

## Notes on Historical Ethnography, Paradigms, and Social Networks in Academia

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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,<sup>1</sup> 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 festschriften, 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 conjunc-

tive 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.<sup>3</sup> 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 post-modern 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).<sup>3</sup> This makes the book, again in Deleuze and Guattari's terms, rhizomatic not arboreal.<sup>4</sup> Further, it makes the volume as a whole Tayloresque 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.<sup>5</sup>

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 bad-mouthing, 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 reconstruction, 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 socio-political 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.<sup>6</sup> 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]).<sup>7</sup>

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 reconstructionist 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 meta-commentaries 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<sup>8</sup>: 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.<sup>9</sup> 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.<sup>10</sup> 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)<sup>11</sup>

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,"<sup>12</sup> 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.<sup>13</sup> 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),<sup>14</sup> 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.

*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.”

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<sup>1</sup> 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 Taylolean 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 Taylolean 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 Taylolean 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.”<sup>2</sup> 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 Taylolean 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).

---

*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

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

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*HERACLEIDAE*, CA. 428 BC

Walter W. Taylor  
and Dissension  
in American  
Archaeology

EDITED BY

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

FOREWORD BY

Linda S. Cordell

**Prophet, Pariah, AND Pioneer**

U N I V E R S I T Y   P R E S S   O F   C O L O R A D O

For the mavericks, synthesizers, and dissenters

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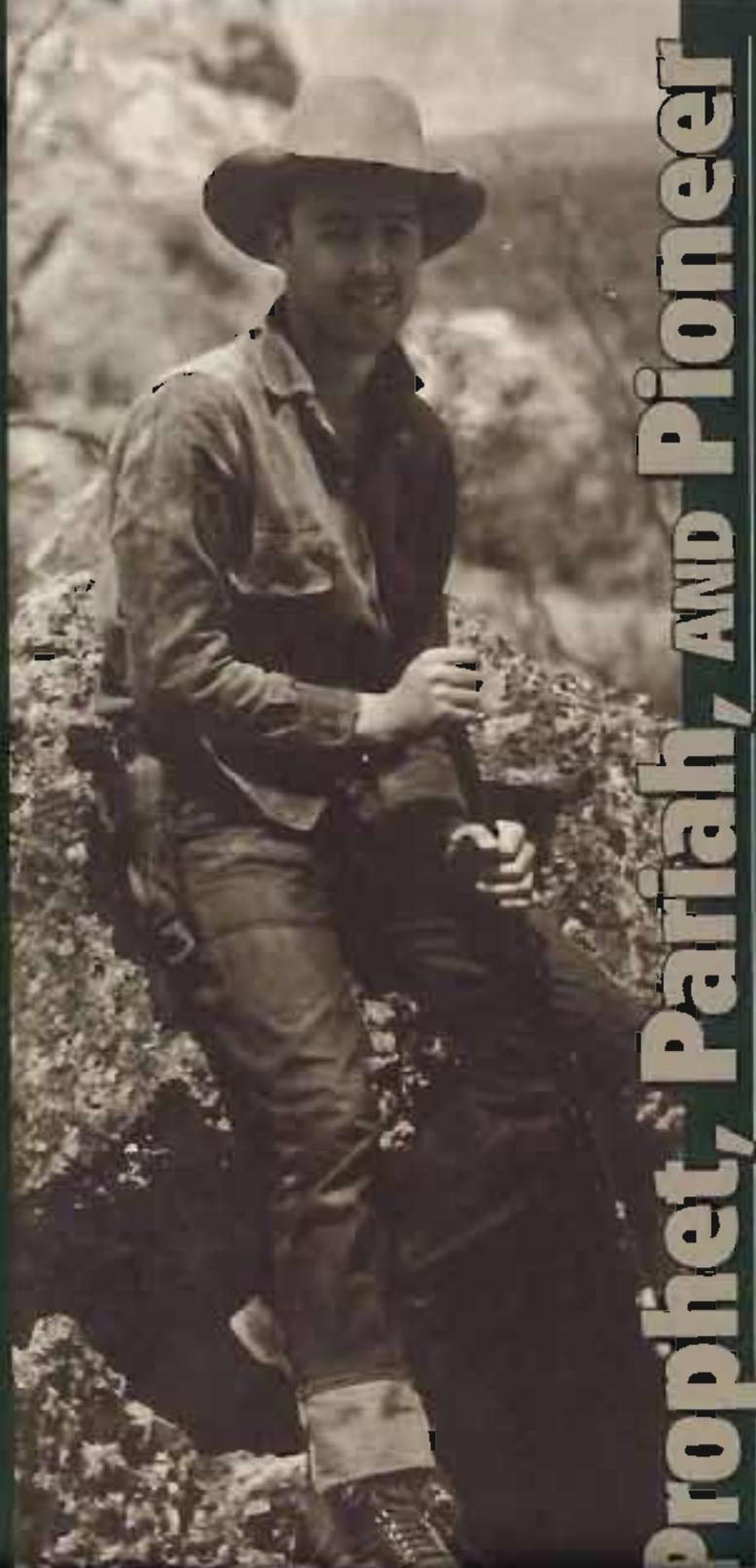
"Taylor made enemies and had difficulty implementing his research agenda for reasons the contributors to this volume explore in detail, but the fact is that the shortcomings of early twentieth-century approaches continue to haunt archaeology. Many perspectives that are seen as innovative today . . . owe an intellectual debt to Taylor."

—Linda Cordell,  
FROM THE FOREWORD

Maca  
Reyman  
Folan

# Prophet, Pariah, AND Pioneer

Walter W. Taylor and Dissension in American Archaeology



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and Dissension  
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In his 1948 work *A Study of Archaeology*, recently named Harvard Ph.D. Walter W. Taylor delivered the strongest and most substantial critique of American archaeology ever published. He created many enemies with his dissection of the research programs of America's leading scholars, who took it as a personal affront. Taylor subsequently saw his research pushed to the margins, his ideas censured, and his students punished. Publicly humiliated at the 1983 Society for American Archaeology meeting, he suffered ridicule until his death in 1997.

Nearly everyone in the archaeological community read Taylor's book at the time, and despite the negative reaction, many were influenced by it. Few young scholars dared to directly engage and build on his "constructive approach," yet his suggested methods nevertheless began to be adopted and countless present-day authors highlight his impact on the 1960s formation of the "New Archaeology."

In *Prophet, Pariah, and Pioneer*, peers, colleagues, and former students offer a critical consideration of Taylor's influence and legacy. Neither a festschrift nor a mere analysis of his work, the book presents an array of voices exploring Taylor and his influence, sociologically and intellectually, as well as the culture of American archaeology in the second half of the twentieth century.

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