Philosophy and Archaeology

Reflections on Thinking: History, Philosophy, and American Archaeology

By Michael J. O'Brien

Thinking from Things: Essays in the Philosophy of Archaeology (2002) ALISON WYLIE. University of California Press, Berkeley xviii + 339 pages. $39.95. ISBN 0-520-22360-8.

Alison Wylie's book *Thinking from Things: Essays in the Philosophy of Archaeology* contains the standard kind of promotional blurb that one expects to find on a book cover or jacket. Editors usually have someone on their staff take the first shot at writing such a piece, then run it by the author for editing. Promotional introductions by force are succinct—there isn't a lot of space available—but are they necessarily accurate appraisals of a book's importance? They might be, but a promotional piece is just that—something written for the express purpose of helping to sell a book. We don't expect to see, for example, an introduction that says, "In this ponderous, overwritten, and poorly researched book, the author begs us to believe that he's proved that a Chinese fleet made its way to the North American continent in 1421."

No editor, let alone an author, would let something like that make its way onto a book cover or jacket, no matter how much veracity the statement might have. Rather, we would expect to see something like what actually appears on the jacket: "*1421: The Year China Discovered America* is the story of a remarkable journey of discovery that rewrites our understanding of history." In reality, the book (Menzies 2003) does no such thing, being at best a work of fiction, but a promotional introduction is not going to point that out. It's there to help sell the book, as are the select quotations that come from a glowing review in the London *Evening Standard.*

Armed with the proper skepticism, let's see what Wylie's book is about. For one thing, it tells us that archaeology is "a deeply philosophical discipline" and that Wylie, "one of the field's most important theorists," explores "how archaeologists know what they know." For another thing, it tells us that she "examines the history and methodology of Anglo-American archaeology, putting the tumultuous debates of the last thirty years in historical and philosophical perspective." The back cover also contains, not unexpectedly, testimonials as to the book's importance—one from philosopher of science Merrilee Salmon and one from archaeologist George Cowgill. Salmon believes that the book is "for anyone who wants to understand contemporary archaeological theory; how it came to be as it is, its relationship with other disciplines, and its prospects for the future." Cowgill opines that Wylie "is a reasonable and astute thinker who . . . commands both philosophy and archaeology to an unusual degree."

Sounds like the standard fare for back covers, so why even bother to highlight what appears there? I do it because for once, an introduction and the testimonials that accompany it accurately reflect a book's content and significance. Wylie *is* an astute thinker; she *does* put the tumultuous debates of the post-1970 period in historical and philosophical perspective; and the book *is* for anyone who wants to understand contemporary archaeological theory. Impressively, Wylie treats theory in a non-snoozy manner. She can't make the story line quite as lively as Chinese treasure fleets reaching the New World, but her writing style helps keep the reader's interest. Wylie, like Salmon, is one of the few philosophers competent to comment on both archaeology and philosophy. What places Wylie in even more of a minority is that she actually was trained as both a philosopher and an archaeologist, first at Mount Allison University in New Brunswick and then at the State University of New York at Binghamton. As witnessed in the early 1970s, many American archaeologists acted as if they were trained as both, but the published record strongly suggests that for the most part they were better archaeologists than philosophers.

I use this essay as a springboard from which to explore a few select aspects of American archaeology's forays into philosophical issues. As such, it is not so much a review of Wylie's book as it is a glimpse at a few points along the path of archaeology's growth, using several of Wylie's essays as a rough guide. I bypass discussion of numerous important topics that Wylie addresses in various places, including critical theory, archaeology and gender, argument from analogy, and ethics. I refer interested readers to Marcia-Anne Dobres's (2004) review of Wylie's book in *American Antiquity,* in which she highlights some of these topics. In the interest of disclosure I note that I reviewed Wylie's book for *American Anthropologist,* although I could accomplish little in 750 words other than to state that the book serves a useful purpose as an introduction to the role of philosophy in archaeology.

The issue that most concerns me here, as the cover blurb on Wylie's book phrases it, is "how archaeologists know what they know" about the past. I would add two words to that phrase, making it read, "how archaeologists *think* they know what they know" about the past. There is a big difference. There also is a big difference between the cover quote, "archaeology is a deeply philosophical discipline," meaning that it is philosophical in the abstract, and "archaeology *as practiced* is a deeply philosophical discipline." Archaeology most definitely *is* a deeply philosophical field of inquiry, although the manner in which it often is practiced does not do much to reflect this point. In fact, I might go so far as to further amend the first statement above, making it now read, "how archaeologists think they know what they know . . . when they take the time to think about it."

Philosophy and History

Philosophy is the rational investigation of questions about knowledge—*how* we know what we think we know. But even if we accept that archaeology is by nature a deeply philosophical discipline, does this mean that we can't do good archaeology without thinking about philosophical issues—or "thinking from things," as Wylie phrased it in the title of her book? Maybe we *can* do "good" archaeology without explicitly thinking too much about such issues, but there is every reason to suspect that we can do better archaeology by adopting a philosophical point of view. Wylie's book, especially Chapter 6, "Between Philosophy and Archaeology," is a good companion guide.

There's something else that all archaeologists could profit from, namely, having a basic knowledge of the history of the discipline, especially where philosophical issues are concerned. There are a number of reasons why someone might benefit from an awareness of disciplinary history, including what Gordon Willey and Jeremy Sabloff singled out as the reason they wrote *A History of Archaeology*: so that "we can appreciate new developments in relation to those that have gone before; and from this historical perspective we may also see more clearly the significance of the new directions in which the field is moving" (Willey and Sabloff 1974:9).

As important as a knowledge of history is in helping us appreciate new directions, the reasons why such knowledge is important go deeper than this. Paul Bohannan and Mark Glazer (1988:xv) argued that we should study the history of a discipline to "save [ourselves] a good deal of unnecessary originality." As humorous as this might sound, their point is well taken. It would be difficult to count the times when archaeologists honestly believe they have devised a new concept or method and published a paper on it, only to have someone point out that someone else said the same thing fifty years earlier. Worse yet is when someone completely misrepresents a fact or argument because he cited a secondary source in which the author scrambled the original information. These embarrassing predicaments result from being ignorant of a discipline's history.

