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LEWIS BINFORD AND HIS MORAL MAJORITY

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ABSTRACT. *This essay looks at the late Lewis Binford's career from the standpoint of sociology of science. His thinking and manner reflect his socialization in Virginia Baptist subculture. As convinced of his authority on science as Jerry Falwell was of his authority on Biblical morality, Lewis Binford and his third wife Sally Rosen Binford excited a group of 1960s students to follow Lewis in an outmoded version of science (hypothetico-deductive) and in trusting statistics. The "frames of reference" he laboriously constructed are* naïve *on environmental interpretation and, because he expressed contempt for "political" aspects of archaeology, fail to take into account effects of colonialism. His work is often scientistic, in the "modern" mode that historian Dorothy Ross describes as characteristic of twentieth-century American social sciences.*

KEYWORDS. *Lewis Binford, biography, processual archaeology.*

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TÍTULO. *Lewis Binford y su mayoría moral.*

RESUMEN. *En este ensayo se analiza la etapa final de la carrera de Lewis Binford desde el punto de vista de la sociología de la ciencia. Su pensamiento refleja su socialización en el seno de la subcultura de la Virginia baptista. Tan convencido de su autoridad en la ciencia como Jerry Falwell lo fuera sobre la moralidad bíblica, Lewis Binford y su tercera esposa Sally Rosen Binford animaron a un grupo estudiantes de los sesenta a seguir a Lewis a través de una versión anticuada de la ciencia (hipotético-deductiva) y a confiar en la estadística. Los "marcos de referencia" que laboriosamente construyó son ingenuos en la interpretación del medio ambiente y, como expresó el desprecio por los aspectos "políticos" de la arqueología, no tienen en cuenta los efectos del colonialismo. Su trabajo es a menudo cientificista, en el sentido "moderno" que la historiadora Dorothy Ross describe como una característica de las ciencias sociales norteamericanas del siglo XX.*

PALABRAS CLAVE. *Lewis Binford, biografía, arqueología procesal.*

The only way to understand Lewis Binford and his impact on American archaeology is to approach from the standpoint of sociology of science. As a close contemporary (three years younger), I watched from the sidelines as he drew disciples into a cohesive little army, assaulted our elders, and claimed the mantle of genius theoretician. From the sidelines, I saw that this emperor was as naked as they come, and puny. Like many emperors, he owed his throne to the gifted, determined woman at his side—Sally Rosen Binford. Like many emperors, he was blinded by the glitter of gold from his crown, abusing his partner until she took the dog and drove away. Lewis, like Henry Tudor, went on to a total of six wives and a reign over a kingdom built on confiscated centers of learning and labor. His vassals evangelized the new religion he proclaimed, the Only True Science. When he turned forty, he wrote his autobiography (Binford 1972). Sally had left him a couple years before.

Lewis Binford was born in Norfolk, Virginia, in 1931. His parents, he said, on his father's side were "hills-south, hard-working, coal-mining" (although his father was an electrician and then managed the H.J. Heinz warehouse in Norfolk), and on his mother's, "in the nostalgic world of the antebellum south" (Binford 1972: 340). For college, Binford chose Virginia Polytech in Blacksburg, the heart of Southern Baptist fundamentalist evangelicalism; Jerry Falwell lived in nearby Lynchburg where he was building up his Thomas Road Baptist Church, and not long after Binford graduated, VA Polytech hired Henry Morris to chair its civil engineering department—Morris who in 1961 co-authored *The Genesis Flood* purporting to use strict science to prove Noah's flood. A *Pacific Stars and Stripes* interview with Corporal Binford, stationed on Okinawa,[1] states: "Binford theorizes that the world

[1] Binford claimed he was appointed interpreter for Japanese when he was drafted and sent to the Pacific Theater. He states he learned Japanese in military language school; it must have been a short course, given his other assignments during his two-year stint (Sabloff

flood, mentioned in religion and verified by geologists, was responsible for the mass migration to the Ryukyus and for the high location of the [pithouse] holes" (*Pacific Stars and Stripes* 10(74): 8).

