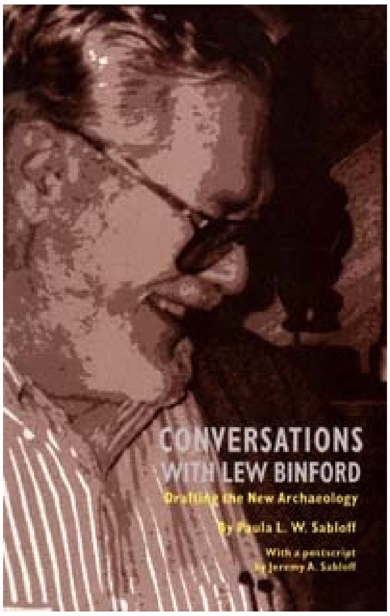

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Conversations with Lew Binford

Drafting the New Archaeology

By Paula L. W. Sabloff

With a postscript by Jeremy A. Sabloff

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Lovingly dedicated to
Joshua and Saralinda Sabloff,
who so enriched our adventures,
and to the memory of Tikal,
who learned "wolf" at the feet of the master

Contents

Illustrations

Illustrations following text page 70

Preface

In 1982 I spent about six hours interviewing Lewis Binford in his office in the Department of Anthropology at the University of New Mexico. Since my husband, Jeremy Sabloff, and I had moved to Albuquerque (in 1978) and he had joined the department, we had become friends with Lew and his wife, Mary Ann. We spent many comfortable afternoons at their Corrales house, with Lew showing our young children his rabbits, turkeys, and sheep, and Mary Ann teaching them about the flowers and vegetables in her garden.

These visits would end with us sitting on the chaise lounges in the patio, sipping iced tea with a sprig of mint from the garden and discussing some issue of political or anthropological immediacy I used to enjoy arguing with Lew just to see if I could hold my own for even a little while. Just when I felt backed into a corner, I would say, "But, Lew, how are you defining this term?" He would tell me, and I would reply, "Oh, I was defining it differently; we are really on the same side." And we would both be content to go on to another subject.

Why would Lew argue with mea sociocultural anthropologist who had been trained in archaeology on the undergraduate levelin the first place? Only Lew can answer that; but I think that his mind craves constant stimulation, and he will talk with whoever is handy. He needs intellectual challenge the way all of us need water to replenish our bodies; it is that essential to him.

It was clear to me that Lew has an active mind coupled with a high level of curiosity. Any situation became a learning opportunity for him. My husband and I had proof of this one day while Lew was visiting in our kitchen. We had recently acquired a puppy, a cross between a Labrador retriever and an Irish setter, whom we named Tikal. Lew informed us that while he was doing fieldwork in Alaska, he taught himself "wolf" by listening to the cries of these animals during that field season. "Sure," we said skeptically. He replied, "Let me show you." He looked Tikal in the eye and began to howl. The poor puppy went skittering out of the room with his tail between his legs, clearly cowed. The next minute, Lew barked quietly, and Tikal came and put his head in Lew's lap. Lew alternately howled and barked softly, causing the poor animal to retreat and advance depending on the call. "Okay," we agreed, "You have, indeed, learned wolf."

The more I knew Lew, the more I wondered how he had come to challenge the entire archaeological establishment and develop the New Archaeology. Why did he do it? What did he not like about archaeology when he studied it? Where did he get the strength of character to buck an entire disciplinethe one in which he planned to earn his living? Who influenced his intellectual growth? Who stimulated the new approach that he proposed for archaeology?

I decided to interview him in order to find answers to these questions. My plan was to use parts of the transcription of the interviews for an article in a popular science magazine. Lew agreed to the interviews, understanding my goal.

I never submitted the article to a magazine, one reason being that the editor of *Science '82* informed me that he never accepted a biographical article written by a friend of the subject. Since this initial, discouraging experience, I have been looking for the right place to publish the interviews. Hoping to keep them as close to the original as possible, I decided that they should stand on their own merit: they allow the reader to learn Lew's answers to the questions posed above. In this sense, the reader must remember that the book is more autobiography than history.