On a different note, it often is stated that by understanding the history of a discipline, one can avoid the mistakes of one's predecessors (e.g., Mayr 1982). This is true, although I don't particularly like the word "mistakes." Many of the things we might count as mistakes in archaeology result from honest attempts to use available information to solve intellectual problems. Hindsight provides a unique perspective, but without clear parameters it can begin to border on smugness and condescension. At best, we become historical revisionists. Take, for example, the term *epistemology,* the branch of philosophy that deals with the origin and nature of knowledge. The odds are small that we will find more than an occasional use of the term in archaeology before the 1970s, when the philosophy of science (or one brand of it) became the centerpiece of a new Americanist movement. (Gordon Lowther used it in his 1962 article "Epistemology and Archaeological Theory" [Lowther 1962], but there probably are a few earlier examples.) At that point, "epistemology," along with "hypothetico-deductive," "nomological," and other borrowed terms became de rigeur in American archaeology.

Maybe archaeologists working in the pre-processualist days were not focused on epistemology, but were they thinking in *any* philosophical terms? As Wylie points out, one early foray into philosophy was by Clyde Kluckhohn (1939), who was technically an ethnographer, but one with considerable archaeological training. Kluckhohn's efforts were directed explicitly toward philosophy, and in this he was unique. Most archaeological ventures into philosophy were implicit or consisted of at best a brief quote and citation. For example, one of Kluckhohn's students, Walter Taylor, cited Frederick Teggart (e.g., 1925) and Maurice Mandelbaum (1938) in *A Study of Archeology* (Taylor 1948). Similarly, Betty Meggers (1955) cited Hans Reichenbach (1942); Raymond Thompson (1956) cited John Dewey (1938); and Albert Spaulding (1962) cited Gustav Bergman (1957) and John Kemeny (1959). Suffice it to say, however, that philosophy was not on many radar screens in the pre-processualist days. Does this mean that archaeologists working before then were not confronting philosophical issues? No, they were confronting them on a continual basis. It simply means that they weren't thinking philosophically in concrete terms. *Should* they have been thinking in concrete philosophical terms? Maybe, but to answer that question affirmatively gets us nowhere; the fact of the matter is, they weren't. It is, however, legitimate to point out that had they had some training in philosophy, American archaeology would have had a far different trajectory than the one it took.

Wylie's book is an excellent place to start if one wants a brief history of how American archaeologists have dealt with (and sometimes been consumed by) various epistemological issues. Specifically, the first five chapters, which were written expressly for the volume as opposed to being revisions of previously published papers, cover American archaeology from the opening decades of the twentieth century up through the post-processualist movement of the 1980s. I happened to pick up Wylie's book as Lee Lyman, Mike Schiffer, and I were finishing our history of American archaeology from about 1960 on, *Archaeology as a Process: Processualism and Its Progeny* (O'Brien et al. 2005). In perusing the opening chapters of her book, I was struck by some of the parallels in how Wylie and we had approached the issues raised by processualism and its intellectual offspring. I also was impressed with her treatment of the pre-1960 "culture historical" period—a subject whose various angles Lyman and I have explored in considerable depth (e.g., Lyman et al. 1997), especially those relating to four archaeologists who figure prominently in Wylie's story—James Ford (O'Brien and Lyman 1998, 1999), W. C. McKern (Lyman and O'Brien 2003), and Gordon Willey and Philip Phillips (Lyman and O'Brien 2001). I mention this to add credence to my claim that there may be no better synopsis of the intellectual history of the culture-history period than what Wylie provides in her first two chapters, "How New Is the New Archaeology?" and "The Typology Debate." In the pages that follow, I examine a few of the philosophical issues that Wylie discusses in those chapters and try to add to her perspective.

The New Archaeology

Just how "new" *was* the new archaeology? Received wisdom has long been split over the question of whether what Lewis Binford proposed in the 1960s as a way of approaching the archaeological record represented a paradigm shift in the sense that Thomas Kuhn (1962) used the term or whether it was a continuation, albeit at a heightened level, of what came before it (e.g., Custer 1981; Meltzer 1979). No doubt the new archaeology ushered in an era of awareness of certain principles that had not previously moved to the forefront of archaeological inquiry, but the same can be said of any period. The question is, was there a structural change in the way archaeology went about its business after 1960? Wylie downplays this question, focusing instead on the cyclic nature of "new archaeologies," which seem to roll around about every two decades or so. Clark Wissler (1917) used the term "new archaeology" early in the twentieth century to herald the stratigraphic work undertaken by Nels Nelson and others in the prehistoric pueblos of eastern New Mexico, and Joseph Caldwell (1959:304) used it four decades later to refer to an archaeology that was "tending to be more concerned with culture process and less concerned with the descriptive content of prehistoric cultures."

For a structural change to have occurred, the new archaeology of the 1960s, which became universally known as *processualism,* would have had to break not only methodologically but also metaphysically with its predecessor, and it is unclear that this happened. To be sure, major changes took place in terms of how archaeology was practiced, but in my opinion (not universally shared) there was no *conceptual* break. The concepts were, for the most part, already there. What Binford advocated so eloquently (e.g., Binford 1962, 1965) had been put forth, if sometimes only in incipient form, by, among others, Taylor (1948), Meggers (1955), Phillips (1955), and Caldwell (1959) during the preceding two decades. Their calls, however, had produced little change in how the discipline at large both conceptualized and approached the past. Binford had the proper mix of words, ambition, and charisma to effect large-scale change in how more than just a few archaeologists viewed what they were doing.