After completing his army draft stint, Binford enrolled at the University of North Carolina to study anthropology and archaeology, and went on to complete graduate work in archaeology at the University of Michigan, 1964. I, too, received the Ph.D. in 1964, from Harvard. My first professional presentation, a paper organizing ceramics from the Northwestern Plains into wares and types (Kehoe 1959), was given at a Central States Anthropological Society annual meeting in Madison, Wisconsin, in 1959 as I recall. A handsome, tall, broad-shouldered, fair young man was another presenter in the session; I would remember Lewis Binford only for standing out against the boring old guys in the session. It likely was his first professional presentation, too.[2]

Potsherds dominated American archaeology then, and James B. Griffiin dominated archaeology east of the Rockies by his incomparable familiarity with sherds. His sherd collections at Ann Arbor were the type specimens, and his identifications, made with lightning speed and usually no explanatory comment, were unassailable. Lewis Binford could not challenge Jimmy. Lewis Binford turned to lithics. Lithics were called "projectile points," never mind that nearly every one excavated came from domestic contexts, plus were not sufficiently symmetrical to allow a projectile to fly straight. Being a housewife, I could see that practically all these points are kitchen knife blades, they are the size of my indispensable little kitchen knife and like it, have one side of the tip thinned and sharp, the opposing side lightly ground so one can put one's finger on it to press in cutting. Guys didn't know kitchen knives.

Lewis Binford saw lithics as hunters' tools, taking him into hunter research. At the time, this was called hunter-gatherer studies. Gatherers being women, their dull business had nothing to engage a big guy like Lew. Lewis Binford changed the term to "foragers," evoking images of Thirty Years War cavalry swooping through the countryside, helmets gleaming, raping women and bayonetting babies and grabbing all the goodies. At least that is the image historians come up with. Agricultural scientists know that foragers are herbivores that graze forage (*Google* the Department of Agriculture's Forage Unit) (Kehoe 1993). With his introduced terminology, Lew could evangelize a new field and do something Griffin didn't, use Michigan professor Albert Spaulding's faith in statistics to "discover" patterns in ancient behavior that no one had seen by merely eyeballing. Entering measurement and location data into statistical formulae, Binford claimed to revolutionize Paleolithic archaeology by identifying lithic variations as functional rather than stylistic (i.e., culturally distinctive). He challenged the doyen of Paleolithic archaeology, François Bordes, and the much lesser light at Harvard, Hallam Movius, on their interpretations of their Dordogne excavation projects. His *entrée* to the Dordogne was Sally, who had spent the summer of 1960 on the Harvard project at Abri Pataud. The Bordes, François and his equally distinguished archaeologist wife Denise de Sonneville-Bordes, had befriended Sally (S. Binford 2005). Her excavations at a Mousterian cave in Israel provided the data she and Lewis used for their statistical approach to analysis.

AGONISTIC ARCHAEOLOGIST

According to his own picture of himself, Lewis Binford considered human culture to be our extrasomatic means of adaptation for survival, carried out through symboling (as in language) (Renfrew 1987: 692). He was parroting Leslie White, the anthropologist at Michigan who inspired the generation who came out of World War II desperate, like Henry Adams after the Civil War, to find an exonerating explanation for the devastation they had witnessed (Adams 1918: 224-226; Peace 2004 on White). Like Adams nearly a century earlier, they eagerly accepted Spencerian evolution, passionately defended by White, evolution as a Vital Force inexorably pushing mankind into Progress, let the chips fall as they may. White's version extolled harnessing energy as the mechanism of Progress, from which Americans in the 1950s could infer that dropping nuclear bombs on hundreds of thousands of civilians proved the United States to be the

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1998: 67-69). His disciple Robert Kelly recounted "About 1984, when I was living in New York, Peggy Nelson invited Lew up to the State University of New York (Buffalo) for a talk. She suggested I come up too, just to visit, and so I did. One night she, Lew, Ben Nelson and I were at dinner at a Japanese restaurant. When the check came, there was the usual scramble and Lew won, apparently by saying something in Japanese to the waitress. I had heard that Lew spoke at least some Japanese (that he had learned in the 1950s while stationed in Japan), but I wondered how well he actually spoke it. So, while the others were putting on their shoes I sought out the waitress and asked her what my friend had said. 'Oh, I have no idea' she said in heavily accented English, 'I'm Korean.' I still don't know how well Lew spoke Japanese" (Kelly 2011b).

