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LEWIS ROBERTS BINFORD
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A *Biographical Memoir by*
DAVID J. MELTZER

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Lewis R. binford[1] was the most influential American archaeologist of the 20th century, yet rarely conducted fieldwork, was indifferent to such traditional goals as defining new artifact types or archaeological cultures, and never made a headline-grabbing discovery. For Binford finding *things* was never as important as *finding things out*, for he was foremost a man of bold ideas and strong opinions, and not shy about expressing either. Equipped with a messianic fervor, an extraordinary work ethic, a spellbinding speaking style, and a gale-force personality (he could be utterly charming one moment, fiercely caustic the next), he sought nothing less than the overthrow of mid-20th-century archaeological orthodoxy. Culture history, it was called, and in Binford's view it scarcely rose above descriptions of artifacts and sites and their placement in time and space, and never grappled with larger questions of how past cultures adapted to their environment or changed over time. Caricature, perhaps, but even in that, there can be truth.

Starting in the 1960s Binford pushed, pulled, or otherwise cajoled archaeology into becoming more anthropological, evolutionary, and scientific. His contributions over the next four decades had breadth and depth, and forced a radical retooling of archaeological theory, method, and explanation;

helped advance work in hunter-gatherer studies, ethnoar-chaeology, zooarchaeology, and archaeological site formation processes (among other areas); and sparked fundamental debates over the nature of early human evolution. He did not have the last word on those subjects but often enough had the first word and, for that matter, a great many of the words in between.

Binford succeeded spectacularly in changing the direction and discourse of the discipline. Along the way he alienated a generation of powerful elders, sharply changed his own course, and came to oppose many who had been inspired by his lead (including former students), but who pursued their own paths as the archaeology he helped create grew ever more diverse and his original vision diffused.

Eventually "young Turks" grow old. Yet, Binford hardly lost his energy, enthusiasm, and creativity—or, for that matter, his zest for a fight. His ideas inspired, guided, or were targets for much of archaeology over the last half century, and archaeologists will continue to grapple with them for a long time to come.

EARLY YEARS

Lewis Binford was born on November 21, 1931,[2] in Norfolk, Virginia. An only child, his father (Joseph Lewis Binford) was a one-time electrician and labor organizer in the Appalachian coal mines, his mother (Eoline Roberts Binford) was descended from Virginia Tidewater high society, though by the time of Binford's birth her family had lost its financial security but evidently not its pretentiousness (Binford, 1972, p. 340). Being told how fine a family it was, yet confronted with the jarring reality of their reduced circumstances, made a strong impression on Binford. It essentially preadapted him, he later supposed, to what science was: using experience to evaluate ideas (Sabloff, 1998, pp. 49-50).

So too did the Boy Scouts. Unhappy with learning in public school and its emphasis on recitation and learning conventions, Binford relished his preteen years "learning while doing" in the scouts. In Virginia's marshes he observed animals and plants, learned how to anticipate seasonal change and understand natural events, and found archaeological sites and arrowheads—sparking (as he admitted) naïve notions of American Indians in their environment (Sabloff, 1998, p. 60). Still, learning about sites and meeting landowners proved valuable when he returned to that part of Virginia a decade later to conduct his dissertation research (Binford, 1964a; Thurman, 1998, p. 31).

Given his family's limited financial means Binford was often shuttled between the homes of relatives, and by his teens was working a variety of jobs, primarily in construction. Initially hired as a manual laborer, he became skilled in construction and was able while in college to help support his family as a builder, and in later years constructed some of his homes (Sabloff, 1998, pp. 55-56). One vestige of that early training is evident in his archaeological publications: his site maps resemble architectural plans, and these and his other illustrations are instantly recognizable by their draftsmanship and clarity.

Graduating high school, Binford enrolled (in 1948) at Virginia Polytechnic Institute on an athletic scholarship, intending to play football and study forestry or wildlife biology. After discovering that athletics demanded too much of his time, he gave up the scholarship to concentrate on academics, but that put a strain on his finances (he was by then married to Jean Mock, with whom he had two children). He enlisted in the army in 1952 with an eye on gaining GI Bill support for his education after his service. He was sent to Okinawa, where he worked with native Ryukyuan peoples. That gave him a first exposure to a culture vastly different

from his own, and the chance to conduct excavations of shell middens and tombs torn up by military construction (Renfrew, 1987, p. 684).

By the time of his discharge in 1954 Binford was far more interested in pursuing anthropology than biology. He enrolled at the University of North Carolina, where he earned his B.A. in 1957, and came to realize that if anthropology was to "cope scientifically with the problems of why cultures change," it needed data with time depth (Binford, 1972, p. 2). That turned his interests to archaeology. Under the tutelage of Joffre Coe, Binford gained valuable field experience, read the literature, and began to question the conceptual underpinnings of the discipline. Though Coe was very much a traditional culture historian (e.g., Coe, 1964), he was something of a subversive one. He encouraged his students to think critically, and suggested Binford read Leslie White (1949), one of the major figures seeking to bring cultural evolution back into the anthropological fold, as well as Walter Taylor's earlier (and largely resented and rejected) manifesto against culture history, "A Study of Archaeology" (1948). Binford did.