Binford's first major article, published in *American Antiquity* in 1962, typically is regarded as the birth announcement of processualism, although there was nothing particularly revolutionary about it at the time. It is clear that Binford himself did not see the article as a revolutionary piece but as more of an attempt to herd archaeology back into anthropology's pen. The title of that paper was "Archaeology as Anthropology," and its opening sentence read, "It has been aptly stated that 'American archaeology is anthropology or it is nothing' (Willey and Phillips 1958, p. 2)" (Binford 1962:217). That phrase had been reworded from an earlier statement by Phillips (1955:246–247), "New World archaeology is anthropology or it is nothing." The corralling of archaeology obviously had been on the agenda of the Phillips-and-Willey generation; Binford was just the latest hand to take a try at it.

Binford followed that seminal contribution with several articles during the 1960s (e.g., Binford 1963, 1964, 1965, 1967, 1968a) that set both the agenda and the tone for the new archaeology. Nothing, however, matched the book that he co-edited with his then-wife, Sally—*New Perspectives in Archeology* (Binford and Binford 1968)—which grew out of a symposium that the Binfords had put together for the American Anthropological Association meeting in Denver in November 1965. Charles Redman, a second-generation processualist, later referred to the publication of *New Perspectives in Archeology* as marking "the crossing of a threshold" (Redman 1991:296).

A number of contributors to the volume were graduate students at the University of Chicago when Binford taught there in the early '60s. Undeniably, part of Binford's success in fomenting change in American archaeology is attributable to his having around him a cadre of smart, ambitious students such as Bill Longacre, Kent Flannery, Leslie Freeman, Stuart Struever, Robert Whallon, and Sally Schanfield (later Binford). They, together with older (e.g., Patty Jo Watson and Frank Hole) and younger (e.g., John Fritz and Fred Plog) Chicago students, would form the core of the new archaeology (Longacre 2000).

The goal of these like-minded individuals was to study cultural processes and to contribute to anthropological theory. Like their predecessors, whom they often derided, the processualists understood that those processes, which are dynamic phenomena, are represented by a static archaeological record. They argued that two requirements had to be met before one could get at those processes. First, the notion of culture had to be changed from a normative, idea-based concept to one that was behavioral, systemic, and materialist. Second, archaeology had to be conducted scientifically, which to most processualists meant working deductively rather than inductively and using analogy, often ethnographic analogy, in a rigorous manner.

The inductive approach came to be equated, wrongly, with an archaeology that began and ended with rote descriptions of artifacts and assemblages. This is what Caldwell in his "New American Archaeology" paper in *Science* (1959:304) had labeled "dull and uninteresting." Inductive archaeology was seen as slow and tedious because, according to the processualists (e.g., Binford 1968b; Longacre 1970), traditionalists had to await the accumulation of sufficient data, which would enable the facts to speak for themselves. But according to the processualists, no accumulation of facts could speak unless the archaeologist asked processual questions and designed deductively oriented research programs to answer them.

Wylie provides excellent coverage of the processual movement, both in her introduction to the book and in Chapter 1. Not unexpectedly, her emphasis is on the interest that processualists had in the philosophy of science. Received wisdom holds that Binford was the person most responsible for infusing philosophy into processualism, but this is incorrect. Binford certainly cited a few philosophers on occasion, but he was not the archaeologist whose arguments had the biggest impact on the discipline. I would give that honor to Albert Spaulding.

When the Binfords organized their 1965 AAA symposium, they selected as chairmen Spaulding, who was on the faculty at the University of Oregon, and Paul Martin, who was on the research staff of the Field Museum of Natural History in Chicago. A specialist in ceramic typology, Martin spent his entire career working in the Southwest (Nash 2003), and over the years he generously provided resources that were used by generations of graduate students to apply their new ideas in his NSF–supported projects (Longacre 2000). By the time of the AAA symposium, a number of Chicago graduate students, including Longacre and Hill, had begun to produce processualist products based on work conducted with Martin's encouragement and financial support. Summaries of some of that work appeared in *New Perspectives in Archeology* (e.g., Hill 1968; Longacre 1968).

Spaulding was a person with whom Binford had taken courses at Michigan, and by his own account (Binford 1972) was someone Binford admired. Martin provided no paper for *New Perspectives in Archeology*, but Spaulding did—one he had presented at the annual meeting of the American Association for the Advancement of Science in 1965. In part because of his well-publicized "debate" with James Ford in the early 1950s (see below), Spaulding had developed a reputation in archaeology as an advocate of using statistics for pattern discovery. But it wasn't statistics that he took up in his paper (Spaulding 1968). Rather, it was the philosophy of science. There were only three references in Spaulding's paper, and they all were to works by philosophers.

As Lyman, Schiffer, and I were writing *Archaeology as a Process*, we wondered where Spaulding had been hiding his philosophical interests all the time he was battling Ford over pottery types and the like in the 1950s. He never cited any philosophers in his articles on typology, nor did he frame his arguments in philosophical terms. We concluded that he must have acquired those interests later, during his stint as program officer at the National Science Foundation. We based this conclusion on the fact that for the first few years at the agency, Spaulding served as the director of the History and Philosophy of Science Program before assuming the helm of the Anthropology Program. In our opinion Spaulding would not simply have served as a titular head. Rather, he would have become familiar with the latest developments in the history and philosophy of science, which at the time included the work of Carl Hempel, a logical positivist who in the 1970s would become the philosopher of choice of the processualists.

Spaulding was clear in his paper for the Binfords' volume, as he had been over a decade earlier (Spaulding 1954a), that archaeology is (or should be) scientific, meaning that research designs and analytical protocols are geared toward producing explanations. He asked if there were not two kinds of explanations for the way the world works, one historical and the other scientific. The scientific was the "nomological or covering-law explanation [of Hempel]. All serious explanations relate the circumstance to be explained to relevant general laws or at least to empirical generalizations. Explanations may be deductive, in which case the covering law admits of no exceptions, or they may be probabilistic-statistical (or inductive, if you prefer), in which case the covering law has the form of a frequency distribution" (Spaulding 1968:34). By "covering law" Spaulding, following Hempel, meant a generalized law that "covers" (explains) specific empirical phenomena.