In chapter 1, Lew reminisces about his graduate years and describes how his intellectual development was stimulated by various institutions, faculty, and fellow students. In recounting his graduate training at the University of Michigan and his first teaching assignment at the University of Chicago, Lew brings to life the context in which the New Archaeology was developed as well as his view of its historical development. He discusses the Darwin Centennial, the differences between Leslie White and Julian Steward, as well as the place of Walter Taylor, Albert Spaulding, François Bordes, Robert Braidwood, and James B. Griffin in the development of the New Archaeology. Lew's description of these years allows the reader to understand his relationship to the growth of modern archaeology.

Chapter 2 is Lew's description of the New Archaeology and its evolution from the late 1950s to the time of the interview, 1982. This conversation lays out the key concepts behind his concerns about archaeology. Because he was not talking to a professional archaeologist, his explanations are in plain language, not the formalized language used in some of his scholarly works.

In chapter 3, Lew explains the early influences on his intellectual and emotional lifehis parents and other family members; the Boy Scouts; and a very special teacher, Mrs. Henning. The foundations for the New Archaeology were laid during this periodLew's youth, college years, and military service. It is clear that the strength of Lew's convictions began to develop at an early age.

I have preserved the tone of the interviews by keeping the transcription as close to Lew's actual words as possible. Once in a while his speech becomes ungrammatical, as happens to all of us when speaking extemporaneously; and sometimes Lew's mind races well ahead of his wordsnew ideas may suddenly enter a sentence in midstream. I hope that those who know Lew will actually be able to hear his speech patterns as though he were talking directly to them. Any omissions, due to minor editing or my inability to understand the tape recording, are indicated by ellipsis points.

The original interviews were more like conversations than interviewsfree to wander when an important idea was raised. I felt that some of the material needed to be rearranged so that all the ideas about the New Archaeology would appear in one chapter and so that Lew's life story would retain chronological continuity. Due to this slight rearrangement, my original questions sometimes needed clarification, and so I changed some of my own words. I did not alter Lew's words without informing the reader, unless it was to make the grammar understandable. Any phrases or words that I added are in brackets.

What excited me in conducting these interviews was Lew's unraveling of his storyhis personal development as a scholar and how he came to challenge the entire field of archaeology. Lew Binford is widely regarded as a seminal thinker, the man who pushed archaeology off the path of culture history and onto the path of scientific scholarship. These interviews offer Lew's insights into how he became that person.

The conversations are followed by Jeremy A. Sabloff's historical evaluation of Lew's contribution to archaeology along with lists of his publications and fieldwork and a section of suggested readings by anthropologists and archaeologists of note. These all give context to the interviews.

I would like to thank Lew for his help, encouragement, and friendship even though we have not been in touch since we all left New Mexico. Special thanks go to Nancy Medaris Stone, who has been important as a long-distance friend and the sheepherder of manuscript and photographs past Lew. I wish I could thank Mary Ann personally rather than posthumously, both for her insights into Lew and for her incredible example of modern womanhood. She had a full-time career in an area that made a tremendous contribution to young people's lives (among other things, she founded

the kindergarten program in Albuquerque), was knowledgeable about her hobbies, and was an insightful and loving wife. She had kindness for everyone no matter what their age. Her untimely death hurt us all. Thanks also to Jeremy Sabloff, my husband, who made this project possible through his encouragement and contribution, and to Bill Woodcock, Merrilee Salmon, and Kathleen Allen, who read the initial manuscript and saw its merit. Finally, I would like to thank all the Mrs. Hennings of America, the teachers who saw more in us than we saw in ourselves. They made us better scholars as well as better people through their extra efforts.

PAULA L. W. SABLOFF
PHILADELPHIA, PENNSYLVANIA

1
An Intellectual History of the New Archaeology

Why did you develop the New Archaeology? Were you ever disgusted with old archaeology or was it a question of stimulation?

