A New Perspective in Arch(a)eology

Indeed, there is so much wonderful new research going on in the Southwest that it is difficult to keep up with it all. I am so encouraged and excited by all these new directions. Whatever the impact of the small beginnings that we started in the New Archaeology so many years ago, I am overjoyed to have been able to witness the astounding growth in Southwestern archaeology over these forty years.

—William A. Longacre, “Exploring Prehistoric Social and Political Organization in the American Southwest”

Bill Longacre (Figure 2.1) was too modest when he referred to the “small beginnings” that he and other “new archaeologists” started in the Southwest back in the 1960s. No other region saw more sustained activity by processualists than the Southwest, and no processualist had a greater impact on Southwestern archaeology than Longacre. There was nothing about Arizona or New Mexico that held a special attraction for Longacre and his processualist colleagues; they worked there because the region offered fieldwork and employment opportunities. For Longacre, his so-called small beginnings can be traced to his graduate years at the University of Chicago, which by all accounts (e.g., Longacre 1998) had an aura of intellectual excitement in the late 1950s and early 60s. Having studied social anthropology as an undergraduate at the University of Illinois, Longacre was perhaps more receptive than most to the new archaeology’s mission to make the discipline more anthropological.

The list of archaeology graduate students at Chicago during that period is impressive. As Longacre (2000) tells it, in his cohort were James Brown, Kent Flannery, Leslie Freeman, James Hill, Stuart Struveer, and Robert Whallon. And, of course, Sally Schanfield—who would become Sally Binford (Figure 2.2)—was there, researching the Middle Paleolithic with Clark Howell. A younger cohort included Margaret Hardin, John Fritz, and Fred Plog, and an older one included Patty Jo Watson (Figure 2.3) and Frank Hole (Figure 2.4). For all practical purposes we can add Lewis Binford to the older cohort. Binford joined the anthropology faculty at Chicago in 1961, three years before receiving his Ph.D. from the University of Michigan. He was only a year or two older than Hole and Watson, and they all had had roughly the same amount of field training. This was a group of smart people, four of whom—Binford, Flannery, Hole, and Watson—were later elected to the National Academy of Sciences, arguably the most prestigious scientific body in the world. This collective of Chicago graduate students would be the first to take up the label “new archaeologists,” or simply “processualists.”
The stated goal of the new archaeology was to study cultural processes and to contribute to anthropological theory. As detailed in Chapter 1, this was not a new idea, although few processualists pointed that out. Like their predecessors, they understood that cultural processes, which are dynamic phenomena, are represented by a static archaeological record. New archaeologists argued that getting at those processes required two things. First, the basic notion of culture had to be retooled from a normative, idea-based concept to one that was behavioral, systemic, and materialist. Second, archaeology had to be conducted scientifically, which to some processualists meant working deductively rather than inductively, and using 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 (Chapter 4). These mundane exercises that created the “facts” of the archaeological record were taken to typify “traditional archaeology”—the archaeology that Joseph Caldwell in his “New American Archaeology” paper in *Science* (1959:304) had labeled “dull and uninteresting” (Chapter 1). Inductive
work to dress them up, are not particularly relevant to discussions of poverty, segregation, or war in Southeast Asia. The nation was in the mood for change, and in some instances people didn't much care where it came from. Increasingly, novelty was something to be prized, and last year's model was quickly replaced by this year's.

This push for novelty went for people, too. The catch phrase of the '60s came from Berkeley activist Mario Savio, who said, "You can't trust anyone over thirty." This was nothing more than one man's view of the well-publicized generation gap in American society. Savio was the same person who said the following to demonstrators before they entered the University of California's Sproul Hall to begin their sit-in in December 1964: "There is a time when

(1972b:13) reported they were called—as they made their way along, encumbered by students and other acolytes vying for their attention. When Binford recalled that even in the early days of processual archaeology graduate students would constantly stop him outside a meeting room with, "Dr. Binford, Dr. Binford, I'm working on ________, and I wonder if you have any suggestions as to how I might analyze that kind of data" (Binford 1972b:13), he wasn't exaggerating. We remember standing on the sidelines and imagining Elvis backstage, silently hoping that one day we, too, would have groupies. Of course, that kind of thinking undermines the notion that intellectual change should be for the betterment of humankind and not for personal gain, and so perhaps we were in a minority.

The changes that were going on in American archaeology were a microcosm of greater changes in the United States. By the early 1960s, students on the nation's campuses were beginning to call for radical change in everything, including university curricula. Classroom learning was now supposed to be "relevant" to everyday life. In archaeology, the diffusionist explanations of earlier years rang hollow. Chronological orderings, no matter how you

Figure 2.4. Frank Hole alongside ground stone artifacts from excavations on the Deh Luran Plain, Khuzistan, Iran, ca. 1968. (Photograph by James Neely.)
the operation of the machine becomes so odious, makes you so sick at heart, that you can’t take part; you can’t even passively take part, and you’ve got to put your bodies upon the gears, and upon the wheels, upon the levers, upon all the apparatus, and you’ve got to make it stop. And you’ve got to indicate to the people who run it, to the people who own it, that unless you’re free, the machine will be prevented from working at all.”

We won’t credit (or blame) Savio and the Free Speech Movement for jamming the apparatus of Americanist culture history, but we will say that it is not especially surprising that some of its wheels ground to a halt when and where they did. The temptation to replace the old apparatus with something new was just too strong to ignore in the early ’60s, especially when several of the most well-respected, older members in the discipline—Gordon Willey and Philip Phillips, for example—had provided clues as to how to go about creating a replacement. And if you were going to create a replacement, you might even change its name, if ever so slightly—or so the story went.

A curious belief arose that one could identify at a glance the writings of the new archaeologists because of how they spelled archaeology—without the second e, the way it had been done for decades in American Anthropologist and for close to a century by the federal government. The Government Printing Office’s explanation for substituting e for the ligature ae around 1890 (Rowe 1975) was to reduce costs, but the processualists supposedly preferred the short version to distinguish their kind of archaeology from culture history. Two important books, New Perspectives in Archaeology (Binford and Binford 1968a) and Anthropological Archaeology in the Americas (Meggars 1968), seemingly gave substance to the belief. Nonetheless, the spelling of archaeology turned out to be a poor guide to an author’s views on new and old. After all, journals and many book publishers have rigid house styles. Binford’s classic papers in American Antiquity, for example, all have the ae.

In the rush to promote their approach, the more radical processualists swept out the earlier literature—“redlined” it, to use Schiffer’s (2000b) colorful phrasing—labeling most of it as little more than descriptive lists of artifacts, all arranged in nice spatial rows and chronological columns, most of it irrelevant to achieving archaeology’s anthropological goals. Binford himself often was uncharitable to his predecessors’ efforts, but he tended to phrase his objections less belligerently than Walter Taylor. But the products of cultural historical research—the site reports, survey reports, and syntheses—could not be redefined if one were going to work in a particular area. New archaeologists had to immerse themselves in the minutiae of culture history—the pottery types and their chronological ordering—even if they believed that the types measured nothing interesting about the past except, imperfectly, time and history. Moreover, almost everyone still used the culture-historical units—phases, stages, periods, and the like—to talk about their pieces of the archaeological record. Otherwise, communication, even among processualists, would have ceased. Thus, the archaeological literature has had, and continues to have, a large hand in reproducing, in generation after generation, the products of culture history—at least artifact types, chronologies, and culture units. As Fred Plog (1973a) observed, all archaeologists are culture historians to one degree or another.

THE ARCHITECT OF PROCESSUAL ARCHAEOLOGY

No one did more during the 1960s and ’70s to make archaeology anthropological than Lew Binford, and no one else had stirred up more controversy along the way. Binford, it seemed, had opinions on everything, and he published them at a prodigious pace. Table 2.1 is a list of Binford’s papers that appeared during a thirty-year span (1962–1991). It is a selective list, containing only articles in major journals and excluding books, book chapters, book reviews, and other sundry pieces. Few archaeologists can claim that they published an average
Table 2.1. Article-Length Publications by Lewis Binford, 1962-1991

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<td>8</td>
<td>American Antiquity</td>
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<td>Ethnohistory</td>
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<td>7</td>
<td>Anthropological Papers, Museum of Anthropology, University of Michigan</td>
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<td>Yearbook of Physical Anthropology</td>
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of 1.3 articles per year in the top journals in anthropology and sustained that effort over three decades. Moreover, it’s one thing to write articles and books but another to have readers become engaged with your ideas. We suspect that Binford is the most read and cited American archaeologist of the last forty years, producing a large share of the “scientific capital” that exists in the archaeological marketplace (Wylie 1983). Certainly a quick perusal of the ISI Social Sciences Citation Index bears out the impact his publications have had, and continue to have, on archaeology both in the United States and abroad.

Binford is widely recognized as the chief architect of the new archaeology—a role that he has acknowledged on more than one occasion. His first major article, “Archaeology as Anthropology,” published in American Antiquity in 1962, typically is regarded as processualism’s birth announcement. It was the last of twelve articles in the issue in which it appeared, suggesting to us that the editor at the time, Tom Campbell of the University of Texas, had no reason to suspect that it was anything out of the ordinary. No one would have. Binford followed that seminal contribution with several articles during the 1960s (e.g., Binford 1963, 1964a, 1965, 1967, 1968d) that helped set both the agenda and tone for the new archaeology. In terms of what it did for the processual movement, nothing matched the book that Binford coedited with his then-wife, Sally—New Perspectives in Archeology (Binford and Binford 1968a). Charles Redman, a second-generation processualist, later referred to its publication as marking “the crossing of a threshold” (Redman 1991:296). The book grew out of a symposium that the Binfords had put together for the American Anthropological Association meeting in Denver in November 1965. (Incidentally, that symposium was so well attended that James Sackett, an Old World specialist who had received his Ph.D. from Harvard in 1965, purportedly leaned over and whispered the famous line, “My God, Lew, they’re standing up in the rear” [Binford 1972b:12]. Whether that story is true or simply part of the processualist lore, by 1965 there was growing interest in what Binford and his Chicago friends were up to.)