Spaulding took his discussion directly from Hempel's (1962) paper "Deductive-Nomological vs. Statistical Explanation," in which Hempel, although he emphasized the physical sciences, accommodated biological phenomena under his explanatory umbrella. Spaulding argued that even though anthropology, and by extension archaeology, could never match "the deductive elegance of physics" (Spaulding 1968:34), they nonetheless were sciences because they sought to discover relationships in their data that could be accounted for by covering-law explanations. Further, "anthropological explanations are characteristically probabilistic-statistical rather than deductive, and they are partial rather than complete. . . . Anthropologists are not forbidden, however, to struggle toward covering generalizations with greater powers of prediction and retrodiction. They can strive to sharpen statements of the frequency distributions underlying probabilistic explanations, to make explanations more complete" (Spaulding 1968:36).

Spaulding's comments paved the way for one of the all-important questions of processual archaeology: Where do laws come from, and what role do they play in explanation? Different archaeologists would come up with different answers, and some, like Kent Flannery (1973), would bypass the matter entirely. Where Spaulding saw anthropology as a statistical science, Binford saw a deductive-nomological science built around the discovery of laws of cause and effect. Spaulding ignored the distinction between empirical generalizations and hypotheses because to him only empirical (statistical) generalizations were possible in anthropology. Conversely, Binford underscored the difference between an empirical generalization and a hypothesis (a tentative law) and discussed how one went about testing a hypothesis: "The accuracy of our knowledge of the past can be measured; it is this assertion which most sharply differentiates the new perspective from more traditional approaches. The yardstick of measurement is the degree to which propositions about the past can be confirmed or refuted through hypothesis testing—not by passing personal judgment on the personal qualifications of the person putting forth the propositions" (Binford 1968b:17).

The latter was a passing reference to a notion that had long been implicit in archaeology and which Raymond Thompson (1958:8) had formalized: The "final judgement of an archaeologist's cultural reconstructions . . . must therefore be based on an appraisal of his professional competence, and particularly the quality of the subjective contribution to that competence." That's an interesting point: The validity of an archaeologist's work should be based on how his or her peers view the person's competence. To that statement perhaps should be added, "or on how persuasively one argues the case." The outcome of one of the most interesting epistemological arguments ever to take place in American archaeology—the so-called Ford–Spaulding "debate"—hinged in part on persuasion (or lack thereof).

To work our way into a brief look at that debate, we can start with Wylie's categorization of three mid-twentieth-century archaeologists: Thompson, Ford, and J.O. Brew. Wylie labels them "constructivists," by which she means that they viewed their analytical units (types, periods, and the like) as "constructions"—units built by the archaeologist for a specific purpose—as opposed to "things" that could be elicited directly from the phenomena being investigated. The latter was Spaulding's view of artifact types—that by using the proper statistical method, the archaeologist could approach what the original artisans had in mind when they made a projectile point or decorated a pot. This stance led Spaulding into a series of exchanges with Ford that brought into sharp contrast two opposing epistemological views that had long been embedded in American archaeology.

Ford and Spaulding took center stage, but their polarized views made them less representative of the discipline than they might have been otherwise. The majority of archaeologists, if the literature is any guide, would have seen themselves as crosses between Ford and Spaulding. Everyone would have agreed that types are constructs that hopefully are useful for bringing chronological control to archaeological deposits. Most would have agreed that if the types serve an additional purpose—for example, if traits used to sort pottery into types "correspond to characters that might have served to distinguish one sort of pottery from another in the minds of the people who made and used it" (Phillips et al. 1951:63)—so much the better. But with rare exception, left unanswered was whether a type could actually perform both duties, or whether separate types—one for chronological purposes, the other for sociological purposes—were required. Also left unanswered was any discussion of how archaeologists would know when they had selected the requisite characters that would allow them to overlay their categories on those of prehistoric artisans. Spaulding was determined to show that methodological rigor could solve that problem, but he was not the first archaeologist so inclined. That honor belongs to George Brainerd.

Wylie doesn't mention Brainerd, but as Lyman and I were examining Ford's work (O'Brien and Lyman 1998, 1999), it became increasingly apparent the influence that Brainerd must have had on Spaulding. Both men were present at a conference on archaeological method sponsored by the Viking Fund and held at Spaulding's home institution, the University of Michigan, in 1951 (Griffin 1951). As Brainerd (1951a:117) put it in his conference paper,"The Use of Mathematical Formulations in Archaeological Analysis," typology is "in itself a generalizing procedure which ultimately depends for its validity upon the archaeologist's success in isolating the effects of culturally conditioned behavior from the examination of human products." Brainerd's procedure for isolating those effects involved selecting attributes that occur most often in combination in single artifacts and then subjecting them to statistical manipulation in order to produce the types. In this way, "the archaeologist can objectively describe the cultural specifications followed by the artisans" (Brainerd 1951a:118).

Brainerd (1951a:123) argued that his suggested techniques would eliminate the problem of existing typologies falling "far short of full utilization of archaeological materials for the recovery of information on culture." Further, "it is conceivable that a bridge may be found uniting the objectivity of the taxonomist to the cultural sensitivity of the humanist. Cultural intangibles can, if they exist, be made tangible. Better technique is the solution" (Brainerd 1951a:124). This statement echoed the point made by A. L. Kroeber (1940) a decade earlier in his paper "Statistical Classification."