[2] I e-mailed Binford, through his final wife Amber Johnson who was handling his mail after they moved to Kirksville, inquiring whether that was in fact his first professional presentation. Central States was trying to compile a list of the famous anthropologists who had first presented in its meetings. Dr. Johnson replied that she had asked her husband, he said he recalled being in a Central States meeting in a session with me, but not whether it was his first presentation. Parenthetically, young women giving archaeology papers were unusual enough then that I can believe he did notice me.

pinnacle of Progress. In spite of armed forces experience, the students who made White's simplistic cultural evolutionism their anthropological framework did not, or would not, perceive he was purveying Socialist Labor dogma (personal communication, Robert Carneiro, September 2001).

However he gave lip service to White (Binford 1972: 6-8), Binford's work does not exhibit much debt there. He accepted the more basic Enlightenment schema of stages of unilinear cultural evolution, restricting his work to the hunter-gatherer "stage". What he did take from White was labeling his work "science" (e.g., White 1959: 49; Binford 1972: 111) and lambasting his predecessors and their students (Peace 2004: 148-153 on White; Sabloff 1998:40 for Binford). Memories of Binford posted after his death frequently mention his house-building skill learned from his first father-in-law, how "he loved to pick up a hammer" (Richard "Dickie" Taylor, posted on ArchaeoAnth 5/10/11). Hammering was his mode of argumentation, too: "He was never retiring when he wanted to argue his point of view. He had a commanding presence and he would plant his feet, move forward as he made his points, and never, ever retreat" (Ezra Zubrow, posted on ArchaeoAnth 4/25/11). Disdain for those he perceived as competitors is replete in his books, for example of his predecessors (Binford 1978: 238-242), and of European archaeologists in general and most particularly Ian Hodder and his 1980 Cambridge students (Binford 1983: 14-18).

Interviewed in Dallas in 1997 by former Chicago student Melburn Thurman, Binford stated concisely, "I want to know how things were constrained by structure and pushed by dynamics, repetitively over time" (Thurman 1998: 40). Tom Riley, in a review of Paula Sabloff's book of interviews, mentions seeing a student paper Binford wrote in 1958 "where he outlined as an engineer [or ecologist] might, how culture was an integrated system, and that culture change was systemic" (Riley 1998: 23). Systems theory was cutting-edge in the 1950s (Wiener 1950). Twenty years later, ensconced in Albuquerque with eager graduate students, Binford articulated the foundation of his work. Seeking domains in which "uniformitarian assumptions" could be supported, he singled out:

1. Ecology, specifically living organisms of species available to humans in the past. Constraints on their availability or use, and the dynamics of their desirability for food and other necessities, can be studied in the present and projected reliably into the past.

2. Anatomy of animals, which is even more constant than their habitat preferences. Bones are often part of the archaeological record. Field and farm butchering of animals whose bones are found archaeologically[3] can be observed today.

3. Space use, usually outdoors as in hunters' camps. In his 1983 book he uses a photo taken by Susan Kent[4] of a Navajo woman cooking outdoors (1983: 150) and a "c. 1920" photo of a Blackfoot woman near a hearth outside a tipi ("house" in the caption) (1983: 176). These support the uniformitarian assumption that non-modern people who live in small shelters in undeveloped landscapes probalistically cooked outdoors.

