and hanging it on a line to dry; it froze and then snapped in half when he went to take it down.

## LIEUTENANT WALTER W. TAYLOR

My log entry for 28.I.45 notes, "This last week has been particularly disturbing and foul." Those in our hut block were told to move and to double up in another one in order to make room for 250 troops. Eight of us had been together in the same room for two and a half years and would now have to disperse. We moved in an appalling day of snow. The Americans, who had been reported as marching to the camp, did not arrive. In our new room—the same size as our one for eight—we were quickly settled in with pleasant company. Suddenly, there was "news that [an] American marine [was] arriving and we were to go up to fourteen: great consternation . . . by about tea time [the new arrangement of the room] was squared off . . . and just livable in." "Walter Taylor, Lieutenant American Marines, arrived before lunch: first impressions: a very nice fellow, quiet, slightly graying, anything from 26–36 . . . Professor of Anthropology." "Taylor teaches anthropology for a living though naturally interested in research and has worked on one or two field projects; his wife is an ethnologist." Arriving with Taylor was another marine, Major Ortiz. We understood that they had been behind enemy lines in the South of France before the invasion there but did not learn anything more about them.[4] There were a few Royal Marines in our camp, so placing marines with us was not out of line, although they were two of only three Americans in the camp, the other a naval lieutenant.

On February 14, I wrote that Walter settled into a confined space very readily. Both of us were on top bunks with our feet facing the others. Walter was "fond of his bunk." "Walter [was] a very pleasant and amusing chap with his stories," particularly one in which he was told to take with him on his way to Africa a very important box and two letters for General Eisenhower, which, he said, he guarded carefully his whole journey until reaching Eisenhower's aide, who took it into the general. On his return, he asked Walt if he knew what was in the box and Walt said, "No." The aide then told him, "They were cookies from Mrs. Eisenhower."

My entry for 11.II.45 notes that we were taken by the Germans to the bathhouse outside the camp for a hot shower: "[W]hen drying myself found dirt scaling off through friction of the towel, so ingrained evidently; a bit horrified but found others experiencing the same thing. Hungry days; dizziness definitely noticeable when stooping or after walking for a bit re weakness and desire for a good square meal by everyone very evident as conversation leaps to the inevitable topic, food, at the slightest excuse." Taylor started his course soon thereafter. For 25.II.45, my notebook reads: "Walter started his lectures last week: [Monday, Wednesday, Friday]. Extraordinarily good, exceptionally clear in emphasizing or explaining a point and an enthusiast who enthuses his audience (attendance opposite to usual which is generally a steady falling off, very good to increase) . . . a useful introductory record to anthropology and a stimulus to discussions with those attending: very well given and most popular."

Just after this, "17 wagons of parcels arrived: terrific excitement . . . one of the best bits of news ever . . . German rations down to 290 grams [of bread]." The period before this our general rations were down to some 600 calories per day. Walter's lectures ended just after 22.III, when we were all in a state of euphoria from the increase to our diet, for "his last rather a poor note on which to end: 'on the implications of the Universal Culture Pattern.'"

From my notes on Taylor's course, I can show how it was structured and the main aspects of anthropology that he covered. These can be seen in relation to the position he established before in his thesis and subsequently in *A Study of Archeology*. The course he later developed at Southern Illinois University was given for seniors and graduate students.

## WALTER TAYLOR'S CLASS AT MARLAG O

I have been selective in the following presentation, but I hope the view is taken that, as Huckleberry Finn said of Mark Twain, "There were things that he stretched but mainly he told the truth."[5] The summary account of the course of lectures and discussions Taylor gave in Marlag shows that he was very much a cultural anthropologist in a general sense, although he specialized in archaeology. Later, in *A Study of Archeology*, he reiterated his interest in culture when he wrote that "culture is a mental phenomenon consisting of the contents of minds" (Taylor 1983: 96). The summary account also gives an idea of the general position of anthropology at the time as Taylor saw it.

The following is taken from my original notes on Taylor's lectures. He divided the course into six main subjects:

1. Relation of anthropology to other social sciences.
2. Component parts of American anthropology.
3. Culture: statistics, what it is, anatomy.
4. Culture: integration, physiology.
5. Culture: dynamics, physiology.
6. Universal culture pattern, cultural variety.

Purpose: to develop cultural tolerance and relativity.

Anthropology Taylor saw as divided into physical and cultural:

*Physical anthropology*

1. Normal human biological variation (morphology).
2. Racial anthropology.
3. Human genetics.
4. Archaeology: nothing but a series of field techniques to produce data for ethnography, ethnology, and social anthropology, art, and architecture.
5. Prehistory: as used, it is preliterate ethnography.
6. Linguistics: as used, it is preliterate philology.

*Cultural anthropology* has three legitimate branches:

1. Ethnography: collection and description of cultural data (done in the field).
2. Ethnology: comparative study of cultures or segments of cultures to obtain chronology or sequence of cultures or traits; construct cultural contexts; goes beyond the limits of ethnography.
3. Social anthropology: comparative and other study of segments (traits) of culture and their context to learn about the culture itself.

Taylor then started with Tylor's definition of culture, considering the nature of its parts: habits, customs, and beliefs as man learns or acquires them as a member of society. Customs and artifacts themselves are not culture, but the ideas, the concepts that produce the objective traits—that is, culture itself—make up a set of mental norms objectified in concrete traits. Disciplines other than cultural anthropology deal with culture, such as history, economics, political science, and art, but these only deal with a segment of culture. From here, he went on to consider the construction of cultural contexts, the selection of data for truth, selection according to one's own ideas, at the level of history, of social cultural context. Early anthropologists were not interested in construction of cultural contexts, only in collecting facts, whereas Boas insisted on structural context. The aim of the modern American anthropologist is to write up the nature and workings of context, to learn about the nature and workings of culture, its statics and dynamics, its anatomy and physiology—that is, culture.

So far, he noted, "nothing has been said about defining anthropology by reference to 'primitives,' aborigines, pre-literate societies, etc. Most of us, in part myself [W. Taylor] do not recognize this limitation. Anthropology pursues its cultural studies wherever there is culture and that means wherever there are humans because human and cultural are synonymous. All that is required for one to practice anthropology is that the primary interest be that of elucidation of the nature and workings of culture as a whole."

In another lecture Taylor considered the roles of anthropology vis-à-vis establishing colonial policies, industrial and governmental applications, and social psychology, citing Margaret Mead's *Growing up in New Guinea*, the global problems of different people living together, and why different people are different. Then he went on to consider instincts, intelligence, and some biological differences of humans: "Biology gives the basis upon which culture makes the variations." Then he considered race, nationality, and language and what accounts for cultural differences, giving various ethnographic examples. What man selects is determined by culture. This led him into a favorite concept of his, the "stool of culture," each of four legs representing a capacity of man: (1) habit forming, (2) intelligence, (3) society, and (4) language. These he then elaborated in some detail over the following lectures, arriving at a significant question: what of death? Biological existence ceases at death. The social and cultural continue after death.

Taylor's view of life was expressed in a consideration of the opposition of cultural and biological thinking, in terms of achievements in social and cultural immortality as opposed to the position of the ascetic, the hermit, and the like. Rather than be a hermit or an ascetic working for his own good, it seems better to believe in social and cultural immortality, in the remembrance by future generations of good actually done; that one has but one chance to do good and gain immortality by being remembered by those we leave behind. This seems better than seeking for one's own salvation in some future world by changing the good of this world. Better, too, than running wild here and depending on a reprieve at the last moment in the form of confession and absolution to ensure an afterlife and immortality in the biological sense of a conscious return to Being.

What makes us human is culture, and what makes us different from others is cultural implication, that is, a difference we have learned. This leads Taylor to consider what the relation of an individual is to culture and to status and role, the former ascribed or acquired and the latter as customary or individual, and to exemplifying them. Then followed a consideration of the nature of cultural integration viewed in terms of the established divisions of universals, alternatives, specialties, and individual peculiarities. He gave examples over one or two lectures, including differences among Plains, Pueblo, and Northwest Coast cultures and patterns of culture that developed along certain lines.

The next principal topic he discussed was cultural change or cultural dynamics. Demonstrable origins give us a relationship of cause and effect. Many modern anthropologists, however, are only interested in context as it is today, but it is essential to find out how it developed that way, in other words, it is a question of cultural dynamics. Culture change is seen as cultural growth, cultural loss, and culture change. For these he provided a number of examples and considered related concepts, such as discovery and invention and cultural threshold. Then he went on to diffusion, the borrowing of traits or independently creating traits, and which of these one must ascertain in the analysis of a culture. The foundation of theoretical anthropology rests on what proportion of all traits is diffused, and what is invented. From this he considered the Evolutionist School as exemplified by Tylor, Spencer, and Morgan. Taylor said this about their thinking:

> Man passed from the simple to the complex through a regulated number of sequential stages; advance was made primarily by the people in the tribe, disregardful of what was going on around them; outside things came in[,] in the normal sequence as the "ladder" was climbed. They believed in Psychic Unity; all men were potentially able to reach a complex form of society; some tribes came into the "ladder" late, some early. This is WRONG. In all this the Victorians found a certain self satisfaction, in regarding themselves at the top of the ladder.

The change in these theories came with Boas, whom Taylor noted was "probably the last anthropologist who had a grasp of the whole field of anthropology." Boas, Taylor said, advocated "going into the field, stop thinking in the armchair." He drew attention to Boas's insistence on fieldwork as opposed to the purely deductive method of the old anthropologists; in addition to fieldwork one must have quantitative data and must understand that a trait cannot be taken out of its context. One case is not enough. One must have a perspective of world culture before making deductions and "contexts, contexts." It is "impossible to take a trait out of its context and to hope to come to correct conclusions about it." Taylor gave an example of an axe blade as used by Western man versus by a Polynesian, who associated it with the sun god, having seen it glint in the sun, and therefore put it in his temple. The two contexts are quite different and the anthropologist must know the contexts to realize the truth of the two different traits of the same object. In the reactions to Boas's work and the indigestible piles of data Boas promoted—collecting data for its own sake—the purpose of anthropology began to be forgotten. But in the last ten to fifteen years, interest in the methodological background of anthropology has grown.

In considering diffusion versus independent invention, Taylor gave some of the classic examples of the former: initial spread of the alphabet, the double-headed eagle, megalithic monuments. Then he moved to studies of distribution and the cautions one must take in analyzing them, the integrativeness of particular traits in each culture, language and elaboration. This led him to the age-area concept and the barriers to diffusion of a trait: geographical, alternative traits in opposition, counter traits as obstacles, and cultural conservatism, innovative or prestigious. As regards diffusion itself, material objects can diffuse more easily than ideas. An elaborate clan structure will not be analyzed by a man: he will take only the more obvious idea. Ideas, though, diffuse very slowly and sometimes not at all, for the outward characteristics of a trait may be copied lock, stock, and barrel with complete ignorance of the ideology behind them. He cited, for example, Indians and Christianity.

Certain institutions are found in every group studied. Early conclusions were that these institutions are formed to fulfill certain human needs. It was first thought that these were biologically controlled (food, sex, self-expression), the institutions of culture being a response to man's biological nature, but knowledge of this is slender. However, there are certain needs that individuals feel and that are filled by creating certain institutions. But how they are created cannot yet be determined.

In *Man and Culture*, Wissler gave a comprehensive list of universal facets of culture. These are (1) subsistence, (2) material culture (housing, clothing, artifacts), (3) aesthetic institutions or institutions for self-expression, (4) family structure, (5) social organization, (6) political organization, (7) religion, (8) theories of disease, (9) ethics and morals, (10) out-group relations and foreign relations, and (11) institutions for the resolutions of conflicts, whether internal or external. The implications of the Universal Culture Pattern are that all cultures are remarkably similar all over the world: other people have the same needs and problems as we do and establish institutions to meet them, but until we know more about our own culture we should not try changing others because we do not know what we are changing. Religion is our most blatant arrogance: forcing other people who think one way about the world into thinking another way. Culture and our relationships with other peoples must be regarded in a relative manner.

The course, as I have sketched it above, may seem cold and clinical but it must be envisioned as taking place within a context of the strange lives we were living, and at that time they were stranger than before when routine went on without major disruptions. So the course should be set against the last three months of World War II, and for us POWs this was a disturbed and anxious period, one of uncertainty. Would the SS come in, line us all up, and shoot us? Barely adequate food was restored by the surprise arrival of Red Cross food parcels, giving us some cheer in consequence. We did know something of what was going on as we had a hidden radio and thus the BBC news. Then, on April 9, there was a flap that we were to be moved. Just prior to this, a Swede, a captain in the RASC and a parachutist who had been captured nine days before arrived in the camp, said everything was in the bag as regards the proximity of "our boys," but he was more optimistic than we were. We were told that we would be moving off at 1900 that evening with what we could carry with us. We delayed our departure—by prevarication—to early the next morning. In the interim, chaos reigned at the sudden upheaval to our lives and there was concern about how we would cope on the march, for it was but a short while ago, when food parcels were few, that we had had dizzy spells. We ambled along the day we left.[6] Several people left our long, strung-out column to disappear into the countryside to try to get back to our lines. Taylor and Ortiz were one such pair and, I understood, hid up in a wood for some days and finally made it. Taylor and I were to meet up three years later in Santa Fe.

How Taylor's life in prison camp was shaped by his commitment to anthropology is a question that has been asked.[7] Other than giving some lectures on the subject, his life was concerned with food and with living in confined conditions with other officers. Among those crammed into a small room a special sort of relationship developed that evened out differences, and required tolerance, but grew into the kinds of relationships found in extended families. Indeed, as we were small in numbers, a close fellowship continued beyond that of incarceration and carried over into continuing friendships after the war. This was strengthened by an annual reunion of Marlag POWs.

As to the effect of his POW experience on Walter as a person, it followed the pattern we all continued to experience throughout life, as various memories—suddenly intensive, strange participations in dreams, some fearful, frightening, often weird or filled with horror—dimmed and fluoresced. How being a POW influenced his future contributions to anthropology is another question that has been asked. Not at all, I would have thought. His path in that field was already set before he got involved with the military as a Marine, one expression of it being his thesis at Harvard.

## AFTER THE WAR

With the cessation of hostilities in Europe, Walter Taylor returned to the United States and I to the United Kingdom, where I studied at the Slade School of Fine

Art and the Department of Anthropology, both in UCL (University College London).[8] That the POW friendship with Walter should develop into that of a close personal one—to my wife and myself and my family—was partly because of happenstance or, more correctly, to Professor Daryll Forde, my mentor at UCL with whom I had a lasting friendship and, as well, from whom I received much guidance. Indeed, it was he who said that to pursue my interest in what was then termed "primitive art," I should continue my studies under Ralph Linton at Yale University, and he wrote to him accordingly on my behalf. I was fortunate indeed to have Ralph Linton as my mentor; he and his wife were kind friends.

Walter had been supportive of my going to Yale University Graduate School and also put us in touch with friends of his in New Haven. Before going to Yale, we visited the Taylors in Santa Fe. This was at the close of the time during which Walter completed the transformation of his thesis into *A Study of Archeology*; this of course led to a period for him of varied receptions of his labors. We all went to the conference at the Point of Pines camp on the Apache reservation in Arizona (the Pecos Conference, 1948), which was attended by Haury, Kluckhohn, and others. A. V. Kidder was there, too, having just arrived from Mexico to announce the discovery of the paintings at Bonampak.

Instead of returning to the United Kingdom after two years, we stayed in the States because Yale kindly gave me a fellowship. This course led to strengthening the initial POW friendship with Walter. However, that it became a close one was also no doubt because of my two-year research project (1951–1953) for the Human Relations Area Files in Santa Fe at the International Folk Art Museum. This was indeed a purely fortuitous development for Walter was still living in Santa Fe at that time.

## SOUTHERN ILLINOIS UNIVERSITY AT CARBONDALE

In 1964, when I was a chairman of the Department of Anthropology at Southern Illinois University at Carbondale (1963–1966), I asked Walter Taylor if he would write a brief account of its history. This he did, and his draft was circulated to various faculty for their comments, which were included in a revised version. It is on the resultant account, completed in the winter quarter of 1964, that I have drawn for what follows. As several of the contributors to this volume were graduate students in the department, how it started may be appropriate to recount (see Kelley and Riley chapters, this volume).

Anthropology started at Southern Illinois University in 1950 when Dr. J. Charles Kelley was appointed director of the University Museum and professor of anthropology in the Department of Sociology. Anthropology thus was based initially in the museum. Briefly, the Department of Sociology was changed to the Department of Sociology and Anthropology until 1955 when the Department of Anthropology was created. In that year, Carroll L. Riley and Charles H. Lange joined it and had appointments in the museum and department. This brought a strong interest in the Southwest, Lange's study of Cochiti being thought of very highly. At this time, a search for a chair of the new department was initiated.

In 1958, Walter Taylor joined the university as the first chair of the Department of Anthropology. The department revised its orientation to lay primary stress on graduate teaching and research—looked upon as being one and the same thing. At the same time, a strong but minimal two-year undergraduate major was started for upper-division students only.

In 1959, Dr. C. R. Kaut, a social anthropologist, joined the department and, at the same time, Dr. M. L. Fowler and Professor Pedro Armillas joined the museum staff and thus added significant specialties in archaeology to the representation of anthropology in the university. Ties between the Department of Anthropology and the museum were maintained by various cross appointments or part-time teaching.

In 1960, Dr. George Grace and I joined the department. He is a linguist who had worked with Kroeber and was then recognized as the leading authority on Pacific languages. We were to lose him after three years when he left to become the chair of linguistics at the University of Hawaii. I had been administrating the West African Institute of Social and Economic Research of the University (College) of Ibadan, Nigeria, and then had been moved to research on Benin art for the institute's Benin History Scheme. My focus was art and technology. That I moved to Southern Illinois from Nigeria and Europe was because of Walt's persuasiveness and the attraction of the graduate and research programs he had initiated.

Consideration was given originally to creating a strong master's program, but it became apparent that without a doctoral program the better students would not be attracted to Southern Illinois University. Consequently, with seven anthropologists at the university by the summer of 1960, the department began to investigate the possibilities of a doctoral program. In the spring of that year, Dr. C.K.M. Kluckhohn, of Harvard University, came to campus as an outside consultant. His formal report was encouraging. In essence, it said the anthropological staff, from both the museum and department, was of high quality and entirely competent to undertake a doctoral program. However, Dr. Kluckhohn did point out the need for some important additions to both staff and facilities, such as library collections, laboratories, and financial support of graduate students. Also during the spring of 1960, Dr. Erna Gunther, University of Washington, was on campus as a visiting professor in the Department of Anthropology. On her departure, she made recommendations for the improvement of the departmental program. Many of these duplicated those mentioned by Dr. Kluckhohn.

The development of the Department of Anthropology was premised on a predominantly graduate and research orientation of the department. Through Taylor's insistence, it was accepted by the university that graduate training in anthropology was meaningless without research, and that research in anthropology meant fieldwork, at least in considerable part. This, in turn, meant time away from the university; thus, it was acknowledged that one quarter in four of any year could be used for fieldwork. Graduate students would, when appropriate, do field trips as part of the academic program and would assist staff members as integral parts of their projects.

We were fortunate in 1961–62 to have Laura Thompson as a visiting professor. When she left, Dr. J. S. Handler joined the department, giving another dimension to its offerings as he was the leading authority on the anthropology of Barbados and a highly regarded specialist in the cultures of the Antilles. Joel Maring, a linguist with specialization in the Southwest, replaced Dr. Grace in 1963.

At this time, the university inaugurated a General Studies program. For anthropology's participation in this program, three more staff positions were allowed, but this meant that graduate assistants and research assistants had to become teaching assistants in order to cope with numbers in the General Studies program. Thus, their training in research with faculty was interrupted.

The establishment of anthropology at Southern Illinois as a graduate department with M.A. and Ph.D. programs was occupying all of Taylor's time and, indeed, that of all of us, for we spent much of our time seeking to structure not only the program of studies but the content: what was vital to be covered? This and the research we were all pursuing is the "other side" of the account of the initial developments of the anthropology program, much of which all academic departments experience, including relationships with university administration and the adequacies of support in laboratory, library facilities, scholarships, and so on. Taylor felt grave concerns at various developments that had not moved as straightforwardly as he had envisaged. However, when he gave up the chair in 1963, the first North-Central investigation gave the department one of its four highest ratings within the College of Liberal Arts and Sciences.

## TAYLOR'S *A STUDY OF ARCHEOLOGY*

Some notes in retrospect on Taylor's monograph may be appropriate. What, in fact, were the influences of British archaeologists on his views? Strangely, he does not have much to say about Gordon Childe, although in talking with Walt, I thought he had great respect for Childe's work. Walter (Taylor 1983 [1948]: 170), in considering synthesis and context, notes that "it will be a rare find that is not amenable to some analysis." He goes on:

> It is hardly coincidental that a most pertinent statement has come from one of the few archeologists who has presented his material under broad cultural categories and written what, in effect, is an archeological ethnogra-

phy (Clark 1940). Grahame Clark says: "Archaeology is often defined as the study of antiquities. A better definition would be that it is the study of how men lived in the past . . . [the archaeologist] has to rely upon circumstantial evidence and much of this time is taken up with details which may appear to be trivial, although as clues to human action they can be of absorbing interest." (1939: 1)

Taylor (1983: 14–15) acknowledges Pitt-Rivers's role in establishing formal archaeology in England "and a definite attempt at accuracy in excavation and recording." I have always understood that Pitt-Rivers's excavations on Salisbury Plain went to great lengths in recording and reconstruction and were a model of procedure. At the museum at Farnham, Dorset, which he built, nine halls were given over to the reconstruction and display of the excavations he made.

Regarding the influence of Walter's work on archaeologists in the British Commonwealth, Peter Gathercole (Emeritus Fellow, Darwin College, Cambridge University) told me that when he built the Department of Anthropology at the University of Dunedin, Otago, New Zealand, his approach was a holistic one in terms of archaeology and anthropology and that his students latched on to Taylor's ideas in conjunction with those of others, such as Childe and Clark.[9] Notably, B. Foss Leach and Helen Leach produced a fine study, *Prehistoric Man in Palliser Bay* (1979: 4–5), that cites Taylor's influence:

> The principal aim of this programme was to construct a well documented regional culture history by the close study of its prehistoric communities, investigating as many facets of their culture as possible. Much of the inducement for this conjunctive approach came from the writings of W. Taylor (1948) whose outspoken criticism of the narrow compass of American prehistory also seemed relevant to the situation in New Zealand in 1959. It was regarded as most important to describe the economy within a matrix of environmental change and stability. Such a programme followed the lead set by British Archaeologists such as Clark (1954), and exemplified in New Zealand by the work of Shawcross (1967) and Higham (1968). Both Taylor and Clark had stressed the need for specialist assistance in biological analysis, and the help of a number of natural scientists was obtained for the Wairarapa project.

Gathercole wrote that he had always considered Taylor's monograph "a fine piece of philosophical writing. Either Roger Green, perhaps in 1959, or Foss Leach in 1963/4 introduced me to the book. But my memory is that it had a major impact on our students at the time—and may well have in the following decade too."

Jim Specht (Emeritus Curator of Anthropology, The Australian Museum) spent the year 1970–71 in the Department of Anthropology at SIU teaching prehistory. He kindly provided the observations that follow and that were initially directed to enquiries from Peter Gathercole that I had prompted.[10]

> Walt was bitter about the lack of recognition his book had received. It was the "New Archaeologists" who objected to Walt's "culture history" focus while acknowledging the value of his contextual approach. For Walt, the reprinting of his book in 1967 brought him some long-overdue credit—he told me that he felt the New Archaeology was really an extension on his book's direction, but no one really gave him due credit.

Specht continued his reply to Gathercole:

> When I arrived in Australia as a graduate student in 1965, the dominant theme in theory was the Willey and Phillips book *Method and Theory in American Archaeology* (1958)—Jack Golson's paper in the Freeman and Geddes volume for Skinner explicitly applied the W&P approach. I do not recall ever discussing Taylor with Jack Mulvaney, and I confess that I did not read Walt's book until I went to SIU in 1970. My bet is that it was Roger Green who raised the book with you. I think Jack's contribution was to acknowledge that the American literature was worthy of attention, which was something that David Clarke had been on about for years (though David was more focused on Steward and Binford). It was Jack's reading of Willey and Phillips that led so many of his students in the 1960s to use W&P in their theses—though others were paying more attention to Binford. Most of us relied on the American literature for method and theory—especially Rouse, Ford, Wheat, Spaulding, and others as there was little else other than Childe and Grahame Clark in the Anglo-Saxon part of Archaeology (David Clarke's *Analytical Archaeology*, 1968, came out too late for some of our dissertations).

Jim Specht wrote that he had looked at Leach and Leach's (1979) Palliser Bay volume, noted what was quoted above, and wrote, "That's as explicit a statement about Taylor's influence as you could find."

Walter Taylor was a man of great courage as displayed in his service in a very hazardous role in the marines in World War II. He was a fine scholar with a matching intellect, a determined person with a strong sense of rectitude. He enjoyed life and some of its special pleasures, such as fishing and growing orchids. He visited us regularly in our retirements and explored parts of England in some depth, such as Hardy country in Dorsetshire, and always read up on the literary and culture backgrounds of the places he visited. This continued until his sad illness and demise. One is fortunate to have had as good a friend and colleague.

## ACKNOWLEDGMENTS

I am most grateful to Jim Specht and Peter Gathercole for the information they gave me about Walt's work and its appreciation in New Zealand and Australia, and to Jim for his personal notes.

## NOTES

1. Although there are many books and movies about POW life in Germany, camps and lives varied. Two books recount accurately various facets of our lives in Marlag O, one by David James (1947) and another by Guy Morgan (1945). Further background data can be gleaned from the catalogue of an exhibition I had at the Honolulu Academy of Arts (Dark 1994). This catalogue was abstracted from a longer and fuller account (Dark n.d.), the text of which I had slanted from my records toward the theme of my art activities in Marlag. A copy is lodged in the Imperial War Museum in London.

2. In the first months of captivity, barbed wire was very confrontational, but later, although it was well embedded in one's subconscious, one learned to blot it out from the conscious, visual world.

3. Some examples of books available may be of interest to the reader. In addition to Kroeber and Mead, there were Huxley's *On the Natural History of the Man-Like Apes*, Robert Marett's *Head, Heart, and Hands in Human Evolution* and his autobiography *A Jerseyman at Oxford*, H.A.L. Fisher's *History of Europe*, zoology textbooks with chapters on heredity and evolution (authored, e.g., by L. A. Borradaile and Curtis and Guthrie), and Hilaire Hiler's *From Nudity to Raiment*. Art history books too were fairly widely representative: Herbert Read's *The Meaning of Art*, a book by Gardener of world art, Talbot Rice's *Background of Art*, and such classics as Adam's *Mont St. Michel et Chartres*, a most remarkable study of the twelfth and thirteenth centuries in Norman France and the Ile de France that is both perceptive anthropologically as well as aesthetically.

4. At that time they needed to be cagey on giving even us information about their activities as they had been very fortunate not to have been shot when captured and to have reached a proper POW camp.

5. I have referred to this quote before in discussing the problems of recounting and selecting with respect to ethnography (Dark 2002: 15). The quote I came across in Prime Minister James Callaghan's (1987: 21) excellent autobiography, in which he confessed to a selective memory and, therefore, asked if he could be trusted to be accurate.

6. The POWs of Marlag marched across country, ending up in Lubeck on April 25. Most of us were flown home to England in an Australian squadron's Lancaster bombers on May 9.

7. I do not know how long he was a POW before reaching Marlag O, but his service record must have that noted.

8. I had decided in Marlag, before Walter Taylor had arrived in our camp, to pursue studies in anthropology in London as adjunct to my main interest and practice as an artist. Wide reading on non-Western art forms and earlier interests had moved me to this decision, and I wrote home for books to be sent. Subsequently, I was fortunate to be able to discuss with Taylor my plan, and his introductory lectures were most helpful in setting the extent and nature of anthropology.

9. Peter Gathercole, personal communication, 2004.

10. These observations were in reply to an e-mail from Peter Gathercole on my behalf (personal communication, 2004).

CHAPTER FIVE

# PROFESSOR WALTER W. TAYLOR AS CHAIRMAN

*J. Charles Kelley*
*(Edited by Jonathan E. Reyman[1])*

## BACKGROUND

I went to Southern Illinois University in 1950 as Professor of Anthropology, within the Department of Sociology, and as Director of the University Museum. At that time I was charged with the development of a first-rate regional museum and a program of research in archaeology and related studies in cultural anthropology and the building of an undergraduate program in anthropology. It was realized that the latter endeavor would require several years for implementation, but I was promised that when this program was sufficiently developed with an adequate faculty, a separate Department of Anthropology would be created.

In the summer of 1957 the anthropology faculty had been increased to four full-time members and one teaching assistant with the addition of Dr. Charles H. Lange, Dr. Carroll L. Riley, and Howard D. Winters, with Ellen Abbott Hannen serving as [the] teaching assistant. Academic courses offered were adequate for an undergraduate major in anthropology as well as for an anthropology minor for a master's degree in sociology and anthropology. After considerable discussion, it was decided that we should request creation of a separate Department of Anthropology and that a chair be brought in to assume responsibility for its continued development. The university administration approved our request but offered the chair to me. I accepted on an acting basis only, and when I went on leave in November 1957, Dr. Charles H. Lange succeeded me as acting chair.

## THE SEARCH FOR A CHAIRMAN

The first candidate for the chairmanship on whom our group could agree was Walter W. Taylor, who was at that time living in Mexico City and working as a member of the Instituto Nacional de Antropología.[2] Taylor politely refused the offer. Subsequently, several other possible candidates were contacted, without success. While on leave in late 1957 and early 1958 in Mexico City, I spent considerable time with Dr. Taylor and eventually persuaded him to reconsider the Southern Illinois position. In May 1958, at the annual meeting of the Society for American Archaeology [in Norman, Oklahoma], he met and talked with the other faculty members and returned with them to Carbondale. There, after several interviews, he was offered the position again and accepted it.

## TAYLOR AS CHAIR

When Professor Taylor joined the faculty of the new Department of Anthropology in 1958, he immediately began an overhaul of the curriculum. His approach from the first was based on the educational philosophy expressed in *A Study of Archeology*: archaeology must be anthropology if it is to be anything. The educational theme of the new department, continuing to the present [1988], was to be that every student, regardless of his [or her] special interests, must first be trained in all branches of anthropology. He emphasized that the highest standards were to be maintained and that no student would be allowed to major in the department unless he [or she] maintained such standards. All faculty members were urged to tighten their standards.

Professor Taylor set the example himself and extended it to members of the department faculty. They should be constantly available and were expected to work long hours, either in connection with their teaching or in carrying out their research. Morale within the department clearly increased in a short time. Unfortunately, undergraduate students in a newly developed regional university were not attracted to such a department; enrollment did not increase, and the addition of several new faculty members needed to develop the desired program could not be justified.

Professor Taylor sought guidance from the university administration regarding this problem. He was presented with a challenge: upgrade the department to allow development of a Ph.D. program in anthropology. They left it to him to make it work. New positions would be approved as needed, and related programs would be supported. Professor Taylor accepted the challenge. At national meetings he made every effort to contact new graduate students, challenging the best of them to attempt the new program. Advanced students had already become familiar with the educational philosophy advocated so strongly in his monograph, and to many of them the challenge offered was attractive. Top-notch graduate students began to enroll, and the new program prospered. Taylor had met the challenge, and the new department began turning out excellent and well-trained Ph.D.s. What had been a struggling undergraduate department had now become a successful graduate department, largely because of the intellectual capacity and forceful personality of Walter W. Taylor.

In writing this short chapter, I sought a balanced appraisal, considering both good and bad aspects of Professor Taylor's influence on the development of the program. I have emphasized his really great accomplishments. Most adverse considerations would be better written by Taylor's students; in conclusion, I will address just a few. For example, some problematic effects were implicit in his basic approach to graduate students. The professor was there, but it was up to the student to do the rest (see Reyman, this volume; Reyman 1999: 689–691). There were other members of the department faculty who pursued a somewhat different approach; to them their responsibility to their graduate students was to search out and develop all talents that the student might have, without lowering standards. The challenge-and-response approach simply does not work with all students.

It was also reported that Professor Taylor, in his zeal for equal treatment of all, actually discriminated against some graduate students. Others said that Professor Taylor firmly believed that women had no place in archaeology and treated them accordingly. For my part, I think that Professor Taylor's only harm to the department he himself had created was his resignation from the chairmanship [1963] after only a few years' time,[3] an act that, however understandable, led inevitably to disruptive trends with the department.

## JONATHAN REYMAN'S ACKNOWLEDGMENTS

The first acknowledgment must go to my former professor, the late Dr. J. Charles Kelley (1913–1997), who provided this manuscript for an earlier, unsuccessful attempt (by William J. Folan and me) to publish a volume that focused on a critical appreciation of Taylor's work while Taylor was still alive. The second acknowledgment goes to Ellen Abbott Kelley, J. Charles Kelley's wife, for her gracious permission to publish this chapter. It adds significantly to our understanding of the founding of the Department of Anthropology at SIU.

## JONATHAN REYMAN'S NOTES

1. In 1978, Willie Folan proposed that a volume of papers be published assessing Walter Taylor's contributions to American archaeology. Subsequently, in the 1980s, Willie and I solicited papers from Walter Taylor's students and colleagues; we hoped to publish

the volume in 1988, the fortieth anniversary of *A Study of Archeology*. We were ultimately unsuccessful in our efforts (Reyman 1999: 695–696), but J. Charles Kelley submitted this chapter for that volume. It is published here, slightly modified from Kelley's original manuscript. I have edited the format to conform to the style of the other chapters, changed minor punctuation, added a few words for clarification [in brackets] that I believe Kelley inadvertently omitted, added a few brief comments and references, and deleted part of one sentence, this last to protect the privacy of certain individuals. In my opinion, the deleted words do not detract from the substance and accuracy of Kelley's text.

2. As students, Kelley and Taylor had been archaeological field colleagues in the Southwest, notably at Chaco Canyon, and classmates at Harvard. Charles H. Lange had also worked at Chaco Canyon.

3. A consequence, I believe, of the premature death of his wife, Lyda, in 1960.

CHAPTER SIX

# REFLECTIONS ON WALTER TAYLOR

*Carroll L. Riley*

As best I remember, I first became acquainted with Walter Taylor in May 1958, during a meeting of the Society for American Archaeology in Norman, Oklahoma. J. Charles Kelley, director of the Southern Illinois University Museum and acting chair of the newly created Department of Anthropology at Southern Illinois University, had previously approached Taylor and offered him the position of departmental chair, an offer that Taylor eventually accepted. Wanting to take a closer look at his new home to be, Taylor returned to Carbondale with me and two other departmental members who had attended the meeting.

I had come to Southern Illinois just three years before, joining Kelley and Charles H. Lange. Howard D. Winters was added to the faculty a short time later and we formed the anthropology group at the time Taylor joined us.[1] Lange, Winters, and I were originally associated with the University Museum but taught courses as part of SIU's Department of Sociology, a very unsatisfactory arrangement because our interests and those of our sociological colleagues were quite far apart. From the first, therefore, there was the plan to form an independent Department of Anthropology. The university had originally wished Kelley to take over chairperson duties as well as direct the museum, but Dr. Kelley, heavily involved in research, chose not to go that route. It was felt that a senior person was needed (both Lange and I were young assistant professors, only a few years out of graduate school, and Winters was still a graduate student); hence, Taylor was hired. Under his direction, the Department of Anthropology at SIU grew rapidly. In 1960 we initiated a Ph.D. program, and by the early 1970s we had a faculty of some eighteen people of whom twelve were full-time. A great deal of this growth was because of Taylor's direction.

A year or so after Taylor's arrival, he and I launched what was to be our only official scholarly collaboration, a festschrift volume for the anthropologist Leslie Spier. Spier had been my major professor in my graduate years at the University of New Mexico. Although Taylor had never studied under Spier, Lyda Taylor, Walt's wife, had worked closely with him in earlier days. In a sense, this was to be a tribute to Lyda as well as Spier, for both died during the early days of the project. Work on this book went on sporadically for a number of years. It virtually collapsed in 1963–1964 when Taylor and I were in Europe but eventually the project was revived and published in 1967 under the title *American Historical Anthropology*.

Taylor was a stimulating person to be around and he and I shared certain interests, one being European archaeology, particularly the Bronze and Iron ages in Western Europe. I still remember with pleasure the "bull sessions" that Walt and I engaged in and the stimulating disagreements that, I think, enriched us both. Although we published only one book together, we collaborated informally on a number of other projects. Taylor gave departmental backing to the formation of an SIU Irish Studies Committee, an organization in which I initially played a major role. In 1967, he was also supportive when I, along with Thomas Kinsella, the translator of the Irish Iron Age epic *Tain Bó Cualne*, and several Irish archaeologists, attempted to initiate an archaeological and historical study of an Irish west country Iron Age tomb complex.

The Riley and Taylor families also interacted a great deal socially, especially in the early years, and my wife and I had a warm relationship with both Lyda and Nancy, Walt's second wife. Outside of family, I was a charter member of an intellectual town-and-gown organization, the Quien Sabe? Club, modeled on a similar club to which Walt had once belonged.

Taylor was an orchid grower and established a hothouse near his new residence south of Carbondale. He suffered severe financial loss when on one of his field trips, the caretaker hired to look after his house and outbuildings allowed the orchids to freeze.

As one might expect from an orchid devotee, Taylor liked convivial gatherings, good food, and good wine. He and I considered ourselves experts in martini making and I remember how shocked and horrified both of us were when, returning to the United States from Europe in the mid-1960s, we found that a new fashion, the "martini on the rocks," was sweeping the United States. I recall the two of us indulging in several drinking sessions while bemoaning the fate of a country that would permit such "decadence." In general, we actively worked together during the late 1950s and 1960s, and indeed my most positive and pleasant memories of Walter Taylor date from that period.

In 1963, Taylor gave up the chairship of anthropology, taking the position as a de facto (although not, as I recall, de jure) research professor with a very light teaching load. The chair was taken by Philip Dark, a Taylor recruit, as the next ranking full professor. Taylor, however, continued to exercise a considerable amount of influence in departmental affairs. In 1967, Charles H. Lange became chair, creating somewhat of a break with Taylor and Dark, but the general trend of the department continued much as we had originally put it into place. This included a heavy emphasis on the four major fields of general anthropology and insistence on foreign-language skills for its graduate student body. There was a great deal of "fine tuning" as the years went on, but the major thrust of the department remained the same. In a real sense, it was still the department Taylor had created.

However, certain centrifugal tendencies began to surface in the late 1960s. One was the Vietnam War, which created somewhat of a schism within the department because of conflicting political feelings among the various departmental members. Actually, the senior professors, (Dark, Kelley, Lange, Taylor, and myself) managed to stay reasonably clear of this controversy, but it affected a number of the younger faculty members and graduate students. SIU was a center for opposition to the war in those years. Many of our students, and several of the junior faculty, were involved to some degree in antiwar activities.

A second split had more serious implications, especially for departmental collegiality. It not only caused contention within the Department of Anthropology but also a break with its original parent body, the University Museum. This came about in 1970, when Lange finished his tenure as chair. Following the custom of rotating the chairship among the full professors, I would normally have been offered the position. At this point, Taylor, in collaboration with Dark, decided to challenge the customary rule and request a more junior individual as chairperson. Taylor and Dark managed to enlist certain of the younger staff in their undertaking. This rebel group represented only a minority of the anthropology faculty, but under our voting rules there was a sufficient number to block any given nominee for the chair position.

The move angered a number of departmental members. As I had strong museum ties, it particularly alienated the museum-associated anthropologists. The matter was eventually settled by a clumsy sort of "troika" arrangement with shared chairperson responsibilities. This worked for a year or so in spite of the higher administration's dislike of the idea. It had basically collapsed by 1972, the year that I accepted the position of director of the University Museum. For a number of years, I was only minimally concerned with departmental administration, although I continued to be fully involved in the direction of graduate students, at both the M.A. and the Ph.D. levels. Parenthetically, I did serve a three-year term as departmental chair, but this was several years after Taylor's 1974 retirement from Southern Illinois University.

After 1970, Taylor and I had very little social and only a minimum of professional contact. Still and all, we were on polite terms and continued to be so for the remainder of Walt's life. In later years, when we did meet (usually somewhere in the Southwest), we were friendly and filled each other in on details of family and career.

Taylor's relationship with students was complex. In my opinion, he had a certain insensitivity to some students' academic and personal problems. On the other hand, although Walt was a hard taskmaster, his lectures and tutorials were stimulating and the students certainly learned a great deal. Of course, during that period, Taylor was generally recognized to be at the cutting edge of theoretical anthropology. His doctoral dissertation, which was published in 1948 as *A Study of Archeology*, was one of the strands that led to the later fashion of Processual Archaeology, the "New Archaeology" of the 1960s. In point of fact, Taylor never really embraced processualism, although his influence was generally acknowledged by members of that school. In conversations with me, he expressed dissatisfaction with the processualists' dismissal of aspects of culture history as not being "explanatory." Moreover, Walt considered himself somewhat of a stylist, and he was scornful of the turgid and convoluted writing style affected by the processualists.

Walt published very little in the later years of his career, particularly failing to build on the theoretical and methodological implications of *A Study of Archeology* or to supply the field documentation for that work.[2] I really do not know why Walt contributed so meagerly to the anthropological field after the publication of *A Study of Archeology*. Whatever the reasons, Taylor's career was a bit like a meteor streaking into the atmosphere, with a brilliant initial blaze that seemed to presage a lifetime of major achievements. But what followed was an ever-more-faint afterglow in the academic skies. Nevertheless, Walter Taylor will always be remembered for the brightness of those intellectually exciting early years.

## NOTES

1. Although Melvin J. (Mike) Fowler and Pedro Armillas joined the SIU anthropologists at some early time, both functioning mainly in the University Museum, my memory is that they came sometime after Taylor's arrival. In any case, both Mike and Pedro were in residence by the 1962–1963 academic year, the first year in which the American Anthropological Association published its *Guide to Departmental Offerings*.

2. See Reyman (1999) for a detailed discussion of Walter Taylor's scholarly output.

# PART III

## SOUTHERN ILLINOIS UNIVERSITY: STUDENTS' PERSPECTIVES

CHAPTER SEVEN

# WALTER TAYLOR IN THE 1960S

*R. Berle Clay*

## INTRODUCTION

Although I cannot speak for his generation of archaeologists, for later ones like mine, Walt Taylor has been almost an enigma in spite of his bold statements in *A Study of Archeology* (1948). He was not an easy person to get to know, nor was he one especially eager to talk shop or to advance the ideas he developed in the early 1940s, either in class or out. In the following I try by reminiscence to pull out from the man another view of some of his ideas, because those few of us who were his doctoral advisees probably shared experiences of the man that most did not. I hope that this will help to contextualize the man in the history of American archaeology. However, my method is not to produce an exegesis of his works: that important task I will leave to others.

I first met Taylor in 1963 when I moved from the University of Kentucky to Southern Illinois University to work on my Ph.D. I knew him fairly well as a student (although not as a person or a colleague) for the rest of the decade, but I saw him only irregularly after that. In our first meeting he quickly established that we were both Yale graduates and that paved the way in our relationship. He was then just ending his roles as chair of the new Department of Anthropology at SIU and as head of its graduate program, which, like any new program in the early 1960s, was in search of good students. I appeared to be a decent candidate and he assigned me to himself as teaching assistant, perhaps because of the "old blue" connection, but more likely because I had Southwestern experience with Douglas Schwartz in the Grand Canyon where Taylor had worked with Robert Euler.

## THE UNDERGRADUATE TEACHER

Walt's text for his advanced Introduction to Anthropology course was Ralph Linton's *The Study of Man* (1936). As his assistant, I bought a copy, still in print even though it had been first published almost thirty years earlier. He came to class armed with well-worn note cards, indicating that this was a class he had taught for several years. Moreover, the textbook was probably one of the first texts in anthropology that Taylor had encountered as a student. In retrospect, Linton's book was fairly straightforward and with a good teacher could make for a very interesting class. I recall of that period, however, that the written word in anthropology had a much greater "half life" than it does now when publication is more fast-paced than one could have ever imagined at that time. Consciously then, teachers looked backward in choosing a textbook; now we tend to do just the opposite and reach for the newly minted statement. Still, Walt was a good teacher at the advanced undergraduate level. He was a careful and precise speaker and the book fit his talents, so the students seemed to enjoy the classroom experience. For my class sections I was given Walt's notes to lecture from, and things went smoothly.

The most concrete thing I got out of the Linton text and Walt's comments woven around it was Linton's distinction (1936: 404) between "use" and "function." For Linton "the *use* of any cultural item is an expression of its relation to things external to the sociocultural configuration; its *function* is an expression of its relationship to things within that configuration" (emphasis added). Any distinction between the two was overlooked in the writings of the most vocal of the "New Archaeologists," who had a tendency to mix the two and could speak in the same breath, for example, of the functions of lithic tools and temple mounds—two quite different levels of abstraction. Such a tendency was to the detriment of discourse.

Since those days, I have harped on the distinction between the terms with students and colleagues and have the pleasure to note that some friends feel it is useful (Mainfort and Sullivan 1998: 5) and not meaningless pedantry. I thank Taylor for that, and his careful elucidation of this distinction is also a reflection of the fact that he was a relatively precise thinker, even though he spoke the anthropological language of the 1930s. I am sure he would have been appalled by the linguistic flexibility that has become so characteristic of our writing today. I also view my interests in these distinctions as the long arm of Ralph Linton reaching through the influence of Walt to me and to my friends, and it signifies that Walt certainly considered himself an anthropologist first and an archaeologist second.

## THE GRADUATE-LEVEL TEACHER AND RESEARCHER IN THE FIELD

As a graduate-level teacher, Walt was more controversial. Some students liked him, others did not, and at SIU there was a tendency for some to dismiss him without trying to get to know him. Perhaps this was because of the personal split that developed between Walt and J. Charles Kelley, a split that, to their credit, neither man foisted on his students, but one that was picked up by those students nonetheless. Kelley had behind him the students with Mesoamerican interests—not inconsiderable in numbers at the time—and I have the recollection that they tended not to take Walt's courses and to look askance at his lifestyle and teaching efforts. Often they arrived at their oral exams with little knowledge of the man and a lot of prejudices.

Those of us who took Walt's courses generally got a lot out of them, although they were different from the teaching of most other professors and required a certain independence and initiative on the part of the student. The courses I had with Walt were on the level of "come along with me and we will learn about xyz." I remember two in particular: Southwestern Archaeology and the European Neolithic. The format and approach were great if you were a self-starter, and I got much out of both. Walt's interest in his classes tended to vary; sometimes he did his preparation or "class work," other times he did not. Interestingly, I never had a course from him in method and theory, or the history of archaeology or anthropology for that matter. Although Walt kept "the monograph" in print through various sources throughout his tenure at SIU, I never had it assigned or even discussed in a single course, including those taught by him.

Earlier, I had a fairly detailed and stimulating introduction to U.S. Southwest archaeology with Douglas Schwartz, including fieldwork in the Grand Canyon. Both the fieldwork and the man were formative in my decision to continue in anthropology, for Doug was an engaging teacher and survey in the Grand Canyon was exhilarating even though I was more interested in Midwestern archaeology. Walt's interest in the Southwest stemmed from his association at Chaco Canyon with Clyde Kluckhohn in a region they both apparently loved. The intellectual stamp of the Southwest on Walt's work clearly was created by Kluckhohn, a stamp that is reflected in the latter's blistering critique of Mesoamerican studies (1940). I can only imagine what the two discussed regarding American archaeology and its branches in Mesoamerica and the Southwest.

Some of Walt's ideas on his work in the Southwest are of course reflected in Walt's (1948) critique of Kidder's Southwestern work and Walt's own fieldwork there during the 1930s and later in the Pueblo Ecology Study. In the latter he attempted to find unexcavated sites in the Four Corners region on which to work out the implications of his conjunctive approach. I understand from Bill Adams, the field supervisor for that project, that although several sites were found, the survey did not lead to the excavations Walt had hoped for. This was in part because many of the small, accessible, data-rich Pueblo sites, such as those Kidder excavated earlier, were looted by the late 1940s and early 1950s. Still, this did not dim his interest in Southwestern archaeology, an interest that was enhanced by his "permanent" residence in Santa Fe. Although he maintained an elaborate home in Carbondale, Illinois, he viewed his residence in Santa Fe as his base and had a relaxed teaching schedule that made it possible for him to use it extensively.

Walt adopted an interesting pedagogical technique in his Southwestern course, one that explains a lot about his rigor with regard to attribution and his understanding of the history of American archaeology as practiced in the Southwest. Walt instructed that we provide the skeleton for an intellectual history of Southwestern archaeology. So we began with the most recent summary articles and then built up a reverse citation chain, tracing each contribution back to an earlier one, on many points, large and small, individual works and grand ideas. This project consumed the term and led to several file boxes of cards that we cross-referenced by topic (see Reyman, this volume). It is a method I have used elsewhere to good advantage despite tendencies in Americanist archaeology since the 1960s to see citations largely as a measure of "paradigm" allegiance and not a more revealing record of an intellectual train of thought (see Maca's Chapter 16, this volume). In this vein I am always amused to see Taylor's manifesto cited in the literature. Across the board these citations lack a specific page reference, leading me to suspect that he is being noted almost as an icon, not for any specific intellectual reason and not because anyone has necessarily read his work very carefully, if at all. I think that, much to his surprise, Walt became a symbol for the New Archaeology in the 1960s, particularly through the writings of Lewis Binford. This left Walt to complain privately that no one really read what he wrote because Binford's ideas did not exactly follow his own and because Taylor was not always cited in detail.

Walt's whole approach to the seminar on the European Neolithic stemmed from quite different interests and led off in somewhat different directions. I believe that he was developing an interest in the archaeology of Europe in the 1960s for a variety of reasons. These ranged from his service there in the OSS during World War II and the fact that his second wife, Nancy, also OSS, had lived extensively in Europe to the reality that he enjoyed speaking Spanish and was passable in French (although he never could quite manage German).

His basic understanding of European prehistory was dated and seemed to stem from the Harold Peake and Herbert John Fleure series *The Corridors of Time* (1927–1930), particularly volumes 3, *Peasants and Potters*, and 7, *The Way of the Sea*. First published in England, they were reprinted in the 1930s by Yale University Press, and I expect the scholarly survey of European prehistory may have been one element in his shift from geology to anthropology as an undergraduate. Much later I discovered that Glyn Daniel (1950: 247) regarded the Peake and Fleure volumes, together with Gordon Childe's (1926) *Dawn of European Civilization*, as the works that cemented in place the culture concept in European prehistory. In other words, they were an appropriate starting point for the seminar, even though they seemed dated when read in the 1960s.

As result, however, of using these old volumes in the Neolithic seminar, we read ahead in time rather than back as we had in the Southwestern course. Taylor bought extensively for his library in European archaeology during the 1960s, hoping to amass the sort of excellent bibliographic resource he already had for the American Southwest. At least one point of the seminar was for him and his students (about three of us) to crunch the incoming purchases into some ideas about the development of European prehistory. This put us all in the interesting position of opening boxes of books and journal runs as they arrived from booksellers and gutting them for what they contained. For example, I remember unpacking the *Bulletin de la Société Préhistorique Française* in its muddy-orange wrappers, cutting its pages, and exploring the contents and then doing the same for the *Proceedings of the Prehistoric Society of East Anglia* and for far more obscure journals (occasionally returned to Taylor's agents as too heavily involved with Greco-Roman or ecclesiastical archaeology).

I learned archaeological French and Spanish, and both were useful to me later on. I struggled with German, discovered that Romanian looked like French with a hangover, and that I could sort of fake Italian. I eventually even started my own account at Blackwell's, one of the booksellers used by Taylor. Importantly, I learned that the problems of archaeological interpretation tend to occur and reoccur throughout the world, despite somewhat different national intellectual traditions. This internationalist perspective has always helped me, and I very much owe it to Walt. In short, I found the work I did for Taylor in this class to be immensely stimulating and to have had a long-term effect on me and my approach to archaeology.

The dynamic of the Neolithic seminar was somewhat unstructured, although it did get Walt started on an interest in "la Cultura del Vaso Campaniform" (Beaker culture, not strictly Neolithic). This ultimately led him to take a field trip to visit Spanish museums with my wife, Brenda, and me in tow. For Walt, the reasons for going were multiple and not all of them concerned with archaeology. For one, there was Walt's continued interest in the Spanish language; add to this the fact that Nancy had actually been vacationing in Spain as a young girl in 1936 when the civil war began. So I sensed in both of them a curiosity regarding what "modern" Spain looked like. The Spanish trip was quite an experience as Luis Ramos, the Spanish archaeological assistant, and I had only the French language in common, and Walt for his part spoke to Luis in Spanish and to me in English.

Occasionally, Walt would shift to French, so as not to exclude me (who spoke no Spanish)—a small but kind gesture—and so we rattled on like this, all of us on our linguistic best behavior. Walt's Spanish accent was amusing to Luis, who suggested to me that it reminded him of the Mexican comedian Cantinflas. This is interesting because I recall that for a period in his life Walt had dubbed Spanish onto American films for Mexican audiences.

The research project was fairly straightforward. It was to test the historical theory that those who made Bell Beakers had been an itinerant group of traders and coppersmiths working around the Mediterranean and ultimately up the European Atlantic coast, an idea strongly championed by Peake and Fleure and still accepted in the 1960s. We would visit Spanish museums and get sherd samples, which would be sent to Walt's friend Fred Matson, at the University of Pennsylvania, for analysis. Matson demonstrated, as I remember, that the Beaker sherds fell into regional paste groups, which, although it may not have disproved the itinerant trader hypothesis, certainly suggested that the pots had been made locally.

## THE CONJUNCTIVE APPROACH?

Lewis Binford (1972: 8) observes of Walt's *A Study of Archeology* that his "examples of the 'conjunctive approach' seemed to lack rigor and to demand some of Griffin's magic rather than the theoretical sophistication of White and the rigorous methods of Spaulding." Simply stated, the problem was that Taylor was statistically naïve. His statistics "text" was the 1939 edition of Simpson and Roe, *Quantitative Zoology* (1939). From a much later Harvard student I collected the 1960 edition of the book, now Simpson, Roe, and Lewontin (1960), and this became my introduction to statistics. Based on Walt's devotion to it, I assume that use of the text was some sort of Harvard tradition. As I recall, however, the 1939 edition lacked even the concept of a sample mean. Without the book before me, I cannot say what else it contained or did not, but clearly Taylor had no way to sort out the significant "conjunctions" from the insignificant with even the simplest of statistics from this text. Interestingly, Walt's mentor, Clyde Kluckhohn, was not (given the era) statistically naïve and is reported (Taylor 1973a: 16) to have used the chi-square test in his report with Paul Reiter (Kluckhohn and Reiter 1939) on Bc50-51 in Chaco Canyon (although I have not checked this myself). As Schneider suggests (in Stocking 1996), however, Kluckhohn, although knowledgeable about quantitative analysis to some extent, seems to have been quite intellectually against it. At least this was the case with one side of his complex character, perhaps the one that reacted to the quantitative interests of sociology in general.

Taylor's analysis of artifact distributions, as it evolved through his work with his Coahuila materials (with which I was never involved), was developing into something he called the "Master Maximum Method." (Taylor also referred to this as a "poor man's chi-square.") It was radical for the time in that it viewed artifact distributions as the source of behavioral inferences that the archaeologist would then weave into his ethnographic site reconstructions as he did "historiography" (see below). I remember that at one site, the use of this method involved casting artifact distributions into something like a four-cell distribution (e.g., front of shelter right and left, depths of shelter left and right) and then making behavioral interpretations from the distribution of artifact classes in the cells. The whole was an easy task for c-square and Fischer's Exact Probability testing, but I do not remember that Taylor even suggested testing the distributions that his "method" generated. As a result, he was left with the mind-numbing task of explaining every distribution as significant or having to disregard some as non-significant without an adequate measure to distinguish between them.

Actually, I believe that by the time I knew him in the 1960s, Walt had done so little "conjunctive archaeology" that the problem of how to do it had never really hit him. He never faced what I think remains an important question: when do the laws of statistical probability trump the inferences archaeologists make from their distributions and associations? As an important extension of this, is 95 percent confidence appropriate to archaeological analysis or should it be 99 percent? Here he was in the same boat with most Americanist archaeologists, for the thread of quantitative analysis between Kroeber's early attempts and the 1960s was slim at best, a point that has been made by Albert Spaulding (1985: 307).

At the level of lithic typology (which became my passing interest), Taylor was, again, of little help, although frankly, few senior archaeologists were. This was not the case with ceramic typology, which was well-established: Doug Schwartz had been an excellent teacher. In the 1960s, at the Abri Pataud in France, I fell in with a group of archaeologists working on the Upper Paleolithic under Hallam Movius of Harvard University. Among this group, Nicholas David was the principal innovator in developing a "non-typological" approach (followed closely by Jim Sackett and Harvey Bricker) to the classification of flint tools (Movius et al. 1968). This was developed as an alternative to the Upper Paleolithic typological list developed by Denise de Sonneville-Bordes and its comparative "cumulative graphs" developed by Denise and her husband, François Bordes. The new attribute approach involved the use of bivariate statistics (parametric and non-parametric) to isolate attribute contributions that could then be used for various types of interpretations. Needless to say, I did not get too much help from Walt on the project (although he was my advisor), and I would have liked to have had a firm hand for this difficult work.

Among other things, I also got into the task of fitting the distribution of certain metric measurements against theoretical normal curves in an attempt to identify technological attributes that could be considered to represent cultural “norms” in lithic tool production. Although the effort was generally thwarted by the reductive nature of lithic technology, which tends to produce markedly non-normal, J-shaped distributions of tool measurements, I was unable to explain either my interest or what I was proposing to Walt. In retrospect, this may have been my fault, for I was hardly articulate. However, his idea for dealing with such a distribution curve was to cut the two tails from the middle and make them three types in all—not quite what I had in mind. Part of the problem was simply mechanical in that we lacked the tools to explore the data. Walt was armed with a truly ancient tabletop (Odhner) calculator, later replaced by a tiny, fascinating, handheld Curta calculator (still mechanical). I remember pushing the department at SIU to finally get a Friden mechanical calculator that would extract square roots (after much animated grinding!). Armed with microchip-based computational tools today, we are far better equipped to analyze data than we were in 1963. Taylor with a computer at hand in 1943 would, I expect, have had quite a different impact on archaeology post-1960!

I did part company with my Abri Pataud colleagues in exploring the distribution of artifact classes over the archaeological “couche” that it was my responsibility to write up. Here I was only in part stimulated by Walt’s interest in artifact distributions, which, as I have suggested, was largely unformed when I worked with him. Perhaps a more important influence was the initial report on the Hatchery Site West (see also Binford et al. 1966), completed as an SIU field project and widely circulated in the department.

## ELITES AND RADICALS: TAYLOR AND KLUCKHOHN

In moving from the 1930s, through World War II and the GI Bill, and into the 1950s, a transformation occurred in American archaeology that has not been fully appreciated by those who write of its history. The practitioners multiplied and, because of the class narrowness of the 1930s experience, the discipline began to draw from the middle class in a way it had not done before. This must be kept in mind when assessing Taylor today, because he bridged this transition with an education that began in the 1920s in one “class context” and ended as the 1940s began in another.

I would gather that as a college student (Hotchkiss preparatory school, undergraduate at Yale, graduate at Harvard), Walt was of an elite class and was precocious if not arrogant. His favorite Yale story was the time he took German and drew an utter blank on a written exam in composition. Having nothing to write, he chose to do the exercise in Greek, which he had learned in prep school. The professor later queried him, “[B]ut Mr. Taylor, you realize this is a course in German?” I do not remember how this story ended—that is, whether he passed or flunked the exam—but I believe he passed and that was the point of the story. Walt proudly listed on his vita his first publication (a piece in a hunting and fishing magazine) and his teaching experience in the German POW camp after being captured during Operation Torch in southern France. He actually made a convert to the discipline during that tenure, Philip Dark, an Englishman who had been languishing in captivity since early in the war (see Dark, this volume). Philip and Walt remained firm friends and Walt brought him to SIU, where Philip's interests settled on Oceanic and African art. Finally, when I knew Walt, he still wore his World War II trench coat with its paratrooper's pips, although by then it was a thing of shreds and patches. I think these threads of Taylor's interests and experiences add up to a rather typical upper-middle-class product of the eastern educational establishment, at least for the time period.

By all accounts, Walt's Harvard advisor and mentor, Clyde Kluckhohn, was also precocious and intellectually arrogant; he was also far more complex as both an individual and an anthropologist than Walt would ever become. Still, I am sure that each in the other may have glimpsed a kindred soul (Stocking 1996). Furthermore, Kluckhohn was clearly an elitist, extending his class-based perspective to his preoccupation with anthropology when he asserted that "[a]nthropology developed in the classes; sociology in the masses" (Andrews, Biggs, and Seidel 1996: no. 32926). Kluckhohn was also a product of an eastern prep school (Lawrenceville) and two Ivy League colleges (Princeton and Harvard), in addition to the University of Wisconsin, and bouts of education in Europe (Rhodes Scholar at Oxford and coursework in Germany).

All told, it is clear to me, as many others recognize, that the one anthropologist above all others who left a significant imprint on Walt's anthropological education was Clyde Kluckhohn. Still, having read Walt's homage to Kluckhohn (1973a), I have difficulty figuring out how he saw that influence. Taylor first met Kluckhohn in 1935–1936 (Taylor 1973a: 23) when Walt was finishing his undergraduate career at Yale (Geology A.B. 1935). So I expect that Kluckhohn was instrumental in shaping Walt's graduate interest in anthropology. Over the next several years they interacted in the American Southwest, for that was Kluckhohn's major regional interest at the time and it became Walt's as well.

Perhaps the most critical interaction between them occurred at the University of New Mexico field school in Chaco Canyon. They both served (Taylor 1973a: 24) on the staff there, and by all accounts (Gifford and Morris 1985: 404–407), there was a highly successful dynamic between students and a variety of anthropologists. Some, like Kluckhohn, were perhaps not really archaeologists but nevertheless contributed substantially to the archaeological research (e.g., Kluckhohn and Reiter 1939). In the relationship between Kluckhohn and Taylor, Walt viewed Kluckhohn as an iconoclastic radical (Taylor 1973a: 14), a notion that has been repeated by other students, for example, David Schneider (Stocking 1996).

It was on Kluckhohn's suggestion that Walt followed him to Harvard. Kluckhohn rose rather slowly through the ranks there (perhaps because of his attitude) and through time removed himself further and further from the Peabody Museum and his earlier immersion in Southwestern archaeology. In spite of Kluckhohn's critique of Middle American studies (1940), it was over his initial objections that Walt wrote his controversial dissertation. Walt maintains that he got from Kluckhohn (1973a: 29) "a sentiment for work, for thoroughness and precision, for the value of thought, and the necessity of making thought explicit." But he may not have gotten these with respect to archaeology because Kluckhohn's work in that area was limited. Although Walt may not have been following precisely on the heels of Kluckhohn, he must have picked up the elder man's sheer intellectual aggressiveness. Personally, I do not think this was part of Walt's character. His association with Kluckhohn and the structure and critique found in his dissertation (1943) and his book (1948)—particularly the pointed ad hominem critiques of named individuals—made it difficult for him later on to gain acceptance for his ideas among his fellow archaeologists.

In general, it is difficult for me to try to reconstruct the 1930s and 1940s scene in American archaeology, especially at Harvard. In reading over Kluckhohn's 1940 contribution to *The Maya and Their Neighbors* (Hay et al. 1940), however, I am struck by what a tactless contribution it was to the Tozzer festschrift, however valid it may have been as a critique of prevailing archaeological practice in Middle American archaeological studies. It was a direct attack on the intellectual basis for the Carnegie Institution–funded program in Maya studies, which at that time was *the* premier grant-funded program of archaeological research in the country. It came, furthermore, at a difficult time for the program, because the directorship of the Carnegie Institution of Washington was changing. The new director in 1939, blunt-speaking New England engineer from MIT Vannevar Bush, soon to become Father of the A-Bomb, slashed the archaeological program's funding. This was an action "little short of catastrophic to the field of archaeology in the U.S." (Zachary 1997: 94) and was a reflection of Bush's general antipathy toward the social sciences and of sagging Carnegie support tied to a notable "ramping up" of research and development for the looming war. I am quite sure that Kluckhohn's short piece may have factored into this decision (see Folan's chapter and Maca's introductory chapter, this volume), but as I am ignorant regarding the connections and the timing, I leave it for the exploration of someone interested in pursuing this period in the history of American archaeology.

Personally, I doubt that it was ever Kluckhohn's intention to sink the Carnegie program in archaeological research, but rather it was in part an unintentional consequence of his 1940 paper. The motives for that critique must lie in Kluckhohn's complex character, in the radical side that Walt lacked (Stocking 1996). Indeed, Kluckhohn warned Taylor against continuing the discussion begun in 1940, or at least continuing it in the same or similar vein, in his Harvard dissertation, perhaps in light of Kluckhohn's own experience. But as we are all now aware, that work would include an acerbic assessment of the total Carnegie program that apparently led to a decision in 1948 on its complete elimination (in 1958) from Carnegie sponsorship.

When I knew Walt, he was far from an aggressive intellectual radical, even though he remained a precocious product of his elite class. I admit I never really knew him in the field, for example, in the sort of stimulating context that must have existed during the prewar Chaco Canyon field school; and there was also a significant generational gap between us. By the 1960s, however, it is clear that he had settled into a role that was more a teacher and only infrequently an anthropological commentator (e.g., Taylor 1967a). By then he had developed and pursued many interests, many of them not precisely related to archaeology. Importantly as well, the anthropological "class context" of 1960s Southern Illinois University was far removed from that experienced by Walt during his formal education.

At some point in our association I remember asking Walt what it was like being the advisee of Clyde Kluckhohn at Harvard. His answer, although I cannot quote it in detail and must only paraphrase, provides another possible clue to the difference between the men. He said something like, "[W]ell, I was the advisee of the only Jew in a WASP department [Harvard Peabody]." I did not explore the matter further and he did not elaborate, and I realize now that this reflects the general aversion of anthropologists to the discussion of anti-Semitism in their discipline. Walt's comment is something of a mystery because I have not met one archaeologist (admittedly, I am of a later generation than Walt's) who thought of Clyde Kluckhohn as in any sense Jewish. I was only able to suggest an interpretation of his comment when I discovered David Schneider's recollections of Kluckhohn in his letter to George Stocking (Stocking 1996). Kluckhohn was clearly deeply affected by the shabby treatment Edward Sapir got at Yale (as a practicing Jew, Sapir was denied membership in the faculty club in the late 1930s, after Kluckhohn had joined Harvard Peabody as a curator [Stocking 1996]). I suggest, therefore, that one aspect of Kluckhohn's intellectual radicalism may have been a chip on the shoulder in dealing with the East Coast archaeological elite, which at the time was almost exclusively Gentile (in contrast to the broader discipline of anthropology). Whether this stemmed from a Jewish background or from a feeling of kinship with a distinguished fellow anthropologist I cannot say. Walt's offhand comment might suggest the former.

Walt lacked this essential ingredient—the radicalism—of Kluckhohn's disposition, at least inasmuch as it involved archaeology (understand, I am speaking here of Kluckhohn *only* as an archaeologist, not more broadly as an anthropologist). Still, Walt was obviously stimulated and guided by the validity of the older man's intellectual critique of the Carnegie program, and he carried this over into his similarly valid critique of Americanist archaeology. His endeavor—and Kluckhohn had warned him against pursuing it—left him in a curious position in the class structure of the system that had educated him. In it, his generational peers could look askance at his very real contribution because of his association with Kluckhohn, leaving it for a later generation in a vastly changed postwar academic field with a new “radical” agenda (the “new archaeologists” of the 1960s, most importantly, Lewis Binford) to rediscover him. It is possibly because he had been stung by the implied criticism stemming from his relationship to Kluckhohn that when I knew him, Walt was hardly what I would describe as an intellectual radical in archaeological matters. Certainly, he never continued his critique of Americanist archaeology in his professional work of the 1950s and 1960s, and there are many ways that he could have advanced it—and I, for one, wish he had—even without completing his Coahuila monograph, a mind-numbing task without the benefit of the statistics training he lacked.

## CONCLUSION

Such are my student memories of Walt. Their value does not lie in what they say about the logic or consistency of his ideas or in how they comment on his ability or inability to live up to his own program but in why he wrote as he did and how what he wrote engaged with the profession. In sum, I ask myself, What did I get from Walt? I cannot really say that I got from him a bible in the form of *A Study of Archeology*. After all, we never consulted it in his courses. Nor did I receive a mission to go forth and carry archaeology at least to the level of “historiography” (as he called it, to the mystification of any historian who reads the book today) or to excoriate those who did not. Perhaps I did get from Walt the ability to move from the Ohio Valley to southwest France, to Papua New Guinea (thanks also to my wife, Brenda J. Clay, a cultural anthropologist), and back to the Ohio Valley; to enjoy and benefit from the varied experiences of eastern United States prehistory, the Upper Paleolithic, contemporary New Ireland cultures, an academic teaching career, years in archaeological management, and my current work in cultural resource management specializing in geophysical survey. Like several of Walt’s other students, I expect I have wound up like Walt himself, as something of an intellectual loner for whom the stampeding herds of paradigm fetishists have never counted for much in the face of my own ideas and experiences. Walt was a good model for such an individualistic spirit in the 1960s; in fact, that spirit may have been why he wrote *A Study of Archeology* in the first place.

CHAPTER EIGHT

# YANACONAS

*James Schoenwetter*

When the editors of this volume requested a contribution from me, my initial response was to refuse. After all, why speak ill of the dead? But they convinced me that as the first of Walter Taylor's students to complete a doctorate under his guidance, my memories of that time and our relationship would be of interest. Although our relationship can only charitably be characterized as rocky, I finally acceded to their request. Colleagues of that era, Drs. Gabriel DeCicco, Mathew Hill, Robert J. Salzer, and Phil C. Weigand, will not be surprised that my late wife, Miriam—surely one of the gentle and gracious women of her time—was greatly relieved to learn that Dr. Taylor would not attend the ceremony at which my doctorate was awarded. "Good," she said, "I was wondering how I could hide the shotgun I'd use to assassinate him once you'd received your degree."

This essay is titled with the name of the dittoed broadsheet established by Horacio Calle during the 1961–1962 academic year at Southern Illinois University. *Yanaconas* published graduate student essays on anthropological subjects with the intent of impressing the faculty with the budding professionalism of those who dreamed of anthropological careers. Calle was from Columbia, where the Spanish term *yanaconas* is used to identify Indian "bondsmen." In colonial times, enslavement of Native Americans was technically illegal, but a yanacona could be assigned to work out his lifetime bond as a virtual slave. The title expressed what we students recognized as Walter Taylor's assessment of our professional status and academic ambitions. Publication was abandoned after three issues, however, as it quickly became clear the effort had no impact on Taylor or other faculty.

When I enrolled at Southern Illinois University, I had already invested four years in a self-designed program to fulfill my ambition to be the first professional archaeologist to apply the methods and techniques of pollen analysis to archaeological research. By that point I had developed expertise as a palynologist and had begun studies of the pollen from archaeological sites (Martin and Schoenwetter 1960; Schoenwetter 1960), and I believed the next step should be to develop a solid understanding of archaeological method and theory and a sophisticated appreciation of the history and character of anthropology. My mentor, Paul Stanley Martin of the Field Museum, advised me that Walter Taylor was the foremost expert on such matters and suggested that I study under his supervision and work under his direction.

I soon found that there would be little opportunity to fulfill either goal. When I arrived at the Department of Anthropology two weeks before the beginning of the 1960 fall quarter, Taylor found time only for a short interview. He informed me that if I planned to obtain my doctorate in anthropology I would be required to take all of the classroom credit hours for both the master's and doctoral degrees because he considered that the M.S. I held in botany was valueless and that the graduate coursework I had completed in anthropology during my undergraduate years at the University of Chicago was irrelevant. He also made it clear that he had neither the time nor the intention to supervise students closely, so I should not expect to interact with him more than once every month or two during the half of the academic year he was at Carbondale. He expressed relief at learning that I was married. He could now direct incoming graduate students to my wife and me and would not be expected to socialize with them.

He obviously wished me to feel uncomfortable, preferably awed, in his presence. Perhaps he wished only to clarify the status differences between us, but I sometimes wondered if it was not his way of belying the fact that his round face, pudgy body, and balding pate gave him the appearance of an easygoing uncle. He certainly did not see himself that way. He aspired to be recognized as a "man's man" whose gruffness, machismo, and interests in athletics and blood sports could be valued as much as his intellect.

He did not restrict browbeating to students. A couple of months after classes began, I was in the main office of the department when a near explosion of temper erupted from his office as Dr. Charles Kaut, then an assistant professor of ethnology, exited. Kaut half turned as he left the room and shouted back that he most certainly would *not* agree to settle their differences with fists on the lawn behind the Anthropology Building.

During the first years he taught at SIU, Taylor made it clear that he expected graduate students to teach themselves and was not particularly concerned with providing them direction or conveying his assessment of their efforts. My impression is that he felt his only teaching function at the graduate level was ultimate evaluation. At the conclusion of a course or a program of study his students either had or had not met a standard he considered satisfactory. If the student had not done so, he had failed and was not worth further consideration. If the student had done so, he was not deterred from continuing. I do not remember Taylor ever commenting on the quality of my class performance or assessing my written work with marginal notes. He spoke of using his concept of a "broad B" when grading student efforts. Grades of A were not granted. A grade of B was satisfactory; a C was not.

I took only two courses from Taylor. The History of Anthropology course was the better organized and the class assignments particularly rewarding. We were required to write biographies of two professional anthropologists, one living and one deceased. I chose Fred Eggan and Alfred Kroeber, and I believe completion of those assignments provided much of the information on the essential character of the profession that I sought at SIU. The other course was Anthropological Method and Theory. In my memory, part of the course focused on Taylor's *A Study of Archeology* (1948), but the larger part used Linton's *Study of Man* (1936) as its text. The course was taught once a week as a three-hour lecture/seminar. As I remember, student questions were neither anticipated nor appreciated. My classmates and I found the course confused and confusing, and I believe the best thing about it was the three hours we spent together following the class when we attempted to dissect and comprehend what we had just been told.

It became clear from comments Taylor made in discussing *A Study of Archeology* that he was totally surprised and deeply hurt by the claims that he had made ad hominem attacks on the archaeologists whose work he had examined. He felt he had chosen those authors because their research was of the highest and best quality, and he believed he had made that point as clearly as possible. I think he honestly could not understand why those archaeologists had not been grateful for the compliment he had delivered by selecting their work for analysis.

The first student to take graduate examinations after I arrived in 1960 took an exam related to the program previous to my own. The second was a foreign national, Dhanidar P. Sinha, who came to SIU following eight years of professional experience as an ethnographer in his native India, with the research for his doctoral dissertation already completed. We students considered it a foregone conclusion that he would pass the exams. I was third. Perhaps I had no clear idea of how best to prepare for the General Examinations because I was part of the first generation of students in SIU's doctoral program. In any case, Taylor's advice was as enigmatic as it was brief: "When you can answer the question 'what is culture' for yourself, you'll know it; and at that point you'll be ready to take your Generals."

I do not remember any attempt to analyze my responses to the written portion of the exam as a way of exploring my strengths and weaknesses. I was simply informed that I had passed that exam segment and was advised when the oral segment was scheduled. At the conclusion of the oral segment I was asked to wait in the department office while a decision was made. After waiting twenty-five minutes I asked the department secretary if she would telephone Taylor's office to see if he had news for me. She advised me that Taylor said I could enter his office. When I did, he waved me to a chair and continued for a few minutes more with the paperwork on his desk. When he raised his head to address me, his first words—forever inscribed on my memory—were "Well. Now we know what's wrong with our program."

What did he mean by this remark? Was it a comment on my work? I honestly do not know, as he subsequently informed me only that I had now passed both portions of the General Examinations and could begin preparing for my specials. I believe he meant that my written and oral responses stimulated faculty discussion on the character and effectiveness of the department's doctoral program, but I cannot say for sure. I did not expect him to comment on the adequacy of my preparation for the exam. As I have said, one met his standards or one did not. He had made it clear that meeting them deserved no analysis.

I was told to prepare for my specials exam by developing bibliographies in two subjects of my choice. As chair of my doctoral committee, Taylor would review them and advise me of gaps and problems, and I would then be expected to have total control of the selected literature when examined. Taylor had no comment to make on the bibliography I generated on the subject of cultural ecology. His principal reaction to my bibliography on the subject of North American archaeology was dismay that it incorporated only 150 to 200 references. He had no concern with the quality of my choices, only with the fact that I had no obvious intention to read everything ever written on the subject. When I argued that the quickening pace of archaeological research in North America since 1945 made it impossible to maintain complete control of the literature and I argued that selective judgment was a skill demanded by modern scholarship, he had nothing more to say. His advice was to expand my bibliography beyond my capability and to absorb its contents in the five-month preparation time I had allotted to the task. If I would not heed that advice, he would offer me no other assistance.

Readers of this essay already will have noted that my memories of Taylor's relationship with graduate students are far more negative than several of those students who matriculated later, and who have also written about their relationships with Taylor. Perhaps that was in part a product of the pressures he faced as chairman of a new department in a rapidly expanding academic environment, combined with difficulties in his personal life. Or perhaps there were greater psychological conflicts between Taylor and my cohort than between him and later students (identifying ourselves as yanaconas must have been symptomatic of *some* sort of problem). In any case, to be fair, I should here recognize two occasions when Taylor's advice was sought and, when given, proved significant to the positive progress of my career.

The first was in the spring of 1962. Two professional meetings were held back to back in Tucson: the First International Palynological Conference (IPC) and the Twenty-seventh Annual Meeting of the Society for American Archaeology (SAA). An extended fieldtrip was scheduled in between them to survey palynological sites in New Mexico and Arizona. During the course of the IPC conference, Fred Wendorf approached me with the offer of a full-time position as an archaeological palynologist at the Laboratory of Anthropology at the Museum of New Mexico. But he required that I accept the position immediately or it would be offered to someone else.

I do not think it is possible for me to communicate just how much I wanted to accept that offer. Wendorf was creating the first-ever position in archaeological palynology established in the United States, and what he had in mind was exactly the sort of work I had dreamed of and planned for during the past six years. My wife's health was badly compromised by Carbondale's climate, and I was without prospects for a salaried position after my arrangement with the SIU Museum ended in six weeks. Furthermore, if I took the job, leaving the doctoral program would relieve me of the responsibility of cramming for the General Examinations I was not sure I could pass anyway; and, at the time, affiliation with the prestigious Laboratory of Anthropology was a much better career launching pad than affiliation with Walter Taylor.

Although sorely tempted to accept, I asked Wendorf if I could delay my reply until Walter Taylor and I could get together during the SAA meetings. Taylor sympathized with my recognition that a similar opportunity might never come my way again, but he advised me that my overall career would be better served if I continued my preparation for General Exams in October and specials the following April. As Taylor correctly recognized, completing the degree program multiplied opportunities that were destined to arise as the character of archaeological work and research changed between 1962 and 1970. To his credit, Wendorf respected this decision—in fact, he later told me he had felt guilty for putting me in such a spot. A month later he was able to hire me to take the position temporarily for the summer of 1962, and he held it for me until my exams were completed in May 1963. As it turned out, most of the events of the next few years would not have been different had I interrupted my graduate studies and pursuit of the doctoral degree. Administrative and policy changes at the Museum of New Mexico severely limited my potential for professional success at that institution by 1965.

The second occasion was in December 1966. I had struggled with the second draft of my doctoral dissertation for the previous two months, trying to respond to what I felt were vague criticisms of the first draft. Taylor had not commented on the first draft at all, but he was scheduled to be in Santa Fe (where I was employed) three weeks after receipt of the second draft, and I had a long-standing appointment to see him and discuss the second draft.

"Don't you know what's wrong with this?" he asked. "It needs editing."

"I'm surprised," I replied. "I've never been criticized on my composition skills in any of the term papers or research reports I've written, or even on the chapters I've written for edited volumes."

"Well, normally I wouldn't coddle a student in this way, but I'll edit the first fifty pages of this draft and you'll have to pick it up from there."

In three days he edited those fifty pages down to twenty pages. I did take it from there, and the final draft of the dissertation was less than half the length of the second draft. It was the best lesson he ever gave me. A year later, I asked him how he had learned to write with such fine sensitivity to parsimonious language. "When I was at Hotchkiss," he replied, "I took a year-long tutorial from Thornton Wilder. I had to produce a new 500-word essay for him every week and re-compose the one he'd marked up from the prior week. Maybe that had something to do with it."

Taylor's tone was not ironic or sarcastic but expressed another aspect of his character and our relationship. Hotchkiss was and remains one of the finest—and most costly—private secondary schools in the nation. I believe Taylor never truly recognized himself as a privileged person whose wealth sheltered him from many ordinary life experiences. He had no sympathy for the financial problems of graduate students simply because such things had no reality for him. In any case, he seemed unable to comprehend or empathize with other lifestyles than his own. My urban, Jewish, lower middle-class background, as well as my complete indifference to sport or athletics, must have struck him as totally enigmatic. Thus, I was not fulfilled in my hope that working under Taylor's tutelage would give me insights into the theoretical character and methodological structure of the professional practice of archaeology. However, his example, his personality, and the professional reaction to *A Study of Archeology* taught me two lessons that have structured a good deal of my published research.

The first is that whatever degree of pride one may take in generating an elegant argument and body of evidence, asserting proposals that are likely to prove unpopular can prove too damaging to one's professional reputation to be worthwhile. For example, early in 1963 I completed my interpretation of pollen samples from house-floor and wall-fall contexts from the Mitchell Site, Tracts 15A and 15B at Cahokia, and other sites in the American Bottoms. In the draft report I distributed to the archaeologists in charge of the relevant excavations, I argued that because the "pollen dates" for certain associated ceramic assemblages were identical, some of the ceramic types widely believed to date sequentially were more likely to be contemporary markers of different social groups. This position would not be highly controversial today. But palynological study of site-context pollen samples was practically unheard of in 1963, and standard archaeological practice proceeded from the assumption that variations in ceramic assemblages reflected historical change in the popularity of fashions. Joseph Vogel, who was responsible for interpreting the ceramic inventory from Tracts 15A and 15B, suspected I had created both my data and my argument from whole cloth.

My original purpose in undertaking pollen studies at these sites was to demonstrate the archaeological advantages of comprehending the pollen record associated with Mississippian archaeology. When I realized that publication of my interpretations of the results would create a firestorm of controversy, and effectively *discourage* further archaeological pollen work, I was reminded of the effect that controversy had had on Taylor's work. I had already published the data resulting from my studies (Schoenwetter 1962b; 1963), so I filed the interpretive report away and let the matter drop. Today, in fact, it is generally acknowledged that Ramey Incised is a ceramic type diagnostic of a sociological, rather than a temporal, aspect of the Mississippian archaeological record.

The second lesson is that one should not publicly propose an archaeological method simply because it is logically coherent and *should* be effective. The focus of my career has been the design and development of techniques and methods for exploiting the potential of pollen analysis to solve archaeological problems. They range from originating the technique of sampling floors to obtain pollen records datable by associated tree-ring specimens and ceramic assemblages (Schoenwetter 1962a) to designing the "adjusted sum" technique that allows records from cultural and non-cultural context samples to be integrated into a single pollen sequence (Schoenwetter and Eddy 1964, Schoenwetter 1968) to developing a palynological method for identifying patterns of land-use change at historic sites (Schoenwetter 1990). But I have always presented new methods in the context of a demonstration of their effectiveness. Taylor's example taught me that the proof of a scientific method is how well it works, not how elegant it seems. The logic and philosophy behind the conjunctive approach is reasonably sound. But because he did not attempt to apply it as an aspect of *A Study of Archeology*, Taylor did not realize that the method demands more expertise than a single individual can command, more analysis time than the results are worth, and more calculations than were possible before electronic computing technology was developed. Taylor's example taught me that designing a good method or technique is not sufficient; one must show that it can do the job envisioned for it.

CHAPTER NINE

# WALTER W. TAYLOR

## Prophet, Pariah, and Pioneer

*William J. Folan*

### INTRODUCTION: "EO" VERSUS "AEO"

It is seldom that one writes a Ph.D. dissertation only to spend the rest of his or her life striving to live up to its expectations. Such, however, was the case of Walter W. Taylor, who, in my mind, represents the principal progenitor of modern archaeology. This chapter is a glimpse of Taylor as a friend, teacher and mentor, department chairperson, and a gentlemen scholar. I address his strengths, weaknesses, and, ultimately, his attempt to exonerate himself from being one who did not fully live up to his own goals. At my coeditors' request, the latter half of the chapter discusses how Taylor and his conjunctive approach have influenced my research. The introductory section provides some brief background and commentary on the controversy surrounding Taylor's attack on American arch(a)eology.

