

Remembering Lew Binford

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Lewis Roberts Binford (Fig. 1), arguably the most influential archaeologist of the late 20th Century, died of heart failure at his home in northeast Missouri, 11 April 2011, six months short of his 80th birthday. He was a controversial figure throughout his professional career, partly because of his long-running intellectual challenge to disciplinary orthodoxy, but also because of his personal style. Physically imposing, he could bring tremendous energy to any exchange, especially if he had an audience of more than one. Many have compared his performances in lectures and seminars to those of the Southern Baptist preachers he'd encountered in his youth. "Riveting" and "inspiring" are among the words often used to describe these displays. His written work lacked that "tent revival" quality, but still packed enormous punch: more than any other single member of his generation, he fundamentally altered and improved the ways in which archaeologists practiced their trade. Not everyone would agree with that assessment, at least not entirely. More than a few saw him as an academic charlatan and self-indulgent bully who failed to respect the achievements of his elders and sought to advance his career by denigrating them. In my view, his enormous contribution to the field far outweighs these criticisms. This is some of what I know of his life and work.



Fig. 1: Lewis Binford in his office. Photo: Southern Methodist University Dallas.

Background

Binford was born in east Virginia, attended Virginia Polytechnic Institute, served a tour in the US Army, and completed an undergraduate degree in anthropology at the University of North Carolina. While at Chapel Hill, he took courses from Joffre Coe, a highly regarded figure in southeast US archaeology, who encouraged Binford to pursue graduate work at the University of Michigan. There he was heavily influenced by Leslie White – a prominent voice in mid-20th Century materialist anthropology, Albert Spaulding – a strong proponent of statistical rigor in archaeology, and James Bennett Griffin – director of the Michigan program and a leader in the American archaeological establishment.

Three years before completing his PhD, Binford joined the anthropology faculty at the University of Chicago, then one of the best in the world. He soon began to attract national and international notice, mainly through a series of journal articles of his own, but also via the work of the excellent corps of graduate students who were strongly influenced by him, notably Jim Brown, Kent Flannery, Les Freeman, Jim Hill, Bill Longacre, Stuart Struever, and Bob Whallon. All were at the forefront of what soon came to be known as the 'New Archaeology'. Denied tenure at Chicago, Binford subsequently held faculty positions at the University of California-Santa Barbara and UCLA before moving to the University of New Mexico (1969), where he remained until his first retirement in 1991. There he attracted another unusually good coterie of students, among them Steve Athens, Bob Hitchcock, Bob Kelly, Steve Kuhn, Mary Stiner, Larry Todd, and Robin Torrence. From UNM he moved to Southern Methodist University in Dallas, where he retired again in 2001.

He was married six times. Three of his wives – archaeologists Sally Binford (née Schanfield), Nancy Medaris Stone, and Amber Johnson – played significant roles in his academic work; a fourth – Albuquerque school administrator Mary Ann Wilson Howell – was a strong, positive influence in other ways. He had two children with his first wife, Jean Mock; his daughter Martha survives him; his son Clint was killed in an accident in 1974.

In his 40-odd-year career, Binford wrote, co-authored or edited 20 books and more than 150 articles, book chapters, and notes, many of them instant classics. Their quality earned him many awards and honors, among them the Distinguished Leslie Spier Chair of Anthropology at UNM, the Huxley Memorial Medal of the Royal Anthropological Institute (UK), the Montelius Medal of the Swedish Archaeological Society, the Fyssen Foundation Prize in Paris, the Centennial Medal of the Portuguese Archaeological Society, the Lifetime Achievement Award of the Society for American Archaeology, and election to US National Academy of Sciences.

Key elements of his professional life, as he saw it, are detailed in three volumes of collected works (Binford 1972, 1983, 1989). Stories in the first of these are particularly entertaining and enlightening. Other important sources include David Meltzer's (2011) Biographical Memoir on the US National Academy of Sciences website, Paula Sabloff's 1982 interview with Binford (not published until 1998: Sabloff 1998), and Jeremy Sabloff's postscript to the latter. A forthcoming issue of the *Journal of Anthropological Research* will include personal appreciations by others who knew him well.