These three domains of research fit the "middle-range theory" he advocated as neither trivial nor inordinately ambitious (Binford 1977: 8-9). Compare Lewis Binford's weeks of summer hunting trips with contemporary Inuit, with Franz Boas' entire year living with nineteenth-century Baffin Land Inuit. One of Boas' hunting trips with Inuit hosts trapped them in a hastily-made iglu, waiting out a blizzard, hoping it would abate before they starved. What impressed Boas during his year with Inuit? Not the formidable constraints of their environment, nor the pushing dynamic of a will to survive, but their songs, poetry, humor, and arts. Binford saw the archaeologist's task to figure out "What are the conditions in the past that brought into being what you see today?" and "to justify your inferences" (quoted in Sabloff 1998: 41). The archaeological record was his universe of inquiry. Spaulding had taught that significance is revealed when statistical manipulations show patterns. Most of what impressed Boas as the essentially human aspects of Inuit life were, to Spaulding and Binford, epiphenomena. Binford's archaeology was highly reductionist in scope while touted as "a vast body of behaviorally controlled material" (his 1978 Nunamiut book) (Binford 1981: 195).

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[3] Binford recalled "I arrived in France [in 1968] with a copy of the then very new study by [T.] Kehoe (1967). I hoped that I could use faunal variability to inform me about the causes of lithic assemblage variability" (i.e., functions as cause of variation) (Binford 1981: 195). I did the laboratory identification of the bones from the Boarding School Drive, using Ted White's forms that Tom had learned working on a River Basin site with White (Kehoe & Kehoe 1960). The site is on the Blackfeet Reservation, the crew was mostly Blackfoot, and we brought elders to the excavations to discuss how the occupation strata compared to what their grandparents had told them about nineteenth-century bison pounds. In 1969, Tom and I volunteered at Jean Combier's Solutré excavation to compare the reindeer and horse strata there with our experience excavating several major bison pound sites, and during the 1980s Tom visited principal Paleolithic painted caves, recognizing schematic drawings of drive lanes and pounds and paintings of herds driven toward them (T. Kehoe 1989).

[4] Lewis Binford used Susan Kent's dissertation work on Navajo ethnoarchaeology and she dedicated her 1990 edited volume on "domestic architecture" to him "whose friendship transcends theoretical differences," but he seldom cites her important series of field studies and theoretical discussions (see Ashmore, Dobres, Nelson & Rosen 2006 for Kent's work, tragically cut short by her death at age 50; Binford was invited but did not contribute to this *festschrift* in her memory).

At the core of Binfordian archaeology is his uniformitarian assumption that climate can be deduced from immense amounts of data statistically analyzed. Amber Johnson Binford explains:

> "We [she as research assistant, and Lewis] went through lots during those years completing the program—entering thousands of weather station records (for a while, I could convert from degrees/minutes to decimal degrees in my head), measuring the area of vegetation types from maps *by hand*, starting over on the linear regression equations for all projected variables after they announced the floating point error in the original Pentium chip.
>
> Once we had the program working through the environmental frames of reference, Lew started the pattern recognition work that fueled *Constructing* [*Frames of Reference*, 2001]. He would come to the lab nearly every day with his canvas bag full of figures. He would spread them out one-by-one on the big table in the lab and say "Look at that!" We would work together to decide which of the HG variables we would try to include in the projections—then I would get to work on the linear regression equations that project the hunter-gatherer frames of reference.
>
> My thesis was the first archaeological research to take advantage of the calculated frames of reference" (Johnson, posted on ArchaeoAnth 6/2/11).

When I read this, I wondered why this young woman spent so much time on basic research that sounds like that performed for decades in Reid Bryson's lab at University of Wisconsin, Madison. Bryson worked closely with archaeologist David Baerreis in the 1960s and remained actively collegial with archaeologists until his death in 2008. Charles Reher's paper in Binford's 1977 edited volume cites and uses several Bryson publications that force him to conclude that bison populations fluctuated and their relation to human societies in Wyoming is not straightforward in the archaeological record (Reher 1977: 36). I asked William Gartner, an archaeologist and geographer who had studied with Bryson, whether Binford had called upon Bryson's expertise. Gartner generously replied with a profoundly insightful note:

> "Bryson's approach to reconstructing past climate change uses forcing factor inputs and correlation & regression. One takes the inputs of modern forcing factors (earth-sun geometry, volcanic eruptions, etc.) and correlates them with modern weather observations at a locale. One then regresses past forcing factor inputs to construct past climate at that locale. There is no room for system complexity and feedback in this approach (e.g., El Niño and La Niña are irrelevant). I'll let you be the judge if there are any similarities in the structure of uniformitarianism and analogical arguments underlying the 'explanations' of Bryson and Binford.