One means of broadening interest in the new archaeology was to take the processual message to anthropologists. Perhaps expecting to secure ringing endorsements for archaeology’s renewed commitment to archaeology, the Binfords asked several prominent anthropologists to comment on the papers presented at the Denver meeting. Not surprisingly, the comments, for the most part, were laudatory. Two sym-
or the processual model of the book that coined the term anthropology (Binford and Edman, 1968). The second chapter refers to the book as a ‘‘reading’’ of the Binford model.

Figure 2.5. Richard Lee interviewing !Kung San (now Jul’hoansi) in northwestern Botswana, ca. 1967. (Photograph by Irven DeVore, courtesy Nancy DeVore and Anthro-Photo File.)

The time of their remarks, Lee and DeVore were preparing for the international Man the Hunter Conference that would be held at the University of Chicago the following April. Their comments at the Binfords’ symposium (DeVore 1968; Lee 1968), coupled with the articles that came out of the Man the Hunter Conference (Lee and DeVore 1968), would have far-reaching effects in archaeology that even they could not have envisioned.

Lee and DeVore’s message to archaeologists at the Denver meeting was to use the ethnographic record as a source of information helpful in understanding the archaeological record. To that end, Lee offered a proposal: the creation of a graduate course in which each archaeology student would be required to spend three to six months doing an ethnographic field project in order to develop insights helpful in assessing the prehistoric record. He suggested calling such a program “interpretive archaeology.”

The light bulb must have gone on for several in attendance that day because within a decade ethnoarchaeology would become a popular approach for acquiring ethnographic information.

Figure 2.6. Irven DeVore studying activities of !Kung San (now Jul’hoansi) in northwestern Botswana, 1965. (Photograph by Stan Washburn, courtesy Nancy DeVore and Anthro-Photo File.)
that could be used to interpret the archaeological record. Ethnoarchaeology had deep roots in the discipline: the term *ethnoarchaeologist* dates to the early twentieth century (Fewkes 1900), but with few exceptions (e.g., Ascher 1962; Gould 1966, 1968a, 1968b; Thompson 1958) it was not widely pursued until the 1970s (David and Kramer 2001; Schiffer 1978a). Then, processualists began heading off to Africa, Australia, Alaska, and other distant locations to see how the natives did things (Chapters 5 and 7).

The Binfords chose as their symposium chairmen two "senior men in the field who have consistently encouraged and inspired many of us and have aided greatly the development and acceptance of new ideas" (Binford and Binford 1968b:vii): Albert Spaulding of the University of Oregon and Paul Martin of the Field Museum of Natural History in Chicago. Binford had taken courses with Spaulding at Michigan and had considerable respect for him (Binford 1972b), as did many in the discipline, especially in light of his efforts to increase the level of funding for anthropology in the budget of the National Science Foundation (NSF), where he served as a program director in the early 1960s after leaving the University of Michigan.

Martin (Figure 2.7) also commanded considerable respect as both a researcher and a teacher. A specialist in ceramic typology, he spent his entire career working on Field Museum-sponsored projects in the Southwest (Herold 2003; Nash 2001, 2003), beginning with Lowry Ruin, a twelfth-century pueblo in southwestern Colorado, in the 1930s. In 1956 Martin moved the headquarters of the Southwest Archaeological Expedition from the Pine Lawn-Reserve region of western New Mexico to Vernon, Arizona (Figure 2.8). Martin was, in Longacre's (2000:293) recollection, someone who "during his whole long career...was anxious to get at the behavior of past peoples and explore their ways of organizing themselves." Within a few years he would get his wish, as the Vernon field station and nearby sites became the major proving ground for processual archaeology (Chapter 3).

Over the years, Martin provided resources that were used by generations of Chicago and Arizona graduate students to apply their new ideas in his NSF-supported field projects (Figures 2.9 and 2.10). By the time of the AAA symposium in Denver several Chicago graduate students, including Longacre and James Hill (Figure 2.11), had produced processualist products based on work conducted with Paul Martin's encouragement and financial support. In inviting him to be part of the symposium in Denver, the Binfords were recognizing his contributions to furthering the careers of Longacre, Hill, and others, and thereby giving some traction to the new archaeology.

Martin did not provide an article for *New Perspectives*, 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 "debates" with Jim Ford in the early 1950s (Chapter 1), Spaulding had developed a reputation for his use of statistics or, more properly, for his advocacy of their use in pattern discovery. But it wasn't statistics that he took up in
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Figure 2.8. Map of northeastern Arizona showing significant archaeological sites in and around Vernon.

his article (Spaulding 1968): it was the philosophy of science. All three references in Spaulding's article were to works by philosophers. In reading his remarks, one might wonder where Spaulding had been hiding his philosophical interests all the time he was battling Ford over pottery types and the like. We suspect that he acquired them later, during his stint at NSF, where for the first few years he served as the director of the History and Philosophy of Science Program before taking the helm at the Anthropology Program. Spaulding was too detail oriented simply to 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 empiricist who in the 1970s would become a household name in processual archaeology (Chapter 4).

Spaulding was clear about his vision for archaeology's future: to make archaeology a science. For Spaulding, being scientific meant...
that research designs and analytical protocols should be geared toward producing explanations. Spaulding (1968) asked rhetorically if there were not two kinds of explanations for the way the world works: historical and scientific. We say "rhetorical" because Spaulding (1968:34) recognized really only one kind of "serious" explanation, "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's discussion was taken directly out of Hempel's (1962) paper "Deductive-Nomological vs. Statistical Explanation." Spaulding might have been attracted to this work because Hempel did not write off humans or other organisms as falling outside the scientific umbrella. Most of his work on laws and explanation dealt with the physical sciences, but biological phenomena could also fit comfortably within his approach. Spaulding knew that despite best efforts, anthropology—and, by extension, archaeology—might fall short of "the deductive elegance of physics" (Spaulding

Figure 2.11. Left to right: Jim Hill, Bill Longacre, and John Fritz at the Vernon, Arizona, field camp, 1965. (Negative no. 103027, courtesy Arizona State Museum.)
1968:34). “Clearly,” Spaulding (1968:36) continued, “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.” Finally, Spaulding (1968:37) acknowledged that anthropology differs in some respects from physics, but it is nonetheless a science because it seeks to discover relationships in its data that can be accounted for by covering-law explanations. By covering law, Hempel meant a generalized law that “covers” (explains) specific empirical phenomena.

Spaulding’s comments sowed the seeds for what would become in the early 1970s the most important aspect of the new archaeology: Where do laws come from, and what role do they play in explanation? We don’t want to preempt the detailed discussion of these issues in Chapter 4, but we need to at least foreshadow it. Spaulding made what we view as a fundamental error by referencing Hempel’s distinction between probabilistic-statistical and deductive-nomological explanations, and then saying that archaeology might fall short of “the deductive elegance of physics” (Spaulding 1968:34). Although he was not alone, Spaulding misunderstood the relationship between stochasticism and determinism. It is not an all-or-nothing proposition, with probabilistic sciences on one side and deterministic sciences on the other. Albert Einstein knew this, hence his famous statement that God doesn’t roll the dice with the universe, meaning simply that things being treated as probabilistic need more investigation before we can understand the underlying principles. Lacking other information, probability theory is a good starting point to justify more investigation, but one should use it as just that, not as the answer to the problem.

Despite Spaulding’s enthusiasm for demonstrating that anthropology and archaeology could be scientific, one ingredient missing from his discussion was “theory.” For him and the other symposium participants, theory was something that went hand in hand with method. Everyone knew what it was, so there was no need to define it. Spaulding appeared to be saying that theory was synonymous with explanation, and like Hempel, he was talking about how to structure explanations (e.g., deductively), not where the generalizations employed in explanations come from. Most scientists today accept that explanations derive in part from theory, in that theories suggest relevant variables and their relationships, and they can lead to the formulation of lower-level generalizations that can serve as covering laws.

For logical empiricists such as Hempel (1962, 1965b) and Ernst Nagel (1961), explanation is a two-tiered affair. Empirical phenomena are explained by lower-level covering laws, including (sometimes) empirical generalizations, and the covering laws in turn are explained by theories. Thus, a “complete” scientific explanation might make reference both to the relevant covering law(s) and to the appropriate theory; however, in the history of science, many covering laws are discovered experimentally and precede the formulation of relevant theory. They can even survive the latter’s demise. Spaulding either did not understand the difference between theory and explanation or, more likely, chose to ignore it. So did most others at the time, including Lew Binford, although that would change abruptly in the 1970s. What Spaulding was advocating was the creation of more empirically derived generalizations that could serve as covering laws. But as Binford (1977b:5) himself later noted, “statistical or probabilistic statements as to relationships between things are simply complex empirical facts.” A mature science would also need theories to explain those relationships—an issue that we discuss in greater detail in Chapter 4.

Binford contributed both the opening and closing chapters to New Perspectives. “Archaeological Perspectives” (Binford 1968a) was much more than an introduction to the rest of the book, for it rapidly became a blueprint for
what the new archaeology was supposed to look like. Yet in reading it today, one gets the impression that Binford was proposing not so much a different approach as a road map around some of the major obstacles that had impeded American archaeology from the start. Few issues raised in New Perspectives were new. Previous generations of archaeologists had talked about, for example, social organization, kinship and marriage, and even methodological issues such as classification, but rarely had there been such unanimity on how to deal with those issues, nor had there been case studies that tackled them with apparent success.