Binford's research can be summarized under four headings: his initial assault on culture-historical archaeology, carried out mainly in the 1960s; his reporting of the results of ethnoarchaeological work among the Nunamiut of north Alaska; his efforts to apply the lessons learned in that setting to big problems in human evolutionary history; and his organization of the literature on historically known hunter-gatherers in the form of a comprehensive predictive framework.

The 1960s: New Archaeology

Mid-20th Century archaeology was dominated by the culture-historical approach to data collection and analysis. Broadly speaking, the exercise involved excavating sites that contained relatively rich, preferably well-stratified arrays of material remains, describing the style-laden elements of their contents in terms of formal artifact types, tracing the distribution of those types through time and space, identifying readily bounded, co-occurring sets of types as archaeological 'cultures', and accounting for changes in their composition and distribution by reference to past movements of people, ideas, or both. Classic North American examples include Kidder's (1924) synthesis of ancestral Pueblo archaeology, contributions to Griffin's (1952) edited volume on eastern North America, and Willey's (1966) overview of the continental sequence as a whole. The approach was unabashedly inductive, its results narrowly historical.

Binford argued that archaeology could do much better than this – in short, that the material record represented a far more complete body of information on past human behavior than the culture historians had imagined, that it could be tapped most effectively through systematic, comprehensive approaches to data collection and analysis, and that properly tackled it offered the best available basis for developing and testing explanations for long term changes in the human condition. He was not the first to make these claims (cf. Taylor 1948) but was far more effective at encouraging a realization of their potential than any of his predecessors. 'Archaeology as Anthropology' (Binford 1962) outlined the basic argument; 'A consideration of archaeological research design' (Binford 1964) offered a template for data collection on a regional scale. The importance of developing hypotheses about past behavior that might be tested archaeologically was emphasized, as was the need to gather datasets suitable for statistical analysis. Both represented sharp departures from previously standard approaches to the record (see Flannery [1976] for engaging characterizations of the contrast). Binford's students and others pursued these leads in a series of highly influential case studies. Longacre (1964), for example, used an ethnographically derived model of social status, kinship, post-marital residence, and craft production as a basis for assessing the distribution of artifact types and ceramic design elements across a prehistoric Southwest US residential site. Thomas (1973) used a similarly grounded model of seasonal transhumance to predict the distribution of subsistence-related tool types across major biotic communities in a central Great Basin valley. Both studies entailed quantitative archaeological tests of these predictions.

In collaboration with then-wife Sally, Binford himself offered one of the most challenging illustrations of the approach, based on an analysis of Mousterian lithic assemblages (Binford and Binford 1966). The prominent French prehistorian, François Bordes (1961), had identified a series of assemblage types widely encountered in the Dordogne and

elsewhere that he read as having been produced by Neanderthal 'tribes', each assemblage type indicative of a particular tribe. Bordes' analysis of assemblage composition was quite innovative, but the 'social' interpretation was standard culture history. The Binfords suggested a different analytic and interpretive gambit, based on the proposition that assemblage composition should vary with the range of activities conducted at various locations, regardless of the social identities of the groups that produced them. A novel, computer-aided statistical analysis of extant, relatively well-sampled collections yielded results broadly consistent with this expectation.

Binford and his followers' impact on the field was dramatic, partly because of its intellectual merit, but also because of its explicit attack on established scholarly authority. Many American archaeologists coming of age in the 1960s and 1970s found it attractive on both counts. Still, there were good reasons to be skeptical. The New Archaeologists' hypotheses about past human behavior were generally guided by reference to ethnography. There was no reliable basis for predictive argument about behavior that was not represented in that literature, at least not beyond common-sense propositions of the sort the Binfords had offered about Mousterian assemblage composition. Moreover, practitioners in the emerging field of ethnoarchaeology were finding that links between behavior and its archaeological consequences were far more complicated than the Binfordians had imagined. "Cautionary tales" about the difficulty of developing such predictions were becoming increasingly common in the literature. Finally, for all the talk about explaining variation in past human behavior, the New Archaeologists of the 1960s failed to deliver much in that domain that was truly compelling.