Armed with the belief that archaeology could and should do far more than merely situate ancient cultures in time and space, and keen to bring it into the mainstream of anthropology, Binford went to the University of Michigan for graduate work. Influential in his education there were White, Albert Spaulding (from whom Binford learned analytical methods), and James Griffin, the quintessential culture historian, dean of eastern North American archaeology, and for Binford graduate adviser and symbol of all that was (and was wrong with) traditional archaeology (Sabloff, 1998, p. 13). Binford earned his M.A. in 1958 and Ph.D. in 1964 at Michigan, though Griffin did not last as his adviser. Early on, the two grew estranged over what archaeology

was or ought to be, a difference exacerbated by the clash of two very strong personalities. Binford (1972) provides a colorful account of his years at Michigan. Griffin, whose recollection of events was very different, felt compelled to publish a rejoinder (Griffin, 1976; Cleland, witness to some of the events, provides another perspective in Quimby and Cleland [1976]).

After teaching at Michigan for a year, Binford joined the University of Chicago anthropology faculty in 1961. Although lacking his Ph.D., he hardly lacked confidence. He opened his first graduate class at Chicago by announcing, "My name is Lewis R. Binford, and the name of this course is Revelations!" (Flannery, 2006, p. 5). The students were thrilled; the senior members of the faculty with whom he soon clashed, less so. Binford left Chicago four years later, still brash though unbowed despite having been denied tenure. By then, at least, he had received his Ph.D. but only after Griffin was persuaded to resign from his dissertation committee (Binford, 1972, p. 11). It was the first overt breach of what was a long, acidic relationship.

CRAFTING THE NEW ARCHAEOLOGY

It was at Chicago that Binford launched what came to be called the "New Archaeology" (later, "Processual Archaeology") with his landmark article "Archaeology as Anthropology" (1962). It proclaimed there was far more to archaeology than time-space systematics: archaeology should further the aims of anthropology and "directly test hypotheses concerning the process of evolutionary change" (Binford, 1962, p. 224). To accomplish that would require radical changes in thinking, method, and attitude—especially attitude. It was often said, Binford wrote, that archaeologists "cannot dig up a social system or ideology." He granted the point (a subtle dig at Griffin) but then pivoted in a way few

could have seen coming in 1962: "but we can and do excavate the material items which functioned together with these more behavioral elements," which together yield a "systematic and understandable picture of *the total extinct* cultural system" (Binford, 1962, p. 218-219, emphasis in the original).

Culture was key. It was not reducible to a list of shared traits (say, pottery or projectile point types), nor was culture change explained by merely tallying differences in traits—the traditional view. Rather, following White (1959), Binford viewed culture as an extrasomatic means of adaptation to the physical and social environment, with artifacts having a role in the structure and function of the cultural system. But not always the same role, for artifacts variously functioned within a culture's technological, social, and ideological subsystems, and behavior might vary (even within the same culture, as he would later emphasize) depending on environmental and other conditions. Only by understanding those roles and conditions could one see how the overall *system* worked, how its material culture might vary over time and space, and how change(s) in one variable—say, an increase in population density following the advent of fishing technology in the Great Lakes—caused change(s) in other variables, such as selective pressure fostering the symbolic communication of status via expensive, nonutilitarian copper artifacts (Binford, 1962). No one had looked at the Great Lakes Old Copper complex (ca. 5000-3500 years ago) in quite that way before.

Archaeological ignorance would no longer be sites unexcavated or time periods unknown. Archaeology should seek to *explain* (not simply describe) how and why changes occurred in past cultural systems; only then could it shoulder its "full share of responsibility within anthropology" (Binford, 1962, p. 224). To get there would require developing methods for making meaningful inferences about the past, though in Binford's view that was the only significant obstacle: "The

practical limitations on our knowledge of the past are not inherent in the nature of the archaeological record; the limitations lie in our methodological naiveté" (Binford, 1968a, p. 23). The New Archaeology was nothing if not optimistic.

To overcome that naiveté Binford advocated, among other things, a regional (rather than site-specific) approach in order to examine how people moved across a landscape and what they did in different places at different times, so as to "capture" the full cultural system. He stressed probability sampling to ensure representative and reliable data on a culture's internal structure, variability, and ecological setting. He called for more comprehensive data collection; no more focusing just on the style-laden artifacts that helped establish chronologies. And he urged a multivariate approach to artifact classification to discern relationships among classes of artifacts (e.g., Binford, 1964b, 1965).

Of course, discerning patterns in the archaeological record was one thing, ascribing meaning to those was quite another. Analogies from the ethnographic record had long been used to interpret archaeologically observed data, but Binford argued that imposing ethnographically known patterns on archaeological remains added nothing to our knowledge of the past. Worse, it denies "the possibility of dealing with forms of cultural adaptation outside the range of variation known ethnographically" (Binford, 1968a, p. 13). Thus, he advocated deducing hypotheses from the analogy to see how it fared when tested against independent data and, if the analogy failed, where that might lead (e.g., Binford, 1967, 1968b).