Much of what Taylor experienced during his academic life owed to his efforts to turn around American archaeology at a crucial point in its existence, in particular through his book *A Study of Archeology* (1948). His book did this via two avenues. The more well-known was his evaluation of the published works of some of the best-known archaeologists in the United States. The other was his proposal of a scientific method, the conjunctive approach, that begins with something as simple as an idea and then leads to a problem and a protocol for solving it through rigorous analysis.

One aspect of the controversy that has surrounded his book is the question of the degree to which its style of presentation—what some have called ad hominem attacks on major American archaeologists—was Taylor's or Clyde Kluckhohn's. It is reported (Reyman 1999: 683) that Kluckhohn specifically asked Taylor to delete the personal references to men such as Alfred Kidder but that Taylor refused because he wanted to maximize the impact of his critique. As to the degree to which Taylor's presentation nevertheless reflects the ideas of Kluckhohn (1940), I feel it would have been difficult for Taylor *not* to have felt the influence of his mentor and friend. It is well known that Kluckhohn was a fellow fieldworker with whom Taylor frequently discussed archaeology during excavations they conducted in the American Southwest, notably at Chaco Canyon. More importantly, Kluckhohn directed Taylor's (1943) doctoral dissertation, which was the basis of his controversial book. In reality, despite his associations with Kluckhohn, Taylor was very much an independent thinker who, like most of us, was inspired by a number of scholars (see introductory chapter, this volume).

In part, Taylor's conjunctive approach owed its strength to the many ideas previously contributed by friends and colleagues, which became formalized within the dissertation. But Taylor (letter to Folan, November 29, 1983), from his own experience, was not sure whether most archaeologists even understood what he meant by the conjunctive approach. From one perspective, and by his own admission, his writings represented a struggle between those who spelled archeology with an "eo" and those who spelled it with an "aeo," thus distinguishing him and the new generation of archaeologists from the great majority of those who came before. Taylor certainly intended to bring about radical change in our discipline and he did so on his own, albeit with ideas, inspiration, and influences from elsewhere.

For some, the long-term impact of the conjunctive approach on archaeology appears to be of less importance than Taylor's published critiques of several North American archaeologists. However, the extent to which these critiques actually damaged reputations and research programs is something that remains poorly understood. According to J. Eric S. Thompson (personal communication, 1975), Taylor's comments critical of the Carnegie Institution of Washington, for example, were not responsible for its discontinuation as a major presence in Maya studies. William T. Sanders (personal communication, 2004) also informed me that it was not Taylor's dissertation or its subsequent publication that brought about the demise of Carnegie's archaeology division but the lack of an adequate research program for future investigations. On the other hand, Michael Coe (personal communication, 2004; 2006: 114; see also Weeks and Hill 2006: 17) suggests that Vannevar Bush, the director of the Carnegie in the 1940s, had read and taken under consideration Taylor's (1943) unpublished thesis in regard to the future of Carnegie's archaeology program. Coe mentioned that once Alfred V. Kidder turned the Division of Historical Research over to H.E.D. Pollock, the latter pulled everyone out of Kaminaljuyu and the Peten in Guatemala, the heartland of Classic Maya culture, and put them to work at Postclassic Mayapan in Yucatan. This apparently went against the interests of the Carnegie archaeologists who had worked in Guatemala on a prolonged basis. This decision was then followed by early retirements and other similar departures. All in all, this leads me to believe that a combination of factors, beginning with Taylor's published thesis and followed by Kidder's retirement, brought about a loss of the type of leadership essential for the times. The Carnegie's archaeological research was brought to its finale by not rising to the challenge presented by the new realities and the burgeoning of various scientific protocols, including Taylor's.

Doom and gloom were not all that came out of Taylor's dissertation, however. He did write positively about and commended the publications of several archaeologists, many of whom did not work with the Carnegie (see the introduction to this volume) and several of whom *did* work with the Carnegie. Sadly, we do not know their reactions to Taylor's positive evaluation of their work. When I let Taylor know that I thought that much of the Carnegie's pioneering work was not only useful but of great importance, including efforts such as the Kaminaljuyu and Uaxactun excavations, he did not appear to agree with me; however, he did not try to argue me out of my opinion. (Although one could disagree with Taylor, convincing him of one's point of view was another matter.)

Taylor was sufficiently interested not only in praising various archaeologists, but also in being recognized for this praise. The 1967 edition of *A Study of Archeology* includes an index wherein Taylor classifies twenty-four archaeologists with commentary such as, "Adams, J: commended by J. W. Bennett" (Taylor 1967b: 255). Bennett himself was commended by Taylor in his monograph: he states (Taylor 1967b: 89) that Bennett's article "Recent Developments in the Functional Interpretation of Archaeological Data" (1943) says, if in slightly different words, "many of the things that have been and will be said in the present study." Walt certainly did not commend James Griffin, Emil W. Haury, and Alfred V. Kidder. Surprisingly, he also did not commend the work of Lyndon Hargrave, whom he credited as the source for many of his ideas. This is salient because others among his major influences are commended, such as Cornelius Osgood (Kehoe, this volume).

One conclusion that can be drawn is that Walt did not choose to criticize individuals because they were considered the best archaeologists at the time. Rather, he chose well-known and, in some cases, beloved archaeologists simply because they did not live up to his ideals. By selecting Haury, Kidder, and Griffin, he ensured himself a broader audience, including both their admirers and their detractors. Unfortunately, however, Taylor was guilty of overkill. Instead of devoting so much time and space to offer critiques of leading archaeologists, he would have better served his stated cause if he had dedicated himself to a greater demonstration of the benefits of his conjunctive approach.

Another interesting point regarding Taylor's choices for critique and praise is that many of those commended by Taylor were, like Kidder, Mayanists or individuals who carried out some form of Maya research, such as George Kubler, Anna O. Shepard, J. Eric Thompson, Harry Tschopik, George C. Vaillant, and Lawrence Roys. This makes a total of eight scholars, or 33.3 percent of the twenty-four overall, commended by Taylor (1967b) in *A Study of Archeology* (*ASOA*). This seems to indicate that although Taylor did not approve of the work of some Maya archaeologists, he was willing to acknowledge the research efforts of a good many of them.

A final point regarding the female members of our profession is that Taylor commended both Mary Butler and Madeline Kneberg. Butler's (1931) research included Maya dress and ornament, which may have influenced Taylor's (1941a) interest in Maya iconography (Joyce, this volume). However, this does not seem to be a reasonable percentage of the American female archaeologists active in the 1940s (see Reyman 1992, 1994), especially because Taylor omitted mention of women archaeologists such as Bertha Dutton, Florence Hawley (Ellis), and Marjorie Lambert, all of whom he worked with and two of whom were among his field supervisors in the Southwest (Reyman, personal communication, 2007). In fairness to Taylor, he was not alone in his attitude toward the contributions of women archaeologists. Willey and Sabloff in the three editions of their *A History of American Archaeology* (1974, 1980, 1993) do not do much better (Reyman 1992: 76).

## TAYLOR AS A FRIEND AND INFORMAL TEACHER

What follows is what I can remember of the numerous, lengthy conversations I had with Taylor when I knew him as my friend and mostly informal teacher from early 1956 to 1965, both in Mexico and the United States; I consider especially the conjunctive approach, its origins and its acceptance. Walt and I spent many an afternoon drinking beer in his comfortable home in Coyoacan, a suburb of Mexico City, or drinking pulque in places such as Acopilco in the State of Mexico, close to Cuajimalpa and Contadero. We also got together on many a Saturday afternoon in his home in Carbondale, Illinois, while he was a senior member of the faculty of anthropology. In general, Taylor was an excellent conversationalist and drinking partner. He could also hold his liquor, beer, and pulque very well.

Taylor wrote that archaeology is neither history nor anthropology but an autonomous discipline that must have strong ties to anthropology. He frequently talked about his conjunctive approach, and some today are apparently leaning toward a version of it (e.g., Fash and Sharer 1991; Marcus 1995; Bell, Canuto, and Sharer 2004; Golden and Borgstede 2004a). At times some researchers invoke the term without really understanding its origins. I heard an archaeologist read a major paper at the 2003 SAA meetings in Milwaukee, Wisconsin, announcing at the end of his presentation that he was calling his methodology "a conjunctive approach" yet never so much as mentioning Taylor or his work. In fact, a conjunctive approach based in the Maya archaeology of Copan, Honduras, has thrived without (until very recently) any mention of Walter Taylor (e.g., Fash and Sharer 1991; see Chapter 16, this volume). Perhaps present-day archaeologists have simply forgotten Taylor, or perhaps they just never thought to look for the origins of the approach they adopted.

For his part, Taylor left many clues as to his influences, citing numerous scholars and mentors, but I believe there are some whom he never clearly identified. For example, I cannot help but recall Radcliffe-Brown's definition of functionalism as "the contribution of a partial activity to a total activity of which it is a part," a notion that drew the attention of Bronislaw Malinowski. This definition seems to approximate Taylor's (1948: 7) definition of the conjunctive approach as "interrelationships which existed within a particular cultural entity" that seems related to the idea that the whole is the sum of the parts. This places Taylor among the antecedent thinkers leading to general and complex adaptive systems modeling (Trigger 1971).

Taylor emphasized a holistic form of archaeology based on interpretations, which produced constructs rather than reconstructions. He frequently insisted that there were differences between use and function, and shape and form. On a Friday afternoon in 1956, while drinking beer in SEP's, a popular restaurant/bar on Avenida Tamaulipas in Mexico City, I recall Taylor reminding me that "everything starts with an idea," a fairly simple concept, but one that has generated criticism. He was interested in ideas and the reason or reasons for taking a first step, and how these influenced the direction of the inquiry. Again, it is fair to say that Taylor was one of the first, if not *the* first, to develop a self-conscious, interpretive approach to archaeology (see Hodder 1991: 190).

Although I have felt that Taylor was or should have been saddened and disappointed by the resistance to his work and to the backlash he encountered even until 1985 (see Longacre, this volume), it is clear to me that he expressed no regrets. There were few archaeologists at that time who recorded, identified, and quantitatively analyzed and interpreted the cultural materials produced by excavations of public structures, dwellings, caves, rock shelters, middens, camps, and other sites, for example. He knew that what he offered was important and novel, and that time would decide the value of his work.

Taylor may have managed to survive (emotionally) the harsh backlash to his work because he had a wide variety of non-archaeological interests. He was an expert on the raising of orchids, successfully competing with Matsumoto, then the major producer of this much sought after adornment in Mexico City. Taylor was also a reasonably good stage actor, a dubbing artist for films, and a polyglot. Moreover, he was an excellent reciter of Garcia Lorca's poetry, a skilled musician and singer, and a first-class athlete. As his earliest publication attests, he also was an expert fly-fisher and hunter. Taylor once told me that the ideal ambience for him was fly-fishing in a stream with a manuscript in hand.

## TAYLOR THE TEACHER AT SIU AND BEFORE

The following is a summary of my graduate student impressions of Walter Taylor, both personally and professionally. I first met Taylor during a Mexico City College field trip to Yagul, Oaxaca, during the winter months of 1956. Taylor, while traveling around the state with a sociologist colleague, dropped in for a visit to the site followed by a brief chat over beers at one of the local cafes around the town plaza. Among the salient features of this meeting was Taylor's willingness to meet and talk with students and his comments on archaeological terminology in vogue in Mexico at that time (and now). Following this brief encounter, and after my graduation from Mexico City College (MCC), I learned that Taylor was to conduct a class in Maya archaeology during a summer-school course I had signed up for at the Universidad del Sureste in Merida, Yucatan, now the Universidad Autónoma de Yucatán. The late Fernando Camara Barbachano had talked me into this series of courses after talking me out of a master's program in the Escuela Nacional de Antropología e Historia in Mexico City; thus I was introduced, through Taylor's course, to the world of Maya studies. Although Taylor was not too impressed by Maya archaeology in general, he had taken a class on the subject from Alfred Marston Tozzer while studying for his Ph.D. at Harvard University. He based the content of his tightly organized lectures on his extremely complete notes from Tozzer's teachings, as well as on his own copious readings, which qualified him as one of the best-read archaeologists in the United States during the mid-twentieth century.

During that time in Merida, I socialized quite a bit with Taylor, often accompanied by John Goggin, who was searching for material to contribute toward a better understanding of Spanish majolica ware, and Larry Heilman, who became one of Goggin's most promising students at the University of Florida–Gainesville. Needless to say, there was little time for study while we examined the nightlife of Merida. But nearing the end of the course, Walt let me know that if I did well on his exam, he would help me work my way up the academic ladder to become a professional anthropologist. Somewhat to my surprise, I decided to take Taylor up on his offer. I immediately made up my mind to cram for the exam because not only did I want to continue in archaeology but I also felt, as did others, that Taylor deserved this type of response for his considerable effort on our behalf both in and out of the classroom.

In general terms, the Merida experience was enlightening. Walt was considered by one and all a good teacher and, after class, an all-around companion. In the classroom he was a strict disciplinarian who took his profession seriously. He was also renowned as a crack shot with a blackboard eraser at five meters, as anyone caught napping in class would soon discover!

Following the course, Walt returned to his home in the Federal District of Mexico (D.F.). After taking advantage of other courses and field trips offered in Yucatan, my classmate Larry and I were going to work with Goggin in Izamal, collecting still more historic ceramics. But Goggin had come down with a classic case of the gout and was unable to leave his room in the old Posada Toledo in Merida. Therefore, invited by Walt to visit him in Mexico City, we decided to leave Goggin to his misery and to fly away to the Federal District where we spent several pleasant days with Taylor and his family in Coyoacan.

Upon my return to Mexico City College to reapply for graduate school in 1957, I contacted Walt and our friendship continued where we had left off (before my South American trip to [unsuccessfully] mine gold and diamonds in the interior of British Guyana). I saw Walt from time to time over beers and pulque but did not take any courses from him at MCC because he did not then teach there. Walt did, however, lend me his personal copy of *A Study of Archeology*, which made a lasting impression on me.

After completing my graduate classes at Mexico City College in 1958, Taylor asked me if I wanted to return to Merida as part of a Middle American Research Institute, Tulane University, project being directed by the late E. Wyllys Andrews IV at Dzibilchaltun. I had fond memories of Merida and had already visited Dzibilchaltun with the late Prof. Alfredo Barrera Vasquez during the summer of 1956; so I jumped at the chance and left for Merida as soon as I could. Following my work with Bill Andrews at Dzibilchaltun (Folan 1961a, 1961b), I continued in Yucatan as a research archaeologist for Mexico's Instituto Nacional de Antropología e Historia (INAH) during the time Román Piña Chan was director of *Monumentos Prehispanicos* for INAH. During those years, the on-and-off contacts with Walt had begun to have their effect on my thinking with respect to what archaeology should be about. I, therefore, tried to apply some of his concepts to my later INAH work in Dzibilchaltun (Folan 1969). This was after I had finished my commitments with Andrews and after Andrews had sagely shifted his interest to the nearby ruins of Komchen, thus further opening the door to the Maya Preclassic on the Yucatan peninsula (Andrews and Andrews 1980).

After some four years in Yucatan, during which time Walt accepted an appointment at Southern Illinois University at Carbondale, he wrote that he was offering me, for the last time, the chance to study for my master's and Ph.D. at SIU (after his attempts to get me accepted at Harvard and Michigan had not borne fruit). He had made the offer earlier, but I had turned down the opportunity because of field commitments. Although I was not particularly enchanted with the idea of being a student again, I requested a leave of absence without pay from INAH to continue my studies.

I began graduate work in anthropology at SIU in January 1963. Although I did not take any classes from Taylor during my stay in Carbondale, I started off as his research assistant for the first semester. My duties as such were light to nonexistent as I struggled through my first series of classes. In the beginning Taylor was supportive, as were Carroll Riley and another pre-Carbondale friend, Philip J.C. Dark. Regarding my academic training as an archaeologist, I quickly learned that Taylor would not let me take classes within my chosen field of interest, stating that if I wanted to be an archaeologist, I would have to assimilate whatever was essential to my professional goals on my own. Therefore, I concentrated my efforts on learning the ethnology and social anthropology of West Africa, New Guinea, and the Pacific, in general, including the glottochronology of New Caledonia, as well as Early Man studies. Although the content of these courses was not my cup of tea, I feel that I at least learned something from all of them. During this early period of my studies at Carbondale, I found the majority of the SIU faculty members to be friendly and helpful and most of my fellow students bright and amiable. However, this pleasant climate was to change rather quickly.

After the first two graduate students passed their comprehensive exams at SIU, most of us thought things were going smoothly. However, after one of the better students failed to pass his General Examinations, for one reason or another, the department changed its course of direction. At that time there was talk about developing SIU as the "Harvard of the Midwest" and it seemed that an added effort was being made to produce graduates loaded with a great deal of information. This included bibliographies, facts and figures, names, places, and dates, plus similar material in the four basic subdisciplines of anthropology. Along with aspiring to Ivy League status, we were all to become holistic anthropologists who could cover the waterfront of any of the world's continents and islands, both large and small. Although many of us were partially in favor of at least some of these goals, both for the department and its graduates, things got a little out of hand. Some worked day and night attempting to memorize anything and everything ever written on man and his works, including the biographies of most of those responsible for reducing all of this material to print.

We were also led to believe that we should consider ourselves to be in competition with each other. This did not sit well with many of us because, above all else, most of us were friends, such as Gabe DeCicco and Phil C. Weigand, who was to become an award-winning archaeologist working in western Mexico. After a time, some felt they were not studying anthropology but mainly the anthropology of anthropology, without learning how to be anthropologists. We were being judged mainly by our general willingness to conform to a system that, we were later to learn, was not applied evenly across the board. As a result, some of the most respected students also failed their exams. I protested before Taylor and, accompanied by Taylor, I also told the chair of the department that there was something terribly amiss, and that this probably reflected a lack of understanding between the faculty and the student body with respect to our mentors' expectations of us. There had to be something wrong given the kind of people who were flunking out. It almost seemed as though a mysterious virus, similar to crib death, was provoking the premature termination of young scholars.

Although I do not know how long this nightmare lasted, I feel its results did not reflect the wishes of most of the faculty. It was the leadership that had faltered. I sensed that many in the department were intimidated by Walt, something that ultimately was harmful both for the department and for Taylor himself. When I asked Walt if he was trying to make everyone afraid of him during a particularly rough period in the department, he did not reply yet let me know that he did not appreciate the question. Then, when I told him I was not among those who feared him, he replied, "That's probably part of the problem, Willie," thus instilling in me the idea that if I had included fear as part of my academic agenda, I would have had a more successful graduate career at SIU. Fortunately, however, most of us who did not make it through the first time finally passed our exams later on, wrote our dissertations, and went on to become professional anthropologists. But many, it seems to me, did not reach their full potential as scholars. In the end, it appears that the reason behind much of what went wrong at SIU from 1963 to 1965, and perhaps a little longer, can be found within an educational philosophy and practice that tried to force all components to live up to the unreasonable expectations of a very few. Insofar as Walt Taylor was one of the leading exponents of the self-defeating, doomed-to-fail exercise described above, I can only state that he, as a teacher, failed to provide the type of environment essential to develop the true potential of his students and the department.

By the mid-1960s, the Walt Taylor of Merida, Yucatan, and Mexico City had largely ceased to exist. Neither teaching nor learning seemed as enjoyable to him, especially after serious disagreements arose among the faculty while he was chair and after the death in 1960 of his beloved wife, Lyda. Moreover, his cherished orchids, brought from Mexico City up to Carbondale, froze during one of his trips when his neighbor failed to light the heater in the greenhouse. Furthermore, a troubled field project to northern Mexican cave and rock-shelter sites exacerbated the situation, and an injury to his foot also prevented him from playing squash as well as he had previously. In spite of the above, however, I freely admit that much of my development as an anthropologist had its beginnings with Taylor, although perhaps not from Taylor the teacher as much as from Taylor the congenial, generous, understanding, and informative friend. I wish I could be more kind in my recollections, but such would not do justice to the reality of my SIU experience.

## THE LATER YEARS

Following my graduation with a Ph.D. from Southern Illinois University in 1972, Taylor and I maintained sporadic contact by letter, phone calls, and also over beers at a few SAA meetings before the fiftieth anniversary debacle. The following includes excerpts from several letters that Taylor wrote to me in the 1970s and 1980s. They go a long way toward explaining where he had arrived in his life and what contributions he thought he had made to American archaeology. In one letter, two years after his retirement from SIU, Taylor (May 12, 1976) wrote, "Anthropology? What's that? I'm not playing MacArthur and fading away. I cut it off sharp. Keep in touch, con afecto, W." However, this was not the case, for he did soldier on in an attempt to move his Coahuila material closer to publication, ostensibly through the University of Pittsburgh (Euler 1997; Reyman 1997). Some five years later, he (November 29, 1983) related: "I should say that I had one hell of a tortuous, tedious, traumatic time for one and one-half years of intensive dedication and single-minded attention to work in writing my contributions to the Pitt volume [dedicated to the Frightful Cave analysis]. I swore then (and intend to abide by that oath) that it would be my last professional chore. R.I.P., Amen." In 1983 I wrote Walt, telling him that Jonathan Reyman and I were planning a festschrift in his honor (see Reyman's chapter in this volume). In response (December 9, 1983), he correctly predicted,

> Your idea of soliciting and obtaining a paper from the top archaeologists in the trade both past and present may produce embarrassingly little response, or enlightenment even if there is response. . . . In regard to your thought that I might write the Conclusions, my position is that I should not do so and will not do so. Willie, I would just be too bitter and full of what would almost certainly be called sour grapes to make any such contribution either appropriate or of value. I frankly do not see that the first forty years have produced much in the writings of my colleagues or of myself, about which I can be very proud—with, of course, some notable and much appreciated exceptions. Any further appraisal of *ASOA* or the conjunctive approach will have to come, and more appropriately, from someone other than myself. I feel that there has already been too much personal and emotional reaction to *ASOA* and the CA (and too little reaction to my other writings that I have designed, and thought, to be expositions of some of my ideas contained therein) without adding a reciprocal reaction which, in all probability, would be hardly less personal and emotional. As for the future that you say we can expect, I'm afraid, Willie, that from where I sit I can see very little prospect. After all the time from 1948, I very much doubt that even my chapters in the forthcoming (I hope) volume from Pitt will have much effect or change the attitude of our colleagues. In fact, my sense of the future can detect more loudly than anything else the cries of out of date, fuddy-duddy, the golden Marshalltown, etc.

He softened his position somewhat in a postscript in the same letter, saying, "Anyway, thanks for your flattering and well-received idea. I only wish that

I could honestly predict its much-to-be-desired success and participate in it. However, if you want to go ahead with it [the festschrift] despite what I have written above, you will have my blessing and any help (other than writing) you ask for. If you want to drop the idea, you will also have my blessing."

In an attempt to let everyone know that Walt did not go off into the fog, dragging his woes behind him, he wrote (April 9, 1983):

> Down here [ostensibly, Alamos, Sonora, Mexico] life is relatively simple and a damn site more serene [than in Tucson, Arizona]. After more than forty years of feeling guilty whenever I felt the urge to read, much less actually got around to reading, something else but anthropology, I have at long last had the chance to read all those things that I have wanted to read. It's more than wonderful! I work the cool hours of the morning in the vegetable garden; I lay out and supervise the work of my man-about-the place; sometimes I have a little carpentry, a little electricity, a little plumbing to do or have done under my supervision (again: and now I realize the splendidness of training in supervision and in stand-still-hands-in-the pocket work that I absorbed in my time in the W.P.A. and such like organizations; now I know its value, I would not trade that on-the-job training and experience for anything); at least once a week, mostly twice, I play a little game of chance and skill, namely poker, with a bunch of (other) Old Goats; in their proper season, there is fishing, both in the Mar de Cortez and in the freshwater lakes behind the numerous irrigation and hydro-electric dams in the foothills of the Sierra Madre; there is also a five-month season on hunting birds (doves, pigeons, ducks, geese, quail, chacalaca and turkey if one wants to climb); and then, of course, there is *gossip* over the afternoon's drinks—I thought prison was bad in this matter, being a relatively small, ingrown society under considerable tension and nervous trauma; but it hadn't a patch on this place, which doesn't seem to me to have a comparable excuse for its indulgence.

Walt also wrote (January 15, 1984), "[L]ife is pretty damn fine right now and, taking all in all, I feel light as the proverbial bird. Hope and wish the same for you! Un abrazo fuerte, W." The manuscript mentioned by Taylor in his letters was to be published by the University of Pittsburgh but was ultimately published by Southern Illinois University as *Contributions to Coahuila Archaeology, with an Introduction to the Coahuila Project* (1988). Some, such as Bob Euler, thought the manuscript was well written. Others were not so kind. Taylor, for his part, was unsatisfied, and according to Euler (1997; Weigand, this volume), he tried to withdraw it from public distribution, probably because it did not live up to *his* expectations. Some copies, however, did get into circulation (see Reyman 1997).

## TAYLOR'S INFLUENCE ON MY LIFE IN ARCHAEOLOGY

I feel compelled to offer that Taylor's influence on my professional life has been almost continuous from 1956 on. My primary interest in Taylor's conjunctive approach was derived from conversations carried out with him outside the classroom. In general, I remember not only being impressed by Taylor as a scholar and conversationalist but by many of his ideas. For example, I was impressed with the possibility of discovering how a particular cultural moment represented by definable spaces, especially within architectural units, was utilized by the inhabitants of those spaces through the identification and comparison of their cultural contents. This could be within, and especially between, the use and function of a specific area of one room and that of other areas of the same room. This allows one to be able to infer (or construct) activities between these definable cultural areas and those associated with other structures excavated or their environs at a particular site (Folan 1969). To holistically determine the use and function of each room and structure within a household or palace group approaches "the interrelationships which existed within a particular entity" (Taylor 1983: 7) to utilize these interrelationships to construct a sociocultural model of the society they represent, much like what Maca (this volume) refers to as site-specific research. Examples of this approach in my research are emphasized in the following sections.

## DZIBILCHALTUN, YUCATAN

Although I talked with Bill Andrews about applying Taylorean principles to our investigations when I was working with him in Dzibilchaltun (1958–1960), he told me that although he agreed with his friend (and distant cousin according to E. Wyllys Andrews V [personal communication, 2004]), Bill had other interests at the time. These, as I remember, were associated with correlating the northern Maya and their architectural styles with those of the Peten while also trying to determine which of the calendar correlations (11:16:0:00 or 12:9:0:0:0) best fit each and every scenario. Regardless of his quest, Andrews was a stickler on maintaining strict stratigraphic control during all excavations, especially of cultural material found at floor level, to better determine the abandonment date of a building and those sealed below it to date its period of construction. During my later, nine-month INAH project in Dzibilchaltun in 1961–1962 (Folan 1969), I excavated three structures forming what I interpreted to be a patrilocal lineage household built on a platform near the northern limits of the central plaza. I followed a modified Taylorean model emphasizing use and function where I had earlier excavated and restored a temple structure associated with this group—under Andrew's direction (Folan 1961a, 1961b; Andrews IV and Andrews V 1980)—which I later interpreted to be an "ancestral lineage" shrine (Folan 1969; see also McAnany 1995). I had interpreted the spaces within and around these structures as civic, ceremonial, culinary, dormitory, and ceremonial areas based on ceramics and ash recorded at floor level and burials; this may have been one of the first attempts to define prehispanic activity areas and the sociopolitical organization of a habitation group in the Maya area and Mesoamerica (Folan 1969). Two of the rooms in Structures 384 and 385 contained thick middens associated with culinary and other activities whereas, for example, the dormitory spaces were free of both sherds and ash, thus providing a vaulted-over space suitable for resting or recreational purposes.

An especially elaborate (for Dzibilchaltun) crypt and offering, located beneath the floor of a small end room with a bench associated with Structure 385, contained several ceramic pieces, including the figure of a warrior and a zoomorphic pendant field identified as the head of a deer, or *ceh* in the Yucatec Maya language. Accordingly, the occupant of this burial was and is thought to be a prominent member of the Ceh family, buried in front of a throne during the Late Classic period (Folan 1969). Because the ruins of Dzibilchaltun, previously known as Holtun Chable, are within the limits of the ancient regional state of Ceh Pech (Folan 1969; Restall 1997), it is reasonable to assume that some of the members of the Ceh Pech household could be buried there. Once again, this is done by interrelating the various components associated with the structure, the location and contents of the crypt, and the related ethnohistoric documents generally known to Mayanists working in northern Yucatan. The household is considered to represent a patrilocal lineage group because, in addition to considerations of the ethnohistoric data, the elaborate burial described above was probably that of an important male member of the Ceh household.

## COBA, QUINTANA ROO

Our next interpretive effort along conjunctive lines was in Coba, Quintana Roo, an impressive Maya urban center on the Yucatan peninsula that approached both Calakmul and Tikal in size and importance (Folan 1977a [1975], 1977b [1976]; Folan, Fletcher, and Kintz 1979; Folan, Kintz, and Fletcher 1983; Folan et al. 2004). This is where Jacinto May Hau and Nicolas Caamal Canche, besides sharing a good deal of cultural information, identified and plotted 3,579 useful living trees according to their distance from the site center. We associated each tree species with an exploitable fruit, fiber, bark, or resin and then compared them according to their use by the ancient Maya, as related to the sociopolitical organization of Maya cities during the Postclassic. We followed Landa's 1566 ethnohistory, translated and annotated by Taylor's teacher, Alfred Marston Tozzer (1941), and compared it with our data from the archaeological settlement pattern of Coba. For example, the *balche* tree (*Lonchocarpus longistylus*), used to prepare ceremonial drinks, and the *pom* tree (*Protium copal*), used to produce incense, were found in greater numbers close to the ceremonial center of the site with its public buildings than in the outlying areas, which were characterized by smaller structures. There, for example, only 4 percent of the balche trees and 9 percent of the pom trees were recorded. Much to my surprise, Taylor wrote thanking me for having sent a reprint of the article, also uncharacteristically praising me for the effort. He, of course, relied on Latin phraseology, that is, *mirabile dictu* (Taylor letter to Folan July 23, 1979), given his education in the Classics.

## HUAMANGO, ESTADO DE MÉXICO

Our next effort along conjunctive lines was the result of excavations in 1977 and 1978 by my wife Lynda and me in Huamango, situated in the northern part of the State of Mexico in the Municipio of Acambay, currently inhabited by Otomi speakers. Two of these Otomi later became colleagues participating as coauthors of a paper on the communication routes linking Huamango with other sites in the highlands and perhaps the Pacific coast and Guatemala (Aguilar, Julian, and Folan 1981). This project, under the direction of Román Piña Chan (1981), included the excavation of a large palace and another similar but smaller structure. It is here we registered the cultural materials recorded vertically and horizontally in 272 one-meter squares to identify the various activities carried out in each section of the larger palace, once again, to discover the "interrelationships which existed within a particular cultural entity" (Taylor 1948: 7). These included the use of ceramics and the reworking of lithics associated with hearths and entranceways, as well as with the interior and exterior of the smaller structure below and to the south at the edge of the site center (Florey Folan and W. J. Folan 1981). To check our interpretations, we drew a half-scale plan of the smaller house on the floor of one of the rooms in our home in Acambay, where we redistributed the ceramics and other cultural materials recorded by us on the floor, including a hearth within the chalked-in house outline. It was here that our two Otomi colleagues, viewing the same cultural materials, came to the same conclusions that we had earlier: the structure appears to have served as a habitation with several different activity areas, including ceremonial, culinary preparations, a dormitory, pulque making, and even lithic production (Florey Folan and W. J Folan 1981). We also carried out a survey and surface collection of a 14 $km^2$ area around Huamango to determine its settlement pattern and activities related to its maintenance (Folan 1981a).

## CERRITO DE LA CAMPANA, ESTADO DE MÉXICO

In the same area of the State of Mexico, Florey Folan, Professor Antonio Ruíz Perez, and I excavated the Teotihuacán garrison of Cerrito de la Campana, situated approximately 140 km northwest of this important center in the predominately Mazahua-speaking municipio of Temascalzingo near the ejido of Aguacatitlan (Folan, Florey Folan, and Ruiz Perez 1987). This project was carried out to determine what the relationship had been between the Teotihuacán pottery-using residents of this garrison and the nearby Otomi and Mazahua communities. We excavated several areas within four structures forming a quadrangle at the base of a definitely ceremonial defensive hill (*mogote*) site with the remains of a megalithic stone wall surrounding the base. In addition to discovering indications of activity areas of both culinary and other domestic functions, as well as a small structure dedicated to ceremonialism, we excavated and recorded two burials with a great many Teotihuacán-type vessels as offerings. We also found several scrapers used to separate fibers from maguey leaves not associated with, but above, the female occupants of the burials (Folan 1989). Scrapers were associated with an Otomi burial in Huamango in context with a spindle whorl (Lagunas R. 1981; Florey Folan, in press) that had been identified by female Otomi speakers as "*raspadores de penca*." Apparently, none of the green obsidian artifacts (mostly blades), probably originating in Teotihuacán, were made at Cerrito but arrived complete, thus reinforcing our garrison-outpost classification. The black obsidian was from Apeo, Michoacan, according to our Otomi-speaking colleagues, and the gray obsidian was from an unknown source, but both were at least worked or retouched locally. This was indicated by the presence of several flakes at floor level in at least one of the structures combined with the absence of cores of any color or provenience.

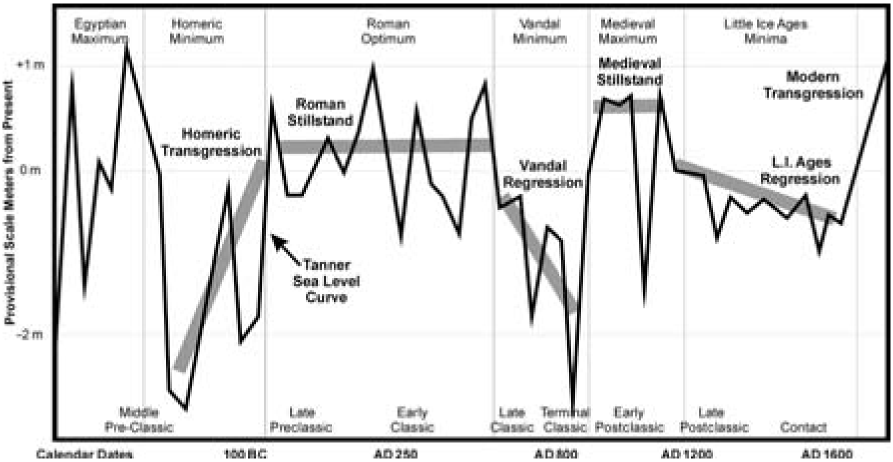
Although Taylor (1948) had not recommended that the "living groups" of the land participate in archaeological interpretations, we had found early on that cultural memory runs deep and that the observations of our Maya, Otomi, and Zapotec colleagues enriched our final results in that they, in a sense, were bearers of at least part of the understanding of the interrelationships within a particular cultural entity. To better understand the activities carried out in each room excavated by us in Cerrito we asked two of our local Otomi-speaking colleagues to estimate, separately, the capacity of all jars excavated in Cerrito based on the shape and size of their reconstructed rims as well as determining the use of each vessel. The results from each informant matched the others in practically all cases because they, or their parents, had depended on ceramic utilitarian vessels for cooking, storage, and pulque production. This enabled us to once again determine some of the use and function of each structure within this household group—again emphasizing "interrelationships which existed within a particular cultural entity" (Taylor 1948: 7).

## PALEOCLIMATE

During a phone conversation with Taylor some twenty years ago, I mentioned that Joel D. Gunn and I were carrying out investigations on the paleoclimate and the sociocultural development of the Maya. Much to my surprise, he told me that this was what we should be doing, perhaps remembering his interest in the paleoclimate of the American Southwest (see Fowler's chapter, this volume).

Although my motivation for participating in climatic analysis stems from what was then a yet-to-be-published article by Gunn and Richard E.W. Adams (1981), Gunn's expertise and Taylor's comments had a positive effect on my efforts along this line of endeavor (Folan 1981b, 1983; Folan, Kintz, and Fletcher 1983). Accordingly, we began formulating a study of climate change in Campeche. Since there were no immediate sources of paleoclimatic data available in the region, we developed a model of modern climate that could be projected into the past.

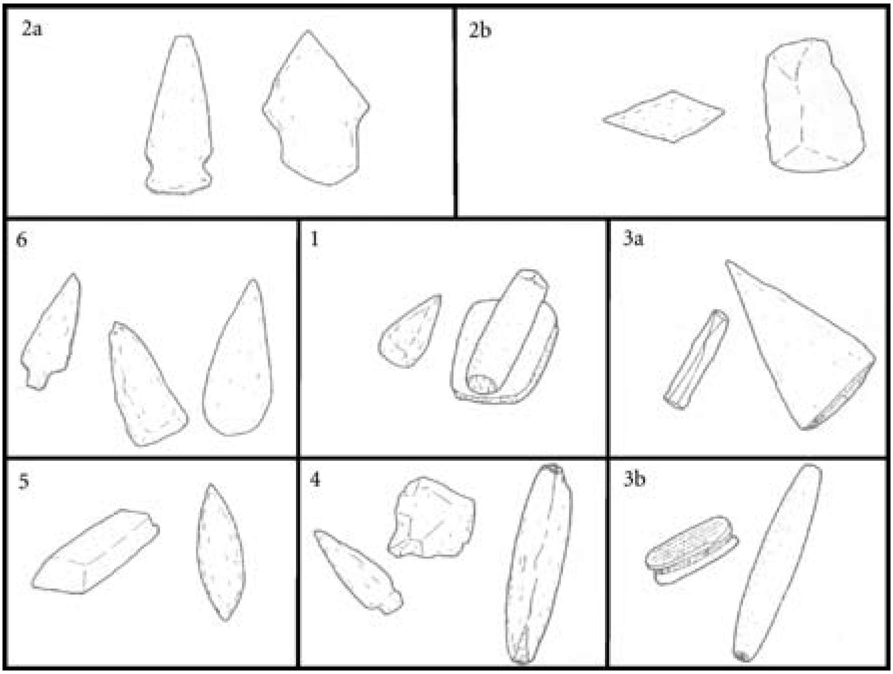
Modern river discharge data were correlated with global annual average temperatures. We then obtained estimates of past global temperatures. Viewing the question of past local climate as a problem of proportions, we were able, with these data, to calculate past runoff as the unknown. We tried this methodology on the Candelaria River with good results (Gunn, Folan, and Robichaux 1994, 1995) (Fig. 9.1). Judging by independent local criteria such as Foss's discovery of an AD 200 drought at El Mirador (Dahlin, Foss, and Chambers 1980), we learned that the model predicted the river discharge, that is, regional precipitation, correctly. The model also predicts the drought correctly, a pattern as yet unreplicated in lake cores from other parts of the peninsula (Hodell, Curtis, and Brenner 1995; contrary to Hodell and colleagues' statement, we did not use pollen data in this analysis). The model also indicates a 200- to 300-year cycle of drought, which we concluded stressed the lowland Maya on a periodic basis, the ninth-century drought associated with the ultimate collapse of the interior urban areas being only a late manifestation of this pattern. It no doubt accounts for some of the belief in fate and prophecy known in Maya cosmology (e.g., Puleston 1979). A 200-year cyclical pattern was later supported by spectral analysis of lake-core sediments (Hodell et al. 2001). This methodology was extended to other rivers in the peninsula (Gunn and Folan 2000), adding considerably to our insights into interregional variations in environment and human impacts on the environment. Adding ethnographic analyses to our prehistoric findings (Gunn, Folan, and Robichaux 1994, 1995; Gunn et al. 2002), we found that rather than temperature (hot and dry) or precipitation (cold and wet), changes in seasonal precipitation were the key to understanding local adaptations to global-local climate change with a balanced wet-dry season regime being most fruitful. Colder global temperatures shortened the rainy season, reducing horticultural productivity. Warmer global temperatures lengthened the growing season, permitting multicropping and other means of extending productivity during just right conditions. When the global temperatures rise greatly, producing overly wet conditions, productivity is suppressed by too long a wet season. Gunn has referred to this as the "Three Bears Model" (Gunn, Faust, and Folan 2006). Similar analyses in river systems from other parts of the world seem to indicate that this pattern is widespread, which requires a general revision of how agricultural productivity and climate variation are understood.




**9.1** Trends in global temperatures from the last 3,000 years estimated from sea levels. The data for sea levels (thin lines) were taken from Tanner (1993). Episodes of stability and change are marked by thick lines. Typical nomenclature for these episodes is applied from the climate change literature (see Gunn 1994). Tanner's sea-level estimates track historically observed climate trends with remarkable regularity, most notably the Vandal Minimum, Medieval Maximum, and Little Ice Age (Gunn and Folan 2000).

## CALAKMUL, CAMPECHE

Our latest efforts along the lines developed by Walter Taylor have been associated with an eighty-four-month mapping project and the excavations of several of the most prominent public buildings as well as a palace structure in the ruins of the regional and urban center of Calakmul, Campeche, Mexico. This includes a 30 km$^2$ map containing 6,250 structures (Folan, May Hau et al. 1990; Folan, Fletcher et al. 2001; May Hau et al. 1990; May Hau, Couoh Muñoz, and Folan 2001), analyzed by Folan, Fletcher, and colleagues (2001). We found many ceremonial as well as domestic areas associated with culinary activities, lithic production and refinement, woodworking, yarn and cloth production, papermaking, arrow and atlatl-shaft production, as well as bead making. Many other Terminal Classic period activities represented by tool kits statistically developed by Joel Gunn (Fig. 9.2) were found in association, especially with the fifty-six rooms on the lower 2,000 m$^2$ facade of the huge public Preclassic to Terminal Classic Structure II. It is here we found that primary lithic production took place at the base of the structure, with the later refinements (associated with secondary and tertiary flakes) occurring farther up the facade as well as in the area surrounding the remains of the temple structure and the base that crowned this 50 m high monument (Domínguez Carrasco and Folan 2001; Folan, Gunn, and Domínguez C. 2001; Florey Folan, in press).




**9.2** A series of tool kits extracted from within-room associations of lithics indicates a broad range of domestic, manufacturing, and military activities on the facade of Structure II and within Structure III of Calakmul, Campeche. (*1*) Food preparation, $p = 0.005$; (*2a*) Cutting and whole punching, $p = 0.006$; (*2b*) Weapons preparation, including atlatl darts, $p < 0.001$; (*3a*) Cutting with obsidian, probability not calculated; (*3b*) Bark-paper production, including bark beaters, $p = 0.005$; (*4*) Chiseling and scraping, perhaps engraving, $p = 0.004$; (*5*) Obsidian and snapped bipoints, function unknown, $p = 0.06$; (*6*) Woodworking with adzes and small points, $p = 0.001$ (adapted from Folan, Gunn, and Domínguez C. 2001: 243–244).

Our interdisciplinary efforts in Calakmul and the surrounding region include epigraphy, ethnohistory, linguistics, paleoclimatology, and agriculture (Folan and Gallegos O. 1999; Folan, Domínguez Carrasco, and Hernández 2006). Among our results is the interpretation of Calakmul's Kan emblem glyph (Marcus 1973; Beliaev and Safronov 2002). This allows us to suggest that the great and little descent (Lizana 1893 [1633]) is associated with the departure of the Kanal lineage from Calakmul and the arrival of the Ah Canul from the Peten to Calkini during a period of drought in the northern and southern Peten some 600 years before the arrival of the Spanish (see Roys in Tozzer 1941). About the same time, the Itza, who spoke Yucatec Mayan brokenly, moved up to Chichén Itzá at the same time during the Terminal Classic (R. Adams 2005; Josserand 2007). It is also suggested that the fifty-six small rooms, some with entrances, daises, and niches, excavated and consolidated by us on the lower facade and summit of Structure II in Calakmul, and those excavated elsewhere in the Peten and the Rio Bec region, are not the result of squatters or cribs to hold construction fill but represent a new archaeological culture that we associate with the people later known as the Cehache (Folan et al. 2007a).

## YUQUOT, BRITISH COLUMBIA

There may be other cases involving my research efforts and the conjunctive approach, but the above are the most representative of Taylor's influence on me. Although we attempted to carry out a conjunctive approach during our stratigraphic excavations in Yuquot, a Nootka summer village on the shores of Nootka Sound on the west coast of Vancouver Island, British Columbia, Canada (Dewhirst 1980), we could not. This was because we discovered that the artifact types recorded three-dimensionally and those screened from 231.7 $m^3$ of shell midden material (including 2.5 $m^3$ of artifacts and faunal material from the total deposit spanning some 4,300 years of occupation) were too homogeneous (Folan and Dewhirst 1980). They consisted mainly of fragmentary bone artifacts representing fishhook shanks; mono-points and bi-points; some whaling, sealing, and otter harpoons; as well as cutting and a few woodworking tools. It was simply too difficult to determine specific activity areas after troweling through the midden, beyond interpreting those artifacts associated with the eighteen stone hearths recorded by us. This may have been the result of a problem with the applicability of Taylor's (1948: 96) methodology, "emphasizing the relationship of item to item" to our sample, or of our analysis of the Yuquot material (see Berle Clay, this volume). We do, however, plan to have another crack at it by using factor analysis as applied by Joel D. Gunn (Folan, Gunn, and Domínguez C. 2001) for our later Calakmul work described above.

With respect to the conjunctive approach, some have asked about what we have done in contrast to other colleagues in the Maya area. In reply, I think that our record is noteworthy because of its unique and (then) timely nature. It is *when* and *how* we carried out our conjunctive investigations that are of note; that is, we were doing this in the 1960s. I do not want to suggest that we are participating in a "can you top this?" paradigm. I am only demonstrating Taylor's influence on our research activities over the years and noting that these began early and with a very specific model in mind. Ultimately, the importance of our work will be for our colleagues and posterity to judge.

## CONCLUDING THOUGHTS

In a way, Walt's place in archaeology seems akin to what the Canadian American John K. Galbraith said about economists several years back. He suggested that one should not lead an intellectual parade but then fall into line after the leaders have gone by. Although Walt Taylor was not what we might call a "paradigm builder," and although he set the archaeological establishment against him, he also ultimately launched his and our profession on a new trajectory. It was he who, recognizing an opportunity, chose to be the leader and then pointed the way for the rest of us who would stumble along in his sizeable footsteps. He not only shined his spotlight on the archaeological efforts of a few of the most prominent members of our discipline, mainly as a way to hold them accountable, but more importantly he taught many of us that there were other and better ways of doing archaeology. I suggest that instead of rejecting Taylor outright, a more balanced consideration of his vital contributions is of value. After all, as Walt wrote (letter, April 9, 1983):

> It might interest you to know (and perhaps revive your flagging regard for me as a producing scientist) that the 7th print of *A Study of Archeology* was supposed to appear in time for the SAA meetings later this month in Pittsburgh; of course, it won't make that date, but should appear, with a Foreword by Patty Jo Watson, sometime a bit later, anyway this year, they say, which will be exactly 40 years after Dear Old Harvard accepted the first draft as my dissertation; I like to think of what other archaeological monographs, particularly one dealing with such needless subjects as theory and method, has been "more or less" in print for 40 years—because I cannot think of one!

Quite obviously, neither Walter W. Taylor nor *A Study of Archeology* has been forgotten. This volume serves as ample testament to his and its enduring importance.


## ACKNOWLEDGMENTS

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CHAPTER TEN

# WALTER TAYLOR

## A Stimulating and Problematic Professor

*Phil C. Weigand*

These recollections about Walter W. Taylor are completely personal. I have no notes or diaries that contain material about life at Southern Illinois University (SIU)—those that I do have contain only ideas and examples of my literary aspirations. However, my memories about various faculty members are fairly clear. It is my purpose here to offer a personal evaluation of Taylor as a professor and individual, viewed through the prism of my experiences and the filter of forty years. At best these memories are mixed. Taylor had a problematic and at times volatile personality, and I could never be completely certain where I stood with him. Nevertheless, I attempt to portray Taylor as a man who held influence over the course of my early career as an archaeologist, and who later tried to become my friend. I address how he influenced my career, and how I responded to him in both formal and informal situations.

As a graduate student in the SIU Department of Anthropology during the 1960s, I had enormous luck to work with five first-rate scholars and professors: Pedro ("Pedro") Armillas, J. Charles ("Kelley") Kelley, Charles ("Chuck") Lange, Carroll ("Cal") Riley, and Walter ("Walt") Taylor. They formed a remarkable team that during their early years at SIU was characterized by high spirits, collaboration, and shared goals in research and education. I was fortunate to have been there during the last half of their collaborative phase, and unfortunate enough to have witnessed the team's disintegration into factionalism and personal disputes that were at times quite vindictive. Still, when things went well for us students, they went very well. We had good research opportunities, almost adequate living stipends, easy access to professors for academic questions, and well-prepared seminars that were both stimulating and intellectually demanding.

Of those five, the two with whom I had the most contact and field training were Pedro and Kelley, although Walt's influence was important to me from the outset. I joined the graduate program at SIU in 1962, beginning with an interview with Walt. Coming from an ancient and Classical history background, I then had no idea who he was, nor knew anything about the hard-nosed reputation he sported. I approached the situation as a complete novice in anthropological archaeology, and apparently without the deference that he was used to receiving. Walt was not impressed with my undergraduate training at Indiana University and I learned much later that he had not supported my admittance into the program.

When I arrived at SIU, Walt, then chair of the department, assigned me as his research assistant for my first year, perhaps thinking that he could correct my obvious deficiencies. Walt had an enormous personal library, which he housed in his home on the far outskirts of Carbondale. My task was to help catalogue as much of that collection as I could. He kept very strict account of my hours, but other then that I had little contact with him in his home. Research assistants were allowed access to the kitchen water faucet and a bathroom and that was it. During my first quarter at SIU, I attended a seminar on northwest Mexico's archaeology and ethnography that Walt team-taught with Pedro, Cal, Kelley, and Chuck. It was well taught and informative and I recall it as my first in-depth exposure to an area that today continues to fascinate me. During this first year, of course, I learned of Walt's reputation as the department's tough man (an image he thoroughly enjoyed), although his treatment of me then was always, without exception, courteous if distant.

My second year at SIU was quite different: I had been assigned as someone else's assistant and signed up for Walt's seminar on archaeological theory. The required readings for the seminar were quite varied and ample but featured his classic *A Study of Archeology*. I had learned from Kelley that he had actually completed that work at Harvard prior to World War II, but that the war, during which he had spent time as a German POW, had prevented its publication. I briefly wondered if my own German background might have influenced his apparent coolness toward me during my first year at SIU. Kelley said that that was impossible and I set aside that thought forever.

Walt told the five of us who had signed on for the theory seminar that he expected candid assessments of his 1948 work and of all other assigned readings. We were each expected to select a corpus of work by a single archaeologist and assess that corpus in light of Walt's monograph. I selected the corpus of Paul Martin and John Rinaldo on the Mogollon, mostly because I wanted to learn about that area. I had already read Walt's monograph, using his copy from the home library, which included all the papers he had stuck between its pages. I studied the book carefully when he was not around to check on me. In preparing my first extended seminar commentary, I carefully chose a direct critique of his use of the term "historiography." Using my history background and my recollections of historiography seminars, especially the classes given by John Snyder at Indiana, I expounded from notes for about ten minutes on what I thought historiography really was; I thought that Walt had missed the spirit of that line of investigation. Using Thucydides as my major example, I said that it was not enough just to carefully and comparatively examine sources for what was actually stated, and thereafter simply write "history." In addition, one needed to understand the spirit of the times during which the history was written in order to fathom its social context. Without that context, the facts recorded in a document will not have the background necessary to allow evaluation of their relevance. A minute of silence followed my conclusions. Walt said that he thought he had met most of the criteria that I had outlined, but no one in the class said a word one way or the other. After the seminar, Walt took me aside. I was fearing the worst, but he simply wanted to tell me that his first reservations about me had been allayed with both that commentary and those from the seminar on northwest Mexico the year before. I walked out of the building on a "high." I had, at least for the moment, Taylor's stamp of approval.

Although Walt was interested in the progress of his students, in reality he seemed either far too critical of them or too distant, and at times mixed the two reactions in a way that often produced confusion. In the year that I arrived at SIU, he had only two Ph.D.-level students with the stamina to take from him the best and to ignore the rest as much as possible: Jim Schoenwetter and Berle Clay. By far, Jim received the best treatment in the preparation of his dissertation. The treatment (later) of Jonathan Reyman was at times patently unfair. Walt was very proud of never giving any grades higher than a B—he called it the philosophy of the "broad B." We had other names for it, but a B was the best that most could expect from him. In the times when a C grade could mean the end of your career in the department, gratitude and relief were more often the only outward response to a B.

Walt considered his critiques to be constructive and wanted them to be taken that way; he never meant them to be polemic or personal. I think that he was truly surprised to learn that often they were received in that fashion. For some students, he was simply far too critical, once bringing a young woman to tears during one of his seminars. In general, his few female students seem to have received a disproportionate amount of criticism, reinforcing to many his mostly unspoken views that archaeology was meant to be a male profession.

Taylor was a firm believer in the “macho” image of maleness. He had struggled hard as a youth to overcome the disability of a clubfoot. As part of this struggle, he attempted to excel at every physical endeavor. This included the ability to drink and to do field research. Walt viewed women as interlopers in this realm, although outside the classroom he was an exemplar for courteous treatment of females. Taken in context, Walt’s view about women in archaeology was widely held during the 1960s, if clearly waning.

From time to time, Walt would invite a number of graduate students to his home where we were expected to try to outdrink him, without lapsing into obvious drunkenness (a no-no). At the same time we were supposed to out-argue him regarding archaeology. Willie Folan was the best performer at these events, with just the right mix of sarcasm and humor that was truly unanswerable by Walt. He was one of the few people that could provoke in Walt uncontrollable laughter. At these gatherings, perfectly frank comments were permitted, which included the occasional observation that Walt should publish his Coahuila cave excavation materials in a fashion compatible with the standards that he had set in his 1948 monograph. Walt allowed all of this without rancor or bitterness, although he would argue back and forth with passion and conviction. The comments about the always-forthcoming Coahuila cave site report clearly touched a sore spot, but as long as they were made in the context of these back-and-forth exchanges, they were permissible. We were even allowed on these occasions to call him “Walt.” He obviously enjoyed the company of our wives at these events. My wife, Acelia García, a native of Jalisco, engaged him in an ongoing polemic on “gringos y mexicanos,” in Spanish, which he thoroughly enjoyed. During his parties, he was always the gentleman, being courteous, even at times solicitous, concerning our actions, needs, and ideas. Once the party was over, however, it was always back to normal, as if nothing had ever transpired.

In conversation, whether at a party or in his office, there were certain themes that were not to be broached. Walt’s politics, at least at that time, were quite conservative and Republican. Although the religious overtones of today’s party would have offended him, he had strong views about the New Deal and what he considered other “socialistic” themes. My conversations with him on these topics were thus very limited and one-sided: he would expound and I would listen as respectfully as possible, never concurring so completely as to make it too obvious that I just wanted the topic to end. It finally would, although he never permitted a conversation with me to close on a negative or confrontational note. In general, I did not have many conversations with him that touched on politics, although I did inadvertently witness a full-scale blowup between him and Kelley about Franklin Roosevelt.

By the late 1960s, Walt became more and more argumentative with other faculty members, even to the point where some old friendships basically came to an end. At times, personalities overcame truly academic or professional considerations and perspective on these latter concerns became lost in the polemics of the moment. This was especially the case in his collapsing relationships with Kelley and, to a lesser extent, with Pedro. The arguments with Kelley apparently began when Kelley became attached to one of their former students. After Kelley divorced his wife, Walt's criticism of him became more intense. In addition, they differed in their views of the cultural dynamics of Mesoamerica's northern frontier and of the character in antiquity of relationships between Mesoamerica and the U.S. Southwest. How much of these differences were truly rooted in scientific dialogue that had just become too bitter or, as most of us believed, in Kelley's relationship with the former student will never be known. The result was that their collaboration on the northern frontier project suffered. Their disagreements became so intense that when Taylor went into the field in 1965, purportedly to investigate the nomadic side of the Mesoamerican frontier in Zacatecas, he instead chose a sedentary site near Sain Alto to try to disprove some of Kelley's characterizations of the Chalchihuites tradition. Walt's field season did not comply with the terms of research set out in the original National Science Foundation proposal, although Kelley was too much the gentleman to create a controversy out of Walt's professional misstep. The fallout between Pedro and Walt was completely personal. Pedro simply disliked Walt, thinking him pompous and insincere. Walt responded by ignoring Pedro's presence as much as possible. There were true differences in opinion about the academic program per se, but as students we were not privy to the details.

As my interests gradually gravitated toward those of Kelley and Pedro, I could feel the tensions between them and Walt beginning to affect my own decisions. This became much more evident when I worked in the field as an assistant to Kelley during two seasons in the Chalchihuites area of Zacatecas (1963 and 1965) and one season with Pedro on his *chinampa* project in the Basin of Mexico (1965). Just before the latter work, Walt had invited me to join him as an assistant on his project for the Chichimeca frontier, which wound up as an excavation of a modest Chalchichuites Tradition structure near Sain Alto, Zacatecas. I turned down the invitation on the pretext of needing to prepare for my Ph.D. advancement examinations set for later that year. When I accepted Pedro's invitation two weeks later, Walt demanded an explanation. I told him that it was not the Basin of Mexico that had changed my mind but rather the chance to gain experience in the "archaeology in the field" approach, or landscape archaeology, as Pedro called it. This was the strategy that O.G.S. Crawford had so successfully used in England and that Pedro had introduced to Mesoamerican studies. I wanted someday to apply this approach in west Mexico and Pedro was encouraging me to experiment with it under his supervision. I told Walt that it was an opportunity that, given my emerging interests, I could not turn down. He accepted the explanation with good grace. The unspoken half of the reason, however, was that I did not want to work with him in the field as I knew it would endanger my ability to work with both Kelley and Pedro in the future. It was a decision and a rationale that I did not completely enjoy making, nor was I particularly proud of myself at the time.

During the rest of that year, I had little contact with Walt, although he attended my Ph.D. advancement oral examination and approved of the performance with his signature. The formal committee was composed of Pedro, Kelley, Cal, and Chuck, with Robert Adams and Robert Braidwood as the invited outside examiners. The topic was comparative early civilizations, and once again I had the opportunity to use my Indiana University background with profit. I still feel that John Snyder, my mentor there, was actually present in the examination room, if only in spirit. Walt's questions to me during that event were balanced and fair, and his congratulations afterward were warm and sincere. For the last time in Carbondale, he invited Acelia and me to his home. I expected that there would be other students there, but there were just the three of us. It was a very pleasant evening.

Although my relationship with Walt never evolved much beyond cordial and formal, with the exception of the aforementioned and informal "lapses" in his home, I always held him in the highest esteem. His 1948 contribution was the true beginning of the New Archaeology, although others, especially Lewis Binford, have claimed the honor. Walt rarely felt the need to promote himself as the, or even one of the, founders of that movement. Despite his ability to be argumentative and confrontational, he simply was not dedicated to self-promotion. He was what he was, and if others could see it, then fine, and if they could not, then that was fine too. I know that he regretted his inability to, by himself, produce an archaeological site record that would have been a dignified follow-up for the standards of research that he set out for the profession in 1948. When Jonathan Reyman did most of the work to produce the site report, using Walt's notes and having never seen the Coahuila site nor most of its artifacts firsthand, Walt became unjustifiably upset and unhappy with the results. He apparently thought that Jonathan, with the nearly impossible constraints under which he worked, could produce the report on his behalf. Nevertheless, many people, myself included, believed the 1948 book was sufficient to place him in the pantheon of the few true theoreticians in archaeology.

The major lesson that Walt tried to impart to us was that there are only three things that an archaeologist can actually know as "fact" about any type of artifact (using the term in its broadest application); each of these was duly addressed in his monograph. These three facts are provenance, chemical (and/or physical) composition, and morphology. Everything else is based on logical induction and analogy, and hence is strictly and completely interpretative. With the conjunctive approach, the better the facts are understood and documented, the better the interpretations will be. There are various levels of interpretation as well, with the primary levels being the most important; the last level, the psychological one, is the most difficult to achieve. The primary levels of interpretation concern cultural affinities, chronology, and the social implications thereof. The validity of these derived observations, insofar as they are possible at all, depends firmly upon anthropological theory, a contextualized appreciation of the ethnographic data set, and the appropriate use of ethnographic analogy. Said in a different way, an archaeologist is first and foremost an anthropologist; the intellectual tools of that discipline allow one to explore and create an ethnography of the past vis-à-vis the methods and techniques of archaeology. Walt insisted that much of what was being passed off as archaeology was simply "dilettantism" (a term I believe he actually used), since there was no anthropological depth or context, or even a concern with these.

Walt was completely immersed in the structural-functionalism of the time, even to the point of once saying to his seminar that it was not necessary to know the time period during which a site was occupied in order to understand the social and cultural life at a specific site. He later said that this statement was meant to be metaphorical, not literal, and stressed that chronology and regional contextualization of a site always were highly important, even basic. Taylor felt that the British school of social anthropology had provided archaeology with a theoretical approach that was testable in archaeology, albeit at the interpretive level of analysis. Although he had clearly read some of the classics in this field, such as Evans-Pritchard's study on the Nuer and Radcliffe-Brown's on the Andaman, I never felt that he was able to articulate their ideas with his own. In other words, his understanding and use of structural-functionalism seemed superficial. Walt wanted to give his ideas an up-to-date theoretical dressing in social anthropology but apparently did not have the intellectual patience to truly do so.

I completely agree with Walt's commentary on the basic and fundamental anthropological nature of archaeological inquiry, but I also must say that I believe Walt's own preparation in anthropological theory was not very profound. He implicitly sensed and valued the importance and validity of archaeology's dependence on, indeed inclusion in, anthropology, but at times he had difficulty expressing specifics. His contribution was the appreciation of the fundamental relationship between anthropological theory and archaeology rather than his ability to produce specific or detailed examples.

I recall once recommending, in writing, a few new (as of the 1960s) references to Walt that I thought would be not only of interest but helpful in making his arguments even more convincing. A week or so later, he stopped me in the department's hallway and invited me into his office. Once there, after the always attentive and sincere courtesies, he reminded me about how every new generation of scholars and researchers always has a new base of theoretical literature upon which to support their ongoing investigations. Often, much (but not all) of this base consisted of simply restating, with new terminology and jargon, the arguments and observations of the past. Therefore, it was contingent upon every student to thoroughly research the older ideas and look for the connective threads that always link them with those of the present. Walt's commentary that afternoon was not at all dismissive of my reading suggestions. I could see two of the books that I had recommended to him on his desktop to one side. One was open and face down above the other. He stated quite firmly that there are, and should be, conceptual breakthroughs both at the theoretical and database levels. This is the fundamental nature of science through the ages, and will be for the future. I mentioned that I thought that his 1948 work was one of those breakthroughs concerning archaeology, but he disagreed, saying that for it to have been a breakthrough there would have had to exist a follow-up site report. Walt had indeed listened to his critics and recognized the validity of their arguments as they applied to him. He said that the social and cultural context of investigation was indeed a fundamental part of historiography. With this conversation, I recognized that he had indeed understood the spirit of historiography, even though it took him years to offer me his measured rejoinder. My respect for him after those two hours in his office grew still more.

In sum, although Walt had acquired a reputation for being confrontational, and at times vindictive, he was never that way with me. After the SIU administration granted him half years off-campus, and he spent more and more time away from Carbondale, a bitterness, perhaps with life in general, seemed at times to emerge more strongly than ever. Unhappy marriages, isolation from his oldest and former friends, like Kelley, and an inability to produce the elusive Coahuila site report seemed to feed upon him.

When I left SIU in 1970, I had already lost contact with Walt. In 1980, here in Jalisco, we received a brief handwritten note from him, consisting of a greeting and a simple inquiry about how we were getting along. A year later, we received another similar note. Our last contact with him was in 1982 when I was a visiting researcher at the Arizona State Museum at the University of Arizona in Tucson. By then, Walt was living on the far reaches of the city and invited us out for a visit just before we were to depart for Jalisco. It was a very pleasant evening, touched with a bit of nostalgia. He seemed out-of-touch with the profession, at times hedging his comments with bitterness. He seemed still more to be simply lonely. That evening, he tried his hardest to come over as a friend as well as a colleague. I tried my hardest to reciprocate, I hope successfully, but he will always remain in my memory the stimulating, although often erratic, professor that he was during the 1960s at SIU. In my mind, his voice and expressions are as clear today as they were then.

# PROFESSOR WALTER W. TAYLOR

CHAPTER ELEVEN

*Jonathan E. Reyman*

## PROLOGUE

When Walter W. Taylor died on April 14, 1997, from complications of Alzheimer's disease, so ended the life of one of the more controversial American archaeologists, one of the "great archaeologists," according to Tim Murray (1999).

This chapter recounts my experiences as one of Taylor's three doctoral students, the nature and consequences of our relationship in terms of my early career, and how Taylor's conjunctive approach influences my archaeological research. Part of this essay derives from my obituary of Taylor (Reyman 1997) and much more is drawn from a biographical essay (Reyman 1999). Here I cover some of the same ground discussed in the two earlier papers to provide background context, but I expand on them in several areas, especially my attempt to apply the conjunctive approach in my dissertation research and in later work that grew out of it. Readers seeking more details about Taylor's professional and personal life and a fuller context into which to place this essay and some of the others in this volume should consult the 1997 and 1999 publications, especially the latter, and also Robert C. Euler's 1997 obituary of Taylor. Patty Jo Watson (this volume) draws from the biographical essay for background and explanatory information, and readers wanting the fuller context for her discussion should again consult the 1999 publication.

One advantage of coediting this volume is having access to the chapters of the other contributors for additional information and for comparative and contrastive purposes—my experiences with Taylor versus theirs. This is most revealing when I examine my experience with Taylor in comparison with the other contributors who were also Taylor's students: my coeditor, William J. Folan; Phil C. Weigand; and Taylor's other two doctoral students, James Schoenwetter and R. Berle Clay.

Such access to other contributors' chapters also provides perspective on Taylor's career and person. Consider Patty Jo Watson's essay in this volume, "Walter W. Taylor's *A Study of Arch(a)eology*: Its Impact, or Lack Thereof, 1943–Present." Watson concludes that Taylor had less impact than one might have thought because he "walked away" from his own thesis. I think Watson is right, in part, but in the foreword to the 1968 printing, Taylor (1968b: 2) writes: "What then of *A Study of Archeology* and the conjunctive approach today? It is my hope that they will become more and more accepted as a source of insights and fundamental ideas for a consistent theory of archaeology and an explicit point of departure for modern practice." Nevertheless, Taylor himself did not push the conjunctive approach except in his teaching.

There is also irony here: to the best of my knowledge, with the exception of Alfred V. Kidder's *An Introduction to the Study of Southwestern Archaeology, with a Preliminary Account of the Excavations at Pecos* (1924), no book on American archaeology has remained in print as long as *A Study of Archeology* (1948), wherein Taylor's greatest criticism (in terms of the number of pages) is directed toward Kidder's work with the Carnegie Institution of Washington in the Maya area, at Pecos Pueblo, and elsewhere in the Southwest.

This chapter also expands upon my 1999 essay in that it provides a more detailed discussion of my student-professor relationship with Taylor and my first postdoctoral year, when I was a research associate working with him on the Coahuila report—"the albatross around my neck," as he referred to it in a letter to me and in subsequent conversations we had in 1970–1972. As such, it provides a complementary perspective to the papers by Clay, Folan, Schoenwetter, and Weigand. This also provides some historical perspective on Taylor's teaching career, especially for younger readers, to whom both the man and the academic climate of the time are either unknown or "ancient history." American archaeology has its own culture history, and Taylor is very much a part of it. So let us start at a beginning—not of Taylor's life, but with his arrival at Southern Illinois University at Carbondale.

## SOUTHERN ILLINOIS UNIVERSITY AT CARBONDALE

In 1957, the administration of Southern Illinois University at Carbondale decided to create a department of anthropology. J. Charles Kelley had come to

Carbondale in 1950 as Director of the University Museum, and Charles H. Lange and Carroll L. Riley joined him in the department in 1955. The search for someone to chair the department began and, as the late J. Charles Kelley noted (this volume), “The first candidate for the chairmanship on whom our group could agree was Walter W. Taylor.”

Taylor, however, needed convincing. Kelley, his friend, colleague, and former Harvard classmate, was up to the task, and Taylor joined the department in 1958. The department, as established, was a graduate and research department; the undergraduate program was minimal, at best.

Additional faculty appointments were made in 1959 and 1960: Charles Kaut, George Grace, Philip J.C. Dark (whom Taylor had met in a German POW camp and to whom he taught anthropology; see Dark, this volume), Pedro Armillas, and Melvin L. Fowler. Taylor and his colleagues designed a broad-based curriculum in the tradition of American historical anthropology with significant input from Taylor’s mentor, Clyde Kluckhohn. By the mid-1960s, SIU-C was considered one of the best new anthropology departments in the United States, as Kelley writes (this volume), “largely because of the intellectual capacity and forceful personality of . . . Taylor.” These qualities were much in evidence when I arrived at Carbondale to begin graduate studies in anthropology. I had wanted to be an archaeologist since age seven or eight, and now there was the opportunity to fulfill this wish.

Clay (this volume) notes that Taylor was not comfortable with statistics; indeed, Taylor referred to his “Master Maximum Method” (MMM) as the “poor man’s Chi-square.” Taylor was well aware of his limitations in understanding and using statistics, but he also realized that statistics were increasingly important to archaeological analyses. I do not know whether Taylor or some other faculty member was responsible for the requirement that all graduate students at SIU-C have statistical training, but by the time I entered the program at Carbondale in 1965, statistics were required and computer programming was strongly encouraged (both were taught in the sociology department). Whatever his shortcomings in statistics, Taylor was committed to seeing that students were trained in them and specifically assigned work from Albert Spaulding and others who were well-versed in the application of statistics to archaeological data.

## PROFESSOR WALTER W. TAYLOR

I entered the SIU-C graduate program in September 1965. Taylor had resigned as chair in 1963. A major factor was the death of his wife, Lyda Paz Averill Taylor, in May 1960. Taylor was devastated by the loss of his wife and intellectual partner (Taylor 1948: 10). Indeed, Lyda was a fine scholar in her own right: her book, *Plants Used as Curatives by Certain Southeastern Tribes* (L. Taylor 1940), was republished posthumously in 1978.

Philip J.C. Dark succeeded Taylor as chair, and Taylor then received a combined teaching/research appointment that enabled him to divide his time between Carbondale (two quarters) and Santa Fe, Europe, or other locales where his research interests took him during the other half year. Sabbaticals and other activities meant that he was often absent from campus for long periods of time. In fact, by the time I took my General Examinations in April 1968, I had not yet taken a course from Taylor, although he had occasionally lectured in courses I took from other professors. This was about to change.

Let me preface what follows with a disclaimer. The following discussion of Taylor as a professor is specific to my graduate career. This volume includes contributions by R. Berle Clay, William J. Folan, James Schoenwetter, and Phil C. Weigand, all of whom were students at SIU-C during Taylor's years there. Folan, however, never took a course from Taylor at SIU-C but had done so earlier in Mexico when Taylor held teaching positions at the Escuela Nacional de Antropología e Historia (1955–1958) and Universidad del Sureste in Merida (summer 1956). Taylor's curriculum vitae (August 1, 1975) also lists him as a professor of anthropology at Mexico City College (1955–1957).

It is clear from their essays that in some ways, Clay, Folan, Schoenwetter, Weigand, and I share similar experiences and relationships with Taylor, but that in more ways theirs were substantially different from mine. For example, both Schoenwetter and Weigand note that, in grading students' work, Taylor used the "broad B"—students could not earn an A. Schoenwetter had left Carbondale and Weigand was well advanced in his graduate career when I arrived at SIU-C (fall 1965). Perhaps Taylor had mellowed by the time I took courses from him, starting with a tutorial in the fall of 1968 while working with him in Santa Fe. I doubt that I was a better student than Weigand (I know little of Schoenwetter's academic work; we did not meet until many years later, and then only once), but I earned A's in all three seminars and in the eleven tutorials I took with Taylor from 1968 to 1971, and my recollection is that one or two other graduate students also earned an A in Taylor's seminars. In light of his later academic career at SIU-C, it is worth noting that Folan received an A in the Mayan archaeology course he took from Taylor at La Universidad del Sureste in Merida, Yucatan.

Again, my experience differs from Schoenwetter's and Weigand's. Weigand (this volume) states that Taylor treated me more unfairly than he did Schoenwetter, but my reading of Schoenwetter's paper in this volume and conversations with him do not support Weigand's statement. Weigand, however, was an observer of both Taylor and us, and he may well have been aware of things that I neither saw nor recognized in my dealings with Taylor. Readers of our papers can judge for themselves.

My experience also differs from Clay's and Folan's. I never called Taylor "Walt" until I began working with him as a postdoctoral research associate in the fall of 1971, and Folan has told me on several occasions, most recently on June

14, 2004, that the Walter Taylor I knew was not the man Folan knew in Mexico when he socialized with Taylor and took one course from him. We all change through time, including how we interact with others. Taylor was no exception, although he might not have recognized, as Folan did, how much he had changed, at least in an academic setting.

Taylor taught a hundred or more graduate students during his seventeen years at Carbondale, but he chaired only three dissertation committees—Schoenwetter's, Clay's, and mine—and a half dozen or so master's theses committees, including Folan's. We wanted other Taylor students to contribute to this volume, and had they done so, we would have a fuller perspective of the man. However, our efforts to recruit them were unsuccessful. Several had left anthropology for other careers and expressed no wish or willingness to look back on their years at SIU-C.

Taylor was a brilliant, intellectually exciting, and often inspiring classroom teacher. He taught students how to read material carefully, analyze it critically, dissect an argument, and evaluate conclusions. Rosemary A. Joyce (this volume) notes that in his paper on the Maya ceremonial bar (Taylor 1941a), Taylor was far ahead of his time in his use of a structural approach to the study of Maya iconography. He was also far ahead of his time in his teaching; he anticipated by some two decades and put into practice in the classroom the "deconstructionist" approach to literature that became popular in the early to mid-1980s, as exemplified by the late Jacques Derrida and others.

My first experience with this teaching philosophy and methodology came in Taylor's archaeology seminar Themes in Southwestern Archaeology. The class met one day each week from 2:00 to 5:00 PM. Taylor gave us a syllabus and reading list, and then he told us to choose a book from the list and to provide an example for each of the terms listed in Table 11.1. The definitions of theory, method, and technique were adapted from Kluckhohn (1940). Students who had taken the archaeology proseminar taught by Taylor had already been exposed to this instructional approach; I had not.

The next week we were assigned the same task, but using an article from the reading list rather than a book. The third week, we had to develop our own examples. Simultaneously, and for the rest of the quarter, we also listed and discussed the major themes in Southwestern archaeology by decade, starting in 1880 and continuing up to the mid-1960s, for example, Mexican-Southwest interaction, the time-space continuum, and the use of typology. If one stuck with it, one emerged from the course knowing how to analyze critically the work of archaeologists and with a deepened understanding of the intellectual history of Southwestern archaeology. It was excellent training for our own careers, and I have continued to begin each new research project with a bibliographic search, that is, by preparing a bibliography of books, articles, comments, and so forth on the subject and then systematically reading the materials and taking notes.

Table 11.1. Course handout, Proseminar A505: Archaeology—Dr. Walter W. Taylor

**TERMS AND DEFINITIONS**

**Theory:** A series of concepts, or a conceptual scheme, by which a discipline or individual orders the experiential facts and derived inferences.

**Method:** The systems of means by which empirical data are produced, ordered, analyzed, synthesized, and expressed.

**Technique:** The individual means and operations comprising the methods characteristic of a discipline.

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**Proposition:** An expression in words, a prediction, of an act or thought, either true or false.

**Concept:** An idea or intellectual experience.

**Percept:** An object or sensuous experience.

**Denotation:** The actual meaning, the individuals or instances falling under or indicated by a word; extension.

**Connotation:** The suggested or implied meaning of a word; intension.

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**Premise:** A proposition taken as the basis of an argument or leading to a conclusion; a stipulation.

**Postulate:** A premise to be accepted without proof for the purpose of furthering an argument; the "if . . . then" form is an example of a postulation.

**Axiom:** A self-evident truth. An established principle, which, although not necessarily true, is generally accepted or utilized.

**Precept:** A rule or principle.

**Theorem:** An established principle or law that has been and can be demonstrated.

**Presumption:** A premise for which the evidence has been given. A "logical presumption" is an inference made on the basis of a known or proved fact connected with it; this is often called a "presumption of fact." A "presumption of law" is an inference required by rule or policy, irrespective of proof or logical presumption; a "presupposition."

**Presupposition:** A premise taken for granted or a proposition required as an antecedent.

**Assumption:** A non-stated or suppressed premise.

**Enthymeme:** An argument consisting of only two propositions, the major premise being omitted.

**Corollary:** An inference from an axiom or a provided proposition.

**Inference:** A conclusion drawn from experiential data or premises; inferences are drawn by people; only people and not things "infer."

**Implication:** A corollary or natural inference which is inherent within the data or premise. People do not "implicate" (they "imply"); data or premises implicate.

**Inductive inference:** The drawing of generalized inferences from particular facts.

**Deductive inference:** The drawing of particular inferences from generalized facts or premises.

**Hypothesis:** An explicitly stated but tentative proposition.

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**Immediate inference:** One in which the conclusion follows directly from a single proposition.

**Mediate inference:** One in which the conclusion is obtained by a comparison of two terms which are interrelated by a third or middle term that is associated with both; a "syllogism."

**Syllogism:** Mediate inference; a logical device by means of which it is possible to determine the relationship of two terms to each other on the basis of their respective relations to some third or "middle" term.

*continued on next page*

**Conclusion:** The proposition in a syllogism which is the one supported by the premises; it consists basically of a subject term and a predicate term joined by a "cupola."

**Major premise:** The premise in a syllogism which contains the "major term." The "major term" is the predicate term of the conclusion.

**Minor premise:** The premise in the syllogism that contains the "minor term." The "minor term" is the subject of the conclusion.

**Middle term:** The term that occurs in each of the two premises but not in the conclusion of the syllogism.

**Distributed term:** A term which refers to the entire class which is by the word.

**Undistributed term:** A term which refers only to part of the class denoted by the word.

**Universal premise:** One in which the subject term is distributed.

**Particular premise:** One in which the subject term is undistributed.

**RULES OF THE SYLLOGISM**

(1) A logical syllogism consists of three terms and only three, and these must be used in the same sense throughout the argument.

***Distribution rules:***

(2) The middle term must be distributed at least once in the premises.
(3) If a term is distributed in the conclusion, it must be distributed in the premise in which it occurs.

***Negative premises rules:***

(4) From two negative premises, no valid conclusion can be drawn.
(5) If one premise is negative, the conclusion must be negative.

***Particular premise rules:***

(6) If one premise is particular, the conclusion must be particular.
(7) From two particular premises, no valid conclusion can be inferred.

Computers enhance the process, many library catalogs are now online, the long hours working with card catalogs are rarely necessary, and the frequent paper cuts are not missed.

Taylor taught the course using a Socratic dialogue derived from his days at Harvard (it is still used in the Harvard Law School), and it is a teaching technique that I have found useful when teaching seminars. The Socratic Method requires the instructor to ask a question to which one or more students respond; another question follows in response to the answer given to the first, and the process continues until the students have been brought to a deeper understanding of the issue under discussion. The instructor also comments on the students' answers and then probes with more questions. To be done properly, the instructor must know the material thoroughly, and the students must have read it carefully (I took voluminous notes). A good instructor rarely, if ever, runs out of questions, but the instructor must be so well-grounded in the assigned material as to be able to shift ground in response to the directions that the students' answers lead, if the instructor thinks these directions are worth pursuing. The Socratic Method is a form of intellectual discipline that, in the hands of a skilled instructor, sharpens students' thought processes and can improve their discussion and debate skills, in addition to helping the students (and also the instructor) reach a fuller understanding of the issues.

Taylor added his own twist to the Socratic Method—the Great so what? as he himself called it. As Taylor employed it, So what? had different meanings or references in different contexts. In one instance, he might ask a student, So what? in an effort to elicit a more explicit or detailed answer to a question. In another context, So what? became what seemed to me a sarcastic brush-off, that is, a way of indicating to the student that the answer was irrelevant, beside the point that Taylor was trying to get the student to make, or, worst of all, stupid. In this last context it was belittling. The final use was to prod the student to a deeper explanation and understanding of the issue at hand. When a student's answer in response to a question was met with Taylor's So what? the student was expected to explain further, followed by another So what? and a further explication, and then another So what? and so on.

Some of us found this probing intellectually stimulating, and for those who knew the assigned material, it was an opportunity to engage Taylor in debate. He would try to bully students intellectually, even physically, but those who stood their ground and argued from a basis of empirical information and reasoned interpretation earned Taylor's respect. At least this was my experience in the classes and tutorials I took with Taylor. Again, my experience differs from that of Schoenwetter (above) and others who preceded me at SIU-C and probably from that of some of my classmates (we have no essays in this volume from students in Taylor's courses during the 1971–1974 period). I know, however, that Taylor's interaction with me was *not* unique; at least one or two of my classmates were similarly engaged with Taylor in the classroom.

Taylor taught with humor, some sarcasm, and numerous anecdotes from his own fieldwork, the most interesting of which, for me, were about his participation, while a graduate student, as a field supervisor at Chaco Canyon, where he worked with Clyde Kluckhohn, J. Charles Kelley, Frank Hibben, Paul Reiter, Bertha Dutton, Florence Hawley, and others. His sometimes scathing comments about fellow fieldworkers, and also humorous ones, went largely unappreciated by most of us who did not know the people to whom he referred; the work had taken place some 25 to 30 years earlier. I later heard similar comments about Taylor from those who had worked with him in the field. I have tried to avoid such sarcasm and personal criticism in my own teaching, although I regret there have been occasional lapses. The same is true in my writing vis-à-vis the style of *A Study of Archeology*. Taylor was instructive, although perhaps in ways he did not anticipate; that is, I rarely make the kind of pointed argument toward an individual's work that Taylor made toward Kidder, Roberts, Haury, Griffin, and others. Furthermore, when I argue a theoretical position and an accompanying methodology, I try to provide empirical examples of how the work should be done (e.g., Reyman 1976a, 1995). Whether I have succeeded is for others to judge (see Lekson 1999: 111n9).

There is a scene in the movie *The Paper Chase*, and in the book from which it derives, where Harvard law professor Charles Kingsfield puts a "shroud" (a bedsheet) over James Hart's head and upper body as Hart is seated in class; Hart is a first-year law student who is unprepared for class and the Socratic dialogue between Kingsfield and his students. He is humiliated by the incident but also determined to prove that Kingfield's judgment of him is wrong. Taylor did not "shroud" students, but he sometimes humiliated them in the classroom. Early in my graduate-student days, in response to a question in a class where Taylor was a guest lecturer, I mispronounced both Hohokam and Mogollon (I correctly pronounced Anasazi). Taylor, cuttingly, informed me of the correct pronunciations and then had me repeat them aloud.

Such tactics intimidated most students, a factor, no doubt, in the small number that Taylor advised or otherwise mentored during his years at SIU-C. It humiliated but did not intimidate me and, like Hart, I was determined to prove Taylor wrong in his evaluation of me as a graduate student. Unlike Hart, it took me a year or two, not less than a semester, to change Taylor's initial impression.

Student fear was exacerbated by Taylor's aloofness and his indirect approach to students. This made it difficult for male students, but it was worse for the few women in the program. Taylor made no effort to conceal his view that women did not belong in archaeology. I did not understand this then and still do not today. Taylor never discussed his attitude with me, even in response to my questions. He simply brushed them off. In retrospect, however, it seems to me that Taylor, for some unstated reason(s)—at least to me—apparently did not think women capable of coping with the rigors of archaeological fieldwork, despite the fact that he knew women archaeologists who *were* fully capable fieldworkers, among them several who had served as his instructors or supervisors at Chaco Canyon and elsewhere: Florence Hawley (Ellis), Bertha Dutton, and Marjorie Lambert. Taylor, however, was not unusual in this regard; many male archaeologists of the time felt as Taylor did (Reyman 1994); some still do. Taylor saw no need to conceal his attitude. Sensitivity toward students was not then what it is now. In the 1960s, he certainly had no reason to fear litigation for sexual discrimination, as is the case today.

Taylor rarely asked students to meet with him about course-related issues; rather, his graduate assistant or even another faculty member would inform us that Taylor wanted to see us during office hours. In this regard, he often treated students and some faculty members as if they were servants (at least two other SIU-C faculty members also treated students similarly). This is illustrated by the following story of how I came to work more closely with Taylor in Santa Fe.