The Nunamiut experience

Binford himself recognized some of these problems. While his initial analysis of Mousterian assemblage composition had identified patterns plausibly interpreted in functional terms, his subsequent work on the larger, more comprehensively collected samples from the French site of Combe Grenal, including both lithics and faunal remains, yielded no similarly intelligible results, partly because of the complexity of the "behavior-archaeological signature" connection. Like other archaeologists working at the time, he looked for a situation that would allow him to observe behavior and its material consequences simultaneously in order to better understand the relationship between the two. With guidance from fellow New Mexico archaeologist Jack Campbell, he found it in the Nunamiut settlement at Anaktuvuk, in the north Alaskan Brooks Range. The community was already relatively well known on the basis of earlier work by Campbell, Helge Larson, and others. Its members still relied heavily on subsistence hunting, particularly of caribou, a pattern of behavior that promised insights especially applicable to the study of faunal assemblages from the European Middle and Upper Paleolithic. The fact that many Nunamiut had recently adopted snowmobiles, firearms and other elements of modern technology did not necessarily undercut its promise as a research venue. If Binford intended to develop an analytic framework relevant to the archaeological record as a whole, then any single case study, framed in general terms, could in principle be informative, regardless of any 'non-traditional' patterns of behavior it might display. As it happened, Binford and his students spent many months over a period of about four years conducting informant interviews, making direct behavioral observations, and carrying

out archaeological surveys and excavations. The results had important implications for many aspects of archaeological practice, including analyses of assemblage composition and spatial organization at both intra- and inter-site scales. The treatment of animal bone assemblages, detailed in Binford's (1978) benchmark monograph, *Nunamiut Ethnoarchaeology*, illustrates some of the most useful of these.

At the time Binford started fieldwork with the Nunamiut, archaeologists had barely begun paying serious attention to the animal bones encountered in prehistoric sites. Analyses were highly selective and purely descriptive, generally aimed at producing relatively low-level inferences about past human diet, local environments, and season(s) of site occupation. Little was made of observed variation in body part representation or condition, especially as they might relate to past human action. Relevant data were in fact rarely collected. Those readings that were offered were almost always poorly grounded in anything other than descriptions of the remains themselves (e.g. Dart 1957; Perkins and Daly 1969). Though efforts had been initiated to improve this situation (e.g. Brain 1967; Wheat 1967), there was still no general theoretical framework or body of broadly derived, well-analyzed ethnographic or experimental data on hand to guide interpretation.

Binford and his students observed Nunamiut caribou hunts under a variety of conditions, recorded carcass treatment from initial butchery through final bone disposal, described the effects of secondary consumers on discarded waste, and documented the archaeological consequences in sites and assemblages they had either seen created or whose recent history they knew from informant testimony (Fig. 2). Binford argued that all steps in the formation process up to the point of discard were guided by a thorough knowledge on the part of handlers of the economic utility of caribou carcass parts, the ways in which these utilities varied across individual animals by age, sex and season of the year, and the handlers' goals in carcass acquisition and processing. He further showed that after discard, body part preservation and resulting patterns in assemblage composition were largely if not entirely a function of part density and 'within-bone' nutrient content, a result confirmed by his parallel study of wolf-created bone assemblages. These findings implied that given a sufficiently detailed understanding of the anatomy of any prey species encountered archaeologically, an analyst could formulate testable hypotheses about the behavior of humans responsible for its presence. Binford was not the first to pursue this line of argument, but *Nunamiut Ethnoarchaeology* remains its most comprehensive statement.



Fig. 2: Lewis Binford butchering a sheep with archaeology students in Southampton, 1980. Photo: C. Gamble.

The body of work that emerged from this project is truly stunning: in addition to the *Nunamiut* monograph, it includes major elements of three other books and at least a score of articles and book chapters, most of them significant contributions to the literature. As Meltzer (2011) and others have observed, many of the terms and concepts that are now part of standard archaeological discourse – *forager* versus *collector* subsistence and settlement strategies, *expedient* versus *curated* technologies, *residential* versus *logistical* mobility, *embedded* versus *direct* resource procurement, *drop* and *toss* zones – were coined or developed in connection with this work.