Brainerd has been afforded little place in the annals of American archaeology, other than as someone who worked with statistician W.S. Robinson (1951) to develop a mathematical technique for measuring the similarity of pairs of assemblages (Brainerd 1951b). What Brainerd had to say about improvement in method, however, would be championed by Spaulding, although if he felt an intellectual debt to Brainerd, he never said so in print. Spaulding several years earlier, in a brief consideration of whether the Midwestern Taxonomic Method (McKern 1939) was of analytical use on the Plains, had lamented that archaeology needed a classification technique that "expressed at one stroke the classifier's opinion of the cultural relationship and the chronological position of an assemblage"; such a technique would allow "a combined presentation of [the] independent units of chronological position *and* cultural affinity" (Spaulding 1949:5; emphasis added). Spaulding was not denying the need to understand the chronological ordering of assemblages; rather, he was advocating the development of artifact types that did more than simply tell time. At that point, however, he had not figured out how to create such types. Brainerd showed him how.

Spaulding published his version of the method in a paper titled "Statistical Techniques for the Discovery of Artifact Types" (Spaulding 1953a). He defined a *type* as "a group of artifacts exhibiting a consistent assemblage of attributes whose combined properties give a characteristic pattern," and *classification* as "a process of discovery of combinations of attributes favored by the makers of the artifacts, not an arbitrary procedure of the classifier" (Spaulding 1953a:305). Following Brainerd (1951a), Spaulding was interested in discovering which attributes more often than random chance would co-occur on specimens from a single locale. The majority of artifact types in common use in American archaeology at the time were based on ceramic samples from multiple locations, perhaps numbering in the dozens or even hundreds (e.g., Ford 1936). Spaulding's types, however, were derived from single assemblages.

To Spaulding, types created by intuition and employing artifacts from multiple sites were too messy to be of much use archaeologically. No matter how carefully the analyst worked to create the types, they were conflations of characters (traits). At best, a type was an across-sample average, which, because it was an average, masked variation—the very feature that Spaulding saw as being so important from a sociological (behavioral) standpoint. He pointed out that "the presence of an adequate method for investigating consistency and range of variation *within the site* obviates a comparative study so far as the questions of the existence and definitive characteristics of a type are concerned" (Spaulding 1953a:305). He continued, "Historical relevance in this view is essentially derived from the typological analysis; a properly established type is the result of sound inferences concerning the customary behavior of the makers of the artifacts and cannot fail to have historical meaning" (Spaulding 1953a:305).

In his response to Spaulding's article, Ford (1954a:391) called Spaulding's approach "amazingly naive," pointing out that although it would "reveal the relative degree to which the people conformed to their set of ceramic styles at one time and place," that was all the approach would do. Spaulding (1954b) replied that Ford still did not understand what a type was, although he was "quite willing to let Ford have his types if he will let me have mine" (Spaulding 1954b:393).

While Ford was preparing his response to Spaulding (Ford 1954a), he was also preparing a more programmatic statement on typology (Ford 1954b). The heart of Ford's discussion focused on the houses constructed by the fictitious Gamma-gamma people, who occupied the Island of Gamma. Cultural, or emic, house types certainly existed, Ford said, as the houses on the Island of Gamma and nearby islands indicated.

But, like with prehistoric artisans, how would archaeologists know when they had "discovered" those emic types? They wouldn't. But for Ford it didn't matter; he wanted type groupings that the archaeologist consciously selected in order to produce a workable typology "designed for the reconstruction of culture history in time and space" (Ford 1954b:52). Ford never specified how such groupings were to be extracted from the flow of culture or how one knew one had such a type (Dunnell 1986; O'Brien and Lyman 1998). He was not alone, as Wylie makes clear.

Despite his lack of specificity, Ford showed keen insight into the typology issue. For example, he pointed out that types are accidents of the samples available for analysis: "[T]he particular locality where an archeological collection chances to be made will be one of the factors that determines the mean and the range of variation that are demonstrated in any particular tradition in the culture that is being studied" (Ford 1954b:49). This was a reiteration of a point he had made in his response to Spaulding (Ford 1954a). Further, "permitting sampling chance to determine typology operates very well so long as the archeologist has only a spotty sampling of the culture history" (Ford 1954b:52). A larger sample would result in typological "creep," where types begin to blend together (Phillips et al. 1951). Here Ford was taking a shot at Spaulding's method. As long as Spaulding had limited samples, Ford was arguing, he could get consistent co-occurrences of attributes. Once the sample grew larger, typological creep would set in, and the types would be much less useful as historical units. Spaulding never addressed this criticism.

Hindsight tells us that Ford's strategy for refuting Spaulding's position didn't work very well for several reasons, not the least of which was that Ford was both a poor writer and a stubborn person. The interplay of these two character traits sometimes overrode clarity and logic, especially critical when the topic was conceptually difficult to begin with. In his responses to Spaulding, Ford's vague allusions to "cultural customs" and his use of a fictitious ethnographic example (the Gamma-gamma people) didn't win him many converts. American archaeologists typically agreed with Ford in how types were to be created, but they emulated Spaulding in assuming that the resulting types were both historical and sociological. If nothing else, the debate between Ford and Spaulding was a catalyst for the new archaeology, as Wylie appreciates.

Spaulding's view—clearly having precedent in Brainerd's work—represented a new approach to the archaeological record, one in which appropriate methods would allow one to detect emically significant properties of that record—properties that revealed human behaviors (e.g., Binford 1968b:23). What the new archaeologists wanted was to study culture and cultures, not to measure the time-space continuum by detailed classification of artifacts. Spaulding and other "nonconstructivists" provided the warrant through reference to cultures and ethnicities, however defined, as being ethnographically visible. If so, then perhaps they were archaeologically visible as well. This caught the attention of the anthropologically oriented processualists and contributed to what became known as "ceramic sociology" (Binford 1983; Longacre 2000), the early results of which appeared in the Binfords' (1968) *New Perspectives in Archeology* (e.g., Deetz 1968; Hill 1968; Longacre 1968; Whallon 1968).