Bryson, in my opinion, never received credit for his role in bringing about a 'scientific archaeology'. Bryson's multi-disciplinary Mill Creek project was in the grant application stages when Binford wrote 'Archaeology as Anthropology'. It was one of the few examples of a priori, as opposed to post priori and other ad hoc approaches, to [explicit] hypothesis testing in Midwestern archaeology. (Another notable example is Fowler's original Mound 72 excavations which tested Fowler's prediction of the location of post pit 1). That said, the stratigraphy of the Mill Creek sites excavated by Bryson *et al.* was greatly simplified and interpreted incorrectly in their report—as suggested by Karl Butzer (who fought with Bryson often) and demonstrated by myself in my dissertation. Rather than climate change over a several century time period, Bryson *et al.* demonstrated local scale human impacts to the environment over a single generation. There are no Pompeii's in Midwestern archaeology. There are no simple nature-society linkages when the environment changes. Environmental change is undeniably important in human affairs. But, so too are social choices, a point well made by Jared Diamond (of all people!) in *Collapse*.

Although I greatly appreciate Binford's Herculean efforts at synthesizing tomes of Hunter Gatherer information (the bibliography is very useful), his book is difficult to read for the same reasons that it is difficult to read most approaches to climate-driven culture patterns and culture change in archaeology. Weather and Climate (and ethnography for that matter) do not conform to the kind of typological thinking that is inherent to archaeology. The frequency, direction, and magnitude of climate change is typically time transgressive and, moreover, vary over small distances. I often use the example of the 1993 floods in the Midwest, which were also a time of record drought in the Southeast. Type in the term 'drought' in *Google News* right now and you'll read about multiple droughts occurring right now somewhere in the U.S. Only, you wouldn't know about it from all of the flooding stories on the National News, would you?

Another example comes to mind. Multiple paleoenvironmental proxies show that the mid-Holocene dry period is time transgressive in the Upper Midwest, on the order of millennia in some cases, and was also quite variable in magnitude. Yet we still read about terms such as the altithermal and, if you are in 'the know', the hypsithermal in archaeology. These terms are meaningless—they imply that this time-transgressive interim of environmental change during the mid-Holocene was the same everywhere. Environmental and culture change happen

> continuously. It is always possible to correlate them. Yet, archaeologists rarely explore the myriad nature-society linkages that accompany such correlations. Binford never did. If you look at his bibliography, you will see that he cites very few works by climatologists or earth scientists. His citations largely consist of works that suggest time and space patterns that suit his needs (E-mail message to Kehoe, William Gustav Gartner, 6/3/11).[5]

Perhaps the most remarkable aspect of Lewis Binford's processual archaeology (he deplored calling it "the New Archaeology" [Binford 1983: 15]) is its thoroughly "modern" American character, described by Dorothy Ross in her 1991 study of American social sciences. By the 1920s, "under the banner of positivist science, [h]istory was no longer the solution [to understanding society], it was the problem. Only a hard, technological science seemed capable of controlling so... slow-moving and retrograde a public consciousness as existed in America" (Ross 1991: 388). "The emphasis on fluid process in their work [1920s American social scientists] constituted perhaps its chief novelty... Process placed them at the intersection of history and nature, seeking to capture both the concrete particularities of experience and universal natural forms... A great deal of the creative richness of their work, as well as the contradictions they never resolved, grew out of these divergent impulses locked together in the metaphor of process" (Ross 1991: 387). It's uncanny how well this historian who likely never read anything by or about Lewis Binford describes his science. She titles her final chapter "Scientism", "with science now defined by its method, scientism demanded that the requirements of natural scientific method dominate the practice of social science" (Ross 1991: 390).