The first plateau graduate students had to attain in the SIU-C curriculum, largely designed by Taylor, was achieved through completion, with satisfactory grades, of the General Examinations—some thirty hours of written exams covering all of anthropology: archaeology, ethnology, linguistics, physical anthropology, social anthropology, and the history of anthropology. Questions could be and were asked about materials from anywhere in the world where anthropologists had worked. I took the General Examinations in April 1968 and upon completion left immediately for Acoma Pueblo to do linguistic fieldwork with Joel Maring, an SIU-C faculty member.

At the 1968 SAA meeting in Santa Fe, the late Charles H. Lange, then chair of the Department of Anthropology, informed me that I had passed all the exams with a high A, something no previous student had done. He said that Taylor was impressed and wanted me to prepare for my Special Examination under his direction. If I passed this second exam, I would be admitted to doctoral candidacy and permitted to write a dissertation. Lange said the department would provide a twelve-month fellowship for me to study with Taylor at his home in Santa Fe, where Taylor would be on sabbatical.

Although Lange indicated that the decision was mine, he also seemed to make it clear that I did not have much choice in the matter: Taylor had designated me as a student with whom he wanted to work, just as he apparently had done several years earlier with Clay, Weigand, and Folan (personal communication, June 11, 2004). Curiously, from my perspective, Taylor never said a word to me about this particular matter at the SAA meeting, even though I spoke very briefly with him once or twice during it, nor did he invite me to his house (less than two miles away) to show me where I would be working with him. Apparently, he assumed that I would accept and show up in Santa Fe at the appointed time in September.

I was pleased with my performance on the General Examinations, but given my earlier experience with Taylor, noted above, and the fact that I had never taken a course with him, his request (demand?) that I be assigned as his student surprised me. I mentioned this to Lange and to the late J. Charles Kelley, from whom I had taken seminars, and both assured me that Taylor's indirect, seemingly indifferent attitude and behavior toward me was not personal but typical of Taylor's relationships with students. They also said this was an exceptional opportunity and that I should accept the offer and the fellowship. One selling point they both made was that Taylor owned the finest professional library of any archaeologist in the United States; everything I needed would be at my fingertips. This proved true; Taylor had an amazing library that included the only copy of the entire Peabody Catalogue in private hands. He did not want to be dependent upon a library for his research; he wanted everything available when he wanted it. This extended to journal articles; he had his secretary/librarian cut and file articles on archaeology and other topics of interest from *Science* and other journals that he did not keep in their entirety. Taylor, as bibliophile, inspired me and continues to do so, although I have never been able to devote the financial resources to my library that he did to his.

Why did Taylor choose me? Berle Clay (this volume) notes that in their first meeting, Taylor established a connection with him, and later a relationship, on the basis that they were both Yale graduates. Schoenwetter, by contrast, suggests that, in part, he provided Taylor with a married graduate student who would relieve him—Taylor—of the obligation to socialize with incoming graduate students. For my part, I was born and partly raised in Greenwich, Connecticut, where Taylor lived as a boy and young man. Perhaps Clay and Schoenwetter are correct in their reasoning, but I never thought that Taylor concerned himself with students' backgrounds and personal issues; I doubt that he thought about such matters. I would like to think he chose me on the basis of my performance on the General Examinations, as the late Charles H. Lange indicated, but I do not really know; Taylor never told me why. Perhaps it was Greenwich.

Regardless, I agreed to study with Taylor in Santa Fe, although not without misgivings because of his reputation for difficult professor-student relationships and my earlier in-class experience with him. Indeed, when I returned to Carbondale from the fieldwork at Acoma and the SAA meeting, several classmates told me I was crazy (or words to that effect that cannot be printed here) to accept the offer to study with Taylor. In retrospect, I do not regret having been Taylor's graduate student—he was, in many ways, an excellent, challenging mentor—but the aftermath of *A Study of Archeology* followed me and other SIU-C archaeology students (almost all of whom were, in fact, not Taylor's students but were perceived to be because they were at SIU-C during his tenure there) so that we were sometimes denied the opportunity to interview for university faculty positions. Despite the admonition in Ezekiel (18:20) to the effect that the sins of the father shall not be visited upon the children, they were visited, on occasion, onto Taylor's students by the students of those archaeologists he had criticized in *A Study of Archeology*: Kidder, Griffin, Haury, and others.

I arrived at Santa Fe in September 1968, found living quarters, and made my way to Taylor's house and library. He gave me a key to the house, showed me around the library, and then sent me to purchase the largest USGS maps available for Arizona, Colorado, Nevada, New Mexico, Texas, and Utah, plus a map of Mexico. When I returned (I had to drive to Albuquerque), he told me to prepare a bibliography of the archaeology and ethnology of the Greater American Southwest, which included northern Mexico. Students at SIU-C were required at that time to cover two areas for their Special Examination, for example, archaeology and ethnology, archaeology and physical anthropology, social anthropology and linguistics, and so forth. Alternately, one could take a problem and study it worldwide. Phil Weigand, for instance, chose the development of ancient urban settlements from a global perspective.

Taylor also instructed me, as I compiled the bibliography, to mark the location of *every* site I encountered on the appropriate map. Some six weeks later, I presented the bibliography to him: two file boxes of 3" × 5" note cards—about 2,000 to 2,200 entries. He flipped through them, added a few references, and told me to read everything listed and to take notes. I said "OK," took my card box, read the first card listed (Adair 1944), went to the shelf, got the book, and began to read it and take notes on 4" × 6" cards that I filed, when finished, in a larger box behind each 3" × 5" card.

For the next year I did this. Occasionally, Taylor would discuss with me what I was reading using the Socratic Method. Some of his questions were trivial: "Where is Roosevelt 9:6 and who excavated it?" ("southern Arizona, northwest of Globe, and Emil Haury"), and "What is Paul Martin's middle initial, and what does it stand for?" ("S, for Stanley"). Others were more significant, such as "What are the benefits and limitations of dendrochronology?" (too numerous to list here, and the limitations of dendrochronology, especially the problems with interpreting missing rings, are still being debated). It was excellent preparation for the Special Examination, and excellent training for my own teaching career, to say nothing of becoming adept at archaeological trivia in conversation. It also introduced me to the concept of the seven-day workweek. Taylor, however, was often absent from the library and from Santa Fe, and weeks would pass without any significant interaction between us.

Moreover, as a consequence of Taylor's divorce from his second wife, Nancy, it was necessary for his secretary and me to leave his library in December 1968 or January 1969. Taylor arranged for me to work at the library at the Laboratory of Anthropology in Santa Fe. I appreciated his effort on my behalf, and the move to that library was beneficial in many ways. Not only were the library holdings excellent (although they did not have the Peabody Catalogue, which Taylor did, the major research tool at my disposal), but also the librarian was Mary Bryan, widow of Kirk Bryan, who did much of the geological research for Neil Judd's National Geographic Expedition at Chaco Canyon in the early 1920s. Mrs. Bryan was a kind woman with a wealth of information, especially about early archaeology in the Southwest, and I profited greatly from conversations with her. One downside to this arrangement was that I could not check out books, so at 5:00 PM, my workday there ended. Taylor, however, was willing to lend me books so I could continue to work in the evenings at home, although my interaction with him was almost nonexistent; he was consumed by the divorce proceedings and building a new house for himself.

I passed my Special Examination in March 1970. In celebration, Taylor prepared a scrumptious Chinese banquet (my choice of cuisine) for me and three guests of my choosing, similar to what he did for Phil and Acelia Weigand (this volume) and for Willie Folan, when the latter finished his M.A. (personal communication, June 11, 2004). Walt was a superb chef. Among other things, he taught me much about cooking.

Taylor then became chair of my dissertation committee and helped me to obtain an NSF Doctoral Dissertation Grant. However, when I began writing my dissertation in December 1970–January 1971, Taylor was either in Santa Fe or traveling elsewhere. I did not see him during the six months it took to write the dissertation but communicated with him via the mail and telephone (in those ancient times before PCs, e-mail, and FAX machines). By the time I submitted the dissertation at the end of spring 1971, J. Charles Kelley had become my de facto advisor. Taylor was not present at the defense, a public lecture followed by an hour or so of questions by the committee members present—Kelley, Lange, Carroll Riley, and Frank Sanders (an astrophysicist)—and other faculty, students, and the public in attendance.

## POSTDOCTORAL WORK—1971–1972

Following graduation, I moved back to Santa Fe to work with Taylor for a year, trying to help lift the "albatross from around his neck." We had an NSF grant to write up the Coahuila report: NSF Senior Grant GS-30560: "Frightful Cave, Coahuila, Mexico." In exchange for working with him and postponing taking a teaching position immediately after graduation, Taylor promised he would help secure a position for me. During that year in Santa Fe I wrote and typed almost 1,200 manuscript pages on more than thirty categories of fiber artifacts, analyzed the quantitative data from several Coahuila caves using Taylor's Master Maximum Method (Taylor 1948: plate 3; 1988: 137–140), and with his secretary, Barbara Peckham, prepared some 400 pages of quantitative data on the excavated materials for the empirical tables that were to accompany the final report. These tables contained all the empirical data recovered during Taylor's excavations at the Coahuila caves, work that owed much to his main field assistant, the late Albert H. Schroeder.

I left Santa Fe in September 1972 to take an assistant professor position at Illinois State University. Taylor had not helped me secure a position, as promised. Indeed, he did little in this regard on my behalf; it was Carroll L. Riley who was primarily responsible for my being hired at Illinois State (Reyman 1999: 692). Ultimately, and regretfully, neither I, nor anyone else, including Taylor himself, was ever able to lift completely the albatross from around his neck (see Weigand, this volume), although the Coahuila report, published posthumously (Taylor 2003), provides an excellent example of the conjunctive approach as applied to two categories of artifacts: sandals and sandal ties.

Taylor's first concern, his primary focus, was always himself. A few students received excellent instruction, but they never received what they might have had

Taylor's focus been other than what it was. From Taylor's classroom instruction, and in later one-on-one conversations, I learned to read carefully, critically, and analytically, skills I have kept and appreciated more as time passes. From such reading, I think I am a better writer than I would have been otherwise. Like Berle Clay (this volume), and in contrast to James Schoenwetter (this volume), I also learned from Taylor's example that it is not necessarily a bad thing to be something of an intellectual loner, outside the mainstream of the current archaeological paradigm (but see Lekson 1999: 111n9). One example of this is my argument, built on Caldwell's (1964) concept and first expressed in a paper (Reyman 1970) and then in my dissertation (Reyman 1971), that the Southwest and Mesoamerica were in a symbiotic relationship, part of a large Interaction Sphere. I also learned from Taylor's example in *A Study of Archeology* that an argument, even if well-intentioned but *perceived* as ad hominem, will not likely be given the serious consideration it might otherwise deserve. Moreover, it does not help when one fails to follow through with a demonstration of what one advocates. Finally, Taylor taught me to think radically—"outside the box," in current parlance—and not to be afraid to take chances in the exploration of new avenues toward understanding the past. It was this willingness to pursue, with Taylor's support, what was, in 1970 and 1971, a different approach to the study of Pueblo architecture that led to my discovery of solstice alignments at Pueblo Bonito and astronomical alignments of architectural features at many Ancestral Pueblo and Sinagua sites (Reyman 1971, 1976a). These are significant lessons to take from one's mentor, although in fairness, I also learned many important, but different lessons, from Carroll L. Riley and from the late J. Charles Kelley and Charles H. Lange.

## THE CONJUNCTIVE APPROACH

No discussion of Taylor as a professor, or at least as my professor, can omit consideration of his conjunctive approach. Taylor's courses on archaeological method and theory focused heavily on the conjunctive approach as a method of analysis, and his substantive courses such as Southwest Archaeology invariably brought it into the discussion. I do not recall that he ever specifically required that we analyze archaeological data using the conjunctive approach, but he did ask what kinds of data might be needed to implement it and what we might discover if we used it.

Favorite Taylor questions—such as, How like is like? How different is different? and What conjoins or connects with what?—intrigued and excited my thinking. As noted above, it was Taylor's prompting to think radically that led me to examine the possibility of, and to discover, astronomical alignments of architectural features at the Ancestral Pueblo and Sinagua sites, as discussed in my dissertation and other publications (e.g., Reyman 1971, 1976a, 1976b, 1978).

These same questions led me to pursue the research topic of both ancient and post-contact interaction between Mesoamerica and the American Southwest and to develop the idea (noted above) that the two areas were in a symbiotic relationship within a large Interaction Sphere (Caldwell 1964; Reyman 1970, 1971, 1995). But it was also Taylor's emphasis on critical thinking that sharpened my perceptions about the conjunctions I found, to accept some and to reject others.

For example, when looking for astronomical alignments of architectural features, it is possible to find an alignment for almost anything; among the moon, stars, and planets, an alignment can be found if one looks hard enough. Therefore, one must restrict one's acceptance of an alignment to the parameters that were possible with naked-eye astronomy at the time the site or feature was constructed, not to what is possible now. One must look for patterns of alignments—patterns of what conjoins with what. For the Southwest, one guideline is to use the ethnographic and ethnohistoric data for Pueblo astronomical observations. This is not to say that things have not changed from ancient times to post-contact times; they have, and there are many cases where this can be demonstrated in terms of astronomical practices (Reyman 1987). Nevertheless, starting with what *is* known makes sense, and the Southwest is rich in ethnographic and ethnohistoric records.

Taylor was skeptical of ethnographic analogy, but he did not reject my use of it or my arguments based on it. I also learned to be skeptical and, perhaps more important, not to be afraid to admit mistakes, as the work at Wupatki demonstrates (Reyman 1978). In the end, patterns of alignments were found consistent with Pueblo ethnographic astronomical practices and with known Mesoamerican practices as described in the ethnographic and ethnohistoric records and found earlier at archaeological sites. So a strong case could be built for Mesoamerican-Southwestern interaction (Table 11.2, a revised version of Reyman 1971: 89, 123).

A second example derives from my dissertation (Reyman 1971: 296–297), which was further developed, refined, and published as Reyman 1976b. Here, mindful of Taylor's statement that both behavior and the products of behavior are cultural, but not culture, "that the concept of 'material culture' is fallacious," and that "the term *material culture* is a misnomer" (Taylor 1948: 102), I explored the idea that wall niches in kivas, the products of cultural behavior, have cultural meaning. Kivas conjoin with Pueblo concepts of their emergence from the Underworld, most notably in the presence of a sipapu (place of Emergence or a passageway to and from the Underworld) or in the idea that the kiva itself is or represents a sipapu. Therefore, because kiva niches often contain materials that are used for rites connected to the Emergence, it seemed plausible that the niches might be placed in the walls to reflect directional significance in Pueblo Emergence and oral traditions about migration. So, in accordance with these

Table 11.2. Some Mesoamerican-Southwest Puebloan parallels in astronomy

| *Astronomical Feature* | *Mesoamerica* | *Pueblos* |
|---|---|---|
| Solar calendar | + | + |
| Observance of solstice rise/set points | + | + |
| Directions based on rise/set points | + | + |
| New Fire ritual associated with winter solstice | + | + |
| New Fire ritual associated with vernal equinox | + | + |
| Lunar phases | + | + |
| *Timing Stars and Constellations* | | |
| Aldebaran | + | + |
| Cassiopeia | + | - |
| Castor and Pollux | + | + |
| Galaxy (Milky Way) | + | + |
| Orion's Belt | + | + |
| Pleiades | + | + |
| Polaris | + | + |
| Procyon | - | + |
| Scorpio | + | - |
| Sirius | + | + |
| Ursa Major (Big Dipper) | + | + |
| Ursa Minor (Little Dipper) | + | + |
| Venus as Morning Star | + | + |
| Venus as Evening Star | + | + |

N.B.: This list is not exhaustive and is generally limited to those astronomical features that can be definitely identified for both areas. Thus, Central Mexican constellations such as *Mamalhoatzli* ("Fire Drill") and the Zuni *A'chiyala´topa* ("Knife Wing") are not included because they have not been defined in terms of specific stars or other celestial features.

traditions, one might expect to find most niches in the north, the most important direction, followed in decreasing frequency by niches in the east, the west, and the south.

To test this hypothesis, it was necessary to define what is meant, in Pueblo terms, by north, east, west, and south and apply these to the kivas. Again using Pueblo concepts, north was defined as the sector between the summer solstice sunrise and sunset; east as that between the winter and summer solstice sunrises; west as that between the winter and summer solstice sunsets; and south as that between the winter solstice sunrise and sunset. Adjustments had to be made for the fact that the north and south sectors were physically larger than the east and west ones. When the distribution of niches was plotted for several hundred kivas studied during fieldwork, with the different sizes of the north and south sectors controlled for, the statistical analysis of the distribution confirmed the hypothesis. The distribution of niches conforms to directional significance as described in the oral traditions (Reyman 1976b).

These are two examples of how Taylor's conjunctive approach helped to inform my research and provided positive results. However, things did not go as well for Taylor in his own work, specifically his attempts to produce the Coahuila report, to lift the albatross from around his neck. By the time we worked together on the report (1971–1972), Taylor had come to realize that the only possible way to apply the conjunctive approach to his Coahuila materials was through the use of computers and statistical analyses.

Taylor knew that he did not have the required computer and statistical expertise to carry out such analyses. Although I had written a computer program (in Fortran IV) to search for and to find astronomical alignments of sites and architectural features for my dissertation, I too did not have sufficient statistical expertise for this task.

So, early in 1972, we drove from Santa Fe to Albuquerque to meet with Robert Vierra, then at the University of New Mexico. Vierra had worked with Scotty MacNeish in Peru and would later work with James Brown on Middle Archaic problems at the Koster site. He was an archaeologist with the necessary computer and statistical background.

We met Vierra for lunch and spent several hours discussing what we had and what we needed. He showed us printouts for the archaeological work in Peru with various categories of artifacts, plant remains, and other materials plotted in space (horizontally) and also through time (vertically in the deposits from the sites). Taylor was excited by what Vierra showed him; this was exactly the kind of data management capability that would allow him to implement the conjunctive approach for his Coahuila materials, especially for Frightful Cave (CM-68).

My recollection (possibly faulty) is that I was less sanguine about the possibilities than was Taylor. What Vierra said to us and showed us was exciting, but I was not sure that Taylor's data were recorded in sufficient detail to allow analyses such as Vierra proposed. I was working on the Master Maximum Method analysis for the fiber materials and was having problems, especially for small categories of objects. The results were not discrete enough to be useful.

During the next couple of days, Taylor and I poured over his notes, records, and the reports that had already been completed. We went back to the original materials and reviewed the 400 or so distributional charts (e.g., Taylor 1948: plate 3). In the end, we knew that the kinds of computer-based statistical analyses we hoped Vierra would provide could not be done. The artifacts and other excavated materials had not been piece-plotted, and the use of 50 cm, artificial vertical units did not allow sufficient control. The use of the conjunctive approach was "defeated" by the excavation procedures.

It was like watching air slowly leave a balloon. Taylor's hope deflated as we realized that we did not have the necessary provenience data. We still hoped to complete the final report, but Taylor knew that even if we did, it would not be the report he hoped to produce, and the report that his critics and supporters were waiting to read. He would not be able to meet the standards he had advocated in *A Study of Archeology*. If Watson (this volume) is correct, that Taylor "walked away" from the conjunctive approach, it might have been then, in the winter of 1972, when he realized that we did not have the provenience data needed to implement the analyses. Professionally, Taylor seemed to close down a bit after that. Two years later, after the spring 1974 quarter, he retired from SIU-C.

And what of his legacy, his mark upon American archaeology? He had few students; as noted above, he chaired only three Ph.D. and six or seven M.A. committees. By contrast to his SIU-C colleagues such as J. Charles Kelley, Carroll L. Riley, and Charles H. Lange, all of whom chaired as many Ph.D. committees in a year as Taylor did in his career at the university, there was no coterie of Taylor students to carry on his work, and certainly not as he conceived it. As I noted (in Reyman 1999: 697 and below), and as is seen in this volume, Taylor's publication record is a modest one. He wrote several papers that I consider important (e.g., Taylor 1941a, 1954, [Haury et al.] 1956, 1957b, 1961, 1966a, 1967a, 1973a), although some were not and are not widely cited, and, of course, "the monograph," but Taylor's overall publication record is disappointing, especially when one considers the promise of *A Study of Archeology*. As Riley notes (this volume), Taylor's career was like a meteor that lit the sky but faded early.

## EPILOGUE

In the last analysis, the following is perhaps instructive. Stephen Vincent Benet, in *John Brown's Body* (1928), writes of Robert E. Lee:

> For he will smile and give you . . . valor and advice, and do it with such grace and gentleness, that you will know you have the whole of him, penned down, mapped out, easy to understand; and so you have. All things except the heart; the heart he kept secret to the end from all the picklocks of biographers.

Walter Taylor was a brilliant teacher, an intellectual, an enchanting raconteur, a fine guitarist and singer, and so much more. Yet, there is also so much more that went unfulfilled—his inability to complete the Coahuila report, his modest publication record (about sixty items; see Chapter 2, this volume), and his seeming abandonment of the conjunctive approach—for reasons he kept secret from the picklocks of biographers.

# PART IV

## ANALYSES OF TAYLOR'S WORK AND INFLUENCE

CHAPTER TWELVE

# REMEMBERING WALTER TAYLOR

*William A. Longacre*

I remember vividly my first encounter with *A Study of Archeology*, ten years after its publication in 1948. The library at the University of Illinois at Urbana had just changed their policy and now allowed undergraduate students direct access to the stacks. I was exploring the archaeology holdings and came across Walt's book. It had a catchy title and I noticed the American Anthropological Association had published it. I checked it out and spent the weekend reading it.

On Monday, I took it into my archaeology professor and asked why I was not told about this book. I was told that Taylor was a "gadfly" and had not had a large impact on the field. I was surprised, for I was especially impressed with Taylor's discussion of what was wrong with the field and that he gave actual examples from prominent archaeologists' writings. I must confess that I did not understand the conjunctive approach at that time. When I did finally meet Walter W. Taylor, I knew much more about him and his impact, and I certainly was convinced he was no gadfly!

The book was a major part of what I have called the Kluckhohn-Taylor attack on American archaeology. Clyde Kluckhohn was a professor at Harvard, a cultural anthropologist with a strong background in archaeology who had directed the University of New Mexico's archaeology field schools for several summers' work in Chaco Canyon. There he had excavated several smaller sites such as Bc-51 and had published the results. Later he joined the Harvard faculty and turned his attention to studies of the living Navajo.

During the 1930s Kluckhohn had become increasingly exercised about the lack of sophisticated theory in archaeology. Cultural anthropology had become invigorated as new theory was being developed to challenge the Boasian historical particularism that had dominated the field for decades. The new directions included what today we call cultural ecology and a new approach to cultural evolution. These newer directions were associated with the contributions of such theorists as Julian Steward (1937) and Leslie White (1949).

In addition, the new theory associated with the rise of structural-functionalism was beginning to have a profound impact. A. R. Radcliffe-Brown had launched that theoretical movement with the publication of *The Mother's Brother in South Africa* in 1924. By the mid 1930s, structural-functionalism had raised the level of theoretical excitement in social/cultural anthropology to high. Radcliffe-Brown and his students, like Evans-Pritchard, had thus changed the direction of anthropology significantly.

Indeed, Radcliffe-Brown and his students were convinced they could explain human behavior by understanding the position of individuals in the social structure of a society. They could predict the behavioral expectations between individuals once the nature of the social structure was understood. Later, his students focused on group-to-group behavior such as clan to clan or lineage to lineage. Social anthropologists such as Evans-Pritchard (1940), Gluckman (1943), and Fortes (1945) were read by all aspiring graduate students.

Archaeology, however, continued on as though nothing had changed. Archaeologists continued in the genre of historical particularism focused on cultural historical issues, including what Taylor called time-space systematics. Studies of culture element distributions through time and space, especially pottery types and other tool types, were typical. Logical positivism was current at that time; its tenets included the notion that science and scientists are totally unbiased and conclusions are based on the evidence: "the facts will speak for themselves." Chronological inference was particularly important.

During the later 1930s, Kluckhohn began teaching a seminar for archaeology graduate students at Harvard to explore the lack of interest among archaeologists with new theoretical directions. Walter W. Taylor was one of the students who participated in the seminar probably in the early 1940s and who prepared a critical paper on contemporary archaeology. It was a scathing review of the work of some of the most senior archaeologists in the country, contrasting what they claimed they were doing against with what they actually did.

Kluckhohn himself prepared a paper critical of the directions of contemporary Middle American archaeology that was published in 1940 in *The Maya and Their Neighbors*, a tribute volume to Alfred Tozzer. In it, Kluckhohn argued that archaeology was atheoretical and out-of-step with modern anthropology. Of course, archaeology was not completely atheoretical but was simply committed to the old Boasian paradigm, ignoring the new and exciting directions that were emerging in other fields of anthropology.

The 1940 Kluckhohn paper was the first salvo in the attack on archaeology of that time. As World War II broke out, the Harvard faculty agreed to facilitate the degree completion process. Taylor was able to use the material he had completed for the Kluckhohn seminar detailing what was amiss in the archaeology of the time and presenting evidence from America's senior archaeologists' own writings. In addition, he had developed an approach he labeled the "conjunctive approach" to rectify the situation. He had excavated cave sites in northern Mexico and was analyzing the materials he recovered. He argued that exploring the distribution of artifacts and other materials, each one to all others, would provide insight permitting a fuller cultural interpretation.

This was, of course, a totally inductive approach to spatial variation that might have provided useful insights. But the technology that would have made this possible was still decades away in the future—the rise of computer technology. Walt became excited by edge-punch-card sorting technology in the late 1960s and 1970s but that turned out to be too awkward and cumbersome to be of much use to him in his analyses.

It is true that Taylor had emphasized the development of a problem-oriented research plan and the testing of hypotheses, a deductive approach, but this was not new. Such an approach was adopted by the logical positivism that was guiding science in Europe in the nineteenth century and introduced to American anthropology by the German anthropologist, Franz Boas, around 1900. The geologist T. C. Chamberlin had published his seminal article, "The Method of Multiple Working Hypotheses," in the journal *Science* in 1890 (reprinted in *Science*, vol. 148, in 1965). But Taylor did not advocate the use of both inductive and deductive approaches that became important in the rise of the New Archaeology after 1960. The conjunctive approach was an examination of the data recovered from an archaeological context in conjunction with all other data, a decidedly inductive approach. Even today it would be impossible to do this for all data from a site as the demands on computer power would exceed the capacity at most universities.

The outward reaction to Walt's book in 1948 was quiet, but inside, the profession was seething. The initial reaction was angry and dismissive. This was how Paul S. Martin described to me the reaction at that time. Indeed, there is only one serious published review of the book, done several years after it was published by Woodbury (1954) in *American Antiquity*. Walt had done the unthinkable by naming names in a negative way. He attacked some of the most senior and respected members of the profession. There was a move to drum him out of the Society for American Archaeology and he was obviously blackballed by the establishment.

But very quickly, things began to change. The 1950 publication by Paul Martin and John Rinaldo attempting to reconstruct Mogollon social organization and the appearance of settlement pattern archaeology and Gordon Willey's 1953 publication on the Virú Valley in Peru opened up the fabulous fifties. The pace of major change in the direction of anthropological archaeology quickened, and I argue it is no coincidence that these changes followed the Kluckhohn-Taylor attack, best expressed in Walt's 1948 book.

Although instrumental, *A Study of Archeology* was not the only causal agent in understanding the subsequent developments. And it is equally clear that the senior members of the field misunderstood the importance and impact of Walt's contribution. At the time, they had little to say publicly. I interviewed Bill Solheim in the Philippines last year about this era. He was taking a "readings seminar" from Emil "Doc" Haury at the University of Arizona when Walt's book came out. Haury assigned it to the class and had them discuss it, but Bill told me Dr. Haury himself had nothing to say about it. I cannot explain Haury's lack of comment on the book to his seminar. It could be as simple as his not having had time to read it before he assigned it to the class and the students were too terrified to repeat what Walter Taylor had said about Haury's work. Prof. Solheim told me that Dr. Haury did not comment on the book or the student discussion of the book's contents.

The tension of that period all came back to me at the SAA meetings in 1985, on the fiftieth anniversary of the society's founding. At a special session, surviving members of the original group were on stage looking back at what had happened over the half century. Jerry Sabloff asked the assembled nobles what they now thought of Walt's book and I swear that smoke came out from some of their ears! Scathing comments were heard; Griffin said that Harvard should never have accepted it as a dissertation! I was shocked. It was clear that they just did not get it. This was especially saddening because Walt was in the audience, and after listening for awhile, he got up and walked out. I read my paper the next day and added a paragraph noting the importance of Walt's contributions and how surprised I was at the reaction of the senior members of the field. Walt was always especially kind to me personally and encouraged me every time we met. I am sorry I did not get to know him as well as many contributors to this volume, but I am so glad that I met his book in the Illinois library almost fifty years ago and so grateful for his friendship for all those years.

CHAPTER THIRTEEN

# WALTER W. TAYLOR'S *A STUDY OF ARCH(A)EOLOGY*

## Its Impact, or Lack Thereof, 1943–Present

*Patty Jo Watson*

### INTRODUCTION

I did not know Walter Taylor personally but did meet him near the beginning of his career (1955) during a materials-analysis conference at the Oriental Institute, University of Chicago. Taylor had organized that conference and subsequently published the proceedings (Taylor 1957b). As a pre-M.A. graduate student in Near Eastern prehistory at the time, with comprehensive exams looming before me, I did not carry away detailed memories of him or the conference. The only other personal encounters between us were in 1974 at his retirement seminar, held on the Southern Illinois University campus, and in 1993 at the Washington University faculty club during a luncheon hosted by Nicholas Demerath, professor of sociology at Washington University and a long-time friend of Taylor's.

Not only did I not know Taylor well, but also I did not even read *A Study of Archeology* until several years after finishing graduate school. It was not on the reference lists for my curriculum in Old World archaeology at the University of Chicago in the 1950s. When I began teaching a seminar in archaeological theory in 1969, however, I assigned Taylor's book (then available in a paperback reprinting by SIU Press). Like everyone else who reads it, the seminar students and I wondered why his critique of Americanist archaeology in Chapter 3 is so strong, and why his attack on A. V. Kidder is so much longer and so much more detailed than what he had to say about anyone else. Taylor's discussion of Kidder's published work occupies twenty-one pages, two and a half times more space than that devoted to Griffin, and five to seven times more space than Taylor expends on the four other archaeologists whom he specifically criticizes: Haury, Ritchie, Roberts, and Webb.

Hence, one mystery about *A Study of Archeology* centers on Chapter 3, "An Analysis of Americanist Archeology in the United States." Why did he include those personal attacks in his dissertation? Why did his committee allow him to include them? Equally difficult to understand is why he retained the ad hominem detail in the published version of his dissertation, and why the publishers[1] permitted it, especially the lengthy, destructive analysis of A. V. Kidder's work.

Was this to be blamed in part on one of Taylor's dissertation advisors, Clyde Kluckhohn? Perhaps Taylor was following a trail blazed by Kluckhohn (1940) in his critique of Middle American archaeology (Bennett 1998: 300, 307; Reyman 1999: 683, 687). Another possibility is that Taylor's independent income and his wartime triumphs underwrote the cockiness and arrogance that some of his colleagues noticed in that late 1940s to early 1950s period (e.g., Woodbury 1954). In any case, the question remains, what motivated Taylor to commit social and political suicide within the Americanist archaeological community and to engender life-long enmity in several of its most prominent members?

Another mystery emerges from an observation made in June 1996 at Harvard's Tozzer Library, when I first saw the carbon copy of Taylor's dissertation. According to the Tozzer Library card catalog, Taylor turned in his dissertation on February 12, 1943. There are several noticeable differences between the 1943 dissertation and the revised manuscript that was published in 1948, one being the dissertation's long subtitle: "A Study of Archaeology[2]: A Dialectical, Practical, and Critical Discussion with Special Reference to American Archaeology and the Conjunctive Approach." Contrary to what I had assumed originally—that the published version of the critique would have been toned down from the dissertation version—the reverse is actually the case. The published version is more combative and longer than the dissertation version, especially the section on Kidder. Taylor added several printed-pages' worth of negative discussion concerning archaeological work carried out in the Maya area by the Carnegie Institution's Division of Historical Research, chaired by Kidder from 1929 to 1950.

Allan Maca (Chapter 16, this volume) introduces the cogent suggestion that Taylor's motivation in criticizing Kidder may have been different from that which impelled him to point out theoretical and methodological inadequacies in the work of other contemporary archaeologists. Because Kidder was chair of a powerful entity that dominated Maya archaeology for decades, Taylor's negative evaluation in *A Study of Archeology* was directed not just at an individual archaeologist but also at a large, wealthy, and extremely influential institution. Maca's inference is strengthened by a fact of which he was unaware: whereas the majority of Taylor's dissertation discussion is focused upon Kidder's research in the Southwest, the expanded coverage Taylor gives Kidder in the 1948 version of his *ASOA* is devoted to Kidder's work for the Carnegie. In this same context, and recalling Kidder's "pan-scientific" initiative in the Middle American work he directed for the Carnegie, Taylor's advocacy of an interdisciplinary "clearing-house" for archaeologists (Taylor 1948: 200–202; 1957b) may also be significant. That is, if, as Maca suggests, Taylor was gunning not just for Kidder but also for his institutional resources, then Taylor may have had some hope of implementing his clearinghouse idea via Carnegie funding.

Nevertheless, the first set of questions noted above concerning Chapter 3 in Taylor's 1948 volume retain their force and must be addressed by anyone trying to understand Taylor's program, the professional context into which it was launched, and the results for the author personally as well as for the program he advocated. In the presentation that follows, I attempt to derive tentative or preliminary answers to this first set of questions, while raising and trying to answer a second set of queries concerning the substantive nature of Taylor's proposals about theory and method in mid-twentieth-century Americanist archaeology. As already indicated, there are some rather striking contrasts between his initial formulation in 1943 and the version he published in 1948. These provide clues aiding further discussion, if not actual resolution, of the ambiguities embedded within *A Study of Archeology*.

## "A STUDY OF ARCHAEOLOGY," 1943, AND *A STUDY OF ARCHEOLOGY*, 1948; WITH SPECIAL ATTENTION TO CHAPTER 3

Taylor's 1943 dissertation is organized somewhat differently from the 1948 publication of it. There are only three chapters in the dissertation, plus a four-page foreword (telling the reader how he came to take up the problems central to his dissertation), the bibliography, and an appendix (containing details from excavations carried out in 1939 at Site Bc-51, Chaco Canyon; see pp. 175–180 in the 1948 version).

Dissertation Chapter 1 is titled "Dialectic Discussion" and addresses topics covered in Chapters 1 and 2 of the 1948 publication. In Chapter 2, "Practical Discussion," Taylor includes ten pages on the deficiencies of contemporary archaeology and then takes up his account of the conjunctive approach (covering the basic territory of Chapters 5 and 6 in the 1948 book). Chapter 3, "Critical Discussion," like Chapter 3 of the 1948 publication, contains specific critiques of several archaeologists (primarily Kidder, Roberts, Haury, and Webb).

As to content, one of the most striking disparities between dissertation and publication is that already remarked upon: the difference between the dissertation version of the specific critiques and the published versions of those critiques. The former are much shorter and also somewhat milder. The numbers of pages devoted to each archaeologist critiqued by Taylor in his dissertation (counts of double-spaced typescript pages), and in the published version of his study (counts of printed pages) are as follows:

| *Archaeologist* | *Pages in dissertation* | *Pages in published volume* |
|---|---|---|
| Griffin | 0 | 8 |
| Haury | 1 | 3 |
| Kidder | 14 | 21 |
| Ritchie | 0 | 3 |
| Roberts | 3 | 3 |
| Webb | 4 | 4 |

Contrasts between several of the man-by-man critiques in the dissertation with those in the published version are even more striking when one remembers the difference between double-spaced typescript and printed pages (several of the former equal only one of the latter) and notes the distribution of footnotes in the printed volume: Griffin gets 2.5 pages of notes, Haury and Roberts together about 0.5 page; Kidder 3 pages; Webb 1 page; Ritchie none. Griffin and Ritchie are absent in the critical discussion chapter of the dissertation but were added to Chapter 3 of the published version. Taylor also added three printed pages to the 1948 version of his analysis in which he briefly discusses, or simply cites, publications by some twenty-six archaeologists whose work he finds good or quite promising (in addition, there is half a sentence at the end of the introduction to the 1948 volume in which he accords very high marks to Lewis and Kneberg's *Hiwassee Island* [1946]). These additions, however, do not offset the powerful impact of the expanded and more pointed personal critiques in the published *ASOA*.

Thus, a comparison of the original (dissertation) version of the third chapter with the published version clearly demonstrates two things. First, the chapter devoted to person-by-person critiques was significantly sharpened and lengthened for publication. Second, the portion of that chapter specifically directed at A. V. Kidder is the focus for most of the sharpening and lengthening.

Regarding the second point, one is tempted to agree with the frequently made suggestion that Taylor was following up Kluckhohn's (1940) strong criticism of Maya area archaeology, especially the work of the Carnegie Institution's Division of Historical Research, chaired by Kidder. Because Kluckhohn disliked what Kidder was doing with the Carnegie's Division of Historical Research, he might have explicitly or implicitly encouraged his student Taylor to attack Kidder. As already noted, the majority of dissertation space that Taylor devotes to Kidder's work (totaling approximately ten pages out of fourteen) is centered upon the latter's research in the Southwest between 1908 and 1928, but Taylor does conclude his dissertation critique of Kidder by referring to and discussing, for approximately four pages, Kluckhohn's 1940 negative appraisal of the Carnegie's Maya area research. These page-count proportions are reversed in Taylor's 1948 publication, which includes about 3.5 pages of commentary on Kidder's work in the Southwest versus 17.5 pages on the research Kidder directed in Middle America for the Carnegie.

Prior to seeing the 1943 dissertation, I had always assumed that Kluckhohn was chair of Taylor's doctoral committee and thus well-placed to exert considerable influence on the dissertation's content. In fact, there is no list of committee members and no acknowledgments section in Taylor's dissertation (at least not in the Tozzer Library copy). The 1948 publication does have acknowledgments, but there is no specific list of dissertation committee members. Clyde Kluckhohn *is* prominently mentioned in those acknowledgments, but many other archaeologists and anthropologists are also listed, as is Taylor's wife.[3] According to Woodbury (1973a: 76–77), the archaeologists named in this roster include many whom Kidder regarded as close friends as well as colleagues yet who seemed to Kidder to have been implicated in Taylor's negative assessment of his archaeological career.

Regarding the issue of Kluckhohn's putative influence on Taylor's published critique of Kidder,[4] Reyman says:

> There is disagreement about whether the form of *A Study of Archeology* reflects Kluckhohn's influence. Taylor always said that Kluckhohn insisted he frame the discussion in chapter 3 around specific archaeologists for maximum effect; he made this point several times over the years during my conversations with him. J. Charles Kelley and others who knew Kluckhohn have said that he asked Taylor to delete the personal references to Kidder and others before publishing the work, but Taylor refused because he wanted to maximize the impact of his argument. Kelley says that Kluckhohn was very explicit about this. At this point, we cannot know exactly where the "truth" lies; perhaps it is a bit of both. Nevertheless, as Taylor's 1968 statement . . . indicates, he stands by his original format and comments. (Reyman 1999: 683)

In any event, Kidder was deeply hurt by Taylor's published evaluation of his work (Woodbury 1973a: 76), and other archaeologists also experienced strongly negative emotional responses, which, in some cases, stayed with them for decades (Reyman 1999: 687; O'Brien, Lyman, and Schiffer 2005: 31; Longacre, this volume). Yet, as Reyman points out in the passage just quoted, Taylor explicitly stuck by the form as well as the content of his 1948 assessment in his new foreword to the 1968 reprinting of *A Study of Archeology*. Moreover, that 1968 foreword is included in the latest (1983) printing as well. Thus, one is led to conclude that Taylor himself was the responsible party throughout. When he returned from World War II, he revised his dissertation, intensifying rather than softening the attack mode of his specific criticisms, and deliberately published them in their enhanced state. Hence, there is considerable weight to the suggestion frequently made (e.g., Watson 1995; Bennett 1998: 309–311n8; Reyman 1999: 682–683, 693–696) that Taylor's detailed, destructive analyses of work by the most authoritative (and most admired, respected, and beloved) archaeologists of his day caused his book and himself to be rejected, ignored, and marginalized to the extent that his analysis had little or no perceptible effect on the discipline. Certainly the volume, although apparently widely read (Woodbury 1973a: 76), was not widely addressed in print; and its author never held elective office in major national or regional professional organizations (Reyman 1999: 693).

## BEYOND CHAPTER 3: THE CONJUNCTIVE APPROACH AND ITS CONCEPTUAL BASIS

For some, however, the above scenario is not entirely convincing. Two other possible explanations for the seeming lack of positive attention to Taylor's program of change and reform have been offered, one intellectual and one pragmatic, the latter following from the former. The intellectual one is advanced by Robert Dunnell (1986: 36), among others. He thinks that the mentalist concept of culture axiomatic for Taylor's whole formulation (see below) was not one that archaeologists of the time found congenial or even comprehensible.[5] John W. Bennett, who would apparently agree with Dunnell's assessment of attitudes among 1940s and 1950s Americanist archaeologists, states that the archaeologists of the day "really were not prepared for intellectually sophisticated endeavors." Bennett, a contemporary of Taylor's who knew him personally and debated archaeology with him in 1948 as well as in later years (Bennett 1998: 304), adds the following observation: "My own recollections of the period also tell me that Taylor's book was probably read more carefully by sociocultural anthropologists than by archaeologists." He concludes that Taylor "deserves the applause of the anthropological profession for contributing a valuable document on the theory of culture" (Bennett 1998: 307).

Taylor says, "The concept of culture has been the greatest contribution which the discipline of cultural anthropology has made to the cooperative project of the Study of Man" (Taylor 1948: 37), and that "cultural anthropology is the *comparative study of the statics and dynamics of culture, its formal, functional, and developmental aspects*" (Taylor 1948: 39, emphasis in the original). Given the great importance of the culture concept in 1930s and 1940s anthropology (Kroeber and Kluckhohn 1952; Bennett 1998: 304–307), it is not surprising that Taylor makes it central to his program for archaeology as anthropology. To this end, he added an entire chapter (Chapter 4) to his 1948 publication that is not present in the 1943 dissertation. The additional chapter is titled "A Concept of Culture for Archeology." In the fifteen pages of this chapter, Taylor lays the foundation for his vision of a truly anthropological archaeology. What exactly is his "concept of culture for archeology"?

Culture "is a mental phenomenon, consisting of the contents of minds, not of material objects or observable behavior" (Taylor 1948: 98). Further, according to Taylor, because the locus of culture is mental, artifacts are cultur*al*, but they are not culture, they are only the objectifications of culture. Ancient artifacts and architecture are at two removes from the real thing because they are only results of culturally guided behavior. To get at culture itself, the archaeologist has to start with these objectifications of culture, infer the behavior that produced them, and then infer from that postulated behavior the cognitive landscape (culture) that it reflects or represents.

Because the major focus of his program was to bring Americanist archaeology from the margins of anthropology to its center, in order to make archaeologists central to anthropology theoretically and substantively, Taylor made the culture concept in anthropology the keystone of his formulation for and about Americanist archaeology. The conjunctive approach is broadly deductivist in form, everything deriving from the basic premises about culture (briefly summarized above; see also Watson 1995 and the discussion in Chapter 1 of this volume) that he lays out in Chapter 4 of his 1948 book. From those premises, he derives methods and techniques to reveal cultural patterning, and culture itself, manifest in the archaeological record. Thus, an archaeologist committed to Taylor's program must first and foremost adhere to Taylor's mentalist view of culture (which was a fairly standard variant among those discussed by anthropologists during what Bennett calls the "Classic" era of cultural anthropology, 1915 to 1955 [Bennett 1998: xi]). A Taylorean archaeologist must then use the methods and techniques Taylor describes and discusses in Chapters 5 and 6 of his 1948 book (see also Chapters 4–7 of his 2003 monograph) to delineate testable hypotheses about cultur*al* patterning potentially enabling well-founded inferences to the patterns of c*ulture* itself once present in the minds of those ancient folk responsible for the archaeological remains under investigation. Taylorean archaeologists could ultimately track many specific cultures (culture viewed partitively, in Taylor's terminology) through time and space to contribute knowledge concerning the fundamental nature and dynamics of Culture (viewed holistically), a uniquely human characteristic that is the central focus of anthropology.

Bennett remarks (1998: 311n9) that Taylor's account of the culture concept in sociocultural anthropology and his references to statics and dynamics in the context of anthropology and archaeology foreshadow the concern with "middle-range theory" in 1970s Americanist archaeology: "However, Taylor should be credited with originality because he developed [the statics/dynamics approach to what Binford later called 'middle-range theory'] entirely out of his knowledge of archaeological methods and thought."

Thus, it can be argued that Taylor's conjunctive approach prefigures Binfordian New Archaeology, or processual archaeology, in that both programs were attempts to make Americanist archaeology central to Americanist anthropology, with systematic use by archaeologists of ethnography and ethnology as the primary means to do this. There are other similarities between Taylor's formulation and the New Archaeology platform: for example, emphasis on environment and ecology, on interdisciplinary research, on a functionalist (systemic) perspective, and on non-traditional artifact categories, such as cordage, quids, sandals, and sandal ties (Taylor 1948: 162–163, 172–173; 2003: 41–151), that were given short shrift by most archaeologists of Taylor's day.

But there are also major differences between the conjunctive approach and processual archaeology of the 1960s and 1970s, at least one of which makes these programs virtually antithetical: the mentalist foundation of the conjunctive approach. Taylor says that the goal of anthropological archaeology is the study of culture, the locus of which is mental. In the case of archaeology, then, the locus of culture is in the minds of people long gone, and everything that archaeologists do should be in the interest of retrieving those ancient cognitive patterns. In contrast to this program, Binfordian processualists viewed culture as the human primate's extrasomatic means of adaptation and focused their work on the central processes of adaptation itself, not on the sociotechnic and ideotechnic epiphenomena functioning in aid of those processes. In so far as processualist research was concerned, any problem relevant to explaining the nature, functioning, and synchronic or diachronic variation in human groups that could be potentially addressed by carefully designed investigation of the archaeological record was fair game, but attempting to reconstruct prehistoric mental templates (cognitive patterning) was of no general interest whatever. In fact, such a goal is absurd according to one well-known and influential proponent of New Archaeology (Hill 1972: 69).

Hence, Taylor's program is ontologically incompatible with that of the New Archaeologists, in spite of various similarities in methods and techniques. Only when explicitly cognitive concerns rose to prominence within Euro-American archaeology during the 1980s was Taylor's concept of culture welcomed by, or at least comprehensible to, Americanist archaeologists (Watson 1995: 686–687).

At least in part because of the major logistical difficulties attendant upon any attempt to translate Taylor's mentalist program into actual field and lab archaeology, the author himself did not during his lifetime produce a full-scale example of the conjunctive approach in action. Therefore, the pragmatic reason (one mentioned at least as early as Woodbury's review [1954] and often thereafter) why most of his colleagues accorded it scant attention is that Taylor never published his own archaeological work in Coahuila caves and rock-shelters to demonstrate the results of applying the program he proposed. Lack of a detailed example heavily biased field archaeologists (who made up the vast majority of the audience being addressed by Taylor) in the period immediately following publication of *A Study of Archeology*. That same lack of example continued for many years to carry considerable negative force, in spite of Taylor's attempt at rebuttal (1972c: 30).

George Gumerman, a former colleague of Taylor's at Southern Illinois University, made this same point when he said (personal communication, ca. mid-1980s) that what Taylor was asking archaeologists to do was just too hard. It was too difficult for Taylor himself to accomplish (see Reyman, Chapter 11, this volume) and too difficult even for the most comprehending, willing, and receptive archaeologists of the 1950s (had there been such a group) to carry out.

Taylor's book did have significant if somewhat muted or subterranean influence, however (e.g., Winters 1969: viii; Binford 1972: 2, 6, 8), and his analysis was taken quite seriously by some archaeologists at the time. Reyman (1999: 682) mentions Robert Burgh, Glyn Daniel, and Carl Guthe, in addition to Richard Woodbury who reviewed Taylor's volume for *American Antiquity*. The Tozzer Library copy of Taylor's 1948 book lists reviews or notices by those four men, as well as by Ignacio Bernal in 1948, James B. Watson in 1949, and Irving Rouse in 1953.

Hence, in spite of everything said above about Chapters 3 and 4, the original question lingers: Why did Taylor's program seem to have so little impact on 1950s Americanist archaeology?

## THE REAL REASON WHY

In addition to those already discussed, another possible answer to this question is that Taylor simply did not systematically promote his views. He obviously put considerable effort into the dissertation version of his formulation and at least as much into revising the dissertation for publication. During the revision process, he significantly expanded Chapter 3, the analysis and personalized critique of Americanist archaeology, and also added a major new chapter on the culture concept in anthropology and archaeology. But then, once his vision for radical disciplinary reform was published in detail, he abandoned it to make its own way in the hostile environment he had created by sharply criticizing his most eminent senior colleagues. He did nothing to ameliorate the powerful emotional effects of Chapter 3 on those he named and publicly chastised. Moreover, unlike later reformers—for example, Lewis Binford, David Clarke, Robert Dunnell, Ian Hodder—he did not advocate his message frequently and vigorously in follow-up articles and books, he did not exhort students and colleagues at professional meetings, and he did not recruit cadres of student disciples.

A relevant issue with regard to students is that Taylor held only one permanent academic position during his entire career (at Southern Illinois University at Carbondale, from 1958 to 1974), and after 1963 he was on campus only half of each year (Reyman 1999: 688–691). During his period of employment in the SIU Department of Anthropology, Taylor graduated a total of three Ph.D. students

(Reyman, this volume). Jonathan Reyman, who was one of Taylor's doctoral students, indicates that Taylor was at best inspirational but idiosyncratic and unpredictable as a mentor; at worst he was dictatorial and tyrannical.

Why Taylor did not become an activist in the cause of the conjunctive approach during the years following publication of his book is still somewhat unclear, but the final sentences of the two essential Taylorean documents—the 1943 dissertation and the 1948 book—may offer a clue. His dissertation concludes quite robustly as follows (emphasis added):

> The conjunctive approach is no practical elixir. It is no patent medicine to be taken in regular doses with an automatic result. It is neither a method of excavation nor a set of rules for archaeological procedure. The conjunctive approach is *a theory of archaeology*, a conceptual scheme based on explicit consideration of related disciplines and proposing a series of practical guides toward the attainment of specifically stated objectives. Not the guides but these objectives and the mental attitude governing the approach to archaeological materials constitute the conjunctive approach. (1943: 282–283)

In contrast, here is the final paragraph of his published *ASOA* (1948: 200):

> The conjunctive approach is not concerned as to whether the particular archeologist has for his objective historiography or anthropology. But it does believe that, to justify itself as a social science as opposed to antiquarianism, archeology must at least write history, must at least construct the fullest possible cultural contexts. Beyond this point, it recognizes the personal inclinations of the individual, either to stop or to go on to another level of procedure, be it the study of culture or any segment of the cultural whole: sociology, art, economics, mechanics, or whatever.

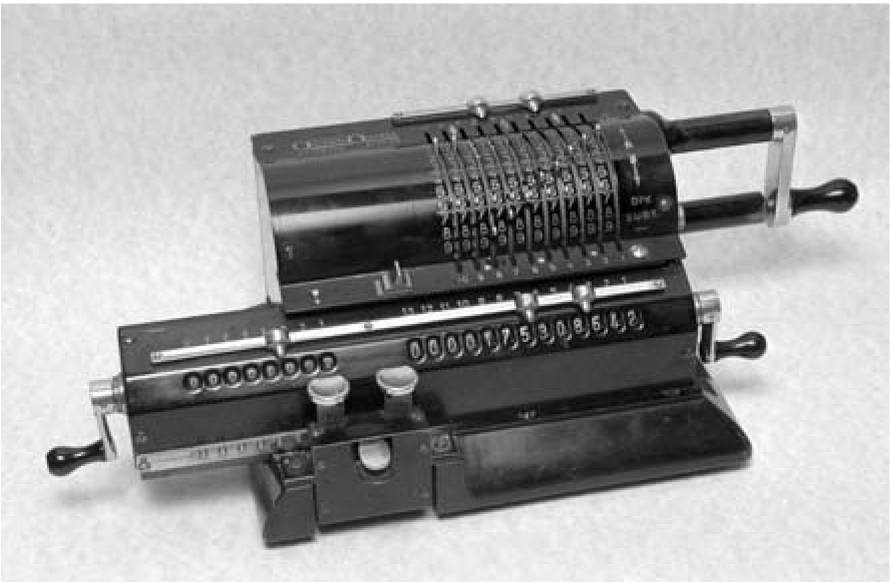
Quite apart from the awkward rhetoric imposed by hypostatizing the conjunctive approach, this is such a feeble finale that it makes one wonder why the author bothered to write the book in the first place. Surely he should have concluded the public presentation of his vision for archaeology with an inspirational call to action, one that could be quoted and rallied around; and he should have continued to exhort, urge, and attempt to persuade his colleagues orally and in print. Perhaps that last sentence to *A Study of Archeology* presages the striking lack of interest he displayed in publicly advocating his own program, which I think is the fundamental reason why that program was almost completely ignored by Taylor's contemporaries. It is true that he did not produce a full-scale example of conjunctive archaeology applied to even one entire archaeological site; but even more importantly—strategically speaking—he did not vigorously and persistently urge others to do so.

Why did he never actively promote the reforms he was so passionate about in 1943 and 1948? Certainly, Walter Taylor was a complex person, and the answer to that question—should a convincing answer ever be found—is also going to be complex. In concluding this discussion, however, I offer a few more comments deriving from my recent experience helping to prepare for publication Taylor's history of the Coahuila Project and his analyses of Coahuila sandals (Demerath, Kennedy, and Watson 2003; Taylor 2003).

Reyman (his chapter in this volume; see also Reyman 1999: 694–695) states that he was hired in 1971 as Taylor's postdoctoral research associate in Santa Fe to assist with the Coahuila report, which was to be published in 1980 by the University of Pittsburgh Press. Taylor's own self-assigned task was "to write up sandals and sandal ties as well as the background material for the report and the application of the conjunctive approach." According to Reyman (1999: 693–696), Taylor persisted fitfully during the 1950s, 1960s, and 1970s in this cultural analysis of the sandals and sandal ties. Publication at Pittsburgh was delayed, however, and in 1985 Taylor withdrew the manuscript. A few years later, a Coahuila monograph was produced at Southern Illinois University, but one that contained solely Taylor's write-up of background material together with his sandal analysis (Taylor 1988). Owing to a series of difficulties between Taylor and the production staff, that volume was withdrawn from distribution by Taylor as soon as it was printed. There the matter rested until the early 1990s, when Taylor's old friend in the Sociology Department of Washington University, Nicholas Demerath, volunteered to help edit the abortive monograph into a condition acceptable to Taylor (who was then in the early stages of Alzheimer's disease), convert it to electronic mode, and find a publisher for it. In 1992 Demerath asked me to act as archaeological consultant for this project, which I agreed to do.[6]

In the course of seeing the Coahuila manuscript through the final production process during the summer of 2003, I had to proofread carefully the entire final draft manuscript, as well as two subsequent sets of page proofs. Just following through exactly what Taylor was saying in Chapters 4 through 7 (pp. 41–151: "Field Recording Techniques and Analytical Methods," "Plaited Sandals," "Other Coahuila Sandals," and "Sandal Ties") and going to and fro among text and tables and graphs and charts, line by line and number by number, makes extremely strenuous demands on the reader. Demands on the individual manipulating all these data by hand, as Taylor was doing in the pre-microcomputer era when he completed the original analyses, would have been even more severe.

Yet implementing Taylor's mentalist concept of culture during excavation, analysis, and interpretation is a task that would have to be completed by every Taylorean archaeologist, not for just a few chosen artifact categories but for every aspect of every excavated site. This task is inexorably imposed by his axioms that anthropology (including archaeological anthropology) is the study of culture and that the locus of culture is mental. It would not be even *potentially* possible to complete such a comprehensive, conjunctive analysis of an entire archaeological site without massive computer and statistical support, which was not available to Taylor during his lifetime. Small wonder that he failed almost completely



**13.1** A Swedish Odhner mechanical calculator, circa 1940. This is probably the model used by Walter Taylor (photo courtesy of Nigel Tout and www.vintagecalculators.com).

to practice what he preached in 1943 and 1948. He did succeed, however, in carrying out a conjunctive study of 958 sandals and 750 sandal ties, wherein each one of the multiple attributes he defined (e.g., proveniences and vertical and horizontal frequency distributions, raw-material selection, shape, size, function, interior and exterior wear patterns, nature of repairs) was carefully recorded and then systematically related to all the other attributes. With very little knowledge of formal statistical procedures and aided only by a pre–World War II Odhner calculator (Jonathan Reyman, personal communication, 2005; see Fig. 13.1), Taylor elicited cultural behavior of Archaic Coahuilans with regard to their footwear by painstakingly collating an immense amount of information on the permutations and combinations among all these attributes. This patterning of cultural behavior manifest in sandals and their ties revealed ancient Coahuilan cultural patterns to Taylor (and reveals them still to the patient reader who follows his argument through Chapters 4, 5, 6, and 7 in *Sandals from Coahuila Caves*), as well as some aspects of cognitive dynamics—*culture itself*—in the minds of the artifact creators.

In other words, in spite of major technological obstacles (first and foremost, lack of statistical expertise and access to appropriate hardware, which in any case barely existed at the time he was working), Taylor did succeed in providing one detailed example of conjunctive archaeology, limited in scope though it is.

Had he been more persistent in carrying out archaeological and anthropological research on other materials, from Coahuila or elsewhere, he might have eventually reshaped the conjunctive approach, conceptually and methodologically, to overcome the problems that stopped him (and everyone else) from applying it.

## CONCLUSIONS

Dunnell and Gumerman are correct: the inferential and interpretative task Taylor set for 1950s archaeologists was much too difficult, both conceptually and practically. Moreover, Taylor was not himself sufficiently committed to working at it hard enough and long enough (see Reyman 1999: 688, 694; and his chapter here) to modify his original formulation into an approach that could be comprehensively applied to produce results appealing strongly to other archaeologists and that could be comprehensibly (and clearly but politely) conveyed to them. Walter W. Taylor was probably one of the most brilliant anthropologists of his generation; unfortunately, he was apparently also one of the most inept sociopolitically. This deleterious combination of character traits may have been responsible for his failure to anticipate the power of the personalized critiques he put into print at the beginning of his career and prevented him—once he had published them—from attempting to mitigate their negative impact, and also revising his program to make it workable.

Could some version of Taylor's program be implemented now? This is an idle and not very interesting question unless one happens to think about the twenty-five-year project Ian Hodder is currently directing at Çatalhöyük in south-central Turkey (Hodder 1996, 2000, 2005a, 2005b, 2006, 2007; Hodder and Cessford 2004; Balter 2005). The international group assembled at Çatalhöyük began work in 1993. Assuming funding remains available, they will continue until ca. 2018, focusing upon environmental, ecological, economic, and—especially—cognitive issues within an overall framework of explicitly self-reflective interaction among all the participants as well as between them and interested local, regional, national, and international publics.

Çatalhöyük is a far cry from Cueva Espontosa and the other Coahuila sites, and of course it is not yet clear what the ultimate results will be, but it is the nearest thing I know to an actual full-scale field test for something significantly like Taylor's conjunctive approach: an endeavor committed to empirically based procedures for eliciting strong inferences about the physical, social, economic, *and* cognitive universes of a prehistoric community.

Walter W. Taylor is gone as a physical presence. If he were still among us, I think he would be quite interested in what is being attempted at Çatalhöyük and would probably conclude, with some justification, that he laid the conceptual foundation for such an effort well over half a century ago.

## ACKNOWLEDGMENTS

I owe thanks to Allan Maca who organized the 2003 SAA Walter Taylor Forum, invited me to participate in it, and subsequently did such a thoughtful job of editing and querying my original draft paper that I was impelled to rewrite it substantially. Moreover, he has my profound gratitude for putting me in touch with Jeffrey Quilter, then at Dumbarton Oaks, who arranged the 2003 publication of Walter Taylor's revised Coahuila monograph.

I am grateful to Jonathan Reyman for kindly sending me the draft manuscript of his chapter for the present volume, which helped a great deal as I revised my own contribution. Jonathan also told me about Taylor's Odhner calculator and recommended the image reproduced here as Figure 13.1.

## NOTES

1. The editorial board of the *American Anthropologist* in 1948 consisted of J. Alden Mason (Editor), Harry Hoijer (Memoir Editor), A. Irving Hallowell (Book Reviewer), Frederica de Laguna, and J. Lawrence Angel. Hoijer was a linguist, so he probably was not especially close to developments in archaeology. Mason was a specialist in Peruvian archaeology, and de Laguna worked in arctic prehistory and protohistory as well as Northwest Coast ethnology; hence their areas of expertise were also somewhat distant from Taylor's. Nevertheless, all these people must have known and respected A. V. Kidder, who in 1950 received the Viking Fund Medal in Archaeology from the Wenner-Gren Foundation and in whose honor (also in 1950) the Alfred Vincent Kidder Award was established, under the aegis of the American Anthropological Association, to recognize notable achievements in archaeology of the Southwest or Middle America (Woodbury 1973a: 81; Givens 1992: ix). It seems odd that none of these editorial board members succeeded in persuading Taylor to soften his critique (if any of them even tried to do so) before it was published as Memoir 69 of the AAA.

2. Alert readers will note that in the title of Taylor's dissertation, "Archaeology" is spelled with a second *a*, but that in the title of his 1948 book (Memoir 69 of the American Anthropological Association) and in all subsequent printings (a total of six so far) the second *a* is omitted. The reason for the change between 1943 and 1948 is that—although the Harvard University spelling was and is with the second *a*—the *American Anthropologist* had adopted the shorter spelling around 1941, during the third year of Ralph Linton's editorship (1939–1944). Further details concerning the spelling of arch(a)eology can be found in Rowe 1975.

3. The final paragraph in the published acknowledgments section concerns Taylor's wife's assistance in revising his dissertation. She remains nameless in that paragraph, but he was referring to Lyda Averill Paz, whom he married in 1937 (Reyman 1999: 681). Taylor says that she served as devil's advocate and also contributed many hours of tedious labor: "I can only hope that our joint effort is worthy of her patience and devotion" (Taylor 1948: 10). In later life, Taylor apparently did not hesitate to use others, without acknowledgment, to do his work (Reyman 1999, and his chapter in this volume), but at least in his first major scholarly publication he did publicly recognize his debt not only to

various academic advisers, professional colleagues, and friends but also to his first wife, whose death from cancer in 1960 had a devastating effect on him (Reyman 1999: 690).

4. A hint of early collegial and friendly relations between Kidder and Kluckhohn is present in a recent account of "the original and obscure A. V. Kidder award" (Gumerman 2003: 19). Gumerman describes a pre-1931 artist's rendering of a Navajo man working on a sandpainting. On the back of the image are six handwritten inscriptions, the first of which is by A. V. Kidder: "Given me by Charles A. Amsden in 1931." The second note, also by A. V. Kidder but unfortunately not dated, says, "For Clyde Kluckhohn, discerning and sympathetic student of the Navajo. A. V. Kidder." The third note records transfer of the painting to Walter Taylor by Florence Kluckhohn "in memory of Clyde" in 1963. The fourth, fifth, and sixth notes document presenting of the painting to Robert Euler by Taylor (in 1970); to George Gumerman by Euler (in 1991); and to Linda Cordell by Gumerman (in 2002) (see Cordell's foreword to this volume). Hence, the image originally presented to Kidder by Amsden was passed on by Kidder to Kluckhohn—seemingly with friendship and respect—at some later date (presumably prior to 1940). Three years after Kluckhohn's death (and the same year Kidder died), Florence Kluckhohn gave it to Taylor, who passed it on seven years later to a Southwesternist colleague whose friendship he prized and whose work he respected. By that time, the painting had obviously become a veritable sacred object, but the relevant point here is that when Kidder gave it to Kluckhohn, he surely regarded Kluckhohn as a friend and valued colleague.

5. Griffin, for example, is very clear on the issue of retrieving ancient cultural meanings: "The exact meaning of any particular object for the living group or individual is forever lost, and the real significance of any object in an ethnological sense has disappeared by the time it becomes a part of an archaeologist's catalogue of finds" (Griffin 1943: 340).

6. The consultant role turned out to be a good bit more demanding than Demerath or I had envisioned and took significantly longer than anticipated (see Demerath, Kennedy, and Watson 2003). In addition to the editorial complexities, there was considerable difficulty finding a publisher for the resurrected and reconstituted book manuscript. Three different presses turned us down before—thanks to Allan Maca and Jeffrey Quilter—Dumbarton Oaks published it.

CHAPTER FOURTEEN

# CORNELIUS OSGOOD, PRECEPTOR

*Alice Beck Kehoe*

> I feel, although I cannot be explicit, that Cornelius Osgood is responsible for much of the manner in which I look upon archeology; the discussions, not to say arguments, in which we engaged during the years from 1931 to 1936 keep coming back in many forms and in many contexts.
>
> WALTER W. TAYLOR (1948: 10)

Walter Taylor's undergraduate years at Yale brought him into close and continuing apprenticeship with Cornelius Osgood, who had joined Yale in 1930 and became curator of the anthropology collections at Yale's Peabody Museum of Natural History in 1934. Osgood was an old-fashioned anthropologist, carrying on primary fieldwork in both archaeology and ethnography and writing up his data in graceful, vibrant prose. The conjunctive approach[1] was his modus operandi, although he may not have used Taylor's favored term. This chapter examines the work and research orientation of Prof. Osgood, with particular emphasis on segments of his thought that seem likely to have contributed to the vision and mission of his student, Walter Taylor.

Cornelius Osgood was born in Massachusetts in 1905 and obtained both his bachelor's (1927) and doctoral (1930) degrees from the University of Chicago, where he was a classmate of Frederick Eggan, both taking a seminar with Fay-Cooper

Cole. Edward Sapir was another of his professors, arriving in Chicago in 1925 and moving to Yale in 1931 (Darnell 2001: 21). After summer 1927, traveling through Sekani country in British Columbia to Chipewyan in Lake Athabasca, Osgood was employed by the National Museum of Canada from 1928 to 1929, doing fieldwork at Great Bear Lake, Northwest Territory, in spring 1928 and remaining there until fall 1929. Further fieldwork in 1931, 1932, 1934, and 1937 (and 1956) with Deg Hit'an ("Ingalik"), Gwich'in ("Kutchin"), and Tanaina made him the preeminent ethnologist on northern Dené ("Athabascans") (see references in Osgood 1936, 1937, 1971; Helm 1981). His culture-area map of the region, published in 1936 as *The Distribution of the Northern Athapaskan Indians*, remains a basic contribution to the ethnohistories of these nations; it was rather precocious in that young Osgood could only have barely sampled that vast territory, yet his penetrating intelligence grasped the lay of the land and the fluidity of ethnic terms among these small communities.

After ten years in the western subarctic, Osgood in 1938 began fieldwork in Yunnan, China. After a World War II hiatus, China and Korea engaged his active research for the remainder of his career. He married a Chinese woman of cultivated artistic taste and, after she died, a younger woman of Chinese descent, a graduate student. Osgood also formally taught and discussed museology and published on anthropology in museums in 1979 at the invitation of the Milwaukee Public Museum. Least known of Osgood's work is his 1930s archaeological research in northern South America and the Caribbean. At Yale, he felt the institution expected him above all to create research projects involving graduate students, giving less attention to teaching other students and even less to curatorial duties at Yale's Peabody Museum of Natural History (Osgood 1979: 2). He was excited in 1941 to be told that Yale would build an anthropology museum. Between 1937 and 1961 he taught a graduate seminar on museums wherein he conceptualized the ideal anthropological museum, utilizing his observations from hundreds of visits to museums around the world. Yale disappointed him by deciding that such an ideal museum was too expensive; Osgood remarked, "I seemed personally unable to accept any substitute for the superiority in achievement to which I believed Yale was entitled" (Osgood 1979: 2).

## OSGOOD'S CONCEPTUALIZATION OF CULTURE

Like Clark Wissler in his American Museum series on Blackfoot culture (Wissler 1910; 1911; 1912), Osgood divided his major work on Ingalik into separate volumes on Material Culture, Social Culture, and Mental Culture. (Wissler titled his third volume *Ceremonial Bundles* . . . , referring to religious matters.) This approach, aside from invoking the Indo-European magic number three, may have derived directly from Wissler, who taught at Yale along with his full-time job at the American Museum in New York and whose influence is acknowledged by

Osgood in the preface to his 1979 study of museums (Osgood 1979: 3). Osgood states that material culture came first because

> a presentation of the physical productions of culture was a logical preliminary to writing about human behavior. It became increasingly apparent that I could not adequately set the stage for social interaction without referring to things, and therefore the things had first to be described. Life in a kashim is rather difficult to comprehend if one does not fully understand what kind of a shelter it is. (Osgood 1958: 3)[2]

Social Culture encompasses village structure and activities, where the kashim is literally the center; group hunting and fishing, trade, warfare, shamans, ceremonies, family life and relationships, and activities are described from the point of view of an individual, since subsistence activities were usually performed alone (Osgood 1958: 280). Mental Culture (Osgood 1959) begins with extensive ethnozoology and ethnobotany; then moves to concepts of person, social class, emotions, material values, cosmology, myths, and histories; and, in a final appendix, "negative traits," items or behavior that might be expected but were denied by Deg Hit'an. A large portion of Osgood's data came from the memory of one gifted consultant, Billy Williams of the Deg Hit'an village of Anvik on the Yukon River, well-known among his people as a singer for the dead. Osgood mentions here and there that he intends to write a final volume on "acculturation" among Deg Hit'an, that is, twentieth-century life, but this apparently was not completed. Because Osgood delved into the past for his ethnography rather than describing his actual experiences, we might see his work as a kind of archaeology. His later work on Korea and China similarly sought to present ethnic cultures winnowed out of contemporary life.

During the winter of 1940–1941, Osgood wrestled with the burden of translating his observations into monograph form. Dividing the task into three volumes made the work more manageable, yet he could not be satisfied with merely aping a precedent. There was a deeper problem, the question of epistemology, or how we know what we think we know. Pondering which data belonged in volume 1, Material Culture, and particularly how to sort between Social Culture and Mental Culture for the subsequent volumes, Osgood concluded that ethnographic data may be empirical, that is, directly perceived, or may be non-empirical ideas that cannot be perceived although they can be communicated. Those that can be perceived are "percepta," those that are communicated but not actually observed are "concepta." We should note that the etymology of the word "empirical" is from the Greek *en peira*, "in a trial" (in the sense of an experiment).

Osgood was bothered that "a quick survey of ethnographic monographs will suffice to show, the anthropologist has consistently ignored these distinctions . . . [as they apply to] ideas in the ethnographer's mind, [whether they] truly correspond to those in the informant's" (Osgood 1958: 22 [quote]; 1951: 210–211). In other words, Osgood was more concerned with the validity ("truth") of ethnographic data than with a distinction between material objects and physical behavior, on the one hand, and native persons' ideas, on the other. The issue lay in the verifiability of percepta, contrasted with the higher potential for misunderstanding concepta. Putting it clearly, Osgood described how one can verify the physical attributes of a canoe and perceiving someone paddling a canoe, as opposed to taking Osgood's word for it that an Indian at Anvik related an idea about canoes. In a court of law, such oral communication could be labeled hearsay and disallowed because it is unverifiable.

From the distinction between empirically verifiable percepta and, in his opinion, unverifiable concepta, Osgood derived his definition of culture: "Culture consists of all ideas of the manufactures, behavior, and ideas of the aggregate of human beings which have been directly observed or communicated to one's mind and of which one is conscious" (Osgood 1951: 208). The point of working out a definition of culture is, Osgood states, to clarify what constitute data for anthropologists. He notes that some archaeologists equate "culture" with "human manufactures" and hopes that his broader scope will move them toward a more fully anthropological understanding (ibid.).

Overall, Cornelius Osgood reveals himself to be preoccupied with profound questions of verifiability and the role of the observer-interpreter. He wants to convey a true and full picture of indigenous Dené life but is painfully aware that much of it was only memory for the people he worked with. In the volume on Social Culture, he remarks,

> [T]he technique of having an Indian make most of the unavailable and unseen items seemed a fairly satisfactory and standard solution, especially when one's best informant knew how to construct them and would tell if there were known variations, as well as if and when he veered away from the standard technique of manufacture. In recording social culture, however, . . . [ongoing] human behavior is difficult to describe in exact words and, furthermore, informants tended to idealize behavior and tell "the socially approved dream." (Osgood 1958: 23)

The concern with epistemology and the hesitation to assert his descriptions are valid and presage the postmodernists' position half a century later.

## OSGOOD'S CONTRIBUTIONS TO ARCHAEOLOGY

Osgood's fieldwork in archaeology is generally less well-known than his enduring monographs on Dené. Archaeologists who came of age in the mid-twentieth century studied Irving Rouse's classic *Prehistory in Haiti: A Study in Method* without realizing how much it owed to the Yale Caribbean Anthropological Program developed by Osgood. The Caribbean Program pushed the issue of epistemology and methodology conscious of epistemological questions, concerns beautifully articulated in Rouse's 1939 dissertation published as *Prehistory in Haiti*. Osgood's Yale Caribbean Program began in 1933 "as an attempt to improve the methodology of archeology through intensive research in a particular area, as well as to resolve the historical problems of the aboriginal populations of the West Indies and related peoples in North and South America" (Osgood 1942: 5). His first project was a spring survey in southern Georgia and Florida with Froelich Rainey, a graduate student, followed by summer excavations by Osgood near Lake Valencia, Venezuela, and examining collections in Chiriqui Province, Panama. With George D. Howard, from the Yale Program, and his wife, Osgood conducted an archaeological survey of Venezuela in 1941 in collaboration with the Venezuelan Museo de Ciencias Naturales (Osgood 1943) and, the same year, participated in a survey of Cuba and excavated a non-ceramic shell mound there (Osgood 1942). After World War II, Osgood initiated archaeological research in Guyana (Osgood 1946), where his "pioneering work in stratigraphic excavation and ceramic classification" is noted in a 1996 overview published by Guyanese archaeologist Denis Williams (1996: 11).

Irving Rouse, like Walter Taylor, acknowledges Osgood's significant involvement with his students. Rouse (1939: 8) says Osgood "gave me the interest in methodology which led to the production of [*Prehistory in Haiti*]. Much of what is contained in the paper was worked out during discussion with him." Rouse also expresses obligation to Leslie Spier and Edward Sapir, from whom he "derived whatever theoretical knowledge I may have of the nature of culture," and to Clark Wissler and George Peter Murdock, who "made valuable suggestions" (Rouse 1939: 8). According to Rouse (ibid., 9), the cutting-edge work discussed at Yale in the 1930s were Cole and Deuel's Mississippi Valley surveys and excavations (Cole and Deuel 1937), Anna Shepard's analyses of Southwestern pottery (Shepard 1936), McKern's taxonomic system (McKern 1939), and James Ford's derivations of chronology from seriations (Ford 1938). Rouse's own theoretical contribution was the introduction of the terms "type" ("an abstract kind of artifact") and "mode" (the separable attributes of an artifact, recognized as "historically significant") (Rouse 1939: 11–12). "Culture cannot be inherent in the artifacts. It must be something in the relationship between the artifacts and the aborigines who made and used them. It is a pattern of significance which the artifacts have, not the artifacts themselves" (Rouse 1939: 16). Walter Taylor uses Rouse's concept of "mode" (Taylor 1948: 118) and on the next page cites Osgood's 1942 report on Ciboney, Cuba, when discussing "type" (Taylor 1948: 119). Taylor's definition of "culture" follows Osgood's concern with epistemology by holding that "[c]ulture, consisting as it does of mental constructs, is not directly observable" so instead is "inferred" from behavior, speech, and material phenomena (Taylor 1948: 110). Both of Osgood's 1930s students, Taylor and

Rouse, evidence their teacher's care about "exact verbal terms," balanced by awareness that collections of artifacts must be related to larger pictures of life at the site.

Osgood himself does homage to his own preceptors in the preface to his masterwork, *Ingalik Material Culture* (1940):

> The research project on the Northern Athapaskans began in 1927, when, as a student at the University of Chicago, the writer [Osgood] was encouraged by his Professors, Fay-Cooper Cole and Edward Sapir, to follow an interest in augmenting the meager data on the ethnography of the vast interior region of northwest Canada and of Alaska. In his lectures on the American Indian, Professor Cole had given a general survey of cultures north of Mexico but for the Mackenzie valley and the interior of Alaska the data were negligible. . . . My first interest was to record a body of data which would serve as a basis for . . . studies, and to Professor Cole I owe my first enthusiasms for this challenging task. During the same period, Professor Sapir was occupied with his comparative and analytical study of the Athapaskan languages. . . . My indebtedness to him as my closest personal adviser over more than a decade of our association is so great that it is beyond my ability to express more than certain basic ideas which were impressed upon me. His work on Athapaskan languages served as an ideal for a parallel study of the general culture of these people. This parallelism was not conceived of as involving the essential details of his methods but rather as one embracing two fundamental points of view. One was the idea of controlling a large amount of equivalent data, and the other was a primary interest in understanding the manifestations of the human mind through the study of data from alien, and consequently more objectively viewed, peoples. . . . From my associate, Dr. Irving Rouse, I have had the benefits of intellectual criticism and support in the vagaries of my thinking during a long period of years. (Osgood 1940: 5, 9)

Between the quoted portions, Osgood candidly confesses his initial naïveté concerning the possibility of finding "the old way of life most intact" in the most inaccessible interior of the Northwest. The Bear Lake people (Sahtú-gotine) proved no more "pure" than Dené to the south, and in retrospect, Osgood realized that by trying to block out "European" traits he was blind to "the commonplace acts of village life" that are the basis of Dené society. Methodologically, Osgood realized that ethnographic "data inevitably are essentially personal," the realization so much ballyhooed in the 1980s without citing Osgood. For him, this meant that rather than studying one community or ethnic group (which in the western subarctic means regional groups) and relying on other ethnographers' work for comparative analysis, he must himself directly experience several communities. His method led him to work with Tanaina, Han, Gwich'in, and finally Deg Hit'an. It was the Deg Hit'an he spent the most time with, because in their village of Anvik in 1934, he met Billy Williams, an ideal collaborator, both profoundly knowledgeable about his people and willing to work hard and long with the young American to record that knowledge. Not a simple bush Indian, Williams had been employed by both Christian missionaries and a Jewish trader, becoming especially close to the fatherly trader (Osgood 1940: 52–55). We might say that Billy Williams's intellectual depth and power enabled Cornelius Osgood to break out of the culture-trait paradigm he had been taught and into a conjunctive approach.

The epitome of the conjunctive approach lies in Osgood's statement, "Something has been said already of lines in Ingalik culture but the longer one works on Northern Athapaskan materials, the more *intense* is one's feeling for their significance" (Osgood 1940: 435; emphasis added). He explains that two-thirds of Ingalik manufactures include lines—lashing, binding, sewing, fishing lines and nets, and dog harnesses. Furthermore, "[i]n form, Ingalik manufactures tend to be made up of curved lines," and overall, there is an "unusual amount of lashing" (Osgood 1940: 430). Taylor picked up on Osgood's statement in *Ingalik Material Culture*: "the concept of culture has been limited to the ideas in one person's mind. . . . *Culture consists of all ideas concerning human beings which have been communicated to one's mind and of which one is conscious*" (Osgood 1940: 25, 26; italics original). Taylor's version is that tangible objects and witnessed performances are not "culture" but only "objectifications of culture" and, from that point of view, to apply the term "culture" to material objects "is a misnomer" (Taylor 1948: 102). That Osgood titled his major work *Ingalik Material Culture* seems to put him at odds with his pupil Taylor until one reads Osgood's scaffolding of the mental, where culture exists (insofar as it has any existence) over its manifestations in the tangible (Osgood 1940: 27).

Osgood's method of analysis and presentation comes out of his Dené data and experience. Among Dené, apparently simple manufactures belie rich and deep knowledge. Osgood recounts, "Much of an Ingalik man's life is [was] spent in manufacturing things and much of the making is done here in the kashim. . . . Inside the kashim, during the day, working and talking continue . . . not loud, however" (Osgood 1958: 35). We can see, from this description, how material objects were enmeshed with daily living and interpersonal relationships among Deg Hit'an.

Osgood's highly personal, often poetic account of his winter at Great Bear Lake narrates his acculturation from a southerner tangling his lines, nets, dog harness, and snowshoes to a climax of his sled dogs streaking over the spring ice in a straight line to home base in the Sahtú-gotine village (Osgood 1953). Simultaneously as he masters subarctic technology, he comes to recognize the entanglements of personal relationships in the village. Underneath Osgood's engagement with relationships among objects, technology, environment, human needs, and social heritage—the *conjunction* of all the elements in living as Sahtú-gotine or Deg Hit'an—seethed his struggle to legitimate his experiences, his insights, and conceptual constructions within, yet going beyond, the approach of his teacher, Sapir.

Darnell quotes a letter Sapir wrote in the last year of his life, 1938: "I'm not particularly interested in 'smoothed-over' versions of native culture. I like the stuff in the raw, as felt and dictated by the natives . . . the genuine, difficult, confusing, primary sources. There are too many glib monographs, most of which time will show to be highly subjective performances" (Sapir, April 25, 1938, quoted in Darnell 2001: 23). (Ah, if only the navel-gazers of the 1980s and their spawn would look at their predecessors' highlighting of the big questions.) Between Osgood and Sapir lay one huge gulf: Sapir refers to *texts*; Osgood saw *objects* (natural and artifactual) as the primary stuff. In the preface to his 1979 survey *Anthropology in Museums*, he explains, "I remember what I see in museums but only relevant details of what I hear—whereas [Mrs. Osgood] remembers what she hears but only specially chosen segments of what she sees" (Osgood 1979: 3). All his professional life, Osgood was a museum man, the curation of objects his constant obligation. *Ingalik Material Culture* is the brilliant outcome of Osgood's labor to convey what it meant to be Deg Hit'an. The critical point is that, like most Northern Dené, Deg Hit'an highly value being quiet. Only the extraordinary Billy Williams was garrulous. Therefore, to present Dené "stuff in the raw," texts would not do.

Osgood's focus on material culture was reinforced by his active interest in archaeology, fostered by Cole. Chronology and distributions were important, evidenced by Osgood's careful scientific method in the field, but not his sole goals. Writing in 1942 on his Ciboney project, Osgood informs us that the "monograph has been written as a demonstration of method in reporting." Like his Ingalik set (and Caesar's Gaul), his presentation of the Cayo Redondo data is divided into three: purpose of the monograph, data, and conclusions. The last section explicates "ultimate cause[s] for undertaking archeology" (Osgood 1942: 15), or archaeologists' interests in doing archaeology: (1) manufacture and use or function of artifacts; (2) their distribution in space; (3) their distribution in time; (4) art; (5) "the culture as a whole"; and (6) the culture's relationships in space and time (Osgood 1942: 33). The second interest forced Osgood and his students to work on defining and operationalizing the concept of type. Tackling "the culture as a whole," Osgood contrasts the empirical data presented in previous pages with the challenge of allowing "some rein to our intuitions" (i.e., allowing some slack) (Osgood 1942: 50). He does not really give much leeway to intuition, as the section is an extended discussion of probable ethnographic analogies. Nevertheless, since he was working through his Dené data along with designing, developing, and initiating fieldwork in the Yale Caribbean project, Osgood must have been highly conscious of the tendency to intuit function and meaning for archaeological artifacts and sharply aware of how much remains veiled by our inability to experience the lived culture.

Surveying Cornelius Osgood's range of work during the time of Walter Taylor's engagement at Yale, relating this work to his doctoral students' accomplishments (particularly Rouse's classic on Haiti [1939]), and reading his wonderful (there is no better word) Ingalik monographs, we can see Walter Taylor's debt. The simplicity of most of the Dené artifacts in Osgood's lab while Taylor assisted him underscored Osgood's struggle to convey the fullness of Dené life he had experienced. It was still conventional in the 1930s to label peoples like the Dené "primitive." The magnitude of Osgood's achievement depicting this indigenous society helps us see why Taylor could not be explicit about his preceptor's influence. Cornelius Osgood's *thinking* was conjunctive, the approach infused all his work, and most importantly, that conjunctive approach was grounded on solid, extended empirical experience.

*Note*: Obituaries of Cornelius Osgood were published in the *New York Times* (January 7, 1985) and the *Anthropology Newsletter* (April 1985).