We could leave the issue there, but from a philosophical standpoint we would be skipping over the most delicious concern of all—one that transcends epistemology and gets directly at the core of philosophy. That core is *ontology*. Whereas epistemology is about knowledge and knowing, ontology is about existence; specifically, it is a systematic account of existence. To this point one could argue that the difference between Spaulding and Ford with respect to types was epistemological—a disagreement about knowledge and knowing. That is, are we better off getting our knowledge from types created by statistical methods and using samples from a single location, or are we better off with types created by inspection and using samples from multiple locations? Undeniably, this is an epistemological question (O'Brien and Lyman 2002), but its roots go much deeper than that. They get at whether types are real, as Spaulding argued, or completely arbitrary, as Ford argued. Reality versus nonreality: That is an ontological issue.

How one views something like archaeological types is one part of a much larger concern, namely, how one views the reality of the natural world. There are two ontologies, essentialism and materialism, and although they contrast sharply, they are not mutually exclusive. By this I mean that a person can hold to both views, consciously calling on one or the other depending on circumstances. The key issue is knowing which one to call on under which circumstance. The interplay of essentialism and materialism has seen considerable attention in biology and the philosophy of biology (e.g., Ereshefsky 2001; Mayr 1982, 1987; Sober 1984) as well as in archaeology (e.g., Dunnell 1982; Lyman et al. 1997; O'Brien and Lyman 1998, 2000).

Under *essentialism*, the essential properties of a set of things define an ideal (archetype), "to which actual objects [are] imperfect approximations" (Lewontin 1974:5). Variation between objects placed in the set, because it contributes nothing to the "essentialness" of the objects, is viewed as "annoying distraction" (Lewontin 1974:5). Under this perspective, only variation between types, not between the individual objects placed in types, is of explanatory significance. Single sets, or kinds, of entities are presumed to be real; thus relations between units can be formulated without reference to time or space. They are redundant, universally true statements (true for all times and all places). Spaulding's types were essentialist constructions, created on the basis of their possessing "essential" properties—specific attribute combinations. They were also "empirical" units, meaning they were viewed as being real.

In contrast, *materialism* does not assume that reality is a unified system. Phenomena are constantly in a state of flux, meaning that they are continually in the process of becoming something else. Relations between phenomena are not timeless, nor can universal statements be made about the relations because no static set of phenomena exists. Time and space are kept separate, and relations between phenomena are time- and spacebound. Kinds, or types, are nonempirical configurations—theoretical units—that are changing constantly, although at any given moment in time and space we can create kinds based on observations. Ford's types were materialist constructions, created on the basis of a more or less informed version of "throw it up and see what sticks." They were built for specific purposes, such as chronological ordering. If the types didn't work too well, throw them out and start over, refining the process until they did work.

Ford's materialist views extended far beyond his treatment of types. From the beginning of his career, he held to the notion that culture was a constantly flowing stream, but one that could be carved up into units of varying scale depending on the analyst's needs. His cultural periods and the like, as with his pottery types, were theoretical (nonempirical) units constructed to perform some piece of analytical work. Because his views were not widely shared, Ford had to constantly trumpet the nonempirical nature of cultural units. His classic collaboration with Philip Phillips and James Griffin on survey and excavation in the Lower Mississippi Alluvial Valley (LMV) was a case in point (Phillips et al. 1951). Although the three men agreed on some things, they parted company on others. It is clear that *in general* Phillips and Griffin were essentialists, whereas Ford was a materialist. This difference in ontology makes their monograph an interesting read, as numerous reviewers have pointed out (e.g., Dunnell 1985; Haag 1953). It is equally clear, however, that neither Ford nor Phillips and Griffin maintained a consistent ontological outlook across the board (O'Brien and Lyman 1998), which gives the monograph a schizophrenic feel. For example, in the pottery section the authors stated that with respect to types,

> . . . we have in mind the concept of a continuously evolving regional pottery tradition, showing a more or less parallel development in and around a number of centers, each of which employs a number of distinct but related styles, each style in turn being in process of change both areally and temporally. With this remarkably unstable material, we set out to fashion a key to the prehistory of the region. Faced with this three-dimensional flow, which seldom if ever exhibits "natural" segregation, and being obliged to reduce it to some sort of manageable form, we arbitrarily cut it into units. Such *created units of the ceramic continuum* are called *pottery types.* (Phillips et al. 1951:62–63)

This is a decidedly materialist view. But notice that on the previous page of the monograph they had indicated that types serve as "expressions of the ideas and behavior of the people who made and used them" (Phillips et al. 1951:61). This is a decidedly essentialist view.

Part of the reason for the schizophrenia may have been that the three authors were bending on some points just to get the report completed. Ford, for example, never accepted that types could serve a sociological purpose because he saw no method to test the correspondence between type and social norm. But he apparently went along with Phillips and Griffin. Another reason for the schizophrenia undoubtedly rested on the fact that it's difficult to maintain consistency in ontology if you are not constantly reflecting on why you think things are the way they are—Wylie's "thinking from things." This lack of consistency is evident in Ford's work (O'Brien and Lyman 1998), although he was more consistent than some of his colleagues. One topic on which he seldom veered from a consistent course was the flow of culture. The only time he saw that flow being interrupted to such a degree that it would be visible ethnographically, let alone archaeologically, was as a result of some cataclysm such as invasion. Otherwise, culture was a quietly flowing stream, albeit a braided one, filled with intersections and splits that resulted from diffusion and other "normal" cultural processes. Given this steadiness, any attempt to divide the flow of culture into analytical units—culture periods, for example—was bound to be arbitrary.