Initially, I kept saying, "What's wrong with me? There's got to be more to this than what I'm seeing. So there's something wrong with me." My assumption was that there had to be much more to the field than I as a student was seeing. There must be something wrong with my scholarship or whatever, or that I was dense, because I had had all this background in biology and other fields, really, and I kept looking for the complexities and I kept not seeing them in the literature. So I assumed, okay, this is a small field; argument is handled in face-to-face situations and meetings and so the literature is impoverished, not the field. And I kept looking for where the hidden complexities were and all the time basically learning that they were not there.

I went to college initially at VPI [Virginia Polytechnic Institute] in Virginia in wildlife management and wildlife biology. That is a technical school, so it has an enormous work ethic in the sense that it was a live-in school, a military school. We had these rigid schedules. You wouldn't believe the work schedule we had. I had this really rigorous background in math, biology, physics, chemistry, and all the basic natural sciences long before I ever even thought of going into anthropology. And when I went into anthropology, I kept looking for, Where are the roots? Where is the basis of this that we are all supposed to be doing? I kept initially questioning myself"Well, I must be dumb, I can't see what is going on"rather than getting upset about the field. Also I had a slightly different set of expectations because when I was in the military, I worked for some anthropologists as their interpreter, in Japanese. And so I got introduced to anthropology through ethnography.

I had this historical orientation out of biologyevolutionary. I wanted to know where forms come from, so once I began to see human societies like speciesthis one's different from that one; how did it get that way?I sort of naturally moved to asking a historical question: How did this one get to be the way it is? And when I started readinginitially I went into anthropology to do ethnography, not archaeologywhen I began to read ethnography, I realized that this is not going to work. The only primary record we have is archaeology, a social science. And all this culture-historical reconstruction stuff that you read from ethnography I did not think was very useful.

What time period are you talking about?

This was during the time I went to grad school [at the University of Michigan].

Who are you referring to?

[Alfred L] Kroeber and all the classic anthropologiststhat was the dominant thinking in ethnography when I went in. [A. R.] Radcliffe-Brown was talked about when I was a studentthis functional approachesbut American archaeology was essentially historical reconstruction from ethnographic case material. It was not the more psychological stuffI mean that was on the scene, culture and personality was there, but still the workhorse of American ethnography was trying to find out where the

Indians came from[those] kinds of problems. So I became convinced that if me really want[ed] to know how these systems came into being, we would have to study the archaeological remains. Well, the irony was that what had led me to the conclusion that ethnography was not going to help was that [the ethnographers'] methods were not historical in fact. That is, you have to assume what you want to know in order to interpret the ethnographic record historically. And I went into archaeology, and what archaeologists were doing was using the methods of the ethnographers to make historical inferences. So right away I just rejected thisit was an intellectual thing that I thought was silly. If we are going to try to understand the past, we cannot do it with these conventionsthat things that look alike must have common ancestors. Cladistic maps of culture based on simple measures of similarity and differencesI didn't think we could work that way.

But wasn't glottochronology valid?

I had studied glottochronology but I did not think it had any [validity]. . . . I just saw that as another silly example of the ethnographers' methods. Glottochronology did not workthere is no way to date a word. If you standardize [a word] on all your records, then, of course, it fits your data. You do not know whether it works on the cases you do not have control on or not. So it is like standardizing against a set of data, then it fits that data set. But the degree that it fits any others is not clear; it's taken on faith.

Anthropology always billed itself as a social science. So you entered anthropology expecting to study another science, again. Is that true?

Not really. I did not have a notion of science with a capital S. But I did expect the reasoning processes to not be tautological, and that you [would] be concerned with evaluating your ideas.

And what I found were all these conventions for interpreting things. You had to take the convention on faith and then everything fell into place. Like, how do you know whether migration occurred? The conventions were laid out very clearly in anthropology that if you had [trait] similarities appearing in a region that seemed to have no predecessors, then [the traits] had to be introduced. Now if there was a group of them [clusters of similar traits], then this meant that a group of people came in and brought a whole bunch of stuff along with them. On the other hand, if it was just one or two traits, then it was probably diffusion.

These were the conventions for interpreting the archaeological record, and in a sense you had your observations over here and your conventions over there. You just fit your observations to your conventions. Then you put them together. Your conventions literally gave conventional meaning to what you saw. And that was something that I was not willing to work with right from the very beginning. That seemed to me to be a truly alien way of thinking.