*Postscript*: Answering the standards of contemporary scholarship, I Googled "Prof. Cornelius Osgood." Right away I hit "Prof. Cornelius Osgood . . . Mangled Myths and Tangled Tales." Click!

> Waukesha [Wisconsin] Civic Theatre, The Magical Mirror. "Mangled Myths and Tangled Tales" by Neil Gregersen and David Hundhausen, October 7 to 13, 2004. Based in part upon ideas submitted by Waukesha area fifth and sixth grade students, "Mangled Myths and Tangled Tales" is the story of Professor Cornelius Osgood who has created a talking computer which can create holographic versions of fairy tales. While testing the computer's program for the very first time, Professor Osgood is outraged when the computer decided to rewrite, with hilarious results, its own versions of Red Riding Hood, Cinderella, Rumplestiltskin and other popular fairy tales.

Waukesha being a suburb of Milwaukee, I found David Hundhausen in the telephone book and asked him why he and his collaborator had chosen Osgood as their play's protagonist. "You mean he's real?" responded Hundhausen. "We just made up the name out of thin air." Bear in mind that Deg Hit'an believe in reincarnation . . . or perhaps this strange coincidence is the work of one of those *diyinin* doctors with exceptional power.

## NOTES

1. Taylor defined his "conjunctive approach . . . [as] a viewpoint, a point of attack . . . rather than a particular method. . . . [which] takes as its first concern the description of the cultures of human groups . . . the interrelationships which existed *within* a particular cultural entity" (Taylor 1948: 7; emphasis in the original). He contrasted the conjunctive approach with "the comparative or taxonomic approach . . . popular with Americanists in

the United States," which "attempts to place the newly discovered material in . . . relationships *outside* the cultural unit" (ibid.).

2. He devotes five vivid pages to "Life in the Kashim" in his 1958 *Ingalik Social Culture*.

CHAPTER FIFTEEN

# WALTER W. TAYLOR AND THE STUDY OF MAYA ICONOGRAPHY

*Rosemary A. Joyce*

> What do we know of the costumes of Maya ceremonial personages? . . . Nor are we able to know which type of sandal, for instance, is found with which type of headdress or what type of accoutrement is found in what sort of depiction upon what sort of monument. . . . [T]he descriptions . . . cannot be used to "reconstruct" the habiliments of Maya personages or used to make inferences as to the role or cultural significance of the figures that wear them.
>
> TAYLOR (1948: 53–54)

Today, there are numerous studies that could be cited in answer to this challenge. At the time Taylor wrote, his comment was an accurate reflection of the state of affairs in Maya studies. Curiously, he failed to cite one example of precisely the kind of study he called for: his own paper, published in 1941 in *American Antiquity*, "The Ceremonial Bar and Associated Features of Maya Ornamental Art."

This volume of the journal is probably best known today as the forum for Julian Steward's presentation, "The Direct Historical Approach to Archaeology," an explication of one of the fundamental arguments for the application of ethnographic analogy (Steward 1942). Included in the same volume were two contributions to the study of Classic Maya civilization, one by Walter Taylor, the other by E. Wyllys Andrews IV. Whereas Taylor's paper received almost no mention by subsequent Maya scholars and was totally ignored by contemporary mainstream Mayanists, Andrews's work entered the canon and is still cited today. The contrast in content and treatment of these two essays by contemporary and later scholars is a vivid illustration of the influence of historical forces, timing, and an accepted paradigm on the fate of research. It also provides a basis to reconsider the relationship Taylor had to Maya archaeology of the time and to explore the perennial question of the roots of his strong negative assessment of it.

Both Andrews (1942) and Taylor (1941a) discussed monumental art of the Maya Classic period (ca. AD 250–800). In this they stayed well within the dominant model of study of high culture and celebration of this period as the peak of Maya cultural history. Both authors produced work based on data from the Carnegie Institution of Washington's intensive program in Maya archaeology. As contemporaries in the doctoral program at Harvard University, both worked within the frameworks delineated by Alfred M. Tozzer, the senior specialist in Mesoamerican archaeology with whom they studied.

Andrews's article was about a fragmentary hieroglyphic text with six preserved glyph blocks, Stela 38 from Piedras Negras, a Maya site on the Mexico-Guatemala border. The preserved text was entirely calendric, recording a date in the fifty-two-year Calendar Round employed by the Maya. He noted that this date, 8 Muluc 2 Zip, marked the anniversary of two cycles of twenty periods of 360 days (*katuns*) elapsed from the Long Count date of 9.12.6.5.9, 4 Muluc 7 Zac, prominently recorded elsewhere at Piedras Negras (Andrews 1942: 367). He compared this base date and the commemoration of its katun anniversaries to the date 6 Caban 10 Mol at Copan and the date 12 Caban 5 Kayab at Quirigua, Maya sites on the Guatemala-Honduras border.

According to the then-reigning model, Maya inscriptions were astronomical and calendrical and generally commemorated the end of even cycles. Dates that did not mark the end of an even number of cyclic periods of time, like those Andrews cited, were considered to represent factors for the adjustment of the astronomical calendars, called "determinant dates" (Thompson 1950: 204–206, 317–318). In Andrews's (1942: 368) words, "once the long-range calculations had been perfected to the satisfaction of the astronomers of the day, it is easy to see how the necessary minor corrections could be made by merely noting the time which had elapsed since these major determinant dates."

This date and others like it are now known to be historical, their anniversaries noted for political purposes, not for the convenience of astronomers. Nonetheless, Andrews's contribution in deciphering the date has survived the test of time and become a standard citation in Maya literature. Andrews addressed a topic that was an accepted object of study, the chronological placement of the inscription, and worked within an interpretive framework that assumed that Maya inscriptions were concerned only with time and calendric manipulations.

A different fate has befallen Taylor's consideration of the iconographic substitutability of the ceremonial bar, bar pendant, and architectural frieze mask. It received no mention in the major works on Maya art, writing, or civilization of its day (Proskouriakoff 1950; Thompson 1950). It was not cited in the *Handbook of Middle American Indians* (Harrison 1976), the vast compendium of the state of knowledge of Mesoamerica assembled during the 1960s and 1970s. Inclusion in research bibliographies (Bernal 1962: 311; Kendall 1977: 292) and citation by later researchers outside of the mainstream of North Americanist Maya archaeology (Kubler 1962; Dutting 1970; Quirarte 1981; Clancy 1994) show that the article itself was not lost.

In the pages that follow, I seek to explain the near complete neglect of this study by contemporary and later Mayanists. I explore the circumstances that led Taylor to carry out this study. In the process, I suggest that understanding the genesis of this paper can shed light on the development of Taylor's critiques of archaeological practice, including the sharp criticism he made of the dominant people and institutions in Maya studies. But I also contend that the specific failure to take up the points Taylor made should not be seen simply as a kind of retaliation in a personalized battle among archaeologists of the time. Rather, because of the perspectives Taylor wished to encourage in contemporary archaeology, his paper was formulated outside the established problematics of Maya archaeology and could only be understood from other theoretical perspectives. By the time Maya archaeology caught up with Taylor, there was no trail of citations to lead new scholars to it; the few descriptions of it, in fact, misleadingly appraised it in terms of the framework of the 1940s, to which it contributed little that was new. To appreciate just what authors since 1941 have missed, we need to consider what it was that Taylor undertook and accomplished in his sole published foray into the study of Maya iconography.

## THE CONTENT OF TAYLOR'S STUDY

The approach taken by Taylor and the specific points he made were promising. The method he employed is structural, similar to that adopted in the later studies of Mesoamerican art that he anticipated (Kubler 1969; Coggins 1980; Schele and Miller 1986; Freidel and Schele 1988a, 1988b). Structural analyses, ultimately to be traced to the linguistic structuralism of Ferdinand de Saussure, begin with the assumption that the patterning of elements in contrastive sets is constitutive of their meaning. Systems of signs, from a structuralist perspective, can be recognized through the substitution of signs for each other in structured symbolic representations. In reference to language, it is usual to illustrate the first assumption with the example of the substitutability of words for each other as contrasting alternatives, so that "cat" gains its meaning not simply by reference to a specific animal but by its distinction from the animal represented by the word "dog." The second assumption, that substitution of signs will be recognized in practice, is illustrated in language by the example of sentences: "the cat bites the girl" and "the dog bites the girl" demonstrate that "cat" and "dog" are signs that substitute for each other, that occupy the same place in symbolic statements and subtly change the meaning. Hence, identifying the substitution of one element for another in regularly recurring associations of elements is a primary concern of structural analyses.

Taylor based his discussion on an explicit assumption that the coherence of the Maya representational system is the result of the coherence of a system of ideas that were expressed in art. "The question arises as to how significant all these substitutions and typological similarities are. Do they have meaning for the interpretation of Maya concepts and ideas or only for the understanding of artistic elaboration? There appears to be evidence to indicate that *the artistic complex holds together because of basic ideas*" (Taylor 1941a: 52; my emphasis). In this passage, Taylor establishes that he is not simply concerned with defining formal patterns of change through time and space but with understanding what the symbolic representations mean. This enterprise is ultimately semiotic.

Taylor begins by isolating three recurrent iconographic elements for study: the ceremonial bar, bar pendant, and frieze mask on architecture. The first two are costume elements and receive the majority of his discussion. The ceremonial bar was Taylor's term for what was later labeled "the Double-headed Serpent Bar, the most important scepter of Maya kings. Sometimes shown with a rigid bar or a naturalistic serpent body between the two heads, the scepter terminates with gaping serpent mouths from which emerge the gods who sanctify the king's position" (Schele and Miller 1986: 121; cf. Proskouriakoff 1950: 88–90).

The bar pectoral, equivalent to Taylor's bar pendant, has been described as "a knot tied from groups of thin twine that ended in three loops on either end, which signalled that the wearer was dressed as one of a complex of gods that included the Palenque Triad, the Paddler Gods and Chac-Xib-Chac" (Schele and Miller 1986: 70). This form of collar ornament was depicted from Early to Late Classic throughout the Maya lowlands, especially in the Usumacinta region in the west (Proskouriakoff 1950: 64–65, fig. 22, C2). Taylor argued that the bar pendant, which hangs from the neck of human figures, substitutes for the ceremonial bar, which is held in the hands, and that they may be considered potentially equivalent in meaning.

Later studies reached similar conclusions from patterns of substitution of these two iconographic elements. Their equivalence is evident in a jade plaque from the British Museum, where "the rank of king is specified by the Double-headed Serpent Bar behind the figure" (Schele and Miller 1986: 111), and the figure also wears a bar pendant. Taylor drew attention to the fact that normally the bar pendant is depicted when the figure holds other paraphernalia in the hands.

Taylor's concern with the exceptions to the dominant patterns distinguishes his study as truly semiotic. He was interested in the meaning of variation as well as the meaning of regularities. For example, he noted that smaller, secondary figures wear only the bar pendant, except in the case of Yaxchilan Stelae 1 and 4. Modern research, integrating an understanding of the texts on these monuments and based on the hypothesis that the figures are historical, provides a partial solution to this exception to the rule. The smaller figures are royal ancestors, and by depicting them holding the ceremonial bar the composition emphasizes their historical importance to the ruler for whom these monuments were erected (Tate 1992).

Taylor's analysis pays special attention to the patterns of substitution of one element for another. This structural study is the source of many inferences that prefigured later research undertaken without any apparent awareness of Taylor's early study. In a footnote, Taylor states that a chain, "terminating in two large serpent heads, is accorded the same treatment as a true Ceremonial Bar. This is seen from the hands, which are in the same posture and position they often take when enfolding a Ceremonial Bar" (Taylor 1941a: 52). This emphasis on the position of the hands as a signal that the meaning of the ceremonial bar is to be inferred was reached independently in a more recent study based on an explicitly structural methodology. An Early Classic bar pectoral from Nohmul, Belize, depicts an anthropomorphic figure with hands in the posture to hold the ceremonial bar; "worn horizontally across the chest of a ruler, this pectoral declared the wearer's supernatural affiliation and rank as *ahau*" (Schele and Miller 1986: 81). A pebble from a cache below Stela 7 at Copan depicts a cross-legged figure with hands in the same position, holding unique objects identifiable as substitutions for the ceremonial bar, signifying kingship (Schele and Miller 1986: 111).

Many of Taylor's suggestions find support in subsequent analyses. Other observations are novel and stimulating today, enhancing and expanding arguments made by contemporary scholars. In discussing the equivalence between the ceremonial bar (likened to a serpent body) and the bar pendant, Taylor (1941a: 52) noted that the bar pendant substitutes a bivalve shell for the serpent body. The concept uniting the shell and the serpent was their association with water. Since the ceremonial bar and bar pendant were both symbols of power, a connection was suggested between power and water symbolism. Subsequent studies explored the association of elite representations of the sources of their power with the ocean in greater detail.

For example, Rands (1955: 275) related the dominance of water imagery to the importance in Mesoamerican religion of supernatural beings responsible for the regular production of rain, including "composite monsters in which serpentine or saurian characteristics are pronounced." Rands (1955: 364), like Taylor, cited Thompson (1939) as the source for the identification of the celestial dragon (embodied in the ceremonial bar) with rainfall but failed to note Taylor's (1941a: 52) similar argument. Following the general acceptance of the historical content of Maya inscriptions and iconography, studies of Maya art from the late 1970s through the 1990s emphasized the connection of water imagery with an underworld realm populated by gods, a region from which the ancestors of rulers came (Hellmuth 1986). A more direct connection between water imagery and symbols of power may have originated in this way as a signal of supernatural origin of the elite. The substitutability of water with blood, and the inclusion of both in a class of volatile substances (along with smoke and mist) that transform from one state to another, was identified as a structural principle underlying this equivalence (Freidel 1985).

Equally prophetic, but ignored, was Taylor's (1941a: 52) suggestion, on structural grounds, that the feathers that form part of Maya elite headdresses might be likened to the bird that perches on the summit of the central axial tree in Maya compositions. Implicit in this suggestion is the notion that the human figures depicted on Maya stelae were symbolically to be equated with the central world tree. The idea of an equivalence between Maya rulers and the world tree was only systematically explored much later (Freidel and Schele 1988b: 559–561), and Taylor's observation concerning the equivalence of the headdress and the bird had not yet been rediscovered when the original version of this chapter was written in 1988.

Following the then-standard interpretation of the actors in Maya art as ritual specialists, Taylor (1941a: 53) suggested that different costumes might indicate different types of priests. The later understanding of Maya texts under the historical hypothesis included identification of the figures as rulers and other members of the elite, but differences in costume have indeed been found to identify different offices or roles. Musicians in the murals of Bonampak wear a particular headdress; royal rank is indicated by a headband with a small maskette; and numerous elements identify participants in specific rituals (Schele and Miller 1986: 66–72).

The final, strikingly modern suggestion in Taylor's all too brief study is his discussion of the relationship between architectural frieze masks and the costume elements he had identified as substitutable for each other (Taylor 1941a: 54). Although details of his argument for an identification between the architectural masks and the other forms may be questioned, the discussion ends with a prescient suggestion that architectural masks may have been a less personalized form carrying the same iconographic meaning. The frieze mask applied to architecture the same complex of symbolism that marked the person in stelae depicting the ceremonial bar and bar pendant. Contemporary studies have confirmed that buildings elaborated with masks are similar to individual costumes elaborated with other royal symbols. Late Preclassic architecture (Freidel and Schele 1988a), preceding the rise of forms such as stelae that celebrate individual rulers, employed large-scale masks to sacralize buildings, just as ruler's regalia later marked their sacred character. In Early Postclassic Yucatan, where the individual ruler was de-emphasized in favor of institutions represented by buildings, architectural masks again were employed to mark the focus of the sacred and powerful as the place rather than the person (Sharp 1978).

A final chronological point made in the paper seems almost an afterthought, following the rich suggestions of the semiotic relationships of different motifs that form the body of the work. By concluding the paper with a narrow chronological issue, Taylor ensured that later discussions would treat it as a chronological essay. An annotated bibliography notes only that it is "technical. Differs with Spinden" (Kendall 1977: 292). The substance of the disagreement was the use by Spinden (1913) of monuments from Copan to construct a single chronology of the development of the ceremonial bar. Taylor (1941a: 57) argued that usage at Copan could well have been archaistic. In this, his argument foreshadowed the opinions of Rands (1969) and Proskouriakoff (1950), although neither of these later studies cite Taylor.

Rands (1969: 1) described Copan sculpture as a variant of that of the central Peten, as exemplified by the Leiden plaque and Stelae 1 and 2 from Tikal. The ties to these particular monuments provide a basis to suggest that this contact occurred in the Early Classic period. The "flaccid serpentine Ceremonial Bar" form found at Copan stems from this early contact, although it continues in use as a deliberate archaicism (Rands 1969: 518). The use of the ceremonial bar, Rands (1969: 520) argued, is a characteristic of a local style elaborated at Copan and contrasts with the holding of other objects at sites such as nearby Quirigua.

Proskouriakoff (1950: 89), in discussing the human figure holding the ceremonial bar (which she called the serpent bar), noted that the Leiden plaque provides a prototype: "It shows the two serpent heads connected by a flaccid body; this form survives into the Formative Phase of the Late Classic Period both at Tulum and at Copan, and is abandoned shortly after 9.11.0.0.0. Long before this time, however, in early Cycle 9, the rigid bar becomes the usual form." The chronological arguments in the conclusion of Taylor's article are hardly the most important contribution the article has to make; but they are the only part of the article that was clearly in line with the then-dominant concern in Maya archaeology: the construction of a chronological scale. The remainder of his study of these iconographic images, with its structural method and semiotic concerns, was outside the range of debate at the time. An exploration of Taylor's papers, preserved at the National Anthropological Archives at the Smithsonian Institution, helps explain how and why Taylor produced this unique study, so clearly out of keeping with the expectations of the day.

## WALTER TAYLOR AND ALFRED TOZZER

Walter Taylor entered the doctoral program at Harvard in 1938, as a protégé of Clyde Kluckhohn (then an assistant professor) and a student of Alfred Marston

Tozzer (long the leader in Maya archaeology and a senior professor). As Tozzer's student, he was exposed intensively to the practice of Maya archaeology at the time, an experience that led directly to the writing of his paper on the ceremonial bar. Reading the documents generated by his relationship with Tozzer, it is evident that the publication of that paper constituted a demonstration of Taylor's competence in Maya studies, a direct challenge to Tozzer and the field over which he presided.

Tozzer was described by Gordon Willey (1988) as a very strong-willed, demanding person, an impression that also comes through in his correspondence, housed in the archives of the Peabody Museum. Tozzer's early ethnographic fieldwork on the Lacandon Maya (Tozzer 1907) was supported by Peabody Museum patron Charles Pickering Bowditch in the hope it would assist in the decipherment of Maya writing (Hinsley 1984). It was followed by limited archaeological fieldwork in the Maya area and central Mexico (Tozzer 1911, 1913, 1921). Afterward, Tozzer left archaeological fieldwork to his Harvard students and the Harvard-affiliated projects of the Carnegie Institution of Washington, staffed in large part by former Harvard students. Instead of conducting new fieldwork himself, Tozzer devoted most of his research efforts to mammoth interpretive projects. Meticulous annotations for his edition of Bishop Landa's sixteenth-century account of Yucatec Maya society (Tozzer 1941), far outweighing the original text, and detailed arguments about the sequence of occupation of the site of Chichén Itzá by different ethnic groups, based on the study of representational imagery in all media from the site (Tozzer 1957), were to be his lasting legacy as a researcher.

Before he entered the Harvard program, Taylor had earned Tozzer's critical scrutiny. He requested a letter of support from Harvard to the Mexican government for his permit application for proposed fieldwork. On February 12, 1938, Clyde Kluckhohn wrote to say that "Professor Tozzer feels that as a question of principle and precedent your application ought to come from Yale. He didn't think that the Mexican government would be pleased with an application from an institution with which as yet you have no formal connection" (Kluckhohn 1938a).

Once at Harvard, Taylor undertook a course of study that can partly be reconstructed from the notes he preserved, and in many cases laboriously typed out. In spring 1939 he took Tozzer's course in Maya archaeology, which was based on detailed files of notes and illustrations, which Willey (1988: 277–278) memorably describes. The course Tozzer taught, based on these notes, emphasized the transmission of a vast and detailed body of propositional knowledge. Taylor's class notes for February 10 and 20 describe specific details of the ceremonial bar and its interpretation that he later took up in his published paper. But his first paper for this course, dated February 23, is on another topic entirely, Maya figurines, apparently reflecting a class assignment based on the galleries of the Peabody Museum. The paper consists of a listing of attributes recorded for a sample of figurines then on display, followed by a summary table of these attributes and a final two-page commentary. The ending paragraph warrants full quotation:

> But that brings us to the old theatrical query, "what's all the shootin' fer?" To make a classification *in vacuo* is a thankless, if not a pointless task. . . . Typological classification (as far as I can see) can serve only two purposes [variation at one time-space, variation in time and space]. . . . If the material is incapable of producing tenable implications along these lines, then classification is merely one man's viewpoint. . . . The upshot of all this is that, holding these ideas and having tested the material, I do not believe it is worthwhile to continue with a classification of the Maya figurines now in Room 34. I think that to do so would be merely to set on paper my own, rather ungrounded, opinions about them as objects, not as units having significance for culture content or dynamics or chronology. (Taylor 1939a)

Clearly, Tozzer was not charmed, as I was, by the blunt honesty Taylor showed in declining to continue the exercise and his measured dismissal of a make-work assignment. Penciled on the cover page of the paper was the note, "See me, please."

Although the documentary record is silent on any exchange between the two over this paper, the second paper Taylor wrote for Tozzer, dated April 11, 1939, took no such casual approach. The original draft of the ceremonial bar paper was a brilliant display of mastery of the data of Maya archaeology that, although based on the kind of taxonomic exercise Tozzer clearly valued, insisted on moving beyond classification and proposing historical, political, and semantic understandings of Maya symbolism. This new paper starts with a note that "it was originally intended that this discussion should be upon the Ceremonial Bar as observed on the monuments of certain Maya sites in the Peten and to the south. Once into the subject, however, I began to notice features on other types of ornament, which suggested connections with the Ceremonial Bar," thus changing his goal to outlining the connections among the materials typologically and then interpreting them in both chronological and cultural terms (Taylor 1939b). The paper was accompanied by a series of pen-and-ink drawings of monuments, some with reference to files of the Carnegie Institution of Washington, indicating that in preparing the paper he was in conversation with members of that project. That the Carnegie staff he consulted included J. Eric S. Thompson is clear from pencil notations in the margin initialed by Thompson. Having rejected the sterile exercise of classifying figurines in the museum cases, Taylor apparently undertook instead an original research paper based on both published and unpublished records of Maya monuments.

Even more than the cheekiness of his first paper, the ceremonial bar paper can be seen as a critique of Tozzer's methods and a demonstration of other ways to get results. It is not difficult to identify likely sources of inspiration for this work. Some tantalizing hints at broader connections go beyond what can be demonstrated from the documentary record but are worth noting as well.

## WALTER TAYLOR AND CLYDE KLUCKHOHN

Generations of readers of *A Study of Archeology* have discussed the role that Clyde Kluckhohn, with his own well-publicized critique of Maya archaeology (Kluckhohn 1940), might have played in Taylor's development of his ideas. Taylor had met Kluckhohn in 1937 in the Southwest. Kluckhohn himself had come to Harvard after studying in Europe and teaching at the University of New Mexico and had only received his Ph.D. in 1936 (Parsons and Vogt 1962). Kluckhohn was already Taylor's most significant advocate before he entered Harvard, intervening between him and Tozzer. A letter from Kluckhohn to Taylor dated July 13, 1938, suggests a very close relationship. Kluckhohn writes that he hopes "very much indeed that all I said and did in my too drunken state will not spoil things entirely" (Kluckhohn 1938b), indicating a degree of intimacy between professor and student out of keeping with the hierarchy of the time. In October 1939 Taylor communicated to Kluckhohn the fact that J. Eric S. Thompson had read the draft of the ceremonial bar paper, spurring him to say, "I guess I'll try to polish it up and see what can be done with it" (Taylor 1939c). Four days later Kluckhohn replied that he was "delighted to hear that Eric Thompson liked the ceremonial bar paper. You really ought to get it out as soon as you can" (Kluckhohn 1939a).

Taylor was clearly strongly influenced by coursework with Kluckhohn, who exposed him to a wider gamut of social thought than Tozzer, who Willey (1988: 283) fairly characterizes as "a Boasian historical particularistic" who "thought of anthropology as essentially straightforward cultural history and the faithful recording of the data that would pertain thereto." The contrast is vividly evident in the notes from Taylor's courses with the two men. In fall 1939, he audited Anthropology 26, Theory and Method, where Kluckhohn assigned readings from Ralph Linton, Paul Radin, Robert Lowie, Franz Boas, Alfred Kroeber, and both Malinowski and Radcliffe-Brown. It is here, as well, that we can document Taylor's opportunity to formally learn about leading semioticians.

Prominently listed in the sources recommended for this course was *The Meaning of Meaning* by C. K. Ogden and I. A. Richards, a classic study of language, thought, and semiotics published in 1923. Ogden and Richards proposed the famous "triangle of interpretation" or "semiotic triangle" as a model of the relations among reference (which they located in the mind), the sign, and its referent, and they called for pragmatics as the basis of interpretation (West 2002: 209). The connections of this work to the semiotics of C. S. Peirce are more than superficial. An appendix to the book included an introduction to his work based on two brief articles and letters sent to Lady Victoria Welby (Nubiola 1996).

Typescript notes from Kluckhohn's course also mention discussion of the philosophers Alfred North Whitehead and W. V. Quine, additional conduits for Peirce's ideas to reach Taylor.

When Taylor prepared his own courses for SIU in the 1960s, they included a proseminar in linguistics, which clearly shows the importance of his exposure, through Kluckhohn, to semiotics and structural linguistic models (Taylor n.d.a). Twelve of the sixteen weeks on the nature of language were based on structural theory. Eight more weeks after that concerned semantics, and two more weeks dealt with pragmatics. The bibliography centrally listed "Foundations of the Theory of Signs" by Charles W. Morris in the 1938 *International Encyclopedia of Unified Science* as the source reading for sign systems. That Kluckhohn was the source for Taylor's familiarity with this material is suggested, if not fully confirmed, by his Harvard course notes and syllabi and the coincidence of materials covered in this course and those he took from Kluckhohn. Parsons and Vogt (1962) have noted the importance in Kluckhohn's own theoretical work of linguistic models, particularly those based on Roman Jakobsen's version of linguistic structuralism. A second source of Taylor's familiarity with this material would be his Yale undergraduate coursework, documented by course notes from both Taylor and his wife, Lyda (Taylor n.d.b), on psychology and culture with Edward Sapir, whose 1921 *Language: An Introduction to the Study of Speech* provided the basis for the section of the course on grammatical structure and meaning.

Other course materials from Taylor's SIU days demonstrate the central place Taylor gave to precise definition of terms, grounded in his reading of broader social theory and philosophy, some of which can be traced to the notebooks from his courses with Kluckhohn. Jonathan Reyman (this volume) reproduces a listing of terms and definitions that Taylor used as part of Anthropology A505, a proseminar in archaeology obviously parallel to the proseminar in linguistic anthropology described above. Set apart among the more expected terms (theory, method, and the like) are five that bear clear traces of familiarity with the work of philosopher W. V. Quine: proposition, concept, percept, denotation, and connotation, the latter two defined in terms of extension and intension. Through Quine (and Whitehead), it is likely that Taylor learned of C. S. Peirce's semiotics, concepts that appear to be evident in his own approach to the study of the Maya ceremonial bar.

Taylor's study went far beyond any model for symbolic analysis then current in Maya archaeology and likely reflects either explicitly or implicitly frameworks for semantics and pragmatics presented by Kluckhohn in some of the first courses that Taylor took at Harvard. This body of reading, a largely unheralded legacy of Taylor's study with Kluckhohn, is ultimately what gives Taylor's work on Maya iconography the surprisingly modern sensibility that recommends it today. The same grounding in semantics, pragmatics, and structural linguistics no doubt contributed to its reception as something alien by the Maya researchers of his own day, whose lack of interest in these approaches may have been reinforced by the powerful critique of their practice soon to come in Taylor's dissertation.

## MAYA RESEARCH IN 1941

Despite winning praise from those who read early drafts, despite being reviewed and accepted for publication in the major American journal of archaeology, and despite being the product of a student of the dean of Maya scholars, Taylor's arguments were not picked up by his contemporaries, Carnegie-affiliated researchers J. Eric S. Thompson (1950) and Tatiana Proskouriakoff (1950), who established the baseline for later Maya research. As a result, the suggestions he made had no influence on the development of interests in Classic Maya costume as a symbolic medium. To understand the loss of Taylor's research contribution we must consider the dominant model of Classic Maya culture and the structure of the developing field of Maya studies that Taylor addressed in 1941.

The Carnegie Institution of Washington, the University Museum at Pennsylvania, and the Peabody Museum at Harvard were among institutions with active programs in Maya studies in 1941. The influence of the Carnegie Institution was considerable, as it supported a full program of research and publication. Becker (1979: 7) has noted that the archaeologists of the Carnegie Institution of Washington were not explicitly interested in constructing theories to explain the vast amounts of data they were accumulating. The aim of complete description (not yet regarded as naïve and unattainable) and the reigning emphasis in Americanist archaeology on culture history (Willey and Sabloff 1974: 88–130) combined to reinforce certain lines of study and to discourage others.

Chief among those encouraged, for Maya archaeology, was the elucidation of a chronological framework. One of the earliest commitments of the Carnegie Institution of Washington was to the recording of Classic Maya inscriptions, through the research of Sylvanus G. Morley (1920, 1938). The primary importance of the texts was to be their use as a means to establish chronology. In pursuit of this goal, Morley (1938) recorded only calendrical information in his *Inscriptions of the Peten* (Schele and Miller 1986: 23). The promise of detailed chronology implicit in the carved stone monuments of Classic Maya civilization was accompanied by a companion concern, to construct relative chronologies tied to this absolute, fine-grained dating system using other lines of evidence. At the same time, the failure of early Maya research to interpret the Maya inscriptions that were not calendrical blocked a fuller interpretation of the content of Classic Maya art.

As Becker (1979: 9) has noted, the only interpretive model of Classic Maya society current during this time was derived from the popular writings of

Thompson (1942). In the "priest-peasant" model that Thompson elaborated, the anthropomorphic depictions in Maya art represented gods, or their priests, who directed the activities of the peasants composing the rest of society. Attention to images in Maya art was seen by Carnegie Institution researchers primarily as an alternative route to chronology. For example, in her landmark "A Study of Classic Maya Sculpture," Proskouriakoff (1950: 2) describes her goal as "to examine variations in the Classic monumental style, which would furnish clues to the relative dates of the execution of individual monuments."

The chronological implications of Taylor's study were only a minor theme, one that served as a correction for the already clearly outdated "A Study of Maya Art" by H. J. Spinden (1913; cf. Rands 1955: 299, who characterizes Spinden's work as unsystematic). The innovative theme of Taylor's paper was the information that could be derived from the comparative study of the actual imagery itself, a perspective common today but under-emphasized in 1941. Proskouriakoff (1950: 88) did note that "the objects and accessories presented with the human figure probably express its function or office" but did not pursue this concern until much later, feeling, like other Carnegie Institution researchers, that establishment of chronology was of primary importance.

Taylor's paper is anomalous against the background of contemporary Maya studies and could only have been written by an outsider. His concern with the nature of certain costume elements worn by the figures in Classic Maya art, figures whose identity (as religious functionaries or gods) was *assumed* in the dominant models of the time, and his assumption that these costumes might have more than chronological and areal significance was not part of the underlying framework of Maya studies. Consequently, the implications the paper had for these topics could be ignored without raising difficulties for continued research. The chronological correction was unimportant, since Proskouriakoff's (1950) work established in more precise and exhaustive detail what Taylor could only suggest: the conservatism of Copan's art. Consequently, the Carnegie researchers, and others who followed in their footsteps, could afford to ignore Taylor's admittedly tentative beginnings.

## LATER MAYA RESEARCH AND WALTER W. TAYLOR

Taylor's study did not disappear without a trace. The two major bibliographies of Mesoamerican archaeology and art history included it (Bernal 1962: 311; Kendall 1977: 292). At least four discussions of topics in Maya iconography have referred to it (Kubler 1962; Dütting 1970; Quirarte 1981; Clancy 1994). Dütting, a German, and Clancy, Quirarte, and Kubler, American art historians, share one characteristic: they were not part of the central group of researchers involved in North American Maya studies, primarily associated with the Carnegie Institution of Washington, whose works form the basic corpus of Maya research from the

1940s. In one way or another, these researchers are marginal to the audience Taylor originally addressed and who chose to ignore his contribution. The way these authors refer to Taylor's paper is instructive.

In an article solely devoted to the ceremonial bar, Clancy (1994: 10) summarizes previous research on the theme, including that of Taylor, who she notes "studied the ceremonial bar in the same manner as Spinden, that is, as an iconographic complex with substitutions and variations," and correctly notes that he "believed its general meaning had to do with water symbolism."

Dütting, a German scholar, cites Taylor in his discussion of a possible textual reference to the ceremonial bar. Discussing the inscription of Lintel 1 from the site of Kuna-Lacanha, Dütting refers to Spinden and Taylor as his authorities for the distribution of "the ceremonial bar and double-headed serpent motifs found so frequently in Maya art" (1970: 210). Clearly, Dütting believed that Taylor's paper was a reliable compendium of instances of this image. At the same time, he specifically differs with Taylor in his interpretation of the ceremonial bar, which he identifies with procreation.

Quirarte (1981: 305) noted Taylor's (1941a: 49) identification of "the clearest representation of the tricephalic unit in Maya art" (in the Temple of the Sun at Palenque) as a ceremonial bar. Taylor (1941a: 53–54) noted that the elements represented in this composition were unusual, because they served as a base for other objects rather than being held by the anthropomorphic figures shown. Taylor included these examples with other ceremonial bars because of their formal similarity, as bars with tripartite ends, choosing to emphasize this continuity rather than the discontinuity (the three heads) that Quirarte discusses. Implicitly, Quirarte rejects the identification, since the ceremonial bar depicts a double-headed serpent; but nonetheless, he cites the prior discussion of this motif.

Kubler, arguably the most influential scholar of Mesoamerican art history, makes the fullest use of Taylor's paper. In a discussion of figural reliefs he notes that

> [t]he personages represent a succession of priest-rulers, whose rank is marked by the ornate serpent bar surrounding the figure, or carried in both hands. By this hypothesis, the bar symbolized the sky, and it conferred the status of "sky-bearer," or temporal governor, upon its possessor; after the fifth century its use was less common, and an effigy scepter replaced the bar, concurrently with the appearance of armed warrior figures in greater numbers than in Early Classic art. (Kubler 1962: 152)

In a footnote, he notes that the hypothesis is original, but "the evidence is collected" in Taylor's study (Kubler 1962: 342).

Kubler was unique in explicitly following through on Taylor's suggestion that the ceremonial bar was associated with a particular status and was able to suggest the nature of that status. The primary difference between the two studies lies in the interpretation of the meaning of the bar itself, for whereas Taylor emphasized its association with water, Kubler emphasizes its status as a cosmogram representing the sky. In fact, these two perspectives are complementary, tied together by the identification of the supernatural, in Maya thought, as composed of both a watery underworld and celestial upperworld, and the complementary view that mediation between the natural and supernatural realms was an elite prerogative and duty.

The common factor uniting these disparate scholars, unique in their acknowledgment of Taylor's study, is their separation from the Carnegie Institution of Washington, the source of the most authoritative works on Maya studies prior to 1960. It is, in fact, Carnegie researchers who were Taylor's contemporaries, who grappled with the issues he raised in the 1941 paper, and who might have been expected to cite it in their later works (e.g., Proskouriakoff 1950; Thompson 1950). Well-documented bitterness on the part of Carnegie researchers (Woodbury 1954) about Taylor's (1948: 46–67) devastating critique of their program in *A Study of Archeology* is in fact the most obvious explanation for the lack of attention to this article. Such a suggestion, however, may not be a sufficient explanation. Although it is obvious that no enthusiasm should be expected from Carnegie researchers following this critique, if Taylor's study had in fact offered information crucial to their own program of research, it would have been impossible for them to ignore it so completely. An equally important consideration must be the lack of fit between Taylor's concerns and those of the dominant school of Maya archaeology, a lack of fit that made the chronological implications of Taylor's conclusions a necessary, if awkward, coda.

## CONCLUSION

Taylor's paper is a remarkable contribution, written before its time. Forty years later, he would have had the copious new data provided by extensive projects, such as that carried out at Tikal by the University of Pennsylvania. The elaboration of the historic hypothesis suggested by Proskouriakoff in 1960 would have provided a context for a concern with the significance of variation in Maya costume as evidence of historical process. Improved understanding of non-calendric texts would have made possible the elucidation of the significance of exceptions that Taylor's structural method identified. In general, the connection of image to meaning would have been far more important to scholars in his audience, ensuring interest not only from the fringes of art historical scholarship but also from the mainstream of Maya studies.

Even today, some of the suggestions Taylor made are refreshing and stimulating. The structural analysis of costume leads to the implication that elaborately dressed human figures are somehow equivalent to elaborated icons like the world tree. By pointing out the equivalence of the feathered headdresses of so many Maya rulers and the bird that perches at the top of the world tree, Taylor offers a more ideological rationale for the predominance of feathers in Maya headdress than the commonsense notion that feathers were abundant and (in some undefined way) "valued." The identification of the basic naturalistic forms of certain Maya regalia as water imagery implies that power was inherent in objects derived from the watery underworld realm, lending a new cast to interregional exchange in marine products. Over the passage of almost half a century, we can look back and consider the intriguing suggestions Taylor made and reclaim a pioneering effort in structural analysis of the content of Maya art.

## ACKNOWLEDGMENTS

The original version of this paper was written in 1988, at the invitation of William Folan and Jonathan Reyman, who I thank for drawing my attention to Taylor's paper. Material from the Walter Taylor Papers, now in the National Anthropological Archives of the Smithsonian Institution, was reviewed at the NAA facility in December 2003, with the aid of Susan McElrath, and based on a preliminary finding aid prepared by Patricia Peñon. Boxes 53 and 122 of the Taylor papers contain the texts and figures of the original class paper and publication of the Maya ceremonial bar study. I greatly appreciate the assistance of the staff of the NAA in providing access to these papers. The Walter Taylor collection was redirected to the NAA by the Peabody Museum, Harvard University, while I was assistant director of the latter institution (1986–1989). In that capacity, I was informed that Taylor wanted the papers accessible to researchers. Although the NAA apparently lacks conclusive paperwork confirming a transferral of copyright from Taylor, I quote from the papers based on my firsthand knowledge of the circumstances surrounding Taylor's intention to deposit his papers in a publically accessible archive for the benefit of future scholars.

*Editors' note*: Natch Taylor, the youngest of Walter Taylor's three children, recently visited the NAA and was able to resolve the status of the W. W. Taylor papers. These are now cleared for general research and publication.

# WALTER TAYLOR'S CONJUNCTIVE APPROACH IN MAYA ARCHAEOLOGY

CHAPTER SIXTEEN

*Allan L. Maca*

Several groups of archaeologists working in the lowland Maya area currently are practicing what they label "conjunctive" approaches (e.g., Fash and Sharer 1991; Chase and Chase 1996, 2009; Sharer et al. 1999; Fash and Fash 2009). Some have advocated conjunctive research for the whole of lowland Maya archaeology (e.g., Culbert 1991; Fash 1994; Marcus 1995; Golden and Borgstede 2004a, 2004b; Sharer and Golden 2004; Buikstra, Miller, and Wright 2009; Yaeger 2009) and interest in conjunctive archaeology has spread within Americanist research (e.g., Rupp 1997; Dunning et al. 1998; Anaya Hernandez, Guenter, and Zender 2003; Joyce et al. 2004; Millaire 2004). There are two main focal points of conjunctive research in the Maya region: highland Guatemala (focused on the Postclassic period, AD 900–1524) and the Maya lowlands (focused mainly on the Classic period, AD 300–900). The conjunctive trend in the highlands is intriguing (e.g., Carmack and Weeks 1981; Fox 1987) and, as Yaeger and Borgstede (2004: 274) note, deserves its own analysis. Because it does not claim Walter Taylor as its progenitor, however, the highland phenomenon is largely unexamined in this chapter. The focus here is on the conjunctive archaeology of the Maya lowlands, its origins, character, and efficacy. My larger goal is to explore critical moments in the history of Maya archaeology that have shaped, and in some cases curtailed, the growth of theory encouraged decades ago by Taylor and Clyde Kluckhohn. Closing sections of this chapter propose that the conjunctive archaeology we are seeing is an unusual yet traceable expression of the Carnegie Institution's legacy in Maya archaeology.

## INTRODUCTION

Two relatively recent edited books, one on the Maya archaeology of Copan, Honduras (Bell, Canuto, and Sharer 2004) and another on Maya archaeology in the new millennium (Golden and Borgstede 2004a), claim Walter Taylor's (1948) conjunctive approach as their operational model and baseline for future research. Both volumes were produced by the "Pennsylvania group" in Maya archaeology, led by University of Pennsylvania professor Robert Sharer and consisting of other University of Pennsylvania luminaries and Sharer's former students.[1] Their foregrounding of Walter Taylor's conjunctive approach represents a new development in lowland Maya archaeology, that is, the adoption and support of a single, apparently coherent, archaeological framework with a known and named founder.[2] This trend was apparent at a 2009 SAA session honoring Robert Sharer, in which numerous presenters (the Chases, J. Yaeger, the Fashes, and J. Buikstra and colleagues) included the term "conjunctive" in their paper titles. This conjunctive school is building currency and can be considered a movement or possibly an emergent paradigm.

Joyce Marcus (1983, 1995) and William Fash (1994), both of whom were doctoral students of Gordon Willey at Harvard University, are participants in the conjunctive phenomenon as well. This highlights the existence of another conjunctive school, a "Harvard group," and suggests that tracing the origins of this movement requires exploration of scholarly work and time periods beyond the obvious and present. It seems surprising that the brainchild of a marginalized mid-twentieth-century scholar has emerged as a standard in current Maya archaeology. Historical aspects of this trend may seem even more surprising. For example, in their sporadic references to the conjunctive approach in the Maya area between 1965 and 1994, the senior members of the Pennsylvania and Harvard groups, including Willey (Willey et al. 1965), never cited Walter Taylor as its progenitor. Why was this the case? And what has changed?

This chapter seeks to address these and other questions and to fill in a number of gaps in this hazy segment of the intellectual history of Maya studies. No one has yet inquired as to how and why the conjunctive model entered Maya archaeology, or why it might possess staying power sufficient to emerge as a paradigm. Nor has anyone considered whether or not at Copan, for example, archaeologists are actually implementing the conjunctive protocols as Taylor envisioned them or if they are merely doing what seems necessary to validate research in an era when colleagues, native peoples, and host governments are questioning archaeology's aims and uses. More importantly, no one has explored whether and how it is appropriate to exercise a research strategy that dates to the pre–World War II era and that was both controversial and poorly understood at that time. I explore these issues by focusing on two periods of time in the history of American archaeology: the 1950s and the era after the New Archaeology had taken root, that is, after about 1965. The first period is characterized by a relative silence regarding Taylor's work; the second by recognition of Taylor's influence. Because of my emphasis on the 1950s, parts of the following discussion continue where Chapter 1 of this volume leaves off (please see that chapter for a more in-depth discussion of and introduction to the conjunctive approach and Taylor's 1948 book).

Part 1 of this chapter provides a brief overview of the general response to Taylor's *A Study of Archeology* (1948; hereafter referred to as *ASOA*), highlighting both the negative and the positive and focusing in particular on Alfred Kidder. I then look at the emergence of theory in American archaeology and then at Gordon Willey and his famous book, *Method and Theory in American Archaeology*, coauthored with Philip Phillips (1958). I consider the relationship between their book and Taylor's *ASOA* and explain how *Method and Theory* was a competing document as well as the politically safe touchstone for the New Archaeology. In closing the first part of this chapter I look at the relationship between *Method and Theory* and Binford's birthing of the New Archaeology, especially in terms of issues of attribution and how Binford has dealt with Taylor's 1948 book over time. Part 2 of this chapter looks at the development of the conjunctive approach in Maya archaeology and especially at the varying interpretations of the approach at Copan. In light of Kluckhohn's (1940) and Taylor's (1948) critiques, Part 3 considers how the history and variety of conjunctive approaches influence and reflect theory development in lowland Maya archaeology.

Because no substantial assessments of the conjunctive approach have ever been published, there exist no significant resources for considering what Taylor's overall and specific contributions have been. The present volume begins to close this rather large gap. Some scholars (e.g., Hudson 2008) maintain that *ASOA* garnered the attention it did mainly because of its famous and inflammatory critique, not because it offered cutting-edge or visionary ideas and protocols. Recent trends in Maya archaeology teach us to question such dogma, for it is evident that we are still discovering in American archaeology identifiable expressions of Taylor's influence and vision. In many cases these expressions are odd, distorted, and/or obfuscated—exactly what we should expect given the stunning force of Taylor's dissension and the strange and opaque place Taylor and his book have assumed in American archaeology.

### *Analytical and Conceptual Approaches Employed in This Chapter*

It is easy today to underestimate—or even to ignore—the effects of Taylor's famous work (1948), for the sweep of history often obscures individual distinction;

moreover, from about 1958 on we see that dominant evolutionary models of culture change are not encouraging where questions of agency are concerned. Taylor's book suffered a long period of neglect—a general silence—in terms of citations and interest, from the early 1950s to the mid-1960s (Sterud 1978; and see Chapter 1 of this volume). Nevertheless, by the time Caldwell (1959) published his pivotal article on "The New American Archeology," Taylor was recognized as a leader in the field and many of his ideas and recommendations could be identified as the basis of the new movement. The idiom of this movement, however, had evolved beyond Taylor, and other players—rising young stars—had stepped into the limelight since 1948. The field *had* changed in ten years, and quite dramatically.

*ASOA* did gain serious attention, and relatively quickly, but this consisted mainly of extremes, that is, either terrific support and accolades or outright denunciations. The more general response in the 1950s was typically muted, delayed, oblique, or under-the-radar. With the exception of Glyn Daniel (1950), who was so impressed with the first chapter of *ASOA* (on the history of archaeology) that he cited and discussed it repeatedly, no scholar spent any length of time or devoted substantial printed space to *ASOA*'s merits and shortcomings. The longest direct response was a mere five-page book review by Richard Woodbury (1954), offered in large part as a defense of Alfred Kidder. This lack of response should strike a careful observer as odd. If everyone read *ASOA* and talked about it, as even Woodbury (ibid.) admitted, if ten years later Taylor was cited by Caldwell in the journal *Science* as a leading figure, and if ten years after that scholars were openly acknowledging Taylor's seminal contributions to the New Archaeology, why is it that no one ever summoned a formal rejoinder or, excepting Melvin Fowler (1959), an application of the conjunctive approach in the first decade after its publication? What was going on?

Some (e.g., Hudson 2008) have seen this not as a silence but as an absence of interest and influence. This view is problematic, however, in light of countless statements to the contrary (see Chapter 1, this volume); the last twenty to thirty years of critical interrogations of the history and sociopolitics of Anglo-American archaeology; and the insights of several thinkers who have examined and theorized the power dynamics that shape and are shaped by academic, institutional, and disciplinary discourses. In scientific interpretations of the past, and in academia, many voices are excluded, muted, or, in the case of Taylor, excommunicated. Archaeology operates in a living context and as such reflects and shapes present-day concerns and formulations of power, including especially those within the discipline. Important works by a range of scholars (e.g., Deloria 1970; Conkey and Spector 1984; Leone 1984; Trigger 1984; Shanks and Tilley 1987; Ucko 1987; Arnold 1990; Gero and Root 1990; Dowson 1998; Thomas 2000; Gosden 2001) demonstrate this quite clearly.

Scott Hutson (2002; 2006) and Eugene Sterud (1978) are among a small group of archaeologists who have undertaken quantitative studies of citation patterns to understand disciplinary trends and biases. Their articles serve as support for the analysis here. Hutson's work consists of refined statistical studies that explore how gender and other biases in archaeology have been played out and are reflected in patterns of publication and citation in leading journals. His research provides enormous support for the importance of studying attribution patterns over time. Moreover, his proposal for exploring "citing circles" as a prestige tactic of the "relational self" in archaeology is crucial (Hutson 2006: 15). Sterud, writing before postprocessualism gained a foothold in the United States, used graphic analyses to show how we can track patterns of attribution in a leading journal and in so doing interpret larger intellectual trends, paradigms, and/or biases in the acceptance of major research. Sterud's data on Taylor's book are intriguing, especially with regard to the silence I highlight (see also Chapter 1, this volume).

Although I follow Sterud and Hutson in addressing patterns of attribution in social and disciplinary contexts, my analysis is exclusively qualitative and textual and is geared more toward issues of discourse practice. I cite and support their work in large part to suggest future areas of research, not because I undertake forms of study patterned on theirs. My approach borrows from analyses and theories of discourse in the fields of history, linguistics, and anthropology and centers on the construction of power. Thus, for example, in my discussion of Maya archaeology I consider what prominent, well-circumscribed groups of archaeologists say regarding justifications and orientations for their research. I also explore how and why they do (and do not) cite one another and how they characterize one another's work within the context of changing historical trends in the field. In this way, I am able to examine discursive practices over time. When I ask (as above) what was going on in the field such that the publication of Taylor's book met with general silence, I am exploring relations of power on disciplinary, institutional, and individual levels. Drawing on Michel Foucault, Norman Fairclough, and others, I consider how these relations have transpired to, on the one hand, establish silences and repression and, on the other, drive a "will to truth" based on alternative (non-Taylorean) claims to knowledge production. The focus is on Mesoamerican and Maya archaeology and the bulk of this analysis is taken up in the concluding sections of this chapter.

## PART 1: TAYLOR'S CRITIQUE AND THE CONJUNCTION OF HISTORY AND HISTORIOGRAPHY

### *Early Responses and the Fabulous 50s*[3]

The silence that greeted Taylor's book cannot be explained by a lack of interest—far from it—or by any other among a range of exclusive or combined explanations (e.g., Watson 1983; Reyman 1999). His book was both a hit and a shock, and this was the problem. It intimidated those who could not understand it; it aroused anger and fear in those whose life's work or mentor's work was dissected; and it provoked envy in those who did not have the courage, the foresight, or the intellect to have taken such a step themselves. At the time of its publication, even Taylor's most vociferous detractors were obliged to admit that *ASOA* "is the first comprehensive and systematic attempt that has been made to formulate a discipline for the practice of archaeology in the North American field" (Burgh 1950: 114) and that it "stands as American archaeology's most ambitious self-analysis and most elaborately proposed reorientation" (Woodbury 1954:292).

Gordon Willey (1953a: 368), in the year his famed Virú Valley report was published, states in no uncertain terms that "Taylor's critique has had a salutary influence on American archaeology. The old problem incentive of chronology and distributions of 'cultures' in terms of a few marker 'fossils' (usually potsherds) was not sufficient to attract archaeologists who were also anthropologists. Taylor's strictures helped crystallize this feeling of discontent."

Several others expressed unequivocal praise. Glyn Daniel, besides acknowledging Taylor in his 1950 book, also briefly reviewed *ASOA*. The eminent British archaeologist wrote that it is "welcome as a most distinguished, thoughtful and thought-provoking study. . . . A very important book, which should be read carefully, and pondered over by every European prehistorian—and archaeologist" (Daniel 1951: 82–83). As well, the prominent Americanist, Carl Guthe (1952: 12), declared, "The most comprehensive statement calling for a critical evaluation of current archaeological procedures and a reorientation of archaeological research has just been published." Moreover, Guthe used Taylor's work as the final important development in the history of eastern U.S. archaeology between 1925 and 1950 (ibid.). In Guthe's view, Taylor effectively closed an entire era of archaeology in the eastern United States.

Emil ("Doc") Haury, a leader of American archaeology and one of the six scholars Taylor criticized, was sufficiently impressed with Taylor's ideas that he chose to put differences and emotions aside. He included Taylor in an important edited volume (Haury 1954) and, more significantly, contributed to a book edited by Taylor (1957b) that may rival *ASOA* as the most important of Taylor's career. This latter was a brief book on non-artifactual materials, published by the National Academy of Sciences, with contributions from many of the most prominent archaeologists of the 1950s. It is one of Taylor's least well-known and least publicized works, but an easy argument could be made for it being Taylor's most significant as well as one of the most influential archaeological publications of the 1950s (e.g., see Jennings 1959). It clearly demonstrated that Taylor was recognized as one of *the* young (he was forty-three years old) leaders of archaeological science in the United States and that he could attract the attention and opinions of the top scholars—including a couple of his esteemed targets, namely, Haury and James Griffin.

More important than the roster was the content of this 1957 publication. The contributors discussed interdisciplinary research and the importance and nature of specialists' contributions to the field. Never before had such a focused effort been undertaken to codify the type of scientific cooperation that would rapidly become the basis and starting point for anthropological archaeology. Alfred Kidder had encouraged a multidisciplinary "pan-scientific" approach while at the Carnegie Institution of Washington (Kidder 1928; Weeks and Hill 2006: 12), and the paleontologist John C. Merriam, president of the Carnegie from 1920 to 1939, suggested multidisciplinary research as basic to science (Castañeda 2005). Taylor, however, in cooperation with the National Research Council, took such interests to an entirely new level, emphasizing more an interdisciplinary approach than multidisciplinary (see Watson 1983; Chapter 1, this volume; note 19, this chapter). That *ASOA* could serve as a theoretical and methodological structure for such interests was an added bonus.

In sum, despite the fact that Taylor lambasted powerful senior scholars, had difficulty finding a job, encountered only superficial published response to his controversial book, and already was struggling to produce his example of the conjunctive approach, he gained recognition both in the United States and abroad. A number of scholars recognized *ASOA* as a treatise of major significance and its author as a leader with the potential to shape the future of the field. Unfortunately, however, Taylor's critiques invited immediate and protracted public and private reprisals and set the tone for his treatment by colleagues and for his career options. The greater part of this backlash owed to his attack on Kidder, a man admired by colleagues and their students, as well as by staff archaeologists at the Carnegie (CIW). Many of these staff depended on Kidder for their livelihood and research and had been trained within the structure of Kidder's home institution, Harvard University.

Kidder held the position of chair of the Division of Historical Research at the Carnegie. As Pat Watson (this volume) cogently discusses, Taylor (1948) reserved the majority of his criticisms for Kidder and the Carnegie projects. Thus, we are not surprised to see that the most clamorous critics of Taylor's book were Robert Burgh (1950) and Woodbury (1954). Burgh worked with Earl Morris in the La Plata River area of Colorado and New Mexico as well as with the Carnegie's Division of Historical Research at Copan, Honduras, where he is known for his revised map of the Copan Valley (Longyear 1952). Woodbury, a Southwestern archaeologist, was a close colleague of Kidder who later served as Kidder's biographer (1973a; see also 1993).[4] In his famous review of *ASOA* Woodbury writes (1954: 293), "Seldom does Taylor temper his criticism with recognition of the fact that Kidder has often set standards far ahead of those of his contemporaries; his 'failures' have been only in relation to Taylor's ideals." He goes on to say (ibid.),

> It hardly seems a justifiable procedure to condemn a scholar, in archaeology or any other field, because his accomplishments fall short of his ambition. In

spite of the justice of some of Taylor's specific points, he leaves the wholly false impression that Kidder's work in particular and Carnegie's in general have been largely wasted effort, devoted to trivial purposes, and achieving little that can contribute to anthropology in general.

Ironically, for decades after the publication of *ASOA*, scholars criticized Taylor for falling short of *his* ambitions, that is, for never publishing his Coahuila report, planned as the grand example of his conjunctive approach (e.g., Martin 1954; Woodbury 1954: 296; Reyman 1999; Flannery 2001; cf. Taylor 1972c, 2003). Taylor (1972c: 30) responded to his critics by bemoaning the fact that no one, including his detractors, ever evaluated *ASOA* on its own terms by producing a dispassionate, thorough appraisal. The problem, however, was that if someone had published a serious evaluation of Taylor's ideas, this might have empowered Taylor and given legitimacy to his critiques. This in turn would have been a grave affront to disciplinary leaders and their formidable cohort. A closer look at Alfred Kidder serves to illustrate this point.

## ALFRED KIDDER

Walter Taylor (1948) offered both blunt and sharp criticisms of leading archaeologists, to the extent that even today his diatribes, in word and image, linger in the conscience of American archaeology. He laid bare, assessed, and found wanting the work and research orientations of senior men whose contributions and names we still recognize: Doc Haury, Frank Roberts, William Webb, William Ritchie, Jim Griffin, and, of course, Kidder (see Taylor 1948: 68–90). The criticisms, however, were not distributed evenly. The great bulk fell on Kidder—*the* leading figure in American archaeology before World War II.[5] Taylor (1948) disapproved in general of the prevailing tradition of time-space systematics[6] in American archaeology, the major practitioner of which was Kidder. In critiquing his work, Taylor was cutting directly at the foundational arbor upon which Americanist archaeology had branched and burgeoned before the Depression and, especially, during the New Deal era. Building from his criticisms of Kidder and others (*ASOA*, Chapter 3), Taylor used his conjunctive approach (*ASOA*, Chapter 6) to advance a wholly new direction of intellectual growth, one based on theoretical concepts tied to context, function, culture, and historiography. However, his ideas, as well as his fervid and upstart voice, were soon censured, particularly by those who, like Burgh and Woodbury, were aligned with or defended Kidder in print and in private.

### *Harvard University*

Taylor earned his Ph.D. (1943) at Harvard, where Willey accepted a prestigious chair in 1950 and eventually succeeded Kidder as the dean of American archaeology. It has often been noted that Taylor's critique in *ASOA* was stinging because it was a critique that arose from within the field of American archaeology, executed by one of its newly minted members. His criticism of Kidder, however, combined with the critiques by Harvard anthropologist Clyde Kluckhohn (1940), should also be seen as an internal critique of archaeology at Harvard and its associated institutions, namely, the Peabody Museum and the Carnegie's Division of Historical Research, which was housed near the Peabody on Harvard's campus. As discussed briefly in Chapter 1 of this volume, Alfred Tozzer, one of Taylor's dissertation advisors, had a pivotal, if still poorly understood, role in this critique. Based on an account by Michael Coe (2006: 114), we know he had a low regard for Kidder (and for Carnegie archaeologist Sylvanus Morley). It is thus likely that Taylor's critique of Kidder was partly a manifestation of localized debates and animosities embedded in the archaeological and anthropological culture of Harvard.[7] Sixty years ago, Southwestern archaeology and "Middle American" studies were the main research areas for the Harvard-trained archaeologists working in the New World; both were spearheaded by Kidder.[8] To gain some sense of the seriousness of Taylor's assault at that time, it helps to consider Kidder's reactions to *ASOA* as well as what we may conclude about its effect on his legacy and his health. This is best done in the context of Kidder's identification with Harvard, the institutional source of the Kluckhohn/Taylor assault.

Alfred "Ted" Kidder (b. 1885) was the quintessential "Harvard man." A member of a prominent Boston family, Kidder was raised in Cambridge, Massachusetts, and received his A.B. (1908), A.M. (1912), and Ph.D. (1914) from Harvard, having studied anthropology and archaeology with Frederic Putnam and Alfred Tozzer (Willey 1988: 306). Kidder was primarily Tozzer's student, and after completing his graduate work in Southwestern archaeology, much of which was funded by Harvard's Peabody Museum, he took a job at the related R. S. Peabody Foundation in Andover, Massachusetts.[9] Kidder's 1929 appointment to the head of a Carnegie division[10] placed him permanently in an office across the street from Harvard's Peabody Museum, where he eventually served as a member of the board. His home was in Cambridge, and not long after Gordon Willey's arrival at Harvard in 1950, he moved into a new office on the museum's fifth floor.

Willey recalls that the move to the Peabody (two years after the publication of *ASOA*) coincided with Kidder's recovery from some undiagnosed illness (ibid., 297). Aged sixty-five, Kidder had been ill for years, had a stroke, and then improved before suffering a heart attack some years later. Willey's short biography of Kidder alludes to the effects of Taylor's critique on Kidder's emotional well-being. Although Willey could never know in any complete way how Taylor's criticisms affected Kidder's health, it is clear from several of Willey's reminiscences that Kidder was profoundly hurt by the publication of *A Study of Archeology*, not to mention the attention it initially received (ibid., 299). Willey (personal communication, 2000) saw that Kidder was the sort of "sensitive" type to have been gravely affected by such a refutation of his life's work (see also Woodbury 1973a: 76), and he concedes that Taylor's book may have facilitated Kidder's punctuated but certain decline in health shortly after its publication. This can be considered in a more serious light when one recalls that in the same year the American Anthropological Association published *A Study of Archeology*, Vannevar Bush, president of the Carnegie, declared the Division of Historical Research to be unscientific and not worthy of sponsorship beyond 1958.[11] Referring to his discussions with Kidder about his leadership and life at the CIW, Willey (1988: 304) writes:

> Quite understandably, Kidder was deeply hurt by what had happened. It added to this sense of failure that beset him at times—to my mind a neurosis of his that was quite unjustifiable in the light of his great gifts and many extraordinary achievements. Thus, it is, perhaps, no wonder that Kidder cut short his autobiographical narrative as we moved into these last years of disappointment with the Carnegie. In fact, he refused to talk about this termination of the Division of Historical Research; what I have related here I learned from others, not from Kidder himself.

In 1950, Kidder stepped down from his post, blaming himself for American archaeology's failure to steer a firmer course toward holism and science.[12] From his new office in the Peabody Museum (and from his sickbed) he watched while Harry Pollock wrapped up the final eight years of Carnegie research in the Maya area.

As chair of the Carnegie's Division of Historical Research, Kidder had been in a position to mandate financially, organizationally, and intellectually the kinds of protocols and interdisciplinary research that Taylor saw as essential. Therefore, Taylor's (1948) critique of Kidder—rendered more severe than his 1943 dissertation version (Watson, this volume)—was directed at the only individual who could have at that time applied the extensive and intensive protocols that defined the conjunctive approach. This may partly explain some of Taylor's motives, for he wanted either to shatter the Carnegie program (and the work he saw as inadequate) or to force radical change in the Carnegie's approach, or both.[13] It is also possible that Taylor launched the attack because he thought that he himself should be appointed the new chair. Kidder was aging; if Taylor's program was recognized as more scientific and rigorous, he might have been granted the power and means to apply and disseminate his conjunctive strategy (see also Watson, this volume). As it turns out, Taylor never found or summoned the resources to publish or exercise his approach, although it is likely, as Coe (2006: 114) claims and Weeks and Hill (2006: 15–17) suggest,[14] that Taylor's criticism dealt the final blow to archaeological research at the Carnegie. Everyone at the time was familiar with his notorious critique, and we can be sure that Vannevar

Bush, as president of the CIW (1939–1955), had a vested interest in reading a major published censure of his institution.

Whatever Taylor's true objectives, an argument can be made that the critique of Kidder was not intended to be personal at all but rather that it merely was leveled at the most powerful and influential American archaeologist because of what he could effect in terms of large-scale change in archaeological practice. In the future it will be useful for historians of American archaeology to look more carefully at the impact of Taylor's words on Kidder and others, by accessing archives, yet-to-be-written books, biographies, and so forth. It may be most rewarding to do so as a means to further address the "negative" evidence—the silences and the disapprobation—which demonstrates that many archaeologists of the day did comprehend the significance of Taylor's prescriptions (e.g., Martin 1954). This comprehension flabbergasted many and rendered them silent, and it provoked others to practice and think about archaeology in new and different ways. As I discuss below, Gordon Willey was one scholar who expressed an unusual middle ground between these two types of response. He understood that theory had been too long neglected and that its day had come. The impacts of the critiques by Taylor and Clyde Kluckhohn were solid evidence of this.

### *Kluckhohn, Taylor, and Theoretical Provocation*

Taylor had an array of goals for both his critique and his conjunctive protocols. Yet, one goal stood above all others: to bring archaeology closer to anthropology. This was to be achieved by encouraging the development of theory. Taylor supported novel theoretical frameworks based on a well-drawn definition of "culture"; an attention to historiographic context; an anti-positivist epistemology stressing the importance of "construction" (versus reconstruction); and the continuous formation and testing of hypotheses. These were *not* standard archaeological goals in the first half of the twentieth century, and although Taylor was not alone in suggesting changes in archaeological practice (see, e.g., Strong 1936; Steward and Setzler 1938; Kluckhohn 1940), he was the first individual to propose an elaborate, varied, and cohesive array of theory and to do so in a treatise-length monograph.

It is certain that some of the stimulus for Taylor's book derived from the writing, teaching, and urging of his mentor and dissertation advisor, Clyde Kluckhohn. One of the preeminent cultural anthropologists of the twentieth century, Kluckhohn (1940) published a critique of Middle American (i.e., Mesoamerican) archaeology, with a special emphasis on Maya studies and the research records of the Carnegie and Kidder, as well as of Harvard-trained archaeologists in general, including Tozzer. The tenor and orientation of this critique are apparent throughout his short paper, the closing statement of which reads, "Factual richness and conceptual poverty are a poor pair of hosts at an intellectual banquet" (ibid., 51). A few select, slightly meatier passages[15] serve to further illustrate his tone and goals:

> I should like to record an overwhelming impression that many students in this field are but slightly reformed antiquarians. To one who is a layman in these highly specialized realms there seems a great deal of obsessive wallowing in detail of and for itself. . . . [T]he industry of workers in this field is most impressive as is, for the most part, their technical proficiency in the field and the scrupulous documentation in their publications, but one is not carried away by the luxuriance of their ideas. (ibid., 42–43)
>
> . . . [T]he greater number of students in the Middle American field ignore the categories "methodology" and "theory" almost entirely in so far as one can judge from their published writings. If they use the word "theory" at all, they tend to use it as a pejorative synonym for "speculation." No anthropologist, however, can perform intellectual operations without some reference to the logics of scholarship in general and a theoretical system of premises and concepts pertinent to the data of anthropology. (ibid., 44)

Kluckhohn thought the main problem in Maya and Mesoamerican archaeology was the lack of a conceptual framework—a theoretical structure—for orienting research and guiding the collection and study of data. Every practicing Maya archaeologist at the time read Kluckhohn's critique and it remains frequently cited as an example of early discontent with Carnegie research and with American (especially Mesoamerican) archaeology more generally (Longacre 2000; Golden and Borgstede 2004b; O'Brien, Lyman, and Schiffer 2005).[16] Further, because he was an established thinker and worked mainly outside of archaeology, Kluckhohn did not hesitate to name names. His paper explicitly criticizes George Vaillant, Alfred Tozzer, Samuel Lothrop, Herbert Spinden, German scholars in general, and the "Pennsylvania group" (1940: 83). He also offers lengthy criticism of Kidder, although from time to time inserting collegial statements of qualified praise before launching his points, as in "[n]o one, of course, has greater abhorrence of an archaeology which is on the intellectual level of stamp collecting than Dr. Kidder" (ibid., 81). On the whole, it is clear that Kluckhohn inspires Taylor's later critique and serves as a sort of vanguard in the attack on prewar practices.

Nevertheless, it was Taylor who took the embers from Kluckhohn's hearth and lit a torch that would ultimately put his own career and reputation at risk (see Taylor 1973a; Reyman 1999; Chapter 1, this volume). But Taylor did not do this simply or primarily to expose poor standards, shatter ideals, and pay homage to Kluckhohn. Taylor had been building a theoretical framework of his own, developed during research, fieldwork, and teaching in the 1930s and 1940s. He had studied with Tozzer, published on Maya art, read widely, and been field trained with many of American archaeology's young leaders, including Willey. Taylor was an archaeologist trained in anthropological theory; he knew what was lacking in the Americanist field and conveyed his professional perspective on how to resolve existing dilemmas. The problem for his legacy, however, was the fact that others thought they had solutions as well. While Taylor was busy after the publication of *ASOA* defending his work and trying to salvage his damaged career, other less controversial, mainstream practitioners used the emergent dialogue on theory to advance their own ideas. The most persuasive and prolific of these was Gordon Willey.

## GORDON WILLEY

### *Theory and Paradigm Making*

In his memoirs, Willey recalls (1988: 299) discussing Taylor with Kidder on several occasions. In the passage below we see that Willey valued Taylor's perspective in *ASOA* and understood that the Carnegie approach was antiquated. He tried to convince Kidder to see beyond Taylor's perceived affront to recognize the importance of *ASOA*'s message regarding cultural change and theory.

> [Kidder] shied away from either agreements or arguments about the causal processes that might have been operative to produce the parallels [between ancient institutions in Mesoamerica and Peru]. I recalled some of Walter Taylor's (1948) criticisms of Kidder, to the effect that while he spoke out in general for the need to understand the ways in which the various civilizations of the world had developed or decayed, he never did much about it. That is, he did not appear to be concerned with "how" and "why" questions in his own research, or in that under his direction in the Carnegie. . . . I tried, without success, to get him to see Taylor's point of view in the light of the kinds of questions I have just referred to. One difficulty was that he had been deeply hurt by Taylor's attack. He saw it as more personal than it was. Kidder, as the major figure of the Americanist archaeological establishment, had taken the brunt of what had been a more general criticism of traditional "ceramic chronology" archaeology. I told him that he was taking the criticisms too hard. Archaeology and the archaeological profession were changing. The number of professionals was increasing; argument and debate was [*sic*] going to become ever more frequent; and some of it was going to become pretty acrimonious. I pleaded that Taylor had a point. A search for cause did have a place in prehistory. True, historians had been arguing since time immemorial about "cause" in history, and it remained to be seen how much agreement we might come to on the issue in archaeology. Still, in my opinion, I felt that it was a part of the human condition to ask "how" and "why" questions and that it would be foolish to ask archaeologists to refrain from doing so indefinitely.

Willey was touched by the conceptual scheme of *ASOA* and came to clearly understand the importance of theory for archaeology. He ultimately borrowed liberally from *ASOA*'s message; however, he never attempted to follow Taylor's specific prescriptions. After all, Willey had been an important young practitioner of the "old ways" and these were regional and comparative—only part of what Taylor recommended. Willey nevertheless understood that the Taylor debacle had exposed the weaknesses in American archaeology and that the new dialogue would provide him a long-awaited opportunity to go beyond traditional practices to address issues of theory—suddenly a legitimate pursuit. Ten years after the publication of *ASOA*, Willey and Phillips (1958) published what would become the theoretical centerpiece of a new and emergent evolutionary archaeology in the Americas (see Sterud 1978: 301; Leventhal and Cornavaca 2007). They did not draw explicitly on Taylor's conjunctive approach; they mainly based their mission on their own earlier work, emphasizing regional comparisons on a large scale. It seems that Willey had been waiting for someone to drag the patient onto the table, so that he could go in and operate. He had certainly taken to heart the major themes and lessons that accompanied the launch of Clyde Kluckhohn's famous, if brief, assault on the leaders of Maya archaeology. He also paid close attention to the tenor and results of Taylor's critique, read *ASOA* with care, and assimilated many of Taylor's ideas seamlessly.

### *Silences, in Retrospect*

In a 1994 interview in *Current Anthropology*, David Freidel asked Gordon Willey to identify the major turning points in American archaeology that he experienced during his lifetime. Willey cited only one: "Walter Taylor's book (1948) and his emphasis on contextual archaeology" (Freidel 1994: 63). Willey goes on to explain:

> [Taylor] and I were agemates, and I knew him way back in the thirties. I had an opportunity to read the manuscript of his book before it came out. We talked about it at some length. Certainly he urged the contextual broadening of what we were trying to do. He focused on the actual finds in archaeological context, in and on the ground—the relationships of one artifact to another, and so forth. All of this was carrying things beyond what had been customary—beyond the traditional typological and classificatory arrangements of the data—and certainly I think that book influenced me. In turning towards settlement patterns, I too was broadening the context of the archaeological information.

Although their careers went in very different directions, Willey and Taylor initially enjoyed relatively parallel life tracks. They were each born in 1913 and became close colleagues in the New Deal era when they worked together on the WPA excavations in Georgia under Arthur Kelly (Willey 1994). They entered graduate school a year apart and knew one another quite well in the 1930s and 1940s, having shared correspondence and drinks on numerous occasions. (For example, recalling a barroom altercation in Georgia, Willey noted with a smile that Taylor was a bit of a "pugilist" [Willey, personal communication, 2000].)

Willey's acceptance and understanding of the conjunctive approach was and has remained incomplete (ibid.). Nevertheless, around the time (ca. 1946) that he began his pathbreaking settlement pattern study in the Virú Valley of Peru, he read an early draft of Taylor's *ASOA*, discussed it with Taylor (Taylor 1948: x; Freidel 1994: 63), and appreciated it (e.g., Willey 1953a, 1988; Freidel 1994). Beginning in 1966, in a retrospective discussion of the field, Willey acknowledged that Taylor directly influenced his settlement pattern studies, especially Taylor's emphasis on context (Willey 1966, 1968, 1988: 299; Willey and Sabloff 1993: 209) and interest in "small mounds" (Willey, personal communication, 2000; see Taylor 1948: 52). Before that time, however, Willey was noticeably silent with respect to the influence of Taylor on his own work.

For example, a 1953 paper (Willey 1953a; see above quotation) briefly recognizes Taylor's advances in contextual studies, but in his classic report on Virú Valley settlement patterns Willey (1953b) never once cited Taylor. Considering the fact that numerous scholars, including Willey himself, later recognized Walter Taylor's book as a major force in the emergence of settlement studies, this is a striking omission. Toward the end of the 1950s, in the grand statement of his own theoretical approach (Willey and Phillips 1958), Willey again chose to ignore (and even override) Taylor. In the following discussion, I show that he did this so as not to distract attention from his own agenda and in order to avoid association with a sociopolitical landmine. We can hardly blame Willey; no savvy young scholar would have chosen to celebrate or openly borrow Taylor's ideas, especially if he sought career advancement. The case of Willey's early silence regarding Taylor allows a fascinating look into the intellectual and social climate of American archaeology at that time. It also helps to elucidate subsequent discussions of how and why the conjunctive approach has endured in Maya archaeology, the lone example (in name) anywhere in the world, and to understand why Taylor's influence was squelched.

### *Method and Theory in American Archaeology*

Willey and Phillips (1958) sought to promulgate a competing theoretical model for American archaeology, an alternative to Taylor's, that would be seen as a unique formulation. They used their book, *Method and Theory in American Archaeology* (hereafter referred to as *MTAA*), to supersede Taylor's conjunctive approach and to establish their own ideas as the foundation for a new practice of archaeology (see also Leventhal and Cornavaca 2007). Needless to say, they were wholly successful in this: Lewis Binford (1962) began his "New Archaeology" revolution by claiming *MTAA* as its benchmark and call to arms.

Like Taylor, Willey and Phillips had developed their ideas considerably earlier than the year in which their book was published (1958). Phillips's thoughts on the topic of regional interactions can be traced back to 1940, when he published

"Middle American Influences on the Archaeology of the Southeastern United States." Willey published his related ideas in his 1948 paper "A Functional Analysis of 'Horizon Styles' in Peruvian Archaeology" (see also Willey 1951). These two scholars had overlapping interests in theory and methodology that led to a series of articles (Phillips and Willey 1953; Willey 1953a; Phillips 1955; Willey and Phillips 1955) that formed the basis for the publication of *MTAA*. However, generating, let alone publishing, articles on theory in the 1940s was a difficult undertaking, especially for a young scholar. As Kluckhohn (1939b, 1940: 44) and others (e.g., Willey and Sabloff 1993) have noted, in the 1940s, theory was akin to speculation and thus discounted, especially by senior archaeologists and anthropologists who dictated the acceptability of research and publications. Although many saw Taylor's work as opening a breach in the discipline for the consideration of theory, many other scholars, and especially Kidder, felt that Taylor's book justified their aversion to theory. Willey highlights this when reflecting on Kidder's response to his "Horizon Styles" paper. Having only recently arrived in Cambridge (ca. 1950), Willey was eager to know Kidder's opinion of his exciting theory, only to be disappointed: "Kidder definitely didn't like it. He told me that it reminded him too much of Walter Taylor" (1988: 296).

*A Study of Archeology* (1948) and *Method and Theory in American Archaeology* (1958) can be seen today in much the same light that they were seen in the 1950s, that is, as rival documents. One of the best ways to understand this is simply to examine the latter book: how does it deal with the reality of Taylor's preceding critique and prescriptions? The short answer is that Willey and Phillips confront Taylor's *ASOA* immediately, downplay its ideas, and then ignore it for the remainder of their volume. They cite Taylor three times at the very start. Two of these references—like Willey's initial reference to a link between Taylor and the Virú report (e.g., Willey and Sabloff 1980: 177)—are in footnotes. The third, only one sentence long, begins on the first page of Chapter 1 and is accompanied by a long footnote. More important than their length or placement, however, is what these references convey. Each one of the three emphasizes that what Taylor did was not anything that had not already been done, written, or considered: "This point has been put very well by Walter Taylor, who also rationalizes the operations of archaeology on a series of levels that differ in detail from ours but can be reconciled with them" (ibid., 6n4); "Taylor, in the work already cited (1948), puts these procedures on two distinct levels of interpretation, which he calls 'chronicle' and 'historiography.' See also Willey's (1953a) use of the terms 'historical' and 'processual'" (ibid., 11n1). The following passage is the most illuminating, both with regard to Willey and Phillips's view of Taylor's work and to the goals of the theoretical approach they lay out in their book:

> Taylor was undoubtedly correct in stating that American archaeologists have placed heavy emphasis on the skeletal chronicle at the expense of the recovery of what he calls "cultural context," but a review of the recent literature indicates a strong trend in the contrary direction. We submit that this is now an area of agreement for American archaeology: *culture historical integration is both the spatial and temporal scales and the content and relationships which they measure*. The essence of this study's departure, if it may be called a departure, is that these objectives are not regarded as being on different and unequally significant levels of interpretation or as even being capable of effective separation operationally. It seems to us that the apprehension and formulation of archaeological unit concepts involve the simultaneous investigation of contextual and spatial-temporal relationships. (ibid., 11–12; italics original, underscoring mine)

This quotation shows that Willey and Phillips reframe the language of theoretical discussion, transforming Taylor's pitch for "conjunction" into "culture historical integration," something upon which (by 1958) Willey and Phillips claim everyone was apparently agreed. They go on to state that their departure (or new approach) does not place chronicle and context at differing or "unequal" (!) levels of significance, nor does it separate the two operationally (as Taylor had). Rather, they claim that these procedures are equally important and can be carried out simultaneously. Therefore, Willey and Phillips's departure from traditional archaeology lies in the *integration* of contextual analysis with time-space systematics, the bread and butter for generations of archaeologists, including Willey and Phillips and the scholars with whom they worked and socialized. This brief statement of theoretical departure (from prewar archaeology as well as from Taylor) is a thinly disguised offer of a "hand up" to all the colleagues who were insulted or felt belittled by Taylor's assessment of their work and goals. At one and the same time, Willey and Phillips publicly put Taylor in his place and provided a justification for culture history as a practice on equal footing with other (newer) forms of interpretive work (i.e., "cultural context").

Taylor saw the "time-space" (or chronicle) approach of culture history as a fundamental step in archaeological practice, but not as an end in itself. He considered it to be prior in practice to formal contextual study ("content and relationships") and a building block to more abstract analyses and interpretations (see Table 1.1, this volume). Many archaeologists at that time believed that Taylor's book simply classified "time-space" systematics as a less important *lower level* procedure. They felt that in one grand sweep Taylor had relegated to the midden heap the life's work of the vast majority of archaeologists practicing at the time (see also Willey 1988: 299). Willey and Phillips's book, on the other hand, presented an explicit middle ground in which were redeemed and validated the significant contributions made by these researchers, including especially those scorched in Taylor's critique. *MTAA*'s introduction intentionally (and explicitly) qualified and contextualized Taylor's work, citing it as nothing particularly new, and in the same breath validated the important work previously disparaged by Taylor in *ASOA*. Willey and Phillips then went on to present their own grand treatise and recommendations. These mainly attempted to characterize the rise and evolution of indigenous civilizations in the whole of the Western Hemisphere.

Willey and Phillips's book is pan-American in its approach, specifically laying out a sequence of developmental stages for the history of the entire New World. This was a combination of culture history or time-space systematics (on a very large scale) with neo-evolutionism derived from earlier thinking by Morgan (1877), Childe (1950), and Steward (e.g., 1955). Taylor's book, on the other hand, privileged a localized approach as a starting point, suggesting that research begin at the level of the site with the objective to rigorously elucidate contexts at this scale *before* moving outward to larger questions and conclusions regarding regional developmental schemes. Such carefully scaled work, Taylor felt, had been lacking in Americanist practices before World War II.

It is common knowledge within American archaeology that Willey deserves the praise he has received as one of twentieth-century archaeology's great synthesizers (Sabloff 1987: xi–xiv; Fash 2002: 174). *MTAA* represents this skill for broad thinking, and when we look as well at much of his other work (e.g., Ford and Willey 1940; Willey 1948, 1953b, 1966, 1971), it is clear that Willey preferred a wide-angle lens. Taylor, however, preferred a microscope and then a telephoto. The differences between these scholars were linked to fundamental issues of scale and scope, as well as to differences in protocols and their concept of culture (see Meggers and Evans 1958: 195, on *MTAA*). The Willey and Phillips's approach to theory ultimately (and quite quickly) won attention and a prestigious following (e.g., see Binford 1962; cf. Meggers and Evans 1958). This owed to their perspective on the value of earlier approaches (excluding Taylor's) and to an emerging evolutionism that prioritized developmental (read *evolutionary*) and comparative frameworks.[17] At their scale of operation, there was no focus or concern with context and function at the level of the site and the community. They encouraged cross-cultural comparisons and cultural evolutionary (stage) models at regional and global scales, research strategies that became defining characteristics of the New Archaeology (see Willey and Sabloff 1993: 214). In fact, Lewis Binford (1962) explicitly launched his "revolution" from the platform that *MTAA* created. Although he (i.e., Binford), for his part, drew liberally from Taylor, it was many years before he admitted this and, besides, his differences with Taylor were and remain quite distinctive. Some are akin to those cited above for Willey and *MTAA*, but others are more philosophical, linked, for example, to disparate views for how to study human history and for how to view one's place in intellectual history.

## LEWIS BINFORD: EVOLUTION, GENEALOGY, AND HISTORY

Alice Kehoe (1998: 110) notes that in their famous 1958 book the two Harvard professors, Willey and Phillips, "starkly divorced American archaeology from history." Their interest in "processual interpretation" and a "search for sociocultural causality and law" (Willey and Phillips 1958: 6) effectively ushered in the evolutionary and scientific archaeology of the 1950s. They sought to align archaeology with cultural anthropology but argued that "anthropology is more science than history" (ibid.). Kehoe (1998: 110) points out that at this time, "[t]he direct historical approach was receding as radiocarbon chronometry demonstrated greater temporal complexities to American prehistory than had been assumed." After the creation of the National Science Foundation and the inauguration of Willey's Bowditch chair for scientific archaeology (both in 1950), historical perspectives waned and evolutionary approaches were confirmed as the standard for postwar archaeology.

In the early 1960s, Lewis Binford debuted the New (evolutionary and ecological) Archaeology. He initially ignored Taylor and instead used Willey and Phillips's publication (1958; see Binford 1962) for his benchmark and social capital. Binford pursued their vision for "archaeology as anthropology" as well as their call for explanation in archaeology through the study of cultural process or change. However, although Binford adhered to and built upon the evolutionary leanings of Willey and Phillips, he was encouraged to push historical concerns farther from science than even they had (Binford 1968c, 1983c: 9). Binford ultimately came to relegate history and historical study to the proverbial garbage pile. For him, history was irrelevant in the face of changing evolutionary structures of being. Historical particulars became inconsequential in light of global evolutionary currents in human culture and universal patterns in human environmental adaptations. This view of and for the past also carried into his view of intellectual history: Binford would disseminate his vision for archaeology as a radical mutation or a "revolution" (Sabloff 1990: xi). This revolution could build on what came before but should not necessarily be understood by reference to it.[18] This perspective would have clear implications for Taylor's legacy.

For example, Binford (1983c: 6) writes, "During the 1960s I came to reject the view that history causes history, accepting in its place the view that the events of history are the phenomena in need of explanation." By events he means evolutionary stages—grand adaptive moments in global human progress. Binford paid explicit homage to Willey and Phillips as the most prestigious standard-bearers in postwar archaeological theory and he responded openly to their clarion call; however, he saw his own contribution as being markedly distinct, like a new genetic structure or evolutionary stage. Binford (1968a: 27) writes: "Despite a recent statement that one should not speak of a 'new archaeology' since this alienates it from the old, we feel that archaeology in the 1960s is at a major point in evolutionary change. Evolution always builds on what went before, but it always involves basic structural changes."