Some of Ford's ideas on culture and its flow as reflected in pottery designs irritated Phillips and Griffin to the point that when they were preparing the LMV report, they wouldn't let him include them. Ford published them the next year in *Measurements of Some Prehistoric Design Developments in the Southeastern States* (Ford 1952). The monograph was a wide-ranging discussion of Ford's views on culture and diffusion as reflected in pottery designs across an area that stretched from East Texas to the Florida Panhandle and covered 1,500 or more years. It was Spaulding's (1953b) review of that monograph that initiated the "Ford–Spaulding debate." Spaulding could not understand the basis for Ford's chronological arrangement of assemblages from the Southeast. Nor could he tolerate what he saw as the arbitrariness of Ford's periods, meaning that the period boundaries did not correspond with any "natural" cultural disjunctions. Ford (1954c:109) retorted that Spaulding was "amazingly naive" (there was that phrase again) and that he (Ford) was "somewhat more uncertain than Spaulding that nature has provided us with packaged facts and truths that may be discovered and digested like Easter eggs hidden on a lawn."

Natural disjunctions have long been an important component of the archaeological metaphysic, especially when stratigraphy is involved (Lyman and O'Brien 1999). Phillips, Ford, and Griffin confronted the issue in the LMV analysis in terms of what to do with "mixed" assemblages, meaning assemblages that represented multiple archaeological "complexes" (O'Brien and Dunnell 1998). For Phillips and Griffin, multiple complexes meant multiple peoples; for Ford, multiple complexes represented nothing more than "a single brief span of time on the continuum, an 'instant' for all practical purposes, when both elements of the mixture were being made and used side by side" (Phillips et al. 1951:427). Griffin and Phillips, "while not rejecting the general theory of continuity . . . have tended to see indications of at least one significant break in the otherwise placid stream of pottery continuity at the point where the tempering material shifts from clay to shell, in other words between the Baytown and Mississippi periods" (Phillips et al. 1951:427). For Phillips and Griffin, those two "periods" meant two different peoples—an earlier, clay-temper-using "Baytown" people and a later, shell-temper-using "Mississippian" people. Ford saw no equivalence between temper and people; to him, periods were nothing but analytical units carved out of the temporal (hence cultural) continuum.

Nothing in American archaeology better exemplifies the difference in metaphysic between essentialism and materialism than what Ford and later Phillips had to say about the cultural sequence for the LMV. The sequence was entirely of Ford's making and was based on a series of surface collections and test excavations he made in the 1930s (Ford 1935, 1936) and on later excavations that he directed as part of the Louisiana Works Progress Administration program (Ford 1951; Ford and Quimby 1945; Ford and Willey 1940). Based on his early work (Ford 1935, 1936), Ford created three periods—(from early to late) Marksville, Coles Creek, and Natchez (Figure 1). Based on later excavations (Ford 1951; Ford and Quimby 1945; Ford and Willey 1940), he added the Tchefuncte period below Marksville, the Troyville period between Marksville and Coles Creek, and the Plaquemine period between Coles Creek and Natchez (later renamed Natchez-Bayogoula).

Almost no one was happy with Ford's handling of the chronological sequence. A large part

Periods as Named in 1936	**Periods as Named at Present**
	Natchez-Bayogoula
Natchez	Plaquemine
	Coles Creek
Coles Creek	Troyville
	Marksville
Marksville	Tchefuncte

Fig. 1. Cultural sequences, LMV.

of the irritation arose as a result of how archaeologists chose to view cultural periods—that is, as "real" units, bounded on either side by visible cultural disjunctions. When Ford and Willey (1940) proposed the first additions to the sequence, the Tchefuncte and Troyville periods, archaeologists used to the old sequence—Marksville, Coles Creek, and Natchez—were angered. Maybe they could understand adding a sub-basement (Tchefuncte) beneath the older basement (Marksville), but why in the world would Ford add a new floor—Troyville—between Marksville and Coles Creek, or, later, make matters worse by adding another floor—Plaquemine—between Coles Creek and the historical period (Natchez)? As Jon Gibson (1982:271) put it, both Troyville and Plaquemine were "transitional units. . . . carved out of ceramic complexes that had formerly been classified as something else. This confounded opponents who simply could not see how some cultural types could be Marksville or Coles Creek one day and Troyville or Plaquemine the next. These individuals apparently did not share Ford's view of culture as a gradually changing flow of ideas, with any one archaeological site encapsulating those elements which comprised a limited span of an unbroken continuum."

In his report on the excavations at the Greenhouse site in Avoyelles Parish, which were completed in the 1930s but not published until 1951, Ford finally answered his critics, and he didn't pull any punches:

> The [WPA] excavation program has made possible the expected subdivision of the rough time scale that I presented in 1936. New classificatory terms have been interposed between each of the time-period names previously set up, thus giving a more accurate measure of the chronology in verbal terms. Of considerably more importance, however, is the fact that the stratigraphic data have produced a picture of quantitative change of ceramic styles. The sequence of period names "Marksville," "Coles Creek," and "Natchez" presented in 1936 was actually the limit of our control over ceramic chronology in this region at that time. While we were aware that these were probably gross divisions of a changing cultural continuum, this could not be demonstrated and had no more validity than a reasonable assumption deduced from experience with culture history in other areas where details were better known. Some of the ignorance that makes such a neat and "air-tight" classification possible has now been dispelled, and the expanded list of period names can be presented as nothing more than convenient labels for short segments of a continually changing culture history. . . .
>
> This readjustment of the named divisions for the time scale in this area seems to have puzzled a few of the archaeologists working in the Mississippi Valley, even some of those who have been best informed as to the fieldwork which led to this rearrangement. Complaints have been made that pottery types that were formerly classified as Coles Creek in age are now assigned to the Troyville Period. Discussion develops the opinion that if this latest chronological arrangement is correct then the former must have been in error. The adoption of new names for all the periods in the more recent arrangement may have avoided some, but not all, of this confusion. These serious and earnest seekers after truth really believe that we have discovered these periods and that this is a more or less successful attempt to picture the natural divisions in this span of history. This is obviously an incorrect interpretation. This is an arbitrary set of culture chronology units, the limits of each of which are determined by historical accident, and which are named to facilitate reference to them. (Ford 1951:12–13)