Above all, he insisted archaeology could be a science, one that sought to understand the archaeological record in terms of laws of human cultural behavior. Accordingly, archaeologists ought to define problems, be explicit in their assumptions and hypotheses, and rigorous in the reasoning

used in tackling them (Binford, 1968d). The idea that the accuracy of knowledge about the past could be *tested* was, in Binford's view, the most radical departure from traditional archaeology. The New Archaeology was not just new methods and theories, it was an entirely "new epistemological perspective" (Binford, 1968a, p. 17).

Heady stuff, and driven by Binford's energy, charisma, and zeal, and put into practice by him and a coterie of talented graduate students from Chicago and colleagues elsewhere, the New Archaeology took off. Binford's career also flourished, though his ascent was bumpier. Following his Chicago stint, he and his wife at that time, archaeologist Sally (Schanfield) Binford, made stops at the University of California at Santa Barbara and UCLA, departing each soon after arriving, sometimes leaving behind bruised egos and hard feelings. As Binford cheerfully admitted, he was fired from some of the best universities in the country.

In 1968 Binford was hired at the University of New Mexico, where he stayed for nearly a quarter century, rising to hold the Distinguished Leslie Spier Chair of Anthropology. It was not always a smooth ride there either: "*Sturm und drang* were often the order of the day," when his "self-assured, domineering brilliance" was roused (Straus et al., 2011, p. 328). That same year he coedited *New Perspectives in Archaeology* (Binford and Binford, 1968) a volume that showcased the New Archaeology and "set the agenda for a whole generation of research" in America and, increasingly, abroad (Shennan, 1989, p. 832).

Even as it appeared that Binford and his program were settling in, he himself was becoming disillusioned with the New Archaeology. He'd not only hit roadblocks in his own research, he'd become increasingly dismayed with the direction some self-styled New Archaeologists were headed, especially those who rigidly applied a hypothetico-deductive testing

approach, emphasizing formulaic philosophical prescriptions over archaeological content (Binford, 1983a, p. 107). And when *American Antiquity*, which had published "Archaeology as Anthropology" (along with several of his other early New Archaeology papers), accepted a response to his article on ethnographic analogy (Munson, 1969, replying to Binford, 1967), yet would not publish his reply, Binford dropped his subscription to the journal and with that his membership in the Society for American Archaeology (Binford, 1983b, p. 19). For a time he even stopped attending its annual meeting, in effect pulling away from the discipline.

Although chided for making the "strategic error" (O'Brien et al., 2005, p. 180) of giving back hard-won ground in the profession by making himself less visible, in fact, Binford was just reloading.

DEVELOPING MIDDLE RANGE THEORY

Binford spent much of 1968 investigating stone tools and faunal remains from the Mousterian site of Combe Grenal, France. The analysis was prompted by Francois Bordes's argument that the different stone tool assemblages occurring in alternating stratigraphic levels at Combe Grenal were the remains of different Neanderthal tribes, with the site recording "a perpetual movement of culturally distinct peoples, never reacting to or coping with their neighbors" (Binford and Binford, 1966, p. 240). Skeptical, Binford pored over the Combe Grenal stone and bone assemblages to see how they varied over time, and whether the variation might reflect different activities by the same historically related group (perhaps occupying the cave at different seasons) or their responses to changing environmental conditions over time.

The result was an "embarrassment of recognizable patterning, and more correlations between things than anyone

had ever imagined" (Binford, 1983b, p. 66). Yet, that wealth of data got Binford no closer to understanding Neanderthal behavior. He realized that in order to make statements about *dynamic* behavior(s) from a *static* archaeological record, he needed a "Rosetta stone" to translate archaeological patterns observed in the present into meaningful statements about past conditions (Binford, 1983b, p. 67). That Rosetta stone would come only with better insight into the adaptive strategies of living peoples, how different cultural systems are organized in different environmental settings, how material items—which ultimately become the archaeological record—are generated or used within cultural systems, and how these could vary over time, space, and conditions (Binford, 1983a, pp. 100-101). The effort to link present (archaeological) statics to past (behavioral) dynamics he called "middle range theory.[3]"

Ethnographers, of course, had long studied human groups but rarely concerned themselves with how material culture was organized or sites were structured, matters of obvious relevance to archaeologists (Binford, 1968b). By the 1960s a few archaeologists were conducting *ethnoarchaeological* research, primarily among hunter-gatherers in Africa and Australia. In 1969 Binford launched his own multiyear ethnoarchaeological study among the Alaskan Nunamiut, one of the few remaining peoples who, like the Neanderthals of Combe Grenal, lived in an Arctic setting hunting caribou. The Nunamiut were not Neanderthals, of course, nor were their adaptations the same as those used in Pleistocene Europe (Binford, 1991b). Rather, Binford went in order to learn how these modern foragers moved across the landscape, organized their technology and material culture, and targeted their prey in hopes of gaining insight into the archaeological record of mobile hunter-gatherers. He also went, he admitted, because "it could hardly fail to be a good educational experience" (Binford, 1983a, p. 101). It was.