Binford was comfortable building on Willey and Philips's ideas to create his own framework, not least because to align himself with such well-regarded thinkers would have had a salutary influence on his career. Positive references to Taylor's work, however, were noticeably absent in Binford's earliest writing. Binford (1972: 451) has claimed: "I have frequently avoided citing Walter Taylor in my writings except in a positive way because his work was inspiring to me. Clearly I disagree with many of his arguments, yet in print I have avoided these issues on more than one occasion." But this is actually not true. His earliest comments either criticize Taylor's culture concept and/or his supposed normative approach (e.g., Binford 1965) or simply ignore him altogether (e.g., 1962). This neglect of Taylor is striking given that many scholars maintain as a matter of fact the great influence of the conjunctive approach on the New Archaeology (see Chapter 1, this volume). It is also noteworthy because Binford (1972: 1–14, 125–134, 451) later demonstrates, overtly and implicitly, basic connections with Taylor's work. Binford even admitted (finally) to having read *ASOA* at length, multiple times, and to using it as a reference volume. He does not mention this until the 1970s, however, after Taylor himself (1969, 1972c) requested that Binford account for the origins of his ideas.

### *Genealogy and Attribution*

As a graduate student, Binford had read Taylor's book thoroughly several times (Binford 1972: 1–14) and returned to it periodically to evaluate and weigh others' ideas and to frame new ideas of his own. Even though no reference to Walter Taylor figures in his signature paper, *Archaeology as Anthropology* (1962), there are later explicit indications that he was stimulated, if unsatisfied, by Taylor's framework. For example, although Binford denies that Taylor formulated the means by which to study cultural process, he credits the older scholar with making great strides toward "the *reconstruction* of the lifeways of the people responsible for the archaeological remains" (1968a: 5–6; italics mine; Taylor never used the word "reconstruction" to define his approach, a point discussed below). And in his now-classic paper on the "Pompeii premise" (1981), Binford again explicitly cites the formative influence of Walter Taylor's optimism regarding the possibility of reconstructing the past. More recently, in interviews with Paula Sabloff (1998), Binford (ironically) emphasizes how important it was that Taylor *broke with tradition* by insisting that more and more could be gleaned from the archaeological record than anyone previously had imagined. Binford (1983c: 5, 16) eventually also recognized that Taylor emphasized construction, as opposed to reconstruction—this is not a minor point (see below) and provides solid evidence that it took Binford more than two decades to grasp the most basic premise of *ASOA*.

The gradual (over twenty years) admission by Binford of Walter Taylor's influence provides some insight into the conservative intellectual climate of Binford's youth in archaeology, and it invites one to question how and why Binford's early writing refused association with Taylor. This is especially interesting in light of the fact that Taylor openly accused Binford of appropriating his work without attribution. In reference to statements made in a 1968 book by Lewis and Sally Binford about the novelty of the "new archaeology," Taylor (1969: 383) says:

> A full discussion of a very similar overall approach to our discipline has been in print since 1948 (W. W. Taylor, *A Study of Archeology*). The systematic view of culture has been a basic premise of American anthropology, including archaeology, certainly since Malinowski, if not since Boas, and as for Binford's other tenets, I can point to passages in *A Study of Archeology* covering each of them, even that of testing hypotheses (pp. 155, 165, 186, 187). What the Binfords have produced in this book is not an exposition of the theory and practice of a new perspective but an explicit restatement of an old one, with some new and modern additions, together with some very pertinent, cogent, stimulating examples of archaeological research resulting from it. (1969: 383)

And in a later paper, the lively *Old Wine and New Skins*, Taylor (1972c: 30) addresses the biological (i.e., genealogical) component of Binford's thoughts on his new approach:

> [D]espite mutterings of denial from some of its practitioners, I allow myself the presumption of looking upon much of the "new archaeology" as practical application of a basic conceptual scheme, the earliest more or less complete expression of which was the conjunctive approach. When progeny will not own their parentage, it becomes the undignified and distressing but incumbent responsibility of parents to claim their posterity as they understand it. False modesty that obscures genealogy can leave a serious blot on the "scutcheon!" (ibid., 30)

An "escutcheon" is a plate for a name or inscription, or a shield or emblem bearing a coat of arms—a reference to intellectual lineage and to the likelihood that, by ignoring Taylor, Binford damages his own reputation. Here, Taylor not only asks to be properly cited but also draws attention to the fact that the lack of due recognition has become a trend—the silence has been reproduced. Thus, Taylor signals that the neglect of one's intellectual forebears is tantamount to denying the importance of history in the *construction* of ideas. This point brings us back to the fact that there were—and are—some fundamental differences between the conjunctive approach of Walter Taylor and the New Archaeology of Lewis Binford. Some of these are reflected in statements made by Binford himself.

For example, Binford (1972: 2, 6, 8) reminisces about the intense attention he gave Taylor's book while a graduate student and emphasizes the sentiment that Taylor just did not go far enough and that he "seemed to lack rigor" (ibid., 8). He also writes that "Taylor had the aims, but not the tools" (ibid., 8; Clay, this volume, makes a similar point). We might ask, what tools were lacking?

Beyond computers and radiocarbon technology, it seems that Binford was referring to Taylor's apparent normative approach (Binford 1965: 203), as well as his definition of culture. These differed markedly from Binford's. However, Binford had difficulty grasping the implications of Taylor's view of culture and its basis in archaeological and cultural interpretations that are constructed and historiographic. Until the 1980s Binford consistently and continuously referred to Taylor's interests in "reconstruction," which is basic evidence that, although he read Taylor's *ASOA*, he simply did not entirely understand it until late in his career (Binford 1983c), that is, after he was forced by Taylor's grumblings to reconsider *ASOA*.

Like Willey and Phillips before him, Binford sought to bring the field of archaeology into an *explanatory* scientific era: "So little work has been done in American archaeology on the explanatory level that it is difficult to find a name for it" (Willey and Phillips 1958: 5, cited in Binford 1962: 218). Binford's interest in positivist philosophers of science (see Meggers 1955, 1956; Hempel 1966) provided ample support for his goals and intentions. Positivism at the behest of explanation led Binford and the New Archaeologists to believe that archaeology can "reconstruct" the past, that is, conjure past realities and arrive at truths about lifeways and culture change (Binford 1968a, 1981). Binford's ideas regarding reconstruction differed immensely from Taylor's. These divergences reflected basic differences in their philosophies of culture (see Chapter 1, this volume) as well as Binford's misreadings of *ASOA*.

### *Historiography and Reconstruction*

Well before Binford surfaced, Taylor had refuted lucidly, explicitly, and as a foundation to *ASOA* (1948:35) the reconstructionist ideals present in archaeology. I address this in some depth in Chapter 1 of this volume but will briefly restate the tenets of Taylor's perspective. Because archaeologists write histories—that is, historicized texts about the field (that cite precedents and previous research) and about prehistory and antiquity—and because the archaeological record is imperfect, Taylor argued that our interpretations of the past are actually "constructions" and that these reflect and shape present cultural conditions and interests. This is the basis of his thoughts on the importance of historiography, a point recognized by Deetz (1988; and see Reyman 1999), who acknowledges that Taylor was decades ahead of his time in this respect. Understanding this difference with Binford is critical for grasping how Binford's approach to archaeology precludes the possibility of recognizing historical precedents to and influences on his work. This also provides greater dimension to Binford's role in perpetuating the neglect of Taylor in the early 1960s, showing again the extent to which archaeology is fundamentally situated in the present.

Taylor (1948: 34–35) wrote that historiography is "projected contemporary thought about past actuality, integrated and synthesized into contexts in terms of cultural man and sequential time" (and see Chapter 1, this volume). In discussing Taylor's thoughts, Deetz (1988: 15) noted that

> past actuality can never be known in its totality. . . . So decisions have to be made. What will be left in, what is to be omitted; what is considered important, what is not? Historiography then becomes by necessity an abstraction, and the manner in which this abstraction is arrived at depends on the interests and concerns held by the historiographer. In this definition, Taylor anticipates the position taken by contemporary critical theorists in their attempts to explain past actuality.

Deetz (ibid., 15) says that some of the positions of critical theorists are extreme but that "contemporary values and interests play a role in that explanation." As such, he suggests modifying slightly Taylor's definition of historiography to emphasize explicitly the existence of a sort of feedback system in which contemporary values and interests shape our thoughts about the past, which is then *constructed* in terms that reinforce the values we began with. A modified definition for historiography in archaeology would be the following: "Projected contemporary thought about past actuality, integrated and synthesized into contexts in terms of cultural man, sequential time, and contemporary values and interests" (Deetz 1988: 15).

Many others (e.g., Leone 1978; Becker 1979; Trigger 1980; Wilk 1985; Parkington and Smith 1986) have made the point that "the theories one espouses about the past depend very much on one's own social and cultural context" (Hodder 1991: 17). Thus, writing about the past requires selectivity and will invariably be influenced by the present. In effectively erasing or denying historical influences and the intellectual events that shaped his thinking and the field of archaeology, Binford imposed on the practice of archaeology the same models that he argued define change in prehistory, namely, punctuated evolutionary stages that are "genetically" different than what came before. This was another type of feedback system that justified both his approach to prehistory and the reasons why he believed his impact and his ideas should remain (r)evolutionary. It is as though Binford's model for archaeological practice legitimized a neglect of Taylor based on his distinctive approach to science. Binford created a strategy that required the death through neglect of the paterfamilias, even as he offered a nuanced homage to Willey and Phillips and even as he set the stage for his own rise as American archaeology's patriarch.

Binford's paradoxical approach was founded during the optimism, tensions, and democratic discourse of the 1960s and developed at a large state university (University of Michigan) far removed from the elite corridors of Harvard's Peabody Museum. There is little question, therefore, that the New Archaeology appealed to a new generation of men and women from all socioeconomic backgrounds. After all, the promotion of hard science in the wake of World War II was expected to be a great equalizer and engine for the American constitutional vision. However, the problem in neglecting to acknowledge whole segments of one's intellectual genealogy, and in justifying this neglect through overtures to philosophies in the natural sciences and to Enlightenment-derived notions of evolutionary progress, is a large and profound one and results in a complex of dilemmas. It discourages diverse opinions, enables the epistemological pathology we see today (neatly explained by Thomas 2000) in all areas of American archaeology, reinforces the neglect of critical analyses of the foundations of our field, and has helped to fuel the survival of pockets of conjunctive research—basically historical and contextual methodologies—that struggle for definition and currency. In many ways, the New Archaeology phenomenon (that continues to conduct itself as scientific or "anthropological archaeology") is an odd facet of the postmodern condition in American archaeology—a set of self-validating methodologies that assume the status of theories and perpetuate many of the problems they were meant to resolve (see Yoffee 2005). The following sections on the conjunctive approach and the Carnegie legacy in Maya archaeology address many of these issues in somewhat greater depth.

## PART 2: EXCAVATING THE CONJUNCTIVE APPROACH

The survival or vestige of Taylor's conjunctive model is seen in areas of lowland Maya archaeology where either New Archaeology methodologies (e.g., settlement analysis and ethnographic analogy) have been mapped onto culture historical approaches or Carnegie-era culture historical approaches have endured with limited modifications or resistance. In both cases the conjunctive trend has emerged largely because of the interest in combining epigraphic and archaeological research into a so-called multidisciplinary approach[19] and to employ this for the study of community histories at the scale, especially, of the Classic period Maya dynastic centers (i.e., localized elite and royal histories). Along these lines, for example, we have seen the appearance of a formal "social history" perspective of the ruling Maya lineage at Copan (e.g., Fash 2005), developed by the Pennsylvania and Harvard groups over the last twenty years (beginning, e.g., with Fash and Sharer 1991). At Copan, regional-scale comparative evolutionary approaches (e.g., Webster, Gonlin, and Freter 2000), which remain the basis of processual archaeology, are not geared to recover or develop these kinds of historical data. For this reason, the conjunctive approach is rarely employed or discussed by cultural evolutionists or cultural ecologists working at Copan or in the Maya region (although see Dunning et al. 1998).

As we will see below, conjunctive Mayanists at Copan and elsewhere are actually struggling with different versions of Taylor's model. None of these parallel the approach in *ASOA* and, perhaps not surprisingly, no publication spends more than a paragraph explaining the rationale behind them. The most apparent problem arising from these efforts, that is, from the appropriation or imposition of some form of Taylor's conjunctive protocols, is that by and large these arrive in print as justifications for work already completed. This is just one sign among many that the fascination with Taylor and use of his ideas have become a special—even odd—case that deserves its own study. The character and implications of this trend are numerous. At best, we see that these groups of scholars are exploring various ways that their complex data can be interpreted more meaningfully and effectively. At worst, they may have found themselves in the twenty-first century, mired in a theoretical and methodological quandary reminiscent of the Kluckhohn-Taylor era. A closer look at the history of conjunctive studies helps us to contextualize some of these issues.

## CONJUNCTIVE ORIENTATIONS IN MAYA ARCHAEOLOGY

> The conjunctive approach . . . stands in contradistinction to that which is currently popular with Americanists in the United States and which may be termed the comparative or taxonomic approach. . . . [The conjunctive approach] is primarily interested in the interrelationships which existed *within* a particular cultural entity, while the comparative approach occupies itself primarily with data which have relationships *outside* the cultural unit and attempts to place the newly discovered material in taxonomic or other association with extra-local phenomena. (Taylor 1948: 7; italics original)

### *Genealogy of Silence*

Walter Taylor (1948) was the only scholar in the history of American archaeology to offer an approach that was "conjunctive" in name. Use of the term "conjunctive," today and fifty years ago, conjures Taylor and his book. In other instances where a new "named" paradigm or an important methodology is discussed in the literature, its leading advocate is also named through citation, frequently over decades. This has been the case, for example, with Petrie, Kidder, Krieger, Willey, Binford, Leone, Hodder, and many others. But not so with Taylor. William Folan (this volume) notes that he (i.e., Folan) has been deeply influenced by the conjunctive approach during the course of his career. Discussing fieldwork at Dzibilchaltun, Folan (1969: 454) is the first in Maya archaeology to cite Taylor; he refers, however, to "Taylor's formula"—never to the conjunctive approach. In Maya archaeology more broadly, the first thirty years of references to the conjunctive approach included no mention whatsoever of Taylor. The earliest example of this—the precedent setter—can be found in a report for which Gordon Willey was senior author, discussing a project he directed. In presenting the theoretical justification for employing settlement as a basis for archaeological inference in the Belize River Valley, Willey and colleagues (1965: 5–6) write: "The settlement data, like any others in archaeology, must be viewed in context. Where the only keys to past actions and the intricacies of the human mind are abandoned artifacts and their contexts no one can deny the wisdom or even the inevitability of a 'conjunctive' approach."

Recently, Wendy Ashmore (2007: 54) has suggested that this early reference to the conjunctive approach indicates Willey's "frank advocacy" of Taylor's model and his "critical openness" in general. Writing in the context of a festschrift, however, Ashmore overreads this passage as a means to celebrate Willey. Simply put, it is difficult to interpret the meaning of Willey's (Taylor-less) statement without considering the wider intellectual currents of the 1950s and 1960s (e.g., Willey and Phillips 1958; Binford 1962; and see Leventhal and Cornavaca 2007). The Belize River Valley report was Willey's first monograph-length example of a settlement-pattern study in the Maya area—an approach he later admits was directly influenced by Taylor. Further, it arrives only seven years after the publication of *Method and Theory in American Archaeology*. Far from advocating for Taylor, Willey declares that settlement research, through its contextual focus, would *naturally* lead to a "conjunctive" approach. In effect, Willey is saying that Taylor was irrelevant; conjunction was inevitable. If one considers the treatment of Taylor in *MTAA*, in which *ASOA* was declared nothing particularly new or different, Willey's reference is better understood. The field's censure of Taylor—for seventeen years by that point—makes the context even more complete.

Willey's 1965 statement, with its simple omission, set an unusual precedent in Maya archaeology for the next thirty years. If the term "conjunctive" had become accepted and frequently used—like cultural ecology, for example—or had diverged in numerous directions like cultural evolutionary research, we might disregard the lack of attribution to one individual. However, references to the conjunctive approach in late twentieth-century Maya archaeology are not numerous, a fact that makes the patterns both remarkable and traceable. In papers that reference the conjunctive approach between 1965 and 1994, none included Taylor's name nor cite him. Joyce Marcus (1995) eventually broke the silence by placing "Taylor" and "conjunctive" in the same sentence. This sparked the recent period of interest in naming Taylor at Copan (Maca 2001), although interpretations of his model and its implications remain idiosyncratic and/or skeletal. Nevertheless, it is intriguing that, although Marcus ultimately remembers Taylor as the progenitor of the emerging Maya conjunctive approach, she was initially among those who contributed to the long period of silence.

### *Joyce Marcus, in the Footsteps of Kluckhohn and Taylor*

Interest in conjunctive archaeology reached a discernible level of intensity among the Pennsylvania and Harvard Mayanists[20] around 1990 (e.g., Fash 1988; Sharer 1991). This developed more broadly in a seminal article on conjunctive archaeology at Copan by Fash and Sharer (1991) in the journal *Latin American Antiquity* and two substantial reviews of Maya archaeology published by Fash (1994) and Marcus (1995). These cited precedents, none of which cited Taylor. The following discussion begins with a look at these apparent precedents and especially at those cited by Fash and Sharer (1991) as foundational to conjunctive research, namely, papers by Sharer (1978) and Marcus (1983). These are very different in scale and scope, and I address Marcus's article first because of her status as a quasi-outsider to Maya archaeology and because it arguably has had more of a direct impact on the field. Note that neither of these mentions the term "conjunctive."

Marcus's (1983) article, called "Lowland Maya Archaeology at the Crossroads," was published in *American Antiquity*. This, her first decade-review of Maya archaeology, offered a sharp critique. Norman Hammond (1984), responding to Marcus, recognized it as following the tradition of the Kluckhohn-Taylor "outsider" assault. The similarities between these two salvos, forty years apart, however, are not in tenor alone. In their defining paper on conjunctive research at Copan, Fash and Sharer (1991) cite Marcus's paper as the basis of a conjunctive strategy in Maya archaeology. Since Marcus never used the term "conjunctive" nor cited "Taylor," we must ask, how and to what extent can this be the case? The answer is simply the content. Marcus encourages ideas that are distinctly familiar to us from earlier discussions by Taylor (1948, 1957b), namely, the importance of investigating individual polities and localized histories as an alternative or complement to regional/comparative approaches and multidisciplinary research. For example, Marcus (1983: 480) complains, "Archaeologists also turn to far-off parts of the world for 'new models,' when neighboring areas of Mesoamerica could more easily provide them; they hold conferences on complex sociocultural topics at which ethnohistorians, linguists, epigraphers, and cultural geographers are not even present." In this passage she highlights the (then) dearth of multidisciplinary research. This had become standard in other areas of American archaeology, but Maya studies lagged behind. She also criticizes the emphasis on cross-cultural, comparative evolutionary approaches in vogue at that time, arguing that there exist tremendously useful data on more localized expressions of Amerindian culture, historically and in the present. Marcus (1983: 482) goes on to explain why Mayanists need to build interpretative foundations on more local data sets before exploring processual questions of culture change: "As for the broader questions that fascinate the layman, such as the collapse of the Lowland Maya, it is difficult to see how we can justify sprinting ahead to those topics until we have resolved our arguments over whether the Maya were something mysterious or, more interestingly, another kind of Mesoamerican state. After all, it's hard to reconstruct how a society fell if we can't even agree on what kind of a society it was."

Taylor's conjunctive approach was explicit in recommending that archaeological research focus first on contexts at the site-level—on the study of a particular society—before moving on to larger regional and global comparisons, one of the major orientations of the conjunctive approach after elucidating local contexts (see Chapter 1, this volume). Marcus's (1983) paper, as well as some of her other work in the 1980s and 1990s, followed a Taylorean bearing in theory and methodology.[21] Further, her 1983 paper opens a discussion regarding the foundations of our ideas (akin to Taylor 1948: Chapter 1), alludes to an "epistemological pathology" (sensu Taylor 1948; and cf. Puleston 1979), and offers a deconstruction of the history and current methodologies of the field, also as Taylor (1948) had done. In sum, Marcus's 1983 paper was a neatly crafted document that borrowed directly much of its rhetorical, methodological, and theoretical force from Taylor. Yet still we must ask, why did she exclude references to Taylor?

Marcus, like others, read and assimilated Taylor's work but avoided the problematic associations with the pariah. Excluding him from her citations is understandable at a time when most leaders in the field still viewed Taylor with disdain. The infamous 1985 SAA anniversary session (Sabloff 2004: 19; O'Brien, Lyman, and Schiffer 2005: 31; Longacre, this volume) is evidence of this pervasive sentiment as are the problems Folan and Reyman encountered when building a festschrift for Taylor in the mid-1980s (see preface, this volume). Today we celebrate Marcus for her influential and widely read critique (i.e., 1983) and overlook her omission. The fact remains, however, that she offered a prescription for a conjunctive strategy—one that remains recognized as such—without ever mentioning Taylor or his approach. The result is an even more profound silence and one that has had a formidable impact on later conjunctive perspectives.

### *Sharer, Fash, and Social History at Copan*

Fash and Sharer's "conjunctive" article (1991), in addition to citing the conjunctive orientation espoused by Marcus (1983), cites also the conjunctive basis of one of Sharer's earlier papers (i.e., 1978). The methodology of Sharer's paper focuses mainly on how to combine history (epigraphy) and archaeology in research at the Classic period lowland Maya site of Quirigua in Guatemala. Sharer never cites Walter Taylor or "conjunctive" research; his stated methodology appears to be something different, called the "complementary-research approach" (1978: 52). This methodology is akin to that found in an article in

*American Antiquity* by Robert Carmack and John Weeks (1981). They combine archaeological and ethnohistorical data in their analysis of a Postclassic period Maya site, also in Guatemala, titled "The Archaeology and Ethnohistory of Utatlan: A Conjunctive Approach." They also do not cite Taylor, yet their work has encouraged other, often related, conjunctive research on the Postclassic highlands of Guatemala (e.g., Fox 1987; see also Fowler 1989). The Carmack and Weeks article has become an exemplar of Taylorean conjunctive research according to a later article by Marcus (1995). Its kinship (methodologically) with Sharer's (1978) paper may be a reason why Fash and Sharer (1991) have cited Sharer's early work as a foundation for conjunctive research at Copan. Whatever the case, Sharer's "complementary" approach becomes the basis of the "conjunctive perspective" he advocates in Honduras (Sharer et al. 1999; Sharer 2000). In fact, he states (2000: 1) that his work "essentially comprises historical archaeology at Copan." Sharer's historical approach, by whatever name, need not have been drawn from Taylor because it qualifies as an "inevitable" strategy with a long history in other parts of the world where ancient texts inform archaeology (Freidel 1994: 64). However, it is *the* basis of Sharer's conjunctive research and arguably represents his main contribution to the larger conjunctive methodology that is promulgated at Copan by him and Fash.

It is especially noteworthy that Fash and Sharer's 1991 article never cites Walter Taylor for this is the publication that, more than any other, lays out the parameters for applying a conjunctive approach in Maya archaeology. As such, we might expect there would be a short literature review for the introduction of the methodology (and attendant theory?) that frames the paper and guides the period of research, but we do not find one. We receive only a 130-word statement—the longest explanation of the conjunctive orientation anywhere in lowland Maya research. Fash and Sharer (1991: 170) write:

> Maya scholars over the past decade have called for a multi-disciplinary or "conjunctive" strategy beginning from the perspective of individual Classic Maya polities (Marcus 1983; Sharer 1978). The longest-running and most comprehensive example of such conjunctive research has taken place at Copan, where the reconstruction of local sociopolitical development is being accomplished by a combination of archaeological, epigraphic, and iconographic investigations in a cross-cutting, self-correcting strategy. In this effort, all forms of relevant evidence, including the results of past and present settlement and population research, are welcome in order to refine and improve our understanding of ancient Copan. . . . Obviously the utility and success of the conjunctive strategy relies on the unique richness of the Classic Maya historical record, allowing back and forth testing of conclusions between textual and archaeological sources.

Like Willey, who emphasizes context and settlement in relation to conjunctive archaeology, Fash and Sharer (1991: 170, 172) acknowledge the importance of settlement data. However, it is clear that their notion of "conjunctive" also includes multidisciplinary research, multiple data sets, considerations of the interplay between archaeology and textual evidence, the study of history, and the investigation of individual polities at the local (or, in this case, dynastic) level. They fall well short of Taylor in their almost exclusive focus on the elite, as well as their interests in *re*construction, as opposed to construction, and methodological work absent general theory. Nevertheless, their briefly stated strategy demonstrates many of the tenets of a Taylorean conjunctive approach and is sufficiently practical (i.e., practicable) that it launches a variety of conjunctive approaches as the standard at Copan and elsewhere. Their article is followed up by Fash's (1994) prescriptions in the *Annual Review of Anthropology*. Therein, Fash (1994: 195) cites twenty examples of conjunctive-type research in the Maya area, none of which cite Taylor and few of which even use the word "conjunctive." This is a form of archaeological "conjunctivitis" because he offers no resolution regarding what exactly a conjunctive approach is; Fash mainly refers to a cross-checking strategy between epigraphy and archaeology. This diluted perspective represents a significant shift from his paper with Sharer. In the follow-up to her 1983 paper, Marcus commits the same kind of redefinition.

### *Marcus Claims Taylor for Maya Archaeology*

In her second decade-review of Maya archaeology, Marcus (1995) became the first among current "conjunctivists" to cite Walter Taylor as the progenitor of the approach currently in vogue in the lowland Maya area. Although she cites for the first time a conjunctive approach—and a Taylorean approach at that—her definition is diluted compared with the views of her earlier (1983) paper. This is seen in several respects, including the additions she (1995: 3–4) provides to the list of earlier examples of conjunctive research:

> At least three trends can be seen in the last decade of lowland Maya archaeology, and I organize my presentation around them. The first trend is a substantial increase in the integration of multiple lines of evidence—in effect, what Walter W. Taylor (1948) called "the conjunctive approach" (Carmack and Weeks 1981; Fash and Sharer 1991; Marcus 1983; Sabloff 1990). This effort rarely has reached the point where it could be called "processual archaeology," because the latter requires that research be designed to answer general questions of culture process. Nevertheless, it is increasingly commonplace to see the staffs of Lowland Maya projects integrating the work of surveyors, ethnohistorians, ceramicists, epigraphers, palynologists, human osteologists, faunal analysts, ethnobotanists, malacologists, chipped stone experts, and the like.

In an ironic twist, apparent with reference to my introductory chapter for the current volume, Marcus cites as conjunctive Jeremy Sabloff's (1990) book on the New (processual) Archaeology in the Maya area. This is noteworthy because, although many scholars argue for Taylor's substantial impact on the New Archaeology, few (except a disgruntled Taylor [1969; 1972c]) have ever argued for *direct* parallels between the two strategies. Sabloff (1990: 65, 72) actually discusses Taylor only twice and never advocates or even mentions a conjunctive approach anywhere in his book. So we must ask, is Marcus suggesting that the New Archaeology in the Maya area derives from or is based on Taylor's conjunctive approach? This is unlikely because, although she cites Sabloff's (1990) presentation of processual research, she goes on to note the dearth of processual archaeology among conjunctivists in the 1990s.[22] Marcus's unabashed support for processual goals makes this passage more striking still because it seems to contradict aspects of the conjunctive orientation she encouraged in her 1983 article; this further highlights the differences between her 1983 and 1995 papers.

Although it is true that Marcus (1995) cites Taylor, it is puzzling that the fullness of her earlier (apparently) conjunctive prescriptions does not carry into her later article. Given that the 1995 paper considers the conjunctive approach only in terms of multidisciplinary research, it is again puzzling that she also cites Fash and Sharer (1991), for whom, early on, conjunctive archaeology is much more complex and nuanced. In any event, it is likely that a continuing lack of clarity regarding historical influences and a long-standing lack of references to Taylor contribute to uncertainty regarding exactly what a conjunctive approach is, who developed it, who employs it, and how it will be used and defined in the future.

### *Twenty-first-Century Conjunction: The Pennsylvania Group Claims Taylor for Copan*

The following looks at the character and implications of the first formal attribution of Copan's conjunctive approach to Walter Taylor. Robert Sharer, now an emeritus professor at the University of Pennsylvania, is well-known for his Early Copan Acropolis Project (ECAP). He and his former students, Ellen Bell and Marcello Canuto, recently edited an eagerly anticipated volume titled *Understanding Early Classic Copan* (Bell, Canuto, and Sharer 2004), published by the University of Pennsylvania Museum. It provides an astounding array of hard-won data from more than a decade of intensive research at Copan, centered mainly on the acropolis. The introductory chapter of their volume provides the first published citation of Taylor as the progenitor of the conjunctive approach at Copan, and we learn that this is the research strategy that apparently has structured and legitimized the ECAP and related research efforts from the beginning (Canuto, Sharer, and Bell 2004). We must remember, however, that Taylor was never cited at the outset of ECAP and other acropolis research (e.g., Fash and Sharer 1991), which makes this a retroactive construction. We will further note that this introductory explanation of the conjunctive approach also is considerably watered down from the baseline conjunctive model at Copan (ibid.). It much more closely resembles the Taylorean conjunctivism laid out by Marcus in her 1995 article and indicates a further dilution taking place, now among all three of the leading adherents of the conjunctive approach in the Maya lowlands (i.e., Fash, Marcus, and Sharer). This leaves us to question the existing state of the art in method and theory at Copan as well as why, yet again, the bulk of Fash and Sharer's recommendations are now absent. A closer look at the published explanation helps to understand this.

The following statement of the conjunctive approach serves as the basis of method, methodology, and theory for the edited volume as a whole, including ECAP research. Canuto, Bell, and Sharer (2004: 8) write:

> Originally defined by Walter Taylor (1948), the conjunctive approach has been adapted to conditions at Copan. In its Copan setting, the conjunctive approach refers to archaeological research designed to solve specific questions about the past. A broad range of specialists has address [*sic*] these questions by studying everything from the construction, styles, and decoration of buildings, to the remains of Copan's ancient people and their activities. These data are combined with a historical perspective based on the decipherment of texts. The resulting multiple data sets are then applied to the original research questions; consistent and complementary findings provide answers, while inconsistent and contrary findings create the need for further research.

As Fash and Sharer (1991) referred to a conjunctive strategy in order to create a guideline for research at Copan in the 1990s, this Pennsylvania volume attempts to do the same thirteen years later, but for the early twenty-first century. It is intriguing, however, that the definition in the introductory chapter, although concerned with the history of Copan as a single polity, fails to include any mention of the importance of studying an "individual" ancient Maya community, nor does it include any discussion of local sociopolitical development (as Fash and Sharer did). Apparently, the model and goals have changed somewhat in the span of thirteen years, further demonstrated by the fact that none of the volume's seventeen chapters is devoted to settlement-pattern studies in the Copan alluvial pocket, that is, in the area in and surrounding the urban core. This shows that their definition also ignores Willey's notions of conjunctive (as contextual) archaeology.

While the absence of a fuller definition of the conjunctive approach may explain the absence of a more conjunctive volume, one would think that the (above) stated interest in multiple data sets would drive the editors to include in the book some examples of settlement studies within Copan's urban community sphere (derived from, e.g., Willey and Leventhal 1979; Fash 1983; Freter 1988; Ashmore 1991). Since this and other apparently fundamental conjunctive strategies are not included in the Bell, Canuto, and Sharer volume, we are left with a conjunctive perspective that is vague in terms of orientation, goals, and results. More importantly, it is a retroactive construction: an after-the-fact assemblage of (roughly) representative research disguised as a body of coherent *methodology*. And what about *theory*? There is no reference whatsoever—anywhere in the ECAP volume—to a theoretical orientation or guideline for the Pennsylvania group research at Copan or for other related Copan-based (e.g., Harvard) research. Undertaking a Taylorean conjunctive approach would imply, and subsequent publications would explicitly state, a problem orientation and a substantial theoretical basis for research.

In sum, the Bell, Canuto, and Sharer volume offers a formidable array of important data. The use of and reference to Walter Taylor and a conjunctive approach, however, represent a post hoc justification for collating dispersed data sets. The result is akin to data dredging. It resembles much of what Taylor (and Kluckhohn) found problematic in the Carnegie's Maya program sixty years ago: no local, site-level community study of the Copan urban sphere; an overemphasis on the elite or, as Taylor said, the "hierarchic" (cf. Sabloff 1990: 65, 72); and, most importantly, no theoretical orientation.

The opening (theme-setting) quotations of the volume's final discussion chapter, provided by Joyce Marcus, playfully refer to the importance of the integration of data with theory. Marcus then begins her formal remarks by carefully acknowledging the absence of a theoretical framework for the ECAP project: "This volume's hard-won empirical data, when integrated with appropriate theory, has [*sic*] the potential to generate a more universally meaningful view of the ancient Maya than ever before" (Marcus 2004: 357). What theory is appropriate? And when will it be applied? These questions remain to be answered. In the meantime, the absence of a carefully delineated theoretical approach—be it conjunctive or other—jeopardizes the future of sustainable research at Copan[23] because it allows and sets precedents for the gathering of data merely for data's sake and post hoc formulations of archaeology's significance. Even more important is a larger problem of which the ECAP research is but one part. The absence of theory and the uncritical adoption of Taylor's approach at Copan may reflect a more systemic problem in Maya archaeology as a whole. We see signs of this in another recent Pennsylvania group volume.

### *Twenty-first-Century Conjunction: The Pennsylvania Group Claims Taylor for Maya Studies*

Published by Routledge, *Continuities and Changes in Maya Archaeology: Perspectives at the Millennium* (Golden and Borgstede 2004a) also was edited by former students of Robert Sharer. It includes contributions by twenty-three authors, eight of whom are or have been directly affiliated with the University of Pennsylvania. The volume purports to offer perspectives on the state of the art in Maya archaeology as well as prescriptions for the future. It does this first by establishing the last sixty years of research as a direct response to Kluckhohn and Taylor and by cementing Taylor's conjunctive approach as the central strategy going forward. Second, it provides numerous chapters on the cutting edge analysis of data, organized by data categories. Third, it includes a final section, "Contemporary Concerns," that encourages a consideration of archaeology's living contexts.

This volume ranges far beyond the Copan Valley and includes thoughts on contemporary issues, but it suffers several of the problems basic to the Bell, Canuto, and Sharer book, and to a greater extreme. Because the volume is regional in its coverage, and because it attempts to assess the twentieth century and set a course for the twenty-first, a number of major statements are made regarding the history and scientific orientation of the field, some of these by leading senior scholars. Most of these statements, however, are at best uncritical exaggerations and at worst are simply inaccurate; in general they represent revisionist, self-validating writings of the history of American (and Maya) archaeology. The following looks in particular at four chapters that discuss Clyde Kluckhohn, Walter Taylor, and the conjunctive approach. These are written by Charles Golden and Gregory Borgstede, Sabloff, Canuto and Fash, and Pat Culbert. Because they introduce the volume's goals, the first two of these are the most significant and I will focus most of my discussion on them. I show that there exist among this group of scholars basic misinterpretations of Kluckhohn's (1940) critique and Taylor's conjunctive approach. These reflect a neglect of theory in the Golden and Borgstede book overall and have led to the adoption of a conjunctive model that emerges as a mere methodology—a vestige at best of the conceptual framework Taylor intended.

In concluding the introductory overview of their edited volume, Golden and Borgstede (2004b: 11) write that "the diversity of the discipline, far from being divisive, fosters cohesion as researchers attempt to incorporate the full breadth of data in conjunctive, holistic studies." A few pages earlier (ibid., 6), they write, "Following the conjunctive approach, a holistic, anthropological archaeology of the Maya consists of the insights drawn from all the data and perspectives available to us (Carmack and Weeks 1981; Fash and Sharer 1991; Taylor 1948)." Golden and Borgstede explain the diversity of their volume (and the future of the field) by reference to a conjunctive model of research that encourages the analysis of multiple data sets and multidisciplinarity. In effect, they claim the conjunctive approach as a methodology that encourages disciplinary cohesion through varied data analyses—a greatly pared down interpretation relative to Fash and Sharer (1991) and simplistic compared to Taylor's (1948) recommendations in *ASOA*. They are not alone in interpreting Taylor's conjunctive approach as a strict methodology, as I discuss above and below. I emphasize this now, however, because in the Golden and Borgstede book a conjunctive methodology assumes the role of a unifying theory.

Golden and Borgstede also say that their book is modeled on the volume (Hay et al. 1940) that included Kluckhohn's famous critique of Maya studies: "Following in the footsteps of *The Maya and Their Neighbors*, the goal of this volume is to provide points of discussion and debate that will advance our understanding and approaches to Maya archaeology" (Golden and Borgstede 2004b: 6). *The Maya and Their Neighbors*, however, was a very different book. A festschrift for one of the leading Maya archaeologists of the prewar period, Alfred Tozzer, it was and remains remarkable because in one short chapter Kluckhohn assailed Maya archaeology for neglecting theory and for being hopelessly outdated. Kluckhohn's chapter also inspired Walter Taylor, whose *ASOA* contains a much more extensive and acerbic critique of Maya archaeology than Kluckhohn offered. The Golden and Borgstede volume does not strike up such debate or controversy and there is not a single dissenting voice. This is relevant for two reasons. First, Golden and Borgstede borrow twice from controversies in the history of the field to frame the importance of their book and to offer future directions. Both of these are misreadings. Second, these misreadings are obvious because both Kluckhohn and Taylor insisted on the importance of theoretical orientations for Maya (and American) archaeology, yet Golden and Borgstede do not offer any nor do they insist that these need further development where they are found. Delving into the details of subsequent chapters better helps us to understand why something is amiss in the directions and goals of this Pennsylvania volume and in particular its exploitation of the Kluckhohn-Taylor era.

### *A "Massive Response"?*

Jeremy Sabloff, former professor and director emeritus at the University of Pennsylvania Museum and an alumnus of the college, contributed the second introductory chapter of the Golden and Borgstede volume. Sabloff (2004: 14) argues that "in many respects, explicitly, implicitly, or in effect, Maya studies over the last five decades have been a massive response to the criticisms that Kluckhohn and Taylor leveled against the field in the 1940s." By use of the word "explicitly," he suggests that facets of this response were both stated and intentional. However, where are these explicit statements? No examples are provided. Both Sabloff and, later in volume, Culbert (2004: 312) state only that Gordon Willey led the charge with his settlement approach. But when exactly was this begun and was it initiated with Taylor's prescriptions, or merely with a conscience that something was lacking? Or, did Willey simply see an opening to move ahead with his own model for archaeological investigation in the Maya area, one based on settlement-pattern research and regional evolutionary and comparative models?

To begin to answer these questions, we may recall Willey's early statement on the inevitability of a conjunctive approach, discussed above (i.e., Willey et al. 1965). We might conclude that perhaps by the 1960s, but not necessarily before then, someone (i.e., Willey) *had* been aware enough of the conjunctive approach to employ it and then to cite it at the outset of a major settlement report, one that then stimulated the field. Taylor's name, however, was never cited, so this clearly does not represent an explicit response to Taylor's criticisms. It seems much more likely that Sabloff's (2004) interpretation, for example, demonstrates only a *sense* that the fifty-years-ago general response or corrective addressed the specific criticisms of Kluckhohn and Taylor. In actual fact, there was no "tailored" response or corrective, intentional or explicit; and to suggest that the response to Taylor was implicit is vague, even a cop-out. Willey's introduction of settlement-pattern research in the Maya area was neither an example nor an application of the conjunctive approach. Kluckhohn's and Taylor's criticisms have still never been dealt with, and we see the chaotic results today at Copan and elsewhere.

In order to confirm that Willey and his settlement research did not (seek to) answer the call of Kluckhohn-Taylor, we can consider the following. On the one hand, Willey (1968; Willey and Sabloff 1993; Freidel 1994) has claimed Taylor as a major influence (in print), so certainly Willey appreciated and may have sought to apply various features of a conjunctive approach. On the other hand, however, many have recognized (e.g., Fash 2002; Vogt 2004; Leventhal and Cornavaca 2007), and I have discussed above, that Willey (with Phillips 1958) offered a *competing theoretical paradigm* to Taylor's—the two were not philosophically nor methodologically parallel, nor were they similar in operational scale. They were not even close. Therefore, Willey could not logically have offered the corrective that Sabloff, Culbert, and others (e.g., Weeks and Hill 2006: 16) mention. A corrective built on Kluckhohn and Taylor would have been one that prioritizes the study of micro-settlement contexts and especially the development of theory. Although settlement-pattern research can and should be used with and in the development of theory, it is not a theoretical approach per se. It is a methodology that can be guided by theory. As this is the case, and because Willey's settlement approach is hailed as the initial and main corrective following the Kluckhohn-Taylor attacks, Sabloff and Culbert must assume that Taylor's *ASOA* and Kluckhohn's prescriptions focused largely on methodologies (not theory) and/or that a new methodology (i.e., settlement studies) may have been sufficient to introduce a concern with theory (in order to correct the highly criticized lack of theory). The Kluckhohn and Taylor attacks and recommendations were fundamentally centered on questions of theory, so at least one of these assumptions would be baseless. But more importantly, the overemphasis on methodology and the fact that these authors do not distinguish theory from methodology should suggest to us a major problem: the absence of theory and of an understanding of what constitutes theory in the history of Maya archaeology appears to remain with us after sixty years, exemplified by the remarks of leading senior scholars.

In order to tie up these points and to understand better the contradictions, disjunctions, misreadings, and vagueness of various statements in this Pennsylvania volume, we might consider yet another statement by Sabloff. In his conclusion, he (2004: 18) claims that Maya archaeology has made such terrific progress that most of the Kluckhohn-Taylor critiques would not be applicable today. Coming from Sabloff, this is a surprising assertion. After all, he authored the 1990 book (on New Archaeology in the Maya area) that champions the kind of comparative and regional research that Taylor cited as insufficient. How is it possible that Sabloff, or his colleagues with similar research orientations, could have advanced a substantial correction to deal with the specific criticisms and general assault of the 1940s? The character, epistemology, and scale of Sabloff's paradigm, built in good part from that of Willey and Phillips and linked to Binford's, could not undertake the shift that Taylor encouraged. In actuality, there has been no corrective, no response to Kluckhohn and Taylor, since theory remains an afterthought (at best) to a large number of Mayanists and, especially, to those of the Harvard and Pennsylvania groups. To conclude this section and to emphasize further my points regarding post hoc formulations and revisionist history, I briefly turn our attention to the chapter in the Golden and Borgstede book by Canuto and Fash.

It is now clear that Taylor and the conjunctive approach have been adopted late in the game so as to coincide with past research, revisionist historiography, and future directions. This largely has occurred to validate work that already has been conducted. A good example of this is seen where Canuto and Fash (2004: 53) reveal a central problem with the Golden and Borgstede book's various overtures to the conjunctive approach (i.e., to a standard or intentional conjunctive approach prescribed by Taylor). They write, "Overall, research at Copan was designed to conform to Walter Taylor's (1948) conjunctive approach since 'archaeology (including settlement studies), epigraphy, iconography, and architectural restoration—in combination can provide more information than any single discipline could provide in isolation' (Fash and Sharer 1991: 172; see also Fash 2002; Canuto, Sharer and Bell in press)." Besides being yet another statement of conjunctive methodology, this statement is fundamentally incorrect. No one working at Copan ever cited Taylor in a publication until 2004, and no one even mentioned conjunctive research at Copan until 1991. Yet the recent phase of "research at Copan" began in 1975.

Regardless of the extent to which Taylorean conjunction is retrofitted, significant contributors to the Golden and Borgstede volume, including the editors themselves, want to impose the idea that Taylor's model shaped the field beginning in the 1950s, right up to and through the recent long phase of archaeology at Copan. This apparent foundation is therefore cited as the model for past research and is encouraged for the future. Yet we are immediately forced to ask, what *is* the "model" for research going forward? Is it a Taylorean conjunctive strategy? Is it the standard of adopting named and formalized methodologies *after* data has been collected, analyzed, and published? Or is it some mix of these and/or something else altogether?

### *Conjunctive Approach or Collective Conscience?*

Among the members of the Pennsylvania and Harvard groups there is a basic and widespread misunderstanding of the issues and criticisms raised by Kluckhohn and Taylor, a fundamental misreading of Taylor's *ASOA*, and a neglect of a critical historiography of the last sixty years of method and theory in Maya and Americanist research. Through the analysis of published work and citations presented here, it is apparent that the conjunctive models being referred to and established in lowland Maya archaeology differ in scope and details from what Taylor proposed and, perhaps more importantly, differ from one another. A powerful and prolific segment of Americanist archaeology is unconsciously searching for a cohesive, guiding, and substantiating model while conscientiously claiming a model no one can define or comprehend. In borrowing haphazardly the conjunctive approach of Walter Taylor, or some variant thereof, the Pennsylvania- and Harvard-group archaeologists have mainly adopted a methodology that can be loosely interpreted and that has been loosely applied. Taylor's conjunctive approach was not and is not a mere methodology; to borrow from Bourdieu (1977), the conjunctive approach is an outline of a theory of practice. To understand this outline, to go beyond the nagging conscience, and to adopt and practice some kind of Taylorean framework, one has to go well beyond Chapter 6 of *ASOA*. That chapter, although titled "An Outline of Procedures for the Conjunctive Approach," is no more the whole picture than Willey's (1988) autobiography is a mere set of biographical portraits. Five chapters lead to the sixth; these were Taylor's stepping-stones and should be foundations for the Pennsylvania and Harvard groups, too, so long as they continue to profess to adopt a Taylorean conjunctive model.

Since World War II, many if not most Mayanists have continued to use a culture history methodology yet have increasingly adapted it to answer questions of a processual nature (e.g., the timing and nature of the Classic period collapse). Willey's settlement-pattern approach has been a key element providing the link between these very different models for archaeological practice: it was designed for flexibility, such that it could operate on the comparative and regional (macro; environmental) scale in such a way as to address historical *and* evolutionary goals, separately or at the same time. Taylor's original conjunctive approach, although geared ultimately to the resolution of regional patterns and processual questions, specified that work must begin at the site level, tapping multiple data sets, integrating local contexts, and building a picture—a critical historiographic construction—of community change through time. The historiographic and constructionist components of the approach reflect Taylor's general (anti-positivist) theoretical perspective. The Pennsylvania and Harvard Mayanists have chosen to employ only vestiges of this approach and to interpret them narrowly, that is, as strict methodologies geared largely to justify their focus on dynastic histories.

Their need for a conjunctive model—for any model—increases in intensity during this era of abundant textual and archaeological data, indigenous movements, and questions regarding the role of archaeology in society and academia. There is a push to justify motives and practices and to, subtly if possible, erase decades or even centuries of archaeology for archaeology's sake and archaeology in the service of empire. The good attention that the conclusion of the Golden and Borgstede book gives to the present context of archaeology (affecting indigenous people and others) is evidence of this need and forms the greatest potential strength of their volume to contribute to the growth of larger and more profound theoretical debates. Commentaries such as these, however, need to be removed from the back of our books, crafted into rigorous theory, and given the prominent context they deserve as introductions, guidelines, signposts, justifications, and grounding. South of the border, where NAGPRA is absent, we are having to answer to living native peoples and to the citizens of the states whose local, national, and global identities we inevitability help to construct. Not least for this reason, we will have to work harder to develop general, high-level, self-reflexive theory that informs *all* Maya archaeology in the twenty-first century. This will need to be done in concert with a more self-conscious and critical consideration of the history of our field and the role we and our mentors—living and dead—play in perpetuating weakly defined approaches that serve neither our host communities nor the future of our archaeological anthropology. Simply put, we will need to confront the fundamentally vague and poorly defined science and social scientific approaches we have adopted and perpetuated.

### *The Carnegie Legacy: Part I*

It is fascinating, although again not surprising, that the conjunctive push is being codified by members of the Pennsylvania group, the main locus of the outstanding legacy of Carnegie-area research. (Take the example of the history of publication of the monumental book *The Ancient Maya* [Morley 1946; Morley and Brainerd 1956; Morley, Brainerd, and Sharer 1983; Sharer 1994; Sharer and Traxler 2006].) Nor is it surprising that the conjunctive approach should find its base at Copan, the last and continuing stronghold of Carnegie-influenced archaeology (Morley 1920; Longyear 1952), and in Honduras, the poorest of countries, with the least well-developed national archaeology in the Mesoamerican sphere.

The Harvard group's role in this also is not unexpected given Willey's influence and the fact that the Carnegie archaeological program was based on Harvard's campus and drew from its graduate ranks.

Just as we archaeologists discover vestiges of forms and ideas, conservative holdouts and traces of earlier patterns, the prewar culture historical bent in Maya archaeology—and especially in the elite branches of Maya archaeology—remains today in many pockets of research. At Copan, it has driven the development of a "social history" approach that employs settlement-pattern research only very selectively and that is guided weakly by references to middle-range theorisms (e.g., galactic polities; see Fash 2005). Since World War II and the publication of Taylor's book, however, a conscience has emerged regarding the long-standing gap between goals and results, and between past and present contexts (see also Leone, this volume); this helps to fuel recent and ongoing experiments with conjunctive research. Although there is an optimism in the search for principles and practices to unite the field, there is also cause for concern.

Conjunctive Mayanists are far from understanding, let alone adopting, Taylor's conjunctive approach, and for this reason vagaries and misinterpretations abound in the published record. These inaccuracies and shortcomings derive in large part from the absence of critical historiographies of archaeology at Copan and elsewhere. This is tied to the earlier long-standing absence of attribution to Taylor, itself certainly the result of most scholars never having read or struggled to assimilate Taylor's *ASOA* and/or, back in the day, fearing reprisals should they have expressed too much interest. There is also the problem of the ongoing, and in many cases healthy, competition between agendas and histories in Maya and Americanist research. For example, seemingly ready-made approaches have been adopted to challenge more widely accepted, authoritative paradigms for the study of ancient Mesoamerica. We see this at Copan in responses (e.g., Fash and Sharer 1991) to cultural ecological perspectives of Copan's history (e.g., Webster and Freter 1990). These and related debates and differences at Copan and beyond often derive from similar research foundations and could be resolved more productively with reference and attention to earlier work and paradigms.

In sum, partly because of the mind-numbing proliferation of literature and data, many Maya archaeologists today are not examining carefully enough the reports and publications of their intellectual predecessors, especially the work of founding thinkers. Moreover, in much of the Maya area, archaeologists are not attending critically to the important theoretical trends that inspired change in Americanist research after World War II. These problems are evident in the almost complete lack of general theory (i.e., "high theory," sensu Trigger 2006) in current Carnegie-derived research; the incomplete, piecemeal use of Taylor's ideas; and, especially, the application of these ideas as mere methodological advances. I take up these points again in my concluding discussion and address further their implications as well as the directions that current and future scholarship might take in efforts toward *re*solution and a stronger engagement with the demands and necessities of the present and the living.

## PART 3: DISCUSSION AND DIRECTIONS

### *High-Level Theory and the Present Context of Maya Archaeology*

Considering that more than half a century has passed since the Kluckhohn and Taylor critiques, the state of the art and status of theory in much—although certainly not all—of Maya archaeology has advanced relatively little. This is emphatically the case with respect to general or high-level theory (Trigger 2006: 415, 519–528), as distinct from more specific or middle-range theory (ibid., 508–519). This limitation is best represented by the present-day legacy of Carnegie culture historical research, namely, the Pennsylvania and Harvard groups' orientations discussed in this chapter.

Gordon Willey has been an important and constant inspiration to the Carnegie legacy, not least for his interest in and abiding respect for historical approaches. However, as Sabloff (2007: 235) notes, the contributions of Gordon Willey to the development of theory in Maya archaeology have been quite limited. Willey (1976) offered incentives for studying ideology in ancient civilizations (Demarest 1990: 7; Fash 2005; McAnany 2007), but these were never placed in the context of epistemology or philosophy nor discussed relative to previous or parallel studies in anthropology, archaeology, the social sciences, or the humanities (e.g., to Marx, Peirce, Croce, Gramsci, Childe, V. Turner, Sahlins, or Crumley; see Trigger 2006: 20, 449–451, 524). His apparent contributions to the advance of anthropological theory (e.g., Willey and Phillips 1958; Leventhal and Cornavaca 2007; Sabloff 2007: 235; cf. Jennings 1958: 1207) have mainly been to facets of cultural evolutionary theory, which, more recently, have been either refuted or recognized as depending too heavily on middle-range or bridging theories that are fundamentally not theoretical at all, but methodological (e.g., Yoffee 2005: 182–188). To the extent that Willey influenced the New Archaeology, these same limitations have been noted as the main, and ultimate, failure of that paradigm to contribute significantly to the development of general theory in anthropology (ibid.).

Many years ago, Kluckhohn (1940: 44) observed that "the greater number of students in the Middle American field ignore the categories 'methodology' and 'theory' almost entirely in so far as one can judge from their published writings." My analyses of the publications of the Harvard and Pennsylvania groups show this still to be overwhelmingly the case sixty years later. Contrary to the prevailing views of esteemed elders and leaders in the field (e.g., Culbert 2004; Sabloff 2004), the introduction of settlement archaeology to the Maya area (i.e., Willey et al. 1965)—basically a methodology, not a theoretical orientation—has done little to help the situation. Settlement studies at all scales have led to an enormous proliferation of data and data classes and to the development of analytical procedures—methodologies—for studying discrete and non-discrete contexts (Willey 1953b, 1956; Ashmore 1981; Willey 1981; Vogt and Levanthal 1983; de Montmollin 1989: 53; Nichols 1996). The epistemology undergirding the vast majority of such studies in the Maya area, however, remains fundamentally positivistic; moreover, these miss the overlap between ancient and modern settlements and therefore ignore archaeology's present context (cf. Breglia 2006).

High-level, non-positivistic, self-conscious theory is what the Pennsylvania- and Harvard-group Mayanists should be aiming for if they seek to adopt Taylor's ideas and adapt them to present-day conditions at Copan or elsewhere (e.g., Canuto, Sharer, and Bell 2004: 8; Golden and Borgstede 2004a: 6). This also would be the minimal requirement for meeting the ideals and standards laid out in 1940 by Clyde Kluckhohn. As Trigger (2006: 519) notes, the development of high-level theory is essential if we want to have a say in how our data are used and interpreted after extraction, analysis, and dissemination: "[A]rchaeologists can ignore high-level theory only at the risk of archaeological data being unconsciously shaped by the largely unexamined beliefs of the societies in which they live. . . . Archaeologists who ignore theoretical debates in the social sciences risk being dominated by the prejudices of their own societies or social groups, which can influence the interpretation of archaeological evidence at all levels." Absent high-level theory—and I continue to use this term as Trigger (2006) does and not, for example, in the strictly processual sense that Thomas and Kelly (2007: 35) do—we neglect a fuller consideration of context and its dependence on the ideas and power relations we create and employ for meaningful operations. Reference and careful attention to Walter Taylor's (1948) book and his conjunctive approach encourage us to see this context in terms of the present.

Taylor's work in *ASOA* was founded on non-Boasian principles tied to anti-positivism, adopted and modified from philosophers of history like Benedetto Croce, structural linguists, and, as Joyce (this volume) shows, semioticians and philosophers of logic and knowledge (i.e., Peirce, Quine, and Whitehead). These perspectives, in addition to his recognition of the fragmentary nature of the archaeological record, were the basis of his ideas regarding "construction." Taylor understood that archaeological practice is presently situated in a living cultural context that derives structure and significance from history and that our efforts at grasping the meaning and function of objects, and interpreting so-called past contexts, will always lead to closest approximations through repeated testing and inference. In many ways, Taylor's approach is a model of the operations of mundane consciousness, especially in terms of our daily negotiations of reality and the signified mediations of the object world (Taylor 1948: 100).

The means to adapt Taylor to the twenty-first century—should scholars maintain this as a reasonable pursuit—lies not in further methodological prescriptions and the use of bridging theories (e.g., ethnographic and ethnohistorical analogies, ethnoarchaeology, energetics studies, taphonomy, and so forth), nor in mere multidisciplinary cross-checking strategies that have been standard in Americanist research since well before Taylor, nor in abstruse references to New Archaeological paradigms and models that are fundamentally positivistic. Taylor (1948: 201) wrote that "archaeology must at least construct the fullest possible cultural contexts." The future of a Taylorean conjunctive approach rests in bringing us into conscious engagement with our present context, our motivations and goals in historical context, and the needs and responses to archaeology of living communities, societies, and indigenous groups. We must theorize the role we play and treat it critically if we are to ensure that our work is relevant and that we are doing the least harm to science and conservation, and to living identities and people. Recent and emerging theoretical advances force us to look beyond our data and objects, and therefore beyond archaeology (and epigraphy) for archaeology's sake.

As a starting point, we have myriad theoretical approaches and suggestions at hand that are more in keeping with the anti-positivist orientation of Taylor's (1948) book. For example, there exist the moderate relativism and pragmatic syntheses of Trigger (2006; Wylie 2006); the hermeneutics of Shanks and Hodder (1998; Shanks and Tilley 1987; Hodder and Hutson 2003); the critical perspectives of Leone (1981, 1986, this volume), Joyce (2003), and others (e.g., Anderson 1996); the insights granted by ethnographies of archaeology (e.g., Castañeda 1996; Breglia 2006; Castañeda and Matthews 2008); and efforts to decolonize archaeological practice and introduce ethical standards for archaeology's engagement with living people—the indigenous, our students, local communities, the public, and one another (e.g., Wylie 2003; Smith and Wobst 2005). Clearly related to this is the necessity of exploring and theorizing the relationship between archaeology and the popular media, such as Hollywood and documentary films, TV, magazines, and video games (e.g., Gero and Root 1990; Ardren 2009), and taking a hard, introspective look at how archaeologists write and produce texts and what this means for archaeological praxis (Hodder 1989; Joyce 2002). What is lacking in some sectors of Maya archaeology is not conscience, but self-consciousness; the former must be deconstructed and the latter eagerly pursued.

For Mayanists or others to explore why the conscience persists, high-level theory must integrate conceptual schemes and methodologies that further reveal and contextualize the Harvard and Pennsylvania groups, their approaches and motivations, their truth claims, and the disciplinary history of these. I agree with Pyburn (2004) that Mayanists must begin to clean house. However, I suggest this requires, on the one hand, critical ethnographies and historiographies of Maya archaeology and, on the other, not a retreat from our engagement with the present (as Pyburn 2004 recommends; cf. Pyburn 2009) but an acceptance of the inevitability of our impact and the beginning of open dialogue within the community contexts of our fieldwork and professional relations (Maca 2009). For conjunctive Mayanists, this will mean exhuming Taylor's original intentions and prescriptions and also tracing the legacy of power discourses as well as the practices of building scholarly consensus and research goals over time (sensu Bourdieu 1977; 1986). In order to examine some of the ways that this might be done I begin by returning to the case of Lewis Binford and the relational self in archaeology (sensu Hutson 2006).

### *The Carnegie Legacy: Part II*

Walter Taylor was a long-standing and central resource for Lewis Binford. However, Binford was more politically savvy in the early years than most recognize or discuss. In pitching his new paradigm, for example, he chose not to cite the useful segments of Taylor's *ASOA* and instead employed as a benchmark for his movement the authoritative and well-received work by Willey and Phillips. This definitely hampered the wider acceptance of and interest in Taylor's work until it began to get some of the recognition it deserved in the late 1960s and 1970s. Binford also held a philosophical aversion to historical context and therefore to valuing the agency of his predecessors, especially those, like Taylor, who laid out major redirections for the field. After a time, Binford even moved beyond the need to acknowledge Willey and Phillips.

By 1966, Binford had assembled a posse, or what he called his "mafia," that consisted mostly of young colleagues and former students from the University of Chicago (e.g., Binford 1972: 13). But membership in this sort of a circle was not something new for Binford: he had been inducted into the Michigan mafia years before by James Griffin, one of the notable victims of Taylor's (1948) critique. Griffin had clear ideas about who among practicing archaeologists was in or out of "the fold" and he encouraged "character destruction" (Binford 1972: 5). He even classified some museums, institutions, and archaeologists' labs as "enemy territory." Binford was among the chosen who traveled with Griffin to sites and labs, listening attentively to the opinions of his mentor. But gradually Binford realized that Albert Spaulding and Leslie White, also Michigan anthropologists, had more to offer him. He then found himself trapped with an angry lion (Griffin) on his dissertation committee who refused to sign—a hurdle that required some time and energy for Binford to negotiate. Having learned territorialism from Griffin, Binford, when still a young professor, distinguished himself by forming his own (new) intellectual circle around the ideas of Spaulding and White. These men, like Binford, got mileage out of Taylor's ideas, at times reacting to them, but they rarely adopted or openly favored Taylor's work. Did this owe to their allegiance to Griffin, longtime master of the Michigan gang?

How might we assess the intellectual implications of such allegiance? Arguably, we would first require a means to identify and characterize such circles, groups, mafias, and gangs and their influence.

Scott Hutson (2006) has explored, among other things, self-citation in journals as a strategy for boosting one's academic prestige through visibility. His preliminary results are, as he admits, equivocal, but in the course of his study he noticed that "a large portion of citations are to writers that have close connections to the author. This suggests that the 'self' in 'self-citation' might be a relational self, and therefore deserves further research. This could center on identifying 'citing circles' that result from patterned yet contingent personal relations with colleagues, as shaped by the author's academic biography" (ibid., 15). Hutson recognizes that this would be time-consuming but notes that an "extended, relational self-citation might be just as strategic as a strictly defined self-citation because promotion of authors who share the same intellectual genealogy as oneself is also a promotion of the self" (ibid., 13). Although Hutson never says so explicitly, such research could have a significant role in delineating intellectual schools, groups, or mafias in academia—posses that patrol territory, police intellectual boundaries, and hunt down perpetrators of divergent practices—or simply groups of colleagues and their students who want to establish precedent, power, and authority in a particular subdiscipline. In American archaeology, the earliest "citing circle" might be identified among the Carnegie archaeologists, although there are probably others as early or earlier, probably associated with the new age of stratigraphy (e.g., Wissler 1917).

Scholars often recognize and complain, mostly privately, that many subfields of American archaeology are incestuous and self-referential—Maya archaeology perhaps most notable among these. Future quantitative work like Hutson's might derive the parameters, characteristics, and significance of such trends. An analysis of the Pennsylvania and Harvard groups, for example, could yield fascinating data on the development of Mayanist research before, during, and after the Cold War. We might even hypothesize the direct development of relational selves from an earlier circle of Carnegie researchers. Such a procedure, properly elaborated, might help us to explore a number of issues and phenomena, including but not limited to how a social history perspective of Classic period Maya dynasties is linked to culture history perspectives and to early and more recent applications of the direct historical approach; how the Carnegie Institution's role in the prewar military-industrial complex and the contributions of its archaeologists to espionage shaped the practical interests of Maya research before and after World War II; the extent to which and how methods, goals, methodologies, and theory (and definitions of these) have or have not changed since the 1940s; how Carnegie-era research developed from earlier circles and trends; and how these and related trends reflect U.S. interests, Anglo-American sociopolitics, global issues, currents in Latin American politics and economics and tie into other developments in the natural sciences and humanities. The study of citing circles would also be the logical starting point for a historical ethnography of Maya archaeology and would naturally build on a graduate teaching strategy that Taylor perfected (see Reyman 1999; Clay, this volume).

Offering criticism of the Carnegie program in archaeology has never been an easy task, for this institution has literally and figuratively dominated Americanist research; its legacy remains formidable. It has certainly been almost impossible to critique the Carnegie from within. Ernest Becker (1979), in his study of J. Eric S. Thompson's ceremonial center theory, is perhaps the only Mayanist to have done this with any degree of acceptance (although he did so decades after the formal end of the Carnegie era). Other critiques have mostly been offered only by outsiders, and excepting Kluckhohn and Taylor, these typically have had little impact on the field. Joyce (this volume) makes precisely this point when discussing Taylor's brilliant (1941a) ceremonial bar article and the fact that it had no apparent influence on Proskouriakoff or Thompson, even though they read and considered it. In fact, various critiques of the Carnegie do exist; all are offered by outsiders and some are forceful. These range from John Bolles (1932) and George Kubler (1990), an architect and art historian, respectively, to Thomas Patterson (1986) and Curtis Hinsley (1989), both historians of anthropology (Patterson is also a well-known archaeologist), to the ethnographer Quetzil Castañeda (e.g., 1996).

The least well-known critique—because it is unpublished—is that of Bolles, contained in countless of his field notebooks currently archived in the Peabody Museum at Harvard. While still a young man, Bolles received his architecture degree at Harvard and worked in Mexico for the Carnegie; he later became famous for designing the Candlestick Park sports complex in San Francisco. In his field notes from Mexico he left us with many acerbic critiques of the Carnegie dating to the time he worked for Sylvanus Morley,[24] Gustav Stromsvik, and Karl Ruppert at Chichén Itzá. Bolles disliked immensely working for the Carnegie Institution and his notes are filled with gems of critique from an outsider who worked on the inside. Among his numerous criticisms is the following: "I'm thoroughly fed up with all the a—sucking that goes on around this bloody institution—and the way people get away with it is so damned disgusting it is amusing. The worse thing here is to sit around the bachelor's house hearing the grand denunciations of some work of S.G.M. [Morley]. I have often brought these very questions up at the table only to have the same parties about face and whiningly 'cowtow'—if that's what the institution wants I'm through." (Bolles 1932, 5: 4). He also characterizes Morley as "beyond a doubt the world's worst judge of art and people." (ibid., 60). In general, Bolles's extensive notebooks make it patently clear that leadership at Chichén Itzá was weak, that work was frequently poorly conceived, and that few could challenge the status quo in any productive way (see also Black 1990). The following account further characterizes the way work was done at Chichén Itzá:

> I can't understand why the rush to complete the work on the Mercado, other than to make a display for next year. It is simply ridiculous for anyone to attempt restoration work there other than Karl. For Morley, who knows nothing of what is planned there, and but little of what has been found, to instruct Stromsvik, who knows even less about it, as to how the work should be done is simply too absurd and ridiculous to be expressed in words. Karl is too sick to offer resistance, but upon his return if the work done is not exactly as he planned he will tear it out. Needless to say, Morley's obvious reason for putting Gus in charge is that he knew no one else would carry out such foolish plans. (Bolles 1932, 2: 52)

This next passage offers criticisms akin to those of Kluckhohn and Taylor, especially regarding the Carnegie approach to science:

> And what is this? We were told it was "Mayan" and now you are not sure—and the "Market place" is Toltec, strange, when you are working in a Maya city! And where do the Toltecs come in—you are not sure!—And to think we thought the Carnegie Institution was an organization for the promotion of science, and here you have spent your money "disseminating" knowledge you do not have.—And how about Maya architecture? You hope to get around to that someday! It does not occur to you that you are studying backwards, that you might determine from data you had gathered in Mexico and Yucatan just where your Toltecs come in. No, you restore a Toltec structure, publish pretty pictures and a story of how great an engineering feat you have accomplished, and make no analyses of the art or architecture in its relation to the entire area. Baloney! No wonder people think archaeology a rich man's hoby [*sic*]. You defend yourselves as "scientist" and yet prove nothing. You build a guest house and interrupt all work for someone who might give to your work (whereas you would rather entertain wealth in your own personal selfishness)—and your net profits are a lot of grief, loss of time playing tourist guide, and a general disruption of work. . . . These are not notes—just sort of an apology [to] the dear old Monjas for the Carnegie. (Bolles 1932, 4: 7)

Bolles makes countless similar critical assessments of the quality and direction of research and also, for example, of the Carnegie reports: "Before continuing might we mention the histories of the Warriors and Caracol [buildings] reports—and the dozens of other reports that have never appeared. Might I remind you of the staff employed on the Warriors and the resulting inadequate report, an architectural problem with no attempt to study it having been made. And the years spent on the job and on the report and salaries paid" (ibid., 5). All of the above comments are intriguing characterizations, even indictments, but perhaps the most devastating of Bolles's assessments of the Carnegie work at Chichén Itzá is the following:

> Best remark the last few days was one Dr. Proctor wrote to Karl. He had had a talk with James Breasted [the renowned Egyptologist] and came away

> convinced that the Carnegie Chichén project would profit if something of Egyptian archaeological methods were known—and referred to Morley's particular lack of such knowledge! Boy, do I agree –it's amusing to watch these Southwestern pot hunters go after architecture! Their lack of understanding of architecture is no better illustrated by their belief that sailors and bond salesmen can make comprehensive drawings and studies of it.

These are just a few of the richer criticisms provided by the Bolles notebooks. I offer them here in large part because the Carnegie Institution, one of the most vital foundations for current approaches and power in American archaeology, is treated with kid gloves in most histories of the discipline. Taylor's rough handling of Kidder and the Carnegie is still considered somehow exceptional and even unwarranted or unnecessary and gratuitous. Other scholars have criticized the Carnegie program for attempting to divorce archaeology from the humanities (e.g., Kubler 1990: 195) and, especially, for serving U.S. imperial goals (Patterson 1986; Hinsley 1989: 82–83; Harris and Sadler 2003; Price 2008). Patterson (1986: 12–13), for example, writes:

> The Carnegie archaeological program was not value free and neutral, for it carried a subtle political message to the revolutionary government of Mexico and to the peoples of Central America. By focusing on the Maya, "the most brilliant culture of the pre-Columbian world," the archaeologists were implicitly questioning the unity of the Mexican state and the cultural attainments of the ancient societies of central and northern Mexico—the regions that controlled the modern state.

Harris and Sadler (2003) and Price (2008) discuss the degree to which and how archaeologists like Morley, and many others associated with the Carnegie, were spies during World War I and later. The insights of these authors, like those of Bolles and Taylor, beg the question of whether or not Carnegie goals for archaeology centered on science, for it is clear that fieldwork was a ruse in some instances (e.g., Harris and Sadler 2003: 61).

Where they exist, critiques of the Carnegie Institution programs in archaeology and anthropology challenged what we might call the truth regime or discursive regime (Foucault 1981) or the regime of power (Blommaert and Blucaen 2000: 449) in American archaeology. Although other critiques emerged in the era of Taylor, such as Strong (1936), Steward and Setzler (1938), and Bennett (1943), these were merely *implicit* criticisms of Carnegie-type archaeology. Only Walter Taylor—not Clyde Kluckhohn—leveled an attack that both dissected at length the fine details of the Carnegie's aims and accomplishments and offered an alternative, philosophically grounded model for American archaeology. As a result of his challenge, he suffered dearly the consequences. The backlash from the power center was palpable and has remained so until the last decade. Much more recently, the ethnographer Quetzil Castañeda (1995, 1996) has revisited and critiqued the Carnegie research at Chichén Itzá and especially the effect of 1930s anthropology on the nearby town of Pisté. Building on the results and lessons of his study, he challenged the goals and truth claims of present-day Maya archaeology, asking that we assess the impositions of our research on living communities, today and historically. In turn, Castañeda, too, encountered terrific professional resistance and has been validated only very recently (e.g., McGuire 2008: 12–14).

It is vitally important to highlight the need for further critical studies of power relations and the community of American archaeology. A preliminary focus on Maya archaeology would be appropriate, for segments of this subdiscipline—still operating outside the reach of NAGPRA—preserve the legacy of the Carnegie's goals, standards, assumptions, biases, and power politics. I argue this legacy is apparent at Copan and in the research and writings of Pennsylvania- and Harvard-group scholars. It will likely be identified elsewhere if we are aware of the need to pursue its expressions and if we adopt useful theoretical and methodological tools to aid our efforts. Tools of the latter variety are not numerous, but ethnographic study of archaeology (e.g., Castañeda 1996; Breglia 2006) and citation analysis (e.g., Hutson 2006) may serve as two basic directions. Another avenue may be found at intersections of theory and methodology in critical discourse analysis, or "CDA" (Fairclough 1992, 1995; Blommaert and Bulcaen 2000). Although centered on linguistic aspects of discursive practices (e.g., in texts) and the methodologies to study these, CDA also seriously considers power relations and the force of ideology in shaping discourse. Theories of hegemony, tied especially to the writings of Antonio Gramsci, are central to CDA and may assist us in examining discourse in a larger theoretical context that includes, for example, the seminal works of Michel Foucault and Pierre Bourdieu. If we are to ever more fully examine the silences surrounding Walter Taylor and his book, the exclusion of Taylor from bibliographies of authors who borrowed his ideas, and, in particular, the uncritical adoption of conjunctive approaches now hailed as Taylorean, we should begin with studies of discourse and power. To conclude this chapter, I offer a brief suggestion for how and why this can be a useful undertaking.

## CONCLUSION: TAYLOR, DISCOURSE, AND POWER

Norman Fairclough (e.g., 1992; see Blommaert and Blucaen 2000: 447–449) has spearheaded the theorization of CDA and conceives the analysis of discourse (in speech acts and texts) along three interrelated lines: discourse as text (linguistic features and instances of discourse); discourse as discursive practice (something that is produced, circulated, and consumed); and discourse as social practice (the ideological effects and hegemonic processes of discourse). In this way, as Blommaert and Blucaen (2000: 449) note, "CDA's locus of critique is the nexus of language/discourse/speech and social structure." Thus, changes in discourse can reflect changes in hegemony, that is, "power that is achieved through constructing alliances and integrating classes and groups through consent" (ibid.). A tidy overlap might be conceived here with, especially, citation analysis and studies of citing circles and the relational self in academia.

The prevailing discursive regime in 1940s American archaeology was closely aligned with the Carnegie Institution (including its affiliates based and trained at Harvard) and its massive support for culture historical approaches. Taylor appeared and offered direct resistance to this regime, establishing new and alternative discursive nodes and content and the theoretical views that gave them life. This was a shock, and in the decade after the publication of Taylor's 1948 book, discourse and its attendant social structure in American archaeology changed dramatically. Thus, by 1959 we see the emergence of a new idiom and a new center of ideological power within the discipline (e.g., Caldwell 1959). After a few years, this discursive regime was formally adopted and codified (e.g., Binford 1962).

In spite of Taylor's book, and because of it, the prewar power structure in American archaeology endured well into the 1950s. Carnegie and related sociopolitical alliances and statuses in the discipline continued to be reproduced in scholarly texts, funding cycles for research, university hiring, and institutional corridors and smoking rooms (see Bourdieu 1986). The exercise of power at that time drew itself around the need to repress Taylor's message, and this meant rallying support and alliances, refraining from extensive responses or rejoinders, and pushing Taylor and his work to the margins wherever this was possible. After a time, however, the proverbial jig was up and, in combination with new social trends in the United States, some of which were associated with the GI bill, there emerged a shift in the power center of the discipline, led by Binford. Although there was a shift in discursive practice that signaled this shift in disciplinary hegemony, the repression of Taylor and his work continued for years and, in many ways, has endured until very recently. This has resulted from the reproduction of values and ideologies tied to status and competition in academia and, especially, to the ongoing support for a positivistic practice of archaeology, one that, whether culture historical or processual, maintains the reconstruction of the past as a central goal. The most unusual phenomenon related to this repression is another power-related trend that cannot be explained in strictly repressive terms, that is, the emergence of conjunctive approaches in Maya archaeology, first without Taylor's name and later with Taylor as the iconic progenitor. The work of Foucault helps us to understand this latter trend.

Foucault's approach to orders of discourse and power have inspired many recent studies, including those (like Fairclough's [e.g., 1992; 1995]) that conceive of power very differently. Foucault conceives of power in terms of its enabling and productive effects and divorces these processes from considerations of morality, fascist repression, and so forth. Partly for these reasons, his interests in the will to truth of any given discipline or society hark to Nietzsche. For example, Foucault focuses on questions of knowledge and power and their productive life cycle, that is, on the effects of power rather than on the effects of ideology per se. "Discursive regimes" are therefore produced as the result of drives toward specific knowledge that seek to define what qualifies as truth and what does not, what are acceptable orientations toward building knowledge and what are not. The ongoing production of truth (i.e., the making of truth claims that are acceptable) shapes and reinforces power structures. In this way, we can consider that every discipline, each society, even an individual institution, possesses "types of discourse that it accepts and makes function as true" (Foucault 1984: 72). Thus, there develops a "regime of truth" that is mirrored in the discursive regime produced through, for example, science and its expressions of knowledge, such as texts. The key here is productivity, production, and what Foucault (ibid., 61) calls a "productive network" that "runs through the whole social body" (see also Bourdieu 1986, regarding social capital). Although not repressive, this productive drive is like other systems of exclusion; thus it is exclusive. Foucault (1981: 55) writes that "[t]his will to truth . . . rests on an institutional support: it is both reinforced and renewed by whole strata of practices, such as pedagogy, of course; and the system of books, publishing, libraries; learned societies in the past and the laboratories now."

The Pennsylvania and Harvard groups have adopted and evolved a discursive regime over the last twenty years that employs models of and for a conjunctive approach. This order of discourse has become especially marked and prominent at Copan (Bell, Canuto, and Sharer 2004), although it is increasingly enlivened and actively produced for broader consumption (Fash 1994; Golden and Borgstede 2004a). As I point out in an earlier section of this chapter, the conjunctive approach manufactured by these groups of scholars is one that centers on a methodology—namely, cross-checking multidisciplinary research—and especially the use of this methodology in the pursuit of dynastic histories, or in Fash's (2005) words, "social history." With an ongoing focus at Copan on inscribed dynastic monuments, relatively unchanged in its intensity and locus (i.e., Copan's Principal Group) over the last 160 years, the "conjunctive" social history approach depends on texts—Classic period Mayan texts on stone stairs, stelae, and altars and on ceramic vessels. The will to truth therefore is built around an interest in textual decipherments and the truth claims these offer and corroborate. At Copan, Morley (1920) codified this interest, but the search for and fascination with inscribed monuments at Copan and other Maya sites begins with John Lloyd Stephens (1841, 1843) and his designs for a museum of the Americas, or a clearinghouse for displaying the inscribed art and history of the Americas. Texts and dynastic art carried supreme value to Stephens in terms of capital and history, and they still maintain this value today—much more so than in the nineteenth century because of the current pervasiveness and power of university and international museums, documentary film networks, and the black market for antiquities.

There have been relatively few attempts to theorize Mayanists' interest in and use of Classic Mayan texts in the present (Rice and Rice 2004), but there is little question that access to texts and their decipherment enhances professional statuses. Therefore, we might see the production of a conjunctive approach, or a variety of conjunctive approaches, as a will to knowledge or a truth regime that is dependent on and reflected in scholarly discursive practices embedded in the study of ancient discursive practices. These are themselves embedded in and reflective of disciplinary social practices that support them and that support the associated museums, university programs, and positions in epigraphy. The point here is that the conjunctive approaches variously produced today, although certainly remnants of conscience originally seeded by Taylor, have become more closely tied to a set of values that direct or drive the pursuit of ancient elite discourses. The negotiation of these ancient discourses underwrites status among those who give this priority in research and who engage the current discourse by adopting so-called conjunctive measures, thereby joining the circle of reference, power, and citation. In this way, the ancient texts, and the conjunctive texts produced as a result of these, become the goal of the will to truth, *obviating the need for a non-text-based archaeology that might require greater attention to theory*. Mere methodology is all that is necessary within this circle or regime of power because the mere study and reinforcement of elite status through specific discursive productions is and has been the norm for generations. The adoption of a conjunctive approach lends cachet—ironically—as well as, increasingly, a means for giving the regime a name.

Textual evidence is vital and fascinating, but it is obviously self-limiting in terms of the archaeological and anthropological goals and data it supports, even when linked to other material contexts. Moreover, it can only be used very specifically at the interface between living communities and archaeology and also may misrepresent or overstate ethnicity in the past (Maca 2009). This is not to suggest prehistory as a more "pure" archaeology than the historic archaeology that now characterizes Classic Maya research (Johnson 1999: 161), but grassroots ("dirt") archaeology in the Maya area is still in the developmental stages. Almost thirty years ago, Joyce Marcus (1983: 482) noted that Mayanists studying the Classic period have sprinted ahead to processual questions (e.g., the collapse) without knowing what kind of society the Maya possessed. As if to explain this gap, ten years later Gair Tourtellot (1993: 293) concluded that none of the best-known Classic period sites and cities are even fully mapped. Since then this situation has improved only in a few select contexts (e.g., Barnhart 2001, 2007). Thus, the question becomes, how can archaeologists more fully elucidate past Maya society now that many more issues and challenges have emerged?

The answer may lie in engaging stakeholder communities, especially through archaeological ethnography, ethnographic archaeology, and forms of ethnographic settlement study (e.g., Breglia 2006) that consider the nexus between ruins and their modern support communities—no doubt a deeper context for a twenty-first-century archaeology of communities. Furthermore, to practice true holism and reflexivity and to build fuller conjunctions that draw out more elusive affinities, we also need to engage the community of those who work mainly to construct the past. Twenty years ago, at the formal end of the Cold War, C. C. Lamberg-Karlovsky offered the edited volume *Archaeological Thought in America* and in his introductory chapter stated that "[a]rchaeology today can be conjunctive, behavioral, ecological, cognitive, New, processual, historical, Marxist, analytical, symbolic, and so forth" (1989: 12). This perspective might seem outwardly to sanction freedom of approaches, and even to validate Walter Taylor. However, Dena Dincauze's (1990) review of the book notes that the viewpoints therein mostly derive from a relatively narrow institutional lineage and that "thought" or theory is not the volume's focus. Aided by Lamberg-Karlovsky's (1989: 10) own notions of "ideological tribalism" and "communal solidarity,"we can examine this lineage (and others) in ways that I have proposed here with respect to the Carnegie lineage that currently disseminates variable and named conjunctive methodologies in the Americas and beyond.

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## NOTES

1. Although it may seem on the surface that this school of thought is emerging just now, Kluckhohn (1940), in his critique of Middle American archaeology, cited the existence of the "Pennsylvania group" and discussed the limitations of their and others' approach to archaeology. See the section in this chapter that discusses Kluckhohn.

2. Two University of Pennsylvania professors, Jeremy Sabloff and Richard Leventhal, both Ph.D. students of and collaborators with Gordon Willey, suggested and/or encouraged the presence of other paradigms. Sabloff (1990) has discussed the New Archaeology paradigm in Maya archaeology, arguing that it is a standard and widely accepted approach. More recently, Leventhal (with Cornavaca 2007) has declared that the only paradigm (ever) in American archaeology as a whole is the one generated and promulgated by Gordon Willey and Philip Phillips (1958), namely, a comparative cultural evolutionary approach, aka the New Archaeology.

3. "Fabulous 50s" is a reference to Longacre's discussions (2000; this volume).

4. In 2001, Woodbury flatly refused to contribute to this volume. He was and, I imagine, remained a loyal supporter of Alfred Kidder and Kidder's legacy. This did not prevent him from acknowledging, however, that "[l]ater, the constructive side of [Taylor's] analysis began to be recognized as an important part of a gradual but profound change in archaeological thinking, culminating in the New Archaeology" (Woodbury 1993: 148).

5. In G. R. Willey's (1988) portrait of A. V. Kidder, the elder man's preeminence in the field is highlighted by an anecdote regarding an approving note from Kidder, received in the mid 1940s: "That I was almost tempted to frame this letter and hang it on my wall is a measure of the prestige that Kidder carried at that time in the American archaeological profession" (ibid., 295).

6. "Space-time systematics" has been characterized as "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).

7. Clyde Kluckhohn's (1940) critique of Maya studies was published in a festschrift for Alfred Tozzer.

8. Pages 47–50 of Taylor's book (1948) discuss Kidder's work in the Southwest.

9. Robert Singleton Peabody (1837–1904) was the nephew of the childless George Peabody (1795–1869), the famous philanthropist and founder in 1866 of the Peabody Museum at Harvard University. R. S. Peabody founded the Peabody Museum of Archaeology at Phillips Andover Academy in Massachusetts in 1901 and is known to have later come to possess the long-lost business papers of his uncle. Alfred Kidder worked for the R. S. Peabody Foundation in Andover after 1914.

10. Quetzil Castañeda (2005) has conducted extensive research on the Carnegie Institution of Washington and has crafted a vital analysis of its aims and administrators. He provides a useful overview of the CIW and writes that it was founded in 1902 "as a research institute during the formative period of U.S. science. The CIW participated in the general emergence of a public sphere that was driven by the great philanthropic foundations and initiatives of the first decades of the twentieth century. It was part of the emergence of a new governmentality that was to transform citizen and society along the lines of scientific knowledge. As a part of its scientific mission, it supported research in many, but not all, areas of science, including archaeology and, eventually, social anthropology. The legacy of the Carnegie also includes, to a greater extent, the shaping of both the U.S. military-industrial complex and the contemporary structure of scientific research in the United States" (Castañeda 2005: 27). See also Castañeda's (1996) book on the Maya ruins at Chichén Itzá and the role of Carnegie scientists in the study of that site and the region. See also note 13 below.