Here Ford was adamant about what in his mind was the illogicalness of seeking "real" cultural units. One of those to whom his comments were directed was Phillips, who never backed away from his disdain for Ford's "arbitrary" periods. In 1970 Phillips published a large two-volume update of the LMV, and in it he took a swipe at Ford's periods:

> The concept of a Troyville "period" in Lower Mississippi archaeology has been a target of criticism since it was first launched by Ford and Willey (1940). Many students have felt uneasy about it. Others have flatly stated that they could not use it in their particular area of interest. . . . The reasons for this almost universal discomfort lie, I believe, in the peculiar nature of Troyville as an archaeological formulation. . . .
>
> Troyville [appears] to have been sliced out of Coles Creek [and] Marksville (Ford, 1951). But this could only work if there is a clear case of continuity between Marksville and Coles Creek. If there is discontinuity (and who can doubt it in this particular case?), that discontinuity would be automatically incorporated in the new Troyville phase. In my opinion it is, but the fact is not brought out in Ford's (1951) description of the Troyville complex. It seems to be nothing more than a mixture of two separate and distinct complexes. . . .
>
> To conclude this digression into methodology, in setting up Marksville and Coles Creek in 1936, Ford was following the classic method of starting new periods with the appearance of new forms. Later it became necessary to subdivide these periods. If Troyville had continued to be simply a division corresponding to early Coles Creek (as Plaquemine to late Coles Creek), which is about what it was as originally defined by Ford and Willey in 1940, there would have been no difficulty. The "natural" (a word which Ford would not allow me to use) line of separation between the old Marksville and Coles Creek would have remained in place. But Ford's description of 1951, in failing to accent the new forms that belong specifically to Troyville, makes it appear to straddle this line. Actually, he is using a new criterion in marking off chronological divisions. Instead of coinciding with the appearance of new features and the disappearance of old, lines of separation are determined by their maximum occurrence. (Phillips 1970:908–909)

It is interesting that Phillips referred to Ford's break with "classic" archaeological method, because in reality he hadn't broken with anything. Phillips liked the Marksville–Coles Creek boundary as well as that between the Coles Creek and Plaquemine periods. He even thought Tchefuncte was one of those "intelligible culture-historical units in the usual sense" (Phillips 1970:908). If, Phillips later lamented, Ford hadn't toyed with the Marksville–Coles Creek boundary and had simply split the Coles Creek period into three pieces—Troyville (early Coles Creek), Coles Creek (middle Coles Creek), and Plaquemine (late Coles Creek)—everything would have been fine. But he had to go and stick the Troyville period between the two periods with which everyone was comfortable—Marksville and Coles Creek—in the process compressing them into shorter periods by squeezing them against either the solid basement period, Tchefuncte in the case of Marksville, or the equally solid ceiling period, Plaquemine in the case of Coles Creek. Neither of those two anchor periods was going to budge, so Marksville and Coles Creek took the brunt of the force (O'Brien and Lyman 1998).

This apparent "rearrangement" threw things out of whack because everyone but Ford was looking for discontinuities in the archaeological record. Certainly he might use an apparent discontinuity as a means of establishing a period boundary, as he did when he used the disappearance of fancy pottery decoration to end the Marksville period, but he didn't rely on them. It just so happened that in almost every case he *had* used highly visible artifacts or designs to mark period boundaries, but this was simply coincidental to his real purpose—to cut up the continuum into a sufficient number of short-term periods so as to allow the measurement of the passage of time and the writing of culture history. That was the method Ford had *always* used; he hadn't made a break with classical method—at least as he defined it. Others defined "classical method" differently, a difference born of ontologies in conflict.

Conclusion

It is tempting to speculate that if Ford had only read some philosophy, he would have sharpened his ontological stance and been able to beat his opponents at their own game. Or at least he would have been able to express his views on culture and cultural units in a logical and consistent fashion. Maybe the same could be said about Spaulding, though he clearly was much more consistent, not to mention clearer, in his thinking and writing than Ford was. Would a healthy dose of philosophy have changed the outcome of the Ford–Spaulding debate or caused Phillips to change his opinion of Ford's arbitrary temporal divisions? Probably not, but I'm guessing the arguments for or against a particular kind of unit, as well as the accompanying discussion of the uses that a unit serves, would have been considerably tighter.

It is difficult to overemphasize the importance of philosophy to any endeavor that involves thinking rationally and logically, and archaeology certainly falls in that category. And yet at the same time, I am hesitant to suggest that philosophy is some cure-all for what I or someone else might see as archaeology's ills. I say this because of what history has taught us about philosophy and archaeology. The interest that the processualists showed in the philosophy of science during the 1970s, specifically Carl Hempel's brand, was more than casual, but there were as many false starts and dead ends as there were successes. Archaeologists were led to believe—primarily by other archaeologists, not by philosophers—that the future lay in the direction of Hempel's deductive-nomological model of scientific explanation.

Why did the processualists choose Hempel as the model for archaeology? Part of the reason, Lyman, Schiffer, and I suggest (O'Brien et al. 2005), had to do with the fact that when Albert Spaulding introduced archaeologists to the scientific approach in his comments in *New Perspectives in Archeology* (Spaulding 1968), it was to Hempel's brand, not someone else's. Binford was simply following Spaulding's lead when he adopted Hempel as a guide. Regardless, the processualists' devotion to strict Hempelian deduction began to fade as they came to realize that research is rarely if ever entirely inductive or deductive. Rather, it combines both. This realization was helped along by another philosopher of science, Merrilee Salmon, whose engagement with archaeology and archaeologists at the University of Arizona in the 1970s demonstrated that philosophers could make positive contributions to the discipline not only by clearing up misunderstandings but also by introducing archaeologically appropriate models. Alison Wylie continues that tradition. I'm not sure what Ford and Spaulding would have thought about Wylie's book, but it *might* have made them a little more aware of just how difficult it is to think about epistemological and ontological issues in a consistent, logical fashion. As archaeologists, we all could use a little more of that kind of awareness as we find ourselves thinking from things. □

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