Binford's Nunamiut study fueled a decades-long run of extraordinary productivity: scores of articles and several of his most influential books, among them *Nunamiut Ethnoarchaeology* (1978b) and *Bones: Ancient Men and Modern Myths* (1981b). One is struck in reading these just how deeply they influenced hunter-gatherer archaeology. It is not only that Binford's terms and concepts—such as forager and collector, residential and logistical mobility, embedded and direct procurement, curated versus expedient technologies, gearing up, site furniture, drop and toss zones—became a lingua franca among archaeologists worldwide. His Nunamiut work was also central to what is now a far more nuanced understanding of the organization of technology among mobile foragers: how and when, for example, material culture is added, carried, or discarded within an assemblage, a site, or across a settlement system, as well as how technology can vary (or not) by individual, activity, and the environmental conditions that structure behavior and adaptations (e.g., Binford, 1978a, 1979, 1980, 1981a, 1982a).

An important element driving the organization of Nunamiut technology, Binford saw, was how they procured, processed, and consumed their prey (caribou). Hunter-gatherers, he pointed out, do not make a kill and then promptly "begin eating an animal at the nose and proceed to the tail" (Binford, 1982c, p. 178). Instead, they segment the animal and differentially discard, transport, store, and consume carcass parts in different places at different rates. But how could that complex skein be unraveled archaeologically?

Binford realized that among the many factors underlying Nunamiut butchering and transport decisions, one of the most important was the relative nutritional value or economic utility of the different parts of a carcass—the meat and marrow available from shoulders and upper limbs, for example, as opposed to phalanges. He made the uniformitarian assump-

tion that such differences were more or less universal among terrestrial mammals and thus could be projected into the past (Binford, 1977a, pp. 8-9; 1981b, p. 28), presumably making for a more secure linkage between the archaeological record and the processes that created it than would otherwise be available from a simple analogy. He then developed a suite of measures of relative skeletal abundance and nutritional "utility curves," in order to "translate" frequencies of skeletal parts found in an archaeological site into evidence of past faunal exploitation patterns and behavior (Binford, 1978b, 1981b). These measures had a profound and lasting impact on zooarchaeological research, well beyond that conducted within the realm of hunter-gatherer archaeology.

In the 1960s Binford had criticized the use of simple conventions to interpret archaeological sites and assemblages (they differ or are the same because they are different or the same cultures). By the 1980s he had demonstrated why such conventions, whether based on stone or bone, were meaningless and, worse, misleading. Throughout he stressed that his Nunamiut work was not aimed at deriving empirical generalizations to be imposed on records from other times and places, but instead to use what he learned there and among other hunter-gatherers (most notably, a brief but productive stint working with James O'Connell among the Australian Alyawara [e.g., Binford and O'Connell, 1984]) to understand the causal processes and conditions that help explain archaeological patterns and variation (Binford, 1978a).

There was another significant yet unanticipated outcome of his Nunamiut study. Binford had asked one of his informants to explain patterns he was seeing in caribou bones found on the tundra. How, Binford was asked in return, did he know those caribou had been killed by people and not by wolves? He didn't, and that set him to studying bones at

wolf kills and dens to understand their predatory signature. That, in turn, sparked a broader investigation into how to distinguish human from nonhuman kills, or even from natural death assemblages, using, for example, attributes of bone breakage, butchering marks, and patterns of bone element survivorship and assemblage composition.

That study, which reached fruition in *Bones* (Binford, 1981b) led him into the deep human past, for he was convinced many skeletal assemblages attributed to early human hunters were instead the work of animal predators and scavengers. But what of assemblages that also contained stone tools? In *Bones* and subsequent publications (e.g., Binford, 1985b, 1988b) Binford took on the conventional wisdom that big-game hunting was a formative element of early human evolution. He argued that even at iconic sites such as Olduvai Gorge (Tanzania) and Olorgesailie (Kenya), which had helped shape that received wisdom, our Plio-Pleistocene ancestors were scavengers who came onto the remains of kills made by other predators and used rocks to break open the remaining bones for marrow (which, incidentally, provided an explanation for the origins of stone tool use). Likewise, he rejected the idea that Australopithecines were killer apes, dismissed assertions for hominin cannibalism at Zhoukoudian (China), and rejected claims of mammoth and cave bear hunting at Paleolithic sites in Africa and Europe (e.g., Binford, 1988c; Binford and Stone, 1986). Binford's arguments shook up paleoanthropology and were vigorously debated. Although his interpretations have not been universally accepted, they spurred considerable research into the processes—natural and cultural—that structured these ancient archaeological sites, and what they reveal of early human adaptations.