Careful reading of *Nunamiut* also reveals an interesting paradox. From the beginning of his career, Binford stressed the importance of theory capable of facilitating the formulation of archaeologically testable hypotheses about past human behavior. As it happened, however, most of the propositions he and his students put forward in the first decade of so of the New Archaeology came directly from ethnographic accounts – they were almost entirely theory-free. *Nunamiut*, on the other hand, has at its heart a theoretical argument, namely that carcass processing and transport decisions are driven by the goal of insuring what Binford called “subsistence security.” At the time *Nunamiut* was written, a series of recently developed formal models, collectively known as optimal foraging theory, could have been used to test this proposition. Yet despite the fact that Binford was aware of these models, he rejected them, preferring instead to develop his own more complex, less readily understood formalisms when the basic ‘diet breadth’ model in particular would have served the same analytic goals far more elegantly and transparently (Metcalf and Jones 1988). More on this below.

Archaeological applications

Many began to pursue Binford's *Nunamiut*-based lead through experimental and ethnoarchaeological work in other settings; others intensified their own ongoing work along similar lines (Gifford 1981). Still others applied the emerging line of argument to the analysis of prehistoric remains (e.g. Speth 1983). In *Bones: Ancient Men and Modern Myths*, Binford (1981) offered a deliberately provocative application of his own in his treatment of faunal material reported from early Pleistocene sites at Olduvai, northern Tanzania. Remains from these and other localities of similar age in northern Kenya had begun to play an important role in arguments about the evolution of early humans. Building on earlier work by primatologist Sherwood Washburn, archaeologist Glynn Isaac (1978) held that these sites offered evidence of the pattern of central place foraging, monogamous pair bonding, and paternal provisioning thought to be typical of ethnographically known hunter-gatherers, but very different from the foraging, mating and provisioning patterns seen among our nearest living primate relatives, the chimpanzees. The combination of stone tools and the body part representation of large ungulates found at these sites was crucial to this argument in that it was seen to represent the acquisition by early humans of complete carcasses, probably by hunting, and the transport of selected, meat-rich parts, especially fully fleshed upper limbs, to residential base camps for consumption by the hunters' families. In short, Isaac was projecting a pattern of behavior commonly reported among modern human foragers back roughly two million years into the past, mainly on the basis of the large animal bones recovered from these African sites.

Countering this scenario, Binford argued that these early sites and their contents might better be interpreted in very different ways: either as secondary accumulations of animal bones and stone tools dropped separately in various locations along a drainage line and later washed together by fluvial action, or as remnants of a wide range of animal and human activities centered on perennial water points. In neither case, he suggested, need humans and large ungulates have had anything to do with one another. He further suggested that if humans had been involved at all with the animals, it was as secondary consumers feeding on the meager remains of kills made by large carnivores near water holes. Inferences about central place foraging, big game hunting, and paternal provisioning were in Binford's view entirely unwarranted by the stone tool/large animal bone association.

A long-running controversy ensued, one that in some ways continues to the present. Nevertheless, it now seems likely that Binford's alternative interpretations of these early assemblages were wrong on most points. Detailed analyses of assemblage content show that the bones and stone tools at most sites are in primary context – they were not brought together by fluvial action. Cut marks and impact scars on the bones themselves definitely reflect human processing for meat and marrow. Patterns in body part representation in the best preserved assemblages indicate that large carcasses were acquired by humans in complete or near complete condition, either by hunting on the part of humans themselves or by aggressive scavenging soon after the animals had been killed by non-human predators. The only issue still in play is the matter of post-acquisition transport. On that point Binford was probably right: most if not all large animal bones found at these sites were likely processed at or very near the points at which the animals of which they were once a part were killed. *The underlying point not to be missed:* all of these conclusions are based on precisely the kind of ethnographic, ethnoarchaeological, and experimental research that Binford pursued among the Nunamiut. Ironically, much of the best of it was undertaken by Isaac and his students, provoked to a significant degree (though certainly not entirely) by Binford's argument.