In the work that I had done as an undergraduate at VPI, almost every professor I had would have set this up as an example of prescientific thinking. It was shown to be the method of thinking in alchemy when we studied chemistry. This was the way they used to do it, and we do not do it that way any more. That was the kind of framework. I remember a course I had in developmental biologyin the development of biological concepts. We started with Carolus Linnaeus [17071778]. Well, the teacher lectured about Nils Ingebar (I think), who was Linnaeus's father. (They have a system in Scandinavia where you take your name from your mother.) It turned out that he, in fact, had written this huge, ten-volume treatise on plants and animals of the world. And he is Linnaeus's father, so obviously this is of interest. How did he classify the animals? He classified all the animals and plants in terms of the roles that they played in curing people. So you had all the plants that were thought to bring the *calor*the heatof the person. They were one kind of plant. And all the plants that caused coolness in peoplethey were another kind of plant. Then you had animals classified by the roles that they played in witchcraft. Frogs were used by black witches . . . so here is a whole system of classification in which the meaning given to the taxons is taken from the functions of that society. And here Linnaeus comes along and he says, "Look, this does not workthis is not the way to classify. We are going to take the strict morphology, and then the meaning of those things is not given by the way they are classified."

I can remember a long lecture on the difference between Linnaeus and his father in approaching biology. To me there was a clear analogy to the way in which lots of taxonomic classes were generated by anthropologists such that the meaning for the class literally was an ethnographic analogy to man's experience in the modern world. That, here is a scraper. Okay, I define a scraper in the following way. Therefore, that one over there is a scraper and that is what it was in the past. It is exactly like Nils Ingebar's reasoning about plants. I had had all this kind of stuff taught in biology and chemistry and physics at VPI, where we had a lot of history of ideas really taught. They would say, this is the way we did it in the Dark Ages, and this is the way we approach it now.

Physics was interestingso was chemistrybecause that era in the late '40s and early '50s was when there was a tremendous expansion in the number of elements thought to exist. I think when I started in school there were 89 elements, and when I finished there were 96 Now there are over 100.

One of the things that the teachers talked about was the impact of adding new knowledge where no new knowledge was thought needed. In other words, there is a big difference between recognizing that you are ignorant about something and seeking to correct it so you expect new knowledge versus a situation in which you think knowledge is self-containedyou do not need any more, and it comes to you anyway and impacts. They were forever using the implication of the recognition of the new elements in terms of the whole notion of compounds, you see. This seemed to be a really interesting intellectual thing to me. When I got into archaeology, I said, okay, all these people are saying all these little conventionsthey are finite. They cover all the possible cases. It could be migration, it could be this, it could be that. Let us suppose we introduce a new possibility that nobody admitted to the system before, like functional variability. How would that impact all the other conventions? They all collapse really quick. They are not only redefined, but they basically collapse as a set of conventions because it is not just a convention of what the world is like; it is the conventions for identification, because you are dealing in archaeology with how you recognize something. So, if you admit that something else not included in the system is a possibilitythat is, you admit ambiguitythen all your definitions are no longer finite; they all fall to pieces. That is really whatthe way I had been trained to think about things, when I had gone to college initially.

Back to the years at Michigan:
Why did you question Julian Steward's approach?

Well, that wasn't me. I adopted [Leslie] White's criticism of Steward, I suppose. The most competitive people are the ones who do the most similar things. Since Steward was viewed by many Americans as advocating evolution, and so did Leslie White, of course the person that White criticized more [and] with greater interest and vehemence was Steward. And so StewardLeslie's criticism of Steward was very instructive because it was the first time I had ever heard in anthropology a criticism that rang true in terms of the things I was talking about a minute ago. It sounded like what I had heard when I was in biology and physics. It was a criticism that was in terms of the logic and approach to learning rather than a criticism . . . that he didn't know this or he didn't know that. White's criticism of Steward was about how to do science. And that really rang with me; it impressed me.