As for understanding Neanderthal assemblage variability, which had prompted the Nunamiut research, Binford found

himself no closer to a solution. "The simple answer is that I don't understand the Mousterian patterning" (Binford, 1982a, p. 27). Even so, he wasn't ceding ground to Bordes; instead, he believed there was something fundamentally different about Neanderthal behavior that could not be resolved by imposing projections from modern hunter-gatherers (Binford, 1982c).

DEBATING ARCHAEOLOGY

In the early 1970s Binford published an anthology of his foundational New Archaeology papers in *An Archaeological Perspective* (Binford, 1972). A decade later he collected many of the seminal articles from his Nunamiut research in *Working at Archaeology* (1983b). His third anthology appeared in the late 1980s, and its title is telling. *Debating Archaeology* (1989a) he called it, for when he turned to applying what he had learned of how to approach the archaeological record to the archaeology and prehistory of hunter-gatherers in work done by others, he got embroiled in disputes on multiple fronts.

This is not to suggest he was unwilling. Binford deliberately sought debate to "make others aware of their own uncritical acceptance of an unevaluated set of assumptive views about the world" (Binford, 1989a, p. 486). These began with his exchanges in the 1960s with K. C. Chang, Jeremy Sabloff and Gordon Willey, and others over the New Archaeology (e.g., Binford, 1967, 1968d), and in the 1970s with Bordes, Paul Mellars, and others over Mousterian variability (e.g., Binford, 1973). Yet, the 1980s and 1990s saw a sharp uptick in the intensity and frequency of his disputes, which now ranged far more widely over the archaeological landscape. Binford debated Glynn Isaac and his students (especially Henry Bunn) over the interpretation of early hominid behavior and adaptations (Binford, 1977b, 1985b); with Richard Gould and John Yellen over the utility of analogy and the goals

of ethnoarchaeology (Binford, 1978a, 1985a, 1991a); with Michael Schiffer over the character and formation processes of the archaeological record (Binford, 1981a); with James Sackett over the meaning and application of the concept of style in material culture (Binford, 1989b); and with Leslie Freeman over the interpretation of adaptations in Paleolithic Europe (Binford, 1982c)—among others.

It is noteworthy that Freeman, Gould, and Schiffer were all former students of Binford's. Indeed, Binford complained of a generation gap, which he surprisingly blamed in large part on "a failure of our educational institutions to promote *descent without modification*" (Binford, 1983b, p. 394, emphasis in the original). Griffin, his one-time adviser, would have appreciated the irony.

Former students or not, Binford's debating style was rough and tumble, and though he insisted it was never *ad hominem* (Binford, 1989a, p. 5), many on the receiving end thought otherwise. Certainly he could be ferocious—downright bullying, even. Still, to Binford's credit he routinely took on opponents who were quite capable of fighting back—and did (e.g., Bunn and Kroll, 1988; Freeman, 1983; Gould, 1985; Hodder, 1992; Isaac, 1984; Sackett, 1986; Schiffer, 1985), even to the point where debate seemingly evolved into feud, and journal editors felt compelled to call exchanges to a halt (as, for example, following Binford [1988a] and Bunn and Kroll [1988]).

In many ways, however, the debates of the 1980s and 1990s that seemed to elicit an especially visceral reaction from Binford were with advocates of postprocessualism. Postprocessualism was a loose confederation of approaches—neo-Marxist, hermeneutic, critical, and poststructuralist—initially united mostly by what they opposed: processual archaeology's vision of archaeology as an objective science in which hypotheses could be tested against data (Hodder, 1992). Various

strands of postprocessualism also rejected the idea of culture as an adaptive system (arguing instead it was meaningfully constituted and must be understood in its unique historical context), emphasized individual action, symbol, and meaning, and argued archaeology was neither neutral nor apolitical in the modern world (Shanks and Tilley, 1987).

All of which cut deeply into the epistemological core of Binford's career-long insistence that archaeology was and ought to be a science, revivified a concept of culture he had rejected decades earlier, and distracted archaeologists into thinking the "costless symbolic dreams" of the people in the past mattered—or could be ascertained using their "methodology of critical divination" (Binford, 1986, p. 468; 2001, p. 474). Having spent decades successfully challenging conventional wisdom in archaeology, Binford's archaeology had become the conventional wisdom—and the target for the next generation.

To be sure his ideas had evolved over those years as well. Early on he defined science using the language (and philosophical framework) of the positivists (Binford, 1968d), but later became less Hempel heavy-handed and more Popper nuanced: "Responsible learning is dependent upon the degree to which research is designed so as to expose ambiguity, inadequacy and inaccuracy in our ideas guiding both the production of data and our attempts to understand it" (Binford, 1987, p. 403). Or as he put it less formally, "You create knowledge by worrying ignorance to death" (personal communication, 1991). And he came to appreciate the subtle influences of one's conceptual paradigm (Binford and Sabloff, 1982), emphasizing the importance of having "some external frame of reference" against which we can appreciate and assess our own (Binford, 1989a, p. 486).