11. In 1938, Vannevar Bush was elected president of the Carnegie Institution. There he authored the proposal to President Roosevelt titled "Science: The Endless Frontier." In a letter to President Roosevelt (July 5, 1945), Bush wrote, "The pioneer spirit is still vigorous within this nation. Science offers a largely unexplored hinterland for the pioneer who has the tools for his task. The rewards of such exploration both for the Nation and the individual are great. Scientific progress is one essential key to our security as a nation, to our better health, to more jobs, to higher standard of living, and to our cultural progress." In 1950, he directed the creation of the National Science Foundation.

12. As Willey explains, "the kind of synthesis of it all that [Kidder] had been hoping for had not emerged—and, of course, because he was a synthesizer, par excellence, he blamed himself" (1988: 303).

13. Colonel Charles Lindbergh resigned from the board of trustees of the Carnegie in 1941 because the Carnegie was conducting military research for the U.S. government (see Weeks and Hill 2006: 17). It is certainly possible that Taylor similarly felt that the CIW, focused as it was on military-industrial science, was not an ideal base for anthropological archaeology.

14. Coe (2006: 114) writes, "Walter Taylor's *A Study of Archaeology* [*sic*] . . . gave the Carnegie archaeologists a severe critical drubbing, and effectively finished Carnegie as an archaeological institution." Weeks and Hill (2006: 15–16) write, "How much influence Kluckhohn's critique of CIW Maya research had on the institution's directors is unknown, but it was soon reinforced by a much more extensive and carefully documented attack [i.e., Taylor 1948] on the program." They go on to say (ibid., 16), "[C]riticism by Kluckhohn and Taylor did not go unheeded and may be seen in the CIW's subsequent Mayapan project."

15. The following are statements included in Kluckhohn (1940). "Maya archaeologists, for example, should not be interested merely in any set of facts as such. . . . [Unless] data are gathered and presented in such a way that they can be so conceptualized by other workers they are intellectually useless. Hence the broad outlines of a conceptual scheme should be present in the consciousness of the investigator and clearly stated" (42). "The content of science cannot be wholly fact. For if it were there would be no 'crucial experiments'" (42). "The charge that theory leads to a 'crippling of experimental research' is tantamount to a denial of the whole history of modern physics. From Copernicus and Kepler on, all the great figures in Western science have insisted, in deed or in word, upon the futility of experimental research divorced from theory" (47).

16. In a large section of his published homage to Kluckhohn, Taylor (1973a) goes to some length to demonstrate that Kluckhohn's famous 1940 paper did not truly engage in criticism of Middle American archaeologists. This part of Taylor's paper reads a bit like a defense of his own (1948) work, and although one may find his argument disingenuous, he makes a number of valid points, many of which ask the reader to question what constitutes "criticism" and why.

17. Although Willey and Phillips (1958) repeatedly mention "developmental" change, they only use the word "evolution" once in their book. Given the topic, objectives, and approach Willey and Phillips encourage, as well as the date of publication, the overall absence of "evolution" in the book is striking and must have been intentional.

18. Sensu Kuhn 1962.

19. Strictly speaking, this is the first indication of a divergence from Taylor's original framework. As I discuss in Chapter 1 of this volume, the approach advocated by Taylor was interdisciplinary—a blending of disciplinary ideas and practices, crossing disciplinary boundaries—rather than multidisciplinary, which maintains disciplinary boundaries and which is cooperative but not integrative (Lattuca 2001: 10–12).

20. Marcus is not (strictly at least) a Mayanist, although her published doctoral dissertation (1976) examined the Classic period lowland Maya.

21. Marcus's close colleague, Kent Flannery (2000), appears to follow a Taylorean perspective where he offers a Mesoamerican *social* evolutionary approach as an alternative to cross-cultural evolutionary studies. See also Marcus 2008.

22. According to Sterud (1978) and Caldwell (1959), Taylor was among the founding processualists.

23. David Webster (1999, 2000) recommends a "cultural ecological" approach at Copan, but this is geared toward the regional scale, not the scale of an urban community (see Maca 2002: chapter 2) and does not focus on the integration of archaeology and history (see Fash and Sharer 1991).

24. Sylvanus Griswold Morley was born in 1883. Although an affront to C. P. Bowditch resulted in Morley being denied a Harvard Ph.D. and the support of the Peabody Museum, he came to define an era of Maya archaeology. Morley was ubiquitous, not least through his substantial roles at the Carnegie Institution. He died two months after the July 1948 publication of Walter Taylor's *A Study of Archeology*.

CHAPTER SEVENTEEN

# WALTER W. TAYLOR IN THE SOUTHWEST

*Don D. Fowler*

The cultural Southwest has been defined as extending from "Durango, Colorado, to Durango, Mexico, and Las Vegas, New Mexico, to Las Vegas, Nevada" (Reed 1951: 428). Walter W. Taylor conducted two archaeological research projects within the cultural Southwest, the Coahuila Project, in 1937, 1939–1941, and 1947 (Taylor 1966a: 59–84; 1972b; 1988; 2003; Arratia 2008), and a Pueblo Ecology Study, in 1949, 1951–1952, and 1954 (Taylor 1958b). In addition to these two projects, he published a paper on the history of Southwestern archaeology (Taylor 1954) in the first and, to date, the last attempt to survey the entire field of Southwestern anthropology (Haury 1954). Finally, he published a paper (Taylor 1961) attempting to develop a genetic model to tie language families to archaeological complexes and what little physical anthropological data there were in western North America.

Taylor's Coahuila Project has been the focus of much attention, both for what he did and did not accomplish. He did, minimally, provide some information on site stratigraphy—much of it the work of the late Albert Schroeder—and a good deal of information on some of the textiles (Taylor 1988, 2003). He did not produce a "conjunctive" study of the sort he so loudly trumpeted in his *A Study of Archeology* (Taylor 1948), nor a full project report. Further discussion of the Coahuila Project is contained in Taylor (2003; see also Arratia 2008) and other chapters in this volume. This chapter focuses on his other Southwestern work.

## THE PUEBLO ECOLOGY STUDY

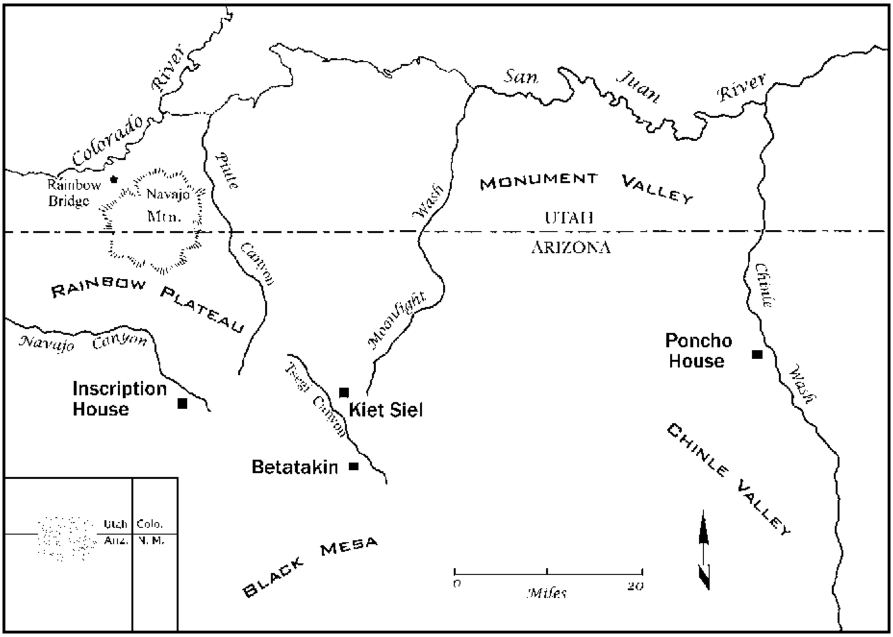
### *Background*

Taylor (2003: 1) began graduate work in anthropology at the University of New Mexico (UNM) in the fall of 1935. He had spent the summer excavating some Kayentan (Fig. 17.1) sites for the Museum of Northern Arizona (MNA) under the direction of Lyndon Hargrave, and worked there in 1936 and 1937 as well. In 1935, the Anthropology Department at UNM was in a state of transition. The department had been founded by Edgar Lee Hewett in 1927, but by 1935–1936 he was being eased out by the younger scholars he had helped hire, among them Leslie Spier, Donald Brand, and Clyde Kluckhohn (Fowler 2003). Taylor (2003) recounts that both Spier and Brand, especially the latter, pointed him toward, and helped facilitate, his work in northeastern Mexico (the Coahuila Project).

Hewett had established an archaeological field school in Chaco Canyon in 1929. From 1935 on, Brand, Kluckhohn, and others in the department ran the field school, with various faculty members or guest faculty as field directors. Taylor was hired as foreman for the field school in 1938 and 1939. The 1939 field director was Frank Setzler, then curator of anthropology in the U.S. National Museum of the Smithsonian Institution. Setzler had worked in the Big Bend country of Texas and suggested that Taylor could profitably work in Coahuila across the Rio Grande. A friend of Taylor's, Walter C. Teagle, put up $2,500, which the Smithsonian administered as a research fund after naming Taylor a collaborator in anthropology of the National Museum (Taylor 2003: 4–5).

After Taylor returned from World War II, he was reappointed as a Smithsonian collaborator. The research fund was still extant and was renamed the Northern Mexico Archaeological Fund (Taylor 2003: 7); it may have been replenished by Taylor's own money. After a "fiasco" in 1946 prevented him from getting to the field, Taylor was able to complete field work in 1947. The subsequent history of his Coahuila Project is told in Taylor (2003: 7–12).

By 1949, Taylor was living in Santa Fe. He had revised his dissertation for publication (Taylor 1948) and turned his attention away from Coahuila to the Colorado Plateau. The archaeological cultures thereon had been intensively studied since the mid-1870s (Fowler 2000: 79–127, 140–143, 148–202, 275–320). The Pecos Classification (Kidder 1927), the famed Basketmaker I–III and Pueblo I–III sequence, provided a chronological framework; subsequent work had defined regional variants, minimally Chaco, Mesa Verde, Little Colorado, and Kayenta. A. V. Kidder (1936) had grouped the variants under the rubric "Anasazi."[1]




**17.1** Kayenta region, Utah-Arizona, the scene of Taylor's Pueblo Ecology Study (map by Patricia DeBunch).

The Kayenta region lies within the northwestern section of the Navajo Reservation in Arizona and Utah (Fig. 17.1). The area is dominated on the north and west by Navajo Mountain and several large mesas, cut by deep saw-cut canyons that drain into the San Juan River (from both north and south), Glen Canyon of the Colorado River, and the Little Colorado River. Three of the most famous, indeed, iconic, Kayenta Anasazi sites, Betatakin, Kiet Siel, and Inscription House, are located in this section of the reservation. Black Mesa and Monument Valley are in the center of the region and the Chinle Valley, the Defiance Plateau, the Lukachukai Mountains, and Carrizo Mountain lie to the east. The area contains some of the most spectacularly scenic—and logistically difficult—country in the Southwest.

By 1949 the Kayenta region had been explored and numerous sites therein excavated for seventy plus years by pack-mule/horse-supplied expeditions led by the Wetherill brothers, beginning in the 1880s; by Byron Cummings and his students from the 1910s into the 1930s; by A. V. Kidder and Samuel Guernsey (1919; Guernsey and Kidder 1921; Guernsey 1931) from 1914 through 1923; by Charles Bernheimer (1923, 1924) and Earl Morris in the 1920s; by Neil Judd (1924a, 1924b) and John Wetherill in 1923; by Noel Morss (1927, 1931) in the late 1920s;

by Julian Steward (1941) in 1932 by boat in Glen Canyon; and by the Rainbow Bridge–Monument Valley (RB-MV) expeditions (Hargrave 1934a, 1934b, 1935a, 1935b; Beals, Brainerd, and Smith 1945; Crotty 1983) from 1933 through 1938. Clyde Kluckhohn, Taylor's mentor at Harvard, while an undergraduate student in Classics at the University of Wisconsin in the mid- to late 1920s, had led a series of packhorse trips into the region to Rainbow Bridge and beyond, across the Colorado River in Glen Canyon and onto the Kaiparowits Plateau (Kluckhohn 1927, 1932). Many sites also had been looted and picked over by mining prospectors who swarmed into the Navajo Mountain / San Juan / Glen Canyon areas in the 1890s and again in the 1930s (Crampton 1959). (See Adams 1960; and Fowler 2010: chapter 13, for overviews and summaries of these and various other expeditions in the region.)

In 1948–1949, Taylor decided that the Kayenta region would be a possible place to develop a new project. As noted above, he had some familiarity with the area, having worked there for MNA and taught at Arizona State College (now Northern Arizona University, Flagstaff) in the late 1930s (Euler 1997: 23; Reyman 1999: 681).

In September 1949, Taylor submitted a proposal to the Smithsonian to establish a Southwest Archaeological Fund, probably using his own money. The proposal was accepted on November 14, 1949 (Taylor to Wetmore, September 22, 1949; Wetmore to Taylor, October 5, 1949; Wetmore to Taylor, November 14, 1949; Setzler to Taylor, November 14, 1949 [TP/NAA]). He then (Taylor 1958b: 1n1) submitted a proposal to the fund for a Pueblo Ecology Study and received an initial $2,500 to get started.[2]

The study had two stated goals. First, to "present an example of the development of an archaeological problem couched in cultural (rather than taxonomic) terms." The "problem . . . was [to be] a cultural one, and the approach thereto would have to be *conjunctive*, rather than strictly taxonomic" (Taylor 1958b: 1–2; emphasis added). The second goal was to address a specific "culture-environmental relationship," that is, "to learn whether the alleged Great Drought of 1276 to 1299 A.D. affected the Anasazi culture of northeastern Arizona, and if so, in what way" (Taylor 1958b: 1). In 1949, the existence and impact of the Great Drought were still debatable issues, hence Taylor's proposal to address them was legitimate.

Taylor decided that to meet his goals, he would need to locate "cliff house" sites of the right tree-ring age (late Pueblo III, in the Pecos Classification) filled with dry, undisturbed deposits containing not only artifacts but ecofacts—floral and faunal remains. The materials, seemingly, would provide the hard data required to demonstrate his approach and resolve the "culture-environmental relationship" of the timing and impact of the Great Drought, if there had been one (Taylor 1958b: 1–2). Taylor does not specify how he planned to use floral and faunal remains as climatic indicators.[3]

> In our conception of the problem and in our approach to it, one of the prime requisites was a most detailed and extensive cultural interpretation of the excavated materials. When the time came to convert the empirical facts of archaeological field work into inferences about culture, culture change, responses to environmental and human pressures, and the whys and wherefores of a mass of conjoined cultural and environmental data, we would need every possible assistance, particularly from the archaeological and ethnographic sources. Therefore, our ultimate choice of the Kayenta region for our first field work was due (1) to the assumption that the modern Hopi, at least in part, are the cultural descendants of peoples once living in the region, and (2) to the existence of a large literature on Hopi culture, to which we could turn for assistance in our task of interpretation. (Taylor 1958b: 2)

Taylor spent two days, October 6–7, 1949, in Tsegi and adjacent Dogoszhi canyons, accompanied by Milton Wetherill, an MNA employee, and archaeologist Dale S. King. They looked at six sites, all previously recorded by MNA and/or RB-MV. None were deemed suitable (Taylor 1949).[4] In a letter to Harold Colton, director of MNA, Taylor wrote:

> Just what will become of our project, I cannot say right now. . . . [I]t is not at all impossible that the sites of deposits which we need . . . are non-existent . . . or at least very rare, especially now after years of potting by various people, including some pseudo-professionals! The crucial 25 years from 1275 to 1300 are very few and probably productive of rather meagre [*sic*] and thin deposits are best. . . . I believe our hope lies in such places as Kiet Seel and Betatakin. . . . The problem is: can we find others like them? That is find them near enough to Black Mesa and the Hopis to make our assumption of cultural continuity not too tenuous. . . . I'll work on the problem some more from the library and 'logical' [*sic*] angle [W. Taylor to H. Colton, October 18, 1949, TP/MNA]

Taylor apparently did not "work on the problem" during the first half of 1950. But a meeting at the 1950 Pecos Conference with then-graduate student William (Bill) Y. Adams helped him to decide to continue the project in the field.

Bill Adams (1927– ) grew up partly on the Navajo Reservation, where his mother was a senior education administrator with the Bureau of Indian Affairs at Window Rock and other reservation posts. He spoke Navajo fairly well and knew the reservation country and many of the Navajo people who lived there. He spent the summer of 1950 conducting an employment survey among Navajo people (Adams 2009: 76–81) in the northwestern part of the reservation, the Navajo Mountain–Tsegi Canyon area (Fig. 17.1). Adams gave a report on his survey at the 1950 Pecos Conference in Flagstaff. Taylor was there and heard the report (Woodbury 1993: 201; Adams 2003).[5] Here was an ideal field person who knew the Kayenta country and who spoke Navajo besides. Perhaps visions of undisturbed "cliff houses," like Kiet Siel and Betatakin, located somewhere in the wilds of the Navajo Mountain country, danced in Taylor's head. Adams would be the one to find them.

In early 1951, Taylor wrote Adams, offering him a summer job to survey "the canyons around Navajo Mountain, specifically in the area between Oljato (Moonlight) Wash on the east and Navajo Mountain on the west, and from the San Juan and Colorado Rivers on the north to the Tuba City–Kayenta road on the south" (Taylor 1958b: 3). According to Adams,

> [h]e offered me $500 for the summer's work, which seemed to me at the time a princely sum, since I was just exhausting the last of my G.I. bill benefits. But as it turned out, out of that sum I had to pay all the costs of the actual survey (i.e., running my truck and hiring Navajo guides, horses and a mule), and in the long run I did so much damage to my pickup that the survey cost me more than I made. . . . Although Taylor . . . talked about "coming over every two or three weeks to look over my work," in fact I had personal contact with him only twice, at the beginning and at the end of the project. At the beginning he came to my home at Window Rock to brief me about the project. He spent a couple of hours in my home, of which my principal recollection was his discourtesy toward my mother. She came home from work to prepare lunch for us (she was a senior BIA official), but he ignored her as though she were a waitress in a café. I think anyone who knew Taylor would confirm that he had a rather stiff and remote personality; whether it was snobbery or shyness or simply total self-absorption, I couldn't say. I won't go on to talk about the survey . . . [except to say that] Taylor wasn't at all involved in any of it. . . . [A]ll the sites I recorded were either shown to me by others (mainly Navajos) or I was told about them. I had the same advantage that John Wetherill always had: if you want to find ruins, just ask the Navajos. (Adams 2003)

Adams covered not only the major canyons and adjacent mesas around Navajo Mountain but also made a reconnaissance into Monument Valley and Chinle Wash to the east (Adams 1951; Taylor and Adams 1951: 1–71). At the end of the summer, he reported to Taylor that "there were two sites that might possibly serve the needs of the Pueblo Ecology Survey, Poncho House on the lower Chinle Wash, and PE 8 in Tsegi-ho-chon Canyon [a tributary of a tributary of Chinle Wash ]" (Adams 2003). He took Taylor on a three-day trip to evaluate both sites.

> We camped for the night below Poncho House (which in those days was very hard to reach), and my most vivid memory was that he had brought along a small barrel full of New England oysters and clams packed in ice. He told me he received a fresh shipment every three weeks, and that he never went camping without them. I suppose my Stoic soul was offended by that blatant display of Epicureanism, though I did enjoy eating the shellfish. Taylor concluded . . . that both PE 8 and Poncho House were too remote and difficult of access to be practically feasible for excavation. I had been thinking . . . about show-

ing him a couple of the sites in Navajo Canyon . . . but they were more remote still. . . . Thus ended my direct association with the Pueblo Ecology Survey. (Adams 2003; see also Adams 2009: 81–84)

In 1952, Taylor shifted his gaze to Monument Valley and adjacent areas both north and south thereof. He learned that a U.S. Geological Survey team, led by Irving Witkind, that was surveying and mapping uranium-bearing strata in the area, had recorded about 100 site locales and wanted some site identifications and dating. Taylor spent five days in the field with the geologists, at their invitation. During that time, Taylor visited forty-one sites, gave eighteen of them Museum of Northern Arizona sites numbers, collected sherds from seventeen of those eighteen, and noted types present on the rest. Three of the sites had been previously described by Kidder and Guernsey (1919).

Taylor (1958b: 7–14) devotes nearly half of his Pueblo Ecology report to a taxonomic discussion of pottery from the forty-one sites; no "conjunctive" data or interpretations are presented. In November 1952, Witkind and his colleagues submitted their own archaeological report to the Bureau of American Ethnology at the Smithsonian, listing ninety-six sites, including the eighteen assigned MNA numbers by Taylor (Witkind, Thaden, and Lough 1952).[6]

In 1953, Taylor submitted a two-page letter report to the Bureau of Indian Affairs office in Window Rock, summarizing Adams's 1951 survey. It closes as follows:

> As a result of this work, it is concluded that, except by the rarest chance, there are no remaining sites in the Navaho [*sic*] Mountain–Kayenta region which will fulfill the requirements of our culture-ecology project. . . . [T]he Thirteenth Century [sites] are now badly disturbed by vandals and at no time were large enough to provide the quantitative and stratigraphic data necessary to our problem. However, by combining the findings of this survey with his already considerable knowledge of the local archaeology, Adams has been able to derive some apparently sound and certainly most interesting hypotheses as to settlement patterns and other cultural aspects of the Kayenta people(s) who lived in the region in aboriginal times. These ideas are incorporated in his long and detailed final report which is being readied for publication at the present moment. (September 1953)

Adams's 125-manuscript-page report unfortunately was never published (see below). Taylor's (1958b: 4–7) brief summary of it provides the only published substantive site-distribution data for the project. Taylor's (1958b: 3) take on Adams's survey was that none of the sites "gave promise of fulfilling our special requirements."

In 1954, Taylor grasped at one last straw. His search "for our 'dream site'" led him to "make a quick trip to Mesa Verde National Park" (Taylor 1958b: 14). No dream sites there either. Taylor basically admits it was an act of desperation.

So much for "special requirements" and renewed hopes of demonstrating his conjunctive approach.

The second part of Taylor's Pueblo Ecology report records a six-day, 300-mile river trip through the Grand Canyon in 1953 aboard two twenty-one-foot Chris-Craft boats with inboard engines. The trip was organized and run by the Riggs brothers, Jim and Bob, owners of a Mexican Hat, Utah, river-running company (Webb 1994: 150–152), who pioneered the use of powerboats in the Grand Canyon. The passengers were several biologists from the University of Arizona, who apparently wanted an archaeologist to come along. Taylor recorded a total of six sites at or near scenery stops and camps along the river (Taylor 1958b: 18–29). The site descriptions and taxonomic data ultimately were useful to Taylor's longtime friend, the late Robert C. Euler, who would later direct systematic archaeological and environmental surveys in the Grand Canyon (e.g., Euler 1984).

In his summary and conclusions, Taylor indulges in a spate of backpedaling, excuse-making, and "what-might-have-beens." "Had there been no previous excavations of the largest cliff houses, had there been no pot hunting, or if in some corner we had been able to find, unexcavated and unpilfered, one or two ruins of even moderate size, I believe our problem would have had an answer" (Taylor 1958b: 14). He goes on to rail against the "tremendous and nauseating amount of vandalism perpetrated upon cliff houses and their contents." The vandalism seemingly precluded "hopes of working on specialized cultural problems . . . [leaving] merely taxonomic ones" (Taylor 1958b: 15). Despite his proclamation of intent to develop a study couched in cultural rather than taxonomic terms, the only substantive contributions of Taylor's report are the site-distribution data collected by Adams and a discussion of sherd taxonomy and cross-dating (Taylor 1958b: 7–14). Otherwise, there was nothing to report except failure. The "Hail and Farewell," as he titled the report, was probably done with deliberate irony. At least his sherd taxonomy contributed slightly to the database on Ancestral Puebloan ceramics that MNA obsessively compiled from 1930 until the early 1960s (Colton 1956).

### *Bill Adams and Taylor's Pueblo Ecology Study*

The real contribution of Taylor's Pueblo Ecology Study was Bill Adams's (1953) unpublished report. As noted previously, Adams partly grew up on the Navajo reservation and had traveled widely across it. From his early youth, he was interested in the archaeological ruins he saw scattered across the reservation. He read extant reports and made careful observations about the sites he saw. After service in the U.S. Navy, Adams entered the University of California, Berkeley; graduated in 1948; and started in the anthropology doctoral program there. But his love of the Navajo Country and participation in archaeology projects in the Southwest led him, in time, to shift to the University of Arizona. The 1950 summer employment survey project around Navajo Mountain, and subsequent work as a livestock drive foreman and Indian trader, ultimately led to his landmark dissertation on Navajo Indian traders, later published by the Bureau of American Ethnology (Adams 1963, 2009: 64–130).

Adams took the job with Taylor because, as he said, he needed the money. He was in the field from June 20 through September 18, accompanied some of the time by Robert Tallsalt, a Navajo friend who knew the country intimately. Adams kept a detailed field diary and an equally detailed set of site records (Adams 1951; Taylor and Adams 1951: 1–71). His 125-page manuscript report is titled "Archaeology and Culture History of the Navajo Country, Report on Reconnaissance for the Pueblo Ecology Study, 1951." He later added a subtitle, "*or* Kidder Up to Date: A Reintroduction to the Study of Southwestern Archaeology." The report is, quite simply, a tour de force, "prescient," as George Gumerman (personal communication, May, 3, 2003) calls it. Adams presents the site data he collected in detail and discusses the pottery types he observed and noted (he made no artifact collections). He then discusses Kayenta culture history, linking demographic and ecological perspectives. Kayentans, from Basketmaker II to Pueblo III times, were small-scale farmers, exploiting whatever arable land was available in any given year, living most of the time on small farmsteads. They came together, seasonally perhaps, for social and ritual purposes and to exchange genes and information. Only in Pueblo III times was it possible, briefly, to assemble in larger villages, some probably for defensive purposes.

One of Adams's key observations is that the Kayentans, and the Navajos who later occupied the same country, responded in basically the same way to the environment. In Adams's view, the traditional Navajo lifeway was centered around small-scale farmsteads. Given the ecology and the general environment, it was an optimal adaptation. Navajo herding was an overlay on this basic lifeway but did not supplant it or substantively change its character. Navajos came together for short periods for ritual and exchange purposes, as had the Kayentans. I cannot do full justice to Adams's argument here, but in my view it is, indeed, a study of Kayentan pueblo ecology, accomplishing what Taylor failed to do. It is most interesting that although Taylor had Adams's full report, he did not use it.

The second half of Adams's report, the "Kidder Up to Date" portion, is equally prescient. Adams rightfully blasts the anal-retentive obsession with pottery types that dominated Southwestern archaeology from before the 1927 Pecos Conference until the mid-1950s and beyond. The obsession reached a pinnacle in Harold Colton's (1956) *Potsherds*. Adams's basic message is that pottery has much more to tell the archaeologist than simply the sterility of time-space relationships, as indeed it has, for example, in the recent work of Patricia Crown and Barbara Mills (Crown 1994; Mills and Crown 1995). Finally, Adams has an extensive and reasoned discussion of the culture histories and interrelationships of the regional variants of the Anasazi, Kayenta, Mesa Verde, Chaco, and Little Colorado. In the fifty-plus years since Adams wrote, an enormous literature has been produced on these topics (see Cordell 1997 and Cordell and Fowler 2005 for succinct summaries).

Adams's knowledge of the Kayenta country and his understanding of the archaeology therein was useful to him and his colleague and spouse, Nettie Adams, in subsequent years (1955–1959) as MNA employees of the joint MNA–University of Utah Glen Canyon Archaeological Salvage Project (Adams and Adams 1959; Adams, Lindsay, and Turner 1961; Jennings 1966; Fowler 2010: chapter 19).[7]

### *Discussion of the Pueblo Ecology Study*

Two comments seem apropos. First, based on his published report, Taylor's Pueblo Ecology Study was a failure. His railing against vandalism and pilfering seems weak justification for what he did not accomplish. Most of Taylor's Southwestern critics took his rant as yet another excuse for, once again, not producing a "conjunctive" study.

Second, in retrospect, it can be argued that Taylor's report was one of a few (e.g., Linton 1944; Woodbury 1956, 1959) in the 1940s and 1950s to discuss evidence for inter-Puebloan warfare prior to 1200–1300 CE in the Kayenta region and the Southwest generally. The prevalent view was the "hostile nomads" theory, hoary in speculation about Southwestern sites since at least 1846 and given credibility by Kidder (1924: 335–336) in his *Introduction to the Study of Southwestern Archaeology* as one factor (of then undetermined magnitude) contributing to the abandonment of late Pueblo III sites throughout Anasazi country. In Kidder's and others' scenarios, the Anasazi themselves were peaceful folk, from Basketmaker II times until the Spanish Entrada, living in the kind of harmony invented for them by many nineteenth-century writers and twentieth-century anthropologists, especially Ruth Benedict (1934: 57–129; see Fowler 2000: 321–365).

Linton, Woodbury, and Taylor thought otherwise. Taylor noted various Kayenta sites that seemed to have been built for "defensive" purposes, especially several "cliff house" sites that are relatively inaccessible. Some of these, he thought, had superimposed deposits of Pueblo I through Pueblo III ages, ca. 700 to 1300 CE. Given that sites dating earlier than Pueblo III were found in places difficult to access and easily defensible, Taylor (1958b: 16) postulated that "such warfare as did exist . . . [was] internecine, sporadic [and] desultory," occurring recurrently in times of stress and scarcity of resources, a view similar to Woodbury's. This is very much in line with recent findings of recurrent, widespread violence and small-scale raiding/warfare, from at least 500 CE until historic times all across the Southwest (Haas and Creamer 1993; Wilcox and

Haas 1994; Blackburn and Williamson 1997; LeBlanc 1999). The point here is that Taylor's view of recurrent, inter-Puebloan warfare was a minority view in Southwestern archaeology from the 1920s well into the 1980s but increasingly has been seen as highly plausible since then.

## TAYLOR'S HISTORY OF SOUTHWESTERN ARCHAEOLOGY

Taylor's (1954) history of Southwestern archaeology, written for a summary conference on Southwestern anthropology (Haury 1954), is a brief treatise on the sociology of knowledge and knowledge-making. He divides Southwestern archaeological research into two periods, a collecting-for-museums phase before Kidder, Kroeber, and Nelson and a "time-space" phase after them and continuing to 1954. Both phases he sees as tempered by an "expeditionary attitude." That is, most of the important research until the 1930s was performed by Eastern-establishment archaeologists who came annually to the Southwest to collect specimens and data in support of their interests, basically "filling in gaps" in the time-space record. Even when local institutions took up the work, they reflected the same attitudes and purposes. Taylor (1954: 570) summed it all up:

> Without injustice, it can be said that the Southwest has progressed very little in theory and method since 1930, by which time basic propositions had been formulated. Within this present and long-standing structure, there are signs of stagnation and even the seeds of eventual recession. The time is past when new facts are their own justification. There must be comparable advances in their application to current and continually revised problems.

Paul S. Martin in his comments agreed with Taylor's position, both in the paper and in *A Study of Archeology*: "Some of us may say, as I did, 'So what! This is much ado about nothing.' But after ruminating on his point of view, I think it worthwhile to revise our approaches. It is very healthy to have among us a gadfly who will sting us into action. We may become irritated but our irritation may well spring from the fact that in our hearts we know Taylor is right" (Martin 1954: 570). Others were not amused and did not agree, as Taylor (1988) himself and Euler (1997) and Reyman (1999) tell us. Indeed, Martin (1954: 571) opined that Taylor's ideas "would have been far more hospitably received and widely accepted if he had first put out an archaeological report embodying his ideas," adding the zinger, "I still await with pleasure Taylor's publication on his archaeological work in Mexico." It would become the party line among Taylor's many critics.

Taylor's Pueblo Ecology Study did nothing to further the cause of his conjunctive approach. I recall hearing at various Pecos Conferences, from 1959 on, then senior (some now deceased) Southwestern archaeologists speak disparagingly of the study as a "boondoggle" and further evidence of Taylor's "failure." Certainly, the number of citations of the paper in post-1958 Southwestern archaeological literature is minuscule compared to citations of *A Study of Archeology*.

## TAYLOR'S GENETIC MODEL

Taylor (1961: 71) begins his article "Archaeology and Language in Western North America" by stating that "[t]he linking of archaeological assemblages and linguistic groupings is a risky business." Indeed! Attempts to build "genetic models"—that is, to link artifact assemblages, linguistic groupings, and (sometimes) human populations (however defined, somatically, phenotypically or genotypically)—have a long genealogy in anthropology.

In the Southwest, various genetic models, based on greater or lesser amounts of valid data, were proposed from the 1840s to the early 1950s (Fowler 2000: 50–70, 148–160, 313–320). In the 1950s, the advent of radiometric dating gave American archaeologists firm "absolute" chronologies of up to ten millennia. The delineation of various "Desert Cultures" in the Southwest and Great Basin allowed, for the first time, an adequate understanding of the nature and time depth of the hypothetical, preagricultural "Basketmaker I" of the Pecos Classification (Kidder 1927). The concurrent development of lexicostatistics and glottochronology in American linguistics (Foster 1996: 64–65) made attempts at genetic modeling within some sort of "absolute" time frame very attractive. Various models attempting to link some or all of the known language groupings with several artifact assemblages and (sometimes) extant osteological data in western North America were advanced by, for example, Romney (1957), Hopkins (1965), and Taylor:

> The following hypothesis is proposed: that the distribution in time and space of the Desert culture(s) and Hokaltekan languages imply a connection, that at one time there was a continuous band of Hokaltekan people practicing Desert culture from the Great Basin to the Texas and Tamaulipecan coasts, that this continuity was disrupted by an incursion of Utaztecan highlanders moving along the cordillera into Mexico, that proto-Shoshoneans entered the Great Basin from the northeast, that the Yuman people are a remnant block and not a disrupting wedge of Mexican origin, and that some of the similarities between cultures in California, the American Southwest, and Mexico are survivals of the basic proto-Utaztecan or macro-Penutian culture, while still others are the result of interchange through a relatively homogeneous cultural medium made up of Utaztecan highlanders living along the western cordillera from central Mexico to the American Southwest. (Taylor 1961: 71)

Other models proposed other linkages. With the advent of processual approaches in the 1960s, genetic modeling fell from favor in the Southwest and elsewhere. However, such modeling is once again fashionable (Gregory and Wilcox 2008; Webster and McBrinn 2008).

## SOUTHWESTERN INFLUENCES ON TAYLOR'S (AND OTHERS') THEORIES

Taylor began doing archaeological work with Lyndon Lane Hargrave (1896–1978), of MNA, in 1935 and spent time with him in the late 1930s while he, Taylor, was teaching in Flagstaff (see above). Hargrave was a self-taught but highly accomplished ornithologist, ecologist, archaeologist, and extraordinary one-on-one teacher (Taylor and Euler 1980). In the acknowledgments for *A Study of Archeology*, Taylor (1948: 9–10) wrote, "Lyndon Hargrave probably started the whole thing by the stimulation of his free-flowing ideas on the archeology of northern Arizona; this volume would have profited had I taken notes during some of those long winter evenings in Flagstaff."

Just so, Hargrave's influence on Taylor's work was twofold. First, he was very familiar with the Kayenta region (Hargrave 1934a, 1934b, 1935a). He also formulated the research design for the 1933–1938 Rainbow Bridge–Monument Valley Expedition, which focused on the Tsegi Canyon / Navajo Mountain / Glen Canyon sections of the Kayenta country (Hargrave 1935b; Beals, Brainerd, and Smith 1945). His knowledge of, and enthusiasm for, Kayenta archaeology stimulated Taylor to focus on it for his Pueblo Ecology Study. Also, Hargrave's "free-flowing," "holistic environmental philosophy" deeply influenced Taylor's thinking about a "conjunctive approach" in archaeology (Taylor and Euler 1980), as well as his research plan for the Pueblo Ecology Study.

In 1931, Hargrave (1931: 1–4) proposed that a full understanding of changes in "economic geography," that is, cultural-environmental relationships, was required to understand the "rise and fall of Pueblo culture." For Hargrave, "Pueblo culture" meant the "Anasazi" (Kidder 1936) and adjacent archaeological cultures on the Colorado Plateau from ca. 500 to 1300 CE. He later substituted "human ecology" for "economic geography." "A necessary consideration in the study of the human ecology of a region is the exact location of the site on a map. The relation of the site to sources of native materials; the effect of local climatic conditions; and animal and plant associations cannot be accurately studied unless the true position of the site can be determined" (Hargrave 1935b: 20). All such information, said Hargrave, needs be considered in relation to the artifactual and floral and faunal remains recovered in site excavations. Those data, and relevant ethnographic analogies, make possible a fuller understanding of the changing "human ecology" of a region. In this framework, culture is seen as a systemic adaptation to environment, not a conglomeration of traits arranged in time-space matrices. Elements of a "conjunctive approach," by any other name!

Hargrave also influenced the thinking of Julian H. Steward. In the summer of 1934, Steward spent some time with Hargrave at MNA, and together they visited archaeological sites in the southern Kayenta region. They discussed Hargrave's "human ecology" approach at length (Kerns 2003: 156–157). Steward put Hargrave's ideas together with his own, derived from his extensive experience in the western Great Basin in the 1920s (Kerns 2003: passim). In late 1934, after visiting Hargrave, Steward wrote in a grant proposal, "Shoshonean tribes could only be very incompletely understood if one failed to recognize that the stark facts of human ecology in a most difficult environment [the Great Basin] are stamped upon them" (cited by Kerns 2003: 157, 356n21). He subsequently used "human ecology" in his classic study of Great Basin aboriginal sociopolitical groups (Steward 1938; see also Steward 1937). As Steward's (1977) theoretical approach matured, he substituted "cultural ecology" for "human ecology," in part because the latter term had taken on other connotations in American sociology (Kerns 2003: 157–162).

The point here is that Hargrave's ideas were seminal influences on the theoretical and methodological thinking of both Taylor and Steward and, through their works, on American archaeology and anthropology generally. A more thorough discussion of the Hargrave-Taylor-Steward triad would be a useful contribution to the histories of method and theory in American anthropology.

## CONCLUSION

Walter Taylor's contributions to Southwestern archaeology were mixed. The Pueblo Ecology Study was designed to validate and demonstrate his conjunctive approach. It failed. Taylor did not acknowledge that Bill Adams's unpublished report contained important insights into Pueblo cultural-demographic-ecological relationships. Taylor's history of Southwestern archaeology paper, like his *A Study of Archeology*, was hortatory: eschew time-space matrices; go ye out and do conjunctive research! But the response of his Southwestern colleagues was basically, Why should we do as you say, since, seemingly, you have not, or cannot? In the end, the true hero of the piece is Lyndon Lane Hargrave, who stimulated both Taylor and Steward to think along lines that proved highly useful and productive in American anthropology. Taylor's greatest contribution to Southwestern, and American, archaeology may have been his service as a transmitting agent for Hargrave's ideas.

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## NOTES

1. “Anasazi,” a Navajo word loosely translating as “enemy ancestor,” is regarded by some contemporary Pueblo people as derogatory. “Ancestral Puebloan” has become a generic substitute. I retain Anasazi herein because it was in general use during the period of concern, the late 1940s and 1950s.

2. An undated and unsigned memo to Alexander Wetmore, then secretary of the Smithsonian Institution, but apparently written in 1951, forwards a personal check for $10,000 to be placed in the fund (Taylor [?] to Wetmore, n.d. [1951?] TP/NAA). George Gumerman (personal communication, May 3, 2003) thinks that the 1945, 1949, and 1951 monies were Taylor’s. In later years, at Southern Illinois University, Taylor established such research funds, using his own money.

3. It should be noted that in 1949, palynology was as yet little used in the Southwest, and pack-rat midden studies were unknown. Both of course have since become major indicators of climatic and ecological change (Nash and Dean 2005).

4. One of the sites was Turkey Cave, which had been tested by a crew from Harold Gladwin’s Gila Pueblo in the 1930s. Taylor’s inquiry to Gladwin concerning the pottery collection from the site led to an exchange of several letters about the putative Great Drought and Taylor’s research project (Taylor to Gladwin, Gladwin to Taylor, various dates, 1949–1950, TP/NAA).

5. There is a photograph of Taylor chatting quite amiably with A. V. Kidder and Emil Haury at the 1950 Flagstaff conference (Woodbury 1993: fig. 6.14; reproduced in this volume).

6. Taylor’s experience with the USGS team led him to suggest a joint program of geological and archaeological surveys, funded by the USGS and the National Park Service, in the Southwest and the Great Basin. There is much correspondence relating to the proposal in Taylor’s papers (TP/NAA) involving Erik Reed and Jesse Nusbaum, the principal National Park Service archaeologists at the time; Jesse D. Jennings, at the University of Utah; Arnold Withers, at the University of Denver; H. Marie Wormington, at the Denver Museum of Natural History; and others. In the end, little came of the idea.

7. Bill Adams left MNA in 1959. Soon after, he and Nettie became involved in major—four decades and counting (as of 2010)—salvage archaeology programs in the Sudan and Egypt; see, for example, his magisterial *Nubia: Corridor to Africa* (Adams 1984). In 1964 he joined the Anthropology Department at the University of Kentucky and remained there until his formal retirement in 1992 (Adams 2009:131–258). Adams (1998, 2004) has also published major works on the philosophical roots of anthropology and comparative religion.

CHAPTER EIGHTEEN

# WALTER TAYLOR AND THE PRODUCTION OF ANGER IN AMERICAN ARCHAEOLOGY

*Mark P. Leone*

The principal goal of this chapter is to build a model to explain the anger directed at Walter Taylor and to consider what this anger means for the field of American archaeology, its history, and its future, and the degree to which the field can or will accept and benefit from cogent internal critiques of practice and theory. To do this, I will put aside temporarily any unique traits associated with Taylor's work, except for the widespread anger and long-standing reactions to his famous book, *A Study of Archeology* (1948). I take that anger as a starting point. It is best to see that from time to time in archaeology there are outbursts of anger over professional matters. These may not be predictable but rather occur and last for several years, are utilized to highlight intellectual issues, and become familiar to archaeological graduate students because the anger produces written debates that are employed in the classroom. If we recognize that the outbursts of professional anger are periodic, we see that the sources of anger are not the authors but ourselves. Ordinary archaeologists are the angry ones; the debaters are merely the spokesmen and spokeswomen for our own discontent. They are speaking for us. I offer this as the central idea of this chapter.

The anger stems from the discrepancy that most practicing professionals are likely to see between the goals of archaeology, learned as students versed in these debates, and the daily reality of archaeological practice. Ordinarily, most of us live with the striking difference between our hope of discovering something that is newsworthy and the mundane nature of the archaeological work we actually do. The reality is that our work typically consists of recovering little broken things, in locales that may or may not represent an original pattern, and then waiting a long time before analyzing these things to discover something that often may not be all that important. I propose that the frustration associated with this discrepancy between the ideal and the reality lies dormant most of the time but that it is always there and produces some tension which can be played on.

The explanation for the hypothesis of this paper derives from the implications of the routine of daily archaeological life. The routine comes from the care of the parks, monuments, historic villages, historic districts of old and valued cities, ancient waterfronts, known archaeological zones, and similarly protected areas and established collections. This is where much archaeology is practiced in many countries. It is often not exciting archaeology, in part because it involves laboring where the primary discoveries have already been made. Thomas Patterson (1986, 2003), Benedict Anderson (1996), and Michael Shanks and Christopher Tilley (1987) have pointed out that these areas and their archaeology help to establish and maintain the political and economic status quo in society and in archaeology. If we can consider this, then no discoveries would be welcome that might change the status of the monument or of the descendant people whose identity is shaped by that monument. From this perspective, we might be able to examine the occasional bursts of outrage in our field as stemming from being caught between thinking we can discover changing cultures and novel histories and the reality that the field acts to preserve an established social order in which we as individuals are minor players. Therefore, I argue here that because some of archaeological practice may have a socially conservative result and function, there is a gap between practice and the ideals and goals we are taught through our training in anthropology, such as cultural relativism, motivation against racism, and the pursuit of histories for those denied histories. This gap between what we achieve and our ideals produces critique and periodic bursts of anger.

## OTHERS LIKE WALTER TAYLOR

Walter Taylor wrote a long and detailed description of the discrepancy between what prominent American archaeologists said we should be doing and the work they actually did. Taylor exposed the tension between the goals laid out for archaeology and the actual results produced by these leading figures (and many others who shared their ideals). The book was considered improper. Taylor's analytical technique produced discomfort, hurt, and anger. Today, how do we get some good out of this?

We archaeologists should choose to see that our professional relationship to Walter Taylor is not unique. In the decades after Taylor's book, many members of our field became angry with others who also pointed out the distance between the goals we acknowledge and the work we publish. Many colleagues were angered by authors who chose to build a presentation of archaeology by explicitly noting the gap between how we would like to understand the past and the interpretations and artifact analyses we provide.

In order to flesh out my central thesis, I cite other outbursts over goals and achievements in archaeology. The most obvious one, arriving right after that provoked by *A Study of Archeology*, was the larger and more public struggle produced by the establishment of the New Archaeology. In the mid-1960s and early 1970s, Lewis Binford and his students Arthur Saxe, James Hill, William Longacre, John Fritz, and others associated with the University of Chicago not only found traditional American prehistoric archaeology wanting, the way Walter Taylor had, but fought loudly to claim a place for a more effective way of achieving anthropological goals, like comparative histories (Binford and Binford 1968; Binford 1972). Because my own professors (Emil Haury and Richard Woodbury) were among the senior generation attacked by Taylor, as well as among those who came to the defense of the attacked, I witnessed the quarrels and the public displays at meetings and heard the private conversations and assessments. However, in the late 1960s and early 1970s the field was much larger than it had been during Taylor's youth. Although the New Archaeology debate grew to a much larger scale, it was fundamentally about the same issue raised by Taylor, that is, how to deal with the gap between goals and achievements (see also Leone 1972a).

This fight in archaeology quieted down by the mid-1970s, but in areas like the U.S. Southwest there was still much turmoil over why and how to perform archaeology. One of the most vocal advocates for the New Archeology there was the late Fred Plog. I cite him because the New Archaeology was still finding a home by proclaiming its abilities to do a more competent job. This was clearly seen in the Southwest and produced debates that continued the tenor associated with the initiation of the New Archaeology (Watson, LeBlanc, and Redman 1971: 26, 52, 102).

Nonetheless, my goal is not to produce a chronology of disputes. Rather, it is to suggest that there are spurts of acute annoyance within archaeology over the appropriateness of achievements and that we distance ourselves from those who articulate the inappropriateness. We might better see them as reflecting our own opinions not so clearly held. We should see them as ourselves, I argue. The bursts of annoyance are our own and would not be so outrageous if we could see that; in fact, they would not even be noticeable if we were not so attracted to them. Simply put, they would hardly produce anger if we could see ourselves in them.

By the late 1970s, the New Archaeology, by then well-established in North American archaeology, came under fierce and long-sustained attack by a group of young archaeologists at Cambridge. They were led by Ian Hodder (1982) and included Christopher Tilley, Daniel Miller, Michael Parker Pearson, and colleagues from Denmark and Sweden. The ensuing debate about appropriate methods of interpretation for archaeology is of the same dimensions as Taylor's and Binford's and, if anything, is more profound, has lasted longer, and has even challenged the legitimacy of the field's very existence. These Cambridge postmodernists soundly criticized Binford and did so in a way that seemed like earlier battles. However, the result has produced a much more long-standing controversy, going on now for more than twenty-five years. This protracted response involves the field more completely than did the response to Taylor's work. The point is, and I restate my thesis, there are moments of anger that erupt in our field and these stem from the gap between goals and achievements.

I invite us to see that the articulators of the gap's existence speak for us. These are not discontent loners in the field; nor the impossibly bright, talented, and disturbed; nor the politically unwise; nor even the shameless self-promoters. They are, or could be productively seen as, reflectors of ourselves and of our tacit recognition of the reality these more verbal and articulate among us describe. In this way, our anger toward Walter Taylor, Lewis Binford, and Ian Hodder—or whomever—is just a projection of our own discontent. If we can see it as our own, then we can own it and not only ask where it comes from but also how to experience it and relate to it more productively. Once we do this, the onus is lifted from Taylor, Binford, and Hodder, and we can confront ourselves and the goals we derive from our discipline and our society. Although Fred Plog, Chris Tilley, Jim Hill, Bill Longacre, and others were able to handle the anger they appear to have caused, they certainly did not deserve it. Neither did Taylor, Binford, and Hodder.

Every time the discrepancy between goals and production is ascribed to individual archaeologists, the anger is predictable. It is predictable partly because of the rhetoric, and partly because this mode of description highlights what we have all been taught: that we can do better than dig up stuff. We are supposed to draw conclusions about human origins, cultural origins, cultural processes, and ancient life. However, very few archaeologists can actually operate at such levels. Most are good scientists but rarely do they contribute to knowledge that receives significant notice from incoming graduate students or the introductory texts of our field. We all endeavor to make general, long-standing contributions, but most of us usually cannot and so produce work that looks unexceptional and is quickly swallowed up in the ongoing and voluminous process of our own society's discourse and thinking. We live with the discrepancy between our goals as we absorb them in graduate school, concerning the original and still fairly radical aims of anthropology, and our written and published work, which often must be done using standards that mandate limited conclusions.

When someone like Walter Taylor, Lewis Binford, or Ian Hodder cogently points out these discrepancies, he gets a large audience and a substantial reaction. Taylor, Binford, and Hodder highlighted the differences between our goals and our work and addressed an inconsistency many of us are aware of, but one we would rather not confront or have to live with. Taylor's portrait of us, for example, would never have been read and would not be important still if we were not already somewhat aware of the problem: we would not be wounded by Taylor if we were not ready to be. His target was there, waiting.

As archaeologists, we are to provide spectacular discoveries, precise dates, clear views of dead societies, explanations better than Genesis, explanations for why society changes, why humans change, insights into the future based on seeing the deep past, breaking news as well as news of virtually Biblical importance. In general, these expectations are unrealistic, no matter how important the questions are. But we all share the hope that we can deliver good explanations. Thus, one of us periodically produces an effort to create a theory and method that will close the gap, such as Walter Taylor did in proposing the conjunctive approach in Chapter 6 of his 1948 book. The announcement of this is preceded or accompanied by a description of the gap (e.g., the famous critique in Chapter 3 of Taylor's book). The degree of our anger depends on how brazen we see the analysis or indictment. We usually attack as if attacked. However, our anger is at our own failure to deliver what we think we should have achieved. Ostracizing these members of our profession is merely a form of despising ourselves, regardless of the generation attacked or attacking. Taylor, Binford, and Hodder were the mirrors and we merely held one up to ourselves and then chose to smash the reflection.

## POLITICS AND THE USE OF ARCHAEOLOGY

Why should there be a gap between goals and results in archaeology? The explanation is that, besides its often scientific or anthropological side, archaeology's work is fundamentally political. Archaeology's product is the duplication and protection of the status quo in the societies that use it. This is now a well-known point (Friedman 1992; Schrire 1995; Anderson 1996; Castañeda 1996) and I introduce it here because using it helps us explore why, even when theories change and the goals do not, the gap in expectations persists.

In the last fifteen years, a number of archaeologists have produced a new kind of history of the field. Thomas Patterson (1986); Joan Gero, David Lacy, and Michael Blakey (1983); many authors connected to the Third World Archaeological Congress publications (e.g., Layton 1988, 1989; Miller, Rowlands, and Tilley 1989; Shennan 1989); Shanks and Tilley (1987); and others have shown that the past, when presented by our field, is often a mirror of the status quo in the present. The effect of such archaeological production is a message that the past looks like the present, and that because the present comes from the past, current conditions must be inevitable. If this is true, then we can conclude that we have an unreflective field and one whose productions can sometimes be seen as merely reactionary.

The implication of this position is that we produce little new knowledge because we have no tools for escaping the class-supporting position we are in. If we see the bulk of archaeology being done by new nations like Mexico and Israel, Kenya and South Africa, if we see much of Canadian and U.S. archaeology done on sites associated with emerging patriotic consciousness, and if we see the claims made by Native Americans and indigenous peoples as saying they can and will control the archaeological process for their own good and have it done better for themselves, then we can also see that much archaeology is not what we are taught as professionals. Archaeology is not about agricultural or human origins; it is far more frequently used to glorify the Near East as the center of modern urban life and Kenya, Ethiopia, and South Africa—indeed, Africa as a whole—as humanity's birthplace. These are important political ideas whose validity stems from our initial work. However, we often are credited with the facts, not with the political force of the interpretations.

Benedict Anderson (1996) argues that nationalist positions are also accompanied by class positions. In the face of his argument, we archaeologists may benefit by reminding ourselves of how Native Americans, African Americans, Chinese Americans, and immigrants have loudly noted the absence of their heritage from historic houses, historic frontier forts, Colonial Williamsburg, Independence Hall, historic Annapolis, and the great museums that feature moveable (versus architectural) archaeological remains. One of the central definitions we use now for historical archaeology is giving voice to the voiceless, forgotten, and deliberately silenced. The new South Africa, for example, contains museums dedicated to exhibiting African cultures and experiences that were often invisible or denied under decades of apartheid. Remembering the calls for openness and inclusion of the last twenty years, it is not difficult to examine the classist uses of some archaeological materials. We can ask ourselves whether we are able to achieve these goals, and if not, why.

## CASTAÑEDA AND THE CREATION OF INFERIORITY

The problem of knowledge production is even deeper. Quetzil Castañeda in his book *In the Museum of Maya Culture* (1996) is concerned with the creation of the gap between the modern Maya and the Classic Maya. Castañeda addresses the devastating impact on a people when their past is manipulated or misrepresented. The power derived from understanding literature like Castañeda's or Jonathan Friedman's earlier work (Friedman 1992) helps us focus on why discussion of archaeology's flaws can become so volatile.

To begin, Castañeda deals directly with the Carnegie Institution's archaeologists, who were among the people Taylor criticized in 1948. Castañeda makes two related points about the archaeologists Taylor described. The archaeologists supported by the Carnegie Institution of Washington saw the modern Maya as dimly related to the ancient Maya and thus as having lost the glory and greatness they once had. Castañeda rephrases the implications of early work, showing that in having something left that we can see, they must have lost a great deal that can only be inferred. So there is a huge, disappointing, and fundamentally mysterious gap. In those cases where the Maya were no longer traditional but were still present, there was little left of their past culture and not much new either, since they had not chosen adequate "progress." These people had what Castañeda, citing Robert Redfield, calls zero-degree culture (Castañeda 1996: 23–28, 44–45, 57–62). They were neither Maya nor Mexican. They had nothing that an anthropologist like Redfield could describe or value. They were the nobodies Mexican society was making them into. Castañeda opines that North American archaeology and anthropology facilitated the process.

I use Castañeda's ideas here to show what may be behind the gap between archaeological goals and published reality. Within the United States the social environment in which archaeology existed and exists is embedded within a political matrix. This environment sometimes is unreflective, may abet disfranchisement of native peoples from their remains and lands, and has little theory to deal effectively with the dynamics of class. Although prehistorians deal effectively with state societies and Kohl (1975, 1978) discusses colonialist policies in the Near East, few deal with how the poor are impoverished and kept that way. Historical archaeology routinely deals with women's housing, immigrants in tenements, asylums, slums, and slavery. But it does not deal with why these circumstances exist, their results, or the stratified wealth that these conditions supported (Funari, Hall, and Jones 1999; Smith 2004). I emphasize this absence here—that is, the absence of a substantial literature analyzing social distances based on wealth and power—so that I can juxtapose this with the gap noted by Walter Taylor.

To conclude this part of my discussion, I suggest that it is politics that creates gaps between goals and findings within the field. It is not only the absence of adequate theory—although this did exacerbate the case in Taylor's day—as much as our field's fulfillment of a social function in all of the modern and newly national societies that use it. We have become aware of this function only in the last generation. The gap is less significant now than it used to be because of the greater maturity of a generation of prehistoric archaeologists who come from parts of society where social action is a part of one's upbringing. For the most part, however, this element of maturity has been slow to arrive in historical archaeology.

## SUMMARY

If archaeologists have produced Walter Taylor, Lewis Binford, Ian Hodder, and others who have similar things to say, and if our reaction to them is anger, then what can we conclude? First, we might be able to say that we know that the object of the critique comes from within our own profession, from the gap between our disciplinary ideals and the political reality of our work. Second, this gap between goals and achievements is not mainly the result of inadequate theory, anthropological or otherwise. The gap exists because much of archaeology's social function is not about education, illumination, or the advancement of science. It is about noticeable but usually disregarded actions and models that protect elites. Archaeologists see these and sometimes deplore them, but most feel they are ill-equipped to deal with them or to change them. Only recently, for example, have we been actively engaged with museums. Most museums had a hostile reaction to NAGPRA and failed to see the exploitation that necessitated new laws to protect Native Americans. Moreover, we do not have a professional understanding of how the media use archaeology to influence our own society.

## WHAT IS TO BE DONE?

If this argument is useful, what should come next as a point of analysis and resolution? We might consider studying the problems that make us angry. Some of us may already do this, but we could be more conscious of it. As Rosemary Joyce shows us in this volume, Walter Taylor (1941) did an early structuralist analysis of elements of Maya iconography because he asserted that chronology was not the only way to understand Maya culture. Lewis Binford redirected our study of hunter-gatherers because of substantial misunderstanding in the field concerning the analogies that were being made between the Paleolithic and modern hunter-gatherers. James Deetz (1965) and William Longacre (1968) were both tired of ceramics used only for chronology and redirected our use of them to understanding social life.

These examples can be made into a paradigm, or a set of rules, to watch or use personally as a professional. First, I argue that we all lead scholarly lives based on making interventions in a preexisting dialogue over something that concerns us. This means that virtually all archaeologists work on some problem, some area, or some people on which work already exists and so we are already embedded in it and are intellectually and emotionally familiar with a problem, area, or issue in some way. In my case, as I worked in Annapolis, I became involved in an issue when something moved me deeply. I then turned that reaction into an intervention after thinking through how my reaction could be framed as an intellectual problem. This naturally requires one to see existing circumstances as involving a preexisting intellectual error. Seeing how to fix a mistake, or correcting an error, is a recognized part of scientific method and is not an innovation on my part.

By the time I began working in Annapolis, I already was annoyed and then puzzled about why so little American history was actually taught at Colonial Williamsburg. Then I turned my feelings of annoyance, disappointment, and even discouragement into a problem, which was the creation of historical consciousness among classes (using Althusser and Lukacs), and tried to solve the problem in Annapolis by using these Marxist theorists and creating a public program.

In Annapolis itself, I was attracted by the beauty of its eighteenth-century planned landscapes and deeply disturbed by how historians ignored them and at the inability of historical archaeologists to study them. Like historians, historical archaeologists ignored formal gardens. So I had to make a problem based on my initial irritation and even earlier sense of how beautiful these spaces were. To articulate a problem, I built a foil out of the description of formal gardens as maps. I was told that gardens were like flat maps. They were also described as baroque and like the street plan of the city itself. Both were part of baroque design. I called the use of these terms errors in order to make a problem.

Finally, I did not want the problems to be specific to Williamsburg or Annapolis. I wanted a general problem to work on so that historical archaeology would or could be scientific, in the sense of dealing with general problems, not only with local circumstances. Ultimately, how to teach American history and see urban planning had to be tied, for me as a scientist, to the operation of capitalism, both in the eighteenth century and today.

If an archaeologist's intellectual autobiography can be couched as interventions in a dialogue based on the scientist's reaction to some event, then Walter Taylor's work is usually characterized backward. He spent an enormous initial effort expressing the reason for his intervention, and the actual results received less attention from him and us. As in the Maya article discussed by Joyce, his actual scientific work was ignored or marginalized. Nevertheless, the reasons he did it are there for us to build on as a model. For most of us the reverse is true. We almost never comment on the personal reactions to the world that lead to our interventions in debates. Instead, we spend most of our scientific lives trying to deal with empirical problems, not recognizing that these typically arise from a personal reaction.

I am not encouraging that we use Taylor's life as a model or that we express our anger in scientific contexts. I am suggesting that our recognition of the gap between our goals and our work is brought to the fore by critics in the field and that when we get angry at the criticism, we use our reaction to guide our own work. We look to ourselves for guidance, not to Taylor, Binford, or Hodder. These scholars, and others, are useful as theorists after we have decided why a problem is attractive to us in the first place. If a person can recognize why he or she is taking something on—disgust, awe, suffering, forbearance, endurance, love—there will follow a political or economic component. Although these sparks are emotional and personal, when they connect us to something we attempt to understand archaeologically, we are both satisfied and connected to a problem we see as worthwhile. Because we start with ourselves, we start with modern life and almost as inevitably with something political or economic or about justice, the functioning of our own society or of some other valued people or society. These ties get us to the heart of the social function of modern archaeology and may help to close the gap between archaeological goals and our own practice. The place of theory then is to connect data to generalizing variables or ideas. The place of knowing your reaction to something is to let you see why you want to examine an issue or people in the first place.

Since the emotional reaction to the gap between archaeological expectations and archaeological performance is our own, I argue that we might be able to short-circuit the need to ostracize others like Walter Taylor. We can recognize ourselves as the source of criticisms and develop our own third ear and listen to what provokes us. Through this we can develop a way to refocus our immediate work.

When I was teaching at the University of Cape Town in 1988, for example, I was appalled by what I saw of apartheid in South Africa and realized I could make no difference in teaching historical archaeology there. At that moment, I made a commitment to go back to Annapolis to work with people there of African descent. I went to African Americans in Annapolis and have worked with their archaeology since then and we have made a difference. Initially, I had no reason to work with people of African descent in Annapolis even though I began work in Annapolis in 1981. I was not even sure there was African American archaeology or how to approach community leaders to see whether there was interest. I did not know where to excavate, with whom, or why. In South Africa, I was a stranger, knew it, and said so. When I saw the police violence and felt it directed at me, and when I saw the actual conditions of the townships, which were virtually indescribable, I was so shocked that I could not speak. I was stunned into silence. I was both furious and inspired; I determined that I had to begin work with Americans of African descent in Annapolis. Rage led to action.

As a further illustration, I owe a deep personal debt to Walter Taylor, who is responsible for getting Southern Illinois University Press to publish my first book *Contemporary Archaeology* (1972a). So my motivation here is to honor him. I never could understand why people were so offended by his work; I found it inspirational when I read it as a twenty-four-year-old graduate student. And so out of gratitude to Taylor, I use the opportunity provided by this volume to take the anger directed at him, turn it around, and ask us to own these reactions productively by making a model out of his circumstances. Because his generation is largely gone and the reaction to Taylor very diminished, however, I have tried to include in the model other bursts of anger at other critics in and of archaeology, so that we see the episodes as a process of call and response of which Taylor's is but one example.

This chapter was initially motivated by my affection for Walter Taylor and by my not being able to understand the anger directed at him. I tried to be scientific and take these personal but otherwise professionally unproductive reactions and make a useful model for them. I do not think there is anything special about this; we do it all the time. I think we would be more productive, however, if we knew the process consciously. It would also allow us to be kinder to the profession's critics.

## POSTCOLONIAL THEORY

Once we notice our reactions and motivations, we can develop our archaeology by employing postcolonial theory and the example of hybridity. On the surface, my discussion of postcolonial theory will seem like a narrow example for and of historical archaeology and even for my own work, but it is more than that.

A postcolonial critique of archaeology has been offered by Benedict Anderson in *Imagined Communities* (1996: 180–185). He points out that archaeology was used sometimes during the colonial era to produce monuments of past greatness that spoke to emerging colonial elites. Archaeological sites of vast proportions and majesty, of impeccable landscaping and with no local communities in evidence, were sponsored by the colonizing power to ground an emerging colonial class with an invented memory of its once great importance.

Monuments discovered through archaeology provided a place, an image, a stage, and a moment in the past for the emergence of a nation, now newly out of a colonial experience. But the archaeology was started by the colonial power as a way to mark not so much a spatial boundary but a temporal one, and one that appeared to be genealogical. Building upon Anderson, Castañeda (1996) has pointed out that the connection was built upon the double fact that the current natives did indeed have a past, and a brilliant one, but that it was gone and all but forgotten. Indeed, it could only be rediscovered because there was some, but only a little, similarity left.

Anderson's originality on this point was to add the idea of virtually infinite duplicability to the monuments. Photographs, reports, textbooks, many smaller ruins, maps, the names of hotels, trails, houses, cookbooks, dishes, and the wide variety of curios send images of Mesa Verde, Chaco Canyon, Chichén Itzá, the pyramids of Egypt, and the Parthenon everywhere. All this appears to make the new nation a reality. But what kind of reality?

> In the first place, the timing of the archaeological push coincided with the first political struggle over the state's educational policies. "Progressives"—colonials as well as natives—were urging major investments in modern schooling.

> Against them were arrayed conservatives who feared the long-term consequences of such schooling, and preferred the natives to stay native. In this light, archaeological restorations—soon followed by state-sponsored printed editions of traditional literary texts—can be seen as a sort of conservative educational program, which also served as a pretext for resisting the pressure of the progressives. Second, the formal ideological programme of the reconstruction always placed the builders of the monuments and the colonial natives in a certain hierarchy. In some cases, as in the Dutch East Indies up until the 1930s, the idea was entertained that the builders were actually not of the same "race" as the natives (they were really Indian immigrants). In other cases, as in Burma, what was imagined was a secular decadence, such that contemporary natives were no longer capable of their putative ancestors' achievements. Seen in this light, the reconstructed monuments, juxtaposed with the surrounding rural poverty, said to the natives: Our very presence shows that you have always been, or have become, incapable of either greatness or self-rule. (Anderson 1996: 180–181; footnotes deleted)

When this idea is taken to North America, we can examine its usefulness for the Mexican and Maya areas. Castañeda (1996: 203–297) describes how the Carnegie Institution of Washington worked to create both a local hierarchy and a contemporary sense of the loss of greatness through its work on the pre-Columbian ruins. He hypothesizes that anthropologists like Redfield created a continuum from authentic traditional Maya to fully modern Mexican nationals. Traditional Maya spoke a Mayan language, might throw pottery, live in thatched houses with apsidal ends, use incense in non-Catholic rituals, and made and wore non-Western clothes. Modern Mexicans in the same area of Yucatan spoke Spanish, could read, believed in the 1917 Mexican Revolution, lived in cities, and were not Indian. In between, in this hierarchy of folk to modern, were people depicted as in a social limbo, between traditional and modern. Having neither Mexican nor Mayan culture, they had none: zero-degree culture. This hierarchy was created entirely by modern anthropological research.

More than this, there was a romantic notion that the Maya were the descendants of the great Maya and not completely gone, as there were some living traces like house shape, occasional rituals, and languages. They were Maya but not the ancient Maya, had some holdovers but not many, and were much changed. Above all, they had lost so much that it was not clear how to comprehend their heritage. That became an archaeological chore. These are Castañeda's ideas about the work of the members of the Carnegie Institution of Washington, and they add to and complement Taylor's initial analysis of the work in Yucatan of the institution's key anthropologists. This example also shows the complexity of archaeology's actual role in making modern peoples' identities.

Within the United States and Canada the very case that Anderson makes against archaeologists as colonialist handmaids is written into the Congressional hearings that led to NAGPRA (Smith 2004). The Native American Graves

Protection and Repatriation Act says that we have been working against native interests for a century. It is as though Senator Inouye of Hawaii, chair of the Senate Indian Affairs Committee, read *Imagined Communities* and wrote legislation against the process described by Castañeda.

Anderson says of normal archaeological knowledge, "While this kind of archaeology, maturing in the age of mechanical reproduction, was profoundly political, it was political at such a deep level that almost everyone . . . was unconscious of the fact" (Anderson 1996: 183). Therefore, I suggest that the scourge of the chasm between archaeology for colonial purposes and the stated anthropological reasons for archaeology is expressed as the anger directed at Walter Taylor and the critics that followed him. When we compare the actual purposes of much of archaeology with what we hoped it would be through anthropological training, there is every reason to suppose that practitioners would be unhappy with the ways their—our—blindness has been taught. This latent anger emerged to target not those who maintained the lie but the ones who exposed the lie. And that is why once Walter Taylor recedes into his proper dimensions, there will be some other object of long-term anger to take his place.

I continue with some discussion of what the result could be if we worked with this idea. Monuments are still being created in the form of historic villages, historic districts, battlefields, and prehistoric monuments. But the truly monumental is not the now-so-recognizable antiquity most tourists already know. The monumental archaeology within our own societies is that created by the laws mandating archaeology almost everywhere and called cultural resource management (CRM), as well as the resulting collection of things. These mountains of things are the more recent efforts loaded on archaeologists who plumb and measure the body of the state. Such surveying goes on around the world and is like a census in its attempt at totality.

The hollowness of CRM has been noticed by many (Cleere 1989; Smith 2004). Aside from understanding that legally mandated archaeology is an important financial base for hundreds of U.S. firms and thousands of archaeologists, its actual political function, analogous to Anderson's analysis of older archaeological monuments, is not clear. I raise this issue here because the inability of CRM archaeology to match the goals that archaeologists are taught in graduate school is, if anything, wider now than the gulf in Taylor's day. And so, if my argument has any merit, the anger of archaeologists with the inability to bridge the distance between the ideal and the real should be even greater in CRM. Besides waiting for the next critic, what can be done about this gap?

## HYBRIDITY AND CULTURE

For direction, I rely on Homi Bhabha's *The Location of Culture* (2002 [1994]: 80) to build a case that expands on Walter Taylor's analysis of archaeologists who did archaeology the way Benedict Anderson described it, that is, for colonial and nationalistic purposes. Bhabha's central idea is hybridity (2002 [1994]: 111–122), or the idea that culture is the actual mix of the colonizer and the colonized. No archaeology has been done to evaluate Bhabha's hypothesis. He implies that most cultures are hybrids and there is little or no unsullied descent from ancient origins. Bhabha's and Anderson's inclusion of archaeology is crucial. Even more helpful to my argument is their, and Castañeda's, inclusion of archaeologists as people in the colonizing, as well as the decolonizing, process. Because Bhabha tends to write semi-autobiographically and because Taylor's book seemed so personal at first, it might be wise here to invite archaeologists to see that many of us are the products of colonial and thus hybrid circumstances. These and the emotions associated with them are a reasonable place to start to work out a solution to how I began this chapter. The end product can be research on a problem of hybridity. To provide opportunities to think this way, I offer a summary of Bhabha's idea in order that we might turn his thesis into a research design.

The colonized not only is subordinate but also overcomes some of that status by absorbing the colonizer's language, government, and economic principles and then by reproducing these in his own life and works. The colonizer, who is usually from a democracy or a constitutional monarchy, is corrupted by the essence of the colonizing process because he helps establish a despotic government, the opposite of what the colonizer started with. Out of this comes hybridity, particularly for the subordinate who, fully aware that he can never be equal or no longer flee into an ethnic past, strives for independence or integrity by taking his difference—hybridity—and mocking its dominant source (Bhabha 2002 [1994]: 90). He speaks English but makes it almost unintelligible, as Indians can do; or far more beautiful than American English, as the Danes do; or far more evocative and meaningful, as the French do. They adopt Christianity and produce Santeria, Voodoo, or North American conjure. Or they take democracy and produce a government that supports Hindu nationalism, conservative Muslim intolerance of women's equality, or a privileged position for Islamic law in Iraq or for ultra Orthodox Jews in Israel.

The point Bhabha makes is that colonialism produces its own culture, and part of this hybrid's defenses and vengeances is the mockery of the most precious possessions of the colonizer. This happens through the process of perceiving that the colonizer will never let the colonized eliminate the imposed differences between the two, and that these differences—race, religion, language, and monuments—are despised by the colonizer. The revenge and independence of the colonized comes from mockery and reversal, which is being the same but different. This is what Bhabha means by the hybrid being less than one and double: the colonial is less than his master and, by himself, is the colonizer's double by declaring autonomy and independence through a mockery that makes the master's system intensely uncomfortable for the master.

Because mockery combines so much that is negative yet expresses this within an admiring stance, it is an unstable form. Freud points out that the multiple truths of subordination are held in the unconscious. Bhabha says that hybridity expresses these multiple truths and that their negations are far more positive and are a—or maybe the—location of culture. He is sure they are the location of freedom. Such freedom is obtained when it is spoken, written, read, or, to use Foucault (1973b; Burchell, Gordon, and Miller 1991), inscribed. Hybridity is expressed when enunciated, that is, when and where we see culture and locate the native's moment of freedom (Bhabha 2002 [1994]: 120). Expression and thus location can occur in markets, courts, schools, art, theater, and archaeological reversals, like Afrocentric archaeology. Examples may include *Black Athena* (Bernal 1987), the search for the Goddess in the archaeology of Çatalhöyük or for ley lines in Britain, or the denial of Old World origins in Native American critiques of North American archaeology (Layton 1989). This is where hybridity creates culture and where it becomes conscious. These are also potential places to excavate.

Here may be a window for an aware archaeology. If an archaeologist sees or reacts to these moments of exposure to his or her identity, then he or she can do archaeology to explore those circumstances in such a way as to understand them generally. This is the archaeology of colonialism, or capitalism. It will automatically have an audience and will be valued. It will inevitably be general and thus anthropological and will be scientific because it can use and cite Anderson (1996), Castañeda (1996), Friedman (1992), and Bhabha (2002 [1994]). In these ways it will close the gap between goals and results highlighted by Taylor, Binford, and Hodder. And it will be post-colonial by freeing the colonized, including the colonized archaeologist.

To close, I cite Bhabha (2002 [1994]: 221), who uses Frederick Jameson (1991) to deal with class. The question for both is locating consciousness, particularly of conditions of work, equality, worth, rights, and right and wrong. Jameson's most interesting point is that class consciousness has been misplaced in a country like the modern United States. It is either missing or underdeveloped. This is worth mentioning because archaeologists have virtually no understanding of their class position. They do little analysis of their employment options, profits of CRM firms, wages, or benefits as a group. It is already clear that we rarely understand the political function of our work. But archaeologists also are not aware as a group of their financial or economic position. A few years ago the Society for Historical Archaeology published in its newsletter a table showing that a major percentage of its membership earned around $20,000 annually. This produced no noticeable reaction, and attempts by archaeologists at collective bargaining over the last decade were weak or failed. There is no class consciousness despite the double whammy of being poorly paid colonial officers who have been taught they should be scientists.

Bhabha writes as an Indian immigrant to the United States. His book, although general, is not explicitly about the United States; his case studies are about India under the British. But he does describe the location of that class of people, like technocrats, bureaucrats, and professionals, who are not native to the colony's former independent cultural life. He also describes those who are from the dominant power, who are no longer influenced by the effects of the techniques they were to use on those they ruled. They are conscious. This is, I propose, a description of the life of an American archaeologist producing monuments to the dead for living tourists or of those who help to pile up the now amazingly useless mountain of archaeological material from legally required but otherwise scientifically unneeded Phase I, Phase II, and Phase III CRM investigations. And there are people, archaeologists, who know it and want to do something about it.

Therefore, what is the archaeology of our colonial life? It begins with autobiography and then can be about garbage, shopping malls, African Americans, Jewish heritage, genocides, British material cultural studies, the components of heritage, or the nature of memory and forgetting. The key is to ask, does it produce the freedom to hold up Clyde Kluckhohn's (1949) "mirror" the way Walter Taylor did, that is, by linking hopes to fulfillment? Can we talk to our fellow brothers and sisters about how they got into their present condition so that we produce illumination? Of course we can!

# PART V

## DISCUSSION

CHAPTER NINETEEN

# "CONJUNCTIVITIS"

## Notes on Historical Ethnography, Paradigms, and Social Networks in Academia

*Quetzil E. Castañeda*

This volume takes one by surprise with many eye-opening moments, which are no doubt welcomed by most readers as enlightening and productive. Despite the benefits of this literal and metaphoric effect, it may nonetheless aggravate the pain and irritation of those few other readers who suffer from a type of "conjunctivitis." This is a *dis-ease*, as it were, of vision triggered by contact not with Walter Taylor per se but with his aura as pariah or with the intellectual labor that the conjunctive approach demands. However, by revisiting his book's theoretical issues and its sociohistorical context, as well as disciplinary controversies, unyielding professional resentment, and the antagonisms that are implied by the question of Taylor's significance to the field, the contributors have managed to offer the reader effective medicine for this sixty-year-old case of archaeological pinkeye. Although the editors offer their book quite literally as a remedy to this situation, they also recognize that their "medicine" may again provoke an outburst of the malady and a rebuke of Taylor and his conjunctive approach. This cure, therefore, is indeed likely to aggravate again the anger that Leone, in his chapter, analyzes as a productive passion and motivation that can and should stimulate intellectual advances. For readers angered by this volume and these suggestions, it is perhaps better for them to find their own answers to such provocation in order for them to see, and to share, what new insights these may conjure. Certainly, for some to find these answers, a (re)reading of Taylor's book will be in order.

As for me, an outsider to archaeology, I welcome the eye-opening chapters for their individual and combined contributions to the history of the fields of archaeology and anthropology. Among the surprises that I found useful and stimulating are the discussions of Taylor's reading of Boas, his insistence on the adjectival use of culture (i.e., "cultural") to identify that which is to be analyzed in order to construct culture and cultural context,[1] his emphasis on historical constructionism, the pressing need for statistics in archaeology at that time, his calls for changing the dynamics of training and professionalism, and, of course, the depth of the archaeological rancor toward Taylor. These are just a few elements—or affinities, to use Taylor's term—about which I have gained substantial understanding. This is an impressive volume and, as I say, it is surprising, but not just for the array of provocative insights. It is also eye-opening for the way in which these fragments of opinions, memories, and analyses cohere and thus drive this collection to evoke much beyond itself. In this commentary, I follow the lead of the book itself and provide a patchwork of notes and reflections. I begin with thoughts on the structure and approach of the volume, which leads me to an exploration of the ethnographic sensibility and qualities of the history of archaeology presented here. From the ethnographic life history I turn to theoretical issues and historical contexts that the volume raises. Consideration of sociopolitical contexts leads in turn to a discussion of the fear that ethnography of archaeology can cause among archaeologists. I conclude with a comparison of two archaeologists, Walter Taylor and Michel Foucault.

## DOUBLE MIRRORS: CONJUNCTIVE REFLECTIONS

As the editors note in their introduction and preface, this is not a festschrift, although it builds on a foiled attempt to produce one. We do not know what the original Folan and Reyman book may have been had it come to fruition, but we are thankful that its destiny, in some sense, has been reincarnation in this hybrid text. The present volume is hybrid because it is at once biography, history, theory, criticism, and, perhaps above all, ethnography. It is an amalgam with a central goal that allows for and encourages varying perspectives, contradictions, airing of laundry, and passionate and calculating inquiries. It jettisons the diagnostic feature of festschrifts, that is, a thematic unity and structural coherence created by the drive to honor, promote, and celebrate the indicated person, which in this case is a man who many either want to forget or have difficulty figuring out how to remember. Nonetheless, there is still a festschrift effect because this volume examines the relationship between the intellectual work of a man and his biography. *Prophet, Pariah, and Pioneer* resuscitates Taylor's conjunctive approach with the first in-depth, sustained assessment and debate about what it was and how it was intended. Taylor's book explored an "idea" that since the time of its publication, and here in this volume, has been variously labeled as a theory, methodology, set of practices, vision, approach, protocol, paradigm, model, and the list goes on. In excavating the tangible and intangible dimensions of this "idea" and the complex myths and realities associated with its author, the editors have strategically, if quietly, structured this hybrid historiography of archaeology in the image and as a reflection of Taylor's elusive idea and troubled ideal. At the same time, this revitalization of Taylor's scholarship offers a powerful exploration of his unsettling experience in academia, which is embodied in this volume with chapters that present again some of those long-lasting passions and conflicting opinions. This volume's conjunctive strategy is particularly fitting given Taylor's own concern with and theorization of the relationships between historiography (i.e., history writing), archaeology, and cultural anthropology (i.e., ethnology). Given the continuing significance of these issues to these fields of study, it is important to understand how the present volume interrogates these issues and thereby contributes to the field of archaeology and its history.

*Prophet, Pariah, and Pioneer* has a double structure, each facet of which is already constituted by a tension between two elements. On the one hand, there is the mirroring of man and his idea. In other words, the biography, life history, and firsthand memories of Taylor are used to mirror and trigger us, the readers, to reflect upon the conjunctive approach. At the same time, the theoretical discussions of Taylor's approach are used to reflect back and mirror Taylor's life history. On the other hand, this mirroring structure is itself doubled by the editors' selection of a conjunctive approach as the strategy and structure, or central lens, through which to investigate Taylor's main intellectual contribution to archaeology. The conjunctive approach therefore becomes a source of and resource for ongoing reflection. Wisely, the editors chose not to use any number of other specific and known historiographic approaches, for example, festschrift, life history, intellectual history, political economy, and so forth. This choice is significant for it opens new insights and directions and, by its structure and hybridization of genres, contributes a possible alternative model for investigating the history of archaeology.

The core of this twin set of double mirrors is an assemblage of the fragments of life—the variegated life documents, class notes, letters, memories of encounters and experiences, recounted anecdotes, oblique references to corridor gossip, bad-mouthing, opinions, value judgments, scraps of paper, remembered attitudes, re-felt passions, images engraved in thought and pixilated in virtual space—that lie scattered on the surface of the past as well as buried deep within silent stratigraphies of propriety, eroded conversations, and imposed visions. If the biographic dimension of this volume is not festschrift, as earlier noted, it is also strictly speaking neither life history nor biography: rather, this history is quite plainly ethnography.

Let us call *Prophet, Pariah, and Pioneer* a historical ethnography, for it uses diverse life documents but in a way that does not adhere to either biographic univocality nor the progressive narrative of intellectual history. As an ethnography it draws on a plurality of "native voices"—not only that of the subject himself but of students, teachers, colleagues, commentators, critics, friends, and interested third parties. It is not an intellectual history because it does not aim to fix a genealogy of intellectual influence, debt, and legacies but rather to grapple with the theoretical, methodological, and conceptual content of the conjunctive approach. We are forced to ask, what was the conjunctive approach then, what is it now, and what is its significance for us today in the present? In this way, *Prophet, Pariah, and Pioneer* is not "history" in a historian's sense of understanding the past for its own sake or to better understand the present. It is less an ethnographic history than a historical ethnography because it is definitively a present-day accounting of how the past is still meaningful today with multiple conflicting and contradictory meanings. This volume is akin to Taylor's book and approach—it is historiographic, assessing social context at every turn, and is also fundamentally anthropological in the sense that it builds on the Boasian tradition of critical romanticism (Stocking 1989). As if taking the title of Kluckhohn's (1949) *Mirror for Man* as its structural motif, *Prophet, Pariah, and Pioneer* uses archaeology's past as a mirror of the present with the goal of triggering and motivating us to *act now* to work to change the future.

## ETHNOGRAPHY OF LIFE / HISTORY OF ARCHAEOLOGY

*Prophet, Pariah, and Pioneer* is a patchwork of original and primary life documents that range from class notes and e-mail letters to published and unpublished memoirs and eyewitness accounts. These primary documents are articulated within chapters that range from historical recollection, biography, and theoretical analysis to social commentary and critical reflection. There is a multiplicity of "native voices" and there is also a multiplicity of voices of distanced and distant commentators. Unlike typical ethnographies and histories in which multivocality and native voices are synthesized into a uniform, singular analysis and perspective, this volume maintains a radical heteroglossia. This is clearly evident from the analytical chapters (in Part IV), but even within chapters consisting of firsthand recollections, especially those that include negative comments, the unitary perspective of authors is fractured and fragmented by their own justification that their viewpoint is "just my own partial opinion." Thus we read: "I sought a balanced appraisal, considering both good and bad aspects" (Kelley); "These recollections about Walter W. Taylor are completely personal. . . . At best these memories are mixed. Taylor had a problematic and at times volatile personality" (Weigand); "Why speak ill of the dead?" (Schoenwetter); "Walter Taylor's contributions to Southwestern archaeology were mixed. . . . [His] greatest contribution to . . . American archaeology may have been his service as a transmitting agent for Hargrave's ideas" (Fowler). It is as if the book were about to burst asunder into contradictory shards of thought and splinters of memories that are contentious and partial in all senses. The reader quickly realizes that a unitary, coherent, noncontradictory grand synthesis of the multiplicity of fragments and perspectives is impossible. This is not a debility but rather quite a great virtue! It is what makes this book ethnographic and one reason why the historical groundings are a significant contribution to the historiography of American archaeology.