Binford had to demonstrate in his introductory chapter that the book did indeed offer "new perspectives." To do that he introduced three traditional topics of archaeological concern—the reconstruction of culture history, the reconstruction of past lifeways, and the study of culture process—and showed how methodological and theoretical impediments had foiled previous investigations. Regarding the reconstruction of culture history, Binford pointed out that despite an interest in genealogical connections among cultural units, archaeologists had no objective means of distinguishing between analogues and homologues. Binford argued that this "inherent unsolved problem of method and epistemology" required "an overhaul of method and theory" (1968a:11). His suggested "new" analytical protocol, however, was simply the traditional one with the addition of statistical techniques such as those outlined by James Doran and Frank Hodson (1968) to assist one to "measure likenesses" (Binford 1968a:11). This, in Binford's view, would allow one to formulate arguments "about the probability of one [cultural] taxon being the cultural ancestor, descendant, or collateral relative of another taxon (e.g., Hodson and Hodson 1966; Hodson, Sneath, and Doran 1966), or whether another unit might be more appropriately considered" (Binford 1968a:11).

The references Binford cited contained descriptions of data-clustering methods that today would be included in numerical taxonomy (Hull 1988b). He was echoing what Spaulding, his teacher, had taught him: statistically significant clusters of archaeological stuff indicated cultural significance. In Binford's case they were not Spaulding's attribute clusters, representative of cultural norms or mental templates, but rather artifact clusters representative of historically related cultural taxa. Whether they indeed were or not was debatable, just as it was in biology (Lyman and O'Brien 2003b; O'Brien and Lyman 2003a).

In discussing the reconstruction of past lifeways, Binford went into considerable detail about the misuse of cultural analogues, claiming that "so long as we insist that our knowledge of the past is limited by our knowledge of the present, we are painting ourselves into a methodological corner" (Binford 1968a:14). Binford devoted some time in the mid-1960s to considerations of ethnographic analogy and how the ethnographic record could be used to interpret the archaeological record (e.g., Binford 1967, 1968b; see also S. R. Binford 1968). It would occur to Binford and others in the 70s and 80s that the real problem was not methodological but ontological. Inferring the nature of past human processes and activities was being constrained by the indiscriminate use of modern analogues; a view that the past was no different than the present denied the existence of change (Chapter 5).

Binford (1968a) criticized traditional efforts to study cultural processes, suggesting instead a multistage procedure of observation, generalization, testing, reformulation, additional testing, and so on. He was largely silent in "Archaeological Perspectives" with respect to the identity of culture processes or how to discern them in the archaeological record. In earlier articles Binford (e.g., 1962a, 1963, 1965) listed the same mechanisms of cultural change as those invoked by culture historians. This should not be surprising given that at the time he was writing, formal discussions of culture change by ethnographers and ethnologists were little more than descriptions of the traditional mechanisms long used by the historical ethnologists (e.g., Murdock 1956; Spindler and Spindler 1959). They continued to be the major mechanisms dis-
cussed well into the mid-'70s (Woods 1975).

"Archaeological Perspectives" (Binford 1968a) was Binford's first thorough presentation of his views on the use of inductive and deductive research strategies. In it he cited Hempel, but his treatment of the philosophy of science was not as extensive as Spaulding’s. Binford (1968a:17) asserted that “our knowledge of the past is more than a projection of our ethnographic understanding [through analogy]. 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.”

Where Spaulding saw anthropology as a statistical science, Binford saw a deductive-nomological science built around the discovery of laws of cause and effect. Binford wrote about the difference between an empirical generalization and a hypothesis (a tentative law), and discussed how one went about testing a hypothesis. Spaulding had ignored the distinction between empirical generalizations and hypotheses because to him only empirical (statistical) generalizations were possible in anthropology. Binford introduced words that would soon become commonplace in the processual literature, such as observation, proposition, deduction, prediction, bridging argument, and confirmation. If he was aware of a major difference between his position and Spaulding’s on the nature of explanation in anthropology, he declined to comment.

Binford concluded his introduction to New Perspectives by reiterating that the kind of archaeology being espoused in the book was indeed new. He also warned that the new archaeology would bring with it a new order, so much so that the “radical” chapters in New Perspectives would render obsolete much traditional method and theory, along with familiar archaeological problems. Speaking in visionary terms, Binford (1968a:27) predicted “an expansion of the scope of our question-asking which today would make us giddy to contemplate. Despite a recent statement that one should not speak of a ‘new archaeology’ since this alienates it from the old (Chang 1967a:3), we feel that archaeology in the 1960’s is at a major point of evolutionary change. Evolution always builds on what went before, but it always involves basic structural changes.”

Binford was right: archaeology in the late 1960s was at a major turning point, and the changes were significant enough to warrant being labeled “new.” But not everyone agreed. K. C. Chang, a Yale archaeologist who had been trained at Harvard by Hallam Movius and Gordon Willey, was “not impressed by the phrase ‘new archaeology’ that one sometimes finds in the current literature” (Chang 1967a:3). Why? Because, as he pointed out in the introduction to his book Rethinking Archaeology, “what is old today was new in its own time, and what is new new will become old tomorrow. To say an archaeology is new is to alienate it from the old, whereas one could more profitably absorb and reorganize the old. Rethinking is a constant and routine mental process that brings about renewal at every turn” (Chang 1967a:3).

Close reading of Chang’s book indicates that he followed his own advice, effectively rewriting archaeological terminology using anthropological and human-behavioral terminology; for example, “a settlement is an archaeological unit of behavioral meaningfulness” (Chang 1967a:15). Here Chang admitted that he was heavily influenced by Clyde Kluckhohn (Ferrie 1995:320). Thus Chang’s notions had roots much earlier (e.g., Taylor 1948) and had come to find more consistent expression in the 1950s (e.g., Caldwell 1959; Willey and Phillips 1958).

Is it appropriate to say that Binford “founded” the new archaeology? Some sources think so. The Britannica Concise Encyclopedia for 2003 states under the entry “Lewis R(oberts) Binford” that he “initiated what came to be known as the ‘New Archaeology.’” Initiated is a synonym for founded or started, so it appears
that the Britannica assigns Binford the seminal role in the processual movement. In one sense this is correct, but it is also naive. Schools of thought do not emerge from a void: they build on what came before them. Binford, who clearly saw the process of archaeology as an evolutionary one, pointed this out when he said that evolution builds on what comes before. Still, we don't want to minimize Binford's impact. He might not have put archaeology on the road toward processualism (that had happened earlier), but he got it running boldly down that road rather than creeping cautiously along.

Binford sensed a general discomfiture with American archaeology as it was being practiced in the late 1950s, and he played off it when he penned “Archaeology as Anthropology” in 1962. Had he not written that article, someone else would have written one similar to it. Why? Because many, if not most, of the new archaeology’s components were already present during the 1950s as intellectual variants. Given the simmering dissatisfaction and the demographic and social changes occurring in American society and the intellectual academy during the early 1960s, others would have stepped forward to create a processual-like program in opposition to culture history. Although there is no denying that Binford was a highly skilled rhetorician, we cannot totally agree with Bruce Trigger’s (1989a) claim that it was Binford’s rhetoric that made processualism appear so different from what preceded it. New approaches are not invented; rather, they coalesce and are personified.

In Western culture all complex inventions, both conceptual and material, are assigned authorship—an inventor—regardless of the many contributions made by the anointed person’s predecessors and contemporaries. The legitimacy of that attribution serves as a focal point for endless discussions regarding the nature and extent of the change that the inventor supposedly wrought (Kuhn 1962a). The bottom line, however, is found in a quote from Francis Darwin (1914:9), who was pointing out the earlier contribution of Francis Galton to Adolph Weismann’s views on germ-cell continuity: “But in science the credit goes to the man who convinces the world, not to the man to whom the idea first occurs. Not the man who finds a grain of new and precious quality but to him who sows it, reaps it, grinds it and feeds the world on it.” And Binford was the consummate farmer, miller, and grocer.

Whether the new archaeology represented a major paradigm shift has been disputed since the late 1960s, when archaeologists discovered physicist-turned-historian Thomas Kuhn’s book, The Structure of Scientific Revolutions (1962b). Did processual archaeology represent a paradigm shift in the way that Kuhn meant, as a wholesale structural replacement of one approach by another? Paul Martin (1971) certainly believed this. In his article “The Revolution in Archaeology” he relates how processualism changed his life. He even hosted Kuhn in the field in 1968 (Figure 2.10). Two young processualists on Martin’s staff, Fred Plog (Figure 2.12) and Mark Leone (Figure 2.9), relentlessly fed Kuhn details on recent developments in archaeology, hoping that this great historian of science would pronounce processualism a scientific revolution. But through it all Kuhn remained noncommittal.

Robert McCormick Adams, on the faculty at Chicago and director of the Oriental Institute from 1962 to 1968, also believed a revolu-
ell continuity: "But the man who con-
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ution had occurred. In a review of archaeo-
logical research strategies written for Science in
1968, he pointed out that the new archaeology
was "less an explosion than a revolution in the
sense that it is a broad shift from one paradigm
to another not unlike the shifts which Thomas
S. Kuhn [1962a, 1962b] has metaphorically
outlined for the history of physics" (Adams
1968:1187). As an aside, Adams spoke of the
new archaeology with a somewhat tempered
approval, pointing out that despite the rhetor-
ic, there had been few processual products.

It might at first seem a bit odd that the di-
rector of the Oriental Institute—that staid or-
ganization founded by James Henry Breasted
and funded by John D. Rockefeller, Jr.—would
agree in principle with what the processualists
were attempting to do. Adams, however, was
only four years older than Binford and had re-
ceived his Ph.D. from the University of Chicago
in 1956, just three years ahead of Patty Jo Wat-
son. Thus, agewise and careerwise, he was not
that far removed from the Chicago-bred proce-
sualists. Also, whether he realized it or not,
Adams was playing a pivotal role in shaping
the future of processual archaeology with his
work on state formation (e.g., Adams 1960,
His work in the Near East, coupled with his
knowledge of the archaeology of the New
World and the Far East, led to his writing The
Evolution of Urban Society (1966), which
became the foundation for a generation of proce-
sual studies of complex society (Chapter 7).