Still, Binford gave no quarter to postprocessualists (e.g., Binford, 1987, 1989c). Just because scientists are "culture-

bearing animals," he stressed, "does not mean that they are intellectually shackled by culture and doomed to the ignorance and subjectivity of their time" (Binford, 1986, p. 466). As for the idea that archaeology's goal should be to explain the uniqueness of cultural forms, or that artifacts are "immediately cultural, not social, and they can inform on society only through an adequate understanding of cultural context" (Hodder, 1982, p. 10), Binford dismissed such positions as scientifically irresponsible.

For even if the thoughts, beliefs, or opinions of the participants in ancient cultural systems could be determined, which he doubted (Binford, 1983b, p. 221), they could not "aid me in solving a problem that arises from a totally different perspective," one the participants neither experienced nor could have even been aware of (Binford, 1986, p. 469). Neanderthals and early modern humans in Europe had no idea they were living through the Middle to Upper Paleolithic transition. The archaeological record was not the ethnographic record where participants could be directly queried. Rather, it "presents us with information vastly different from that which was available to the participants within past systems," the trajectory of which was not determined by what its bearers thought about it (Binford, 1986, p. 473). For Binford, archaeology and *only* archaeology had the ability and opportunity to "understand humankind in a way that no participant, or no social scientist addressing the quick time events of direct social experience, could ever imagine." To fail to recognize this potential to probe cultural processes and change over the *longue durée* was "quite literally to 'abandon our birthright'" (Binford, 1986, p. 474).

A WORLD OF HUNTER-GATHERERS

In 1991 Binford retired from the University of New Mexico and accepted a faculty appointment at Southern

Methodist University in Dallas. There he could teach less and have more time to devote to a project he had started in the 1970s (previews of which appeared as Binford [1990, 1997]), which would become his last major book: *Constructing Frames of Reference: An Analytical Method for Archaeological Theory Building Using Ethnographic and Environmental Data Sets* (Binford, 2001).

Binford's Nunamiut research had involved the intensive study of a single North American Arctic group; *Constructing Frames of Reference* was in a real sense its mirror image, an extensive study of hunter-gatherers worldwide. The data for it were culled from centuries of ethnographic research among several hundred groups. These data were put in context (those frames of reference) by a compilation and analyses of climatic and environmental conditions—particularly habitat variables relevant to hunter-gatherers, such as terrestrial biomass—from nearly 1500 weather stations around the world. The result is a monumental, lifetime-culminating work that is encyclopedic in scope and effort, if not in sheer bulk (over 500 closely lined pages).

In it Binford sought to identify and understand global patterns and variation (present and past) in hunter-gatherer mobility, technological organization, site structure, demographics, and adaptation (among other features), and their underlying adaptive and ecological principles. It is, in effect, an effort to bring order to hunter-gatherer organization. At the same time it was intended as a methodological contribution, showing "*the development of a method for productively using ethnographic data in the service of archaeological goals,*" one that would allow firmer inferential leaps beyond the known ethnographic cases into the unknown and likely very different archaeological past (Binford, 2001, p. 2; emphasis in the original). It is also, admittedly, a daunting book, one far too substantial and complicated to have had the lightning-

strike impact of, say, *Bones.* More likely it will be mined for years to come for ideas and hypotheses to test. Time will tell of its ultimate influence on hunter-gatherer studies specifically (nowadays dominated by human behavioral ecology, an approach Binford early on rejected [1983b, pp. 219-220] and largely ignored since) and on archaeological methodology more generally.

Binford originally chose to study hunter-gatherers based on a remark by a fellow graduate student who suggested that if Binford was interested in the evolution of a form, say the rise of agricultural systems, he had to know something of what preceded it, namely, hunters and gatherers (Sabloff, 1998, p. 22). That passing comment became for Binford a career-spanning focus on hunter-gatherers past and present. Yet, with the exception of a few early and influential statements on agricultural origins (e.g., Binford, 1968c, 1983a) he paid little notice to the topic (or for that matter to subsequent significant cultural evolutionary processes, such as the rise of cultural complexity). With *Constructing Frames of Reference* he paid on that graduate school note. He brought together detailed evidence for the tactical role of mobility as a form of insurance for hunter-gatherers when local environmental conditions deteriorated. Then, in a chapter aptly titled "The Last Act Crowns the Play" he shows how as groups pack into a region and foraging ranges are reduced, the option of moving declines. This forces the use of increasingly smaller segments of the habitat, ultimately triggering strong selective pressure to intensify food production (Binford, 2001, p. 438).

And another bit of symmetry to the arc of a career: for all its complexity and depth one can still hear in *Constructing Frames of Reference* an unmistakable echo from 1962, when Binford insisted that archaeologists "should not seek explanations for observed differences and similarities in 'mate-

rial culture' within a single interpretive frame of reference" (Binford, 1962, p. 218). He didn't.

Binford retired for a second time in 2003, this time as a Southern Methodist University Distinguished Professor of Anthropology, and moved to Kirksville, Missouri, where his wife, archaeologist Amber Johnson, had accepted a faculty position at Truman State University. There he continued to write, occasionally teach and meet with students (Binford, 2008), and there he passed away from heart failure at age 79 on April 11, 2011, survived by his wife, Amber, and daughter, Martha (from his first marriage to Jean Mock[4]).