So my reading of Steward. . . . Steward's anthology [*Theory of Culture Change*] came out in the '50s [1955]. It was not something that was available when I was a beginning student. I read it and discussed it during the time that I was a graduate student, I guess. He was at University of Illinois-Urbana. When his book came out, Leslie was all over it. We all had to read it, but we read it in the context of Whites criticism. That really turned me onWhites criticism, verbally in class and otherwise, of Stewardbecause it was really the first time I saw anyone in anthropology talking about how to do science. And I liked that.

You had a special relationship with White?

It was the year of the Darwin Centennial [1959].* I spent a good part of that year in Chicago because Leslie [White] was very active in that and his wife was ill in a Chicago hospital. He did not know how to drive. So I was White's driver; I drove him back and forth between Ann Arbor and took him everywhere in Chicago at the Darwin Centennial.

What did they do for the Darwin Centennial?

Oh, my God. For the centennial, Chicago brought every person ever heard of including Darwin's offspring, [Thomas] Huxley, et

* See Sol Tax (editor), *Evolution after Darwin: The University of Chicago Centennial* (1960) for the papers presented during the centennial celebration.

cetera, and anybody who was doing basic research that could in any sense be said to be a derivative of Darwin's ideasso it was all on the evolution of modern man. So it was all these Old World people that were researching[Louis B.] Leakey and all his bunchand all the people who were doing research on human evolution were all brought to Chicago for the Darwin Centennial. It went on for weeks with endless press releases, and newspaper [coverage], and television appearances, and debates, and all the religious people had to get their act in and come up and have debates with Huxley and Charles Darwin II. It was a show.

And White was involved in this very, very heavily. He gave his Sputnik speech there, which hit every newspaper in the country. Within weeks there were bomb threats and they tried to blow his house up in Ann Arbor. He said that he thought it was not an accident that the only modern nation to officially disclaim God as part of their political structure was the first nation to put up Sputnik. He did this in the context of arguing about the relationship between religion and technologywhat causes culture change. Well, my God, every newspaper in the country: "University of Michigan professor says godless communism will succeed because.. . ." And so every fundamentalist outfit in town was sending bomb threats. It was astonishing.

Anyway I was his driver. It takes about six to seven hours to Chicago. Mary [White] all during this time was deathly ill with cancer in the hospital in Chicago. So he was going back and forth not only for the Darwin Centennial but because of her illness. She then died there. White's presenceI met all of the great men, literally; and most of them were still alive [at that time]. I met Kroeber; I spent hours listening to him and Leslie talk. This kind of stuff. It was a tremendous thing. White introduced me to all kinds of people at the conference.

I have never heard of Fay-Cooper Cole, even though I was an undergraduate major and started grad school in archaeology.
And yet you talk about him a lot.

Yeah. As I had learned archaeology, Fay was basically said to be the man who started "modern archaeology" in that he tried to develophe took the first steps of which Spaulding was still taking. That is, how you record the archaeological record in an accurate way rather than impressionistically. How do you excavate so as to see what you are digging through rather than just dig for things? All of these major changes in field method were basically referable to Fay-Cooper Cole; I mean he started them all, most of them.

He started this as a program out of Chicago in about 1928 with the experimental fieldwork in the upper Illinois Valley'27 to '28. They learned an awful lot about how to dig. Almost all of the people who later ran the WPA projects* were trained by Cole in the '20s. So Cole's techniques became the standard for good archaeology during the WPA [era]. Some really incredible worklike that done by Madeline Kneberg, and [David] DeJarnette's excavations in Alabama, which are still superbwere done in terms of standards that were being discussed and pioneered by Fay-Cooper Cole in the Upper Valley in the '20s.

Almost all of in a sense putting American archaeology on a methodological footing was started by Fay-Cooper Cole. Everybody who thought of themselves as increasing the methodological skill of archaeology saw the history of it in terms of Cole and his training and his influence, and it continues into the present. I thought of myself that way in several ways. First, I thought of trying to continue the progress. Almost all of the

* Work Projects Administration, 19351943. The WPA was a New Deal agency that undertook extensive building, artistic, and educational projects, including archaeological excavations, during the depression.