Binford's genius was to intuit what people wanted to buy: in the heyday of Eisenhower's military-industrial complex, production systems schematized as closed-loop adaptations of populations to given environments. NSF was the principal source of funding for archaeological projects in the 1960s, an outgrowth of mid-century patronage for social-science efforts to control societies (Ross 1991: 400-401). Ralph Linton, certainly inclined to be a humanist, wrote in 1945, "The aim of this science [anthropology] is the same as that of all sciences. It seeks to ascertain the processes and continuities involved... with a view to the prediction of events and ultimately their control" (Linton 1945: 17). Lewis Binford expressed this conservative, one could even say fascist, goal as archaeologists' aspiration. Fittingly, he advocated philosopher of science Carl Hempel's already outmoded hypothetico-deductive method, apparently oblivious to its tautology of stating a hypothesis, deducing what data could validate it, then looking for those data. *Where does the hypothesis come from?* From what one already is familiar with. Truly an ivory-tower science, unlikely to bring in questions arising from experiencing other societies' realities,[6] or even to notice variables not amenable to Indo-European morphemes and syntax.

BINFORD AS A SOUTHERN BAPTIST PREACHER

The appeal of Binford and his New Archaeology is, to me, best understood by seeing him within the Virginia Baptist society he grew up in. "Lew would often slip into a southern Baptist preacher mode and talk... and talk", remarked his disciple Kelly (Kelly 2011: 928). Longtime colleague Charles McNutt said that "I learned that Lew was a compulsive story teller. By 'compulsive' I mean that Lewis would begin to recount some situation, then warm to it, and finally elaborate it to a climax that could usually be refuted quite easily. And Lewis was completely aware of this—but he frequently ploughed ahead" (McNutt 2011).[7] This is exactly the technique that linguist Susan Harding identifies in the Baptist preachers Binford heard as a child and college student. She "listens to the cadence and phrasing of [the preacher's] words, to the esthetic shape of his story and the multidimensional... universe it presupposes, and hears nothing but the truth, that is, the world evoked, the world constituted, by the story" (Harding 2000: 54). Jerry Falwell's public discourse, she reports, was "a system of narrative gaps. The storied gaps... captured attention, induced interpretive action, and wove semiotic webs between a preacher and his people" (Harding 2000: 98). These Baptist preachers look listeners directly in the eye, they speak with passion, they talk on and on, to weave those semiotic webs.

[5] For examples of Gartner's work, see his dissertation (Gartner 2003) and his rich blend of scientific ecological analyses, archaeology, ethnohistory, and First Nations traditions in Gartner 1997.

[6] My Blackfoot colleague Darrell Robes Kipp said in August 2010, at the Blackfoot History Symposium in Browning MT, that he no longer uses the word "culture," what he as a Pikuni experiences and knows is a reality different from that he experienced and learned during his graduate work at Harvard University.

[7] Sally Binford said the same as McNutt: "One of Lew's fatal flaws is that he's a pathological liar—and most of the time he didn't know he was doing it. He is truly incapable of distinguishing what he wants to believe from what is real. He had a distressing tendency to 'improve' data. He would generate a large number of original and intriguing ideas—90% of which bore little or no relationship to reality, but the 10% that were valid were great. I would attempt to steer him away from his more imaginative notions and help him in finding data to support the sounder ones, then help him write them up in comprehensible English" (S. Binford 2005).

They brim with self-conviction. Yes, Binford thought himself an atheist, but his faith in scientism is not dissimilar to the faith of Scientific Creationists (Kehoe 2007 on these).