THE PERSON

In his prime Lewis Binford was, quite literally, a large and powerful presence. He acted and spoke with unmatched self-confidence, a high-spirited manner, expressive gestures, and a distinctive staccato laugh. Sharp and quick thinking, he was a formidable debater.

Though Binford could have little patience with peers, he relished his time with students, undergraduate and graduate alike. Self-described as "primarily a teacher" (e.g., Binford, 1989a, p. xiii), he was something of Pied Piper, and was only half kidding when he would announce, as he often did when headed to the classroom, "I'm off to misguide the youth!" At the various university stops he made in his career he advised or served on the graduate committees of some 80 Ph.D. students, many of whom went on to become major figures in archaeology.

A Binford talk bordered on performance art, and on occasion took on rock-star trappings. He would enter a packed conference hall trailing a cloud of students and admirers and mesmerize an audience. He could speak at length without notes and with an ease that masked his behind-the-scenes preparation, vigorously conjuring imagery and sounds (he

had a repertoire of animal calls—his specialties were wolves and hyenas), accented and delivered in a way that more than hinted of the southern Baptist preachers of his father's church. But there was no mistaking the fact that, like those preachers, he had a message. This was not mere archaeological entertainment. If the audience included fellow archaeologists, almost inevitably some were offended. *In Pursuit of the Past* (Binford, 1983a), a book transcribed from a series of Binford's lectures, neatly captures the color, content, and combative edge of his talks.

Even when he wasn't the scheduled event, Binford seemed to find his way to center stage. On a scientific exchange to the Soviet Union in the late 1980s to examine Upper Paleolithic sites, the American delegation was treated to a command performance by a local folk dancing troupe in the small town of Soroki. At the end of the performance a local Communist Party official took to the stage, praised the dancers (and the party), and then asked if the visiting Americans wished to say anything. Before any of us could react, Binford was striding to the stage. There, flanked by the dancers, who from their puzzled looks understood not a word of English, he briefly praised their performance, and then offered in exchange a song, a coal-miner's lament he'd learned from his father's Depression-era labor-organizing efforts (fitting for a worker's state). He sang *a cappella* and beautifully.

Yet, for someone who craved the spotlight and who the spotlight often sought, Binford was quite capable of muzzling his formidable ego if the occasion demanded. The self-effacing Binford emerged when he was conducting ethnoarchaeological fieldwork where *he* sought to learn: as he readily admitted, his teachers among the Nunamiut and the Alyawara were surprised to realize he did not know things "every child should know" (Binford, 1984, p. 173). One cannot be a successful anthropologist by informing

one's informants, and if Binford railed against his own elders and peers, he accorded great respect to those outside his culture, and because of that was highly effective in eliciting and absorbing their knowledge (O'Connell, 2011).

Quite aside from the learning opportunities his ethnoarchaeological experiences provided, they were also grist for Binford's storytelling mill, and he was a superb storyteller. His stories sometimes hewed close to the facts, and sometimes they were just awfully good stories. That desire to tell a good story could spill over into his substantive analytical work, as critics noted (e.g., Bunn and Kroll, 1988; Freeman, 1983; Grayson and Delpech, 1994; Klein, 1986; O'Connell, 2011; Zeleznik et al., 1988).

Although the author of more than a dozen books and over 150 articles, Binford the writer was not always an easy read. As he recalled when he circulated the first draft of *Archaeology as Anthropology* to his Chicago graduate students, it came back with the "inevitable suggestions of translating my writing into English" (Binford, 1972, p. 10). In later years he insisted his style was deliberate, or at least saw its virtues: "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" (Sabloff, 1998, p. 63). Under the circumstances one must admit some sympathy for Binford's critics whom he frequently accused of misrepresenting his work (Binford, 1989a). So far as Binford was concerned it merely proved his point about postprocessualists: "If they can't understand me, how can they pretend to understand other peoples in other times?" (personal communication, 1991).

Over his career Binford lectured throughout Europe, Asia, and the Americas, and was the recipient of multiple honors, honorary degrees, and even the naming of an asteroid: "(213629) Binford = 2002 QK67." Early on, most of these came from overseas universities and societies, not least the Huxley Medal from the Royal Anthropological Institute (1986) and election to the British Academy (1997). Election to the National Academy of Sciences did not come until 2001, not coincidentally after the last of the previous generation's titans had passed away, testimony to the operation of Planck's Principle. In 2008 the Society for American Archaeology bestowed on Binford—who had published in its journal *American Antiquity* the papers that shook the foundations of the discipline, yet who had once resigned from its membership—its Lifetime Achievement Award. A circle, opened in 1962, was closed.

Tributes delayed, perhaps, but not denied. It would be shallow indeed to claim Lewis Binford was a prophet without honor in his own country. He was arguably the most read, demonstrably the most cited, and by most any measure the most influential archaeologist of the 20th century, one who had a deep and profound impact on the practice and practitioners of American archaeology, and indeed much of the world of archaeology.