The authors of this book agree that proce-
sualism was a "reform movement," to use Guy
Gibbon's (1989:11) term, but they disagree as to
whether processualism represented a paradigm
shift. Schiffer—who studied with the Binfords
as an undergraduate at UCLA, spent four sum-
mers at Vernon, and did his graduate work at
the University of Arizona—believes that the ad-
vent of the new archaeology was a scientific
revolution in the larger Kuhnian sense ("larger
because there are several senses in which Kuhn
used the term). O'Brien and Lyman agree with
David Meltzer (1979) that to count as a para-
digm shift in the larger Kuhnian sense, as a sci-
entific revolution, processualism would have
had to break with its predecessor metaphysi-
cally, not simply methodologically (see Custer

In Meltzer's view this break did not occur
because paradigm replacement entails structural
change in a discipline. No structural change,
no paradigm shift. Binford probably would
agree with Meltzer that a paradigm shift must
involve structural change, but he would argue
that processual archaeology did produce such
a change (Binford 1968a:27). Schiffer would
add that the structural change had many com-
ponents, the most far-reaching of which was at
the level of explaining cultural/behavioral vari-
ability and change. In Schiffer's view this led to
the replacement of what Binford (1965) re-
ferred to as "normative theory" (the "aquatic
view" of culture) by materialist ecological and
neo-evolutionary theory (Schiffer 1988).

O'Brien and Lyman wouldn't necessarily dis-
agree, but they don't see a clean break between
the traditionalists' undeveloped use of norma-
tive theory (in which culture is viewed as an
abstraction from behavior) and the processualists'
use of that theory to underpin their first sub-
stantive products (Chapter 3). A bending, yes—
and one that affected considerably more than
simply method and technique—but not a com-
plete break. The difference initially was an ad-
ditional explicit definition of culture as hu-
mankind's extrasomatic means of adaptation,
which meant that the functions of artifacts, not
just their styles, became an additional analyti-
cal focus. This focus had modest beginnings
long before 1962 (Kirch 1980). But speaking
of Kuhn, we collectively wonder if Binford
would agree with him that scientists have long
supported theories not only on epistemological
grounds but because of personal predilection
and even prejudice and animosity. We come
back to this point throughout the volume.

Whether or not a paradigm shift occurred,
major changes took place in how archaeolo-
gists went about their business. Recognition
of those changes was immediate, not something
that came through retrospection. When Binford
suggested in 1962 that archaeology needed
theoretical and methodological overhauling to become more anthropological, he was following a familiar trail blazed by Walter Taylor (1948), Betty Meggers (1955), Philip Phillips (1955), and Joseph Caldwell (1959). But their calls, issued a decade and a half or less before Binford’s article, produced little significant change. In Taylor’s case, the reaction was a mix of hostility and benign neglect because of the offensive manner in which he framed the call. Binford also challenged people, but his comments tended to be aimed at disciplinary practice in general rather than at individuals (with a few exceptions, discussed later). Binford made positive suggestions on how to make the discipline into an anthropology of the past—an anthropology that he had learned in large part from Leslie White while a graduate student at the University of Michigan.

Binford achieved such rapid and widespread success for several reasons. First, his alternative to traditional archaeology, unlike Taylor’s, was a relatively complete program. In less than ten years he put out a package that covered the gamut of archaeological method and theory as it was viewed in the 1960s. Second, he was a charismatic leader whose overpowering lecturing and debating styles inspired confidence in the message. Schiffer recalls that Binford fostered a cultlike following. His lecture style mimicked, perhaps unconsciously, that of a southern Baptist preacher. He did not use lecture notes, yet his lectures were so engaging that few students complained when he ran ten or fifteen minutes past the end of class. His sheer physical presence and the forcefulness of his delivery (Figure 2.13) added nonverbal elements to his lectures that inspired people to believe his every word. And although he counseled against ad hominem arguments in print (e.g., Binford 1968a), in class he would dismiss ideas and arguments he did not like as “nonsense” or “garbage,” implying that their advocates were dolts, sort of like Groucho Marx’s “Who are you going to believe—me or your eyes?” By discarding the traditional means of evaluating the work of others based on their expertise and credentials (Thompson 1956), Binford “was wip-

Figure 2.13. Lewis Binford lecturing at UCLA, 1967. (Photograph by Jerry Morris.)

ing the slate clean, saying in effect that a young person entering archaeology could write on that slate something significant” (Schiffer 1993a:3). As a student, it was exhilarating as well as a strong recruitment incentive to be told you could take on the establishment on a level playing field and also make significant contributions.

Another reason for Binford’s success was that at Chicago he was surrounded by smart people who, when they began teaching at other universities, also attracted smart people. But the key factor underlying the Binfordian program’s broad influence was the coincident demographic and social changes taking place in American society and universities, which enabled new generations of processualists to find academic positions and reproduce the program. Thus, by the early 1970s processualists were able to dominate theoretical and methodological discourse in American archaeology.

Domination does not signify a replacement, however. After all, the new archaeologists made precious few converts among traditionalists (Paul Martin being the most visible), and as of 1970 the old guard still retained most of the professional positions in American archaeology. And even though processualism was making ever-greater inroads, there was a wide middle ground between the traditional and the new. That is where one would have found most ar-
archaeologists, especially those not recently trained. In the mid-1970s David Hurst Thomas took an informal poll of 640 archaeologists, asking if they were "new," "traditional," or something else. Not unexpectedly, the answers were mixed, with one person responding that the survey was a "particularly loathsome example of simplistic reductionism." So much for labels. One interesting result was that even in the mid-1970s, at the height of the processualist movement, fewer than one in five Americanists felt comfortable calling themselves "new archaeologists" (Thomas 1989:58).

So what were Thomas's own feelings about labels and movements, particularly processualism?

I suggest that the new archaeology is best used to describe a commanding development within the history of Americanist archaeology. I relegate the new archaeology to historical contexts, for it describes a movement within archaeology that began in the early 1960s with the work of Lewis Binford; much of this agenda has been absorbed into mainstream archaeology. Those people who actually called themselves new archaeologists were really affirming that they liked what Lewis Binford said. I like what he said too, but I also liked several things that the "traditionalists" had said decades before and a number of things people have said more recently. (Thomas 1989:60)

Thomas's comments probably reflected the views of most Americanists at the time.

Using Thomas's data as a benchmark, we bring the story up to 1994, when the Society for American Archaeology conducted a survey of its 5,000-plus members. One of the questions on the comprehensive survey form was, "If you were asked to label the type of archaeology that you practice, or the 'school' of archaeology to which you belong, what label would you adopt?" The questionnaire listed eight options, but when she analyzed the data, Melinda Zeder (1997) combined some of the categories. Evolutionary archaeology was included with processual archaeology, and Marxist archaeology, critical theory, and gender studies were included with post-processual archaeology. The data, by age cohort and gender, are shown in Figure 2.14.

Zeder's intriguing analysis gives us a pretty clear picture of archaeologists' views in the mid-1990s. In brief, archaeologists in the two youngest cohorts (20–29 and 30–39), regardless of gender, identified themselves most often as processualists. Those in the two oldest cohorts (60–69 and 70–79) identified themselves most often as culture historians. Cultural ecology is represented fairly consistently across the cohorts, indicating that any attempts to bury ecology as an archaeological interest are premature (Chapter 3). Post-processualism was well represented among younger archaeologists, but not among those over forty. One of Zeder's (1997:128) conclusions was that "there is no evidence that processual archaeology (which, along with its founders, is now decidedly middle-aged) is about to be eclipsed by a nascent post-processual paradigm." We agree. Whatever it was that Binford and the early processualists crafted, three decades later 40–45 percent of the 150 youngest archaeologists filling out the SAA questionnaire didn't mind labeling themselves processualists.

MOTIVES AND RED HERRINGS

What motivated Binford to propose a change in the way Americanists practiced archaeology? Several of his autobiographical sketches offer some insight. In An Archaeological Perspective, composed primarily of articles he published while a graduate student at Michigan and an assistant professor at Chicago, Binford talks about the reasons he wrote some of them. "Archaeology as Anthropology" (1962a) grew out of a growing dissatisfaction Binford had with the state of American archaeology in the late 1950s. Succeeding articles—"Red Ochre Caches from the Michigan Area: A Possible Case of Cultural Drift" (1963), "A Consideration of Archaeological Research Design" (1964a), "Archaeological Systematics and the Study of Cultural Process" (1965), and "Smudge Pits and Hide Smoking: The Use of
Analogy in Archaeological Reasoning" (1967) — were written to elaborate some point Binford was thinking about or working on at the time. For example, the 1963 article resurrected the anthropological concept of "cultural drift" as a means of explaining nonfunctional variation within artifact assemblages. In several key respects the paper raised the same issues that Robert Dunnell would address fifteen years later — issues surrounding the analytical dichotomy between style and function (Chapter 8). Unlike the other articles, "A Consideration of Archaeological Research Design" is more methodological, the major points being the value of a regional approach in processual archaeology and the need for probability sampling (Chapter 3). The article was written after Binford’s first field season in the Carlyle Reservoir of southern Illinois (Binford 1964b). That work eventually included the excavation of Hatchery West (Figure 2.15), which received considerable notice because it was published in
the Society for American Archaeology Memoir series (Binford et al. 1970).

These papers are important from a historical perspective because of what they tell us about Binford's view of processual archaeology, but our main interest here is in “Post-Pleistocene Adaptations” (Binford 1968c), which is the final chapter of New Perspectives. Binford (1972d:341) claimed that not until he published that piece had he “achieved a processual model that was not developed for the explicit accommodation of a specific body of archaeological data. This was a different kind of model; its implications began to reach in the direction of law-like propositions, the goal of science.” Here Binford was championing deductive methods. “Post-Pleistocene Adaptations” was a reanalysis of the origin of agriculture in the Near East and its causes, and Binford’s conclusions were quite different from those reached by previous investigators.