As I expounded in my 1998 book, Lewis Binford's version of science is the nineteenth-century science that expected to find immutable laws in nature, because God created a lawful universe. Clerk Maxwell, Joule, the Thomson brothers (William became Lord Kelvin) and their Scottish circle of physicists and engineers worried about entropy, the dissipation of energy: does it prove the Calvinist doctrine of our fallen world, or is energy conserved within the universe so that Progress is possible? (Smith 1998). Seemingly purely scientific questions may reflect profound philosophical issues. Binford's disciples were a Moral Majority convinced their leader spoke the one and only truth. His own unshakeable belief infused them with confidence and a sense of power, the way Jerry Falwell's self-belief inspired his followers with confidence the Holy Spirit moved them. Robert Chapman said in *Antiquity*'s page of eulogies, "Enthusiasm, optimism and challenge were as important as theory" (Chapman 2011).

Some of us cannot agree. Lewis Binford convinced most of a generation that primary research is to be pursued to validate propositions, that simply adding to the store of knowledge is feckless. He talked and talked about philosophy of science although he admitted to Colin Renfrew that he hadn't read much of it before he went to Chicago, 1961, and his publications indicate little serious reading in the field subsequently (Renfrew 1987: 686). Contrast Guy Gibbon, who spent a sabbatical at the London School of Economics to study with leading philosophers of science there [Gibbon 1989], or Jane Holden Kelley, who co-authored *Archaeology and the Methodology of Science* with a degreed philosopher of science [Kelley & Hanen 1988]. Binford relied on Carl Hempel, already rejected by historians and the great paleontologist George Gaylord Simpson when Binford took him up, and on Wesley and Merrilee Salmon's expositions on formal logic in science (Salmon 1982). He seemed unfamiliar with Peirce's stimulating discussion of induction, deduction, and abduction, the logic of dealing with surprising facts, or Kuhn's development of that to highlight anomalies as the crux of scientific breakthroughs. Constraints limiting dynamic pushes make a very narrow research domain.

Particularly disturbing is Binford's tendency to assert a finding that his own documentation fails to support—presumably arising from that enthusiasm for a story that overrides veracity. Binford considered his 2001 tome, *Constructing Frames of Reference: An Analytical Method for Archaeological Theory Building Using Ethnographic and Environmental Data Sets*, his magnum opus. Michael Shott published a detailed review of the book in *Antiquity*, 2002. Shott took the trouble to carefully examine the plethora of tables and statistics, revealing gross errors and lack of congruence with text. He was forced to conclude, "throughout, analysis rests on subjective interpretation of evidence. CFR suggests much, and is worth reading for this reason, but does not persuade of its chief theses" (Shott 2002: 268). Ernst Mayr called laying-on of statistics "window-dressing" (Mayr 1982: 850; parenthetically, Mayr's masterpiece is thoroughly pertinent to archaeology, the one book I would advise for every archaeologist who aspires to work intelligently).

Constructing a frame of reference is a necessary step in scientific method. Premising that statistics will be key to interpreting the human past is not only not necessary, it can be a crucial error. Singular occurrences are statistically insignificant. The single Pachuca obsidian flake in Craig Mound at Spiro is only a far outlier in any statistical rendering of obsidian sources in the Spiro collections. Looked at in a frame of reference constructed on accepted Mississippian sourced trade contacts, it is an anomaly. Peirce's science can accommodate that, requiring scientists to accept "surprising facts" (the Pachuca source of the blade in a mound on the middle Arkansas River) by widening the frame of reference, in this case to Mississippian-Mesoamerican contacts (Barker *et al.* 2002).[8] Similarly, Cahokia's unique, for America north of Mexico, grid of plazas surrounded by large mounds, and the number of filed teeth found in Cahokia and environs, unique north of Mexico except for some in contemporary Chaco, can be accommodated in a similar frame of reference that includes the Mesoamerican Early Postclassic. Cahokia's engineered site plan and the modified teeth are as much facts as any sherd or lithic artifact. Science dealing with humans needs to stretch frames of reference, as Boas learned in Baffin Land.