Binford also tried to apply the findings of Nunamiut work to other archaeological problems, mainly faunal assemblage formation processes and their human behavioral implications at several well-known Middle and early Upper Pleistocene sites, including Klasies River Mouth Cave, Grotte Vaufrey, and (again) Combe Grenal; but the results were less fruitful. As with his work on the Olduvai remains, Binford's main goal in each of these studies was to develop models of early human subsistence patterns different from those favored by most paleoanthropologists, but more faithful to the archaeological data as he saw them. In each of the Upper Pleistocene cases, he also pushed the argument further by using his faunal analyses as the basis for models of the settlement patterns and social organizations characteristic of the human groups that created these younger assemblages. The resulting formulations were (as always) novel and provocative. They were also widely discounted on at least two grounds. First, commentators with intimate knowledge of the collections in question challenged both the accuracy of Binford's basic descriptions and the validity of his analyses. The Grayson and Delpech (1994) critique of Binford's (1988) Vaufrey study is especially trenchant in this regard. Further, the behavioral models emerging from Binford's faunal analyses were immediately recognized as entirely intuitive and almost perversely idiosyncratic. They were completely uninformed by the increasingly sophisticated theoretical work then being done on

primate (including human hunter-gatherer) subsistence and social organization, and as a result came across as imaginative, but essentially unwarranted fantasies (e.g. Binford 1984, 255-256, 1992). Binford's focus on finding the answer he "knew" was right, coming to it analytically in his own way, and challenging professional consensus in the process led him to see patterns in the data that other investigators could not replicate and to draw conclusions about past human behavior that were unlikely to be valid.

Frames of Reference and hunter-gatherer archaeology

In 2001, the University of California Press released Binford's last major publication, the massive, ambitiously titled *Constructing Frames of Reference: An Analytical Method for Archaeological Theory Building Using Ethnographic and Environmental Data Sets* (Binford 2001). Its goal was consistent with the longest-running theme in Binford's work: using descriptive data on the subsistence and settlement patterns of historically known hunter-gatherers as a basis for predictions about the behavior of people practicing similar economies in the distant past. The specific approach pursued was foreshadowed in his widely cited 'Willow smoke and dogs' tails' paper (Binford 1980), in which he suggested the presence of a global pattern in hunter-gather economies – the *forager-collector* continuum – defined by the length of the growing season and the consequent degree of reliance on food storage. Pervasive implications for related aspects of behavior and their archaeological consequences, including site location, the organization of technology, intra-site spatial structure, and the details of faunal assemblage composition, were seen to follow.

In *Frames*, Binford offered a more comprehensive statement of that environment-behavior relationship through an analysis of the links between climate data from roughly 1400 globally-distributed weather stations and behavioral information on more than 300 ethnographically reported hunter-gatherer populations (see Bettinger 2001; Ames 2004; Shennan 2004 for reviews that go well beyond what can be covered here). In simplest terms, Binford used temperature data to estimate local annual above ground plant productivity. He used those productivity values to generate predictions about local human group sizes, the sizes of geographical areas exploited, and the degree of group reliance on terrestrial hunting, plant collecting, and aquatic resources. He then tested the predictions against his hunter-gatherer database. He used the results of those tests as a warrant for further discussion about anticipated patterns in regional population density, mobility, reliance on storage, residential group size, the extent of polygyny, and the scale of intra-group cooperation, again all relative to environmental parameters. Finally, he speculated about the implications of his findings for questions about economic intensification among terminal Pleistocene and early Holocene foragers, the origins of agriculture, and the development of social complexity.

At minimum, Binford merits great praise for the amount of data brought together in this work. It will be an important source of information on ethnographic hunter-gatherers for decades to come. Beyond that, its likely impact remains far less clear. The scope of the exercise undertaken is daunting enough for any reader, but appreciation of its worth is further inhibited by uncertainty about the validity of some data points, the difficulty of following the links between data and analyses, the complexity of those analyses, the reliance on intuitive rather than statistical assessment of results, and an

unfortunate number of computational and copyediting errors. Perhaps most frustrating is the problem highlighted in Stephen Shennan's (2004) review, namely Binford's continuing refusal to make use of well-developed theory pertinent to his project – in this case, the framework of behavioral ecology – in favor of a Rube Goldberg alternative. Laudable in many ways as this *opus magnum* is, it also represents an extraordinary missed opportunity on his part.