One might think that the work was a natural outgrowth of earlier work—that by 1965, when he wrote the first draft for the Denver meeting, Binford had realized that simple inductive pattern recognition was not getting him where he wanted to go. Instead, he needed a model that dealt with culture process in a deductive manner. Undoubtedly this is true; otherwise Binford could not have written the paper as he did. But in one of the autobiographical notes in An Archaeological Perspective, he tells us that this work had a unique motivation. Whereas his earlier articles were “battles with ideas,” “Post-Pleistocene Adaptations” derived from his battles “with men” (Binford 1972d:339). To be exact, it derived from his battles with one man—Robert Braidwood (Figure 2.16), an Old World archaeologist with whom Binford had taught at the University of Chicago.

Binford could not have picked a more impressive opponent. Braidwood had achieved a sterling reputation as a Near Eastern archaeologist, earned through fieldwork conducted under the auspices of the University of Chicago’s Oriental Institute. Braidwood’s name became synonymous with scientific archaeology as a result of his excavations at the Neolithic site of Jarmo in the Zagros Mountains of northern Iraq, which he carried out between 1948 and 1955 (Braidwood and Braidwood 1950, 1953). The project was designed to study early village life in the Near East and to recover evidence of early food production (Braidwood 1958, 1960; Braidwood and Howe 1960, 1962). What made it especially innovative was the research team Braidwood assembled, which included not only archaeologists but also specialists in several aspects of the environment, including botany and zoology. At the time, the only similar team had been put together by Grahame Clark for his excavations at Star Carr in North Yorkshire, England (Clark 1954). The next comparable team would be one assembled by Richard MacNeish in the 1960s for work in the Southern Highlands of Mexico (Chapter 3).

According to Binford (1972b), Braidwood, as a member of the anthropology department at Chicago, had recommended against granting him tenure, labeling Binford "incompetent." Braidwood, by Binford’s own admission, had hurt him deeply, and Binford wanted to hurt him back. That was why he wrote “Post-Pleistocene Adaptations.” After reading it, we’re not sure how it was supposed to “hurt” Braidwood. Although critical of Braidwood’s ideas on the origins of agriculture and settled life in the Near East, it by no means belittles them (but see Binford and Binford 1966b), nor does Binford launch an ad hominem attack against Braidwood. Instead, he lays out an argument that calls into question Braidwood’s nuclear-zone model, which postulated that the change from food procurement to food production took place within “potential nuclear area[s]...where a whole constellation of plants and animals possible of domestication were available” (Braidwood 1963:322). Binford even kept himself in check when remarking on Braidwood’s infamous claim (Braidwood and Willey 1962:342) that pre-Neolithic groups had not become involved with food production because their “culture was not ready to achieve it.”

Binford (1968c:334) pointed out that “it was not that culture was unready, but rather
that the selective conditions favoring such changes had not previously existed." For Binford, the new selective conditions occurred at the end of the Pleistocene (about 10,000 years ago), when a worldwide rise in sea level led to the exploitation of seasonal resources such as migratory fowl and anadromous fish. This led to sedentism and "established for the first time conditions leading to marked heterogeneity in rates of population growth and structure of the ecological niche of immediately adjacent sociocultural systems" (Binford 1968:334). Local populations grew, which led to pressure on the resource base. To counteract this, groups fissioned, and daughter groups ventured off to the surrounding countryside—"contexts of a
much less spectacular character,” as Braidwood and Willey (1962:343) referred to them. There, the daughter groups intensified their relations with wild plants, which eventually led to domestication and food production.

“Post-Pleistocene Adaptations,” still one of Binford’s most-cited papers, would be responsible in part for a wholesale reconsideration both in anthropology and archaeology of the role played by population growth in culture change (e.g., Cohen 1975, 1977; Dumond 1965; Spooner 1972). But not everyone was impressed with Binford’s case study in prosessual archaeology. One detractor was Joseph Caldwell, who reviewed New Perspectives for American Anthropologist (Caldwell 1971). It was Caldwell who a dozen years earlier had heralded a shifting interest in American archaeology and the emergence of new issues and problems—what he had labeled the “new American archaeology” (Caldwell 1959). In his review of New Perspectives, Caldwell sniffed that “Post-Pleistocene Adaptations” was a “tedious and extraordinarily formal attempt to show that all who have worked in this range of time in the Old World have been using the wrong assumptions” and did “not offer any impressive improvements” on earlier efforts (Caldwell 1971:412). Caldwell’s harsh words seem a bit puzzling until one reads in his “new American archaeology” article that part of what he saw as the shift of interest in 1959 “must be ascribed to the outstanding work of V. Gordon Childe and others in the Old World” (Caldwell 1959:303). Binford hadn’t thought much of Childe’s (1934, 1936, 1944) ideas and had said so in “Post-Pleistocene Adaptations.”

Caldwell might have had another reason for dismissing Binford’s piece. He had received his Ph.D. from the University of Chicago in 1957, having gone back to school long after completing his undergraduate education. Caldwell’s most substantial contribution was a modified version of his doctoral dissertation, Trend and Tradition in the Prehistory of the Eastern United States, which was published jointly by the American Anthropological Association and the Illinois State Museum in 1958. One of the most important contributions of the monograph was Caldwell’s introduction of the concept “primary forest efficiency” to account for the traditional belief in the conservatism of Eastern Woodlands prehistoric groups. One could invoke efficiency, for example, to account for the then-apparent lack of agriculture in the East: “The hunting-gathering pattern was developed to a peak of efficiency and jelled, so to speak, in the very heart of eastern cultures” (Caldwell 1958:327; see also Caldwell 1962).

Caldwell’s notion of a primary forest efficiency was the very kind of construct that the processualists would pursue across the landscape in an effort to rid their kingdom of such demons. Yet Binford didn’t do this in “Post-Pleistocene Adaptations.” To the contrary, he cited Caldwell’s monograph with apparent approval. He did it, however, not because of the usefulness of this construct but because Caldwell was presenting evidence that groups can regulate their populations for long periods of time, provided they have a stable food supply. This was evidence Binford wanted for his own argument—that once in a while something happened to upset the balance, causing human groups to cross the critical threshold. That “something” was a growing population.

Caldwell’s notion of primary forest efficiency was derived directly from Braidwood’s “primary farming efficiency,” which Braidwood (1951) saw as the economic basis of civilizations. Braidwood, along with Fred Eggan, had
been a mentor to Caldwell while he was in the doctoral program at Chicago, a role for which Caldwell thanked him in the preface to Trend and Tradition. Binford had dared to criticize Braidwood, and Caldwell stood up for his former professor when he reviewed New Perspectives.

We have no way of judging, at least from the published record, whether Binford was ever successful in convincing Braidwood that he was wrong about Binford's intellect. What we find interesting are the external social factors that, at least in Binford's view, fostered his antagonism toward Braidwood and contributed to the motivation for writing "Post-Pleistocene Adaptations." Consider, for example, Binford's reminiscence about his family's social standing in the South:

As far back as I can remember I was told what a fine family I had come from; yet we lived during the [D]epression in anything but grand style. The society into which I emerged, where I saw my father humiliated by snobbish, socially pretentious men, left its mark. I was frequently embarrassed by my thick accent. I was made to feel that my family and I had to earn any respect we received on a day-to-day basis. Respect was not something to be expected because of a secure social position. Braidwood's manner, his style of living, and his voice all prompted emotions going back to my childhood. These are the social scars we all carry. (Binford 1972d:340)

All of us encounter people in daily life whom we would prefer to avoid. What makes Binford's story noteworthy is that he cited the social differences between himself and Braidwood as having played a role in his writing of "Post-Pleistocene Adaptations." This story has obvious salience to Binford. Indeed, its elements reappear in an interview Binford did with Colin Renfrew that was published in Current Anthropology (Renfrew 1987a) and in one he did with Paula Sabloff, published as Conversations with Lew Binford: Drafting the New Archaeology (Sabloff 1998).

Can we make too much of the cultural environment in attempting to contextualize a discipline's development? Yes, for there is the danger that we may focus only on those elements of the environment that make the course conform to our expectations and that further other agendas. To a large extent, in Land of Prehistory Alice Kehoe falls into this trap when discussing Binford. As mentioned earlier, Kehoe doesn't shrink from confrontation, but in several places in the book an unspoken agenda—to discredit Binford and, by extension, processesualism, at any cost—taints her interpretations. For example, in one place she employs Binford's autobiographical notes not as a means of understanding his actions and motives but to draw a parallel between processual archaeology and, of all things, scientific creationism.

Much of the New Archaeology of the first postwar generation parallels Scientific Creationism in its obsolescent conceptualization of science, and this is no coincidence because both movements were drawn from the worldview taught in conservative American Protestant congregations. Lewis Binford acknowledges his Southern "hills-south, hard-working" origin (Binford 1972[d], 340), though he hasn't discussed his undergraduate training at Virginia Polytechnic, a school that would soon after his graduation hire Henry Morris, later a founder of the Institute for Creation Research. The hills-south, hard-working society that conditioned young Binford owes a substantial portion of its heritage to the Scots emigration that also gave America the Presbyterian Princeton Seminary, fountainhead of Fundamentalism. Contextualizing intellectual history does uncover some unexpected bedfellows. (Kehoe 1998:xii-xiv)

Virginia Polytechnic Institute did hire Henry Morris, a hydraulic engineer, and later he did found the Institute for Creation Research. But VPI did not hire Morris because he was a creationist. Neither did Rice University before that.
This is tantamount to claiming that the University of Michigan hired Leslie White because he was a socialist. White was hired because Julian Steward’s departure in 1930 created an opening for an anthropologist, not for a socialist. When university administrators later learned of White’s political leanings, they were not pleased (Carneiro 1981; Peace 1993). One can only wonder what administrators at VPI thought of Morris’s creationist creed. What Kehoe does here is use social history to “support” her story of how a “remarkable shift” occurred in the 1970s from a profession visibly nearly exclusively white, male, Protestant, and American-born, to one that now reflects a far broader range of social positions” (Kehoe 1998:xiv). Kehoe views this shift as the “true revolution” in American archaeology, not the new archaeology of Binford and the other processualists. In an anthropology generally committed to scientific evolution, Kehoe could not have found a better way to denigrate processualism than by linking it, red-herringly, to scientific creationism.