CONCLUSION

Historical particularism needs scientific methods to identify myriad elements of the environment and human biology, and how they change. Binford despised British archaeology's practice of allying with other sciences, "little technical subfields treating archaeological remains in their own frameworks" (Binford 1983: 16). Such collaboration has become common in the United States, too,

[8] It is pertinent that Alex Barker was my student, learning my holistic empirical approach to archaeology. Binford's Chicago student James A. Brown, considered the expert on Spiro Craig Mound, had not recognized the significance of the green obsidian scraper.

primarily because consulting archaeological businesses have been adding "little technical subfields" to their staffs. In this respect and because consulting archaeologists work on closely specified projects within the framework of "heritage," historical particularism characterizes most of archaeological practice today. None of these practitioners tried to overthrow Lewis Binford, he was simply irrelevant to their profession. Where is American archaeology at today? Take a look at SRI's website <http://www.sricrm.com/>, the mission statement[9] for a multimillion dollar business led by Jeffrey Altschul, a 2011 candidate for presidency of the Archaeology Division of the American Anthropological Association. Ironically, SRI is the acronym for the company's original name, Statistical Research, Inc.; Altschul long ago outgrew equating that with archaeology.

Susan Trencher lamented that the late-twentieth-century generation of "postmodern" anthropologists represent a retreat from truly engaged scholars, from the tireless civic responsibility exemplified by Boas and Mead, to a "me generation" deriding past practices, overweeningly confident in their own capacities, seeing no need to advocate for the less-privileged "Others" they wrote about (Trencher 2000: 188-189, 191 n. 6). Binford was a member of this generation. He seems to have seen himself as pure scientist, objectifying the several hundred small nations, nearly all in colonial situations, he termed "foragers." NAGPRA, WAC, "indigenous archaeologies," First Nations' struggles, were outside the science that he advocated. He took no part in the Society for American Archaeology's heavily attended debates about NAGPRA and about accepting non-academic, especially non-Western, histories and interpretations of data.

Objectifying small non-Western nations as resources for quantifiable data on our remote ancestors is nineteenth-century archaeology, like John Lubbock's 1870 *The Origin of Civilisation and the Primitive Condition of Man*. There is, of course, an unconscious racism in this supposedly scientific attitude, tellingly described by Choctaw archaeologist Joe Watkins (Watkins 2000, 2010). Reflecting on his graduate studies during the heyday, 1960s, of the Binfords' assault on the discipline, Watkins concludes that "the 'hard science' its practitioners felt it needed to be... [was] pseudo-science, social science, or non-science... Its practitioners were afraid to admit they were humanists rather than scientists" (Watkins 2010: 322). Perhaps Watkins should have phrased it, "they were all too human, socialized into racism".

I read deeply into history/philosophy of science in the 1970s and 1980s in an effort to understand the loudly touted New Archaeology. In 1989, I took a sabbatical in Edinburgh in order to research Daniel Wilson's creation of "prehistory", and equally valuable, to discuss archaeology from the standpoint of sociology of science with the "Edinburgh School", Barry Barnes, David Bloor, and Steven Shapin. Joe Watkins and I are not just friends, we have been outsiders watching the emperor parade at the head of his horde of admirers. Standing with us are several dozen archaeologists who are members of First Nations, and others who, like me, hang out with collaborators in First Nations communities—not for brief shepherded visits but year after year. Now the parade has passed, its emperor entombed in his massive unreliable database culled without evaluation of colonial effects. The field is free for an empirical archaeology that begins with the syntagm in the ground and moves along a careful chain of signification to a paradigm drawn from rich compendia of ethnographic and historical data, nuanced by firsthand experience with First Nations collaborators and postcolonial appreciation of their histories.

NOTE

For a fuller treatment of Binford's philosophy of science and the New Archaeology, please see my *The Land of Prehistory* (1998), chapter 7, pages 115-149. Some of the book can be read online on Amazon (Look Inside).

About the author

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[9] "SRI was established in 1983 by [Dr.] Jeffrey H. Altschul to provide a vehicle for creative people to do interesting and exciting work on the human condition. In meeting the goals of this unique mission, we respond to our nation's goal of preserving its diverse historical and cultural values by integrating exciting research with compliance work."

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