“Lew” stories

In addition to his strictly scientific contributions, Binford also generated a sizable body of (for lack of a better word) *lore*, familiar to many of us of a certain age – most of it unpublished, some of it unprintable, all of it highly entertaining in one way or another. Common themes include references to his overbearing behavior in a wide range of social settings, his transparently ‘tall tales’ about various aspects of his personal and professional life, and the degree to which he was willing to ‘bend’ if not entirely misrepresent relevant information in the course of an argument about science in order to make a point (that is, to win the argument). Less often repeated, but equally on the mark to me, are stories about his clear sense of question, his astonishing energy and imagination in pursuit of answers, his great strengths as a teacher, his often spell-binding performances at the podium, and his capacity for personal warmth and generosity.

Most of my “Lew” stories come from the time he spent with me in central Australia in June 1974. I'd been living off and on over nearly a year in an Alyawarra encampment of about one hundred souls, near the cattle station known as MacDonald Downs. Binford arrived on a two-week visit, organized by Peter Ucko, Principal of the Australian Institute of Aboriginal Studies, and Jack Golson, Head of the Department of Prehistory, Research School of Pacific Studies, Australian National University. He had a clear idea of what he wanted to see, most of it related to projects I was already pursuing. The schedule was ambitious: over those two weeks, we hunted kangaroo and recorded the subsequent butchery and intra-camp distribution of meat shares, visited several important sacred sites and discussed their respective roles in the ritual landscape, took part in a ‘drafting-paper-and-marking-pen’ seminar on traditional kinship and social organization, helped use fire to facilitate the manufacture of stone blades, and mapped the distribution of scores of household and related work areas on a recently abandoned, 25 hectare residential site. Our guides and companions were senior men born before this part of the ‘Red Centre’ was first occupied by Europeans, and who thus were familiar with all aspects of traditional technology, subsistence, settlement, and ritual life. Much of what Binford learned with and from them he later reported in several journal articles, book chapters, and in the various parts of the published version of his subsequent UK lecture tour (Binford 1983). It was quite a fortnight.

Contrary to what one might have expected from what critics often say about him, Binford proved to be a very good field man, not least as an ethnographer. He knew the relevant Australian literature well enough to ask good questions on a wide range of topics, and was prepared to engage productively when informants gave him an answer he did not expect. He also knew when to keep quiet and simply observe.

Did he ever 'bend' facts or observations, either at the time or later in print? Only once that I can recall. We arrived late from Alice Springs on the first night of his visit, met briefly with my friend and mentor JJ, then retired for the evening. The next morning, after coffee, Binford asked what men of his age did at that hour. My reply: "The dozen or so fellows over age forty in this camp usually congregate for a while in the senior men's household area. Visitors from far-away camps are sometimes present as well, sharing the news. JJ will be there by now; he'll introduce you." Binford left and returned about an hour later, pleased with the interaction, and with many questions. Shortly thereafter, JJ came up and took me aside. "Good fellow that one," he said, referring to Binford. "But does he really have four wives?" "Yes," I replied, "but not all at once." With that one bit of (mis)information, probably reported in response to a simple question about family, Lew had captured the attention of the group he most needed to impress. In the senior men's eyes, he was clearly a man to be reckoned with.

Coda

Binford (1972) reported that François Bordes recognized him, almost from the time they'd first met, as a fellow professional 'heavyweight', an assessment with which he (Binford) certainly agreed. His strong sense of himself put many people off. But it also emboldened him to ask big questions and pursue the answers aggressively. In most cases, his intellectual courage and enthusiasm served both him and his profession well: the breadth and overall quality of the Nunamiut work represents the best, but certainly not the only example of that service. The New Archaeology was vital for its time; in the long run *Frames* may also have an important impact. Sometimes that sense of self got the better of him, leading him to see what he wanted to see in the data and to shape their analysis in ways that a more dispassionate player would have avoided. Ironically, the importance of the questions he addressed and the level of attention he drew to himself and his work insured that shenanigans such as these were likely to be identified and challenged in print. A self-styled 'tall poppy' always invites the chop. Nevertheless, as my Australian colleague Peter White has long been fond of saying, "Binford is good to think." We archaeologists have benefited enormously from his intelligence, drive, and audacity.

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