Kehoe (1998:xiv) claims that to understand the shift she is talking about, we need to comprehend not only “the societal revolution instigated by the mid-century G.I. Bill, but also the contributions of professional women omitted from the standard histories of archaeology.” The G.I. Bill did make it affordable for many working men to go to college, and certainly the roles of women in the field (and the conditions under which they worked) have been underestimated until recently (e.g., Claassen 1994; Conkey and Gero 1997; Cordell 1993; Hays-Gilpin and Whitley 1998; Nelson, Nelson, and Wylie 1994; Sorenson 2000; White, Sullivan, and Marrinan 1999). Kehoe (1998:xiv), however, asserts that the change in the course of American archaeology was because “the old guard lost its power to exclude” as a result of the “influx of men from working-class and ‘ethnic’ backgrounds into the academy, and the protection given women by the 1964 Civil Rights Act” (see also various chapters in Kehoe and Emmerichs 1999). “Only now,” she continues, “in the middle 1990s, are consequences emerg-

No doubt the Civil Rights Act of 1964—specifically Title VII, which prohibits employment discrimination on the basis of race, color, religion, sex, and national origin—had a beneficial effect on the status of women in archaeology, but the act itself was the result of changing perceptions of women in the workplace that had begun decades earlier. Similarly, contrary to Kehoe’s claim, the “influx of men from working-class and ‘ethnic’ backgrounds” into the academy was nothing new in the 1960s. In fact, men from working-class families had long been at the center of American archaeology. Jim Ford grew up in Water Valley and Clinton, Mississippi, the son of an Illinois Central railroad engineer. Jimmy Griffin was born in Atchison, Kansas, to a railroad-equipment supplier. Gordon Willey was born in Chariton, Iowa, to a pharmacist. These men, arguably among the midcentury leaders in archaeology, were decidedly not from positions of prestige. Insofar as the distribution of social power is concerned, the institution from which one graduated and where one was employed—the basis for building the “old-boy networks”—were far more important than one’s social class.

In another move to link Binford to creationism, Kehoe reached back to a small Pacific island and 1954. Binford had mentioned in his interviews with Colin Renfrew (1987a) and Paula Sabloff (1998) that during the mid-1950s, while an army corporal assigned to occupation forces in Okinawa, he had become interested in archaeology (Figure 2.17). Binford did not, however, bring up an article published in the March 16, 1954, edition of Pacific Stars and Stripes. In an interview with an army reporter, Binford opined that the “world flood, mentioned in religion and verified by geologists, was responsible for the mass migration to the
Ryukyus [Islands] and for the high location of the [pithouse] holes" (Pacific Stars and Stripes 10[74]:8). We have all said or written things we later wish we could retract, just as we've all changed our views as we gain new knowledge and insight. Binford's early belief in the literalness of a biblical story does not explain his capability less than a decade later to effect significant change in American archaeology. If anything, one can regard the more mature Binford's untempered materialism as signaling rejection of his earlier beliefs.

Kehoe used the Pacific Stars and Stripes interview in an effort to embarrass Binford and to reinforce her contrived connection between scientific creationism and the new archaeology:

Both uphold an obsolete model of science that premises a real world out there awaiting discovery through human reason; a corollary premise is that humans have the capability to comprehend fully this world, if they use proper methods of discovery and interpretation. Scientific Creationists hark back to the concept of a Book of Nature presented by God, alongside the Book of Scripture. Spaulding, Binford, and their disciples left God out of the matter. Scientific Creationists and New Archaeologists are positivists insofar as they assume data exist in a pure state, and it is the task of scientists to recover these uncontaminated and offer unvarnished interpretations hewing closely to the observations. (Kehoe 1998:119)

As we shall see in later chapters, archaeologists have called into question many of processual archaeology's fundamental assumptions without resorting to comparing it to scientific creationism.

Did processualists sometimes overreach in their claims to having acquired knowledge about the past? Yes. Was there unbridled optimism that the past could be understood pretty much in its entirety? Yes. Certainly Binford was optimistic, stating in "Archaeology as Anthropology" that the "formal structure of artifact assemblages together with the between element contextual relationships should and do present a systematic and understandable picture of the total extinct cultural system" (Binford 1962a:219). But did processualists believe that data exist in a pure state? No. After all, they argued vociferously for new theoretical and methodological tools for handling data. Were the processualists positivists? Taken as a whole they were ("empiricists" is probably a better word), but a commitment to positivism is not a felony offense in science, despite what Kehoe implies. The main objective of processual archaeology was to do science, and Binford suggested that Hempel's deductive-nomological model of explanation and the hypothetico-deductive method furnished the keys. As several philosophers of science pointed out in later years (see Chapter 4), the construal of positivist scientific methods by some processualists was overly narrow, but this did not prevent them from doing innovative and influential research.

We should also appreciate that although the processualist program was generally positivist, practitioners had different views on how to make archaeology more scientific. Processualism was not nearly as unified as the stereotypes attacked by later critics would imply. For example, not all processualists were Hempelians (Flannery 1973a), nor did everyone find Leslie White's culturology inspiring. Processualism consisted of some widely shared theoretical and
methodological commitments, but significant variation—even some dissent—was present. In the 1970s and ’80s, a number of these non-conforming positions would develop into major departures from the program.

If one feels compelled to label processualism’s epistemological and methodological commitments, we would eschew “positivism,” which has come to mean little more than “explicitly science-oriented method in search of general laws and theories,” in favor of “pattern recognition.” Above all, the new archaeology was a search for pattern in data sets, regardless of their source or nature. The pattern-search approach, tied to the belief that cultural behavior in the past was patterned, was a key methodological commitment. Curiously, it was never appreciated that searching for patterns in data sets was not entirely compatible with a strict hypothesis-testing approach, unless one deduced from a hypothesis nothing more specific than the existence of some pattern (as did the early ceramic sociologists, as we explore in Chapter 3).

Pattern recognition seemed to trump other elements of processualism, as even Binford’s methodological and empirical works demonstrated. Binford spent the 1960s trying to put into practice what Albert Spaulding, the quintessential statistical archaeologist, had taught him. Why else did Lew and Sally Binford return to France in 1968 to search for patterns in François Bordes’s Mousterian data from the site of Combe Grenal in the Dordogne (Figure 2.18)? Bordes (1961) believed that the patterned differences in artifact layers that he saw in Mousterian sites were best explained by positing the existence of different Neandertal groups. As one group moved out of a rock-shelter after leaving behind its distinctive combination of tools, another group moved in and deposited its own tool combination atop the older ones, and so on through time. The Binfords disputed this interpretation, arguing early on (Binford and Binford 1966a) that the differences were functional—the result of different activities—not ethnic and stylistic.

Despite the importance of pattern recognition, it was not the end product of a processualist’s analysis. Finding patterns was only a preliminary step, as Binford (1972d:338) pointed out: “It is a common error to feel that something has been explained when a particular form of patterning can be subsumed under a general cognitive category.” Explanation, to many processualists in the late 1960s and early ’70s, lay in the use of the deductive-nomological model (based on deterministic laws) (e.g., Fritz and Plog 1970; Watson, LeBlanc, and Redman 1971). Ironically, the inductive-statistical model, also discussed by Hempel (1965b) and seemingly more appropriate for the new statistical orientation, for the most part was overlooked until it was introduced to archaeologists by philosophers of science in a more general version known as the “statistical relevance” model (Salmon 1982; Salmon and Salmon 1979). But deterministic laws were not emerging in profusion from processual studies. Nor did the early new archaeologists seem interested in exploring the possible role of theory for explaining patterns discerned in the archaeological record.

As mentioned earlier, the processual literature of the 1960s is strangely silent on the subject of theory. For example, the term is absent from the indices to New Perspectives and An Archaeological Perspective, whereas “explanation” is prominent in both. It is a mistake to think that processualists had no idea that theory could play a role in archaeological explanation; they did understand that point, but it was more of an implicit understanding. The reason for this is clear, at least as it pertains to the early days of processualism. The processualists did not have to create theory; they merely had to borrow it from ethology, specifically from Leslie White’s culturology and evolutionism, and from Julian Steward’s cultural ecology. The filter through which this theory was funneled from ethology to archaeology was Lew Binford.
BINFORD ON CULTURE
AND CULTURE PROCESS

Shifting the ontological basis of American archaeology in the 1960s meant redefining two concepts: culture and culture process. As noted in Chapter 1, for many anthropologists and archaeologists of the 1950s, culture processes included the standard ones observed in ethnographic settings: invention or innovation, diffusion, migration, and the like. White had different views. In The Evolution of Culture, White (1959:17) implied that culture processes are the “functions” of a cultural system. Earlier he had defined the culture process as “a stream of interacting cultural elements... In this interactive process, each element impinges upon others and is in turn acted upon by them. The process is a competitive one: instruments, customs, and beliefs may become obsolete and eliminated from the stream. New elements are incorporated from time to time. New combinations and syntheses—inventions and discoveries—of culture elements are continually being formed” (White 1950:76). In “Archaeology as Anthropology,” Binford (1962a:217) defined process as “the operation and structural modification of systems.” He was not the only person at the time who was adopting White’s views on process. Robert Carneiro, a cultural anthropologist and another of White’s students, defined a process as “the interaction through time of the elements of a system as the system changes from one state to another” (Carneiro 1960:145). That change from one state to another was cultural evolution.