HONORARY DOCTORATES

1983 University of Southampton, Great Britain
1999 Pierre Mendez France University, Grenoble, France
2000 University of Leiden, Leiden, The Netherlands
2005 University of Verona, The Italian Republic

PROFESSIONAL RECORD

1960-1961   Instructor, Department of Anthropology, University of Michigan
1961-1965   Assistant Professor, Department of Anthropology, University of Chicago
1965-1966   Assistant Professor, Department of Anthropology, University of California, Santa Barbara
1966-1968   Associate Professor, Department of Anthropology, University of California, Los Angeles
1968-1991   Associate Professor (1968-1972), Professor (1972-1984), Distinguished Leslie Spier Professor (1984-1991), Department of Anthropology, University of New Mexico
1991-2003   Professor (1991-1999), Distinguished Professor (1999-2003), Department of Anthropology, Southern Methodist University

AWARDS AND HONORS

1956 Phi Beta Kappa, University of North Carolina
1956 Ford Foundation Fellow
1969 Sigma Xi, University of New Mexico
1984 Distinguished Leslie Spier Professor, University of New Mexico
1986 Huxley Memorial Medal, Royal Anthropological Institute of Great Britain and Ireland
1989 Eminent Scholar, New Mexico Commission on Higher Education
1989 First Annual Distinguished Teaching Award, National Association of Student Anthropologists
1990 Montelius Medal, Swedish Archaeological Society
1991 Centennial Medal, Portuguese Archaeological Association
1997 Elected to the British Academy (Corresponding member)
2001 Elected to the National Academy of Sciences

2002   Society for American Archaeology Book Award for
       *Constructing Frames of Reference: An Analytical Method for
       Archaeological Theory Building Using Ethnographic and
       Environmental Data Sets*
2004   The Academy of Medicine, Engineering and Science of
       Texas
2006   Fyssen Foundation Award Prix International
2007   La Sociedad Argentina de Anthropologia
2008   Society for American Archaeology Lifetime Achievement
       Award

MEMBERSHIPS

American Anthropological Association
Missouri Archaeological Society
Society for American Archaeology
South African Archaeological Society
The Prehistoric Society, Great Britain

NOTES

1.   OVER HIS CAREER BINFORD gave several extended interviews (Page, 1992; Renfrew, 1987; Sabloff, 1998; Thurman, 1998). These along with the interstitial autobiographical chapters in Binford (1972, 1983b, 1989a) provide information on his personal history and his views of archaeology: what it is, should be, how its goals are best accomplished, and how his own thinking changed over the years. His contributions are also discussed at length in virtually every history of recent archaeology or volume on archaeological theory (Wylie [2002, pp. 57-77] provides a particularly insightful and philosophically informed assessment of the conceptual core of Binford's New Archaeology). Given the breadth of Binford's contributions and the word limits on a National Academy of Sciences *Biographical Memoir*, my coverage has been necessarily selective. Indeed, saying something (but only *some* things) about someone who said just about everything is no easy task, and for that reason I am particularly grateful to Donald K. Grayson, Robert Kelly, Joyce Marcus, James O'Connell, Jeremy Sabloff, and Mark Stiger for their advice, thoughts, and suggestions on the draft of this memoir. Amber Johnson and Martha Binford provided Binford's up-to-date curriculum vitae along with other helpful information.

2.   There has been considerable confusion surrounding the year of Binford's birth, variously listed—even on different editions of his curriculum vitae—as falling between 1929 and 1931. The source of the confusion is unclear. The year 1931 is generally accepted.

3.   He distinguished middle range theory from general theory, which sought to explain the broad processes of evolution, such as the origins of culture, the shift to agricultural production, and the rise of complex societies (Binford, 1981b, p. 22).

4.   Binford had six wives: Jean Mock (the mother of his two children, Martha and Clinton [deceased]), Catherine (of whom little is known), Sally Schanfield, Mary Ann Wilson, Nancy Stone, and Amber Johnson. Schanfield, Stone, and Johnson were archaeologists. Binford's wives provided him a measure of unearned status among the polygynous Alyawara. As J. O'Connell recalled of one morning's conversation in 1974: "Lew asked what men his age and older did at this hour. The answer: they assembled for tea and talk at the senior men's camp, about 50 meters behind my tent. With that information, Lew went up by himself and returned about an hour later, happy with the interaction, and with lots of questions. Shortly thereafter, my mentor came up and took me aside. 'Good fella that one,' he said, referring to Binford. 'But does he really have four wives?' With that one bit of information, probably reported in response to a simple question about family, Lew had their attention. As Jacob said: 'Four wives! How can he manage it? Old Dick Mill (another senior man in camp) has only three and look at the trouble he has!' Obviously, the fact that Binford didn't have four wives simultaneously had somehow not come up, but no matter—in the senior men's eyes he was clearly a person to be reckoned with" (J. O'Connell, personal communication, 2011).

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