Exemplifying the centrifugal force of the volume is Kehoe's chapter, which ends not once but twice; or rather, there are two overtly disconnected sections, each of which offers a possible end point to the chapter. First, the bulk of the chapter is devoted to a discussion of Cornelius Osgood's research agenda, which ends suddenly without any synthesis. Second, a section titled "postscript" subsequently appears that consists of a brief anecdote in which Kehoe relates her discovery of another, completely unrelated and fictitious Dr. Cornelius Osgood; in a half-jesting comment she suggests this is a fable—half Dené, half academic historiography—of reincarnation. Where or which is the conclusion—or even, "is" there one? And what is the meaning of the hybrid Dené-academic fable? Is it a random *reflection* or a crack in the double mirrors of *Prophet, Pariah, and Pioneer* that allows us to see the inner workings of . . . what? The volume? Taylor? The conjunctive approach? Archaeology? Social scientific inquiry? Or perhaps all of these?

In the analytical terms of Deleuze and Guattari (1987), Kehoe's fable—or its disjunctive coherence—is called a "line of flight" from the totalizing structure or epistemological system. At the point of solidification or totalization (of a system of ideas, machines, or knowledge) there is always seepage and a break or tear that prevents complete totalization or fulfillment.[2] The line of flight is therefore the principle that there is always an idea or element that veers off the plane of operation (or coherence) to generate a new configuration of ideas and potentialities elsewhere. Could it be that Kehoe is obliquely asking in what sort of postmodern cyborg or what (past or present) school of archaeological theory have Osgood and Taylor been reincarnated? Her fable and (non)closure may seem extravagant, but her chapter is not unique in this volume for its dispersion and scattering of a potential synthesis. Lines of flight, or what might be identified and labeled as loose ends, extra information, observations without conclusions, clues without resolution, are everywhere in evidence—especially in the chapters by Dark, Kennedy, Folan, Weigand, Clay, Schoenwetter, and Riley. Based on the broken remnants, bits and pieces of life—or, to invoke Walter Benjamin, "detritus and debris"—the compositional style and rhetorical force of these chapters is symbolic not allegoric, imagistic not narrative, askew not rectilinear, aphoristic not analytical, multiple not unitary (see Benjamin 1968, 1978, 2002; Buck-Morss 1991).[3] This makes the book, again in Deleuze and Guattari's terms, rhizomatic not arboreal.[4] Further, it makes the volume as a whole Taylorean in that it is a kind of conjunction or conjunctive analysis of the far-flung associations and relationships that make up Taylor and his place in archaeology.

The results of this mix of well-crafted analytical and experiential commentary, in which there is an accumulation of affinities, provide not a realist photograph of a man and his work but rather something vastly more interesting and significant: a cubist painting with skewed lines, odd angles, oversized features, miniaturized elements, closeups, incongruent perspectives, surrealist shadings, misplaced shadows, and concrete abstraction. In this ethnographic cubism or cubist "portrait of an archaeologist," the disjunctions and contradictions among the personal opinions, perspectives, experience, social history, and theoretical commentary of the contributors stand out and grab the reader tightly—and do not let go.[5]

These chapters are raw texts. They provoke countless images and sensations: eating clams on ice while deep in Pueblo backcountry (Fowler), the smells of a house stinking stale from exploded home-brew beer, imposing or dictatorial classroom pedantry (Folan, Kelley, Riley, Weigand, Reyman, Schoenwetter), suggested sexism (Kelley, Kennedy), successive marriages ending early in death or hostilities (Kennedy, Reyman), friendships gone awry over departmental and national politics (Riley, Kelley, Clay), midnight intellectualizing and verbal jousting in kivas or talking anthropology in prison camp (Dark, Reyman, Kennedy), the iconoclastic graduate upstart (Joyce), an erudite and sophisticated thinker (Joyce, Watson, Maca's introduction) yet intellectually narrow-minded and constrained (Weigand), staged professionalism with colleagues whose hostility smiles for the camera at conferences (Kennedy, Dark), drinking pulque while reciting Garcia Lorca's poetry (Folan), growing orchids and cooking (Folan, Riley). These chapters communicate a man with a range of character traits, including deep respect for intellectual integrity, resilience and humor in dire situations of imprisonment, sensitivity to and suffering from academic badmouthing, cheap and penny-pinching with hired help, hands-off yet inspirational pedagogical style, entrenched if muted social-class pretensions, and yet an overarching attitude of a regular, manly man who insisted on his privacy and did not judge himself a savior to the field.

These are engaging images in the best sense, that is, they ignite the prurient, gossip-mongering passions that inhabit, like an incurable pathology or an incorrigible virtue (depending on your viewpoint), any "good" ethnographer. Indeed, regardless if this is a vice or a virtue, we want more! And we get frustrated at times with the restraint of those authors who seek some register of professional portraiture instead of fully delving deep into the rich stories, experiences, and passions that would communicate even more of Taylor's soul and spirit. A conjunctive historiography of Taylor might aim at "drawing the completest [*sic*] possible picture of [a] past human life" by bringing in the greatest number of "affinities" (Taylor 1948: 95–96), which in this case means ethnographic and biographic details. The restraint is understandable out of respect for his privacy and as a way to preempt any possible further negative twisting of the legendary tale of Taylor the iconoclastic gadfly, or simply to not appear inappropriate for "speaking ill of the dead." Nonetheless, we still do want more storytelling about late-night camaraderie, cooking, gardening, teaching, conferences, poetry, and even the negative experiences, interactions, and opinions of Taylor. We are left asking for more. Why? Because this vibrant, vivid portrait of Taylor marks and makes pale the monochrome halftones that characterize countless biographies in anthropology and archaeology.

In this way the volume expresses a strong recommendation to those who work on the history of anthropology. For those who write biography to construct legacies, genealogies of influence, and/or life-history contexts of archaeological ideas, there is a lesson to be learned here. The approach embodied in *Prophet, Pariah, and Pioneer* provides a model for anthropological historiographies of archaeology, demonstrating that they can be written with a greater ethnographic sensibility for the human complexity of the persons they write about, including conflicts and disjunctive evidence. In this way, these might achieve more coherence and less totalization, more construction and less fashion. The editors of *Prophet, Pariah, and Pioneer* have endeavored to *not* force all the details into a perfectly woven, harmonious textile, but, like artists who work with wood, they used the knots, flaws, and aesthetics of the material itself to reflect greater appreciation and understanding.

On the one hand, these primary/raw materials, which bring out the tones and complexity of Taylor as a human being and person, suggest that his pivotal presence had less to do with anything he actually did or did not say, write, or do. Rather, as a number of authors in this volume point out, it was what his colleagues created: a persistent and powerfully negative mythology. According to Leone's analysis, Taylor the person became an enduring target for the anger that Taylor's *ASOA* triggered by exposing the gap between archaeology's practices and its highest ideals, such as *re*construction, truth, and past reality. The construction of Taylor, an archaeologist and a human being like any other, as a legendary evil cloaked in a dangerous aura of pollution was forged by the public and private actions and chatter of his cohort and their mentors. The authors reiterate, as Taylor himself did (1969, 1972c), that no one had the ability or character to respond to and assess his book in any substantial way. It seems they felt that the only option they had was to retaliate by creating both grand and petty negative myths around his work and persona. Furthermore, regardless of whether his critical analyses of the archaeological work of Kidder and others seemed ad hominem, whether they were motivated by personal issues such as having been excluded perhaps from some clubby Harvard cabal, whether they were too sophisticated to be fully grasped by his audience, it is clear that Taylor preferred the tranquility of nature—hunting, fishing, canoeing, cooking, gardening—over networking with colleagues. If he felt wounded at times by being blackballed, it seems that at other times he simply could not have cared less about it. Imagine the thought of having "to defend yourself" or create a network of academic allies after having served active duty in World War II, during which you literally were wounded in a grenade attack and spent time in a prison camp,

Clearly, Taylor did not have a taste for the trivial and the banal and had no time for the routine absurdities of academia. He had no interest, for whatever reason, in "building cadres" of followers to create a paradigm-breaking "school" and had zero fascination with careerist self-promotion. It seems singularly bizarre and flagrantly ideological to suggest, as some of this volume's authors do, that the absence of these ambitions are because he was an academic of an upper-class background—a logic, one should note, that construes all academics of middle- or lower (god forbid!) class status as inherently self-serving, careerist, money-grubbing, ambitious, and who knows what else! Taylor is certainly a different generation from most of today's academics who, in order to survive, must have strategic career-planning tools, preferably pre-installed on a BlackBerry, groups of pre-networked allies, in addition to shameless self-aggrandizing skills. But, more significantly, he was evidently a unique individual with his own distinct personality and an intellect quite different from a majority of archaeologists of that era.

On the other hand, this portrait of Taylor's complexity and the enduring negativity that continues to surround his person finally reach a limit with the reader of this volume, provoking a startling rebound or reactive redirection. At a certain point, we stop asking about Taylor the man and say, let him be. Let him go fishing or hunting, gardening or cooking. Let us ask instead, as do Leone, Maca, Folan, Reyman, and others, about the sources of anger and dis-ease in each of us and in the state of archaeology. The volume pushes us to ask with speculative bewilderment, what was going on with everyone else? What were the sociopolitical, intellectual dynamics such that this man and his work were received as they were? It demands us to ask, ethnographically, about the social relations and politics of archaeology, and of academia generally, then *and* now. How do these relations, these contexts, enable, condition, and propagate the formation and persistence of academic reputations, facilitate character assassination, and compel cutthroat careerism, all offered with a happy face of harmonious collegiality often sans intellectual substance. The obsession with Taylor suddenly and definitively rebounds away from the author, and even from his text, away from the hermeneutic of the man and his work, and toward other underlying sociopolitical contexts, some of which remain buried deep under the surface, others scattered about or hidden in plain sight. These contexts, these conjunctions of elements and affinities, may be shown to have contributed much in the construction of a pariah "for our anger." This "rebound" can, and in this volume indeed does, move in two directions, one toward the sociopolitical contexts of archaeology and the other toward the intellectual content of Taylor's work. Further, this redirection away from Taylor the man is significant and valuable: it is what makes this historiography of archaeology anthropological. This is what makes *Prophet, Pariah, and Pioneer* not only good historical ethnography but good anthropological history of archaeology.

## LINES OF FLIGHT: SPECULATIONS ON THEORY AND SOCIOHISTORICAL CONTEXTS

The deflection of light away from Taylor, as person and text (i.e., *ASOA*), to the sociopolitical relations of archaeology is something I wish to explore in more depth, not least because such relations can dramatically affect the reception and dissemination of new, alternative, and/or dissenting approaches in academia. The deflections we see in *Prophet, Pariah, and Pioneer* are actually multiple, subtle, and strategic. Note that the second and third parts of the volume largely concern Taylor the person. These chapters are assembled as context for his work at SIU and his relations with colleagues and students. Therefore, there exists much professional commentary, but overall it is here that we confront personal memories and anecdotes regarding Taylor the man. Then, however, the volume rebounds into sustained discussions of Taylor's intellectual work, its content, impact, and value for different subfields of archaeological study. In the treatment of these intellectual issues, the question of the ethnographic contexts continues to be reflected as a major concern, as was the case in the ethnographic life-history parts of the volume. In this section, I comment further on these "lines of flight" away from the questions of personal biography.

First, I offer a type of meta-commentary regarding impressions and receptions of Taylor's "theoretical scheme" and the question of paradigms in archaeology and anthropology. Second, I extend the ethnographic analysis of the volume with a discussion of additional sociopolitical affinities in archaeology. I close this chapter with comments on the need for ethnography in and of archaeology. Although the archaeologist reader may not recognize this need, or even may deny it, *Prophet, Pariah, and Pioneer* actually quite forcefully argues for an explicit discussion of this topic. The argument of the volume—in its structure, in the content of its chapters, and by its conjunctive approach—forms a powerful proposition to the effect that a more thoroughgoing exploration of the historical associations, social relations, and political contexts can move us away from hazing, anger, and the pigeon-holing of innovative scholars and ideas and move us toward more productive, positive understandings of what archaeologists are doing right now, what has been done in the past, and how it can be done better and with ever-increasing integrity.

## PARADIGMS LOST

The historical ethnography and textual structure of *Prophet, Pariah, and Pioneer*, as just elaborated, prompts a comparison with *I, Pierre Rivière, Having Slaughtered My Mother, My Brother and My Sister* edited by another "archaeologist," Michel Foucault (1979 [1973]). Both texts consist of primary and secondary life-history documents that serve as the structural core around which other layers of historical, analytical, and interpretative commentaries revolve. Both are histories that turn away from the ethnographic details of a life to the sociopolitical contexts that embed the person and the event (or intellectual work) that they created. Although it would be instructive to draw out further this comparison in terms of the editors' and Foucault's respective approaches (i.e., conjunctive versus archaeological) to historiography, I instead focus on theoretical issues to see how Foucault might reflect for us a greater understanding of Taylor.

Although published in 1973, in transition to his genealogical analysis, *I, Pierre Rivière* manifests a unique expression of Foucault's archaeological method of historiography. His archaeological history combined structuralist and semiological tools and concepts within a poststructuralist research agenda. If we are persuaded by Joyce's analysis of Taylor's use of semiotics and structuralist principles in his iconographic study of the Maya ceremonial bar (see also this volume's introduction as well as chapters by Dark and Reyman), we might also characterize Taylor's archaeology in a similar way. Indeed, as discussed by a number of authors, Taylor's concern for patterned series of relationships among affinities, especially conjunctive patterns of similarities and differences within contexts of data production of increasing scale—that is, first, specific contexts of excavation; second, the site as a whole; and then, third, across regions of related sites—reveals a structuralist logic wedded to a semiotic mode of analysis.[6] Tellingly, and certainly as a result of these strategies, both Taylor and Foucault have been the objects of confusing and conflicting speculation as to what exactly is the "theory" or "method" they proposed. With regard especially to Taylor, we need to ask, why?

A number of chapters (the introduction, chapters by Kennedy, Reyman, Maca, and Dark) make it clear that Taylor was a uniquely creative thinker who combined elements of different theories to develop his own unique framework. For example, just as Foucault used structuralist concepts for poststructuralist purposes, Taylor used a modification of Boas's culture concept for "non-Boasian"—and anti-positivist—analyses of the historical development of (partitive) cultures and the evolution of (human) Culture. Taylor also employed, for example, functional, contextual, historicist, and ideational concepts in his modeling of the conjunctive approach. This creative "making-do," pastiche, or bricolage approach to theory construction is important to highlight because it bears consequences that can be far-reaching and/or unintended. First, for example, it makes the pursuit of influences on Taylor extraordinarily difficult (note nearly everyone's struggle to define the Kluckhohn connection) and this historiographic pursuit in general of questionable utility. It is clear that Taylor used some concepts in ways that were either contrary or unfamiliar to the sources of these concepts, and in some cases, there is no tangible or discrete expression of the inspiration (e.g., Dark regarding the "influence" of Childe; Kehoe regarding Osgood; Joyce's and the editors' ruminations on the significance of Tozzer). Second, because this creativity derives from and leads to hybrid conceptual tools, difficult readings are almost guaranteed; the opacity, density, and complex language of *ASOA* is cited again and again, both in the chapters of *Prophet, Pariah, and Pioneer* and in the half century of commentary on *ASOA*. Third, given the disciplinary milieu of professionalization at that time, in which theory was considered as speculation and "indecent," Taylor's sui generis thinking and theorizing were not simply difficult, they were incomprehensible: "It was clear that they just did not get it" (Longacre). Fourth, where Taylor's synthesis of ideas displayed identifiable borrowings, he was not hailed as genius, but rather his unique intelligence was denigrated (Taylor the "gadfly") or his visionary goals degraded, belittled, or ignored (e.g., Taylor as merely a transmitter of other scholars' ideas [Fowler]).[7]

For decades it seems that only clandestine readings of Taylor's *ASOA* were possible, or else that his book was used as a reference volume or in a way that precluded the necessity for citation and attribution. Consider that it took years, even decades, for many archaeologists, including several in this volume, to publicly admit that they read (and even liked!) *ASOA*. The clandestine reading of Taylor's book as well as the other apparently varied readings and uses of the book, I suggest, created a particular mode of interpreting and receiving *ASOA*. This is made evident by most of the chapters of this volume. The predominant tendency, especially evident in dismissive interpretations, has been a "piecemeal" approach. By this I mean that the uses of Taylor's book, and thus the discussions of its contributions, tend to reduce the conjunctive approach to one or another specific, tangible, and easily grasped (although sometimes misinterpreted) aspect of Taylor's vision for archaeology. These run the gamut: a developmentalist post-Boasian theory of historical particularism; an analytical tool kit of types and typology; a methodological protocol for documenting quantity, distribution, and association of artifacts; a standardization of training protocols and practices; a constructionist philosophical position (rarely); a *re*constructionist empirical position (much more often); an endorsement of hypothesis testing and the formulation of research problems; a strategy and methodological program of five hierarchized research phases (often); a strategy of flexible phases and protocols (rarely); a cultural historical interpretive model; a multidisciplinary collection and analysis of multiple types of contextual data (e.g., geological, environmental, biological, climatological); a research agenda targeting either "site-specific" problems or "region"-focused issues; and so on.

The piecemeal approach has certainly allowed different "clandestine readers" and selective borrowers of Taylor to use *ASOA* as a source for ideas that are appropriated and transformed into new and often different kinds of archaeology than Taylor explicitly envisioned. Piecemeal (or wholesale, for that matter) appropriation of any great thinker's work is predictable and leads to hybridization of elements. Because such appropriations and attendant hybridization is in itself neither erroneous (bad) nor virtuous (good), it raises the question of how we should define and identify the uses, abuses, misuses, and dis-use of Taylor's vision of archaeology—as well as the visions of other important and controversial scholars and thinkers. I am not qualified to enter these debates in archaeology and do not want to seem as though I seek to police the field; however, I can note that the piecemeal mode of interpreting "Taylor" (i.e., *ASOA*) seems to have definitively prevented an understanding of his conjunctive "approach." And what shall we call it without prejudging and predetermining the answer? Is it a theory? An approach? A set of protocols? A method, scheme, attitude, or guide? Further, the piecemeal approach and clandestine readings have certainly created obstacles to rigorous considerations of "it" as a sui generis paradigm. Cordell, in the foreword to this volume, comes the closest, I think, to identifying the conjunctive approach as a paradigmatic vision. However, she nowhere uses this word or even a close synonym and, thus, like many others, interprets and refers to Taylor's work as a collection of fragments. For example, she, too, ends up providing only a list that is not conceptualized (constructed) as a holistic, coherent paradigm. This "step-short" view is made evident in her claim that "Taylor's analysis was so penetrating and accurate that his work became a starting place for many scholars." Although Cordell does not reduce the conjunctive approach to a "mere critique," and although she certainly does conceptualize it in some sense as holistic, she does not take the logically plausible (and somewhat politically charged) next step to declare that it was indeed a paradigm.

I am not saying that it was or was not a paradigm per se. Rather, I am suggesting that piecemeal appropriations and interpretations of Taylor's work limit our understanding of Taylor. On the one hand, this hampers the possibility of raising certain issues, specifically philosophical-conceptual questions about the text of *ASOA*. On the other hand, this is an obstacle to explaining the sociopolitical contexts of academic archaeology. The fact that his text was a kind of forbidden fruit, and that the use of his name in citations would have scandalously aligned authors with a career-demolishing demon, means that a moment and a paradigm may have been lost. I do not want to stumble upon a regretful tone, resurrect Taylor as a savior-martyr of the field, or go over the top with metacommentaries couched in biblical rhetoric or paradisiacal metaphors. However, recognition of this situation allows us to understand two significant points. First, although we often (and uncritically) accept piecemeal interpretations of scholarly works as components of hybrid conceptual schemes, and although the case can certainly be made that Taylor himself practiced this type of formulation, it seems empirically evident that the case of Taylor merits special consideration in the history of archaeology. This owes mainly to the fact that he proposed (what he called) a scheme—in fact a multilayered theoretical scheme—that was totally out of the ordinary, unexpected, and over the heads of the vast majority of archaeologists practicing at that time.

The second point is more general and this is that *Prophet, Pariah, and Pioneer* has opened up a few areas of inquiry that demand further interrogation and exploration. Specifically, by raising questions about the theoretical, philosophical, analytical, and methodological basis of the conjunctive approach, this volume raises the question of "paradigms." Is Taylor's proposal a paradigm? What is or could be a paradigm in archaeology? What in fact *is* a paradigm—versus a theory, school, or methodological array of practices? Should we follow Kuhnian analysis? Are the postprocessualist anti-paradigmatic arguments more valid? How can this topic of inquiry be properly justified and explored? Is a paradigm—or even an anti-paradigm—only a paradigm when a "school" or "group" develops to support and explain it? Although similar questions were formally posed to anthropology about Anthropology beginning in the 1980s, and to archaeology about Archaeology at around the same time, the problem of understanding paradigms, paradigmatic traditions, and disciplinary modes of archaeology is even more obvious and urgent today. Currently, there is a proliferation of archaeologies. Are any of these paradigms? There is a widespread negotiation (if not struggle) to define the entire enterprise, agenda, and project of archaeology. Thus, it is salutary and even cutting-edge that *Prophet, Pariah, and Pioneer* breaks open this topic of inquiry again, but in a novel and productive way that is based on discussions that return us to a crucial moment in archaeology, that is, when the dilemmas and optimisms about the field as a scientific endeavor both grew and experienced fundamental challenges.

## AFFINITIES, CONJUNCTIONS, CONSTRUCTS

Threaded throughout *Prophet, Pariah, and Pioneer* is the theme of the social networks, political dynamics, and motivations that contextualized the publication and reception of *ASOA*. In a line of flight that leads to provocative new areas, Reyman (this volume) briefly intervenes on this topic with the comment that there is no "extant evidence of which we are aware that [J. Alden] Mason [as editor of *American Anthropologist*] urged Taylor to modify or tone down [for final publication] what has sometimes been referred to as his ad hominem style of critique." In this section, again with the "aim of drawing out the completest [*sic*] picture" (Taylor 1948:95–96), it seems particularly pertinent to use a conjunctive approach to thicken the ethnographic description with additional "associations . . . relationships, affinities" (ibid.). During the post–World War II era and the publication of *ASOA*, there may have been other conflicts, motivations, and allegiances that shaped the behavior and attitude of numerous actors and "agents."

Without a smoking gun, as it were, interpretations of psychological motivations, as periodically expressed by some authors in this volume, should be offered only as hypotheses. I suggest instead that we actively search for greater and better evidence about the sociological contexts in which Taylor and *ASOA* are embedded. An anthropological strategy for doing this should include tracking the networks and conflicts of social actors. So let us begin with Mason. He was one the four known archaeologists who conducted covert espionage in Mexico during World War I for the U.S. Navy. The four were later referred to by Boas in his famous critique of ethics in anthropology; a critique that resulted in him being publicly denounced (Price 2000, 2001; Harris and Sadler 2003). In contrast to Sylvanus G. Morley, whose espionage has been touted as the most exemplary, successful, and patriotic (by Harris and Sadler 2003), Mason, with great naïveté, botched his job[8]: he apparently made it no secret in Mexico and Chicago (the Field Museum, specifically) that he was working as a spy. This not only jeopardized his cover, thereby forcing him home even before he began an assignment, but clued Boas to the fact that archaeologists were conducting covert intelligence work. By noting these facts, we immediately raise a significant issue: there was—and perhaps still is—a quasi-invisible network of alliances, friendships, antagonisms, and collaborations that feeds through archaeologists and anthropologists working, covertly and overtly, for the U.S. government in one or another branch of intelligence.

In addition to the four archaeologists (Morley, Mason, Samuel K. Lothrop, and Herbert Spinden; see Price 2001) who Boas referenced (without naming, I should add), each of whom worked for the U.S. Office of Naval Intelligence, there was at least one other well-known anthropologist who worked not for the U.S. Navy but for the Military Intelligence Division during World War I: Alfred Marston Tozzer (Harris and Sadler 2003: 289n16, 413). This remains little publicized and it is unlikely that Boas knew of it.[9] Harris and Sadler (ibid., 60) note that Tozzer helped Morley to recruit Lothrop for the ONI, but not much else is known about his activities. There is more widespread knowledge that Lothrop, Harvard professor and Carnegie Institution researcher, continued his covert espionage during World War II by joining the Special Intelligence Service, a unit of the FBI devoted to Latin America (Price 2000). And there were others. Taylor, although a covert spy (Maca [Chapter 1] and Dark) in the Office of Strategic

Services, the precursor to the CIA, was also a Marine lieutenant overtly engaged in active military service. This fact, we should note, very likely makes his involvement a different kind of ethical "case" than situations of anthropologists who worked with Japanese internment camps; conducted real covert cloak-and-dagger espionage (e.g., Morley and Lothrop); sat in a U.S. government office translating German- and Japanese-language newspapers; fed the government ethnographic intelligence about local networks, politics, and leaders; or used positions of institutional authority in academia to facilitate funding of covert intelligence with federal grants. Although Kidder apparently was not a spy, he certainly covered up espionage in his role at the Carnegie Institution of Washington (CIW) (Kidder 1930, 1941; Castañeda 2005). This last affinity raises anthropological questions about the role of the CIW not only as a pioneering sponsor of non-university, non-museum, non-government archaeology but as an institution that had quite an explicit, if also secret, agenda of establishing American science in the service of the U.S. government in times of both war and peace (Reingold 1979; Castañeda 2005).

The issue of espionage during World War I is not new and many of us are now familiar with the politics and anti-Semitism surrounding the denunciation of Boas (e.g., Stocking 1968; Pinsky 1992). However, these are affinities that must be explored in any conjunctive, contextual analysis of Taylor and *ASOA*. One clear connection to investigate is the CIW, which was both the institutional home target of Taylor's intellectual critique and a behind-the-scenes hotbed of Boas's intellectual and political enemies. This applies not only to trustees, such as Charles Walcott, and researchers, such as Morley, Lothrop, and Kidder, but also to CIW president John C. Merriam (1919–1938). Merriam, for his part, participated in the founding of the Galton Society (of eugenics) and led the assault, as president of the National Research Council in 1919, to strip Boas of his NRC membership. He also actively promoted a paradigmatic vision of anthropology in which eugenics and evolution were central and that Boas and his students therefore viewed as a major threat to their conception of anthropology. Without going further into the reasons why Merriam had selected Kidder as early as 1925 to serve as the director of the CIW's Division of Historical Research, it should be clear that these networks are significant issues to investigate here in an anthropological study of the history of archaeology. In particular, these associations, relations, and affinities raise questions about how the personal antipathies and secret alliances among archaeologists, including Taylor, across several generations, from the 1910s to the 1950s, map onto the affiliations that many archaeologists had with various U.S. universities and with various intelligence units of the U.S. armed services.

But let us return to Mason for a moment. He is the only one of the four implicated by Boas's 1919 letter to *The Nation* who later apologized to Boas for his error of judgment in his failed adventures in espionage (Harris and Sadler

2003: 50–53, 289–290). In accepting *ASOA* for publication, did Mason feel sympathy for Taylor because of an antipathy toward the targets of his critique or because Taylor and his mentors were strong promoters of Boas's ideas and writings? Was their bonding or friction—we may never know which—the result of their opposing war experiences? Did that matter? And then of course there is always one among many potential conspiracy theories: is it possible that Mason did not suggest toning down the critique because he knew it would provoke repercussions that he wanted Taylor to suffer? Where is Mason located in the social networking, professional and intimate friendships, personal sympathies, and antagonisms that pervade the context of the publication of *ASOA*? And, again, where is Taylor? Was he networked with alliances to anthropologists that worked in the OSS or Army-based intelligence units that competed with Naval intelligence? Or was Taylor himself critical of spies who posed as archaeologists, or archaeologists who used their jobs as cover for espionage? We do know that he ultimately married another former OSS agent.

Tozzer, as mentioned earlier, has quite an elusive position in all of this and, in general, his role in academic power plays, from Boas and espionage to Taylor and *ASOA*, has not been explored. He had been Taylor's professor and was one of his dissertation advisors. On the one hand, Joyce (this volume) implies an antagonism between Taylor and Tozzer. Yet, on the other hand, Maca suggests there might actually have been a powerful alliance among Tozzer, Kluckhohn, and Taylor. This latter possibility actually gains support from the near invisibility of at least part of the triangle: Tozzer is nowhere cited or evaluated in *ASOA*. Maca's introductory chapter (this volume) suggests collusion, that is, that Tozzer was steadily if quietly supporting Taylor in his mission (see Maca's endnote regarding evidence of Tozzer's appreciation of Taylor). Clay, in his chapter, brings into play significant factors of a political and personal dimension that would enrich a conjunctive analysis, specifically, Taylor's position regarding the negative treatment of Jewish anthropologists, such as Boas and Sapir. Clay suggests, too, that Kluckhohn was Jewish. Although this may not have been the case, Kluckhohn's sympathy for the plight of Jewish scholars appears sincere.

What is the purpose of asking about all this? What are we to make of these crosscutting associations and intrigues? Returning to Reyman's original question, what is the significance that there was no backlash to Taylor's critique *before* publication? First, I simply think these historical connections help us to offer testable hypotheses regarding the intersections of sociopolitical affinities; such questions are vitally important to the history of the field yet are too frequently ignored or seem too risky or abstruse to pursue. Second, I believe that Reyman's question deflects the light of interrogation from Taylor, the usual suspect, directly toward one of the conjunctions of social and historical contexts in which the event of *ASOA* is embedded. Thus, it moves us out of the narrow confines of biography and influence-based histories of the great men of archaeology and toward a more fully anthropological historiography and historical ethnography. This moves us to ask what are and what underlies academic networks, how these map onto or intersect with institutions, and what is the sociopolitical place of archaeology in the world.

After all, our archaeology must be anthropological and historiographic. In the case of Mason and *ASOA*, no doubt we still lack data. But it is possible that the evidence lies buried somewhere in a rich cache of archival documents. Until those are found, or the topic at least more earnestly pursued, a scattering of surface lines is all we have to go on. No matter what, these affinities suggest that there is a lot we do not know—may not ever know—about the dynamism of sympathies, alliances, antagonisms, and politics that make up the on-the-ground networks of schools, disciplinary forms, and traditions. Whether in archaeology, history, or ethnography, we are left only to propose possible constructions of the past, possible constructs of "culture," and the mental-emotional motivations, intentions, and unintended effects of actors, agents, and their deeds.

## FEAR AND LOATHING

Taylor tells us that archaeology, history, and anthropology are each wrapped up in the goals of historiography; he argues that these are different ways of writing and constructing the past. Ethnography, we should remember, might be about the present, but it is always just another way of writing history. It is a mode of historiography just as archaeology is: both are constructions of the past and strive to understand culture change, conjunctions, and contexts. As a historical ethnography of archaeology, *Prophet, Pariah, and Pioneer* forcefully reiterates Taylor's themes as lessons to further develop. In so doing, this volume evokes the ethnography of archaeology as a path to pursue.

Following Leone, and based on my own experience studying the social and political context of archaeology in Mexico (Castañeda 1996), I would like to offer an observation. Archaeologists loathe the confrontation with the gap between the ideal image they have of themselves and their work and the actual image reflected in the mirror of critique offered by new theory, revised histories, or ethnographic studies of how archaeologists engage practically and theoretically with living communities and people. Ethnographers love to write books about the shortcomings of their work and discipline, but the majority of archaeologists, despite experimental studies in postmodern archaeologies, still are uncomfortable with the thought of ethnographic study of archaeology. Archaeologists tend to fear, or at least can be preoccupied by, becoming the subject of study of ethnography and ethnographers.

Kluckhohn (1940) no doubt set the precedent for this concern. In the festschrift for Tozzer, he offered a critical appraisal of the use of theory in American archaeology. Although he had training and had conducted research in archaeology, as was typical for his generation, his critique may have been viewed as external, from an outsider; thus, it was more or less ignored. In contrast, Taylor's assessment was unavoidable for several reasons, if also marginalized. One of these reasons is the way he framed his targets, as already noted by many authors and on which I comment below. Many have also noted that Taylor built on Kluckhohn's critique of theory in a way that continued and extended the "ethnographic commentary" on archaeology. In particular, Taylor's succinct sociopolitical history of archaeology (1948: Chapter 1) and the philosophical assertion of construction versus reconstruction of the past (Chapter 2) lay the groundwork for radical historicist descriptions and analysis of archaeology in its social contexts via ethnography.

From my reading, the only seemingly grand error Taylor made in *ASOA* is that his critical discussion named names. But Taylor did not make any ad hominem attacks. If he had added excessive praise or malicious denigration to every identification of a writer's shortcoming, that would have been ad hominem. Instead of asking if Taylor intended to personally vilify the six chieftains of American archaeology or asking what were his ulterior motives, perhaps we should be asking, how would I have written a thorough critical commentary on the work of colleagues? How can this be done successfully, what is fair game, and what tones of critique are acceptable and effective? The point here is that critical assessment is a necessary function of intellectual work and a routine dimension of scientific debate. However, archaeologists apparently are not always prepared to engage in this nor are they generally receptive to being subjects of such assessments. There is, at least historically speaking, an unwillingness to confront failings, risk reprisals, and open honest dialogues. Taylor clearly was willing and was not afraid to engage.

Another alternative to asking if Taylor's attack was ad hominem is to ask why neither Kidder nor any other of the Carnegie archaeologists responded at length to the critique and treatise? Was it a matter of character: no one was brave enough or man enough? Leone and Longacre (this volume) explain the extent of their response and show that it was not professional: "I witnessed the fights at meetings, the public displays, and heard the private conversations and assessments"; "[H]e was obviously blackballed by the establishment. . . . [I]t is equally clear that the senior members of the field misunderstood the importance and impact of Walt's contribution. At the time, they had little to say publicly." I think the lack of a direct professional response in print is relatively easy to understand: the second part of *ASOA,* in which he offered constructive guides for remaking archaeology, was too conceptually sophisticated for archaeologists of the day. How else can one understand the lack of published response? Anger at the critiques is not sufficient. Longacre confirms it with his statement: "I must confess that I did not understand the conjunctive approach at that time." Today, this silence appears to have been a concerted effort to convert Taylor's entire intellectual project (not just the critique) into a personal affront. An overly self-conscious and fearful response (and non-response) to mere critical assessment diminished the growth of legitimate discourse and prevented many from confronting the merits and weaknesses of their own work.

An example of this neutering maneuver is manifest in Fowler's conclusion (this volume) that the substance of Taylor's work was "hortatory": he asserts that the real value of Taylor was that he transmitted Hargrave's teachings to Americanist archaeology. Fowler is clearly unimpressed with Taylor's work and diminishes—even disparages—his contributions in Southwestern archaeology. Even a few of the sympathetic authors in this volume exercise a related mechanism: emphasizing ulterior, deep-seated, psychological motives in *ASOA* based on vengeance, on "getting back at" this or that academic clique, or else attributing an all-powerful causal agency to Taylor for slaying the Carnegie program. As Leone points out, every academic has personal, even emotional, reasons for choosing the research problems they do, and many of the volume's authors confirm this with their interest in Taylor's motives. This provides a justification for moving beyond psychoanalysis of archaeologists and for taking up the task of writing ethnographies of the sociopolitical, economic, and historical contexts of archaeology. Building on Leone's analysis of anger, we can state that archaeology's fear of ethnographers and ethnographies of archaeology is, on the one hand, actually a "fear of the mirror," not unlike the mirror offered by Taylor. On the other hand, this fear of ethnography is tied to a certain ignorance and an inclination to ignore; for example, there is a profound *lack of knowledge* about the real, lived, short- and long-term, sociopolitical and economic effects of archaeology in the world. We hardly know what archaeology does and what it consists of—in sociological terms—inside, much less outside, the trench, transect, lab, museum, classroom, and community.

Virtually all anthropological assessments (up to the present moment) of the effects and consequences of archaeology in society are ideologically driven (both pro and con), historically short-sighted, lack historical time depth, lack ethnographic grounding in rigorous sociological investigation, and/or reference an abstract level of disembodied politics.[10] Take, for example, Robert Redfield's (1950) and Morris Steggerda's (1941: 9–30) offhand speculation on the "impact" of Carnegie archaeology on the communities near Chichén Itzá, (Castañeda 1995, 1996, 2003). One community (Chan Kom) is memorialized in the anthropological record in part because Redfield claimed that archaeology motivated the Maya to "progress." The other town (Pisté) is blotted out of anthropological memory except as a culture-less community in part because Steggerda believed there were neither positive nor long-lasting effects of archaeology. Neither "assessment" has a strong claim to accuracy, and historical facts prove them both to be baseless ideas. The anthropology of archaeology, based in sustained and rigorous ethnographic study of archaeological research projects and their interactions with communities, has yet to emerge as a fully legitimate or even robust area of inquiry. The development of historical ethnographic studies of archaeology along diverse lines of inquiry, of which one possible course is presented in this volume, is urgent and necessary.

The disjunction between the lack of knowledge about the social role and consequences of archaeology and the profound desire that one's science do "good" triggers anxiety (or productive motivation) for archaeologists. The anxiety easily transforms into "fear and loathing" of ethnographers and the ethnography of archaeology since they could reveal serious blemishes. Taylor's book and *Prophet, Pariah, and Pioneer*, like Kluckhohn's study of archaeology, reveal an array of complex blemishes and provoke conjunctivitis. The fact is that the effects and practice of archaeology are not always good but are in fact always "good" and "bad" for different social actors and that for whom it is "good" or "bad" can change over time and according to circumstances. Furthermore, not all of the consequences of archaeology are directly caused by or result from the intentions of archaeologists and archaeological research. Many archaeologists feel simultaneously much too morally accountable and not ethically responsible enough. I suggest that the anthropology of archaeology can assuage rather than fuel the anxiety produced by the gaps that Leone, I, and others cite between, for example, archaeologists' ethics of social responsibility and our general lack of understanding archaeology's consequences and between their ideals of reconstruction and the fact that they can do little more than approximate past reality. By producing more ethnographies of these on-the-ground situations and the anger and uncertainty these may generate, the field of American archaeology will accelerate its fusion with anthropology, achieving that endlessly touted grandest of ideals and removing an albatross present for us all since the publication of Taylor's book. *Prophet, Pariah, and Pioneer* brings us infinitely closer to attaining these goals.

## ARCHAEOLOGICAL ARTIFACTS

The prism of light created by the double mirrors of this volume offers yet another, highly relevant, if also more obscured, reflection for us to observe. Consider the following comparison of our two archaeologists, Walter Taylor and Michel Foucault. In the introduction to *The Archaeology of Knowledge* (1973a [1969]), Foucault, the archaeologist, addresses his critics:

> No, no, I'm not where you are lying in wait for me, but over here, laughing at you. What do you imagine that I would take so much trouble and much pleasure in writing. . . . Do not ask who I am and do not ask me to remain the same: leave it to our bureaucrats and our police to see that our papers are in order. At least spare us their morality when we write. (Foucault 1973a: 17)[11]

Unlike Taylor, who produced a vision of archaeology but not an example of it, Foucault produced three studies using his archeological approach. Yet Foucault never provided a methodological treatise that explained "do X then Y." To complicate things for Foucault's critics (and followers), his exemplary models of archaeological history were not copies of each other that mimetically or mechanically reproduced the same analysis or the same methods. Rather, each study had crucial changes and shifts in focus, analytical framework, objects of study, problems, concepts, and goals. Of him, his naysayers demanded a singular, unitary programmatic statement on how to "do" his analytical methodology to resolve these "contradictions," as well as a statement that would clarify "once and for all" if he was a historian, philosopher, literary critic, or Marxist: "Are you or are you not a Marxist? Are you or are you not a structuralist?" They were looking for a recipe book and a signed testimony of allegiance to one or another established philosophical tradition. He tells them, laughing, that they should let the police—the bureaucrats and administrators, not the intellectuals and researchers—check to see if one's identification papers are in order. By setting aside the police work of thought, one can begin to think freely, openly, creatively, and productively on the intellectual tasks at hand. In his subsequent publications, Foucault began to develop a different approach ("genealogy"), showcased in *Discipline and Punish* (1977a [1975]). This increasingly focused on power, politics, and non-discursive social practices. This shift was developed by Foucault as a way to overcome the weaknesses, myopias, and dead ends of his earlier studies, all the while building from the essential tools and principles that he had developed in the earlier "archaeological" approach.

As for Walter Taylor, he did in fact provide a concise visionary statement of new theory and method. Yet his audience could not understand it and so demanded a demonstration. However, Taylor was not able to provide an exemplary model study or even devise a modified, more practical approach. Painfully aware at times of the thought police closing him up in a "prison-house of archaeology,"[12] Taylor at times ignored or hid from the patrols. However, he seems to have set himself up with the burden of having to hide because of the combination of his own intellectual drive to be rigorous and what seems today like his inability to recognize the limitations of his own context. Some of these inadequacies have been identified in this volume to include his era's lack of statistics and computer technologies, his own theoretical thinking, his personality, the fundamental atheoretical mentality of his colleagues, the brutality of academic gossip, the power of orthodoxy, and the sociopolitical demands of pushing a scientific paradigm in academia.

There is no doubt that *ASOA* is a brilliant piece of intellectual work that, regardless of statements that might suggest otherwise, charts a new vision of and for archaeology. Yet despite his conceptual and theoretical insight, Taylor was unable or unwilling to think through the problem of how to create a practically modified, tangible methodological manifestation of the conjunctive approach. One may ask, of course, to what extent was he actually interested in providing the exemplary guide to a paradigmatic approach? Some accounts, including his own (Taylor 2003) and certainly those of Reyman (1999), suggest that he did hope to achieve this. Yet, against the reiterated image of an albatross around his neck, there is often the image of Taylor repeatedly disappearing from the scene as he goes off on a canoe trip or hunting. Looking back in the mirror you can see the valiant and virtuous Reyman, wide-eyed and silent, his pulse racing as he observes his mentor, time after time, disengaging from and avoiding the Coahuila report.[13] For some reason, perhaps for one or many reasons mentioned here or elsewhere in this volume, Taylor was unable to think through ways to overcome the hurdles and troubles—intellectual, professional, and personal—strewn along his path.

Watson points out that he seemed definitely uninterested in paradigm-busting platforms, rallying and leading an avant-garde school of archaeology, or even addressing his critics' policing demands for a tangible example of what he actually intended. It is clear—clear even to those suffering from pinkeye—that Taylor was a pioneer but that he was not a promoter. Pioneer and prophet, Taylor may have been, but he was not a political boss of a new school, intellectual guru of a new disciplinary movement, or a visionary guide to an archaeological Shangri-La. But we should be aware that this does not mean he did not offer a paradigm. Or does it?

Certainly, over the last sixty years Taylor has been constructed as a pariah, but it seems we should stop reinventing him as such in the present. Let us get rid of the pinkeye. If we do, we could then announce Taylor's "death as author" (Barthes 1978) and begin instead to interrogate "him" as author-function in archaeology (Foucault 1977b),[14] that is, a landmark text to be revisited, reinterpreted, and resourced repeatedly and explicitly. Thankfully, this book goes a long way toward curing this conjunctivitis. Taylor and his contributions to the field stand as a monument of archaeology that still demands extensive excavation and reanalysis. This volume, indeed, is a monument to his archaeology.

Let us now leave Taylor the man alone. Let him be. Permit the following image to be a monument that his intangible heritage leaves us: his back turned, walking away to go hunting or fishing or maybe, with sandals kicked off, eating New England clams in a canoe with warm beer on a river somewhere in New Mexico's countryside. Listen. Listen closely and we might hear, rippling over the water and through the canyon, the laugh of the archaeologist and, if so, we may wonder which one—Taylor or Foucault? Whose laughter? Is it rich and fertile or swollen with bitterness and resentment? Maybe it comes not from that distant figure of our imagination but from inside us, in anger or delight, perhaps from one among us, here reading, who is still busy constructing new archaeologies.

## ACKNOWLEDGMENTS

I thank the editors, Allan L. Maca, Jonathan E. Reyman, and William J. Folan, for the invitation to participate in this volume. I especially appreciate Allan Maca for our intellectual exchanges, which lacked only a midnight kiva; Willie Folan, for talking to me long ago in Campeche about the Carnegie and Chichén; and Mark Leone, for his support and illuminating readings of my work, from which I have always gained new understandings.

## NOTES

1. This use resonates with a dominant trend among cultural anthropologists in which "culture" as a concept of holism capable of analyzing phenomena has been abandoned in favor of "cultural" as the working concept to identify types of issues and of forms of analysis.

2. A tangible and popular manifestation of this idea is present in the movie *The Matrix*. The matrix, despite its screen-monitor appearance as random computer code, is an arboreal structure in which everything is connected, systematized, totalized, and predetermined. When the human heroes seek to return to the free-floating, emotionally chaotic, rhizomatic world of humans, they must locate a telephone in a building abandoned (or not yet colonized) by the arboreal matrix machine. The telephone is literally an escape route from the machine; analytically, it is a line of flight from totalization. The black cat, as the glitch in the system that allows the matrix to track the location of the real human rebels, is also a line of flight.

3. Interestingly, the standard festschrift is marked by a manifest multiplicity and disjunction that borders on incongruence and incoherence. Yet, underlying the dispersal of the festschrift is the powerfully unifying, synthesizing allegory of the subject-author. It is this author-function as a singularized and totalizing intellectual that gives a festschrift its unity. *Prophet, Pariah, and Pioneer* extends the plurality and force of dispersion of the festschrift genre by stepping up the life history documents and by discarding the thematic and totalizing holism of the subject. There are *many* more Walter Taylors in this book than the three in the volume's title.

4. Rhizomatic thinking is characterized by disjunction, dispersal, and difference. In contrast, the model of the Tree of Knowledge makes arboreal thinking connected, hierarchical, and based on identity.

5. This is a multiple allusion that needs to be made explicit: Vincent Crapanzano's (1982) ethnographic life history of a Tuhumi is subtitled *Portrait of a Moroccan* and has on its cover the kind of cubist-surrealist image that comes to mind.

6. Although he may not be "a" structuralist, nor perhaps a linguistic structuralist or a social structuralist (as Longacre points out), structuralist logic and thinking is apparent in Taylor's *ASOA*.

7. In support of Taylor's synthetic "originality," Watson cites John Bennett's 1998 survey of classic anthropology as in agreement. What does it mean for that one confirmation and recognition of Taylor's genius to be published fifty years after *ASOA*? Practically, it means that this volume is still a necessary and timely contribution.

8. See Rutsch (2000), Harris and Sadler (2003), and a letter by Leslie Spier quoted in Price (2001).

9. Harris and Sadler (2003) identify Tozzer as a military intelligence agent. To my knowledge, no one has published anything about the nature or circumstances of his activities.

10. My own work also does not provide long-term historical cause and effect analysis. For example, Leone notes that I (QC) have not proven, nor argued, any cause-effect relationship. I argued that multiple forms and agents "of archaeology" have participated in and contributed to the creation of lived reality.

11. Also see De Certeau's analysis of this passage (1986: 193–198).

12. The reference is to Jameson (1975), who offers a Marxist structural-linguistic analysis of how thinking is constrained (imprisoned) by language. The comments in this chapter reference many structuralist and poststructuralist thinkers in large part because I believe there is an underlying "structuralism" (which is neither social nor linguistic) to Taylor's *ASOA* that demands greater excavation and understanding. I suspect that there are substantive theoretical-conceptual affinities between Taylor's conjunctive approach and Foucault's archaeology.

13. After reading this line, Reyman recalled that there was nothing valiant and virtuous about his response. Rather, he was pounding his head and fists on his desk.

14. By invoking Barthes's essay, I suggest that we let Taylor as author die a double death. This means, first, let us leave him, the person, alone; and, second, let us discard the negative constructions of Taylor the pariah. By invoking Foucault's essay, I suggest that we investigate the sociopolitical contexts of archaeology that constructed "the meaning" of both Taylor the author and the predominant interpretation of *ASOA*. These ideas lead to the recommendation of this volume, that we read more closely over and again the actual text of *ASOA*.

# EPILOGUE

*Editors' note.* The following are two sets of correspondence received by the senior editor. The first was written in June 2009 by Kevin McLeod, a producer and director in the field of visual media [mstrmnd ltd]. McLeod currently lives in New York City. He was born in Michigan and is a member of the Sault Ste. Marie Tribe of Chippewa Indians. The second involves a dialogue in March 2004 among participants in the 2003 SAA forum on Walter Taylor (see the preface to this volume). We include the segment of this exchange where Don Fowler, Rosemary Joyce, and George Gumerman (the elder) discuss the fate of the "Taylor papers."

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Dear Allan:

Thanks for passing along that draft of the Taylor book. I just finished it and then I rode the subway and realized I had just read a new genre of textual narrative. Maybe this quote from Erich Neumann[1] summarizes what I thought:

> When, at particular moments of emotional exaltation, or when the archetypes break through—that is, in extraordinary situations—there comes an illumination, a momentary uprising of consciousness, like the tip of an island breaking

> surface, a flash of revelation which interrupts the humdrum flow of unconscious existence. These isolated or habitual phenomena have always been regarded . . . as characterizing the "Great Individual" who, as medicine man, seer, and prophet or later as the man of genius, possesses a form of consciousness different from the average. Such men are deemed as "godlike," and their insights, whether they take the form of visions, maxims, dreams or revelations vouchsafed by an "apparition," lay the first foundations of culture.

I hate hubris but the tone is just right and the thing, your book, gnawed me alive. (I also hate to admit, since I think the abstract expressionists are a blank, but Clement Greenberg once said, "All masterpieces are ugly at first." And of course he's wrong, but here he's right.) I was bored initially; then I became confused. The book grew on me. Its tools are plain, unhidden; the only wooden areas are graduate reminiscing that you might reject at first and then realize is as complex as vanguard thinking by Patty Jo (holy m), who liminally employs the word "primate" in her chapter and expands the underneath of archaeology. Too many incredible details are *not* overwritten, as I thought at first but later realized are actually in distinct voices. You can even hear the conversations with Taylor blending from other chapters' voices in *parallel*, a construction more akin to fiction or documentary film. Then you have Quetzil's effort at pushing semiotics as a poststructural retroaction. In a sense, his chapter was the last hope to pool significance the way almost every single narrative in human history does or attempts, to send us home completed. But his failure is also his success (and is the result of this book's genius). Like a sheriff at the end of a Western, Q struts in to finish the job with six cartridges. Aiming wildly for the half-dozen targets he saw walking through the book and plugging archeologists with sarcasm, romanticism, nods to postmodernism, raw text, even cubism, he coils his multitude of devices around the word "conspiracy" and ultimately snipes Taylor through a Spies-Like-Us approach: *the spy narrative eats archeology alive*, starring a bunch of highfalutin' Harvard and Yale boys, snickering as they conquer the West backward during World War II and the West forward all throughout the twentieth century. Yet Taylor knew the game is controlled by the writer of the history, not the culture or persons gazed upon. He wanted the controller to know itself and its game, the tools, the age, and the language. Quetzil gets this, but ultimately his move toward closure—his retroactions, allowing Taylor his watery paradise and beer, granting the author a death and us a release—cannot get us far enough. We still need Taylor; we can't let him go because no one else individually is going to help us comprehend your book's innovation in narrative. That's the point: no one is going to "get this book"; no one quite gets Taylor's. Especially not archeologists (or anthropologists or ethnographers) who are too conditioned to step back and employ Taylor to examine Taylor (or his *ASOA*).

Paradoxically, Quetzil is the most current thinker in the book and the most habituated despite (or *because* he's trying Taylor's approach with diffused results) using the greatest typological possibilities, the largest number of analytical categories. Thus, the most potentially Taylorean chapter (Q is one of the few who even attempts to employ theory) suffers the same dissonance it identifies: the lens turns unreflexive since the keys are obviously made by European practitioners of arts like cubism and semiotics (itself on a plateau made of text and its alphabets, something Taylor had already moved past, demanding we question even the notion of summarizing anything in such linear construction/thinking). Foucault's slack premeditation, preparing for his dismissal by his critics by laughing at them, is a stance I was somehow sure Taylor would have taken, but he didn't. Taylor's critical, characteristic nuances included a guilelessness of certainty that his message was instrumental and could stand a test of time. And therefore your book here is built out of Taylor's stoicism. It's not even a myth; it's real. Quetzil's parading of parallels, *structural* opposites (without explaining to us just how opposite they are) like *ASOA* and Foucault's *Archaeology of Knowledge*, left me puzzled, but I had already succumbed to the deeper probe your book offers and Q signals: the continual argument over Taylor exists because his theorem predates the vast majority of the structures employed to study his work. Taylor goes for a knowledge that is self-informed and self-aware on all levels.

You, Jonathan, and Willie have assembled a book that will never be complete. The coup de grace is the lack of finality—perfect that it ends with a shot at completion, with the sort of theory that could least possibly comprehend what the Taylor theorem was, if it even existed for any of the authors (it didn't). The future vantage to interpret Taylor is neurophenomenology, the *real* primordial soup for knowledge, since in the end we are talking about human consciousness. Employ Taylor properly and we keep going down the rabbit hole. Would it stop at linguistics, at semiotics, at text? No. From *ASOA* to the classroom, Taylor keeps us vaguely aware that definitions and language *are the problems*. He encourages us beyond these structures. And it's appropriate that your book does as well. The empirical memory cycles—the raw texts, the recalibrated recollections, the spite—that thread *PPP* take us beyond the text, to something deeper, more basic, more conscious and self-aware. You guys have allowed these without the clouding of schools of thought or even selective editing; they become isomorphic, framing not simply what minds the memories came from, but the mind substrate they all came from: these archetypal memories are downrange to the brain's structure as the locale of consciousness unfolding, integrating, and dispersing.

Your book breeds the conjunctive manner, primordially, in the overlapping anecdote, the multiple POV definitions, in a form of media that we recognize primarily employed by a motion media. The *plot* of your book is so reduced that it easily reveals archetype—journey, attack, defeat, hermeticism—even in narrative structures as short as six pages. My first reaction was that this story is a movie. And I realized it was my first Taylorean move, albeit unconsciously. As I began to notice the *Rashômon* quality, my first reaction was annoyance ("too simple"), but then after a few more chapters I was swayed by the stark complexity and kinship with *Rashômon*, an entirely beyond-structural tale about multiple variations of the same conflict, derived from a shrunken technique inside *Citizen Kane* in which the shot structure and visual framing were as key to the film as the retold narrative seemed to be. How you perceived (or had perceived for you) these simple gestures, these characterizations, these definitions that keep reappearing (that led to a good ten or twelve words [words *like* conjunctive] and a few conflicts to conduct the plot with) expose the structure as not simply text narrative; it's a form of dream state (cinema). When your book's system seeps in, it avoids the neatness of recent adventurers (e.g., Taussig), the clinical mojo of anecdotal biographies, and the diffuseness of Festschriften and begins generating more questions than answers. Our only real dilemma (the "treasure") was why Taylor even attempted practicing archeology after 1948 since really he was done; he had dropped his golden bombshell, a complex of strategies that even *he* didn't know the extents of. Is *ASOA* even a book? Is *Prophet, Pariah, and Pioneer* even a book? If *PPP* is akin to *Rashômon*, the ultimate Taylorean film is *Ghostbusters*—a transformation from disbelief to belief occurs through a new conjunctive science.

And once we go back and take Taylor for all he's got, aren't we missing some basic connections? It is unusually striking that Kluckhohn was a primary inspiration for Taylor because certain elements of Navajo knowledge, the basis for Kluckhohn's complex ethnographic research (that Taylor was witness to), are likely intertwined with Taylor's own process in *ASOA*, another reference Taylor apparently omitted; or perhaps he didn't know where the reference emerges since he, in effect, leapfrogs over his tutor. Language and knowledge were paramount to Taylor and linked to Clyde Kluckhohn's most basic teachings. Recall where CK works to explain vital Navajo concepts: "[T]he difficulty with translation primarily reflects the poverty of English in terms that simultaneously have moral and esthetic meaning."[2] Exploring the influence on Taylor of Navajo epistemology and ontology no doubt would be a vertically challenged next chapter for this book's future editions. I'm also sure that a few cog scientists, ethnobotanists, even comparative religion candidates will be licking their chops after reading your experiment. Moreover, following on Quetzil's and Mark's metaphors, I think the most vital mirrors in here are the ones structured in *time*, the book's unequivocal mastery of things like the moment Taylor realizes his data don't support the conjunctive approach when arrayed via software versus the proofs the other archeologists gain by employing Taylorean conjunctions (systems). And as the man driven west, in the golden age of cinema, dubbing commercial films into Spanish while living in Mexico, employing conversationally a Spanish accent reminiscent of Mexico's famous comedic actor Cantinflas (which would be like me using a Serge Gainsbourg accent in Paris—it takes bizarre wit to employ it), Taylor gains modern archetype access, a polyvalent polyglot polymath of exceptional genetic terminuses like orchids and oysters and pheasant (grown on site, flown to site, killed on site) backdropped by a library rivaling a university's. Not only is he "a movie" but the narrative lens here is more in the spirit of a movie than a tried text narrative: the book. In effect, this tale, this book, is the primer for a more complex look at Taylor and at his own pre-Socratic-through-Navajo jab into the "formula" of paradox; whereas theory breeds formula, which breeds proof, which breeds a—or the—science.

Best,
Kevin

## NOTES

1. E. Neumann, *The Origins and History of Consciousness* (1962: 286).

2. This is Kluckhohn (1968: 686) on Navajo philosophy. Taylor (1948) references Kluckhohn's (1941) paper "Patterning as Exemplified in Navaho Culture" in the edited book *Language, Culture, and Personality* (Menasha, WI: Spier, Hallowell, and Newman, 1941).

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*From D. Fowler:*

Dear All,

I can't help on who signed the diss., but I can testify to the value of Taylor's papers in the NAA. After I shipped off a draft of my paper on Taylor and his Pueblo Ecology Study to Allan, I got wondering about the source of the "Southwest Archaeological Research Fund." Rob Leopold arranged to have copies of all the Taylor correspondence, field notes, etc. relating to the project copied and sent to me—300+ pages. It's quite clear that the fund was Taylor's own money, laundered through the Smithsonian. He had apparently done the same thing with some of the funds for his Coahuila work (see his intro to his sandals paper recently out from Dumbarton Oaks—congrats to Patty Jo Watson and her colleagues on that). George Gumerman and I have corresponded about the funds and agree he was doing it as a tax write-off—perfectly legit; still done today. The field notes are very rich—almost all by William Y. Adams, who did basically all the fieldwork for the Pueblo Ecology study. Rob says that Taylor's papers were sent from Harvard to NAA because Harvard felt Taylor's connection with Harvard was not "strong enough" to warrant their keeping them. *Sic transit gloria mundi*, or something like that.

Good cheer,
Don

*From R. Joyce:*

As it happens, I know precisely the circumstances of this transfer because it happened when I was first at the Peabody. I am not sure, in retrospect, if it was during the first nine months when I was Assistant Curator, and no one was Assistant Director, or just after I was appointed Assistant Director to fill the position left vacant by Garth Bawden; but either way, I was consulted by the woman who was then the collections manager about the Taylor papers.

These were among a large volume of papers at that time stacked inaccessibly in the closed Hall of the North American Indian. The gallery had been closed in the early 1980s to provide storage space for other rooms in the museum that were being remodeled to provide modern storage spaces. The museum was about to embark on its remodeling of the North American Indian gallery, and everything in the gallery had to be moved out to somewhere else while that happened. Vicky Swerdlow, the collections manager, wanted to show me the latex rubber casts and paper molds that were from Maya archaeological sites ca. 1900 ± 20 years.

Once I had expressed my opinion about these Maya materials, Vicky began showing me some of the other things that were in the gallery space for which no one around was responsible. Among the boxes were the Taylor papers, which led me to say to her that these were incredibly important. She told me that they did not fall within the Harvard archives' scope of collecting, since Taylor had never worked for the institution. The Peabody itself, at that time, had no archives space, archivist, or separate collection strategy (other than keeping documents directly related to collections). I said at the time that she should see if the National Anthropological Archives would be interested in Taylor's papers, given the significance of the person to the discipline (if not to Harvard history).

So, just to say that there are multiple ways to read a text: the argument that the connection to Harvard was not strong enough (which was the reason why there was no way to integrate them in Harvard archives) was only half of the story. And given the relative ease of access to archives at NAA versus the Peabody, and the professional work that NAA is doing on the papers, I think they found a far more appropriate resting place there.

*From G. Gumerman:*

I was at SIU when Taylor went back to the Peabody about a year after he had donated his archives to the Peabody. He returned to Carbondale from the Peabody extremely upset. He wanted to get some data about the Coahuila caves from the collection and found that nothing had been done with the collection and they still rested in the boxes he had brought them in.

George

# REFERENCES


Adair, J.

1944 *The Navajo and Pueblo Silversmiths*. University of Oklahoma Press, Norman.

Adams, R.E.W.

2005 *Prehistoric Mesoamerica*. University of Oklahoma Press, Norman.

Adams, W. Y.

1951 *Pueblo Ecology Survey*. Taylor Papers, Box 20, National Anthropological Archives. Smithsonian Institution, Washington, DC.

1953 Archaeology and Culture History of the Navajo Country, Report on Reconnaissance for the Pueblo Ecology Study, 1951. Or, Kidder Up to Date: A Reintroduction to the Study of Southwestern Archaeology. Unpublished ms. Museum of Northern Arizona, Flagstaff.

1960 Ninety Years of Glen Canyon Archaeology, 1869–1959. *Museum of Northern Arizona Bulletin* 33 (Glen Canyon Series no. 2).

1963 Shonto: A Study of the Role of the Trader in a Modern Navaho Community. *Bureau of American Ethnology Bulletin* no. 188.

1984 *Nubia: Corridor to Africa*. Princeton University Press, Princeton, NJ.

1998 *The Philosophical Roots of Anthropology*. CSLI Publications, Stanford, CA.

2003 Letter regarding Pueblo Ecology Study, ed. D. D. Fowler, personal communication, Lake Tahoe, CA.

2004 *Religion and Adaptation*. CSLI Publications, Stanford, CA.

2009 *The Road from Frijoles Canyon: Anthropological Adventures on Four Continents.* University of New Mexico Press, Albuquerque.

Adams, W. Y., and N. K. Adams

1959 Inventory of Prehistoric Sites on the Lower San Juan River, Utah. *Museum of Northern Arizona Bulletin* 31 (Glen Canyon Series no. 1).

Adams, W. Y., J. Lindsay, J. Alexander, and C. G. Turner II

1961 Survey and Excavations in Lower Glen Canyon, 1952–1958. *Museum of Northern Arizona Bulletin* 36 (Glen Canyon Series no. 3).

Adovasio, J. M.

2004 Review of *Sandals from Coahuila Caves,* by W. W. Taylor. *Journal of Anthropological Research* 60(4):607–609.

Aguilar, J. E., F. Julian, and W. J. Folan

1981 *La Red de Comunicaciones y Rutas de Comercio. Apéndice II.* Turismo del Estado de México, Toluca.

Anaya Hernandez, A., S. P. Guenter, and M. U. Zender

2003 Sak Tz'i', a Classic Maya Center: A Locational Model Based on GIS and Epigraphy. *Latin American Antiquity* 14(2):179–191.

Anderson, B.

1996 *Imagined Communities, Reflections on the Origin and Spread of Nationalism*. Verso, London.

Andrews, E. W., IV

1942 The Inscription of Stela 38, Piedras Negras, el Peten, Guatemala. *American Antiquity* 7:364–368.

Andrews, E. W., IV, and E. W. Andrews V

1980 *Excavations at Dzibilchaltun, Yucatan, Mexico.* Tulane University, New Orleans.

Andrews, R., M. Biggs, and M. Seidel

1996 *The Columbia World of Quotations*. Columbia University Press, New York.

Aoyama, K.

1995 Microwear Analysis in the Southeast Maya Lowlands: Two Case Studies at Copan, Honduras. *Latin American Antiquity* 6(2):129–144.

Ardren, T.

2009 Twenty-First-Century Reinventions of Alexander, Xerxes, and Jaguar Paw: A Critique of *Apocalypto* and Popular Media Depictions of the Past. *Archaeological Review from Cambridge* 24(1):149–158.

Arnold, B.

1990 The Past as Propaganda: Totalitarian Archaeology in Nazi Germany. *Antiquity* 64:464–478.

Arratia, L. G.

2008 Contributions of Walter W. Taylor to the Archaeology of Coahuila, 1937–1947. In *Archaeology without Borders: Contact, Commerce, and Change in the*

*U.S. Southwest and Northwest Mexico*, ed. L. D. Webster and M. E. McBrinn, 373–383. University Press of Colorado, Boulder.

Ashmore, W.
1991 Site-Planning Principles and Concepts of Directionality among the Ancient Maya. *Latin American Antiquity* 2(3):199–226.
2007 Legacies of Gordon Willey's Belize Valley Research. In *Gordon R. Willey and American Archaeology: Contemporary Perspectives*, ed. J. A. Sabloff and W. L. Fash, 41–60. University of Oklahoma Press, Norman.

Ashmore, W. (editor)
1981 *Lowland Maya Settlement Patterns*. University of New Mexico Press, Albuquerque.

Balter, M.
2005 *The Goddess and the Bull: Çatalhöyük, An Archaeological Journey to the Dawn of Civilization*. Free Press, Simon and Schuster, New York.

Barnhart, E. L.
2001 The Palenque Mapping Project. Ph.D. dissertation, Department of Anthropology, University of Texas, Austin.
2007 Indicators of Urbanism at Palenque. In *Palenque: Recent Investigations at the Classic Maya Center*, ed. D. B. Marken, 107–121. Altamira, Lanham, UK.

Barthes, R.
1978 *The Death of the Author*. Hill and Wang, New York.
1984 *Le Bruissement de la Langue*. Le Seuil, Paris.

Bayard, D. T.
1969 Science, Theory, and Reality in the "New Archaeology." *American Antiquity* 34(4):376–384.

Beals, R. L., G. W. Brainerd, and W. S. Smith
1945 Archaeological Studies in Northeast Arizona. *University of California, Publications in American Archaeology and Ethnology* 44(1):1–236.

Becker, M.
1979 Peasants, Priests, and Ceremonial Centers: The Intellectual History of a Model. In *Maya Archaeology and Ethnohistory*, ed. N. Hammond, 3–20. University of Texas Press, Austin.

Beliaev, D., and A. V. Safronov
2002 Kanal Kings in Quintana Roo. Unpublished ms.

Bell, E. E., M. A. Canuto, and R. J. Sharer (editors)
2004 *Understanding Early Classic Copan*. University of Pennsylvania Museum, Philadelphia.

Belmont, J. S., and S. Williams
1965 *The Foundations of American Archaeology*. Peabody Museum, Harvard University, Cambridge, MA.

Benedict, R.
1934 *Patterns of Culture*. Houghton Mifflin, New York.

Benet, S. V.
1928 *John Brown's Body*. Doubleday, Doran, and Company, Garden City, NY.

Benjamin, W.
1968 The Work of Art in the Age of Mechanical Reproduction. In *Illuminations*, ed. H. Arendt, 217–252. Harcourt Brace Jovanovich, New York.
1978 *Reflections: Essays, Aphorisms, Autobiographical Writings*. Harcourt Brace Jovanovich, New York.
2002 *The Arcades Project*. Belknap Press, Cambridge, MA.

Bennett, J. W.
1943 Recent Development in the Functional Interpretation of Archaeological Data. *American Antiquity* 9:208–219.
1998 *Classic Anthropology: Critical Essays, 1944–1996*. Transaction, New Brunswick, NJ.

Bennett, W. C.
1934 Excavations at Tiahuanaco. *Anthropological Papers of the American Museum of Natural History* 34(3):359–494.

Bernal, I.
1962 *Bibliografia de Arqueologia y Etnografia: Mesoamerica y Norte de Mexico: 1514–1960*. Memorias vol. 7. Instituto Nacional de Antropología e Historia, Mexico City.

Bernal, M.
1987 *Black Athena: The Afroasiatic Roots of Classical Civilization*. Rutgers University Press, New Brunswick, NJ.

Bernheimer, C. L.
1923 Encircling Navajo Mountain with a Pack-Train. In *National Geographic Magazine* 43:197–224.
1924 *Rainbow Bridge: Circling Navajo Mountain and Exploration in the "Bad Lands" of Southern Utah and Northern Arizona*. Doubleday Page, New York.

Bhabha, H.
2002 [1994] *The Location of Culture*. Routledge, London.

Binford, L. R.
1962 Archaeology as Anthropology. *American Antiquity* 28(2):217–225.
1965 Archaeological Systematics and the Study of Culture Process. *American Antiquity* 31(2):203–210.
1967 Smudge Pits and Hide Smoking: The Use of Analogy in Archaeological Reasoning. *American Antiquity* 32(1):1–12.
1968a Archaeological Perspectives. In *New Perspectives in Archaeology*, ed. S. R. Binford and L. Binford, 5–32. Aldine Publishing Company, Chicago.
1968b Methodological Considerations of the Archaeological Use of Ethnographic Data. *Man the Hunter*, ed. R. B. Lee and I. DeVore, 268–273. Aldine de Gruyter, Chicago.

1968c Some Comments on Historical Versus Processual Archaeology. *Southwestern Journal of Anthropology* 24(3):267–275.
1972 *An Archaeological Perspective*. Seminar Press, New York.
1981 Behavioral Archaeology and the "Pompeii premise." *Journal of Anthropological Research* 37(3):195–208.
1983a *In Pursuit of the Past: Decoding the Archaeological Record*. Thames and Hudson, New York.
1983b Pompeii Premise in Archaeology. In *Working at Archaeology*, ed. L. R. Binford. Academic Press, New York.
1983c *Working at Archaeology*. Academic Press, New York.
1989 *Debating Archaeology*. Academic Press, San Diego.
2001 Where Do Research Problems Come From? *American Antiquity* 66(4):669–678.

Binford, L. R., S. R. Binford, R.C.J. Whallon, and M. A. Hardin
1966 Archaeology at Hatchery West, Carlyle, Illinois. *Southern Illinois University Museum Archaeological Salvage Report* 25.

Binford, S. R., and L. R. Binford (editors)
1968 *New Perspectives in Archaeology.* Aldine Publishing Company, Chicago.

Blackburn, F. M., and R. A. Williamson
1997 *Cowboys and Cave Dwellers. Basketmaker Archaeology in Utah's Grand Gulch*. School of American Research, Santa Fe, NM.

Blommaert, J., and C. Bulcaen
2000 Critical Discourse Analysis. *Annual Review of Anthropology* 29:447–466.

Boas, F.
1896 The Limitations of the Comparative Method of Anthropology. *Science* 4:901–908. Macmillan, New York.

Bolles, J.
1932 Field Notes from Excavations at Chichen Itza. Peabody Museum Archives. Harvard University, Cambridge, MA.

Bourdieu, P.
1977 *Outline of a Theory of Practice*. Cambridge University Press, Cambridge.
1986 The Forms of Capital. In *Handbook of Theory and Research for the Sociology of Education*, ed. J. G. Richardson, 241–258. Greenwood Press, New York.

Breglia, L.
2006 *Monumental Ambivalence: The Politics of Heritage*. University of Texas Press, Austin.

Brew, J. O.
1946 *Archaeology of Alkali Ridge, Southeastern Utah: With a Review of the Prehistory of the Mesa Verde Division of the San Juan and Some Observations on Archaeological Systematics*. Peabody Museum, Cambridge, MA.
1968 *One Hundred Years of American Anthropology*. Harvard University Press, Cambridge.

Buck-Morss, S.
1991 *The Dialectics of Seeing*. MIT Press, Cambridge, MA.

Buikstra, J., K. Miller, and L. Wright
2009 Robert Sharer and the Conjunctive Approach: A Bioarchaeological Perspective. Paper presented in the invited session Understanding Maya Civilization: Papers in Honor of Robert Sharer at the 74th Annual Meeting of the Society for American Archaeology, Atlanta, Georgia.

Burchell, G., C. Gordon, and P. Miller
1991 *The Foucault Effect*. Wheatsheaf Harvester, London.

Burgh, R. F.
1950 Comment on Taylor's *A Study of Archeology*. *American Anthropologist* 52: 114–119.

Butler, M.
1931 Dress and Decoration of the Maya Old Empire. *The Museum Journal* 22:155–183.

Caldwell, J. R.
1959 The New American Archeology. *Science* 129(3345):303–307.
1964 Interaction Sphere in Prehistory. In *Hopewellian Studies*, ed. J. R. Caldwell and R. L. Hall, 12:133–143. Illinois State Museum Scientific Papers, Springfield.

Callaghan, J.
1987 *Time and Chance*. Collins, London.

Canuto, M. A., and W. L. Fash
2004 The Blind Spot: Where the Elite and Non-Elite Meet. In *Continuity and Change in Maya Archaeology: Perspectives at the Millennium*, ed. C. Golden and G. Borgstede, 51–76. Routledge, New York.

Canuto, M. A., R. J. Sharer, and E. E. Bell
2004 Understanding Early Classic Copan: A Classic Maya Center and Its Investigation. In *Understanding Early Classic Copan*, ed. E. E. Bell, M. A. Canuto, and R. J. Sharer, 1–14. University of Pennsylvania Museum of Archaeology and Anthropology, Philadelphia.

Carmack, R. M., and J. M. Weeks
1981 The Archaeology and Ethnohistory of Utatlan: A Conjunctive Approach. *American Antiquity* 46(2):323–341.

Castañeda, Q. E.
1995 The Progress that Chose a Village: Measuring Zero-Degree Culture and the Impact of Anthropology. *Critique of Anthropology* 15(2):115–147.
1996 *In the Museum of Maya Culture: Touring Chichen Itza*. University of Minnesota Press, Minneapolis.
2003 New and Old Social Movements. *Ethnohistory* 54(4):611–642.
2005 The Carnegie Mission and Vision of Science. *Histories of Anthropology Annual* 1:27–60.

Castañeda, Q. E. and C. N. Matthews (editors)
2008 *Ethnographic Archaeologies: Reflections on Stakeholders and Archaeological Practices.* AtlaMira Press, Lanham, MD.

Chamberlin, T. C.
1965 The Method of Multiple Working Hypotheses. *Science, New Series* 148(3671): 754–759.

Chang, K. C.
1958 Study of the Neolithic Social Grouping: Examples from the New World. *American Anthropologist* 60:298–334.
1962 A Typology of Settlement and Community Patterns in Some Circumpolar Societies. *Arctic Anthropology* 1(1):28–41.
1967 *Rethinking Archaeology*. Random House, New York.
1968 *Settlement Archaeology*. National Press Books, Palo Alto, CA.

Chase, D. Z., and A. F. Chase
1996 Maya Multiples: Individuals, Entries, and Tombs in Structure A34 of Caracol, Belize. *Latin American Antiquity* 7(1):61–79.
2009 Multiple Hats: Conjunctively Researching the Ancient Maya. Paper presented in the invited session Understanding Maya Civilization: Papers in Honor of Robert Sharer at the 74th Annual Meeting of the Society for American Archaeology, Atlanta, Georgia.

Childe, V. G.
1926 *Dawn of European Civilization*. Knopf, New York.
1950 The Urban Revolution. *The Town Planning Review* 21(1):3–17.

Christenson, A.
1989 The Past Is Still Alive: The Immediacy Problem and Writing the History of Archaeology. In *Tracing Archaeology's Past: The Historiography of Archaeology*, ed. A. Christenson, 163–168. Southern Illinois University Press, Carbondale.

Clancy, F. S.
1994 The Classic Maya Ceremonial Bar. *Anales de Instituto de Investigaciones Estéticas* 65:7–45.

Clark, G.
1939 *Archaeology and Society*. Methuen and Company, London.
1940 *Prehistoric England*. Batsford, London.
1954 *Excavations at Star Carr*. Cambridge University Press, Cambridge.

Cleere, H.
1989 *Archaeological Heritage Management in the Modern World*. Unwin Hyman, London.

Coe, M. D.
2006 *Final Report: An Archaeologist Excavates His Past*. Thames and Hudson, New York.

Coggins, C. C.
1980 The Shape of Time: Some Political Implications of a Four-Part Figure. *American Antiquity* 45(4):727–739.

Cole, F.-C., and T. Deuel
1937 *Rediscovering Illinois*. University of Chicago Press, Chicago.

Collins, H. B., Jr.
1937 Archaeology of St. Lawrence Island, Alaska. *Smithsonian Institution Miscellaneous Collections* 96(1). Smithsonian Institution, Washington, DC.

Colton, H. S.
1956 Potsherds: An Introduction to the Study of Prehistoric Southwestern Ceramics and Their Uses in Historic Reconstruction. *Museum of Northern Arizona Bulletin* 25.

Conkey, M. W., and J. Spector
1984 Archaeology and the Study of Gender. In *Advances in Archaeological Method and Theory*, ed. M. B. Schiffer, 1–38. Academic Press, New York.

Cook, S. F., and R. F. Heizer
1968 Relationships among Houses, Settlement Areas, and Population in Aboriginal California. In *Settlement Archaeology*, ed. K. C. Chang, 79–116. National Press Books, Palo Alto, CA.

Cordell, L. S.
1997 *Archaeology of the Southwest*. 2nd ed. Academic Press, San Diego.

Cordell, L. S., and D. D. Fowler (editors)
2005 *A Century of Southwest Archaeology*. University of Utah Press, Salt Lake City.

Crampton, C. G.
1959 Outline History of the Glen Canyon Region, 1776–1922. *University of Utah Anthropological Papers* 42(Glen Canyon Series 9).

Crapanzano, V.
1982 *Tuhami: Portrait of a Moroccan*. University of Chicago Press, Chicago.

Crotty, H. K.
1983 *Honoring the Dead: Anasazi Ceramics from the Rainbow Bridge–Monument Valley Expedition*. Monograph Series 22. UCLA Museum of Cultural History, Los Angeles.

Crown, P. L.
1994 *Ceramics and Ideology: Salado Polychrome Pottery*. University of New Mexico Press, Albuquerque.

Culbert, T. P.
2004 Continuities and Changes in Maya Archaeology: An Overview. In *Continuities and Changes in Maya Archaeology: Perspectives at the Millennium*, ed. C. W. Golden and G. Borgstede, 311–320. Routledge, New York.

Culbert, T. P. (editor)
1991 *Classic Maya Political History: Hieroglyphic and Archaeological Evidence*. Cambridge University Press, Cambridge.

Dahlin, B. H., J. E. Foss, and M. E. Chambers
1980 Project Acalches: Reconstructing the Natural and Cultural History of a Seasonal Swamp at El Mirador, Guatemala; Preliminary Results. *New World Archaeological Foundation Papers* 45:37–57.

Daniel, G. E.
1950 *A Hundred Years of Archaeology*. Gerald Duckworth, London.
1951 Review of *A Study of Archeology*, by W. W. Taylor. *Man* 139:82–83.

Dark, P.J.C.
1994 *In Captivity: Images from World War II.* Honolulu Academy of Arts, Honolulu.
2002 Persistence, Change and Meaning in Pacific Art: A Retrospective View with an Eye Towards the Future. In *Pacific Art, Persistence, Change, and Meaning*, ed. A. Herle, N. Stanley, K. Stevenson, and R. L. Welsch, 13–40. University of Hawaii Press, Honolulu.
N.d. Look Back This Once: Prisoner of War in Germany in WWII. Copy on file at Imperial War Museum (Department of Documents), London.

Darnell, R.
2001 *Invisible Genealogies: A History of Americanist Anthropology*. University of Nebraska Press, Lincoln.

De Certeau, M.
1986 *Heterologies*. University of Minnesota Press, Minneapolis.

Deetz, J.
1965 *The Dynamics of Stylistic Change in Arikara Ceramics*. University of Illinois Series in Anthropology 4. University of Illinois Press, Urbana.
1972 Archaeology as a Social Science. In *Contemporary Archaeology: A Guide to Theory and Contributions*, ed. M. P. Leone, 108–117. Southern Illinois University Press, Carbondale.
1988 History and Archaeological Theory: Walter Taylor Revisited. *American Antiquity* 53(1):13–22.
1989 Archaeography, Archaeology, or Archeology? *American Journal of Archaeology* 93(3):429–435.

Deleuze, G., and F. Guattari
1987 *Thousand Plateaus*. University of Minnesota Press, Minneapolis.

Deloria, V.
1970 *Custer Died for Your Sins: An Indian Manifesto*. Avon Press, New York.

Demarest, A. A.
1990 Archaeology, Ideology, and Pre-Columbian Cultural Evolution: The Search for an Approach. In *Ideology and Pre-Columbian Civilizations*, ed. A. A. Demarest and G. W. Conrad, 1–13. School of American Research, Santa Fe, NM.

Demerath, N. J., M. C. Kennedy, and P. J. Watson
2003 Foreword. In *Sandals from Coahuila Caves,* W. W. Taylor, x–xi. Dumbarton Oaks Research Library and Collection, Washington, DC.

De Montmollin, O.
1989 *The Archaeology of Political Structure: Settlement Analysis in a Classic Maya Polity*. Cambridge University Press, Cambridge.

Dewhirst, J. T.
1980 The Indigenous Archaeology of Yuquot, an Outside Nootkan Village. In *The Yuquot Project*, ed. W. J. Folan and J. Dewhirts, 1:1–358. National Historic Parks and Sites Branch, Parks Canada, Environment Canada, Ottawa.

Dincauze, D.
1990 A Good Product Mislabeled: Review of *Archaeological Thought in America*. *Current Anthropology* 31(20):221.

Domínguez Carrasco, M.d.R., and W. J. Folan
2001 Cerámica, Figurillas y Litica. In *Las Ruinas de Calakmul, Campeche, México: Un Lugar Central y Su Paisaje Cultural*, ed. W. J. Folan, L. Fletcher, J. May Hau, and L. M. Florey Folan, 67–74. Coordinadores y colaboradores. Universidad Autónoma de Campeche, Campeche.

Donne, J.
1959 [1624] *Devotions upon Emergent Occasions*. Ann Arbor Paperbacks. University of Michigan Press, Ann Arbor.

Dowson, T. A.
1998 Homosexuality, Queer Theory and Archaeology. *Cota Zero* 14:81–87.

Dozier, E. P.
1964 The Pueblo Indians of the Southwest: A Survey of the Anthropological Literature and a Review of Theory, Method, and Results. *Current Anthropology* 5(2):79–97.

Dunnell, R. C.
1986 Five Decades of American Archaeology. In *American Archaeology Past and Future*, ed. D. J. Meltzer, D. D. Fowler, and J. A. Sabloff, 23–49. Smithsonian Institution Press, Washington, DC.

Dunning, N., D. J. Rue, T. Beach, A. Covich, and A. Traverse
1998 Human-Environmental Interactions in a Tropical Watershed: The Paleoecology of Laguna Tamarindito, El Peten, Guatemala. *Journal of Field Archaeology* 25(2):139–151.

Dütting, D.
1970 On the Inscription and Iconography of Kuná-Lacanhá Lintel 1. *Zeitschrift für Ethnologie* 95(2):196–219.

Earle, T. K., and R. W. Preucel
1987 Processual Archaeology and the Radical Critique. *Current Anthropology* 28(4):501–538.

Euler, R. C.
1997 Walter Willard Taylor, Jr., 1913–1997. *Society for American Archaeology Bulletin* 15(4):23.

Euler, R. C. (editor)
1984 *The Archaeology, Geology and Paleobiology of Stanton's Cave, Grand Canyon National Park, Arizona*. Grand Canyon National Park Association, Grand Canyon.

Evans, R. T.
2004 *Romancing the Maya: Mexican Antiquity in the American Imagination 1820–1915*. University of Texas Press, Austin.

Evans-Pritchard, E. E.
1940 *The Nuer: A Description of the Modes of Livelihood and Political Institutions of a Nilotic People*. Clarendon Press, Oxford.

Fagan, B. M.
2005 *A Brief History of Archaeology: From Classical Times to the Twenty-First Century*. Pearson Prentice Hall, Upper Saddle River, NJ.

Fairclough, N.
1992 *Language and Power*. Longman, London.
1995 *Critical Discourse Analysis: Papers in the Critical Study of Language*. Longman, New York.

Fash, W. L.
1983 Reconocimiento y Excavaciones en el valle. In *Introducción a la Arqueologia de Copán, Honduras*, ed. C. Baudez, 1:229–470. Instituto Hondureño de Antropología e Historia, Tegucigalpa.
1988 A New Look at Maya Statecraft from Copan, Honduras. *Antiquity* 62(234): 157–169.
1994 Changing Perspectives on Maya Civilization. *Annual Review of Anthropology* 23:181–208.
2002 Sprinter, Wordsmith, Mentor, and Sage: The Life of Gordon Randolph Willey, 1913–2002. *Ancient Mesoamerica* 14:169–177.
2005 Toward a Social History of the Copan Valley. In *Copan: The History of an Ancient Maya Kingdom*, ed. E. W. Andrews IV and W. Fash, 73–101. School of American Research, Santa Fe, NM.