Binford (1962a:218) paraphrased White’s (1959) definition of culture as “the extrasomatic means of adaptation for the human organism,” but his discussion indicates that another reading for the word “means” could have easily and perhaps more accurately been “system.” According to this definition, a culture no longer is defined as a set of ideas transmitted from individual to individual, shared by a group, and resulting in similarities and continuities across time and space. Although this normative definition had been widely adopted by archaeologists, Binford viewed it as being inadequate for generating any kind of useful hypotheses of cultural process. It did not allow the measurement of multivariate phenomena, permitting the measurement only of “unspecified cultural differences and similarities,” as if these were univariate phenomena” (Binford 1965:203). Changes in trait frequency through time were seen as results of diffusion, drift, or migration, all viewed as being quite natural and regular occurrences.

Cultural differences and similarities are expressed by the normative school in terms of “cultural relationships” which, if treated rigorously, resolve into one general [interpretive] model. This model is based on the assumption of a “culture center” where, for unspecified reasons, rates of innovation exceed those in surrounding areas. The new culture spreads out from the center and blends with surrounding cultures until it is dissipated at the fringes, leaving marginal cultures. Cultural relationships are viewed as the degree of mutual or unilateral “influence” exerted between culture centers or subcenters. (Binford 1965:204)

Binford was especially critical of the “aquatic view” of culture:
Interpretive literature abounds in phrases such as “cultural stream” and in references to the “flowing” of new cultural elements into a region. Culture is viewed as a vast flowing stream with minor variations in ideational norms concerning appropriate ways of making pots, getting married, treating one’s mother-in-law, building houses, temples (or not building them, as the case may be), and even dying. These ideational variations are periodically “crystallized” at different points in time and space, resulting in distinctive and sometimes striking cultural climaxes which allow us to break up the continuum of culture into cultural phases. (Binford 1965:204)

The “cultural phases” to which he referred were the Willey-and-Phillips units: archaeological complexes of shared artifact types that were treated as if they were a historical ethnologist’s culture. Binford (1965:205) made the point, which was appreciated at some level by many archaeologists, that these “normative” constructs obscured potentially informative variation: “This emphasis on shared traits in our system of classification results in masking differences and in lumping together phenomena which would be discrete under another taxonomic method…. We should partition our observational fields so that we may emphasize the nature of variability in artifact populations and facilitate the isolation of causally relevant factors.” Binford was calling for a new archaeological systematics that placed a priority on discovering and explaining nonnormative variation. Yet, as discussed below, his concept of “pattern” included a normative element. His real target of criticism was the mentalist component of traditional normative theory, for which he wanted to substitute a behavioral component: pattern.

To Binford, a culture was a system of interrelated subsystems and elements that varied in composition and structure depending on its context in time and space and also on situational factors such as a group’s demographic composition. Only archaeologists had access to the entire time depth of the existence of cultures, and thus they were the ones who could contribute to an anthropological understanding of how and why cultures changed. Binford (1962a:224) echoed Walter Taylor when he stated that archaeologists had a “responsibility” to further the aims of anthropology and to use their data to solve “problems dealing with cultural evolution or systemic change.” Heady stuff, but how were archaeologists to operationalize those ideas? Binford (1962a:219) presented in two sentences a plan for putting these ideas into practice, thereby establishing a basis for the research agenda he would follow for the remainder of his career: “I would consider the study and establishment of correlations between types of social structure classified on the basis of behavioral attributes and structural types of material elements [artifacts] as one of the major areas of anthropological research yet to be developed. Once such correlations are established, archaeologists can attack the problems of evolutionary change in social systems.”

Ideas about how cultural systems worked and evolved had to be tested “against ethnographic data” (Binford 1962a:223) because it was only in the ethnographic context that one could actually witness the operation of culture processes (“dynamics”). Ethnographic data would form the basis of explanatory models, or what Binford (1962a) referred to as “frames of reference.” As an aside, he used that term fifteen years later (Binford 1987b) and also as part of the title of his most recent book, Constructing Frames of Reference (Binford 2001)—a magnum opus that serves as a fitting culmination to a forty-year career of trying to educate archaeologists about how to learn about the past. The volume contains a plethora of useful ethnographic and environmental data that will, as Stephen Shennan (2004:511) put it, be “pillaged by researchers for years to come.” Similarly, some of Binford’s conclusions will undoubtedly be quoted by students and professionals “like preachers citing scripture” (Lekson 2001:538). Following a precedent he set about twenty-five years earlier (Binford 1977b), Binford does not start with a general,
high-level explanatory theory, instead presuming that the data will speak for themselves. Such a patently inductive approach is at odds with his even earlier arguments regarding deduction and hypothesis testing as the way to make archaeology a science.

The key to the concept of frames of reference was in using ethnographic information in a new way. Ethnographic analogues had previously served as interpretive devices, as exemplified in Robert Ascher’s (1961a, 1961b) work. Ascher (1961a:317) wrote that in a general sense “interpreting by analogy is assaying any belief about nonobserved behavior by referral to observed behavior which is thought to be relevant.” Modern nonindustrialized people used tools of a particular form when undertaking a particular task. A prehistoric archaeological example of that same form was interpreted by analogy to have been used for the same task and thus to represent the same behavior. Binford argued that were archaeologists to continue this kind of analogical argument, they would “at best increase [their] understanding of archaeological observations in terms of ethnographically described situations” (Binford 1967:10). This represented a set of potentially limited possibilities and reaffirmed that the past was no different than the present.

To escape this limitation, Binford (1967:10) recommended that one study the covariant relationships between independent classes of phenomena because this would eventually produce “general laws of cultural variability.” Ruth Tringham (1978:188) described the protocol a decade later: Binford’s (1967) procedure “includes examining (1) spatial correlates, (2) temporal correlates, and (3) formal correlates and associated activities of the hypothesized ethnographic activity... and also observing the presence or absence of the same correlates in the archaeological data.” LeRoy Johnson (1972:369) identified the key interpretive aspect of this procedure when he noted that by phrasing the analogy-based inference as a hypothesis, the deduced consequences of the hypothesis (typically referred to as “test implications” by the processualists) are “linked together with other things [via the hypothesis] in a highly insightful way to produce a new understanding.” Not everyone comprehended these critical aspects of studying the covariation of multiple variables in order to escape the confines of ethnoarchaeological data (compare Monson 1969 with Binford’s [1972g] rebuttal).

Binford’s point was that human behavior, irrespective of spatial and temporal coordinates of particular behaviors, was patterned. Further, “data relevant to most, if not all, the components of past sociocultural systems are preserved in the archaeological record” (Binford 1968a:21). Here again was the optimism Binford had first expressed in “Archaeology as Anthropology” (Binford 1962a). Similarly, several processualists (Deetz 1968a; Hill 1966; Longacre 1968) noted that their operating assumption could be phrased something like this: “Human behavior is patterned, and the patterns detected in the archaeological record reflect those behaviors.” More optimism. But to carry out this anthropologically oriented agenda, with its focus on culture processes, the processualists needed data. Those data could come from a number of sources, the primary one being artifacts.

CHANGING VIEWS OF ARTIFACTS

The idea that artifact types had emic meanings had been implicit in American archaeology from the start, but it was made explicit by Albert Spaulding (1953b, 1960a), who advocated the use of statistical procedures to discover clusters of attributes (one cluster per specimen) that occurred more frequently than random chance allowed. In combination with refinements periodically suggested for classifying artifacts (e.g., Kluckhohn 1960; Krieger 1960; Phillips 1958; Rouse 1960; Sears 1960; Smith 1962), more and more archaeologists became explicit in suggesting that artifact types had some sort of “cultural significance,” as James Gifford (1960) put it. Gifford advocated the use of the type-variety method of artifact classification first formally described by Joe Ben Wheat, Gifford, and William Wasley (1958).
On one hand, when Wheat, Gifford, and Wasley spoke of an artifact “variety,” they had in mind an analytical (etic) unit that measured a small spatiotemporal range. It was a subunit under artifact “type,” with types having larger spatial and temporal distributions than varieties. Gifford (1960), on the other hand, while agreeing that types and varieties were useful tools for purposes of culture history, made two further assumptions that he and his coauthors had not made earlier. First, varieties approximate actual material ceramic manifestations of individual and small social-group variation in a society. Second, because types generally include multiple varieties, they are summations of individual and small-group variation. Types both reflect cultural values and are a “ceramic idea or ‘esthetic ideal’ the boundaries of which were imposed through the value system operative in the society by virtue of individual interaction on a societal level” (Gifford 1960:343).

Gifford argued that most people in a culture conform to the demands of a majority of the norms. To substantiate this claim, he quoted various statements by Alfred Kroeber and Clyde Kluckhohn, both of whom had argued that a culture is patterned over time and space as a result of limitations and constraints on innovation that originate in a culture’s values or standards. For Gifford (1960:343), then, artifact types “equate themselves with the crystallization of conscious or unconscious…esthetic images conditioned by values.” This basic notion that artifacts were made to some sort of culturally dictated convention was reiterated two years later by Watson Smith (1962:1167), who argued that a fictional female potter was subject to “an alarmingly ramified set of intellectual complexes that act to control her ultimate [ceramic] output. Some of these are restrictive, some are expansive. We call them Tradition and Imagination, Conservatism and Invention,” respectively.

This is what Binford (1965) had referred to as the traditional, “normative” view of culture, with its emphasis on innovation, diffusion, and migration. Binford (1981b:6) later referred to this as “the view that history causes history.” This simply would not do in the new, prosessual archaeology. Redefining culture as a means of adaptation provided a host of explanatory options centered around the notion that culture will change when its environmental context changes, where “environment” is broadly construed to include cultural as well as natural phenomena. Thus a culture changes because of adaptive necessity, reducing, at least for Binford, the circularity in saying that culture (ideas) changes because ideas (culture) change. It could be argued that there also is circularity in saying that culture (adaptation) changes because adaptive necessity changes.

But if artifact types were to have emic connotations and also to reflect their functions in terms of adaptation, they had to exhibit two rather different properties. Binford (1963) recognized this. In effect, he argued that what today would be termed “cultural transmission” unmediated by selection would be operative only “within the individual’s cultural idiolect or the shared behavioral aspects of culture”; the cultural system, on the other hand, would be modified “through processes of readaptation or evolutionary change” (Binford 1963:92). This is because formally different elements of culture, such as decorative motifs, can be “functional equivalents” when their formal properties crosscut functional classes, and thus they can change by the vagaries of cultural transmission—what Binford labeled “drift”—without affecting the basic structure of the cultural system.

Cultural drift was “a process of formal modification in culture content, particularly within classes of functional equivalents or in relative frequencies of stylistic attributes which may crosscut functional classes” (Binford 1963:93). Stylistic variability was that which varies “with the social context of manufacture exclusive of the variability related to the use of the item”; “historical continuity and social phylogeny are particularly amenable to analysis through the study of stylistic attributes” (Binford 1965:208). Again, “stylistic attributes are most fruitfully studied when questions of ethnic origin, migration, and interaction between
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groups is the subject of explication” (Binford 1962a:220).

One curious feature of the Binfords’ work with Bordes’s Mousterian materials was their use of his types. They indicated that these types were “descriptive” (shape related) rather than strictly stylistic or functional. Thus, Bordes’s types were at best a very crude measure of functional variation—assuming general correlations between form and function. It was not that Lew suddenly forgot the style-function distinction that he had made a few years earlier (Binford 1963). We suspect that other reasons led to the use of this typology. First of all, the Binfords had to show that, even using Bordes’s types, there was a considerable amount of unexplained interassemblage variation. Second, they had to remain in Bordes’s good graces to obtain access to his Mousterian collections. Using his collections to create another typology would have been unthinkable, especially to Sally, who had established good rapport with the sometimes volatile Frenchman. Third, the Bordes typology was the unquestioned gold standard of Mousterian lithic analysis, and Middle Paleolithic archaeologists flouted that convention at the risk of their reputations.

The debate over the meaning of variation in the Mousterian artifact assemblages continued until Bordes’s death in 1981 (Binford 1989b). It has not yet been resolved. Early on, James Sackett (1968:73) had made clear how critical it was to classify artifacts such that human activities could be “consistently isolated and interpreted.” He stressed that the “question of tool function is especially crucial in view of the emphasis a cultural ecological approach places upon the reconstruction of economic tasks” (Sackett 1968:73). These concerns finally led to what came to be referred to as “use-wear” studies—microscopic examination of wear patterns on tools to determine how they were used (e.g., Frison 1968; Keeley 1974b; Morse and Goodyear 1973; Nance 1971; Semenov 1964; Tringham et al. 1974; Wilmsh 1968a, 1968b). We examine some of these studies in Chapter 5. Lew was aware that use-wear analysis would be required to construct a credible functional typology for the Mousterian artifacts—he said so in a class at UCLA in 1968, as Schiffer recalls—but that would have been regarded as an undertaking far too ambitious at the time.

Although social and practical reasons led the Binfords to use the Bordes system, even though it was an imperfect measure of artifact function, other processualists had a Spaulding-esque faith in the power of statistics to reveal past behavior. Indeed, some processualists apparently believed that the objectivity of statistical methods would detect patterning among artifacts and variously identify types (statistically significant clusters of attributes) and tool kits (statistically significant clusters of types), and thus the archaeologist didn’t need to worry about what the classification actually was measuring. Simply input the attributes or types, and the computer would do the rest. As might be expected, some who commented on statistical and computer-assisted classification efforts (e.g., Benfer 1967) found such methods useful, but others wondered about the appropriateness of various techniques in archaeological settings (e.g., Sackett 1969). Jim Hill (1972), in a detailed processualist treatment of classification, affirmed J. O. Brew’s (1971) point that classifications were not rote methods but rather problem-oriented measuring instruments whose construction required great care. Again, Binford (1968a) had argued that numerical taxonomy would solve culture history’s problem of how to distinguish between analogues and homologues, but whether it could be used for addressing other problems was unclear.

Alex Krieger (1960:146) identified the most serious problem in using statistical methods to discover artifact types when he noted that “a different set of ‘types’ would result for each site or run of material treated”; that is, the types would be specific to the set of specimens analyzed. This was disputed by Donald Tugby (1965), who in an early overview of statistical methods argued that they allow one to identify clusters of phenomena at various scales (attributes, discrete objects, sets of discrete objects). He noted that “at the first step in archaeological research—the definition of a
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basic unit—there is some uncertainty about the criteria to be employed in choosing the relevant attributes” and that, following Julian Steward (1954), one could distinguish among morphological types, historical-index types, functional types, and cultural types (Tugby 1965:4). Tugby failed to indicate, however, how attributes were to be chosen to build any one of these typologies, yet this is where the problem identified by Krieger actually originates. Indeed, by choosing different attributes, one will create different types. The nature of the resultant types, whether generated statistically or by other methods, depends entirely on the attributes chosen. Kluckhohn (1960:136) had emphasized that “one makes certain that the criteria [attributes] chosen are actually relevant to the purpose or purposes at hand,” but he admitted the difficulties of executing this dictum and furnished no procedure or guidance for choosing attributes. Spaulding (e.g., 1960a) seems never to have thought of this as a significant problem.

The excitement in American archaeology created by Lewis Binford’s early thoughts on such topics as culture, systems, and analogy snowballed throughout the 1960s. Archaeology entered the next decade on a rush of enthusiasm, for seemingly processualism had become, like postwar science in general, an “endless frontier.” For diverse reasons—some of them academic, some of them undoubtedly personal—Binford made it his mission to reconfigure archaeology into something an ethnologist would recognize. That Binford’s plan all along was to create an anthropological archaeology should not have been too surprising to anyone reading American Antiquity in 1962. The title of his first article, after all, was “Archaeology as Anthropology,” and its opening sentence was, “It has been aptly stated that ‘American archaeology is anthropology or it is nothing’ (Wille and Phillips, 1958, p. 2)” (Binford 1962a:217). As we noted earlier, that phrase had been recycled from an earlier statement by Phillips (1955:246-247) that read, “New World archaeology is anthropology or it is nothing.” Strengthening the link between archaeology and anthropology had been on the agenda of the Phillips-and-Willey generation long before Binford’s pronouncement.

Binford added a new, and strident, voice expressing the general unrest in American archaeology when he wrote “Archaeology as Anthropology.” But his commitment to anthropology went beyond underscoring Phillips’s one-liners, for he followed up the 1962 publication with a series of articles that by the end of the decade had sketched out the shape of a “new,” anthropological archaeology. Much of Binford’s success can be attributed to having landed his first academic job in a prestigious department that attracted very bright and ambitious students. Although ensconced in a conservative department, these students were ready to countenance change. Had he gone to a college or small university, especially one lacking a graduate program in anthropology, his impact might have been far different. Binford found a receptive audience for his ideas, but the give and take that occurred at Chicago was hardly one sided. Others worked out not just details of Binford’s program but also key elements and their implications.

Binford was a master at using every tool at his disposal to promote processualism, in the process promoting himself—too much, some would say, including Jimmy Griffin (1976), under whom Binford had worked at Michigan. Binford would have been the perfect front man for most sales promotions; the only difference in archaeology was that he was selling his own product. And it was a product in which he firmly believed. In some of his early publications Binford revealed a quality employed by good salespeople: the ability to be ambiguous or to resort to confusion when pushed for specifics. This works particularly well when you haven’t quite figured out everything and are making it up on the fly, or when you say something and later have to make it sound as if that’s not exactly what you said. Take this sentence, which James Stoltman (1984) highlighted in his review of Binford’s Working at Archaeology: “If we recognize that cultural systems are differentially localized in different places and are
internally differentiated, then we must expect some of the more visible archaeological patterning to refer to these internal differentiations and their differential disposition in space, not systems change or differences in the identity of systems per se." (Binford 1983:65).

Binford's sometimes confusing style wasn't limited to the written word, as he demonstrated in an interview with Paula Sabloff in 1982.

Paula Sabloff: You have a reputation as being rough around the edges in language.

Lewis Binford: If I'm trying to say something that I don't think has been said, there's no trite way of saying it. A cliché is usually pretty meaningless and also obvious to anyone who reads it. If you're trying to say something with the same words that everybody else is using, but you think you don't want them to think about it the same way, you have to play with the way you use words. If an editor or person reads my sentence, which I wrote in clear prose, and says, "Yeah, I know what you're saying," then I know that he missed the point; and I take that sentence and make a whole paragraph out of it to make sure that he understands what is different about what I am saying. I write so that people have got to read and reread it so that maybe they have got the meaning.

PS: Why? Why didn't you think the first time worked?

LB: Because they translated it into what they thought I was saying, not what I was saying. In a sense, the clearer writing is, the more ambiguous the terms are. In other words, the clearest sentence would be the sentence that everybody would give meaning to immediately. But the degree [to which] they all do it the same way is not at all clear.

PS: You mean in scientific writing.

LB: That's right. And if I'm trying to manipulate a reader, I can't do it by making him think he knows what I'm saying. Because if I think I'm saying something that he doesn't know; or I think I'm saying something new, then why should he think it's all so clear and he's thought it all along? (Sabloff 1998:65)

Any serious challenges to Binford were still far off in the 1960s. Discovering cultural processes was still firmly at the top of the agenda, and it was becoming increasingly clear, at least to some processualists, that the path to those processes led through the philosophy of science, deductive methods in particular. Hints in this direction had been dropped by Albert Spaulding (1968), but by 1970 those hints had become full-fledged statements by archaeologists-turned-philosophers—a topic that we examine in Chapter 4. But first, what about products of the new archaeology? One can talk all day about culture and processes, but without salable products, customers will begin to drift away. In the case of processualism, those products were being cranked out even as Binford was penning "Archaeology as Anthropology." We inspect some of those early products in Chapter 3.