Fash, W. L. and B. Fash
2009 What about Bob? Conjunctive Conjuring of K'uk' Mo' and Copan-Quirigua Connections. Paper presented in the invited session Understanding Maya Civilization: Papers in Honor of Robert Sharer at the 74th Annual Meeting of the Society for American Archaeology, Atlanta, Georgia.

Fash, W. L., and R. J. Sharer
1991 Sociopolitical Developments and Methodological Issues at Copán, Honduras: A Conjunctive Perspective. *Latin American Antiquity* 2(2):166–187.

Flannery, K. V.
1973 Archeology with a Capital S. In *Research and Theory in Current Archaeology*, ed. C. L. Redman, 47–53. John Wiley, New York.
1982 The Golden Marshalltown: A Parable for the Archeology of the 1980s. *American Anthropologist* 84(2):265–278.
2001 "There Were Giants in Those Days": Richard Stockton MacNeish, 1918–2001. *Ancient Mesoamerica* 12(2):149–156.
2006 On the Resilience of Anthropological Archaeology. *Annual Review of Anthropology* 35:1–13.

Florey Folan, L. M.
In press Definiendo la Presencia Femenina en Cobá, Quintana Roo, Humango y Cerrito de la Campana, Estado de México y Calakmul, Campeche México. In *Localidad y Globalidad en el Mundo Maya Prehispánico e Indígena Contemporáneo: Estudios de Espacio y Género*, ed. J. A. Hendon and M. J. Gallegos Gómora. Universidad Autonoma de Campeche, Campeche.

Florey Folan, L. M., and W. J. Folan
1981 Arqueología. *Investigaciones sobre Huamango y Region Vecina* 1:249–298.

Folan, W. J.
1961a Completion of Excavations at Structures 33, 36, 39 and 50. In *Preliminary Report on the 1959–1960 Field Season*, ed. E.W.I. Andrews IV, 11. Tulane University Dzibilchaltun Program. M.A.R.I. Tulane University, New Orleans.
1961b Excavations and Restoration of Structure 38. In *Preliminary Report on the 1959–1960 Field Season*, ed. E.W.I. Andrews IV, 11. Tulane University Dzibilchaltun Program. M.A.R.I. Tulane University, New Orleans.
1969 Dzibilchaltun, Yucatan, México: Structures 384, 385, 386: A Preliminary Interpretation. *American Antiquity* 34(4):434–461.
1977a Coba Archaeological Mapping Project, 1975. *Boletín de la Escuela de Ciencias Antropológicas, Universidad de Yucatán* 4(22, 23):29–51.
1977b Coba Archaeological Mapping Project, 1976. *Boletín de la Escuela de Ciencias Antropológicas, Universidad de Yucatán* 4(22, 23):52–81.
1981a CA Comments: In the Late Postclassic Eastern Frontier of Mesoamerica; Cultural Innovation along the Periphery by John W. Fox. *Current Anthropology* 22(4):336–337.
1981b San Miguel de Huamango: Un centro regional del antiguo estado de Tula-Jilotepec, Huamango y su región; Introduction. In *Investigaciones sobre Huamango y Región Vecina*, ed. P. D. Román Piña Chan, 1:205–248. Gobierno del Estado de México, Dirección de Turismo, Edo. de México, Toluca.
1983 Paleoclimatological Patterning in Southern Mesoamerica. *Journal of Field Archaeology* 10(4):453–468.
1989 More on the Functional Interpretation of the Scraper Plane. *Journal of Field Archaeology* 16:486–489.

Folan, W. J., and J. T. Dewhirst
1980 *The Yuquot Project*. History and Archaeology. 3 vols. National Historic Parks and Sites Branch, Parks Canada, Environment Canada, Ottawa.

Folan, W. J., M.d.R. Domínguez Carrasco, and A. A. Hernández
2006 *Calakmul, Campeche, México: Development and Decline in the Northern Peten; 1000 B.C. to A.D. 1600. Hierarchy and Power in the History of Civilizations*. Russian Academy of Sciences, Russian State University for the Humanities, Moscow, Russia.

Folan, W. J., L. Fletcher, and E. R. Kintz
1979 Fruit, Fiber, Bark and Resin: The Social Organization of a Maya City, Coba, Quintana Roo. *Science* 204(4394):697–701.

Folan, W. J., L. Fletcher, J. May Hau, and L. M. Florey Folan
2001 *Las Ruinas de Calakmul, Campeche, México: Un Lugar Central y Su Paisaje Cultural. Monografía y 32 mapas encajonadas*. Universidad Autónoma de Campeche, Campeche.

Folan, W. J., L. Fletcher, J. May Hau, A. Morales L., M.d.R. Domínguez Carrasco, R. González H., J. D. Gunn, and V. Tiesler Blos
2004 *Calakmul Campeche, Mexico: Patterns Representative of Its Urban Capital and Regional State*. Pennsylvania State University Press and INAH/CONACULTA, State College, PA.

Folan, W. J., L. M. Florey Folan, and A. Ruiz Perez
1987 *Cerrito de la Campaña, una Avanzada en la Ruta Teotihuacana el noroeste de la Gran Mesoamérica*. Universidad Autónoma de Campeche, Campeche.

Folan, W. J., and S. Gallegos O.
1999 Unas Observaciones sobre el Uso del Suelo del Sitio Arqueológico de Calakmul, Campeche. In *Los Camellones y Chinampas Tropicales*, ed. J. J. Jiménez-Osorio and V. M. Roríve, 55–68. Memorias del Simposio Taller Internacional sobre Camellones y Chinampas Tropicales, 1991, Villahermosa, Tabasco. Universidad Autónoma de Yucatán, Tabasco.

Folan, W. J., J. Gunn, and M.d.R. Domínguez C.
2001 Triadic Temples, Central Plazas and Dynastic Palaces: A Diachronic Analysis of the Royal Court Complex, Calakmul, Campeche, Mexico. In *Royal Courts of the Ancient Maya*, ed. T. Inomata and S. Houston, 223–265. Westview Press, Boulder, CO.

Folan, W. J., E. R. Kintz, and L. Fletcher
1983 *Coba: A Classic Maya Metropolis*. Academic Press, New York.

Folan, W. J., J. May Hau, R. Couoh Muñoz, and R. Gonzáles H.
1990 *Calakmul, Campeche, Mexico: Su Mapa; Una Introducción*. Universidad Autónoma de Campeche, Campeche, Mexico.

Folan, W. J., A. Morales L., R. González H., M.d.R. Domínguez C., A. Anaya H., and J. Gunn
2007a Calakmul, Campeche: El desarrollo de la Estructura II desde el Clásico Temprano hasta sus últimos días durante el Clásico Terminal. *Los Investigadores de la Cultura Maya* 15(1):137–154.

Folan, W. J., A. Morales L., R. González H., M.d.R. Domínguez C., A. Anaya H., J. Gunn, and J. K. Josserand
2007b The Regional State of Calakmul, Campeche, México: Recent Discoveries. Paper presented at the 52nd International Americanist Congress, Seville, Spain.

Ford, J. A.
1936 Analysis of Indian Village Site Collections from Louisiana and Mississippi. Department of Conservation, Louisiana State Geological Survey, Anthropological Study 2.

1938 A Chronological Method Applicable to the Southeast. *American Antiquity* 3:260–264.

1952 Measurements of Some Prehistoric Design Developments in the Southeastern States. *Anthropological Papers of the American Museum of Natural History* 44(3):311–384.

Ford, J. A., and G. R. Willey

1940 An Interpretation of the Prehistory of the Eastern United States. *American Anthropologist* 43:325–363.

Fortes, M.

1945 *The Dynamics of Clanship among the Tallensi: Being the First Part of an Analysis of the Social Structure of a Trans-Volta Tribe*. Oxford University Press, London.

Foster, M. K.

1996 Language and the Culture History of North America. *Handbook of North American Indians* 17(Languages):64–110.

Foucault, M.

1973a [1969] *The Archaeology of Knowledge and the Discourse on Language*. Pantheon Books, New York.

1973b *The Order of Things: An Archaeology of Human Sciences*. Vintage Books, New York.

1977a [1975] *Discipline and Punish*. Pantheon Books, New York.

1977b What Is an Author? In *Language, Counter-Memory, Practice*, ed. M. Foucault, 124–127. Cornell University Press, Ithaca, NY.

1979 [1973] *I, Pierre Rivière, Having Slaughtered My Mother, My Sister, and My Brother: A Case of Parricide in the 19th Century*. Pantheon, New York.

1981 The Order of Discourse: Inaugural Lecture at the College de France, 2 December 1970. In *Untying the Text: A Post-Structuralist Reader*, ed. R. Young, 51–76. Routledge and Kegan Paul, Boston.

1984 Truth and Power. In *The Foucault Reader*, ed. P. Rabinow, 51–75. Pantheon Books, New York.

Fowler, D. D.

2000 *A Laboratory for Anthropology. Science and Romanticism in the American Southwest, 1846–1930*. University of New Mexico Press, Albuquerque.

2003 Edgar Lee Hewett, James F. Zimmerman and the Beginnings of Anthropology at the University of New Mexico, 1927–1946. *Journal of Anthropological Research* 59(3):305–327.

2010 *The Glen Canyon Country: A Personal Memoir*. University of Utah Press, Salt Lake City.

Fowler, M. L.

1959 *Summary Report of Modoc Rock Shelter*. Illinois State Museum Papers, no. 8. Illinois State Museum, Springfield.

Fox, J. W.

1987 *Maya Postclassic State Formation: Segmentary Lineage Migration in Advancing Frontiers*. Cambridge University Press, Cambridge.

Freidel, D.
1985 Polychrome Facades of the Maya Preclassic. In *Painted Architecture and Polychrome Monumental Sculpture in Mesoamerica*, ed. E. H. Boone, 5–30. Dumbarton Oaks, Washington, DC.
1994 A Conversation with Gordon Willey. *Current Anthropology* 35(1):63–68.

Freidel, D., and L. Schele
1988a Kingship in the Late Preclassic Maya Lowlands: The Instruments and Places of Ritual Power. *American Anthropologist* 90(3):547–567.
1988b Symbol and Power: A History of the Lowland Maya Cosmogram. In *Maya Iconography*, ed. E. P. Benson and G. Griffin, 44–93. Princeton University Press, Princeton, NJ.

Freter, A. C.
1988 The Classic Maya Collapse at Copán, Honduras: A Regional Settlement Perspective. Ph.D. dissertation, Department of Anthropology, Pennsylvania State University, State College.

Friedman, J.
1992 The Past in the Future: History and the Politics of Identity. *American Anthropologist* 94(4):837–859.

Funari, P.P.A., M. Hall, and S. Jones
1999 *Historical Archaeology: Back from the Edge*. Routledge, London.

Gero, J., D. M. Lacy, and M. L. Blakey (editors)
1983 *The Socio-Politics of Archaeology*. Department of Anthropology, University of Massachusetts, Amherst.

Gero, J., and D. Root
1990 Public Presentations and Private Concerns: Archaeology in the Pages of *National Geographic*. In *The Politics of the Past*, ed. P. Gathercole and D. Lowenthal, 19–37. Unwin Hyman, London.

Gifford, C. A., and E. A. Morris
1985 Digging for Credit: Early Archaeological Field Schools in the American Southwest. *American Antiquity* 50(2):395–411.

Givens, D. R.
1992 *Alfred Vincent Kidder and the Development of Americanist Archaeology*. University of New Mexico Press, Albuquerque.

Gluckman, M.
1943 *Essays on Lozi Land and Royal Property*, vol. 1: *Lozi Land Tenure*; and vol. 2: *Property Rights of the Lozi King and Royal Family*. Rhodes-Livingstone Institute, Livingstone, Northern Rhodesia.

Golden, C., and G. Borgstede
2004b Continuities and Changes in Maya Archaeology: An Introduction. In *Continuities and Changes in Maya Archaeology: Perspectives at the Millennium*, ed. C. Golden and G. Borgstede, 3–12. Routledge, New York.

Golden, C., and G. Borgstede (editors)
2004a *Continuities and Changes in Maya Archaeology: Perspectives at the Millennium*. Routledge, New York.

Goldenweiser, A.
1933 *History, Psychology, and Culture*. Alfred A. Knopf, New York.

Gosden, C.
2001 Postcolonial Archaeology: Issues of Culture Identity and Knowledge. In *Archaeological Theory Today*, ed. I. Hodder, 214–240. Polity, Cambridge.

Goulet, J.-G.
1998 *Ways of Knowing: Experience, Knowledge, and Power among the Dene Tha*. University of British Columbia Press, Vancouver.

Gregory, D. A., and D. R. Wilcox
2008 *Zuni Origins: Anthropological Approaches on Multiple Scales*. University of Arizona Press, Tucson.

Griffin, J. B.
1943 *The Fort Ancient Aspect: Its Cultural and Chronological Position in Mississippi Valley Archaeology*. University of Michigan Press, Ann Arbor.
1985 The Formation of the Society for American Archaeology. *American Antiquity* 50(2, Golden Anniversary Issue):261–271.

Guernsey, S. J.
1931 Explorations in Northeastern Arizona: Report on the Archaeological Field Work of 1920–1923. *Papers, Peabody Museum of American Archaeology and Ethnology* 12(1). Cambridge, MA.

Guernsey, S. J., and A. V. Kidder
1921 *Basket-Maker Caves of Northeastern Arizona*. Harvard University Press, Cambridge, MA.

Gumerman, G. J.
2003 A Painting, Not a Medal: The Original and Obscure A. V. Kidder Award. *The SAA Archaeological Record* 3(5):19–20.

Gumerman, G. J., and D. A. Phillips
1978 Archaeology beyond Anthropology. *American Antiquity* 43(2):184–191.

Gunn, J.
1994 Introduction: A Perspective from the Humanities-Science Boundary. *Human Ecology* 22:1–22.

Gunn, J., and R.E.W. Adams
1981 Climatic Change, Culture, and Civilization in North America. *World Archaeology* 13:87–100.

Gunn, J. D., B. B. Faust, and W. J. Folan
2006 Horticultural Productivity and Global Climate. Annual Meeting of the American Anthropological Association, San Jose, CA.

Gunn, J., and W. J. Folan
2000 Three Rivers: Subregional Variations in Earth System Impacts in the Southwestern Maya Lowlands (Candelaria, Usumacinta, and Champotón, Watersheds). In *The Way the Wind Blows: Climate, History and Human Action*, ed. R. J. McIntosh, J. A. Tainter, and S. Keech McIntosh, 223–270. Columbia University Press, New York.

Gunn, J. D., W. J. Folan, and H. R. Robichaux
1994 Un Análisis Informativo Sobre la Descarga del Sistema del Río Candelaria en Campeche, México: Reflexiones acerca de los Paleoclimas que Afectaron los Antiguos Sistemas Mayas. In *Campeche Maya Colonial*, ed. W. Folan, 174–197. Colección Arqueologia, Universidad Autonoma de Campeche, Campeche, Mexico.
1995 A Landscape Analysis of the Candelaria Watershed in México: Insights into Paleoclimates Affecting Upland Horticulture in the Southern Yucatan Peninsula Semi Karst. *Geoarchaeology: An International Journal* 10(1):3–42.

Gunn, J. D., J. E. Foss, W. J. Folan, M.d.R. Domínguez Carrasco, and B. B. Faust
2002 Bajos Sediments and the Hydraulic System of Calakmul, Campeche, México. *Ancient Mesoamerica* 13:297–315.

Guthe, C. E.
1952 Twenty-Five Years of Archeology in the Eastern United States. In *Archeology of the Eastern United States*, ed. J. B. Griffin, 1–12. University of Chicago Press, Chicago.

Haas, J., and W. Creamer
1993 *Stress and Warfare among the Kayenta Anasazi of the Thirteenth Century A.D.* Fieldiana Anthropology New Series No. 21. Field Museum of Natural History, Chicago.

Hammond, N.
1984 Two Roads Diverged: A Brief Comment on "Lowland Maya Archaeology at the Crossroads." *American Antiquity* 49(4):821–826.

Hargrave, L. L.
1931 The Influence of Economic Geography upon the Rise and Fall of the Pueblo Culture in Arizona. *Museum Notes* 4(5):1–4.
1934a A Recently Discovered Basket Maker Burial Cave in the Tsegi. *Museum Notes* 7(3):13.
1934b The Tsegi Country. *Museum Notes* 6(11):51–54.
1935a Archaeological Investigations in the Tsegi Canyons of Northeastern Arizona in 1934. *Museum Notes* 7(7):25–28.
1935b *Report on Archaeological Reconnaissance in the Rainbow Plateau Area of Northern Arizona and Southern Utah: Based upon Fieldwork by the Rainbow Bridge–Monument Valley Expedition of 1933*. University of California Press, Berkeley.

Harris, C.H.I., and L. R. Sadler
2003 *The Archaeologist Was a Spy*. University of New Mexico Press, Albuquerque.

Harris, M.
1968 *The Rise of Anthropological Theory*. Thomas Y. Crowell, New York.

Harrison, M. W.
1976 Sources Cited. In *Handbook of Middle American Indians*, ed. M. W. Harrison and R. Wauchope, 16:3–255. University of Texas Press, Austin.

Haury, E. W.
1954 Southwest Issue. In *American Anthropologist*, ed. E. W. Haury, 56:529–740.

Haury, E. W., R. L. Rands, A. C. Spaulding, W. W. Taylor, R. H. Thompson, R. Wauchope, and M. E. White
1956 An Archaeological Approach to the Study of Cultural Stability. In *Seminars in Archaeology: 1955*, ed. R. Wauchope, 31–57. Society for American Archaeology, Salt Lake City, UT.

Hay, C. L., R. Linton, S. K. Lothrop, H. L. Shapiro, and G. C. Vaillant (editors)
1940 *The Maya and Their Neighbors*. D. Appleton-Century Company, New York.

Hegmon, M.
2003 Setting Theoretical Egos Aside: Issues and Theory in North American Archaeology. *American Antiquity* 68(2):213–243.

Heizer, R. F., and S. F. Cook (editors)
1960 *The Application of Quantitative Methods in Archaeology*. Quadrangle Books, Tavistock, Chicago.

Hellmuth, N.
1986 The Surface of the Underwater World. Ph.D. dissertation, Department of Art History, Karl Franzens Universitaet, Graz, Austria.

Helm, J. (editor)
1981 *The Subarctic: Handbook of American Indians*. Vol. 6. Smithsonian Institution Press, Washington, DC.

Hempel, C. G.
1966 *Philosophy of Natural Science*. Prentice-Hall, Englewood Cliffs, NJ.

Higham, C.F.W.
1968 Prehistoric Research in Western Southland. *New Zealand Archaeological Association, Newsletter* 11(4):155–164.

Hill, J. N.
1966 A Prehistoric Community in Eastern Arizona. *Southwestern Journal of Anthropology* 22(1):9–30.
1968 Broken K. Pueblo: Patterns of Form and Function. In *New Perspectives of Archaeology*, ed. S. R. Binford and L. R. Binford, 103–142. Aldine Publishing Company, Chicago.
1970 *Broken K Pueblo: Prehistoric Social Organization in the American Southwest*. University of Arizona Press, Tucson.
1972 The Methodological Debate in Contemporary Archaeology: A Model. *Models in Archaeology*, ed. D. L. Clarke, 61–107. Methuen, London.

Hill, J. N., and R. K. Evans
1972 A Model for Classification and Typology. In *Models in Archaeology*, ed. D. L. Clarke, 231–273. Methuen, London.

Hinsley, C.
1984 Wanted: One Good Man to Discover Central American History. In *Harvard Magazine* 87:64A–64H.

1989 Revising and Revisioning the History of Archaeology: Reflections on Region and Context. In *Tracing Archaeology's Past: The Historiography of Archaeology*, ed. A. Christenson, 79–96. Southern Illinois University Press, Carbondale.

Hodder, I.
1982 *Symbolic and Structural Archaeology*. Cambridge University Press, Cambridge.
1986 *Reading the Past: Current Approaches to Interpretation in Archaeology*. Cambridge University Press, New York.
1989 Writing Archaeology. *Antiquity* 63:268–274.
1991 *Reading the Past: Current Approaches to Interpretation in Archaeology*. 2nd ed. Cambridge University Press, New York.
2001 Introduction: Review of Contemporary Theoretical Debates in Archaeology. In *Archaeological Theory Today*, ed. I. Hodder, 1–13. Polity Press, Cambridge.

Hodder, I. (editor)
1996 *On the Surface: Çatalhöyük 1993–1995*. McDonald Institute Monographs, vol. 1. Çatalhöyük Research Project, Cambridge, England.
2000 *Towards Reflexive Method in Archaeology: The Example of Çatalhöyük*. McDonald Institute Monographs, vol. 2. Çatalhöyük Research Project, Cambridge, England.
2005a *Changing Materialities at Çatalhöyük: Reports from the 1995–99 Seasons*. McDonald Institute Monographs, vol. 5. Çatalhöyük Research Project, Cambridge, England.
2005b *Inhabiting Çatalhöyük: Reports from the 1995–99 Seasons*. McDonald Institute Monographs, vol. 4. Çatalhöyük Research Project, Cambridge, England.
2006 *Çatalhöyük Perspectives: Themes from the 1995–99 Seasons*. McDonald Institute Monographs, vol. 6. Çatalhöyük Research Project, Cambridge, England.
2007 *Excavating Çatalhöyük: South, North and KOPAL Area Reports from the 1995–99 Seasons*. McDonald Institute Monographs, vol. 3. Çatalhöyük Research Project, Cambridge, England.

Hodder, I., and C. Cessford
2004 Daily Practice and Social Memory at Çatalhöyük. *American Antiquity* 69:17–40.

Hodder, I., and S. R. Hutson
2003 *Reading the Past: Current Approaches to Interpretation in Archaeology*. 3rd ed. Cambridge University Press, Cambridge.

Hodell, D. A., J. H. Curtis, and M. Brenner
1995 Possible Role of Climate in the Collapse of Classic Maya Civilization. *Nature* 375:391–394.

Hodell, D. A., J. H. Curtis, M. Brenner, and T. Guilderson
2001 Solar Forcing of Drought Frequency in the Maya Lowlands. *Science* 292: 1367–1370.

Hopkins, N. A.
1965 Great Basin Prehistory and Uto-Aztecan. *American Antiquity* 31(1):48–60.

Hudson, C.
2008 Walter Taylor and the History of American Archaeology. *Journal of Anthropological Archaeology* 27:192–200.

Hutson, S. R.
2002 Gendered Citation Practices in *American Antiquity* and other Archaeology Journals. *American Antiquity* 67(2):331–342.
2006 Self-Citation in Archaeology: Age, Gender, Prestige, and the Self. *Journal of Archaeological Method and Theory* 13(1):1–18.

Hymoff, E.
1986 *OSS in World War II*. Ballantine Books, New York.

James, D.
1947 *A Prisoner's Progress*. William Blackwood and Sons Ltd., Edinburgh.

Jameson, F.
1975 *The Prison-House of Language*. Princeton University Press, Princeton, NJ.
1991 *Postmodernism, or The Cultural Logic of Late Capitalism*. Duke University Press, Durham, NC.

Jennings, J. D.
1958 Reviews of *Method and Theory in American Archaeology* and *Archaeology and Society. American Anthropologist* 60(6):1207–1208.
1959 Review of *The Identification of Non-Artifactual Archaeological Materials,* by W. W. Taylor. *American Antiquity* 24(4):434.
1966 *Glen Canyon: A Summary*. University of Utah Press, Salt Lake City.
1986 American Archaeology, 1930–1985. In *American Archaeology, Past and Future: A Celebration of the Society for American Archaeology 1935–1985*, ed. D. J. Meltzer, D. D. Fowler, and J. A. Sabloff, 53–62. Smithsonian Institution Press, Washington, DC.

Johnson, M.
1999 *Archaeological Theory: An Introduction*. Blackwell Publishing, Malden, MA.

Josserand, J. K.
2007 Literatura e Historia en Los Textos Jeroglíficos Clásicos. *Gaceta* 17(93):39–46.

Joyce, A. A., A. G. Workinger, B. Hamann, P. Kroefges, M. H. Oland, and S. M. King
2004 Lord 8 Deer "Jaguar Claw" and the Land of the Sky: The Archaeology and History of Tututepec. *Latin American Antiquity* 15(3):273–297.

Joyce, R. A.
2002 *The Languages of Archaeology*. Blackwell Publishers, Oxford.
2003 Archaeology and Nation Building: A View from Central America. In *The Politics of Archaeology and Identity in a Global Context*, ed. S. Kane, 79–100. Archaeological Institute of America, Boston.

Judd, N. M.
1924a Beyond the Clay Hills. In *National Geographic Magazine*, 45:275–302.
1924b Explorations in San Juan County, Utah. *Smithsonian Miscellaneous Collections* 76(10):77–82.

Kehoe, A. B.
1998 *Land of Prehistory: A Critical History of American Archaeology*. Routledge, New York.

Kendall, A.
1977 *The Art and Archaeology of Pre-Columbian Middle America: An Annotated Bibliography of Works in English*. G. K. Hall, Boston.

Kerns, V.
2003 *Scenes from the High Desert: Julian Steward's Life and Theory*. University of Illinois Press, Urbana.

Kidder, A. V.
1927 Southwestern Archeological Conference. *Science* 486–491.
1928 The Pan-Scientific Approach. *Bulletin of the International Committee of Historical Sciences* 5:749–753.
1930 *Division of Historical Research*. Carnegie Institute of Washington, Washington, DC.
1936 Speculations on New World Prehistory. In *Essays in Anthropology; Presented to A. L. Kroeber in Celebration of His Sixtieth Birthday, June 11, 1936*, ed. R. H. Lowie, 143–152. University of California Press, Berkeley.
1941 *Division of Historical Research*. Carnegie Institute of Washington, Washington, DC.
1950 Division of Historical Research. *Carnegie Institution Year Book* 49 (1949–1950):192.

Kidder, A. V. (editor)
1924 *An Introduction to the Study of Southwestern Archaeology, with a Preliminary Account of the Excavations at Pecos*. Papers of the Phillips Academy Southwestern Expedition, 1. Yale University Press, New Haven, CT.

Kidder, A. V., and S. J. Guernsey
1919 Archaeological Explorations in Northeastern Arizona. *Bureau of American Ethnology Bulletin* 65.

Kleindienst, M. R., and P. J. Watson
1956 Action Archeology: The Archeological Inventory of a Living Community. *Anthropology Tomorrow* 5:75–78.

Klejn, L. S.
1977 A Panorama of Theoretical Archaeology. *Current Anthropology* 18(1):1–42.

Kluckhohn, C.
1927 *To the Foot of the Rainbow*. Century Company, New York.
1932 *Beyond the Rainbow*. Christopher Publishing House, Boston.
1938a Letter to Walter W. Taylor, dated February 12, 1938. Walter Taylor Papers. Box 9. Correspondence. National Anthropological Archives, Smithsonian Institution, Washington, DC.
1938b Letter to Walter W. Taylor, dated July 13, 1938. Walter Taylor Papers. Box 9. Correspondence. National Anthropological Archives, Smithsonian Institution, Washington, DC.
1939a Letter to Walter W. Taylor, dated October 6, 1939. Walter Taylor Papers. Box 9. Correspondence. National Anthropological Archives, Smithsonian Institution.

1939b The Place of Theory in Anthropological Studies. *Philosophy of Science* 6:328–344.

1940 The Conceptual Structure in Middle American Studies. In *The Maya and Their Neighbors*, ed. C. L. Hay, R. Linton, S. K. Lothrop, H. L. Shapiro, and G. C. Vaillant, 41–51. D. Appleton-Century Company, New York.

1941 Patterning as Exemplified in Navaho Culture. In *Language, Culture, and Personality: Essays in Memory of Edward Sapir*, ed. L. Spier, A. I. Hallowell, and S. S. Newman. University of Utah Press, Salt Lake City.

1949 *Mirror for Man: The Relation of Anthropology to Modern Life*. Whittlesey House, New York.

1968 The Philosophy of the Navajo Indians. In *Readings in Anthropology*, vol. 2, ed. M. H. Fried, 674–699. Crowell, New York.

Kluckhohn, C., and W. H. Kelley

1945 The Concept of Culture. In *The Science of Man in the World Crisis*, ed. R. Linton, 78–105. Columbia University Press, New York.

Kluckhohn, C., and P. Reiter

1939 Preliminary Report on the 1937 Excavations, Bc-50-51, Chaco Canyon, New Mexico. *University of New Mexico Bulletin*, Anthropological Series 3(2):30–48.

Kohl, P. L.

1975 The Archaeology of Trade. *Dialectical Anthropology* 1(1):43–50.

1978 The Balance of Trade in Southwestern Asia in the Mid-third Millennium B.C. *Current Anthropology* 19(3):463–592.

Krieger, A. D.

1944 The Typological Concept. *American Antiquity* 9:271–288.

Kroeber, A. L.

1936 So-called Social Science. *Journal of Social Philosophy* 1:317–340.

1948 *Anthropology*. Harcourt, New York.

Kroeber, A. L., and C. Kluckhohn

1952 Culture: A Critical Review of Concepts and Definitions. *Papers of the Peabody Museum of Archaeology and Ethnology, Harvard University* 47(1).

Kubler, G.

1962 *The Art and Architecture of Ancient America: The Mexican, Maya and Andean Peoples*. Penguin Books, Baltimore.

1969 *Studies in Classic Maya Iconography*. Vol. 18. Memoirs of the Connecticut Academy of Arts and Sciences, New Haven.

1990 Architectural Historians before the Fact. In *The Architectural Historian in America*, ed. E. B. Dougall. University Press of New England, Hanover, NH.

Kuhn, T. S.

1962 *The Structure of Scientific Revolutions*. University of Chicago Press, Chicago.

Lagunas Rodriguez, Z.

1981 Entierros Humanos Prehispanicos y Recientes de Huamango, Acambay, Estado de México. In *Investigaciones sobre Huamango y su Región Vecina*, ed.

R. Piña Chan, vol. 1. Talleres Gráficos de la Nación, Toluca, Estado de México, Turismo.

Lamberg-Karlovsky, C. C.

1989 Introduction. In *Archaeological Thought in America,* ed. C. C. Lamberg-Karlovsky, 1–16. Cambridge University Press, Cambridge.

Lattuca, L.

2001 *Creating Interdisciplinarity: Interdisciplinary Research and Teaching among College and University Faculty.* Vanderbilt University Press, Nashville, TN.

Layton, R. (editor)

1988 *Who Needs the Past? Indigenous Values and Archaeology*. Unwin Hyman, London.

1989 *Conflict in the Archaeology of Living Traditions*. Unwin Hyman, London.

Leach, B. F., and H. M. Leach (editors)

1979 *Prehistoric Man in Palliser Bay*. Bulletin of the National Museum of New Zealand no. 21. Wellington, New Zealand.

LeBlanc, S. A.

1999 *Prehistoric Warfare in the American Southwest*. University of Utah Press, Salt Lake City.

Lekson, S. H.

1999 *The Chaco Meridian: Centers of Political Power in the Ancient Southwest*. AltaMira Press, Walnut Creek, CA.

Leone, M. P.

1986 Symbolic, Structural and Critical Archaeology. In *American Archaeology Past and Future*, ed. D. J. Meltzer, J. A. Sabloff, and D. D. Fowler, 415–438. Smithsonian Institution Press, Washington, DC.

Leone, M. P.

1972b Issues in Anthropological Archaeology. In *Contemporary Archaeology: A Guide to Theory and Contributions*, ed. M. P. Leone, 14–26. Southern Illinois University Press, Carbondale.

1972c Part 1: The Scope of the Changes in Contemporary Archaeology. In *Contemporary Archaeology: A Guide to Theory and Contributions*, ed. M. P. Leone, 1–3. Southern Illinois University Press, Carbondale.

1978 Time in American Archaeology. In *Social Archaeology: Beyond Subsistence and Dating*, ed. C. L. Redman, 25–36. Academic Press, New York.

1981 Archaeology's Relationship with the Past and the Present. In *Modern Material Culture: The Archaeology of Us*, ed. R. A. Gould and M. B. Schiffer, 5–14. Academic Press, New York.

1984 Interpreting Ideology in Historical Archaeology: Using the Rules of Perspective in the William Paca Garden in Annapolis, Maryland. In *Ideology, Power and Prehistory*, ed. D. Miller and C. Tilley, 25–35. Cambridge University Press, Cambridge.

Leone, M. P. (editor)

1972a *Contemporary Archaeology: A Guide to Theory and Contributions*. Southern Illinois University Press, Carbondale.

Leventhal, R. M.
1979 *Settlement Patterns at Copán, Honduras.* Ph.D. dissertation, Department of Anthropology, Harvard University, Cambridge, MA.

Leventhal, R. M., and D. E. Cornavaca
2007 Willey and Phillips. In *Gordon R. Willey and American Archaeology: Contemporary Perspectives*, ed. J. A. Sabloff and W. L. Fash, 61–71. University of Oklahoma Press, Norman.

Lewis, T.M.N., and M. Kneberg
1946 *Hiwassee Island, and Archaeological Account of Four Tennessee Indian Peoples, Partially Based on Field Reports by Charles H. Nash*. University of Tennessee Press, Knoxville.

Linton, R.
1936 *The Study of Man: An Introduction*. Student's edition. D. Appleton-Century Company, New York.
1944 Nomad Raids and Fortified Pueblos. *American Antiquity* 10:28–32.
1955 *The Tree of Culture*. Alfred A. Knopf, New York.

Lizana, B.
1893 [1633] *Historia de Yucatán: Devocionario de Nuestra Señora de Izamal y Conquista Espiritual*. Museo Nacional, Mexico City.

Longacre, W. A.
1968 Some Aspects of Prehistoric Society in East-Central Arizona. In *New Perspectives in Archaeology*, ed. S. R. Binford and L. R. Binford, 89–102. Aldine Publishing Company, Chicago.
1970 Current Thinking in American Archaeology. *American Anthropological Association. Bulletin* 3(3):126–138.
2000 Exploring Prehistoric Social and Political Organization in the American Southwest. *Journal of Anthropological Research* 56(3):287–300.

Longyear, J. M.
1952 *Copán Ceramics: A Study of Southeastern Maya Pottery*. Carnegie Institution of Washington, Washington, DC.

Lyman, R. L., and M. J. O'Brien
2004 A History of Normative Theory in Americanist Archaeology. *Journal of Archaeological Method and Theory* 11(4):369–396.

Maca, A. L.
2001 Valley Bottom, Meet the Foothills: Socio-Spatial Conjunction and the Legacy of W. W. Taylor in the Archaeology of Copan, Honduras. Paper for invited session Social Organization at Late Classic Copan, Honduras. 66th Annual Meeting of the Society for American Archaeology, New Orleans.
2002 Spatio-Temporal Boundaries in Classic Maya Settlement Systems: Copan's Urban Foothills and the Excavations at Group 9J-5. Ph.D. dissertation, Department of Anthropology, Harvard University, Cambridge, MA.
2009 Ethnographic Analogy and the Archaeological Construction of Maya Identity at Copan, Honduras. In *The Ch'orti' Maya Area Past and Present*, ed.

B. E. Metz, C. L. McNeil, and K. M. Hull, 90–107. University of Florida Press, Gainesville.

MacNeish, R. S.
1958 *Preliminary Archaeological Investigations in the Sierra de Tamaulipas, Mexico*. American Philosophical Society, Philadelphia.
1960 Rejoinder to Taylor. *American Antiquity* 25(4):591–593.
1978 *The Science of Archaeology?* Duxbury Press, North Scituate, MA.

Mainfort, R. C., and L. P. Sullivan
1998 *Ancient Earthen Enclosures of the Eastern Woodlands*. Ripley P. Bullen series. University Press of Florida, Gainesville.

Marcus, J. P.
1973 Territorial Organization of the Lowland Classic Maya. *Science* 180(4089): 911–916.
1976 *Emblem and State in the Classic Maya Lowlands: An Epigraphic Approach to Territorial Organization*. Dumbarton Oaks, Washington, DC.
1983 Lowland Maya Archaeology at the Crossroads. *American Antiquity* 48(3): 454–488.
1995 Where Is Lowland Maya Archaeology Headed? *Journal of Archaeological Research* 3(1):3–53.
2004 Primary and Secondary State Formation in Southern Mesoamerica. In *Understanding Early Classic Copan*, ed. E. E. Bell, M. A. Canuto, and R. J. Sharer, 357–374. University of Pennsylvania Museum, Philadelphia.
2008 The Archaeological Evidence for Social Evolution. *Annual Reviews in Anthropology* 37:251–266.

Martin, P. S.
1954 Comments on Taylor's "Southwestern Archaeology, Its History and Theory." *American Anthropologist* 56(4):570–571.
1971 The Revolution in Archaeology. *American Antiquity* 36(1):1–8.

Martin, P. S., and J. B. Rinaldo
1950 *Sites of the Reserve Phase, Pine Lawn Valley, Western New Mexico*. Chicago National History Museum, Chicago.

Martin, P. S., and J. Schoenwetter
1960 Arizona's Oldest Cornfield. *Science* 132:33–34.

Mayer-Oakes, W. J.
1963 Complex Society Archaeology. *American Antiquity* 29(1):57–60.

May Hau, J., R. Couoh Muñoz, and W. J. Folan
2001 El Mapa. In *Las Ruinas de Calakmul, Campeche, México: Un Lugar Central y Su Paisaje Cultural*, ed. W. J. Folan, L. Fletcher, J. May Hau, and L. M. Florey Folan, 17–24. Universidad Autónoma de Campeche, Campeche.

May Hau, J., R. Couoh Muñoz, R. Gonzáles H., and W. J. Folan
1990 *El Mapa de las Ruinas de Calakmul, Campeche, México*. Universidad Autónoma de Campeche, Centro de Invest. Históricas y Sociales, Campeche, Mexico.

McAnany, P.

1995 *Living with the Ancestors, Kinship and Kingship in Ancient Maya Society*. University of Texas Press, Austin.

2007 Culture Heroes and Feathered Serpents. In *Gordon R. Willey and American Archaeology*, ed. J. A. Sabloff and W. Fash, 209–231. University of Oklahoma Press, Norman.

McGuire, R. H.

2008 *Archaeology as Political Action*. University of California Press, Berkeley.

McKern, W. C.

1939 The Midwestern Taxonomic Method as an Aid to Archaeology Study. *American Antiquity* 4:301–313.

Meggers, B. J.

1955 The Coming of Age of American Archaeology. In *New Interpretations of Aboriginal American Culture History*, B. J. Meggers and C. Evans, 116–129. Anthropological Society of Washington, Washington, DC.

1956 Functioning and Evolutionary Implications of Community Patterning. In *Seminars in Archaeology: 1955*, ed. R. Wauchope, 129–157. Society for American Archaeology, Memoir 11, Washington, DC.

Meggers, B. J., and C. Evans

1958 Review of *Method and Theory in American Archaeology,* by Gordon R. Willey and Philip Phillips. *American Antiquity* 24(2):195–196.

Millaire, J.

2004 The Manipulation of Human Remains in Moche Society: Delayed Burials, Grave Reopening, and Secondary Offerings of Human Bones on the Peruvian North Coast. *Latin American Antiquity* 15(4):371–388.

Miller, D., M. Rowlands, and C. Tilley (editors)

1989 *Domination and Resistance*. Unwin Hyman, London.

Mills, B. J., and P. L. Crown

1995 *Ceramic Production in the American Southwest.* University of Arizona Press, Tucson.

Morgan, G.

1945 *Only Ghosts Can Live*. Crosby Lockwood and Son Ltd., London.

Morgan, L. H.

1877 *Ancient Society*. C. H. Kerr, Chicago.

Morley, S. G.

1920 *The Inscriptions of Copan.* Publication 219. Carnegie Institution of Washington, Washington, DC.

1938 *The Inscriptions of Peten*. Publication 437. Carnegie Institute of Washington, Washington, DC.

1946 *The Ancient Maya*. Stanford University Press, Stanford, CA.

Morley, S. G., and G. W. Brainerd

1956 *The Ancient Maya*. 3rd ed. Stanford University Press, Stanford, CA.

Morley, S. G., G. W. Brainerd, and R. J. Sharer
1983 *The Ancient Maya*. 4th ed. Stanford University Press, Stanford, CA.

Morris, C. W.
1938 Foundations of the Theory of Signs. *International Encyclopedia of Unified Science* 1(2). University of Chicago Press, Chicago.

Morss, N.
1927 Archaeological Explorations on the Middle Chinlee, 1925. *American Anthropological Association*, Memoir 34.
1931 Notes on the Archaeology of the Kaibito and Rainbow Plateaus in Arizona. *Peabody Museum of American Archaeology and Ethnology, Harvard University Papers* 12(2).

Movius, H. L., N. David, H. Bricker, and R. Berle Clay
1968 The Analysis of Certain Classes of Upper Paleolithic Tools. American School of Prehistoric Research, *Bulletin* 26. Peabody Museum, Harvard University, Cambridge, MA.

Murray, T. (editor)
1999 *Encyclopaedia of Archaeology: The Great Archaeologists*. ABC-CLIO Books, Santa Barbara.

Nash, S. E., and J. S. Dean
2005 Paleoenvironmental Reconstruction and Archaeology: Uniting the Social and Natural Sciences in the American Southwest and Beyond. In *Southwest Archaeology in the Twentieth Century*, ed. L. A. Cordell and D. D. Fowler, 125–141. University of Utah Press, Salt Lake City.

Neumann, E.
1962 *The Origins and History of Consciousness*. Vol. 2. Harper and Brothers, New York.

Nichols, D. L.
1996 An Overview of Regional Settlement Pattern Survey in Mesoamerica: 1960–1995. *Arqueología Mesoamericana: Homenaje a William T. Sanders*, ed. A. G. Mastache, J. R. Parsons, R. S. Santley, M. C. Serra Puche, 59–95. Instituto Nacional de Antropología e Historia, Mexico City.

Nubiola, J.
1996 Scholarship on the Relations between Ludwig Wittgenstein and Charles S. Peirce. In *Studies on the History of Logic: Proceedings of the III Symposium on the History of Logic*, ed. I. Angelelli and M. Cerezo, 281–294. Walter de Gruyter, Berlin.

O'Brien, M. J., R. L. Lyman, and M. B. Schiffer
2005 *Archaeology as a Process: Processualism and Its Progeny*. University of Utah Press, Salt Lake City.

Ogden, C. K., and I. A. Richards
1923 *The Meaning of Meaning: A Study of the Influence of Language upon Thought and of the Science of Symbolism*. Harcourt, Brace, and Company, New York.

Osgood, C.
1936 *The Distribution of Northern Athapaskan Indians*. Yale University Press, New Haven, CT.
1937 *Ethnography of the Tanaina*. Yale University Press, New Haven, CT.
1940 *Ingalik Material Culture*. Yale University Press, New Haven, CT.
1942 *The Ciboney Culture of Cayo, Redondo, Cuba*. Yale University Press, New Haven, CT.
1943 *Excavations at Tocorón, Venezuela*. Yale University Press, New Haven, CT.
1946 *British Guiana Archaeology to 1945*. Yale University Press, New Haven, CT.
1951 Culture: Its Empirical and Non-Empirical Character. *Southwestern Journal of Anthropology* 7:202–214.
1953 *Winter*. W. W. Norton, New York.
1958 *Ingalik Social Culture*. Yale University Press, New Haven, CT.
1959 *Ingalik Mental Culture*. Yale University Press, New Haven, CT.
1971 *Han Indians: A Compilation of Ethnographic and Historical Data on the Alaska-Yukon Boundary Area*. Yale University Press, New Haven, CT.
1979 *Anthropology in Museums of Canada and the United States*. Milwaukee Public Museum, Milwaukee, WI.

Parkington, J., and A. B. Smith
1986 Guest Editorial. *South African Archaeological Bulletin* 144:43–45.

Parsons, T., and E. Z. Vogt
1962 Clyde Kay Maben Kluckhohn, 1905–1960. *American Anthropologist* 64:140–161.

Patterson, T. C.
1986 The Last Sixty Years: Toward a Social History of Americanist Archaeology in the United States. *American Anthropologist* 88:7–26.
2001 *A Social History of Anthropology in the United States*. Berg, New York.
2003 *Marx's Ghost, Conversations with Archaeologists*. Berg, New York.

Pauketat, T.
2000 The Tragedy of the Commoners. In *Agency in Archaeology*, ed. M. A. Dobres and J. Robb, 113–129. Routledge, London.

Peake, H.J.E., and H. J. Fleure
1927 *The Corridors of Time*. Oxford University Press, London.

Petrie, F.W.M.
1899 Sequences in Prehistoric Remains. *Journal of the Anthropological Institute* 29:295–301.

Phillips, P.
1940 Middle American Influences on the Archaeology of the Southeastern United States. In *Maya and Their Neighbors*, ed. C. L. Hay, R. Linton, S. K. Lothrop, H. L. Shapiro, and G. C. Vaillant, 349–367. D. Appleton-Century Company, New York.
1955 American Archaeology and General Anthropological Theory. *Southwestern Journal of Anthropology* 11(3):246.

Phillips, P., and G. R. Willey
1953 Method and Theory in American Archeology: An Operational Basis for Culture-Historical Integration. *American Anthropologist* 55:615–633.

Pike, K. L.
1954 Language in Relation to a Unified Theory of the Structure of Human Behavior, vol. 1. Summer Institute of Linguistics, Glendale, CA.

Piña Chan, R. (editor)
1981 *Investigaciones sobre Huamango y Región Vecina*. Gobierno del Estado de Mexico. Dirección de Turismo, Toluca, Edo. de México.

Pinsky, V.
1992 Archaeology, Politics, and Boundary Formation: The Boas Censure (1919) and the Development of American Archaeology during the Inter-War Years. In *Rediscovering Our Pasts: Essays on the History of American Anthropology*, ed. J. E. Reyman, 161–189. Avebury, Aldershot, Hampshire, UK.

Preucel, R. W.
1991 *Processual and Postprocessual Archaeologies: Multiple Ways of Knowing the Past*. Center for Archaeological Investigations, Southern Illinois University at Carbondale, Carbondale.

Price, D. H.
2000 Anthropologists as Spies. *The Nation* 271(16):24–27.
2001 "The Shameful Business": Leslie Spier on the Censure of Franz Boas. *History of Anthropology Newsletter* 27(2):9–12.
2008 *Anthropological Intelligence: The Deployment and Neglect of American Anthropology in the Second World War*. Duke University Press, Durham, NC.

Proskouriakoff, T.
1950 A Study of Classic Maya Sculpture. *Carnegie Institute of Washington* Publication 593, Washington, DC.
1960 Historical Implications of a Pattern of Dates at Piedras Negras, Guatemala. *American Antiquity* 25:454–475.

Puleston, D. E.
1979 An Epistemological Pathology and the Collapse, or Why the Maya Kept the Short Count. In *Maya Archaeology and Ethnohistory*, ed. N. Hammond and G. R. Willey, 63–71. University of Texas Press, Austin.

Pyburn, K. A.
2004 We Have Never Been Post-Modern: Maya Archaeology in the Ethnographic Present. In *Continuities and Changes in Maya Archaeology: Perspectives at the Millennium*, ed. C. Golden and G. Borgstede, 287–293. Routledge, New York.
2009 Practicing Archaeology—As If It Really Matters. *Public Archaeology* 8(2–3): 161–175.

Quilter, J.
2003 Preface. In *Sandals from Coahuila Caves*, W. W. Taylor, viii–ix. Dumbarton Oaks, Washington, DC.

Quirarte, J.
1981 Tricephalic Units in Olmec, Izapan-style, and Maya Art. In *The Olmec & Their Neighbors: Essays in Memory of Matthew W. Stirling*, ed. M. W. Stirling, M. D. Coe, D. C. Grove, and E. P. Benson, 289–308. Dumbarton Oaks, Washington, DC.

Radcliffe-Brown, A. R.
1924 *The Mother's Brother in South Africa*. Bobbs-Merrill, Indianapolis.

Rands, R. L.
1955 Some Manifestations of Water in Mesoamerican Art. *Bureau of American Ethnology Bulletin* 157 Anthropological Papers 48:265–394.
1969 The Relationship of Monumental Stone Sculpture at Copan with Maya Lowlands. Paper presented at the 38th International Congress of Americanists. Stuttgart-Munich, Germany.

Redfield, R.
1950 *A Village that Chose Progress*. University of Chicago Press, Chicago.

Redfield, R., and A. Villa Rojas
1934 *Chan Kom*. Carnegie Institution of Washington, Washington, DC.

Reed, E. K.
1951 Culture Areas of the Pre-Hispanic Southwest. *New Mexico Quarterly* 21(4): 428–439.

Reingold, N.
1979 National Science Policy in a Private Foundation: The Carnegie Institution of Washington. In *The Organization of Knowledge in Modern America, 1860–1920*, ed. A. Oleson and J. Voss, 313–341. The John Hopkins University Press, Baltimore.

Renfrew, C.
1989 Comments on "Archaeology in the 1990s." *Norwegian Archaeological Review* 22:33–41.

Restall, M.
1997 *The Maya World*. Stanford University Press, Stanford, CA.

Reyman, J. E.
1970 Southwestern Pueblo Conservatism: A New Look at an Old Myth. Paper presented at 35th Annual Meeting of the Society for American Archaeology, Mexico City.
1971 Mexican Influence on Southwestern Ceremonialism. Ph.D. dissertation, Department of Anthropology, Southern Illinois University, Carbondale.
1976a Astronomy, Architecture, and Adaptation at Pueblo Bonito. *Science* 193:957–962.
1976b The Emics and Etics of Kiva Wall Niche Location. *The Journal of the Steward Anthropological Society* 7(1):107–129.
1978 Room 44, Wupatki: Rejecting False Profits. *American Antiquity* 47(4):729–733.
1987 Priests, Power, and Politics: Some Implications of Socioceremonial Control. In *Astronomy and Ceremony in the Prehistoric Southwest*, ed. J. B. Carlson

and W. J. Judge, 2:121–147. Papers of the Maxwell Museum of Anthropology, Albuquerque.
1989 The History of Archaeology and the Archaeological History of Chaco Canyon, New Mexico. In *Tracing Archaeology's Past: The Historiography of Archaeology*, ed. A. Christenson, 41–53. Southern Illinois University Press, Carbondale.
1992 Women in American Archaeology: Some Historical Notes and Comments. In *Rediscovering Our Past: Essays on the History of American Archaeology*, ed. J. E. Reyman, 69–80. Worldwide Archaeology Series. Avebury Press, Aldershot, Hampshire, UK.
1994 Gender and Class in Archaeology: Then and Now. In *Equity Issues for Women in Archaeology*, ed. M. C. Nelson, S. M. Nelson, and A. Wylie, 83–90. Archeological Papers of the American Anthropological Association, Washington, DC.
1995 Value in Mesoamerican-Southwest Trade. In *The Gran Chichimeca: Essays on the Archaeology and Ethnohistory of Northern Mesoamerica*, ed. J. E. Reyman, 271–280. Worldwide Archaeology Series, vol. 12. Avebury Press, Aldershot, Hampshire, UK.
1997 Walter Willard Taylor. *Anthropology Newsletter* 38(6):44.
1999 Walter W. Taylor, 1913–1997. In *Encyclopedia of Archaeology. The Great Archaeologists*, ed. T. A. Murray, 2:681–700. ABC-CLIO, Santa Barbara, CA.

Rice, D. S., and P. M. Rice
2004 History in the Future: Historical Data and Investigations in Lowland Maya Studies. In *Continuities and Changes in Maya Archaeology: Perspectives at the Millennium*, ed. C. Golden and G. Borgstede, 77–95. Routledge, New York.

Riley, C. L., and W. W. Taylor (editors)
1967 *American Historical Anthropology: Essays in Honor of Leslie Spier*. Southern Illinois University Press, Carbondale.

Ritchie, W. A.
1980 [1965] *The Archaeology of New York State*. Harbor Hill Books, Harrison, NY.
1985 Fifty Years of Archaeology in the Northeastern United States: A Retrospect. *American Antiquity* 50(2):412–420.

Roberts, D. D.
2007 *Historicism and Fascism in Modern Italy*. University of Toronto Press, Toronto.

Romney, A. K.
1957 The Genetic Model and Uto-Aztecan Time Perspective. *Davidson Journal of Anthropology* 3(2):35–41.

Rouse, I.
1939 *Prehistory in Haiti: A Study in Method*. Yale University Press, New Haven, CT.
1953 The Strategy of Culture History. In *Anthropology Today*, ed. A. L. Kroeber, 57–76. University of Chicago Press, Chicago.
1954 Comment on Taylor's "Southwestern Archaeology: Its History and Theory." *American Anthropologist* 56:572–574.

Rowe, J. H.
1975 The Spelling of "Archaeology." *American Anthropological Association* Anthropology Newsletter 16(6):11–12.

Rupp, D. W.
1997 Constructing the Cypriot Iron Age: Present Praxis, Future Possibilities. *Bulletin of American Schools of Oriental Research* 308:69–75.

Rutsch, M.
2000 Franz Boas und Ezequiel A. Chávez: Internationale Wissenschaftsbeziehungen in politisch unruhiger Zeit 1910–17. *Zeitschrift für Ethnologie* 125(2000): 39–52.

Sabloff, J. A.
1987 Foreword. In *Essays in Maya Archaeology,* G. Willey. University of New Mexico Press, Albuquerque.
1990 *The New Archaeology and the Ancient Maya*. Scientific American Library, New York.
1998 Intellectual Legacy of Lewis R. Binford. In *Conversations with Lew Binford: Drafting the New Archaeology*, ed. P. Sabloff, 77–91. University of Oklahoma Press, Norman.
2004 Looking Backward and Looking Forward: How Maya Studies of Yesterday Shape Today. In *Continuities and Changes in Maya Archaeology: Perspectives at the Millennium*, ed. C. Golden and G. Borgstede, 13–20. Routledge, New York.
2007 Conclusion. In *Gordon R. Willey and American Archaeology*, ed. J. A. Sabloff and W. Fash, 233–236. University of Oklahoma Press, Norman.

Sabloff, P.
1998 *Conversations with Lew Binford: Drafting the New Archaeology*. University of Oklahoma Press, Norman.

Sapir, E.
1921 *Language: An Introduction to the Study of Speech*. Oxford University Press, London.

Schele, L., and M. E. Miller
1986 *The Blood of Kings: Dynasty and Ritual in Maya Art*. Kimball Art Museum, Fort Worth, TX.

Schiffer, M. B.
1972 Archaeological Context and Systemic Context. *American Antiquity* 37(2): 156–165.

Schoenwetter, J.
1960 Pollen Analysis of Sediments from Matty Wash. M.S. thesis, Botany Department, University of Arizona, Tucson.
1962a A Late Post-glacial Pollen Chronology from the Central Mississippi Valley. In *American Bottoms Archaeology, First Annual Report: Illinois Archaeological Survey*, ed. M. L. Fowler, 39–48. University of Illinois Press, Champaign-Urbana.

1962b The Pollen Analysis of Eighteen Archaeological Sites in Arizona and New Mexico. In *Chapters in the Prehistory of Northern Arizona 1*, ed. P. S. Martin, 53:168–209. Chicago Natural History Museum, Fieldiana: Anthropology, Chicago.

1963 Survey of Palynological Results. In *American Bottoms Archaeology, Second Annual Report: Illinois Archaeological Survey*, ed. M. L. Fowler, 42–45. University of Illinois Press, Champaign-Urbana.

1968 An Ecological Interpretation of Anasazi Settlement Patterns. In *Anthropological Archaeology in the Americas*, ed. B. J. Meggers, 41–66. Anthropological Society of Washington, Washington, DC.

1990 A Method for the Application of Pollen Analysis in Landscape Archaeology. In *Earth Patterns: Essays in Landscape Archaeology*, ed. W. M. Kelso and R. Most, 277–296. University Press of Virginia, Charlottesville.

Schoenwetter, J., and F. W. Eddy

1964 *Alluvial and Palynological Reconstruction of Environments, Navajo Reservoir District*. Museum of New Mexico, Albuquerque.

Schrire, C.

1995 *Digging through Darkness: Chronicles of an Archaeologist.* University Press of Virginia, Charlottesville.

Schuyler, R. L.

1971 The History of American Archaeology: An Examination of Procedure. *American Antiquity* 36:383–409.

Shanks, M., and I. Hodder

1998 Processual, Postprocessual, and Interpretive Archaeologies. In *Reader in Archaeological Theory: Postprocessual and Cognitive Approaches*, ed. D. Whitley, 69–95. Routledge, New York.

Shanks, M., and C. Tilley

1987 *Re-Constructing Archaeology: Theory and Practice*. Cambridge University Press, Cambridge.

Sharer, R. J.

1978 Archaeology and History at Quirigua, Guatemala. *Journal of Field Archaeology* 5(1):51–70.

1991 Diversity and Continuity in Maya Civilization: Quirigua as a Case Study. In *Classic Maya Political History: Hieroglyphic and Archaeological Evidence,* ed. T. P. Culbert, 180–198. Cambridge University Press, Cambridge.

1994 *The Ancient Maya*. 5th ed. Stanford University Press, Stanford, CA.

2000 *Early Copan Acropolis Program: 2000 Field Season*. University of Pennsylvania Museum, Philadelphia.

Sharer, R. J., and W. Ashmore

1987 *Archaeology: Discovering Our Past*. McGraw-Hill, Mountain View, CA.

2002 *Archaeology: Discovering Our Past*. 3rd ed. McGraw-Hill, Mountain View, CA.

Sharer, R. J., and C. W. Golden
2004 Kingship and Polity: Conceptualizing the Maya Body Politic. In *Continuities and Changes in Maya Archaeology: Perspectives at the Millennium*, ed. C. W. Golden and G. Borgstede, 23–50. Routledge, New York.

Sharer, R. J., and L. P. Traxler
2006 *The Ancient Maya*. 6th ed. Stanford University Press, Stanford, CA.

Sharer, R. J., L. P. Traxler, D. W. Sedat, E. E. Bell, M. A. Canuto, and C. Powell
1999 Early Classic Architecture beneath the Copan Acropolis: A Research Update. *Ancient Mesoamerica* 10(1):3–23.

Sharp, R.
1978 Architecture as Inter-elite Communication in Preconquest Oaxaca, Veracruz and Yucatan. In *Middle Classic Mesoamerica AD 400–700*, ed. E. Pasztory, 158–171. Columbia University Press, New York.

Shawcross, F. W.
1967 An Investigation of Prehistoric Diet and Economy on a Coastal Site at Galatea Bay, New Zealand. *Proceedings of the Prehistoric Society* 33:107–131.

Shennan, S. J.
1989 *Archaeological Approaches to Identity*. Unwin Hyman, London.

Shepard, A. O.
1936 The Technology of Pecos Pottery. In *The Pottery of Pecos*, ed. A. V. Kidder and A. O. Shepard, 2:389–587. Papers of the Phillips Academy Southwestern Expedition 7. Yale University Press, New Haven, CT.

Simpson, G. G., and A. Roe
1939 *Quantitative Zoology*. Harcourt, Brace and World, New York.

Simpson, G. G., A. Roe, and R. C. Lewontine
1960 *Quantitative Zoology: A Revised Edition*. Rev. ed. Harcourt, New York.

Smith, C., and H. M. Wobst (editors)
2005 *Indigenous Archaeologies: Decolonizing Theory and Practice*. Routledge, London.

Smith, L.
2004 *Archaeological Theory and the Politics of Cultural Heritage.* Routledge, London.

Spaulding, A. C.
1953 Statistical Techniques for the Discovery of Artifact Types. *American Antiquity* 305–313.
1960 Statistical Description and Comparison of Artifact Assemblages. In *Application of Quantitative Methods in Archaeology*, ed. R. F. Heizer and S. F. Cook, 60–93. Publications in Anthropology, vol. 28. Wenner Gren Foundation, New York.
1968 Explanation in Archaeology. In *New Perspectives in Archaeology*, ed. S. R. Binford and L. R. Binford, 33–41. Aldine, Chicago.
1985 Fifty Years of Theory. *American Antiquity* 50(2):301–308.

Spencer-Wood, S. M.

2000 Strange Attractors: Feminist Theory, Nonlinear Systems Theory, and Their Implications for Archaeological Theory. In *Social Theory in Archaeology*, ed. M. B. Schiffer, 112–125. University of Utah Press, Salt Lake City.

Spinden, H. J.

1913 *A Study of Maya Art*. Memoirs 4. Peabody Museum of American Archaeology and Ethnology, Cambridge, MA.

Steggerda, M.

1941 *Maya Indians of Yucatán*. Carnegie Institution of Washington, Washington, DC.

Stephens, J. L.

1841 *Incidents of Travel in Central America, Chiapas, and Yucatan*. Harper, New York.

1843 *Incidents of Travel in Yucatan.* Harper, New York.

Sterud, E. L.

1978 Changing Aims of Americanist Archaeology: A Citations Analysis of American Antiquity, 1946–1975. *American Antiquity* 43(2):294–302.

Steward, J. H.

1937 Ecological Aspects of Southwestern Society. *Anthropos* 32:87–104.

1938 Basin-Plateau Aboriginal Sociopolitical Groups. *Bureau of American Ethnology Bulletin* 120.

1941 Archaeological Reconnaissance of Southern Utah. *Bureau of American Ethnology Bulletin* 128:277–356.

1942 The Direct Historical Approach to Archaeology. *American Antiquity* 7:337–343.

1955 *Theory of Culture Change: The Methodology of Multilinear Evolution*. University of Illinois Press, Urbana.

1977 Evolution and Ecology. In *Essays on Social Transformation*, ed. J. C. Steward and R. F. Murphy. University of Illinois Press, Urbana.

Steward, J. H., and F. M. Setzler

1938 Function and Configuration in Archaeology. *American Antiquity* 4:4–10.

Stocking, G. W.

1968 The Scientific Reaction against Cultural Anthropology. In *Race, Culture and Evolution*, ed. G. W. Stocking Jr., 270–307. Free Press, New York.

1989 *Romantic Motives*. University of Wisconsin Press, Madison.

1996 Schneider on Kluckhohn, 1964: Myth and Memory, the Oral and the Written, Fact and Fabrication, Historical Retrospect and Self-Representation in the Historiography of Modern American and British Anthropology. *HAN* 1996(2).

Straus, L. G.

1999 Review of *Conversations with Lew Binford: Drafting the New Archaeology,* ed. P. Sabloff. *Journal of Anthropological Research* 55(2):293–295.

Strong, W. D.

1936 Anthropological Theory and Archaeological Fact. In *Essays in Anthropology Presented to A. L. Kroeber*, ed. R. H. Lowie, 359–368. University of California Press, Berkeley.

1952 The Value of Archaeology in the Training of Professional Anthropologists. *American Anthropologist* 54:318–321.

Tallgren, A. M.

1937 The Method of Prehistoric Archaeology. *Antiquity* 11:152–161.

Tanner, W. F.

1993 An 8,000-Year Record of Sea-Level Change from Grain-Size Parameters: Data from Beach Ridges in Denmark. *The Holocene* 3:220–231.

Tate, C.

1992 *Yaxchilan: The Design of a Maya Ceremonial City*. University of Texas Press, Austin.

Taylor, L.A.P.

1940 *Plants Used as Curatives by Certain Southeastern Tribes*. Botanical Museum of Harvard University, Cambridge, MA.

Taylor, W. W.

1935 Quantitative Analysis in Connecticut Archaeology. *Connecticut Archaeological Society Bulletin* 2:2–23.

1937 Report of an Archaeological Survey of Coahuila, Mexico. *New Mexico Anthropologist* 2(2):45–46.

1938 Preliminary Report on Sites in Coahuila, Mexico [summary]. *New Mexico Anthropologist* 84.

1939a Draft of Paper on Maya Figurines, dated February 23, 1939. National Anthropological Archives, Smithsonian Institution, Washington, DC.

1939b Draft of Paper on the Maya Ceremonial Bar, dated April 11, 1939. National Anthropological Archives, Smithsonian Institution, Washington, DC.

1939c Letter to Clyde Kluckhohn, dated October 2, 1939. National Anthropological Archives, Smithsonian Institution, Washington, DC.

1941a The Ceremonial Bar and Associated Features of Maya Ornamental Art. *American Antiquity* 7(1):48–63.

1941b Review of *The Elements of Mazatec Witchcraft*, by Jean B. Johnson. *Journal of American Folklore* 54(211/212):111–112.

1943 A Study of Archaeology: A Dialectical, Practical and Critical Discussion with Special Reference to American Archaeology and the Conjunctive Approach. Ph.D. dissertation, Department of Anthropology, Harvard University, Cambridge, MA.

1948 *A Study of Archeology*. American Anthropological Association Memoir 69. American Anthropological Association, Menasha, WI.

1949 *[Field Notebook] Survey in Tsegi Area*. TP/NAA, Washington, DC.

1950 Rejoinder to Comments on Taylor's *A Study of Archeology*, by Robert F. Burgh. *American Anthropologist* 52:117–119.

1954 Southwestern Archaeology: Its History and Theory. *American Anthropologist* (Special Southwest Issue, ed. Emil W. Haury) 56(4):561–575.
1956 Some Implications of the Carbon-14 Dates from a Cave in Coahuila, Mexico. *Texas Archeological Society Bulletin* 27:215–234.
1957a A Clearing House or Central Agency. In *The Identification of Non-Artifactual Archaeological Materials*, ed. W. W. Taylor, 565:61–62. National Academy of Sciences, National Research Council, Washington, DC.
1957b *The Identification of Non-Artifactual Archaeological Materials*. Publication 565. National Academy of Sciences, National Research Council, Washington, DC.
1957c What the Archaeologist Needs from the Specialist. In *The Identification of Non-Artifactual Archaeological Materials*, ed. W. W. Taylor, 565:11–13. National Academy of Sciences, National Research Council, Washington, DC.
1958a Archaeological Survey of the Mexican Part of Diablo Reservoir. In *Appraisal of the Archaeological Resources of Diablo Reservoir, Val Verde County, Texas*, ed. J. A. Graham and W. A. Davis, Archaeological Salvage Program Field Office, Austin, Texas.
1958b *Two Archaeological Studies in Northern Arizona, The Pueblo Ecology Study: Hail and Farewell*, and *A Brief Survey through the Grand Canyon of the Colorado River*. Museum of Northern Arizona Bulletin 30.
1960a Reply to Macneish. *American Antiquity* 26(2):263–266.
1960b Review of *Preliminary Archaeological Investigations in the Sierra de Tamaulipas, Mexico*, by Richard S. MacNeish. *American Antiquity* 25(3):434–436.
1961 Archaeology and Language in Western North America. *American Antiquity* 27(1):71–81.
1963 Leslie Spier, 1893–1961 [Obituary]. *American Antiquity* 28(3):379–381.
1964 Tethered Nomadism and Water Territoriality: An Hypothesis. *Actas y Memorias*, XXXV International Congress of Americanists, Mexico City, 1962, 2:197–203.
1966a Archaic Cultures Adjacent to the Northeastern Frontiers of Mesoamerica. *Handbook of Middle American Indians* 4:59–94.
1966b The Concept of Culture and the Analysis of Difference. In *Actas y Memorias*, XXXVI International Congress of Americanists, Sevilla, 1964, 3:89–94.
1967a The Sharing Criterion and the Concept of Culture. In *American Historical Anthropology; Essays in Honor of Leslie Spier*, ed. C. L. Riley and W. W. Taylor, 221–230. Southern Illinois University Press, Carbondale.
1967b *A Study of Archeology*. Reprint of 1948 edition. Arcturus Books, Southern Illinois University Press, Carbondale.
1968a A Burial Bundle from Coahuila, Mexico. *Papers of the Archaeological Society of New Mexico* 23–56.
1968b *A Study of Archeology*. Arcturus Books, Southern Illinois University Press, Carbondale.
1969 Review of *New Perspectives in Archaeology*, by Sally R. Binford and Lewis R. Binford. *Science* 165:382–384.
1972a Emic Attributes and Normative Theory in Archaeology. *Actas y Memorias*, XL International Congress of Americanists, Rome, 67–69.

1972b The Hunter-Gatherer Nomads of Northern Mexico: A Comparison of the Archival and Archaeological Records. *World Archaeology* 4(2):167–178.

1972c Old Wine and New Skins: A Contemporary Parable. In *Contemporary Archaeology: A Guide to Theory and Contributions*, ed. M. P. Leone, 28–33. Southern Illinois University Press, Carbondale.

1973a Clyde Kluckhohn and American Archaeology. In *Culture and Life: Essays in Memory of Clyde Kluckhohn*, ed. W. W. Taylor, J. L. Fischer, and E. Z. Vogt, 14–29. Southern Illinois University Press, Carbondale.

1973b The Nature and Nurture of Archeology: A Prospect. In *Research and Theory in Current Archaeology*, ed. C. L. Redman, 281–285. John Wiley and Sons, New York.

1973c Storage and the Neolithic Revolution. In *Estudios Dedicados a Prof. Dr. Luis Pericot.* Publicaciones Eventuales, 23:193–197. Instituto de Arqueologia y Prehistoria, Universidad de Barcelona, Barcelona.

1983 *A Study of Archeology* (reprint of the 1948 edition, with a foreword by Patty Jo Watson). Center for Archaeological Investigations, Souther Illinois University Press, Carbondale.

1988 *Contributions to Coahuila Archaeology, with an Introduction to the Coahuila Project*. Southern Illinois University Press, Carbondale.

2003 *Sandals from Coahuila Caves, with an Introduction to the Coahuila Project, Coahuila, Mexico: 1937–1941, 1947*. Dumbarton Oaks Research Library and Collection, Washington, DC.

N.d.a *Anthropology 505, Proseminar in Linguistics: Preliminary Topical Outline and Bibliography*. National Anthropological Archives, Smithsonian Institution, Washington, DC.

N.d.b *Psychology of Culture—Sapir*. National Anthropological Archives, Smithsonian Institution, Washington, DC.

Taylor, W. W., and W. Y. Adams

1951 Pueblo Ecology Survey: Permanent Site Record. Unpublished field notes. TP/NAA, Washington, DC.

Taylor, W. W., and W. C. Boyd

1943 Blood Groups of the Pre-historic Indians of Coahuila by Serological Tests of Their Mummified Remains. *Yearbook of American Philosophical Society* 178–180.

Taylor, W. W., and R. C. Euler

1980 Lyndon Lane Hargrave, 1896–1978. *American Antiquity* 45(3):477–482.

Taylor, W. W., J. L. Fischer, and E. Z. Vogt (editors)

1973 *Culture and Life: Essays in Memory of Clyde Kluckhohn*. Southern Illinois University Press, Carbondale.

Taylor, W. W., and F. G. Rul

1960 An Archaeological Reconnaissance behind the Diablo Dam, Coahuila, Mexico. *Bulletin of the Texas Archeological Society* 31:153–165.

Thomas, D. H.

1978 The Awful Truth about Statistics in Archaeology. *American Antiquity* 43(2): 231–244.

1979 *Archaeology*. Harcourt College Publishers, Fort Worth, TX.
2000 *Skull Wars: Kennewick Man, Archaeology, and the Battle for Native American Identity*. Basic Books, New York.

Thomas, D. H., and R. L. Kelly
2007 *Archaeology: Down to Earth*. Thompson Wadsworth, Belmont, CA.

Thompson, J.E.S.
1939 The Moon Goddess in Middle America: With Notes on Related Deities. In *Contributions to American Anthropology and History* 5(29):121–173. Carnegie Institution of Washington, Washington, DC.
1942 The Civilization of the Mayas. *Field Museum of Natural History Leaflet* 25.
1950 *Maya Hieroglyphic Writing: An Introduction*. Publication 589. Carnegie Institution of Washington, Washington, DC.

Thompson, R. H.
1995 Emil W. Haury and the Definition of Southwestern Archaeology. *American Antiquity* 60(4):640–660.

Tourtellot, G.
1993 View of Ancient Maya Settlements in the Eighth Century A.D. In *Lowland Maya Civilizations in the 8th Century*, ed. J. A. Sabloff and J. S. Henderson, 219–241. Dumbarton Oaks, Washington, DC.

Tozzer, A. M.
1907 *A Comparative Study of the Mayas and Lacandones*. Archaeological Institute of America and the Macmillan Company, New York.
1911 A Preliminary Study of the Prehistoric Ruins of Tikal, Guatemala: A Report of the Peabody Museum Expedition, 1909–1910. *Memoirs of Peabody Museum American Archaeology and Ethnology* 5(2).
1913 A Preliminary Study of the Prehistoric Ruins of Nakum, Guatemala: A Report of the Peabody Museum Expedition, 1909–1910. *Memoirs of Peabody Museum American Archaeology and Ethnology* 5(3).
1921 Excavation of a Site at Santiago Ahuitzotla, D.F., Mexico. *Bureau of American Ethnology Bulletin* 74.
1937 Prehistory in Middle America. *Hispanic American Historical Review* 17:151–159.
1941 *Landa's Relación de las Cosas de Yucatán: A Translation*, ed. with notes by A. M. Tozzer. Papers of the Peabody Museum of Archaeology and Ethnology. Peabody Museum, Harvard University, Cambridge, MA.
1957 *Chichen Itza and Its Cenote of Sacrifice: A Comparative Study of Contemporaneous Maya and Toltec*. Memoirs 11–12. Peabody Museum of American Archaeology and Ethnology, Cambridge, MA.

Trigger, B. G.
1965 *History and Settlement in Lower Nubia*. Yale University Press, New Haven, CT.
1968a Archaeological and Other Evidence: A Fresh Look at the "Laurentian Iroquois." *American Antiquity* 33(4):429–440.
1968b Major Concepts of Archaeology in Historical Perspective. *Man* 3(4):527–541.
1971 Archaeology and Ecology. *World Archaeology* 2(3, Subsistence):321–336.

1978 *Time and Tradition: Essays in Archaeological Interpretation*. Columbia University Press, New York.

1980 Archaeology and the Image of the American Indian. *American Antiquity* 45(4):662–676.

1984 Alternative Archaeologies: Nationalist, Colonialist, Imperialist. *Man* 19(3): 355–370.

1989 *A History of Archaeological Thought*. Cambridge University Press, New York.

2003 *Archaeological Theory: The Big Picture*. Brigham Young University, Provo, UT.

2006 *A History of Archaeological Thought*. 2nd ed. University of Cambridge, Cambridge.

Tylor, E. B.

1871 *Primitive Culture: Researches into the Development of Mythology, Philosophy, Religion, Art, and Custom*. J. Murray, London.

Ucko, P.

1987 *Academic Freedom and Apartheid: The Story of the World Archaeological Congress*. Duckworth, London.

Uhle, M.

1903 *Pachacamac*. University of Pennsylvania Press, Philadelphia.

Vaillant, G. C.

1930 Excavations at Zacatenco. *Anthropological Papers of the American Museum of Natural History* 32(1):338–340.

Vogt, E. Z.

2004 Gordon Randolph Willey. In *Biographical Memoirs*, vol. 84. National Academies Press, Washington, DC.

Vogt, E. Z., and R. M. Leventhal

1983 *Prehistoric Settlement Patterns: Essays in Honor of Gordon R. Willey*. University of New Mexico Press, Albuquerque.

Walker, I.

1978 Historic Archaeology: Methods and Principles. In *Historical Anthropology: A Guide to Substantive and Theoretical Contributions*, ed. R. L. Schuyler, 208–215. Baywood, New York.

Watson, J. B.

1949 Review of *A Study of Archeology,* by W. W. Taylor. *Archaeology* 2(1):55.

Watson, P. J.

1973 The Future of Archaeology in Anthropology: Culture History and Social Science. In *Research and Theory in Current Archaeology*, ed. C. L. Redman, 113–124. John Wiley and Sons, New York.

1983 Foreword to the 1983 edition of *A Study of Archeology*, by W. W. Taylor, ix–xvi. Southern Illinois University, Center for Archaeological Investigations, Carbondale.

1986 Archaeological Interpretation, 1985. In *American Archaeology Past and Future*, ed. D. J. Meltzer, D. D. Fowler, and J. A. Sabloff, 439–457. Smithsonian Institution Press, Washington, DC.

1995 Archaeology, Anthropology, and the Culture Concept. *American Anthropologist* 97(49):683–694.

Watson, P. J., and M. Fotiadis
1990 The Razor's Edge: Symbolic-Structuralist Archaeology and the Expansion of Archaeological Inference. *American Anthropologist* 92:613–629.

Watson, P. J., S. A. LeBlanc, and C. L. Redman
1971 *Explanation in Archeology: An Explicitly Scientific Approach*. Columbia University Press, New York.
1984 *Archaeological Explanation: The Scientific Method in Archaeology*. Columbia University Press, New York.

Webb, R.
1994 *Call of the Colorado*. University of Idaho Press, Moscow.

Webster, D.
1999 Archaeology of Copán, Honduras. *Journal of Archaeological Research* 7(1): 1–53.
2000 The Not So Peaceful Civilization: A Review of Maya War. *Journal of World Prehistory* 14(1):65–119.

Webster, D., and A. Freter
1990 Settlement History and the Classic Collapse at Copan: A Redefined Chronological Perspective. *Latin American Antiquity* 1(1):66–85.

Webster, D. T., N. Gonlin, and A. C. Freter
2000 *Copán: The Rise and Fall of an Ancient Maya Kingdom*. Harcourt College Publishers, Fort Worth, TX.

Webster, L. D., and M. E. McBrinn (editors)
2008 *Archaeology without Borders: Contact, Commerce, and Change in the U.S. Southwest and Northwestern Mexico*. University Press of Colorado, Boulder.

Weeks, J. M., and J. A. Hill
2006 *The Carnegie Maya: The Carnegie Institute of Washington Maya Research Program, 1913–1957*. University Press of Colorado, Boulder.

West, D. W.
2002 Practical Criticism: I. A. Richards' Experiment in Interpretation. *Changing English* 9:207–213.

White, H.
1973 *Metahistory: The Historical Imagination in Nineteenth-Century Europe*. Johns Hopkins University Press, Baltimore.

White, L. A.
1949 *The Science of Culture: A Study of Man and Civilization*. Farrar, Strauss, New York.
1959a The Concept of Culture. *American Anthropologist, New Series* 61(2):227–251.
1959b *The Evolution of Culture: The Development of Civilization to the Fall of Rome*. McGraw-Hill, New York.

White, L. A., and B. Dillingham
1973 *The Concept of Culture*. Burgess Publishing Company, Minneapolis.

Wilcox, D. R., and J. Haas
1994 The Scream of the Butterfly: Competition and Conflict in the Prehistoric Southwest. In *Themes in Southwestern Prehistory: Grand Patterns and Local Variations in Culture Change*, ed. G. J. Gumerman, 211–238. School of American Research, Santa Fe, NM.

Wilk, R.
1985 The Ancient Maya and the Political Present. *Journal of Anthropological Research* 41:307–326.

Willey, G. R.
1945 Horizon Styles and Pottery Traditions in Peruvian Archaeology. *American Antiquity* 11:49–56.
1948 A Functional Analysis of "Horizon Styles" in Peruvian Archaeology. In *Reappraisal of Peruvian Archaeology*, ed. W. C. Bennett, 8–15. Society for American Archaeology and the Institute of Andean Research, Menasha, WI.
1951 Peruvian Horizon Styles. *American Antiquity* 16:353–354.
1953a Archeological Theories and Interpretation: New World. *Anthropology Today* 361–385.
1953b *Prehistoric Settlement Patterns in the Viru Valley, Peru*. Bureau of American Ethnology Bulletin 155. Smithsonian Institution, Washington, DC.
1954 Tradition Trend in Ceramic Development. *American Antiquity* 20:9–14.
1956 *Prehistoric Settlement Patterns in the New World*. Wenner-Gren Foundation, New York.
1966 *An Introduction to American Archaeology*. Prentice-Hall, Englewood Cliffs, NJ.
1968 One Hundred Years of American Archaeology. In *One Hundred Years of Archaeology*, ed. J. O. Brew, 29–56. Harvard University Press, Cambridge, MA.
1971 *An Introduction to American Archaeology*, vol. 2: *South America*. Prentice-Hall, Englewood Cliffs, NJ.
1976 Mesoamerican Civilization and the Idea of Transcendence. *Antiquity* 50(199–200):200–215.
1981 Maya Lowland Settlement Patterns: A Summary Review. In *Lowland Maya Settlement Patterns*, ed. W. Ashmore, 385–415. University of New Mexico, Albuquerque.
1988 *Portraits in American Archaeology: Remembrances of Some Distinguished Americanists*. University of New Mexico Press, Albuquerque.
1994 Macon, Georgia: A Fifty-Year Retrospect. In *Ocmulgee Archaeology 1936–1986*, ed. D. J. Hally. University of Georgia Press, Athens.

Willey, G. R., W. R. Bullard, J. B. Glass, and J. C. Gifford
1965 *Prehistoric Settlements in the Belize River Valley*. Papers of the Peabody Museum, Harvard University 54. Peabody Museum, Cambridge, MA.

Willey, G. R., and R. M. Leventhal
1979 Prehistoric Settlement at Copan. In *Maya Archaeology and Ethnohistory*, ed. N. Hammond and G. R. Willey, 75–102. University of Texas Press, Austin.

Willey, G. R., and P. Phillips

1955 Method and Theory in American Archeology, 2: Historical-Developmental Interpretation. *American Anthropologist* 57:723–819.

1958 *Method and Theory in American Archaeology*. University of Chicago Press, Chicago.

Willey, G. R., and J. A. Sabloff

1974 *A History of American Archaeology*. W. H. Freeman, San Francisco.

1980 *A History of American Archaeology*. 2nd ed. W. H. Freeman, San Francisco.

1993 *A History of American Archaeology*. 3rd ed. W. H. Freeman, New York.

Williams, D.

1996 Archaeology in the Guianas. *SAA Bulletin* 14(1):10–12.

Winters, H. D.

1969 The Riverton Culture: A Second Millennium Occupation in the Central Wabash Valley. *Reports of Investigations: Illinois State Museum* 13.

Wissler, C.

1910 Material Culture of the Blackfoot Indians. *Anthropological Papers of the American Museum of Natural History* 5(1):1–176.

1911 The Social Life of the Blackfoot Indians. *Anthropological Papers of the American Museum of Natural History* 7(1).

1912 Ceremonial Bundles of the Blackfoot Indians. *Anthropological Papers of the American Museum of Natural History* 7(2).

1917 *The American Indian: An Introduction to the Anthropology of the New World*. Douglas C. McMurtrie, New York.

Witkind, I. J., R. E. Thaden, and C. F. Lough

1952 File Report on the Archaeological Sites in the Monument Valley Area, Arizona. Submitted to Bureau of American Ethnology, Smithsonian Institution. Copy in TP/NAA, Washington, DC.

Woodbury, R. B.

1954 An Appraisal of *A Study of Archeology* by Walter W. Taylor. *American Antiquity* 19(3):292–296.

1956 The Antecedents of Zuni Culture. *Transactions of the New York Academy of Sciences* series 2, vol. 18:557–563.

1959 A Reconsideration of Pueblo Warfare in the Southwestern United States. *Actas del XXXIII Congreso Internacional de Americanistes* 2:124–133.

1973a *Alfred V. Kidder*. Columbia University Press, New York.

1973b Getting Round Archaeologists out of Square Holes. In *Research and Theory in Current Archaeology*, ed. C. L. Redman, 311–317. John Wiley, New York.

1993 *60 Years of Southwestern Archaeology: A History of the Pecos Conference*. University of New Mexico Press, Albuquerque.

Wylie, A.

1982 Positivism and the New Archaeology. Ph.D. dissertation, Department of Philosophy, State University of New York at Binghamton, New York.

2002 *Thinking from Things: Essays in the Philosophy of Archaeology*. University of California Press, Berkeley.

2003 On Ethics. In *Ethical Issues in Archaeology*, ed. L. J. Zimmerman, K. D. Vitelli, and J. Howell-Zimmer, 3–16. Altamira, Oxford.

2006 Moderate Relativism / Political Objectivism. In *The Archaeology of Bruce Trigger*, ed. R. R. Williamson and M. S. Bisson, 25–35. McGill-Queen's University Press, Montreal.

Yaeger, J.

2009 A Conjunctive Approach to Understanding the Classic Maya Collapse. Paper presented in the invited session Understanding Maya Civilization: Papers in Honor of Robert Sharer at the 74th Annual Meeting of the Society for American Archaeology, Atlanta, Georgia.

Yaeger, J., and G. Borgstede

2004 Professional Archaeology and the Modern Maya: A Historical Sketch. In *Continuities and Changes in Maya Archaeology: Perspectives at the Millennium*, ed. C. Golden and G. Borgstede, 259–286. Routledge, New York.

Yoffee, N.

2005 *Myths of the Archaic State: Evolution of the Earliest Cities, States, and Civilizations*. University of Cambridge Press, Cambridge.

Zachary, G. P.

1997 *Endless Frontier: Vannevar Bush, Engineer of the American Century*. Free Press, New York.

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